AN

ACCOUNT

OF THE

REV^{D} JOHN FLAMSTEED,

THE FIRST ASTRONOMER-ROYAL;

COMPILED FROM

HIS OWN MANUSCRIPTS, AND OTHER AUTHENTIC DOCUMENTS,

NEVER BEFORE PUBLISHED.

TO WHICH IS ADDED,

HIS

BRITISH CATALOGUE OF STARS,

CORRECTED AND ENLARGED.

BY FRANCIS BAILY, ESQ.

VICE-PRESIDENT OF THE ROYAL ASTRONOMICAL SOCIETY,

FELLOW OF THE ROYAL, LINNÆAN, AND GEOLOGICAL SOCIETIES, VICE-PRESIDENT OF THE ROYAL GEOGRAPHICAL SOCIETY, ONE OF THE VISITORS OF THE ROYAL OBSERVATORY, HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, CORRESPONDING MEMBER OF THE ROYAL ACADEMY OF SCIENCES AT NAPLES, ASSOCIATE OF THE ACADEMY OF SCIENCES AT PALERMO, FELLOW OF THE AMERICAN ACADEMY OF ARTS AND SCIENCES, ETC. ETC.

PRINTED BY ORDER OF THE LORDS COMMISSIONERS OF THE ADMIRALTY.

LONDON: 1835.

TABLE OF CONTENTS.

TABLE OF CONTENTS.

PART II.

Containing the British Catalogue, &c.

PREFACE.

SOME time during the year 1832 I was informed that an opposite neighbour of mine (Edward Giles, Esq. No. 5, Tavistock Place) was in possession of a large collection of original manuscript letters, written by the celebrated Mr. John Flamsteed to his friend Mr. Abraham Sharp, who had formerly been his assistant at the Royal Observatory at Greenwich, and who made the mural arc then in use. These letters were found some years ago, at Mr. Sharp's house, in a box deposited in a garret, filled with various books and papers; and Mr. Giles was good enough to send them over to me for my perusal *. I immediately recognised the hand-writing of Flamsteed, and found that they contained much interesting and original matter, connected with his astronomical labors, not generally known. The whole collection (at least that part of it which relates to Flamsteed) consists not only of the letters written by Mr. Flamsteed (124 in number), but also of one letter from Mrs. Flamsteed, and 60 from Mr. Joseph Crosthwait, his assistant likewise at the Royal Observatory; all addressed to Mr. Sharp, who at that time resided at Little Horton, near Bradford in Yorkshire, on an estate of his own †, where he lived a very secluded life, passing most of his time in astronomical calculations. When Flamsteed set about reducing his observations, he requested Mr. Sharp to undertake the computation of the places of several of the stars in his catalogue, and also of the moon and planets, from the original

* Mr. Giles is since deceased; and the letters are now the property and in the possession of his widow. They had been hitherto kept *loose* in a portfolio, but are now bound up in one thick volume.

† These letters seem to have been all sent by the general post; the office-stamp of which they respectively bear. They are undoubtedly the same as those mentioned by Dr. Hutton in his *Mathematical Dictionary*, under the article "Sharp;" and they appear, from what is there stated, to have been seen and examined at various times by several persons, not one of whom however has given us any particular information as to their contents. I have recently caused a copy of all these letters to be taken, which has been presented to the Royal Astronomical Society, and is now deposited in their library. This copy has since been collated with the original manuscripts by two of the Members of the Council of that Society.

observations: and an extensive and friendly correspondence was thus commenced and kept up between them till the time of Flamsteed's death; which was afterwards continued with Mr. Crosthwait, who superintended the printing of Flamsteed's works after his decease. This correspondence embraces a variety of subjects: but the principal, the most novel, and the most interesting, is the account of the repeated difficulties and impediments which delayed and almost prevented the publication of the *Historia Cœlestis*; and the new light which it throws not only on the history of that transaction, but also on the whole of Flamsteed's labors in the science of Astronomy. Of this I shall speak again in the sequel.

In the meantime, having recollected to have formerly seen, at the Royal Observatory, some manuscript papers originally belonging to Mr. Flamsteed relative to this subject *, I proceeded thither to examine them more minutely, in order to see if any additional information could be obtained on this point; the Astronomer Royal kindly affording me every assistance in the pursuit of my inquiries. To my great surprise and delight I found there a vast mass of MS books, papers and letters belonging to Flamsteed, which had been lying on the shelves of the library for the last sixty years, unnoticed and unknown. These manuscripts were purchased by the late Board of Longitude in 1771, for the sum of £100, at the suggestion or recommendation of the Royal Society †. At the time that I discovered them, they were in great confusion and disorder: the major part of the books had lost their covers, most of the letters and papers were loose and scat-

* See *Memoirs of the Astron. Soc.* vol. iv. page 137; where allusion is made to a manuscript fragment of the history of Flamsteed's life, which is now bound up and preserved in MSS, vol. 32 D. It is not in Flamsteed's hand-writing as I have there supposed; but is a fair transcript of the statement which was written by him.

† The following entry is to be found in the *Council* book of the Royal Society: viz.

"November 29, 1771. The President [James West, Esq.] mentioned that he had acquainted "the Commissioners of Longitude with the large collection of manuscript volumes and papers of "the late Mr. Flamsteed, mentioned by Mr. Raper, and lately found at Islington by Mr. Belchier, "and brought to the house of the Royal Society; and that the Commissioners had ordered their "Secretary to pay the sum of £100, being the price for the whole, demanded of Mr. Belchier. "And that the said volumes and papers be examined by the Astronomer Royal [Dr. Maskelyne] "and Mr. Robertson, to select such as related to the Royal Society, to be kept in their house; and "the rest to be deposited in the Royal Observatory for the benefit of Astronomy and Navigation."

tered about, and those which were pasted into guard-books were very ill-arranged, and moreover fastened with such a *mass* of paste, that they were literally mouldering away. Amongst the confused heap, I was fortunate enough to find a *catalogue* of these manuscripts, apparently in the hand-writing of the late Dr. Maskelyne, or compiled under his superintendence. This catalogue is by no means arranged with any method, and I suspect that this distinguished astronomer had not examined very minutely the contents of the several volumes: it assured me however that very few (if any) of the manuscripts were missing, and that they were nearly in the same state as when they were first deposited at the Observatory.*

My first object was to detach the letters from the guard-books, and to free them from the injurious effects of the paste, which was visibly destroying the color of the ink and the texture of the paper: then, having arranged them according to their subjects and their dates, I caused them to be neatly bound, in order that they might be conveniently referred to hereafter. The other parts of the manuscripts (that were loose) were treated in a similar manner, and bound up in different volumes according to their contents; the books also were repaired; and the whole collection lettered and numbered in regular order, agreeably to the *Catalogue* which will be found at the end of this Preface. In this manner the several volumes may be readily and conveniently consulted at any future time; and it is in this manner, and according to this arrangement, that I have referred to them in the several quotations that I have found it necessary to make in the progress of the present work.

These manuscripts, which I consider of inestimable value in an astronomical point of view, ought certainly to be (and no doubt in future will be) preserved with great care. They contain, amongst a mass of valuable matter, the *original entries* not only of Flamsteed's astronomical observations made at the Royal Observatory, but also those which he previously made at Derby and at the Tower; as well as duplicate copies of the same; a great variety of computations connected with his astronomical labors and researches, more especially those from which

* This Catalogue is preserved in the box MSS, vol. 67. See page lxxiii.

the *British Catalogue* has been deduced; several of his catalogues, in various states of progress; many particulars relative to the history of his own life; the *original preface* that was intended for the third volume of the *Historia Cœlestis* but which was suppressed by his editors; a vast collection of letters from various individuals in this and in foreign countries, amongst which are *nearly the whole of Mr. Sharp's answers* to those of Flamsteed already mentioned in the early part of this Preface; together with much other matter, the nature of which will be best learnt from an inspection of the catalogue above alluded to: nearly all of which (with the exception of the correspondence) are in Flamsteed's own hand-writing.

Having minutely examined the whole of these manuscripts, I soon found that the character of Flamsteed had not been fully developed by his biographers; that these documents opened a new view of the great obligations which are due to him for his unparalleled exertions in the cause of astronomy, in the midst of vexations and difficulties that would have weighed down a mind of a less powerful temperament; and that they exhibited him in a light very different from that in which he has been generally viewed. Instead of the mere selfish and indolent observer, pursuing his observations at his own ease and for his own amusement, regardless of his fame, and unwilling to communicate the result of his labors to others, as some of his contemporaries and even his more recent biographers have too incautiously represented or insinuated him to have been*,

* In the *Dictionnaire Historique du Moreri*, published in 1759, it is stated, under the life of "Flamsteed," that his Observations were about to be published by the English Government, in 3 volumes; *but that the quarrel with Newton, who had found many of his observations incorrect, being brought before the Academy of Sciences of Paris, that learned Society decided in favour of Newton, which decision stopt the progress of the work!!!* It is needless to answer an accusation so totally devoid of truth. Moreover, in the *Biographie Universelle*, published in 1816, it is stated, under the life of "Flamsteed," that the public were very desirous of seeing his Observations printed; *but that, from the character of Flamsteed, this desire was a reason why they should not expect it from him: and the English Government was obliged to use its authority, by directing Halley to supply that which the author would not give.* Mr. North also, in his life of the Lord Keeper North, has indulged in a vein of sarcasm and misrepresentation evidently intended to depreciate the character and labors of Flamsteed, of whose true merits he appears to be wholly ignorant. And Sir David Brewster, in his recent life of Newton, has (by a singular error, to which I

we find him not only actively employed in making and dividing his own instruments, with his own hands, and at his own expense, but also devoting his spare hours to the investigation of the lunar and planetary theories, suggesting remedies for the various anomalies that he too frequently met with, forming tables for the more accurate computation of their places, and communicating the result of his inquiries with the greatest readiness to those who were prosecuting the same studies; at the same time struggling not merely with illness, but with difficulties and obstructions of various kinds. But that which I consider, in these manuscripts, as of most importance to the practical astronomer of the present day, is the discovery of the original computation book above mentioned, from which the major part of the *British Catalogue* has been deduced; and which has enabled me not only to detect many errors in that catalogue, but also to discover the source of them, and thus correct them with more confidence. It was under an impression that so much additional light might thereby be thrown on the history of Flamsteed's life and labors that I drew up a representation of the facts here stated, and suggested the propriety of republishing the *British Catalogue* with such amendments and additions as might thus be afforded; to be accompanied with such extracts from these newly discovered manuscripts as would tend to exhibit the character of Flamsteed in its true and proper colors: offering at the same time not only to select the requisite portions of the MSS, but also to make the necessary corrections and additions to the catalogue, and to superintend the printing of the work. This representation was contained in a letter which I addressed to His Royal Highness the Duke of Sussex, President of the Board of Visitors of the Royal

have alluded more at length, in page xxxiii) exhibited Flamsteed also in a character which he by no means deserves, and which indeed is totally at variance with Flamsteed's whole history.

From such partial statements and unfounded remarks, there is perhaps no person whose private character has suffered so much as Flamsteed's. At the time of his death, his two powerful opponents were *Lords of the Ascendant*, and standing deservedly high in public estimation; and no friendly hand was stretched forth to stop the idle rumours that had been propagated relative to his temper, his labors, and the utility of his pursuits. Even the *vindication*, which he left behind him, was suppressed; and chance only has now first brought it to light. I trust that the present volume will remove all these false impressions.

Observatory, by whom it was (at the Visitation in June, 1834,) ordered to be transmitted to the Lords Commissioners of the Admiralty, with a recommendation that it should be carried into effect: with which recommendation their Lordships were pleased to comply, and likewise to order that the work should be printed at the public expense. Such is the origin of the present volume, which is divided into two distinct parts: the *first* containing Flamsteed's history of his own life, as drawn up from his own manuscripts and papers, confirmed and illustrated by various other documents given in the Appendix: the *second* containing his British Catalogue corrected and enlarged, with an account of the mode in which the same was constructed, and of the various alterations and corrections now made therein. As it is not my intention to disturb the order or spirit of Flamsteed's narrative, but to give every part of it *in his own words*, it will perhaps be best previously to give a short abstract of the history of Flamsteed's life; with reference principally to those points connected with these manuscripts, which are not so fully alluded to by his biographers.

The best and only authentic account of the life of Flamsteed (as far as it extends) is that which is given in the *General Dictionary**; the materials for which appear to have been furnished by Mr. James Hodgson, who was formerly his assistant at the Observatory, and whose family, in consequence of his marriage with Flamsteed's niece, eventually inherited a great portion of his property. Those materials however were very scanty, and consisted only of the two MS papers which form the first and second divisions of Flamsteed's autobiography, given in the first part of the present volume, (viz. *The self-inspections of J. F.* and *Historica narratio vitæ meæ*; see page 3:) together with some letters which were written in the early part of his astronomical career to Mr. John Collins. At least this is all that the biographers acknowledge to have received; and from which they extracted such portions as they conceived would be most interesting. Why Mr. Hodgson, who was so deeply interested in Flamsteed's fame, should have kept back or suppressed those important docu-

* See the different titles, under which this work is quoted, in page 3 of the present volume.

ments in his possession which throw so much light on Flamsteed's true character; and which refer—to his labors in rectifying the places of the sun, moon, and planets—to the vast mass of computations which he made in order to correct not only those places but also the positions of the fixed stars—to the vexations and annoyances which interrupted the peaceful and steady pursuit of these subjects, from those whose duty it was to have acted otherwise—to the personal sacrifices of time, of money, and of health, which he made to carry on his observations with effect, to deduce the most important consequences therefrom, and afterwards to print them at his own expense—to the opposition he met with, and the repeated contests which he had with Sir Isaac Newton and Dr. Halley in the prosecution of these measures—and to the vindication which he left behind him, in order to justify his conduct to posterity—why these and many other interesting portions of his history were withheld, I am at a loss to imagine: unless indeed it were, that the editors of that voluminous and respectable work might not have thought it prudent or politic to risk an article reflecting in such strong terms on characters so distinguished, and standing so deservedly high in public estimation, as Newton and Halley*. Probably the same motives might also have induced Mr. Hodgson to withhold from publication that portion of the Preface to the *Historia Cœlestis*, which Flamsteed had expressly drawn up for his own vindication, and which appears to have been actually written out for the press during his lifetime; a portion of such copy being still in existence amongst Flamsteed's MSS. These personal motives however have long passed away, and now cease to exist: and however unpleasant and painful it may be to an enlightened mind, to find two such eminent characters as Newton and

* That some feeling of this sort must have guided their conduct is, I think, evident from the fact that Dr. Birch (one of the editors of the *General Dictionary*) was furnished with copies of *all* Newton's letters in the library of Corpus Christi College, Oxford, alluded to in page xxi: and which *copies* are now in the British Museum, and designated by the title of Birch's MSS: a few of which letters only are inserted in the life of Newton. On comparing these with other copies from the *originals*, furnished me through the kindness of Professor Rigaud, I have observed a few slight variations, sufficient to convince me that Dr. Birch's copies had been written in great haste, and not afterwards collated.

Halley mixed up with subjects of the kind to which I shall presently allude, and pursuing a line of conduct towards Flamsteed, which tends to make them appear less amiable in our eyes, yet a proper regard for truth and justice prevents any suppression, at the present day, of the many curious and important (though often at the same time lamentable) facts which these manuscripts have, for the first time, now brought to light.

I have indeed, in justice to the parties here alluded to, endeavoured to procure information of a contrary tendency from various sources, and sought for documents which might tend either to extenuate and explain the conduct of Newton and Halley in these proceedings; or to throw some light on the origin and nature of the quarrel that, at a certain period of this history, existed between Flamsteed and his two distinguished contemporaries: but, notwithstanding all my researches, I regret that it has been hitherto without success. Through the obliging civility of Sir Henry Ellis I have had free access to all the documents which the British Museum affords; Professor Rigaud also has been good enough to search the several libraries at Oxford, where information was likely to be obtained; by the kindness of the Rev. W. Whewell and the Rev. G Peacock I have examined the collection of Newton's MS letters which are preserved in the library of Trinity College, Cambridge *; and by the interest of Walker Skirrow, Esq. I have been permitted to inspect the large and valuable collection of Newton MSS belonging to the Earl of Portsmouth, now in the custody of H. Fellowes, Esq. who was so obliging as to afford me every facility for that purpose: but at none of these places have I been able to throw any light on the special object of my inquiries; although I have in the course of my search met with several interesting documents of another kind,

* In this latter collection there is a letter from Mr. W. Jones to Mr. R. Cotes, dated Sept. 17, 1711, wherein he states that "Dr. Halley has almost finished the printing of the Greenwich Obser- "vations, which will be a work of good use, *especially as it is now freed from the trifles it was* "*loaded with:*" a flippant remark, evidently caused by the rumours of the day; for Mr. Jones was a man of too sound a judgment to have used such an expression, had he *known* the merits of the case.

which have never yet been published, and which I have considered of sufficient importance to form part of the present volume*.

Flamsteed was always, from early life, of a very pious and religious turn of mind; he very soon decided on entering the Church; and, at a more advanced period, could scarcely be persuaded from devoting himself *entirely* to the duties of a minister. "My desires (he says) have always been for learning and "*divinity:* and though I have been accidentally put from it by God's provi- "dence, yet I have always thought myself *more qualified for it than for any* "*other employment;* because my bodily weakness will not permit me action, "*and my mind has always been fitted for the contemplation of God and his* "*works.*" All his letters breathe a spirit of piety and resignation to the will of

* These are principally the letters of Newton, procured by Professor Rigaud from the library of Corpus Christi College, Oxford, in the Appendix (No. 19—34): and some letters of Flamsteed to Dr. Bernard, which he afterwards found in the Bodleian library, but which I did not receive till the Appendix was printed off.

I would also here remark, that it was not till after this work was nearly finished, that I had an opportunity of inspecting the above mentioned Newton MSS in the possession of the Earl of Portsmouth; a most valuable and important collection of documents. A list of these MSS is given by Dr. Hutton, in his *Mathematical Dictionary*, at the end of his life of "Newton:" but, from what is there stated, it has generally, though erroneously, been inferred, that they are of little or no value; because Dr. Pellet (who was chosen by the administrators to peruse them, and to give his opinion whether there were any that might be published with advantage) has judged them *not fit to be printed.* Dr. Pellet, however, (as I conclude from the perusal of some of the papers in this collection) was consulted merely as to the prospect of any *profit* or *pecuniary* advantage likely to arise from such a publication, in order that the proceeds thereof might be estimated as a portion of the assets of Newton's estate, which were eventually to be divided between the parties interested in the distribution, but not that he should be required to give any opinion as to the intrinsic merits, character, or importance of those MSS. But, whatever view Dr. Pellet might have taken of the subject in his time, I can state most decidedly that there are, amongst those MSS, many documents and much information connected with Newton's life and pursuits, that are *now* highly interesting, and not generally known. And I trust that these very important papers will be carefully preserved, and that they will, at no very distant period, be submitted to the examination of some person, not only for the purpose of their being better arranged and preserved, but also with a view to publishing such portions of them as may tend to illustrate more fully (as I am sure they will do) the life and labors of our illustrious countryman.

Amongst these MSS I found the letter from Dr. Wallis to Newton, dated Jan. 9, 1698–9, alluded to in page xxxiii, as well as some other documents bearing on the history of Flamsteed's life, which, together with the letters to Dr. Bernard above mentioned, are noticed in the *Addenda.*

Heaven: and even amongst his private memorandums and documents, written when no eye could witness the workings of his mind, we meet with constant expressions of gratitude to the Deity for the blessings which he enjoyed*. It was not to be expected therefore that the loose and irreligious conduct of Halley, both in his conversation and in his principles, could be at all congenial to a mind constituted like Flamsteed's: and although he might endure with it for a season (as he evidently did, at one portion of his life, and a familiar and friendly correspondence was carried on between them), yet this intimacy appears to have been at length broken up, from causes which are only faintly developed by any document I have hitherto met with. But, for Newton, Flamsteed appears to have had a high esteem; and, till the open rupture between them, always spoke of him with the greatest respect. "Mr. Newton's approbation is "more to me (says Flamsteed) than the cry of all the ignorant in the world." And even after the breach of their friendship Flamsteed writes in one of his letters to Mr. Lowthorp (see page 175), "I believe him to be a good man at the "bottom; but, through his natural temper, suspicious." It is very difficult, at this distance of time, to account for the cause of that animosity which ultimately existed between these parties: but I suspect, from the tenor of some of the documents which have come to light, that Halley was the principal cause of it, and that Newton was rather the dupe of Halley's intrigues, than the original mover in so unworthy a contest†. It probably began, like most disputes of this kind,

* At the termination of many of his investigations we find such expressions as these: *Sit Deo cuncta laus et gloria: - - - - In laudes Dei sempiterni cœlorum conditoris perpetuas: - - - - Deo gloria et laus supra cœlos:* &c. &c. At the end of one of his observation books (MSS vol. 15) he has written *Tot vigiliarum tantorumque laborum sola sit laus et gloria Supremo cœlorum Conditori.* And in MSS, vol. 55, where he appears to have accomplished the solution of a geometrical problem he had written at bottom *Deo summo geometræ gratias.* In the year 1709 when he had nearly completed his computations relative to the fixed stars, he has, in one of his computation books (MSS, vol. 26, C.) written as follows: *Jan. 1, 1708-9. Divino fretus auxilio, quod mihi per totum vitæ curriculum adfuerit, fixarum ordinationem fere perfeci. Ultro concedas, oro Optime et Maxime cœlorum Conditor, ut opus hoc totum ad perficiendum vires mihi sana valetudo cum mente sana suppetant; semper ut in tui gloriam in publicum cedentibus inimicis edam et emittam.* See also page lxv, and several other places.

† It was Flamsteed's opinion that, towards the latter period of his life, there was a scheme to deprive him of his situation at the Observatory, in order that Halley might obtain it.

from some slight misunderstanding; and was perhaps increased either by the natural reserve of the parties, or by the interference and officiousness of others who hoped to profit by this breach in their friendship. Flamsteed says that Newton took offence at some errors he had discovered in his *Principia* and in his *Optics*, the nature of which however he has not explained: they differed also in opinion on many astronomical points, on the theory of comets, and on the rectification of the lunar and planetary motions. But this, instead of placing them more at variance with each other, ought to have brought them nearer together in their common search after truth. The whole history of these proceedings however, which will be more fully explained in the sequel, divulges this lamentable fact, that, even amongst men of the most powerful minds, science is no protection against the common infirmities of human nature: and that, however much we may admire their intellectual attainments, we must ever regret their exhibition of any human frailty.

But I must revert to the narrative of Flamsteed's life. Flamsteed was born at Denby, near Derby, on August 19, 1646; and was educated at the free-school at Derby, where his father lived*. In the summer of 1660, being then about 14 years old, he caught a violent cold, from bathing (see page 8): the effects of which he felt as long as he lived, and which at this time rendered him so weak, for many years, that he was scarcely able to go to school; and at length, in May 1662, he finally left it. Being thus withdrawn from school (although not quite 16 years old) he commenced at that early age a system of study and observation which he pursued unremittingly till the time of his death. In the very same year that he left school, he observed and recorded an eclipse of the sun (see page 10), a circumstance which shows his early predilection for astronomy: and nearly the whole of his leisure time (leisure, alas, from sickness) was, even

* As doubts have been sometimes expressed as to the exact manner in which Flamsteed spelt his name, I will here remark, once for all, that I have seen many hundred signatures of his name written by himself, and have never observed it to be spelt differently from the manner here adopted. Indeed the anagram of his Latin name (*Johannes Flamsteedius*, see page 28) would not be correct, on any other assumption.

in this period of boyhood, employed in mathematical studies and astronomical observations, which he pursued self-taught and unassisted; the details of which, written by himself, will be found in the present work. A portion of his time also was occupied in mechanical exercises: for he contrived and constructed a quadrant for taking altitudes, and moreover employed himself in grinding glasses for telescopes. Flamsteed was naturally of a weak constitution, which was probably increased by the accident just mentioned. His father tried every means of alleviating and removing his complaint; and, finding that the disorder did not yield to medicine, at length assented to his son's request to proceed to Ireland, in order to be *touched* by Mr. Valentine Greatrakes, a celebrated empiric of that day, who pretended to cure his patients by a process somewhat similar to the modern practice of *animal magnetism*. He started for Ireland on August 16, 1665; and he appears even then to have attained that remarkable habit of noting down in regular order the most minute occurrences and opinions of his life, which he retained to the day of his death: for he has left on record a complete narrative of this journey, detailing a variety of circumstances that occurred on the way*. He returned to Derby on September 13, having been absent nearly a month from home.

I have been thus minute in these early dates for a reason which will appear in the sequel, where it will be seen that they bear materially on a very eventful and critical period of Flamsteed's life. For he is accused by a modern writer (with what appearance of truth, or even probability, the reader will presently have an opportunity of judging) of having committed, about this time, a *highway robbery*, for which he was tried, convicted, and sentenced to be hanged!!! Leaving this subject however for the present, and passing over many things that will be found fully detailed in Flamsteed's autobiography, in a subsequent part of this volume, I shall proceed to state that he pursued his

* This narrative is the tract entitled "The Self-Inspections of J. F." above alluded to, a small portion only of which is inserted in the *General Dictionary*. The whole of it is given in pages 7, &c. of the present volume. It is a singular document to have been written in those days by so young a man.

mathematical and astronomical studies at home, and became celebrated in the neighbourhood for his talents; till at length he attracted the notice of several Fellows of the Royal Society in the year 1669 (see page 28): and in the following year he paid a visit to London, where he became acquainted with many scientific persons, but more especially with Sir Jonas Moore, who proved one of his best friends and greatest admirers; and who afterwards (in 1674) proposed to establish him in a private Observatory which he intended to erect at Chelsea College; and indeed invited him to London, in order to consult with him on the subject. Whilst in London, he resided at Sir Jonas Moore's house in the Tower, where he carried on his astronomical observations, which are all duly recorded in his manuscript books, and (together with those made at Derby) printed in the first volume of the *Historia Cœlestis*. About this time a circumstance occurred which induced his Majesty, Charles II. to found an Observatory at Greenwich (see page 37): Sir Jonas Moore's proposal of the private Observatory at Chelsea was therefore abandoned, and Flamsteed was, through his interest, appointed Astronomer Royal, on March 4, 1674-5*. From this period we may date the *commencement* of modern Astronomy: the invention of the telescope and the introduction of the clock, then first used for astronomical purposes, were vast improvements on the ancient mode of observing; and their beneficial effects were immediately apparent. Hitherto the catalogue of Tycho Brahé, meagre and imperfect as it was, had been the only help and guide to the astronomer for the places of the stars; and the *Rudolphine Tables* (or corrections of the same) for those of the sun, moon, and planets: but Flamsteed resolved to reform and amend the whole system, and he has set a noble example for future astronomers.

Whilst the repairs and fitting up of the Observatory were in progress, Flamsteed carried on his observations at the Queen's house in Greenwich Park, till

* I do not know that this title was either given to Flamsteed or assumed by him: for after his appointment, he usually annexed the initial letters M. R. (*Mathematicus Regius*) to his name. In the King's Warrant he is styled "Our Astronomical Observator;" a term, which is retained in those documents at the present day.

July 10, 1676, on which day he removed to the Observatory; the only instruments with which he was then furnished being an iron sextant of 7 feet radius*, and two clocks, given to him by Sir Jonas Moore, together with a quadrant of 3 feet radius and two telescopes which he had brought with him from Derby †: consequently none of these articles were provided at the public expense.

He had not been long in this situation before he was invited by Dr. Bernard, of Oxford, to become a candidate for the Savilian Professorship of Astronomy, then about to be vacated by the Doctor. His reply (February 8, 1677-8) shows the state of his religious feeling at that time, and how far he was satisfied with the situation in which he had been so recently placed: for in declining the invitation he says, "I have resolved for the present to content myself with a place "which I have furnished with instruments of my own contrivance (but *full of* "*trouble and no gains*) till I see an opportunity of removing to some one more "advantageous; and where I may have a better air with lesser or fewer distem-"pers. *I am as weary of the place, as you of yours:* my inclinations are for "an employment that may render me more useful in the world, and promote "more glory to my Maker; which, as you well intimate, is the *sole end of our* "*lives, and to which I would direct all my labors*‡."

In June 1678 he borrowed a quadrant from the Royal Society, which he employed till October 1679, "when the ill-nature of Mr. Hooke forced it out of "his hands" (see page 45): after which, Flamsteed made one of 50 inches radius, at his own cost. Finding however that he could not determine the equinoctial points, nor pursue his astronomical investigations successfully, without an instrument *fixed in the meridian,* he applied to Government from time

* This sextant was at first furnished only with a male screw and moveable index at its edge, for noting the divisions: but, finding that the screw wore, he dismounted the sextant in December 1677, and cut diagonal divisions on the limb, on which he could rely with greater confidence. After this date therefore we find *both readings* recorded in the observation book.

† Mr. Hooke afterwards made another (much larger) quadrant at Sir Jonas Moore's expense: but it was so ill-contrived and constructed as to be totally useless. See page 118.

‡ This letter, which I did not receive from Professor Rigaud till after the Appendix was printed off, is given in the *Addenda.*

to time to furnish such an one for the Observatory. This was repeatedly promised him, but never carried into effect: and Flamsteed was for some time obliged to make shift with his sextant, brought into the plane of the meridian, and fixed there as well as he was able *. At length, finding all his applications to Government fruitless, he resolved to make a *mural arc* at his own expense: the instrument was finished about the end of the year 1681; but, conceiving that it was too slight, and that it was not so accurately made as he could wish, he did not erect it till the year 1683, when he fixed it against the wall, and divided it with his own hands. It proved however (as he anticipated) to be a failure; and he was obliged to continue his observations with the sextant only, for several years longer.

During all this time (a period of nearly 15 years), Government had not furnished him with a single instrument. It is true they had given him a house to live in, and had appropriated a *precarious* salary of £100 a year †; but, at the same time, although his employments were sufficiently laborious, the King had ordered that he should instruct monthly two boys from Christ Church Hospital, which was a great annoyance to him, and interfered with his proper avocations (see page 115). The Government had however provided him with "a surly silly laborer" to assist him at the sextant; but another assistant was necessary for the ordinary work of the Observatory, and Flamsteed was obliged to provide such additional help at his own charge: for, it was not in those days, as at the present times, when the Astronomer Royal is not only provided with a competent salary, but with all the requisite instruments and assistants likewise; and when all the comforts and conveniences for carrying on an extensive and

* It was about this period (viz. in November 1680) that the great comet appeared: which, after having passed its perihelion, was visible again in the following months. Flamsteed, having investigated its path in the heavens, *immediately* pronounced that the two appearances were *one* and the same comet: whilst Newton for a long time maintained that they were *two* separate comets. Before the *Principia* were published, Newton had discovered his error; and in that work acknowledges that Flamsteed was right. See page 50.

† See his letter to Sir Jonas Moore, No. 9, and to the Bishop of Salisbury, No. 10 in the Appendix. In the former of these he says, "I cannot conceive that you have any real design to stop my "salary, which I have earned by labor *harder than thrashing.*"

regular system of observations, and for reducing the same, are furnished at the public expense. In order to meet these and other charges which Flamsteed had incurred in carrying on his observations, and which he could ill afford, he entered on the laborious task of a *teacher;* by which it is true he derived a scanty addition to his means, but was at the same time unavoidably drawn away from the main object of his appointment. With such miserable shifts and such obstructions as these, he was obliged continually to struggle: so that his progress was necessarily slow, and he could not make much advancement in the fundamental points of astronomy. It is true that he observed an immense number of intermutual distances of the stars with the sextant, but he was obliged to depend on Tycho's catalogue, for their positions with respect to the equinoctial points, having no instrument for determining such quantities.

When this first mural arc was finished, Flamsteed found (as I have already stated) that it was made too weak for his purpose: nevertheless he contrived to take with it the meridional altitudes of a great number of stars; by means of which, and the intermutual distances taken with the sextant, he formed an *approximate* catalogue of a few of the principal stars, to serve his present purpose. The reader is requested to bear this circumstance in mind, as it explains and justifies a part of the conduct pursued by Flamsteed towards Newton, as related in the subsequent pages. Yet, notwithstanding all these difficulties under which Flamsteed labored, notwithstanding the obstructions thus thrown in his way, the public (the scientific public of that day, not the ignorant and unwary multitude, for they knew nothing of the matter) were repeatedly asking "why he did not print his observations*." Flamsteed replied very justly that he had as yet made no observations that could be turned to any valuable account, for want of the requisite instruments; indeed it could scarcely be expected of him that he should be able to "make bricks without straw."

About this period (1684) he was presented to the living of Burstow, by the

* "Some people (says Flamsteed) to make me uneasy, others out of a sincere desire to see the "happy progress of my studies, not understanding *amid what hard circumstances I lived*, called hard upon me to print my observations." See page 54.

Lord Keeper North: soon after which his father died (1688); and Flamsteed, finding his income somewhat increased by these events, resolved on expending a portion of his property in constructing a new mural arc, much stronger than the former. He had been assured by Lord Dartmouth, the Master of the Ordnance, that whatever he laid out on this occasion should be repaid to him: but in this also he found himself, eventually, most grievously disappointed, as he never received a farthing for the monies expended on this instrument, which cost him upwards of £120. The instrument here alluded to is the celebrated mural arc, made and divided by Mr. Abraham Sharp, with which Flamsteed subsequently made all those observations from which the *British Catalogue* is deduced*. From this moment (September, 1689, when the instrument was first used) every thing which Flamsteed did, every observation that he made, assumed a tangible and a permanent form, and was available to some useful purpose: his preceding observations being only subsidiary, and dependent on results to be afterwards deduced from some fixed instrument of this kind, which he had long sought for. It was at this point only that the Observatory could be considered as complete; and from this period we must date the commencement of his valuable and fundamental observations†. In reading the subsequent history of Flamsteed's life, it is necessary to attend to these several divisions of his labors.

The Observatory had now been established upwards of 14 years; it remained under Flamsteed's superintendence upwards of 30 years more (being nearly half a century from his first appointment of Astronomer Royal): nevertheless during this long interval the Government had not furnished it with a single instrument; nor had they allowed him the cost of a single computer to reduce his observations. Even those which were lent to him by the Royal Society

* In the *Prolegomena* Mr. Sharp is designated as "servus meus:" but in MSS, vol. 3, page 113, he is called "adjutor et minister," which is the more appropriate title.

† I do not wish to be considered as hereby intending to depreciate Flamsteed's previous labors with the sextant, and which are printed in the first volume of his *Historia Cœlestis:* on the contrary, I consider those observations as equally correct with those made with the mural arc, and as available in many instances in determining the relative positions of the fixed stars; though not so frequently appealed to, on account of the trouble required in computing the results. They had however all been reduced by Flamsteed; and many of the results compared with those obtained from the mural arc.

were taken away from him, as soon as his patron, Sir Jonas Moore, died. (See page 45.) The *whole* of the instruments were Flamsteed's own; the Government not having been at the expense even of *repairing* them: and the *whole* of the observations had been *reduced* at Flamsteed's own charge (many of them in duplicate) and arranged by him into catalogues and tables. Yet (*proh pudor!*) in the latter portion of his life (as we shall presently see) the fruit of his long and laborious services was *forced* from him*, and treated as the property of Government: at his decease the instruments also were actually claimed by the Government as their own, and his executors were annoyed with a vexatious and troublesome law-suit on that account. (See pages 341 and 342.)

As soon as Flamsteed had verified the position of his mural arc, he set about the determination of the equinox, of the latitude of his Observatory, of the obliquity of the ecliptic, and of other fundamental points for ascertaining the correct positions of the fixed stars, and the true solar, lunar, and planetary motions. His observation book, as published in the second volume of the *Historia Cœlestis*, and the Prolegomena in the third volume, show the manner and the order in which he pursued his inquiries, and will be a lasting monument of his zeal and perseverance in the cause of astronomy. Some of his methods are original; and continue in use even at the present day. The formation of a correct and enlarged catalogue of stars, at that time much wanted, and anxiously expected, was his first object; since no other valuable catalogue was then in existence except that of Tycho Brahé, containing the places of about 1000 stars, determined very roughly without the use of the telescope, which had not then been invented.

In the pursuit of this inquiry he did not neglect any opportunity of watching the motions of the sun, moon, and planets, nor of applying from time to time such corrections to the theory, and such improvements in the tables, as would more truly represent their places in the heavens: in fact, a great portion of his time was occupied in such investigations, and there is, amongst his MSS, an immense mass of computations carried on for the express pur-

* I speak not here of *manual*, but of *mental* force: of that undue influence over the mind, which is capable of being exerted in a thousand ways, and is oftentimes more powerful than mere physical violence.

pose of elucidating various intricate points in physical astronomy: which is a sufficient answer to those persons who have hitherto considered him as a mere observer. Indeed, it appears that at this period he was in friendly intercourse with Newton, to whom he freely communicated his observations, and with whom he frequently discussed the subject of the lunar and planetary theories*. Many inquiries were again made by the public relative to Flamsteed's publishing the Catalogue, upon which it now became well known that he was deeply engaged: and, amongst others, Newton also suggested to him (by letter dated August 10, 1691), only two years after the mural arc had been in use, the utility of publishing the places of a few of the principal stars, before the completion of the whole catalogue. Flamsteed, in his reply, justifies the course he is pursuing, and points out the inconvenience and difficulty that would arise, if he were to adopt a different line of conduct†. This answer of Flamsteed, however, is remarkable and interesting as giving us the first intimation of the breach between himself and Halley; and, if we may judge from the tenor of Flamsteed's language, the quarrel had already proceeded to a great length‡. Flamsteed's intimacy with Newton, however, does not appear to have suffered any diminution, on this account: for we find that, soon after this, when Newton had again turned his attention towards the lunar theory§, he paid a visit to the

* See their correspondence on this subject in the Appendix, No. 16—34.

† See Newton's letter in the Appendix, No. 14; and Flamsteed's answer thereto in No. 15. Had Flamsteed published his catalogue at this time, he would have fallen into the very same error that Halley did; who, having determined the intermutual distances of the southern stars by means of the sextant only, was obliged to depend on Tycho's observations for his fundamental points, and has thus given us a catalogue, which is of no use whatever to the practical astronomer. It was reserved for Mr. Abraham Sharp to perfect what Halley had neglected to perform.

‡ I have not been able to ascertain the precise cause of the quarrel between Halley and Flamsteed. They were certainly of very different habits and manners, and not likely to accord on many points. It would seem, from some documents inserted in the Appendix, No. 54, that Flamsteed suspected that Halley had obtained, in a surreptitious manner, the magnetical papers of Mr. Perkins, the mathematical master at Christchurch Hospital, and published them as his own; and perhaps Flamsteed mentioned his opinion upon this subject rather too freely. I find that Flamsteed's private sentiments were, that this was not the only instance in which Halley had pirated from other persons. (See page 150.)

§ This was after the attack of illness with which Newton was so seriously afflicted, as to lead (in the opinion of some persons) to a temporary aberration of mind.

Observatory, on September 1, 1694, where Flamsteed, "esteeming him to be "an obliged friend," explained to him what progress he had made in his catalogue, and in his lunar and planetary investigations: and also showed him about 150 computed places of the moon, with their differences from the places observed, at that time a most valuable document; copies of which he gave to Sir Isaac, for his private use in rectifying the lunar theory; on this express condition however that he should not impart them (or the results obtained therefrom) to any person without Flamsteed's consent: for this obvious and just cause, that the places of the moon were determined only by means of his *approximate* catalogue above mentioned *. (See page 150.) This interview led to a correspondence between them relative to this and other astronomical subjects, the major part of which has never before been made public †. In the spring of 1696, Newton was made Warden of the Mint, and came then to reside in London; where Flamsteed says that he sometimes visited him in Jermyn Street, that they continued civil towards each other, but that Newton was not so friendly as formerly. Here then we trace the first symptom of that coolness between them, which soon afterwards broke out into an open rupture: the immediate cause of which appears to be as follows.

Dr. Wallis having understood that Flamsteed had written a paper "On the "parallax of the Earth's annual orb," requested a copy of it, for the purpose of its being published in the third volume of his Mathematical Tracts, then in the

* This request was not only reasonable, but *mutual:* for Newton frequently enjoined the same restrictions upon Flamsteed. In one of his letters (No. 25 in the Appendix) he proposes to send Flamsteed a new table for the moon, on the express condition that he shall keep it to himself till Newton has perfected the lunar theory, because it would need correction. And that Newton acknowledged Flamsteed's claim is evident from a letter which he wrote about the same period (No. 26 in the Appendix), wherein Newton says, "I only assure you at present that, without your consent, I "will neither publish them nor communicate them to any body whilst you live, nor after your "death without an honorable acknowledgment of their author."

† These letters are now given in the Appendix, No. 16—34. Some of Newton's letters (more especially No. 30 and 31) do not seem to have been written in a very courteous style. Indeed Flamsteed has remarked that Newton's *conversation* was not always of the most engaging kind, since he was sometimes so presumptuous as to ask him "why he did not hold his tongue?" (See page 73.)

press*: and Flamsteed accordingly furnished him with a copy of it, in English, which Dr. Wallis translated into Latin†. It appears that there was (in the original) the following paragraph alluding to his having furnished Newton with several observations of the moon, as above mentioned, viz.: "Con- "traxeram etiam cum D^o. Newtono, doctissimo tunc temporis in Academiæ "Cantabrigiensi Professore, necessitudinem, cui lunæ loca ab observationibus "meis ante habitas deducta 150 dederam cum locis simul è tabulis meis ad "earum tempora supputatis, tum similium in posteriore prout assequerer pro- "missorum, cum elementis calculi mei, in ordine ad emendationem theoriæ "lunaris Horroccianæ." At which Newton (on hearing of the circumstance through the officiousness of Dr. Gregory) was very indignant, and wrote that most extraordinary letter to Flamsteed, dated January 6, 1698–9, which is inserted in the Appendix, No. 43. "I do not love (says Newton) to be "printed upon every occasion, much less to be dunned and teased by foreigners "about mathematical things; or to be thought by our own people to be *trifling* "away my time about them, when I should be about the King's business. - - - "You may let the world know, if you please, how well you are stored with "observations of all sorts, and what calculations you have made towards "rectifying the theories of the heavenly motions: but there may be cases "wherein your friends should not be published without their leave, and there- "fore I hope you will so order the matter that I may not, on this occasion, "be brought upon the stage‡." There is surely nothing in Flamsteed's

* This is the celebrated *Letter to Dr. Wallis*, in which Flamsteed clearly points out the effect of *Aberration;* and indeed defines its amount, which accords remarkably well with modern observations. A similar effect had been noticed, many years previous thereto, both by Hooke and by Picard, almost immediately after the application of the telescope to astronomical instruments; and in fact it was a necessary consequence of that invention. Flamsteed however, as well as his predecessors, mistook the cause, which they attributed to the *Parallax of the Earth's orbit:* and it was reserved for Bradley to develop and explain the true theory of the phenomenon, and its application to the purposes of astronomy.

† At least, so it is distinctly stated both by Wallis and Flamsteed; but if we may judge from the specimen contained in the letter which Wallis wrote to Newton, mentioned in the text, and which is given at full length in the *Addenda*, we can scarcely imagine the Latin to have been composed by Wallis himself.

‡ Sir David Brewster (in his recent Life of Newton, page 243) has, through some singular error

letter which could warrant expressions of this kind from Newton: and Flamsteed's reply to him (see page 168) was written in a very different style. "I "could not think (says he) you would be unwilling our nation should have "the honor of furnishing you with so many, and good, observations for this "work [the lunar theory] as were not (I speak it without boasting) to be had "elsewhere. - - - I thought not it could be any diminution to you, since you "pretend not to be an observer yourself*. - - - You will pardon me this "freedom, and excuse me when I tell you, if foreigners come and trouble you "it is not my fault, but those who think to recommend themselves to you, by "advancing the fame of your works as much as they possibly can. - - - I "wonder that hints should drop from your pen, as if you looked on my business "as *trifling:* you thought it not so, surely, when you resided at Cambridge: "its property is not altered. - - - The works of the Eternal Providence I "hope will be a little better understood, through your labors and mine, than "they were formerly. Think me not proud for this expression: I look on "pride as the worst of sins; humility as the greatest virtue. This makes me "excuse small faults in all mankind, bear great injuries without resentment, "and resolve to maintain a real friendship with ingenious men, to assist them "what lies in my power, without the regard of any interest, but that of doing "good by obliging them." Flamsteed immediately wrote also to Dr. Wallis to request him to withdraw the harmless but offensive paragraph†.

or confusion, attributed this letter to *Flamsteed* instead of *Newton;* stating at the same time (I know not upon what authority) that it is "characteristic of Flamsteed's manner:" and thence draws the conclusion that "Flamsteed, not sufficiently aware of the importance of the inquiry, "received Newton's requests as if they were idle intrusions, in which the interests of science were "but slightly concerned." This inference however now falls to the ground, and the erroneous impression cannot be too speedily removed. The history of the whole affair will be found in the Appendix, No. 35—46.

* Newton himself confesses this, in his letter inserted in page 151, where he says, "All the world "knows that I make no observations myself, and therefore I must of necessity acknowledge their "author: and, if I do not make a handsome acknowledgment, they will reckon me *an ungrateful "clown.*"

† Dr. Wallis likewise wrote to Newton on the subject as already stated: and in my late visit at the Earl of Portsmouth's, to inspect the Newton MSS, I found the original letter, from which the above-mentioned paragraph in page xxxiii is extracted, and which is inserted in the *Addenda.*

This short, but unexpected correspondence appears to have terminated all amicable relations between Newton and Flamsteed: and from this period we must consider their friendship at an end, although the outward forms of civility were still kept up. (See page 175.) The reader however may be somewhat surprised to learn that not more than a month previous to this time (namely on December 4, 1698,) Newton had paid a visit to the Observatory, late in the evening, for the express purpose of procuring 12 more computed places of the moon, which he had previously requested from Flamsteed, for some special purpose in his investigations*. And in order to understand the value and importance of these favors, it should be constantly borne in mind that there was no other source in this country (nor on the continent, as far as I can learn) from which such information could be obtained. The Paris Observatory had been established ever since the year 1671: but hitherto only detached observations had been published.

Flamsteed continued for several years to pursue his observations as well as his health and circumstances would permit: and in the course of that time had not only formed a catalogue of two or three thousand stars, whose positions he had determined with his new mural arc, but had also suggested several corrections to the solar, lunar, and planetary tables, which he was by such means

* In page 65 the reader will find the entry, which Flamsteed made in his Observation Book, relative to this visit of Newton: but I have since discovered the following entry of the same event, in MSS, vol. 16, which is rather more minute. "Decem. 4 die ⊙ post preces vespertinas visum "me veniens Ds. Is. Newtonus, Cantabrigiæ Matheseos Professor, &c., Ascent. rectas ☽ cum "distantiis à polo, ab observationibus computo deductas 12, e pag. 184 et 185 libri 5 Calculationum "transcriptas quas petiit communicatas habuit." And on referring to the said 5th book of calculations (MSS, vol. 55) I find in page 181 a memorandum that Newton had requested to have the computed places of the moon for the following days, viz. June 22, 1694, April 25, May 13, 24 and 26, June 11, 16, 25 and 27, July 7, 9, 11 and 15, and August 8, 1695: all of which (except those of June 27 and August 8) are calculated by Mr. Hodgson in page 183, and copies of them were forwarded to Newton. Flamsteed however discovered, soon after, that these computations were erroneous; and has, himself, calculated them anew on pages 184 and 185 as above mentioned. And it was to obtain these 12 corrected values, that Newton paid this visit to the Observatory. Yet within a month after this event, as I have just stated, he wrote Flamsteed that most extraordinary letter.

enabled to supply. He now began to entertain serious intentions of publishing the result of his labors, and wrote an estimate of the number of printed sheets it would fill: he had already expended upwards of £2000 in furnishing instruments for the Observatory, and in hiring assistants, and computers; all of which ought in fact to have been defrayed by the Government; from whom, however, during this long period, he had never received a single farthing beyond his scanty salary. Although by no means a mercenary man, he might have indulged a hope of being enabled to get a return for some portion of this outlay, by means of subscribers to his work: but this specific plan was in some measure obviated by the interference of Prince George of Denmark, who towards the end of the year 1704, having heard of these extraordinary labors of Flamsteed, and being himself a patron of science, proposed to print the Observations and the Catalogue at his own expense. (See page 75.) A Committee, consisting of Sir Isaac Newton (then President of the Royal Society), Sir Christopher Wren, Dr. Arbuthnott, Dr. Gregory, and Mr. Roberts, was appointed to inspect the papers; who reported favorably upon them, and recommended them *all* to be printed*. The publication of the work was therefore placed under their superintendence; and Flamsteed, who did not anticipate much benefit from Newton's interference, thus found himself unwarily involved in fresh troubles and contentions. For the *Referees*, as this Committee was called, or rather Sir Isaac Newton (for he appears to have assumed the principal management of the affair) seem to have conducted the business without Flamsteed's privity or concurrence, and notwithstanding Flamsteed's repeated remonstrances, to have thrown every obstacle in the way of despatch: at least, this is Flamsteed's version of the matter, and his view of it appears to be confirmed by the documents in the Appendix. Sir Isaac pretended to have discovered several errors, and demanded the books containing the original entries, in order that he might compare and examine

* Although the Referees here recommend that the *whole* of the observations should be printed, yet we shall find in the sequel that their opinion upon this subject experienced some alteration; at least, if we may judge by the result.

them. Having got these into his possession, he next required that that portion of the catalogue which was completed (but which was not to be sent to press till after the whole of the observations were printed, so as to allow time for its being perfected) should be placed, sealed up, in his hands. Flamsteed at first resisted: he told Sir Isaac that the catalogue was not complete; that it would eventually contain a great many more stars than he had yet observed and rectified; that it at present contained only about 1500, but that he hoped to make it up 2500 stars; that these were the results of all his labors, in which he had spent above £2000 more than his salary; and that it would not be either prudent or safe to trust a copy of it out of his own keeping. He at length however found himself obliged to comply, or else to give up the prospect and advantage of having the work printed at the Prince's expense: and the catalogue (imperfect and incomplete as it was) was accordingly sealed up in the presence of Sir Christopher Wren, and delivered into Sir Isaac Newton's possession. (See page 81.) New difficulties however were afterwards started, oftentimes frivolous and vexatious, and it was May 16, 1706, before the first sheet was struck off; and it was Christmas, 1707, (three years after the first undertaking) ere the whole of the *first* volume only was finished: during which time the press was frequently stopt by Sir Isaac without any assignable cause. The whole details of these proceedings are given by Flamsteed in the following history of his own life; and supported by various documents which are inserted in the Appendix.

This *first* volume, which contained only his Sextant observations, being thus completed, arrangements were entered into for proceeding with the *second* volume, which was intended to contain the observations made with the Mural Arc. After a great deal of unnecessary procrastination on the part of Sir Isaac Newton, a meeting with the Referees was appointed to take place on March 20, 1707-8; when Flamsteed took up with him the whole of the observations made with the mural arc, from Sept. 1689, to Decem. 1705, fairly copied out on 175 sheets of large paper, together with a more extensive and perfect copy of his catalogue of the fixed stars. At this meeting new articles

were suggested, and finally imposed upon Flamsteed: for he was not only obliged to leave the whole of the 175 sheets of manuscript in Newton's hands, but also bound himself to complete, and return within 16 days, the catalogue which had previously been delivered, sealed up, to him; Sir Isaac retaining the one which Flamsteed had brought with him, as a pledge for the performance of the contract*. Notwithstanding this compliance, however, on the part of Flamsteed, the work of the press does not seem to have been expedited: further obstructions were thrown in the way of proceeding, the nature and cause of which are not sufficiently apparent; and Prince George died (October 28, 1708) before the second volume was entered upon. The work was now completely stopt: and although by this melancholy event the power of the Referees ceased, the papers were still left in their hands.

Being now undisturbed (as Flamsteed expresses himself), he proceeded to carry on such observations as he wanted for the purpose of his astronomical inquiries; and added many new stars to his catalogue. Nothing more was heard about Sir Isaac Newton, or the printing: and Flamsteed says, in one of his letters to Mr. Sharp, " I shall not urge it forward again till I see a good " fund settled, and secured, for carrying it on without any danger of impedi- " ment, or obstruction, from him or any of his tools." (See page 270.) But, in the midst of this apparent quiet, he was again annoyed, when he least expected it, by being privately informed that his catalogue (which he had delivered, sealed up, into Sir Isaac Newton's hands, as a sacred deposit) *was in the press;* but more so, by a letter from Dr. Arbuthnott (dated March 14, 1710-11), demanding the deficient parts of such catalogue, and informing him that he (Dr. Arbuthnott) was commanded by the Queen to superintend and complete the publication of the *Historia Cœlestis*, undertaken by the late Prince. Dr. Arbuthnott, however, appears to have put the business into the hands of the Royal Society, who thus became in some measure mixed up with the subsequent pro-

* This continued suspicion appears to me to have been exerted on the wrong side: for it was Flamsteed that had most reason to be cautious, since *he* would have been the only sufferer by any breach of the agreement.

ceedings; but Newton and Halley were evidently the prime movers on every occasion: Halley was (I believe) at that time Clerk to the Society*. Flamsteed was much annoyed at this new step: he requested and obtained an interview with Dr. Arbuthnott, and at the conference that ensued (March 29) he asked the Doctor in direct terms "whether the catalogue was printed or not:" to which the Doctor replied "that not a sheet of it was printed." Flamsteed doubted the assertion at the time, and which indeed turned out to be *false;* for a friend sent him, within four days after, the constellations of *Aries* and *Taurus* fairly printed; and, in a day or two after, that of *Virgo*. He learnt also that Halley had the superintendence of the press, that he pretended that he had found many faults in the catalogue, that he had moreover showed some sheets of it publicly at Child's coffee-house, and that he boasted of the pains he had taken in correcting the errors. Flamsteed was of too high a spirit to be thus treated, without remonstrance: he found that he had been made the dupe of some intrigue, and he resented it accordingly. In one of his letters to Dr. Arbuthnott (April 19, 1711), complaining, amongst other things, of the alteration in his catalogue, he says, "I have now spent 35 years in the composing and "work of my catalogue; which may, in time, be published for the use of her "Majesty's subjects, and ingenious men all the world over. I have endured "long and painful distempers by my night watches and day labors. I have "spent a large sum of money above my appointment, out of my own estate, to "complete my catalogue, and finish my astronomical works under my hands. "Do not tease me with banter, by telling me that these alterations are made *to* "*please me*, when you are sensible nothing can be more displeasing nor injurious, than to be told so. Make my case your own, and tell me ingenuously "and sincerely, were you in my circumstances, and had been at all my labor, "charge, and trouble, would you like to have your labors *surreptitiously* forced "out of your hands, conveyed into the hands of your declared, profligate enemies,

* I shall still call these parties *the Referees*, for want of a better designation: for although the original committee was dissolved, yet it is evident that the same *animus* existed in those who formed the new body of advisers.

" printed without your consent, and spoiled, as mine are, in the impression? " Would you suffer your enemies to make themselves judges of what they really " understand not? Would you not withdraw your copy out of their hands, " trust no more in theirs, and publish your own works rather at your own ex- " pense, than see them spoiled, and yourself laughed at, for suffering it?

" I see no way to prevent the evil consequences of Dr. Halley's conduct, but " this. I have caused my servant to take a new copy of my catalogue; of which " I shall cause as much to be printed off as Dr. Halley has spoiled, and take " care of the correction of the press myself, provided you will allow me the " naming of the printer; and that all the last proof sheets may be sent to " Greenwich, at my charge, by the penny post, and not printed off till I have " seen a proof without faults. After which, I will proceed to print the remain- " ing part of the catalogue as fast as my health, and the small help I have, will " suffer me. But if you like not this, I shall print it alone, at *my own charge*, " on better paper, and with fairer types than those your present printer uses; " for I cannot bear to see my own labors thus spoiled, to the dishonor of the " Nation, Queen, and People. If Dr. Halley proceed, it will be a reflection " on the President of the Royal Society; and yourself will suffer in your repu- " tation, for encouraging one, of whom the wisest of his companions used to " say, that the only *way to have any business spoiled effectually, was to trust it* " *to his management.* But I hope better things of you, and that you will en- " deavor to make me easy after all my long, painful, and chargeable labors, by " affording me your assistance, as occasion shall serve: whereby you will ever " oblige, Sir, your humble servant and sincere friend."

This remonstrance being of no avail, it appears that Flamsteed addressed the Queen upon the subject: for there is, amongst his MSS, the copy of a petition, dated April 16, 1712, stating the circumstances of the case, and requesting that this surreptitious edition of his catalogue might be suppressed. (See page 295.) Flamsteed however remonstrated here likewise in vain: for he found, soon after, not only that the printing of the spurious catalogue was completed, but also that the Observations made with the mural arc (contained in the 175 sheets, which

were left in the hands of the Referees, as above mentioned) were sent to the press, in *a garbled and incorrect manner;* the observations of those stars *only* being retained which passed the meridian at the same time with the moon and planets, and nearly on the same parallel; *the rest being wholly rejected**. He also found that the places of the moon, inserted in the margin of the book, and considered to be deduced from those observations, were *the very same places* (at least, those in the more early periods) that he had, some years before, given to Newton under the express stipulation that they were not to be made public, because they were deduced from an *approximate* catalogue of the fixed stars. (See page 293.) This was not just either to Flamsteed, or to the public; who had a right to expect that the most correct determinations should be given†. It is true that the Editor thereby saved himself a vast deal of intricate and troublesome computation: but the character of Flamsteed suffered in proportion; and

* In order that the reader may fully understand the nature of this charge (which is by no means a light one, and of which Flamsteed might justly complain), it may be proper here to state that the edition, above alluded to, does not contain the journal of the observations made with the mural arc, in the manner in which they were entered in the MS books (and as they are, in fact, now printed by Flamsteed in the *second* volume of the *Historia Cœlestis*), but merely *partial extracts* from the same, where they had reference to the moon or any of the planets: *all the remaining observations being wholly omitted.* And these extracts were arranged under different heads, according to the body with which the stars (generally 2 or 3 only in number) were compared. Thus on Sept. 15, 1690, although there were 119 observations made, yet only the 5 which relate to Jupiter, and the 4 which relate to the moon, are extracted for the press, and placed in *different parts* of the volume: the remaining 110 observations being *wholly omitted*, and no notice whatever taken of them in any part of the book. So that the future astronomer has no means of correcting the error of the instrument or of the clock, nor of ascertaining whether the catalogue of the fixed stars had been correctly deduced. (See the last note in page 99.) Flamsteed knew, much better than the Referees, the practical advantage of having *all* the observations recorded, day after day, *in their regular order.* He was therefore perfectly justified in destroying (as he afterwards did) this garbled and abortive production: and both the present and future astronomer will duly estimate the obligations which they are under to him, for having had the public spirit afterwards to print at his own expense the whole of his observations in the order in which they were made. Flamsteed's motive however was but little understood, in his day, if we may judge from the opinion of Mr. Jones already alluded to in page xx.

† The *early* computations of the places of the moon are to be found in MSS, vol. 54; and correspond exactly with those published in Halley's spurious edition. The subsequent lunar computations, deduced from the *correct* places of the stars, are to be found in MSS, vol. 60, and correspond with those published afterwards by Flamsteed himself. The difference is frequently very considerable. See Mr. Sharp's opinion on this subject in page 323.

we cannot be surprised that he should be indignant on the occasion. And if he has expressed his opinion of Halley's conduct (in his confidential letters) in terms which sound, at the present day, extremely harsh to our ears, it must be confessed that he had much to irritate and excite him.

Flamsteed however had not sufficient interest to stop the press; for the work thus *mutilated and corrupted* ultimately appeared in one volume, accompanied with a disingenuous and illiberal preface by Halley, who superintended the edition *. This conduct of the Referees was evidently unjustifiable; as they had no right to break the seals of his deposit, without his consent and approbation, even at the command (as they pretend) of the Queen †. The whole of the documents were clearly Flamsteed's own; the observations had been made with his own instruments, and reduced at his own expense; the Government had not (as I have repeatedly remarked) contributed anything beyond his paltry salary of £100, and that charged with the execution of duties that belonged not to his situation. The least therefore which they could have done, should have been to let him print his works in his own way; not only on account of the labor, the anxiety, and the money which they had cost him, but also and more especially because there was no one so competent as himself to judge of the most proper manner in which they ought to appear before the public for the promotion of astronomy. The *whole* would then have been finished in much less time than this single volume of Halley's.

This spurious and premature publication of his works was a mortifying circumstance to Flamsteed and annoyed him very much: and it cannot be won-

* This edition will frequently be referred to, in the subsequent pages, as " Halley's edition of 1712." It contains, besides the spurious *Catalogue* and the garbled *Observations*, nearly the whole of what now forms the first volume of the *Historia Cœlestis*. In the preface, Halley has made many misrepresentations and misstatements: some of these I have pointed out in pages 385 and 386; and I will here further add, in contradiction to what Halley has stated, that it was *not* agreed that the catalogue should be prefixed to the first volume; and that he has, in many other parts of the said preface, given a colouring to facts which leave a false and erroneous impression on the mind of the reader. There are very few copies of this work now in existence; nearly the whole of the edition having been destroyed by Flamsteed, as will be related in the sequel.

† Flamsteed says that the order of the Queen was obtained *after* the offence was committed. This is a question, however, of but little moment, in a case of absolute wrong.

dered at, that he should so feel it, and resent it accordingly. In his correspondence with Mr. Sharp on this subject *, he opens his whole mind upon the subject, calls Halley " a malicious thief," and makes use of other opprobrious epithets which could only be palliated by a consideration of Flamsteed's high state of excitement. But, I apprehend that, at that day, a much greater license of expression was allowed, or taken, on such occasions; for a circumstance occurred about the same time which showed that even Newton himself could for a moment, in a similar manner, forget his rank and station: the occasion of which was as follows. In the year 1710 her Majesty was pleased to appoint the President of the Royal Society, together with such others as the Council of the said Society should think fit, to be *Visitors of the Royal Observatory.* Flamsteed calls this measure " another piece of Sir Isaac Newton's ingenuity:" and, after the treatment he had received, he might naturally conclude that this also was done to annoy him. There is no evidence however to show that Newton had any hand in it whatever: but in consequence of this appointment a scene occurred, the particulars of which would perhaps never have been divulged, had not these manuscripts of Flamsteed, belonging to two distinct parties, been simultaneously brought to light. It appears that a meeting of the Council of the Royal Society was summoned for October 26, 1711, at which Flamsteed was desired to attend " to know from him if his instruments were in order, and " fit to carry on the necessary celestial observations." (See page 96.) Flamsteed attended accordingly; and a scene ensued which he has minutely described in three or four of his MSS, and in his letters to Mr. Sharp, without much shade of difference. It appears that Newton, not satisfied or pleased with the answers that he received from Flamsteed, forgot himself and the duty he was then performing under the Queen's Warrant, " ran himself into a great heat and " very indecent passion, and used him so as he was never used before; called him " *a puppy* and many other hard names, but *puppy* was the most innocent of " them." Dr. Mead, who was present, joined in insulting him: till at length

* See Mr. Sharp's opinion of Halley's spurious edition, in his letter inserted in the Appendix, No. 214, at page 323, above referred to.

Flamsteed, evidently disgusted at such treatment, withdrew from the scene, desiring them to restrain their passion, and telling them that " it was a dishonor " to the Nation, her Majesty, and that Society (nay to the President himself) " to use him so." (See pages 97, 228, and 294.) When we consider that Newton was, at that time, nearly 69 years of age, and that Flamsteed was upwards of 65, and so infirm that he was obliged to be assisted both up and down stairs, it must be confessed that this scene exhibits but a miserable picture of the frailties of human nature; and every friend to science, or even to humanity, must lament its ever having taken place.

Soon after this occurrence, it appears that Flamsteed, finding that all faith with him had been broken, that his catalogue had been thus surreptitiously and clandestinely printed, and that his observations also had been sent to the press in a garbled and improper manner, broke off all communication with Dr. Arbuthnott and his coadjutors in this affair, resolving in his own mind to appeal to the public on the occasion. He drew up a statement of all the proceedings that had taken place, with a view to its publication: and afterwards set about a re-examination of his observations (see page 294), in order to collect together, for insertion in his catalogue, such stars as had escaped his notice on his former reviews; determined to perfect the Catalogue as much as possible, and to reprint it at his own expense: and before the end of the year 1712, he received the last sheet from the press*. He then proceeded to do the same with his Observations: and for this purpose he applied to Sir Isaac Newton for the MS copy not only of the catalogue and of the 175 MS sheets of observations which had been deposited in his hands, but also of the MS books of *original entries* which had been left with him some time before; but without effect. Flamsteed therefore found himself obliged to commence legal proceedings against him for their recovery; but with what success I have not been able to ascertain. Some of the books

* This revision of the catalogue appears to have been made in too much haste: or perhaps I ought rather to say that it was printed too soon after the revision was made. For, after Flamsteed's death, Mr. Crosthwait found several errors in it, and reprinted some of the sheets (see page 359): but a false economy prevented the whole from being re-computed, re-modelled, and re-printed.

were returned to Flamsteed, but there is still one of them missing (containing the MS observations from November, 1702, to January, 1712) which perhaps is the one that Flamsteed denies ever having received back. (See page 322.) With respect to the 175 sheets of MS observations, it appears that Newton eventually handed them over to Halley; which Flamsteed calls "the height of "trick, ingratitude, and baseness." (See page 325.) And it is certain that Flamsteed was ultimately obliged to recopy not only the catalogue, but also these 175 sheets of observations, for the press, at an expense of nearly £200; and at a great loss of time and labor, independent of the additional risk of error. This conduct was indeed unaccountable, and scarcely to be justified on any view of the case *.

Whilst employed however on this work, two events occurred which in some measure changed the prospect of Flamsteed's affairs: these were the death of Queen Anne, who died on August 1, 1714, and the death of the Earl of Halifax, the great patron and supporter of Sir Isaac Newton, on May 19, 1715. The officers at Court were changed: the new Lord Chamberlain knew Flamsteed well; and a hint was given to him that he might, with very little trouble, get all the spurious copies of his printed observations into his own hands. He accordingly drew up a memorial and petition to the Lords of the Treasury (Sir Robert Walpole being then First Lord); whereupon 300 copies of this obnoxious work (probably all that remained, out of the 400 printed, after the presentation copies† and a few sales were deducted) were delivered up to him, *which he immediately committed to the flames,* "that none might remain to show the "ingratitude of two of his countrymen, who had used him worse than ever the "noble Tycho was used in Denmark." Rejoiced at this circumstance, he set himself in earnest to print his observations in the order in which they were made, and as they now appear in the second volume of the *Historia Cœlestis:*

* The MSS here mentioned are those to which I have alluded in my "Account of Dr. Halley's Astronomical Observations" inserted in the *Memoirs of the Royal Astronomical Society*, vol. 8, page 187, as being in the hands of Dr. Halley.

† See a list of these presentation copies in page 318.

for though, as he candidly states, " he was unwilling to impoverish his nearest " relations, whom he was bound in justice and conscience to take care of, since " they were in no capacity to provide for themselves," yet he was determined that the labor of nearly 40 years should not be thrown away, and therefore resolved to print them *at his own expense.* Fortunate indeed has it been for the astronomer that Flamsteed was so resolute and pertinacious on this point; and that he had courage and public spirit enough to bear up against his two powerful opponents, whose views upon this subject are by no means in accordance with those of modern astronomers.

It should here be remarked that when Flamsteed obtained the 300 copies of his printed work from the Lords of the Treasury, he destroyed only the catalogue and the spurious part of the work which professed to be his observations made with the mural arc. That portion of it which contained his observations with the sextant was separated from the rest, and (together with the observations of Gascoigne and Crabtree, and of his own at Derby, as well as the computed places of the moon and planets, and a few subsidiary tables, all printed afterwards at Flamsteed's own charge) now forms the first volume of the *Historia Cœlestis.* So that, of all the three volumes of the *Historia Cœlestis,* there were only 97 sheets (of this first volume) that were printed at the public expense: all the rest having been edited at the risk and private cost of Flamsteed himself.

Flamsteed however did not live to see the termination of his labors: he died before the second volume was quite completed: and the remainder of that volume, as well as the whole of the third, was finished under the care and superintendence of Mr. Joseph Crosthwait, his assistant at the Royal Observatory, aided by Mr. Abraham Sharp. In the complete and perfect execution of this undertaking, they met with many difficulties: for although Mrs. Flamsteed appears to have been a woman of high spirit, and impressed with a proper sense of, and regard for, her husband's honor and fame, yet a too strict attention to economy prevented the work from appearing before the public in the most advantageous light. The catalogue, which had been reprinted by Flamsteed, was

still found, on a new comparison with the observations by Mr. Crosthwait, to contain many errors: some of the sheets were again reprinted with amendments, but others were suffered to be ultimately published with all their faults. Yet, had it not been for Mr. Crosthwait's extraordinary, and in some measure gratuitous exertions, the work would never have been completed; and the world must have been satisfied with the meagre and garbled edition published by Halley*. The Preface cost Mr. Crosthwait much trouble: it was written in English by Flamsteed, but it was now required to be translated into Latin; no one however could, for some time, be found adequate to the task, though repeatedly attempted. Mr. Pound, at one time, undertook it, but eventually, after much procrastination, declined it; and it was at last accomplished by a dissenting minister: a considerable portion of it however being suppressed, as already mentioned. The whole work was at length published in three volumes in 1725, six years after Flamsteed's death. The distribution of its several parts will stand thus: the first volume and the major part of the second volume were printed during Flamsteed's lifetime; but the remainder of the second volume and the whole of the third volume were printed under the superintendence of Mr. Crosthwait. This latter portion therefore may, in some measure, be considered as a *posthumous* work.

There remained now only the *Maps*, the construction and engraving of which appear to have cost as much trouble and vexation as the letter-press,

* Mr. Crosthwait had a great esteem and veneration for Flamsteed. He attended him in his last illness, on his death-bed: and, in the account which he gives of that scene to Mr. Sharp, he says, "He often called for me, and would gladly have said something to me, but was not able, though he "called for me by name; and continued to do so till the last moment. You will see, by this, that "he has not left me in a capacity to serve him, notwithstanding he has often told me he would: but "this I impute to his not being sensible of his near approach till it was too late: but the love, "honor, and esteem I have (and shall always) for his memory and every thing that belongs to him, "will not permit me to leave Greenwich or London, before I hope the three volumes are finished." (See page 333.) And in another letter he states, "Had it not been for the love and honor I bear to "Mr. Flamsteed's memory (knowing how many potent enemies he has left behind, and how few "friends capable of serving him in these affairs) I had before this time left Greenwich, and should "have had a due regard to my own future support: but this I have refused upon his account." (See page 336.)

but arising from a totally different source. It seems that only one of them was completely finished (*Orion**), when Flamsteed died: for the rest we are indebted to Mr. Sharp, who constructed them anew, according to Flamsteed's principles, from the catalogue. Sir James Thornhill drew the figures of the constellations, and recommended engravers for the work; but the charges of the English artists were considered so enormous, that Mr. Crosthwait went over to Holland for the express purpose of engaging some of the best Dutch engravers to complete the work†. The vexatious delays which necessarily occurred by adopting this method, its increased expense, and the constant attention requisite to prevent mistakes, dispirited Mrs. Flamsteed: and a temporary stop was consequently put to the work; although Mr. Sharp (now much advanced in years) and Mr. Crosthwait were willing to continue their services. At length some English engravers being found who offered to execute the maps at a more moderate charge, the labors of these gentlemen were renewed, and continued till the time of Mrs. Flamsteed's death, which took place on July 29, 1730.

Here the correspondence, from which these facts are taken and which is inserted in the Appendix, ceases; probably on account of the circumstances mentioned in the last letter of Mr. Crosthwait (No. 280 in the Appendix), whereby it appears that Mrs. Flamsteed did not leave either Mr. Sharp or Mr. Crosthwait a single farthing for all their services, neither had they received any remuneration since Mr. Flamsteed's death for all their unparalleled exertions on her behalf. It does not appear that Mr. Hodgson exerted or concerned himself at all in the business of printing or publishing *any* of Flamsteed's works.

That the above mentioned circumstances, attending the publication of Flamsteed's works, should never before have come to light, is somewhat singular;

* There is no separate map of *Orion* in Flamsteed's atlas; nor is the *whole* of that constellation depicted in any *one* map. It was probably obliged to be re-drawn and re-constructed, in order to suit the subsequent arrangements.

† I cannot find that more than 4 of the maps were engraved by the Dutch: viz. *Gemini*, *Aquarius*, *Cetus*, and one other.

and it is much to be regretted that some explanation was not given, at the time, of the circumstances under which they eventually appeared. But that many of the facts were well known at that period, both in this country and on the continent, (although not detailed at length) appears from the testimony of contemporaneous writers: and that Mrs. Flamsteed partook of the spirit and indignation of her husband, is evident from the letter which she addressed to the Vice Chancellor of Oxford; requesting that the copy of Halley's spurious edition, presented to the Bodleian Library by Sir Robert Walpole, might be removed therefrom, as not being the genuine works of Mr. Flamsteed. This letter is given in the Appendix, No. 281.

I have thought it proper to give this preliminary, but short and connected, view of Flamsteed's labors, in order that the reader may more readily combine the several statements in the present volume. For as Flamsteed's autobiography, here detailed, is made up of various manuscripts (some on loose sheets of paper and others bound in books) composed at various times, and under different circumstances and feelings, and as many repetitions of the same events frequently occur (not only in his own statement, but also in the documents inserted in the Appendix), his memoirs might otherwise be liable to some confusion. With this assistance however I trust that every objection of this kind will be removed: for I have considered it much better that Flamsteed should relate his own history, and in his own words (more especially as it contains such severe comments on names which stand so deservedly high in public estimation), than that any version of mine should give a coloring to the facts, which he has detailed, different from that which was intended by the author, and which has been so forcibly expressed by himself.

I shall now revert to the singular charge, alluded to in page xxiv, which has been made by a modern writer against Flamsteed's moral character. Mr. William Hutton (late of Birmingham), a voluminous and well-known author, has published the following statement in his *History of Derby*, (London, octavo,

1791,) pages 281, &c. "John Flamsteed, the great mathematician, (who in "1675 was concerned in erecting the Observatory at Greenwich, and in the "reigns of Queen Anne and King George I. presided over it as Astronomer "Royal) was a native of Derby, some say of Denby: his father however resided "at Derby. John was born in 1646, and continued in Derby till 1670. The "first rudiments of his extensive learning he acquired at the free-school in "St. Peter's churchyard. Amongst the early follies of his youth he was "accused, with some degenerate companions, as being concerned in a highway "robbery, for which he was tried and condemned. Circumstances and friends "appearing in his favor, the Royal pardon was procured from Charles II. "This piece of discredit was not generally known in after life. The bent of "his own mind being then pursued, he became one of the greatest ornaments "of man. He discovered new worlds in the heavens, which he communicated "to posterity. Instead of pursuing unjustly the things of this world, he followed "with applause those of others. He died in 1719, at the age of 73, leaving a "most amiable character. Among his papers the pardon was found. John "Webb, who was an intimate acquaintance of his, and afterwards of mine, "gave me the anecdote."

As this extraordinary account, if true, might tend to diminish our respect for Flamsteed's character, I have taken some pains to ascertain its probability. It appears to rest wholly on the credit of Mr. John Webb; of whom I shall speak more hereafter. In the mean time it will be necessary to bear in mind a few leading points as our guide in this inquiry. In the first place, it is scarcely probable that such an occurrence could have taken place *whilst* he was at school; for (setting aside the improbability of a parcel of school-boys seriously embarking in an enterprise of this kind) if it happened *before* the summer of 1660 (the date of the restoration of King Charles II. who is stated to have granted the pardon) Flamsteed would scarcely have been admitted again to the school, both on account of the ignominy attached to the transaction, and by reason of his mature age (he being then 14 years old), which rendered it less necessary to try so doubtful an experiment in opposition to public opinion. And

that it could not have occurred *during* the summer of 1660 will, I think, appear probable from the circumstance that in such case he would have been tried during the summer assizes; and, as the King's pardon could not be immediately obtained, he would have been in prison at the very time that he represents himself to have caught that violent cold from bathing, which enfeebled him all the rest of his days. It remains therefore only to investigate the probability of its occurring *after* the summer of 1660.

Now it appears that Flamsteed was so weak for two years after the summer of 1660 that he could scarcely walk to school, and in 1662 entirely left it. I ought moreover to add here that Flamsteed was afflicted with a natural lameness, arising from some organic disease, which would not only incapacitate him from being very active in an affray of this kind, but also render him more easily caught in any pursuit, and more readily identified when taken. Common prudence therefore, in which Flamsteed was by no means deficient even at that tender age, would point out to him the impolicy of embarking in any scheme of this kind, even if other motives still more powerful and correct had not influenced his conduct. If it still be contended that the occurrence may have taken place after the year 1662; yet even this indefinite period must be circumscribed by probabilities: and here I apprehend we may fairly admit Flamsteed's own minute and circumstantial account of the employment of his time as of equal, if not superior, weight to the hearsay evidence of Mr. Webb; more especially as we find that Flamsteed's statement is confirmed by his well-known subsequent history, whereas Mr. Webb's (as I shall presently show) is contradicted by every information that I have been able to obtain.

If therefore Flamsteed's own statement be admitted, the alleged offence could not have been committed before the end of the year 1665; since he has satisfactorily accounted for his time, during the period preceding that date, in that minute and circumstantial account referred to in the preceding part of this Preface; and which in fact forms the first division of his autobiography in the present volume. Whether it occurred afterwards or not, may I think be fairly left to the unbiassed judgment of mankind. For, it should be remembered that

Flamsteed had, about this period, made himself distinguished by his talents: he drew sun dials, and calculated solar eclipses, made astronomical instruments and observations; no mean acquisitions in those days, and for so young a man, being then scarcely 20 years of age, and which had rendered him celebrated in the neighbourhood. In the year 1669 he computed an almanac for the ensuing year; not (as he says) after the usual manner, but much more accurately. The results of some of these calculations he sent to the Royal Society, which brought him acquainted with several of the most learned and intelligent men of that day: and from this period Flamsteed's character becomes a kind of *public* property, which must be defended on *public* grounds. This communication to the Royal Society was not signed in his own name, but under an anagram (see page 108): so that the Secretary, Mr. Oldenburg, was for some time ignorant of the quarter from which it came; and he at length ascertained it only by dint of an active inquiry. "Though you did what you could (says Mr. Oldenburg) to hide your "name from us, yet your ingenious and useful labors for the advancement of "astronomy, addressed to the noble President of the Royal Society and some "others of that illustrious body, did soon discover you to us, *upon our solicitous* "*inquiries after their worthy author.*" (See page 106.) This trifling incident, I think, bears materially on the point in question: for, before Mr. Oldenburg wrote the answer to Flamsteed's communication (which is given in page 106) he would naturally endeavour to ascertain the character of the person to whom he was writing in such flattering terms; and if Flamsteed (a person then well known to all the scientific neighbourhood) had, *within any short period previously thereto*, been tried and condemned to be hanged for a highway robbery, it is scarcely within the limits of probability that such an event could have been concealed from him: since there is always, in every part of the world, a sufficient quantity of *busy* people, ready to keep alive any stories of this kind, and zealous to promulgate them, with perhaps some additions, amongst their neighbours. Much less likely is it that such an offence could, *within* 8 *or* 10 *years only of its alleged commission*, have been kept secret from so many persons about the Court, to whom Flamsteed must have become known when he was made

Astronomer Royal in 1675. But the *universal silence* in every quarter, *during* Flamsteed's life time, respecting such an occurrence, is *primâ facie* evidence that it never took place: there is no document, public or private, that I know of, that contains any account of the alleged fact. I have perused and examined every one of Flamsteed's papers, of which there is a great quantity; I have been indulged with the sight of many other manuscripts, bearing upon Flamsteed's history, and some of them written by his bitter enemies; but in none of them have I ever found the most remote allusion to the extraordinary circumstance, related with so much confidence by Mr. Webb, and sanctioned by the publication of Mr. Hutton.

But it does not appear *how* Mr. Webb became acquainted with the circumstance. He does not speak of it as a common report, as a fact well known in his day, *during Flamsteed's life time* (in fact, he seems to imply that it was *not* generally known till *after his decease*), but appears to rest his conviction of the truth of it merely from the alleged circumstance of the King's pardon being found amongst his papers after his death; without even mentioning his authority for such a discovery, or how he became so minutely acquainted with the circumstances of the offence for which the pardon was granted. As if the knowledge of the fact with all its concomitant details had then, for the first time after a lapse of nearly 60 years, burst upon the world; no other person however having thought proper to notice the extraordinary occurrence, except this obscure individual Mr. Webb. But, I would ask, who were the parties likely to be present at the examination of his private and confidential papers, secured with a privacy and care in proportion to the risk and fear of discovery? Surely not Mr. Webb, nor any other stranger to the family: but his nearest and dearest relations, his widow and his niece, whom he had appointed executrices of his will. Are these persons (even supposing them to have discovered such a distressing document) likely to have blazoned abroad the disgrace of their departed relative? to have stamped at once with ignominy the high character which Flamsteed had all through life maintained, and which it was their object to perpetuate? And from what motive? Would they not rather (if such a

discovery had actually been made) have locked up the secret in their own breasts, and used every precaution to prevent its being known? But the *universal silence* in every quarter, even *after* Flamsteed's decease, respecting such an occurrence (except this oral testimony of Mr. Webb *eighty years* after the alleged offence) is another additional evidence that it never took place. Shakspeare's "early follies of youth" may perhaps be true: but Flamsteed's *anecdote* (as it is quaintly called by Mr. Hutton) rests on a very slender foundation, and ought not to have been hazarded on such frail evidence.

Thinking that I might be able to obtain more accurate information at Derby respecting Flamsteed's early life, I went down there for the express purpose of ascertaining whether the books of the free-school would throw any light on the subject: whether, indeed, there was any entry therein of Flamsteed's admittance to the school, or of his *expulsion* therefrom. But it unfortunately happened that there were no books, of so early a date, in existence: nor, in fact, was it at all known whether such books were ever in use.

However, there remained one source of information to which I could appeal with confidence; and the result of that appeal will, I trust, for ever set the question at rest: for, if a pardon was actually granted by King Charles, some trace of it was sure to be found amongst the public records in the State-Paper Office. I therefore took the liberty of detailing the circumstances of the case to Robert Lemon, Esq. Deputy Keeper of State Papers, requesting him to furnish me with such information upon the subject as his office might afford. With the greatest readiness and kindness he immediately entered into my views; and, impressed with the same feelings, went through the search *himself*, without trusting to the contingent carelessness of others. It is with much pleasure, and I am sure to the satisfaction of all Flamsteed's admirers, that I communicate the result of Mr. Lemon's inquiries, and in his own words. "I have (says "he) *myself* made a careful search through the whole of our Warrant Books, "Petitions, References, Reports and Domestic Correspondence from 1660 to "1670 inclusive, and *can state in the most explicit manner* that there is no "trace of any grant of pardon to the celebrated John Flamsteed to be found in

" them; nor do I believe that any such ever existed: if it had, it *must* have " been entered among our Warrants or Petitions, the series of which, at that " period, in my custody, is *particularly perfect.*"

Having thus satisfied myself (and, I trust, the public) that this extraordinary charge is void of foundation, I shall next endeavour to ascertain the responsibility of the parties (Mr. Hutton and Mr. Webb) to whom we are indebted for the circulation of the report. Of Mr. Webb we know nothing more than what Mr. Hutton has recorded of him; and which I shall presently relate: but Mr. Hutton is the author of several works, most of which have gone through more than one edition. Two of these only have I read; viz. the *History of Derby*, and the *History of his own Life:* and, if I may judge from those two productions, he was a great lover and collector of anecdotes, and never so well pleased as when he met with a person who could add to his stock. He was born in the town of Derby, in the year 1723, of very low and indigent parents; but, by his industry and talents raised himself to a state of independence. He was indeed of so low an origin, that when only 7 years of age he was set to work at the silk mills in that town, where he continued till he was 14; when he was sent to Nottingham, to be apprenticed to his uncle, who was a stocking maker. Whilst there, he played some wild pranks which offended his uncle; but being restored to favor he continued with him till the end of his seven years' apprenticeship. It was at this place, and during this period (viz. in the year 1742) that he became acquainted with the above-mentioned Mr. Webb: and the only account, which I can collect of this person, is from the statement which Mr. Hutton has given in the History of his own life, in the following words: viz. " An old gentleman of the name of Webb (who " had passed a life in London, brought £3000 into business, lived in genteel " life, and had filled many offices, but was reduced) came to reside with us. " He was one of the most sensible and best of men, completely formed for " an instructor of youth. It was my fortune to attend him, sleep with him, " and love him. I treated him as a father, a monitor, and endeavoured to " profit by him. He had many acquaintance, all men of sense, to whose

" conversation I listened by the hour*." This, then, is the whole of our knowledge about Mr. Webb: so that, after all, it appears that we are called upon to believe this improbable story, on the authority of a garrulous old man retailing his " anecdotes" to a youth only 19 years old, at the distance of *eighty years* after the alleged offence!!! Surely, no character is safe if such testimony, as this, is allowed to have the least weight in public opinion.

But, another remarkable circumstance attending this business is that Mr Hutton, who was a native of Derby, should, at the end of half a century after he had been informed of this " anecdote," (namely in the year 1791, or about *one hundred and thirty years* after the alleged offence is said to have been committed) write and publish a history of that town, and of the principal characters to whom it had given birth, without having taken any pains in the mean time to ascertain the accuracy of his informant's report. The whole leisure of his life seems to have been employed in collecting and recording public and private incidents and events: he must therefore have well known the sources of information, and more especially in a place where he was so completely at home; and he ought to have stated the success of those endeavours (if made) before he had suffered the paragraph to have been inserted in his book. We can scarcely suppose that Mr. Webb was the only person that was acquainted with the report; and, whether true or false, such report (if indeed it ever existed) would be sure to reach Derby, Flamsteed's native place. The *total silence* however (as I have before observed) respecting this subject, for nearly *one hundred and thirty years*, is in itself a sufficient proof of the falsity of the " anecdote."

Desirous however of examining every source of information on so important a subject, and having learnt that Mr. Hutton's daughter (Miss Catherine Hutton) was still residing in the neighbourhood of Birmingham, I took the liberty of writing to her, to request her to furnish me with any additional evidence or information relative to the point in question, that might be in her possession: with which request she immediately and very obligingly complied. The follow-

* *Life of William Hutton*, 1817. Second edition, page 121. In the following page Mr. Hutton says that Mr. Webb died on the 22nd of July in the next year, 1743: so that their acquaintance was but of short duration.

ing is an extract from her letter: viz., " I have heard my father say that " Mr. Webb had been in some wholesale business in London, and had failed " through having trusted a friend to a very large amount, and having been deceived " by him. The particular business and circumstances I have forgotten. I have " heard my father mention and quote Mr. Webb, I believe literally, a thousand " times. He always spoke of him with the highest respect and veneration; " and I am certain he would as soon have doubted the evidence of his own " senses, as any thing affirmed by Mr. Webb. I do not recollect having heard " my father mention the anecdote of Flamsteed: if he ever did, it could not " have been with doubt." This letter therefore, it seems, adds nothing to the authority of the information: nor indeed does the second letter which Miss Hutton was good enough to write to me on this subject, wherein she says " I do " not comprehend how the anecdote respecting Flamsteed should be void of " foundation. My father's memory was so tenacious that I cannot imagine *him* " to have been mistaken. He wrote what he called *The Book of Memory*, " which consisted of the incidents, the most trifling and the most remote, that " had happened to him on every one of the 365 days of some one year or other; " with the exception (I think) of 13 days on which he could recollect nothing " to record. The manuscript is now in the possession of my brother. Mr. " Webb's veracity was unquestioned, as he was a cautious man; and Flamsteed " was a person too well known to have been easily mistaken for another."

It should here be remarked that I do not accuse Mr. Hutton of *misrepresentation* either wilful or accidental; since it is very probable that he heard (or believed that he heard) the " anecdote" related by Mr. Webb, in his boyhood. But I do think that he may be charged with a great want of *prudence and caution* in publishing such a tale, *fifty years afterwards*, when he was advanced to a mature age, without having taken some pains, in the meantime (for there is no evidence of such pains having been taken) to ascertain the credibility of his informant's statement. The concluding passage, however, in Miss Hutton's last letter brings to my recollection the fact, that Flamsteed has recorded, amongst his list of pupils and *assistants* at the Observatory, the name of " J. Flamsteed;"

and calls him "*cognatus meus.*" (See page 49*.) Whether any occurrence, of the kind alluded to by Mr. Webb, was ever alleged against *this* branch of the family, and which, from the identity of their names and residences, and the similarity of their employments (after a lapse of *eighty years*, for this is an important feature in the case), might have given rise to the report in question, it would perhaps at this distance of time be impossible to determine. But, even in this case, the result of the search at the State-Paper Office would be a sufficient answer to the charge: unless indeed it happened *after* the year 1670, beyond which period the search was not extended. It is the only solution, however, of the difficulty that I can suggest: and some persons might perhaps consider that further inquiry is requisite for the purpose of ascertaining such fact. But, I apprehend that I ought rather to apologize for the great length of the present discussion: and I certainly feel little disposed to attempt to affix the stigma on another branch of the family, being sufficiently contented with having removed it from the subject of the present memoir.

* Flamsteed left by his will a portion of his property to the children of his "kinsman John "Flamsteed, of Little Hallam." Probably this was the same person as that designated by the title of "cognatus meus." Flamsteed's will, which is *dated* on the last day of February, 1717, was *proved* in the Prerogative Court of the Archbishop of Canterbury, on Jan. 7, 1720. See the substance of it in page 333.

A CATALOGUE OF FLAMSTEED'S MANUSCRIPTS.

In arranging the following volumes of MSS, I have endeavoured, as much as possible, to keep the several subjects distinct and separate: and, although this end could not, from the circumstances of the case, be strictly accomplished, yet I trust that the plan which has been adopted will facilitate the inquiries of those who may hereafter have occasion to examine the several volumes. The arrangement is nearly in the following order: viz. those volumes which contain,

1°. The *original* entries of observations.
2°. The *copies* of such observations.
3°. The various *catalogues*.
4°. Letters and other documents relating to Flamsteed's *history*.
5°. Detached *memoirs* and papers on various subjects.
6°. Subsidiary *tables* used in his computations.
7°. *Computations* of various kinds.
8°. *Miscellaneous* papers.
9°. Manuscripts by *other hands*.

Vol.

1. Entitled by Flamsteed "Minutes, or *first* notes of Observations:" being those made with the Sextant, from April 11, 1676, to October 29, 1679, both inclusive. Small quarto, bound in vellum.

These observations (as well as those in the subsequent volumes, unless otherwise expressed) are entered *chronologically*, and not collectively as in the first volume of the *Historia Cœlestis*. Under the date of August 31, 1678, he states that he went to Derby for about a month, on account of a dangerous illness with which he was seized: and that during his absence Dr. Halley had made some observations which Flamsteed says are described in another little book, of a few pages; and for the insertion of which, blank pages have been left in the present book. I have not been able, however, to discover either the little book, or the observations. Under the date of July 30, 1679, there is the following entry: "Ego in puteo subterraneo transitum observavi lucidæ in capite Draconis "proprius ad verticem quam Faber viderit Junii 20, cum meridiem media nocte strinxerit." This is the only notice, that I find recorded, of his having made use of the *well* for observations. At the end of the Observations there is the following entry: "The second "book, from Decem. 1, 1678, to Feb. 19, 1684, is detained in the hands of Sir Is. Newton." On the last page of the book is a Catalogue of 18 principal stars, reduced to the year 1677, which he used in his astronomical computations. In this volume there is frequent mention made of his two clocks; one of which is designated by its long pendulum, and the other by its short pendulum: I apprehend therefore that they were not both *two-seconds* pendulums. Halley's name is frequently mentioned as having made observations.

Vol.

2. Minutes, or *first* notes of Observations with the Sextant, from November 1, 1679, to February 15, 1684, both inclusive. Quarto, bound in leather.

In the first page of this book Flamsteed has written the following memorandum: viz. "Librum hunc, postquam detinuisset fere 12 annos, remisit Newtonus Eq. mense Octobri "exeunte anni 1716." At the end of the book there are some observations of the Tides at Greenwich; and also of the *pinch-water* at the Tower, which latter cost him £10 to obtain: also an account of Disbursements for repairs, &c. at the Observatory in 1677—82: also an account of the angles of various objects seen from the Observatory: also a list of stars whose intermutual distances are to be taken for parallax. Besides the two clocks mentioned in the preceding volume, we have allusion here made to horologium *majus*, horologium *ambulatorium*, horologium *axiculare;* mention is also made of a *spring* pendulum, and a *pivot* pendulum, but nothing from which we can learn any description of them. Halley's name is here also frequently mentioned as having made observations.

3. Minutes, or *first* notes of Observations with the Sextant, from February 19, 1684, to September 3, 1689, both inclusive. Quarto, bound in leather. It is entitled by Flamsteed "Libri tertii observationum pars prior."

In the first page of this book Flamsteed has written the following memorandum: viz. "Commoda Eq. N. Feb. 27, 1715–16, recepi post multas frivolas excusationes et fictas "morarum vel pretextas causas, misso J. C. ad petendum July 15, 1715, cum libro 1° "observationum Grenovicensium, post 9½ annorum captivitatem." Under the date of August 18, 1688, he has also made the following note: viz. "Vesp. venit Ds. Sharp, ut "mihi ab observationibus adjutor et minister esset."

4. Minutes, or *first* notes of Observations with the Mural Arc, from September 12, 1689, to January 17, 1691, both inclusive. Quarto, bound in leather. It is entitled by Flamsteed "Fixarum Planetarumque observationes Arcu Murali habitæ, &c."

This book was originally bound up with the preceding one, in one volume, as the pages are continued from one volume to the other. The pages of the present volume are numbered at the foot. Under the date of October 31, 1690, Flamsteed mentions his having added a new clock. Under the date of November 4, 1690, he has written the following memorandum: "Hora $10^h\ 10^m$ per horologium, Minister meus per biduum et 3 fere menses, "A. Sharpius me reliquit ut mathematica Londini doceret." At the end of the book there is a collection of the observations of *Polaris*, and some computations of the latitude: also a small table of the errors of the mural arc, in azimuth; also a logarithmic table of the distances of Jupiter from the sun: also some incomplete tables of Jupiter.

5. Minutes, or *first* notes of Observations with the Mural Arc, from January 17, 1691, to January 14, 1694. Quarto, bound in leather. It is entitled by Flamsteed "Diarium Observationum Cœlestium, &c."

At the beginning of the book there is a short table of the errors of the mural arc, arising

Vol.

from the sinking of the wall, during the years 1691–4; against which Flamsteed has written the following note: "His erroribus usus fui in Apographis observationum quas in "manus Di. J. Newtoni, ut prælo propediem committerentur credidi March 20, 1707–8." This table differs from that which is given in page 297 of the same volume. Under the date of October 23, 1692, is a memorandum of his marriage, which has been copied in page 61 of the present work. I would here remark that there are two leaves cut out of this book; one containing pages 68 and 69 (which is noticed in page 350 of the present work), and the other containing pages 244 and 245: but the journal of the observations does not appear to be deranged or lost thereby.

6. Minutes, or *first* notes of Observations with the Mural Arc, from January 18, 1694, to March 16, 1698, both inclusive. Quarto, bound in rough leather. It is entitled by Flamsteed "Diarium Observationum Astronomicarum Cœlestium, &c."

In page 43, under the date of September 20, 1694, Flamsteed has written the following memorandum: "Anna, sororis meæ Katherinæ et Rob. Hemingii filia improvisa, Greno- "vicum venit, hora 4 p. m.: nupsit Jacobo Hodgsono, ministro meo, me inscio, Octob. 31, "1702: Londinum concessit ut cum marito conviveret, Octob. 16, 1706." Under the date of Sept. 27, 1694, it is noted that Halley, together with Mr. Nelson, paid a visit to the Observatory, in order to see the synopses of the observations which Flamsteed had previously shown to Newton. At the end of the book are some statements and computations relative to the errors of the mural arc, arising from the sinking of the wall, for the years 1694—1698: also relative to the position of *Polaris* for the years 1689—1696. After this, there is the following memorandum respecting his niece and her family: viz. "Anna, neptis mea, "dilectissimæ sororis meæ Katherinæ et Roberti Hemingii, filia unica, nata erat anno 1680, "Octob. 7, hora 10 matutina, die ♃, Marefeildiæ in com. Leicestriæ.—1694, Sept. 20, "me visum veniens in Observatorio, a patre impetravi ut mecum maneret.—1702, Octob. 31, "nupsit, me inscio, Jacobo Hodgsonio.—1706, Sept. 15, Londinum concessit, ut cum marito "viveret, gravida.—1706, Decem. 8 ⊙ circa 13^h p. m. Katharina ejus et Jacobi Hudsoni "filia, Londini nata, q. d. g.—1707-8, Johannes, filius, natus Londini $5\frac{1}{2}^h$ p. m. Januarii sic "puto 26.—1710, Jacobus filius." There are 4 or 5 leaves torn out of this book: but none of the observations appear to be lost.

7. Minutes, or *first* notes of Observations with the Mural Arc, from March 18, 1698, to November 17, 1702, both inclusive. Quarto, bound in rough leather. It is entitled by Flamsteed "Diarium Observationum Cœlestium, &c."

At the beginning of the book there are some memoranda of dates relating to his own life. At pages 18, 90, and 91 are some memoranda relative to the variation of the magnetic needle. Under the date of December 4, 1698, is the entry stated in page 65 of the present volume. Under the date of November 23, 1699, Flamsteed has written as follows; viz. "Mane "D°. Poundio, in Indiam navigaturo, postquam noctem integram hic manserat, valedixi: "optimus ille Geometra et Mathematicus." On December 3, 1699, he remarks that the barometer fell to 28·40 inches; and on February 3, 1702, to 28·08 inches: which latter,

Vol.

he says, is nearly 3½ inches lower than he ever saw it*. At the end of the volume are the usual computations for determining the errors of the mural arc, for the current years, arising from the sinking of the wall.

N.B. There is no book of the original entries of the observations, from November 17, 1702, *to January* 2, 1712, *exclusive; nor is any such book entered in Dr. Maskelyne's list of the Manuscripts.*

8. Minutes, or *first* notes of Observations with the Mural Arc, from January 2, 1712, to December 27, 1719, both inclusive. Quarto, bound in rough leather.

At page 79, Flamsteed remarks that on September 13, 1716, at 6 o'clock, the barometer stood at 29·30 inches; and that on the following morning at 8 o'clock it had fallen to 28·05 inches, where it continued till about the middle of the day, when it began to rise again. At the end of the volume, are the usual computations for determining the errors of the mural arc, arising from the sinking of the wall, for the current years.

9. A *Copy* of the observations of Gascoigne and Crabtree, from December, 1638, to December, 1642, both inclusive: also a *copy* of Flamsteed's observations made at Derby, from June 22, 1666, to February 16, 1673–4; at the Tower of London, from April 18, 1675, to July 11, 1675; and at the Queen's House in Greenwich Park, from September 26, 1675, to November 25, 1676. Quarto, bound in vellum. It is entitled by Flamsteed "Excerpta Astronomiæ, &c."

There are several observations in this book which are not printed in the first volume of the *Historia Cœlestis;* especially the whole of the observations for the year 1676.

10. A *Copy* of Flamsteed's observations made at Derby, from October 25, 1668, to April 8, 1674; at the Tower of London, from April 17, 1675, to July 17, 1675; and at the Queen's House in Greenwich Park, from September 26, 1675, to November 25, 1676. Folio, bound in a marbled paper cover. It is entitled by Flamsteed "Officina Astronomica, &c."

11. A *Collection* of Flamsteed's Observations made in 1676 and 1677. Quarto, bound in vellum. It is entitled by Flamsteed "Observationes Brittanicæ Regiæ."

Those observations are *copied* from some other books, and are arranged under different heads, as follow: viz. 1°. at the beginning are the spots in the sun, no page; 2°. intermutual distances of the fixed stars, at page 1; 3°. an account of the comet which appeared in April 1677, at page 35; 4°. vertical distances, on the meridian, of the fixed stars taken with the sextant, at page 37; 5°. intermutual distances of the planets and fixed stars, at page 41; 6°. the same of the moon, at page 62; 7°. observations of Jupiter's satellites, at page 91. After these follow 12 maps of the principal stars in the constellations of the zodiac. There

* I presume he means lower than the highest point at which he ever saw it.

Vol.

are some observations and remarks in this book which are not printed in the first volume of the *Historia Cœlestis*.

12. Another *Collection* of Flamsteed's Observations, from 1666 to 1677. Quarto, bound in leather.

The observations in this volume are *copied* from some other book, and are, as in the preceding one, arranged under different heads, as follow, viz. 1°. Intermutual distances of the stars in the Pleiades, taken at Derby, in 1671—73; 2°. eclipses of the sun in 1666 and 1668, and other astronomical phenomena in subsequent years; 3°. spots seen on the sun in 1676: 4°. intermutual distances of the fixed stars, from September 29, 1676, to December 18, 1677; 5°. the same of the moon and planets from the fixed stars; 6°. observations of Jupiter's satellites; 7°. vertical distances of Polaris; 8°. an account of the comet which appeared in April 1677. There are some observations and remarks in this book which are not printed in the first volume of the *Historia Cœlestis*.

13. Another *Collection* of Flamsteed's observations, from 1671 to 1676. Quarto, bound in vellum.

The observations in this volume are also *copied* from some other books, and like the two preceding ones are arranged under different heads: viz. 1°. diameters of the sun and moon; 2°. intermutual distances of the stars in the Pleiades, all observed at Derby in 1671—73; 3°. observations made at the Tower of London from April 17, 1675, to July 11, 1675; 4°. observations made at the Queen's house in Greenwich Park, from September 26, 1675, to November 29, 1675; 5°. observations made (probably at the Queen's house also) in 1676; 6°. the history of a solar spot in July and August 1676. This book has evidently been bound since it was written; as many of the figures are cut off by the tool.

14. Another *Collection* of Flamsteed's Observations in 1679 and 1680. Quarto, bound in leather.

The observations in this volume also, like the 3 preceding, have been *copied* from some other books, and consist wholly of intermutual distances of the moon from the fixed stars: at the end of which are tables of the Sun and of Saturn.

15. A *Copy* of the Observations made with the Mural Arc, from September 11, 1689, to December 19, 1693, both inclusive. Folio, bound in rough leather.

In page 200 are some remarks respecting the adjustment of the mural arc, and determining its errors: and in page 279 a notice relative to the new wires to his telescope, which took place at the following dates, viz. January 2 and 6, 1690–1, June 7, 1691, and Sept. 21, 1693. At the end of the volume is a collection of observations of *Polaris*: also a copy of Dr. Plume's will: also a list of his pupils and *assistants*, with the dates when they came to him: also the variation of the magnetic needle at different periods from 1680 to 1716: also an index to the stars observed.

Vol.

16. A *Copy* of the Observations made with the Mural Arc, from January 3, 1694, to December 24, 1703, both inclusive. Folio, bound in rough leather.

At the beginning of this volume there is a list of the stars from which the error of the instrument has been determined. At page 40 is a synopsis of the observations of *Polaris* in 1689–95. In page 181 is the note respecting Sir I. Newton's visit to the Observatory quoted in page xxxv. At the end of the volume is given a list of the stars and planets observed; also an account of Flamsteed's ancestors, whom he traces back as far as the year 1514.

N.B. In the preceding volume, and in the present one as far as the end of 1698, the zenith distances are copied from the originals *with the correction of the instrument applied;* which does not always accord with that which Flamsteed ultimately adopted. I would also remark that I have occasionally met with entries of observations which are not to be found in the original MSS: so that it will be necessary to examine these copies as well as the originals, in case of any revision of the observations.

17. A *Copy* of the Observations made with the Mural Arc, from January 11, 1704, to November 12, 1715, both inclusive. Folio, bound in calf.

On the cover and first three pages of this volume Flamsteed has written some notes of the history of his life, alluded to in page 4 of the present work.

N.B. The last three volumes are referred to, by Flamsteed, under the title of *Apographa.*

18. A *Collection* of Flamsteed's Observations with the Sextant, from 1676 to 1680. Folio, bound in vellum.

These observations are *copied* from some other books, and are arranged according to the constellations, &c., as in the first volume of the *Historia Cœlestis:* except the first division, which contains the intermutual distances of the fixed stars in 1676 and 1677 only. There are however several remarks, which are not in the printed copy; more especially at page 8. On the first leaf, Flamsteed has drawn the horoscope, alluded to in page 34 of the present work. On page 3, he has drawn the ground-plan of the Observatory, from which the plan in page 40 of the present work is taken. On page 4 is a list of several "eminent places," with the angles which they make with the Observatory.

19. A *Collection* of Flamsteed's Observations made with the Sextant, from 1676 to 1680. Folio, bound in vellum.

These observations also are *copied* from some other books, and, like those in the preceding volume, are arranged according to the subjects. They consist of intermutual distances of the fixed stars, moon, planets, comets, &c: and are nearly a repetition of what is given in vol. 18. It appears to be the book that was deposited in the hands of Ed. Sherbourn, Esq., as mentioned in page 125 of the present work.

20. A *Collection* of Lunar Observations, from 1675 to 1689, both inclusive. Folio, bound in vellum, gilt leaves.

Vol.

The observations in this volume are *copied* from some other books: and are the same as those printed in the first volume of the *Historia Cœlestis*. After the lunar observations, there is a copy of Ptolemy's catalogue; then follow some lunar computations; and at the end is the catalogue of Hevelius.

21. A *Collection* of Flamsteed's Observations from 1681 to 1691. Folio, bound in vellum, gilt leaves.

The observations in this volume are also *copied* from other books: they relate to the fixed stars only; and many of the entries are unfinished. At the end are some observations for refraction: and also some meridional distances from the vertex, taken with the sextant in 1678–1680.

22. A *Collection* of Flamsteed's Planetary Observations with the Sextant, from 1681 to 1689. Folio, bound in vellum, gilt leaves.

These observations also are *copied* from some other books. At the end of the planetary observations, there are the observations of the two comets which appeared in 1682 and 1683. Then follow a copy of the catalogue of Tycho Brahé: some extracts from the observations of Lawrence Rook: observations of Jupiter's satellites: observations of solar spots: and lastly a synopsis of the stars used in his lunar and planetary comparisons.

23. A *Synopsis* of all the Fixed Stars, observed with the Mural Arc, from 1689 onwards; with computations of their positions: arranged under their respective constellations. Folio, bound in vellum, gilt leaves.

This book, which is entitled by Flamsteed "*Zodiacus Britannicus*," is a very important book, since it contains the original computations of the places of most of the stars inserted in the *British Catalogue*. See page 370 of the present work. I have supplied an index at the end. On the first page Flamsteed has written "Labores cuncti mei, tum foris tum in museo, suscepti in supremi Numinis cœlorum conditoris majorem gloriam et laudes dicantur, et continuati semper diriguntur."

24. Another *Synopsis* of the Fixed Stars observed with the Mural Arc, but more limited. Folio, bound in black leather.

This volume is quoted, by Flamsteed, by the title of "Liber Niger." The synopsis appears to have been intended merely for the purpose of making a preliminary catalogue: with the exception of the stars in *Hercules*, which are given more in detail at page 141, &c., than in the preceding volume. In page 119, there is the copy of a letter from Flamsteed to Mr. Caswell, dated March 8, 1708–9. At the end of the book are some remarks on dialling.

25. *Catalogues* of the Fixed Stars. Folio, bound in rough leather.

At the commencement of this book are some subsidiary tables for computing the longitude and latitude of the fixed stars. The first catalogue commences at page 33, and is arranged

Vol.

nearly in the same manner as the catalogue published by Halley; with the addition however of the right ascensions in time, and of the distances from the vertex. Towards the end there is a small catalogue of 130 principal stars, reduced to 1686; which is probably deduced from the synopsis alluded to in the preceding volume, and used by Flamsteed prior to the completion of the *British Catalogue*. And, immediately following it, there is a repetition of the stars in the zodiacal constellations, apparently copied from the first catalogue here mentioned.

26. A *Portfolio* containing five books marked A to E, each in a marbled paper cover. Folio.

The books A, B, C, may be considered as Appendices to vol. 23, since they contain the computations of those stars whose places are not to be found determined therein; together with duplicate calculations. The book D contains the meridional distances from the vertex of the sun, planets, and fixed stars, made with the sextant in 1683 and 1684, *copied* from some other book: at the end there is another catalogue of 130 principal stars reduced to 1686, similar to that in vol. 25. The book E contains some observations, *copied* from another book, of intermutual distances of the sun, planets, comets, and fixed stars, taken with the sextant in 1681–1689.

27. Another *Portfolio* containing three books marked A, B, C, each in a marbled paper cover. Folio.

The book A contains a fragment of the zodiacal catalogue, and two other fragments of the general catalogue. This fragment of the zodiacal catalogue contains the *six latter signs* only of the zodiac, and (I have no doubt) is the catalogue alluded to by by Flamsteed in his letters to Dr. Arbuthnot and Mr. Sharp (see Nos. 157 and 163 in the Appendix). There are several figures on the first page, which are evidently in Dr. Halley's handwriting, and clearly show that the document was once in his possession. There are several alterations in the catalogue, but I cannot ascertain whether they have been made by Halley or by the amanuensis. I apprehend however that several of them have been made by Halley, because some of them are such as have been adopted by him in his edition, but not afterwards acceded to by Flamsteed in the *British Catalogue* The book B contains one of the early copies of the general catalogue: but it is not the same as that printed by Halley, nor as that printed in the third volume of the *Historia Cœlestis*. The book C contains another early copy of the general catalogue: but, although it has all the printer's marks on it, and has evidently been sent to press, yet it is not the same as that printed by Halley, nor as that printed in the 3rd volume of the *Historia Cœlestis*. None of these copies are exactly alike. In this portfolio there are also 3 loose leaves which have evidently been used by the printer in printing the second volume of the *Historia Cœlestis*.

28. Another *Portfolio* containing one book in a marbled paper cover. Folio.

This book contains the MS copy (used by the printer of the *Historia Cœlestis*) of the catalogues of Ptolemy, Ulugh Beigh, Tycho, the Prince of Hesse and Hevelius: and also the MS

Vol.

copy from which the places of the planets, &c., at the end of vol. 2 of the *Historia Cœlestis*, were printed. The pages 1—8 of the catalogues of Tycho and the Prince of Hesse are wanting: and at the end of the catalogue of Hevelius there is inserted the constellation *Corvus*, which is not printed in the *Historia Cœlestis*.

29. Another *Portfolio* containing 3 books marked A, B, C: the first in boards, the two latter each in a marbled paper cover. Half folio.

Each of these books contains a catalogue of the zodiacal stars: but the first of them (A), which is in octavo, has only the longitudes and latitudes of the stars.

30. Copies of the Catalogues of Ptolemy, Ulugh Beigh, and Hevelius. Folio, bound in rough leather.

At the end of these catalogues there is another copy (the third) of a small catalogue of 130 stars reduced to 1686, similar to that in vol. 25. After this, are inserted the computed places of the moon and planets from the observations made with the sextant and the mural arc.

31. A portion of the printed copy of the *British Catalogue*, published by Halley in 1712, interleaved, and with MS notes. Also four MS maps of some of the constellations. Folio, in a marbled paper cover.

32. A *Portfolio* containing four books marked A, B, C, D: all in marbled paper covers. Folio.

The book A contains the history of Flamsteed's early life, and was entitled "The Self-Inspections of J. F." The book B is entitled "Historica Narratio Vitæ meæ." The book C contains a portion of the original Preface in English (see pages 3 and 4 of the present work). The book D contains a letter (consisting of 30 leaves) giving a short history of Nautical Astronomy: and also a fragment, of 8 leaves, of the above-mentioned original Preface, apparently copied out for the press, and differing very slightly from the corresponding portion contained in book C. It is this fragment to which I have alluded in my paper on Flamsteed's inedited stars, inserted in the *Memoirs of the Astron. Soc.* Vol. 4, page 137; and which I then thought was in the hand writing of Flamsteed: but I am not now of that opinion. At the end of this fragment there is also another fragment (of 6 leaves) of a Latin translation of a portion of the said original Preface; which differs a little from the printed copy.

33. The *Letter Book* A (so designated by Flamsteed). Folio, bound in vellum.

The beginning and end of this book are occupied with private accounts and memoranda relative to an estate belonging to Flamsteed's ancestors: but the middle contains *copies* of a great number of letters and other documents connected with Flamsteed's life, and the history of the Observatory. The pages are numbered from *each* end.

Vol.

34. A collection of *original* letters from Mr. Abraham Sharp to Mr. Flamsteed, from February 2, 1701–2, to October 16, 1719. Folio, bound in a marbled paper cover.

These letters are the answers to those which are in the possession of Mrs. Giles, as mentioned in the first page of the preceding Preface.

35. Another collection of *original* Letters, Papers, and other Documents, connected with the history of Flamsteed's life and labors. Folio, bound in a similar manner to the preceding volume.

36. Another collection of *original* Letters, Papers, and other Documents, connected with the history of Flamsteed's life and labors. Folio, bound in a similar manner to the two preceding volumes.

37. Another collection of *original* Letters, Papers, and other Documents, connected with the history of Flamsteed's life and labors. Folio, bound in a similar manner to the three preceding volumes.

38. Flamsteed's *Lectures* on Astronomy read at Gresham College in 1681—1684. Small thick Quarto, bound in a marbled paper cover.

39. A *Common-place Book.* Quarto, bound in rough leather.

The first paper in this book is on "The inequality of the Earth's motion." At page 81, is the paper entitled "Cœlum Brittanicum," a portion of which is printed in page 54 of the present work. At page 113 is the fragment of a Latin letter addressed to Sir I. Newton, and dated January 16, 1698–9, on the Parallax of the Earth's orbit. At page 135 is another letter, in English, addressed to Sir I. Newton, which is printed in page 176 of the present work. At page 175 are several computations and remarks relative to Mars. And at the end are the computations for determining the errors, in azimuth, of the mural arc.

40. Another *Common-place Book.* Small folio, bound in leather.

This volume is entitled "Miscellanea Mathematica," by Flamsteed. At page 18 there is a long account of the MSS of Gascoigne and Crabtree. At page 46, an account of the observations made by Mr. Townley, from September 9, 1665, to September 21, 1672. At page 77 is a paper entitled a "Preface to his Astronomical Observations at Derby," printed in page 108 of the present work: with a full account of those observations in chronological order, from June 2, 1666, to February 16, 1673–4. At page 115, are copies of various warrants connected with the Observatory. At page 121 are copies of observations of meridional zenith distances made with the sextant, from November 6, 1678, to September 15, 1680. At page 141 is a copy of a letter from Flamsteed, dated December 27, 1703, respecting the renewal of his salary. Then follow copies of various observations of Jupiter's Satellites in 1672; of Mars in 1671 and 1672; and a synopsis of distances of the moon and planets from the fixed stars in 1676—1680.

Vol.

41. Another *Common-place Book.* Quarto, bound in leather.

This volume contains various memorandums, observations, extracts from other works, copies of correspondence, &c: an index to some of which is given in the first page of the book. At the other end (turned upside down) is inserted the paper entitled "A restitution "and rectification of the Earth's motion," which is printed in page 44 of the present work.

42. Another *Common-place Book.* Quarto, bound in leather.

This volume contains copies of various letters to and from distinguished persons both at home and abroad; such as Hevelius, Roemer, Cassini, Halley, Newton, Molyneux, &c: amongst which is one, in page 140, showing his opinion upon Earthquakes. At the end (turned upside down) is a list of the books in his library: also some lunar computations.

43. Another *Common-place Book.* Small Quarto, bound in vellum.

This volume also contains copies of various letters to and from distinguished persons; such as Cassini, Hevelius, Oldenburgh, Halley, &c. Also Cassini's account of a spot on Jupiter: some remarks on the transits of Mercury over the sun: and Mr. Townley's description of his micrometer.

44. Another *Common-place Book.* Small Quarto, bound in vellum.

This volume contains a memoir "De motu Solis correcto," written by Flamsteed when he resided at Derby: another "Of the rays of light transmitted:" another on the rays of light reflected: another on the celebration of Easter: another on the method of determining the difference of meridians. At the end of the book (turned upside down) are some notes respecting the Tides in 1687: also an account of the glebe and tithes let at Burstow; by which it appears that they were worth £153. 5*s*. 0*d*. in the year 1685.

45. Another *Common-place Book.* Small Quarto, bound in vellum.

This volume contains various extracts from old authors on astronomy: observations made at Derby, from September 12, 1669, to May 23, 1670: copies of observations of the moon, by ancient astronomers, for determining her parallax: further observations made at Derby in 1674: the fragment of a letter to Mr. Townley on the parallax of Mars: and tables of the moon's southings in 1679—1682.

46. *Subsidiary Tables.* Quarto, bound in leather.

This volume contains nonagesimal tables for Dantzic, Uraniburg, and Greenwich; tables of Jupiter's satellites; a tide-table for London Bridge; and various other tables. The first 11 leaves are lost.

47. *Subsidiary Tables.* Quarto, bound in leather.

This volume contains tables of Jupiter's satellites; a tide-table for London Bridge; a table for the equation of time; a nonagesimal table for Greenwich; a table of the declination of the ecliptic; and various others.

Vol.

48. A *pasteboard box* containing ten books, marked A to K, each in a marbled paper cover. Small Quarto, marbled paper cover.

The first book contains an original letter from Mr. Abraham Sharp to Mr. Flamsteed, dated May 25, 1705; the rest of the book, as well as the books B and C, are occupied with Mr. Sharp's computations of the moon and planets, from observations made with the sextant. The book D contains the computed eclipses of Jupiter's satellites for the years 1700—1705. The book E contains a catalogue of the stars in the southern hemisphere; and various loose computations. The books F—I, contain sundry computations relative to various subjects. In this last book there are some memoranda relating to the printing of his works, and an account of some dreams that he had, about the same period. The book K contains tables of Jupiter and Saturn.

49. *Subsidiary Tables.* Folio, stitched in a marbled paper cover.

This book contains tables relative to the sun and moon for the years 1705—1707; tables of Jupiter and of his satellites; a fragment of a letter relative to the parallax of the earth's orbit; further tables of Jupiter and his satellites; another fragment of a letter on the parallax of the earth's orbit; a table of the values of the revolutions of the *cochlea* of the mural arc; and some others.

50. Another *pasteboard box*, containing ten books of subsidiary tables, marked A to K, each in a marbled paper cover. Quarto, marbled paper cover.

The book A contains solar tables. The book B contains tables of Saturn. The book C contains logistic logarithms. The books D—G contain some subsidiary tables. The books H and I contain lunar tables. The book K contains sundry papers as follow: viz. computations and diagrams of solar and lunar eclipses in 1675; a list of stars that may be occulted by the moon; a nonagesimal table for Greenwich; solar and lunar tables; a list of some stars omitted by Tycho and observed by Halley; a memoir on the motion of Jupiter; and at the end (turned upside down) a letter to Dr. Pell on the formation of the Board of Longitude; a paper on the French measures; and an optical essay, proving the truth and accuracy of celestial observations made with long telescopes.

51. A *Computation Book.* Small quarto, bound in vellum.

This book contains computations of various kinds, most of them relative to observations made prior to 1680. There is an index at page 415. The first 20 leaves are lost.

N.B. The preceding and 5 following books are quoted by Flamsteed as "Libri supputationum," and are marked on the front of the leaves Vol. 1, 2, 3, 4, 5, 6.

52. Another *Computation Book.* Quarto, bound in leather.

This book contains computations of various kinds, most of them relative to observations made in 1681–1683. There is an index at page 217.

Vol.

53. Another *Computation Book.* Quarto, bound in leather.

This book contains computations of various kinds, most of them relative to observations made in 1684–1689. There is an index at page 421.

54. Another *Computation Book.* Quarto, bound in rough leather.

This book contains computations of various kinds, most of them relative to observations made with the mural arc in 1689–1692.

55. Another *Computation Book.* Quarto, bound in rough leather.

This book contains computations of various kinds, and of various dates: there are also several subsidiary tables.

56. Another *Computation Book.* Quarto, bound in rough leather.

This book contains computations of various kinds, principally relating to observations made subsequent to 1700. There is a partial index at the end.

57. Another *Computation Book.* Quarto, bound in rough leather.

This book contains computations principally relating to observations of the moon, made with the mural arc: there are also some subsidiary tables.

58. Another *Computation Book.* Quarto, bound in leather.

This book contains computations principally relating to observations of the planets, ancient and modern: to which is prefixed a copy of the observations of the planets made with the sextant in 1679 and 1680. There is an index at the end.

59. Another *Computation Book.* Folio, bound in rough leather.

This book consists of several quires of paper bound together, containing computations of the moon and planets from observations made with the sextant: with an index to each part.

60. Another *Computation Book.* Folio, bound in rough leather.

This book consists also of several quires of paper, bound together, containing computations of the moon and planets, from observations made with the sextant and mural arc: with an index to each part.

61. A *pasteboard box*, containing five books, marked A to E, each in a marbled paper cover. Quarto, in a marbled paper cover.

The books A and B contain computations relative to the moon: the book C contains also computations relative to the moon, as well as to Jupiter's satellites: the book D

Vol.

contains computations relative to the planets: and the book E contains also computations relative to the planets, as well as to some of the fixed stars; likewise some subsidiary tables. All the observations appear to have been made with the mural arc.

62. Another *pasteboard box*, containing seven books; marked A to G; the first four in brown covers, the last three in marbled paper covers. Quarto, in a marbled paper cover.

The book A contains computations of zodiacal stars, from observations made with the sextant, and compared with those made with the mural arc: the book B contains similar computations of other stars, and some lunar computations: the book C contains similar computations of other stars, and also (at the other end, turned upside down) a collection of observations with the mural arc, prepared for computation similar to those in vol. 23: the book D contains also a collection of observations with the mural arc arranged in constellations for computation, similar to those in vol. 23: the book E contains computations relative to the moon, and at the end has some notes relative to the history of Flamsteed's life: the book F contains computations relative to the comet of 1680-1681: and the book G a few loose computations of stars observed with the sextant.

63. Another *pasteboard box*, containing nine books, marked A to I, each stitched in a marbled paper cover. Small folio, in a marbled paper cover.

These books contain the computations of the longitudes and latitudes of various stars, made by Mr. Luke Leigh, as mentioned in page 64 of the present work.

64. Another *pasteboard box*, containing nine books, marked A to I, each stitched in a marbled paper cover. Folio, in a marbled paper cover.

The books A—D contain computations of the longitudes and latitudes of the moon, from observations at various periods, ancient and modern: the books E—G contain similar computations of the planets, and some of the fixed stars: and the books H and I contain similar computations of the fixed stars only. An index is given to each book.

65. Another *pasteboard box*, containing ten books, marked A to K, the first in a parchment cover, the rest in marbled paper covers. Folio, in a marbled paper cover.

The first book is marked on the cover "S. 1690," and is so quoted by Flamsteed; it contains Mr. Sharp's computations (principally lunar) mixed with those of other persons: at page 91 there is an index. The books B and C contain lunar computations: the book D contains planetary computations, ancient and modern, and (at page 48) tables of Mars: the books E and F contain Mr. Ryley's computations of the longitudes and latitudes of certain fixed stars: the book G contains similar computations by Flamsteed: the book H contains various computations and remarks on different subjects; amongst others, the mode of his determining the value of the divisions of his micrometer: the books I and K contain various detached computations and remarks, of no importance.

Vol.

66. A large quarto book, bound in a stiff marbled paper cover.

This volume contains various subsidiary tables: viz. the distance of the ecliptic from the pole, for every degree of right ascension, and other useful subsidiary tables.

67. A *pasteboard box* containing 14 books, marked A to O, each in a marbled paper cover, except the last two. Folio, bound in a marbled paper cover.

The book A is in quarto, and entitled by Flamsteed "Miscellanea Flamsteediana," and consists of various geometrical problems. The book B is entitled by Flamsteed "Officina "et Repositorium Astronomicum," and contains the computations of several very old eclipses. The book C contains a synopsis of his planetary observations made with the sextant and mural arc. The book D contains a "Treatise on Levelling," the author unknown. The book E contains "A sequel to a Discourse on Local Motion," the author also unknown. The book F consists of synoptical tables, entitled "Cosmographia;" the author also unknown. The book G is a long memorial to the King from John Shaw, respecting the finding of the longitude. The book H contains Lord Brouncker's Demonstration of the vibration of a Pendulum in a cycloid. The books I—M contain miscellaneous subjects of no apparent interest or importance. In this portfolio is also deposited a large sheet of paper marked N, containing the observed and computed places of Mars from 1671 to 1704: and also Dr. Maskelyne's catalogue of these MSS, marked O.

68. A *pasteboard box* containing 10 books, marked A to K, each in a marbled paper cover. Quarto, bound in a marbled paper cover.

The book A is by Jeremiah Horrox, and entitled "Anti-Lansbergianus, &c." The book B is also by Horrox, and entitled "Philosophical Exercises." The book C is entitled "Venus in sole visa, Nov. 24, 1639." The book D is entitled "Explicatio brevis et perspicua diagrammatis Hipparchi: Lansbergii errores 20." The book E is entitled "Jeremiæ "Horroxi Præludium Astronomicum." The book F is by Flamsteed, and entitled "Epis-"tola prima . . . Ricardo Townleio Arm°. April 1674, aucta et transcripta:" and is the letter quoted by me in the note in page 33. The book G is a Lecture read on Monday, Feb. 25, 1705-6. The book H contains a paper entitled "Tractatio Maximi Domini "Dunstani Archipiscopi Cantuariensis, Viri Philosophi, de Lapide Philosophorum." The books I and K contain miscellaneous subjects of no apparent interest or importance.

69. A *Portfolio* containing various fragments of letters, papers, diagrams, computations, &c. &c. Folio, in a marbled paper cover.

70.
71. } A *Treatise on Perspective*, in two volumes. Quarto, bound in rough calf.

These volumes are not in Flamsteed's hand writing: nor can I ascertain the author of them.

PART I.

FLAMSTEED'S *History of his own Life and Labors*, compiled from Original Manuscripts in his own handwriting.

TO WHICH IS SUBJOINED,

An APPENDIX, containing a variety of *Original Documents*, confirming and illustrating the several facts therein recorded; and extending that history beyond the period narrated by himself.

Introduction to FLAMSTEED'S *Life, &c. By the Editor.*

THE following narrative of Flamsteed's life is separated into *seven* different divisions: being deduced from that number of separate and distinct documents, written at various times by himself; and scattered through several volumes of his manuscripts deposited, and now existing, at the Royal Observatory at Greenwich.

The *first* division is taken from a manuscript which has been designated by the title of "*Self-Inspections of J. F.*, being an account of himself in the "actions and studies of his twenty-one first years, written at several times by "his own hand," and hitherto known only by the extracts given from it in the *General Dictionary, Historical and Critical*, by Bernard, Birch, and Lockman, folio, 1737,* under the article "Flamsteed." The cover of the book is however now lost, and there is at present no title to it. The manuscript itself consists of only fifteen leaves of small post paper, and is preserved in MSS, vol. 32, A. It was finished in May, 1667. The facts therein recorded, although written in a quaint and imperfect style, and not partaking much of an astronomical character, are important, as bearing on a very eventful period of Flamsteed's life.

The *second* division is taken from MSS, vol. 32, B, entitled, "*Historica* "*Narratio Vitæ Meæ*, ab anno 1646 ad 1675," written in November, 1707. Some of the facts therein related have also been already published in the *General Dictionary* above-mentioned, under the article "Flamsteed." This division, although including a portion of the former, may be considered as containing his history from 1667 to 1675.

The *third* division is taken from a paper inserted in MSS, vol. 41, page 47, (numbered from the *end* of the book,) entitled, "*A Restitution and Rectifica-* "*tion of the Earth's Motion*, and places of the fixed stars, from my own "observations," written in June, 1685. This division continues the history from 1675 to 1683, and has never yet been made public.

The *fourth* division is taken from a paper inserted in MSS, vol. 39, page 81, entitled, "*Cœlum Brittanicum:* the Restitution of the places of the

* This work is generally quoted as the *Biographia Britannica*, and sometimes as *Birch's Dictionary*.

" fixed stars, from observations made at the Observatory:" written in June, 1695. The part which is extracted commences at page 95 of the MS; the preceding pages containing only a repetition of facts, noticed in the former divisions. This division contains the history of his life from 1683 to 1690, and has never yet been made public.

The *fifth* division is taken from some *Notes,* relative to his life, from his birth to the year 1704, written on the cover and first three pages of MSS, vol. 17. This portion of his life has already been published by Mr. William Hone, in his *Every Day Book,* vol. 1, page 1091. I have not given the whole of the MS, as several of the facts will be found already noticed in the former divisions of his life: and I would here remark that I have added a few other facts, in the order of events, that have been extracted from other documents, in Flamsteed's handwriting, noticed in the references. These Notes were probably written in the year 1705. This division (after the repetition of a few facts) may be considered as containing the history of his life from about 1690 to 1704.

The *sixth* division is taken from a MS paper, inserted in MSS, vol. 35, page 163, entitled, " *The brief History of the Observatory,*" written in January, 1710–11. This paper consists only of three leaves, and although written in the *third* person, is in Flamsteed's own handwriting. The part here extracted commences at the fourth page of the MS; the preceding pages containing only a repetition of facts, noticed in the former divisions. This division includes a portion of the former, and contains the history of his life from 1695 to 1704, and has never yet been made public.

The *seventh* and last division consists of that portion of the " *Original Preface* " which has been suppressed by the editors of Flamsteed's *Historia Celestis.* It is inserted in the MSS, vol. 32, C, and the document itself commences (at page 38 of the MS book) with the description of the mural arc; which corresponds with the Latin translation of it, as given in page 108, &c, of the Prolegomena, in the third volume of the *Historia Celestis;* and continues till we come to page 160 of the Prolegomena, where, at the bottom of the printed page, there seems (from the vacant space there left) to have been something originally inserted but afterwards excluded. It is at this place that the suppressed portion of the original preface commences, and ought to have been inserted. It begins at page 74 of the MS book; and, after extending to the end of that book, is resumed at the beginning. It was written in February, 1716–17. This division contains the history of his life from 1704 to 1716, and has never yet been made public.

Besides the documents above-mentioned, there are a few others, to which I have occasionally referred for the elucidation of various portions of Flamsteed's history, in the notes to the principal narrative; these are

1. *Excerpts from Mr. Newton's letters*, bound up in MSS, vol. 35, page 151.
2. *Short note of dates for my works*, December 19, 1710, bound up in MSS, vol. 35, page 155.
3. *Notes to my state of the Observatory*, inserted in MSS, vol. 33, page 51.
4. *Notes copied from my old Almanacks*, inserted in MSS, vol. 41, page 201.
5. *Memoranda* made at the end of a book of calculations, MSS, vol. 62, E.
6. *Memoranda* made on half a sheet of paper, preserved in MSS, vol. 35, page 171.
7. *Memoranda* made on another half sheet of paper, preserved in the same volume, page 173. This was written in March, 1707.
8. *Memoranda* made on another half sheet of paper, preserved in the same volume, page 195.
9. *A diary of events* from November 8, 1704, to April 15, 1707, scattered through several pages of the letter-book marked A, (MSS, vol. 33,) containing a variety of other documents connected with Flamsteed's history. The pages of this book are numbered from each end.

These nine documents, as well as the *whole* of those above described, are in Flamsteed's own handwriting; except, perhaps, the "Excerpts from Mr. Newton's letters," which, however, are signed by him, and therefore of equal authenticity.

The commencement of each of the above divisions of his history (except the first) generally goes *back* a few years, which sometimes causes a repetition of facts. This however has been obviated as much as possible in the selection and arrangement of the several parts; and, on the whole, I do not apprehend that any confusion from this source will arise.

The APPENDIX consists of a variety of documents, which, for the most part, have been preserved by Flamsteed himself (some original and some copies), and are now existing at the Royal Observatory. The greater portion of them are collected together by himself in a book which he called his letter-book, above-mentioned (MSS, vol. 33): others (which were found by me loose and scattered

about) are now bound up together in MSS, vols. 35, 36, and 37; and some few will be found distributed through his other various MS volumes. Advantage has also been taken of the discovery of his original MS letters to Mr. Abraham Sharp, and the recent disclosure of their contents, which throw great light on many portions of his history. And I have availed myself of the kind assistance of Professor Rigaud, of Oxford, who has procured for me copies of Sir Isaac Newton's original Letters to Flamsteed, deposited in the library of Corpus Christi College in that University, most of which have never yet been published. I have also extracted some documents from the Minute-books of the Royal Society, which, by permission of his Royal Highness the President, and the Council, are now for the first time made public. In a very few instances I have reprinted some letters that had been previously before the public, with a view to save a reference to other works of difficult access.

The reader is thus apprised, and will therefore bear in mind, that the *whole* of the text of the following narrative of Flamsteed's life, and of the subjoined Appendix, is printed *verbatim et literatim* from the documents above-mentioned; except as to the orthography. And that the Editor is responsible only for the *Notes* appended thereto, which are signed with the initials of his name.

F. B.

First Division.

FROM BIRTH TO 1667.

GOD suffers not man to be idle, although he swim in the midst of delights; for when He had placed His own image (Adam) in a paradise so replenished (of his goodness) with varieties of all things, conducing as well to his pleasure as sustenance, that the earth produced of itself things convenient for both,—He yet (to keep him out of idleness) commands him to till, prune, and dress his pleasant verdant habitation; and to add (if it might be) some lustre, grace, or conveniency to that place which, as well as he, derived its original from his Creator. We may suppose man, in his innocency, did strictly prosecute the just injunctions of his Divine Creator; and Scripture shows us that he did retain the pleasure of this gorgeous habitation, till, striving to equal his Creator in knowledge, he lost the pleasure of his paradise, together with the presential knowledge of his Creator. Man's active soul had acted now too far to gain by a recession what his over-active inquisitiveness had induced on him; for he ejected from his pleasant habitation his children, made heirs of the fruits of his fall; and the earth (which formerly produced, of its own accord, sufficient for human necessity) is cursed for his sake, that he might earn his bread forth of his labour, and keep himself from worse employment by his necessary action: for we, who are Adam's heirs by birth, observe that those are generally worst employed who have least to do; and idleness is the prodrome of other evils.

To keep myself from idleness, and to recreate myself, I have intended here to give some account of my life, in my youth, before the actions thereof, and the providences of God therein, be too far passed out of memory; and to observe the accidents of all my years, and inclinations of my mind, that whosoever may light upon these papers may see I was not so wholly taken up, either with my father's business or my mathematics, but that I both admitted and found time for other as weighty considerations.

I was born at Denby, in Derbyshire, in the year 1646, on the 19th day of August, at $7^{h.}$ $16^{m.}$ after noon. My father, named Stephen, was the third son of Mr. William Flamsteed, of Little Hallam; my mother, Mary, was the daughter of Mr. John Spateman, of Derby, ironmonger. From these two I derived my beginning, whose parents were of known integrity, honesty, and fortune, as they [were] of equal extraction and ingenuity; betwixt whom I

[was] tenderly educated (by reason of my natural weakness, which required more than an ordinary care) till I was aged three years and a fortnight; when my mother departed, leaving my father a daughter, then not a month old, with me, then weak, to his fatherly care and provision. She died on September 7, 1649.

It was three years after my own mother's death, that my father could so well digest as to accept a second marriage; which then he did, and married Elizabeth Bates, who, after she had lived with him a year and ten months, brought him my sister Katherine: after which, just on that day two years after my father brought her home, she died (November 1, 1654). And now my father had me, my sisters Elizabeth and Katherine, left to his care and protection, when I was aged eight years and two months.

My first ten years were spent in such employments as children use to pass away their time with; affording little observable in them. But afterwards my practices began to show my inclination more plain: for when, by my father's care, I had gotten at school so much Latin as might make me understand an elegant English [author], I began to affect the volubility and ranting stories of romances; and, at twelve years of age, I first left off the wild ones, and betook myself to read the better sort of them, which, though they were not probable, yet carried no seeming impossibility in the fiction. Afterwards, as my reason increased, I gathered other real histories; and by that time I was fifteen years old, I had read, of the ancients, Plutarch's Lives, Appian's and Tacitus's Roman Histories, Holingshed's History of the Kings of England, Davies's Life of Queen Elizabeth, Sanderson's of King Charles the First, Heyling's Geography, and many other of the moderns; besides a company of romances and other stories, of which I scarce remember a tenth at present.

But now the providences of God became more observable upon me, and unto me; for in the latter end of the year 1660, and the beginning of 1661, it pleased God to inflict a weakness in my knees and joints upon me. What natural cause might give it an occasion I know not; but in [the] summer preceding, being bathing myself, together with some boys, my companions, (we might, out of a general consent, enter those baths which Lord Aston had erected on the side of the river,) whence returning I found no hurt; but when I arose the next morning, my body, thighs, and legs were all so swelled, that they would not admit me to get my usual clothes upon them; which swelling (being laid by rubbing my body and legs with vinegar and clay, but its original being not evacuated) might, I suppose, fall into my joints, and thence cause my present impending weakness. This was, as near as I can remember, the first beginning

of my distemper: what other natural cause God made use of in inflicting it upon me I am ignorant. In the year 1662 it increased upon me, and had brought me so weak, that I was hardly able to go to school. When I left it*, my master at that time motioned my going to the Universities, of which my father (fearing, I suppose, my desire of going thither) told me not till afterwards. Other reasons perhaps he might have; as, knowing the negligence of servants, he might suppose that my presence at home might bridle, if not remove, those disorders which they were prone unto. Because I was now of more years and discretion than to be anyways obliged (either by menaces or intreaties, by carelessness or fear) to connive at those faults which my sister, although discreet, or rather witty, enough for her time, had not the judgment, care, or knowledge either to discover or prevent: she hardly then beginning to leave off her children's sports and trifles. My natural weakness might be another moving cause for his retaining me at home: hard study he perceiving already to distemper my body, argued that, where my studies would be my constant labour, my disease would be so much the more violent; and that if a day's short reading caused so violent a headache, a week's, or constant, study would make my disease intolerable. But I suppose that colds did oftener cause this disease than reading; and yet, if reading should promote it, yet moderation and reason might have prevented it: and he is not a man, or not himself, that cannot use his studies with moderation. Besides, the Universities might have afforded me so many advices and helps from the ablest physicians the world affords, and physic as light [and] cheap, anywhere for my disease, as no other place could yield me. But since that God hath otherwise disposed of me, I shall say no more of it, but only this,—that my desires have been always of learning and divinity: and though I have been accidentally put from it by God's providence, yet I have always thought myself more qualified for it than for any other employment; because my bodily weakness will not permit me action, and my mind hath always been fitted for contemplation of God and his works.

Being thus withdrawn from school, I, within a month or two after, had Sacrobosco's Spheres, in Latin, lent me, which I set myself to read without any director in it, but not unsuccessfully. For here I laid the ground of my mathematical knowledge; and in that winter, before Christmas, my father taught me arithmetic, with the doctrine of fractions, and the Golden Rule of Three, direct

* Tuesday before, or Whitsuntide. I cannot well remember whether it was Tuesday before: Whitsuntide being the 13th of May, 1662.

and converse, which I learned sufficiently promptly *. At Christmas, or a little after, I went to Uttoxeter, whither my father sent me for my health's sake, and took with me Fale's *Art of Dialling*; and having seen a quadrant formerly, whose fabric, it was told me, was laid down in that book, I set myself presently to calculate a table of the sun's altitudes, at all hours, in the equator, tropic, and some intermediate parallel in the latitude of 53°, by his tables of natural Sines; which I did (in Lent that year) without any help, and before that I heard of any artificial tables; and accordingly framed myself a quadrant, of which I was not meanly joyful.

This winter I was weak, and my disease held on with me till the summer, when it mended a little. This summer (1663) I prosecuted my studies; for, returning home, I was brought into company with Elias Grice, who told me of the artificial tables, and showed me (as I remember) Wingate's *Canon*. I likewise now got Mr. Stirrup's *Art of Dialling*, which I read this summer, and some other authors on mathematical subjects,—as Mr. Gunter's *Sector* and *Canon*; and soon after I acquired Oughtred's *Canon* of mine own. In all which I read some parts cursorily, not abiding a tedious prolection of any throughout, without the help or directions of any one; not being permitted (because they were scarce to be met with) the help of any one so much as to expound a term unto me.

My studies were discountenanced by my father as much in the beginning as they have been since; but my natural inclination forced me to prosecute them through all impeding occurrences. And, indeed, I think this mathematical quality no other than innate unto me; my father, in his younger years, having been as much affected with arithmetic as I at present with geometry and astronomy.

Having gotten the artificial Canon, I calculated several both general and particular tables, fitting the particular ones to the latitude of (Derby, my residence) 53° 0′, which will be found amongst my papers. I had some violent pains and a shortness of breath afflicting me; which, by God's mercy, and the means applied by my uncle, John Spateman, were removed: but my weakness held as ill as at first, and neither amended nor impaired this year.

* At this early age it appears that Flamsteed had commenced his astronomical career. For, in a paper entitled *Short note of dates for my works*, mentioned in page 5, he says, "I began my "studies in 1662: observed the sun's eclipse." This must have been the eclipse which happened on Sept. 12th in that year; and cannot be confounded with the one he observed in the year 1666, which is *also* mentioned in the *same paper*. This circumstance is one of very considerable importance, since it bears materially on a very eventful period of Flamsteed's life, as mentioned in the Preface. F. B.

I collected a calculative method of dialling from Mr. Gunter's *Sector*, and transcribed it (with a method for the construction of the quadrant, and tables fitted thereto, calculated by my own hand) in a small paper book; in which task, and perusing some other authors of various subjects, I spent my vacant time this year and the beginning of the following.

The winter came on, and my father thought it fit that I should undergo a course of physic, to try if thereby my weakness (which, according to its usual course, began to increase with the year upon me) might either be diverted or decreased. But it being thought too far in the year, it was remitted to the spring (1664); when Mr. Cromwell was cried up for cures by the Nonconformist party, to whom my father sent me, to be his patient, under whom I passed a course of purges and cordials: after all which I found myself no better than formerly, and so was by him left off to the mercy of God. My disease was, indeed, inscrutable by the physicians: its cause (for aught I perceived) being not understood by any of them. However, I am bound to acknowledge the mercy of God in that he hath removed my pains, and left me only under my weakness; whilst others, smaller offenders, suffer both weakness, intolerable pains, and other incommodities all together. And further, I am bound to bless and praise Him, for that he hath afforded my father a competent means and fortune to maintain me; whilst to a meaner man I might have been a burthen—nay (without a mighty Providence), an undoing.

This year I also became acquainted with my friends Mr. George Linacre and William Litchford. I affected the friendship of the former because of his knowledge of the fixed stars (few of which were unknown unto him, and by whom I learned those few I know); of the latter, for his knowledge of the erratic, and judgments on them. Somewhile it was ere that he would admit me that knowledge of his studies after our first acquaintance; but that day when he confessed it unto me, he also told me (amongst several answers he made my inquisitiveness) that he had calculated (and could promptly do it) the places of the planets to a given time by the tables in Mr. Gadbury's works. (Horrox's *Tables*, published by Mr. Shakerly, but perfected and reduced to current account by Mr. Gadbury.) I was desirous to essay all sorts of mathematical knowledge; and therefore (because I would not be seen with Mr. Gadbury's book, lest I should be suspected astrological) I bought Mr. Street's *Caroline Tables*, intending, when I had time convenient, not only to learn to calculate the places of planets,

but also to study their motions, and understand their difficult theory; but, being someways hindered, I did nothing in it till the year was over.

I had now completed eighteen years, when the winter came on, and thrust me again into the chimney; whence the heat and the dryness of the preceding summer had happily once before withdrawn me. But, it not being a fit season for physic, it was thought fit to let me alone this winter, and try the skill of another physician on me in the spring.

The year was newly entered, when, on the first day thereof (viz., the 1st of January, 1665) I, having some vacant time, set myself to calculate the true places of the planets to a given time, by my formerly-mentioned tables: and accordingly effected it, though not so exactly as by my following calculations, yet so auspiciously, as gave me a further encouragement to prosecute these endeavours; in which I observe it was my fault to err more through want of care than knowledge, which, since I animadverted it, I have striven with double care to prevent. I busied myself afterwards in writing an *Almanac Burlesque* for the year 1666, but never offered it to the press.

The spring now approached; and on the 8th day of April, about half an hour past two in the afternoon, I applied myself to that no less honest than able physician, Mr. Willoughby, who (not willing to weaken nature, that was low enough already, before he strengthened it) prescribed a cordial yet cleansing drink, which I used for some time; but without any apparent recruit of strength to my legs at this time. I had, in the summer of the preceding year, calculated several new tables, and digested some of them into a convenient book; and this year I added some more unto it, though I had not time, nor ever shall have, I fear, to conclude and finish it. I also busied myself very much in calculating the nativities of several of my friends and acquaintance, which I have since corrected, and shall transcribe on a convenient paper.

The former part of this year had been famous for the appearance of the comet; and this was much celebrated by the report of the cures done in Ireland by Mr. Valentine Greatrackes, by the stroke of his hands, without the application of any medicine. At first, we supposed this to be only a fiction; but when the report was confirmed by a particular relation of several strange cures effected, my father (who intended not to pretermit any occasion of [my] recovering in strength) resolved to send me over into Ireland, to try if I might, by God's blessing, receive my strength again. But, upon some occasion, this journey was put off till the 26th of August following; when, in the interim, having some

small time, I set myself to write the construction and uses of a quadrant, with necessary tables for the framing of the same, as also of a ruler, which I had drawn with my own hand, fitting both for the latitude of 53°. I performed it for my loving friend, William Litchford, beginning it on the 8th of August, and finishing it on the 24th day of the same month. I called it my *Mathematical Essay*, it being the first piece that ever I wrote for any one; and it is still to be found in his hands, for aught I know to the contrary.

And now, on August the 26th, 1665, being aged nineteen years and six days, ($19^{yrs.}$, 6^{days}, $11^{hrs.}$,) I set forth for Ireland, with Clement Spicer with me: and on Tuesday, about noon, we came to Liverpool; where we stayed till Friday the 1st of September, when the wind turned east. We embarked in a vessel, called the Supply, about noon: and on Saturday night came within sight of Dublin; but, by reason that we wanted water, could not cross the bar that night. In the mid night we thought to have gotten in with the tide, but had like to have run upon the Lambay: so that we cast anchor again, and lay still that night; and on the next day, at noon, we put in, but could not be suffered to land, by reason that the sickness being very hot at that time in London, all passengers were examined whence they came, and we not unstrictly. At last, our master went forth to fetch our tickets, or a license rather, but returned not; so that we paid the master's friend, and several slung down the ropes: till at last a ladder was set, down which I and the rest of our company descended, and framed our course on the sands towards the King's End. And here I have cause to remember the providence of God, who preserved me when I had like to have been led a wrong way by my aged guide, had not those who came behind us happily turned our course to the right place; and so we came to the King's End.

It was night, the doors were shut: and we ran from door to door to inquire for entertainment; which at last we got at a paltry inn, where was no meat I could eat, but brown bread and ale; of which I made a hearty meal, and lodged that night in a straw bed, with a sheet and a half; and yet, God be praised, I both fed and slept very well. Next day we got to Dublin, where we stayed at the Ship, in Dame Street, till Thursday following, when (Sept. 6) we set [out] on our journey towards the Assaune. We dined at the Naas, a town accounted handsome amongst them, twelve miles from Dublin; and lodged that night at a small town called Tomalins, paying for our meals sixpence a piece, and yet no great accommodation. We thought to have lodged at Killcullen, a town six

miles from the Naas; but finding that we had time, we came forward to this town, some four miles farther.

We travelled, with the mountains on our left hand, on a fair champaign, free from all difficulties of passage or bogs: the way being sometimes gravelly, sometimes pasture, or beaten road, and one of the greatest in the kingdom, not easy to be missed, except by a traveller that will mislead himself. It leads from Dublin to Kilkenny, Clonmel, and Cork. Few hedges to divide the lands or enclosures, but only banks of about a yard high; seldom with ditches to supply their office, which are easily passable by a traveller (or, indeed, almost anything) anywhere. And in this day's journey I saw but one wood, besides the Park at Dublin, which is not accounted any: a thing I thought observable in a country reported to be so full of them.

The house we lodged at, at our coming in, was strewed over with gorse, (the usual fuel of the country in that part, where coals are not to be had, except [at] too large rates,) and a barefoot boy was called in to bait a fire, which made me fear such an entertainment as might be afforded from an Irish house: but we were brought afterwards into a back room, indifferently handsome, where we had a table neatly spread with as fair and fine linen as ordinarily in England, and accommodation better than I expected.

In the morning (Sept. 8) we rose early to be going on our journey, and by noon we reached to Carlow, some fourteen miles from Killcullen, where we baited at a handsome inn; and this town is one of the fairest I saw in our journey. It stands by the side of a river, of an indifferent depth, and seemed to be indifferent large to me, who had not leisure to perambulate it, or any other we passed through.

We went forward to Laughton Bridge, five miles farther; a little town standing upon a large river, passable only, as I was informed, at the bridge, on which stood a large stone house, builded, I suppose, for a fort to command that place. Here we stayed not, but went forward to Goaren, five miles beyond it. Here we thought to have lodged: but having time to go farther, we resolved to proceed, and so came to Bennit's Bridge; a little town, where, at that time, was held a fair, composed, for aught I saw, of nothing but sheep, kine, and oxen, of the Irish sort. A company of bouzes were raised, covered all over with blankets, sheets, rugs, and linen cloth, fashioned like those in our fairs in England, but that they were scarce so handsome. They were covered on every side, so that you could not see into them, except they were opened, as one of them was by

chance as I passed by it, in which I could perceive nothing but a company of people set round about the sides of [it] : and whether they were eating and drinking by turns, as they use to do, I could not, without too much boldness, attain to perceive. This place is three miles from Goaren, and stands upon a large river. Here we thought to have lodged ; but supposing that because of the fair we could neither have quiet rest nor good accommodation, we were persuaded by a Nottinghamshire man, seated there, to pass forward to Barneschurch, three miles farther, a little town, standing partly on a hill ; whither we went with the people from the fair, and lodged at one Sharman's house, where we had indifferent good accommodation. In this day's journey, as I remember, we saw no woods at all. When we were at Bennit's Bridge we were but three miles distant from Kilkenny, the second place in the kingdom : and hitherto we had a fair road, not easy to be missed ; but now, having lost it, we had much ado to direct us in the following part of our journey, which we rose indifferent early in the morning (Sept. 9) to prosecute. Leaving Barneschurch, we passed by Newton and Ballatoben, two towns of Irish-built houses ; the first two, the second three, miles distant from Barneschurch ; and so forward to Nine-Mile House, distant some seven miles from Barneschurch : thence to Clonmel, nine miles farther, where we baited, having passed by some small, poor places by the way, whose names I know not. Here we crossed the mountains, which before were on our left hand ; and here only, in our going, we lost our way, yet were we never far out of it. It was after four o'clock in the afternoon when we left Clonmel ; so that we reached that night no farther than Castleton, called commonly Four-Mile Waters. And were advised by a woman, with whom we rode in company, to cross the waters that night, because the least plash of rain would cause an extraordinary flood, by reason that the waters running from off all the adjacent mountains conjoining constitute this river.

Our landlord came from Uttoxeter, in Staffordshire, and was acquainted with my grandfather Spateman : so that we were, in all things, very well accommodated for our acquaintance. On Sabbath morning (Sept. 10), I inquired where they went to church ; but was answered that their minister lived twelve miles off, and that they had no sermon amongst them, except when he came to receive the tithes, which was but once a year. And the woman with whom we came hither told me, in a complaining manner, that *they had plenty enough of every thing necessary except the word of God ;* and therewith told me that their minister lived twelve miles off, at the old Assaune, and came but once a year at them, as I told you afore. Considering which, I thought it better to prosecute

our journey on the Sabbath day than to lie in the alehouse; and so we discharged ourselves, and went to Cappoquin, eight miles farther, whither we got by noon; and now we had fixed our feet at the utmost extent of our journey forward. This is a small town, and lies upon the river Blackwater, eight miles up it from Younghall. It had formerly a bridge to pass over the river; but now hath nothing but a boat for passengers.

We heard that Mr. Greatrackes used to cure on the Lord's-day, Tuesday, Thursday, and Saturday, of course; and that the people who lodged at that place when we alighted were gone, expecting to be touched after sermon. Therefore, having refreshed ourselves, we went on foot to the Assaune, about a mile or more distant from Cappoquin, and entering into his house, we saw him touch several; some whereof were nearly cured, others on the mending hand, and some on whom his strokes had no effect,—of whom I might have said more, but that he hath been since in England; and so both his person, cures, and carriage are well enough known amongst us. And though some seem to asperse him each way, for my part I think his gift was of God; and for the course of his cures, I dare fully acquiesce with what Dr. Stubbs hath written of him. For though I am an eye-witness of several of his cures, yet am not able to remember or fitted to write them out as I saw them.

I was touched by him on my legs this afternoon (Sept. 11), but found not my disease to stir. Next morning I came again towards his house, and found him in his own yard, looking at his cattle. He had a kind of majestical, yet affable, presence, a lusty body, and a composed carriage. I desired the privilege of his touch, and was granted it presently; and saying to him I would not have been so hasty, had not our horse (which was a gentleman's courtesy to us) been on so bad a pasture, he very freely bade me bring him down to his house—he should have good feeding, and I should pay no more than I was to pay to my former host. I did so, and saw him put into a good pasture. And now I was stroked by him all over my body; but found, as yet, no amends in anything but what I had before I came to Cappoquin.

This Tuesday morning (Sept. 12) I went down to the Assaune, and was by him the third time touched; but not finding any amends, I determined to depart, and therefore went to Mr. Greatrackes, purposely to pay him for my horse's grass, and give him thanks for his courtesies. But he would not take anything of me: and when I urged him, saying I had not deserved this civility from him, he answered me I was a stranger, and he must be so to strangers. So we came back to Cappoquin, discharged our host, [and] departed to Clonmel that night,

where we lodged at a stately inn, whose master came out of Derbyshire, our county. This town is one of the seats of justice in this kingdom, and here all law businesses may be transacted, as at Dublin. It is built after the English manner, well fortified with a strong wall of limestone or marble; which I have observed, in several of their demolished small castles, to be made of small pieces, about as thick again as slates, laid thick in lime, which will damp any bullet. We entered over a drawbridge, at which a soldier stood centinel; a part of that river (as I remember) running under it, upon whose banks it is seated. This river is both deep and broad, so that the town is almost every way impregnable; and, in my mind, it is exceedingly pleasantly seated. From Cappoquin hither it is just twelve miles, but long ones.

We departed from Clonmel (Sept. 13), and by that time we had gotten some eight miles, we perceived that our horse had lost a shoe. We called at Nine-Mile House, but could not get a shoe. At one place (I think it was Grangymicleare) we found a smith, to whose shop, when we came, we saw nothing resembling his trade but the hearth, bellows, and anvil; neither iron nor shoes ready-made to be seen, so poor was the place and the people; amongst whose houses, as I remember, I saw but one with a chimney at it—a certain sign there were no more English inhabitants at this place.

We travelled nine miles farther to Bennit's Bridge without a shoe, where we baited, hoping assuredly not to miss of one here; but the smith was not at home: and because it was four of the clock, we resolved to go forward to Goaren, three miles forward, that night. We rode on hence thither; where, because it was late when we entered our inn, we had not time to get him shod this night.

At our entrance we met with some gentlemen going into the inn, whom we followed; and being alighted, and a little refreshed, we met with Mr. Toplady, (whose father was of Nottingham, and whose brother I had known,) who travelled towards Dublin to gather his master's debts, who was [a] tradesman in London. He hearing me accidentally name the place whence I came, inquired several things of Derby. I asked his name; but he civilly declined an answer, telling me he would let me know more next morning (Sept. 14), when [we] were on our journey to Dublin, whither we agreed to travel together: with which answer I rested satisfied.

Goaren is a town consisting of houses built but slenderly, many after the Irish manner; only our inns were capacious, and carried a handsome aspect with them. Hence, having with some trouble got our horse shod, we departed;

and when we were on our journey, I renewed our former demand to Mr. Toplady; who told me his name, and that he was servant to Mr. Jekell, of London, and on his business to travel to the north of Ireland. And as we were inquiring of his forepassed journey, he told us that the preceding day, coming over the mountains, and being out of his way, he met with an Irishman; of whom inquiring the road to Goaren, he could get no answer in English, which he supposing to proceed rather from the man's knavery than ignorance, threatened him, and struck him with [his] whip: which nothing availing, he laid his hand to the hilt of his hanger, and threateningly told him—" Now, sirrah, if you answer not presently in English, here will [I] make an end of your days;" at which words the fellow spoke English presently, and directed him his way very readily. Since which, he would say, as he travelled with us, he carried his tobacco by his side. For he used afore to give the Irish tobacco (of which they are very desirous) to show him his way; but now he relinquished that custom, and resolved to make them do it perforce, and yet not to trust their perfidiousness.

This morning we got our horse shod with some trouble, and then discharging our host, departed. We came first to Laughton Bridge, a very commodious pass, upon a broad and deep river. Here was a fair kept when [we] passed by; in which I saw nothing but Irish beasts, and booths after their manner: it is five miles from Goaren, and hath some English-built houses in it. Here we stayed not, but passed on to Carlow, where we drank. It is five miles forth from Laughton Bridge: it is a very handsome place, and one of the fairest towns we passed through. But it being too soon to bait, we passed on three miles farther to Castle-Derman, where we baited at a pleasant inn; and afterwards passed on to Kilcullen Bridge, eleven miles farther, where we lodged that night, well accommodated in an inn that promised not much at first sight.

Hence next morning (Sept. 15) we hasted indifferent early for Dublin. At Kilcullen there was nothing observable, but that it consisted most of Irish houses and buildings. The bridge is a long mile nearer Dublin than the town, and is better accommodated with inns, by reason (I suppose) the river is only passable at that place. From Kilcullen to Racoole (the next place we came to of note, and where we baited) is twelve miles: we alighted at the sign of the Postboy, and had good accommodation for the time we stayed; and after dinner we passed from it. It is a small town; the buildings seem ancient; here a many Irish inhabit; and it is a dirty place. But leaving it that time, we came to Dublin, six miles farther, soon enough to make an ill market afore bed-time; which, for the tediousness of the story, I shall not relate.

We lodged at our former inn, and stayed here from Friday night, Sept. 15, till Tuesday the 19th of Sept. When, in the morning about nine o'clock, we went down to King's End to take shipping, in the Martin, of Liverpool, to return; and quickly came aboard our vessel, which was none of the best, and we had a sufficient number of our company. But before we left the city, we had returned Mr. Mabbot his horse, which he lent us, with thanks for so obliging a courtesy, which we could not have merited or expected from any one. He lent us 40*s*. at our departure, which we returned him by Mr. Arthur Bulkeley, who was the occasion of our bad bargain; and criminal, I fear too, otherways towards us, by whom he made his own markets: but I shall forbear him, because time may perhaps afford me satisfaction from him.

Tuesday, in the afternoon, near three o'clock, we set sail; but because we had delayed time too much, we were forced to borrow help to haul over the bar. We sailed that night, and the next day came before Chester bar about noon; but stayed so long in expectation of the high water, that the tide began to turn before we could get over; yet we came to harbour at Liverpool soon after sunset, and landing, betook ourselves to our former host for entertainment. We had fair weather and quick speed in our travels and passage over sea, the winds standing fair for us, both as we went and came; for which providence I have cause to praise God continually.

We heard this night that there was a carrier in town, on whose horses we might travel homeward as far as Holmeschapel. We met and agreed with him; and the next day, being Thursday, Sept. the 21st, about noon, we left Liverpool, and came that night to Zanchy Bridges, where we lodged that night. And the next day, being Friday, the 22nd day, we passed from thence to Warrington, and so, by the Cock at Budworth, to Holmeschapel; where the carrier set us down, and would not be persuaded to carry us any farther. We saw nobody on the way to Congleton that might carry us thither: till at last a carrier passed by with three horses, whom, with much ado, after he was passed by, we got to come back. With him we bargained; and, discharging the other, set forward for Congleton, whither we came at night, and where I alighted at Mr. Hunford's. But intending to lodge at Mr. Mottershead's, my father's host, I was told by him that he durst not afford me lodging, because the sickness (which was then rife, and raged much in several places) was reported to be in Liverpool, whence we came, and his neighbours would asperse him for it if he should admit us. So that I was forced to change my intended lodging, and lie at Mat. Lowneses's, who was one of my father's customers; where I was indifferently well accommodated. Next day, being Saturday, the 23rd of Sept., we parted from Con-

gleton, and rode to Longshaw, by Leek (where we had left a horse of our own) and paid for the horses which had brought us thither. It was before noon that we got to Longshaw, where we stayed not long, but passed on for Ashbourne; and at night, when we came to Brailsford, our horse stumbled and overthrew us both, but (I thank God) without hurt. And so we sped safe to Derby at night, after daylight was ended, which we had left on that day month before. For God's providences in this journey His name be praised. Amen, Amen.

Being returned, I was visited by my friends, I being so discomposed by my journey that I was not very fit to appear at church that day. Yet had I not been so ill, but that riding on a dull horse (who trotted hard) betwixt Holmeschapel and Congleton, I was a little galled. For I would not use that practice which an Irish gentleman reported, who had his horse's back galled always when he was ridden by one of his boys; at which wondering, he by chance meets his said boy, who was a natural Irishman, riding upon his galled horse with his breeches hanging buttoned about his neck; of which inquiring of him the reason, he answered it was because the horse should not gall him: but by that means the rider escapes and the horse is galled himself. This story I could not omit, because such passages are not usual amongst the English, to whom this scarce was known.

Not long after my return, I added an Appendix to my *Mathematical Essays*, which I had left in the hands of my friend W. Litchford, and intended for him; and I gave it him when I had finished it. I added to it the projection of an universal dial, and a catalogue of seventy of the fixed stars, with their right ascensions, declinations, longitudes, and latitudes, to the year 1701; which I had composed by the Tychonic places, and allowing the annual precession of the fixed stars 50″.

I also proceeded to perfect the calculation of the solar eclipse which should happen June the 22nd, 1666, in the morning, according to the *Caroline Tables:* in which I noted some incongruities and difficulties of calculation I now remember not; only I found by his tables at Derby

In the eclipse of the sun, June 21st, 1666, or 22nd *mane*,

	h.	m.	s.
Initium eclipsis	17	53	7
Hora conjunctionis	18	43	36
Maxima observatio	18	48	37
Finis	19	48	46
Duratio tota	1	55	39

Digiti eclip. 7° 28′ ad Aust.

In the winter following I was indifferent hearty, and my disease was not so violent as it used to be at that time formerly. But whether, through God's mercy, I received this from Mr. Greatrackes's touch, or my journey and vomiting at sea, I am uncertain; but by some circumstances, I guess that I received a benefit from both.

Feb. 12, 1665-6, I went to Worcester, where Mr. Greatrackes, who was then come to England, was; and was once stroked by him, but with no better effect than formerly, though several then were cured.

At Lenten Assizes, 1666, on the Sabbath, after the evening prayers, I was visited by Mr. Imanuel Halton, of Wingfield Manor. I had heard of him, and he of me, formerly, by my cousin Wilson. We being strangers to each other, and not having seen one the other formerly, to our knowledge, talked somewhat reservedly at first: after, more openly. Amongst other of my papers, I showed him my calculation of the aforesaid solar eclipse, which he accounted of more than any other, and desired a transcript of it. I likewise showed him a small Canon of natural and artificial Versed Sines, which he much commended, and of which I likewise afterwards sent him a copy. So we parted at that time with mutual promises of a future acquaintance. Not long after, he came to town, and we met again; when he promised me a sight of the *Richleian Tables* (which soon after he sent me), composed by Natalis Durret, a Frenchman, more laborious, in my opinion, than ingenious—if, at least, those tables be his which he exposes in that name; for I suppose they are rather the *Rudolphine*, reduced and enlarged by him. But the prescript to the tables (which is full of various faults, not to be excused by the press) I suppose may be wholly his; for the ingenious Kepler could hardly be thought guilty of such oversight, or rather errors. However, because the introduction was filled with some things I had not seen before, I translated it for my own use into English; and it will be found amongst my papers. However, that I might not seem to find a fault, and leave it as I found it, I corrected the piece in the margin; and so returned [it] to its master, with thanks for that obliging courtesy.

Soon after, having occasion to write to him again, to desire him to observe the solar eclipse I had calculated, I intimated in my letters that I wanted some solar observations. Which, when he understood, he sent me the first tome of Riccioli's new *Almagest*, in Latin; which I joyfully received, because it showed a method of finding the sun's true parallax (by observations of the moon's dichotomy), which I was very desirous of investigating at that time. But more of this hereafter.

I spent some part of my time in astrological studies, but so as my labours were rather astronomical. Amongst others, I spent some time on Mr. Linacre's and another great person's schemes; yet could I not anyways satisfy myself in the arcs of directions for the measuring of time; nor am I yet perfectly satisfied. Yet I think Kepler's measures most rational and best grounded: though, in the great person's nativity which I directed, I used Naboyd's measure, which is most in use amongst astrologers. In fine, I found astrology to give generally strong conjectural hints, not perfect declarations.

Healthful, and in these studies, I spent the summer [of] 1666. And now, August 19th, 1666, I was aged just twenty years, whence I begin a perfecter account of myself. After I had received Riccioli's *Almagest*, I set myself to read him, when an intermission from my father's business happened; which usually did at night in winter: and I took much pleasure in him. I found he differed from Tycho in the obliquity of the ecliptic $1' 30''$; making it but $23° 30'$, whereas Tycho makes it $23° 31' 30''$; and that he varied much from Tycho in the places and distances of the fixed stars sometimes $4'$. His obliquity of the ecliptic Riccioli confirms by his own observations: to which, and the sun's parallax, deposed by him, I shall say more in my astronomical works.

Thus I held on till December the 5th, when I found myself much pained with the headache and some other distempers; which, after a while, reduced me to my usual winter weakness, and left me as ill as formerly. I continued afflicted with a small pain and some grudgings of the headache for a month after; so that I ended the year 1666 and began 1666-7 with it.

I transcribed some things from Riccioli; and taking occasion to peruse his method of finding the sun's distance by the moon's dichotomy, I could not but observe how he introduced an arc for correcting the apparent dichotomy, and reducing it to the true; which cannot be admitted. For he supposes the moon's parallax to cause her to appear hollow at the dichotomy next succeeding the new moon, and more than half full at [the] dichotomy preceding the change; which I shall prove not to be so in my astronomical works. In the meantime, whoever will but read what he hath written (page 733, the third problem) will find how groundlessly he introduceth it, if they but seriously consider that the difference of the parallax of the moon's centre and her superior horn is equal to the difference of the parallax of her centre and her inferior horn; with a very small difference, which will scarce ever arise to half a second, were her diameter double the breadth it is.

Some considerations, likewise, of the different equations of time used by several

astronomers, though well demonstrated by none, caused me to strive for a demonstrable equation. I studied hard in this, and at first was of opinion that the natural days were always equal, and that there needed no equation of time. Whilst striving to demonstrate this, I proved the contrary: first, that the excentricity of the earth's orbit from the sun's centre caused an inequality; and afterwards, that the ecliptic's obliquity caused another inequality of the apparent day; which two causes applied together would make the absolute equation of time. But because I have elsewhere said enough of this already, in a letter of three sheets to Mr. Halton, I shall say no more of it in this place. I likewise endeavoured something in the obliquity of the ecliptic, the sun's true distance from the earth, and the mean length of the tropical year; in all which I have laboured with much difficulty this last April. And now I have brought my sheets up to my age, and have finished this the 8th day of May, 1667. *Deo gloria.*

J. FLAMSTEED.

[Here this portion of the MS terminates. But Flamsteed has added a kind of *postscript* thereto: which, being short, I shall here transcribe. F. B.]

Afterwards, I followed my mathematical studies closer, but kept no special account of my proficiency. I met with new authors, read something of Euclid, and employed myself in several readings, till the latter end of the year 1669, when I wrote an *Almanac* for the following year, not after the usual method, but much more accurately; inserting an eclipse of the sun that might have been observable, but was omitted in the Ephemerides, and five appulses of the moon to fixed stars. But this being rejected, as beyond the capacity of the vulgar, and returned me, I excerpted the eclipse and appulses, and addressed them, with some astronomical speculations, to the Royal Society; suppressing my name under my anagram. My little labour was better accepted than I expected: I received a letter of thanks from Mr. Oldenburg, the Secretary of the Society. My papers I sent to Mr. Stansby: he delivered them to Mr. Ashmole, the great lover of curiosities; and he presented them to the Royal Society.

These procured me a letter from Mr. Collins also, with an account of several new authors, and a promise of a good correspondence, which he maintained very ingenuously afterwards, procuring me many things I wanted. The second letter I had from him was dated Feb. 3, 1669-70. My first from Mr. Oldenburg was dated Jan. 14, 1669-70.

About Easter Term I made a voyage to see London: visited Mr. Oldenburg [and] Mr. Collins. And was, by the last, carried to see the Tower and Sir Jonas Moore, who presented me with Mr. Townley's micrometer, and undertook to procure me glasses for a telescope to fit it, for which I left three guineas in Mr. Collins's hands, but got not the glasses (being for a twelve-foot tube) till Sept. 18th following. This was the beginning of my acquaintance . . . . .

[The MS here ends abruptly. F. B.]

Second Division.

FROM 1666 TO 1675.

FINDING that the edition of my works is stopt, and not likely to proceed very speedily *; that, in the mean time, my distempers increase, whereby I shall be disenabled from carrying them on as I intended; and that, after all the pains I have been at, and the expenses I have borne, it has been suggested sometimes that I had little to publish, at others that I was averse to the publishing of them:—to clear myself from these calumnies and aspersions, I intend (with the assistance of that Good Providence, which I must ever acknowledge to have directed all my endeavours) to give an account of all my labors and studies, their beginning and progress, with the helps and assistance I have either received from others, or afforded them for carrying on of theirs, that those who come after me may honestly and sincerely prosecute these studies, depending on the favor of God, and giving Him only all the praise. And if I begin a little higher than I need, I hope it will not displease my reader: for ingenious men are much delighted to know both the beginnings and progresses of their studies, and the circumstances of their lives whom God has made eminent in their times.

I was born at Denby [5 miles from Derby] † in Derbyshire, August 19, 1646, at a ¼ of an hour past 7 at night; as I find in some old notes of my father's, who was the third and youngest son of Mr. William Flamsteed of Little Hallam in Derbyshire. My mother Mary, was the daughter of Mr. John Spateman of Derby. ‡ In my infancy, sickly. I was educated [at the free-school] at Derby, where my father lived, [till 16 years old. My father removed his family to Denby, because the sickness was then in Derby.] At 14 years of age, when I was nearly arrived to be the head of the free-school, visited with a fit of sickness, that was followed with a consumption, and other distempers; which yet did not so much hinder me in my learning but that I still kept my station till the form broke up, and some of my fellows went to the Universities:

* The reader will bear in mind that this was written in the year 1707. F. B.

† These additional particulars, included within brackets, have been taken from the early part of the document, in MSS, vol. 17, which forms the *fifth* division of Flamsteed's life. F. B.

‡ In MSS, vol. 16, at the end, Flamsteed has given a short account of his ancestors: whereby it appears that he was a descendant from "Mr. William Flamsteed who came out of the north, bought "the land at Hallam Mere, of one Robert Everet, it being then rated at 40*s.* per year rent, and died "in 1514." F. B.

for which, though I was designed, my father thought it not adviseable to send me, by reason of my distemper. [Recovered by God's blessing: went a journey to Ireland in the months of August and September, 1665.] Wrote *De æquatione dierum*, and made the tables for it, 1665*.

Languishing then at home, I had Sacrobosco *De Sphærâ* put into my hands. I had read a great deal of history, civil and ecclesiastical, before. This was a new subject to me; and having turned so much as I thought necessary for my use into English, I proceeded to make dials by the directions of some ordinary books: and having changed a piece of Astrology I found amongst my father's books, for Street's *Caroline Tables*, set myself to calculate the planets' places by them, and thus enquire the reasons of them: in which I found small satisfaction; that author being very concise and short, and leaving the reasons of his processes to be learnt from others.

Having calculated an eclipse of the Sun, by these tables, that was to happen June 22, 1666, I imparted it to a relation of mine who showed it to Mr. Imanuel Halton of Wingfield Manor; who, coming soon after to see me, and finding I was not acquainted with the astronomical performances of others, sent me Riccioli's *Almagest*, and Kepler's *Rudolphine Tables*, with some other mathematical books to which I was, till then, a stranger. He was a person of great humanity and judgment, a good Algebraist, and endeavoured to draw me into the study of Algebra by proposing little problems to me: which, having not long before made myself acquainted with Euclid, I gave him geometrical resolutions to; and never troubled myself with algebra till I came to London, where I found every small pretender to mathematics set up for an Algebraist.

This eclipse I observed afterwards: but, not being furnished with proper instruments, nor yet acquainted with the best way of observing, I cannot think the observations exact enough to be published†.

Another eclipse of the sun happening two years after, on the 25th of October, 1668, I calculated the times of the appearance from the *Caroline Tables;* and afterwards observed it. But, not being yet furnished with convenient instruments for measuring and correcting the times, I could not believe it accurate

* This latter passage, relative to the Equation of Days, is taken from the paper entitled *Short note of dates for my works*, mentioned in page 5. F. B.

† The following is the memorandum which Flamsteed has left of this observation:—"Anno "1666, not being then full twenty years of age, and having little experience of the best manner of "observing, on June 22nd in the morning I viewed the eclipse of the sun through a foot perspective, "at 6h. 44m. *mane*," &c.—MSS, vol. 40, page 81. See also MSS, vol. 12, page 3. F. B.

enough to be published: though I found by it that the tables differed very much from the heavens*.

The French Observatory was built this year, and Signor Cassini called from Italy to direct it: who now published his Tables for finding the Eclipses and Configurations of Jupiter's Satellites. These fell into my hands some three or four years after; and were of good use to me, however faulty when I began to observe them.

In the following years, 1669 and 1670, I compared Jupiter and Mars with some fixed stars, near which they passed: but, the observations (being made with short glasses of two feet, and only by estimation of the planets' distances from them, and comparing them with the small distances of fixed stars derived from Tycho's places) were not to be relied on. Only, I learnt by them that those distances were faulty; and the planets' places much different from those given in the ephemerides.

Mr. Street's equation of natural days being very much different from that used by Tycho, Bullialdus, and Wing, I had spent many thoughts upon it, at the same time as I remember I was calculating the solar eclipse: and at last found that supposing the earth's revolution to be equable about her axis, it could be no other than the difference of her mean and true right ascension; and consequently that the equation of the earth's orbit turned into time must make one of the ingredients or parts of it, and the difference of her longitude and right ascension the other. Whereupon I wrote a small tract about the inequalities and equations of natural days; which, having turned into Latin, I showed to Mr. Halton, who approved it: and six years afterwards it was printed with Mr. Horrox's posthumous works, and put an end to all that controversy.

The following years, till 1669†, I employed my spare hours in calculating the places of the planets, observed by Hevelius, and related in his *Mercurius sub sole visus*, from the Caroline Tables: whereby I found they agreed not so well with the heavens as I presumed they had; and that further observations were requisite to correct them.

I could not think of any more proper than those of the moon's and planets'

* It is, however, published in the first volume of the *Historia Cœlestis;* and forms the first printed observation made by Flamsteed. F. B.

† Four of the preceding paragraphs are, in the original MS, written on the opposite page of the book; which accounts for this apparent confusion of dates. F. B.

appulses to fixed stars, or transits by them: considering that they required but a slender apparatus of instruments, and might be taken by a single observer with ordinary assistance. I collected some remarkable eclipses of fixed stars by the moon, that would happen in the year 1670; calculated them from the Caroline Tables; directed them to the Lord Viscount Brouncker, then President of the Royal Society, and conveyed them into his [hands]. This labour was well accepted both by him and them, and brought me letters of thanks both from their Secretary Mr. Oldenburg, and Mr. Collins one of their members, with whom I had a faithful friendship and ingenious correspondence afterwards, so long as they lived. My letter was dated November 4th, 1669: Mr. Collins and Mr. Oldenburg, in January following*.

From this time I began to have accounts sent me of all the mathematical books that were published either at home or abroad. In June 1670, my father, taking notice of my correspondence with them and some other ingenious men whom I had never seen, would needs have me take a journey up to London, that I might be personally acquainted with them: that being the time of the year when his affairs would allow me liberty. I embraced the offer gladly, and

* It appears, from the account in the *General Dictionary*, under the article "Flamsteed," that the original MS of this communication to the Royal Society was in the possession of Mr. William Jones, the father of the late Sir William Jones. It is inscribed thus:—"To the right honorable "William Lord Brouncker, President of the illustrious Royal Society; also to the right worshipful, "worthy and truly ingenious Henry Oldenburg, Esq., Christopher Wren, M.D., and all other the "Astronomical Fellows of the said Society: J. F. humbly presents this epistle." At the close of it he writes thus:—"Excuse, I pray you, this juvenile heat for the concerns of science, and want of "better language from one who, from the sixteenth year of his age, to this instant, hath only served "one bare apprenticeship in these arts, under the discouragement of friends, the want of health, "and all other instructors except his better genius. I crave the liberty to conceal my name, not to "suppress it. I have composed the letters of it in Latin, in this sentence, *In Mathesi a sole fundes.* "I had many materials to add; but they would have swelled my letter beyond its prescribed limits. "If I may understand that you accept of these, or think them worthy your notice, you shall ere "long hear more from yours, &c., J. F." The letter of Mr. Oldenburg, in answer to this communication, dated Jan. 14, 1669-70, is given in the Appendix No. 1: and Flamsteed's reply thereto, dated Feb. 7, following, in the Appendix No. 3. Flamsteed's letter to Mr. Collins, of Jan. 24, 1669-70, is given in the Appendix No. 2. There are many other valuable letters, addressed by him to Mr. Collins, amounting to upwards of thirty in number, from Jan. 24, 1669-70, to Dec. 27, 1673, which are published in the *General Dictionary* above-mentioned. There are also preserved, in the MS Letter-books of the Royal Society, upwards of forty original letters addressed by him to Mr. Oldenburg, dated from Derby, and extending from Feb. 7, 1669-70, to Jan. 25, 1674-5; as well as some others dated from Greenwich in the years 1676 and 1677; and some copies of letters addressed to Sir Jonas Moore, Mr. Collins, Cassini and Hevelius. F. B.

there became first acquainted with Sir Jonas Moore [His Majesty's Surveyor of the Ordnance], who presented me with Mr. Townley's micrometer, and undertook to furnish me with telescope glasses at moderate rates. I left monies in Mr. Collins's hands to pay for them: and in my return visited Dr. Barrow, and Mr. Newton, the Lucasian Professor of Mathematics at Cambridge *; and Dr. Wroe, then a fellow of Jesus College there, with whom I corresponded frequently the four following years. Entered myself at Cambridge in Jesus College.

About this time Mr. Newton was engaged in experiments about Light and Colours, and the improvement of telescopes; of which I had some account sent me by Mr. Collins: though his theory and the description of his new contrived telescope came not out till February 1671-2; when it was published in the *Transactions*, No. 80.

I could not at first yield to this theory: but, upon trial, found all the experiments succeeded as he related them; which kept me silent and in suspense. For, I could never think that whiteness was a compound of all the different sorts of rays of light mixed; because I found always that what he called whiteness was only sun-light, or solar rays: and that when the rays, which he called whiteness, were mixed with the blue, they formed a green; which showed they were of the nature of yellow.

My first telescope glasses were not procured me till about Michaelmas 1670: but the eye-glasses suited not with them. And both Mr. Jonas Moore and Mr. Collins having employments that kept them continually in business, I could not procure such eye-glasses for them till the next autumn 1671. [Here the description of Mr. Townley's micrometer is to be inserted; with the tables for turning the revolves and parts, into minutes and seconds: as also the figures and descriptions of my own, with the like tables †.]

In the mean time, some affairs of my father's requiring it, in the month of June this year I made a journey into Lancashire, and called at Townley, to visit Mr. Christopher Townley, who happened to be then in London. But, one

* In MSS, vol. 17, mentioned in page 4, Flamsteed says that in the year 1674 was "my *first* "*acquaintance* with Sir Isaac Newton at Cambridge, occasioned by my fixing the microscope, which "he could not; the object-glass being forgot by him." Probably the visit mentioned in the text was one of mere ceremony, and might not be considered by him as amounting to a claim of friendship. F. B.

† This passage, within brackets, is written on the opposite page, in the original manuscript. The description of the micrometer still exists, and may be seen in MSS, vol. 43. F. B.

of his domestics kindly received me, and showed me his instruments, and how his micrometer was fitted to his tubes: and from this time forward we often conferred by letters. I procured Mr. Gascoigne's and Crabtree's papers from him *; and Mr. Horrox's theory of the moon, to which he had begun to fit some numbers; but perfected none that I remember.

About this time, Mr. Horrox's remains and observations, having been collected by Dr. Wallis, were in the press. [I found his theory (of which a correct copy had fallen into my hands) agree much better with my observations than any other. Hereupon I fitted numbers to it, which with an explanation of it were printed with his works†.] Mr. Collins advised me to print my discourse concerning the Equation of natural days with them: which I consented to do; and sent it up to him for that purpose, translated into Latin.

[In March 1671 set up a pole to raise my glasses, at Derby‡.] It was October 1671 before I could get my tubes and micrometers in good order for observations. I had no pendulum movement to measure time with: they being not common in the country at that time. But, I took the heights of the stars, for finding the true time of my observations, by a wood quadrant about eighteen inches radius, fixed to the side of my seven foot telescope; which I found performed well enough for my purpose.

For, I had before resolved not to attempt anything that lay out of my power, or for which I had not made such provision as might probably afford me success: and therefore I resolved to confine myself to such observations as required no very accurate knowledge of the times. Such were the diameters of the luminaries; small distances of the fixed stars; the greatest elongations of Jupiter's satellites, &c.; which might be of use to me in the further progress of my astronomical studies. To such as these I confined myself at first: and that Good Providence, that had designed greater things to be afterwards done by me, gave me success beyond my hopes or expectations §. Having determined

* A long account of the contents of these papers is given in MSS, vol. 40, page 18, &c. F. B.

† This part, within brackets, is taken from page 91 of the paper entitled *Cœlum Brittanicum*, mentioned in page 4. F. B.

‡ This part, within brackets, is taken from MSS, vol. 17, mentioned in page 4. F. B.

§ In a letter dated Oct. 24, 1705, preserved in MSS, vol. 36, (the superscription of which is torn off,) Flamsteed says, "The Derby measures of the sun's and moon's diameters, and small distances "of fixed stars, &c., I take to be *very accurate*. For I had good leisure to examine my nice instru- "ments often and carefully." See a MS tract which he wrote on the apparent diameters of the planets, in MSS, vol. 41, page 220. F. B.

the diameters of the sun, in his apogee and perigee, I saw the excentricity of the earth's orb was bisected. And observing the moon's diameters in her appulse to the *Pleiades* Novem. 6th, 1671, when she was near the opposition of the sun, and again February 23rd, 1672, when she was not far from her quartile, I found that whereas the visible diameter ought, according to the lunar theories of Bullialdus, Wing, and Street, to have been greater at the quartile, or latter time, by about 45″ than at the opposition in November, on the contrary it was less by about 1′. 20″. Which showed that, from the opposition to the quartile, she removed from the earth: whereas their theories made her approach nearer to it, making her diameters bigger at the quartile than at the opposition by 1′. 30″; and that they erred also very sensibly in her visible place.

But, enquiring her visible place and diameters, by the tables I had fitted to Mr. Horrox's lunar theory, I found her place agree nearly; and her diameter at the full moon bigger than at the quadrature by about 50″: which convinced me that Bullialdus's, Wing's, and Street's theories were erroneous; and Horrox's near the truth. I did not then think the theory perfectly agreeable; for I found a dissent in my observations from it, by reason I had not yet attained the knowledge of a further necessary diminution of her diameters depending on her distance from the sun, with which Mr. Newton's corrections and emendations of that theory have furnished me since. These observations I imparted to Mr. Oldenburg, with the same remarks upon them; which occasioned their joint desires that, now Mr. Horrox's remains were in the press, I would add the tables I had fitted to his theory, with an explication of the theory itself, and directions to calculate the moon's places, &c., by the tables: which I willingly did, fitting the radixes of my mean motions to the meridians both of London and Derby, where I then thought my abode fixed, and hoped to carry on my observations to greater accuracy: for which, in my thoughts, I was frequently forecasting.

In the spring of the year 1672 I excerpted several observations from Mr. Gascoigne's and Crabtree's letters, that had not yet been made public; which I had turned into Latin, and resolved to publish in the first volume of Celestial Observations taken at the Observatory. Amongst Mr. Gascoigne's letters I found some wherein he showed how the images of remote objects were formed in the distinct base of a convex object glass. From these I got my dioptrics in few hours; having read Descartes' *Dioptrics* before, but learnt little by

them because he discourses not of this subject: his main business being to show how by elliptical or hyperbolical glasses all the rays of light that fall on the object parallel to the axes may be collected into one point of the image in the distinct base, supposing all the rays of light of the same species and liable to the same law of refraction; which yet Mr. Newton demonstrated they were not, by many experiments published in this year's *Transactions:* and this is the only thing that I can perceive for which Descartes' *Dioptrics* have been so celebrated. I finished my transcript of Mr. Gascoigne's papers May 12th, 1672. The spare hours of the remaining part of the year were employed in my observations, as the weather suffered me; in preparing advertisements of the Appulses of the moon and planets to fixed stars for the following year; which were printed by Mr. Oldenburg in his *Transactions*, with some observations of the planets I imparted to him.

Whilst I was enquiring for the planets' appulses to the fixed stars by the help of Hecker's ephemerides, I found that, in September, 1672, the planet Mars, then newly past his perihelion and opposition to the sun, would pass amongst three contiguous fixed stars in the water of *Aquarius:* and that, by reason he was then very near the earth, this would be the most convenient opportunity, that would be afforded of many years, for determining his, and consequently the sun's horizontal parallax. I drew up a *monitum* of this appearance, and sent it with a letter to Mr. Oldenburg, who printed it in his *Transactions*, No. 86, August 19th, 1672: having before sent my admonition into France, where the gentlemen of their Academy took care to have it observed in several places. My father's affairs caused me to take a journey into Lancashire, the very day I had designed to begin my observations: but God's Providence so ordered it that they gave me an opportunity to visit Townley, where I was kindly received and entertained by Mr. Townley, with whose instruments I saw Mars near the middlemost of the three adjacent fixed stars*. My stay in Lancashire was short: at my return from thence, I took his distances from two of them at distant times of the night †. Whence I determined his parallax then 25″, equal to his visible diameter; which therefore must be its constant measure; and consequently the sun's horizontal parallax not more than 10″. This I gave notice of in the *Transactions*, No. 96: and the French, soon after,

* These observations of Mars are recorded in MSS, vol. 39, page 175, and vol. 40, page 76; and are printed in the *Historia Cœlestis*, vol. 1, page 15. F. B.

† See these observations in the first volume of the *Historia Cœlestis*, page 16. F. B.

declared that from their observations they had found the same. Whether they will give such exactness, I leave to those who are skilful in these things to determine *.

It was this year that the French sent Monsieur Richer to observe the southern fixed stars at Cayenne; where he also observed this transit of Mars amongst the three fixed stars in the water of *Aquarius*. His observations are printed in the *Voyages Astronomiques*; from whence I have transcribed them to be printed after my own in the end of this Preface †. I have altered the method in which they are published, purposely to bring them into less room and better order for the service of those that have occasion to make use of them.

In the same *Transactions* were printed some observations of the greatest elongations of Jupiter's satellites from him; whereby the diameters of their orbits were determined in such parts as Jupiter's is *one*. These I found larger than Signor Cassini had determined them, in his Satellite Tables, 1668: but I suspect them less than the real truth, by reason that the diameter of the planet appears bigger than it is, by reason of the breadth of the pupil of the eye.

In the month of March of the following year, 1673, from the observations of Jupiter's distances from the 9th of *Virgo*, or the last of the four in the left wing, I determined the greatest inclination of the orbit of Jupiter to be less than the latitude of this star, by 26′ 40″ ‡. Its latitude in my new catalogue is 1° 46′ 10″ south: whence the greatest inclination of his orbit will be 1° 19′ 30″. These observations, with the process whereby it was determined, were printed by Mr. Oldenburg in his *Transactions*, No. 94, for May 19, 1673.

It was this year also, as I remember, I wrote a small tract in English § concerning the true diameters of all the planets, and their visible, when at their nearest distance from our Earth, or their greatest remove from it; which I sent to Mr. Newton in the year 1685, who has made use of it in the 4th book of his *Principia*.

From some observations of the eclipses of Jupiter's Satellites made this year, their mean motions were corrected by me: those of M. Cassini, published in the year 1668, and imparted to me by Mr. Townley, having showed themselves very faulty.

* Flamsteed drew up a paper on this subject, in the form of a letter to Sir Jonas Moore, dated Derby, April 11, 1674, which is inserted in MSS, vol. . There is also the fragment of another letter to Mr. Townley on the same subject, dated Jan. 15, 1673-4, in MSS, vol. 45. F. B.

† They are not given in the MS from which this is copied. F. B.

‡ See these observations in the first volume of the *Historia Cœlestis*, page 18. F. B.

§ See the note in page 30. F. B.

Sir Jonas Moore sometimes wrote to me; and, in his letters *, testified the pleasure he took in the success of my endeavours, and in what I imparted to Mr. Oldenburg, and was printed by him in the *Philosophical Transactions*.

By Mr. Oldenburg's means I changed some letters with M. Cassini. Having no longer glasses yet than of thirteen feet, I had not taken notice that the body of Jupiter was not perfectly round; and in one of my letters affirmed that, to me, he appeared always round, which he took notice of; and which caused me to consider him more attentively. And, in my view afterwards in the same glass of thirteen feet, I saw I had reason to suspect my heedless assertion; and, when I came to employ longer glasses, that he was (as Cassini had asserted) oval.

Besides the observations I imparted to Mr. Oldenburg, I took others that might be of use to me afterwards: though, because the times were not so accurate as I thought was requisite, I did not publish them †.

In 1673, besides my usual task, I wrote an Ephemeris, wherein I showed the falsity of Astrology, and the ignorance of those who pretended to it ‡: wherein I gave a table of the moon's risings and settings, carefully calculated; together with the eclipses and appulses of the moon and planets to fixed stars. This fell into the hands of Sir Jonas Moore, for whom (at his request) I made a table of the moon's true southings for that year. From which, and Mr. Phillips's theory of the tides, the high-water being made, he found they showed the times of the turn of the tides very near: whereas the ordinary seamen's coarse rules would err sometimes two or three hours. It was the summer of the following

* Some of these letters have been preserved by Flamsteed, and are to be found bound up in MSS, vol. 36. The first of them is dated March 7, 1673-4, and contains the following passage:—"I am "resolved, God willing, further to assist you with either books or instruments, as you will please to "call for them. I am ashamed such hopes, as we might have from you, should be discouraged by "*your* charges and pains: so little encouragement is there for poor Astronomy. Therefore to lessen "your labour I have proposed you will choose such a person as may be capable to do it, to be "attendant upon you and commanded by you; and to make observations and to write and compute "as you direct. And to such I will, during my life, bind myself to pay £10 per annum; and I "question not to get £10 per annum more. For Mr. Thereburn and Capt. Geo. Wharton, both in "the Tower, are willing to give £5 per annum, each, more." F. B.

† After he left Derby, he wrote a letter to Sir Jonas Moore relative to the state and progress of his observations there, which is given in the Appendix No. 4. F. B.

‡ In MSS, vol. 18, page 2, Flamsteed has drawn the horoscope of the heavens at the moment of laying the foundation of the Royal Observatory on Aug. 10, 1675: in the interior of which he has written in pencil *Risum teneatis, amici*. The horoscope is given in Mr. Hone's *Every-Day Book*, mentioned in page 4. F. B.

year, 1674, that I came to London, in my way to Cambridge*; whither Sir Jonas Moore (hearing of my intent) invited me, and where he received me very kindly †: told me how acceptable a true account of the Tides would be to his then Majesty King Charles II; offered me the help of his servant to make this table or any other work of the like nature. We resolved together to compose a small ephemeris for His Majesty's use: which was set upon, and in good part finished, before Midsummer; but not completed till near Christmas after, by reason that I returned to Derby about Michaelmas.

Sir Jonas heard me often discourse of the weather-glasses or barometer; and the certainty of judging of the weather by it. I had seen one of them at Townley; and Mr. Townley had told me his observations and rules deduced from them: which caused me to set up one at Derby, where, for three years together before this, I had noted three times a day commonly the height of the mercury in the barometer, and of a tinged spirit in the thermometer; and found, considering our different situations, that Mr. Townley's remarks agreed very well with mine, which were,—first, that upon every sinking of the mercury, the air was more moved, and that either wind or rain followed; not the same day always, but one, two, or three days after, according to the time and height it had been stationary at. Something of this had been noticed by Mr. Boyle, but not prosecuted, by reason that daily watching its motions and noting them was perhaps thought a trouble that such a trifle as the weather-glass deserved not. But, now, at Sir Jonas's request, I set him up a pair of these glasses, and left him materials for making more. It had been long settled fair weather when I left London: soon after that, my glasses began to sink, but no rain followed till the fourth or fifth day after. This made him esteem his glasses and rules very much: of which informing the King and Duke of York, he was ordered to fit them with them, the next day; which he did, together with my directions for judging of the weather from their rise or fallings. He had showed them my telescopes and micrometer before; and whenever he acquainted them with anything he had gathered from my discourse, told them freely it was mine. Whereby he confirmed them in their just opinion of his sincerity and candour;

* On May 2 he was in London; and on May 29 at Cambridge. F. B.

† In the letter quoted in page 34, Sir Jonas writes thus:—"I rejoice much that I may again "hope to see you; and do with all earnestness beg from you that, whilst you stay at London, you "will make my house your abode. I have a quiet house; a room fitted for you, and another for "your servant; and I have a library and all things else at your command." This invitation is repeated most pressingly in two subsequent letters. F. B.

prevented all envious reflections on himself, which courtiers are too apt to make, to prevent others from enjoying more of their Prince's favour than themselves; and procured me more than ordinary regards from them, and others of our nobility and gentry about the Court, that was very useful to me both during his life and after his decease.

Having taken my degree of A. M. at Cambridge *, I designed to take orders as soon as I conveniently could, and to settle in a small living near Derby, which I knew was in the gift of a friend of my father's, and would be at his disposal; and therefore I went to Okeham, in order to proceed to Peterborough at Christmas for that purpose. But the Good Providence of God, that had designed me for another station, ordered it otherwise: there was no ordination there. In the mean time Sir Jonas Moore, having been informed of my intent, wrote to me to return, as soon as I could, to London †; where he doubted not but I should find encouragement for my studies to my satisfaction; and invited me to make use of his house as formerly. I returned at Candlemas following [February 2] 1674-5, and was very kindly and cordially entertained by him. He had designed an employment for me, wherein I might have been helpful to his son, for whom he had procured the reversion of his place: which, though in

* This was on June 5, 1674. He had arrived in London May 2, and went to Cambridge on the 29th, where he took his degree. He returned to London on July 13; left it on August 17; and got to Derby on August 29. During his stay in London, he was at the house of Sir Jonas Moore in the Tower. F. B.

† By a letter from Sir Jonas Moore to Flamsteed, dated Oct. 10, 1674, and preserved in MSS, vol. 36, it appears that Sir Jonas proposed to fit up a house at Chelsea as an observatory, and to appoint Flamsteed to the care of it. This is confirmed by the following extract from the Minute-Book of the *Council* of the Royal Society: viz.

"October 19, 1674, Mr. Hook acquainted the Council that Sir Jonas Moore had been with him "at Chelsea College, and made an overture of engaging a gardener (a sufficient man) to take a lease "of the house, and land about it; allowing withal to the Society a power to make hortulan experi- "ments there: as also to build an astronomical observatory; which latter, the said Sir Jonas Moore "himself would undertake to do, at his own charge, to the value of £150 or £200."

"This proposition was well accepted by the Council, and Mr. Hook desired to prosecute the busi- "ness by urging Sir Jonas Moore to proceed further in that affair."

It should be borne in mind that Chelsea College was then the property of the Royal Society. The negotiation with Flamsteed appears to have been carried on: for in another letter, preserved in the same volume, dated Dec. 15, 1674, Sir Jonas writes thus:—"I desire to see you at the Tower, "where you will be extremely welcome to all of us, and where you may look after such instruments "as are needful for observation; and question not, long before we are satiated with your company, "we shall have provision made for your future maintenance." F. B.

nothing like his father, he enjoyed for the few years he lived. But, finding that I persisted in my resolution to take orders, and that his son's temper was such as would make me as uneasy as himself, he did not dissuade me *. March the 4th following, he brought me a warrant that designed me the King's Astronomer, with the allowance of only £100 per annum, payable by the Office of Ordnance, to commence from the Michaelmas before †; and the Easter following I took orders at Ely house, at the hands of Bishop Gunning, who ever after conversed with me friendly and freely, and would frequently discourse with me of the new philosophy and opinions, though himself had always maintained the old ‡.

Betwixt my coming up to London, and Easter, an accident happened that hastened, if it did not occasion, the building of the Observatory. A Frenchman, that called himself Le Sieur de St. Pierre, having some small skill in astronomy, and made an interest with a French lady, then in favour at Court §, proposed no less than the discovery of the Longitude: and had procured a kind of Commission from the King, to the Lord Brouncker, Dr. Ward (Bishop of Salisbury), Sir Christopher Wren, Sir Charles Scarborough, Sir Jonas Moore, Col. Titus, Dr. Pell, Sir Robert Murray, Mr. Hook, and some other ingenious gentlemen about the town and Court, to receive his proposals; with power to elect, and to receive into their number, any other skilful persons: and, having heard them, to give the King an account of them, with their opinion whether or no they were practicable, and would show what he pretended. Sir Jonas Moore carried me with him to one of their meetings, where I was chosen into their number; and, after, the Frenchman's proposals were read: which were

1°. To have the year and day of the observations:

2°. The height of two stars, and on which side of the meridian they appeared:

3°. The height of the moon's two limbs:

4°. The height of the pole:—All to degrees and minutes.

* During the few months he was staying with Sir Jonas Moore at the Tower, he made several astronomical observations, which are recorded in the first volume of the *Historia Cœlestis*, pages 26, &c. F. B.

† A copy of the warrant for the payment of this salary, is given in the Appendix No. 5. F. B.

‡ In another MS, entitled *The brief history of the Observatory*, mentioned in page 4, Flamsteed says (page 163) that "a larger salary was designed him at first: but, on his taking orders, it was "sunk to this." F. B.

§ In a MS paper by Flamsteed, entitled *Notes to my state of the Observatory*, mentioned in page 5, it is stated (page 51) that this lady was the *Duchess of Portsmouth*. F. B.

It was easy to perceive, from these demands, that the Sieur understood not that the best lunar tables differed from the heavens; and that therefore his demands were not sufficient for determining the longitude of the place, where such observations were, or should be, made, from that to which the lunar tables were fitted: which I represented immediately to the company. But they, considering the interests of his patroness at Court, desired to have him furnished according to his demands. I undertook it; and having gained the moon's true place, by observations made at Derby, Feb. 23, 1672, and Nov. 12, 1673, gave him observations such as he demanded. The half-skilled man did not think they could have been given him; but cunningly answered *they were feigned.* I delivered them to Dr. Pell, Feb. 19, 1674-5; who returning me his answer some time after, I wrote a letter in English to the Commissioners, and another in Latin to the Sieur*, to assure him they were not feigned; and to show them that, if they had been, yet if we had astronomical tables that would give us the two places of the fixed stars and the moon's true places, both in longitude and latitude, nearer than to half a minute, we might hope to find the longitude of places by lunar observations, but not by such as he demanded. But, that we were so far from having the places of the fixed stars true, that the Tychonic catalogues often erred ten minutes or more: that they were uncertain to three or four minutes, by reason that Tycho assumed a faulty obliquity of the ecliptic, and had employed only plain sights in his observations: and that the best lunar tables differ one-quarter, if not one-third, of a degree from the heavens: and lastly that he might have learnt better methods than he proposed, from his countryman Morinus, whom he had best consult before he made any more demands of this nature. I heard no more of the Frenchman after this; but was told that, my letters being shown King Charles, he startled at the assertion of the fixed stars' places being false in the catalogue; said, with some vehemence, "He must have them anew observed, examined and corrected, for the "use of his seamen;" and further, (when it was urged to him how necessary it was to have a good stock of observations taken for correcting the motions of the moon and planets,) with the same earnestness "he must have it done." And when he was asked Who could, or who should, do it? "The person (says he) that "informs you of them." Whereupon I was appointed to it, with the incompetent allowance aforementioned: but with assurances, at the same time, of such further additions as thereafter should be found requisite for carrying on the work.

* The draughts of these letters are in MSS, vol. 50, K; at the end of the book. F. B.

The next thing to be thought of was a place to fix in. Several were proposed: as Hyde Park and Chelsea College. I went to view the ruins of this latter, and judged it might serve the turn: and the better, because it was near the Court. Sir Jonas rather inclined to Hyde Park: but, Sir Christopher Wren mentioning Greenwich Hill, it was resolved on. The King allowed 500*l.* in money; with bricks from Tilbury Fort, where there was a spare stock; and some wood, iron and lead from a gatehouse demolished in the Tower; and encouraged us further with a promise of affording what more should be requisite *. In July following, I removed from his house, where I had been kindly entertained all this summer, to Greenwich, to have an eye upon the workmen. The foundation was laid August 10, 1675; and the work carried on so well that the roof was laid, and the building covered, by Christmas: of which I need give no description, because the figures of the part, the ichnography of the house, and several prospects of it, will inform the reader better than a long description †.

Whilst I lived at Sir Jonas Moore's, I contrived the large sextant of six feet nine inches radius: but, because the workmen might mistake the draught, Sir Jonas caused it to be formed in wood, with its axles and semicircles, by the Tower smiths at his own charge. The chief workman dying before the semicircles were formed, I could not make his successor to understand how the bigger of them might, and was to be moved by a perpetual screw: but forced to suffer him to move it by wheel work. But, before the lesser semicircle, whereby the plane of the sextant was to be inclined, was ready to be applied,

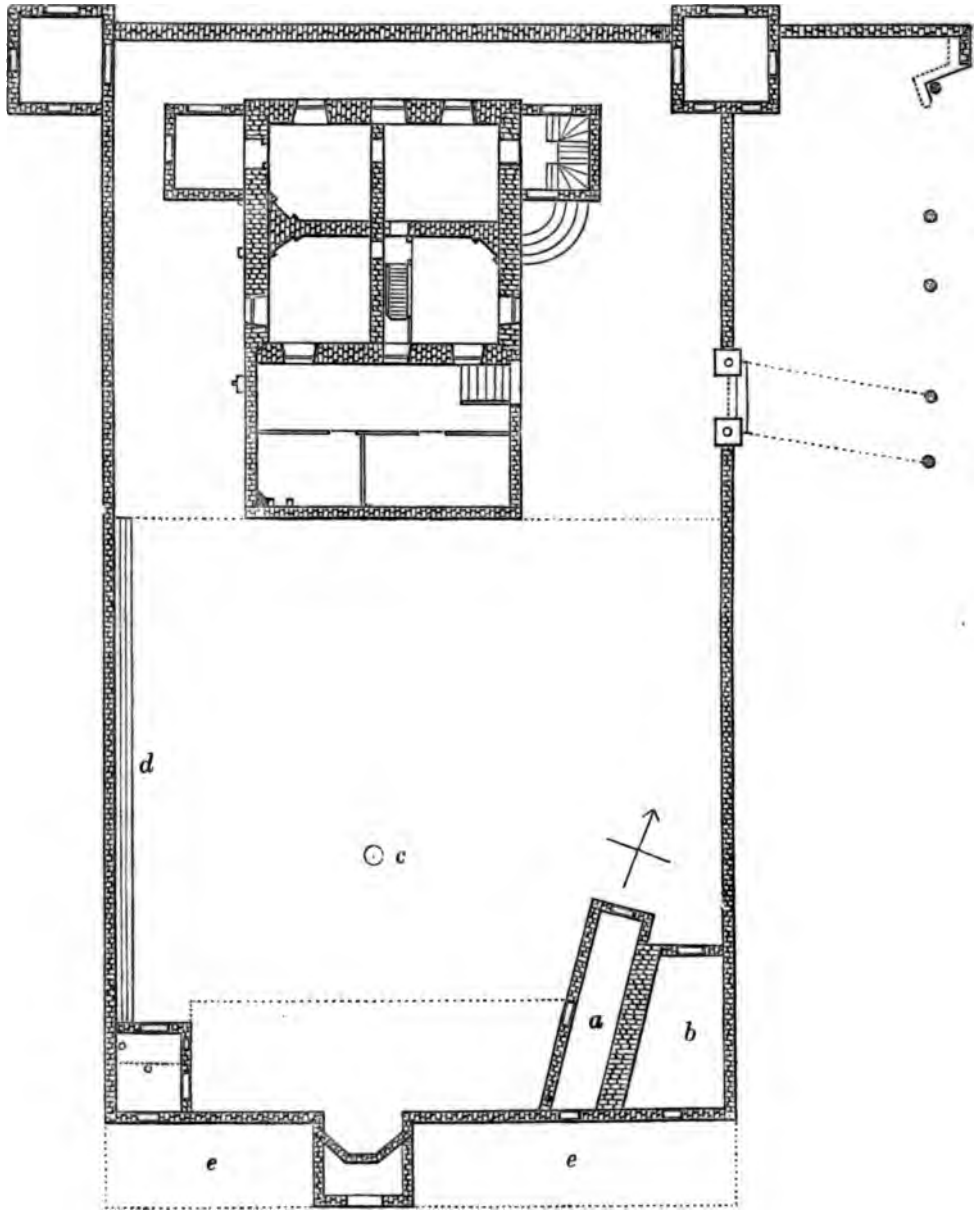
* A copy of the warrant for building the Observatory, dated June 22, 1675, is given in the Appendix No. 6. It appears from a copy of the Impress warrant in MSS, vol. 40, page 119, that the cost of the requisite alterations and repairs amounted to £520 9*s.* 1*d.* F. B.

† I find, by some MS notes in a copy of Dr. Maskelyne's observations, that this Observatory was formerly a tower built by Humphrey, Duke of Gloucester, and repaired or rebuilt by Henry VIII. in 1526. That it was sometimes the habitation of the younger branches of the royal family; sometimes the residence of a favourite mistress; sometimes a prison; and sometimes a place of defence.

Mary of York, fifth daughter of Edward IV, (betrothed to the King of Denmark,) died at the tower in Greenwich Park in 1482. Henry VIII. visited "a fayre lady whom he loved" here. In Queen Elizabeth's time it was called *Mirefleur*. In 1642, being then called Greenwich Castle, it was thought of so much consequence, as a place of strength, that immediate steps were ordered to be taken for securing it. After the Restoration, Charles II, in 1675, pulled down the old tower, and founded on its site the present Royal Observatory.

In the following page is given the ground plan of the buildings, &c, of the Observatory as drawn by Flamsteed himself in MSS, vol. 18, page 3: but it has been much enlarged since Flamsteed's time. F. B.

Ground Plan of the Royal Observatory at Greenwich.



a. The room for the mural arc.
b. The room for the sextant.
c. A perpendicular pole for the moveable telescopes.
d. The place for keeping the telescope tubes.
e. A flower garden.
f. The well, in which observations were sometimes made.

he understood me, and wrought it off so that it performed very well. So that I was the less concerned for his misapprehensions of my first design: since the wheel-work performed well enough, except in some particular cases, which could seldom happen, and might be remedied, when they did, by care and caution.

The frame of the sextant being finished, with the axis and semicircle, the brass limb was fitted on it, with the telescopes; and the limb screwed to carry the moveable index gently upon the limb, by Mr. Tompion. But this being the first instrument that was contrived of this bigness and manner, the cells for bearing the glasses and threads were not so neatly nor conveniently formed, as they have been since in some lesser instruments of the same sort that I have contrived for my friends and ingenious acquaintance.

The description and figure of the Sextant, to be inserted here *.

Mr. Hook pretended to have been the inventor of this way of screwing the limbs of instruments, which he published as his own in his animadversions on Hevelius's *Machina Cœlestis:* though 'tis evident, from the preface to Tycho Brahé's *Historia Cœlestis*, that the Emperor Ferdinand was the first contriver of it, and that Mr. Hook learnt it from the said preface. He contending then that the account by the revolves of the screw, and parts numbered, would give the arcs observed much more accurately than any diagonal divisions could do, Sir Jonas was persuaded by him to suffer no other divisions, but the revolves of the screw, to be inscribed and numbered on the limb. I was sensible that screws would wear, and that the weight of the index, as the plane of the instrument happened to be inclined, would alter the parts of the revolves considerably: therefore I inscribed them close to the edge of the limb, leaving room enough for diagonal divisions within, nearer the centre. And now our next business was to find how many revolves, and parts of the screw, would answer to any number of degrees, minutes, and seconds.

Close by the foot of the hill, on which the Observatory stands, there is a terrace, that had been found very plain and even when the walks were made. At the west end of this I caused a frame to be built, parallel to the plane of the walk, whereon to lay the sextant, with contrivances on it to fix the instrument very firm: from whose centre, to the other end of the walk, by the help of strong pikes I measured the distance 8762 inches precise. At this distance

* The description and figure of the sextant, here alluded to, are not given by Flamsteed in this MS; but in the next division of his history (see page 44) will be found some account of it. F. B.

I placed a long, strong, flat rail, well fixed and supported: and having computed how many feet answered to a degree, marked them off on this rule; which was made so long as to subtend an angle of five degrees. Then bringing the threads in the fixed telescope to the beginning of the divisions, and firming it close, I moved the screw; and carrying the moveable index along the limb, noted what revolves and parts were marked by it when the threads covered the divisions of the five next degrees. And repeating these trials oft, and carefully, made at last the table for turning the revolves and parts observed, into degrees, minutes, and seconds, which I made use of in all my observations of this, and the next following year *.

Whilst the Observatory was building and fitting up for my habitation, my quadrant and telescopes were kept at the Queen's house, where with them I observed her [the moon's] appulses to the planets and the fixed stars, as often as convenient opportunities offered themselves; which will be inserted after my observations made at Derby, in this preface †.

In the month of July or August this year, I also took the angles of the elevation of several parts of the observatory from two stations in Friar's road; whereby I found that the top of the rails was elevated about 182 feet above the level of the low water of the Thames ‡.

The sextant being no proper instrument for determining the distances of the stars from the vertex, and consequently from the visible poles, by reason that it could not easily be fixed in the plane of the meridian, I very much desired to have a Mural Arc, or semicircle, allowed me for that use. But, the sextant having cost more than was expected, in making and fitting up, Sir Jonas Moore feared lest I should so contrive it as to be no less chargeable. Mr. Hook persuaded him to leave the contrivance of a ten feet quadrant to him, upon his promising to frame it at a lesser charge. I was not to see it till finished: when

* I presume that this is the table in the first volume of the *Historia Cœlestis*, page 40. F. B.

† The observations made at the Queen's house are printed in the first volume of the *Historia Cœlestis*, pages 29, &c. F. B.

‡ This accords nearly with the value assumed by Capt. Foster in *Phil. Trans.* for 1826, page 22, as deduced from the Trigonometrical Survey: but differs materially from the results deduced by Capt. Lloyd from actual levelling, as given in the *Phil. Trans.* for 1831, page 134.

It may be here mentioned, in the order of dates, that Flamsteed was admitted a Fellow of the Royal Society on Feb. 8, 1676-7. In November, 1681, he was elected into the *Council;* of which he continued a member three years. He was again elected a member of the Council in November, 1698; in which he continued two years. F. B.

I found it such an one as was not likely to be of use to me. So that though at Mr. Hook's instance it was fixed up and rectified by me, and the brass studs on the limb had every five degrees marked out upon them, as it required, yet I could never attain to any certainty with it, by reason that the weight of a double index suffered not the arc of five degrees to remain fixed, whilst the other was moved upon it. Besides, being only a fourth part of a circle, had it performed as it ought, it could only serve to observe those stars that culminated on the south meridian: so that all that lie between the vertex and the pole could never be observed on it.

[Here ends the document, from which the above portion of the history is taken. F. B.]

Third Division.

FROM 1675 TO 1683.

HAD I been thoroughly furnished with instruments for this purpose [the Restitution and Rectification of the Earth's motion] when I first sat down to make celestial observations, the business I am now setting upon had been perfected long ere this time. When I came to inhabit here [Greenwich] I had only a sextant provided me, and two clocks. The sextant is of iron, the limb covered with brass about half an inch thick: its fiducial edge, by a peculiar contrivance, receives a male screw fixed on the end of the moveable index; which, by the help of a crown wheel and handle, is easily turned round, and thereby carries the index gently along the limb, and holds it immoveable in any place. The revolves of this screw are numbered on the face of the limb, and are readily turned into the degrees and parts of a circle by the help of a table I made for that purpose, by trial at land angles, before I mounted it on its axis and semicircles; which was in September, 1676 *. From that time till December, 1677, I had no other divisions on the limb: but then, having found, by repeated trials, that the screw wore, and was not on that account to be confided in, I took the sextant down, and divided the limb into degrees and minutes, after the method of Hevelius and Tycho; and comparing the revolves of the screw with the diagonal divisions, I made a new large table (the first in my book of numbers †) for correcting them. And always, when I measured a distance in the heavens, to the degrees and minutes numbered on the diagonals, I added the revolves of the screw and centesimal parts: whereby the small faults it committed were easily discovered and made apparent; and I became very confident and assured of the truth of my measures. For, if those two agreed, it was plain I had not erred in writing down my observation: if they differed, I rejected it, or noted it as dubious.

My pendulum clocks were the work of Mr. Tompion ‡: the pendulums,

* From the *Notes* in MSS, vol. 17, already mentioned in page 4, it appears that Flamsteed entered into the Observatory, to inhabit it, with T. Smith and C. Denton his servants, on July 10, 1676; and that on Sept. 19 he began to measure distances with the sextant. F. B.

† I have not been able to discover either this table or the book: but I presume the table is the same as that inserted at the end of the *first* volume of the *Historia Cœlestis*, page 390, &c. F. B.

‡ The wheel-works of these clocks appear (from a letter of Flamsteed to Sir Jonas Moore, dated Jan. 17, 1677-8, preserved in MSS, vol. 36) to have been exposed to dust and the open air; and not inclosed in a case, as at the present day. F. B.

13 feet long, make each single vibration in two seconds of time; and their weights need only to be drawn up once in twelve months*. For rectifying these, I had provided a quadrant of about 3 feet radius, which I brought with me from Derby. It was no very good contrivance; but with it, however, I could take the sun's or a star's height so exactly that the differences betwixt the errors of the clock, collected from 4, 5, and sometimes 6 several heights of the sun or a star, was scarcely more than 10″; but commonly much less. This quadrant I employed till June, 1678; when, Sir Jonas Moore having procured me a neater that belonged to the Royal Society, I divided and employed it till October, 1679, when the ill-nature of Mr. Hook forced it out of my hands†. Yet I lost nothing by it; for it was so ill contrived by him that I could not make it perform better than my first. And now he obliged me to think of fitting up one of my own, of 50 inches radius, wherein by peculiar contrivances I had avoided all the inconveniences I had met with in his. This gives an observed

* These instruments were *given* to him by Sir Jonas Moore, as will appear from the subsequent part of his history; and also from the following memorandum preserved in MSS, vol. 35, page 173, alluded to in page 5: viz.—"I came to Greenwich and laid the foundation of the Observatory "August 10, 1675. Was at great expense during the building. Entered it in July, 1676. Sir "Jonas Moore furnished me with sextant, the two great clocks, a telescope object glass of 52 feet, "and some books; which he gave me before good witness, to dispose as I thought fit: but with "some private intimations how he thought I might best dispose of them, in case of my removal or "decease; which I have regarded in my will. All my other instruments I made at my own charge." F. B.

† In another paper, entitled *The brief history of the Observatory*, alluded to in page 4, Flamsteed (who here writes in the third person) says, "He also borrowed a small quadrant of the Royal "Society, of Mr. Hook's contrivance, which lay useless by them; which Mr. F. fitted up, made "voluble, and divided (as he did all his other instruments) with his own hands. But, soon after "Sir Jonas Moore's death, Mr. Hook got this called for back. It was returned, and lies useless "ever since in the repository of the Royal Society." From an inspection of the Minutes of the *Council* of the Royal Society, I find that on January 25, 1676-7, the instruments were lent to the Royal Observatory; and that on September 22, 1679, they were ordered to be returned. The following entry, relative to this last step, is made by Flamsteed in his Observation-book, MSS, vol. 1, viz.—"Sept. 26, 1679, Sir Christopher Wren, Sir John Hoskins, and Mr. Hook came down to "Greenwich; and Mr. Hook produced an order to remove the instruments of the Royal Society to "Gresham College. They took away the small quadrant of 5 inches with the screw limb, another "quadrant with 2 telescopes on it, a dividing plate, and Mr. Hook's 3-foot quadrant." Flamsteed might well record (as he has done in another place) that "on the death of Sir Jonas Moore the "business of the Observatory languished for want of a good support from the Navy and Admiralty; "and had sunk, if the Good Providence of God had not blest the observator with a competent "estate of his own; and given him resolution to carry it on for the honor of the nation and their "Majesties, whom he has served without any respect to his own advantage." F. B.

height to half a minute: and now, by it, I am sure of the observed times to three seconds; which I could not have expected from either of my other instruments.

Besides these, I furnished myself with tubes and telescope glasses of 16 and 8 feet long (to which I applied my micrometers, and used them frequently in observing the moon's transits by, and over, fixed stars, before the sextant was fitted for use) at my own charge. But I design not here to give an account of all my instruments: that will require an entire work, and shall be done hereafter in a peculiar treatise, if God indulge us the great blessing of peace, and spare me life, health and leisure.

It was requisite that I should have first been furnished with one or more large wall quadrants, fixed in the meridian, for taking the meridional distances of the sun and stars from the vertex; which I was also promised. But, considering that the sextant would be the much more chargeable instrument, it was resolved that should be first begun: and, when it was finished, the workman demanding an unreasonable rate for his work, my friends were afraid I would contrive the quadrant as costly as the sextant. In the mean time Mr. Hook boasts how large and precise an instrument he would make at a small charge: whereupon Sir Jonas Moore allows him the contrivance of it. He ordered it to be made: but with so little circumspection, that the limb was two inches less than a quadrant; and the double indexes so weighty and unmanageable, that they could never be conveniently counterpoised. So that Sir Jonas was sensible of his fault; and that this was an instrument wholly useless; though Mr. Hook affirmed, with his usual confidence, it was well made. But I would not make it (what I could not) useful.

I was much concerned at this mishap, and endeavoured what I could to remedy it: but found all my pains bestowed in vain, by reason of the great weight of the double index, and ill contrivance of the limb. I complained often of my want of this instrument: but found still all my complaints were insignificant in those ticklish times; and therefore to remedy it began to think how I might obtain some meridional heights by the sextant: which I soon found a way how to fix in the meridian; and, by a particular contrivance, whether it altered its position whilst I moved the index from one star to take another. My first attempt, of this kind, with it was to get the greatest heights of some stars that passed near the vertex, and thereby to find the error of the instrument: my next, to find the greatest and least heights of the pole star, which I did. I happily gained both in December, 1676; and the year following

at the same time. Whence I concluded the simple latitude of the observatory 51° 28′ 50″ ; but correct by refraction 51° 28′ 10″.

But, finding it very difficult to keep the same point of the limb always perpendicular, by reason that, the index being removed, its weight alters ; and though the sextant being strong and weighty was formed as well as possibly I could, yet it would give way and yield some very small but sensible part, so as to make sometimes above half a minute difference in the height of the same star, taken two several nights, but immediately succeeding one another,—I grew weary of this work. And though I made some number of observations of meridional heights, I inserted none of them into my catalogues*. I knew they ought to be better determined : and I hate to recommend anything to the public, of which I am not very certain. Coarse observations, made by honest well-meaning men, have more perplexed the astronomer than all their labors and dreams upon them can make him satisfaction for. Their pretty thoughts and conceits in the theories are always excusable and sometimes to be commended: but when rude and ill-managed observations and experiments are brought to confirm them, though they may serve the author's present turn, yet they become a load on the science, and at last turn to his shame and reproach : of which we have two great instances in Lansberg and Riccioli ; not to mention others of our neighbours who have seen, and I hope are ashamed of, their faults of this kind.

I conceived nevertheless that I might correct the earth's motion (without the true knowledge either of the obliquity of the ecliptic, or the exact places of the fixed stars) from such observations as I could make with the sextant alone. By which, if the sun's distances were taken from the planet Jupiter or Venus by day, and either of theirs from the stars by night, his longitude from those stars would be given : and that too perhaps more precisely than could be obtained from his meridional heights ; in which an error of 24″ would cause a minute's error in longitude : and I cannot conceive but that his distances from a fixed star might, by this means, be determined to half a minute's exactness. 'Tis true the distances themselves are vitiated by refraction, as well as the meridional heights : but I find that a small error in the refraction of height has little influence on the distance, in which, as I ordered my observations, a small fault would be wholly imperceptible. For I ever took care to have the sun and

* The MS register of these observations will be found in MSS, vol. 1 : and a synopsis of them in vol. 11, page 37. F. B.

Venus in equal altitudes, when I measured their distances: and the like afterwards, when her distance was to be taken from a fixed star, if possible; otherwise I observed her when she was as high as I could possibly have her at that season. If she was in equal height, it will be found that a degree difference of her height altered not the contraction sensibly: nor will it be perceptibly less or greater if the refractions be supposed one or two minutes more or less than the table makes them. Whence I concluded that I could not pitch upon any better method than this, for finding the true place of the earth *inter fixas*, and the inequalities of her motion and place of her aphelion: especially considering that although the refractions of altitude are the greatest and most unequal near the horizon, the contraction of the distance of any two stars, in equal height, is then the least. But, if one of them be near the zenith and they are almost vertical to each other, then the contraction is nearly equal to the difference of their refractions: which being small is easily allowed for, as I shall show more fully hereafter.

The year 1678 afforded me many convenient opportunities for observations of this sort: and though I was often ill*, yet I made good use of them, and gained such a number both of the sun's distances from Venus, hers from fixed stars, and their meridional distances from the vertex, that I thought myself sufficiently furnished for an attempt to restore the earth's motion†. The tables I had made from Tycho's and Cassini's observations gave the greatest equations of her orbit 8 minutes less than Kepler's and his numbers made them. This trial I knew would determine the controversy: and therefore though I resolved to make use of future opportunities to state the earth's motion better, yet I was

* At this early period of Flamsteed's labours, he appears to have been much troubled with that state of constitutional sickness and distempers which afflicted him during the whole of his life: for we find him thus writing to Sir Jonas Moore, in a letter dated October 5, 1677, preserved in MSS, vol. 36; viz.—"My distempers stick so close I cannot, by any means I have hitherto used, remove "them: nevertheless I had observed the occultations and appulses of late, but that clouds and ill "weather bereaved me of them. My headache is the more troublesome pain; but I more fear a "small but constant cholic in my bowels." Again, in another letter dated March 5, 1677-8, preserved also in the same volume of MSS, he speaks of his being so constantly ill for several days that he could not even examine the clocks. F. B.

† Whilst Flamsteed was carrying on these early observations at Greenwich, he corresponded frequently with his friend and patron, Sir Jonas Moore; giving him an account, from time to time, of his proceedings. Twenty of these letters have been preserved, and are bound up in MSS, vol. 36: three of which (namely, those of March 7, 1677-8, April 30, 1678, and July 16, 1678) are inserted in the Appendix, Nos. 7, 8, 9, for the elucidation of this portion of his history. F. B.

resolved, for my own satisfaction, to begin now with what stock I had; and accordingly set upon it in the spring of 1679. The place of some one fixed star was requisite for this purpose. I took the longitude of the bright star of *Aries* from Tycho's catalogue, and reduced it to that year by allowing 50″ for the annual precession *: but its latitude I corrected by my own meridional height, taken with the sextant. The places of such other stars as I had occasion to make use of, I corrected by their observed distances from this star, and each other, and their meridional distances from the vertex. I knew that Tycho had not erred much in determining the place of this star, which is the ground-work of his catalogue: and that, if he had erred more than we can probably suppose him, the true longitude of the earth's aphelion from the first star of *Aries* would nevertheless be exactly got, together with the greatest equation of the orbit. For a small error in the latitudes of the stars, or Venus, could not make any sensible one in calculating the longitudes of the sun from them: the only fault would be in the equinoxes, where the error in the sun's true place (being equal to the error in the supposed place of the star) would become more evident †.

* See his computations relative to this subject in MSS, vol. 41, page 9 (reckoned from the *end* of the volume): and which, in fact, form the ground-work of the paper from which this division of the history is taken. F. B.

† It was about this period that Flamsteed, finding his expenses too great for his income, was reduced to the necessity of entering on the laborious task of a *teacher*. For we find the following passage in a paper of memoranda, mentioned in page 5, and preserved in MSS, vol. 35, page 171: viz.—" Hired an ingenious youth to attend and assist me in my work: but finding I still wanted " help, and my salary not being sufficient for my expense in so public a place, took young gentlemen " to teach; who supplied my want of assistance, and a competent allowance." Again in another paper of memoranda preserved in the same volume, page 173, he says—" In the mean time, wanting " assistance, I took an ingenious young man to be my servant, and some young gentlemen to teach. " I employed them in my night observations, to tell the clock, write for me, and such like things as " I might safely trust them in. Which saved the public the charge of a pair of necessary assistants; " and helped to bear the further unavoidable charge of an expensive habitation." At the end of MSS, vol. 15, there is given a paper in Flamsteed's handwriting, entitled " A list of my pupils' " names and employments, as far as my memory will serve me," containing the names of about 140 persons, extending from the year 1676 to 1709. Amongst them, we find the names and relations of several of the nobility, (such as the Dukes of Marlborough and Hamilton, the Earls of Essex, Lichfield, &c, Lords Castleton, Coningsby, Dartmouth, Ferrers, Guildford, &c,) besides Sir Wm. Hussey, the Ambassador in Turkey, Sir Wm. Caley, and a number of captains of vessels, and persons destined for the East India service. I apprehend that the grandson of Sir Jonas Moore was also one of his pupils; but his name is not in the list above-mentioned. There is however the following entry:—" May 13, 1692, J. Flamsteed *cognatus meus;*" which I consider of sufficient importance to record in this place, for a reason which is mentioned in the Preface. F. B.

This I foresaw might be remedied afterwards: for, by the sun's meridional distances from the vertex, and observed near either equinox, two times might be found when his declinations from it were the same. And, by the tables, the arc the earth had moved over in the mean time might be found; half whose excess above a semicircle (if it were more than one) or half the defect (if less) would be his true distance from either equinoctial point at those times. Whence the true places of the fixed stars, and the sun's longitude then from either equinoctial point would be given. And though the sextant was not so steady an instrument as is requisite for this purpose, yet it was much better to depend upon it, than have conjectures till such time as I could procure a fixed wall quadrant for this purpose: which I resolved to have, if not at my master's, yet at my own expense, as soon as I could.

The tables were finished that spring; and the autumn following Sir Jonas Moore died, on the 27th of August [1679], at Godalming in Surrey, in his return from Portsmouth; with whom, in a manner, fell all my hopes of having any allowance of expenses for making such instruments as I still wanted *. He left a book in the press, which he designed for the use of the King's boys at Christchurch Hospital, which stuck without any progress till one of his sons returned from Ireland; who put it forward again, and agreed with their master, Mr. Perkins, and myself to see it finished. Mr. Perkins wrote the *Navigation;* myself, the *Doctrine of the Sphere,* to which I added new tables necessary for calculating the places of the luminaries and eclipses. The solar are those very numbers I had last made: the lunar are a correction of them I had formerly published with Mr. Horrox's works †. This labor being over in the

* Soon after this event Flamsteed addressed a letter to Dr. Seth Ward, then Bishop of Salisbury, dated Jan. 31, 1679-80, relative to his astronomical pursuits and prospects, which is inserted in the Appendix No. 10. F. B.

† In order to preserve a continuity in the narrative of events, I would here remark that in another paper written in the third person, but with Flamsteed's own hand, entitled *The brief History of the Observatory,* and alluded to in page 4, he says, "In the latter end of the year 1680 and beginning of 1681, the great comet appeared, and was diligently observed by him. Before it had fully ceased to be seen, he drew all its places observed by him into a little table; which, with his thoughts concerning it, he imparted to a friend in Cambridge in a long letter, wherein he showed that the comets of November and December were probably one and the same. This being shown to Sir Isaac Newton, then only Lucasian Professor of Mathematics there, he wrote a letter to Mr. Flamsteed, wherein he used some arguments to prove they must be two different ones, and moreover magisterially ridiculed the contrary opinion, for which Mr. Flamsteed thought the arguments convincing and unanswerable." Again, in another paper, preserved in MSS, vol. 35, page 173, also alluded to in page 5, Flamsteed says, "In November and December 1680, a comet appeared.

spring of the year 1681, I was at leisure to prosecute the observations, I was employed upon for rectifying the places of the fixed stars, and planets' motions, more vigorously: though in the mean time I had been always mindful of them, especially such as might serve either to correct or confirm my new solar tables.

But, finding that it was impossible to determine the true longitude of the equinoctial points, from the fixed stars, without a fixed instrument for determining their distances from the pole of the world, and that I could not probably be allowed the expenses necessary for one, by the King, at this time, I resolved to make a large mural arc at my own charge. The narrowness of my salary would not afford me to bestow much money on one: I began therefore to think how I might make it as little chargeable as could be. At last I resolved to make it near the same radius with the sextant, that it might show the meridional distances as exactly as that measures intervals in the heavens. I began it in August 1681; and the instrument itself was finished that year. But being forced to make use of an ill workman, who respected nothing but the getting of wages by his work, I found the limb faulty, and was discouraged from proceeding with it that year. The next, it lay by me also: but, the following, I

" I wrote a letter to Mr. Crompton, Fellow of Jesus College, Cambridge, concerning it: wherein I " affirmed the *two* comets (as they were commonly thought) to be *one* and the same; and described " the line of their motions, before and after it passed the sun. This was imparted to Mr. Newton, " who in a long letter to Mr. Crompton *for me*, argued strongly that they must be *different* comets. " But [afterwards], in one to me, Sept. 19, 1685, yielded that it was probable they were both the " same. I imparted the place of the comet, deduced from my observation, by repeated calculations; " and he published them in the *Principia*, 1687: but with slight acknowledgments of so laborious " a work." Two of Newton's letters on this subject have been long before the public; having been printed in the *General Dictionary* (mentioned in page 3) under the life of Newton. One of them is addressed to " Mr. Crompton to be sent to Mr. Flamsteed," and dated Feb. 28, 1680-81; the other is addressed to Mr. Flamsteed himself, and dated April 16, 1681: but in neither of them can I find any foundation for Flamsteed's censure. Newton however afterwards altered his opinion on the subject of this comet as above stated: and in his *Principia* (page 494 of the first edition, 1687) acknowledged that Flamsteed was right. The reader will find a correspondence on this and other astronomical subjects, in the *General Dictionary* above mentioned, between Newton and Flamsteed, during the years 1685 and 1686: wherein the former acknowledges his obligations to Flamsteed for his assistance in these inquiries. The dates of these letters are, besides the two above alluded to, as follow, viz.

Sept. 19, 1685,	Oct. 14, 1685,	Jan. (?), 1686,
Sept. 25, 1685,	Dec. 30, 1685, (?)	Sept. 3, 1686.

These letters are interesting, as showing us the state of the science at that period; and the good understanding that *then* existed between Newton and Flamsteed. F. B.

brought it very near the meridian, fixed it there, and divided the limb beyond the pole*. For 'tis an arc of more than 140 degrees; and I order it so on purpose that I might observe all the stars, visible in our horizon, on it with the same index. So that if (as it has happened) it should be stirred from its first position after the rectification, though it did not show the meridional heights exactly, yet it might give the precise apparent distance of any star, observed on it, from the pole. Which I find it does as well as I could expect; considering how bad a workman wrought it, and how inconvenient it was to divide it. And I am so well pleased with this part of the contrivance, that were I to make another to be placed in its room, and had all necessary expenses (which I account could not exceed £100) allowed me, I should order the limb to contain as many degrees as this does.

The fault in this instrument is, that in many places of the limb the index applies not closely to it, by reason that it warped either when first hung upon the wall, or when I forced it into the meridian. Nevertheless I conceive this fault cannot create any perceptible error in an observation; especially if due care be used in considering and copying the measure.

Tycho Brahe's declinations of the fixed stars are frequently and deservedly found fault with by Hevelius: and that his own instrument was none of the best, but that it gave him the height of the sun and stars often faulty, will be evident to any one that duly considers them. For, the length of the year from equinox to equinox ought to be always $365^{d.}\ 5^{h.}\ 49^{m.}$: but his observations differ egregiously.

I do not design this for young artists or students, but for those who are conversant with the heavens, who very well know the truth of this assertion. Nor do I design what I write here for the public. My intent is in this book to enter all the calculations I make for the restitution of the places of the fixed stars and the sun's motion, in order, as they pass under my hands; with such deductions as by the way I shall make from them. Which when I have finished, if God spare me life and health, I shall put into a proper order and method †. If otherwise, those who come after me will find how far I have proceeded, and what remains afterwards to be done. May the great Author of the Universe, the all-

* The reader will bear in mind that this is *not* the mural arc that was made by Mr. Abraham Sharp; and with which all the astronomical observations recorded in the second volume of the *Historia Cœlestis* were made: but another, made *prior* thereto. F. B.

† See a letter dated July 12, 1682, addressed to Ed. Sherburne, Esq, relative to his astronomical pursuits, in the Appendix No. 13. F. B.

wise Disposer of the Heavenly Bodies, assist me in this undertaking. May He grant me health and leisure to accomplish it, and render my ideas of His works agreeable to the Prototype; that mankind may have the use, and He the glory of my labors.

[Here ends the historical part of this narrative. The remainder of the MS is taken up in applying some of his observations, then recently made, to the purposes alluded to in his paper. But as this has been since more particularly explained and exemplified by better observations in the Prolegomena to the 3rd volume of the *Historia Cœlestis*, page 134, &c, it is unnecessary to dwell further on it here. F. B.]

Fourth Division.

FROM 1683 TO 1690.

FINDING the distances of the fixed stars from the vertex, observed with the sextant, so uncertain and incoherent, in the year 1683 I contrived and built a mural arc of so many degrees that it might take in all the stars that passed the meridian betwixt the pole and the south intersection of the meridian and horizon; as also the pole star itself under the pole (that is, of about 130 degrees in the limb), and fixed it on a meridional wall. With this I took the distances of the sun and planets from the vertex, from that time till the autumn of the year 1686*. It was built too slight, and could not be well fixed: so I durst not confide in the measures taken with it. But, however, the year following I determined the place of the sun near his mean distances from the earth, and the greatest equation of his orbit, from observations made with it, compared with others made with the sextant; confiding in the intermutual distances taken with it, though not in the meridional distances from the vertex.

My good friend Sir Jonas Moore died in August [27th] 1679: the King in February 1684†. I had now no hopes of having any allowances made me for new instruments: it was well if I could keep my post, and proceed in the following reign. Some people, to make me uneasy, others out of a sincere desire to see the happy progress of my studies, not understanding amid what hard circumstances I lived, called hard upon me to print my observations. I had often answered that I had not any instrument for taking the meridional distances of the stars from the vertex, with such exactness as I could their intermutual distances from each other; that I wanted these to connect them; and that without them my labors would appear lame and imperfect. At the same time desiring

* This is the *first* mural arc, already alluded to in page 51. F. B.

† It would appear that, on the death of Sir Jonas Moore, Flamsteed had a dispute with his son relative to the instruments which had been given to him. For in a paper entitled *Short note of dates for my Works*, mentioned in page 5, Flamsteed says, under the date of 1681, "Difference "with young Sir J. M. Proved the instruments to be my own." And in another MS paper, also in Flamsteed's own handwriting (but written in the third person), entitled *The brief History of the Observatory*, and mentioned in page 4, Flamsteed writes thus: "In the mean time, Sir Jonas Moore "caused a sextant to be made at the Tower, of seven foot radius, after his directions, with two large "pendulum clocks: which with some books he gave to Mr. Flamsteed; as was proved at the Board "of Ordnance, by substantial evidence, upon a controversy with his son about them, after his father's "decease." See also the note in page 45. F. B.

them to have patience till I could afford to build such an instrument as I wanted, at my own charge; which I was resolved to do, if I could not get one allowed me by the King. In the mean time, considering that the distances I had taken, though not so exact as I desired, were much better than Tycho's; and finding the want of a better catalogue of the fixed stars than his, I set myself to calculate a small one of those most useful to me, from my own observations made with the sextant, and this slight arc; and perfected it in the spring of the next year, 1687*: having in the mean time been frequently interrupted not only by other employments, but particular affairs of my own.

And now my good aged father dying †, and the difficulties under which men of my profession then laboured increasing, to avoid being taken notice of, I resolved to employ some part of my estate (which was increased by what he left me) in building such a strong mural arc as I had long before designed. I acquainted the then Master of the Ordnance, the Lord Dartmouth, with my intent; who kindly assured me that what I laid out on this occasion should be repaid me by the Office. Hereupon I caused the frame of my arc to be made of the same radius with the former, but much stronger. My then servant Stafford was taking care of the work: but he dying in May 1688, and A. Sharp, who had served me six months, four years before, being then at liberty, I hired him. He was not only an excellent geometrician and ready calculator, but (which I no less valued him for, at this time) a most expert and curious mechanic. In the following autumn he set to work on the arc, screwed the edge of the limb, prepared the index, and having fastened up and fixed the arc on the wall, I caused him to plane it anew by a peculiar contrivance that rendered it as true and flat as if it had been turned in a lathe. This cost us three months' labor: afterwards it was rectified, divided, and engraved by his hand, so curiously as I cannot think could have been done by any, less skilful and expert than himself; but was not completely finished till the month of October 1689: having now employed us above 14 months, and cost me more than £100 out of my own pocket.

But, now comparing some observations I made with it, when new rectified, with others taken in the month of November 1689, I found it changed from its first position, and the distances of the stars from the vertex in the southern quadrant of the meridian, 1′ 0″, or 1′ 5″ bigger than they ought to be. This, I thought, at first, proceeded from some small strain it might have received whilst

* Probably the small catalogue of about 130 stars, reduced to the year 1686, which is inserted in MSS, vol. 25; or in vol. 26 D, page 52; or in vol. 30, page 59. F. B.

† See *Historia Cœlestis*, vol. 1, page 345. F. B.

we were working it after the rectification: but, since, I find it proceeded from the sinking of the wall; though I then suspected it not.

In November, at the full moon, I took the distance of her limb, both before she passed over the meridian and after, from some fixed stars (the star of *Aries*), the times of whose transits and distances from the vertex I observed with hers: and, calculating her places from the several and different observations, found them so very nearly the same, that I concluded the different ways of observing were equally good; but that, by the transits, the rather to be preferred, because the easier, safer for my health, and the observations themselves the easier to manage. The same trial I made afterwards; comparing the moon with the stars of the *Pleiades*, and the other planets with other stars: whereby I was fully assured of what I had before determined, that the two different ways of observing were equally good; but that the mural arc, for the reasons before alleged, preferable.

And now I saw that the observations, made with this instrument, would have many advantages above Tycho Brahe's made with plain sights. The collimation through the telescopical index was perfect, easy, and accurate; and therefore the observed distances from the vertex exact: which his, certainly in the moon, were not. The observed differences of the times of the transits of any two observable points would give the difference of their right ascensions with much more certainty than his large Armillas, and more accurately; because, here, no bending or inclination of the sights, or misplacing of them, was to be feared. I seldom needed more than one assistant, to count the clock and write for me: whereas, when I measured distances with the sextant, two were scarce sufficient. In fine, I found that whatever I sought with the sextant might, with less labor and more accuracy, be obtained with this instrument and the clock alone: and therefore, from the time I got it finished till this present, I have seldom used any other: and now I have as many ways of proving the truth of a determined place of a star as Tycho had. One good instrument is of as much worth as a hundred indifferent ones. My arc has two sorts of divisions: one on its limb, as the sextant has, by which I number, as on it, the measured distances from the vertex. If any error be committed in numbering the degrees, minutes, and seconds, it is easily found by examining (by a table made for the purpose, in my black book of tables *) whether the revolves and parts, noted at the same place, give the same or not. If they do, the annotation is indubitable; and it

* Probably the table inserted at the end of MSS, vol. 46. But the correct tables are given at the end of the *second* volume of the *Historia Cœlestis*. F. B.

could be no more, were the observation made at the same time with different instruments. If the difference of the right ascensions betwixt any two stars, found by the clock, be questioned, it may be examined by the given distance of both from the pole, and their intermutual distances taken with the sextant: of which a large store will be found in my papers and books that contain them; wherein I have always kept them transcribed fair, in such order as I design to publish them if God spare me health and time.

One particular advantage I soon found by it: that whereas, when I observed in the open air with the sextant, I could not see some small stars of the 6th light, that lay near the great ones, by reason the greater light of all the circumambient stars, entering mine eye with theirs, rendered their appearances confused; now, through a slit only 1½ foot wide, that, being fenced off, I could see stars of the 7th light plainly with my naked eye, and observe them too with very little labor, and no waste of time *. These therefore, whenever I saw them within the space of the zodiac, I have noted: nor have I omitted them elsewhere, when they might be taken without omitting others more remarkable.

What care I took to observe the moon, ever since the arc was finished, and with what success, will appear from the large synopses of her places deduced from my observations, and imparted to Mr. Newton. What I have done in the other planets may most properly be said in another place †.

When the pole-star became next again observable, both evening and morning, in December 1690, its distances from the vertex decreased, and of the stars that passed the meridian in the south, as much increased: which I could attribute to nothing but the sinking of the northern end of the wall.

* Another important personal advantage is also mentioned by Flamsteed in another paper, entitled *The brief History of the Observatory*, alluded to in page 4; when he says (page 164) that "Whenever he observed with the sextant, he caught cold; which caused him frequent tortures of the "stone and gravel: but now he found he could employ his time with less danger to his health." F. B.

† In the paper, alluded to in the preceding note, Flamsteed (page 165) writes thus: "The "observations with the mural arc were begun on Sept. 12, 1689: but all things were not got into "good order till the middle of November following: when, every day after that the moon had "been observed, he calculated her place from the observations and from his own tables; having "hired servants that were skilful and proper for his purpose. And, by this means he had got above "150 places of the moon compared with his tables, in 3 large synopses; and knew where the errors "were greatest, and in some measure knew how to remedy them. In the mean time all the other "planets were observed, as they came conveniently over the plane of his arc. But his chief care "was to get a stock of observations of the fixed stars; whereby he might determine their distances "from the pole." F. B.

In the mean time, this year I often observed the times when the sun had the same heights before and after noon, together with his transits over the meridional thread in the telescope index of my arc: whereby I found that he always passed the thread, sooner than he crossed the true meridian in the heavens, by 38″ on the tropic; and in other places according as may be seen in the table of them on the *first* page of this tract*; which is always to be made use of when the difference of the right ascensions of two stars that have different heights on the meridian, is inquired from the difference of the times of their transits.

In January last (1694†), collecting all my observations of the pole-star into one synopsis, as is to be seen in the first page of this year's observations‡, I was surprised to find that its distances from the pole were always bigger in March, April, July, August and September, than in December. But, considering with myself that this must necessarily so happen, if the parallax of the earth's orb were sensible in the pole-star, I concluded it to proceed from thence. The light of the star is brisk, which argues it nearer than some that are more languid than bigger. Allowing this parallax to be about half a minute, and the instrument to subside equally, the observations will agree as exactly as can be expected §.

I doubt not but that a sensible parallax of the earth's orb will be found in my observations of other stars that lie remote from the ecliptic. At present I waive the examination of them; but shall consider them when I come to determine their true places. This is the first time, I am apt to think, that any real parallax hath been observed in the fixed stars.

But, I intend not to write the history of my observations here: only thus much I have premised for the better understanding of such corrections as I have used in the following applications of them.

* It is, in fact, on the *eighth* page; which is the *first* that is written on. The table corresponds very nearly with the particulars detailed by Flamsteed in the *Prolegomena* in the third volume of the *Historia Cœlestis*, pages 134, 135, 140, and 141. The table itself will be given in the sequel, in the *Introduction to the British Catalogue*. F. B.

† The reader will recollect that this MS was written by Flamsteed in June 1695. F. B.

‡ I have not yet been able to discover this synopsis. F. B.

§ This is the embryo of the celebrated letter, dated Decem. 20, 1698, that Flamsteed wrote to Dr. Wallis, under the title of *De Parallaxi Orbis annuæ telluris observata*, inserted in 2nd volume of his *History of Algebra*, page 701: which led to an angry correspondence respecting Dr. Gregory; and a most remarkable letter from Newton to Flamsteed, dated Jan. 6, 1698-9, inserted in the Appendix, No. 43. In order more fully to comprehend the nature of Newton's caustic remarks, it is necessary to read the several letters in the Appendix, No. 36—46. F. B.

In the preceding tract, whilst I was inquiring the inequalities of the earth's orbit, and the place of the aphelion, I have, by the way, determined the right ascensions of about 40 fixed stars. I shall now inquire their distances from the visible pole; and then proceed from these to state the places of others, in their proper order.

[Here ends the document from which the above portion of Flamsteed's history is taken. F. B.]

Fifth Division.

FROM 1690 TO 1704.

1676. SIR JONAS MOORE gave me the sextant, some books and glasses; with charge to dispose of them by my will. All the other instruments, and tubes, provided at my own expense.

1680. Made the voluble quadrant at my own charge. Decem. 12, first saw and observed the great comet: observed it till Feb. 5. 1680-81.

1681. Imparted my observations of the comet, with the line of its way derived from them [to Sir Isaac Newton, as a friend *].

1685 or 1686. Gave him the diameters of the planets in all positions of the earth, and then in their orbits. Got it back again, with much difficulty, after 2 years' detention. He disputed against the comets of November and December being the same, in 2 long letters in February and March 1681: now (in 1685) he owned they might be so as I had asserted; and slightly mentioned me as *disputing* for their being the same, in the 4th book of his *Principia*. Whereas I *affirmed* it; and himself *disputed* against it †.

1687. His *Principia* published. Little notice taken of Her Majesty's observatory ‡.

1688. [Built a strong mural arc, which I hitherto wanted, at about £120 expense (on account of board and hire for my servant, that was 1¼ year in making it), with which I began to observe about Michaelmas 1689. Whereby I have calculated and formed a catalogue of the fixed stars, containing the places of 3000 of them, with all the requisites relating to them. And together with them have got about 1000 places of the moon, and 1000 places of the planets, deduced from them and the observations made with the sextant §.]

1689. Began my observations of the stars' distances from our vertex with it, Wednesday September 12th, and Thursday September 13th. Got the clock removed by Friday November 15th.

* This part within brackets is taken from page 155 of the paper entitled *Short note of dates for my works*, alluded to in page 5. F. B.

† See the note in page 50. F. B.

‡ In the paper alluded to in the last note but one, Flamsteed says—"1687. Sir Isaac Newton's "*Principia* published; with very slight acknowledgments of what he had received from the observa- "tory." F. B.

§ This paragraph also is extracted from the paper alluded to in the preceding note. The mural arc, here alluded to, is the one commenced by Mr. Stafford, and completed by Mr. A. Sharp. F. B.

1689, December 10th. First observation of the moon's place compared with my lunar tables, in the 4th book of Calculations, page 5 *. After this, observed the moon and planets frequently with the new arc: examined the lunar observations commonly the morning after they were got; and compared them with my tables, till April 1692: whereby I saw the faults of the tables were near one-third of a degree †.

1694, Saturday September 1st. Mr. Newton came to visit me ‡. Esteeming him an obliged friend, I showed him about 150 places of the moon, derived from my observations and tables by myself, and servants hired at my own expense; with the differences or errors, in three synopses written on large sheets of paper, in order to correct the theory of her motions. On his earnest request I lent them to him, and allowed him to take copies of them (as I did not doubt but that by their help he would be able to correct the lunar theory), upon these two conditions however: 1°. That he should not impart or communicate them to anybody without my consent: for the places of the moon deduced from the observations (I told him) were got with the help of a small catalogue of fixed stars made from observations taken with the sextant only, and rectified to the beginning of the year 1686 §; whereby I found their places were not so correct as they ought to be; and that when the stars were rectified by the new instru-

* This book is MSS, vol. 54: in which the computations are still extant. F. B.

† I would here state, in order to preserve a connexion in the order of events, that it was about this time that Newton suggested to him by letter the expediency of publishing a catalogue of stars; which produced a spirited and strong reply from Flamsteed, explanatory of his reasons for not doing so. These letters are inserted in the Appendix, No. 14 and 15.

I would also, for the same reason, here remark that Flamsteed was married in October 23, 1692. The fact is recorded by him in his Observation Book (MSS, vol. 5, page 209) in the following words, viz.: "Octob. 23. ☉. hora 9 mat: Uxorem duxi Margaritam filiam Radulphi Cooke, in "ecclesia Sti Laurentii Londini: officia recitante Dr Mapletoft." F. B.

‡ This visit of Newton to the Royal Observatory, and his consequent correspondence, are important events in elucidating the subject-matter of the subsequent history: and were evidently so considered by Flamsteed, as he has left behind him no less than three statements of the circumstances, besides other scattered allusions; all agreeing in the principal points, but varying in a few trifling particulars. I have in the text brought the whole into one connected narrative. The documents from which the several parts are taken, besides the principal account derived from the source mentioned in page 3, are—1. A paper entitled *Excerpts from Mr. Newton's Letters.* On the back of this paper there is written, "'Tis as impossible for Mr. N. to hide what he has received from the Observatory, "as to cover St. Paul's with a Scotch bonnet." 2. A paper entitled *Short note of dates for my Works;* and 3. A paper entitled *The brief History of the Observatory,* all mentioned in page 5. F. B.

§ Probably the catalogue of about 130 stars, alluded to in page 55. F. B.

ment, I would calculate the moon's places anew, and then should be ready to impart them both to him and to the public. 2°. That he should not in the first instance impart the result of what he derived from them to anybody but myself: for, since I saved him all the labor of calculating the moon's place both from the observations and tables, it was not just that he should give the result of my pains (the correction of the theory I had furnished with numbers) to any other but myself. All this he approved; and by a letter of his dated confessed *. Nevertheless he imparted what he derived from them, both to Dr. Gregory and Mr. Halley, *contra datam fidem*. The first of these conditions I was not much concerned whether he kept or not: but he has, I believe, kept it. The latter (which was the most material) he has forgot or broke; through the insinuation, I fear, of some persons that were little his friends till they saw what friends he had in the Government; and I presume will be less so, when they see them laid aside.

I had thus hoped to have gained leisure to begin my Catalogue of the fixed stars; for which I was now furnished with a stock of observations, sufficient for a beginning. In order to which in the following year I made new tables for finding the sun's true place. But I found myself soon deceived: for instead of saving me labor, this brought more upon me. Mr. Newton frequently called upon me for new observations of the moon: whilst some of his creatures in town cried up his success in correcting the lunar theory; but said not one word of his obligations or debt to the Royal Observatory. And one of them publicly gave out that all my pains would be well employed to serve him. When I demanded therefore the performance of his promise, I was put off with excuses and delays, and sometimes even with injuries. Nevertheless I continued to supply his demands, as my other employment of observing (that I might enlarge my stock for carrying on my catalogue) would permit: for, at the same time, I had the restitution of the sun's motions, besides my night-work, on my hands.

This request of Mr. Newton for more observations, caused an intercourse of letters between us, wherein I imparted to him about 100 more of the moon's

* The date is left blank in the MS, but the acknowledgment is manifest from the tenor of several of Newton's letters at that period inserted in the Appendix, as hereafter mentioned. Amongst some memoranda which Flamsteed has written at the end of a book of calculations (MSS, vol. 62 E), he states, " All I impart to him [Mr. I. N.] is *under his hand* that he shall not communicate it to " any without my leave till I print it. I study not for present applause. Mr. N.'s approbation is " more to me than the cry of all the ignorant in the world." This passage is a strong proof of the feeling and regard which he had towards Newton at that period. F. B.

places; which was more than he could reasonably expect from one in my circumstances of constant business and ill health. The year following (1695) I was ill all the year with a periodical head-ache: which was carried off in September by a violent fit of my dreadful distemper, the stone. In the mean time, frequent letters passed between me and Mr. Newton *, who ceased not to importune me (though he was informed of my illness) for more observations; and with that earnestness that looked as if he thought he had a right to command them; and had about 50 more imparted to him. But I did not think myself obliged to employ my pains to serve a person that was so inconsiderate as to presume he had a right to that which was only a courtesy. And I therefore went on with my business of the fixed stars; leaving Mr. Newton to examine the lunar observations over again: which had he done, he had found that he needed not be so importunate for new,—the old would have been sufficient for the purpose and design for which I had imparted them to him. I was therefore forced to leave off my correspondence with him at that time; having found that his correction of my numbers still gave the moon's places 8 or 9 minutes erroneous: though Dr. Gregory † and Dr. Halley had boasted they would agree within 2 or 3 minutes.

1695, or 1696 ‡. Sir Isaac Newton, being made an officer of the Mint, came to London §. I sometimes visited there, or at his own house in Jermyn Street. We continued civil: but he was not so friendly as formerly, because I could not [confirm] Mr. Halley's and Dr. Gregory's || assertions concerning his corrections of the Horroxian lunar theory.

1696. A correspondence began with Mr. Bossley, an apothecary of Bakewell in Derbyshire, and Mr. Luke Leigh, a poor kinsman of Mr. Halley's, of

* All the letters, which I have been able to find relative to this subject, have been collected together, and printed in the Appendix, No. 16—34. They extend from Oct. 7, 1694, to Sept. 14, 1695. The originals exist in the library of Corpus Christi College, Oxford; from which they have been copied by the kindness, and under the superintendence, of S. P. Rigaud, Esq., the Savilian Professor of Astronomy there, expressly for this work. *Copies* of them also exist in the library of the British Museum. F. B.

† The initial only of this name is given: but it is evident that Dr. Gregory is the person alluded to. F. B.

‡ Here the narrative, derived from the principal source above-mentioned, is resumed, and copied again verbatim. F. B.

§ He was made *Warden* of the Mint in March 1695-6; and three years afterwards (in 1699) he was made *Master* of the Mint. F. B.

|| These names are not written at full length in the original MS, but there can be no doubt that these were the persons intended. The word within the brackets is wanting in the original MS, but I have supplied it from the context. F. B.

the same clan, and myself. Mr. Bossley wanted observations for correcting the planets' places: I furnished him; and set him on Jupiter and Saturn. Mr. Leigh I hired to calculate the *places* * of the fixed stars from their right ascensions, and distances from the northern pole, determined by myself. [For I wanted now calculators to ease me in such a burden of work, as never any astronomer had before on his hands. God Almighty provided them for me unexpectedly. Sir Christopher Wren would needs put a relation of his, an ingenious youth, since well known for his mathematical abilities, into my service. Another I hired in Derbyshire; and made use of a third that lived there too. By whose help I entered on the business of the fixed stars; and in a few months after, of the superior planets.

I wanted some person that had a faculty of drawing, to design the figures of the constellations. For Bayer, who first drew them since Tycho's time, almost a hundred years ago, had so formed them that they plainly contradicted Ptolemy's description; whom all the catalogues since have translated and followed, as all other chart-makers have Bayer. The same Good Providence that furnished me with 2 or 3 calculators, sent me an ingenious but sickly youth (Mr. Weston) into my service, who was addicted to this practice: and by my directions drew the charts of the constellations so well, that a good designer said he needed no directions but his draughts to perfect them †.]

1696, December 11th. I received from him [Mr. Leigh] the places of the stars in the constellations of Gemini, Cancer, and Leo: which whilst he had been doing, the same were done by my then servant Mr. Hodgson in the Observatory. So that I easily found the errors of either, and corrected them. I received

1696-7, Jan. 22. Virgo, Libra, Scorpio, Sagittarius.

1697, March 27. Capricornus, Aquarius, Pisces.

—— June 16. Aries, Taurus.

1698-9, Jan. 10. Cetus, Eridanus, Lepus, Canis Major, Canis Minor, Navis.

1699, Aug. 19. Orion, Hydra, Crater, Corvus.

1700, July 25. Serpens, Serpentarius, Aquila cum Antinoo, Sagitta, Delphinus, Equuleus, Pegasus, Triangulum.

* That is, the longitudes and latitudes: a work of great labor and expense, which might well have been spared F. B.

† This part within brackets is taken from a MS paper in Flamsteed's handwriting, entitled *Short note of dates for my Works*, mentioned in page 5. F. B.

1701, April 26. Cassiopea, Cepheus.

—— June 5. Andromeda, Perseus, Auriga, Coma Berenices, Corona Borealis, Bootes, Hercules, Lyra, Cygnus *.

The stars in Hevelius's Sextant, Monoceros, Lynx, Camelopardalus, Canes Venatici, were calculated afterwards in 1705-1708 by my servants J. Woolferman, and J. Crosthwait: and the constellations of Hercules and Cassiopea, enlarged with the addition of many stars observed in the years 1705-1708, by them and Mr. Ab. Ryley.

In the meantime, as often as I met with Sir Isaac Newton, he was very inquisitive how the catalogue went on. I answered as it stood: and, when he came here, commonly showed him how it stood in my books; not suspecting any design, but hoping he might serve me as kindly as I had assisted him freely with my pains when he desired me †.

1698. At Allhallows, was at Derby and Bakewell.

—— [In the spring of this year (Feb. 6th) the Czar of Muscovy visited the Observatory: was four times in it; and sometimes present at my observations, which were carried on vigorously both this and the five following years: though, my best servant leaving me in April 1702, I found it more difficult to carry them on than formerly. But by the end of 1703 I had finished all but the constellations of Hercules, the Great Bear, and such as lay within 30° of the north pole, for which I wanted observations. I afterwards attained them by the help of Mr. Weston, who was then expert in observing ‡.

1703. In November, and in April 1704, I got some of the maps of the constellations to be anew delineated by P. Vansomer, an excellent draughtsman, but in years: the charts being prepared by Mr. Weston.] §

* The major part of these computations by Mr. Leigh still exist, and are to be found in MSS, vol. 63, bound up in seven separate volumes, as they were received from him; and are marked A to G, respectively. F. B.

† One of these visits to the Observatory was made on Sunday evening, Decem. 4, 1698, and is thus recorded by Flamsteed in his Observation Book (MSS, vol. 7):—" Decem. 4, dum preces " vespertinæ fiunt, me visum venit D. Isa. Newton; retulitque hinc 12 observata loca: sc. AR et " decl. limb. a polo." This visit is the more remarkable when coupled with that extraordinary letter which Newton wrote Flamsteed about a month afterwards: viz. on Jan. 6, 1698-9. See Appendix, No. 43. F. B.

‡ The following entry appears in the *Journal* Book of the Royal Society, at this period, viz.: June 9, 1703. " A letter was read from Mr. Flamsteed to Mr. Caswell, concerning his Observations " on the Parallax of the Earth. Mr. Flamsteed was thanked, and desired to suffer some parts of " the letter to be printed." See the two letters in the Appendix, No. 68 and 69; which probably relate to this subject. F. B.

§ This part, within brackets, is taken from the same MS paper as that mentioned in the note in page 64. F. B.

1704, Tuesday, April 11th. Mr. Newton came to the Observatory; dined with me; saw the volume of observations; so much of the catalogue as was then finished; with the charts of the constellations, both T. W.'s and those copied by Vansommer; desired to have the recommending of them to the Prince. I knew his temper; that he would be my friend no further than to serve his own ends; and that he was spiteful, and swayed by those that were worse than himself. This made me refuse him. However, when he went away he promised he would recommend them; though he never intended me any good by it, but to get me under him, that I might be obliged to cry him up as E. H[alley] has done hitherto.

1704, November 8th. Wrote the estimate, which was read without my knowledge at the Royal Society. The members thought it ought to be recommended to the Prince: the President joined with them: a committee was appointed to attend his Royal Highness: done without acquainting me with it: an estimate of the charge drawn up without my knowledge: the Prince allows £1200: Mr. Newton says £1100. He concludes me now in his power; does all he can to hinder the work, or spoil it, by encouraging the printer to commit faults. We must print the observations; though I had showed in my printed estimate that, for very good reasons, the charts of the constellations ought first to be set upon.

Mr. Newton told me he hoped I would give a note, under my hand, of security for the Prince's money. This I know was to oblige me to be his slave. I answered that I had (God be thanked) some estate of my own, which I hoped to leave, for my wife's support, to her during her life, to my own relations afterwards: that therefore I would not cumber my own estate with impress or security: but if they would please to take his Royal Highness's money into their hands, I would sign the workmen's bills to them, whereby they would see if they were reasonable at the same time. I was told I should have *all the printed copies*, save what his Royal Highness should have to present to the Universities. And Mr. Newton granted that, since I refused to handle any of his Royal Highness's money, there was no need of security or articles: nevertheless . . . . .

[Here this MS ends abruptly. F. B.]

Sixth Division.

FROM 1695 TO 1704.

WHEREFORE having now got his stock of observations much increased by his continual labors both by night and by day, he enquired for help to employ them *: and that good Providence that had given him strength and opportunity to carry on the work found him, in November 1694, what he desired. He had help offered him seasonably by a poor but very ingenious person that lived no nearer than 124 miles from him †, whom he hired to serve him on reasonable terms. He had a servant waited on him, that was very good at numbers ‡. The right ascensions of the stars, and their distances from the visible pole, he determined himself; with the help of his domestic sometimes 2 or 3 different ways, for greater certainty, both by distances observed and by transits §: which, being transcribed by himself, were transmitted to his calculators in the country, with easy tables for deducing their longitudes and latitudes from them. At the same time they were copied by his domestic in the observatory; and the same places derived from their right ascensions, &c. by him. As soon as the country calculations were returned they were collated with the servant's: where they agreed, there was no doubt of their truth; for the calculators were too far distant to hold any correspondence together: where they differed, the calculations were forthwith repeated and corrected. This care was absolutely necessary to prevent errors. So the work went on well: the zodiacal constellations being begun in November 1695, and finished in June 1697. The other constellations, which lay south of the zodiac, were not finished till about Michaelmas 1701: because the observations requisite to complete them were not got till the year before. And, for the same reason, the more northern constellations of the Bear and Dragon, not till 1709 ‖.

* The reader will recollect that this division of the subject is written by Flamsteed in the *third* person. F. B.

† Probably Mr. Luke Leigh, already alluded to in page 63, who lived somewhere in Derbyshire. F. B.

‡ I cannot find any trace of the name of this servant. F. B.

§ Many of these computations still exist, amongst the MS books deposited at the observatory; to which I shall more particularly allude in my *Introduction to the British Catalogue*. F. B.

‖ See pages 64 and 65. F. B.

In the meantime, a friend of my country calculator, who lived near him *, was desirous to try how he could represent the motions of the two superior planets, by new numbers. Mr. Flamsteed furnished him with a large stock of the places both of Saturn, Jupiter, and Mars, derived both from his observations and others, for this purpose: by the help of which he made new tables for Saturn that, at that time (1696), took off the enormous errors of the *Rudolphine* and *Caroline tables* in that planet; and represented the observed places of Jupiter much better than either of them: but did not so well agree with the heavens in the following years, as they had done in the preceding. Which his friend taking notice of, after he had sent him the last year (1710) the observations of Jupiter's place at his opposition to the sun with the observed places of some former years, he made a new correction of his motions. On the receipt of which, Mr. Flamsteed set himself to consider them, and found numbers that would still represent the observations something better, and [give] reason to hope they would not err more than one-sixth or one-fourth of a degree for the next 200 years: whereas the Rudolphine were now two-thirds. But, coming to consider the motion of Jupiter and his numbers, he found an inequality that was small, and seemed more difficult to be answered; which caused him to remit the further consideration of them, till he might learn more of it by future observations: and thought the next six years might afford him sufficient both for his and Saturn's, if God spared him life and health.

His friend had done nothing in Mars, being frighted with the enormous errors of the tables in that planet. He set therefore upon him himself: and after some often-repeated trials last summer, found he could express all his own observations of him pretty well. But wanting help to manage those of Walter, Tycho, and Hevelius, and being interrupted by an accident † that threatens not only to stop all his endeavours but even ruin the observatory itself, and to deprive the world of the use, and himself of the advantage, of 40 years' labors, and more than £2000 expenses out of his own small income, he is forced to dismiss the further prosecution of his endeavours to correct the motion of the planets for the present, that he may acquaint you‡, and all ingenious persons with you, how the

* This is evidently Mr. Bossley, of Bakewell, already mentioned in page 63. There are four letters of his to Flamsteed, on astronomical subjects, which have been preserved, and are bound up in MSS, vol. 36.

† This was written at the beginning of the year 1711, when the quarrel between Newton and Flamsteed was at its highest. F. B.

‡ It would appear from this expression that this paper was intended to be addressed to some friend. F. B.

storm was caused that threatens us. Whereby you will also see whence it comes that the mathematics languish in both Universities, in spite of the munificence of generous benefactors, and the noble designs of persons of public spirit, that have bequeathed a large part of their estates to be employed for their improvement.

Whilst Mr. Flamsteed was busied in the laborious work of the catalogue of the fixed stars, and forced often to watch and labor by night, to fetch the materials for it from the heavens, that were to be employed by day, he often, on Sir Isaac Newton's instances, furnished him with observations of the moon's places, in order to carry on his correction of the lunar theory. A civil correspondence was carried on between them*: only Mr. Flamsteed could not but take notice that as Sir Isaac was advanced in place, so he raised himself in his conversation, and became more magisterial. At last, finding that Mr. Flamsteed had advanced far in his designed catalogue by the help of his country calculators, that he had made new lunar tables, and was daily advancing on the other planets, Sir Isaac Newton came to see him (Tuesday, April 11, 1704); and desiring, after dinner, to be shown in what forwardness his work was, had so much of the catalogue of the fixed stars laid open before him as was then finished; together with the maps of the constellations, both those drawn by T. Weston and P. Van Somer, as also his collation of the observed places of Saturn and Jupiter with the Rudolphine numbers. Having viewed them well, he told Mr. Flamsteed he would (*i. e.* he was desirous to) recommend them to the Prince *privately*. Mr. Flamsteed (who had long been sensible of his partiality, and heard how his two flatterers cried Sir Isaac's performances up, was sensible of the snare in the word *privately*) answered that would not do: and (upon Sir Isaac's demanding "why not?") that then the Prince's attendants would tell him these were but curiosities of no great use, and persuade him to save that expense, that there might be the more for them to beg of him: and that the recommendation must be made *publicly* to prevent any such suggestions. Sir Isaac apprehended right, that he was understood, and his designs defeated: and so took his leave not well satisfied with the refusal.

It was November following ere Mr. Flamsteed heard from him any more: when, considering with himself that what he had done was not well understood, he set himself to examine how many folio pages his work when printed

* This, I presume, is the correspondence alluded to in page 62. F. B.

would fill; and found upon an easy computation that they would at least take up 1400. Being amazed at this, he set himself to consider them more seriously; drew up an estimate of them: and, to obviate the misrepresentations of Dr. S[loane] and some others, who had given out that what he had was inconsiderable, he delivered a copy of the estimate to Mr. Hodgson, then lately chosen a Member of the Royal Society, with directions to deliver it to a friend, who he knew would do him justice; and, on this fair account, obviate those unjust reports which had been studiously spread to his prejudice. It happened soon after, Mr. Hodgson being at a meeting spied this person there, at the other side of the room; and therefore gave the paper to one, that stood in some company betwixt them, to be handed to him. But the gentleman, mistaking his request, handed it to the Secretary [Dr. Sloane] who, being a Physician, and not acquainted with astronomical terms, did not read it readily. Whereupon another in the company took it out of his hands; and, having read it distinctly, desired that the works therein mentioned might be recommended to the Prince: the charge of printing them being too great either for the author or the Royal Society. Sir Isaac closed in with this.

[Here ends the document from which the above portion of the history is taken. F. B.]

Seventh Division.

FROM 1704 TO 1716.

I HAD been acquainted with Mr. Newton ever since the year 1674; had given him the diameters of the planets observed by me at Derby in the years 1671, 72, and 73; as also the greatest elongations of Jupiter's satellites (of both which he made use in his *Principia*); and, since I came to London, the line of the great comet of the years 1680 and 81; affirming that the comet which was seen in November before was the same with that I observed in the following December: which he would not then grant, but contended earnestly that they were two different ones, as appears by a couple of very long letters of his to me, dated Feb. 28, 1680-1, and April 16, 1681*. In which opinion he persisted till September 1685; when, in a letter dated the 19th of that month, he writes, "I "have not yet computed the orbit of a comet, but am now going about it, and "taking that of 1680 into fresh consideration. It seems very probable that "those of November and December were the same comet †." This is what he before contended against with some virulency, but he had no mind to remember it, and at that time I took no great notice of it, till I found when his *Principia* were published in 1687, and therein a draught of the comet's orbit, he was pleased to acknowledge that I had *disputed* that the comets seen in November and December were one and the same; and that I had given him the line of its way not much different from his parabolical one there described. Whereas himself had *disputed* against their being one, and consequently against that one describing any parabolic line as he now asserted, and will appear by his own forementioned letters to me. From this time till the year 1695, we corresponded civilly; especially about the years 1694 and 1695‡, when, on his repeated requests, I imparted to him about 150 places of the moon deduced from observations made with the mural arch, and compared with my own tables, fitted to Mr. Horrocks's theory; but covenanted at the same time that he should not impart

* Both these letters are printed in the *General Dictionary* (mentioned in page 3), under the article "Newton." F. B.

† This letter is also printed in the work mentioned in the preceding note. See the note in page 50. F. B.

‡ All the letters which I have been able to collect, connected with this correspondence, are (as already mentioned in page 63) inserted in the Appendix, No. 16—34. They extend from Oct. 7, 1694, to Sept. 14, 1695. F. B.

them to anybody without my consent. For, I told him (and he knew it very well) that I had made use of an old catalogue of the fixed stars, made to the beginning of the year 1686, from observations taken with the sextant*: that I was busy now with a better and more convenient instrument; and that, as soon as I had got the new catalogue, I intended, perfected, all those places of the moon should be calculated over again, and imparted to him: but the hopes he had of making that theory his own, and the glory of restoring the moon's motions, would not suffer him to stay so long for.

It was not a full year after but I was told that he had perfected the lunar theory: and Dr. Gregory gave out that there was no need of further observations; for his number would answer all my observations within two or three minutes, or less. I had covenanted with him to have his emendations first imparted to me, because I imparted to him the observations from which they were derived. But, his promise was overlooked or forgot: at last it came to my hands. I found the solar numbers were the same I had freely given him: and the lunar but little altered; save that he had added a parcel of very small equations which, whether the heavens would bear or not, was only to be found by comparing his numbers with good observations. I therefore made new lunar tables exactly agreeable to his sentiment: but when I compared the moon's places, calculated from them, with her places deduced from the observations, I found that those numbers which were said to agree with the observations within two or three minutes, would very seldom come so near, but often differed 8, 9, or 10 minutes; which I did not admire then at all, being very sensible that the persons who so loudly on all occasions cried up his performances in amending the lunar theory and tables, did it to oblige his friendship, who had then a great interest in a great courtier†: and considering also that [they] were persons of very ordinary skill in that part of mathematics which was concerned with the heavens and the lunar theory.

* See the Note in page 55. F. B.

† Probably Charles Montague, who had been President of the Royal Society, and was afterwards (in 1700) created Earl of Halifax. On the death of his wife he conceived a strong attachment for Mrs. Catherine Barton, the widow of Col. Barton, and the niece of Newton, a beautiful and accomplished lady; but who did not escape the censure of her contemporaries. The Earl of Halifax, prior to his elevation to the peerage, promoted Newton to the Wardenship of the Mint, and on every subsequent occasion lent him his powerful support. At his decease, he left Newton, by will, only £100: whereas he bequeathed to his niece, Mrs. Barton, "for her excellent conversation," property in houses, lands, jewels, &c, to the amount of £25,000: a considerable sum at that period. She afterwards married Mr. Conduit, who succeeded Newton in the Mint. F. B.

But Mr. Newton was not displeased with their flattery; nor ever (that I could hear of) endeavoured to correct them. We conversed civilly as oft as we met accidentally: and he failed not (as if he were a great master of my methods) always to ask "how the catalogue went on." To which I always gave him sincere answers; telling him how far I had proceeded, and that I wanted more hands both to carry on the observations and calculations that were necessary. But this I could not get him to take notice of. In the mean time, some friend of mine (that was frequently in company with me, and saw how the work went on with such assistance as I hired and paid myself, and was informed what the charge would be of printing the observations of 30 years, and engraving the maps of the constellations I had prepared) acquainted Prince George of Denmark with my performances. Mr. Newton lived near the Court: I, always at a distance. He was the President of the Royal Society, and had a great courtier as his friend, and one who was frequently at his office, required at Court, and attending on the Prince*. So that he could not but hear of the Prince's inclination to make me easier in my work; nor could Mr. Newton fail to be informed of it. So, on the 10th of April, 1704, he came down to Greenwich, visited me on my request, staid and dined with me. At his first coming he desired to see what I had ready for the press. I showed him the books of observations, together with so much of the catalogue as was then finished (which was about one half), and a fair copy of it: and with it the maps of the constellations drawn both by my amanuensis and Vansomer. Which having looked over carefully, he desired me to let him have the recommending of them to the Prince. I was surprised at this proposition. I had formerly tried his temper, and always found him insidious, ambitious, and excessively covetous of praise, and impatient of contradiction. I had taken notice of some faults in the 4th book of his *Principia:* which, instead of thanking me for, he resented ill. Yet was [so] presumptuous that he sometimes dared to ask "why I did not hold my tongue." I considered that if I granted what he desired, I should put myself wholly into his power, or be at his mercy, who might spoil all that came into his hands, or put me to unnecessary trouble and vexation about my own labors: and all the while pretend that he did it to amend faults, where none were but what were unavoidable, or easy to be corrected, and therefore excusable. I had further irritated him by not concealing some truths that are since published in print, and notoriously known; and therefore civilly refused what he desired. But still he told me he

* Probably Mr. Montague. See the last note. F. B.

would recommend them to the Prince, and parted with me in the evening with a short expression of very good advice, " Do all the good in your power :" which it would have been very happy for him if he had followed himself, and has been the rule of my life from my infancy ; though I do not know that it ever has been of his *.

But, I heard no more of his recommendations. On the contrary, his flatterers and such small mathematicians about London as hoped to get themselves esteemed very skilful even by crying up his book, began to ask " why I did not print." As if I were obliged to publish my works just when they pleased † ; though they understood no more of my works than they did of his book, which they so much cried up. To obviate this clamour, I examined all my books of observations, and took an account what number of folio pages they might fill when printed ; and found it much greater than I expected. Whereupon I drew my *Estimate* into a short paper, wherein I both showed what the number of pages were, and also in what order the press was to work them off ‡. And chiefly urged that the maps of the constellations should be first of all set upon :

* The character which Flamsteed has here drawn of Newton, and which he insists on throughout the whole of his statements, is so much at variance with that mild and modest behaviour which most of his biographers have attributed to him, that it might seem like the excess of spleen and malice on the part of Flamsteed to dwell so much on these topics, were not his opinions strengthened by that of some of his contemporaries. Whiston, who knew him well, says he was impatient of contradiction, and that he was of the most fearful, cautious, and suspicious temper that he ever knew. See Whiston's *Memoirs*, page 294. And, in a pamphlet, published in the year 1710, entitled *An account of the late proceedings in the Council of the Royal Society, in order to remove from Gresham College into Crane Court, Fleet Street*, a copy of which is preserved in the library of the British Museum, Sir Isaac Newton is accused of partiality and precipitancy, and of having acted (at a meeting of the Society, called for the express purpose of considering the propriety of such removal) with a degree of warmth, and to have assumed an air and tone, not very suitable to the candour and impartiality which might have been expected from the President of so distinguished a body. I shall not embark in the odious task of attempting to multiply such instances : indeed it is with much reluctance that I advert at all to a subject of this kind ; but justice to Flamsteed's memory requires that he should be defended even from the suspicion of misrepresentation. F. B.

† See the 2nd note in page 61 ; where reference is made to a letter of Flamsteed, explanatory of his reasons for not printing his observations at that early period. It should be borne in mind, however, that that letter was written long before the quarrel between Newton and Flamsteed. F. B.

‡ I apprehend that the estimate, here alluded to, was not an estimate of the *expense* of printing the work, but an estimate of the number of *pages* that the work would contain : and that the original document, which was read at the meeting, now exists in the library of the Royal Society, bound up at the end of the collection of Flamsteed's letters mentioned in the note in page 28. The estimate of the *expense* was made by the Referees, and will be mentioned in the sequel. F. B.

that, being carried on apart, they might be finished by the time the observations were printed off. Vansomer, an excellent designer, who had drawn about a dozen figures for me, was then alive, and ready to go on with the rest. My amanuensis had not yet left me, and might have been hired again to continue in my service: Mr. Hodgson's help might also have been purchased. Some of my acquaintance had fallen into a suspicion that my labors answered not what might reasonably be expected from me. That I might cure them of their misapprehensions, which had been impressed upon them by the false and malicious suggestions of some few arrogant and self-designing people, I gave a copy or two of this Estimate to an acquaintance of mine; desiring him to show it to those of my friends who had been possessed with these unjust suspicions. At one of the meetings of the Royal Society, some of them were present: he got my paper handed to one of them who sat at a distance (for their meetings were thronged with company, however thin they are at present), who, opening the paper and finding the contents, delivered it to the Secretary, who read it at the board. This convinced the members present that I had been unjustly aspersed: and it was moved that the printing of the whole should be recommended to Prince George by the Society.

Accordingly a committee was appointed, who with Mr. Newton waited on the Prince. But, who they were, when they waited on him, and how they made their recommendation, I was never informed: nor did they vouchsafe to consult me about it, or take me along with them. All that I can tell of is, that the estimate was wrote in November 1704; the Prince chosen into the Society November 30th: a letter from the Prince's secretary, Mr. George Clarke, directing Mr. Roberts, Sir C. Wren, Dr. Gregory, and Dr. Arbuthnot, with Mr. Newton, to inspect my papers, dated Decem. 11, 1704*; which they did,

* The following entries appear in the *Journal* Book of the Royal Society; illustrative of some of the events of this period, viz.:

Nov. 15, 1704.—"A paper was read from Mr. Flamsteed, giving an estimate of a book, wherein "he intends to print and engrave his observations on the stars for 30 years past. The Society ordered "their thanks to be given to Mr. Flamsteed for communicating this his design, and promised to en- "courage the same as far as they can."

Nov. 22, 1704.—"The Society desired the President to recommend Mr. Flamsteed's design, as "far as he can, in order to the publication of his Observations."

Nov. 30, 1704.—"The Prince of Denmark was unanimously chosen a member of the Society: "and the Society were extremely pleased with the honor the Prince did them, in suffering them to "choose him a member."

Dec. 20, 1704.—"A letter was read from Mr. Clarke to the President (by order of the Prince)

and some time after gave in their report of the charge of preparing and printing the observations and catalogue mentioned in the estimate, about £863. viz.

	£	s.	d.
283 reams of paper for 400 copies, at 20*s.* per ream . .	283	0	0
Composition and press-work for 300 sheets, at 20*s.* per sheet . .	300	0	0
Charges of an amanuensis for copying, comparing, correcting, and examining the papers	100	0	0
To compute the planets' places, for two calculators . .	180	0	0
In all	£863	0	0

But, the last particular of the charge (£180 for two calculators) was not mentioned *in* it; but added in a note *under* it: for what reason those know best who drew it up *. Nor the charge of designing and engraving about 50 plates of the constellations: though this was likely to be the heaviest part of the charge, and the observations could not be understood without them. I had further proposed them to be the first taken care of and begun. I had them all drawn; and twelve of them, anew designed by a skilful workman, by me. These were the most sumptuous part of the work: and had it not been for them, I had no, or little need to crave the Prince's help to print. Why they were neglected, Sir Isaac Newton best knows. Betwixt March 22, 1704-5, and April 21, 1705, Mr. Newton was knighted by the Queen at Cambridge †.

Hereby I was plainly convinced that Sir Isaac Newton was no friend to [my] work; and every step he took afterwards proved plainly that, whatever he pretended, his design was either to gain the honor of all my pains to himself, to make me come under him (as Dr. Arbuthnot some time after expressed), or to spoil or sink it: which it was my chief concern and business, if possible, to pre-

" concerning the publication of Mr. Flamsteed's papers. The President was desired to take " what care in this matter he shall think necessary, towards the most speedy publication of so " useful a work. And the President was desired to return the most humble thanks and ac- " knowledgments of the Society to the Prince, for so great a favor, in such manner as he shall " think fit."

Jan. 3, 1704-5.—" The President reported that he had waited on the Prince, with the thanks " of the Society; and that the publication of Mr. Flamsteed's papers is carrying on as fast as " may be."

The letter of Mr. Clarke, above-mentioned, is given in the Appendix, No. 78. F. B.

* There are two copies of this Report amongst Flamsteed's MSS: one in MSS, vol. 33, page 49, and the other in vol. 35, page 33. But this £180 is not mentioned in either of them. They are both alike, and a correct copy is inserted in the Appendix, No. 84. F. B.

† He was knighted on April 21, 1705. F. B.

vent. I therefore printed my estimate and gave it to my friends *: that they might see what my works were, and how I thought it best to proceed in printing them.

To screen himself from the just imputation and blame that would probably follow such disingenuous and ungrateful practices, he made use of these gentlemen, to whom he had got the inspection of my books of observations ordered by the Prince, and called them the Prince's *referees*. Of these, Sir C. Wren was then about 70 years of age: and though he was a skilful person, yet being full of other business, he was sure to have him, who lived in his neighbourhood, to consent to all his orders, and subscribe them. Mr. Roberts was an easy, good-natured man, but knew little of the business. Mr. Aston had been fellow of the same (Trinity) college in Cambridge, at the same time with him, knew nothing of the business, lived in the Court, had been my friend and guest at the Observatory, was too much a courtier to withstand any one that had a noble patron in the ministry, and therefore was taken into the number of referees, sometimes for special purposes. Dr. Gregory, though he published a piece of astronomy, knew but very little of that part of it that was cultivated here. Nor was Dr. Arbuthnot skilled in it †: but being one of the Prince's physicians, he was taken in to serve Sir Isaac Newton's purposes. He saw what was designed, and testified to me, by some expressions, that he approved not such proceedings; promised once to assist me in a particular affair; and, though he met with obstructions, performed it handsomely.

With these persons Sir Isaac Newton began to act his part, and carry on his designs. I dealt honestly and openly with him, as will appear by the copies of some letters I wrote to him upon several occasions; having no other design but to have my work handsomely printed, and as soon as possible: for the Prince was very infirm ‡. But, I soon perceived that he designed only to hinder the work

* The estimate alluded to in the note in page 74. F. B.

† Dr. Arbuthnot, whose name will be frequently mentioned in the sequel, was a native of Scotland, and took his degree of M.D. at Aberdeen. For some time after he settled in London, his practice produced so little emolument that he found it necessary, for his support, to teach the mathematics. Like many others of the medical profession, he was indebted to a circumstance, wholly fortuitous, for his first elevation. Prince George of Denmark was suddenly taken ill at Epsom, at the time Dr. Arbuthnot happened to be there: he was accordingly called in: the Prince recovered; and he was ever afterwards employed by him as his physician. In 1709, he was appointed physician in ordinary to Queen Anne. He is well known as a satirical writer, and died in 1735.—*Hutton's Abridgment of the Phil. Trans.*, vol. 5, page 606, note. F. B.

‡ Besides the correspondence, which has for the most part been preserved by Flamsteed, he also

by delays, or spoil or sink it, or force me to comply with his humour, and flatter him, and cry him up as Dr. Gregory and Dr. Halley did *. I was forced therefore to act with more caution than I had done hitherto, that I might give him no cause of pretensions to stop the progress of the work: to forward which, I used my best diligence and honest endeavours. I hired one, and employed him to copy specimens of the several parts of the work: 1°. the observations of the fixed stars made with the sextant: 2°. of the moon made with the same instrument: 3°. of observations made with the mural arch: 4°. of the new catalogue which I sent him, with a list that gave an account of them, dated Jan. 5, 1704; but could not get them printed off till March 22 following. In the mean time Sir Isaac Newton appointed a meeting of his referees, March 5 following. Mr. Churchill was not there: but Sir Isaac, with Dr. Arbuthnot, Dr. Gregory and Mr. Aston, dined at Churchill's: and a fortnight after, Mr. Aston told me of it (for I dined not with them), and that all things, he thought, were then agreed but paper. Now I understood that Mr. Churchill was to be the undertaker †: he had been recommended for that purpose, by one that I took to be my friend, without my knowledge; for I did not conceive that we had any need of one, and so did some of the gentlemen of the Society. But Sir Isaac Newton was resolved to make friends at my cost. For, as he ordered the matter, the undertaker was here to reap the sole advantage of all my labors and great expenses: and he was so confident of it, that when I intimated it to him, he answered boldly "The Prince would reward me for them."

However there was no receding: for then Sir Isaac Newton's criers-up would have clamoured "that I hindered the printing of my own works myself." To avoid that imputation I was silent. Though I complained oft to some friends in private, but never did anything whereby it might appear I allowed him. At this meeting on the 5th of March, the specimens of the undertaker's printing were produced, but found to be ill done. I got others done very well, and paid the printers myself.

kept a *Diary* of his proceedings, relative to this subject; which is scattered through various pages of the letter-book A (MSS, vol. 33). It commences on page 47, with the date of Novem. 8, 1704, and terminates with the date of August 6, 1713. The facts are all collected together, and inserted in the Appendix, No. 75. F. B.

* The initials only of these names are given in the MS, but it is evident that these are the parties intended. F. B.

† The term *undertaker* is here used by Flamsteed as synonymous with bookseller, or rather, *publisher*. F. B.

June the 11th following, Dr. Gregory and myself, with Mr. Churchill, dined at Sir Isaac Newton's; where they agreed to give Mr. Churchill £1 14*s*. per sheet. They signed the agreement, but I would not, although they urged me much. I desired to be excused; for it was now plain to me that he designed not the good of the impression or my advantage, but to make him a friend of a great name, by obliging a person I never had any acquaintance with, and enriching him at my cost*. This point being over, I was in hopes that the press should have been set to work immediately: for I had about 50 sheets of observations, made with the sextant, ready copied, and the rest of that sort would easily be finished before these could be printed off. But I found myself deceived: we were as far off from printing as if no such bargain had been made.

At midsummer following I paid my amanuensis and calculators a quarter's pay myself; and Sir Isaac Newton, to encourage me to do it, talked often of drawing the Prince's money. But, when I waited on him, July 4th following, and told him that I must go into Surrey to reap my harvest (as usually I did, every year, about this time), he put me off again, before I could say anything to him of it, by telling me that Dr. Arbuthnot's daughter was so very ill that the Doctor could do nothing till her recovery; that it was not fit we should begin to print till we had received his Royal Highness's money; and that it would be soon enough at my return. I had put 12 sheets, ready for the press, into his hands a week before. He thought to work me to his ends by putting me to extraordinary charges in maintaining and paying an amanuensis and calculators myself, at my own charges. But, I resolved to bear this expense patiently, and defeat his designs.

After this, I caused my amanuensis and calculators to go on with their work, and carried on the observations for completing the catalogue and others, according as I had opportunity. But, Sir Isaac became daily more perverse, and sought by several vexatious pretences to discourage me, and weary me if possible. I paid my calculators and amanuensis 3 quarters, without any present prospect of being any way re-imbursed. But yet I had hopes, if once the press began to work, they would not find any new tricks or pretences to delay repaying me. But, herein too I found myself mistaken: those that have begun to do ill things, never blush to do worse and worse to screen themselves. Sir Isaac Newton had still more to do, and was ready at coining new excuses and pretexts to cover his

* It was but reasonable in Flamsteed to expect that, if any profit arose from the publication of the work, it ought to belong to him, rather than to the bookseller. See his own opinion on this subject, in a paper dated August 29, 1705, inserted in the Appendix, No. 103. F. B.

disingenuous and malicious practices. I had none but very honest and honorable designs in my mind: I met his cunning forecasts with sincere and honest answers, and thereby frustrated not a few of his malicious designs.

Finding that I persisted unwearied in my purposes, he demanded to have my *First Night Notes* put into his hands, that he might compare them with my copy. These were wrote in quarto volumes *; and from them were commonly transcribed correctly into large folios, next morning, from which the copies were taken †. I knew that he would be mistaken, and that they would not serve his design: they were put into his hands February 23, 1705-6. Mr. Hodgson acquainted me that Sir Isaac had showed him 3 or 4 pages of errata, that were committed in transcribing as he supposed: and a table, made by Dr. Gregory, for turning the revolves of the screw into degrees, minutes and seconds, wherein he *wisely* had supposed the screw everywhere equal and equable. I smiled at this, and promised to send them my own tables for that purpose, and showed them their mistakes, and that there were no material errors committed. This was some small mortification to them: but they had learned not to be ashamed.

Though I had refused to handle any of the Prince's money but what was to repay my proper disbursements ‡, and Sir Isaac Newton had granted that then it was not necessary I should sign any agreement with the referees, yet now he became very positive for Articles. He had said to some of his confidants, "that "he would hamper me with Articles." It had come to my ears: and therefore, on his urging me, I drew up some for the undertaker to sign; as, that he should

* These quarto volumes are still in existence, and are the MSS, vols. 2, 3, 4, 5, 6, 7, and 8; some of the former of which are the books here alluded to. For, in the first page of MSS, vol. 2, (containing the *original* entries of his observations from Nov. 1, 1679, to Feb. 15, 1684,) Flamsteed has written this note with his own hand, viz.:—"Librum hunc postquam detinuisset fere 12 "annos remisit Newtonus Eq. mense Octobris exeunte anni." And in the first page of vol. 3, (containing the original entries of his observations from Feb. 19, 1684, to Sept. 3, 1689,) he has also written with his own hand, the following note:—"Commoda Eq. N. Feb. 27, 1715-6, recepi "post multas frivolas excusationes et fictas morarum vel pretextas causas misso J. C. ad petendum "July 1, 1715, cum libro 1° observationum Grenovicensium post 9½ annorum captivitatem." These facts and dates will be borne out by the subsequent history. See Flamsteed's letter to Sharp, July 9, 1715, and other documents confirmatory thereof in the Appendix. F. B.

† These folio MS books (called by Flamsteed the *Apographa*) are also in existence; and are MSS, vols. 15, 16, 17; and contain *copies* of all his observations made with the mural arc. F. B.

‡ Flamsteed very wisely objected to have any thing to do with the disposal of the Prince's money; as he well knew, from experience, that he would probably have much trouble in passing his accounts, and getting his final discharge from Government. F. B.

print only 400 copies; that he should have no interest in the original, &c. But these were not to his purpose: I would not court him. To bring about his low designs he makes Articles himself: in which some things of mine were inserted; and in them he covenants the undertaker should print 5 sheets per week; and for reprinting of faulty sheets; and that I should have £125 paid me when 10 sheets were printed off. These were read to me once; and I was required to sign them immediately, else the work was at a stand: no time would be allowed to consider of them, or mend anything I thought amiss in them. I was then near £140 out of pocket *: all my copy was ready for the press, or soon would be. If I refused, the work would be broken off immediately, and the fault would be thrown upon me. For Sir Isaac Newton lived in the neighbourhood of the court: I at six miles' distance. He had his close friend the Lord Halifax † to support him there, with the Prince's physician: I had nothing but my sincerity and God's blessing to depend upon. Trusting on these alone I signed them; not doubting but now the press would begin. The Articles are dated November the 10th, in the 4th year of Queen Anne, or 1705 ‡.

But, herein I soon found myself deceived. This would not satisfy: I would not yet cry up Sir Isaac as others did. To bring me to that baseness, now he has got my books of *Night Notes*, he wants a copy of so much of the catalogue as I had gone through with, to be trusted into his hands. He therefore demanded it. I answered that it was not then perfected; that I believed it would contain a good number more than I had yet observed and rectified; that the stars already in it were about 1500, but probably I should make them 2500; that these were the result of all my labors, in which having spent above £2000 of my own money above my allowances, it would neither be prudent nor safe to trust a copy of them out of my own keeping. He answered that I might then put them into his hands, sealed up: whereby I understood they were to be so kept by him till I had finished the whole, and was ready to print it. I considered

* This sum relates only to the *additional* expense which he had incurred in preparing the work for press, in consequence of the interference of the Prince. The particulars of these disbursements are given in the *Diary* inserted in the Appendix, No. 75. F. B.

† The initial only of this name is given in the MS, but there can be no question about the person intended. F. B.

‡ There are 4 copies (or draughts) of these articles (all unexecuted) amongst Flamsteed's MS papers, which are to be found in MSS, vol. 35, pages 63—73. These are similar to each other, and differ only in a few unimportant particulars. I have given in the Appendix, No. 111, that which is the most comprehensive; with Notes where there is any remarkable deviation from the rest. I cannot ascertain *which* was the one actually agreed upon. F. B.

also that this half of my catalogue would be of no advantage to him, and consented. I therefore delivered the copy of so much of the catalogue as was finished into Mr. Hodgson's hands, with orders to seal it up in Sir Christopher Wren's presence, and deliver it to Sir Isaac Newton when 10 sheets were printed, and £125 (which would then be payable by the Articles) should be paid me *. This was *March* 8th, 1706†: but this direction I waived afterwards; and it was put into his hands, the week after, without receiving a farthing for the board or pay of my amanuensis or calculators. For, *honest* Sir Isaac Newton (to use his own words) would have "all things in his own power," to spoil or sink them; that he might force me to second his designs and applaud him, which no honest man would nor could; and, God be thanked, I lay under no necessity of doing.

This business being over, a week after, meeting me in London, he told me he would now draw £800 of the Prince's money: but said nothing of paying me what I had disbursed. However, we must now put the work into the press: for, after such unreasonable concessions on my part, his pretences for further delay were all taken away; and he had no excuses for further delays.

April 4th, being in London, I was told that all the errors which he, by mistake, thought he had found in my copy, were quitted; and that the first sheets would go to the press this week.

April 19th. I waited upon him again: he told me gravely that, the Prince having subscribed a great sum to the Emperor's loan, the money could not be received; but that he had taken up money for Mr. Churchill. This was to provoke me: but he failed of his design. Whatever I had hitherto expended, I was content to adventure a little more. Mr. Churchill was put upon me; had never been at any expense; must have monies put into *his* hands beforehand to buy paper and pay the printer: whereby he was sure to have him at his command. And, though it was covenanted that he should print but 400 copies, might take as many as he pleased: for I never heard nor found that he had given any bond or security for his fair dealing: however it was highly reasonable he should. But, this was not all. The printer, being to be paid by the under-

* It is difficult to account for the motive which could have influenced Newton to have proposed and insisted upon this cautious step. It showed great suspicion of the person with whom he was dealing: and such conduct would not be tolerated or even attempted at the present day on either side between individuals of their rank in society. F. B.

† In the MS this is stated to be *April* 8th: but, on reference to the *Diary* and other documents, it is evident that Flamsteed has mistaken the month. F. B.

taker, and not by me, was likely to be careless of his work: which I urged, but to no purpose.

It was May 16th ere the first sheet was printed off; and June 3rd ere we got a second: and the third on the 7th of June. So here was a whole month since the first was wrought off; and not two sheets (in the room of 20 that, by the Articles, ought to have been printed) in a month's time. I complained boldly of the dilatoriness; but in vain. All the answer I got was from Sir Isaac's own mouth, "that we must proceed slowly at first, and make more dispatch after." This was one of the fruits of our having an undertaker, and leaving the printer to be paid by him, who neglected the *Historia Cœlestis* if they had but a sorry pamphlet to print.

We had got two alphabets (that is, about 46 sheets) out of the press by Christmas 1706; and the whole (5 E) or 97, before December 21, 1707: that is, 97 sheets in about 89 weeks. In which [time] had they printed 5 sheets per week, according to their Articles, all the observations made with the mural arch from 1689 to 1706, might have been easily printed, as well as those made with the sextant.

In the meantime Sir Isaac Newton sometimes stopt the press without assigning a reason for it, or any occasion given by me: but upon my complaint at the first, and afterwards, without any solicitation of mine at all, let it go on again. I happened once to visit the press when he was there, and took the opportunity to show him how ill the compositor had placed the types of the figures, and how much awry to the lines to which they belonged. (Sheet Kkk, page 224.) He put his head a little nearer to the paper, but not near enough to see the fault, (for he is very near sighted,) and making a slighting motion with his hand said, "Methinks they are well enough." This encouraged the printer in his carelessness: the sheet was printed off, and the fault not mended; and caused me to be more watchful over the printer. For, now it was plain to me that the referee, as he called himself, was not displeased with the faults he committed; and the undertaker never concerned himself about them. He was sure of certain gains by the paper and press-work; and something more, probably, than we were aware of*.

* The following entry appears in the *Journal* Book of the Royal Society; as connected with the history of this period: viz.

Jan. 8, 1706-7. "The President was desired to endeavour to get the MSS of Tycho Brahe to "be printed with Mr. Flamsteed's observations. These MSS are said to be in the King of Den-

The printing of the sextant observations being finished, I expected the press should have gone on, after Christmas, with the volume of observations made with the mural arch, which were double the number of the other. But, Sir Isaac Newton had put a full stop to the press, though he knew very well that the copy was ready, fairly transcribed on 175 sheets. What excuse he made for it, I know not; for none of his confidants would acquaint me *.

In the meantime I had complained to one of the referees, who was often at Court, and waited frequently on the Prince, of my ill usage; that care was taken of the undertaker and printer, but that none was taken to reimburse me in the entertainment and pay of three calculators, and in transcribing the copy for the press, which came to more than £173 †; though I accounted nothing for my own and my servant's attendance on the press. He was ashamed of it; promised it should be redressed; and I am apt to think procured a meeting to be appointed on the 20th of March following which was notified to me: and I was then desired to bring with me what I had more by me, ready for the press.

The press had now stood three months, by Sir Isaac Newton's only procurement ‡. For, to keep all things wholly in his own power, he had brought in an

" mark's library: and the President was desired, in the name of the Society, to address the Prince " in that matter." F. B.

* In the original MS there is a short paragraph inserted here which is evidently *out of place;* as it refers to the order of July 13, and the letter of July 19, 1708: both of which will be found in the sequel. F. B.

† This alludes to the *additional* expense mentioned in the note in page 81; and which had now accumulated to this amount. The particulars of the disbursements may be seen in the Diary, which is inserted in the Appendix, No. 75. F. B.

‡ For the more perfect understanding of the subsequent disputes and difficulties, the reader will bear in mind that the *first* volume of the *Historia Cœlestis* had now been printed off; and that in the progress thereof there seems to have been no difference of opinion as to the *manner* in which that part of the work was to be arranged and set up; although we find much complaint about the *delay*. In the arrangement of the subject matter of the *second* volume, however, it would appear that Flamsteed and the referees were completely at issue. Flamsteed, for reasons which he has very well explained, insisted not only that the catalogue should be printed under his own immediate superintendence and control, but that it should not be printed till *after* the observations: and moreover he naturally expected that the observations were to be printed in the order, and in the manner, registered in his books. On the first point he was decided; and would not yield to the importunities of the referees, who wished to prefix it to the first volume, now finished. On the second point, he appears to have been kept in ignorance of any intended alteration; and was consequently very much surprized and justly indignant, when he found that the referees had *garbled* his observations in a manner unworthy of the establishment over which he had presided with so much honor, and dis-

undertaker who was useless to the business, and served only to spoil the work, or worse; and a printer whom I believe he paid. I am sure he never consulted me about the payment of either, though there was sufficient cause; all the articles relating to them having been broken: but by this management he had them wholly at his devotion. On the day appointed (March 20, 1707-8) I took up with me to London, all the observations here made betwixt September 1689 and December 1705, fairly copied in 175 sheets of large paper. Six sheets were of the planets' places, calculated from the observations made with the sextant, which ought to have been printed next after the said observations; as also a fair copy of the places of the stars in the ecliptical, and as many of the southern constellations, as I had then rectified. The referees viewed them: and Sir Isaac Newton, after some time, withdrew; and calling Dr. Arbuthnot out to him, produced the following paper which the other referees, as I remember, signed. He would not deliver it to me, but *graciously* permitted me to take a copy of it, which I have here inserted*.

London, March 20, 1707-8.

It is agreed betwixt Sir Isaac Newton and Mr. John Flamsteed, 1°. That the second volume of the astronomical observations, with the figures of the first volume, shall be presently delivered into Sir Isaac Newton's hands.

2°. That the catalogue of the fixed stars, here present, shall likewise be delivered into Sir Isaac Newton's hands.

3°. That the catalogue of the fixed stars, now in Sir Isaac Newton's hands, shall be delivered to Mr. Flamsteed in order to have the magnitudes inserted, and to be returned with the magnitudes after sixteen days.

4°. That upon the delivery of that catalogue, Sir Isaac Newton shall pay to Mr. Flamsteed, one hundred and twenty-five pounds on the Prince's account.

5°. That upon the delivery of the catalogue of the fixed stars, as far as it can be completed at this time, Mr. Flamsteed shall have the rest of the money stipulated betwixt him and the referees: he undertaking to correct the press, and appointing correctors who live in town, that the work may not be retarded.

Underneath the copy I had wrote the following memorandum, "That at the "same time the 2nd volume of the observations with the figures herein men-

graceful to the scientific character of the nation. He knew too well the advantage of having *all* the observations published in detail. The Prince's death put a temporary stop to these proceedings: but they were afterwards revived in full force, in the year 1711. F. B.

* This copy is taken from the original draft, which is to be found in MSS, vol. 35, page 81. There is also another copy of it (identical with the present one) in MSS, vol. 33, page 68, and which seems to be that which is alluded to by Flamsteed. The name of Sir Christopher Wren, however, does not appear amongst those who are stated by Flamsteed (see page 86) to have been present at the meeting. F. B.

" tioned, was left in Sir Isaac Newton's hands, together with a corrected copy " of the ecliptical constellations, and all the southern of the catalogue. But, that " I covenanted that the said 2nd *copy* * should be returned to me to be again " revised, and delivered to the press, as the printers should work it off; and the " correct copy of the ecliptical constellations returned me, as soon as I should " return the copy now in Sir Isaac Newton's hands, with the magnitudes in- " serted."

There were present at this meeting, at the Castle Tavern, in Pater-Noster Row, Mr. Roberts, Sir Isaac Newton, *Sir Christopher Wren* †, Dr. Arbuthnot, Dr. Gregory, Mr. Churchill, Mr. James Hodgson, and with myself, my amanuensis Isaac Wolferman.

The conditions, on which I was to deliver this second volume, were very hard and unjust: for the observations contained [there]in were most of them made with the new mural arch, which I had built at my own cost, and lay me in above £120 out of my own pocket. My other instruments were all my own too: and my assistants were paid and maintained at my own charge. I had laid out moreover above £173 in carrying on the work; of which I had given a bill both to Sir Isaac Newton, and several of the referees ‡. I considered that, if I should not consent to this order, Sir Isaac Newton [would say] that I had hindered the printing of my own works myself: which would serve to justify a report, spread by his partisans very industriously, that I was averse to the publication of them. Whereas I had always endeavoured to carry them on as advantageously as I could: and he had done all he could to hinder me, in order to make me comply with them, and cry him up at the same rate they did. Further, I saw that if I did not lay hold of this opportunity, I could not hope to be reimbursed any part of the £173 I had spent in preparing the copy for the press, and performed my part of the agreement in the time agreed. But the £125 was not paid me till above 2 months after; and then I was still above £48 out of purse: for which I had nothing but 3 copies,—one that I gave Mr. Sharp, and another in which I have corrected the faults of the press with my own hand, and a third not complete.

I was now in hopes that the press would begin again to work with the 2nd volume: but, when after 3 or 4 months' delay I found that, for all my instances, there was not the least step made towards it, I complained of this behaviour of Sir Isaac Newton, both paying me short of what I had disbursed, and of his

* In the MSS, vol. 33, page 68. it is " 2nd *volume*." F. B.

† See the note in page 85. F. B.

‡ See the note in page 84. F. B.

keeping the 175 sheets of copy for the second volume in his hands. This I believe was (as intended) carried to him. Whereupon, to throw all the fault upon me, 8 months after he had stopt the press, he sent me the following order * :

> At a meeting of the gentlemen to whom his Royal Highness the Prince hath referred the care of printing Mr. Flamsteed's astronomical papers,
>
> It was agreed that the press should go on without further delay: and that if Mr. Flamsteed do not take care that the press be well corrected, and go on with dispatch, another corrector be employed.
>
> F. Roberts.
> *Whitehall, July* 13, 1708. Chr. Wren.
> Is. Newton.
> *Vera Copia* D. Gregory.
> Is. Newton. Fran. Ashton.

To prevent the designed effect of this malicious order or agreement, I wrote a letter to Sir Christopher Wren (who I believe hated such practices) and sent it him in a few days after. I declined writing to Sir Isaac Newton, because he might suppress it: and I doubted not Sir Christopher would impart it both to him and the other referees. I took a copy of it myself, to show my acquaintance, friends, and some gentlemen that had an opinion of Sir Isaac Newton before, and could not think he could be guilty of such collusion as this order and my letter proved upon him. The copy follows † :

> *The Observatory, Monday, July* 19, 1708.
>
> Sir,
>
> The copy of the agreement made by the gentlemen referees on Tuesday last, reflecting upon me as if by my dilatoriness I had obstructed the progress of the press, I find myself obliged, that I may clear myself of so unjust an insinuation, with your leave, to acquaint you,
>
> That though I had got 50 sheets of the first volume ready copied for the press on May 2, 1705, yet upon several pretences the printing was obstructed; and it was May, 1706, before the first sheet was printed off.
>
> That, by the agreement, the undertaker was to print off 5 sheets per week: yet it was from May 1706 to October 1707 before we could get 100 sheets, comprehending the observations of the first volume, wrought off; that is, near 75 weeks. So that, taking all together, the printer dispatched not a sheet and a half per week: though I did all I could to expedite the work, as will appear by the copy of my letters to Mr. Churchill, Mr. Mathews, and Mr. Hudson, that I have by me. I offered to discharge the expense of penny post letters that brought the proofs. If the post brought them in the evening, I returned them next morning:

* The original order here alluded to, in Sir Isaac Newton's own handwriting, is bound up in MSS, vol. 35, page 83. That which is here given by Flamsteed is an exact copy. F. B.

† The original draft of this letter in Flamsteed's handwriting is in MSS, vol. 35, page 85. F. B.

if in the morning, they were sent back that evening without fail; except once, on May 1, 1706, when the great eclipse of the sun happening, company hindered me from correcting and returning the proof till the morning following, and no longer.

The greatest dispatch was made both this year 1706, and the following 1707, in autumn; when I was absent in Surry. Yet that was less than the five sheets per week; and then the work was always worst done. At my return after the last year's harvest, I found a whole sheet had been omitted by the printer, who had either lost or mislaid it. I copied it immediately from my manuscripts and sent it to him, with directions to print it, and reprint the next. I caused also Sir Isaac Newton to be acquainted with it: and informed both Sir Isaac and the printer that I had about half-a-dozen sheets more, comprehending the planets' places, derived from the observations made with the sextant, contained in this volume, to be added to it. But, this was not taken notice of. The six sheets were not called for; and the press has stood still ever since.

March 12th last I received a letter from Mr. Roberts, yourself, and Sir Isaac Newton, desiring me to meet them in London on the 20th; and bring with me what papers I had ready for the press. I attended them with the 2nd volume containing the observations made between September 1689 and 1705 complete; in about 175 sheets of paper. I exhibited also at the same time the forementioned six sheets that were to be added to the first volume; desired that the dropt sheet might be printed, and the next following reprinted; or at least the two first pages of it, which I thought then had been accorded. The 2nd volume by agreement was put into the referees' hands. I desired the press, after the first volume was complete, might go on with it. At this meeting also I had £125 ordered to be paid me, in part of above £170 it had cost me in paying and entertaining three calculators and copiers, whom I had dismissed for want of it at Midsummer 1706. Sir Isaac Newton required that I should insert the magnitudes of the fixed stars into a copy of so much of the catalogue as I had gone through with, that I had deposited in his hands: which was done for him, and part of a third more perfect copy left in [his] hands, as a gage for returning it*.

At this meeting the undertaker urged to have a corrector appointed in London. This I looked upon only as a contrivance to throw the delays of the press, caused partly by his own and his printer's neglect, upon me: and therefore having answered it then, as I have done in this paper, to the satisfaction, as I thought, of the referees present, I took no further notice of it.

Since you now know that the printer has had the dropt sheet in his hands full nine months; that he may have six sheets more whenever the referees please; that they have also 175 sheets of the 2nd volume in their hands; that I never delayed correcting and returning the proof sheets; and that I am ready to take care of the correction of the last proofs as usual; I hope you are satisfied I have not been guilty of any dilatoriness or neglect; and that you will not suffer me to be supposed or insinuated to have been guilty of any.

But, if Sir Isaac Newton insists upon proceeding to print the catalogue immediately before the second volume, I cannot at present consent to it†: for, since the press has stopt, I have

* This continued suspicion is unaccountable, unwarrantable, and extremely revolting. The parties had better not have met, if they could not place greater confidence in each other. F. B.

† The subsequent conduct of the referees shows that Flamsteed was right in insisting upon this course. See, on this subject, the note in page 84. F. B.

set myself to complete it; and having got two paid servants to help me have perfected some constellations that were not complete before. I have begun the most difficult: and am going into the country, as I use always to do at this time of the year, to look after my occasions there, I hope to perfect a good part of what remains, and the whole in a few months after my return. Now you will say yourself, were it your own case, 'tis not fit to set to printing the catalogue before it be as complete as I can render it at present. I must say further, that 'tis altogether improper to print it before the observations of the second volume: because 'tis almost wholly derived from them. The observations of the planets are much more numerous than in the first; and, I will add, much exacter. And, if any one be of another opinion for want of experience, I shall bring such proofs of it as no equal and candid person shall ever resist.

As for correcting the press, I am altogether unwilling that the last sheets shall be printed off in the remaining volume, till I have seen them myself. But, the catalogue is of that importance that I shall never consent that any page of it should be printed off till I have fully corrected and received from the press a proof without faults.

I am as willing as you can be that the press may proceed: but to have it hurried on at this time when I cannot possibly look after it, and only to find a printer in work who at other times has neglected it, would be a piece of folly for which I am confident all the referees would condemn me. I must therefore entreat them that this resolve be suspended till my return out of the country; when, God sparing me life and health, I hope, with the assistance of the referees, to put the press into such a method as it may have no stops, if any heed may [be] given to my advice.

I beg your pardon for so long a letter: the occasion has forced me to be more troublesome than I ought to one of your age and employment. If you excuse me now I hope no further occasion will be of repeating it: and I shall ever own myself, Sir, your most obliged and humble servant,

JOHN FLAMSTEED, M.R.

This letter was delivered and imparted to Sir Isaac Newton, as I desired it should be: yet I never received any answer to it. But the press was stopt; and no more talk of it this year: in the latter end of which the Prince of Denmark died, on October 28, 1708; in whom the Observatory lost one that would have been a great and noble patron, had he not been prevented by one of his physicians, who was influenced and governed by Sir Isaac Newton *.

Being now not disturbed by him any more at present, I set myself to carry on such observations as I wanted, and made good advances in it; adding many stars to some constellations that I had gone through before. But, when I least expected it, I was afresh disturbed by another piece of Sir Isaac Newton's ingenuity †. After the Prince's death, the old ministry was changed; a new one

* Dr. Arbuthnot is evidently the person here alluded to. F. B.

† I apprehend that, on the death of the Prince, the office and duties of the referees ceased. For, in the first place, we hear no more of them after this event: and secondly, there has been recently

introduced: his patron was well with the chief of them, the Queen's physician was in his interest, and the new Secretary of State's. It was not enough that Sir Isaac Newton had got my observations (made with the mural arc) into his hands by surprise, together with above half the catalogue, whatever my expenses had been, or pains in making it, so long as I would not leave myself and pains wholly at his disposal: and therefore he procures, by the means of the Physician Minister, and Secretary St. John, an order constituting the President (Sir Isaac Newton) of the Royal Society, the Vice President, and whom else they should think fit of the Society, *Visitors of the Observatory*. 'Tis dated December 12th, 1710, and was sent me by the office-messenger on the 14th, with the Queen's letter intimating it: copies whereof I have here inserted.

> To our trusty and well-beloved the President of our Royal Society for the time being.
>
> ANNE R.
>
> Trusty and well-beloved, we greet you well. Whereas we have been given to understand that it would contribute very much to the improvement of Astronomy and Navigation, if we should appoint constant *Visitors* of our Royal Observatory at Greenwich, with sufficient powers for the due execution of that trust, We have therefore thought fit, in consideration of the great learning, experience, and other necessary qualifications of our Royal Society, to constitute and appoint, as we do, by these presents, constitute and appoint you, the President, and in your absence the Vice-President of our Royal Society for the time being, together with such others as the Council of our said Royal Society shall think fit to join with you, to be

discovered, in the hands of Dawson Turner, Esq. of Great Yarmouth, the official account of the disposal of the Prince's money, delivered in upon oath, by the Trustees; which I presume closed their duties. I am indebted to the Rev. W. Whewell for procuring me a copy of this document, which is inserted in the Appendix, No. 146. It will appear, in the sequel, that the Royal Society became now the managers of this affair: but, as Newton was the Director in either case, the system was not changed.

I would here remark, as connected with the order of events at this period, that on Novem. 9, 1709, the Council of the Royal Society ordered Flamsteed's name to be left out of the list of Fellows, on account of his not having paid up his arrears; although, in the course of that very year, Sir C. Wren, Dr. Halley, Dr. Lister, Dr. Smith, Mr. Lowthorp, and 7 other Fellows had been excused from such payments: and although Sir Isaac Newton, Sir Hans Sloane, Dr. Wallis, and many even of the *nobility* had been favoured (some of them more than once) in a similar manner, in other years. These facts may be ascertained by a reference to the Minute Books of the *Council* of the Royal Society.

And here I would take the opportunity of correcting an error, into which all the editors of Newton's life appear to have fallen; where they have converted this fact, of his being excused his weekly payments to the Royal Society, into a proof of the *low state of his finances* at that period. Whereas it seems to have been no uncommon event for the Society, in the early years of its existence, to remit the arrears of members (even of those in high stations) either on application from the members, or on account of some other circumstances with which we are now unacquainted. F. B.

constant *Visitors* of our said Royal Observatory at Greenwich: authorising and requiring you to demand of our Astronomer and Keeper of our said Observatory, for the time being, to deliver to you within six months after every year shall be elapsed, a true and fair copy of the annual observations he shall have made*. And our further Will and Pleasure is that you do likewise, from time to time, order and direct our said Astronomer and Keeper of our said Royal Observatory to make such astronomical observations as you in your judgment shall think proper. And that you do survey and inspect our instruments in our said Observatory; and as often as you shall find any of them defective that you do inform the principal Officers of our Ordnance thereof†; that so the said instrument may be either exchanged or repaired. And so we bid you farewell. Given at our Court of St. James's, the 12th day of December, 1710, in the ninth year of our reign. By Her Majesty's Command,

H. St. John.

Sir, *Whitehall, December* 12, 1710.

Her Majesty commands me to acquaint you that she has thought fit, for the improvement of astronomy and navigation, to appoint the President, and in his absence the Vice-President of the Royal Society for the time being, together with such other as the Council of the said Society shall think fit to join with them, to be constant *Visitors* of the Royal Observatory. And for the better enabling you to make the necessary observations for these ends, directions are likewise given for repairing, erecting, or changing Her Majesty's instruments in the said Observatory, as well as for purchasing those that belong to you‡.

* The Council of the Royal Society did not lose much time before they commenced their new duty: for we find that on the very day that they received the Warrant, they appointed a Committee of Visitors. The following is an extract from their Minute Books, viz.:—"Decem. 14, 1710. Dr. "Arbuthnot delivered the Queen's letter impowering the President, &c., to be Visitors and Directors "of the Observatory. [The letter is dated Decem. 12, 1710.] The President, Dr. Sloane, Dr. "Halley, and Dr. Arbuthnot were desired to acquaint the Secretary with how much thankfulness "the Society received this letter from Her Majesty.

"The President, Mr. Roberts, Dr. Arbuthnot, Dr. Halley, Dr. Mead, Mr. Hill, Sir Christopher "Wren, Mr. Wren and Dr. Sloane were ordered a Committee to go to Greenwich, any three of them "(of which the President or Vice-President to be one) to be of the Quorum, and to report their "opinion of the condition of the Observatory and the instruments therein, and to take an inventory "of the instruments." F. B.

† Besides this injunction, it appears (from Mr. Secretary St. John's letter to the Board of Ordnance, dated the same day as the Queen's Warrant) that "Her Majesty is likewise pleased to direct "that you should have regard to any *complaints* the said Visitors may make to you of the *misbeha-* "*viour* of Her Majesty's Astronomer and Keeper of the said Observatory, in the execution of his "office." This letter does not appear to have been known to the Royal Society, till Decem. 24, 1713, (upwards of 3 years after it was written,) when it was forwarded to them by Mr. Secretary St. John; and read in open Council by the President, on Feb. 18th following. See these documents in the Appendix, Nos. 188 and 190. F. B.

‡ The fact was, that the Queen had no instruments at the Observatory: they *all* belonged to Flamsteed. F. B.

The Queen does not doubt but you will readily comply with the instructions the said Visitors shall think fit to give you. However I am commanded to signify Her Majesty's pleasure to you that you do deliver to them, within six months after every year shall be expired, a fair and true copy of the annual observations you shall have made: and you do also make such astronomical observations as the said Visitors in their judgment shall at any time think fit to direct you. I am, Sir, your most humble servant,

H. ST. JOHN.

The next morning after I received this, I waited on Mr. Secretary St. John and told him that I was injured; and should be hindered by this new constitution of *Visitors:* that I wanted no new instruments; and that, if I did, the Visitors were not skilful enough to contrive them: that for my repairs of the Observatory *, the Office of Ordnance had hitherto taken care of them, and would now, as soon as the weather should be fit: that the instruments and clocks in the house were all my own, and that I had hitherto repaired them all at my own charge: that I had expended above £2000 more than my appointments in instruments and assistants: and that it would be very unjust to go about to deprive me both of the honor and benefit of my own labor and expenses, and confer them on those who had done nothing but obstruct and hinder me in all they could, and wanted to boast of their merit in preserving my labors, because they had nothing of their own worth the public view. Mr. Secretary St. John seemed not to regard what I said, but answered me haughtily, "The Queen would be obeyed." The Lord Rochester, the Queen's uncle, living near the Secretary's office, I also waited upon him; and showed him what tricks and disingenuous usage were put upon me by Sir Isaac Newton: and though I found no immediate advantage by it, yet I am apt to believe it was of use to me afterwards †.

Sir Isaac Newton valued himself very much upon the suggestion that it "would contribute very much to the improvement of astronomy and navigation "if there were constant Visitors appointed of the Observatory, &c:" and one of the principal of the Council of the Royal Society could not forbear to speak of it to me in public company. Whereas the contrary is evident from what happened to the noble Tycho, who had no Visitors of his Observatory appointed over him during the reign of his patron King Frederic II. When some persons were appointed in the following reign of King Christian, they were such as were very unfit for that purpose, much less skilful than himself, and

* That is, the repairs of the *building*, not the *instruments*. F. B.

† Amongst Flamsteed's manuscripts, there is the draught of a remonstrance to the Queen, upon this subject: but whether it was actually sent to her, or not, I have not been able to ascertain. A copy of it is given in the Appendix, No. 149. F. B.

made use of purposely to asperse him only, to make him uneasy and withdraw, that the courtiers might get his appointment (which were 2000 dollars a year allowed him from the treasury, a fee in Norway worth 1000 dollars a year more, and the prebend of Roschild, of 1000 more) into the King's hand again, which they did; and soon, by him, were conferred on the Templars. My appointments, though very small in comparison of his, were also designed by Sir Isaac Newton for other persons that would be dependent on him; and this expedient of Visitors was to perform strange things. But the good Providence of God so ordered it, that I received but little damage by it: and he got little but shame and disgrace for his ingratitude to me in disturbing me in my business, which he was bound by his oath to assist me in, as President of the Royal Society, and as chief (as he had made himself) of the Prince's referees, or indeed *the all of them.*

But now that he got another pretence of authority to make me sensible of it, a report was spread that a letter was coming to me from the Royal Society. This was in the beginning of December 1710; and was occasioned, I believe, by their knowing of Mr. Secretary St. John's letter, that was brought to my hands on the 14th. I heard nothing of any letter from them: if they then designed any, I believe on better thoughts it was laid by. But in March following I was surprized when I was privately told that my catalogue (which I was then working upon to complete it as far as I then could) was in the press: but more with a letter of Dr. Arbuthnot, dated the 14th of March, 1710-11, wherein he very confidently required of me the copy of the stars' places of 6 constellations, viz. Draco, Ursa Major, Ursa Minor, Cepheus, Cassiopea, and Hercules, that had not been delivered into Sir Isaac Newton's hands when he got the rest into his possession by tricks and pretences. This I believe was one of the boldest things that ever was attempted. None that had less dexterity and boldness and art than the Doctor would have had the confidence to have mentioned such a demand *. I had

* Hitherto, the whole of the arrangements relative to the printing, and the consequent remonstrances and reproaches, had been confined to Flamsteed and the Referees: the Royal Society do not appear (as a body) to have been concerned in the matter, since the first recommendation to the Prince. But, from this time forward we find them so mixed up with the business, as to lead to a supposition either that a fresh order had been made by the Queen, appointing the Society the Referees in this affair; or that the President and Council had assumed that office, by virtue of Her Majesty's recent Warrant appointing them Visitors of the Royal Observatory. Dr. Arbuthnot, in his letter quoted in the text, states that *he* is commanded by the Queen to take care of the publication: but this duty appears to have been very soon transferred to the Royal Society. For it is a fact that this very letter of Dr. Arbuthnot's (a copy of which is given in the Appendix, No. 151) was the subject

The Queen does not doubt but you will readily comply with the instructions the said Visitors shall think fit to give you. However I am commanded to signify Her Majesty's pleasure to you that you do deliver to them, within six months after every year shall be expired, a fair and true copy of the annual observations you shall have made: and you do also make such astronomical observations as the said Visitors in their judgment shall at any time think fit to direct you. I am, Sir, your most humble servant,

H. ST. JOHN.

The next morning after I received this, I waited on Mr. Secretary St. John and told him that I was injured; and should be hindered by this new constitution of *Visitors:* that I wanted no new instruments; and that, if I did, the Visitors were not skilful enough to contrive them: that for my repairs of the Observatory *, the Office of Ordnance had hitherto taken care of them, and would now, as soon as the weather should be fit: that the instruments and clocks in the house were all my own, and that I had hitherto repaired them all at my own charge: that I had expended above £2000 more than my appointments in instruments and assistants: and that it would be very unjust to go about to deprive me both of the honor and benefit of my own labor and expenses, and confer them on those who had done nothing but obstruct and hinder me in all they could, and wanted to boast of their merit in preserving my labors, because they had nothing of their own worth the public view. Mr. Secretary St. John seemed not to regard what I said, but answered me haughtily, "The Queen would be obeyed." The Lord Rochester, the Queen's uncle, living near the Secretary's office, I also waited upon him; and showed him what tricks and disingenuous usage were put upon me by Sir Isaac Newton: and though I found no immediate advantage by it, yet I am apt to believe it was of use to me afterwards †.

Sir Isaac Newton valued himself very much upon the suggestion that it "would contribute very much to the improvement of astronomy and navigation "if there were constant Visitors appointed of the Observatory, &c:" and one of the principal of the Council of the Royal Society could not forbear to speak of it to me in public company. Whereas the contrary is evident from what happened to the noble Tycho, who had no Visitors of his Observatory appointed over him during the reign of his patron King Frederic II. When some persons were appointed in the following reign of King Christian, they were such as were very unfit for that purpose, much less skilful than himself, and

* That is, the repairs of the *building*, not the *instruments*. F. B.

† Amongst Flamsteed's manuscripts, there is the draught of a remonstrance to the Queen, upon this subject: but whether it was actually sent to her, or not, I have not been able to ascertain. A copy of it is given in the Appendix, No. 149. F. B.

made use of purposely to asperse him only, to make him uneasy and withdraw, that the courtiers might get his appointment (which were 2000 dollars a year allowed him from the treasury, a fee in Norway worth 1000 dollars a year more, and the prebend of Roschild, of 1000 more) into the King's hand again, which they did; and soon, by him, were conferred on the Templars. My appointments, though very small in comparison of his, were also designed by Sir Isaac Newton for other persons that would be dependent on him; and this expedient of Visitors was to perform strange things. But the good Providence of God so ordered it, that I received but little damage by it: and he got little but shame and disgrace for his ingratitude to me in disturbing me in my business, which he was bound by his oath to assist me in, as President of the Royal Society, and as chief (as he had made himself) of the Prince's referees, or indeed *the all of them.*

But now that he got another pretence of authority to make me sensible of it, a report was spread that a letter was coming to me from the Royal Society. This was in the beginning of December 1710; and was occasioned, I believe, by their knowing of Mr. Secretary St. John's letter, that was brought to my hands on the 14th. I heard nothing of any letter from them: if they then designed any, I believe on better thoughts it was laid by. But in March following I was surprized when I was privately told that my catalogue (which I was then working upon to complete it as far as I then could) was in the press: but more with a letter of Dr. Arbuthnot, dated the 14th of March, 1710-11, wherein he very confidently required of me the copy of the stars' places of 6 constellations, viz. Draco, Ursa Major, Ursa Minor, Cepheus, Cassiopea, and Hercules, that had not been delivered into Sir Isaac Newton's hands when he got the rest into his possession by tricks and pretences. This I believe was one of the boldest things that ever was attempted. None that had less dexterity and boldness and art than the Doctor would have had the confidence to have mentioned such a demand *. I had

* Hitherto, the whole of the arrangements relative to the printing, and the consequent remonstrances and reproaches, had been confined to Flamsteed and the Referees: the Royal Society do not appear (as a body) to have been concerned in the matter, since the first recommendation to the Prince. But, from this time forward we find them so mixed up with the business, as to lead to a supposition either that a fresh order had been made by the Queen, appointing the Society the Referees in this affair; or that the President and Council had assumed that office, by virtue of Her Majesty's recent Warrant appointing them Visitors of the Royal Observatory. Dr. Arbuthnot, in his letter quoted in the text, states that *he* is commanded by the Queen to take care of the publication: but this duty appears to have been very soon transferred to the Royal Society. For it is a fact that this very letter of Dr. Arbuthnot's (a copy of which is given in the Appendix, No. 151) was the subject

made my instruments and maintained my assistants at my own charge, without complaint of it, so long as I could be quiet and undisturbed by the small people that cried him up. I had put a copy of that part of my catalogue which was in order, into his hands, to be preserved in case of my mortality, and to prevent it from being lost by accidents, and to let him see that I could go on with it as soon as I had determined the right ascensions and distances from the pole, of other stars in other constellations. I gave him also copies of them: never designing or intending that he, or any but myself, should publish them. Nor indeed could any one else: for more observations were still wanting to complete it; and I was adding to it, adding or correcting something in it every day. Some letters passed betwixt me and the Dr. Arbuthnot*; wherein he still urged me to give them the copy of the constellations only wanting, as he thought, to complete my catalogue: which I always answered civilly with such just excuses as are before suggested; desiring still that I might see him either at the Observatory or in London, where at last he met me on March 29th, and when I enquired of him whether the catalogue were printed, or no, he assured me "not a sheet of it "was printed." I answered him not, for I was sure it was †; because he then

of no less than four several nights' debates at the ordinary meetings of that Society, as will be seen by the following extracts from their Journals, viz.:

"Feb. 21, 1710—11. The President in the Chair. Dr. Sloane was ordered to write a letter to "Mr. Flamsteed, desiring him to furnish the deficient part of his catalogue of the fixed stars, now "printing *by order of the Queen*; for the publication of which the Society had addressed His "Royal Highness the late Prince of Denmark: part of which is already printed.

"Feb. 28, 1710—11. Dr. Halley was desired to wait on the President to have his thoughts "about the letter to be wrote to Mr. Flamsteed.

"March 7, 1710—11. The copy of a letter to be sent to Mr. Flamsteed was read, approved "of, and ordered to be sent to him by Mr. Hunt; if it shall be approved of by Dr. Arbuthnot, "who had directions in it.

"March 14, 1710—11. The President in the Chair. Dr. Arbuthnot having given him a letter "to Mr. Flamsteed, to the same purpose with that designed by the Society, it was ordered to be "sent to him by Mr. Hunt."

These occurrences appear to have taken place at the *ordinary meetings* of the Society; and were consequently considered as part of the regular business of the day. The proceedings were evidently conducted with great circumspection and caution, as if some opposition was expected. It does not appear that there was any other *order of the Queen*, than Dr. Arbuthnot's letter above-mentioned. F. B.

* All the correspondence which I have been able to find, relative to this subject, will be found in the Appendix, Nos. 151—162. F. B.

† Flamsteed was right: and it seems scarcely possible that Dr. Arbuthnot could have been igno-

offered (in the hearing of Mr. Hodgson and another gentleman I had taken with me to be a witness of our conversation and discourse) to pay me £10 for every press fault I should find in it *: and within four days after, a friend sent me the constellations of Aries and Taurus fairly printed; and, in a day or two after, that of Virgo. So that I was now convinced that the press was at work, and that the Doctor had told me *what he knew was not true.* I learnt at the same time (what had been intimated to me before) that Mr. Halley took care of the press, and pretended that he had found many faults in my catalogue, showed some sheets of it publicly in Child's Coffee House at St. Paul's, and boasted what pains he had been at in correcting them.

I had told Dr. Arbuthnot in one of my letters (April 18, 1711†) that one of Dr. Halley's best friends, and the wisest of them, had said of him "that the "only way to have my business spoiled effectually, was to trust it to his manage-"ment." Now the truth of this expression was proved: for I found not only the names of the stars in my catalogue altered, but the numbers also in many places changed, and others put in their room that were sometimes 15 minutes false; and therefore it was very effectually spoiled. And by boasting of these corrections, as he called them, he would insinuate to the world, that they were more obliged to him for his pains in correcting, than they were to me for above 30 years spent in composing it, the cost of making instruments, and hiring assistants at my own charge. For, by altering the names (to make them agree with his own faulty hemisphere) he had made himself in some sort (but a very bad one) a proprietor in that catalogue he printed without my name to it, or ever consulting me about it: which I would never consent to, as they well know by my letter to Sir Christopher Wren, which had been imparted to Sir Isaac Newton; and Halley was not ignorant of it ‡.

On June 23, 1711, he delivered to my niece, Mrs. Hodgson, a fair copy of all

rant of the fact. Some gross deception was evidently carrying on: and Flamsteed was justified in breaking off all negotiation with parties that could act in this manner. Nevertheless it appears that he was still willing to abide by his original agreement with the referees. F. B.

* Flamsteed drew up a statement the very next morning, of what took place at this interview with Dr. Arbuthnot; which is given in the *Diary* inserted in the Appendix, No. 75. F. B.

† I have not been able to discover this letter. F. B.

‡ In order to preserve the chronological chain of events, I would here remark that the Royal Society on May 24, 1711, ordered Mr. Flamsteed to observe the ensuing eclipse of the sun: and on May 30, 1711, the Council of that Society demanded of him his astronomical observations for the preceding year. See the documents relative to these subjects, in the Appendix, Nos. 164 and 165. F. B.

the sheets of the catalogue*; but without any preface to it. When I examined it, I found more faults in it, and greater, than I imagined the impudent editor either could, or durst have committed. He had taken no care to put those into their proper order which I had left digested to his hands; because I had not yet got occasion to complete the constellations to which they belonged: particularly the stars of Hevelius's new constellations, with Hercules, Cassiopea, and the two Bears. In some places he had altered the stars' right ascension and distance from the pole; and made them false which were true before: and in the constellation Draco there were not above 6 or 8 stars that he had not corrupted. Besides, I had added above 30 stars to the constellation; as many to Hercules; and so many on others, that the total number of them in my own catalogue would be near 400 more than there were in those papers I had intrusted Sir Isaac Newton with, to preserve in case of accidents: and which he had betrayed into Halley's hands, when he had been told of his qualifications before. Therefore finding no other remedy, I resolved to reprint it at my own charge†. I procured a couple of expert calculators (Mr. Ab. Ryley and Mr. Crosthwait), corrected his faults and blunders, got the places of the stars, lately observed, calculated by both of them for greater certainty, made a new copy in which the ancient names were restored, Hevelius's constellations inserted amongst the rest in their proper places, and in the order I first designed. But paper was exceeding scarce and dear, because of the war with France, not yet over; which delayed the printing my intended edition, corrected and enlarged‡.

In the meantime Sir Isaac Newton summons me to meet him at the house of the Royal Society in Crane Court, October 26, 1711§; where I found him

* Flamsteed says that the catalogue was delivered to his niece; which, indeed, may be true: but there appears also to have been a letter of the same date delivered with it; a copy of which is inserted in the Appendix, No. 166. F. B.

† I have never yet been able to find a copy of this edition of the catalogue: but the MS from which I believe it was printed, still exists at the Observatory, and is preserved in MSS, vol. 27 C. That MS has evidently been used by the printer, and differs not only from Halley's edition of 1712, but also (though slightly) from the copy in the 3rd volume of the *Historia Cœlestis*. F. B.

‡ In the Appendix No. 170, I have inserted the draught of a Remonstrance which Flamsteed appears to have intended for the Queen. The date of April 16, 1712, is affixed to it: but I apprehend it must have been drawn up at an earlier period. F. B.

§ This was an intended meeting of the *Council* of the Royal Society, and not of the *Referees;* as appears by the following Minute, extracted from the *Council* Books of the Society, viz.:

"October 16, 1711, the President appointed a *Council* to be called on Friday come sevennight, "when Mr. Hunt is ordered to desire Mr. Flamsteed to meet the *Council* on that day at 11 o'clock,

with Dr. Sloane, Dr. Mead, and one more that I knew not, but I believe was his or their clerk at the time. He called these three, with himself, a Committee; and told me they had sent for me to know what repairs I wanted, or instruments. I told them that the Office of the Ordnance took care of my repairs*; that it was now too late in the year to set about them; but that as soon as the spring came, I should have that done which was necessary: and as for my instruments, they were all my own, either given to me by Sir Jonas Moore, or made by myself at my own charge, and always repaired at my own expense. And further that I would not suffer any one to concern themselves about repairing of my own instruments, in which and necessary assistance I had spent above £2000. The impetuous gentleman hereupon said, "As good have no observatory, as no in-"struments:" and soon, conceiving that I apprehended his design, and obviated it by my answers, broke out into a passion, and used me as I was never used before in my life. I gave no answers; but only desired him to be calmer and moderate his passion; thanked him for the many *honorable* names he gave me; and told him God had blessed my endeavours hitherto; that his wisdom was beyond the wisdom of men, and that I committed myself to him. Dr. Mead seconded him, unprovoked, in his ill language: but Dr. Sloane held his peace. I thanked him for his civility, permitted him to help me down stairs, and at the door met Halley, who had not been far off all the time; and I believe had heard Sir Isaac Newton show his best g**** †. It would be too long to give an account of it all: there is a longer in my old Book of Letters A, page 104, 105; where those who come after me will find it. I pray God forgive him: I do ‡.

I do not remember that I ever saw the observations of mine (printed § at the

"at their house in Crane Court in Fleet Street; to know of him if his instruments be in order, "and fit to carry on the necessary celestial observations."

Flamsteed attended: and a scene ensued, over which it were desirable that a veil should be thrown for ever. But the recent disclosure, by the discovery of two distinct sets of manuscripts relative to this subject, and belonging to two different owners, now prevents the possibility of its suppression. Fortunately for the Society, as a body, there were not enough members present (five) to form a Council: and consequently their pages do not record the humiliating scene. In fact, there is no entry of any proceedings of the Council on that day. F. B.

* That is, of the *buildings*. F. B.

† The remainder of the word is illegible. F. B.

‡ The account here alluded to in the letter book A (MSS, vol. 33, page 104) is inserted in the order of the *Diary*, in the Appendix, No. 75. Another account, written about 2 months after the event, in a letter to Mr. Sharp, dated Decem. 22, 1711, is also inserted in the Appendix, No. 168. F. B.

§ It would appear, from this circumstance, that the printing of the sheets, intended for the *second* volume of Flamsteed's observations, had been carrying on clandestinely, and in a secret manner. F. B.

same press with my corrupted catalogue) till 3 years after; when there were 300 copies of the printed edition of the observations given me (as they were designed) by King George *. The whole were intended for me by the Prince George of Denmark: but I was forced to be content with this part of them, and took them with thanks. I found them † as much corrupted as the catalogue: but, if God spares me life, I hope to present the world with a perfect edition of them; the editor having transcribed only the observations of the planets, and made a sorry and fallacious excuse for his omitting the observations of all the fixed stars that were not employed for finding of the planets' places ‡.

On the 18th of June 1712, the impudent editor, with wife, son, and daughters attending him, and a neighbouring clergyman in his company, came hither. I said little to him. He offered to burn his catalogue (so he called his corrupted and spoiled copy of mine, of which I had now a correct and enlarged edition in the press, and the second sheet printing off) if I would print mine. I am apt to think he knew it was so, and was endeavouring to prevent it. But to render his design ineffectual, I said little to him of it: so he went away not much wiser than he came.

Saturday, August 1, 1712 §, Sir Isaac Newton came himself, accompanied

* This fact, and the destruction of the garbled part of these copies, will be more particularly mentioned in the sequel. F. B.

† That is, the sheets intended for the *second* volume. F. B.

‡ The modern astronomer will enter into Flamsteed's feelings at thus finding all his observations garbled and mutilated, to serve one particular end. For the Referees, instead of printing his observations in detail, as practised at the present day, selected such only as tended to show the place of the moon or a planet when they passed the meridian: rejecting all the other observations of the stars, and the means of verifying and correcting the catalogue, as totally useless. This might be pardonable where it was an object to save expense: but ought not to have been adopted where no such excuse was to be pleaded. Fortunately for the science, Flamsteed viewed the subject in a more comprehensive manner: and to show his decided and fixed opinion upon this matter, he *burnt* the spurious edition, published at the expense of Government, as soon as he got it into his possession; and *at his own cost* printed a correct transcript of all his observations. F. B.

§ The *year* is interlined in the MS, and I suspect it is erroneous, and that it should be 1713. In the first place, the first day of August, in the year 1712, did *not* fall, and that in the year 1713 *did* fall, on a *Saturday*. Secondly I find the following entry in the *Journals* of the Royal Society, viz.:

"July 30, 1713. The President in the chair. The President acquainted the Society that on "Monday last, waiting upon her Majesty with a present of his *Principia Philosophiæ Naturalis* "*&c*, her Majesty was pleased, of her own accord, to express her desire that he, and the rest of "the gentlemen of the Society, would take care of Mr. Flamsteed's Observatory at Greenwich. "Whereupon it was ordered that Dr. Halley, Dr. Thorpe, Mr. Machin and Mr. Hodgson, or any "two of them (whereof Dr. Halley to be one) with Mr. Rowley, should go to Greenwich to

with Dr. Thorp, Mr. Machin, Mr. Rowley, and Mr. Hodgson, who had given me notice of their coming beforehand. I had provided Mr. E. Clark and Mr. Ryley to attend our conversation, and accompany them to view [the] house and my instruments, being a little lame myself with the gout. They had a view of what they pleased, except my library. I gave them a glass of wine. Sir Isaac promised to return me a Greek Ptolemy he had borrowed of me, and four volumes in quarto of the first Night-Notes *, which he had kept in his hands now about six years, to no other purpose but to show his authority and *good* nature; and returned [them] not till more than four years after, when I had commenced a suit against him for them.

This business being over, and Sir Isaac Newton finding that his visitation had not the effect he promised to himself, he took care to let me know, by the Secretary's letter †, as soon as the year 1711 was expired, that the Royal Society (my visitors) expected the copy of the observations of that year. I returned an answer to him that they should have them in the time prescribed by the order: and accordingly caused my amanuensis, Jos. Crosthwait, to transcribe and leave them at their house in Crane Court, some days before Midsummer 1712. I expected that they should have sent me a receipt for them: but *civil* and *just* Sir Isaac Newton esteemed it too great a favour for me. I did the same for the year following, on a second letter from the Secretary of the Royal Society. And in the next year 1713-1714 I found them both printed, abridged and so spoiled by the editor of my catalogue, that I would no longer own them for mine. The most material observations were omitted, and the rest so managed that it seemed to me he had designed to spoil them out of spite ‡. He had inserted some that

" examine the present state of the instruments there, and make their report to the Society. Mr. " Hodgson was desired to acquaint Mr. Flamsteed with her Majesty's commands; and also with " the Society's intention of applying to the Board of Ordnance, for the repairing such instruments " as may be defective, after they have sent some of their own members to inform themselves of " the present state of them."

This clearly points out the *year*. Besides, there are other interesting particulars connected with this visit, which are inserted amongst the documents in the Appendix, that verify the date here given. F. B.

* See the note in page 80. F. B.

† This letter is dated July 3, 1712, and is inserted in the Appendix, No. 171. The *original* is in MSS, vol. 35, page 129. F. B.

‡ Flamsteed is, I think, in error as to the motive. Unfortunately it appears to have been the opinion of Halley, and of Newton also, (or probably of the age in which they lived,) that it was not necessary to place on record more than the observation of the planet, and that of some star near it in right ascension, and nearly on the same parallel of declination: conceiving that the position of the

were imperfect; and given the right ascensions and distances of the planets from the pole, deduced from the observations; but not their longitudes and latitudes. This was too much drudgery for his acuteness, and who was used to procure what he published as his own, at easier rates.

After the same manner he got my observations of the year 1713 into his hands * ; abridged, spoiled, and printed them in his Transactions for the year 1715, No. 344. But the Queen deceasing before they could lay any claim to the next year's †, and their authority ceasing, I declined answering their further demand ; for their authority ceased. Yet, their confidence did not: and the editor [Dr. Halley] who now was one of their Secretaries, sent me a bold letter to demand them, as if he had never done me any injury; which I laid by me, and kept that year's from being spoiled ‡. How unfaithful he was in his copy I hope the skilful may see ere long: for my amanuensis, J. Crosthwait, is now copying the volume of observations that Sir Isaac Newton got by surprize into his hands, and has nearly finished it. And I hope I may live, through the blessing of God, to see it published, with the observations of 12 following years: but if his good Providence shall not continue my life so long, I trust my executors will do it according to the directions of my will §.

The last sheet of my corrected and enlarged catalogue was printed off, Decem. 5th, 1712: after which I designed to have had the press to proceed with the observations, from which it was derived, made with the mural arch. But, whatever instances I made to Sir Isaac Newton, to have the copy I had trusted into his hands to be printed, I could not prevail with him to return it. So I set myself to continue my observations at such times as were fit for them, and to calculate the planets' places, from such observations as I had made with it, and

planet (the great object of research at that time) would be determined thereby. See the note in page 98. But Flamsteed, who was a better practical astronomer, knew the advantage of recording every part of a day's work. F. B.

* The several letters that were sent by the Royal Society to Flamsteed, relative to his observations for the years 1712 and 1713, as well as the intermediate proceedings that took place in that Society relative to the Royal Observatory, are given in the Appendix, No. 177—181, 183—185, 188—193. F. B.

† The Queen died August 1, 1714. F. B.

‡ The following is an extract from the *Journals* of the Royal Society:—"May 10, 1716. The "President ordered that Mr. Flamsteed be sent to for his observations of the year ending at Christmas "1715, and the close of the year before; pursuant to the Queen's Warrant." See the letter which was sent, in Appendix No. 214. I cannot find that any separate order was sent in the year 1715. F. B.

§ *It has been done:* and forms the *second* volume of the *Historia Cœlestis*. F. B.

to correct the places of the planets' motions. In which, I bless God for it, though I had not the success I expected, yet I had such as gave me light, and will be of use to those that come after me; and may serve to perfect our knowledge of the heavens, wherein the height of wisdom is shown of our Creator; if, after me, there shall be any found that will prosecute these studies with the same sedulity, patience, and sincere love of truth that I have now for above these five and fifty years.

August 1st, 1714. King George succeeded to the crown of Great Britain. Soon after, a noble peer died, who, during his life, had supported Sir Isaac Newton *. The officers at Court were changed. The new Lord Chamberlain knew me well: and one, that was frequently employed by him, wrote to me that, through his means, I might get the printed copy of my observations, that had been designed for me by the Prince George of Denmark, into my hands with little trouble: the Lord Chamberlain having, by his office, the care of his library. I thanked God for so good an opportunity. My friend, with the Duke of Bolton, did his best: but, after all, we find the Lords of the Treasury had the power of disposing of them. Mr. Walpole † was first commissioner: Mr. Methuen unasked became my friend: Mr. Newport (now Lord Torrington) I [had] been acquainted with long since. I caused a memorial and petition, wherein my case was truly represented to them, to be drawn up and delivered. Whereupon, 300 copies were ordered to be delivered to me by the undertaker, Mr. Churchill; who, by his Articles, was bound to print but 400. I brought them down to Greenwich: and, finding both Halley's corrupted edition of my catalogue, and abridgment of my observations, no less spoiled by him, I separated them from my observations; and, some few days after, I made a *Sacrifice of them to Heavenly Truth:* as I should do of all the rest of my editor's pains of the like nature, if the Author of Truth should hereafter put them into my power; that none of them but what he has given away and sent into foreign countries ‡

* The Earl of Halifax, who died on May 19, 1715. See Flamsteed's letter to Sharp, dated July 9, 1715, inserted in the Appendix, No. 201. F. B.

† Afterwards Sir Robert Walpole. He it was that presented a copy of Halley's edition of Flamsteed's Observations to the Bodleian Library at Oxford: which caused that spirited letter of Mrs. Flamsteed to be written to the Vice-Chancellor (dated March 22, 1726) requesting him to eject it from the library. This letter is given in the Appendix in the order of the dates. F. B.

‡ See the official return relative to the disposal of the printed copies of Halley's edition not delivered up to Flamsteed; and Flamsteed's reasons why Sir Isaac Newton ought not to have the distribution of those copies, in the Appendix, No. 206 and 207. And here I would remark, that most of the second-hand copies of this edition, that are now to be met with for sale, are splendidly bound,

may remain to show the ingratitude of two of my countrymen, who had been obliged by me more on particular occasions, than any other mathematical acquaintance; and who had used me worse than ever the noble Tycho was used in Denmark. And I should have felt the effects of their malice and envy more had not the good Providence of Almighty God prevented them.

Whilst I was soliciting this affair in the Exchequer, Sir Isaac Newton was passing his accounts there, concerning the disbursement of the Prince's monies *. He would never own to me what the Prince allowed for the charge of printing; lest he should quit any part of that power he pretended (and he would gladly have me have thought him) to have had. I have heard that the Prince designed £1200 for the printing: Dr. Keil told me £2500, which I am apt to believe is true; the other £1300 being not less than the engraving of the maps of the constellations, and other figures, will cost. But here I learnt that Sir Isaac Newton's accounts specified £150 given to Dr. Halley for the pains he had been at in correcting (as he calls it) and publishing my catalogue: and to one of his servants, for assisting him in calculating the places of the stars, £30: so that Sir Isaac Newton had wasted £180 in spoiling of it. Besides, he told me that he had given £20 more to the poor Frenchman that drew and engraved the flattering figures for the frontispieces or capitals; upon his complaint that the first agreement was too hard a bargain. So that here was £200 of the Prince's money thrown away, only to show his liberality unnecessarily; which evidently proves his ignorance of the business. For, the catalogue was very correct, before his editor corrected it: and the designer or engraver of the frontispiece and capitals knew, no doubt, how to make a bargain for his pains. The editor and his calculator were both indigent: [and he]† found this way of relieving them, without any expense to himself, and making them open their mouths wide in crying him [up for] his liberality, as they had done before for his skill in what

with the Queen's arms on the cover of the book: which I presume are a portion of the 30 copies reserved by the Treasury, and one of which was sent by Sir Robert Walpole to the Bodleian Library at Oxford, as already mentioned in the preceding note. F. B.

* Flamsteed is wrong in considering this to be the disbursement of the *Prince's* money. That account had, long before, been delivered in, upon oath, by the *Referees*. See the note in page 88. The present account must have been the disbursement of the *Queen's* money; and consequently delivered in, and passed, at the Exchequer: probably by Sir Isaac Newton as President of the Royal Society. I have not yet, however, been able to get a sight of this account. F. B.

† These words, within brackets, are torn off from the MS, and are here supplied from the tenor of the context. F. B.

he is [no master] of. Whilst my amanuensis, J. Crosthwait, was at more pains in [correcting] their faults, and calculating the places of 400 stars [more] than were in my first copy, without any allowance [more] than the yearly wages I gave him.

Having thus got my own printed observations and catalogue into my own hands, I caused the observations of Mr. Gascoigne and Mr. Crabtree, made in Yorkshire and Lancashire in the years 1638-1642, together with my own made at Derby betwixt the years 1669 and 1675, which I had mentioned in my Estimate (as these were to compose a part of my first volume of Observations) to be printed in Latin: together with a small table for turning the parts measured by the micrometer (either in the longer or the lesser tube) into minutes and seconds of a degree. I also sent to Sir Isaac Newton to return me the 175 sheets trusted into his hands March 20th, 1708-9, to be printed. But, finding he delayed to restore, or even flatly denied to do it*, I set my amanuensis to copy them, in order to have them printed; that they might be published together with the catalogue in their proper order, which I had first proposed in my said estimate, and which I endeavoured always to preserve: whilst Sir Isaac Newton as pertinaciously contended to obstruct and break, that he might thereby force me to some mean submission to procure his consent. Though the work was nothing of it his, he had concerned himself with the Prince George of Denmark, without my consent, in the edition; and was so bold as by his creatures to intimate to me what he wanted: but the cunning failed him; the sheets will be copied in a short time; and I hope, if God spares me health one year more, I may see them all printed and fit to be published.

Having thus given the history of my observations of the fixed stars, and shown both what the true obliquity of the ecliptic, or the inclination of the earth's axis is, as the assertors of their motion would rather call it, and how it came to pass that I have met with so many obstructions and hinderances in the preparing the catalogue of them for the press, and publishing of it; having also shown how I determined the inequality of the earth's motion and the true places of some of the principal [stars in the catalogue], and from them all the rest inserted in it, I shall next give an account of such variations as may be caused in their right

* See the letters from Flamsteed to Sir Isaac Newton, demanding the restitution of these documents, dated June 30, 1715, and April 23, 1716, in the Appendix, No. 200 and 211. It is difficult to account for Newton's conduct in retaining these MSS to the evident injury of science: since Flamsteed was now desirous of printing the work at his own expense. Possibly the copy was mutilated in getting up the spurious edition; and could not be returned in its original state. F. B.

ascensions and distances from the visible pole by the *Parallax of the Earth's orbit.*

From my first year's observations of the pole-star's meridional distance from the vertex, I supposed that the parallax was sensible. Some observations, I had taken with the sextant, of the intermutual distances of bright fixed stars, had caused me to suspect it before: for, I found that I had them, at some times of the year, some little bigger than at others. But, the sextant being an unfixed instrument that required two persons to make use of it, and the air being changeable and different at different times of the year, and consequently the distances being more or less contracted by refractions according to the greater or less density of the air, or greater or less inclination of the planes, passing through the two observed stars, to the vertical circles falling upon them, it was very difficult to make any good conclusion from them. Continuing therefore my observations of the pole-star yearly, I found always a small but sensible difference betwixt those I took in September and the following months of each year: which argued a sensible parallax at that star. At that time Mr. John Caswell, the Savilian Professor of Astronomy, with whom I had been acquainted ever since the year 1680, kept a friendly correspondence with me. In one of my letters to him I gave him some account of these observations, and what I deduced from them. This letter he showed to Dr. Wallis, who was then reprinting an edition of his works in folio, and writing to me earnestly pressed me for a larger account of those observations*; which I soon drew up and sent him, and he printed. My letter was dated Decem. 22, 1698. This was the first time that any thing appeared in public on this subject: and therefore the French, who have boasted, by the pen of Mons. Fontenelle, their Secretary, that the world is either indebted to them for all the curious discoveries of the last productive century, or they have found the way of making all the inventions of it their own, found fault with my demonstration of it. And the younger Cassini published something about it in a preface to the *Hist. de l'Acad. des Sciences*, for the year 1699; which caused me to consider that subject anew in the year 1702: when, on the 22nd of November, I wrote a letter concerning it, wherein I explained it as follows:

Conceive the eye placed at an infinite distance in the plane of the Earth's orbit A, B, C, D, (Fig. A): it will be represented by the line A C, its diameter; and let &c, &c, &c.

[As there is no figure annexed to this description, and as the description

* See some of this correspondence in the Appendix, No. 39—46. F. B.

itself is, shortly after, broken off abruptly, by a reference to another paper *, it is needless to proceed further with this fragment; which terminates with the following passage. F. B.]

By the observed distances of the Pole-star from the pole, it is found that the greatest exceeds the least by 40″ or 45″: and therefore the greatest parallax of the orb, at this star, is more; and probably 50″, or very near a whole minute. But, it will be more accurately determined upon a severe examination of all the observations of it: for I have not yet examined all the observations made in the opposite months of March and September.

[The author then proceeds to describe *The Order and Uses of the Catalogue*, as given and printed in Latin, in page 161 of the 3rd volume of the *Historia Cœlestis*, which finishes the Preface. F. B.]

* Probably the letter of Flamsteed to Sir Christopher Wren, dated Nov. 17, 1702; a copy of which is preserved in MSS, vol. 35, page 68 (from the end). The part, here alluded to, commences at page 71. F. B.

*** The subsequent history of Flamsteed's life and labors may be deduced from the correspondence which he still kept up with Mr. A. Sharp; a portion of which will be given in the Appendix. And the progress of his executors, aided by Mr. Sharp, in the laborious task of superintending the press of the *Historia Cœlestis*, and the formation and engraving of the *Maps*, may be obtained from the letters of Mr. Crosthwait to Mr. Sharp, inserted also in the Appendix. Mrs. Flamsteed's celebrated letter to the Vice-Chancellor of Oxford will close the history of the whole. F. B.

APPENDIX.

No. 1.) *Letter from Mr. Oldenburg to Mr. Flamsteed.*

London, Jan^{y}. 14, 1669-70.

Sir,

Though you did what you could to hide your name from us, yet your ingenious and usefu. labors for the advancement of Astronomy, addressed to the noble President of the Royal Society and some others of that illustrious body, did soon discover you to us, upon our solicitous enquiries after their worthy author. The said Society having been made acquainted with your endeavours and performances too, and duly considered the importance and usefulness of these studies, and astronomical predictions of yours, tending so much to state the motions of celestial bodies, especially that of the moon, have given me order to present you with their hearty thanks, both for your singular respect to them, and to congratulate with you the progress you have made in the excellent science of astronomy; and withal to assure you that you can do them no greater kindness than to continue this industry and study from year to year: and that in compliance with your design, they will take what care they can to commit the province of observing those phenomena you have noted, to some of their most industrious and most skilful members. And, to the end that the better and ampler notice may be given of what you have so ingeniously and worthily begun to perform in this matter, it is intended that the most necessary part of your papers shall be forthwith made public by the press; and that perhaps in the *Philosophical Transactions* of this month: reserving the rest, that cannot be conveniently concluded in the narrow bounds of those tracts (which is to contain some variety of subjects) unto another opportunity. Which when done, I shall not fail (God willing) to see a copy of that book conveyed to you: whom I shall herewith desire to let me know the readiest and easiest way of sending things of this nature to you.

What occasion you may have to employ my service in here, you need no more but signify it by a letter to me, sent by the ordinary post, addressing to me at my house in the middle of the Palmal in St. James's Fields, Westminster. Meantime, you must look upon me as the meanest of the Fellows of this Society, though I am, with all readiness and sincerity, Sir, your very affectionate friend and real servant,

H. Oldenburg.

[Extracted from the MS *Letter* Book of the Royal Society.]

No. 2.) *Letter from Mr. Flamsteed to Mr. Collins.*

Derby, Jan^{y}. 24, 1669-70.

Sir,

Yours of the 15th I received; by which I understand that my papers, however unworthy, were read in the Royal Society: an honor which I could as little expect or deserve, as it merited their applause, or I their thanks. My intent in addressing those *calculi* to his Lordship, was only

to give notice what phenomena the heavens were about to afford us: which, since I was not accommodated with instruments sufficient to observe them with the requisite accuracy myself, I made bold to present to his honor; whom I supposed to be amply accommodated for such observations. But, prescribing the use of a telescope before other instruments, and taking occasion thereon to urge something too boldly on Mr. Hooke, and in another on Mr. Street, concerning the ephemerides, being likewise conscious of my rude addresses to those worthy persons with my papers, and fearing that my heat for the promotion of science might be worse taken by them than I intended: knowing also the smallness of my merits, I resolved to suppress my name, till I understood how it was like to speed with my papers. I fear I addressed myself too rudely to his Lordship: but since he is pleased to take notice of those unpolished papers, and enquire after their author, you may please (if you have not already) to tender my humble and sincerest services, with my name to his Lordship, and desire his excuse for me, that I have not explained my method of calculation. I suppose it cannot be hid to those who are versed in trigonometry. It is the accuratest I could choose: and the numbers were twice, some thrice, repeated for more certainty. If any desire to be more fully satisfied, I hope I shall answer their expectations in an epistle to the excellent Hevelius, containing a commentary on, and a correction of, several particulars in his *Mercurius sub sole visus*, and commentary on Mr. Horrox's *Venus sub sole visa;* which I have lying by me, written in Latin about half a year since. But, my style not pleasing some of my more judicious friends, I have since then given myself to the perusal of the classic old Roman, and some modern authors; so that I may bestow a better language on it before I expose it to the view of such severities as I shall be sure to meet with. I intend to present it, with an epistle *De æquatione temporis*, to the view of the Royal Society, before I commit it to the public. But, by reason of my frequent distempers, my parent's affairs, and the coldness of the season, I shall be forced to protract the time I have set myself for the perusal of my papers; which yet I hope to present you with, completed within six months. I have solar tables by me, composed above two years since (to wit, before I was 21 years of age); which I intend to expose with my epistle *De æquatione temporis*, directed to Mr. Street; whom, on occasion, you may let know I shall write to him before the term be over, by my kinsman, an attorney. But, I shall be forced to trouble you with the letter, because I know not the place of his habitation. I desire to transact things fairly with him; as I have done with his deceased antagonist Mr. Wing, with whom I had a fair correspondence. And, though we differed *de parallaxi et æquationibus systematis solaris*, and several other things, yet our dissent made us not the less friends. And, though I may not subscribe to Mr. Street's opinions *de fixatione apheliorum et nodorum in superioribus, et de æquationibus lunæ oscillatoriis*, yet I hope he will not refuse that we may friendly communicate by letters of such things as concern the heavens and our studies. As to the severity of the calculations, I suppose that, if the artist be but careful and curious enough, we have canons sufficiently large. In the study of my especial friend, Mr. Halton, I once saw one of Ulaccus to every ten sexagenary seconds: and if our supputations be exact to seconds, I think it is enough; and more than ever we may hope, by instruments, to obtain from the heavens: I mean in defining the places of the luminaries fixed, and the planets. As for their diameters, I dare not deny but that, by the help of glasses, we may be enabled to define them to seconds, or smaller parts; especially if a short telescope may be made to perform as much as a long one: which you say that Mr. Newton hath not only proved by demonstration, but fact. Sir, if it be no concealed secret, if you have the liberty and may do it, I desire that you would please to inform me of what glasses his small telescope is composed; how and in what figures ground; and how disposed in the tube. I intend to work some for my own use, and am framing

such an engine as Mr. Hooke describes for the grinding and polishing of them. I intend to grind with ordinary fine sand, dressed; and polish first with chalk, and after with putty. I know no better powders: but would be much obliged to any one that would inform me that, or anything else, either concerning the usual method, or necessaries for grinding and polishing them. If you know anything which you may freely impart, I should be much obliged by a communication.

[Copied from the *General Dictionary*, article "Flamsteed." Note D.]

No. 3.) *Letter from Mr. Flamsteed to Mr. Oldenburg.*

Derby, February 7, 1669-70.

Sir,

I intreat your pardon if you have not received sooner those cordial thanks I owe you for the favor you were pleased to afford my poor endeavours, and the respects you were pleased to yield their author, by your letter, which I was so far from expecting, that I looked upon myself as having thereby rather merited the censure than thanks of that illustrious Society, which had the patience to hear and see my heat for science vented with less respect than became so young a professor of the arts. I was conscious to myself of my harsh addresses to the noble President, and of my over violent heat and language, as also that I had urged so freely on some ingenious persons, who had deserved well not only of the Royal Society, but also of the whole commonwealth of learning: and therefore resolved to obscure my name in my Anagram. But, since the illustrious Society hath been pleased to make me understand their acceptance of my slight endeavours, with which I suppose I have a pardon of my faults included, I desire you to present my humblest services, with the acknowledgments of their favors to the Royal Society, their noble President, and all such persons as have been pleased to respect my endeavours; with an assurance that I shall, for the future, endeavour to answer their expectations with greater accuracy, by an annual exhibition of the prediction of such phenomena as I have there afforded in my papers, with such additions as each year shall require. I would gladly know if the times of the moon's apparent dichotomies have been observed in the long tubes by any English astronomer, and should be overjoyed to hear that the patronage of Astronomy was undertaken by any worthy English personage.

Present my service to those who have undertaken the observation of the appearances I have predicted: let them understand that I intend to wait for the phenomena with such rude instruments I have framed with my own hands myself. But, by reason of their incompetency, must be forced to rely upon them (to whom I wish all possible conveniences to their observations) for the utmost and requisite accuracy. I am, Sir, your humble Servant,

John Flamsteed.

[Copied from the *original* in the MS *Letter* Book of the Royal Society.]

No. 4.) *A Preface to my Celestial Observations addressed to Sir Jonas Moore, in* 1674.

Sir,

Though our age may justly boast of more accurate, learned, and diligent compilers of celestial numbers, than any of the preceding, yet has the restitution of astronomy gone but slowly forward, and with no great success, since Kepler (who has in some things done as much better than

[redacted], as they than he in others) for want of such observations as might reveal the latent [redacted] hypotheses seemingly ingenious. I having therefore, for some three years last past, been [redacted]ant on the heavens, have thought it necessary to impart my observations to the ingenious [redacted]er, and have hopes they may prove of some use in his labours, where others are wholly [redacted]. For, by the observations of the moon's diameters, he will find that the now commonly [redacted]lunar hypotheses of Kepler, Lansberg, Bullialdus, Wing, and Street, are absolutely rejected [redacted] heavens, wherein the diameter of the perigeon full moon is greater, and consequently her [redacted]ce from the earth lesser, than in the perigeon quadratures: the hypotheses and tables, in the [redacted]me, rendering the diameter in the quadratures considerably greater, and the distance less, [redacted]n the perigeon full moon. These may therefore prompt him to some expedient whereby her [redacted]s to, and recesses from, it may be justly solved: which once effected, her motions in longitude [redacted]latitude will be much more easily represented; but never without it.

[redacted]e will also find that the places of the planet Jupiter have been, for these two last years, some 13 [redacted]14 minutes forwarder in the heavens, than Kepler's numbers represent: and that his motions are [redacted]much better solved by any others; except we admit of Mr. Horrox's corrections, by which, if we [redacted]ake the annual motions of the fixed stars only 50 seconds, which is something less than Kepler's, [redacted]believe the calculation will not differ above 2 or 3 minutes from the appearance. He will also [redacted]perceive, by the observations of Mars, that his motions are much better represented by Kepler's [redacted]numbers than some others who have pretended to amend him: in which no wonder yet if they failed [redacted]whilst they would wrest them to some particular observations of their own, for little errors in the [redacted]chronical appearances of this planet become very discoverable, in which therefore that laborious [redacted]and subtle astronomer seems to have bestowed the main part of his pains; and indeed the restitution [redacted]of this planet's motions is his masterpiece.

How the motions of Venus and Mercury agree with the best-esteemed numbers I dare not assert, for that I have made only four observations of her appulses to fixed stars, and but one of his: from which I have not yet had time to calculate their places, and compare them as I desire with the Caroline and other tables; partly by reason of some distempers, but more through other affairs and employments incumbent on me, which have commonly either detained me from this exercise, or called me off when intent upon it. It was my purpose also to have added in every appulse, how much the planet by that observation was found in antecedence or consequence of the star, and with what more or less latitude, whereby the astronomer might readily determine its place, and latitude, according to the place and latitude he allotted to the fixed star. But I could not gain so much leisure, and therefore have described the bare and naked observations, plainly and without any ornament but what their own accurateness may afford them, in which I think they need not yield to any, if they be not much more exact than most that have yet appeared in public, not by my sole care and diligence, (far be it from me to boast of that, which though it was not wanting, was not I believe more in me than is usual in other observers,) but by the assistance of Mr. Townley's curious mensurator or micrometer, whereby I have attained to the preciseness of 5 seconds, which what proportion it bears to the preciseness attainable in the ancient or modern instruments without glasses, you have experienced, and I leave to you to judge.

And because I received this little instrument from your hands, I presumed to present you first with the effects of it, both because I esteem them your due, as the first fruits of that gift, and for that I know you expect not the compliments usually made on such an address, in the framing of which, having never been happy, I take all opportunities to decline them.

Though I received the micrometer at your hands, about Midsummer, 1670, yet was that year, with the summer and autumn of the following, almost elapsed before I could procure glasses and tubes fit to apply it to, and in good order: after which, my first work was to seek how it measured the parts of a degree, which to effect I at first set the pointers to a certain distance, and then laying them on a box ruler, struck fine lines by their edges thereon, of which taking the distances betwixt my compasses, and measuring it on a good diagonal scale, I found that 35 revolves with $\frac{1}{100}$ would open or close them an inch. Upon this I framed my table for finding the minutes and seconds measured by the instrument; but a little while after informing Mr. Townley of it, with whom by the means of encouragement I had then begun a correspondence, in his return dated December 21, 1671, he assured me that by several repeated trials of which he therein gave me an account, he had found that a screw, made in the same box with mine, made only 34·65, in an inch, and added that he conceived mine did the same. It was winter and a very inconvenient season for making such trials as he had used, and I thought I needed not doubt, but that the threads of two screws both made in the same box, were both of the same precise bigness; but yet because my repeated trial by the diagonal inch would scarce permit them so little, I adventured to state them 34·85, a little bigger than his: on which I calculated a new table for converting the revolves and parts of the screw into minutes and seconds, which I used from that time to the August following, 1672.

But I was not well satisfied with this determination, and therefore resolved on the first opportunity to make a more satisfactory trial, by the method which Mr. Townley had very successfully used and directed me. And therefore, August the 5th following, having chosen a level place in the open field, I settled my bigger tube upon it, and from the object glass forward, with a surveyor's chain, I measured 908 feet 7 inches, at which distance exactly across to the chain, I placed a very substantial ruler with black marks in white upon it, at 1, 3, 6, 36, 72, and 108 inches distance. I drew out the tube to 165½ inches long, where I could best see the object, and then found 108 inches measured within the tube, by 57·55 revolves, but by reason of the wideness of the object, the observation somewhat difficult. Afterwards 72 inches distance by 38·33 much better. Now because the breadth of the distant image projected in the tube is in proportion to its length betwixt the object glass and the place of projection, as the wideness of the visible object to its distance from the object glass, I say,

	Inches	Log.	Inches	Log.
As the distance of the object from the glass .	10903	= 5.962454	10903	= 5.962454
to the tube's length	165½	= 2.218798	165½	= 2.218798
So the breadth of the object	108	= 2.033424	72	= 1.857332
to the breadth of the image projected in the tube	1.639	= 0.214676	1.092	= 0.038584
And again,				
As the breadth to the parts measuring it	5755	= 3.760045	3833	= 3.583539
So is one inch to the parts measuring one inch	3511	3.545469	3507	= 3.544945

I esteem my latter observation, and the measures deduced from it, rather more accurate, because I could in it perceive the blacks more distinctly: and therefore have founded my table of minutes and seconds answering to the revolves and parts shown by the bar and index upon it, whereby I have corrected all the observations here recorded, except between January and March the 15, 1672, which not heeding, I transcribed as I had wrote them in my *Journal:* all which, if you find not done to your hands, may be corrected by a note annexed in the margin to that evening's observations. I made several other trials, repeating all very often, which still confirmed this measure; which here

I omit, because over prolix to relate, and these may serve to satisfy you and my readers of the care and diligence I used to find the exact minutes and seconds, and their preciseness. These trials, and indeed all my observations betwixt March 28, 1672, and August 19, 1673, were made only with the bigger segment of my object glass, which at the first time here mentioned broke as I was smearing it over a candle: yet need you not fear them less accurate, since it is easily demonstrable that the segment shall carry the rays and perform as well as the whole glass with only an equal aperture upon it, and therefore all the observations made with it were not the less certain for the accident.

I had no sooner certified my measures but the Almighty Providence of Heaven was pleased to afford me two or three more advantageous opportunities of observations, that might determine some things uncertainly held and much controverted amongst astronomers, in which such preciseness of measuring was altogether necessary: for, first, in the next immediate month, the planet Mars passing almost achronical by 3 contiguous stars in the water of *Aquarius*, I observed his distances from them twice in one night; whence I derived both his and the sun's parallax and distances from our globe vastly more than was ever before conceived either by our contemporaries or antiquity: of which you will find a full account in a paper of letters to our mutual friend Mr. Richard Townley*. In the following March, I observed the planet Jupiter several nights passing almost achronical by a star of the third light, seated near his *limes boreus*, whence I so determined the latitude of his orbit, that if ever the star's true latitude shall be found, by such observations as we may confide in, that of his orb will be given (by subducting only 26′ 40″) for the time of my observation. Since then I have examined the extreme removes of Jupiter's satellites from his centre, which I have found not much different from the antecedent determinations of Mr. Townley; and so exact that an eminent foreigner acknowledges them better than his own, though I expect longer tubes ere I dare adventure to determine these last precisely. The altitudes noted in the observations were commonly taken by a quadrant of 20-inch radius, applied or fixed to the tube, whose position sometimes being inconvenient, has rendered the altitude difficult to note, and therefore the moments less precise than they would have been taken with a good clock, which I have more wanted than any other instrument.

These I have made under a private roof, without any assistance but an ignorant servant, with few instruments, and in no little scarcity of time. Now God is making me more leisure, I hope to be better provided for the carrying on of these studies with some others, and to perform much greater things with such instruments as are designed by your servant,

J. Flamsteed.

[Extracted from MSS, vol. 40, page 77.]

No. 5.) *Warrant for the payment of Mr. Flamsteed's Salary.*

Charles Rex.

Whereas, we have appointed our trusty and well-beloved John Flamsteed, master of arts, our astronomical observator, forthwith to apply himself with the most exact care and diligence to the rectifying the tables of the motions of the heavens, and the places of the fixed stars, so as to find out

* Probably the letter alluded to in the note in page 33.

the so much-desired longitude of places for the perfecting the art of navigation, Our will and pleasure is, and we do hereby require and authorize you, for the support and maintenance of the said John Flamsteed, of whose abilities in astronomy we have very good testimony, and are well satisfied, that from time to time you pay, or cause to be paid, unto him, the said John Flamsteed, or his assigns, the yearly salary or allowance of one hundred pounds per annum; the same to be charged and borne upon the quarter-books of the Office of the Ordnance, and paid to him quarterly, by even and equal portions, by the Treasurer of our said office, the first quarter to begin and be accompted from the feast of St. Michael the Archangel last past, and so to continue during our pleasure. And for so doing, this shall be as well unto you, as to the Auditors of the Exchequer, for allowing the same, and all other our officers and ministers whom it may concern, a full and sufficient warrant.

Given at our Court at Whitehall, the 4th day of March, 1674-5.

By his Majesty's command,

J. Williamson.

To our right-trusty and well-beloved Counsellor, Sir Thomas Chichely, Knt, Master of our Ordnance, and to the Lieutenant-General of our Ordnance, and to the rest of the Officers of our Ordnance, now and for the time being, and to all and every of them.

[Extracted from MSS, vol. 40, page 115.]

No. 6.) *Warrant for building the Observatory.*

Charles Rex.

Whereas, in order to the finding out of the longitude of places for perfecting navigation and astronomy, we have resolved to build a small observatory within our park at Greenwich, upon the highest ground, at or near the place where the castle stood, with lodging-rooms for our astronomical observator and assistant, Our will and pleasure is, that according to such plot and design as shall be given you by our trusty and well-beloved Sir Christopher Wren, Knight, our surveyor-general of the place and scite of the said observatory, you cause the same to be fenced in, built and finished with all convenient speed, by such artificers and workmen as you shall appoint thereto, and that you give order unto our Treasurer of the Ordnance for the paying of such materials and workmen as shall be used and employed therein, out of such monies as shall come to your hands for old and decayed powder, which hath or shall be sold by our order of the 1st of January last, provided that the whole sum, so to be expended and paid, shall not exceed five hundred pounds; and our pleasure is, that all our officers and servants belonging to our said park be assisting to those that you shall appoint, for the doing thereof: and for so doing, this shall be to you, and to all others whom it may concern, a sufficient warrant.

Given at our Court at Whitehall, the 22nd day of June 1675, in the 27th year of our reign.

By his Majesty's command,

J. Williamson.

To our right-trusty and well-beloved Counsellor, Sir Thomas Chichely, Knt, Master-General of our Ordnance.

[Extracted from MSS, vol. 40, page 117.]

No. 7.) *Letter from Mr. Flamsteed to Sir Jonas Moore.*

Observatory, March 7, 1677-8.

Sir,

I am sorry the sharpness of the weather and your affairs are so great they will not permit you to make a short voyage to Greenwich. I have deferred to give you an account of my proceedings here, hoping of such an opportunity, which, because it presents not itself, I shall no longer forbear, but give you in this, which, by your last, I find there is need of. The first sentence is so short and urgent, that had not the heat which the latter part conveyed prevented, the first might have put me into a cold fit again, a day before I expected it. I would not have you think that, because our clocks go so much worse than we expected, all is out of order: whilst I know their errors, they serve very well to give the true times of such observations as I make with your sextant, which is now in excellent order, the limb being curiously divided by the great semicircle; so as, though I make no observations of Venus the day preceding, I can yet at any time by it so place your instrument, that it shall find me either that planet, or any other star, visible in the day-time, with as little labour almost as I can place it upon them in the night. Thus, February 11, last past, at $0^{h.}\ 2^{m.}$ p.m., I got the distance of Venus from the sun's centre, 45° 55′ 00″; and at $6^{h.}\ 28\frac{3}{4}^{m.}$ from Aldebaran, 46° 35′ 50″: whence I find the difference of the longitudes of the sun's centre and Venus at the noon preceding, 92° 00′ 30″ ferè *. The observations were taken, first, when the sun and Venus, and after, when Venus and the stars, were nearly in equal altitude, where the refractions contract the difference least; and should we err more than can be allowed in their quantities, it would cause an error altogether imperceptible and inconsiderable in the determined difference of the longitudes of the sun and Venus. This method, therefore, I prefer, far before that of meridional altitudes, for finding the inequalities of the sun's orbit, since it has nothing to do with the height of the pole, nor with the uncertain quantities of refraction; in either of which, or the error of an instrument, or the negligence of an observer, 24″ error in the altitude, nearer the equinox, would cause a whole minute's in the sun's place; whereas, it is scarce possible for a careful observer with such a sextant as yours is to err half that quantity; I am confident I scarce ever do a third of it. Besides, this method is general, and will give the sun's place at each solstice, which, in the other way, is altogether impossible, and that too as well as at the equinoxes or better: and that you may not think I only boast of things I have not practised, as some others use, I assure you that you shall find some of this sort made when the sun was on his perigee in seven days of ♈ in those papers I am now transcribing for you, which, for your credit, I affirm are the first that ever were made of this sort, and obtained only by those encouragements you have been pleased to afford astronomy and my studies. These, with some others of the same kind, I got in November and December 1676: the last year was barren of these opportunities, but this instant, I hope, will afford us enough to correct the sun's motions by, or discover the inequalities of his orbit, which is the only firm basis of all astronomy, if God spare me health to make use of them.

My theory of the equation of days I looked upon but as a dream at first, because one part on which

	°	′	″
* Aldebaræ locus	5	16	46
Longitudo ⊙ ab Ald. . . .	92	0	30
Locus ergo ⊙	3	16	16
Ephemeris habet	3	15	25
Error . . .	0	51	

it was founded, viz. the isochroneity of the earth's revolutions, was only supposed, not demonstrated, by me; but the clocks have proved that rational conjecture a very truth, which I shall not fail to make out, God assisting, in the aforementioned papers I am now transcribing; and hope, if he restore my health, to finish in good time, though my distempers have cast me much behindhand in my work.

I have also some observations of Mars in his last opposition to the sun, which may either correct or confirm those I made at Derby; and some of Saturn and Jupiter made in the same places nearly, and from the same stars from which the noble Tycho observed them 90 years ago, whereby their mean motions will be much better determined than from the uncertain and coarse notes of the ancients. I have some good ones also of Mercury; and have made corrections of all their motions from them for my own private use: but those must be reviewed and corrected again, when we have found the inequalities of the sun's orbit, and corrected the catalogue of the stars, which you know is a work not of a few days, but years; in the meantime no notice is to be taken of them.

We shall only want the obliquity of the ecliptic, which, in our sphere, cannot be well observed by the sun, by reason of the uncertainty of his refractions on the southern tropic; but if Mr. Halley bring his instruments and observations safe home, I doubt not but, by comparing both with ours, we shall so settle it as none coming after us shall know how to amend it.

The quadrant I am making with Mr. Hook's small one, I intend to use jointly for the examination of refractions this summer, and the following winter; whereby I doubt not but to attain a more perfect knowledge of it. I have met with a very good workman, and when it is finished, if you like it, it shall be yours, upon very reasonable terms. I have no other design in making it so privately, but to have it wholly made to my own mind, and not to be so much cheated in my rates, as I fear we have been by the Tower smiths, and those that are acquainted with us.

The bright star in the Dragon's head begins now to pass the zenith in the morning, before sunrise: it was not very reasonable to expect to find it in the tube in the day-time, before we had seen it pass in the night. I know the first sentence of your letter, that "*All our astronomy affairs were amiss,*" has some relation to this: but I assure you I have oftener thought of it, and perhaps with as much concern as yourself. I shall now take care of that experiment, and when we have observed the night transits, I doubt not but we shall safely afterwards find it in the day, if our glass be good; only, I entreat you, ask Mr. Hook, when you see him, what aperture he used upon his object-glass in his day observations, for that is very material.

I have given you a scheme of what I have hitherto attained, and in part of what is to be done hereafter; and now I hope you are satisfied that I am not negligent, nor need any spurring in an employment to which my genius forces me beyond what my reason sometimes suggests is for my interest, health or quiet; which two last are the only things I value on this side heaven, all other enjoyments being unsavoury without them. I am not much solicitous about our clocks, since I doubt not but Mr. Tompion's dexterity will put them soon into such order, as that a little pains of mine, in some weeks, may get them into good going again. I reckon not that time lost since they have gone amiss; nor, were I wealthy, should I value the expense of this trial, since we have learnt by it how small an alteration in the works, without any change in the length of the pendulum, will serve to make it go 11 minutes in a day too fast; and that it will not be convenient, if we get them once more into order, ever to alter them afterwards any more than, when they want it, to new clean and oil them.

My ague I hope abates; my fit is not much more than an hour long, but very gentle; only I am a little feverish, and can get no rest twelve hours after. I take nothing for it but a little carduus

posset drink to fetch the phlegm off my stomach, which it had formed or raised into it. I use a very slender and spare diet: my cordial is a glass of sack and some Mithridate, which I find fits me best of any physic; more I intend not to make use of, but to abide till it waste and pass off of itself. Your grandson's flits as mine does, but he will not be kept within doors when his fit is over: Mrs. Stanley thinks it abates, and would give him something which she has given her daughter with good effect for it, but I advise her to forbear till she understands your pleasure.

I am much engaged, and obliged to you for the assurances of your kindness, than which nothing under heaven can be more gratefully entertained; nor is there anything he more desires to merit, who is your very humble servant,

JOHN FLAMSTEED.

I had more to add, but want both room and time.

[Copied from the *original*, preserved in MSS, vol. 36.]

No. 8.) *Letter from Mr. Flamsteed to Sir Jonas Moore.*

Observatory, April 30, 1678.

SIR,

Yesterday two of the Blue-coat boys were sent down hither from Christ Church Hospital. I find Mr. Parkins has taken a great deal of pains with them, but wanted time to give them trigonometry, without which it is impossible for them either to understand or retain what they learn of navigation. The boys are prompt and ingenious; and if I thought I should be recompensed for my pains, I should give them that knowledge, with the application of it in problems of the spheres, and about the stars: but the masters, when they were here last, took so little notice of any such things, I know not whether I shall have so much as their thanks for my pains. They told me the King had ordered that I should receive two boys to instruct monthly; but not a word of any satisfaction for my labor, which I am very sensible (and so I suppose you will be) cannot be small. I hope they will think of a suitable recompense, ere they change these for two others; otherwise I must desire to be excused the trouble of them, since you know very well I have work of another nature under my hands, that requires more time to do it than I have or can gain to employ in it. I said not anything, however, to them of this, because I know you have it in consideration, and may procure me better satisfaction than perhaps they would proffer, who understand not that I have any thing to do here but to teach children; and perhaps it might have been in vain to go about to persuade them of the contrary. The bed is wanting for the boys' bedstead, which is yet in the further summer-house: last night, for want of it, they lay in the town, at their quarters. If it be provided, Cuthbert may take care to bring it down: but except you please to order him to hasten with it, the boys must take another night's lodging in the town; for he seldom uses to return home before midnight, if so early, when he goes to London.

One of our clocks goes well; the other may be made to do so too, if Mr. Tompion could be prevailed with to come and bestow a little pains upon it. I am proceeding to divide Mr. Hook's quadrant; but the windy weather will not permit me to adjust it as yet. I hope it will change in a day or two; after which, I shall soon have perfected it for use, only I want the pedestal. My ague, I thank God, is wholly departed; but it has left me pains in my feet, legs, and arms, which yet I

hope to wear off as soon as we shall have warm weather. He wishes you all health and happiness who is your humble servant,

John Flamsteed.

[Copied from the *original*, preserved in MSS, vol. 36.]

No. 9.) *Letter from Mr. Flamsteed to Sir Jonas Moore.*

The Observatory, July 16, 1678.

Sir,

I have now one of the prints of Mr. Halley's plate of the stars in the southern hemisphere in my hands: I have also seen a part of his catalogue, whereby I am satisfied he has done all that lay in his power towards their rectification. I would not therefore have you understand anything I shall here write concerning his works to his disadvantage; for if your letter had not forced me to it in my defence, I should, in this particular, have been wholly silent.

He has made a long voyage to observe the southern constellations, and been unexpectedly crossed with ill weather. At his return, his friends, however, expect an account of his pains. He has not had an opportunity, nor time, to examine the sun's motions, nor his distances from fixed stars, nor their latitudes, by due observations: 'tis a work rather of years than months. He is afraid it would be said he had done nothing if he make not something immediately appear. To shun this imputation, therefore, he assumes the places of some of Tycho's fixed stars, and the latitudes of many of them to be true, *which may be erroneous;* and, by the distances of the rest observed from these, he determines their places and latitudes. So that, whatever errors were committed by Tycho in the fundamental stars, are transmitted by Mr. Halley into this his new catalogue, which yet will be more accurate than the Tychonic, if considered all together; since he could determine his observed distances with more certainty than the noble Dane could possibly [do]; and therefore, for our sailors, his catalogue will be exceeding useful. Further we need not inquire.

I have here measured about 1000 distances of the fixed stars, and 500 of the planets from them: besides what I have done lately in examining refractions, and the corrections of your movements. So that, if I had thought it suitable to what is expected from me by the knowing astronomers of our age, I could have corrected the places of more than 300 stars, and have added besides about 100, omitted by Tycho, near the ecliptic. This might have made a noise, and looked a little glorious at the present; but there are a sort of intelligent men in the world that would not be imposed upon, who, upon one design or other of their own, would immediately fall to examine the work, and soon perceive the defect; whereby I should incur as just an odium as that which loads the memory of Lansbergius, and you would gain but little credit for having afforded your patronage to so ill an astronomer.

No, Sir: ever since I perceived the fault of the Tychonic catalogue, I have determined not to rely on any part of it; but, if God bless me with health, and success in my endeavours, to begin a new one, in which I will suppose as little as may be given me. To this purpose I have made several observations of the sun, whose places and inequalities are the first requisite, and that in such a manner, as that it shall not be difficult to determine the greatest equations, certainly to less than a single minute; whereas now there is 10 minutes difference in the quantity of it, as stated by several

late astronomers. I hope this autumn will afford me opportunities to determine something concerning it; though, to do it very accurately, we might stay till two years more be over, whereby we may see the two next returns of *Mars* to the place of the opposition I observed in Dec. 1676; after which, the inequalities of the earth's orb may be otherwise also determined by Mr. Halley's problem, published in the *Philosophical Transactions*.

Till this be done, it will be impossible to determine the true place of any one fixed star: so that you see it is wholly impossible for me, at present, to dispatch anything of the like nature with Mr. Halley's catalogue and hemisphere. This, I doubt not, but you are well satisfied of; yet am I laying in a stock of observations, whereby, as soon as the sun's places shall be once truly determined, the work may soon be dispatched, and a great deal made to appear that will be useful, and I hope need no correction by those that come after us.

My first observations, before the sextant was fitted up, you have in your hands: the last year's I have half-transcribed, during the time I had my ague; but finding how prejudicial that was to my health, I was forced to intermit it then. Afterwards I was hindered by the coming of the Christ Church boys before I was recovered. Of late I have been almost constantly employed in making such observations as the clear weather has afforded me an opportunity for, and such calculations as were necessary for recording of them, which were not a few. Besides that, the dividing and examining the quadrant has spent me some time, and more [has] been employed in some astronomical *parerga:* so that I have not had time to resume my transcriptions till this week; and now I am visited again with pains in my feet and legs, which yet, while they are anyways tolerable, prevent not my proceedings; though to get totally rid of them, more respite is required than I can afford myself at present.

I have given you this large account of my endeavours, that from thence you might know how busily I have always been employed since I sat down here; though, I doubt not, if you recollect yourself, you might easily understand it without this information: and therefore I cannot conceive that you have any real design to stop my salary, which I have earned by labor harder than thrashing, and with the expense of the most precious of all enjoyments, my health, not yet recovered. I can therefore only suppose that that expression was inserted in your letter to spur me on in those endeavours to which my genius hurries me, beyond what any reason will warrant. But, Sir, I cannot but resent it ill that you should think me of that carter-like temper that I cannot move without a goad. The pleasures of my studies, if I may be permitted to follow and enjoy them quietly, are the greatest incentives that can be to prosecute them vigorously; and I assure you, that, as soon as ever I shall have obtained anything from my observations, so certain as that I think it may not be liable to error, or need correction, I shall be as desirous to expose it to public view as you can be to have it. In the mean time, if what you desire be only to have an account of my observations, I am busy about the transcription of them; and though I may not neglect the opportunities of making more, which are something scarce to be had, for copying the former, yet shall I use all diligence to put them into your hands.

I know the busy and least intelligent of the world are still the most inquisitive: and that, too, where they are least concerned. If the papers you have of mine in your hands will not satisfy those gentlemen, who ask what we are doing here, that I am not idle, you may do well to inquire whether they understand the trouble of making astronomical observations, and the labor of calculations necessary in applying them: and you may further add, if you please, that we are about that work which employed the noble Tycho, and six or eight constant ingenious assistants, about 20 years, and

Hevelius almost 30; that I hope to correct and amplify the first, and to yield more certainty than the latter, though neither in the number of my instruments, nor skill of my assistants, I may compare with either of them.

Permit me, Sir, one free line. I have often tried to make Mr. Hook's wall-quadrant give me altitudes, and it has as often deceived me, and lost its rectification. I tore my hands by it, and had like to have deprived Cuthbert of his fingers. Except something more manageable may be put in its place, it will be a great let to our proceedings, as I shall further inform you when I see you.

I have got our clocks now nearer the true mean movement than I had them last year, if I be not deceived. A few weeks will satisfy me; and I hope they will now answer our expectation. Excuse this length, and assure yourself I am ever, at command, ready to serve you, whilst I am

J. Flamsteed.

The Commissioners will not quit me of the payment of the £5 for poll-money on my salary, and I have none in my hands to pay it.

[Copied from the *original*, preserved in MSS, vol. 36.]

No. 10.) *Letter from Mr. Flamsteed to Dr. Seth Ward, Bishop of Salisbury.*

The Observatory, January 31, 1679-80.

My Lord,

I cannot but esteem myself condemned of great ingratitude in your Lordship's judgment, for that having received so signal and extraordinary a testimony of your affection and kindness for me and my employ, I should not find time, in almost three months since past, to return you the acknowledgments due to so great a favor: nor should I be less culpable in my own opinion, if I did not hope the reasons, I shall give you for this delay, might both excuse and procure me a full pardon for it. I informed your Lordship when I waited on you, that my allowance had been in danger of a total retrenchment, but that I had found such friends in the Tower, that I thought it then out of danger. But I had not reached Whitehall, in my return from Knightsbridge, when I met with a person of quality, and one of the present Privy Council, who assured me that those I esteemed my friends, were not so cordial as I had taken them to be; that my salary was in greater danger than I apprehended; and advised me to speak to his Majesty myself, who he believed had too great a kindness for the Observatory to suffer it to sink for the want of so small an allowance. He likewise promised me, on my humble request, to speak on our behalf, whenever he met with a fair occasion. It was not long ere I waited on him again; and then he told me, he had waited on his Majesty, spoke to him, and was assured by him, my salary should be continued and paid, as formerly, forth of the office of the Ordnance. But he still urged me to speak myself. My Lord, I am but an ill orator on any account, but worst and least bold in my own concerns. However, this must be done, lest it should be imputed to my modesty, or neglect, that an employ was lost which has already procured us some repute amongst foreigners, and may be of great use to our ingenious friends and posterity.

I constrained therefore myself to consent, or rather obey him; and provided a table of the tides for the year entered, calculated from an hypothesis, which by long experience has been

found to represent them well, when the common rules and almanacs have erred near two hours: and a useful addition I had contrived for the seaman's quadrant; but by that time these were ready to be presented to his Majesty, my friend was seized with some distempers which forced him to keep his bed, so that I could not gain an opportunity till after the holidays. Then I waited on him to Whitehall, and was by him introduced to the King, who received my small present kindly, and approved well of my contrivance; and being moved again concerning our affair, he answered *He would take care of it.* This promise has encouraged me not to despair; but I cannot hope, in the mean time, that this business will pass easily off: for, should his Majesty pass the book of retrenchments, now before him, without examination (as is most likely), my salary would be comprised, and then it would be no small trouble and charge to me, to get it re-allowed.

During this suspense, I expected letters from Dantzic and Paris, which might give me an account of what is doing abroad: and having nothing to inform you of, which I had not in some measure acquainted your Lordship with before, I thought it best to forbear till I might be able together to give you an account both of what I should learn from abroad and of my own concern here. At the same time, I had the happiness of more than usual serenity, which was to be employed in taking the observations I much wanted; and a couple of gentlemen to read to: for I am forced to supply the want of my short and ill-paid allowance, by my extraordinary labor. My observations I was more intent upon, and have been ever since; that I might lay in a stock, and have the more to work upon, in a country retirement, in case such a thing as a retrenchment should force me from my instruments, and interrupt the series of my observations. Thus I was almost wearied with continual labor, night and day, till cloudy weather gave me an unwelcome relief. And now I understand, by a friend from Paris, that the things I particularly desire thence, were not to be had, nor any answer to be expected thence till some new occasion were administered for it. From Dantzic I cannot expect any: I must therefore intreat your Lordship to pardon my first fault, to the commission of which so many contributing causes have forced me; and, in the room of what I intended you from others, to accept an account of the progress of my own studies and endeavours, in which whilst I am successful, as I have lately been, I cannot esteem myself unfortunate: but if they may appear such to your Lordship as may deserve your esteem, I shall think myself exceedingly happy.

I informed your Lordship when I waited on you, that my chief design was to rectify the places of the fixed stars; and, of them, chiefly those near the ecliptic, and in the moon's way: and that now I thought I had a sufficient stock of observations, to ground a catalogue upon; which I was resolved to begin with my first leisure, or cloudy weather. Accordingly I examined my books of observations to see what more notable fixed stars I had made use of to determine the places of the less, or of the planets, by; and found their number near 100. To begin with these, I selected the seven following, all save the first lying betwixt the northern tropic and the equator, to try whether their calculated differences of right ascension would make an entire circle, as they ought to do, if their meridional distances from the vertex and intermutual distances from each other were truly stated.

The latitude of the Observatory correct by refraction I have stated $51^\circ\ 28'\ 10''$: by which, and their meridional distances from the vertex alike correct, I have stated their differences from the point of the earth's northern pole as underneath: their intermutual distances I have used as they were observed without any such correction. The reason your Lordship will understand immediately: hence I have calculated the annexed differences of the right ascensions.

Anno ineunte 1680.	Distantia a Polo.			Distantiæ intermutuæ.	Ascensionum rectarum differentia.	Contractæ per refractiones.
	° ′ ″			° ′ ″	° ′ ″	′ ″
Lucidæ Arietis...	68 4 40	Distantia Observata.	a Calce Castoris..μ	58 21 25	63 34 46	0 54
Calcis Castoris...	67 22 50		a Regulo	54 33 40	56 54 42	0 56
Reguli	76 29 50		ab Arcturo	59 46 25	62 27 02	1 2
Arcturi.....	69 8 20		a Capite Ophiuchi..	48 6 15	49 44 47	0 42
Capitis Ophiuchi.	77 11 00		ab Aquilâ Lucidâ...	33 31 30	33 46 24	0 30
Aquilæ Lucidæ ..	81 56 00		a Markab Pegasi...	47 48 10	48 25 16	0 44
Markab Pegasi ..	76 29 40		a Lucidâ Arietis ...	43 37 40	45 6 25	0 36
			Differentiarum Summa		359 59 22	5 24

Hence I might have concluded, as some before me have done on the like experiment, both that the observations were sufficiently accurate; that the parallax of the earth's orb was insensible *inter fixas;* and that these (being correct by the rational partition of 38 seconds amongst them, to fill up the circle) might be a firm foundation to build the rest upon: but, to me, they seemed to intimate something further. For though we find our earth so inconsiderable a thing, that its diameter subtends an angle not anyways sensible at the fixed stars, yet reason and some seeming experience would persuade us that its vast orb has a parallax not wholly imperceptible at them. Again, since the air, by whose renitency the celestial refractions are caused, circumvents our earth on every side, it must necessarily follow that all rays, save the perpendicular, shall be refracted; contrary to Tycho, who thought that above 20°, and Ricciolus, that from 45° to the zenith, the stars were not at all raised by refraction: and frequent experience has told me that the quantities of the table, I send your Lordship herewith, in distances less than 86° from the vertex, (or more than 4 deg. above the horizon,) will not be convinced of any error. Seeing therefore that refractions make all the stars appear higher than really they are, they must also make the distances observed appear less than they would otherwise, if the earth had no such atmosphere as we find it encompassed with. To avoid therefore these effects as much as was possible, it has been my constant care to forecast my measures so, as the stars observed might be nearly in the same altitudes: in which case, both the refractions are least, and may be calculated with the least labor, and danger of error. Having therefore found what was the height of each of the seven before-mentioned stars, when the distance was measured, I thence computed what was the contraction of that distance by refraction: these your Lordship will find in the last column of the foregoing table. Their sum makes 5′ 24″, and so much less than a circle ought the sum of all the differences of right ascension to have been found; that is 359° 54′ 36″.

But, since they make an entire circle, wanting only 38″, it is evident they were nearly as much dilated by some other cause, as they were contracted by refraction; which can be no other than the *parallax of the earth's orb:* for which some of my ingenious friends esteem this so good an argument, that they tell me the anti-Copernicans cannot otherwise evade it, but by suggesting that the sun carries not only our system, but the whole sphere of the fixed stars round our earth once a year.

But this they esteem so improbable and unreasonable, that they cannot think any one will have the impudence to assert it.

I am very desirous to try whether our rounds of lesser stars will confirm what I am taught by this; but I am not yet furnished with measures enough for my purpose, and therefore at present am compelled to forbear. When I shall be accommodated with such, and so many, as I think expedient, I shall make further experiments; and your Lordship shall know my success.

If you ask me what are the right ascensions of the before-mentioned stars, I must needs confess I have not yet absolutely determined; nor can I, for want of a good meridional fixed quadrant. But, by such observations as I have made with the sextants, I find that if the right ascension of the bright star of Aries be stated 27° 18′ 20″ to the beginning of this year 1680, it will answer the sun's meridional heights, as near as I can expect; the right ascensions of the rest may be easily made by the addition of the first difference to this, and the following to the sums; and their places thence easily computed in longitude and latitude, supposing the obliquity of the ecliptic only 23° 29′ 00″, which is the most my observations will warrant.

In observations of the planets' places I have ever had a special regard to your Lordship's advice, to observe them carefully, and as near as I could when they were in the same plane with the earth and sun. Thus I have got 4 achronical appearances of Saturn, 3 of Jupiter, and 2 of Mars, since I sat down; which is as much as I find noted of the like sort in 20 years by Mynheer Hevelkye: besides one return of Mars to the place of the first conjunction I observed in December, 1676. The errors of our Ephemerides are so great, in the places of Saturn, as your Lordship will scarce believe; and therefore I shall give you my observations of him, nearest his last achronical fulsion.

Decembris 10, 1679, st. vet. p.m. Saturnus.

h.	m.		°	′	″
9	54	ab Aldebara	30	21	0
	59	ab oculo 8 boreo	31	24	25
10	5	a calce Castoris	4	31	25
	15½	a Polluce	15	20	40
	25	ab hum. seq. Aurigæ . . .	24	15	50
Pollux		ab isto humero	26	59	30

By the three last distances, and my own correct places of the fixed stars, I determine the true longitude of Saturn in ♋ 5° 20½′, his latitude 0° 49½′ south. The ephemerides, calculated on Kepler's tables, gives his place in ♋ 5° 48′, latitude 0° 51′ south; exceeding his observed place in longitude 27′, in latitude 2′. The Rudolphine numbers, on which Hecker's numbers are built, are esteemed, and justly, as good as any extant; yet you see they are almost half a degree too swift on this planet.

In the next under him, Jupiter, they have been found half that quantity too slow in the years 1672 and 3: but in the year 1677 only 10′; and at the last conjunction of the Sun and Jupiter scarce 4′. This happened 18th October last: the same night I measured.

Octobris 18, 1679, st. vet. p.m. Jupiter.

h.	m.		°	′	″
7	31	a media et lucida Pleiadum . . .	20	28	50
7	34	a lucida in pede Persei, prec. ζ . .	26	4	35
7	43	a secunda Arietis	11	43	15
7	50	a lucida Arietis	11	41	10

h. m.		° ′ ″
8 0	a succedente in Aquarii λ . . .	58 39 15
10 38	ab extremitate Alæ Pegasi . .	33 52 0
10 44	ab Aldebarâ	29 46 50
	Lucida Arietis et Lucida Pleiadum . :	22 57 20

Jupiter's place, computed from his distances from the middlemost of the Pleiades, and the bright star of Aries, is in 8 5° 47′; his latitude 1′ 28½″ south. Hecker's ephemeris has it 8 5° 43′, latitude 1′ 25½″ south. Therefore the error in longitude is 3½′, in latitude 3′.

In the observations of the superior planets, I have had careful regard to such stars, from which the noble Tycho observed them, at their achronical appearances in the next nearest place of the ecliptic: and repeated their distances from them: thereby if possible to discover whether their middle motions were subject to secular inequalities, as Kepler suspects, and I have reason to believe not without good cause: or whether they be equal in all ages, as all astronomers hitherto have supposed them.

In Mars, Kepler's numbers err, but inconsiderably: this planet was his masterpiece; and his great pains bestowed in the limitation of his motions seem to have had suitable success.

For determining the Sun's place, most astronomers hitherto have made use of his meridional heights: my want of a convenient instrument for that purpose has forced me on a much different, and I think far better method. I have, as often as I had any settled serenity, measured his distances from the planet Venus by day, and hers the night next preceding, or following, from fixed stars; whereby it is not difficult to determine his place *sub fixis* much more accurately than could probably be done by his meridional heights; in taking of which, an error of 24″ would mistake the Sun at least a whole minute. Whereas he must be a very negligent observer, that with our sextant should commit a fault of half that quantity in 2 distances.

Our cloudy island weather has not permitted me so many of these as I desire; but, by what I have, I find the greatest equations at least 6 minutes less than Kepler makes them; and I have some measures far more convenient for this purpose, than any that can be got by meridional heights; viz. on the Sun's perigee and apogee where he is otherwise unobservable.

In Venus, the common tables are often 15 minutes false, and I fear more in Mercury: though, by reason of his small elongations from the Sun, and the declivity of our sphere, I can but very rarely observe him.

The errors in the Moon I often find 10 or 11 minutes; I am therefore the more sedulous in lunar observations, because a true knowledge of her motions would be more useful to us, than all the rest laid together. For I have found a way of determining the times of her appulses to fixed stars in any latitude, without any calculation of parallaxes: so that if we can but once arrive at a true knowledge of her motion and inequalities, we shall have better means for finding the longitude of places, than have yet been known. But to gain this, a long-continued series of observations will be necessary, and indefatigable pains; for, *hic labor, hoc opus est.*

The equations of time I can prove, by many and careful experiments, (made with the large clocks, which my deceased kind friend Sir Jonas Moore has furnished us with,) to be no other than astronomical; and therefore those which Kepler supposed, to help to represent the Moon's motion, must needs be some part of the latent inequality which we are in quest of; and whereof only time and continual observations can give us an account.

My Lord, I am forced to a length beyond my intent, to give your Lordship an account of my endeavours, and their success; which, considering I lie under want of fitting instruments, due assistance and my irremediable avocations, I conceive my understanding friends will think not less than was to be expected from me.

I am now proceeding to the catalogue of stars lying on or near the ecliptic; but by the account I have given you of my other concerns, you will find all this hopeful series is like to be interrupted; except it shall please the Architect of the Heavens to raise us up more patrons like your Lordship.

I fear, in excusing the fault of a long delay, I have committed as ill, by too long a letter. I humbly therefore beg your Lordship's pardon for it, and that you would please to accept of what I can only pay you, for all the testimonies of your favor, my humble thanks; and that, without being chargeable to you, I may be esteemed still, my Lord, your Lordship's most obliged and humble servant,

JOHN FLAMSTEED.

Then followed a table of refractions.

This letter was superscribed for the Right Reverend Father in God, Seth, Lord Bishop of Salisbury, &c.

[Extracted from MSS, vol. 42, page 18.]

No. 11.) *Extract of a Letter from Mr. Flamsteed to Dr. Halley.*

The Observatory, Feb. 17-27, 1680-1.

SIR,

I had yours of the 22nd of Jan. old style, to which I had returned an immediate answer but that the yachts were all abroad; which caused me to await their return, that I might know which went next for Dieppe. Yesterday, at London, I learnt that they were all ordered other voyages; so that if you have no conveniency offers itself of transmitting the things in your hands, by such of our countrymen as are coming from Paris, the best way will be to send them directed to Monsieur Jeane le Angleshe, in the Bastile at Dieppe; where the next yacht that goes thither will call for them.

Since I wrote to you last I observed the comet again on the 25th and 30th of Jan., the 2nd and 5th of Feb.: which, if you require, are at your service. I find a difference of 4 or 5 minutes betwixt my observations of the comet on the 29th of Dec. and 13th of Jan. from yours: I suppose you understand the reason of it. I made use of my own places of the fixed stars: did not the Messieurs of the Observatoire employ Tycho's? I suspect it. You will do well therefore to send me the distances from whence its places on the 3rd, 8th, and 23rd of Jan. French style, in your Synopsis, are collected; if it lie in your power.

You tell me you have meditated upon comets and come to a result; yet desire my thoughts as to the philosophical part of them. If you have resolved, I doubt not but it is on such good grounds and consideration that my thoughts will be needless. Might I not also, on this intimation from you, have expected yours first? You seem too close; but you shall not accuse me of that fault; I shall willingly answer your desires and reckon myself a gainer: for, betwixt friends, the agreement of opinions confirms them; the difference helps to correct the faults of either.

I must first thank you for the account you sent me of Gallet's coarse observations at Rome: from them I draw my first arguments. I conceive the comet which appeared in November to be the same we lately observed. You may remember that I told you, before you went hence, when I had

only heard of it, that we should see it again, when it had passed the sun. Since, you have seen that prediction verified: but the reason I must acknowledge of its late appearance is much different from my conceptions at that time. It appears by such observations as were made here (before Gallet's, though as coarse) that the comet had north latitude first, then pierced the plane of the ecliptic twice, and so passed on towards the sun. I conceive, therefore, that the sun attracts all the planets, and all like bodies that come within our vortex, more or less according to the different substance of their bodies, and nearness or remoteness from him.

[*Flamsteed then enters into a statement of his opinion of comets, their formation, motion, &c: and concludes his letter as follows. F. B.*]

I have given you my opinion fully of it, and I think answerable to your desires: I shall now expect yours at your leisure. You tell me in your first letter that your friends have a way of bringing their instruments to the plane, much different from any you have formerly seen. You will oblige me much if you can procure and send me any prints of their instruments or your own description of them.

The contrivance of Mons. Roemer's planetary instruments, and for eclipses, I expect from you: as also a larger account of your entertainment, and what other instruments you have seen amongst them; for I cannot think they would show you all at first, nor could you then so well judge of them. You will now, ere long, I suppose, be for Italy; whence it would be more difficult to hear from you, else I should not make so troublesome a demand. Your first letter made me suspect they are not so well accommodated as, on Mons. Roemer's report, I had believed them. Pray be plain: their esteem with me will depend very much on your opinion: and with me, I hope you will deal freely; it shall never hurt you.

Pray let me know whether Mons. Roemer has yet left Paris, and on what account: my humble service to Mons. Cassini, with thanks for the map he has given you for me. You will very much oblige me if you please to let me know what they employ themselves most on at the Observatory.

I did your commands, and presented your French observations of the comet to Sir Christopher Wren, who is now president of the society. I hear of others from Strasburg, Dantzic, and other places, which you sent to Mr. Colson. I seldom see him. You may therefore do well, if they were made in November, to send them in your next to, Sir, your most obliged and real friend, &c,

John Flamsteed.

P.S. I sent Mr. Colson mine; but have not seen him since. The humid part of the body of the comet being outmost might cause it to have a large atmosphere: and, from both, when it was near the sun, the violent action of his rays upon it might carry forth plentiful steams of matter to a vast distance, which caused the tail to appear double the length, when near the sun, it did to the length on its perigee, where it lay most convenient to be seen, and should on that account have appeared longest. Conceive how the smoke would appear from a chimney in a moving ship; or the steams from a drop of water let fall on a moved hot iron; you will apprehend the reason of the deflection of the tail, I think, very naturally. J. F.

Note by Flamsteed, written on the letter. This letter was designed for Mr. Halley at Paris, but not sent; or I believe another was sent to the same, since.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

No. 12.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Trin. Col., April 3, 1682.

Sir,

The bearer hereof, Mr. Edward Paget, becoming competitor for the Mastership over the boys of the King's foundation in Christ's Hospital for teaching navigation, and having no mathematical acquaintance in London, I conceived it might help forward his design to be introduced to such acquaintance that, by conversing with them, he might make himself known. And you being a person most eminent by your deserts, as well as by your place, I have therefore taken upon me the freedom to salute you by him: begging this favour that, as you find him by converse, so you would represent him upon occasions; or, if it lie in your way, introduce him to other mathematicians, who, after conversing with him, may have occasion of representing him to the electors. I have given him a character according to my judgment of him, which he will show you: but it will be more satisfactory to know him by converse. If you please also to let him have your advice in what he is to do, you will, in all this, much oblige your affectionate and humble servant,

Is. Newton.

Note by Flamsteed, written on the above letter. Mr. Paget was chosen master of the mathematical school on my recommendation: for I found an able mathematician of him to the hospital within about a month after. And the hospital governors were so well pleased with the choice, that, to show their gratitude, they sent me a staff, and made me of their number the summer following.—Ebrietati deinde post annos 7 nimium addictus immemor officii, pueros neglexit, in Flandriam transiit, deposuit mimas, in Indiam tandem navigavit: faxit Deus ut sanus et sobrius redeat.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

No. 13.) *A Letter to Edward Sherburne, Esq., concerning my Observations.*

The Observatory, July 12, 1682.

Honoured Sir,

To obviate the common accidents of mortality, and to prevent, as much as in me lies, such casualties as have sometimes deprived the world of the like labors of industrious men, I have thought fit to put a copy of my book of observations into your hands, to be preserved by you till they shall be called to the press*. And now that you may not be put to the trouble of turning over 60 sheets of paper, to know the contents, and what I have done since I sat down at Greenwich, I shall here acquaint you both with the original and progress of my endeavours; what I chiefly regarded in these observations; how far I have carried them on; and what is wanting to render them complete. Somewhat of these you might collect from the papers themselves; but the greatest part is of such a nature, that it cannot be well understood, except declared by the author.

When I came to London, in the beginning of the year 1675, a bold and indigent Frenchman, who called himself Le Sieur de St. Pierre, had solicited the King to take notice of his deserts: he pretended no less than the absolute discovery of the *longitude* from easy celestial observations; and demanded *the heights of 2 stars, and on which side of the meridian they were, with the heights of the moon's 2 limbs, with the pole's height, to be given to ¼ minutes, as also the year and day of*

* This book is still in existence (or at least one that, in every respect, answers the description) at the Royal Observatory; and is MSS, vol. 19. F. B.

the observations—whence he undertook to show under what meridian these observations were made. His Majesty appointed the Lord Brouncker, the Bishop of Salisbury, Dr. Pell, and several other ingenious persons to receive his proposals, and furnish him with the observations he required, to try his skill. These met at the house of Colonel Titus, whither Sir Jonas Moore took me to one of their meetings; at which, according to the power given them by his Majesty, I was admitted into their number, and desired to provide the observations demanded: which I did, contrary to our Frenchman's expectation, and showed that the observations he required were not sufficient for his purpose, by reason that the best astronomical tables erred sometimes 12 minutes in the moon's place. He had no way to come off, but by pretending that the observations were feigned: I showed him that they were not, yet had they been so, they might have served for his purpose in some cases; that he had only betrayed his own ignorance; and that we knew better methods. Upon which, he huffed a little, and disappeared; since which time we have heard no further of him.

This occasioned much discourse, concerning the invention of the longitude. It was agreed that observations of the moon's distances from fixed stars were the most proper expedient for the discovery of it: but these, I observed, would suppose two things that would not be granted. 1st, That *the theories of the moon, and lunar tables grounded on them, were both true:* whereas my observations of the moon's diameters, and her appulses to fixed stars, plainly proved that the theories were enormously false, and the best tables at least 12 minutes erroneous. 2dly, *That the places of the fixed stars in Tycho's catalogue were exactly true:* whereas I had found, by the same observations, that they were commonly 5 or 6 minutes false or uncertain, and sometimes more. So that, *whatever any person might pretend or affirm concerning his numbers, it was utterly impossible to get exact tables of the moon's motion, and to attain the discovery of the longitude by her, except these errors in the places of the fixed stars were first corrected and amended, which had been occasioned by Tycho's measuring their distances with plain sights, and the naked eye, before the discovery of the telescope.*

This the ingenious gentlemen easily understood, and therefore readily joined with Sir Jonas Moore to move the King that an Observatory might be built, and furnished with convenient instruments for making such observations as were necessary for correcting the places of the fixed stars, the luminaries, and planets, in order to the discovery of the longitude, which was not to be otherwise expected; and myself to be employed in it, with a salary for my support in the work, which his Majesty was graciously pleased to grant. My salary was first settled; next the Observatory was ordered to be built; yourself and Sir Jonas being to take care of it: which you did so effectually, that in July, 1676, it was fit for habitation.

I entered into it; and, in two months after, began those accurate observations, of which the papers I give into your keeping contain an account. Which I therefore chose to depose with you, rather than with any other, because I thought I could not better secure the fruits of my endeavours, than in those hands which had cherished the plant, from whence they sprang, and defended it oft from threatened ruin.

As soon as my salary was ordered, I began to think of continuing my observations, which had now been intermitted for near 12 months, during my journeys between Derby, Cambridge, and London. I caused first the new micrometer, of a different contrivance from the former, to be made; and some tubes: with which, and a pendulum clock of Sir Jonas Moore's, I observed some few appulses of the moon to the fixed stars, whilst I was his guest, in your neighbourhood at the Tower. At the same time I contrived by his order, and saw the sextant made; but, as soon as the building

was begun here, I came down to Greenwich, and fitted up my instruments in the gallery of the Queen's house; where I continued my observations of appulses, till the Observatory was fit to receive me: well knowing they would be very useful in the restitution of the moon's motion, so soon as the places of the fixed stars should be corrected; for which I resolved it should be my first business to lay in a good stock of observations.

The sextant was fitted in its place in September, 1676; but had then only the revolutions of the screw figured and numbered upon its limb: to which I had found what degrees and minutes answered by observations made at land angles. This is the reason why, in all the distances taken from this time, till the end of the year 1677, you find the revolutions and parts only noted, with the degrees, minutes, and seconds answering them. But now I found a fault committed by the screw, and therefore in the Christmas holidays following, I caused the instrument to be taken down; and with my own hands divided the limb diagonally, as Tycho and Hevelius before me had done their instruments. Whereby I both avoided the fear of error in my measures, and in transcribing them: for I continued to note the parts and revolves as formerly; and if at any time I found the equipollent degrees, minutes, and seconds, to differ much more than a minute from the measure numbered by the diagonals, I always rejected such observations, (except the corrections were obvious,) till I could repeat them again: which, if possible, I took care to do, the first following opportunity.

The sextant is an instrument which cannot be managed with less than three persons, of which the two observers ought to be skilful in the business; for the third, any indifferent person, of a strong able body, may serve. A skilful assistant was promised me, besides our labourer to move it; but I never could obtain one, which has put me to more than £20 per annum expense, in a servant for that purpose; with whom, when I had furnished myself, I set close to my business: and because the fixed stars lying within the zodiac are of most use, I began with them, omitting no convenient opportunity when my health permitted. Considering also the restitution of the moon's motion was a great part of my business, I took care to measure her distance from fixed stars frequently, and as often as I could, when she was in the nonagesimal degree of the ecliptic: such observations being most esteemed, because the parallax of longitude ceases there.

Nor was I forgetful of what might otherwise conduce to the improvement of astronomy; and therefore often observed the places of the other planets, the superiors, Saturn, Jupiter, and Mars, when in any notable configuration with the Sun, especially their oppositions. It is much suspected that these have their motions involved with secular inequalities, as well as annual; that is, that Saturn moves slower, Jupiter swifter, in our age than formerly: my observations seemed to countenance this opinion. That it may be evident whether they are or not, when the opposition drew nigh, I have enquired in Tycho's *Historia Cœlestis*, from what stars he observed either of these planets, when they were achronical near the same place of the ecliptic; and measured their distances from the same stars: whereby posterity will be able to determine the controversy.

The planet Mars at his opposition is nearer the earth than the sun is: and therefore his parallax is then greater than the sun. To determine it at 3 several oppositions which have happened since I came here, I have measured his distances from the same fixed stars, at a good distance from the meridian, on each hand of it: whereby I have found his scarce sensible. Whence I collect that I need not recede from the quantity of 10 seconds, which I had stated it at Derby.

The common way of observing the sun's place has been by his meridional heights observed by a good large quadrant. Such a one I moved to have had made; but Sir Jonas would needs have Mr. Hooke to contrive it; which he did without any consent of mine, but so ill that it was impossible for

me to render it useful, though I employed my utmost endeavours to make it serve: which causes me to think he ordered it so on purpose to hinder my progress. But this put me on thinking how to gain his place, by the help of the sextant: and my thoughts in part succeeded. I considered that, by the help of the distances of Venus from the Sun taken by day, and from the stars by night, I might collect the sun's true longitude from the fixed stars, without any considerations of unknown refractions, or parallaxes; and without being concerned in the pole's height: as I should have been by observations of the sun's meridional heights. This method I therefore put in practice, and I doubt not but I have a sufficient stock of observations for determining the inequalities of his orbit, and his longitude from the fixed stars.

But, to determine his longitude from the vernal equinox, a quadrant will be altogether requisite; whereby his meridional heights may be observed, and consequently it may be known after what spaces of time near the equinoxes his declinations are the same: which being once known, we shall no longer be ignorant of his true longitude from the equinoctial points at any given time.

The distances of Venus from the Sun, you will find taken sometimes when she was not more than 16 degrees removed from him; whereas Tycho and Hevelius could not observe her when less than 40°. These observations will determine all the inequalities of her orbit, much better therefore than any of theirs. For this advantage we are beholden to our telescopic sights, and the contrivance of the axis; without both which, it would be alike impossible to find or see her at those small distances from the Sun, in which I have observed her.

The observations of Mercury are few, by reason of our cloudy island situation: yet I esteem them much better than any made before my time, because I had the planet commonly higher than Tycho or Hevelius could find him.

The method of these observations is thus: first, I have caused the notes of those stars, that lie in the constellations on the zodiac, to be transcribed in the same order, as the signs succeed each other; then the rest in an alphabetical order. You will not find distances sufficient in every constellation, for there were some unfinished at the end of the year 1680, with which that volume concludes: but I think I have taken so many since, as may serve to complete them, if I had but the meridional heights of the stars whose distances I have observed. At present, I want instruments to take them, but if you please to encourage the work, as you have done hitherto, I hope we may hereafter obtain a quadrant or two, to complete the work.

Next the observations of the fixed stars, follow what I took of a pair of comets: the first, in April 1677, remarkable for a large head and small tail; the latter in December, 1680, January, February, 1680-1, on the contrary notable for the smallness of the head, the largeness of the train, and the arch it moved over, which was above $\frac{2}{3}$ of the ecliptic. These I esteem of the same matter with our planets; but because their motions are more remote, I place them before these, and after the fixed stars.

The observations of the planets follow; of which I shall only add to what I have told you before, that their true places in the ecliptic, grossly transcribed from the Ephemeris, are wrote in the margin against every day's observation.

The papers I delivered last into your hands contain, first, my observations of the Moon's distances from fixed stars, her appulses or transits by, and over . . . .

[Here this letter, which is extracted from MSS, vol. 42, page 32, terminates abruptly.]

No. 14.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

London, August 10, 1691.

Sir,

'Tis almost a fortnight since I intended, with Mr. Paget and another friend or two, to have given you a visit at Greenwich; but, sending to the Temple Coffee-house, I understood you had not been in London for two or three weeks before: which made me think you were retired to your living for a time. The bearer hereof, Mr. Gregory, Mathematic Professor of Edinburgh College in Scotland*, intended to have given you a visit with us. You will find him a very ingenious person, and good mathematician, worth your acquaintance. I hope it will not be long before you publish your catalogue of the fixed stars. In my opinion, it will be better to publish those of the first six magnitudes observed by others, and afterwards, by way of an appendix, to publish the new ones observed by yourself alone, than to let the former stay too long for the latter. I would willingly have your observations of Jupiter and Saturn for the 4 or 5 next years at least, before I think further of their theory: but I had rather have them for the next 12 or 15 years. If you and I live not long enough, Mr. Gregory and Mr. Halley are young men. When you observe the eclipses of Jupiter's satellites, I should be glad to know if in long telescopes the light of the satellite, immediately before it disappears, incline either to red or blue, or become more ruddy or more pale than before. Sir, I am your most humble servant,

Is. Newton.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford: but it is printed also in the *General Dictionary*, under the article "Gregory." F. B.]

No. 15.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, February 24, 1691-2.

Sir,

Though I have long delayed to return an answer to yours of the 10th of August last, yet I have always had it in my mind: and having now got a fit opportunity, I shall not longer decline it, lest you think me unmindful of our former friendship, or as unwilling, or unprepared to answer it, as I am represented to you. I did Mr. Gregory*, who brought it, all the kindness I could, without prejudice to an ingenious old friend who was much solicited by the University to put in for the vacant Professorship, but was prevailed with to decline, by the management of a person, who is always putting the question to my friends, why I do not print my observations? He might have satisfied you, and all others with whom he converses, if he pleases, of the reason; but I perceive, by yours, he is not desirous: but rather to gain me the ill opinion of my friends, from whom I have ever desired to deserve the best, and am confident I have ever endeavoured to serve very heartily. You advise me (and I am sure it is upon his suggestions and misrepresentions) to publish first a catalogue of the correct places of such fixed stars of the first six magnitudes as have been observed by others; and afterwards, by way of an appendix, to publish those new ones, observed by myself alone.

I take your advice very kindly, because I know you are sincere in it, and wish me all the success

* Dr. David Gregory, afterwards Savilian Professor of Astronomy at Oxford. F. B.

I can desire in my labours, and all the reputation they can deserve from them: and I shall give you very substantial reasons why I cannot do this at present, and show you what you may expect from me, and in what time hereafter; nor shall I forget to give such an answer as it deserves to our *friend's* question and calumny, in the close of my letter*.

It would be needless, as well as a tedious task, to give you the history of my observations; since I believe you are acquainted with it sufficiently, by what discourse I have had with you formerly: otherwise it would be requisite to give it, to vindicate myself in every particular of my conduct. I will only say that all my observations, made with the sextant, of the distances of the fixed stars from each other, were but *arena sine calce* till I had made my new meridional arch, which was not completed, but with more than 12 months' labor and £100 expense out of my own pocket in November was two years†.

On the first hour's work with it, I found all those conveniences in it, I had foreseen in my thoughts; and some more: my health was less exposed, and I could endure twice as long in the cold air, as I could with the sextant. And the difference of the observed times of the transits, with the meridional distances of any two stars from the vertex, gave the same difference of right ascensions that the observed distances with the sextant gave me; if the stars had nearly the same declination: if not, but the parallels were wide, a small correction was requisite, by reason the plane of the limb was not exactly in the plane of the meridian. But this is easily answered for, by a small table I made of the errors, from several observations taken the next summer, 1690. I found also that several of the fixed stars of the 4th, 5th, and many of the 6th light were omitted, both in Tycho's catalogues and Bayer's maps; and that I might as well take all these (as they applied to the meridian) as leave them out. If I omitted them, I must sit still from the time that one of the stars in the charts had passed till the next came in, sometimes 10, 12, or 14 minutes. If I took them, I kept myself employed; and my scribe's pains in writing down the notes was not to be valued.

I noted moreover that I could see several stars of the 7th light (though the catalogues account them of the 6th) with my bare eye, where no bright stars were near them; but where bright stars of the 2nd, 3rd, yea even 4th light, were near stars of the 6th, their splendor diminished their magnitudes so that I could see them with the bare eye but by glimpses, when the candles were hid; though the telescope on my index gave them their true magnitudes. All these, especially where they lay near the ecliptic, were to be inserted and taken: the telescopical I resolved to omit, as useless even in the appulses of the moon to them, except her light be very small, near the change.

I have lately transcribed my observations of all the fixed stars into a particular volume; where I have ranged those that belong to every constellation apart under it, beginning with those on the ecliptic, and placing the rest in the order of the alphabet. On the left-hand page are every day's observations of the times of the transits (of every star observed) in hours, minutes, and seconds of time; next this, their meridional distances from the vertex, in degrees and minutes only, uncorrect by refraction: in the 3rd column, their distances from the visible Pole; in the 4th, the magnitudes of such as were left out by Tycho, or Bayer, or had no letter in the charts of the latter. On the head of the right-hand page, is placed the right ascension of some principal star of that constellation, or some eminent one near it that past at the same time; and under it the rest of the stars are ranged

* The reader will very soon see that this pretended *friend* is no other than Dr. Halley. F. B.

† This is the *second* mural arch made by Mr. Abraham Sharp. F. B.

in such order as they pass the meridian one after another, supposing the principal to pass exactly at the time its right ascension gives. Next after this, are the distances from the vertex and pole, with the magnitudes as on the other page.

These tables of transits I design first for a guide to larger tables, where the true correct distance will be given, both from the vertex and pole; and the times of the transits will be turned into the true right ascensions of the stars, by the help only of two little tables prepared for that purpose; which, if I live some little time longer (as I hope through God's goodness I may) I shall do myself, and calculate the true longitudes and latitudes of the stars from them. If not, as I have ordered them it may be done by any faithful and diligent person that shall come after me: but I do not think the St. Helena observer of that number*.

A second use I make of these tables, is for drawing the charts of the constellations, which I do after a new manner (too long here to be described) so as the appearance to the naked eye is less distorted than by any projection I have yet seen. I intend to send you a specimen of a constellation, and the table of transits, ere long; at present I want leisure. Ten of the constellations are plotted; the rest will be done as soon as the cold weather ceases to interrupt us.

From these tables of transits I have taken lately a view of my work, and collected the epitome of it, into the included paper: wherein next after the name of every constellation, you have the number of stars I have observed in each. Those constellations that have a simple asterisk placed after them I account finished. Those which have a line after them thus (*—) I fear have some stars omitted. Those that have none on the left hand of the page, I know are all something defective, though nearly finished, Hydra excepted. Those on the right hand of the page, I have but begun. They seem not above one-third of what is done; but they will require twice the time and pains of as many as lie near the equator, to take them, by reason they pass high, near the vertex; or betwixt it and the pole, and so move slowly through the glass on the index. Besides, the observer that waits for them must be content with a very uneasy posture whilst he attends them. You see though I have been scarce 27 months at work, yet in this short time I have observed near twice the number of stars that are in Tycho's first catalogue; I believe I might have made them full twice as many, if I had had leisure to search some pages of my fair day-book diligently. And, by what I have before told you of the tables of transits, you will easily apprehend that, if it should please God to call me hence suddenly, they are in no danger of being lost, or rendered useless.

Tell me now sincerely (for I know you will do it) if you think it would be prudently done of me to leave off where I am, whilst I have strength and vigour (God be praised) to prosecute them? Would it, I say, be wisely done of me, to cease my designed observations of the constellations that yet remain to be taken or completed, to transcribe what I have done for the press, and to attend to it for 12 months, to gain a little present reputation? Would not even those men, who ask so peevishly why I do not print them? would not they tell me I might have staid another year or two, for all their idle talk, and have given them the whole complete?

The stars that are left out by Tycho and Bayer, are observed by me, together with those inserted in their tables: it will be a needless task for me to separate them, and distinguish mine from theirs; besides it will not only be useless, but breed confusion in my tables, and look more ambitiously than I can bear. I am sure those, that take a pride in judging what I do, would laugh at it, as a piece of vanity: though perhaps they would not blush to be guilty of it themselves. I despise their calumnies; and if you and Sir Christopher Wren, and my friend Mr. Caswell, to whom I shall send copies of

* This is another hint respecting Dr. Halley. F. B.

this letter, and one or two more ingenious men of my acquaintance, approve of my proceedings, I value not the little tricks or suggestions, of any malicious or envious pretender to what he understands not*.

I hope this answer satisfies you; I shall only add that whilst I have been thus busily employed on the constellations, I have not neglected the planets. They have been taken at all their remarkable appearances in this small space of 27 months. I have observed the moon about 150 times in the meridian, and calculated her true place, not only from the observations, but from my own tables: whereby I find their errors sometimes near 20 minutes: which I formerly thought scarce ever exceeded 16. The number of lunar observations are five times as big as any other astronomer ever saw together; and for exactness I am confident (and speak it without vanity or ostentation, because it is demonstrable) they are not to be doubted of, nor yield to any that went before me: and the method of calculation is so easy, that any one that has a mind to try them after me, needs not grudge the labor; though I think it needless, having caused them all to be done twice over for certainty.

There remains but one particular of your letter to answer, and that relates to Jupiter's satellites. I am so intent, whilst observing, on the moments they disappear, that I seldom give heed to such circumstances as you enquire; only thus much I can say, that they begin to lose their light 2 or 3 minutes before they disappear, and grow fainter and duller and smaller till they diminish to a point, and vanish. I cannot say that I ever saw any change to a bluish or red; but duskish when I used a glass of 27 feet. I cannot make use of it now, because the planet moves too high; but, when I do next, I shall be mindful of your query.

It only remains that I give you the answer I would make to our suggesting *friend*, when he asks me why I do not print my observations? 'Tis first I do not find myself under any obligations to receive instructions what to do, or be governed by him and his associates, the *Muss's*†. Secondly, I would not thrust such an incomplete catalogue on the world as he has done from St. Helena: nor be obliged to compliment the best reputed astronomers of our time (as he has done all of them) by telling them that, had their catalogues been extant, he would have called his a supplement to theirs, as he has done (for want of them) of Tycho's. Nor will I give any one occasion to tell the world I have erred a 60th part of what La Hire has published he does in a star of the Crosiers and one of the Centaur: that I understand what I have to do, much better than he; and when, and how, it will be best for me to publish my own labors: that I will not be beholden to him for his assistance or advice: that if he wants employment for his time, he may go on with his sea projects, or square the superficies of cylindric ungulas: find reasons for the change of the variation, or give us a true account of all his St. Helena exploits; and that he had better do it, than buffoon those to the Society, to whom he has been more obliged than he dares acknowledge: that he has more of mine in his

* In another draught of this letter, alluded to in page 133, Flamsteed expresses himself thus:—"Tell me now "sincerely whether you think it would be discreetly done of me, to leave the prosecution of my work, which has "thriven thus successfully under my hands in 27 months, to satisfy the clamors of unreasonable and malicious "people, or not? I make you judge, with Sir Christopher Wren and Mr. Caswell, whether I ought to publish "some part of my observations before the rest; or only those stars in Tycho's catalogues, and leave others, more "remarkable than they, for an additional work. Tell me, if you please, whether it would be prudently done of me "to leave my remaining constellations for a year, to copy papers and put them in order for the press, to gratify "one only calumniating libertine, and stop his mouth, and those he has filled with his trifling and envious "suggestions?" F. B.

† I do not know what is meant by this word, unless it is intended, in a dictatorial sense, for the word *must*. It is used in another draught of the letter, alluded to in page 133. F. B.

hands already, than he will either own or restore; and that I have no esteem of a man who has lost his reputation, both for skill, candour, and ingenuity, by silly tricks, ingratitude, and foolish prate: and that I value not all, or any of the shame of him and his infidel companions; being very well satisfied that if Christ and his apostles were to walk again upon earth, they should not escape free from the calumnies of their venomous tongues. But I hate his ill manners, not the man: were he either honest, or but civil, there is none in whose company I could rather desire to be.

But my letter makes you now do penance. I beg your pardon for a just indignation, to which some very foolish behaviour of his very lately has moved me: and desire you to assure yourself, that no one is more sincerely your servant, than your affectionate friend and brother,

JOHN FLAMSTEED.

[Extracted from MSS, vol. 42, page 129. There are, however, three other draughts of this letter, varying from each other in some particulars, all in Flamsteed's hand writing, and all inserted in MSS, vol. 35, pages 1–14: but that which I have here given, is (I believe) the copy of the one that was actually sent to Sir Isaac Newton. F. B.]

No. 16.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, October 7, 1694.

SIR,

Since my return hither, I have been comparing your observations with my theory, and now I have satisfied myself that, by both together, the moon's theory may be reduced to a good degree of exactness: perhaps to the exactness of 2 or 3 minutes. I forbore writing to you a few days, till I had considered your observations, that I might be able to acquaint what further observations are requisite. And besides those 50, which you tell me you have, ready calculated, and those I have already, your observations of this winter will be very material: and therefore I am very glad you have ordered your servant to calculate them. There are requisite also your observations for the last 6 or 7 years, made in the months of March, June, September, and December, when the moon's perigee or apogee is in the syzygies or quadratures; or within 5 or 6 degrees of those cardinal points: and the moon in the quadratures or opposition, and in an eclipse of the sun. When the moon, in these cases, is in the quadratures, or opposition, it will be requisite to have two observations; one a few hours before the quadrature or opposition, and the other a few hours after: there being a day between the observations. If in the lunation of this present month, you can get two or three observations about the first quadrature, pray will you endeavour to get as many opposite to them about the last quadrature? For, observations opposite to one another (when the moon's apogee is in the octants) are of great moment.

By such a set of observations I believe I could set right the moon's theory this winter. Only, it would be requisite to have about 50 of them, such as I should select, set right by the *new* places of the fixed stars. The observations in March, June, September, and December above-mentioned will not be many.

I thank you heartily for your receipt. At present I beg your observations of Jupiter and Saturn: and what you send by penny post direct for Mr. William Martin, a Cambridge carrier at the Bull in Bishopsgate-street; and order it to be delivered there before 2 of the clock on Monday, lest he be gone: for he goes every Monday, at 2 o'clock, from London to Cambridge. I am yours to serve you,

I^S. NEWTON.

[Copied from the *General Dictionary*, Article "Newton."]

No. 17.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

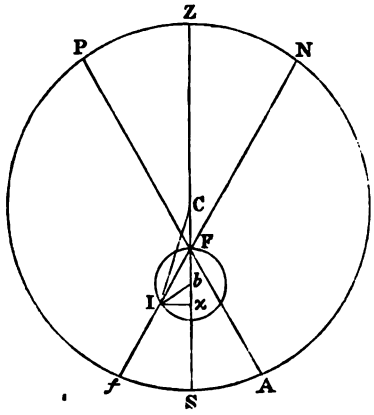
Observatory, October 11, 1694.

Sir,

I have yours of the 7th instant: before it arrived I had prepared a letter to you, which I sent not: because I was too late for the post. I shall give you the contents of it; and then answer that I received last night.

After you were gone hence, Mr. Halley applied himself to me, and desired I would allow him to see the lunar observations I had imparted to you. I told him that I should not be unwilling, provided that he in like manner would impart what he had talked so much of to the Society; his amendments of the lunar theory. We had some discourse of it: and he told me that there was an equation of about 9′ necessary in the quadrature: that this was begun and ended in the line of the syzygies; and occasioned the variation in the octants to be 7′ or 8′ greater or less than the tables make it. This I perceived was your equation; and told him so. He was silent.

Soon after, he came to Greenwich with one friend only in his company. I was surprised at it; and took the occasion of minding him of his disingenuous behaviour in several particulars; which he bore because he could not excuse it. Afterwards I showed him the synopses; and suffered him to take a very few notes of the greatest differences of the observations from the tables; and affirmed the equations of the tables generally too small, by reason the excentricity was too little.



In Mr. Horrox's system, the doubled distance of the sun from the moon's apogee being numbered from F in the periphery of the little libratory circle to I, a perpendicular let fall from this point I on the syzygiacal line Z S, where it cuts it in χ, makes the present excentricity C χ. But he affirms that not C χ, but C I, is the excentricity in this case, and in all other cases.

Mr. Street changed the diameter of Mr. Horrox's libratory circle a very little, so as the angle I C χ was always the equation of the apogee: so altering the diameter of the circle, all the present equations will be increased and diminished with it, except the variation.

To make the equations bigger in winter than in summer, it will be requisite to make the diameter of this libratory circle bigger in winter than in summer; which on your principles I affirmed and he assented to: but in what proportions he said not. So I perceive he is still in the dark in this point; and wants to know your determination. He mentioned an inequality, depending on the moon's distances from the node, of which as I remember you gave me a hint in discourse; and yesterday, at London, desired me to help him to observations made in

1687, from the quarter to the full.
1688, in March, about the last quarter.
1687, in November, about the first quarter.
1692, increasing in March, decreasing in December.

Your letter makes demands something like these, and pointing at the same thing: I shall endeavour to satisfy you within a week's time. But hence I gather that you have given him some hints where the greatest errors lie; and he is in pursuit of them.

Whilst you were at London, I began to examine my observations of the distances of the declining sun, from Venus, in order to find what the refractions were, and in what proportions they increased as he descends. I employed for this purpose her distances from him observed February 23rd, April 11th, 21st, 23rd, 25th, and 26th, 1681. The calculations were long and troublesome; the result I have drawn up in a synopsis too large to be transcribed into a letter, of which yet I may give you a copy when I have better leisure: at present, I send you an empirical small table of the difference of the refractions of the Sun and Venus in height. When the Sun set, Venus was in all of them above 30 and not more than 37 degrees high. Where the French, and my old tables, make her refraction about $1\frac{1}{2}'$, add so much to the differences, it gives the entire refraction of the Sun.

The observations of February 26th make these differences bigger than those of April 11th, 21st, 23rd, 25th, by almost a minute; those of April 26th are the least, and about $1\frac{1}{2}$ less than February's. Yet all agree to make this difference, when the point observed is truly 1 degree high, to be $19\frac{1}{2}$; which shows that the refractions are not so irregular near the horizon, as they are commonly esteemed.

The refractions at	M. Cassini.		La Hire.		Boucher.		The Observations.	
°	′	″	′	″	′	″	′	″
90	32	20	32	0	30	0	33	0
89	27	56	26	25	25	12	23	0
88	21	4	20	43	18	24	17	30
87	16	6	15	44	13	55	14	0

Mr. Boucher is an English gentleman, now in Jamaica if living, who formed his table on Tycho's observations and the Cartesian theory.

But you see all the theories err in this, that they make the refractions to decrease but about 5′ betwixt 89 and 90: whereas betwixt 88 and 89 they decrease above 6′. On the contrary, the observations make $5\frac{1}{2}'$ betwixt 88 and 89, but 10′ betwixt 89 and the horizon. What may be the occasion of this, I have not leisure at present to inquire. It seems only the medium, in which the refractions are made, is not equable as supposed by those who build their tables upon theories. This subject deserves your consideration: I desire your thoughts of it at your leisure. The observations, and what I deduce from them, are incontestable.

Whilst I was on this subject of refractions I received from France the *Voyages Astronomiques:* but the title is *Recueil d'Observations*, &c, in folio. It contains what Mr. Richer did at Cayenne, Mr. Pichart's voyage to Uranibug, his, Cassini's, and La Hire's to the sea-ports of France, with their longitudes and latitudes determined by observation. The voyage to Goree is not omitted. But the best part of it, and greatest, is Cassini's new tables of Jupiter's satellites, wherein he has corrected the motions of the first. He fits his radixes to the oppositions of the Sun and Jupiter (very inartificially) to cover the equation of light (arising from the motion of the Earth in its orb); which he makes when greatest 14 minutes, and always by this means has it additional. That other part of it which arises from the change of Jupiter's distance from the Sun he omits. Again, whereas the first satellite moves about 2° 0′ in 14 minutes of time, he divides this in the proportion of versed sines, and makes a table of it; which he applies to all the satellites, without any reason that I can perceive, but because it helps to solve two eclipses of the 2nd, which may be solved perhaps without

it. By applying this to the 3rd, he renders its motions worse than they were; to me it seems equable, and to need no equations at all. He gives no examples of calculating the places of the 3rd or 4th; I am apt to think because he found this device would not agree in them. The motions of the 4th are the same with those of his old tables.

I told you that my observations would allow your greatest elongations bigger than I had stated them; it seems his does the same, and that he allows their distances to be in sesquialter proportion to the periods of their revolutions. Though, to be thought a good Catholic, he says nothing of it, but conceals it; as he does his allowing the equation of light: for he makes their distances from Jupiter in semidiameters and sexagessional parts to be

	Sem.				Sem.	
Of the 1st,	5°	40′	Or in semidiam. and cents,	1st,	5°	66′
2nd,	9	00		2nd,	9	00
3rd,	14	23		3rd,	14	38
4th,	25	18		4th,	25	30

I give you them thus, to prevent mistaking them; as my friend Mr. Townley did.

He still supposes them to have one plane of their orbits: but I am apt to believe the orbit of the 2nd lies out of the plane of the orbits of the rest; whose inclination to Jupiter's he now makes 2° 55′; whereas formerly he allowed it but 2° 40′. Had he known how to calculate the length of the line of the passage through an oval shadow, as well as through a circular, he needed not to have enlarged it so much; for he does it only to make the durations of the eclipses shorter, as the observations require, and the oval shadow renders them.

You asked me once when with you, what were the diameters of the satellites. It is impossible to determine them exactly: but, as well as I could when I made my tables, I stated the angles of their diameters, subtended at Jupiter, of the

1st,	28′,	or in parts, such as Jupiter's semid. is 1000.	46
2nd,	15		40
3rd,	12		53
4th,	4		25

I must add that, whereas you told me that the parallactic equation proving double to what you esteemed it, before you saw my observations, argued the earth to be bigger, it seems to me the contrary: that her flying off further than you thought, from the common centre of gravity betwixt her and the moon, argues she should be less. I desire to be better informed in this particular at your leisure.

Sir, I am yours to serve you,

John Flamsteed.

P.S. I shall write to you again, as soon as I can get another synopsis transcribed. At present I am very busy about some other papers I am to send to a philosophical friend. J. F.

To Mr. Isaak Newton, at Trinity College, in Cambridge.

[Extracted from MSS, vol. 42, page 151.]

No. 18.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Trin. Coll. October 24, 1694.

SIR,

I return my hearty thanks to you for the communications in your last; and particularly for your table of refractions near the horizon. The reason of the different refractions, near the horizon, in the same altitude, I take to be the different heat of the air in the lower region. For, when the air is rarefied by heat, it refracts less: when condensed by cold, it refracts more. And this difference must be most sensible when the rays run along in the lower region of the air for a great many miles together; because 'tis this region only which is rarefied and condensed by heat and cold: the middle and upper region of the air being always cold. I am of opinion also that the refraction in all greater altitudes is varied a little by the different weight of the air discovered by the baroscope. For, when the air is heavier, and by consequence denser, it must refract something more than when 'tis lighter and rarer. I could wish therefore that in all your observations, where the refraction is to be allowed for, you would set down the weight of the baroscope, and heat of the air; that the variation of the refraction by the weight and heat of the air may be hereafter allowed for, when the proportion of the variation by those causes shall be known*.

A day or two before I left London, I dined with Mr Halley, and had much discourse with him about the moon. I told him of the parallactic equation, amounting to about 8′ or 9′, or at most 10′: and of another equation, which is the greatest in the octants of the moon's apogee, and might there amount to about 6′ or 7′; though I had not yet computed anything about it. He replied that he believed there might be also an equation depending upon the moon's nodes: to which I answered that there was such an equation, but so little as to be almost inconsiderable. But, what kind of equation this was, I did not tell him; and I believe he does not yet know it, because it is too little to be found out by observations, or by any other way than the theory of gravity. He told me, some years ago, his correction of the moon's eccentricity, and repeated it when I was with him last in London: and this made me free in communicating my [many?] things with him. By your observations I find it to be a very good correction. I reckoned it a secret which he had intrusted with me; and therefore never spake of it till now. Upon my saying that I hoped to mend the moon's theory by some observations you had communicated to me, and that those observations made the parallactic equation in the quadratures between 8′ and 10′, he was desirous to view them. But, I told him he must not take it ill if I refused him that, because I stood engaged to communicate them to nobody without your consent. I am very glad that there is like to be a new correspondence between you; and hope it will end in friendship.

The parallactic equation depends not upon the common centre of gravity of the earth and moon, but upon another centre, whose distance from the centre of the earth is as the square of the diameter of the moon's orbit: and therefore makes that equation proportional to that diameter. But, this equation is less than I took it to be when I saw you last. 'Tis so involved with other equations that I cannot determine its just quantity till I have your observations in other positions of the moon's apogee.

In that new synopsis of observations you are drawing up, pray insert the distance of the moon from

* It would have been fortunate had Flamsteed attended to this important hint given by Newton, to observe the thermometer and barometer. Not that it would have made much difference in the places of the stars in the *British Catalogue;* because the major part of the observations, from which they were deduced, were made prior to the period here mentioned. F. B.

the sun: for I must correct the variation, which I cannot well do without your numbers. In the second of those two synopses you communicated to me, I was fain to compute it: but that was not so well as to have those very numbers by which you computed the moon's place. Pray insert also the columns which relate to the moon's latitude; because the theory of her latitude needs some amendment. I am, your very humble servant,

I^S. Newton.

[Copied from the *General Dictionary*, article "Newton."]

No. 19.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, Novem. 1, 1694.

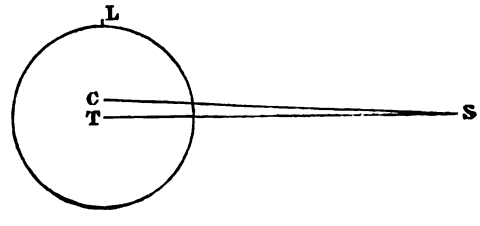
Sir,

A day or two after I wrote to you I received your letter, with an emendation of your observation, Feb. 27, 1691. You say the moon's place at that time ought to be 3ˢ. 7° 56′ 8″, and so the error, 9′ 38″. I suppose it should be 3ˢ. 17° 56′ 8″, and so the error, 10′ 33″. For her observed place in the synopsis you gave me is ♎ 17° 45′ 35″; unless you have corrected it by the *new* places of the fixed stars.

There are some other faulty observations, particularly those of Feb. 21, March 12, April 7, May 22, July 1, July 30, Sept. 6, 1690. But whether the faults lie in the calculated places, or in the observed ones, or in the places of the fixed stars, I cannot tell.

Mr. Caswell's magnetical observations you need not send me; for I have no occasion for anything of that kind. Neither need you send me your larger synopsis of the refractions: that short one, which I have already, is sufficient for me. I desire only such observations as tend to perfecting the theory of the planets, in order to a second edition of my book: and would not give you the trouble of superfluous communications. The greatest equation of physical parts, I told you, was by my calculation 13′: and now by your observations I find it is about 12′ or 13′. 1. The variation in spring and autumn is about 36′ or 35½′: in winter it is greater, and in summer less, by two or three minutes; and in the moon's apogee it is greater by two or three minutes than in her perigee. 2. The eccentricity and equation of the moon's orbit is sensibly greater in winter than in summer, and seems to be sometimes as great as Mr. Halley makes it; but the law of its increase I am not master of; nor can be, till I have seen the course of the moon as well when her apogee is in the summer signs as in the winter ones. For those observations, you gave me at London, contain only her course when her apogee is in the winter ones. The equation which depends upon the moon's nodes is too little to be sensible by your observations, till they are corrected by the new places of the fixed stars. I only see, in general, by my theory, that there is such an equation, and by your observations that the theory and the heavens agree so far as I have been able to compare them hitherto.

In my two letters I quite forgot to explain to you the menstrual parallax of the sun. Let S be the sun, T be the earth, L the moon in the first quarter, and C the common centre of gravity of the moon and earth. This common centre of gravity, whilst the moon and earth move above it, moves regularly in the *orbis magnus*: so that when you have computed the place of the earth, you are to place the point C in that place, and set the earth T



forwarder by adding the angle C S T to the computed place: but if the moon be in the last quarter, you are to subtract that angle. The quantity of this angle I do not yet know certainly: it is not so great as I thought when I was at London. If you assume it 16″ or 20″, and find that by such an assumption the greatest errors of the sun's place are diminished, you may retain that quantity, till it shall be determined more exactly.

I am, Sir, your faithful friend and humble servant,

I^S. NEWTON.

Note written on the letter, by Flamsteed. Thursday, Nov. 25.—I wrote to Mr. Newton that I would send him the synopsis of refractions. I sent them on Monday, 29th, with the 3rd synopsis of lunar observations. He writes me this answer, Novem. 1st, when he had received them if the carrier performed his duty. Query, why he says nothing of the receipt?

[Copied from the *original* MS, in the Library of Corpus Christi College, Oxford.]

No. 20.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, Novem. 17, 1694.

SIR,

The carrier came without your parcel, but had it sent after him; and I received it about three hours after I had sent away my last letter to you. I like Mr. Caswell's experiments well: they deserve to be made public. I have taken copies of your other papers, and designed to return them the last week; but that I could not get my copies collated soon enough. They shall be sent to-morrow, together with a table of refractions, which I have computed by applying a certain theorem to your observations: for, being at a stand about the moon's theory, I set myself to compute this table. The first column expresses the refraction in mid-winter in time of a gentle frost, and agrees almost with your observations of Feb. 23. The third column expresses the refraction in the usual heat of July, and agrees almost with your observations of April 26. The middle column expresses the refraction in a middle degree of heat, and agrees with your observations of April 21, 23, and 25, most nearly. The proportion of the first to the third, I determined by the rarefaction of the air in winter and summer; which I found some time ago, by certain experiments, to be as 8 to 9, or thereabouts. You may communicate this table to Mr. Halley, if you think fit.

I believe there may be more faulty observations in your synopsis than I have yet discovered, and I suspect that of Sept. 30, 1690; though I cannot well judge of it, because there are no other observations near it to compare it with. So also, that of Feb. 6, 1691, seems faulty. Pray see in your book if these observations be not dubious. For, as for the places calculated from the tables, I will give you no trouble about them: my servant has lately learnt arithmetic; and, if I go on with this business of the moon, he shall learn astronomical calculations and examine them, and I will send you his corrections.

I believe you have a wrong notion of my method in determining the moon's motions*: for I have not been about making such corrections as you seem to suppose, but about getting a general notion of all the equations on which her motions depend; and considering how afterwards I shall go to work, with least labor and most exactness, to determine them. For the vulgar way of approaching by degrees is bungling and tedious. The method which I propose to myself, is, first to get a

* *Note, written on the letter, by Flamsteed.* I had; and he of me: and still has.

general notion of the equations to be determined, and then by accurate observations to determine them. If I can compass the first part of my design, I do not doubt but to compass the second: and that made me write to you, that I hoped to determine her theory to the exactness of two or three minutes. But I am not yet master of the first work; nor can be, till I have seen something of the moon's motions when her apogee is in the summer signs: and to go about the second work, till I am master of the first, would be injudicious; there being a complication of small equations which can never be determined till one sees the way of distinguishing them, and attributing to each their proper phenomena. Sir, if you can have but a little patience with me till I have satisfied myself about these things*, and make the theory fit to be communicated without danger of error, I do intend that you shall be the first man to whom I will communicate it†.

And because I would give you as little trouble as may be, if you please to communicate to me the right ascensions and apparent meridional altitudes of the moon, as you have found them in your observations, without allowing for the refraction and parallax, I will take care of all the rest, and return your synopsis of her longitudes and latitudes, &c. But I desire her right ascensions by the correct places of the fixed stars: for otherwise, your observations will not reach to distinguish and determine those small equations which remain to be found out: and I would not have the work to do over a second time. This may give you a little trouble at present, but it will save you ten times the trouble which you must otherwise undergo hereafter; and that perhaps without bringing the moon's theory to half that perfection which I think I have a prospect of. If you please to do me this favor, then I desire that you would send the right ascensions and meridional altitudes of the moon, in your observations of the last six months. You may do it in three columns under these titles—

Tempus apparens Grenovici.	*Lunæ ascensio recta observata.*	*Lunæ altitudo meridiana apparens.*

And for the trouble you are at in this business, besides the pains you will save of calculating (and that upon an erroneous hypothesis as I must do) the observations you communicate to me, and the satisfaction you will have to see the theory you have ushered into the world brought (as I hope) to competent perfection, and received by astronomers, I do intend to gratify you to your satisfaction: though at present I return you only thanks; as I do heartily for what you have already communicated.

I am, your affectionate and humble servant,

Is. Newton.

P.S. I sent your papers‡ back by the carrier yesterday, and this letter should have been sent by the post before.

* *Note, written on the letter, by Flamsteed.* As much as he pleases: I have waited 5 years for them.

† This passage is in reply to a remark which Flamsteed had made, in a preceding letter, that Dr. Halley had asserted "that Mr. Newton had done the theory of the moon:" whereupon, says Flamsteed, "I wrote to him for the "performance of his engagement, not taking any notice he had forgot it." On this answer of Newton, Flamsteed makes the following remark: "Satisfied herewith that Mr. Halley's talk was only boast, I troubled him no more "about it: though I found he had forgot his first engagement, as he has done his *intention* (for so he termed it) "since." See the statement of these particulars in MSS, vol. 35, page 152. F. B.

‡ *Note, written on the letter, by Flamsteed.* The 2 synopses containing above 100 places of the moon observed, compared with my old tables, and the synopses of the ☽'s places calculated and compared together with the elements of the calculations.

Tabula Refractionum.

Altitudo apparens.		Refractio æstiva.		Refractio verna et aut.		Refractio hyberna.		Altitudo apparens.	Refractio verna et aut.		Altitudo apparens.	Refractio verna et aut.	
gr.	′	′	″	′	″	′	″						
0	00	31	30	33	20	35	10	gr.	′	″	gr.	′	″
0	30	26	06	27	45	29	24	31	1	28	61	0	29
1	00	21	50	23	12	24	34	32	1	24	62	0	28
1	30	18	51	20	2	21	13	33	1	21	63	0	27
2	00	16	27	17	29	18	31	34	1	18	64	0	26
2	30	14	31	15	23	16	15	35	1	15	65	0	24
3	00	12	52	13	40	14	28	36	1	13	66	0	23
3	30	11	32	12	15	12	58	37	1	10	67	0	22
4	00	10	25	11	4	11	43	38	1	8	68	0	21
4	30	9	29	10	5	10	41	39	1	5	69	0	20
5	00	8	40	9	13	9	46	40	1	3	70	0	19
6	00	7	24	7	52	8	20	41	1	1	71	0	18
7	00	6	27	6	51	7	15	42	0	59	72	0	17
8	00	5	42	6	3	6	24	43	0	57	73	0	16
9	00	5	5	5	24	5	43	44	0	55	74	0	15
10	00	4	36	4	53	5	10	45	0	53	75	0	14
11	00	4	11	4	27	4	43	46	0	51	76	0	13
12	00	3	51	4	5	4	19	47	0	49	77	0	12
13	00	3	33	3	46	3	59	48	0	48	78	0	11
14	00	3	18	3	30	3	42	49	0	46	79	0	10
15	00	3	4	3	16	3	28	50	0	44	80	0	9
16	00	2	52	3	3	3	14	51	0	43	81	0	8
17	00	2	42	2	52	3	2	52	0	41	82	0	7
18	00	2	33	2	42	2	51	53	0	40	83	0	6
19	00	2	24	2	33	2	42	54	0	38	84	0	5
20	00	2	17	2	25	2	33	55	0	37	85	0	5
21	00	2	9	2	17	2	25	56	0	35	86	0	4
22	00	2	2	2	10	2	18	57	0	34	87	0	3
23	00	1	57	2	4	2	11	58	0	33	88	0	2
24	00	1	51	1	58	2	5	59	0	32	89	0	1
25	00	1	46	1	53	2	00	60	0	30	90	0	0
26	00	1	42	1	48	1	54						
27	00	1	37	1	43	1	49						
28	00	1	33	1	39	1	45						
29	00	1	30	1	35	1	40						
30	00	1	26	1	31	1	36						

[*The following statements are also written on the preceding letter by Flamsteed; but evidently at some distance of time after the receipt of it. F. B.*]

Lunar observations and the Moon's calculated places imparted to him.

In the 1st synopsis	52	from Nov. 16, 1689, to June 11, 1690, inclusive.	
2nd	50	April 27, 1691	
3rd	55	16, 1692	
In a letter, Feb. 7, 1694-5	6	June 16, 1692	
Toto	163		
	26		
	189		

1694, Sept. 15	1694, Oct. 30	1694, Dec. 31	Moon's obs.		
21	31	1695, Jan. 8	1695, Feb. 3	1695, Apr. 9	1695, Maii 27
22	Nov. 17	9	14	15	Junii 6
27	18	11	16	17	11
Oct. 1	Dec. 11	12	25	18	12
16	13	13	Mar. 8	20	14
18	16	14	9	:: 23	15
21	28	18	13	Maii 13	16
22	30	—	14	15	17
		Toto = 26	16	24	18
calculated, and imparted to Mr. Newton.			Apr. 6	26	25
					—
					not calculated = 30

Let the world judge whether Mr. Newton had any cause to complain of want of observations, when all these were imparted to him. I was ill of the headache all the summer, which ended in a fit of the stone: yet I forbore not, as I was able, to serve him without reward, or the prospect of any. I contend it.

1st synopsis . . .	52
2nd „ . . .	50
3rd „ . . .	55
In a letter	6
More as above . .	26
More, pp. 81 and 83 Lib. Calc.	12
Calculated =	201

Besides appulses and eclipses published.
Places of Saturn.
„ Jupiter.
Refractions calculated.

Taken with the arc .	30
Taken with the sextant—	
1677, Januarii . . .	16
Feb. . . .	8
Martii . . .	27
——	29
——	30
Aprilis . . .	28
Maii . .	30
Junii . . .	10
——	27
——	28
Julii . . .	4
	6
Uncalculated =	243

[Copied from the *original* MS, in the Library of Corpus Christi College, Oxford.]

No. 21.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, Dec. 4, 1694.

SIR,

The table of refractions I sent you I do not design to publish. 'Tis not so accurate as it may be made, and I believe the refractions above the altitude of 15° are something too little, but if you go to examine it by the hypothesis of refraction being made at the top of the atmosphere, you are upon a wrong bottom; for this table was computed upon a better foundation. However, there being a certain circumstance omitted in computing it, I intend to examine it with allowance for that circumstance, and when I have set it right I will send you a new copy of it. Perhaps for determining the difference of the refractions in winter and summer, it would not be amiss to observe the refractions of a fixed star in the altitude of 3° or 4° some time this winter in frosty weather.

I thank you for complying with my request of sending me the observed right ascensions and meridian altitudes of the moon, and for the catalogue you have given me of your observations. If you please to send me those of August, September, and December, 1692, and those of January, March, April, and October, 1693, and all those of the year 1694, except the three first (that is, the observation of Jan. 25, and all those that follow) you will oblige me. Also in the year 1693 add the observations of Sept. 30 and November 2.

I am glad your cold is going off. I hope you are pretty well recovered of it before this time. Pray, this next moon, make all the observations you can, and begin your observations when the moon is in the first octant if you can. For the position of the apogee in the Sun's opposition in mid winter is a case of great moment and will not return in many years. The observations in the full and both the quadratures are of greatest moment, but all the rest are useful; and my method does best where the observations are continual. A little diligence in making frequent observations this month and another month or two hereafter, will signify more towards setting right the moon's theory than the scattered observations of many years.

I am, in haste, your very humble servant,

I^S. NEWTON.

[*Note, written on the letter, by Flamsteed.*]

* * * I was ill now of the head-ache; not being able to calculate, I sent him the observations that he might compare the ☽'s places from them himself. My work of the fixed stars was interrupted also by my distemper.

[Copied from the *original* MS, in the Library of Corpus Christi College, Oxford.]

No. 22.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, December 6, 1694.

SIR,

I am glad I did not impart your table of refractions to any body (since I find you have better considered and think of altering it) since you were not pleased to impart the foundations on which you calculated it to me. I have been seeking of them and at last found a way of answering them, admitting 2 spheres of vapors, one the usual height about 2½ miles, the other much less, with two horizontal refractions; and with little labor have answered those under 5 degrees within half a minute, those above much nearer.

By the way I have examined the tables of refraction of Kepler, Cassini, Picard, Boucher, and

La Hire. The four first I find built all on the same foundation, which supposes the refractions made in an equable sphere of vapors, about 2½ miles high: some more, others a little less. But La Hire's, the last, is not built on the same principle. For his refractions from the horizon upwards are all too big for his horizontal, and more at a distance than near it. If you have not his table I will send you a copy of it in my next.

Considering the uncertainty of these refractions, I continued to find out the inequalities of the earth's motion, without any consideration either of them or the latitude of the observatory. But now I come to settle the distances of the fixed stars from the visible pole, I must determine them, and therefore should be glad to know the foundations of the table: which, if you please to impart, I shall as a suitable return afford you what other observations I have made of them, which are no less considerable than those I have already imparted*.

I know very well the equations of the moon's motion are the highest this month and the next, that they can be again this 9 years: and had therefore determined to let slip no opportunity of observing her. My indisposition has not hindered me; but the fogs and clouds have kept her from my view since the first quadrature of the last month till now the clouds seem to break, and if it proves frost I promise myself fair weather, and frequent opportunities of determining her place in the meridian, which you need not doubt but will be imparted to you. But, I must intreat you to be patient and bear with me for a little time: for I must visit my cure at Christmas, and prepare before for my journey to it, which will employ me some days: so that I cannot give you the places of the moon you desire, till after the holidays. But then you shall have them, if God spare me life and health; and without any consideration or recompense but such communications as are usually made betwixt persons conversant in the same sort of studies.

I admire at the P.S. of your last letter that mentions another sort of recompenses. But I considered that you might be possessed with the character which a malicious false friend had spread of me, and so resolved then to take no notice of. But in my next, when I was less moved, to assure you that I never received any reward for any thing I imparted to any ingenious person, and always scorned the thought of it. I am a friend, I confess, to frugality: but not for the sparing of money, but to avoid ostentation, useless disturbance, and especially, as much as I may, for the saving my time, which is very precious with me, by reason of my frequent indispositions and avocations by company and visitors to which this place subjects me. I have always had money, more perhaps than I desired, at my command; I bless God for it: and I never took any thing of any for communicating of my skill or pains, except of those who forced themselves upon me to devour my time, and could not make me any other recompense otherways than by their pay. Pray therefore lay by any prejudicial thoughts of me, which may have crept into you by malicious suggestions: and assure yourself that without the prospect or thought of any other reward than like communications, you may and shall ever freely command the pains of,

Sir, your affectionate and sincere friend and servant,

JOHN FLAMSTEED.

P.S. I design to bestow a little pains again on the correction of the satellites' motions: and should therefore be very much obliged to you, if you would mind a request I made to you in one of my first letters after you went hence, what the physical parts are in each of the satellites.

In my last, I desired to be informed whether you had not been presented with a Latin geometrical

* There are some investigations and tables by Flamsteed, relative to refraction, in MSS, vol. 33, page 24, numbered from the *end*. F. B.

tract of Viviani's in 4to: you give no answer. If you please to afford me one, I shall make no ill use of it, and it will much oblige me.

You requested of me the places of Saturn, observed these three last years, and the differences from the Rudolphine tables. I have recalculated them during my sickness; and, unasked, again present you with them.

Date.	Hora apparens.	Saturni anomal.	♄ log. dis. ☉ curtatæ.	loca ☉'s.	loca ♄ supp.	loca ♄ observ.	Differ.
	d. h. m. s.	s. ° ′ ″		° ′ ″	° ′ ″	° ′ ″	′ ″
1692, May	16 12 10 30	11 9 7 51	6·000645	♊ 6 26 9	♐ 8 49 20	♐ 8 43 30	−5 50
1692, May	19 11 57 30	11 9 13 53	6·000658	♊ 9 17 4	♐ 8 39 34	8 30 15	−5 19
1693, June	2 11 48 —	11 21 53 42	6·001887	♊ 22 26 43	♐ 19 46 47	19 41 45	−5 2
1694, June	15 11 43 —	0 4 32 31	6·002109	♋ 4 34 51	♍ 1 0 9	♍ 0 56 0	−4 9

These you may add to the first large table of Saturn's computed and observed places compared. I think I gave you the observations of May 5, 1691, some time since. If I did not, acquaint me, and it shall be sent you in my next, by yours, J. F.

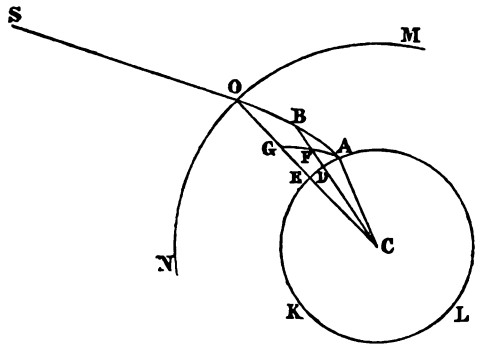
[Copied from the *original* letter in Birch's *additional* MSS, No. 4292, in the British Museum.]

No. 23.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, December 20th, 1664.

Sir,

The foundation of the table of refractions I concealed not as a secret, but omitted through the haste I was in, when I wrote my last letter. But, since you desire it, I will now set it down. Let A K L represent the globe of the earth, and suppose this globe is covered with an atmosphere of air whose density decreases uniformly from the earth upwards to the top which is here represented by the circle M O N. And let S O be a ray of light falling on the top of this atmosphere at O, and in its passage from thence through the atmosphere to the spectator at A, continually refracted and bent in the curve line O B A. From any point of this curve line B to the centre of the earth, draw the right line B C, cutting the surface of the earth in D, and take C F a mean proportional between C B and C D, and let A F G be the locus of the point F; that is, the curve line in which the point F will be always found: and if this curve line A F G cut the right line O C in G, the whole refraction of the ray in passing from O to A will be proportional to the area A F G C; and the refraction in passing through any part of that line O B or B A will be proportional to the corresponding part of the area G F C G or F A C F.



This theorem is geometrically demonstrable, but the demonstration is too intricate to be set down in a letter.

Now as my table of refractions, computed from this theorem, agrees much better with your observations than the vulgar ones, so I believe you will allow the theorem itself to be a better foundation than the vulgar ones of a single refraction on the top of an uniform atmosphere.

What you desired me about the equations of the mean motions of Jupiter's satellites I did not understand in your first letter; and thank you for putting me in mind of it again in your last: because it tends not only to perfect the theory of Jupiter's satellites, but also to confirm the theory of gravity. The rule for determining those equations is this.

Let A be the time of the earth's revolution (which is a year, or 365¼ days), B the time of Jupiter's revolution (which is 12 years, or more exactly 4320 days and 14½ hours), C the time of the Moon's revolution (which is 27 days 7 hours 43 minutes), D the time of the revolution of a satellite, P the greatest equation of the earth's orb (which is 1° 56′ 20″), and Q the greatest equation of Jupiter's orb (which is about 5° 32′ 0″): and the greatest equation of the mean motion of a satellite shall be to the greatest equation of the mean motion of the Moon, as $A \times D^{quad} \times Q^{cub}$ to $B \times C^{quad} \times P^{cub}$. Whence if the greatest equation of the Moon's mean motion be assumed 12′ 0″, the greatest equation of the mean motion of the outmost satellite (whose periodical time is 16 days 18 hours 5 minutes) will be 8′ 52″. And this being found, it's easy to find the equations of the mean motions of the rest of the satellites: for they are as the squares of the times of their revolutions. Whence the greatest equation of the second satellite will be found 1′ 37″: that of the third 24″: and that of the 4th or innermost 5″.

These are minutes and seconds of degrees in the orbits of the satellites: which, being converted into time, give the greatest equations of the times of the eclipses of the satellites, to be added and subducted when ♃ is in the quartile of his orb: that is, to be added when ♃ is in the quartile, ascending from his perihelium to his aphelium; and subducted when he is in the opposite quartile, descending from his aphelium to his perihelium. And this greatest equation in the outmost satellite will be found 9′ 54″ of time, which must be very sensible: but in the next satellite it will be only 46″: in the next between 5″ and 6″: and in the innermost not much more than half a second. These computations I have done but once, knowing that you'll examine them.

I intend to determine the orb of ♄ within a few days, and then I'll send you the result*. And before you return to the work of the fixed stars, I hope to have the Table of Refractions ready. But pray let me have your lunar observations as soon as you can, that I may be about the moon whilst you are about other things.

What you say about my having a mean opinion of you is a great mistake. I have defended you when there has been occasion, but never gave way to any insinuations against you. And what I wrote to you, proceeded only from hence, that you seemed to suspect me of an ungrateful reservedness, which made me begin to be uneasy. But if you please to let all this pass, and concur with me in promoting astronomy, I'll concur with you, being your faithful friend to serve you,

I^S. NEWTON.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

* *Note, written on the letter, by Flamsteed.* It was never sent: query, if done.

No. 24.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jan. 15, 1694-5.

Sir,

I presume you are by this time returned to Greenwich. You need not fear that Saturn can sensibly disturb the motions of the satellites of Jupiter. The theorem of refractions I sent you has this fault, that it makes the refracting power of the atmosphere as great at the top as at the bottom. This has put me upon thinking on a new theorem, and I think I have found one; but intend to consider it a little further. The areas in that theorem I sent you, are to be determined by the 5th lemma of my third book of *Principia Math.* But the calculation is intricate.

I thank you for your observations about the morning and evening refractions. The reason why the former are greater in summer than the latter, I take to be nothing but the different heat and coldness of the air. For the air cools all night, and is coldest at sunrise: and heats all day, and is hottest about 12 hours after sunrise. The cold condenses the air, and makes its refraction greatest at sunrise; and the heat rarefies it, and thereby diminishes its refractive power in the evening.

I thank you for the two observations you sent me; and since you have calculated the moon's places in these and the other three observations of the last month, you will oblige me by a synopsis of the calculations. But for the rest of your observations, I desire you would leave the calculations to me, and only send me your naked observations. For otherwise I cannot correct the errors which sometimes happen in the calculated places, nor can I go over the calculations again, as perhaps I may do when I have carried the theory a little further, and know better how to allow for the refraction and parallax: for you make the horizontal parallax too great by a minute or above; but how to rectify it I do not yet know exactly enough. I thank you for your offering to be at the pains of these calculations, but I will give you no other trouble about them than to send me the moon's right ascensions and meridian altitudes, according to your observations, without any allowance for the refraction or parallax.

In trying to compute the mean motion of the moon from the *tempus apparens* in some of your observations, I find that the mean motion, gathered by my computations, differs sometimes from that in your synopses 5″ or 6″, or above. Which makes me suspect that, in determining the *tempus apparens*, your servant followed some tables which are not sufficiently exact: such as are those which Tompion uses for his watches: for those err sometimes 6″ or 8″. For avoiding this inconvenience I desire you would, instead of the *tempus apparens*, use sidereal hours counted from the appulse of the equinoctial point to the meridian, or rather from that of the Dog-star or of some other notable star. If you use canicular hours counted from the appulse of the Dog-star to the meridian you may note them thus [1695, Jan. 7, 5^{hc}. 44′ 15″] putting hc for hora canicularis. By this means the equation of time will be wholly avoided, which is troublesome to calculate, and makes the work liable to errors. For where the equation of time must be considered it must be twice computed: first to get the *tempus apparens* and then to get the *tempus verum*, which is to go a great way about. For in the way which I propose, the time in canicular hours is the *tempus verum*, and it is found without any other labor than by seeking the right ascension of the Dog-star from the meridian and turning it into time. But that there may be no mistake in the day to which the canicular hours belong I would count them from the appulse of that star to the meridian next after the midnight which precedes the day, that is, which comes between the midnight which begins and the midnight which ends the vulgar day. So that for instance, the canicular day which begins at any time between midnight and midnight on Jan. 7 shall be called Jan. 7: and so of all other days.

I should say something to you of your book: but the post is going and I must reserve it to the next. Your most sincere friend and humble servant,

I^S. Newton.

[*Notes, written on the letter, by Mr. Flamsteed.*]

Received Thursday, Jan. 17th, 1694-5, at dinner. Answered Saturday, Jan. 19, but no observations imparted.

I grant the heat and cold of the air is the chief cause of the change of its refractive power, but not all: for other days were as hot as that, yet had less refractions.

The sun rose then over the Thames and adjacent marshes. Set over a dry hill on the west end of London. It was a misty morning, and a great fog over the meadows.

I should be glad to see this business of refractions finished. It will be of use to me.

I have not time to send the synopsis now; may do it hereafter: but would gladly see what places you have derived from the given A R first. Shall give more after.

The semidiameters of the luminaries, in my *Doctrine of the sphere*, are just being settled by my own observations; excepting what error may be caused in the ☽'s by the fault or defects of the theory.

But the horizontal parallaxes were settled by the observations of the lunar eclipse of Octob. 18, 1678, in whose observation the French and I agreed very nearly. These ought to be settled by eclipses observed when the sun is in the mean distance. This I guess a plain reason why in all the months from Feb. to October my parallaxes may be too big. But from October to Feb. perhaps they will be found too little, some small matter.

Tompion's a true table of equations: but, made for a particular year perhaps, fits not the present.

Those in Parker's Almanacks true, supposing the old solar equations just. But now I have changed them and translated the aphelion, they may be 6, 8, or 10″ erroneous. Being made for 4 years, may serve for an age, with a small table I have made to correct them.

The old way of numbering days not to be left: I have a clock for that purpose.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

No. 25.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jan^y. 26, 1694-5.

Sir,

That which I would have said in my last about Viviani's book was only this. That, about 3 or 4 months before Dr. Gregory was made Astronomy Professor in Oxford, an Oxford gentleman, a student in mathematics (I think his name was Rook) called on me in his way from London, and showed me a new book published either by Viviani or some other Italian, but I think by Viviani*. He offered to leave it with me to peruse. Whereupon I turned over the leaves, and then returned it to him again; and he took it away with him, I think to Oxford, and I saw it no more. I forbore to answer your first enquiries about it, because I feared it might tend to widen the breach between you and Mr. Halley, which I had rather reconcile if it were in my power. And now I hope that what

* Probably Viviani's treatise, published in 1659, entitled *Divinatio in v. lib. Apollonii Conicorum*: for an account of which, see Montucla's *Hist. de Mathem.* vol. i. page 249. F. B.

I have told you will not be made use of to that purpose, lest it should also do me an injury. For your offering to present me with Viviani's book, I thank you as much as if it had been left with me. The equation of time I derived not from your new theory of the sun, but from the old one. For having by me two of Mr. Tompion's tables of the equation of time, I examined them by your tables printed in Sir Jonas Moor's works, and found some difference. And at the same time I tried whether the equation of time, you used in computing the mean motion of the moon in your synopses, would agree with the rest; and found the like differences. Yet these differences were of no great moment, being seldom above 4″ or 5″. I have got your table in Parker's Almanack for this year.

Whether the sidereal hours be counted from the equinoxial point, or from the Dog-star, is of no great moment. The computation is readier from the equinoxial point, but the star is a point better defined and immoveable: and it's proper to refer the motions and positions of things moveable to the positions of things immoveable.

The diminution of the horizontal parallax by about a minute I seemed to collect from your observations in your two first synopses: for those in your third I have not yet considered. But if a minute be too much, what think you of half a minute? Would that, or the parallax in your printed theory, agree best with total eclipses of sun and moon?

I agree with you that the dense vapors, which always stagnate upon the surface of the sea and often upon fenny places, cause a strong refraction. And it's probable that those, which rise to a greater height, may increase the refraction of the horizontal sun. But can you tell whether the refraction of the sea-vapors or fen-vapors be greater in hot weather or in cold? at morning or at night?

To make a new table of refractions has taken up almost all my time ever since the holidays: and I have hitherto lost my pains in fruitless calculations, by reason of the difficulty of the work. For considering that such a table is the foundation of astronomy, and very necessary for your great work, and that you have taken so great pains in providing materials for it, I was desirous to complete it; that I might have something to present you with for the pains that you have taken for me about your observations. Yet I have not wholly lost my labor; for I have found a new theorem which makes the calculation very easy, and which I must content myself with, if I can think of nothing better. At present I am a little indisposed, but hope in a few days to be well enough again to finish this business. I hope you have your health perfectly.

I think Venus was about 36 degrees high at sunset Feb. 22, and Apr. 11; and about 32 or 30 degrees high at sunset on the following days of April. If I had her altitude on every day at sunset more exactly, I could make the table of refractions more accurate.

The places of the moon from your two observations I have not yet computed: for I thought it superfluous to do what you had done to my hands; and desired a copy of your computations only to save myself that labor. But since I perceive you have a mind to see whether we can compute exactly, if you please to send me the latitude of Greenwich I'll send you what you desire. The sun's greatest declination I think you make 23° 29′; I had rather make it 23° 29′ 12″: the difference is inconsiderable.

I told you in autumn that it would be necessary for me to have about one half of the observations in your synopses set right by the correct places of the fixed stars. If you please to do it at your leisure, I'll send you a catalogue of the observations. And because, to perfect the theory of the moon's parallax, besides the subduction of some seconds, there is requisite an equation, which some-

times amounts almost to a minute, and which I know exactly, I'll make a table of it and send it to you: and then you'll be perfect in that part of the moon's theory which consists in computing her longitude and latitude from observations, and which is the foundation of all the rest.

One thing I did not consider. The observations being yours, perhaps you had rather have them perfectly your own in all respects, by determining the moon's longitude and latitude from them all yourself. If so (for that's what you have a very just right unto) I will stay your time. And when I have got a little further in the theory, and satisfied myself about something I am yet in the dark in, I'll make a new table of the moon's eccentricities and equations of her apogee for finding her mean anomaly, and send you a copy of it, to be used instead of that printed in Sir Jonas Moor's works, page 94, provided you will keep it to yourself till I have perfected the moon's theory, because it will need correction. Chuse you therefore whether you will compute the moon's places from the observations, or leave that work to me. Three or four observations in the end of this moon, and as many opposite to them in the beginning of the next, would be very significant. I am,

Your most affectionate humble servant,

I^S. Newton.

Note, written on the letter, by Flamsteed. Rec. Jan. 26, and answered the same day in haste, but more fully Feb. 7: and then sent him the 4 tables of the equations of days and the lunar observations of June, &c, 92, and December and Jan. last: 1692, May 16, 17, 19, Junij 13, 15, 16; 1694, Dec. 28, 30, 31, ☽ obs. communicavi in responso ut et 1695, Jan. 8, 9, 11, 12, 13, 14, 18, cum differentiis a meis tabulis: 1692, May 16, $8^{h.}$ 59′ 11″; Arg. 2^{s} 19° 17′ 0″; anom. med. 6^{s} 28° 59′ 12″; ♎ 23° 10′ 2″; 199° 39′ 20″; 103, 27, 47; A. R. 199° 39′ 20″; D. *a* P. 103° 22′ 50″; ♎ 23° 10′ 58″; + 0′ 56″; 4° 41′ 40″; − 2′ 50″.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

[*Draught of an answer to the above, by Mr. Flamsteed.*]

That I omitted several things in my last.

Syrius a fit star for being seen in the day: unfit by reason of the parallax of the orb. Syrius nearer us than the rest, because his light is briskest. This very sensible in my observations. A remarkable star: none of the biggest.

The light of Jupiter, faint near the sun; brisk in the opposition.

I shall mind my business of the fixed stars, and give him an account of my progress, whilst he is employed on the moon: and shall be very well pleased with an account of his success.

That I shall not impart any thing I receive from him, without his leave: and expect the same kindness from him.

About E. H. [Edmund Halley]: that he is very much mistaken in him: that I never found any thing so considerable in him as his craft and forehead, his art of filching from other people, and making their works his own; as I could give instances, but that I am resolved to have nothing to do with him, for peace sake.

That I believe he told me Mr. Newton had Viviani, on purpose to make a division betwixt us.

That I forbore not his company, till I found that a part of his character was thrown upon me: and that I had rather be without his acquaintance, to purchase it with the loss of an honest reputation.

[Copied from MSS, vol. 62, E, at the end.]

No. 26.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, Feb. 16, 1694-5.

SIR,

I received your two last letters, with your tables of the equation of time, and your observations of December and January: for all which I thank you. I have been, ever since I wrote to you last, upon making a new table of refractions and have not yet finished it: 'tis a very intricate and laborious piece of work; yet something I have done towards it. For, supposing the atmosphere to be of such a constitution as is described in the 22nd proposition of my second book (which certainly is the truth) I have found that, if the horizontal refraction be 34′, the refraction in the apparent altitude of $3^{gr.}$ will be 13′ 3″: and if the refraction in the apparent altitude of $3^{gr.}$ be 14′, the horizontal refraction will be something more than 37′. So that, instead of increasing the horizontal refraction by vapors, we must find some other cause to decrease it. And I cannot think of any other cause besides the rarefaction of the lower region of the atmosphere by heat.

And indeed the rarefaction and condensation of the air by heat and cold seems to have a much greater hand in the phænomena of refractions, than we are yet aware of. For even [these] very refractions, which you have ascribed to the sea-vapors and fen-vapors, seem to me, upon second thoughts, to arise from the condensation of the air by cold. For in travelling we find it always colder upon the water than upon the land, and that very considerably: and therefore the water doth cool the air to the height of some fathoms above it, and by cooling condenses it and increases its refractive power. This therefore is certainly one cause of those refractions, and I take it to be a sufficient cause. But as for vapors, we have now no one experiment, that I know of, to prove that they increase the refraction of the air; unless perhaps where they cool it. And, were the air upon the sea overloaded with vapors, it would scarce be so transparent as to let Calais with its buildings and church steeples be seen through it cross the Channel.

I am still laboring at a new table of refractions: and as soon as that's done I intend to make the table I promised you for the moon's parallax (for this will be quickly done), and after that, as soon as I can get time from some other occasions which begin to press me, I will make the new table of the moon's excentricities, and the equations of her apogee. This last table I shall make more for your use in determining the moon's longitude and latitude, than for my own. For when I enter upon the work of determining the moon's motions I shall stick to no tables, but alter the equations daily, as I shall see occasion, till I have made them exact.

As for your observations, you know I cannot communicate them to any body, and much less publish them, without your consent. But if I should perfect the moon's theory, and you should think fit to give me leave to publish your observations with it, you may rest assured that I should make a faithful and honorable acknowledgment of their author, with a just character of their exactness above any others yet extant. In the former edition of my book, you may remember that you communicated some things to me, and I hope the acknowledgments I made of your communications were to your satisfaction: and you may be assured I shall not be less just to you for the future. For all the world knows that I make no observations myself, and therefore I must of necessity acknowledge their author: and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown. And, for my part, I am of opinion that for your observations to come abroad thus with a theory which you ushered into the world, and which by their means has been made exact, would be much worse for their advantage and your reputation, than to keep them private till you die or publish them, without such a theory to recommend them. For such theory will be a demonstration of their exactness, and make you readily acknowledged the exactest observer that

has hitherto appeared in the world. But if you publish them without such a theory to recommend them, they will only be thrown into the heap of the observations of former astronomers, till somebody shall arise that, by perfecting the theory of the moon, shall discover your observations to be exacter than the rest. But when that shall be, God knows: I fear not in your life-time, if I should die before it is done. For I find this theory so very intricate, and the theory of gravity so necessary to it, that I am satisfied it will never be perfected but by somebody who understands the theory of gravity as well, or better than I do. But whether you will let me publish them or not, may be considered hereafter. I only assure you at present, that without your consent I will neither publish them, nor communicate them to anybody whilst you live, nor after your death, without an honorable acknowledgment of their author *.

When I have finished the table of refractions, I will endeavor to make you understand the grounds of it as far as I can. But the demonstrations being very intricate, I have not yet set them down in writing. I am very glad you have got so far on your great work, as to be able to rectify the places of the fixed stars within 10 degrees of the ecliptic on both sides, and by them to set right your observations of the moon. I shall make what haste I can to furnish you with what I am about, being, Sir, your most affectionate friend to serve you,

I^S. Newton.

P. S. Pray, till April be ended, make what observations you can in the last quarter of each moon opposite to those you make in first quarters.

Note written on the letter, by Flamsteed.

Answered March 2, 1695, and given him the heights of ♀ at sunset.

	′	″		′	″
Feb. 23 . .	35	10	April 23 . .	36	46
April 11 . .	39	14	„ 25 . .	36	12
„ 21 . .	37	17	„ 26 . .	35	33

Together with the refractions observed at Cape Sete by Mons. Picart. Vindication of myself for not imparting my observations, and an account of my northern correspondence.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

No. 27.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, March 15, 1694-5.

Sir,

The last week, about three or four days before I received your letter, I wrote to your treasurer, Mr. Hawes, about a successor to Mr. Paget, and proposed three persons: Sr. Collins, of

* This paragraph is in reply to a remark which Flamsteed had made, in a preceding letter, to the following effect: viz., "Upon Mr. Halley's boasting again of his [Mr. Newton's] performances in the moon's theory, and "that Mr. Newton had imparted it to him, I gave him [Mr. Newton] an intimation of it (as I think) in mine of "Feb. 9, 1694-5; and that he imparted the result of my observations without my consent, contrary to his promise "in his answer." On *this* answer of Newton, Flamsteed makes the following remark: "He adds a great many "words to persuade me that to have the theory of the moon published with my observations, would be a great proof "of their accuracy: whereas, theories do not commend observations; but are to be tried by them: and theories "are then only probable, when they agree with exact and indubitable observations." See the note in page 140, and the MSS there referred to. F. B.

this University, Mr. Caswell of Oxford, and Mr. Newton late of Yarmouth. Sr. Collins has mathematics enough, but is young and inexperienced. If they choose him, it will be requisite that the Governors oblige Mr. Paget (if they can) to inspect the school next winter, and teach him to design and draw: and then, if he take hold of that advantage to improve himself, and continue as industrious as they of his college tell me he has hitherto been, I believe he will prove a good master. But because he is young, I added Mr. Caswell; and because I knew not whether he would accept of the place, I named also Mr. Newton. I remember Mr. Caswell's character pretty well since his competition with Dr. Gregory; and am satisfied that he is a man of very good morals and great industry, and so well skilled in teaching mathematics, that could he have drawn prospects, I would have recommended none but him, till he had refused to accept of the place. However, I gave him a recommendation to the following purpose: that he is sober, industrious, and well-skilled in the mathematics, and will make a good master if he will accept of the place; and that Dr. Wallis and you are able to give a fuller account of his abilities. I would have sent you the words of the recommendation I gave Mr. Caswell, but that I have lost the copy: however, you may see it in Mr. Hawes's hands. Mr. Newton I am a stranger to, but had an opportunity about two years ago of knowing his abilities. To the best of my remembrance he wants algebra: in other respects he has mathematics enough, having taught navigation for some years at Yarmouth.

I have now finished the table of refractions, and send you enclosed a copy of it*. In a regular sky, when in the altitude of 3 degrees, the refraction is 13′ 20″, you may rely upon it that the table is exact to a second minute for all altitudes above 10 degrees; and that in the altitudes between 3 and 10 degrees, the greatest error cannot be above 2 or 3 seconds. If the refraction in the altitude of 3 degrees be greater or less then 13′ 20″, it must be increased or diminished in the same proportion in all altitudes above 3 degrees. Within a few days I will send the other tables I promised.

I am your affectionate friend to serve you,

I$^{s.}$ Newton.

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

No. 28.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, April 23, 1695.

Sir,

I now send you the tables I promised. They are accurate enough for computing the moon's parallax, and thence her longitude and latitude from observations. The little table of the equation of the moon's parallax is founded on the 28th prop. of the 3rd book of my Principles; where I show that the moon's orb (without regard to her eccentricity) is oval, and that her distance in quadratures is greater than her distance in the octants in the proportion of 70 to 69. In the table of her horizontal parallaxes, I make her horizontal parallax in the syzygies less than you make it in the printed tables, by about half a minute; and in the quadratures I make it less than you do by about $1\frac{1}{3}$. Were the French mensuration of the earth to be confided in as exact, these parallaxes ought to be still less: but I am unwilling to diminish them any further as yet. In computing the moon's mean anomaly for finding her parallax, add 12 minutes to the mean motion of her apogee. When I set myself wholly to calculations (as I did for a time last autumn and again since Christmas in making the table of refractions) I can endure them and go through them well enough. But when I am about other things

* This table is not found. F. B.

(as at present) I can neither fix to them with patience nor do them without errors*; which makes me let the moon's theory alone at present, with a design to set to it again and go through it at once. When I have your materials I reckon it will prove a work of about three or four months: and when I have done it once I would have done with it for ever. In autumn, when I was tracing the moon's motions by your observations, I found that where they were continued, two or three or four of them would agree with one another to half a minute or less: and then would follow two or three others which would again agree with one another, but disagree from the former 2 or 3 minutes; and whether to follow the former or latter I knew not, and so could not come to the conclusions I would have made by reason of their disagreement, but wrote to have your observations set right by your new places of the fixed stars. And I am glad your work is now so far onward.

Upon Mr. Paget's resignation I understood that a great interest had been made among the governors (by the seamen as I presume) for a Tarpolian master, which would have ruined the school. To stop that gap, I recommended three persons; and I believe the Tarpolian interest struck in more readily with Newton than they would have done with any University man. Concerning the table of refractions, I will write to you in my next. Yours, I^s. Newton.

Moon's mean Anomaly.	Moon's Horizontal Parallax. Eccentricity. 4356	6685	Moon's Mean Anomaly.	Moon's Horizontal Parallax. Eccentricity. 4356	6685
s. deg.	′ ″	′ ″	s. deg.	′ ″	′ ″
0 00	55 05	53 54	3 00	57 23	57 14
06	55 05	53 54	06	57 39	57 39
12	55 08	53 57	12	57 55	58 04
18	55 13	54 02	18	58 11	58 29
24	55 18	54 09	24	58 26	58 53
1 00	55 24	54 18	4 00	58 41	59 16
06	55 31	54 29	06	58 56	59 40
12	55 39	54 42	12	59 11	60 02
18	55 49	54 55	18	59 24	60 23
24	56 00	55 11	24	59 34	60 41
2 00	56 11	55 28	5 00	59 43	60 57
06	56 25	55 47	06	59 51	61 11
12	56 39	56 07	12	59 58	61 22
18	56 54	56 29	18	60 02	61 30
24	57 08	56 51	24	60 05	61 35
3 00	57 23	57 14	6 00	60 06	61 37

Moon's distance from Conjunction or Opposition.	An Equation to be subducted from the Moon's horizontal parallax. Horizontal Parallax. Least 53′ 54″ Subduct	Greatest 61′ 37″ Subduct
Deg.	″	″
00	0	0
10	8	9
20	16	18
30	23	26
40	30	34
50	35	40
60	40	46
70	44	50
80	46	52
90	46	53

These tables are grounded on the supposition that the mean distance of the moon in the octants is 60½ semidiameters of the earth; and, by consequence, her horizontal parallax in that mean distance 57′ 5″ 39‴. And that her mean distance in the syzygies is less, in the proportion of 69 to 69½; and in the quadratures greater, in the proportion of 70 to 69½. And that her mean distance in the syzygies is to her greatest and least distance in the syzygies, as 1000000 to 1066850 and 933150.

* *Note, written on the letter, by Flamsteed.*—The same with me.

The Equations of the Moon's Apogee; and the Eccentricities of her Orbit, in such Parts as the Radius is 100000.

Add the Equations of the Apogee.													
Annual Argument.	Sign 0—VI.			Eccentr.	Sign I.—VII.			Eccentr.	Sign II.—VIII.			Eccentr.	Annual Argument.
	°	′	″	Parts.	°	′	″	Parts.	°	′	″	Parts.	
0	0	0	0	66850	9	22	50	61855	11	32	17	50406	30
1	0	20	54	66845	9	36	57	61537	11	22	59	50022	29
2	0	41	46	66827	9	50	31	61211	11	12	37	49645	28
3	1	2	38	66798	10	3	40	60878	11	1	10	49274	27
4	1	23	27	66757	10	16	14	60438	10	48	39	48908	26
5	1	44	12	66705	10	28	17	60192	10	35	2	48551	25
6	2	4	54	66638	10	39	47	59838	10	20	21	48201	24
7	2	25	31	66562	10	50	41	59479	10	4	36	47859	23
8	2	46	0	66475	11	0	58	59113	9	47	47	47527	22
9	3	6	24	66375	11	10	40	58742	9	29	55	47204	21
10	3	26	41	66265	11	19	42	58366	9	10	59	46891	20
11	3	46	50	66146	11	28	5	57986	8	50	58	46588	19
12	4	6	48	66012	11	35	46	57600	8	29	57	46298	18
13	4	26	37	65870	11	42	44	57211	8	7	57	46019	17
14	4	46	15	65716	11	48	58	56819	7	44	58	45753	16
15	5	5	41	65549	11	54	27	56422	7	21	1	45500	15
16	5	24	55	65373	11	59	11	56023	6	56	8	45260	14
17	5	43	53	65185	12	3	6	55622	6	30	23	45034	13
18	6	2	38	64988	12	6	12	55218	6	3	49	44824	12
19	6	21	9	64779	12	8	28	54814	5	36	28	44628	11
20	6	39	22	64562	12	9	53	54408	5	8	22	44447	10
21	6	57	20	64343	12	10	25	54001	4	39	34	44283	9
22	7	14	55	64094	12	10	1	53595	4	10	8	44134	8
23	7	32	14	63847	12	8	43	53190	3	40	10	44003	7
24	7	49	11	63590	12	6	28	52784	3	9	41	43888	6
25	8	5	47	63323	12	3	16	52381	2	38	45	43789	5
26	8	22	0	63046	11	59	6	51980	2	7	27	43709	4
27	8	37	51	62761	11	53	58	51581	1	35	51	43647	3
28	8	53	17	62467	11	47	45	51185	1	4	2	43602	2
29	9	8	17	62165	11	42	14	50794	0	32	3	43575	1
30	9	22	56	61855	11	32	17	50406	0	0	0	43566	0
	Sign V.—XI.				Sign IV.—X.				Sign III.—IX.				
Subtract the Equations of the Apogee.													

[Copied from the *original* MS in the library of Corpus Christi College, Oxford.]

No. 29.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

April 25, 1695*.

Sir,

The table of the equations of the apogee and eccentricities serves for all the year, winter and summer, as well as spring and autumn, without any correction. For, the equations of the parallax which arise from the earth's being in its aphelium or perihelium, can never amount to above three or four seconds, in excess or defect; and, therefore, I consider them not. The actions of the sun for varying the lunar motions, I reckon to be as the cubes of the sun's apparent diameter; and the menstrual equations which arise from thence to be nearly in the same proportions: but the annual ones, and those of longer periods, arise from a mixture of impressions in summer and winter, spring and autumn, compounded together, and observe such laws as I cannot yet determine. Nor have I been considering this point, since I wrote to you last about it.

The table of horizontal parallaxes was made by such limits as I gathered in autumn from your two first synopses of observations. I do not pretend to be accurate in it. But what you object from lunar eclipses overthrows it not, because these and the solar ones disagree. You think to reconcile them, by supposing that the parallax is greater in the sun's perigee, less in his apogee: whereas the contrary is true. The sun in his perigee draws the moon off from the earth, and thereby diminishes her parallax in winter; and, on the contrary, increases it in summer, though not sensibly. The reason, therefore, why the lunar eclipses make the parallax greater than the solar ones do, is to be inquired. One reason you hint; namely, that the diluteness of the shadow near the limb makes it seem broader than it is. Another may be, that all the sun's light which passes through the atmosphere within 20 or 24 miles of the earth, is scattered by the refraction of the atmosphere, and goes not to the edge of the shadow. A third may be, some mistakes in your calculation. For you make the moon's mean anom. $5^{s.}$ 15° 28′ 26″, and thence her horiz. parallax 59′ 57″; you should have said 60′ 49″: for 59′ 57″ is the parallax agreeing to the mean anom. $4^{s.}$ 15° 28″. See, therefore, if there be not some such mistake in your calculations. But yet if my table satisfy you not, you may use your printed one, and only apply to it that little menstrual equation which I sent you.

As for the late election, it belongs not to me to inquire what made the Governors so much against Mr. Caswell: but now Mr. Newton is in, the best way is to make the best of it.

I am your humble servant,

I^s. Newton.

P. S. I suspect that Mr. Caswell put in too late, and that the Governors were afraid lest that should come to pass which you tell me did come to pass; I mean, that Mr. Caswell, having another more easy way of living, should, upon any occasion, be glad to be clear of them.

[Copied from the *original* MS, in the library of Corpus Christi College, Oxford.]

* *Note, written on the letter, by Flamsteed.*—Received, May 6. I suppose it is misdated, and ought to have been dated *May* 4th.

No. 30.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, June 29, 1695.

SIR,

I received your solar tables, and thank you for them. But these, and almost all your communications will be useless to me, unless you can propose some practicable way or other of supplying me with observations. For as your health and other business will not permit you to calculate the moon's places from your observations, so it never was my inclination to put you upon such a task, knowing that the tediousness of such a design will make me as weary with expectation as you with drudgery. I want not your calculations, but your observations only. For, besides myself and my servant, Sr. Collins (whom I can employ for a little money, which I value not) tells me that he can calculate an eclipse, and work truly. I will therefore once more propose it to you, to send me your naked observations of the moon's right ascensions and meridional altitudes; and leave it to me to get her places calculated from them. If you like this proposal, then pray send me first your observations for the year 1692, and I will get them calculated, and send you a copy of the calculated places. But if you like it not, then I desire you would propose some other practicable method of supplying me with observations; or else let me know plainly that I must be content to lose all the time and pains I have hitherto taken about the moon's theory, and about the table of refractions.

I am glad you betake yourself to riding for your health, rather than to physic. It is certainly the best and safest remedy for an ill habit of body, arising from bad blood in most cases; and therefore you may do well to continue it. I am your humble servant,

I^S. NEWTON.

Note, written on the letter, by Flamsteed. Answered July the 2nd, with an offer of the lunar observations made with the sextant, from 1679 to 1690. Wrote the same day to Mr. Bossley, and sent him my new solar tables. [*Besides this Note, Flamsteed has also written on the back of this letter, the statement given in page* 142; *and which I have there incorrectly stated to have been written on the letter dated Nov.* 17, 1694. F. B.]

[Copied from the *original* MS, in the library of Corpus Christi College, Oxford.]

No. 31.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, July 9, 1695.

SIR,

After I had helped you where you had stuck in your three great works*, that of the *theory of Jupiter's satellites* †, that of your *catalogue of the fixed stars* ‡, and that of calculating *the moon's places from observations* §, and in all these things freely communicated to you what was perfect in

[*These notes are written on the letter by Flamsteed.*]

* I know not that I stuck anywhere. All my three works go on without him.

† An answer to my query, whether the physical parts were sensible in the satellite's motions, or not. He answered me not.

‡ A table of refractions for correcting their merid. zenith distances.

§ The table of the equations of the apogee and eccentricities, with that of horizontal parallaxes, and their correction.

its kinds (so far as I could make it), and *of more value than many observations* *, and what (in one of them) cost me above two months' hard labor, which I should never have undertaken but upon your account, and which I told you I undertook that I might have something to return you for the observations you then gave me hopes of, and yet, when I had done, saw no *prospect of obtaining them* †, or of getting your *synopses rectified* ‡, I despaired of compassing the moon's theory, and had thoughts of giving it over as a thing impracticable, and occasionally told a friend so who then made me a visit. But now you offer me those observations which you made before the year 1690, I thankfully accept of your offer, and will get as many of them computed as are sufficient for my purpose §. As to the greatest parallactic equation, I know no more of it than when I wrote to you last about it. It is but a small equation, scarce exceeding 2, or 3, or at most 4 minutes, and so involved with other equations, that when you have computed your 30 observations, you will know no more of it than at present. I have no thoughts of writing about refractions. The table of refractions I would not have yet communicated. The observations I shall chiefly want, are those when the moon's apogee is within 12 degrees of the seventh degree of ♈, ♋, ♎, ♑, both in antecedentia and consequentia, and those of the years 1687 and 1689, when the apogee is within 12 degrees of the 22 degree of ♌ and ♏. I am your most humble servant,

Is. Newton.

[Copied from the *original* MS, in the library of Corpus Christi College, Oxford.]

No. 32.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, July 20, 1695.

Sir,

The report you mention was much against my mind, and I have written to put a stop to it. I thank you for your communications of the table of fixed stars and your lunar observations. So soon as I have got some business off my hands, I intend to get such of them calculated as I have need of, and send you the places. The moon's mean motion is not much amiss, and may be retained as you printed it till I can determine it more exactly. I believe there is an equation requisite in your syzygies, but I am not yet master of it. Such niceties I have not yet determined, and you must have patience with me till I can compass them, otherwise I must desist; as your impatience had once made me resolve to do. The Horroxian Theory, by the table of eccentricities and equations of the apogee which I sent you, never errs above 10 or 12 minutes; and so is twice as exact as your printed tables, which err sometimes 20 or 21 minutes: but I would not advise you to spend your time in calculating by it till I have compassed the small equations, which I cannot do till I have observations for a sufficient number of cases. Such expostulations or expressions, in your last

[*These notes are written on the letter by Flamsteed.*]

* Without my observations, he had never found them out.

† My sickness has hindered.

‡ I have desired him to show what he thinks faulty, he has not yet.

§ I was ill all this summer, and could not furnish him as I had done formerly. He mistook my illness for design, and wrote this hasty, artificial, unkind, arrogant letter. Answered it July 13th, and sent him the lunar observations from January to July, 1677, marked N in the margin of the book.

and some other letters as tend to a difference, I pass by. Pray take care of your health. Dr. Battely (chaplain to Archbishop Sancroft) was much troubled with violent headaches, and found it a certain cure to bind his head straight with a garter till the crown of his head was numbed: for thereby his head was cooled by retarding the circulation of the blood. 'Tis an easy remedy, if your pain be of the same kind.

I am your humble servant,

I^S. Newton.

[Copied from the *original* MS, in the library of Corpus Christi College, Oxford.]

No. 33.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, July 27, 1695.

Sir,

The other day I had an excuse sent me for what was said at London about your not communicating, and that the report should proceed no further. I am glad all misunderstandings are composed. I thank you for your nonagesimal table: I designed to make such a table, and it saves me the labor. You may continue your observations if you please till Octob. 10th, 1677. But I had rather you would send me those from Aug. 24th, 1685, to July 5th, 1686, when the aphelium was in the same position as in the year 1677. For when I see all your observations together in this position of the aphelium, I can tell better what to select for this case. The transcribing of these things gives your servant trouble: and for encouraging him I shall order Will Martin, the Cambridge carrier, (who lodges every week, from 9 in the morning on Saturday till 3 in the afternoon on Monday, at the Bull in Bishopsgate-street,) to pay him*. I shall not have time to go through all your observations, but will send you the times, for which I would have them, when I have done with these for this position of the aphelium.

I am your thankful, humble servant,

I^S. Newton.

[Copied from the *original* MS, in the library of Corpus Christi College, Oxford.]

No. 34.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Cambridge, Sept. 14, 1695.

Sir,

When I received your last, Mr. Halley was with me about a design of determining the orbs of some comets for me. He has since determined the orb of the comet of 1683 by my theory; and finds, by an exact calculus, that it answers all your observations and his own to a minute. I

* In the copy of this letter, preserved in the British Museum, [No. 4292, Birch's *additional* MSS,] the words *two shillings* appear here, to which the following note is appended, viz.:—"Mr. Flamsteed altered it so from the "word *guineas*, which is in the original, as is evident through the erasure." On mentioning this to Professor Rigaud, he was kind enough to re-examine the original MS letter; and he informs me that the following words had been written by Newton, but *afterwards crossed out with the pen*: viz. "two shillings if you please to "let him call for it." But that there is nothing, in the original, of any substitution of *guineas* for shillings. F. B.

am newly returned from a journey I lately took into Lincolnshire, and am going another journey: so that I have not yet got any time to think of the theory of the moon, nor shall have leisure for it this month or above: which I thought fit to give you notice of, that you may not wonder at my silence. I hope you get ground of your distemper, and that I shall ere long hear that you are well recovered.

I am your humble servant,

I^S. NEWTON.

[*Notes written on the letter by Flamsteed.*]

Answered Sept. 17. It commends and confirms the theories of the comets, that they will represent exact observations of them within a minute. Mr. Halley has set a friend of mine to desire some observations of the comet of the year 1682 from me. If I am not mistaken I imparted them to him, as well as those of 1683. Whatever he may say to you to the contrary, his behaviour towards me has been the most impudently and ungratefully base. I know him, and you do not, therefore am resolved to have no further concern with him; but if you want any of that comet, I shall give you them, and leave to employ them as you please.

My distemper abates; the pains of my head are not greater, but I am rarely free from them but when I am travelling. I am setting on that work that was interrupted by them in the spring. My exercise will devour no small part of my time, and therefore I shall desire my friends to excuse me if I answer not their letters so fully nor readily as formerly; however, when you want more of my lunar observations I shall cause them to be transcribed, and it will be no trouble.

[Copied from the *original* MS, in the library of Corpus Christi College, Oxford.]

No. 35.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Marston, near Brackley, Northamptonshire, May 25, 1695.

SIR,

I am well pleased to understand by yours of May the 14th (which came to my hands yesterday, where I am at present) that you are so well stored with observations for the earth's annual parallax; and therefore desire you will suffer them to be made public forthwith, for the reasons mentioned in my last, which I need not repeat. The brisk light of the star mentioned, though small, may argue it to be nearer perhaps than some, which seem bigger; and its situation is very convenient: yet I would not have you neglect the shoulder of the lesser Bear. Perhaps by darkening of your room, you may gain a sight of them in the daytime. I am not displeased to find you distinguish between the thread's touching and covering a star; which seems to argue they have some apparent magnitude, contrary to what some good observers would persuade us. Your diligence to perfect your catalogue of fixed stars I approve likewise; and your reasons for it are good. Yet I would not have you delay that of the parallax; for, the observations being already made, it will require no great time to digest them: and if you think that little time may not be spared for it, be pleased to furnish us with materials, and either Mr. Caswell or I will help to digest them for you: and if you be excluded elsewhere, we will take care to have them printed at Oxford. You are to be careful also (in so nice a point) that the motion in longitude (as it is wont to be called) of the fixed stars do not impose upon you; and that some very small variation of the measuring thread may

not deceive you. I could suggest some other things in order to this parallax, which I may hereafter do at leisure; but I would not divert you from what is before you. I am, Sir,

Your friend to serve you,

John Wallis.

[Extracted from the *General Dictionary*, Article "Wallis."]

No. 36.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Oxford, Aug. 13, 1698.

Sir,

I have understood from Mr. Caswel, a good while since, that you had very considerable observations (for divers years together) to prove a parallax of the fixed stars to the earth's annual orb. I have desired Mr. Caswel to press you to let us have an account of them, so as to have them published. I do again request it of you, and hope you will not refuse it. I am now drawing toward an end of publishing a third volume of Mathematical Tracts, wherein will be a collection of letters relating to such matters. If you will do me the favour to draw up those observations in form of a letter to me, (or to whom else you please,) and to let me have it, I will (with your leave) publish it amongst those letters. The thing will be an honour to you and to our nation. I know your hands are full of other work; but I hope you may spare so much time as to draw up such a letter in Latin, and I will take upon myself the trouble of seeing it printed here: and you shall command from me the employing of as much time to serve you in what way you shall propose to me. But pray let it be done quickly, lest it come too late. 'Tis pity you should lose the honour of being the first who hath made such a discovery. I am, Sir, yours to serve you,

John Wallis.

[Extracted from the *General Dictionary*, Article "Wallis."]

No. 37.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Oxford, Aug. 26, 1698.

Sir,

Yours of August 23rd I received this morning. I am glad you are inclinable to draw up those observations of yours concerning the parallax of the earth's annual orb. The time you mention will, I believe, be soon enough; for I find our printers more slow than I could wish. I saw your letter to Mr. Caswel, with which I was pleased. I think so much of it as concerns the rectifying of your instrument may be spared. It will be sufficient to give us the observations, as they would have appeared if the instrument had been rectified; and as to the rectifying of your instrument, we may trust you. Nor will it be necessary to give a large account of the form of your instrument; it will be sufficient to say it is a mural quadrant, or larger arch fixed to a wall in the plane of the meridian, and furnished with telescopic sights, such as to distinguish a very small arch of a few seconds; for I think this is the case. And then that (the position and sights of the instrument being duly rectified) the observations were such as there set down. A more large account of particulars may afterwards be done at leisure, with more observations to be henceforth made; but I am willing that at least a short account be given of the observations you have, to preserve the memory and reputation of it to yourself, as the first who have effectually discovered it. The letter to

Mr. Caswel (if I do not misremember) gives but the observations of one year; but intimate more to have been made in confirmation of it. It is not necessary that the parallax of other stars should justly agree to that of the polar; for we are not to presume that they be all at an equal distance from us: some may possibly have a discernible parallax, and others not. The greater stars may be reasonably thought (but we are not sure) to be nearest, and those nearest to the pole of the zodiac most liable to a parallax. But if it can be discerned in any, it is a demonstration of the earth's motion.

I am, Sir, yours to serve you,

JOHN WALLIS.

[Extracted from the *General Dictionary*, Article " Wallis."]

No. 38.) *Letter from Mr. Flamsteed to Mr. Colson.*

The Observatory, October 10, 1698.

SIR,

Mistakes and misapprehensions cause more differences in the world than malicious reports. My servant James is guilty of them frequently, and I am afraid of no small one: he brought in a report from you, he told me that both you and Colonel Bruce told him that Mr. Newton had perfected the theory of the moon *from Mr. Halley's observations*, and imparted it to him, with leave to publish it; and that Mr. Halley would publish it in a short time. Sir, I can scarcely believe that Mr. Halley, however indiscreet, could be the author of this report; since he has seen a synopsis of 152 observed places of the moon with her calculated places, and the elements of the calculation, all done by my own hand, and knows I imparted them with as many more as made them above 200 to Mr. Newton; and that there is a very fair correspondence kept up between us for this purpose; and some have been given him very lately. But to clear you wholly, and take off all occasions of your injuring either Mr. Newton or me by spreading this or the like false stories for the future, I must acquaint you that Mr. Newton assures me he has not imparted his lunar theory to Mr. Halley, (so that all he knows of it must be only collected from discourse he has had with him,) *nor made use of one of Mr. Halley's observations in rectifying of it.* The reasons I shall tell you when I see you.

I suspect, therefore, there is some mistake in my servant's relation of what you and the Colonel told him: and, to prevent any ill consequences of it, desire you would let me know the truth of it, by a line or two from your own hand.

I would not injure Mr. Halley either with Mr. Newton (on whom I know he has a dependence) nor the Colonel (by whom he may make some advantage): therefore when I found Mr. Newton concerned at the report, (which I gave in near as few words as I have wrote it,) I added no more, but that I wondered why or by whom it should be spread. Nor would I write to the Colonel, nor had to you, but that I find my servant discoursed to you on his own head, and omitted what I chiefly enjoined him, which I have marked before with a line underneath*. You may now believe as you please, but pray report nothing further in this affair but what you know to be true. Mr. Newton's theory, when perfected, must needs agree with my observations, since it is built, as he freely owns, upon them and his doctrine of gravitation: and the one without the other will not do the business; but both together will, as he says himself. Mr. Halley's could be of no use to him,

* These words are printed in italics. F. B.

because he used the Tychonic places of the fixed stars to rectify and state the moons by: the Hevelian were not extant, and had they been published, they were got with plain sights.

I shall make a new table of refractions for the Colonel in a day or two on a true theory. I have not leave to impart Mr. Newton's; and I believe he will see cause to withdraw it. I intend to be in London, God willing, on Friday next, but, the days being short, I shall not have leisure to see the Colonel at your house. I shall be at Garraway's betwixt one and two. If you come down hither in the mean time let it not be on Wednesday, for I have company that day; at any other you shall be welcome to

Your friend and servant,

T. Flamsteed, M.R.

To Mr. Colson, Teacher of Mathematics, at his house
in Goodman's-fields, London.

[Copied from MSS, vol. 33, page 11.]

No. 39.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Oxford, November the 8th, 1698.

Sir,

I am entering upon the printing of some *Latin Letters* in an *Appendix* to a *third volume* of mine, which hath been now in the press for some years; amongst which I would be glad of yours concerning the parallax of the pole-star. Mr. Caswel tells me it will contain your observations thereof for seven years. I desire I may have it by the end of this month, that it may not come too late; for we are now drawing to an end. I think you need not incumber it with those particulars which relate to the rectifying of your instrument, but give us the observations as they would appear, supposing the instrument so rectified. It will, I think, be not to your disadvantage to have it there; and it will be to the reputation of our nation to be the first that have been able to make out the parallax.

I am, Sir, yours to serve you,

John Wallis.

[Extracted from the *General Dictionary*, Article " Wallis."]

No. 40.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Oxford, December the 10th, 1698.

Sir,

I have finished the translation of your letter into Latin some while since. I find nothing of it but what is fit to be published, and therefore leave out nothing of it. I sent you the two first sheets of it by Dr. Gregory, who said he should see you, and would give them to you. If you desire I should send you the third sheet, it shall be done. I am well satisfied with it, and think it proper that the English letter be published in the *Transactions;* for it well deserves a place there: and to that end I shall return you the English letter, when you give me order so to do. But I would advise that it be printed at Oxford, (as in such cases is sometimes done,) that I may see to its being correctly printed; for I find the correctors of the press in London are apt to mistake in things mathematical. Meanwhile, I desire you will look carefully to the numbers, that they be truly

written, and then return me the two sheets I have sent you; particularly in the tenth collation, (if I do not misremember,) where I think you subtract 5″, instead of adding it.

I am, yours to serve you,

John Wallis.

[Extracted from the *General Dictionary*, Article "Wallis."]

No. 41.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Oxford, December the 28th, 1698.

Sir,

I received on Monday last, December the 26th, your letter of December the 24th, and the packet of papers directed to me, of which I shall take care: and at the same time I received another letter from one in London, which desires me not to print any paragraph of your letter, which speaks of your giving to Mr. Newton observations of the moon. He is a friend to both of you; but he doth not give me his reasons why. I thought best to acquaint you with it, and desire your advice upon it. If you order me to leave out that paragraph, (and the next which follows, about your two friends in the north, of a like nature,) instead thereof I was thinking (if you like it) to put this:— *Aliaque intervenerunt negotia (quæ nunc narrare non est opus) quibus destinebar ne potuerim isti rei vacare; atque (inter alia) ut amicis aliquot, id flagitantibus, exhiberem planetarum (Saturni, Jovis, et Lunæ) loca plurima ex observatis meis calculo deprompta. Quæ moneo, ne putes negligentiæ desidiæve meæ imputandum, quod non citius huic operi me accingerim:* which I think may serve your turn as well. And toward the beginning, where you speak of Riccioli, I think it not amiss to add: *Idemque questi sunt alii (hoc est præterito seculo) post receptam hypothesin Copernicanam.* I do not think of making any other considerable variation from your copy as it is now sent, otherwise than as to prepare it for the printer: but (because you omit the time in the date) I shall supply December the 2nd, 1698, or what other time you shall appoint.

I am, Sir, yours to serve you,

John Wallis.

[*Note written on the above by Flamsteed.*]

You say Dr. Gregory is a friend to both of us. I much doubt it. Had he been my friend, he would have sent me word that paragraph would displease Mr. Newton. A letter would have come hither, and an answer have gone back, in almost as little time as one goes from London to Oxford. It is much to be suspected he is only Mr. Newton's friend for Mr. Montague's sake: since his countrymen gave out formerly, that he had found abundant errors in his *Principia;* now that Mr. Newton gave them to him. To deal plainly with you, his friends resort to Hindmarsh's shop, in Cornhill; and who they are you may easily be informed, even at Oxford.

That I was at London the Friday they arrived, and the following. That I wrote to Mr. Newton on Monday, and sent him an account of what Dr. Gregory wrote to Dr. Wallis; as also the paragraph of my letter, which Dr. Gregory would suppress. That receiving no answer by Thursday morning, I then wrote to him again for one. That since he takes no notice of my letter, I conclude I need not take notice of Dr. Gregory's, nor you neither, and therefore think you need not alter the paragraph at all. Dr. Gregory is a friend of Mr. Halley, though he was his competitor; but I per-

ceive, by this transaction, he is no friend of mine: though I shewed him more friendship than he could reasonably expect on that occasion, and Mr. Halley as much enmity. But he thinks Mr. Halley has an interest in Mr. Newton, and therefore is become his friend, and takes the same courses Halley did to ingratiate with him, whose favor may be of use to him with Mr. Montague.

[Extracted from the *General Dictionary*, Article "Wallis."]

No. 42.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, December 29, 1698.

Sir,

I have examined the times of the determined right ascensions, and distances *à polo*, of the moon's limbs, which I gave you when you were last here: and find them all just, save the second, April 25, 1695; which makes $18^h.\ 38^m.\ 7^s$, and they are all fit for use.

In a letter of yours, of April 23, 1695, you gave me a new table of the equations of the apogee, wherein you made

The greatest equation, near . . .	12° 10′½
The greatest eccentricity	66850
The least do.	43566
The greatest horizontal parallax of the moon .	61′ 37″
The least do. . .	55′ 5″
The diminution of this parallax in the least distance	46
„ in the greatest do. .	53

In the same letter, you direct to add 12′ 0″ to my place of the apogee. In some others, you make the greatest physical parts 13′; the mean variation 35′½.

You add 2′ nearly to the moon's mean motions; which I find the observations in the synopsis required.

I give you this brief of your communications already made, lest you should have forgot what I had in my hands already; and put yourself to a needless trouble of causing them to be copied over again. If you have made any alterations in these, or any additions to the theory, that will make the numbers answer my observations better, you will oblige me if, at your leisure, you will please to impart them to, Sir, your affectionate and humble servant,

John Flamsteed, M.R.

P.S. In your letter, you say, these corrections will answer all my observations within 10 minutes. Mr. Halley boasts that those you have given him will represent them within 2 or 3, or nearer. I wish you many happy years. J. F.

To Sir Isaac Newton, Warden of the Mint,
at his house in Jermyn Street, near St. James's, London.

[*Note, written by Flamsteed, in the margin of the book.*]

Mr. Newton came to see me Sunday, December 4th, in the time of evening service. I imparted to him the right ascensions, and distances from the pole, of the moon's limbs calculated in my 5th book of calculations [MSS, vol. 55] page 183: but, not having examined the times, I told him I

could not assume they were truly stated by my servant, James. Having since examined them, I wrote this letter on the occasion, and sent it December 29th.

Since this was wrote, I find 45″ ought to be added to the distances of the moon's limbs from the pole; which I was not then aware of. I acquainted him there was a further fault in them, when I was last with him. He is reserved to me, contrary to his promise. I lie under no obligation to be open to him.

[Extracted from MSS, vol. 33, page 12, numbered from the *end.*]

No. 43.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jermyn Street, Jan. 6, 1698–9.

Sir,

Upon hearing occasionally that you had sent a letter to Dr. Wallis about the parallax of the fixed stars to be printed, and that you had mentioned therein with respect to the theory of the moon, I was concerned to be publicly brought upon the stage about what, perhaps, will never be fitted for the public, and thereby the world put into an expectation of what, perhaps, they are never like to have*. I do not love to be printed upon every occasion, much less to be dunned and teased by foreigners about mathematical things, or to be thought by our own people to be *trifling* away my time about them, when I should be about the King's business †. And, therefore, I desired Dr. Gregory to write to Dr. Wallis against printing that clause which related to that theory, and mentioned me about it. You may let the world know, if you please, how well you are stored with observations of all sorts, and what calculations you have made towards rectifying the theories of the heavenly motions ‡. But there may be cases wherein your friends should not be published without their leave: and therefore I hope you will so order the matter that I may not, on this occasion, be brought upon the stage §. I am your humble servant,

Is. Newton.

[Copied from the *original* MS, in the Library of Corpus Christi College, Oxford.]

[*Four Notes, written on the letter, by Flamsteed.*]

* When Mr. Halley boasts 'tis done, and given him as a secret: tells the Society so, and foreigners. See Mr. Colson's letter to me.

† Was Mr. Newton a *trifler*, when he read mathematics for a salary at Cambridge? Surely, astronomy is of some good use, though his place be more beneficial.

‡ I know what to do without telling.

§ Where persons think too well of themselves to acknowledge they are beholden to those who have furnished them with the feathers they pride themselves in: when they have great fr. &c.

No. 44.) *Letter from Mr. Flamsteed to Dr. Wallis.*

The Observatory, January 7, 1698-9.

Honored Sir,

Though yours of the 28th arrived here on Friday was seven nights, yet, being then at London, I received it not till my return home on Saturday night. I wondered to find by it that Dr.

Gregory should concern himself any further with my letter about the parallax of the polar star than only to transmit it; had he been my friend as you suppose him, he might as easily have wrote to me to advise me to alter that paragraph as to you to suppress it. The penny post comes something sooner than the general post does to Oxford. I fear you mistake him much: his friends are neither friends to you nor me; they resort commonly to Mr. Hindmarsh's, a bookseller's shop in Cornhill; and who they are you may learn at Oxford.

The truth is, the Doctor is suing for the mathematical tutorship to the young Duke of Gloucester, who will not have occasion for a tutor in mathematics this 4 or 5 years: he knows I was named for that employ when the settling of his household was first discoursed of, and that I have an interest though I do not look after it, for reasons not to be recited in this letter. He hopes to gain it by his interest with the Bishop of Salisbury, and that Mr. Newton may be of good use to him by procuring him the favor of Mr. Montague. For this reason he has taken the occasion to ingratiate with Mr. Newton, by suggesting I have wrote something that may derogate from him: but I am apt to believe that he will rather injure than help himself by this piece of flattery.

For Mr. Newton owns not only the 150 lunar observations I fitted him with, to examine whether the moon's motions answered those he thought she ought to have by the laws of gravitation; but moreover, that he has made use of no observations but mine in rectifying of her theory. He vindicates me from the suggestions of those of Dr. Gregory's friends and party, and does me justice whenever any occasion offers. So that I am apt to believe that Dr. Gregory's letter was his own contrivance without the knowledge of Mr. Newton, who cannot be offended at the mention of 150 observations imparted to him; since I have accommodated him with as many as would make up 300, together with 100 at least of the superior planets ♄ and ♃, and about 100 of refractions; besides my observations of comets and the diameters of the planets, of which nothing is said in that epistle, lest I should seem to boast.

This I have said to Mr. Newton in a letter I wrote to him last Monday morning. I expected an answer on Thursday: and none coming, wrote to him then again to desire him to let me know whether what Dr. Gregory had wrote to you was by his direction or not; and having no return conclude he thinks not fit to take notice of it, or that he is not in town. I think it concerns not Dr. Gregory to have been thus busy, and that neither you nor I ought to take any more notice of it than Mr. Newton does; and, therefore, you may please to let that paragraph, and the next, stand as it is, without alteration. But as for what you think fit to add at the beginning, I am obliged to you for the intimation: you may add it if you please. I approve it.

Only I desire you that hereafter you acquaint not Dr. Gregory with anything that passes betwixt you and me: that so he may have no opportunity of making friends against me at my cost. Mr. Caswell is a very honest as well as a very ingenious person, and scorns flattery and baseness. We have been long friends: you need not be so reserved on his account. You may impart your mind to him in anything that concerns me: he will write to me and save you the labor.

I beg your pardon for the length of this letter. I have only to add, that my observations lie the King and nation in more than £5000, and myself in £1000, out of my own pocket to build my instruments and hire assistance. I have suffered much in my health by my night labors: the pains I have employed in calculations have been inconceivable: all the recompense I expect is acknowledgment of my industry; which those that would deprive me of, at the same time are unjust to our nation as well as injurious to me. The Doctor, I am apt to think, is not so much displeased at that paragraph, as at the whole letter; which he fears may contribute to undeceive some people that had

taken up false notions of me from the misrepresentations of his party; and may do me too great heed to the prejudice of his pretensions. I have never yet opposed him: but if he takes these ways of making friends, he must expect that I shall take notice of it. Be he what he will, I shall ever be what I ought, that is, Sir, your obliged and most humble servant,

JOHN FLAMSTEED, M.R.

To the Rev. Dr. Wallis, at his house in Oxford.

[Extracted from MSS, vol. 33, page 13, numbered from the *end.*]

No. 45.) *Letter from Mr. Flamsteed to Dr. Wallis.*

The Observatory, January 10, 1698-9.

HONORED SIR,

Yesterday I received a very artificial letter from Mr. Newton. It had been 3 days in coming from Jermyn-street by the general post: I am sorry it arrived not sooner, for then it had saved me the labor of contradicting mine the last Saturday. To oblige Dr. Gregory, Mr. Newton will not approve of that paragraph, and gives me reasons for it, such as they are. I have answered him this afternoon: but, at the same time, desire you to alter the offensive innocent paragraph as you intimated; so as you think it will give no offence, and as you wrote it in your last of December the 28th. I should be glad to know of you (or Mr. Caswell by your order) what forwardness the volume of letters is in, and when we may expect it extant. I shall send him a copy both of Mr. Newton's letter and my answer, by the next post, or next but one. My servant is absent; so I have nobody to transcribe it. I hope now all the trouble, Dr. Gregory's officious flattery has caused, is over; and that you will remember hereafter not to commit anything to his hands for me, but it shall be sealed up in a cover. Excuse me, and assure yourself I am always,

Honorable Sir, your affectionate servant,

J. FLAMSTEED.

To the Rev. Dr. Wallis, at his house in Oxford.

I think your alteration is thus:—Aliaq intervenerunt negotia (quæ nunc narrare nec est opus) quibus distinebar ne potuerim isti rei vacare; atq inter alia ut amicis aliquot id efflagilantibus exhiberum planetarum Saturni Jovis Martis loca plurima tum ex observationibus meis tum e tabulis calculo deprompta. Quæ moneo ne putes negligentiæ desidiæve meæ imputandum quod non citius huic operi me accinxerim.

[Extracted from MSS, vol. 33, page 15, numbered from the *end.*]

No. 46.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

January 10, 1698-9.

SIR,

Yours dated Jermyn-street, January 6th, arrived here last night, the 9th, with the General Post mark and charge upon it, as if it had come from some place less than 80 miles remote from London. I waited for it from the 2nd to the 7th instant, Saturday night; and then wrote to Dr. Wallis, that I thought he needed not take any notice of Dr. Gregory's letter to him, to forbear printing that clause in mine wherein I had mentioned you, since you took no notice of two of mine I had

wrote t[illegible]at week, concerning it, which made me think, you thought it not worth your while to concern yourself about it. Now I find you did desire Dr. Gregory to write so to him, I shall write to him myself to alter that passage, so as he was advised, and so as I believe you will find no just cause of offence in it: my letter goes to him this night, the altered paragraph you have at the foot of this letter *.

I did not think I could have disobliged you, by letting the world know that the King's Observatory had furnished you with 150 places of the moon, derived from observations here made, and compared with tables; in order to correct her theory: since (not to seem to boast) I said nothing of what more it has furnished you freely with. As I had leisure, and Mr. Halley has not stuck to tell it abroad, both at the Society and elsewhere, that you had completed her theory, and given it to him as a secret, I could not think you would be unwilling our nation should have the honor of furnishing you with so many and good observations for this work, as were not (I speak it without boasting) to be had elsewhere: or that it should be said you were about a new work, which others said you had perfected. I thought not it could be any diminution to you, since you pretend not to be an observer yourself. I thought it might give some people a better notion of what was doing here, than had been impressed upon them by others, whom God forgive. You will pardon me this freedom, and excuse me when I tell you, if foreigners come and trouble you it is not my fault, but those who think to recommend themselves to you, by advancing the fame of your works as much as they possibly can. I have sometimes told some ingenious men, that more time and observations are required to perfect the theory; but I found it was represented as a little piece of detraction, which I hate, and therefore was forced to be silent. I wonder that hints should drop from your pen, as if you looked on my business as *trifling;* you thought it not so, surely, when you resided at Cambridge: its property is not altered: I think it has produced something considerable already, and may do more, if I can but procure help to work up the observations I have under my hands, which it was one of the designs of my *Letter to Dr. Wallis* to move for. I doubt not but it will be of some use to our ingenious travellers and sailors; and other persons that come after me, will think their time as little mispent in these studies, as those did that have gone before me. The works of the Eternal Providence I hope will be a little better understood through your labours and mine, than they were formerly. Think me not proud for this expression; I look on pride as the worst of sins: humility as the greatest virtue. This makes me excuse small faults in all mankind, bear great injuries without resentment, and resolve to maintain a real friendship with ingenious men: to assist them what lies in my power, without the regard of any interest, but that of doing good by obliging them.

To Mr. Newton.

[Copied from the MS in the library of Corpus Christi College, Oxford. The manuscript, however, is not in Flamsteed's hand-writing: but the address to Newton, and two or three of the corrections, are in Flamsteed's own hand: which confirm its authenticity. F. B.]

No. 47.) *Extract of a letter from Mr. Flamsteed to Mr. Bossley.*

January 11, 1698-9.

I gave Mr. Newton, about 4 years ago, the same places of Saturn and Jupiter I gave you on our first acquaintance. He tells me he had tried to answer those of Saturn, and found he could do

* Not there. F B.

it nearly by only liberating his aphelion: but that it liberates very oddly: and that Kepler's eccentricity was well. In some discourse I had with him lately, he added further that the greatest fault in the superiors was when they approached each other nearest; and that, when they were more remote, each pursued his own way with his primary inequality little disturbed. You will hereby understand that he accounts the inequality to be restored every revolution of Saturn to Jupiter, which they would precisely if their distances of the one planet from the other were exactly the same in every conjunction: but these changing, and not being restored but in 59 years nearly, these inequalities are only restored at that period: and at all other times, they are less according as the planets are more remote from each other.

And I doubt not but Saturn moves swiftest in that revolution in which the sum of all his distances from Jupiter is least; and Jupiter, on the contrary, slowest. And again Saturn is slowest in that revolution wherein all his distances from Jupiter make the biggest sum, and Jupiter then swiftest: and that this is the reason why the motions of these two planets are found swifter in some ages, or parts of an age, than in others.

I give you these thoughts to assist you: in the mean time you may try whether your own way will do; and you may see whether these thoughts are agreeable to what you find by experience on comparing the observed places with the calculated. On the other side I give you a short table of the conjunctions of Saturn and Jupiter, for this and the past age, to help your consideration.

1484	=	♏ 14°	1603	=	♐ 2°
1504	=	♋ 18	1623	=	♌ 5
1524	=	♓ 21	1643	=	♈ 8
1544	=	♏ 24	1663	=	♐ 11
1564	=	♋ 27	1683	=	♌ 14
1584	=	♈ 0	1703	=	♈ 17
			1723	=	♐ 20

[Extracted from MSS, vol. 33, page 16, numbered from the *end*.]

No. 48.) *Letter from Mr. Flamsteed to Sir Hans Sloane.*

The Observatory, March 28, 1699.

Sir,

I have ever kept a very fair correspondence with your predecessors, Dr. Crone, Mr. Oldenberg, Mr. Collins, Dr. Grew, &c., and desired to have done the same with their successors. How I have been prevented, you know best. I am sure I never gave an occasion; and to let you see I will omit none of obliging you, when it lies in my power, I have herewith sent you a copy of my *Letter to Dr. Wallis*. Let it not surprise you that you find two different styles in it. I have told Mr. Bridman, in a letter I sent him with one, the occasion of it. I doubt not but he will show it you, if you desire it; if not, I have reserved a copy, which I shall show you when you please. I can only tell you now briefly, that I wrote it originally in English in great haste: that Dr. Wallis gave it in Latin: and that the first page, and part of the second, was added in my own Latin afterwards: which I am accountable for. But, for the language of all the rest, I am obliged to the Doctor.

I was told that it had been said at a meeting of the Council of the Society, that I had spoke ill of

them*. I suppose you smiled at this general sort of accusation. Doctor, I speak ill of no man willingly. I have an honor for the Society, and desire they may flourish heartily. If I come not at their meetings on Wednesdays, it is because both the day and time are unsuitable to my circumstances and occasions. Pray, if the same thing be said again by any one when you are present, please to testify for me that I am far from any such practices: and that, however I am used by others, I would do no otherways by them, than I would they should do by me; and you will exceedingly oblige,

Your friend and humble servant,

JOHN FLAMSTEED.

Dr. Sloane, to be left at the Repository,
in Gresham College.

[Extracted from Sloane's collection of MSS, No. 4044, in the British Museum.]

No. 49.) *Letter from Mr. Flamsteed to Mr. Bossley.*

Observatory, Novem. 2, 1699.

KIND SIR,

I have been informed by Mr. Leigh, in one of his, of your illness and recovery. I bless God for restoring your health, and pray for your continuance of it. That you may long enjoy it, it is my opinion that you ought to clothe yourself very warmly when you travel over your bleak moors and hills, and [not] put off your extraordinary clothing till you have rested some time within doors; but, above all things, be careful not too feed too fully: moderate and abate your diet, and feed sparingly. Drink no strong liquors; for nature, as you grow in years, grows weaker, and cannot discharge the load it did in your youth: if you forbear malt drink, and content yourself with milk and water, as you know I do, perhaps you will find the same good effect I do of it, for I am now, I bless God, seldom ill but when I forget to observe these rules. Serve God, and by the use of these easy means, I hope you will long enjoy your health to the comfort of your family (whose welfare you put to hazard whenever you expose yourself), and the joy of the friends with whom you either converse or correspond.

Mr. Leigh tells me you are like to have more leisure than formerly this winter, and therefore desire to have the places of the superior planets gathered from this year's observations, in order to proceed to the correction of Jupiter's motions, now you have done with Saturn. I give you them at the foot of this letter. But because I have been slower than usual in answering yours of late, I shall here give you the reason of it, which will not, I persuade myself, be unacceptable to you.

* In the minute books of the *Council* of the Royal Society, March 20, 1698-9, there is the following entry: viz., "It was ordered that Mr. Flamsteed should be desired to produce in writing to the Council, any charge or complaints "he had against Dr. Sloane, as Secretary, or otherwise." It was probably the report of what had thus taken place at the Council (of which Flamsteed was then a member) that dictated this letter. In a paper of memoranda relative to the above letter, in Flamsteed's hand-writing, preserved in MSS, vol. 35, page 195, he remarks, "I never had "any difference with them [i. e. the Secretaries], nor with Mr. Hook, more than others have: and should not with "the present, if Mr. Halley, their clerk, had not, out of malice, slandered me in his Transactions, and drawn "Dr. Sloane into his party. Never said worse of the Society than what they now complain of: though I have been "traduced to foreign ambassadors and gentlemen, undeserved." Notwithstanding these disputes, Flamsteed was *again* elected a member of the Council, at the ensuing anniversary. F. B.

About the same time that I began to correspond with you, I renewed a correspondence with Mr. Newton, to whom I imparted, in the years 1694-95-96, about 200 observations of the moon, together with her places, calculated from my tables, and the elements of the calculation and differences from the observed places, deduced by the old catalogue of the fixed stars made to the beginning of 1686, of which I gave copies both to you and him, that he might rectify her theory whilst I was rectifying and completing the catalogue of the fixed stars; he promising to impart his corrections to me, of which he has acquainted me with a part. But finding him not so open as he pretended to be, and that he was persuaded to neglect the performance of his promise by some persons that courted and flattered him for their own ends, and boasted that he had perfected the theory so as it would answer observations within 3 minutes, not to be beholden to him, I set first to calculate the places of about 60 moons observed on or near the limits of her orb. Which I did the beginning of this year: whereby I found, using the new catalogue of the stars, that the mean distance of the moon from the earth was about a semidiameter of the earth greater than I had made it in my tables in the *Doctrine of the Sphere*, and, therefore, that the horizontal parallaxes of those tables must be diminished as many seconds as they consist of minutes. That the atmosphere of our earth, to 60 miles high, casts a shadow on the moon's face in eclipses which augments the diameter of the earth's shadow so much, that, in lunar eclipses, it may be taken as by the old tables without sensible error.

You will wonder that I should assert that our atmosphere should cast a shadow: but if you consider what a shadow the clouds cast on our earth in a sun-shine day, and that they are of the same consistence with the air they swim in, the wonder will cease. I find further, from the old observations compared, that the moon's greatest latitudes agree not to the theory, but there is sometimes an error of near 2 minutes: but it seems regular; which makes me hope, when I have leisure to think further of it, that I may both find a reason of it from the laws of gravity, and a salve. After this, I caused the places of about 80 full moons to be calculated from the observations and tables. At present I have 90 places near the quadratures under my hands. My servant has but just entered on those; and I cannot proceed till he has calculated her places to the given times from the tables, in order to find the parallaxes for her true places from the visible.

By such time as I have finished these, I shall be able to tell you how to find the moon's true places by numbers, (to make you a recompense for your pains on Saturn and Jupiter). I intend to impart them to you as soon as I have perfected them; or my present corrections immediately if you desire them. But you know the vast labor I must be at in this work, and what time it will require to find, correct, and examine numbers by observation, only. Now I have the happiness to see a return of the moon's apogee since I began my observations with the mural arc. So that now I shall see easily the distance of the moon from the sun, or whether she be liable to secular inequalities as the superiors are, but it seems not probable she should be.

You see I deal openly with you, and let you know the result of my labors, as you acquaint me with yours. I have not calculated the places of ♄ or ♃ from your numbers (but I foresee ♄ will agree as last year). I leave it to be done by yourself, and desire to be informed of the agreement of your calculation at your leisure.

I caused a guinea and a half to be paid Mr. Crundale for Mr. Leigh, which I hope you will tell me he has received in your return. I pray God continue both your health and his, being ever, Sir,

Your affectionate friend and servant,

John Flamsteed, M.R.

	1699.		Ascen. recta.	Dist. a Polo.	Longit.	Latit.	
		h. m. s.	° ′ ″	° ′ ″	° ′ ″	° ′ ″	
Jupiter	Maii	9 13 58	266 38 10	113 0 5	♐ 26 54 33	0 26 44	Bor.
	Junii 8 ☉	1 12 13	263 43 50	112 57 25	24 13 44	0 24 1	
		2 12 8	263 35 10	112 57 15	24 5 44	0 23 50	
	Augti. □	22 6 29	258 26 50	112 52 30	19 22 0	0 10 57	
Saturn	Julii	21 13 26	333 3 0	102 55 35	♓ 0 21 50	1 40 10	Aust.
	Augti. 8 ☉	18 11 35	331 4 10	103 41 10	♒ 28 17 30	1 42 10	

Sent him the same again, Novem. 28th, fearing this miscarried.

[Extracted from MSS, vol. 33, page 14.]

No. 50.) *Letter from Mr. Flamsteed to Sir Christopher Wren.*

The Observatory, March 28, 1700.

HONORED SIR,

My man has given you an account of the works I have under my hands, but I find he forgot what I chiefly gave him in charge; which was to add that I am now provided both of a competent number of places of the moon derived from observations, taken with the mural arc, of Saturn and Jupiter, and also of necessary and convenient tables for examining any theory of those planets' motions that shall be brought: which you may please to take notice of whenever you fall into company where my labors are mentioned, if you think it convenient.

I am glad you are satisfied with what he acquainted you concerning my want of help is no pretence, and am very much obliged to your kind offer of affording your assistance to procure supplies: but before that be attempted I am of opinion it would be very proper that some persons who understand well the nature, tendency, and use of my labors should inspect them.

The gentlemen of our Society (however ingenious and well-wishers to all sorts of ingenious study) know very little of mine. There are none about town but yourself, and the Master of the Mint (Mr. Newton), that thoroughly understand how they conduce to the improvement of navigation, geography, and natural philosophy. You are both my friends; both zealous for the honor of the King and nation; and equally desirous that they should be published with all convenient expedition. My desires are altogether the same: I request the favor of you, therefore, to allow me to agree, some time immediately after Easter, when you will both come down and take a dinner with me in the Observatory, without other company than your alone selves; that I may show you what I have by me ready for the press, and what I am preparing for it; and have your advice how to proceed. I am going into Surrey for 6 or 7 days: after my return I will endeavor to attend you in London; and, if God spare me health, I doubt not but, with His blessing and the assistance of yourself and the Master of the Mint, (and that, too, such as will create you little or no trouble,) the world may quickly see a large and useful produce of the labors of your most obliged, humble servant,

To Sir Christopher Wren, Kt. J. FLAMSTEED.

[Extracted from MSS, vol. 33, page 31, numbered from the *end*.]

No. 51.) *Letter from Mr. Flamsteed to Mr. Lowthorp.*

The Observatory, May 10, 1700.

My Friend,

I esteem it the duty of all Christians as much as in them lies to have peace with all men, and it was the sole consideration of this duty, and a tenderness for you, that induced me to advise you to be careful of your behaviour towards Mr. Newton. I have been with him thrice since I received your first letter, but he never so much as mentioned your affairs to me, and I found no opportunity of discoursing it with him, he is so possessed with prejudices against me by some people's suggestions whom you know very well, that I can have no free discourse with him: this day was a sevennight I waited on him, and shall give you the sum of our discourse, whereby you will be satisfied that I have no reason to have any tenderness in respect of him but only on the consideration of Christianity.

He inquired first how forward I was in the catalogue. I told him that, since the fixed stars of the zodiac were finished, I had also finished those of the southern constellations, and had now about 400 of the northern under my hands; that I had, besides 50 places of ♀, forty of ♃, and 30 of ♄, at the opposition of the sun and quadratures, calculated from the observations made with the mural arc from the years 1689 to 1699, besides above 200 of the moon at the like positions. Some occasion of discourse about comets happening, I acquainted him that Dr. Gregory gave out that since he had altered his paths of comets, and instead of parabolas made them ellipses, his theories would represent all Mons. Cassini's observations within a minute, whereas I thought he had only my observed places to represent, and that it was not only an injury to me, but the nation, to rob our Observatory of what was due to it, and further to bestow it on the French. He seemed not much moved at this, but confessed fairly he had employed no observed places of the comets but what I had given him, and the same he confessed of the lunar places, which drew us again to discourse of her. I acquainted him with the new tables of equations I had made, for the easier computing of her place and their method: he fetched down some of his own and showed me, which I regarded but slight, because they were as inconvenient as the old. Next we must talk about printing: I told him it would not be convenient to print the catalogue of the fixed stars till the observations were published; that these must be printed first as being the ground on which it was built, and from which it was derived; that the charts of the fixed stars must be engraved to accompany the catalogue. As to the charts, he urged they would only show map-makers to steal and transcribe into their globes; but for this I told him I should take care to prevent them by procuring privileges, and that I did not intend they should reap the fruit of my labours gratis, after the catalogue was printed. I went on and told him the book of tables should follow; at this he started, and asked me "what tables," and "if I would publish any for the moon." My answer was, that she was in his hands, and, if he would finish her, I would lend him my assistance; if not, I would fall upon her myself when I had leisure, and I doubted not of good success; but that the tables I intended were such as I made use of for deducing the places of the stars and planets from my observations with more than usual expedition, and some others that would be of good use. Hereupon he recollected himself and was calm. He said something more to move me to desire earnestly his lunar theories and new additional tables, but I would not understand him; for having been at much more pains in calculating her's and the planets' places for him than ever his Captain Halley was, and having had a promise from him that I should be the first man to whom he would impart them, and that he would impart the observations to nobody whatever without my leave, I looked upon his imparting what he had deduced from them to Dr. Gregory and the Captain as a

greater breach of promise than if he had imparted the observations themselves, and so would not request that as a favor which was my due, and which he could not in justice grant to any other. I designed indeed to save myself some labor by putting the moon into his hands, but I never thought of depriving him of the honor of his pains. He detracts from himself by this reservedness, and does that which his flatterers would persuade him I do: during this discourse he once complained that I was *reserved*. I answered I had not been so in the least; as my many communications would testify. He would have said something to excuse his Captain and Doctor, but, to avoid reflections, I would not hear him, but only desired that he would oblige me so far as to come down hither with Sir C. Wren some morning alone and take a dinner with me, and he should then see in what forwardness my work was, and we would consider how to forward it to the press. When I urged this again that he would come down, he asked me a little quick, "what for?" My reply was as before; but I added, that by his seeing what was done I hoped to stop the mouths and clamors of some people that asked why I did not print; that it was soon said, but required some consideration before it could be done; that it was a popular reflection, and apt to take, and therefore to be timely obviated; but that my work was like the building of St. Paul's; I had hewed the materials out of the rock, brought them together, and formed them, but that hands and time were to be allowed to perfect the building and cover it. This, with some uneasiness, was allowed, and I obtained a promise that he would accompany Sir Christopher, with whom he would agree a time that might be convenient for both them and me. Thus we parted: you see I have promised him nothing, nor he me. Since he is so reserved, I think it concerns me to be no less so; but when he comes hither I shall not be averse to impart more lunar observations to him, provided that he withdraw what he has imparted to others, or stop their reflecting discourses, and own before Sir Christopher what he has already received, and what I then imparted to him; but if otherwise, I know what I have to do, and still am resolved I will give him no cause to quarrel with me. We will part as good friends as we meet, and I will leave Time (the mother of Truth) to vindicate me to the world, and I foresee she will do it justly and fully.

I must request the favor of you to keep this letter by you, but let it not be seen till I acquaint you with the result of our next meeting at the Observatory, when perhaps we may have an occasion to talk of your affair, and he may be brought to a right understanding. I believe him to be a good man at the bottom, but, through his natural temper, suspicious, and too easy to be possessed with calumnies, especially such as are impressed with raillery. To cure him of it, finding a Bible in his room where I waited his rising, (for I got to his house before he was up, and spent a part of the time I waited in reading,) meeting with a sheet of paper I wrote upon it this distich, which I remembered from a late satire—

A bantering spirit has our men possessed,
And wisdom is become a standing jest.

Read Jeremiah, ch. ix. to the 10th verse.

I do not know whether he has seen it, but I think he cannot take it amiss if he has; and if he reflects a little on it, he will find I have given him a seasonable caution against his credulity, and showed him the way of the world much better than his politics or a play could do.

I have lost your letter, but, to the best of my memory, the only thing in it that needed an answer was what related to Mr. Newton. I know you love not lingering answers, therefore rather choose to give you this now, than wait till I find it that I might give you one more perfect; if I have forgot

anything material, pray let me know it, and, God sparing me life and health, you shall have a speedy return from, Sir, Your affectionate friend to serve you,

JOHN FLAMSTEED.

P.S. 'Tis given out at Oxford that Mr. Newton has improved his doctrine of gravity so far that he can answer all my lunar observations exceeding nearly; and that now there is little need of them, since all the inequalities of the moon's motions may be discovered by the sole laws of gravitation without them. I said nothing of this, because I had moved him enough with what I had said about the comets; but, to the honest man that told me of it, with some indignation I answered that he had been as many years upon this thing, as I had been on the constellations and planets altogether: that he had made lunar tables once to answer his conceived laws, but when he came to compare them with the heavens, (that is, the moon's observed places,) he found he had mistook, and was forced to throw them all aside: that I had imparted above 200 of her observed places to him, which one would think should be sufficient to limit any theory by; and since he has altered and suited his theory till it fitted these observations, 'tis no wonder that it represents them: but still he is more beholden to them for it than he is to his speculations about gravity, which had misled him. Mr. Hobbs boasted that his laws were agreeable to those of Moses. Dr. Eachards tells him he doubted not of it, for being drawn from Moses' works, and copied into his, he might be sure they would agree, except the laws of Moses were flown, which he was sure they were not.

As I am folding this up, my wife puts yours of May 3rd into my hands. I thank you for it, and shall answer it after the holidays, by which time I hope to enjoy Mr. Newton's company here.

J. FLAMSTEED.

Superscribed to Mr. John Lowthorp.

[Extracted from MSS, vol. 33, page 32, numbered from the *end.*]

No. 52.) *Letter (unfinished) from Mr. Flamsteed to Sir Isaac Newton*.*

Began June 18, 1700.

SIR,

That the earth's axis is not always inclined at the same angle to the plane of the ecliptic, is a discovery wholly owing to you; and strongly proved in the 4th book of your *Princip. Phil. Nat. Math.* How much the alteration of this angle ought to be, you have not shown: and whether you have yet determined or no, I know not. But, having found it sensible in the observations made with the mural arc, described in my *Letter to Dr. Wallis*, (wherein the parallax of the earth's orb, at the pole, is proved by eight years' continued observations,) and not doubting but an account of the observations, that show it, would be grateful to you, I have resolved to impart them to you, as to one who not only delights in these things, but are able to judge of them, and has a particular interest in this, which proves the gravitation of the parts of matter, and the truth of your theory of our earth.

* This letter is entitled "A Letter to Mr. Is. Newton, Warden of the Mint, showing what have been the errors "of the Mural Arc." It appears to be only the draught of a letter, intended to have been sent; but whether it was actually completed and forwarded, I am not able to ascertain. As it, however, explains Flamsteed's mode of proceeding, for determining the subjects therein alluded to, I have thought it right to present it to the public, for their information. F. B.

When I wrote that letter to the Dr., I told him that, to determine the parallax and this nutation to some competent exactness, we ought to have instruments, of 15 or 20 feet radius, for taking the meridional distances of the stars from the vertex. I then had employed only such of these as I thought most proper for that particular discovery; and no more than was just necessary, by reason of the haste of the press, which forced that letter out of my hands before I had time to examine such observations as I foresaw would most probably make the nutation sensible. But, having now had occasion to consider and compare a competent number of them for another use, and finding this nutation sensible whenever proper observations are compared:—since no great favorer of a science so useful to our nation appears who might, by the easy charge of a large instrument, at once oblige all ingenious men, and bring certain honour to himself and country thereby:—since my cost and pains, already bestowed, have met with quite other rewards than they deserved:—I shall lay by the hopes of seeing any such formed, till I may have some settled station where I may build a larger than I proposed, at my own expense; and, from my present stock of observations, examine what may be the utmost possible quantity of this nutation. And this I rather choose to do, because when I acquainted you with my discovery of the parallax of the orb at the pole-star, you suspected that the nutation had caused those alterations in its zenith distances, which I took for the parallax: which makes me think you esteem this nutation to be something more than it really is; as you perhaps judged the parallax less than it was really found.

I determined the least quantity of the parallax of the orb at the pole-star, that my observations would allow; and showed that, whatever this nutation shall be found, it must be added to the parallax determined, which was diminished by it. This was needful, that you might not mistake it for the nutation. It will be necessary now to show what is the greatest quantity of the nutation; that you may not esteem it greater than it really is: which, your opinion of the parallax makes me think you may.

Had the wall, to which my instrument is fixed, continued stable and unmoved, had it continued fixed, both the parallax of the orb, with the nutation of the earth's axis, would have been discovered and determined, as exactly as the smallness of my arc would permit, in the compass of one year's observations. Whereas, now I find myself obliged to examine all I have made, to come to some near guess at it; and to confirm what is derived from the first year's observations, by the consent of the following. But, since it does not retain its first position, but sinks every year, these cannot be found without the errors, caused by the gradual sinking of the arc, being first known. I suspected some such thing might happen, when I first began to use it: and therefore concluded that frequent observations of the meridional zenith distances of the stars in the foot of Castor would show the errors soonest, and most easily: both because these stars, lying very near the solstitial colure, alter their distances from the pole insensibly, as also because they culminate within less than 30 degrees of our vertex; so that they were not liable to uncertain and variable refractions. Whereas those stars of Sagittarius, that lie near the winter solstice, and opposite points of the ecliptic, pass the meridian low; and so may be entangled with refractions, suspected to be variable.

I considered further that these stars, in the foot of the Twins, were the more proper for this inquiry, because, their latitudes from the ecliptic being small, the parallax of the earth's orb must vanish, and become insensible at them. I was sensible that the nutation of the earth's axis (if so much as to become sensible) must be perceived in these stars, because 'tis made directly towards, and from them: but I esteemed it then so small, that it would not be sensible in my observations. But, however, that it would not affect those stars that lie near the equinoctial points and colure: because

their distances from the pole of the world are not altered by it, as those of the stars in the foot of the Twins are. Only in those stars which lie in the constellations of Virgo and Pisces, the declinations, or distances from the pole, vary fastest (about 20″ per year), which is no obstacle to the enquiry: the variation being readily found by tables prepared for that purpose, and easily allowed for.

'Tis necessary to have the errors of the instrument truly stated, in order to find the true distances of the stars and planets from our vertex; which are corrupted (as they are copied from the instrument) by the sinking of the wall on which 'tis fixed. I shall therefore first seek out these by stars both near the solstitial and equinoctial colure. But I must advise you that those, nearest the equinoctial points and colures, are most to be relied on: and, of them, such as have least latitude from the ecliptic are chiefly to be regarded; because those, which are farther distant from the ecliptic, may be suspected to be corrupted by the parallax of the orb; and those, that are farthest removed from the equinoctial points, to be entangled with the nutation. Some little effect, and some small errors, both these may have caused in those many places of the moon and planets I have imparted to you: but so small, that it needs not be regarded. I only mention them, that you may not think that I was not aware of them.

1689, July 15, 16, 17. The beginning of the divisions on the mural arc was found and determined by myself and Mr. Sharp, then my servant, by the transits of the bright star in the Dragon's head, near our zenith, both on the instrument and western plane.

	Dist. from Vertex.
By the transit of a star in the Swan, over the meridian observed on the western plane, Oct. 5 following	22′ 5″ Bor.
But, on the instrument, Sept. 19 and Oct. 8 following . . .	23 55
Difference =	1 50
Error =	0 55

By the revolves of the screw on the edge, these distances were about 10″ less. I propose therefore the error at present 1′ 0″: whereby all the distances, taken to the south, are too much; those towards the north, or on the pole-star side, too little.

I thought not of the sinking of the wall, and therefore doubted not but this fault came by some stroke, or injury, the arc might have received since its first verification and division: but, to be certain of it, Nov. 15 following I took the distance of ζ in Cassiopeia, from our zenith, and found it

		°	′	″	Rev.	No.
On the instrument	Novem. 15 =	0	42	0	= 15.78 B.	1
	Decem. 15 =	0	41	50	= 15.69	2
On the western plane	Decem. 13 =	0	44	20	= 16.71	3
	Decem. 16 =	0	43	55	= 15.72	4

Comparing the 1st and 3rd of these together, the difference (2′ 20″) halved gives the error of the instrument .	1′ 10″
The 2nd and 3rd	1 15
The 1st and 4th	1 10
The 2nd and 4th	1 2½
The mean is nearly . . .	1 10

	°	′	″	Rev.
Again, τ Persei, on the western plane, Decem. 16 =	0	0	25	= 0.14 Bor.
on the instrument, Decem. 17 =	0	1	50	= 0.39 Aust.
Half their sum makes the mean =		1	7	

		°	′	″	
Again, in the shoulder of Perseus, γ					Rev.
on the western plane, Decem. 16	=	0	48	10	= 18.14 Aust.
on the instrument, Decem. 17	=	0	45	55	—
Their difference (2′ 15″) halved, gives the error of the instr.	=		1	7	

Whence the error of the instrument may be concluded, at this time, some little less than 1′ 10″. I allow it: though, in copying the observations from the first notes into my fair journals, I have made it only 1′ 0″; and used it so all the following year: not suspecting then the subsiding of the wall, and not thinking it worth regarding when I first began to perceive that the error caused by it was some little augmented.

Both from these experiments, and the observations of the following year, 1690, compared with those of November 1689, I conclude the error of the instrument, in the November observations, to have been only 1′ 10″, as in December: which I make use of, everywhere, in reducing them to the truth in the next page *; whereon, for finding the error of the instrument in the following years, I have transcribed the meridional zenith distances of such stars as will be convenient for the purpose; and some others that have been frequently observed; such as the stars in the head of Aries, the Lion's Heart, the Virgin's Spike, the stars of Sagittarius and Capricornus, that may be employed for the same purpose with due allowances.

Before I enquire what arguments for the nutation of the earth's axis my observations afford me, it will be necessary to enquire what were the errors of the instrument, both before the middle of December 1689, when it was determined by experiment, and after: and how it stood in the whole of the year 1690 following.

		°	′	″
(1.)	1689, Decem. 16 and 17, α Tauri, à vertice in merid.	35	38	$\frac{1}{2}\frac{0}{3}$
	1690, Decem. 16, eadem	35	38	15

The annual access of this star to the pole is 10″: therefore, if the instrument had not sunk, the star's meridional zenith distance, that was 35° 38′ $\frac{1}{2}\frac{0}{3}$″ in December 1689, would have been on December 16, 1690, 35° 38′ $\frac{1}{1}\frac{5}{8}$″. But, it was then found 35° 38′ 35″: whence 'tis evident that the error is increased since the 16th of December 1689, near 20″. And because then it was found 1′ 10″, in December 1690 it will be 1′ 30″.

In like manner,

		°	′	″
	1689, Decem. 17, γ Tauri, its merid. zen. dist.	= 36	38	45
	Corrected by its annual access to the pole, 10″, gives its distance in 1690 .	= 36	38	35
	Which was then found by the instrument	= 36	38	50
(2.)	More than it ought, by		0	15
	Add the error of Decem. 1689 . . .		1	10
	It makes the error, Decem. 1690, to be . .		1	25

These observations I have pitched upon for the trial, because, both being made on the same days of the year, were equally affected with the nutation, and parallax of the orb, if we suppose them sen-

* This alludes to the page of the MS letter, whereon certain observations of zenith distance, made in Novem. and Decem. 1689, and in Jan., Feb., March, June, Sept., Novem., and Decem. 1690, are arranged for the purpose of this enquiry; but which I have not considered it necessary to transcribe. F. B.

sible at the star. And therefore the difference of the observed zenith distances, after they are corrected by the annual access to the pole, can proceed from nothing but the sinking of the instrument.

(3.)

		° ′ ″		° ′ ″
1689, Nov. 28,	η Piscium .	= 37 45 10	ν Piscium .	= 47 34 30
Dec. 26,	.	= 37 45 15	.	= 47 34 30

Hence it is evident that the error was the same, on the 28th of November, that it was on the 26th of December: that is, 1′ 10″ in both; or but 5″ bigger at the latter.

(4.)

		° ′ ″		° ′ ″
1689, Dec. 16,	η Gemin: .	= 28 56 0	μ Gemin: .	= 28 51 30
1690, Jan. 6 and 10	.	= 28 55 55	.	= 28 51 30

Whence 'tis evident again that the same error continued to the 10th of January following. For, the alteration of the nutation could cause no sensible alteration of these stars' zenith distances, in this place, and at this small interval of time.

To avoid all suspicion of change of meridional zenith distances by the nutation, I shall next enquire the increase of the error of the instrument by such stars, on which it could have little or no influence: such are they as lie near the equinoctial colure. But the sun approaching the stars of Pisces, I cannot longer find them, to employ; and therefore make use of the Virgin's Spike, which, though it may be a little, cannot be much affected by it.

(5.)

	° ′ ″
1689, Decem. 1, the star's merid. zen. dist. is . . .	60 59 40
1690, Feb. 14, I find it noted	61 0 10
Its annual access to the pole is 20″, and therefore ought now to be	61 0 15

Whence it appears that the error in February 1690 is about 30″ more than it was in December 1689. I state it 1′ 30″.

		° ′ ″
1690, April 11 and 12,	α Virginis .	= 61 0 $\frac{5}{10}$
26	„ .	= 61 0 10
May 12	„ .	= 61 0 15
June 5	„ .	= 61 0 15

From all which, compared together, 'tis manifest that the error was the same Feb. 14 to June 5, 1690: that is, 1′ 30″.

That it continued the same all this summer will be evident by comparing observations of the same solstitial star, taken at both the equinoxes, when the earth's equator librated widest from the ecliptic, and the effects of the nutation were the same, and therefore not to be regarded.

(6.)

		° ′ ″
1690, March 7,	μ Geminorum .	= 28 51 45
Sept. 15,	„ .	= 28 51 45
19,	„ .	= 28 51 50

Therefore the instrument continued unmoved from March 7 to Septem. 19; and the error 1′ 30″.

In September, the stars of Pisces, that had been observed in November last, became again observable. I will examine therefore by them how much the error of the instrument is increased since Novem. 28, 1689.

			°	′	″		°	′	″		°	′	″		°	′	″
1690, Sept. 15	.	ε =	45	16	10	e =	47	28	40	ζ =	45	33	10	f =	45	30	25
1689, Nov. 28	.		45	15	50		47	28	25		45	33	0		45	30	15
			—	0	20		—	0	15		—	0	10		—	0	10
Error, Sept. 15, 1690		=		1	30			1	30			1	30			1	30
Error, Nov. 28, 1689		=		1	10			1	15			1	20			1	20
1690, Sept. 16	.	β =	49	19	0	γ =	49	52	55	$\varkappa^1$ =	51	54	40	$\varkappa^2$ =	52	3	15
Esset . . .			49	18	40		49	52	20		51	54	25		52	2	50
			—	0	20		—	0	35		—	0	15		—	0	25
Error, Sept. 15, 1690				1	30			1	30			1	30			1	30
Error, Nov. 28, 1689				1	10			0	55			1	15			1	5
1690, Dec. 3 and 11		$\varkappa^1$ =	51	54	$\frac{11}{30}$	$\varkappa^2$ =	52	3	5	λ =	51	24	$\frac{5}{10}$				
1689, Nov. 28	. .		51	54	25		52	2	50		51	23	45				
				0	20		—	0	15		—	0	20				
Error, Sept. 15, 1690				1	30			1	30			1	30				
Error, Nov. 28, 1689				1	10			1	15			1	10				
1690, Sept. 20	. .	λ =	51	24	0	ω =	46	19	45								
Esset . . .			51	23	45		46	19	30								
			—	0	15		—	0	15								
Error, Sept. 15, 1690				1	30			1	30								
Error, Nov. 28, 1689				1	15			1	15								

Which being done as above*, 'tis evident that on November 28, 1689, the error of the instrument was about 1′ 10″, as it was found by the experiments of December 13 and 16 following.

The errors being thus got from Novem. 28, 1689, to Jan. 10, 1690, to be 1′ 10″, and from Feb. 14 to Decem. 16, 1690, to be 1′ 30″, it remains to be enquired what it was from Sept. 13 to Nov. 28; and when it was that it increased from 1′ 10″ to 1′ 30″ betwixt the 10th of Jan. and 14th of February. For the first,

				°	′	″				°	′	″
(7.)	1689, Sept. 13,	γ Capric.	. =	69	29	10	.	.	δ =	68	57	30
	Oct. 26,	„	. =	69	29	10	.	.	„	68	57	25
	1690, Sept. 15 and Oct. 29		. =	69	29	40	Sept. 25	„		68	57	55

The annual access of these stars to the pole is 15″; which, being applied to the error, will be found in September, 1689, to be 45″ or 50″.

				°	′	″				°	′	″
(8.)	1689, Novem. 15,	λ Piscium	. =	51	23	50	.	.	η =	37	44	50
	Novem. 28,	„	. =	51	24	5	.	.	„	37	45	10
						15						20

Whence 'tis apparent that the error was greater by about 15″ or 20″ on the 28th of November than on the 15th or 16th: and since on the 28th it was 1′ 10″, on the 15th it will be 0′ 50″ or 0′ 55″, as it had been found by experiments on the 5th and 8th of October.

* These stars all alter their declination 20″ per annum. I have allowed so much in these collations: the alteration of 2 months being only 3⅓″, which is scarce sensible on the limb. See my *Letter to Dr. Wallis.*

1690, Jan. 10 and 13, the meridional distances of the Bull's south eye from our vertex were the same, 35° 38′ 25″ = 807·72rev.: so that the same error still continues.

		°	′	″		Rev.
I find it, Jan. 23	=	35	38	11/10	=	807·75
Feb. 2	=			30	=	·77
„ 5	=			30	=	·76
„ 8	=			35	=	·79
„ 10	=			35	=	·80
(9.) „ 14	=			35	=	·75
„ 18	=			35	=	·80
„ 19	=			40	=	·79
„ 22	=			40	=	·79
March 13	=			11/13	=	·78

If we allow the parallax of the orb sensible at this star, the earth receding from it, its latitude must become less, and consequently its zenith distance must be diminished on this account. And if we admit the nutation perceptible in it, the pole of our globe approaches it on this account, and its zenith distance is still more diminished by the nutation. But, after all these causes working the same way to make the zenith distances less, they are found increasing continually from the 23rd of January to the 22nd of February: not accounting anything for its access to the pole, on the account of the recess of the equinoxes, because altogether insensible in a month's time at this star. Hence I consider the error of the instrument increased 20″ betwixt the 10th of January and the 22nd of February: that having stated it on January 10 only 1′ 10″, on the 2nd of February it was increased to 1′ 20″, and on the 7th or 8th to 1′ 30″; which error continued all that year following, to Dec. 16, on which day the observations end, the threads being broke. Before the beginning of the new year, 1691, the instrument again sunk; and the error found bigger afterwards.

From these collations, I have stated the errors of the instrument from the 13th of September, 1689, to the 16th of December, 1690, as follows:

			′	″		
1689, Sept. 13,	error	=	0	50	per collat.	(7)
Oct. 26,	„	=	0	55	„	(7)
Nov. 15 and 16,	„	=	0	55	„	(8)
„ 28,	„	=	1	10	„	(3)
Dec. 16,	„	=	1	10	per experimen.	
1690, Jan. 10,	„	=	1	10	per collat.	(4)
Feb. 2,	„	=	1	20	„	(9)
„ 7,	„	=	1	30	„	(5)
Dec. 16,	„	=	1	30	„	(1) and (2)

and hereby corrected the zenith distances, observed and copied from the instrument, of a good number of remarkable fixed stars, chosen conveniently for finding the errors of the instrument in future years, and discovering the nutation. To each of the stars, when first observed, I have added the variations of their distances from the pole, for one degree increase of their longitudes: whereby their true distances from the vertex, or pole, may be gained for any time to come, within an age: and comparing them (correct by the variation) with the observed, the error of the instrument and nutation (if sensible) discovered and determined.

In the end of [the] year-books* I have enquired the errors of the instrument by several stars; not having any regard either to the parallax of the orb, or nutation (which their agreement shows to be small). But, here, I shall pitch only on such as may serve to determine the errors more accurately, and give the nutation, if sensible. And therefore I shall make use of those stars that lie nearest the equinoctial colure; employing also the solstitial stars, as often as I find them observed at the times of the equinox.

Had I been aware of the meridional zenith distances being corrupted by the parallaxes and nutation, when I first began to employ the mural arc, I had been as careful to forecast for these, as I was for the pole-star on another account. Since I was not, it cannot be suspected that any of them are wrested to show what they would not afford. I give the observations simply as they were copied from the instrument: my reader, if skilful, will see whether they are justly applied or no. I must only acquaint him that when the zenith distances of any stars have been observed several times together, one after another, and that there is some small difference betwixt them, I make use of that which is biggest: because that when, through haste, due care has not been taken to clear the edge of the index, the little dust and filth adhering to it sometimes makes the distance, numbered on the diagonals, 10 or 15 seconds less than it ought to be, or really was and would have been numbered, had the dust been wiped off. I find, by comparing my observations of the sun's meridional distances from the vertex at the solstices, and the latitude thence deduced with the latitude found by the pole-star, that some such fault has been committed as requires this allowance of about $\frac{1}{4}$ of a minute, to be added to all the zenith distances observed †: which might happen by the stretching of the feet, or bending of the beam compasses, when the points of 60 and 30 degrees were laid off. And that this must be applied in all the measures taken, whether the stars passed the meridian to the north or south of our vertex: or rather, 20″ when above 40 degrees south or north; 10″ when less. When therefore these observations come to be applied, 15″ must be deducted from the errors; which are always to be subtracted from the zenith distances, in the southern part of the arc: or the zenith distances, correct by the simple errors, must be augmented 15 seconds.

I shall copy but a few observations of those many I have employed at the end of the year-books, or diaries of my observations, where I have sought out the errors; but rather excerp such observations as I have not yet made use of, in the enquiry of the errors; and which I think most proper, on all accounts: whereby the errors, I have formerly determined, will either be confirmed or corrected. And herein I shall take care to compare observations of stars made at the same time of the year, chiefly for finding the errors: because then, neither nutation, nor parallax of the orb, can affect them. But, for enquiring the nutation itself, I must compare observations of stars lying near the solstitial colure, and taken about the solstices, with other observations of the same stars, got near the equinoxes: or of the stars of Virgo and Pisces taken at the same times. Hence I conclude the errors of the instrument,

		′	″	
1691, Jan. 20,	=	2	5	per stars in Gemini.
Feb. 20,	=	2	10	—— Taurus, Gemini, Virgo.
March 10,	=	2	15	per ν and γ Virginis.
April—August	=	2	15	per α Leonis and α Virginis.

* These are the original observation books (MSS, vols. 4—8), where the steps of the process, pursued by Flamsteed, are given in detail. F. B.

† See this note, in page 186; there not being room for its insertion here. F. B.

		′	″	
1691, Sept. ineunte	=	2	20	per β, κ, $\varkappa$ and λ Piscium.
October	=	2	25	
Novem. 2	=	2	25	per α Leonis.
„ 22	=	2	35	per stars in Gemini.
Decem. 1	=	2	30	
„ 9	=	2	35	per stars in Gemini and Pisces.

It may be an argument for the nutation of the earth's axis, that the errors of the instruments are found greater, by about 15″ or 20″, by the stars in Gemini, than by the stars of Pisces, in December. For, admitting the nutation to be about $\frac{1}{4}$ or $\frac{1}{5}$ of a minute,—— *

In the months of March and September, the stars in the feet of Gemini are nearest the vertex, and the error will be [the] same that it is found by the stars of Pisces and Virgo. But, in June and December, the said stars of Gemini, with those that lie near the opposite parts of the same colure, will be remotest from our pole; and therefore the error of the instrument greater by them, than by the stars of Virgo and Pisces, by about $\frac{1}{4}$ or $\frac{1}{5}$ of a minute.

I find but few observations of the meridional zenith distances of the stars of Virgo and Pisces taken this year [1692 ?] because most of my pains were employed in constellations remote from [the] ecliptic, and therefore can only determine the error of the instrument from other stars, as I have done in this page. But, from some few of them, arguments may be drawn for a nutation of the earth's axis.

			′	″	
1692, Jan. 1,	Error	=	2	40	per α Libræ.
„ 23,	„	=	2	45	per α Leonis and η, μ Gemin.
Feb. 16,	„	=	2	50	per stars in Gemini.
Mar. 4,	„	=	2	50	per γ Geminorum.
April 22,	„	=	2	50	per α Virginis and α Leonis.
May 4,	„	=	3	0	per α Virginis.
„ 16,	„	=	3	0	per α Virginis.
„ 19,	„	=	3	20	per δ and β Scorpii.
July, and Aug. 15,	„	=	3	30	per π Sagittarii. (Vide *Year-Book.*)
Sept. 14,	„	=	3	5	per β and γ Piscium } Arg. nutat, si conferetur cum obs. Decem.
„ 28,	„	=	3	15	per π, ρ, o, η, μ Gemin. }
Nov. 27,	„	=	3	20	per α Virginis.
Dec. 5,	„	=	3	20	per β, γ, ϵ Piscium } Arg. nutationis.
„ 11,	„	=	3	35	per h, η, μ Gemin. }

N.B. The errors come bigger by the stars of Scorpio and Sagittarius in June and July, than α Virginis before. September 28, rather bigger by the stars of Geminorum than of Pisces. December 5 and 11, certainly bigger. An argument of a very small nutation, for it was increasing Sept. 28th. The errors in March and September may be taken from the observations of the stars of Pisces, Gemini, and Virgo indifferently: but, in June and December, only from the stars of Virgo and Pisces, which are good all the year.

* This sentence is left incomplete in the MS. It appears, from the computation in the margin of the book, that, in December, the observations of the stars in Gemini gave the error equal to 2′ 40″; whilst those in Pisces gave errors varying from 2′ 20″ to 2′ 35″. F. B.

				′	″	
1693, March	4,	Error	=	3	40	per η and μ Geminorum.
„	18,	„	=	3	40	per stars in Virgo.
May	5,	„	=	3	55	per stars in Virgo.
July	21,	„	=	4	0	per stars in Sagittarius.
Sept.	4,	„	=	3	35	per stars in Pisces.
„	25,	„	=	4	0	per η and μ Geminorum.
„	26,	„	=	4	0	per π Sagitt.: and β, $\varkappa$, ϵ Piscium.
Nov.	20,	„	=	4	0	per γ and λ Piscium.
Dec.	19,	„	=	4	25	per stars in Gemini.

This year the errors are found less by the stars of Pisces on the 4th of September, than by the stars of Gemini and Sagittarius, by $\frac{1}{4}$ or $\frac{1}{5}$ of a minute: as they were last year.

				′	″	
1694, Jan.	5,	Error	=	4	20	per stars in Virgo.
„		„	=	4	25	per stars in Gemini.
Feb.	21,	„	=	4	25	per α Leonis and stars in Gemini.
March	14,	„	=	4	25	per α Leonis and γ Virginis.
„	29,	„	=	4	25	per α Leonis and η Virginis.
April	25,	„	=	4	30	per α Leonis and β, η, α Virginis.
June	15,	„	=	4	$\frac{1}{3}\frac{1}{3}$	per stars in Sagittarius.
Aug.	24,	„	=	4	20	per $\varkappa$ and λ Piscium.
Sept.	21,	„	=	4	15	per β and $\varkappa$ Piscium.
„	22,	„	=	4	20	per β, $\varkappa$, λ, ϵ Piscium.
„	23,	„	=	4	30	per h, η, μ Gemin.
„	30,	„	=	4	25	per μ, ν, γ, ξ Gemin.
Octob.	27,	„	=	4	$\frac{1}{3}\frac{0}{3}$	per α Leonis and β, γ, δ, ϵ Piscium.
Dec.	13,	„	=	4	$\frac{1}{3}\frac{0}{5}$	per η Piscium and γ, α, Virginis.

In this year the errors, found by the stars of Gemini and Virgo, agree very well together: those found by the stars of Sagittarius agree very well with them. But, when the stars of Pisces come observable in September, the errors are found less than by the stars in Gemini: whereas, admitting the nutation, they ought to be the same, and only less than those of December, found by the stars of Gemini. This year, I find no observations of the stars in Gemini, taken in December; whereby I might have resolved this doubt. The errors seem increased not above $\frac{1}{4}$ of a minute, betwixt the 5th of January and the end of the year. Last year I wanted observations of the stars of Virgo and Pisces in December. The reason of these defects is because at these times I was busy about getting a stock of observations for rectifying the stars of the northern and other constellations, and therefore thought not of taking any observations of these stars for determining the nutation. Those I use are such as I had taken with a different view: their agreement shows the excellency of observations made with telescopical sights, and what exactness may be expected from instruments of a bigger radius, firmly fixed.

Note to page 183.—As the supposed error in the division of Flamsteed's mural arc, mentioned in page 183, is a circumstance of much importance, and, if true, would affect all his zenith distance observations, I shall here state at length the evidence on which he considered that it was justly founded. The comparative observations, to which he alludes in the text, are collected together, and quoted in the fragment of a Latin letter addressed by him to Sir Isaac Newton, relative to the parallax of the earth's orbit, and dated January 16, 1698-9, inserted in MSS, vol. 39, page 113: the parts here extracted are to be found at page 116 and page 127.

Exeunte anno.	Latitudo.	⊙ à Polo.
	° ′ ″	° ′ ″
1689	51 28 42	2 21 27
1690	55	21 0
1691	50	20 35
1692	37	20 17
1693	42	19 57
1694	42	19 42
1695	47	19 17
1696	47	18 57
Intermedia =	51 28 45	

	⊙ Tropici à Vert. dist. minima.		⊙ Tropici à Vert. dist. maxima.	Obliquitas Ecliptica.	Latitudo Observatorii.
	° ′ ″		° ′ ″	° ′ ″	° ′ ″
1690, June 10	27 59 20	1689, Dec. 13	74 57 7	23 28 53	51 28 23
1690, June 12	30	. . . .	. . . .	28 48	18
— 10	20	1690, Dec. 9	57 18	28 59	19
— 12	30	1690, Dec. 12	57 13	28 51	21
1691, June 11	25	1691, Dec. 10	56 58	28 46	11
1691, June 12	22	1691, Dec. 14	57 22	29 4	23
1692, June 11	10	1691, Dec. 14	57 22	29 6	16
	10	1692, Dec. 10	57 0	28 55	5
	10	1692, Dec. 11	57 10	29 0	10
1693, June 12	43	1693, Dec. 11	56 43	28 30	13
1694, June 11	28	. . . .	. 56 43	28 37	5
1694, June 12	43	. . . .	. . .	28 30	13
	28	1694, Dec. 13	57 4	28 48	16
	48	1694, Dec. 14	57 2	28 47	15
1695, June 11	60	1695, Dec. 11	56 15	28 8	18
		1695, Dec. 14	56 32	28 16	26
1696, June 9	40	1696, Dec. 11	56 43	28 31	11
1697, June 10	50	1696, Dec. 11	56 50	28 30	20
			Intermedia . . . . =		51 28 15

The difference between these two results is 30″; the half of which Flamsteed considers to be the error of division. F. B.

[Here the substance of this letter, which is copied from MSS, vol. 39, page 135, is brought abruptly to a close, by the following note, viz.: " I was ill, all this year [1695?], till Michaelmas, with the " head-ache; which ended in a fit of the stone. Afterwards, well of the head-ache and stone both,

" except when I got great colds." In the subsequent pages of the book, the results of the computations for the error of the instrument are, in a similar manner, given for the years 1695, 1696, 1697, 1698, and 1699: but as there are no remarks annexed to them, and as enough has been already transcribed to show Flamsteed's method of proceeding in these cases, I have not thought it necessary to copy them here. The composition of this letter appears to have extended to July 20, 1700; up to which date the calculations are carried on, in the margin of the book. F. B.]

No. 53.) *The State of the Observatory: (written in October* 1700.)

Several persons, about the year 1674, pretending to the discovery of the longitude, and the most skilful of them proposing to find it by comparing the moon's apparent places (got by observing her distances from fixed stars) with her places given by astronomical tables (A*), it was represented to his then Majesty, King Charles II, (by the Lord Brouncker, at that time principal officer at the Navy Board, Sir Jonas Moor, Surveyor-General of the Ordnance, and several other able mathematicians about the court)—that this method was indeed the most likely to prove useful to our sailors, because most practicable; but that the catalogue of the fixed stars made by Tycho Brahé, a noble Dane, an age agone, and now used, was both erroneous and incomplete:—that the best tables of the moon's motions (which, with the places of the fixed stars, must necessarily be employed in the enquiry of the longitude by this method) erred sometimes above 20 minutes; which would sometimes cause a fault of 15 degrees, or 300 leagues in the determination of the longitude by it:—that the longitudes of the coasts in our sea charts having been laid down from coarse accounts of sea voyages of our first navigators, and not from celestial observations, as they ought, were very erroneous; so that our sailors could expect no help from this method, till both the places of the fixed stars were rectified, and new tables of the moon's motions made, that might represent her places in the heavens to some tolerable degree of exactness; for which, a large stock of very accurate observations, continued for some years, was altogether requisite, but wanting: and that therefore his Majesty would give a great and altogether necessary encouragement to our navigation and commerce (the strength and wealth of our nation) if he would cause an Observatory to be built, furnished with proper instruments, and persons skilful in mathematics, especially astronomy, to be employed in it, to take new observations in the heavens, both of the fixed stars and planets, in order to correct their places and motions, the moon's especially; that so no help might be wanting to our sailors for correcting their sea charts, or finding the places of their ships at sea.

Hereupon his Majesty was pleased to order an Observatory to be built in Greenwich Park (B): Mr. Flamsteed was appointed to the work, with the allowance of only £100 per annum, payable out of the office of the Ordnance; and a labourer in ordinary from the Tower, to move the instruments, count the clock, and call him at hours in the night proper for his business (C).

The foundations of the Observatory were laid in the summer of the year 1675; it was finished in 1676: Sir Jonas Moor, by his Majesty's order, taking care of the structure, and furnishing it with two movements, a large sextant of 7 feet radius, of Mr. Flamsteed's contrivance, for measuring arches in the heavens, and some telescope glasses, at his own charge.

* See these notes in page 189, &c. F. B.

A mural semicircle (of the same radius with the sextant) with a voluble quadrant, a skilful assistant, and calculators to help to compute the places of the stars and planets from the observations, as soon as he should have gained a competent stock of them, were *demanded* by Mr. Flamsteed, as absolutely necessary, and *promised* him; but delayed at that time because the charge of the building had something exceeded what was expected. The public distractions followed in 1678; and Sir Jonas Moor died in 1679 (D); whereby the Observatory was deprived of its best friend, and not only of these necessaries, but many others wherewith (out of a love to useful knowledge, and an earnest desire to promote the honor and good of his country) he had designed to adorn and support it.

As soon as the Observatory was habitable, Mr. Flamsteed began to observe the intermutual distances of the fixed stars, and the planets from them (E); in order to restore the places of the one, and motions of the other: having hired an ingenious servant to assist him, and maintained one in the same post ever since. He made a voluble quadrant and other necessary instruments also at his own charge (F), with which he continued to work from the year 1676 to 1689. The observations are fair described in four folio volumes, under proper heads, that they may be easily recurred to (G). But, now finding it high time to get another sort of observations, to connect them with, and for obtaining which he hitherto had wanted a convenient instrument, in the years 1688 and 1689, he contrived and built a large mural arc, of 7 foot radius, and 140 degrees in the limb, *at his own charges* (H): wherewith, finding it alone sufficient, he has continued his observations ever since. Which, of what value they are for exactness and certainty, skilful persons will easily judge, by a specimen of them he has printed in a late *Letter to Dr. Wallis;* wherein he has demonstrated, from eight years' continued observations of the pole star, that the parallax of the earth's orb is sensible at the fixed stars: which was not done before, nor could be proved by any observations taken with instruments formerly made.

The observations, taken with this instrument in 10 years past, since it was built, are fair described in two folio volumes; and are in number above five and twenty thousand. From the solar observations (a part of them) in the year 1694, he derived solar tables: and from them, has lately made tables of the sun's declinations, for the use of our sailors, that will serve them 100 years (I).

The number of the fixed stars, visible with the naked eye, observed by him with this instrument, he accounts about 3000: of which he has, since then, determined the right ascensions, distances from the pole, longitudes and latitudes of above fifteen hundred; adding the variations of both, answering to one degree change of longitude. Whereby the right ascensions and declinations of them may be got, to any time past or to come; which will be of great use to our sailors. And this has been done with the help of only one servant, and a calculator hired at a great distance in the country, at his own charge, in five years past, since he made his new solar tables.

In the mean time, his observations have been continued, as occasion required; and persons of known ability and skill (K) have been furnished with some hundreds of measures of refractions, and places of the superior planets and moon, derived both from them and the best tables extant; in order to the rectifying the tables of refractions, and determining the motion of the planets, the moon's especially; of which we hope, in a short time, to have, by this means, such tables as shall represent her places in the heavens, very nearly.

The eclipses and configurations of Jupiter's satellites have also been continually observed, as the weather permitted. And new tables of their motions will be made, which will be of great use to our sailors for correcting the longitudes of known coasts, or finding those of unknown (L).

There remain:

1. The right ascensions and distances of about 1500 fixed stars, to be determined from the observations already taken; with their longitudes and latitudes: and the above-mentioned variation, to be determined from them, in order to complete the catalogue (M).

2. The places of the moon to be calculated, from the observations taken with the sextant, betwixt the years 1676 and 1689; which require persons of more than ordinary skill and patience to compute them, being in number betwixt four and five hundred.

3. As also her places, to be derived from observations made betwixt 1689 and 1699, above 300.

4. Maps of the constellations to be made anew (or copied from those already made), to be printed with the catalogue of the fixed stars.

5. The volumes of observations to be copied for the press (that the originals may be preserved, to be recurred unto in case of any doubt, mis-print, or suspected error), together with tables already made, to be used with them.

Comparing what he has been able to do in five years last past, with the help of his servant and hired calculator in the country, with what remains to be done, it will be readily concluded that, without more help of calculators, he will not be able to finish the remaining part of his work in less than ten or a dozen years. That half a dozen expert persons are no less than will be necessary to finish, in two or three years: which, if they be allowed him, the observations may be immediately copied by one of the most skilful of them, and committed to the press. The requisite calculations may be made, the catalogues finished, the maps of the constellations engraved, and all be ready to be printed before the observations be wrought off.

In the mean time, the planets' places may be calculated from the observations, and with the necessary tables for the ease of calculation, prepared for the press, and printed next after. Whereby those persons, who are at work on the theories of the planets, will have all they can desire for the restitution of them, put into their hands. And our sailors will, in a short time, be furnished with all those helps, which astronomy can afford them, for correcting their sea charts, and finding the true places of their ships at sea. Natural philosophy will be improved with several useful discoveries; and his Majesty acknowledged the greatest and best patron of the useful arts of peace, as well as of war (N).

He hopes that, with the calculators, allowance will be made him for building larger and more firm instruments, than those he has made at his own charge, to remain to posterity: which will turn to the honor of his Majesty and the nation, by further discoveries, and confirming or correcting what he has hitherto done. And that, to reimburse him above £1000 he has expended in hiring assistants and making instruments, the charge of the impression of his works will be allowed him by his Majesty.

[Copied from the *original*, in MSS, vol. 35, page 27. There are two other drafts of it, slightly differing from the present, in the same volume, pages 15 and 19. F. B.]

[*Notes to the above, by Mr. Flamsteed.*]

(A). This gentleman was a Frenchman, called himself Le Sieur de St. Pierre, and by an interest in the Duchess of Portsmouth *, got himself recommended to the King Charles II, who gave a

* The initial only of this name is given in the MS: but in another draft of these notes, alluded to in page 193, the name is stated at full length. F. B.

commission to the Lord Viscount Brouncker, the Bishop of Salisbury (Dr. Ward), Sir Robert Morray, Sir Charles Scarborough, Sir Jonas Moor, [Col. Titus], Dr. Pell, and other eminent mathematicians about the Court, to hear his proposals. They met at Col. Titus's house; where, by a power given in their commission, I was chosen into their number, furnished him with such *data* as he required, and showed the insufficiency of them for the end he proposed; and suggested to them what they represented to the King, which they apprehended very easily.

(B). Chelsea College [then ruinous] was proposed by some persons; and I went to see it. But, Greenwich Hill being mentioned by Sir Christopher Wren, the King approved of it, as the most proper; the same having been proposed to King Charles I, as I was told by Mr. Moore, an ingenious old mathematician then living: and that the Observatory was to have been built on the other hill in the park; and an instrument, as large as any the Arabs boast of, fixed in it for determining the meridional heights and declinations of the fixed stars, and other astronomical observations.

(C). The laborer being paid by the Office of the Ordnance as well as myself, looked upon himself as the King's servant: and being a person only fit for hard labor, was rather a hinderance than help to me; but always a certain charge. For, I was forced to allow him diet; or want his attendance when I had occasion for him, on his pretence of providing it. Till, in the year 1694, the officers allowed me to name my own laborer: since which time I have named one of my own servants, and received his pay for his maintenance: which is a favor I must ever acknowledge: [and I valued it the more, because it made me easy, and furnished me with an ingenious and tractable youth, instead of a surly laborer.]

(D). August 17, 1679. When I came to serve the King, Sept. 1674, the principal board officers, Sir Jonas Moor, Sir George Wharton, Sir Edward Sherbourne, were skilful persons in several parts of mathematics; and the others, lovers of them. But, upon the decease of the two first, and removal of the last, gentlemen succeeded that were not accomplished, as their predecessors had been: from whom, therefore, I could not expect the like support. 'Tis usual with people to despise those arts of which they are not masters, or have but little relish: yet I must confess that, from some of them, I have received favors I expected not.

(E). September, 1676, with the sextant: [from April, 1676. to this time].

(F). A mural arc, of 7 foot radius, in 1683; which proved too slight: though he finds the observations taken with it much better than he esteemed them. [The telescope tubes and glasses I provided at my own charge.]

(G). About 20000, in number, of the planets' distances from the fixed stars, and of the stars themselves from each other, measured with the sextant.

(H). Above £120. [Lord Dartmouth was Master of the Ordnance; who promised to reimburse what it cost me: but he being removed from that post before it was finished, and dying soon after, and a chargeable war following, I lost all my hopes of being repaid; and resolved to lose no time now I had got an instrument that answered all the ends for which I designed it.]

(I). In the year 1688, from such observations as I had taken with the sextant, and the slight mural arç made in 1683, I had rectified the places of about 120 principal fixed stars: from which, whilst I was laying in a new stock with the mural arc, I derived the moon's places as oft as I observed her in the years 1689—1692, with the new strong mural arc. [I calculated also her place from my lunar tables, printed in my *Doctrine of the Sphere*, to the time of every observation; and drew the work into 3 large sheet synopses: which, with the elements of the calculation, showed the difference

between the moon's observed and computed places.] Which were imparted, in September 1694, to ——.

(K). Mr. Newton; with the moon's places derived from my old lunar tables, and elements of calculation: by which he hoped to rectify her theory, and save me that labor. I name him not, because he is unwilling to be mentioned; because further observations (perhaps continued 19 years) will be requisite to perfect the theories of the moon, and he would not be dunned for his work before he can accomplish it. Others have employed their pains on the superior planets, whom I have not mentioned, for the same reason that made me spare the name of Mr. Newton. [Mr. Newton coming to see me Sept. 4, 1694, and discoursing of the theory of the moon, to let him see what I had done in order to restore her motion, I produced and showed him these 3 sheets of her observed and calculated places compared. He told me immediately that, if I would impart them to him, he doubted not but by them, with the help of his Doctrine of Gravitation, the true theory might be discovered, and her motion restored; and he would undertake that task. I was glad of the proposal, and put them freely into his hand; because that thereby I should save myself a great deal of pains and time, which might be more usefully employed on the work of the fixed stars, on which I was then entered. At the same time I also showed him a stock of observations of refraction; and imparted them to him soon after: from all of which he derived a theory of them, and made new tables that answered them, as near as could be expected or desired: which none, hitherto published, would. Afterwards I continued since furnishing him with lunar observations, as I gained them, till Midsummer 1695: when being troubled with a distemper which suffered me not to prosecute my studies or labors (and after six months' continuance ended in a fit of the stone) I was forced to intermit my correspondence with him. I have not named him in this paper (which has been seen by some of my friends) because some people, to ingratiate with him, have been very loud about what he has done in the theory of the moon; and thereby caused others to dun him about it: whereas it is not yet complete, and will perhaps require observations continued for 20 years, to be accounted from the year 1698, before it can be. I have imparted also observations of some of the other planets to other persons whose names I mention not, because longer experience is required to complete their work, and more materials with which his Majesty's Observatory can only furnish them, as well as time and opportunity afford them.]

(L). But more years' experience, and further observations, are also required for them.

(M). This paper was wrote about twelve months agone: at which time the ecliptical constellations, with those that lie south of the equator, were only finished. Wherein the stars, whose places are rectified, are as hereunder. [This table shows the number of stars whose places are given in Ptolemy's, Tycho's, and Hevelius's catalogues; to which I have added the number observed and rectified by me in each: whereby you will see how much more ample, as well as useful and complete, the catalogue of the English Observatory will be than those of our predecessors. I must acquaint you further that I have given the places only of such stars as are visible to the naked eyes; except now and then a bright telescopical star was adjacent to some such: in which case, I have inserted it, and noted its light. But I believe there are not 20 of those in the whole catalogue: which shows the deficiency of former catalogues, and that the instruments of our predecessors were not proper for observing them. I do not account my constellations so complete, but that some few stars visible with the naked eye may be still found and added; especially in Hydra and Serpentarius.]

		Ptol.	Tycho	Hev.	J. F.
In 1696	Aries	18	21	24	66
	Taurus	44	43	51	135
Dec. 1696	Gemini	25	25	38	94
	Cancer	13	15	27	66
	Leo	35	40	50	96
Feb. 1697	Virgo	22	39	50	76
	Libra	17	18	21	33
	Scorpio	24	16	20	58
	Sagittarius	31	16	26	49
April 1697	Capricor.	28	28	30	53
	Aquarius	45	41	48	112
	Pisces	38	36	39	105
	Total	340	338	424	943

		Ptol.	Tycho	Hev.	J. F.
Aug. 1699	Cetus	22	21	39	98
	Eridanus	34	19	34	71
	Orion	38	62	58	71
Jan. 169⅞	Lepus	12	13	16	19
	Canis Maj.	18	13	20	25
	Canis Min.	2	5	8	17
	Navis	40	11	5	21
Aug. 1699	Crater	7	8	10	96
	Corvus	7	7	8	
	Hydra	27	24	29	
	Total	214	183	227	418

In the mean time, betwixt May 1699, and January following, 1699-1700, I calculated all the places of the full moons and quartiles, with her places on her limit observed from 1689 to 1700, (about 300 in number,) both from the observations and tables. And betwixt that and February 14th, 1700, all the places of Jupiter, Saturn, and Mars, observed at the oppositions and quadratures of the sun betwixt 1689 and 1700. And betwixt February 25, and April 16, 1700, new tables of equations for the systems of Saturn, Jupiter, and the Moon.

This summer, 1700, rectified in

Serpens	61	Equuleus	10
Serpentarius	72	Pegasus	97
Aquila et Ant.	76	Triangulum	24
Sagitta	23		
Delphinus	18	Total	381

These are not fully finished; but will be, as soon as I can gain time to compare my country calculator's work with my domestic's.

I have under my hands at this time the constellations of Andromeda, Perseus, Auriga, Cassiopea, Bootes, the Crown, Hercules, Lyra, Cygnus, and Cepheus: but my want of health and assistance makes the work go on heavily. The two Bears and Draco will complete the catalogue; which my servant has entered upon: but further observations are wanting, which will be made as we have occasion, and opportunities of fair weather.

New view, June 21, 1703.

Aries	66	Sagittarius	49	Canis Maj.	25
Taurus	135	Capricornus	53	Canis Min.	17
Gemini	94	Aquarius	113	Navis	25
Cancer	67	Pisces	115	Monoceros	33
Leo	96	Cetus	78?	Hydra	94
Virgo	86	Eridanus	73	Corvus	9
Libra	33	Orion	70	Crater	12
Scorpio	60	Lepus	19	Sextans	35

Serpens . .	61	Auriga .	71	Vulpecula .	10
Serpentarius	76	Coma . .	44	Lupus . .	3
Aquila et Ant.	72	Bootes .	57	Centaurus .	8
Sagitta .	23	Corona . .	22	Cassiopea .	55
Delphinus .	18	Hercules .	120	Cepheus .	36
Equuleus .	10	Cygnus . .	108		
Pegasus . .	96	Lyra . .	22		2774
Trianguli .	24	Ursa Maj. .	247		59*
Andromeda 21 esset	80*	Ursa Min.			
Perseus . .	57	Draco . .	56	Total	2833

Sit summo Deo, sola et summa laus.

(N). [Since this was wrote I have derived, from the observations of the moon, 200 places; Saturn, 30; Jupiter, 40; Mars, 50. The observations of the planets, taken with the sextant, cannot be employed till the catalogue be nearer finished. But, since God has blessed my labors with success beyond my expectation, I hope I may complete it in less time than I proposed when I wrote this paper, and with some less help. But, unless some skilful persons are allowed me to go on with the calculations, it will be impossible to get the tables, that are to be derived from them, wrought off during my life. I have lost my health by my night-watches; the labors that have succeeded them my studies suffer me to enjoy but little. And if they be not perfected during my life, many will pretend (but few will be found able) to finish what, through God's blessing, I have thus far carried on. October 10, 1700. J. Flamsteed, M.R.]

[Copied from the *original* in MSS, vol. 35, page 23. There is also another draught of these Notes, in MSS, vol. 33, page 51 from the *end*: wherein there is some additional matter, which I have inserted above within the brackets. F. B.]

No. 54.) *Letter from Mr. Flamsteed to Mr. Thomas Perkins.*

December 11, 1700.

Sir,

'Tis not long since that you told me, in some discourse I had with you, that Mr. Halley purchased some books and papers of your sister-in-law after your brother's decease. I desire you to recollect yourself, and let me know what you remember of this purchase by a note under your hand, or cause my servant to write down what you remember, and put your hand to it. As also for further confirmation of it, that you would write to your sister's husband this night to inquire of her, and let you know what she remembers of it.

My asserting this 17 years agone, that I might honestly and fairly give your brother the honour of his own labors, has been the cause of all those villainous and silly calumnies Mr. Halley has spread of me ever since; I pray God forgive him for them,—I do heartily; but still hold myself obliged to defend the truth, which I think every honest man is tender of; nor will a just man wink at lies. I believe you are unwilling your brother should be deprived of his just esteem, and therefore doubt not you will comply with both my requests.

I would not have you think I am going about to derogate any thing from Mr. Halley's merits, or to undervalue what he has done. No, I assure you on the contrary; I always said (and sometimes in your hearing) his voyages would be of good use; and I further affirm, that he who owns he has only prosecuted the invention of another man, and improves or perfects them, deserves more honour commonly than the first inventor. For in first discoveries or inventions there is something owing to chance or accidents; but there is study, industry, and labor required to improve and perfect an invention: and he that owns what he received from another, tells the world, and will have it acknowledged, that he is just and acts favorably. The Captain's pains, study, and labor, must, and shall be, ever acknowledged by me; but I must be just to my deceased friend, your brother, whose inventions could not be perfected in his life; and I must assert of his discoveries that they were the result of much and long labor, study, and thought, and not of chance or accidents, as others usually are. Mr. Halley, or any other, is much mistaken if he thinks I intend to derogate from him, whilst I only desire to ascribe what is due to Mr. Perkins, which is all the design of, Sir,

JOHN FLAMSTEED.

[Extracted from MSS, vol. 33, page 25.]

Mr. Perkins's reply thereto.

J. Hodgson having delivered the letter, copied as above, to Mr. Perkins, he told him as follows, December 12, 1700.

"About a fortnight after the burial of my brother Peter (the Mathematical Master "at Christ's Hospital), I went with my brother Eysum to visit my sister at Christ's Hospital; where, "amongst other discourse, she told me that Mr. Halley had been with her some time before, and "had looked over her husband's papers, and took some away with him, for which he gave her 7 or "8 shillings; and would have given her more, but she refused, saying she was very well satisfied. "She proffered me the rest, but I refused them, telling her they would be of no advantage to me; "but my brother Eysum took some of them with him, parts of which he gave to Mr. Flamsteed."

Mr. Peter Perkins died December 12, 1680, was buried December 16. I saw in his hands a great many collections of the variations of the compass, and know that he was seeking a theory to solve them. He had made a good progress in it, and had many documents upon the compass and dipping-needle. Some time after his death I waited upon his wife and desired to see his papers: she told me that Mr. Halley had got them, but I might take what was left. I gathered what I could find relating to the variation of the compass or magnetical documents, which I have by me, with some few others that I believe were given me by his brother Eysum.

About a month or two before his death I persuaded him to give the Royal Society some intimation of what he had discovered of the theory of the variation, which he did in a short paper that was then entered on their books.

Three years after, Mr. Halley published a Theory of the Variation in the *Philosophical Transactions* of , 1683, No. . It will be easily seen by comparing it with the paper recorded in the books of the Society, with those I have in my hands, and those he had of Mrs. Perkins (if he please to produce them), whether this theory be the same with that of Mr. Perkins or not. J. FLAMSTEED.

On the receipt of my letter, Mr. Thomas Perkins wrote to his sister's husband (she is married to

one Mr. Baker, at Guildford) to know what she remembered about her parting with the said papers, and received the following answer:

Brother Perkins,—Yours I received; and according to your request I examined my wife, who informs me Mr. Halley had from the hands of Eysum, your brother, the choicest books and manuscripts her husband had: she remembers not what she received for the same, but is confident not half a quarter of the value of them; therefore I request you (if it is in your power) to prevent the publication of any such thing in any man's name, except from the defunct author thereof. We both greet you, and wish you a merry Christmas and a happy New Year, and 'twill not only be acceptable to you, but likewise to your friendly brother, William Baker.

Guildford, December the 15th, 1700.

[Extracted from MSS, vol. 33, page 31.]

No. 55.) *Letter from Mr. Flamsteed to Sir John Worden.*

The Observatory, May 22, 1701.

Honored Sir,

Though I did not know of any printed tables I had left when I last waited on you, I have since found a couple in the hands of my servant, which I send you included, and desire you to accept.

Mr. Shardeloe, the bearer, has seen these charts of the constellations perfect, of which I only showed you the first imperfect draughts, with what I have besides under my hands, and can give you some better account of them than I durst presume to do: he has more skill than most other sea captains, and apprehends well some of the uses they will be of in navigation, but the principle will not be understood till they are public.

If God spare me life and health, I hope I may get all the rest of the constellations designed by Michaelmas next; but I cannot hope to get them completely perfected in less than 12 months time after, for want of observations that cannot all be got this year.

I have been at more than £1000 charge in building necessary instruments and maintaining assistants to carry on this work; it will cost above as much more to publish it, which I think it cannot be expected I should disburse till I am someways recompensed for what I have expended already, the loss of my health by my night-watches for these many years, and my day labors in an employment for which there are few fit in this nation: and none but myself, I am apt to think, would have carried it on through so many discouragements as I have done. King Charles built the Observatory for the improvement of navigation, and placed me in it to provide those helps which were wanting and absolutely necessary to that end. God has blest my endeavours; with little help, at a great expense, I have obtained them. The real grounds of true philosophy have been fetched from her Majesty's Observatory. The necessary helps for navigation will be furnished from the same place, if her Majesty will but be pleased to look favourably upon us, and some way afford a support for the growing charge, which will not be much more than what I have already disbursed out of the small estate God has blessed me with.

The French have made mighty boasts of their Observatory, but done nothing considerable. Foreign nations, as well as our own, will derive the helps to their ingenious studies from her English Majesty's Observatory, and acknowledge her Majesty the best and greatest patron of

learning and the sciences that are most useful to her subjects. I had not the confidence to speak this in discourse when I waited on you; I fear I have had too much in committing it to paper, except your goodness pardon, Sir,

Your most humble servant,

John Flamsteed, M.R.

[Extracted from MSS, vol. 33, page 36.]

No. 56.) *Letter from Dr. Wallis to Mr. Flamsteed.*

Oxford, June 3, 1701.

Sir,

I have not heard of you a great while, but do believe that you be well employed. I would be glad to hear that your observations are in the press, that so great a treasure be not lost, of which we are in great danger in case you should die before they be printed. Those of Hevelius were in a good forwardness before he died; but I presume it would have been better that he had lived to see them printed: and I do not know that you have any amanuensis who doth so thoroughly understand yours as to publish them if you were gone. I understand, that in Germany (as M. Liebnitius signifies in a letter to me), they are going to erect large instruments for observing the earth's annual parallax, which is, I suppose, in pursuance of what you have done. I wish you would pursue that business yet further; and, particularly, that you would examine your observations of *lucida Lyræ*, which is a great star (and may be presumed nearer us than those that seem lesser), and is as near to the pole of the zodiac as is the pole star, and (being bigger than it, and brighter) may be fitter for that purpose. I am not likely to live so long as to see your observations published; but, however, I would not have the public lose them. I am, Sir,

Yours to serve you,

John Wallis.

[Copied from the *original* letter in MSS, vol. 33, page 54, from the *end.*]

No. 57.) *Letter from Mr. Flamsteed to Dr. Wallis.*

The Observatory, June 24, 1701.

Rev. Sir,

'Tis not for want of respect, but only to gain time to perfect my catalogue of the fixed stars that I have forborne to write to you this two years. You very well apprehend that my observations would be of little or no use if printed without it. Last week I received the places of the fixed stars of 9 constellations, computed by a calculator I have hired in Derbyshire: my domestics have compared them with the same done here; and I hope they will be inserted into the catalogue before this week be ended. After which I shall have the places of 2200 fixed stars determined in it, and only 5 constellations remaining, viz. Cepheus, Cassiopea, Draco, the Greater and the Lesser Bear: of which, in Draco there remains a good part, in the Lesser Bear only some few stars to be observed.

Whilst these have been carrying on, I have determined above 100 places of the planet Mars from my own observations, taken betwixt the years 1671 and 1701, with the new places of the fixed stars;

and compared them very lately with the Rudolphin numbers, whereby something is discovered in his motions that the theorists think nothing of.

I think I told you formerly that I had by me 30 places of the moon near her northern limit, and as many about her southern, and above 60 on her quadratures, and some more on her oppositions to the sun, derived from observations taken here, betwixt the years 1689 and 1701, with the new places of the fixed stars. Those 200 places of the moon I imparted to Mr. Newton, taken in all places of her orbit, extend no farther than from the year 1689 to 1695, and were determined by the help of a small catalogue of the fixed stars, on which (though they were much more accurate than Tycho's) I durst not rely; many of them are repeated in those above specified, and the rest shall be re-calculated when I have leisure.

Of Saturn and Jupiter I have by me above 100 places, computed (with the new places of the fixed stars) from observations taken with the mural arc between the years 1689 and 1701. The observations themselves lie all fair transcribed in the same order wherein they are to be printed; and my amanuensis, James Hodgson, knows very well what corrections are to be made in any of them, and how to find them out when required. He is a sober young man, about 22 years of age; a very good geometrician and algebraist; understands the series and fluxions, though I have not suffered him to spend much time in them, because I could not spare him from the calculation work; he understands the Latin tongue indifferently, having got [it] since he became my servant; he knows my method, and is acquainted with all my labors, and will easily finish and print them, if God should call me hence before I shall have perfected them myself. But since the all wise Creator of the heavens has thus far prospered my endeavours beyond my hopes or expectations, I cannot doubt but he will afford me both life, health, and means to finish and publish them myself. My youngest servant, Thomas Weston, has been educated with learning, has a good talent at drawing, and I design to set him to draw the maps of the constellations this summer, and, perhaps, to engraving plates for them; for those that draw well seldom fail of engraving as well.

I give you this account of the present posture of my affairs, that you may see that if I should die before the catalogue be finished, there is not the least danger of losing either it or any part of my long and painful labors. To perfect which after the catalogue shall be finished, the places of all the planets are to be calculated from the measure taken with the sextant, betwixt the year 1676 (when I first sat down) and the year 1689 (when I built the large mural arc), which being numerous and much more difficult to manage than those taken with it, will require good and skilful assistance to calculate them; as also the places of the planets since observed with the meridional arc, for I have only calculated the principal at the □ and ☌ of the sun, or at remarkable times, as I have given you a hint before. These cannot be set upon till the catalogue is finished, but ought then to be done to render my work complete; for though we have all Tycho Brahe's observations by us, yet I find not that Kepler, Bulliaud, Wing, Street, or any of our theorists, have been at the pains to compute the places of the planets from any of them, but take such of them as they found ready calculated to their hands.

Mr. Newton had done nothing in the theory of the moon if I had only given him the observations here made; I was forced to give him the places computed by myself and servants from them, and repeated carefully*; as also her places, computed in like manner, and repeated from my own tables (grounded on the Horroccian theory), with all the elements of the calculation: whereby he was

* Three years pains at spare times whilst I was laying in a stock of observations with the mural arc.

showed at once in what parts or positions of her orbit in respect of the sun the notable errors happened, and comparing them with such as ought to be, according to his theory of gravitation, how they might be taken away.

Since the world will have the use, and the king and nation the honor of the work under my hands, it ought to be rendered as complete and perfect as it can. I shall publish tables with it, that will render the calculation of the planets' places from them easy and expeditious; but our theorists are clamorous, and will complain if the whole pains be not spared them. I will do what lies in my power that it may; and if I cannot procure the help requisite for this purpose, I shall let the world know that it is not my fault, but theirs who have (for reasons I will not mention) misrepresented my labors: of which, that you may have a truer and more perfect apprehension, you will permit me to tell you, that the calculations I have been obliged to make, or made by my servants, fill above a dozen handsome quarto volumes, besides what has been done by my country calculator, and a couple of folios of collections and synopses of the constellations employed in the work. Our theorists know little or nothing of this; yet by clamoring and calling for my observations, as if they were as easily wrought up as to set off theorems and corollaries, have given to the world a false idea of my labors, and prejudiced and hindered me from obtaining the help and assistance I have need of to render my work as complete as it ought to be. If you would particularly advise your colleague, Dr. Gregory, to have a care of discoursing of things he is not acquainted with, and has only false, imperfect, or prejudiced information of, you would oblige me much. Had he pleased to visit the Observatory when he was lately in town, as Mr. Keile did, I should have taken it kindly; he should have met with a civil reception, and found (as Mr. Keile did) that I would not have remembered what passed 30 months agone. I wish him health and success in his labors.

I am glad to find that though our nation takes little notice of my letter concerning the parallax of the pole star, yet foreigners are excited by it to build large instruments on purpose to examine it. My state of health permits me not any longer to sit up for hours together in the night for observations as formerly; nor, I bless God for it, have I now the reason I had. I have a large stock by me, and it fully employs mine and my servants' time to work them up; nevertheless I take care to observe myself the planets and the eclipses of the satellites as formerly, in order to correct their motions as soon as my servant shall have gotten skill enough in the doctrine of gravitation to settle the motion of Jupiter, their primary planet, which I find will create more trouble than at first I expected.

I must, therefore, leave the further inquiry into the parallax of the orb to my young men or foreigners, till I can build larger and better instruments: which, if I may not hope for at the public charge, I may, perhaps, in good time make at my own. Your life and health is ever heartily prayed for by, Sir,

Your respectful and humble servant,

J. Flamsteed.

Though your letter was dated June the 3rd, it came not to my hands till the 19th, which is the reason you have not this answer sooner.

[Extracted from MSS, vol. 33, page 55, from the *end.*]

No. 58.) *Letter from Mr. Flamsteed to Mr. A. Sharp*.*

The Observatory, February 6, 1701-2.

SIR,

I have long desired to hear from you, and intended to have wrote to you; but, having no particular occasion, and more work on my hands than I can well perform with the help of a couple of servants, forbore till I should have better leisure, or opportunity offered itself: you prevent me; I am glad [of] the occasion, and take no longer time than the next post to answer yours of the 2nd instant.

I heard the bodies were before the Society, but saw them not; particular care being taken (that I have good reason to think) that I should not, by the Secretary, who with the help of another friend, that you know very well, has filled it with a company of ingenious young gentlemen, that are not all of them proper for carrying on the design; and by whom others are carried on that are very foreign to it, which makes me abstain [from] their meetings: but I have a greater, the saving or gaining of time for my own laborious employment. My business succeeds, I bless God for it, very well under my hands; but not at that rate that some people, out of a malicious design, represent it. They have emissaries, that understand nothing of the business, that sometimes visit me that they may give them an account of my pains, which they turn as they please. I receive them and use them as Scipio did Hannibal's spies, show them what they desire, dismiss them smiling; for I wish they understood all as well as they would be thought to do. I shall give you a just account of the state of my studies as soon as I have done with your letter.

Pray let me know the radius of the new instrument for taking heights and azimuths; and, if it be not a secret, a sketch of the contrivance: you say it shows both to near a minute; hence I guess 'tis about 2 foot radius, and then a six foot glass applied to it will make it warp with its weight: in such like instruments the telescope ought not to be much longer than the radius of the instrument.

At the eclipse of the ☉ Sept. 13th, 1700, we had here a thick fog, which broke up but some very few minutes before the end, at $10^{h.}\ 32'\ 27''$: the limb was not freed at $10^{h.}\ 32'\ 27''$; it was perfectly round, so I conclude the end at $10^{h.}\ 32\frac{1}{2}'$. A construction, by my old lunar numbers, on a large pasteboard, gave it at $10^{h.}\ 25\frac{1}{2}'$. I account that the duration was longer and the eclipse greater with you than here; and therefore you have erred considerably in the calculation of the moon's place, if you find your observation differ an hour from the tables. Pray what is the latitude of Horton, and the difference of meridians from London in your opinion? I will take a little pains to satisfy you when you acquaint me.

I will take care to furnish you with a map of the moon, ere long, of Ricciolus's names; which will serve you as well as Hevelius.

My man has calculated the eclipses of ♃'s satellites for 4 years to come. I shall send you copies of them, or the numbers I use at present, that you may compute them yourself.

But I covenant with you that you shall not impart any observations I send you without my consent to any persons whatsoever: nor any tables. I had an emissary here, at the solar eclipse, who, as a friend, would have sponged the true time out of me. I knew it, and civilly dismissed him without it. I have great reason to be thus cautious, which I am sure you will approve, when you shall know it: and that you may, in a little time.

* This is the first of that series of letters between Mr. Flamsteed and Mr. Sharp, which has recently been brought to light, as already mentioned in the Preface; and which continued for nearly 18 years. F. B.

Such tables of right ascension and declination as you mention, to every 10th degree of latitude as far as 60, I have had by me ever since the year 1695; and perhaps may print with my large catalogue of the fixed stars: part of them were calculated by James Hudson*, your present successor, the rest by myself. But I made use of a new method and expedients which cut off much of the labor; and if you made use of the obvious and ordinary way, you have been at a great deal of pains that you might have spared, if you had wrote to me. The work, however, is laborious and long; which makes me rather think of calculating what I shall still want, after the ordinary way, than to continue the general tables up to the pole.

As for your odometer, or way-wiser, I doubt not but 'tis very ingeniously contrived and well wrought; as everything is that comes from your hands. Mr. Jonas Moore gave me one, which you saw when here. I am not yet either wealthy enough to keep a coach, nor so infirm as to need one: but, to encourage you whenever I do it, I desigu to have no way-wiser in it but of your work and contrivance.

I shall acquaint Mr. White, when I next see him, that you have perfected both your engines for rose and oval work. And now I think I have said as much to your letter in all other particulars as it requires, I shall acquaint you with the state of my new catalogue of the fixed stars.

I have caused my younger servant, T. Weston, to copy you the shortest constellation I could find, in the same method and order in which I design to print it. The 1st column on the left hand gives you the number or order of the stars. The 2nd the name annexed to it in the old Ptolemaic catalogue; which I have translated anew from the original Greek, finding that both the old translation printed by Gauricus, a much better of Copernicus, and the Arab translation of Dr. Hide, were all exceeding faulty, by reason that neither the Arabs nor Copernicus were critics in the Greek, nor yet Trapezantius, though a Greek born. The 3rd column gives you Bayer's Greek letters put to every star. The 4th their right ascension in time: and here I break all the old order, and place them in a more natural one according as they succeed to the meridian. And the next column gives you their distances from the vertex observed correct by refraction, on the great arc of your making. These two columns, you will say, may be omitted by reason the next give the right ascensions and distances from the pole; but seeing they are derived from the other, and may be corrected from them, it was convenient to add them, that if any error be committed in copying or printing it may be corrected by them.

But the last two columns, of whose want you complained in Hevelius, are the most material: they show you how much the right ascensions and declinations, or distances from the pole, vary whilst the longitude increases one degree. These are added along the catalogue everywhere to make it complete; and without them it would have been imperfect: and the mark — at the head of the var. D *a* P tells you that the distances from the pole are continually decreasing.

I have looked for a paper, to copy for you the number of the stars in every constellation; but cannot find a perfect one at present: and can only tell you that my zodiac alone has the places of about 950 stars thus ordered. That all the constellations are finished excepting Cassiopea, Cepheus, Ursa Major and Minor; and that the two first have been nearly perfected these 3 months, and the two last lie by me in good forwardness, but delayed for want of help, and by reason of other work: for when I came thus near an end I thought it time to think of new maps: examining Hevelius and

* I apprehend that this is James Hodgson, who married Flamsteed's niece. The name appears to have been spelt indifferently Hudson and Hodgson by Flamsteed. F. B.

Bayer, I found theirs altogether ungeometrical; and, moreover, they described the human shapes always with their back-sides towards us, whereas Ptolemy's descriptions seemed to represent them with their fore-sides upon us: having recourse to the Greek Ptolemy, I soon found the reason. The word εν νοτω they always took to signify in dorso: whereas νοτως signifies that part of the breast that reaches from the collar-bone to under the armpits, and νοτον the same space on the back and shoulders; and this with some other like, but not light, faults being corrected, I made the translation sense, and each part of the description agreed with another.

But now I found a many errors committed in the different copies of Ptolemy's numbers: by comparing the old copy of Gauricus with two of Copernicus, 2 of Trapezantius, the Arab of Dr. Hide, and the Basil edition of the Greek, I corrected a many; but was forced at last to send to Oxford to consult a manuscript by the help of my old friend Mr. Caswell: and by this means I hope I have restored and may reprint his with my own. I am fully tired with the work of a critic; 'tis the worst I was ever upon, and I am glad I am got through it, for it is enough for me to correct or prevent my own faults: 'tis pride to do nothing but talk of other men's.

I have copied Tycho's and Hevelius's catalogues into my own method, to be annexed to Ptolemy's. And I intend to annex the Arab of Ulug Beig to the last; that so none of the old catalogues may be wanting to compare with the new.

T. Weston has a gift of drawing, and with much pains I have caused him to form all the figures of the zodiacal constellations agreeably to Ptolemy's old descriptions, and my new translation of him. The southern constellations observed here are in a manner [done]: but every day we find corrections to be made; and some maps will be new described. 'Tis as much as Hudson can do in one day to prepare a chart; and it will be a week's work after for Weston and him together to fit in the oblique circles and parallels, and finish it.

All the maps are done to the same scale, and the great circles are of the synoidal form: we have patterns made for some of them; and, when they come to be described on the brass plates, must make a many more: at present I am only solicitous to represent the ancient figures, the rest will be added at leisure.

You will hereby collect what I have done; but you will not so easily guess what remains: it will require a great deal of time to finish these figures; more, to complete the constellations wanting, to copy the observations for the press, to calculate the moon's and planets' places from those made with the sextant. Hudson is nearly out of his time, Weston must not spend his time in copying papers; his talents may be better employed otherwise: a new servant will not have skill to do business in 2 or 3 years: my distempers begin to return upon me: I shall neither spare cost nor pains to carry on my business, on which I am above £1000 out of pocket already: but I will not prostrate my wealth and pains to the pleasure of those, who only talk and judge impudently of what they understand neither the nature, pains, nor cost. But of this more hereafter. At present shall only tell you that J. Hudson (who is a very good algebraist and understands both the series and fluxions well) tells me he has seen in London at the press a treatise of these wrote by one Mr. Cheyne, a Scotchman; of which his countrymen make great boast. 'Tis about 8 sheets, and will be printed with the same letter that Mr. Newton's book is. Dr. Gregory's Astronomy will be finished at Oxford, according to his proposals, by Midsummer next. A treatise of conic sections is there in the press by Mr. Mills, whom I think you once saw here. Mr. Halley is trying to get the command of a ship this summer: he loses his credit daily; and his friend does not increase in his.

I have wrote you a long letter because I had a great deal to tell you: pardon this fault, and I shall not willingly give you cause to complain of his committing the like again, who is ever your friend to serve you,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 59.) *Extracts of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Feb. 28, 1701-2.

Having been much troubled with the headach these three months past, and finding nothing in yours that required an immediate answer, I delayed returning one till now that I find myself something better, and must entreat you accept of a short one, for I fear that much study makes me recover slower than I should, had I less thought and more diversion.

* * * * * * * * * *

The places of the fixed stars of my catalogue are rectified to the beginning of the year 1690, when you lived with me, when the ground work observations were got, you assisting. I forgot to intimate the year to you in my last, till it was gone from here; then I remembered my fault, for I would not conceal anything of this work from you: though in a like specimen I sent to some other ingenious men I purposely omitted it, because I would not have anything of it printed but by myself, and when I have reviewed, perfected, and completed the whole.

I have not yet determined in what years the equinoctial points receded a precise degree, nor shall till I have finished the catalogue: but when I have done it, I shall not alter the numbers of the two last columns; for if any skilful person shall think the time is determined too long or short, he may apply what time he please for that *recess;* I have not tied him to any; I have a table ready by me for reductions, supposing it a degree in 72 years; but 'tis so easily made, that it is not worth while to save you the labor of copying it, else I would have had it transcribed for you and sent it.

* * * * * * * * * *

I never receded from the sun's parallax of 10″, nor know I any reason why I should. Mr. Newton would make it 40″, or 24″ at least; either you mistake Mr. Whiston, or he has misapprehended me. It may be less than 10″, but I know no observations that will make it bigger: and Mr. Newton remits a many of his former notions, and will take it ill to be held to his determinations in his *Principia*, now he knows that the force of gravitation in the moon raises our tides but so many inches, as he supposed it did feet, in the American Pacific Ocean.

I have one loose copy of my *Letter to Dr. Wallis* by me, concerning the parallax of the pole-star, which I would present you with if I had another; 'tis two sheets of paper, and if you will direct me how to send it when you want any books from hence, I will send it you, and you may return it at your leisure.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 60.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 14, 1702.

You seem to take some expressions in my last letter in another sense than I designed them; have a care of this, 'tis the frequent ground of quarrels betwixt friends; if you would retain yours you must give the most favorable construction you can to all their expressions, and understand them no otherwise than you would if they had been spoke in common discourse. I hate ingratitude; and to trouble my friends without making them a recompense: and intended no more than to tell you friendly I would pay you in your own valuable coin, with some advantage to yourself, and no loss to me. This I hope will put you to rights.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 61.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 30, 1702.

We expect Dr. Gregory's book very speedily; but the change of affairs, I fear, has spoiled a great part of his design. I am told Mr. Halley has a treatise of comets finished. I shall be glad to see the first, because I am now provided to tell them *whether their theories are true or not;* and, as for the latter, I have a pair of letters of Mr. Newton's, dated in February and March, 1680-1, *directly denying that the comets of November,* 1680, *and December and January following,* were the same, or that its path was bent, as I described it, and [as] he after allowed, in his theory of it in 1687. These two letters are three sheets of paper; and he ridicules in them what he blames himself upon afterwards. He gives out now he has done, and told me so. I am apt to think 'tis only because he has no mind to own what he has received from the Observatory, and thinks that Dr. Gregory and Mr. Halley may excuse themselves well enough for saying nothing of it. But if God (who has prospered my labors) spares my life and health, I shall account with him in such a manner as shall make him sensible of his fault, without giving him any cause to complain of, Sir, your real friend to serve you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 62.) *Letter from Mr. Flamsteed to Mr. Caswell.*

Burstow, in Surrey, July 30, 1702.

Kind Friend,

I received Dr. Gregory's book, with two of Mr. Milnes's *Conics*, the week before I came down hither. I had Sir Christopher Wren's present in my hands two days before: and now I must tell you that I saw the Doctor in Garraway's Coffee-house the Friday before he left London; but not being sure it was he (for I thought he had been gone out of town) I did not speak to him, only out of fear I should be mistaken, as I sometimes am in taking one person for another.

As to his book, assure yourself I hold to my first resolution—*it will fetch its own character;* and (whatever he thinks) I do not believe that many in the world will be the wiser for it, or approve his way of compiling it.

He has two or three flings at me, for which I can easily forgive him. Page 274, he tells us, " *Neque muros* licet firmissimos, neque ipsas rupes montesque eandem perpetuo sitam servare

" certissimum esse." This is a fling at my wall arc, whereby all the observations were taken, by which Mr. Newton found out the necessary corrections of the lunar theory. If he ruins it, *both that and his own must fall.* But how had he known it, if I had not fairly and ingenuously told him of it? He might have added, if he pleased, that though rocks shake, yet there are ways of finding out their errors; which though he be ignorant of, it does not follow that all mankind besides must; and I often have said that I know a way of finding out the errors of my wall instrument. It had been for his reputation to have added this.

Page 275, he allows all the observations which he endeavoured before to ruin by undermining my wall; and what I affirmed besides, that the parallaxes observed could not proceed from Mr. Newton's nutation. But then he will ruin it, if possible, by a fair *supposition* that it might be caused by the southern hemisphere of our globe being heavier than the northern: " Vel propter minorem illi æsta-" tem majusque frigus, &c.: vel *aliam quandam causam nobis ignotam.*" As to the frosts and winters being longer at the southern pole than at the northern, I grant 'tis so by about eight days; but then he ought to have considered, that the perigee of the sun is made in ♑, and the sun then nearer the earth than in ♋, by $\frac{1}{1\,8\,1\,0}$ of his mean distance: so that we may reasonably suppose their summer heats are proportionably greater than ours in the southern regions; and whatever extraordinary congelations or condensations were made by the longer winters are resolved by the greater heats of their summer: which might serve for an answer to his poor supposition.

But to satisfy him more fully, look on the map of our globe. You will find that the northern hemisphere consists most of land; and you that have surveyed countries know very well that great tracts of countries lie some considerable height above the seas. The southern hemisphere has no great quantity of earth laid down in it: so that we may reasonably suppose the northern hemisphere is much heavier than the southern; except he prove that the waters of the southern part of our globe, " *propter aliquam causam nobis ignotam,*" may be heavier than the earth of the northern, which I hope he will not.

Certainly the Dr. has a mighty opinion of his own authority, or else he would never have attempted to overthrow a well-grounded truth on suppositions. But he is a closet astronomer; and having never thought of throwing away money on instruments, he had rather do his business by a party, on supposals, than observations. For the future, betwixt you and me, he shall be *Doctor the fair Supposer.*

But lest this should not do, and other persons, as perverse as myself, should reject his suppositions with scorn, page 277, he will kill me with a demonstration upon another supposition still, that the diameters of the fixed stars are insensible; and, to be sure of it, he does not say it of any experience of his own, but from Mr. Huygens, who had tubes and glasses that did not lie always by him unemployed, as some instruments do that I got to be made for the Astronomy Professor at Oxford. Now I am resolved not to be murdered; and therefore desire him to read that whole paragraph, page 7 of the *Systema Saturnium*, where Mr. Huygens makes this assertion *. He will find that he adds, immediately after, " Quoties vitris usus sum fuligine leviter infectæ ad auferendos radios." And a little after he tells [us] that it was the eye-glass that he covered over with smoke: which shows the reason of his mistake; for the small interstices betwixt the particles of smoke that covered over his eye-glass were not sufficient to receive the whole image of a big star: they were only points; and the biggest stars could only appear as shining points through them. But my old friend, Mr. Huygens, in that very place, says enough to have convinced the *supposer* that his assertion was a

* This quotation is not given in the present letter, but it will be seen in the following one, page 206. F. B.

mistake; for he adds, "At ex Hevelii consilio, exterius vitrum contegens, ut exiguum tantum fora-"men relinquatur, aliquam magnitudinem præ se ferre illas vidi." This is sufficient. But if he would see more of this, let him read Riccioli's *Almag. Mag.*, page 716. He will find that Kepler was of the same opinion with Mr. Huygens (perhaps he misled him); and a full answer to it, with Riccioli's own determination of the diameter of Sirius to be 18″; a very sensible quantity. After these two authorities of Riccioli and Hevelius, I suppose I may be allowed to quote an observation of my own, made at Derby, October 22, 1672, in the morning, which I showed Mr. Keile, and which I will give you in the very words I wrote it down as soon as taken.

"1672, October 22. When Mercury was about 10 deg. high, I observed him in the garden with my longer tube (of 14 foot); but could not with it see the fixa (near him), the daylight being too strong; only I noted his diameter 45 parts = 16″, or a little less; for, turning the tube to Sirius, I found his diameter 42 parts = 15″, which I judged equal to Mercury's. The aperture on the object-glass was ¼ of an inch: so that Sirius was well deprived of spurious rays, and shined not turbulently, but as sedate as Mercury; the limbs of both well defined, but Sirius best."

I was with Mr. Newton on Friday last, and told him of this observation. He would have said something in defence of his friend, from the nature of the difform rays of light; but when I urged the smallness of the aperture on the object-glass, he let his discourse fall. There is nothing to be said against this observation; for the fault of the glass was as great in Mercury as Sirius. If Mercury had a sensible diameter, so had Sirius. If he will say Sirius had none, Mercury must have none; for both were observed with the same glass, and the same aperture on it. Mr. Huygens says this may proceed "*ab aliqua visus fallacia*" (as *Dr. Supposer* does that the south part of the earth may be heavier than the north, *ex aliqua ignota causa)*; but 'tis plain prejudice, for his own method had prepossessed him: and having showed, then, the reason of his mistake, you ought to pardon him for the sake of his many useful inventions, and excellent treatise *De Horologio Oscillatorio*.

Pray take notice that I do not herein attack any part of the Doctor's works: I only show you his unreasonable suppositions and wilful mistakes. It seems very strange to me that he cannot let me forget an injury he once did me in the conveyance of my letter *De parallaxi orbis annui**, but must refresh my memory by a worse repetition. I pray God forgive him. I have other business on my hands than to mind his spite; only pray, if he glories in his demonstration, page 277, tell him (or let some bold friend of yours do it) that I have obliged myself not to take any notice of what arguments shall be produced by contemplative men against my observations; that he must build larger instruments, and fix them better than mine, and repeat his experiments for as many years as I have done, (as I tell him at the foot of my *Letter to Dr. Wallis*,) and then I doubt not but he will find cause to be ashamed both of his *suppositions* and demonstrations.

[Copied from the *original* draught of the letter, in MSS, vol. 35, page 179.]

No. 63.) *Letter from Mr. Flamsteed to Mr. Caswell.*

The Observatory, Sept. 5, 1702.

MY FRIEND,

I received Dr. Gregory's book [*Elementa Astronomicæ*] but 2 days before I went into Surrey, so had not time then to tell you what I had to say to it, nor shall I say anything to it

* See No. 44 in this Appendix, page 166. F. B.

now, but only to a pretended demonstration or two, in which he attacks my *Letter to Dr. Wallis* about the parallax of the earth's orb; the book itself will fetch its own character; but as to his suggestions and pretended demonstration, it may have some effects both upon you and Dr. Wallis, and therefore I shall tell you the faults of both.

In the conclusion of my letter to the Doctor, I told him that I should not concern myself to answer any arguments [that] should be brought against the subject of it, and that those who could not admit it must make larger instruments than mine were, fix and divide them better, and continue their observations for as many years as I had done mine; and then I doubted not but they would find the truth of what I asserted. The Doctor never scruples to admit the observations, nor argues against my inferences; but, page 275, he suggests, that the observed diminution of the pole-star's distance from the pole in December, every year, may be caused by the southern hemisphere of our earth being something denser than the northern; " vel propter minorem illi estatem, quam huic " majusque frigus; *vel aliam quandam causam nobis ignotam;*" that is, it may be so by reason of some occult quality: it may as well be otherwise. When the sun is in the southern signs, he is nearer our earth than when in the northern; and if I should say that he has sufficient power then to resolve all the extraordinary condensations made by a winter 8 days longer than ours, 'tis a better answer than his supposition deserves, but I will give him a serious one. 1. If such suppositions as his may be allowed, any confident and drolling supposer may overthrow and ridicule all the demonstrated truths of Mr. Newton's *Principia* and his book, by one or two such hardy suppositions. 2. Let him cast his eye on our maps of the globe, he will see that there is much more continent in the northern than southern hemisphere. Whence it follows that the northern, contrary to his supposition, must be the heaviest: except he will suppose again, that " *propter quandam causam* " *ignotam*," earth and rocks may be lighter than water, and that a weight removed from the centre of a balance weighs no more than when it was placed nearer; for the hills are farther removed from the centre of the earth than the surface of the sea.

But grant that by some accident or accession of matter the southern hemisphere should be made heavier than the northern, the Doctor's consequence will not follow: he forgets that the earth's centre of gravity will only remove from its former place nearer the accessional weight, and it will continue revolving on its new centre and axis as it did on its old, without any other nutation than what it had before the southern hemisphere became the heaviest. This is a very natural consequence; he ought to have considered it before he made his glorious conclusion.

The Doctor, I am apt to think, was sensible that his supposition would not do the business, and therefore, page 277, he will prove by a demonstration, that the parallax of the earth's orb is altogether insensible. But at the very entrance of it *he supposes again* that the diameter of a fixed star is equal to the diameter of the sun; I can allow it, though it be but a supposition; but others will say, he might as well have supposed it a great deal bigger or less. He goes on with a further supposition again, that an assertion of my deceased very good friend Mr. Huygens is true, which is demonstrably false, and the only false one I remember him to be guilty of. 'Tis page 7 of the *Systema Saturnium*, and had the Doctor but read the whole paragraph, he might have found the reason of Mr. Huygens' mistake; and in the very next words after it, great reason to suspect the truth of his assertion. His words are " fixarum autem diametros, nulla unquam latitudine cernere potui, " sed tantum minime puncti instar, *quoties vitris usus sum fuligine leviter infectis*, ad auferendos " radios;" in the conclusion of the paragraph he tells us that it was the *lens oculo proxima* that was lightly covered with smoke, which, had the Doctor considered, he would have found the reason why

the fixed stars through such an eye-glass appeared only as points; for the particles of smoke being spread all over the eye-glass, the stars could only be seen through the interstices or spaces betwixt the particles, that were only points, and not wide enough to receive the image of a large star passing over them; so that *the largest stars appeared no bigger* than the point-like spaces through which they shone, *or as points.* Mons. Huygens takes no notice of any difference he found betwixt the apparent magnitudes of the biggest and least stars viewed through a smoked eye-glass; the reason is evident, the species of a bright 5th or 6th light star would probably fill any of the interstices betwixt the smoky particles, and no more of the light of a great star than filled one of them could be transmitted to the eye (the solid particles of smoke intercepting the rest) through the glass: so that the 5th and 6th light stars to Mr. Huygens appeared this way as big points as the 1st or 2nd light. But Mr. Huygens very candidly and ingeniously adds immediately, "*At ex Hevelii consilio** *(exterius "vitrum contegens ita ut exiguum tantum foramen relinquatur, aliquam magnitudinem præ se illas "ferre vidi.*" This expression might have suggested to the Doctor that Mr. Huygens was to be suspected, for his own sight and confession contradicts his first assertion, though to evade it he adds immediately "quam proinde non stellarum propriam sed ex *aliqua visus fallacia nasci arbitror;*" if he had said, with the Doctor, "*ex aliqua ignota causa,*" it had been as much to his purpose, and as good as what he goes on with, "nam nostra quidem, illa methodus, transfumum, *quo lens oculo "proxima tincta est,* stellas inspiciendi *certa est.*" It was certainly a pretty good way (but not the best) of viewing the sun through a telescope; for the particles of the smoke on the glass retarded so many of his rays, that those few (which passed through), only served to paint his image distinct on the retina of the eye without hurting it; but no one, besides Mr. Huygens, ever affirmed that this was a good way of taking the spurious rays of the stars or planets; and I am confident when the Doctor has well considered it, he will think it no good one at all. For the spurious rays derive their original from the object-glass, and fall, with the true, on the eye-glass, and are transmitted to the eye through it (though smoked) by the small interstices betwixt the smoky particles as well as the true, though Mr. Huygens could not perceive them: because that the spaces betwixt the particles were so small, and the different light passed so swiftly over them, that he could see it only as points of light, without being able to distinguish anything of the colours in it.

Mr. Huygens' book was printed in the year 1669. The true theory of light and colours was not published till 2 or 3 years after, in the Philosophical Transactions of 1672, &c., by Mr. Newton; nor the true reason of these spurious rays known till then: so Mr. Huygens is excusable for his opinion; but the Doctor can hardly excuse himself for allowing it. For Riccioli, in his *Almagest*, vol. i, page 716, § 3, takes notice that Kepler had affirmed that the fixed stars were but as lucid points, answers it, and shows that Hevelius's method was true and good, and thereby determines the diameter of Sirius 18″ of Alior 4″: so that the diameter of a fixed star of the 5th or 6th light is as large as the diameter of ☿ or ♂, at the greatest distances from our earth, according to Riccioli. And if the Doctor will be further satisfied, there are telescope glasses at Oxford, and conveniences for managing of them; let himself view any of the planets, or large fixed stars, without any aperture on the object-glass, he will find its image compassed with colours: let him put apertures on the object-glasses (less and less till the last be but ½ or ¼ of an inch wide), he will find that as he diminishes the aperture, the perceiving of rays and colours will diminish, till at last the star appear with its limb distinct and free from colours, whereby he will be fully convinced of Mr. Huygens' mistake.

In the meantime I shall give you a copy of some notes from my own book, which I took at Derby, in the year 1672, October the 22nd, mane, which I showed Mr. Keile when here, and mentioned to

* Selenog. p. 37.

you, which will fully prove that the fixed stars have sensible diameters, and perhaps save the Doctor's labours.

" 1672, Oct. 22, mane, when Mercury was about 10 degrees high, I observed him in the garden " with my longer tube (of 14 feet), but could not with it see the fixa near him, the day-light being " too strong. Only I noted his diameter 45 parts = 16″, or a little less: for turning the tube to Sirius " I found his diameter 42 parts = 15″, which I judged equal to Mercury's; the aperture on my object " glass was ¼ of an inch, so that Sirius was well deprived of spurious rays, and shone not turbulently, " but as sedate as Mercury, the limbs of both well defined, but Sirius best."

I suspect my diameters, both of Sirius and Mercury, are 2″ or 3″ too big, both by reason of the difficulty of measuring so small an angle with the micrometer, and because the different refractions of different rays might add so much to them, as Mr. Newton shows in his theory of light and colours; but the effects were the same in the planet and Sirius; if Mercury had a sensible diameter, Sirius must be allowed one too; if Sirius had none, Mercury must have none.

Now this assertion of Mr. Huygens proving false, and the Doctor's first supposition wanting to be proved, his demonstration grounded on them falls of itself, and I have no more to say to him. I have endeavoured hitherto to oblige him whenever I had opportunity; I have given him no provocation to this, nor know any reason he had to attack it, except he envies the Royal English Observatory the honour of that discovery, which if it had come from Edinburgh had not been enough valued or applauded. You live in Oxford, and think 'tis your concern to keep fair with him; I leave it therefore to your discretion to convey the substance of what I write to him in such a manner as may let him see that you are his friend, and (though I have but a very mean esteem for persons of his temper) yet I am not his enemy, but can be his friend, even before he makes me reparation, as a good Christian ought.

Pray let Dr. Wallis see this letter. I suppose he saw the Doctor's book, and these fine demonstrations before they were printed; he had done the part of a friend if he had acquainted me with them, and prevented the shame that will fall on his colleague by publishing such things when nowayes provoked; I hope you have paid him for the book. If he pretends to present me with it, I shall return it him, or to some public library, with my notes on it.

There are some other places he attacks me in without naming me; I pass them by and forgive him; they show both his ignorance and malice, to those that understand them; to others they signify nothing. I am often ill of the headach, which hinders me much in my business; but it gives me ease to hear my friends enjoy their health. I pray God give you yours, and the same to Dr. Wallis and his colleague. Give my service to Dr. Dunster, acquaint him and Mr. Keile with the contents of this letter, which had been abridged if I had had leisure.

I am yours ever,

John Flamsteed.

[Extracted from MSS, vol. 33, page 62, from the *end*. There is also another copy of it in MSS, vol. 35, page 183.]

No. 64.) *Letter from Mr. Flamsteed to Dr. Wallis.*

The Observatory, October 10, 1702.

Honored Dr.

Some business that has occasioned me to be often in London of late, hitherto has hindered me from returning an answer to yours of the 26th instant. I am obliged to you for it, and

the candour in acquainting Dr. Gregory so freely with your dislike of his endeavours to invalidate a truth, which has been incontestably proved, by eight years continued observations, in my letters to you, before you had seen mine to Mr. Caswell.

You saw in it, that to decline all occasion of controversy with Dr. Gregory, I had avoided taking notice of anything in his book wherein I was not directly mentioned, and attacked, and that I have passed by several places wherein he reflects upon me without mentioning my name. For I know very well that those sorts of controversies serve only to make our professions disesteemed by those that understand them not. And I have always been desirous to maintain a fair correspondence with the professors of our own nation, to support the honor of it.

But Dr. Gregory I know has been excited to what he has done by the misrepresentation of a certain gentleman who thinks no places so fit for him as either the Doctor's or mine: I pity them both, and pray God forgive them. I shall persist in my business, let them do or say what they please: only I think it might be for Dr. Gregory's advantage and reputation to keep a fair correspondence with the Observatory, or at least not to attack it unprovoked. To prevent a further disgrace to him, I must answer a suggestion he makes in his book, and which was lately repeated to me by one of his own friends, though, to avoid controversy, I mention it not to Mr. Caswell: 'tis that the parallax of the earth's orbit might better be tried by taking the distance of two fixed stars at different times of the year, and that in this method there could be no room for his cavils. 'Tis a specious suggestion; but if the Doctor understands instruments, and the methods of observing with them, he must know that the difference of the meridional height of any one fixed star may be much more accurately taken with a fixed instrument than the distances of two stars with a moveable one. I pray God continue your health, and I am, Reverend Sir, your affectionate, obliged friend, and servant,

JOHN FLAMSTEED.

[Extracted from MSS, vol. 33, page 67, numbered from the *end.*]

No. 65.) *Extracts of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, December 14, 1702.

Another reason why I wrote not to you was distemper: my headach still troubles me; but, I bless God for it, not so ill as formerly. But the main was, my man Hudson has left me, and is removed to London, where he teaches mathematics; and my younger servant is not yet ready at his business, which therefore lies most on my hands: I have had a great deal of late, but now 'tis pretty well over, and you may expect seasonable returns to yours.

I do not know whether I sent you a copy of a letter I wrote to Mr. Caswell about some demonstrations in Dr. Gregory's book; if I did not, let me know; by that time the answer arrives he will be at leisure to copy it for you, and it shall be sent; 'tis a full sheet of my writing.

Since then, the French have published some remarks upon my *Letter to Dr. Wallis.* Young Cassini is the author, and they are printed in Fontenelle's history of the *Academie des Sciences.* He undertakes to show what will be the result of the Copernican hypothesis, or the earth's motion in respect of the fixed stars and the poles of the earth and ecliptic; and that he finds an oversight com-

with inter-mutual and inter-columnar differences; which renders the collecting of the equations much more expeditious and easy than in the old form: and for this reason I have a particular table for taking out the greatest equations, instead of the eccentricities, which you will easily apprehend how I form. The tables for taking out the variations and small equations are in the same form, but have four columns for one in the old. I add some small tables besides, which you will find on perusal the theory requires; besides tables of parallaxes to every degree of apparent and true distance from our vertex, reductions and angles of the moon's visible way with the ecliptic, which cost me about four months' pains, with my man, Hudson, to make: but I have this advantage in them, that now, by the lunar equations, I can take out the equations for the orbits of Jupiter and Saturn, with the logarithms of their distances from the sun, for any theory, with little trouble. I have not yet got another servant; but as soon as I have one, if you desire it, I may furnish you with copies of the tables of equations, semi-diameters, and parallaxes, for your private use.

You need not be shy of letting anybody know that I have these tables, or the catalogue of the stars' places in such forwardness by me. I desire it should be known and spoken of by such persons as you, that know the labor and pains required to make the observations first, and then to derive the catalogue and planets' places from them. I desire to have them published as soon as may be; and, in order to it, that you would speak freely of them; but I desire that no copies of what I impart to you be given abroad, lest Captain Raymer should say they need no more, and thereby hinder me from procuring an allowance for publishing the whole. The charge of the plates and press will not be so little as £1000. I am more money than this out of pocket already in making instruments and hiring help to manage them, and work up the observations made this 26 years: 'tis but just the public should defray all this charge: yet I ask no more than to hire help to copy my papers and books for the press, and to finish some necessary calculations and tables, and to print them at *my own* charge; and I have some hopes of obtaining it. God's providence brought me to this work, has prospered me in it, and I will not doubt but he will give it a happy conclusion. Now I have told you for what reason I would not have what I impart to you communicated to others; I hope you are satisfied that 'tis not out of any reserved humor, as Mr. Newton's friends gave out, who would have obliged the narrow soul at my cost, but only to do the public greater service, and give it the honor of my pains, to whom and not such self-glorious persons 'tis due. 'Tis known very well whence he had his materials and principles; he makes but a small figure now, and will probably make a less in a little time, though, had he been wise, he might have made a much bigger than he did formerly. But enough of him (in whom the justice of providence is remarkable); he pretends to have left his mathematical studies, I leave him therefore to his other.

We are like to be overwhelmed with treatises of series and fluxions. Mr. Sheene's is not yet extant; but a non-conformist, Mr. Ditton, I am told, has a piece in the press; and one Mr. Hayes, a Scotchman, has one too, on the same subject, going to the press. Assure yourself that, as anything of this kind comes out, it shall be sent you, or, at least, a just account of it, by your obliged real friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 68.) *Letter from Mr. Flamsteed to Mr. Caswell.*

The Observatory, March 25, 1703.

SIR,

By yours of the 22nd instant, I find you are as little concerned for the reception of your new contrived baroscope as I was when I imparted it to the Royal Society: you are certainly in the right, and as good a judge of that contrivance as the objectors against it; but your objections are over since Mr. Halley's return, and I am apt to think I shall hear no more of them: for they are answered in calculation, as I told some of them they should find them.

I am not concerned how he manages his affairs at Oxford, being very well pleased I had no hand in recommending him, and as for a reconciliation with him, I know of no occasion for it: having never done anything that was injurious to him willingly, except in vindicating myself from his aspersions and calumnies, much more fiercely to his face than behind his back; and if in this I have done him any injury, he must thank himself for it, I am sure I have done him none designedly. Last time I saw him many words passed betwixt us; he complained of my unkindness highly, and asked loudly what he must do to gain my friendship; I answered roundly *he must become a just, serious, and virtuous man*, and then I should be his friend immediately. This answer the company took notice of, but he passed [it] over. I am of the same mind still, and if he can be such, there is no need of any reconciliation; if he cannot, it would be highly to my prejudice to make an accommodation with one whom I never willingly injured.

You may remember he made a difference with you, came to an accommodation, and did you more injury after, by his clamour and unjust complaints, than he had done before. God turned it to your good, and I know you are thankful. If I should not be wary when I have such an instance before me, I should not deserve your care, or his providence.

I should be glad to see your new instrument, when you have leisure to impart it to me. And much more to see you here, that we may discourse freely of our small affairs, and acknowledge together the goodness of that Providence that preserves us from suffering by the practices of those that regard not his glory, but their own. Pray let me know what time you intend to be in London, that I may so order my affairs as to meet you there, and enjoy your good company here.

I have answered all the particulars of your letter, and shall now tell you what I am doing. Soon after Mr. Newton's book came out, I made new tables for calculating the moon's place according to all his additions and corrections of the old, on the Horroccian theory. But I applied them not till lately, when I set myself to examine them by 30 appulses of the moon observed here betwixt the years 1675 and 1689. I have repeated a dozen of them twice, for greater certainty, and find the new numbers err commonly 5 or 6 minutes, sometimes in the excess, and in the opposite point of the orbit in the defect, and *vice versâ;* and sometimes the errors rise to 8′ or 9′ in longitude, and that not in the quadratures, but at a distance from them.

The errors in latitude are frequently 2, 3, or 4 minutes, which is intolerable. They result not only from my own observations, but from those of others taken at the same time. 'Tis much for the Doctor's and his author's credit, and shows how skilful they both are, that such errors are found in the first careful examination.

I hoped, however, they would agree better at the full moons, and in order to examine them, I collected the observation of 16 lunar eclipses taken here by myself with Mr. Halley, or by the

with inter-mutual and inter-columnar differences; which renders the collecting of the equations much more expeditious and easy than in the old form: and for this reason I have a particular table for taking out the greatest equations, instead of the eccentricities, which you will easily apprehend how I form. The tables for taking out the variations and small equations are in the same form, but have four columns for one in the old. I add some small tables besides, which you will find on perusal the theory requires; besides tables of parallaxes to every degree of apparent and true distance from our vertex, reductions and angles of the moon's visible way with the ecliptic, which cost me about four months' pains, with my man, Hudson, to make: but I have this advantage in them, that now, by the lunar equations, I can take out the equations for the orbits of Jupiter and Saturn, with the logarithms of their distances from the sun, for any theory, with little trouble. I have not yet got another servant; but as soon as I have one, if you desire it, I may furnish you with copies of the tables of equations, semi-diameters, and parallaxes, for your private use.

You need not be shy of letting anybody know that I have these tables, or the catalogue of the stars' places in such forwardness by me. I desire it should be known and spoken of by such persons as you, that know the labor and pains required to make the observations first, and then to derive the catalogue and planets' places from them. I desire to have them published as soon as may be; and, in order to it, that you would speak freely of them; but I desire that no copies of what I impart to you be given abroad, lest Captain Raymer should say they need no more, and thereby hinder me from procuring an allowance for publishing the whole. The charge of the plates and press will not be so little as £1000. I am more money than this out of pocket already in making instruments and hiring help to manage them, and work up the observations made this 26 years: 'tis but just the public should defray all this charge: yet I ask no more than to hire help to copy my papers and books for the press, and to finish some necessary calculations and tables, and to print them at *my own* charge; and I have some hopes of obtaining it. God's providence brought me to this work, has prospered me in it, and I will not doubt but he will give it a happy conclusion. Now I have told you for what reason I would not have what I impart to you communicated to others; I hope you are satisfied that 'tis not out of any reserved humor, as Mr. Newton's friends gave out, who would have obliged the narrow soul at my cost, but only to do the public greater service, and give it the honor of my pains, to whom and not such self-glorious persons 'tis due. 'Tis known very well whence he had his materials and principles; he makes but a small figure now, and will probably make a less in a little time, though, had he been wise, he might have made a much bigger than he did formerly. But enough of him (in whom the justice of providence is remarkable); he pretends to have left his mathematical studies, I leave him therefore to his other.

We are like to be overwhelmed with treatises of series and fluxions. Mr. Sheene's is not yet extant; but a non-conformist, Mr. Ditton, I am told, has a piece in the press; and one Mr. Hayes, a Scotchman, has one too, on the same subject, going to the press. Assure yourself that, as anything of this kind comes out, it shall be sent you, or, at least, a just account of it, by your obliged real friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 68.) *Letter from Mr. Flamsteed to Mr. Caswell.*

The Observatory, March 25, 1703.

Sir,

By yours of the 22nd instant, I find you are as little concerned for the reception of your new contrived baroscope as I was when I imparted it to the Royal Society: you are certainly in the right, and as good a judge of that contrivance as the objectors against it; but your objections are over since Mr. Halley's return, and I am apt to think I shall hear no more of them: for they are answered in calculation, as I told some of them they should find them.

I am not concerned how he manages his affairs at Oxford, being very well pleased I had no hand in recommending him, and as for a reconciliation with him, I know of no occasion for it: having never done anything that was injurious to him willingly, except in vindicating myself from his aspersions and calumnies, much more fiercely to his face than behind his back; and if in this I have done him any injury, he must thank himself for it, I am sure I have done him none designedly. Last time I saw him many words passed betwixt us; he complained of my unkindness highly, and asked loudly what he must do to gain my friendship; I answered roundly *he must become a just, serious, and virtuous man*, and then I should be his friend immediately. This answer the company took notice of, but he passed [it] over. I am of the same mind still, and if he can be such, there is no need of any reconciliation; if he cannot, it would be highly to my prejudice to make an accommodation with one whom I never willingly injured.

You may remember he made a difference with you, came to an accommodation, and did you more injury after, by his clamour and unjust complaints, than he had done before. God turned it to your good, and I know you are thankful. If I should not be wary when I have such an instance before me, I should not deserve your care, or his providence.

I should be glad to see your new instrument, when you have leisure to impart it to me. And much more to see you here, that we may discourse freely of our small affairs, and acknowledge together the goodness of that Providence that preserves us from suffering by the practices of those that regard not his glory, but their own. Pray let me know what time you intend to be in London, that I may so order my affairs as to meet you there, and enjoy your good company here.

I have answered all the particulars of your letter, and shall now tell you what I am doing. Soon after Mr. Newton's book came out, I made new tables for calculating the moon's place according to all his additions and corrections of the old, on the Horroccian theory. But I applied them not till lately, when I set myself to examine them by 30 appulses of the moon observed here betwixt the years 1675 and 1689. I have repeated a dozen of them twice, for greater certainty, and find the new numbers err commonly 5 or 6 minutes, sometimes in the excess, and in the opposite point of the orbit in the defect, and *vice versâ;* and sometimes the errors rise to 8′ or 9′ in longitude, and that not in the quadratures, but at a distance from them.

The errors in latitude are frequently 2, 3, or 4 minutes, which is intolerable. They result not only from my own observations, but from those of others taken at the same time. 'Tis much for the Doctor's and his author's credit, and shows how skilful they both are, that such errors are found in the first careful examination.

I hoped, however, they would agree better at the full moons, and in order to examine them, I collected the observation of 16 lunar eclipses taken here by myself with Mr. Halley, or by the

French with me: and these last ten days I have employed in examining them, and found them differ commonly 5 or 6 minutes of time, and not seldom 9′, 10′, or more in the phases of a lunar eclipse, which shows that they err 5 or 6 minutes in the moon's longitude at the syzygies. You need not be afraid of speaking of this, for the calculations that differ most from the heavens have been carefully repeated.

If Dr. Gregory, or any other, says I did not impart observations sufficient for completing the numbers, you may tell them I was at the pains to calculate above 200 places of the moon from my observations first, and after from my own tables, for Mr. Newton; which employed all my spare hours for more than three years: that in the mean time I was carrying on the work of my great catalogue, that these are ten times as many, and infinitely more certain than any that were ever seen before of the moon, and would be thought sufficient by any judicious and skilful astronomer. That I know no obligations I lay under to spend all my time to serve Mr. N., who would needs question the observations when they agreed not with his theories (or rather conceptions), and gave me continual trouble about them at a time when I had a violent distemper that continued near a year with me, and suffered me not to prosecute my own necessary business.

I know how to take off a part of the error, by throwing away part of Mr. Newton's equations which the heavens allow not, and, therefore, you may add, if you please, *that if either Mr. Newton or Dr. Gregory* (who by this time may be supposed to have made tables for the moon according to his own theories or conceptions) *will send me their tables, I will assuredly tell them whether they agree with the heavens or not, and how much they differ:* this you may say without any just offence to them.

I have not yet examined how the new corrections of Mr. Newton, with his additions to my old tables and Horroccian theory, agree with the observed meridional places of the moon, because he told me when I saw him last *they agreed in their appulses to fixed stars, within* (*not* 2 *or* 3 *minutes, as Dr. Gregory wrote, but within*) 3 *or* 4. They are his own very words to me, but I find the errors of them double, even in appulses. I have a good and large stock of other observations by me, whereby I can further examine them when I please, and with much less labour.

Feb. 25 last he sent me his new *Optics*, by his bookseller; the next day I ordered the same person to return him my thanks. In the Philosophical Transactions, No. 80, for Feb. 19, 1671-2, page 3079, he affirms that the *object glass of any telescope cannot collect all the rays which come from one point of an object, so as to make them convene at its focus, in less room than in a circular space, whose diameter is the* 50*th part of the diameter of its aperture.* In his *Optics*, page 72, near the bottom, he contracts this space; for, says he, *the sensible image of a lucid point is, therefore, scarce broader than a circle whose diameter is the* 250*th part of the diameter of the object-glass of a good telescope, or not much broader, whence he concludes that in a telescope of* 20 *or* 30 *feet, with an aperture of* 2 *inches, it may be* 5 *or* 6 *seconds, and scarce above.* Read what follows, you will find this is to help out Dr. Gregory, who will have the fixed stars to have no sensible diameters, that they may have no sensible parallaxes. I have showed his *mistakes* already, in a letter to you long since; I shall only say now to Mr. Newton, that if his assertion be good, and the different rays bestow a circular body of 5 or 6 seconds diameter on a lucid point, then even the smallest lucid point in the heavens, and consequently all the small fixed stars, ought to answer as circles of 5 or 6 seconds diameter. But if he compared them with ♂ or ☿ when visible near the sun, and their greatest removes from the earth, he will find *that two-thirds at least of all the fixed stars in the heavens have not one single second diameter*, so that his assertion is false in fact: and what he says after about

fouling the eye-glasses, &c., only shows either his great *insincerity*, fondness for his own conceptions (theories as he is pleased to call them), and unacquaintedness with the heavens.

But to put the business out of doubt, and to show that the dilatation of the rays of light have no such monstrous effects as he conceits, I have thought of and made an experiment, that will fully convince you or any ingenious person *(that values truth above flattery, or his little interests)*, that there is no such enormous augmentation of visible lucid bodies made by telescopes as he imagines. What will be the consequence of it I foresee, but shall not mention at present, wanting room to add more than that (I bless God for it) I enjoy my health, pray heartily for yours, and am ever, Sir, your most affectionate friend, &c.

JOHN FLAMSTEED, M.R.

Mrs. Flamsteed gives you her service.

Please to let me know what is the use of the new instrument: I will find room in my next for my experiment.

[Extracted from MSS, vol. 33, page 45.]

No. 69.) *Letter from Sir Hans Sloane to Mr. Flamsteed.*

London, June 22, 1703.

SIR,

Mr. Hill the other day gave me your letter to Mr. Caswell, to be read at a meeting of the Society, which was accordingly done. The Society were very willing and desirous any part of it that may relate to your own justification, or the information of the world, should be printed in the *Transactions*, if you think it proper. I therefore send it you herewith, that you may fit it as you please for the publication. I also transmit you part of a letter from Mr. Leibnitz to me; concerning which, especially that part of it relating to a new star, the Society will be very glad to have your opinion. I am, Sir, your most humble servant,

HANS SLOANE.

[Copied from the *original*, in MSS, vol. 35, page 187.]

No. 70.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Decem. 18, 1703.

De Moivre's tract against Sheen goes on. Dr. Wallis is dead: Mr. Halley expects his place, who now talks, swears, and drinks brandy like a sea-captain: so that I much fear his own ill behaviour will deprive him of the advantage of this vacancy. Last St. Andrew's day, Mr. Newton was chosen president of the Royal Society; and your successor, Mr. Hodgson, a member. My affairs suffer me not to attend the Society; they have a limb of me, however, but he is honest and discreet, and I believe and hope will not serve some men's small design, as they expect he should. Mr. Newton's book of *Colours* is in the press, and near finished. Mr. Hodgson has seen the first sheet of it: 'tis English; but he could make no estimate of it from what he saw. Subscriptions are offered for

Mr. Hayes's book of *Fluxions:* the author is gone to Guinea, but the work will be printed by Lady Day next. I would send you one of the papers, but that I think it not worth the postage. I have lately been bit by subscriptions, therefore resolve for the future to wait the publication of the book, and see it before I purchase it.

The ill weather and accidents hindered me from observing those eclipses of ♃'s satellites you saw at Horton: pray continue your observations as oft as you have good opportunities. I shall wait them by myself or servants here, that I may have a good many to compare with yours, and settle the difference of our meridians, which will determine a great many eminent towns about you, particularly Leeds, near which Mr. Gascoign dwelt, and the parts of Lancashire next you, where Mr. Horrox and Crabtree inhabited. You must wait for the appearances of the 2nd satellite, at least 20 minutes before the time in the tables, for the inequality anticipates its eclipses now so much: the 3rd's orb, I fear, is out of the plane of the 1st and 4th, as well as the second's; for the durations of its eclipses are now much less than that supposition allows: so you must expect its immersions later much, its emersions not so much later, than the calculus: but when ♃ comes into ♋ and ☊ they will agree again very nearly with the tables. I bless God for it, I enjoy my health; may He, who has kept me all my life, keep and preserve you: ever I am to serve you.

One Mr. Derham, rector of Upminster in Essex, has observed many satellite eclipses: I shall write to him to know if he has any that will fall in with yours, for which again I heartily thank you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 71.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 30, 1704.

I have been employed, of late, in calculating about 20 eclipses of the moon observed by myself here, with Mr. Halley or the French (few others being to be relied on) from my new tables, fitted to Mr. Newton's additions to, and corrections of, my old and the Horroccian theory. I think I told you formerly that these erred 8 or 9 minutes of longitude extra syzygias. I can now tell you that, in the first eclipses I calculated, they erred 10 minutes of time in the beginnings and ends; in some more, in others less afterwards. You see what their great boast of answering my observed places of the moon within 2 or 3 minutes comes to: the calculations have been twice repeated.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 72.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 4, 1704.

My discourse, about the faults of Mr. Newton's *Optics* and correction of my lunar numbers, brought the subtle gentleman down hither the 12th past. I thanked him for his book: he said then he hoped I approved it. I told him truly, no: for he gave all the fixed stars bodies of 5 or 6 seconds

diameter, whereas 4 parts in 5 of them were not one second broad. This point would not bear discussion: he dropt it, and told me he came now to see what forwardness I was in. The books of observations were showed him, my catalogue, with Tycho's and Hevelius's, as also the charts of the fixed stars. He seemed pleased, and offered to recommend them *privately* to the Prince: but was told he must do it *publicly* as he could for some good reasons, which not being able to answer he was silent. Plainly, his design was to get the honor of all my pains to himself, as he had done formerly, and to leave me to answer for such faults as should be committed through his management. But, having known him formerly, and his sole regard to his own interests, I was careful to give him no encouragement to expect I should give him any thing gratis, as I had done formerly. I showed him also my new lunar numbers, fitted to his corrections; and how much they erred: at which he seemed surprised, and said "It could not be." But, when he found that the errors of the tables were in observations made in 1675, 1676, and 1677, he laid hold on the time, and confessed he had not looked so far back: whereas, if his deductions from the laws of gravitation were just, they would agree equally in all times. I have caused some of my maps to be anew designed by an able workman: these I also showed him; they are very masterly done; each plate is 24 inches long and 20 broad, and will cost at least £12 a plate, with the engraving and finishing. These he seemed not to take much notice of: whether, because he is no great judge of these things, or out of discretion I dare not pretend to judge: though I tell you that they will be the glory of the work, and next the catalogue, the usefullest part of it, if God spare me life to see it perfected, which I cannot fear or doubt he will, when I consider how his Providence seems to have hitherto directed and taken care of it.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 73.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Septem. 2, 1704.

You will excuse me for not writing to you so frequently of late as I used to do: I have a couple of young pupils in the house; one of them the son of my Lord Ferrers; the other of Sir Matthew Bridges deceased, and his uncle is Surveyor of the Ordnance. I have been forced to carry them both into the country with me, and to stay at Burstow seven weeks this summer to look after my affairs and make way for a new curate; my last, Mr. Sheppey's son of Greenwich, having accepted a vicarage in Essex. In the mean time I have heard of little done by our society, though Mr. Raymer is in town: they have enough to do to express their cried-up Mr. N.'s blunders, which neither his sagacity, nor their noise will ever clear him of: for only native light contributes to the forming of the distinct picture of the object; the coloured rays are not perceived whilst mixed with it, but only fringe its limbs with an edging of colours.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 74.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, October 21, 1704.

My servant, that has drawn the constellations, is ill: I fear a consumption will carry him off, which will be a very great loss to me, now the Prince has made some offers to print my works, and I designed him to look after the engravers of the plates, and at the same time to copy for the press from the first night books of observations. I shall want more hands, but he was to be the chief; but I dare not promise myself much at present from this proposal: you shall hear more of it, if it go on; and I hope in a short time. God has conducted and blessed my work hitherto, and I will not doubt of his bringing it to perfection, and affording means to publish it, for the more manifesting the wisdom of his works and the good of an ingenious people, that loves truth both for its own sake and its usefulness.

* * * * * * * *

As to Jupiter's satellites I have had worse fortune than you. I dare not sit up to observe them, because I cannot bear the cold evening air as formerly. My servant was taken ill about 3 months agone: his first attack was a fever and ague; and he was struck senseless for two hours: the second deprived him again of his senses. The doctor gave him the Jesuit's bark in a large quantity; and since then he has never had his health; so that I could not employ him to attend these eclipses. He is very weak, but still can walk about; which makes me hope that, though he be much wasted, he may recover. Pray God he may, otherways I shall be destitute of help.

Our society decays and produces nothing remarkable, nor is like to do it, I fear, whilst 'tis governed by persons that either value nothing but their own interests, or understand little but vegetables, and how, by making a bouncing noise, to cover their own ignorance. I am told that Dr. Gregory has been lately in London for some time, and intends to practise physic there. Mr. Halley, his colleague, has been in London all this vacation, but designs not to reside at Oxford. Dr. Wallis's son offers to give his father's house to the professors of mathematics, if they will constantly reside in it and the university; to make it into two tenements for them: but, by what I hear, it seems they have no mind to comply with the condition; so the university will not have the honor of their company, who are angling for better preferments at court, but, being pretty well understood, I am apt to think, may fail of their expectations: their ill examples I hope will have the less effect by this unsettledness of theirs.

I lately had a letter from Mr. Pound, Chaplain to the new joined East India Companies. Their factory is removed from Chusan on the Coast of China, to Pulo Condore, whose latitude he tells me is 8° 41′ north: he has received a 3-foot quadrant I saw made for him, with which I hope he will attempt the rectification of the southern fixed stars: he has another quadrant of half that radius with him, and a clock or two, for that purpose. I shall write to him in a week's time to encourage the work, which will help to complete mine. I fear I tire you with insignificant accounts: I beg your pardon for it: when I have better things to impart, expect them, and assure yourself of them.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 75.) *Diary of Events*, 1704—1713.

1704, *Nov.* 8. Wrote an estimate of the number of pages that my books of observations, and catalogues, when printed, might be comprehended in; and by J. Hodgson imparted it to the Royal Society. At one of their meetings it was agreed that it was fit it should be recommended to the Prince, who was chosen into the Royal Society Novem. 30th following. Had it presented to him some time after, and gave them hopes he would undergo the charge, and bear the expense of printing it. I had intimation of his intent some months before; but no opportunity offered of attending him till———.

1704-5, *Jan.* 3. Waited on Mr. Newton, who told me he had not yet made his report to the Prince. Desired copies of some papers, which I sent him January 6th, with the following letter, by T. Weston *.

N.B. The report here mentioned was made (signed by Mr. Newton, Mr. Roberts, and the other gentlemen concerned) before Mr. Newton went to Cambridge: and I saw it not till his return.

Feb. 28. I was at London: met with Mr. Newton accidentally at Garraway's: talked with him about the printing, and an honorable recompense for my pains, and £2000 expense: had been with Sir C. Wren, the Monday before.

	£.	s.	d.
March 3. At London met with Mr. Newton: staid at his house till ½ past 3, in expectation. Had a letter from him at my return, directing me to meet him at the Castle Tavern, in Paternoster Row, on Monday the 5th. Expense . .	0	3	0
March 5. Met the gentlemen at the tavern: saw the specimens printed: discoursed of calculators: Mr. Churchill not there. Expense	0	5	4
March 7. Went to London: delivered specimens of each part of the book of observations to Mr. Barber, to print: met Mr. Newton at Garraway's: told him of it. Expense	0	3	6
March 12. T. Weston began to copy the observations of distances: and Mr. Witty, afternoon, Mr. Gascoigne's observations. God gives us success. Same day received Mr. Sharp's answers, signifying that he would afford me his assistance: 'tis dated March 9, 1704-5.			
March 19. At London to inquire about Barber's specimens: will not be ready till Wednesday: got Hevelius's Uranography of Mr. Joy. Expense . . .	0	1	6
March 21. Went to London to see the specimens: found them not done: Mr. Ashton told me he dined with Mr. Newton, Mr. Roberts, Drs. Arbuthnot and Gregory, at Mr. Churchill's, when we met at the Castle Tavern, March 5th last, after I was gone thence: and that all things, as he thought, were agreed but paper. Expense in coach and water passages, 2*s.* 6*d.*; myself at coffee-house, &c., staying for information of people, about 8*d.*; in all	0	3	3

I had mislaid Mr. Newton's first letter to me, on the occasion of printing my works. I have ordered it, and some of Mr. Newton's, to be hereunder copied, to preserve them: the rest shall be entered, God willing, as they arrive; if they be of any use, or proper to be preserved. [I have found it since †.]

* This letter is dated January 5, 1704-5; and is given in the Appendix, No. 83. F. B.

† This appears to have been subsequently added, as the ink is of a different color. F. B.

	£.	s.	d.
March 24. Went to London: the specimens not done. Expense, waterage 7*d.*, horse 6*d.*, coffee 6*d.*	0	1	7
1705, *March* 28. Again there: specimens done, corrected, &c., wholly: next morning sent them back to be printed: Mr. Smith there, and another. Gave the printers 1*s.*, coffee 1*s.*, horse 6*d.*	0	2	6
The printer's business to oblige the stationers: fear to offend them.			
March 27. Lent Thomas Weston, to be accounted for; and before . .	4	4	0
March 30. In the evening Mr. Witty came to serve me.			
April 3. At London: waited on Sir C. Wren and Mr. Roberts: received the 2nd specimen. Expense in waterage 1*s.*, horse 6*d.*, coffee 4*d.* . . .	0	1	10
April 5. Lent Thomas Weston, more, 1½ guinea*	1	12	3
April 21. At London: waited on Sir I. Newton. Expense, fares 1*s.*, spent 1*s.* 6*d.*	0	2	6
April 24. Wrote to Mr. Sharp that I would send him work next week. Paper, 1 ream, Dutch demy for copy, &c.	0	8	6
May 2. Sent Mr. Sharp, Argol's Ephemerides; my manuscript lunar tables; his copy of my book of distances; observations of the distances of Saturn, Jupiter, Mars, Venus, and Mercury, taken in the years 1676—1680, to derive the planets' places from them; a catalogue of fixed stars; and Dr. Cheyne's book, per Holdsworth the carrier. My work stands, by reason of Sir I. Newton's absence: pray God it may go on at his return: made two or three fruitless journeys to London about it: 50 sheets copied for the press by T. W.: Mr. Witty goes on well with his calculations. *Laus Deo.*			
May 24. At London: waited on Sir I. Newton: acquainted him with the progress of my work: desired him to draw the Prince's money: proposed Mr. Koffman to Mr. Chelwin. Expense in fares, by water and coach hire, &c. . . .	0	2	6
May 30. Ordered J. Hudson to repeat my instances; and wrote to Mr. Sharp: sent him observations of Saturn and Jupiter, from 1680 to 1689.			
June 1. At London with Sir I. Newton. Expense, coach 2*s.*, fare 6*d.*, expenses 1*s.*	0	3	6
June 11. Dined with Sir I. Newton: agreed with Mr. Churchill at 34*s.* per sheet: Mr. Roberts and Dr. Gregory there: I dissented. Expense, this day, in all .	0	4	5
June 13. At Garraway's saw Sir I. Newton: gave him a note that the undertaker was to have no interest in the copy, nor any printed copies: that he ought to give £1000 security to print no more than the 400 agreed on: and that he will not change the paper: that care ought to be taken that the printed copies may be put into such hands that my executors may come by them without trouble, in case of my mortality, or Mr. Newton's, or of other accidents.			
June 15. Sir I. Newton came down: saw my copy: brought Mr. Churchill and Bowyer with him: not a word of any recompense for 30 years' pains, and extraordinary expense, though occasion enough offered him to speak of it, and told it would be for the Committee's honor to provide for that first. God send a good issue to this business, and me peace.			
June 20 and 22. At Sir I. Newton's. Coach fares and horse . . .	0	5	0
June 27. Again met him at Garraway's: he has drawn no monies.			
July 1. Paid Mr. Whitty £5: for washing, 7*s.* 6*d.*	5	7	6

* A guinea, at that time, was worth £1. 1*s.* 6*d.* F. B.

	£.	s.	d.
July 4. Waited on Sir I. Newton: had acquainted him with my going into Surrey, by letter, the day before: he told me that Dr. Arbuthnot's daughter was ill: the Doctor could do nothing till her recovery: that it was not fit we should begin to print till we had received His Royal Highness's monies: would be soon enough at my return, &c. Expense	0	2	8
I was in Surrey from July 16th to August 22nd, where I carried on the lunar calculations, and the copy of the second volume. But T. Weston was taken with fits: cost a guinea in advice and physic. After my return, being ill, went not to London, but on my own particular and extraordinary occasions.			
August 28. Being ill, sent T. Weston to London, with a letter to J. H. to wait on Sir I. Newton. Paid him then	5	0	0
Last April, and before	5	12	3
October 2. Paid Mr. Witty then, for a quarter due £5, for washing 7*s.* 6*d.* .	5	7	6
October 12. Met Mr. Roberts, Sir C. Wren, and Sir I. Newton, at Sir C. Wren's office: showed my paper of Articles: 'twas laid by: Sir I. Newton would like nothing I proposed, though he could not say it was unreasonable: drew up another paper: appointed another meeting on the 18th following: which (Sir C. Wren being to be absent then) was put off till Monday following.			
October 22, when we met: read over all the Articles very * : I did not assent to many of them: much talk, little done: in the mean time sent to the Prince, by Mr. L., that I would throw myself on God's providence and his favor: had a favorable answer from Mr. L. next morning: saw Mr. Barterman, by him gave thanks to Mr. L. I had seen Raymer and Mr. Aston before: went in the coach with Mr. Churchill from Sir C. Wren's to the Temple.			
October 25. Sent two sheets and a title for the press: God gives us happy proceeding: wrote to Sir Isaac for my Ptolemy: sent him a letter, and for the maps of Orion, Aquarius, and Serpentarius: reasons for placing the beginning of the *Historia Celestis* at the Observatory: about the titles of the book, and that I doubt not of God's providence.			
October 25. Paid T. Weston, more, 2 guineas	2	3	0
Novem. 17. For a ream of Dutch demy paper, for copy, &c. . . .	0	14	0
For a ream of lesser fine paper, for calculation, &c.	0	11	0
Sent Mr. Sharp, in part of payment, and returned per Mr. Knap, in Basinghall-street, ten guineas	10	15	0
Decem. 27. Paid Thomas Weston, more	0	10	0
1705-6. *Jany.* 2. Paid Mr. Witty, for a quarter more, now due . .	5	7	6
Jany. 8. Paid Thomas Weston, more	1	1	6
Sent Mr. Sharp, last summer, Hayes's Fluxions	1	0	0
Chayne's Mathem:	0	5	0
Ditton's Laws of Motion . . .	0	5	0
Ditton's Fluxions	0	5	0
1706, *April* 4. Paid T. Weston, now 10*s.* 0*d.*, before 10*s.* 0*d.* . .	1	0	0
Paid T. Witty, 5 guineas	5	7	6
May 10. Paid T. Witty (and dismissed him) for one quarter . . .	5	0	0

* A word is wanting here in the original. F. B.

	£.	s.	d.
Given him more, at several times, last year	2	0	0
A ream of paper, more	0	11	0
July 1. Paid Mr. Barber, for printing the specimens	2	0	0
July 4. Mr. Witty, in full, for 5 quarters, at £30 per annum	16	0	0
	£66	2	3*
Mr. Witty's board, 5 quarters	37	10	0
Mr. Weston's	37	10	0
Mr. Weston	7	10	0
	£148	12	0

In these accounts I have charged nothing for my attendance on the Referees; though my expense was not small: nor for my own and my servants' charges in going often to correct the press: nor for my entertainments, which I have not mentioned (but Sir I. Newton mentions his, and aggravates them; though his were very few), nor gratuity to the compositor and pressmen; nor expenses in designing the charts of the constellations.

[Then follows another account, of the same kind, but made out in a different manner, evidently with differently coloured ink, and to a *subsequent date*, as under. F. B.]

	£.	s.	d.
Paid Mr. John Witty, for 5 quarters' service, ending Midsummer, 1706	37	10	0
To Mr. Weston, the same	25	0	0
For printing specimens, to Barber	2	0	0
Paper for copying and calculation, and letters to Mr. Sharp	2	10	0
Paid Mr. Sharp in money and books sent	16	0	0
Owing to Mr. Sharp, still	15	0	0
Mr. Witty's board, 5 quarters, and washing	37	10	0
Mr. Weston's do. do.	37	10	0
	£173	0	0

A copy of this bill was given to the Referees, Sir I. Newton and Dr. Arbuthnot, with about £13 more charged in it. They ordered £125 to be paid me, March 20, 1707-8; which I received about two months after. So there is still due to me, at least, £48 10*s*.

1705-6, *Feb.* 23. J. Hudson acquainted me that Sir I. Newton had shown him 3 or 4 pages of errata in my manuscript copy of the observations.

Feb. 25. I went to Sir I. Newton, who told me first, that he and Dr. Gregory desired to be informed better concerning my way of observing, very civilly; and showed me the papers. J. Hudson was not yet come thither. Dr. Gregory came soon after me. We set to examine them: I found none material. The Doctor had made a table for turning the revolves of the screw into degrees, minutes, and seconds, by considering my parts equipollent, and comparing them with the revolves and cents: but, supposing them more equal than they were, had erred in it, and made a great

* It is evident, from this sum total, that Flamsteed has not included the small expenses enumerated in the Diary. I would here remark that the dates of April, May, and July are here placed out of the chronological order of events afterwards recorded: but it is evident they have been subsequently added by Flamsteed; and the Diary commences again in another part of the book, with the date of Feby. 23, 1705-6. F. B.

number of faults where there were really none. Some slight mistakes in my copy were found; but none that could be of any consequence. I dined with Sir I. Newton. The Doctor and J. H. came home that night. Caused my own table to be copied out next day: and with the 2nd book of observations, and as much of the 3rd as reached Sept. 10, 1689, left it with J. H. on the 26th, to be delivered to Sir I. Newton, that the Dr. might continue his examination, without danger of the like mistakes hereafter.

March 1. Examined their pretended faults: answered them together; and wrote a paper (that shows what care I have taken to prevent error; and that probably none, committed in the measures taken with the sextant, can corrupt the catalogue) to be made use of hereafter.

March 4. Wrote a letter to J. H.,* directing him to call for the Articles, take care of my MSS, and told him I would put the imperfect copy of the catalogue into the President's hands of the Royal Society, Sir C. Wren's, Mr. Roberts, and his, to be printed (or a more complete copy in its room) as soon as the two first volumes of observations were printed off; provided I might receive monies to pay my calculators and amanuensis, at the same time that I delivered [it]; and that it might be kept sealed up till all the observations were printed. I showed him also a correction of the errors found in the copy by Dr. Gregory and Sir Isaac; and that they were of no moment.

March 6. James was here again: offered to send up the catalogue by him, but he could not take it by reason of the ill weather.

March 8. Was at Sir C. Wren's: left the catalogue with him for James, to be sealed up, and delivered to Sir I. Newton, when 10 sheets are printed, and the £125 paid: complained of Sir I. Newton's dilatoriness: he seemed much concerned.

March 15. Met Dr. Bentley at Garraway's: Sir I. Newton was there: we discoursed first about Dr. Plume's Astronomical Professorship: the Doctor would have had my hand to a paper for the election of Mr. Cotes to be professor: I refused till I saw him: he told me Mr. Whiston and Mr. Cotes should wait on me next week: then we began to discourse of my press business: Sir Isaac told me he heard I had left the catalogue in London: I told him I had left it in Sir C. Wren's house, to be delivered to James: he seemed nettled, as if I would not trust him with it: but, on my telling him that James was to seal it up, and leave it in his hands, he recalled his threats, and told me then he would take it into his keeping, and receive £800 of the Prince's money; but not a word of paying me for my amanuensis and calculators†.

James was come to Greenwich, with Dr. Burnett's son: I wrote a letter to order him to seal up the catalogue, and leave it with Sir I. Newton; which he tells me he did, and a meeting is appointed on Saturday next, the 23rd instant. God give us good success.

March 23. Went with Sir C. Wren to Sir I. Newton's: met Dr. Arbuthnot, Dr. Gregory, and Mr. Churchill there: they agreed to draw the Prince's money: Sir Isaac asked me if things went not now to my content: I returned that it was strange that I should be so little taken notice of, who was the person mainly concerned: at which he seemed chagrined. Before we parted he asked me what the first copy cost me transcribing: I answered, I could not tell, for that was not the whole work of my amanuensis, but he was to prepare and copy work for Mr. Sharp and Mr. Witty besides.

* The person, so frequently alluded to by these initials and sometimes designated as *James*, was Mr. James Hodgson, who married his niece. F. B.

† In the margin of the MS book there is an account of the expenditure of sundry small sums, amounting to £1. 14*s.* 5*d.*, which I have not thought it worth while to transcribe. F. B.

1706, *March* 26. Dr. Gregory came to see me: I showed him Mr. Witty's work, Mr. Sharp's papers, and some other things; he came *tanquam explorator*: inquired what I had done in order to framing statutes for Dr. Plume's professorship: I said I had thought of something; particularly that no one should enjoy it above 10 or 15 years, and for his employment: he urged me to talk more on this head: I forbore.

April 4. At London: hear that all the mistaken errors are quitted, and that the first sheets will go to the press this week.

April 18. Mr. Hudson here told me, if I would go up, Sir I. Newton would go to the Prince's treasurer with me; urged me much: I went on the 19th *mane*: Sir Isaac was very grave: told me that, the Prince having subscribed a great sum to the Emperor's loan, the whole money could not be received: that he had taken up monies for Mr. Churchill: would say nothing, when I asked if he had taken up also to pay me for my calculators; but that he must give bond to Mr. Churchill: I told him he had my catalogue and papers in his hands: he answered slightingly, that the catalogue was imperfect, which he knew when he received it sealed up, and was contented with it: I desired my MSS back, to correct the faults of the press: he told me we must go on slowly at first, quicker after, that in a few weeks he would return my MSS: Dr. Grey is at Oxford; suppose will not return till after term time: he must be paid for the needless collations, and they cannot be finished till his return: all this insincere practice I must bear, so long as God thinks fit: may his goodness deliver me speedily.

May 16. The first sheet is wrought off: was promised a second, but it was not ready.

May 22. No second sheet ready yet: promised on Thursday, but not ready.

Copy of my letter to Messrs. A. and J. Churchill.

The Observatory, Friday, May 24, 1706.

Sir,

By the covenant with Sir I. Newton, you are to print 5 sheets, per week, of the *Historia Celestis*. It is some weeks since the first sheet was wrought off: and though I have been several times in London, and sent often to Mr. Mathews for the proof of the second, I have not yet received it. I wish, for your own interest, as well as mine, you would inquire into the cause of this neglect, and inform me of it; that care may be taken to prevent the like for the future; and that you may make good your agreement: whereby you will oblige, Sir, your humble servant,

John Flamsteed, M.R.

P. S. 'Tis three weeks since the first sheet was wrought off, if I am truly informed.

This letter was not sent till Monday, May 27.

May 28. Was at London: corrected the 2nd sheet: gave orders to have all the notes put in Italics: and to keep a space betwixt the constellations of about an inch, always for the future.

June 3. The proofs of the second sheet are not come down, though promised on Saturday night: gave the following note to J. Hodgson.*

* In the margin of the MS book there is an account of the expenditure of other small sums, amounting to 18*s.* 10*d.*, which I have not thought it worth while to transcribe. F. B.

Observatory, June 3, 1706.

Mr. Hodgson,

Pray take two proof sheets of every one printed: mark the faults in both, exactly alike as you possibly can: return one to the printer, to be kept for his vindication; and keep the other by you for vindication of yourself; and bring it down hither to be preserved by me, both to vindicate yourself, and, Sir, yours,

John Flamsteed, M.R.

[The following letter, which occurs here without any address, is evidently designed for Messrs. A. and J. Churchill. F. B.]

The Observatory, Thursday, June 7, 1706.

Sirs,

I wrote to you May 24 last, to complain of the slowness of the press: on the 28th I corrected the 2nd sheet, which I am told is wrought off, though there are none of it sent me. This morning I have corrected the 3rd proof. To-morrow, God sparing me health, I intend to call upon you before noon, to take care with you both that the printer may perform his business according to your agreement, and that the first proofs may have fewer faults, and be better corrected by him. I am, Sir, yours,

John Flamsteed, M.R.

June 7. Was with Mr. Churchill: sent for Mr. Mathews, the printer, but he came not, because the sheet was not finished: on Sunday he sent his man Jackson with the 2nd and 3rd sheet B and C, and for proof of the 4th, which was returned by Mr. Hodgson on Monday morning.

June 11. James received the 4th sheet D to correct the second time: returned it by the coach: was at the gate for horseback: found it stock locked: it began to rain, continued violent: I returned home, sent up the corrected sheet by the coachman at $8^h \frac{1}{2}$ *mane* on the 12th, with a letter to J. Hodgson.

June 16. Corrected the 5th sheet E.

June 19. Was at London: corrected the 6th, F: met Mons. Fatio: and on the next day (Thursday) sent him Sir I. Warden's account of the Aurora Borealis on Nov. 29, 1681, of Mr. Sharp on March 24th last, and of the eclipse at Bern, on May 1, 1706.

June 27. Corrected the 8th sheet, with proof of the 9th brought by Isaac: returned it by Mr. Hudson on 28th *mane*.

July 1. First proof of K.

July 3. First of L: 2nd of K passed.

July 19. At London: waited on Sir I. Newton about printing 100 or 150 more copies: represented that I thought it needless, contrary to our agreement, &c.: he seemed to assent, and that we should go on, on the old foot: I suggested that it was probable Mr. Churchill had caused more to be printed than he ought, by 200: that if any besides myself had copies to sell, I should not make anything of mine: he agreed that nobody but I ought to have any copy to sell; and that, as I desired, the plates should be put into my hands, that I might cause them to be engraved and drawn off: promised to pay me £100, and I to send J. Hudson to him, to inform him about the Prince's treasurer: promised to wait on him next week.

1707, *April* 11. Was at London: met the Referees at the Castle Tavern in Paternoster Row: carried Mr. Witty and Mr. Weston there: showed good fair receipts: Sir I. Newton was perverse,

yet promised to come down to Greenwich: though J. Hudson says it was concluded otherwise: Mr. Churchill there with Mr. Mathews: this meeting hindered my journey to Burstow, &c.*

April 15. Sir I. Newton came down with Dr. Gregory: viewed the second volume and catalogue: declared they would stop the press, and pay me nothing, till they had *both* in their hands: I dined with them afterwards: we parted quietly, I cannot say very friendly: they had seen the bill of my disbursements the Friday before.

1711, *March* 29, *mane*. Mr. Clark came to me, about 11 o'clock, at Garraway's; Mr. Hudson soon after; and at last Dr. Arbuthnot. I began the discourse with him: told him it could not be conceived by any reasonable man, that, after I had spent 40 years in my observations, and had bestowed a large sum of money (more than my appointments) in making my instruments, hiring assistants and calculators, and other help at my own charge, I did it only to have the work lie by me in my study, till my death, and be spoiled by booksellers, engravers and ill workmen or ignorant. Afterwards I discoursed with him from some notes I had drawn up; and added, I was not only willing, but desirous, to publish them. I shall cause the notes to be transcribed hereunder.

He urged to have the catalogue made up: I told him I was willing: that Sir I. Newton had *two* imperfect copies in his hands: I desired the latter might be returned, to save me the pains of transcribing, and I would fill it up with all I had finished: but that the variations of Right Ascension were wanting in those stars that were within 30° of the pole, by reason these variations altered so enormously, the tables I used would not seem to find them so exactly as in those above 30° from it: he seemed satisfied: I desired him to come down to see the work, which he neither consented to, nor seemed to refuse: I inquired of him, whether the catalogue were printing or not: he assured me "*not a sheet of it was printed;*" † though I am assured by others that some sheets are wrought off; I desired that if it were to be printed, I might have the last proof sheets sent to me, to be examined and corrected: he stuck at this; but promised (and *pronounced it with great earnestness*) he would give me £10 for every error or fault from my copy, that should be shown him in the press-work: I presented him with my printed estimate, and written copy of my letter to Sir C. Wren, occasioned by Sir I. Newton's cunning order, or agreement: he said I had spoken ill of Sir I. Newton, and particularly in a paper he had seen in Mr. Harley's hand, that I had charged him with having *embezzled* £500 of the Prince's money: I know of no paper put into Mr. Harley's hands, but the aforementioned copy of my letter to Sir C. Wren (in which I am sure there is no such thing), and my estimate that was wrote before the Prince designed to print my works: I had said to Dr. Gastell indeed that I had heard the Prince had assigned £1200 for it, but that only about £300 had been bestowed: what was become of the rest, I knew not; and I told Dr. Arbuthnot that I do not remember that I said he ought to account for the remaining £900: the Doctor returned, that it was the same thing as if I had said he had embezzled it: Mr. Hudson and Mr. Clark, laughing, said "No surely:" the Doctor held his tongue: the Doctor then said the Prince had assigned £1100: that it had cost Sir I. Newton £100 *in feasts*: I smiled at this: he ceased. after this, I told him I was very desirous to proceed, *provided that I might have just, honorable, equitable and civil usage*: which he assured me I should, and added, he would recommend the remaining work to the Queen, and doubted not of a reward: I returned, I would not have him throw

* The Diary here seems to have been discontinued for some time; as we meet with only this and the following entry, between July 1706 and March 1711. F. B.

† See the last of the notes in page 94. F. B.

such chaff before me: Mr. Clark [said] that I was no covetous person: I desired to know (if I should let them have the catalogue) *whether they would demand or desire any more of me:* he answered "No, they would be fully satisfied:" I told him this was well, for I had by me 1000 observed places of the moon, and as many of the other planets, which I was glad I could now dispose of as I pleased, or to that purpose; and that I now saw they understood not my business: I added that the *neglect* of me, and the *ill-usage* I had met with, was a dishonor to the Queen and the nation, and *would cause just reflections on the authors of it in future times:* which he seemed not to regard: I insisted much upon it: he answered with some suggestions of fair reward: I called them chaff, and desired him to spare them.

This is the sum of what passed at this meeting: I cannot say all was spoke in the same order I write it; but I am sure that the sum of what was said was what I have related.

I had forgot to insert that I showed Dr. Arbuthnot the errata of the printed second book of observations betwixt 1676 and 1689; and told him that Mr. Mathews was a clumsy printer. That I also showed him the chart of Orion: told him we began at the wrong end of the work: that these [the maps] ought to have been first set upon and finished: that these would be most taken notice of by all ingenious people: and it would be the Queen's, the nation's, and the deceased Prince's honor to have these done sumptuously: a great dishonor to them if they were not done as well as possibly they could be got: he said little, to the purpose, to it; *perparum prospicit nec intelligit, &c.* I mentioned, moreover, the letter I had received from Mr. Secretary St. John, about the visitation of the Royal Society, and purchasing my instruments: he told me "Yes, I should be paid for them all;" and he would pay me himself whatever it cost me transcribing the remaining part of the catalogue. As for my instruments, I answered, I would not sell them: this stopt him short: he talked of the Royal Society, and said there was never such a place as mine designed, but visitors were appointed. I answered, that when my place was settled, there were no people fit to make visitors of, or that understood my business, but myself. He answered, it would be good for me, however, to admit the Royal Society, that I might have my house repaired, and new instruments made, and money for my old [ones]. I understood the snare, and told him I wanted none: and Mr. Clark said, freely, it was known I was not covetous. I see Sir I. Newton's designs. We had a pretty deal of talk about getting a transcriber: I mentioned Mr. Ryley. Mr. Hudson says the Doctor promised that, on the delivery of the constellations, my manuscripts should be returned. I mentioned Dr. Halley's meddling temper to him: he would take no notice of it*.

April 11. I was at London.

April 12. Wrote to Dr. Arbuthnot.

April 13. Went to see him at ½ past 11, and again about 4: missed of him: dined with Lord Pembrook: came home the 15th. In the mean time he had sent me the following letter [April 6, 1711]†, with the latter half of my copy.

April 18 and 19. Was at London at Mr. More's. On the 19th, wrote the following letter [April 19, 1711]† to Dr. Arbuthnot.

October 19. As I was going out of my doors for London, Mr. Hunt came to them, and entering, brought me four *Philosophical Transactions;* and told me I was required to meet a Committee of the Council of the Royal Society, on Friday the 26th, at 12 o'clock, at their house in Crane Court,

* The whole of these minutes, relative to this interview, were written in the book by Flamsteed on the following morning. F. B.

† These letters are inserted in this Appendix, Nos. 155 and 158. F. B.

Fleet Street. I answered that I was lame, and not well: but, God sparing me health, I knew of nothing to hinder me from meeting them.

October 26. Accordingly I went thither with no other company but my servant J. C. [J. Crosthwait?] Dr. Halley met me as I entered, and would have had me drink a dish of coffee with him. I refused: went straight up to the house: my man helped me up stairs, where I found Sir I. Newton, Dr. Sloane, and Dr. Mead. These three were all the Committee that I found there: and the two last, I well knew, were the assertors of the first, in all cases, right or wrong.

After a little pause, Sir I. Newton began; and told me that the Committee desired to know what repairs I wanted, or what instruments in the Observatory? I answered that my repairs were always made by the Office of the Ordnance: that I had applied myself to them; but the season of the year not being fit, it was thought best to forbear them till February next, when I doubted not they would be taken care of. As for the instruments, they were all my own; being either given to me absolutely by Sir Jonas Moore, or made and paid for out of my own pocket. This he well knows, though he dissembles it. He answered, "As good have no Observatory as no instruments." I gave him, hereupon, an account of Sir Jonas Moore's donation, in the presence of Mr. Colwall and Mr. Hanway his son-in-law: how he soon after died, and a controversy about his gift arising betwixt his son Sir Jonas, and myself, we had a hearing before the Board of the Office; whereat Mr. Colwall and Mr. Hanway both attested what I affirmed, that the instruments, books, goods, &c. were given me by Sir Jonas Moore. Whereupon he seemed much moved; and repeated what he had said before, "As good have no Observatory as no instruments;" asked Dr. Mead if it were not so, who assented. I proceeded from this to tell Sir Isaac (who was fired) that I thought it the business of their Society to encourage my labors, and not to make me uneasy for them. He asked Dr. Sloane what I said: who answered, that I said something about encouragement. Whereupon I told him that a frontispiece was engraved for my works, and the Prince's picture (without any notice given me of it), to present to the Queen: and that hereby *I was robbed of the fruits of my labors*: that I had expended above £2000 in instruments and assistance. At this, the impetuous man grew outrageous; and said, "We are, then, robbers of your labors?" I answered, I was sorry that they owned themselves to be so. After which, all he said was in a rage: he called me many hard names; *puppy* was the most innocent of them. I told him only that I had all imaginable deference and respect for Her Majesty's order, for the honor of the nation, &c.: but that it was a dishonor to the nation, Her Majesty, and that Society (nay, to the President himself), to use me so. At last, he charged me, with great violence (and repeated it), not to remove any instruments out of the Observatory: for I had told him before that, if I was turned out of the Observatory, I would carry away the sextant with me. I only desired him to keep his temper, restrain his passion, and thanked him as often as he gave me ill names: and, looking for the door, told him God had blessed all my endeavors hitherto, and that he would protect me for the future: that the wisdom of God was beyond the wisdom of men; and that I committed my all to Him: or words to that purpose.

I cannot remember everything that was said by the hot gentleman, in its proper place; nor have I given it in its order. I may put it into better, upon recollection, hereafter. I remember more at present, that after I had said that it had cost me above £2000 in instruments and assistants, he told me fiercely that I had said he owed me £6000: which, without much moving, he set himself to make out thus: first, I had said that nobody could live in the Observatory for less than £300 a-year; that I had had but £100 paid me, and that £200 a-year in 36 years would come to that money. This I never reckoned; but I have said that a man cannot live in this place for less than £300

a-year: the rest is his own accounting. He told me, moreover, I had received £3600 of the Government. I answered, what had he done for £500 a-year salary that he had, or to that purpose? Which put him to a stand: but, at length, he fell to give me his usual *good* words: said I was proud and insolent, and insulted him. Dr. Mead said the same thing. I only desired him (as I had often done) to restrain his passion, keep his temper, &c. He said I had called him *Atheist*. I never did: but I know what other people have said of a paragraph in his *Optics;* which probably occasioned this suggestion. I thought it not worth my while to say anything in answer to this reproach. I hope he is none.

1712, *June* 13. I dined with the Lord Mayor at Greenwich. Mr. Stubbs told me Dr. Halley would visit me the Monday following: but he came not.

June 18. Dr. Halley came; and brought his wife, son, and daughter with him. Asked if I wanted preferment (a snare!): said he would burn his copy of my catalogue, if I would print my own. N.B. It was now in the press again, and the second sheet printing. I told him of his blockish fault in his charts. He answered, he was a young man when he did them: upon which account they might be excused, so [that] they were right on the backside of the paper; with much other impudent banter. Mr. Stubbs endeavored to excuse him: but, being told he was ignorant of the business, gave over; having nothing to say to the purpose.

Decem. 5. The 18th last sheet of my catalogue printed off.

1713, *August* 1, *Saturday*. Sir Isaac Newton having, as I was told, presented his book of *Principia*, new printed, to the Queen, came to Greenwich, attended by Dr. Thorp, Dr. Halley, and his sons, Mr. Machin and Mr. Rowley. Mr. Hudson was with them, who had given me an intimation of it, the night before. But I had a letter of advice of it, directly from Mr. Machin. Sir I. Newton came first, about 3 o'clock; the others, half an hour after. Sir I. Newton said little till they entered; then he rose up and told me that by a Royal Order, by word of mouth, they were come down to visit the Observatory; to see what repairs were wanting, and what instruments. I gave them leave to go where they pleased, and sent my servant to wait on them, and show them all the places where repairs were wanting: and Mr. Clark and Mr. Ryley (whom I had sent for, on purpose to be witnesses of all that passed) accompanied them. I kept in my chamber: for I could not walk about with them. But, before they went out, I told them that the cogs in the greater semicircle were much worn; and that the instrument, for several reasons, was not very serviceable. And because Sir I. Newton had asked how we could observe a comet without it, I told him I could easily observe any comet that was visible in any part of the heavens, by a particular method that I knew of; but it was not now a time to talk of it; and that that instrument was my own. My friends and servants remember all that passed: I trouble not myself to report it. At parting, Sir I. Newton told me he had a Ptolemy of mine, and the minutes or night-notes of my observations, which he would return. I was glad to hear it; and told him I would retain his receipt for them. I pray God he be as good as his word.

April 6. Met Mr. Pound in London; and at Garraway's Dr. Sloane joined us. Mr. Pound went to dine with them at Pontack's.

[Copied from the *original* entries, in MSS vol. 33, pages 47, 51, 52, 53, 54, 55, 59, 62, 220, 221, 223, 89, 90, 91, 104, 105, 106, 107.]

No. 76.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Decem. 5, 1704.

I have yours of the 25th past, and hopes that the Prince may perform what I told you in my last: but Courts move slowly; my habitation is a great way off from St. James's; my health permits me not to be in London; I want one necessary accomplishment for a person that would carry on his honest endeavours at Court, and make an interest there; that is (not to call it by its more proper name) *assurance*. I hope nevertheless something may be done in time: but those are solicitors for it that, I fear, mind their own interests more than those of the public, and seek hence an occasion of promoting them by having frequent opportunities of discoursing the Prince on this affair. I have no ways to hinder them from obstructing hereby, but taking care of my health at home, and letting them go on till my attendance be necessary and unavoidable; by which time a sincere friend that has a real interest with his Royal Highness may be again in England (he has been in Denmark and Poland, and is now in Holland waiting a safe passage home), and then through God's blessing (who has hitherto conducted all my affairs) I hope the business will proceed easily and effectually. My servant is (I praise God for it) recovering; but a great deal of help to calculate and copy will be wanting if the work goes on: and I have hopes of yours, and though perhaps your circumstances may not permit you to leave Yorkshire, yet I may find employment for your leisure hours even there, and to your advantage.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 77.) *Unfinished draft of a petition to Prince George of Denmark.*

If it shall please his Royal Highness, in order to promote the improvement of navigation, and the full knowledge, by affording the expenses necessary for printing the *Historia Britannica Cœlestis*, with a large catalogue of the fixed stars, charts of the constellations, and other deductions from them mentioned in another paper, then Messrs. Gascoigne's and Crabtree's observations, with my own, from 1668 to 1689, being all fair wrote and in good order in Latin, may be immediately copied and put into the press. Whilst these are printing off, a skilful hand or two may copy the observations from 1689 to 1700 complete, examine, and compare them with the first night notes, whereby any errors committed for want of assistance in that laborious work may be corrected. In the mean time the large plates of the constellations may be begun to be designed on copper, or rather brass plates, which work cannot be done but at the Observatory: that so it may be continually under the inspection and care of the author, and the engravers prevented from committing such intolerable errors and faults as are everywhere to be found in works of this nature, when they have not been under the eye of the first designer.

The catalogue of the fixed stars, in which two or three constellations are not completed, may in the mean time be assorted, and printed off before the 50 plates can be wrought.

If a couple of assistants to help to calculate them be allowed, and added to my present servant, with two more to copy the papers for the press: the skilful assistants ought also to be employed in calculating the places of the moon, from the observations taken with sextant betwixt 1676 and 1689, which are very troublesome to manage, and require more than ordinary sagacity in a calculator. More places of the other planets ought also to be deduced from the said observations,

and may be done with safety and certainty now we have a correct catalogue of the fixed stars, but could not before for want of one.

The work of making a catalogue is so great, and the necessary watchings by night and day endangering the health, the calculations (to be made) so troublesome and laborious, that I am persuaded posterity will not attempt the like again, and his Highness will have the sole honor of obliging posterity with [an] useful work, and not be———

[Copied from the *original* in MSS, vol. 35, page 31.]

No. 78.) *Letter from Sir G. Clark to Sir Isaac Newton.*

Dec. 11, 1704.

SIR,

The Prince has perused the estimate of the intended *Historia Cœlestis Britannica*, which you presented him. His Royal Highness is persuaded of Mr. Flamsteed's fitness for a work of this nature, and being unwilling that the observations designed for the benefit of navigation, and encouraged so well in the beginning, should want any necessary assistance to bring them to perfection, he has been pleased to command me to desire yourself, Mr. Roberts, Sir Christopher Wren, Dr. Gregory, Dr. Arbuthnot, and others of your Society, as you think proper, and will share the trouble with you, to inspect Mr. Flamsteed's papers, and consider what is fit for the press: and when His Royal Highness knows your opinions, you may be sure he will do anything that may conduce to the making them of use to the public. I am, Sir, your most humble servant,

GEO. CLARK.

[Extracted from MSS, vol. 33, page 48.]

No. 79.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jermyn Street, December 18, 1704.

MR. FLAMSTEED,

I received last night a letter from the Prince, wherein his Highness expresses that he is unwilling that your observations designed for the benefit of navigation, and encouraged so well in the beginning, should want any necessary assistance to bring them to perfection; and, therefore, desires me, Mr. Roberts, Sir Chr. Wren, and some others of your friends to inspect your papers, and consider what is fit for the press; and when his Highness knows our opinions, he is ready to do anything that may conduce to the making your observations of use to the public. This is the substance of the letter wrote by the Prince's secretary by his Highness's order: and to-morrow Mr. Roberts, Sir Chr. Wren, and the rest of the gentlemen to whom his Royal Highness has referred the inspection of your papers, are to dine with me, in order to consider of this matter, and speak with you about it: and, therefore, I desire the favor of your company at dinner with them; and if you please to come in the morning, and bring your papers with you, or such parts or specimens of them as may be sufficient, you will oblige me and the rest of your friends to whom the inspection

of them is referred, and promote the dispatch of this affair. If you bring the papers themselves, you expedite your business, and you may rest assured that they shall not go out of your hands.

I am, your very loving friend and humble servant,

ISAAC NEWTON.

[Copied from the *original* in MSS, vol. 35, page 37.]

No. 80.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

London, Decem. 26, 1704.

SIR,

I thank you for the information you give me about the charges of printing. I am sorry your servant is ill, but if you do not bring your papers there will be nothing done, for the business of the meeting is to view your papers, according to the Prince's order, and give him an account of them. We had but little time at my house to view those you brought, and did not meet at the coffee-house for that purpose, but appointed to-morrow morning at ten o'clock, that we might have time to view them, and come to a conclusion: and I hope you will not disappoint your friends.

I am, your humble servant,

I^S. NEWTON.

[Copied from the *original* in MSS, vol. 35, page 41.]

No. 81.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Decem. 30, 1704.

Mr. Newton is become exceeding kind of late; was here to visit me yesterday; stayed from 12 till near 5 o'clock; dined with me; took a new view of my books and papers; and becomes solicitor with the Prince on their behalf. I may allow him to do himself the honor, and regard his own interest in it too, since he saves me the labor of attendance and solicitation.

I know him very well; and if he manage it so as may be for the advantage of the work, shall not fail to acknowledge it: but if otherwise, the blame will not be upon me, and I shall be as cautious as I can that he do me no injury. I have no other news to tell you: this finds all the talk for the Royal Society, and I bless the good providence of God that makes those, who were not friendly, to produce this work to the public. I commit all to his disposal, and you to his keeping; being, Sir, ever your affectionate friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 82.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, Jan. 2, 1704-5.

SIR,

Yesterday I sent you, according to your desire and my promise (by Mr. Hodgson), my old charts of Orion, Ophiuchus, Aquarius, and Pisces; those have all the circles of longitude, and

parallels of latitude inscribed in them, as you will find them in the new one of Orion, which I think is completely finished, that by it you may judge of the rest, which I have designed of the same bigness. I have added the new constellations of Aquarius and Ophiuchus, whereby you will see the charts cannot conveniently be made less; but I have not caused the circles of longitude and latitude to be inscribed in these, because I esteemed it needless: the chart of Orion alone serving sufficiently to show how they will fill the copper-plate when they are inscribed.

With these, I have sent you my Greek Ptolemy, and my Latin version: concerning which I must inform you, that because I differ from the common translations, I have thought it necessary to keep as close to the Greek as I handsomely could; and therefore you must carry the *Quæ est* that begins the head or second line of every constellation, to the beginning of every line following it. And further, that if you think it not advisable to print Ptolemy's Greek text, it will be best to put those words, in whose interpretation I differ from others, in the margin; for if they be inserted with a parenthesis, in the text, they will make the line too long.

I have also sent you a Bayer, that by comparing his descriptions and figures with the Greek and mine, you may see how (by taking the meaniug of Greek words from Lexicons) he makes all those stars to lie on the left side, legs, arms, &c., which the original, and all the printed catalogues (except his own), put on the right, and *vice versâ;* and, moreover, often thrusts such Greek words into his text as are no where to be found in Ptolemy.

I would have sent you a scale to the charts, but that I have never a clean one by me; if the ill weather should hinder me from coming to London to-morrow, as I intend (God continuing my health), I shall cause a new one to be drawn: and when a good day affords me an opportunity, you shall be attended with it by, Sir, your obliged and humble servant,

JOHN FLAMSTEED, M.R.

I wish you many happy years.

[Extracted from MSS, vol. 33, page 50.]

No. 83.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, Jan. 5, 1704-5.

SIR,

Herewith my servant will deliver you the papers you desire, if his fit prevents him not, and force him to send them by the penny post; if he reaches the house, pray send me by him that volume of Petruvius that has the fragment of Eudoxus in it: perhaps it may be of use to me. I shall take especial care of it, and return it safe whenever you please. I have hastened these, that the want of them may be no hinderance to your making your report to the Prince, or concluding about the impression of the observations, and catalogue.

No. 1 is a page of the observations of the stars' inter-mutual distances, taken with the sextant: these are such as you conclude cannot be printed double on one page of paper.

No. 2 (the second page on the same sheet), is copied from the book of lunar observations, taken with the sextant: the five last lines of this ought to stand a line or two lower, to make room for titles to stand over the heads of the columns of numbers. I was absent when my servant copied it, but shall take care the like error be not committed, when it shall be copied for the press.

No. 3 is the first page of the observations of the year 1699, and make a part of the second volume;

I chose it, because it appears by the lines I had wrote at the entrance of the year, that I had determined six years ago to transcribe them all thus, as you lately hinted you thought it would be best, and have caused them to be so copied ever since.

No. 4 is an extract of a double page of my catalogue: I have caused my servant to transcribe only a few lines of it, they will be sufficient for a specimen; the rest of the page may safely be conceived filled up in the same manner. I pray the good God who has blest my labors hitherto with success, to bless the endeavor for their publication, send you many happy years, and am, with all due respect, Sir, your obliged humble servant,

JOHN FLAMSTEED, M.R.

[Extracted from MSS, vol. 33, page 51.]

No. 84.) *Report of the Referees on the Estimate for Printing.*

London, Jan. 23, 1704-5.

MAY IT PLEASE YOUR ROYAL HIGHNESS,

According to your Royal Highness's order, signified to us by Mr. Clark's letter of December 11th last, we have inspected Mr. Flamsteed's papers, and are humbly of opinion that *all* the observations which he proposes to be printed in the first and second parts of the work are proper to come abroad, together with his two catalogues of the fixed stars in Latin; all which are ready for the press, and with prefaces will take up about 1200 pages in folio when printed. Which agrees with what he has represented; and the expenses of printing 400 copies, according to the estimate, will be as follows:

	£
For 283 reams of demy paper, of 16½ inches, by 22, at the rate of 20*s.* a ream .	283
For composing and press-work of 300 sheets, at the rate of 20*s.* a sheet . .	300
For the charges of an amanuensis to copy and correct the press, and to compare and examine the papers	100
Total .	£683

It may be also very proper to print the places of the moon and planets, and comets, derived from the observations: of this, six hundred are already computed, and fourteen hundred remain to be computed; and the charges of two calculators to finish them, and of the paper, press-work, and printing, will be about £180: so that the whole charge will be about £863.

This set of observations we repute the fullest and completest that has ever yet been made: and as it tends to the perfection of astronomy and navigation, so, if it should be lost, the loss would be irreparable; and we have no prospect that a work so expensive will ever see the light, unless your Highness will please to be at the charge of publishing it. We are,

May it please your Royal Highness,

Your Highness's most humble and most obedient servants,

Signed by Mr. ROBERTS, Sir CHR. WREN, Mr. NEWTON, &c.

[Extracted from the copy in MSS, vol. 35, page 33.]

No. 85.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

London, March 2, 1705.

MR. FLAMSTEED,

The gentlemen to whom his Royal Highness has referred the care of printing your Observations have agreed to meet on Monday morning, at eleven o'clock, at the Castle Tavern, in Paternoster Row, to set forward the printing thereof; and I desire earnestly that you would be pleased to meet us there at the time appointed, that we may agree with you about an amanuensis and calculators, and what else you have to propose for dispatching the work.

I am your humble servant,

I^S. NEWTON.

[Copied from the *original* in MSS, vol. 35, page 45.]

No. 86.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 6, 1704-5.

SRI,

Yesterday I was at London with Sir C. Wren, Mr. Roberts, Mr. Newton, and some other gentlemen, to view some specimens of a printer's page of my book of Observations: they were ill done. I am causing them to be copied again in order to have them printed by another, if the gentlemen think fit.

You were pleased to offer me your kind assistance in calculation. I shall have an allowance of about £40 per annum each, for two calculators: and I think myself obliged to make you the tender to be one of them. But I cannot expect that you, who have much business on your hands, should spare your whole time for this. I desire therefore that you would consider, 1°, what spare time you have that you can employ in this business; 2°, how many planets' places you can calculate in a week, from the observed distances; 3°, what reward you expect for your pains: that is, whether I must account with you, and pay you so much by the score, or hundred, or whether you would have an allowance proportionable to the time. And then I must acquaint you further,

That I can easily transcribe and send you the observed distances of the moon and planets from fixed stars to a given time; together with the places of the stars in my new catalogue; by the general post as many at once as may be handsomely contained in one single sheet:

That I desire you would calculate them in a book made up in 4to, of 3, 4, 5, or 6 sheets, as you think fit; which when 'tis filled by your calculations, you may send me hither by the carrier:

That whatever you calculate in Yorkshire must be repeated by a calculator here, for certainty: we are all so prone to error in calculations of this nature, that no single hand is to be relied on.

And for the easier discovery of any error, either of yours or my calculators here, I shall desire you would always follow that same method that I use, of which I shall send you an example.

I shall allow you the charge of postage, carriage, and paper; and a printed copy of the books when finished. Your answer to this proposal, as soon as you conveniently can, will oblige,

Sir, your sincere friend and servant,

JOHN FLAMSTEED, M.R.

You see God takes care of my business: I do not very well like the hands by which 'tis managed,

but I doubt not his good Providence will turn all to good, and make me easy, and give me health to finish this work to his glory: may he always preserve yours, J. F.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 87.) *Letter from Mr. Flamsteed to Mr. Bossley.*

The Observatory, March 8, 1704-5.

Sir,

I received a letter from Mr. Leigh a good while ago, to acknowledge the receipt of a relief I sent him to support him in his sickness; but returned him no answer, because I had not what he desired to serve him with. I heartily commiserate his condition, and pray the good God of his mercy to give him ease, that he may enjoy the great comfort of better health in his old age than he has had hitherto.

But prayers are helps that are lightly esteemed: to render them more valuable to him, I have laid by a guinea for him, which, if you pay him, I shall repay on the order, as you please, in London. I would have left it, as usual, with Mr. Crundale, but that I find by the last, you have not such frequent dealings with him as formerly.

I am going to print my works very speedily. Yesterday I delivered specimens of some sheets to a printer, to be composed and printed on several papers, for trial, to see what work he will make, and which paper will best fit. I have retained one amanuensis here, another I have in my house; one that was my servant formerly, living now in Yorkshire, I have retained to calculate for me. Another I want: if Mr. Leigh has health enough to undertake any work, I would employ him; and therefore desire you to let me know how he does, and whether his distemper does not affect his head. I would gladly serve him rather than any other, because he has served me formerly; and I will endeavor (though my allowances are but narrow) to make his pay much better than formerly, when I had none, but disbursed all out of my own income.

I hope now, in good time, to have an opportunity to acknowledge your pains in correcting the motions of Saturn and Jupiter. I shall not arrogate anything of yours to myself; but it will be a pleasure to me to let the world know how much, and without any view of your own advantage, you have obliged, Sir, your real friend and servant,

John Flamsteed, M.R.

I shall want my Vlack's *Canon* home for my amanuensis here: if Mr. Leigh could borrow Mr. Halton's, and return mine, he would oblige me.

[Extracted from MSS, vol. 33, page 53.]

No. 88.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 17, 1704-5.

Sir,

I am glad you accept of the offer I made you in my last, and hope it will be not only to your great satisfaction, but something to your advantage; though not so much as I wish it might. I have gathered five years' observations of the superior planets from my books this week, in order to

be sent to you in convenient time: the moon's will cost me no small pains to collect for you; I shall enter on them, God willing, on Monday next. In the mean time, till they can be ready, I send this, to let you know that I depend on you, and that I hope in a fortnight's time to have them all ready to be copied and transmitted to you.

I hope you have Vlack's *Canon of Sines and Tangents* of your own; if not, pray let me know it, I have one in Derbyshire, which I have sent for up. I think Mr. Hodgson has one by him; one of these shall be sent you, with the copies of the observations, by the carrier, except you know, and can direct some better way.

One hour's thinking and practice will make you as ready at calculation as ever you were. I am only sorry you cannot have my new tables for calculating the moon's motions, in order to find out her semidiameters and parallaxes; they are not contained in less than 50 quarto pages. I have two copies of them by me; I would willingly trust you with one of them (for I know you will not impart them to anybody without my consent), that you might copy them, and have them by you, if you think them not too many and troublesome: they render the calculation easy and sure, and you will have directions with them how to use them.

Perhaps you may find some ingenious youth near you, that for a little money would calculate the mean motions and first equations, so far as to get the mean anomalies, and consequent horizontal parallaxes and semidiameters to the given times of the observations for you; if you can, I will gladly pay the charge to ease the labor, and think myself, moreover, much obliged to you.

I have the book of old calculations for the ☽'s places, in 1679 and 1680, which will be sent you with the lunar tables: it will be of use to you. As also the distances you copied from my book of distances, if you think it may be serviceable.

Though your modesty make you refuse a reward for the pains of calculation, I cannot, in conscience, accept them: nor in gratitude, without making you a valuable one, and such as may become me to afford, and you to accept from, Sir, your friend to serve you,

JOHN FLAMSTEED.

P.S. It will be necessary for you to have ephemerides of the moon's places from the beginning of my observations; if you have none, I will send my Argoll, which will serve your turn as well as Hecker, or better, because he begins with 1671, and continues to 1700 complete. J. F.

[Extracted from MSS, vol. 33, page 54.]

No. 89.) *Letter from Mr. Flamsteed to Mr. J. Hodgson.*

The Observatory, March 22, 1704-5.

SIR,

After you dealt so freely with me yesterday, I believe I may deal as freely with you. I shall always do so: for there is nothing can preserve a man from injuries by deceitful people, but plain, open dealing; and, whatever happens, you may assure yourself *the uprightness of the upright man will preserve him.*

It seems to me that Mr. Barber is in with Mr. Churchill, and acquainted with their management; if he be, I expect he will not send me any specimens, but keep them by him, to show Mr. Newton first. Mr. Churchill will convey them, and accompany him, and he shall be Mr. Churchill's servant.

But if not, he will deliver them to you this morning; you will send them to me at noon; and on Saturday (God willing), I'll come to London again, and wait on Sir Christopher, and Mr. Roberts with them, as Mr. Newton advised me before he went to Cambridge.

I think to be very plain with Mr. Aston, and desire that he, I, and Mr. Churchill may understand one another fully, and know what each shall advantage themselves by my pains; for his and Mr. Churchill's will be little or nothing, but to accept their shares: and this will be no equal bargain for me, that must be at all the labor and trouble here, nor for Mr. Newton, who saves us the labor of soliciting for the Prince's bounty at Court. And, therefore, I think he too ought to be acquainted with what advantage every one of us shall make, and go and share with us. I shall say this to him when he returns from Cambridge.

Mr. Aston said something about printing only 40 or 50 copies for presents, on the best paper, the other on worse; this is to make the bookseller's and partners' gains the greater, and mine the less: and deprive the work of a part of its credit, and the Prince with the undertakers, Mr. Newton, Sir Christopher, Mr. Roberts, and the Drs., of the honor and reputation of a sumptuous and useful book, and must therefore be plainly spoke of and obviated.

I know nothing can prevent the spoiling the book at the press, but this open and plain dealing. I have no other design, but to have it printed so as may be for the honor of the nation, the Queen, Prince, and gentlemen concerned; and to secure myself an honorable reward for my long pains, patience, and great expense, which it will be much for their's and the nation's honor to secure before we go to the press, to, Sir, your real friend, &c.

John Flamsteed, M.R.

P.S. Pray, if you can, step to Sir Christopher Wren's to-night, give him my service, and show him this. I should be glad to meet you at his house on Saturday morning, by 9 o'clock, when I intend to wait on him, if God spare me health. I forgot to tell you above, that I find the booksellers are very averse to printing at Greenwich, though the printers are willing enough. The reason is plain: at London they can print what copies they please more than they agree for, and they will be sure to sell all their's off, before I can sell one of those the Prince allows me; and deprive me of the advantage designed me. Printing at Greenwich would both prevent errors, and such practices.

[Extracted from MSS, vol. 33, page 58.]

No. 90.) *Letter from Mr. Flamsteed to* ———*.

The Observatory, April 5, 1705.

Sir,

Since you went hence, I have got almost 30 sheets of my observations copied for the press: I shall cause the figures belonging to them to be transcribed upon a folded sheet, in order, if you like that method best, to have them engraved; but I desire to discourse with you again about them before you fully resolve.

I have got specimens done by another printer as they ought to be, which shall be left at your house to be ready for you at your return, which, I fear, will be delayed by the Queen's and Prince's coming to Cambridge in Easter week. I am going into Surrey on Saturday next, but hope, God sparing

* Probably addressed to Mr. Hodgson. F. B.

me health, to be back here the Wednesday following; in the mean time, the copies go on, and the calculation work is preparing, that there may be no stop on this account.

I have got some further information concerning the bookseller's and printer's practices. I find the latter dare not disoblige the former; and that the paper stationers are so in with them, that I cannot now learn the prices of paper from them: which, before I had to do with a printer, was no difficulty. I have taken another course to be informed, and therefore desire you not to proceed to any agreements with the bookseller till I have waited on you with Mr. Roberts, Sir Christopher Wren, &c., that we may take the best way to save the Prince's bounty, and make it reach as far as we can; for it will be a terrible reflection on us, if we suffer a bookseller to devour that as his gains, which the Prince designed to employ for the honor of the nation and the Queen. Good success in your affairs: health, and a happy return, is heartily wished you by, Sir, your obliged and humble servant,

JOHN FLAMSTEED, M.R.

[This not sent, he returned too soon.]

[Extracted from MSS, vol. 33, page 60.]

No. 91.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, April 24, 1705.

I intend by the same conveyance to send you, with them, Argoll's Ephemerides I promised you, and Mr. Shene's book; together with a few printed copies of my estimate of the number of pages my works, when printed, will contain: whereby you will see what you are to expect from me, and may inform your friends that are curious of the same. 'Tis the same paper I drew up for the satisfaction of my friends; and which Mr. Hudson, by God's providence, without my order delivering in at the board of the Royal Society, was there read; and occasioned their recommending it to the Prince, by the President and Council; and his taking the charge of printing the two first volumes and catalogue on himself. The maps of the constellations he has not engaged himself in: but these, being the most sumptuous and usefullest part of the work, I doubt not but they will be taken care of in good time: for I am fully persuaded that the good Providence, which has hitherto governed and guided all my endeavours, will produce them all to the public, by easy methods, in his good time. Mr. Newton is knighted; stands for parliament man at Cambridge; and is going down thither, this day or to-morrow, in order to his election. 'Tis something doubtful whether he will succeed or no, by reason he put in too late. I expect him back about a fortnight hence; and, within a month after, we may begin to print, if God spare me life and health. I was with him on Saturday last to wish him joy of his honor; he was more than usually gay and cheerful: but I well perceived the same temper that I had always found under it; and therefore took care to be no more open than formerly. I dealt plainly and sincerely with him as I used to do; and this keeps me always safe: but I take care to inform him no further of my business, than he does me of his, or necessity requires, since he makes such uses of it (when I do) as no deserving man would allow. He will see his error in a short time, and be the firmer friend to the Observatory hereafter. I pray God keep you in peace and health, and am ever your affectionate friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 92.) *Letter from Mr. Caswell to Mr. Flamsteed.*

Holywell, in Oxford, May 8, 1705.

Loving Friend,

I have been negligent in writing to you, not for want of affection, but partly for want of matter, and partly from indisposition of body, which makes me now for the most part listless to those studies which I once much loved; colic pains deprive me from most of that rest which others have in the night, and which refreshes and qualifies them for the work of the succeeding day.

I am glad to hear of the Prince's noble intention to print your works at his own charge, and that you are pleased therewith. I think it an excellent opportunity of preserving your papers to posterity in your own name; for I have formerly thought, with Dr. Wallis, that your manuscripts being upon your decease left to your widow, or some other relation, they would be sold to any one that would give most for them. And in that case they would fall into the French King's hands, for that nobody would bid so high: and then the French astronomers would maim your observations, they would suppress some, and print others of yours in their own names. Mr. Halley and I agree very well, and I have known him begin your health in company (wherein I was) as my friend. He has translated from the Arabic a piece of Apollonius, which is not found in Greek: Dr. Bernard began the translation, but finished only about a tenth part; but that which Dr. Bernard did served as a key to Mr. Halley to go on with the rest, the whole tract being to effect but one proposition with its cases. I intend to go to London some time this ensuing summer if God permit, and then to give you a short visit. Pray let us renew our correspondence; I should be glad to hear of your welfare, as you have occasion to write to

Your affectionate friend and servant,

John Caswell.

[Copied from the *original* in MSS, vol. 35, page 77.]

No. 93.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 21, 1705.

As for my lunar tables, I am in no haste of them, having a copy of them still by me for my own use: so that if you retain them two or three months, it will be no hinderance to me, for I fear 'twill be more than so long time before I shall have occasion to set my servants to calculate places of the moon, for further examining her theory; in which the many equations need not fright you. I have contrived the tables so that the greater are taken out with much more ease than formerly: when you are used to them, you will find you may dispatch a calculation by these almost as soon as by the old tables, and with something more certainty.

Of the two last equations of longitude, the first may be rejected: for the tables always agree better with the heavens, when 'tis omitted, than when 'tis employed: but the other, which is called the parallactic equation, must be retained; but I think 'tis too small.

Some ten years agone Mr. Newton desired me to impart a table of refractions to him, which I had derived from my observations; and within two or three months after, sent me a table, built on a theory that supposed the upper regions of our air were colder than that part of our atmosphere near the

earth, and the refractions consequently greater above and less near our earth; because the rays were bent more back as they approached the earth by reason of the increase of warmth. This I could not allow, because I conceive the refractions of warm water little less than of cold, and the like of warm air: but, what swayed more with me, I considered our air near the earth as a compound of various sorts of vapors, aqueous, saline and others, that rendered it much denser near us than at a distance: and hence concluded that at a small distance, not more than half a mile from the earth, the rays of light began to be more bent by it than they are in the purer upper regions. On this theory I built the table of refractions I sent you; which differs no where above ¼ of a minute from his: and answers all my observed refractions as well. You must lay the faults in my copy of it on my haste and spectacles; and correct them as in the margin.

Mr. Godolphin and Sir Is. Newton are not chosen at Cambridge. I hope the latter is returned*; and that, with God's blessing, we shall begin to print in a short time, having half the copy of my first part in readiness; that is, about 60 sheets transcribed. I pray God continue your health, and am ever, Sir, your affectionate friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 94.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

London, Jermyn Street, June 8, 1705.

Sir,

The gentlemen, to whom the Prince has referred your matters, are to meet at my house on Monday next, at twelve o'clock; and Mr. Churchill being returned to London will be one of the company. Pray do me the favour to meet us at the time appointed, and dine with me, that we may set the press a going as soon as possible. I am your most humble servant,

Is. Newton.

[Copied from the *original* in MSS, vol. 35, page 51.]

No. 95.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, June 12, 1705.

Yours of the 4th arrived yesterday, after my return from London, where I dined with Sir I. Newton, to put the press-work forward: he does not act so openly as I expected and hoped; but I must be content to let things go at present as they will, and wait an opportunity of putting them in better order, as it shall be afforded by that good Providence who has hitherto ordered all my affairs: and I do not fear but He will still direct them for the best.

[Copied from the *original* letter in the possession of Mrs. Giles.]

---

* That is, *returned* to London. F. B.

No. 96.) *Letter from Mr. Flamsteed to Mr. J. Hodgson.*

The Observatory, June 19, 1705.

Mr. HODGSON,

Pray buy me a ream of the same (Genoa demy) paper on which my observations are to be printed. Carry a quire of it to Sir Isaac Newton, with the price of the ream wrote on it. Let him know there is a much worse sort with the same mark: that sometimes three or four different sorts of paper are marked alike, and that I send him this purposely for his information, and to prevent the changing of the paper.

Acquaint him, further, that I think it might be convenient, and both for his advantage and the work's, that the undertaker give bond, both to him and me, jointly for the performance of what covenants he has made or shall make with him; especially that he will not print more than four hundred copies, and that he will deliver them to us, and our executors on demand, for the Prince's use.

Inquire of him if he has yet taken up any of his Royal Highness's money, for I shall want to pay my calculators and an amanuensis, in a fortnight's time.

Desire him to send me my old Ptolemy, with the chart of the constellations of Orion and Pegasus, if he has no use of them; and let him know that if he wants another Ptolemy, I have one by me at his service. Put the charts up into the case, that they may not be rumpled in bringing; and let them not be seen by any body. Let him know that I intend to wait on him at his own house on Wednesday morning next, or Thursday, with 10 or 12 sheets of copy for the press, if God spare life and health to his obliged servant, and, James, Yours, &c.

JOHN FLAMSTEED.

[This not delivered, but the contents intimated to him.]

[Extracted from MSS, vol. 33, page 61.]

No. 97.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, June 30, 1705.

I intend for Burstow a fortnight hence, or within 3 weeks at farthest, and shall be here very probably when Mr. Stanfield comes to London. I am not in haste for your calculations: you may send them before I go, if you can have copied from them, what you think fit, in convenient time. If you send them by Mr. Stanfield, I will order Mr. Hodgson to receive them for me of him; or you may send them by the carrier, by the latter end of August or beginning of September: for now that you have done your work, I would have you keep in your hands, to copy, all you think may be of use to you hereafter, at your best leisure and conveniency.

I would desire you to compute the visible places of the moon from the given distances, without any correction or allowance, save that necessary one of the ☽'s semidiameter; which, being seldom observed, you may take from the table copied at the foot of the observations. This method will be the fairest and most ingenious; for 1° I impose no author's parallaxes or refractions on those that have a mind to employ the calculated places. 2° from the ☽'s places observed, her visible zenith distance may be calculated, and the parallactic angle: from the first of these, her parallax in height, and her refraction, may be stated by every author as he pleases; and hence the difference will be the absolute variation of the ☽'s place in altitude, whence, the parallactic angle given, the variations

in longitude and latitude, and consequently her true places in both, will be had. I am heartily glad you have copied my lunar numbers: you will now have it in your power to compare them with the heavens; from which I found, by my mural arch, they sometimes deviated 7 or 8 minutes. I tried but a dozen of observations, and a friend of mine half as many; perhaps you will find greater errors. Pray let the result of your calculations not be imparted to any body but myself; since both the observations and numbers are mine. You will easily guess a further reason: if not, I shall give you one at better leisure: for I intend, God willing, to write to you again before I go into Surrey; and by that time I hope I may have settled my affair of the press. My first volume is ready copied, and I suppose I may have something more to the purpose to inform you of in a few days.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 98.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 12, 1705.

I send you here 3 years' further observations of the moon, ready set for calculation, that you may not fear want of work during my absence at Burstow. I go thither, God willing, on Saturday next, and shall return again to the Observatory, his good Providence preserving me, about 5 weeks hence. During my absence I hope to prepare all the rest of the lunar observations, taken with the sextant, for calculation; and to send you them at my return. I am afraid you now think that I throw this work too fast into your hands: I must therefore tell you, that you need not slave yourself for that, but take your own time, and go on with them as your health and circumstances will allow you. I make haste purposely to rid this work off my hands that I may be at leisure to write the preface to the first part (which will be a troublesome work), and go on with the second volume, which will require a great deal of my own pains, and such work as I cannot trust to my servants. When these calculations are over, I shall be a whole volume beforehand with the press, except Sir I. Newton act more vigorously than he has hitherto done. If you please, now you have my lunar tables by you, as soon as you have got a competent number of the ☽'s places calculated from the observations, you may compute her visible places from the tables: whereby you will soon see whether these numbers represent her place so nearly as Mr. Raymer and the astronomical professor at Oxford boast. If you have any, please to let me know the result.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 99.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

Burstow, July 25, 1705.

I told you Mr. Halley has printed an investigation of the periphery of the circle from the tangent of 30 degrees: 'twill be out in a short time and sent you. If he writes anything to you about the edition of my works, you may do very well to inform me. I have not changed a word of what I said in commendation of him in my observations from 1676 to 1680. I have a many proofs by me of his falsehood and lies, but I would not be the man that should tell the world that so good a

mathematician, my countryman and acquaintance, was so ill a man: and if he force me not to it I shall be the last man that shall publish his faults. I wish I could make him better, I should rejoice to see him become sincere and honest: I pray God give him grace to see his follies and repent. I hope to be at Greenwich again, God sparing me life and health, about 3 weeks hence, and then to send you about 60 observations more of the moon; which will finish the observations made with the sextant, and I shall think myself very happy if you work them off before winter.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 100.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

Burstow, August 20, 1705.

You must let me be Mr. Hodgson's advocate with you: he is of a very mild and easy temper, so that people do not much fear putting tricks upon him. When your quadrature of the circle came up, it was resolved at the Royal Society that it should be printed, and he was spoke to, to correct the press, as I remember: but Mr. Raymer, seeing it, found the quadrature might be performed otherways: the secretary of the Royal Society is his crony; delays were contrived; his is printed, to make a needful (and to Raymer) a useful noise; yours is postponed. Were I in your case, I would forbid the printing yours, which must now appear as a consequence of his; whereas yours preceded, and gave occasion to it. I thank you for your plain dealing with me, in your answer to my request; if hereafter either Raymer himself, or any friend of his, writes to you to know what you have done on my account, you will please to acquaint me of it: and return them a very civil insignificant answer, such as in your discretion you think fit: for I expect you will be solicited ere long. I would make no enemies if I can avoid it; and, to prevent it, I have given you this advice beforehand.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 101.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, August 26, 1705.

Yours of Aug. 3 came to Burstow on the 19th: I sent you an answer to it, on 21st from Rygate, which I hope you have. If my friend were careless and has not sent it, please to let me know it. I have the minutes of my answer by me, and can soon copy them. Mr. Hodgson received my tables; and your calculations, safe included, came to my hand by his on Tuesday the 23rd. I am much obliged to you for them, and a great deal in your debt. I shall endeavour to satisfy you for your pains very shortly; but I fear I shall not have leisure yet, for some time, to compare yours with Mr. Witty's calculations: whenever I do, it will be a heavy work; and I shall send you an account of the errors we find in yours, that you may have the moon's place confirmed by two distinct calculations, to compare with the tables of mine you have copied.

I observed the moon's diameters very rarely in the years 1680, 82, 83, and afterwards; by reason I found that, allowing corrections of them according to my 24th and 25th tables, they would be represented well enough. I think I told you formerly I made use of the Tychonic places of the

moon in Argolus's ephemerides. I esteem them the best; but an error of half a minute in the ☽'s diameter I regard not much, for it can make no more than ¼ of a minute's error in her place; and I dare not say the places of the fixed stars, or any measured distance, are certain to less than a quarter of a minute. I wish I was sure of no greater error in either of these.

* * * * * * * *

All these faults I have corrected in my books and copies for the press; and this is one of the advantages I expected and designed to reap by these calculations, but the least: the chief being to have such a stock of observed places, ready to compare with any theories or tables as were never had before, either for number or exactness, in order to discover the laws of the planets' motions and correct the theories and tables. And now, through God's blessing on my endeavours, I have the end of my labors fairly in my view; and I hope, yea I doubt not, but he will give me life and strength, and afford me means both to accomplish and publish them, to his glory; which shall ever be the chief end of all my labors.

Sir Christopher Wren is gone down to Winchester to view the buildings there which the Queen has a mind to finish: he will not be in town again these ten days. After his return, we shall conclude about printing: he is a very sincere honest man; I find him so, and perhaps the only honest person I have to deal with. As soon as we conclude you shall hear further.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 102.) [*Paper of Memoranda relating to the Master of the Mint, for Sir C. Wren.*]

Mr. Halley has set the Master of the Mint at distance from me by false suggestions: a base prejudice to my work. I have had no opportunity of preventing him, by reason of my living so far from London, coming rarely thither but upon business, much employment at home, indifferent health, and further I esteemed him a wise man, that would not easily be possessed with suggestions, and thought the many communications I had made of my labours would not easily be forgot. Or if he pleased to call it to mind, my open and plain behaviour would justify me against such triflers and sycophants, who have used him as ill as they have done me sometimes when it might serve their interest, and when they thought themselves served, and that it might turn to their advantage. Entreat him for the future to do by me as I have done by him, and reject all their imputations till proved; or acquaint me with them, that I may answer them.

I am ready to assist him, but the *Observations* ought to be *first printed, the Catalogue of the fixed Stars next*, then *his Theories*, grounded on his principles, and then together, and afterwards *the Tables for calculating the Planets' places*, grounded on those theories, by any that will be at the pains to make them. The Observatory having supplied him with materials for the discovery of his theories, 'tis fit the honor of his Majesty and the nation (at whose expense and for whose use it was built), should be first regarded, our own afterwards: any other method will only tend to Mr. Newton's dishonor, and deprive me of the recompense I may justly claim for doing that which is not done elsewhere, and it is not probable will be again attempted either in our own or neighbour nations.

I shall be at more leisure hereafter, if God spare me health, than I have been these dozen years, and shall not fail to attend him oftener. Only I desire him *to have patience* till I can have

completed the catalogue, and made my works fit for the public, which will require help (that he may be instrumental in procuring), and he shall command anything that may be useful to him, even before it has passed the press, out of it.

Were it understood what time and pains are required to gain the observations necessary for a catalogue of the fixed stars, the trouble of calculations afterwards, and the vast labor in them for deducing their places from the observations, and dressing them up into a frame fit for the view of the public, they would not betray their own ignorance by asking why I do not print: but rather inquire how far I had gone in the work, and take care, if they know the importance of it, to procure me help to finish it. They may as well ask why St. Paul's is not finished.

[Copied from the *original* in MSS, vol. 35, p. 139.]

No. 103.) *Mr. Flamsteed's Reflections on his situation.*

Aug. 29, 1705.

'Tis very hard, 'tis extremely unjust, that all imaginable care should be taken to secure a certain profit to a bookseller, and his partners, out of my pains, and none taken to secure me the re-imbursement of my large expenses in carrying on my work above 30 years.

'Tis a great dishonor to the Queen, his Royal Highness, and the nation, that no reward is proposed for so long, difficult, and laborious a work: and that the small one I might justly expect is cast upon those that have had no part in the labour and expense, nor hazarded their health, nor felt my severe pains of the stone and other distempers, caused by my night watches and day studies. My instruments are my own; my assistance has been hired at my own charge. "Impius hæc tam culta novalia." My copy is my own; I am ready to deliver it to the press on just and reasonable conditions: it concerns others to make them honorable, but I am weak. They must make haste and do those things which may make me easy, if they intend to preserve my labors.

J. F.

[Copied from the *original* in MSS, vol. 35, page 55.]

No. 104.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Sept. 11, 1705.

I have examined the observation of the distance betwixt η Serpentis and β Capricorni, which you calculated from the stars' places to be 31° 42′ 7″, Mr. Witty, 31° 42′ 12″, in my first book of Observations; and find no reason to imagine any error in numbering the distance by the diagonals, because the revolves of the screw gave the same. So that I rather conclude my servant Smith, who assisted me then, and took β Capricorni in the fixed telescope, being short-sighted, mistook the star in that observation, and took some other of the bright ones near it; which he was apt to do when I took not care to prevent his mistake. But it signifies not much, for when the moon was observed that night, I had the star in the moveable index myself, and in the cross distance he had the moon, in which there was no danger of mistake. This refers to an omission in my letter of Aug. 20, from Burstow, where an answer to it was omitted till my return hither.

I must also beg you to excuse me for some misinformation I gave you in the same letter. I hear

Mr. Halley's quadrature of the circle will not be printed in the Transactions, as I wrote to you; but with the new book of Logarithms. Yours was forgot or mislaid, ignorantly, no doubt, and I am told will be published in the Transactions: the rest you will believe as you see reason.

The inspectors of my works make no haste to forward the edition of them; but all possible care is taken to secure a private advantage to a courtier: but I must not complain at present. The Court will, ere long, return to London, and you shall hear more.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 105.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jermyn Street, in St. James's, Sept. 17, 1705.

SIR,

You have now been I think above a fortnight in town, and no step yet made towards putting your papers into the press: and now Mr. Churchill is going out of town for a fortnight. But, however, he has left matters with his brother till his return; so that your papers may go into the press as soon as you please. If you stick at anything, pray give Sir Chr. Wren and me a meeting as soon as you can conveniently, that what you stick at may be removed.

I am, your humble servant,

I^S. NEWTON.

[Copied from the *original* in MSS, vol. 35, page 49.]

No. 106.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Septem. 25, 1705.

I am glad you have finished all the lunar calculations. The way you propose, of putting them into my hands, is both the easiest and safest that can be: therefore pray make use of it. For, should I send my servant to receive them, 'tis not improbable (but as most servants do) he may mind his own business more than his master's; and so miss the carrier: and, should the carrier neglect his business, they may be received from the porter at the inn, when he is returned home. I have one favor to beg of you; that you would please to let me know how many of these calculations of the ☽ or planets' places from two cross distances you commonly dispatch in a day, when you are not hindered by other business: it will be no detriment to you, and an advantage to me. Pray also let me know now what I am in your debt for your obliging assistance in this business; that I may take care to pay you before I engage myself deeper.

The calculations hitherto have been very perplexed, troublesome, and so intricate that it was morally impossible to avoid mistakes. I know you to be as expert at numbers, and as intent on the work, as any man; yet, having compared your calculations of the moon's places received with Mr. Witty's, there are sometimes great differences, often small ones no less difficult to rectify than larger. Mr. Witty was afraid, when I produced yours to compare with his, that only his would have been found faulty; but he recovered himself when he saw that you could commit small mistakes as well as he. We have gone through with all the observations of the moon you have returned; and to the year

1681 in Saturn: and I have entered the places, deduced from both the calculations, with the distances whence they were derived, in tables. If you have kept any copies of the first calculations by you, I could point you to the places in them where your oversights are committed: but this would be a great trouble, and therefore, if you please, I should rather choose to send you copies of tables in which I have drawn up and compared them. My man Thomas Weston is pretty well recovered and has had no fits this 3 weeks; so that now I dare put business upon him again. He writes very close when desired. I can cause him to copy them as finished; and (if you desire it should be so) he shall transcribe them the next post after that I have got so many as will fill a sheet, and send them to you.

I forgot before to request you to let me know what charge I have put you to by post letters or carriage, that I may also repay you. The examining of the lunar calculations yet behind in your hands, and the other planets, will take up some time. I must stop here to clear these calculations of faults, and fix the planets' places true: as soon as these are over I have a greater number of the moon's visible places to be computed from the observations made with the mural arch; but they will be much easier to manage by the help of some tables I have made for that purpose; of which I shall send you a copy in good time, and two simple proportions are all afterwards that will be required to give the moon's visible place.

But my work meets with obstacles: a gentleman, whom you know very well*, that used formerly to shelter himself with me at the Observatory, and is frequently in attendance at Court, pretends to oblige us with naming a bookseller to undertake the printing of my works; who is such an one as will divide the gains with him, and leave me nothing but the bare honor of having obliged the world with them, after 30 years' pains and above £2000 expenses more than my appointments. Sir I. Newton minds nothing but his own particular ends: I hope to weather all, through God's assistance and blessing, with a little delay, which will be no loss of time: for, in the interim, I am carrying on my business as fast as I can; but all at my own charge, having not had a penny allowed me yet, though I had hopes given me of competent allowances for help both of an amanuensis and for calculations. I bless God for it, I have done what I thought was my duty studiously, and I will never distrust his Providence, nor fear but he will, in his good time, give an happy issue, with desired success, to all my endeavours.

Sir I. Newton would have the great catalogue printed without the maps. I cannot consent to so sneaking a proposition. He pretends to save charges by it. His Royal Highness is able and willing to bear them, for ought I hear to the contrary. I shall tell you more in a short time. I am often ill since I came out of the country: God keep you in health.

About 30 sheets of the 2nd volume are copied, but they want to be examined, and to have the last volume (of zenith distances) filled up with my own hand, Whilst we are doing this, you will have time to compare some of the moon's observed visible places with the ☽'s tables; whereby you will see how near they agree with the heavens.

[Copied from the *original* letter in the possession of Mrs. Giles.]

* In the draft of this letter, preserved in Flamsteed's Letter Book (MSS, vol. 33, page 64), this gentleman is stated to be Mr. F. Aston. F. B.

No. 107.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 9, 1705.

Sir,

I would not have wrote to you till I had received the lunar calculations remaining, (which I expect on Thursday or Friday next,) but that yours tells me you expect a catalogue of your mistakes by that. Truly, my friend, I do not intend to send you so uncomfortable a sight; but in the room of it, I shall (as I promised you in my last) send you a catalogue of the places calculated both by you and Mr. Witty; for I have caused him (who has committed as many, or more, material faults as you) to correct them, where the difference between you was enormous; where it was but small, I have passed it as of no moment.

I have been frequently ill since my return out of Surrey, which has been a great hinderance to me; and have had continual employment for my servant; so that I have neither had leisure myself to direct, nor for my servant to transcribe for you: but before the week be run out, I hope to set him to work on a transcript; and by the latter end of the next week, to put it into your hands. In the mean time pray be not so much concerned about your mistakes. I assure you that many of them are such as you would wonder at: in the easiest things, as in adding the two usefullest angles together, you have sometimes made them a degree too much or too little, by carrying a degree where you ought not, or omitting to carry it where needful. This is only an argument of human frailty, and shows that there is no trusting to a single calculation in things of this nature; though we may safely trust a single one in making of tables, where the regular change of the difference shows the work well done, and an irregular one an error; as you well observed when you calculated some tables for me formerly.

Mr. Witty has made more faults than you; which, by the help of yours, he has corrected; and he has commonly made them as you did, where he was most secure. Why should I trouble you to repeat any calculation, which he has made true by repeating it already? but, when you have the result of his, you may do as you please. I do not find but in taxing of the moon's diameter, he has nearly agreed with you: that you may see it, I shall give you his in the transcript, and a small example at present: "1678, Oct. 19, was an eclipse of the ☽: Mr. Halley was here with me to help "to observe it: at 11^h 0′ 8″ we took ☽ limb. rem. à lucida ♈ 12° 9′ 25″, and her nearest à "lucida Pleiadum 17° 34′ 25″." Here you both allow 16′ 25″ for the moon's semidiameter: whence the distance of her centre is à lucida Pleiadum 17° 50′ 50″, à lucida ♈ 11° 53′. I had given you the first in my last observations 17° 18′, as you make it: but you must amend it, and may repeat the calculation if you please: for my assistant, to spoil the Observation, had caused me to write *rem:* for *prox:* in the dist. à lucid. Pleiadum: and I may tell it you freely, that since I have come to examine the observations made here by him, in my company or absence, I generally find them either deficient or incumbered with some mistake, which I can not think ignorantly committed. Last night I calculated the ☽'s place from these distances correct; and find it ♊ 8° 18′ 25″, lat. A. 0° 46′ 16″; but I fear a fault in the calculation. In the calculation of the moon's place from these stars, though you had stated the ☽'s distance à Pleiadum lucida 17° 18′ 1″, yet, in your work, in the very next line, you wrote twice 17° 8′ 1″, and so use it: a plain oversight, but excusable by those who are versed in calculations, and find they cannot always look about them as they ought.

Be not discouraged at this in the least; our nature is such that 'tis morally impossible to avoid these easy mistakes, nor is there any way that I know of to discover and remedy them, but that I

have always taken, by employing two calculators at a distance, patiently comparing their work, and repeating the faulty calculation; and the certainty of the work, on the repetition, is to me a sufficient recompense for the double labor of calculation.

I use not to tell you public news, but having spare room I will now give you some. We were long fearful that the English forces would return from Spain, without doing much; but our last news makes us not doubt but that Barcelona is taken: some say the Spanish nobility begin to come in to King Charles; and we hope a good issue to this expedition.

I am told Mr. Ditton's book of *Fluxions* is finished, and will be published the approaching term: if you desire it, I shall send you one of them when out.

Some experiments have been made of producing light by violently shaking the mercury in the Torricellian tube: they succeed as well when the tube has the common air in it, as in a void; nay even in compressed air. I have moved to have it tried whether the light or flame would fire gunpowder; whether it has been tried or not I do not yet hear.

I have one thing more to add: that though the edition of my work should not go forward at present, yet I shall, with God's assistance, go on to prepare all things for the press, as if it should: and therefore your assistance, in a great many things, will be useful and necessary to me: and though I cannot afford to pay you out of my own pocket, as I might do out of the public, yet I shall always endeavor to make you such recompenses as I can afford, and endeavor on all occasions to serve you. Sir I. Newton, I much fear, minds nothing but his own ends: he will lose what he designs to gain by his selfishness. Let what will happen, I shall ever be,

Sir, your obliged and thankful friend,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 108.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 18, 1705.

Kind Sir,

I send you herewith the places of ♄, ♃, and ♂, calculated by Mr. Witty from my observations: your own you have by you. When there was any fault in his, I caused him to repeat the calculation; so that I think you may rely upon them: but I will not be confident, because I am very sensible to what circumstances of fallibility mortality subjects us. I had caused him to correct the observations of ♀ and ☿, by refractions, and calculate their places from the distances so corrected: but, when I came to compare them with yours, I found so much difference in the judgments about refractions, as made considerable difference in the longitudes and latitudes thence deduced: and therefore I thought it best to do, as I had done in the moon, and caused him to calculate their places from the simple distances, uncorrect by refractions, that so every one, that has a mind to have the true places, may allow for refractions (as is to be done in the moon for parallaxes and refractions both) according to his own judgment. This is the most ingenuous way, and the safest; for I had rather another should have cause to blame his neighbour's or author's judgment than mine.

I have entered on the examination of the moon's places calculated by you both, and find fewer material errors than I expected; some slips of yours I have easily corrected, others I cause to be determined by Mr. Witty. 'Tis a great advantage to me to have your original calculations before me; for, by comparing them with his originals, the fault in either of them is soon discovered; which

otherways would cost a vast deal of trouble. You shall have a copy of them, with my first leisure after I have gone through them all, which I cannot promise will be very speedily, by reason I have sent the first sheet of my observations made here to the press this week; and on Monday next am to meet my inspectors, to agree how to carry on the press-work. I fear I shall have a great deal of trouble; but, if God spares me health and vouchsafes me his blessing, I hope to get to my intent in good time, and with the greater ease: for this long delay has given me an opportunity to put all things into good order. My only grief is, that the narrow souls of my inspectors will not afford me, I fear, such allowances, for those who have assisted me, as I designed them. I shall however make you such an acknowledgment as I can afford, hoping for a time when I may enlarge it. I am sorry you should think I design to find you no more employment. I have by me the much greatest part of my lunar observations, taken with the mural arc, to calculate, or indeed all of them: for, having assumed parallaxes and refractions according to my own theories, others may not admit them, and therefore it would be more ingenuous and proper to give the simple visible places of the meridional moon's centre, and leave such authors as have a mind to employ them to make use of such parallaxes and refractions as they think most just to derive the true from them. In this work I would employ you, if I had a competent allowance for you: but till I have one, I shall not move it to you, nor suffer you to do it, however ready you might be to oblige,

Sir, your sincere obliged friend and servant,

John Flamsteed, M. R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 109.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 26, 1705.

Kind Sir,

I have just now received yours of the 23rd instant: the six sheets of calculations, you are in pain for, came very safe to my hands, as did also your letter of October 1, which I answered the 9th instant; and therein designed to give you an account of the receipt of your calculations. How I came to forget it I cannot tell; you must pardon this fault of my memory; as my years increase it grows weaker, and I am apt to forget: excuse me this fault, I will endeavor to avoid it for the future.

I examined the first of your calculations myself; Mr. Witty, with my help, the most of the 9 sheets that followed them. He has the rest now under his hands: as we find any mistakes or errors committed, I cause the calculation to be repeated by him. It will be a needless trouble for you to repeat those wherein you have erred. You shall have the result of the examinations and corrections of the ☽'s places sent you, as you have already had of the superior planets; which will save you all your designed labor. If you want extracts of any of my observations, or your own, or his calculations, pray let me know it. I am bound in gratitude to furnish you with them; and therefore as soon as you let me know what it is you want, I shall order it to be transcribed and sent you.

You seem too much concerned for the mistakes you have committed: I am glad that, at this distance, you cannot see them. I shall never remit them to you, but I have told you formerly that Mr. Witty's are not fewer nor less material; and I know no privilege God has given any of us whereby we may pretend to be free from error. Be not concerned: all the faults in yours, and his

calculations shall be corrected, and the result sent you; and if you doubt any, you have liberty to try them over again.

You complain that there is some difference betwixt the times of the observations sent you in my first, and in the last synopsis of them: it may be so, but the last are to be only relied on: for I made a review, when I set Mr. Witty to work, and found reason to alter some of them; though the fault cannot be great, take which times you please; the motion of the superior planets is so slow, it makes no sensible error. When you have the moon's, you will find none of this (except where my pen has slipt, or memory has failed), which I shall not be ashamed or disturbed to correct, as soon as advised of. You may, if you please, repeat the calculations of the places of Mercury and Venus, from the pure observed distances, without the correction for refractions: but, if you let it alone, I shall not blame you: they are few, and if I had thought of it at first, should have been so calculated. But we cannot perceive all inconveniences at first: the lunar observations led me to it: the differences from the correct places, or those deduced from the distances correct by refractions, are not great, and therefore I conclude both true. But 'tis most ingenuous to leave our reader to make use of what refractions he thinks most just in every altitude; and the corrections for the latitude and longitude may be thence deduced, as the parallaxes of latitude and longitude in the moon easily enough by the help of the nonagesimary table.

I had a meeting with my referees, or inspector, on Monday last: some progress is made towards printing. Yesterday I sent a copy of the 2 first sheets to London; that I sent before being altered: one cause of this slow progress is, the persons, I have to do with, know not what is to be done: they are to pump it out of me or my servants, and then make their demands accordingly. This I like not to have them do: but however bear with at present. It will be time enough to show them their folly, when half a score sheets are printed; by which time I hope we may come to a friendly understanding.

I am ashamed to be so much in your debt, and so long. I will leave ten guineas in Mr. Knap's hands within a fortnight's time, to be returned upon Mr. Stanfield if you like it. I do not look upon this as a sufficient reward for the pains you have taken on my account, but desire you to accept it as a friendly acknowledgment of my own, till I have an allowance that will afford me to make you a better.

Mr. Ditton's book of *Fluxions*, I am told by our printer, is out. I shall send you one of them about 10 days hence; and, by that, I hope all the errors in the lunar calculations will be cleared and corrected; and I may send you a copy of her correct places, together with Ditton, by the carrier, directed as usually to Mr. Stanfield.

The cold weather coming fast on, I am more affected with it than formerly; and frequently ill. When the press sets to work, I fear it will take up a great part of my time to correct the second proof sheets: the first I leave to James Hodgson, so that I fear I shall not have leisure suddenly to prepare more lunar calculations. A few weeks will tell me what leisure I shall have; at present I have no more to trouble you with. I pray God keep you in health, and send us peace and truth here, eternal happiness hereafter. I am ever your affectionate obliged friend and servant,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 110.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jermyn Street, Nov. 14, 1705.

Mr. Flamsteed,

On Saturday next, about twelve o'clock, the referees meet at my house, to finish the agreement and sign the Articles about printing your book; and I shall be glad to have your company here at the same time, and that you will be pleased to dine with me.

I am, your humble servant,

Isaac Newton.

[*Note written on the letter by Mr. Flamsteed.*]

I was there and signed the Articles, but covenanted that the catalogue of the fixed stars mentioned to make a part of the first volume should not be printed but with the last. Dr. Arbuthnot was there, with Mr. Roberts, and Mr. Churchill, but neither Sir Chr. Wren, nor Dr. Gregory.

[Copied from the *original* in MSS, vol. 35, page 57.]

No. 111.) *Proposed Articles of Agreement between Mr. Flamsteed and the Referees*.*

Nov. 10, 1705.

Whereas it hath been agreed by Sir Isaac Newton, Knt., President of the Royal Society, the Honourable Francis Roberts, Esq., Sir Christopher Wren, Knt., Surveyor-General of her Majesty's Works, Dr. Arbuthnot and Dr. Gregory, in order to the printing all the Astronomical Observations of John Flamsteed, her Majesty's Professor of Astronomy at the Observatory in Greenwich Park, in the county of Kent, under the title of *Historia Cœlestis*, &c., at the sole charge of his Royal Highness Prince George of Denmark: that Mr. shall be the undertaker for printing the same. It is hereby covenanted and agreed betwixt the said Sir Isaac Newton, Francis Roberts, Esq., Sir Christopher Wren, Dr. Arbuthnot and Dr. Gregory, and the said John Flamsteed, on the one part, and the said undertaker, on the other,

1. That neither he the said undertaker, nor his executors, administrators, nor assigns, shall have or claim to have any right, title, or interest either in the original copy, or any part of the printed copies. [But that the same shall, after the printing, be and belong to the said John Flamsteed, his executors, administrators, &c.]

2. That the said undertaker shall not print, nor cause to be printed, any more than 400 copies of the said *Historia Cœlestis;* that he shall print, or cause them to be printed, on the same paper, and with the same letter, with the specimen hereto annexed, with all convenient expedition, at the rate of shillings per sheet, with all necessary care and exactness. And for preventing errors, to which works of this nature are most liable, he shall not suffer any sheet to be wrought off by his printers till the proof be allowed to be thoroughly correct by the said Mr. Flamsteed, or a person who shall from time to time be named by him for that purpose; and that he send all the proofs to be collated with the originals, to the Observatory, at his own charge.

3. That in order to prevent any more than the copies agreed upon, from being printed, he, the said Mr. Flamsteed, his agents and servants, shall have access to the press at all times; and it

* There are four copies of these Articles: see the note in page 81. The material deviations from each other are included in brackets: but other slight variations are not here noticed. F. B.

shall be lawful for, and permitted to him, and his servants or agents by him employed at any time, to stand by the press to see the said number of copies of any sheet or sheets wrought off, and as soon as that just number shall be wrought off, to break the press, without any delay, let, hinderance, or molestation, from him the said undertaker, his printers, [and his or her agents,] or servants, on any pretence whatsoever.

And whereas the said *Historia Cœlestis* contains the Observations of the said Mr. Flamsteed, continued more than 30 years with great labor and diligence, frequent injuries to his health, and no small expense in framing instruments, hiring assistance, &c., at his own charge: in order to secure unto him some fruits of his own labors, it is agreed, with the approbation and consent of the said Sir Isaac Newton, Francis Roberts, Esq., Sir Christopher Wren, Dr. Arbuthnot, and Dr. Gregory,

4. That he, the said undertaker, his executors, and assigns, shall and will at any time, and at all times hereafter, on the reasonable demand of him the said John Flamsteed, made by himself, his servants, or agents employed by him, deliver, or cause to be delivered, unto him the said Mr. Flamsteed, all the sheets printed at the time of such demand, fair and undefaced, at the Observatory in Greenwich Park, in the county of Kent, at his the said undertaker's own charge; that all the copies of the said *Historia Cœlestis* may be there preserved, to be presented to his Royal Highness, to dispose of as he pleases.

5. That whosoever shall be the undertaker shall consent to these articles, and give at least a thousand pounds security for due and punctual performance of them *.

[Copied from MSS, vol. 35, page 73.]

No. 112.) *Mr. Flamsteed's Reflections on the conduct of the Referees.*

The sextant wherewith I measured distances in the heavens, when I first sat down at the Observatory, with the two great clocks, were given me by Sir Jonas Moore, and are no less mine than the quadrants, telescopes, and other instruments I employ, all which were made at my own charge. The extraordinary help necessary for carrying on my work has also been hired and maintained at my own expense. My observations have been carried on day and night, when others slept. The night colds havé given me the painful distempers of the stone and cholic; my days have been spent in laborious and painful calculations, to prepare a catalogue of the fixed stars, and the places of the planets, for the use of all ingenious lovers of art and promoters of science all the world over; and I think I may say it without vanity, such a stock or treasury was never made before by any person's labors whatsoever, though they had ten times my helps.

His Royal Highness, desirous that the world should enjoy the benefit of my labors, has been pleased to appoint some gentlemen of the Royal Society to view my manuscripts, and on their report, to order them to be printed at his own charge, for which he has given his directions for above £800 to be issued by his treasurer. The persons directed to view them called me to one of their meetings, where I showed them, and to another where an undertaker for printing them was present. I do not remember that I was present at any more of their meetings but one, where nothing material

* *Note by Flamsteed, written on one of the copies.* After which, I shall immediately put the first volume, which is fair copied, into the hands of such persons as they, the said Mr. Roberts, Sir I. Newton, and Sir C. Wren, shall require, to be put into the press.

was determined whilst I was present; though I considered that being the person chiefly concerned, I ought either to have been present at all, or at least to have had their resolves immediately signified to me, of which I had nothing but at second hand, and sometimes knew less than those that were altogether unconcerned. I have been informed that Mr. Churchill is the person to be employed as undertaker. I have always declared it to be my opinion that there was no need of any bookseller to undertake this business; that the employing of one would be a needless and great charge to the work; that it would be a misemploying of his Royal Highness's liberality, who, I conceive, designed not the gratifying of a bookseller, but the author of the work. And further, that it would be a reflection and dishonor to the gentlemen concerned; as if they could not continue to answer his Royal Highness's design without the help of a bookseller, who, having all the copies in his power, will force the author to dispose of what number his Royal Highness shall be pleased to bestow upon him to himself at what rate he pleases; and make other pretensions which cannot be thought of or obviated.

I know not but that Mr. Churchill may be as honest a man as any of his trade, but I think it very *hard* that all the meetings of the gentlemen entrusted should be to secure the edition to him, and a certain immediate advantage from volumes in which he has had no concern; and that none should be taken to secure me a recompense for 30 years employed in a difficult and laborious work, and all my extraordinary expense in the mean time. 'Tis extremely hard that I should be deprived of that reasonable advantage I might expect from my own copy, and that it should be conferred on a person that is not so much as acquainted with me or my studies; for I know not that I ever saw him till he was recommended to this business. 'Tis *harder* still that I should be deprived of the recompense his Royal Highness designed and intended me for my said 30 years pains and expenses; that it should be wrested from me by those contrivances, and given to another that has no pretence to it. . 'Tis *hardest* of all, that I must be represented to his Royal Highness and my good friends and acquaintances as a humorous person, and unsteady, because I cannot yield to see his Royal Highness's liberality so misemployed; nor grant myself to be trampled upon, and all my labors serve to enrich a wealthy and purse-proud undertaker and his partners, who are ready to insult before I part with my copy, and are not likely to be more civil or just afterwards. I hope, therefore, that the gentlemen entrusted will not take it amiss that I insist on what they consented to at those two meetings when I was present:

1. That the undertaker shall be he that will print the cheapest and the best on the best paper.
2. That he shall renounce all right, title, and claim to the original copy, and to all the copy when printed.
3. That he shall not print, nor suffer to be printed, above 400 copies, or so many as shall be agreed upon, without the consent of the gentlemen entrusted and myself under our hands.
4. That in order to prevent the printing more than shall be stipulated and agreed upon, it shall be allowed to me, my agents, or servants, to break the press as soon as the number agreed on of any sheet or sheets shall be wrought off.
5. That he shall give good bond and security for the performance of these covenants;

For which, if Mr. Churchill shall give good security, and consideration be had of what I have before intimated, I shall not be unwilling that he have the printing of my papers.

[Copied from the *original* in MSS, vol. 35, page 61.]

No. 113.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Nov. 20, 1705.

Sir,

I doubt not but on the sight of the bill above written, your kinsman, Mr. Stanfield, will pay you the ten guineas he has received of me for your use: pray, when you have received them, let me know it: for I shall be in some concern till I hear this bill is come safe to your hands and paid.

Sir Isaac Newton has, at last, forced me to enter into Articles for printing my works with a bookseller, very disadvantageous to myself: but 'tis not time yet to tell you the story of his behaviour: I shall hereafter, and how much he has thereby injured me: and I see not that we are nearer the press than before. As soon as we make a real beginning you may expect more work, and to hear further from, Sir, your real friend and servant,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 114.) *Extracts of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Nov. 28, 1705.

Yours of the 26th tells me that you have received the ten guineas: the odd fifteen shillings will about pay the charge of the post letters and carriage since March last. The ten pounds I designed you as a recompense for your pains in calculation; and am glad that you accept it as such. I wish I could have afforded you more: I assure you 'tis all out of my own pocket. Had some people stood to their promises, I had made you a further acknowledgment: but I now find, what I before suspected, they have not any regard to their word.

* * * * * * * *

I think I told you, in some of my last, that ♄'s place in the heavens, at the last opposition to the sun in the end of October, was 36 minutes slower than the Rudolphine numbers, with which in 1698 he agreed within only 4 or 5 minutes; and that I had examined how this error increased in the intermediate years from 1698 to this. If you desire it, I shall send you a copy of the collations. I am now fitting up tables to see how the observed places of ♃, in the same time, agree with some numbers made by a friend in Derbyshire: when I have finished them (which may be a month hence) you may have the result of both together, if they will be acceptable to you.

Mr. Witty has calculated 100 true places of the moon, from my tables, of which you have a copy, in order to compare them with 100 of the first visible places computed from the observations by you and him: but I shall not fall on this comparison till I have done with Jupiter.

* * * * * * * *

I have signed Articles for the printing of my works; though there was no necessity for doing it, by reason I deliver the copy of the first volume of observations at once, to the referees, as Sir I. Newton calls himself, without any consideration of my own interest. I question whether Sir Isaac would have done so, in the same circumstances; but I was forced to do it quietly, to avoid his calumny. It may please God that this may turn to good; for now they have all the just reason in the world to proceed; but I fear he still will find ways to obstruct the publication of a work, which perhaps he

thinks may make him appear less. I have some reason to think he thrust himself into my concerns purposely to obstruct them; whether justly or not, a few days now will show. I may be mistaken; but I believe another in my circumstances would conclude as I do. However, it will be wisdom to say nothing at present. I trust that the good providence of God will still take care of and direct all my affairs.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 115.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, December 12, 1705.

I have put 100 sheets of my first volume, contaiuing all the observations made here betwixt Sept. 1676 and Sept. 1689, with the figures for them, into Sir I. Newton's hands last Saturday: so that now there can be no pretence of obstruction or delay on my part. If he forms any new, he must bear the blame, I will not. I have 70 sheets of the second volume in good forwardness. You shall be acquainted with other proceedings upon every proper occasion; but I will deal plainly with you: if he deals fairly and does really promote the press, I shall attribute it wholly to the over-ruling hand of God's providence. For, when I consider his temper and behaviour, 'tis more than I expect, if he does. I have work for your new tables, and can gratify you for your pains: if not, I shall only impart what I do to you as a friend, and shall be careful not to press you further than one friend may another. I have not been in London these 18 days, by reason of my great cold.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 116.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Jan. 17, 1705-6.

I have no more to add but that, now my manuscripts have been 7 weeks in the referees' hands, I am told that the undertaker has not yet agreed with a printer; which makes me expect slow proceedings. I shall be glad to hear of your health, and to receive copies of the tables you promised, that are different from mine. I pray God keep you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 117.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Feb. 2, 1705-6.

I hope in a fortnight's time to have an entire sheet from the press; and that afterwards we shall go on vigorously: though I fear not so fast as some people imagine. I am preparing the 2nd volume: near 100 sheets are transcribed by Mr. Weston, who is this day out of his time; and 20 of them are ready filled and fitted up by my own hand, which all of them must pass: and each will cost me more than an hour's labor, to insert the correct zenith distances from those transcribed from the

instrument: for I copy the original notes of the foul books, or protocolla, and add those in the 3rd column on the right hand. I shall send you the first printed sheet, as soon as I conveniently can, after it comes to my hands; and an account of our proceedings, as God spares me health, and opportunities happen.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 118.) *Letter from Mr. Flamsteed to* ————*.

The Observatory, Feb. 26, 1705-6.

Sir,

I return you my thanks for the entertainment yesterday: had you acquainted me with the design before, I had saved both you, Dr. Gregory, and myself, the trouble I gave you, for I designed always to compare the printed sheets with the first notes, and would have done it 6 months ago, if you had not urged me for my copy; but it happens well: you now see that there was not one material fault committed. I depend not on the arcs got by the revolves of the screw, but when I find them conformable to those given by the diagonals, or where several distances give the same place of a star or planet.

You will ask the reason why the diagonals were not inserted at the first? 'tis a history too long to be told you in a letter. I shall give it in its proper place in my books.

I have got one of my young people to copy my table for turning the revolves into arcs. I hope 'twill be transcribed by to-morrow, and sent you with the books of observations. I had no copy of it by me, otherwise you had received both this morning together.

I have examined those few notes I brought from you, but want the manuscript for one of them. I wish the Doctor to go on orderly hereafter to compare the minute and copy together, and write his notes as he has hitherto done, one under another; but with a larger margin, for my emendations, and then I shall easily give them. The more strict he is, the more he will oblige me.

I forgot, through my earnestness to go through with Dr. Gregory's notes, to speak to you about the copy of the articles which you have not yet given me, and about Dr. Plume's Professor of Astronomy, which Dr. Bentley has determined, without even so much as letting me know that he was about such business, and, I fear, directly contrary to the archdeacon's design: wherewith, I am apt to think, none of the trustees in Cambridge were so well acquainted as I am. I had not known of it but by an accident. I have wrote about it to Mr. Whiston, who tells me the thing is done as to the nomination of a Professor, and past remedy. I am sorry for it, because this first election will be a precedent for the future, and I fear a very ill one.

John Flamsteed.

[Extracted from MSS, vol. 33, page 66.]

No. 119.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 2, 1705-6.

You desire to hear how my works go on. I shall give you a brief account. In November last I delivered in 100 sheets of copy. About a month ago, Sir Isaac desired to see my first notes, and

* I cannot discover to whom this letter was intended to have been sent. F. B.

I sent the first book of them. This day se'nnight Mr. Hodgson told me he had been with Sir Isaac, and had seen 4 or 5 folio pages of differences he had noted betwixt the original and copies. I visited him last Monday, and desired to see them: he told me Dr. Gregory had collected them. The Doctor soon came; when we sat down to examine them. Sir Isaac told me he did not believe them to be errors, but desired that himself and the Doctor might be informed of my ways of observing: they were proper judges. In the mean time I ordered James to come to me; for I have resolved not to talk with them without good witness. He came in good time: we set to work, and found a great many differences, but all of the Doctor's making. He had formed a table for turning the revolves, and parts, into degrees, minutes, and seconds: and, supposing the threads of the screw everywhere equal, wondered that his equipollent degrees, minutes, and seconds agreed not with mine. I told him I wondered he should adventure to make this table, wiped out his emendations from my margin, engaged to give them an account of the other differences, dined with them, and returned home. Next day I caused my own large table to be copied; and the day following sent it them, with the rest of my first notes to Sept. 1689, to be compared; and now I expect to hear, by Wednesday next, what will be done. I told them I had been at great expense in this work and expected a recompense: but I fear Sir Isaac had rather stop it, than give himself any further trouble: for he finds I do not court him, and his temper wants to be cried up and flattered.

I have always hated such low practices; but carried with that care that I have not afforded him any opportunity to recede. He thrust himself into the business, purposely to be revenged of me, because I found the fault both of his *Optics* and corrections of my lunar numbers, and would not suffer him to recommend my works *privately* to the Prince, when he desired it about two years ago. However I take no notice of this, but carry as if I thought he only wanted better information, and take care to oblige him with enough of it. I have got near 100 sheets of the 2nd volume copied: all the right ascensions and distances of the planets from the pole are gone through by Mr. Witty; the moon perfectly; the superior planets as far as 1699; so that I hope in a week or two's time to have them finished. As soon as we are through these, I shall cause them to be copied, and let them be seen; and, if he then goes back, all the world must acknowledge that I have done what I ought, and all the blame will be at his door.

When we print, I intend, God blessing me, to send you the sheets as they come from the press, with directions how to examine the observations, and a full narrative of our daily proceedings; that so you may be acquainted with the whole history, and be able to give an account of it, if it should please God to call me hence before 'tis finished. But I have hopes, that wise and good Providence, that has hitherto directed and guided all my concerns, will succeed me in this, and grant me life to see it published: which, that it may be to his glory, is what has ever been desired by, Sir, your obliged friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 120.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, June 11, 1706.

I am sorry to hear of Mr. Kirk's death: my friends here die off apace. I grow gouty, and pains of my feet hinder me from stirring much abroad; so I am confined in a manner to my business,

have more time to prepare for death, and now can thank God that we are to die: for life, to one so weak as I am, is no very pleasant state at 60. I pray God continue your health, and make you in all ways comfortable.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 121.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

Burstow, Aug. 12, 1706.

Sir,

I would not have answered you with a letter at this time, had it not been to excuse my not having sent you the printed sheets of my *Hist. Cœlestis* before my coming hither, according to my promise. I had then but 11 from the press; I have since received 5 more, and before I go back to the Observatory hope I may have 2 dozen complete, which I shall send you as soon as I can get them out of the printer's hands. All things are made as difficult to me, as can be easy to others. This will not make me say the Diopt. are true, or the lunar numbers agree to a minute or two. I am silent at present, and prepare my work as fast as I can. All the observations, made with the mural arc, are copied up to the entrance of this year: they make about 170 sheets. Mr. Witty I have dismissed; and he is now chaplain and companion to a young gentleman in Hampshire, on better terms than I could afford him. I bless God I enjoy my health indifferently well. Yours is heartily wished by, Sir, Your affectionate friend and servant,

John Flamsteed, M.R.

[Copied from the *original* letter, in the possession of Mrs. Giles.]

No. 122.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Sept. 14, 1706.

Sir,

I returned hither, I bless God for it, on the 5th instant, in good health: the press was advanced, when I went home, to sheet M. They are now through the alphabet, and the sheets A A, B B are set; but I know not whether they are wrought off or no. I send my man with this to London, and to go to the printer's for them if finished, that I may correct them. The observations of the fixed stars end on the sheet B B. I would send you them altogether, but I could not get them ready to send by Mr. Stanfield. I doubt not but he has goods to send down after him, and shall therefore send them, as soon as corrected, to Mr. Knapp's for you: if no goods be going thence, I will deliver them to the carrier, God sparing me life and health.

I do not hear that the book of *Logarithms* is yet published. I shall be glad to see your *Prosthaphereses*, when finished; but you need not be very hasty. I fear it will be some time yet ere I shall have occasion to make use of them. I thank you for your offer of assistance, and should make use of it, if I could do it to your advantage: but Sir Isaac is the same man he ever was. I have not received a farthing from him (who has drawn the Prince's money into his hands and forced himself into my business) though I have been at a large expense upon it. I must be patient. No one ever yet served his country honestly and honorably, but he was ungratefully used for his pains.

Mr. Pound is returned from India; has left his books, papers, and instruments at Pulo Condore, is now in Holland, returns to England in a short time. I shall tell you ere long what I have, both from his conversation and letters; at present want time to add more, than that I am, Sir, ever yours,

JOHN FLAMSTEED, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 123.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, Sep. 14, 1706.

SIR,

I have consulted Tycho's *Mechanica*, where he says that at that time, when he wrote it, he was 50 years of age complete, and that his volumes contained the accurate observations of 21 years; which shews they commenced in the year 1575. Tycho was born in 1546: Decembris 13, $22^h\ 47'$. But the observations of the *Historia Cœlestis* begin no sooner than the year 1582; so that, by this account, there are 7 years' observations wanting in the very beginning.

Besides all the observations of the year 1593, which were not to be found in Germany, in the same place he says he had observed seven comets; whereas, in the *Historia Cœlestis*, there are no observations that I can find either of that of the year 1582 or 1590, of which he gives an account in his *Epistles*. The first part of his *Progymnasmata* gives his tables for calculating the ☉'s and ☽'s places, with his observations of the new star of 1572, and deductions from them. The second part is concerning the comet of 1577; so that we have the observations of but 3 of his 7 comets: and of those, only such as he thought fit to employ. This makes me think that his observations of the comets is made a book by themselves, and that probably it is still to be found in Denmark, with the 7 or 8 years' observations that are missing.

Whatever his Royal Highness determines concerning the rest of Tycho's works, it may be much for his honor to bestow these on the world, with the errata of the German edition, if he can procure the originals, as I doubt not but he easily may, to be sent into England. After this account, it will be needless to send you the *Progymnasmata;* but if you have a desire to see them, please to intimate it by a note, and I will send them as you shall direct.

Allow me to mind you, that by the articles I was to have a note, signed by the referees, for the payment of £125 to me, as soon as ten sheets were printed: that number was printed off before I went into the country. I have dismissed my amanuensis and calculators, because they lay me in a greater sum, &c.: I could not promise them recompenses suitable to their work. I could desire a meeting of the referees to sign the order, that I might have, what you agreed to, readily paid me; and we may take care together to prevent the press from making delays upon false pretences, where you will oblige, Sir, your very humble servant,

JOHN FLAMSTEED, M.R.

[Extracted from MSS, vol. 33, page 67.]

No. 124.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Octob. 12, 1706.

The sheets of the comets are done, and the planets begun; which I shall continue to send you as they come out, but we go on slowly. Some short-sighted people put all the obstacles they can in our way, upon suggestions and pretences: the printers will print any newspapers or pamphlet sooner than this work, because news and pamphlets bring them ready money. Whereas the booksellers often pay not the printers till the whole volume is finished, or once a year if the work be large: so that I expect that we shall not clear above two sheets a week, except we can get the press to Greenwich, which I cannot expect. I comfort myself with this, that nothing can happen to me but by the will of God, who knows best what is good and fit for me: to whom I therefore submit; and smile at those who presume it lies in their power either to injure me or my works. Sir I. Newton takes particular care that I shall not receive a farthing for all my expenses; nor what Mr. Witty and Weston, or calculators, have cost me. These are great discouragements, but to be borne with, till God sees fit to raise us better friends; which I am in hopes of, and therefore go on cheerfully.

[Copied from the *original* letter, in the possession of Mrs. Giles.]

No. 125.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Dec. 9, 1706.

I was not solicitous to write a direct answer to your last, of Octob. the 15, because one of mine, dated near the same day, told you, as I remember plainly, that the tables were arrived. I received them safe by the penny post, and acknowledged your pains, as I must do still gratefully, till I have an opportunity of doing it more substantially than I can at present. Plainly it has cost me more than £150 out of my pocket to forward this edition. Sir I. Newton plays all the tricks he can to keep me from receiving one penny towards the reimbursement of this expense; conceals what he has received from the Prince, though at the same time he owns that he has received monies for a useless undertaker; and has paid him for the paper (which by-the-by is far from being what it ought), but contrives pretexts to delay paying the monies due to me, out of which I designed to have made you a further acknowledgment. I am patient at present, but show the account to everybody I handsomely can, to make him ashamed of his false behaviour: for the truth is, he designed by what I can collect, absolutely to hinder the publication of the work. I had no other way to prevent him but by consenting to conditions altogether unreasonable. It goes on; we approach the lunar observations; X X is printed; I expect three or four sheets more this week, when the whole of the first volume is done. Before I give them the planets' places calculated and repeated by you and Mr. Witty, I expect to have my monies, or the calculated places shall be detained: I keep a correct copy for you, which shall be carefully sent you whenever you desire.

I doubt not but the good Providence of Heaven is ordering all things for the best; and am therefore easy under all Sir I. Newton's ill usage, of which I could fill some letters with the relation: but at present I forbear. You shall have a particular account, if he change not, in a short time, God sparing me life and health.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 126.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Jan. 20, 1706-7.

My business of the press goes on very awkwardly; yet I have hopes I may, with God's blessing, find some way to quicken it ere long: what success I have in my endeavors I shall inform you.*

I have had much the same success, with yourself, in my observations of satellite eclipses. Our ill weather has often hindered me from seeing them; but my own constitution more: for I am not able to endure the cold air of the nights, as formerly; and therefore must leave that business to you, and younger persons, who have strength and health to proceed with it.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 127.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, April 3, 1707.

I have sometimes found an error of near half an hour in the calculated times of the eclipses of the second satellite, but never so big an one as your observation makes. The satellite's motion is certainly unequal; and I fear the inclination of its orbit greater than in the rest, and its ☊ different from theirs. I could wish you would, however, examine the errors of your clock again, for there is but seven days betwixt the eclipses of the 13th and 20th, or two revolutions, and look what error there is in that of the 20th: there ought to be the same in that of the 13th, of which you complain not: but I am apt to think your seeing of this much sooner than you expected, caused you to watch so much earlier than ordinary for that of the 20th. Jupiter is hastening towards his aphelion: perhaps, as the sun gravitates less on him, he has more power on his satellites, draws them more in, and they, especially the second, revolve swifter. I have long thought so, and hope I may have time yet, during my life, to examine whether it be really so or not.

* * * * * * * *

I am troubled with my headach still; but I hope it will be over in a day or two. My work goes on very awkwardly: about 63 sheets are printed. Sir I. Newton is very perverse. I pray God keep you in health, and from having anything to do with such proud people as he is.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 128.) *Letter from Sir Isaac Newton to Mr. Flamsteed.*

Jermyn Street, April 9, 1707.

Sir,

The referees meet on Friday next, at four o'clock in the afternoon, in Paternoster Row, at the next tavern to Mr. Churchill, the bookseller. You will hear of them at Mr. Churchill's. I desire you would not fail to meet them, because after the Queen returns to Windsor they will scarce have

* In another letter, dated March 15, following, Flamsteed writes thus: "I have got but 56 sheets yet from the "press, but corrected the 60th. Sir I. Newton is the triflingest gentleman, and the printer the most dilatory, I ever saw." F. B.

an opportunity of meeting any more before next winter; and that all things may be now settled and adjusted. I desire that Mr. Witty and your amanuensis may be there, and that you will bring your bill, and the three or four folio leaves of MS copy which you had from the printer.

I am, your humble servant,

I^s. Newton.

[Copied from the *original* in MSS, vol. 35, page 75.]

No. 129.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 29, 1707.

The press-work goes on but slowly: only three alphabets, or about 70 sheets, are wrought off. Worthy Sir I. Newton has twice or thrice been stopping the press: he does all he can to hinder it, or break off, and to perplex me; but an accident has lately happened, that has discovered his proud and insolent temper, and exposes him sufficiently. He has been told calmly of his faults, and could not contain himself when he heard of them. My affair was not forgot. I hope God will turn all to good. This accident was unexpected; and seems to be sent. You shall hear more of it hereafter. I design, by your kinsman, to send you all my printed sheets you want.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 130.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 1, 1707.

I have corrected so many sheets for you as will complete three alphabets: but there are some faults still I cannot set right, for want of my first notes, which Sir I. Newton still detains. I hope I may get them back to-morrow; if I do not, the sheets I shall have ready before I go to Burstow (I suppose for a fortnight hence), shall be left at Mr. Knap's for your kinsman; and I will give you all further necessary corrections at my return.

I thank you for giving me a further account of your observation of Mercury's exit from the sun. I am glad you saw so much of him, and hope that others in Germany and Italy, or the East Indies, have seen more than you did.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 131.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 12, 1707.

J. Hodgson now takes care of the press. I scarce know how it goes on, but I helped him to correct the sheet H h h h on Monday last. I am not at all concerned at Sir I. Newton's false dealing: 'tis what I expected. He injures not me, but himself, and the work; and loses his reputation by it. Heaven is just, and nothing shall happen to me but by the will of God, who will direct all to my good and his own glory. May he preserve your health and happiness!

[Copied from the *original* letter, in the possession of Mrs. Giles.]

No. 132.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, October 21, 1707.

Though I have had the last sheets of my book in my hands for a fortnight, yet, having some money concerns in London to manage, I could not get time to examine and collate them till this last week: and this morning, coming to the observations of the solar spots, I find the printer has omitted a whole sheet, which, I suppose, is lost. Had not this accident happened, I might have sent you them perfect this week or the next: now I shall forbear, till I find some remedy. In the mean time, my servant sets to copy the sheet, the printers have lost, anew this afternoon: his next work will be to correct the sheets for you after my copy; and assure yourself I shall ever be careful of your part of it, and you shall have it as soon as ever I can get it ready; this being the least part of what I owe you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 133.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Decem. 24, 1707.

On Sunday last was five weeks, after having been, by God's blessing, 12 years free from the torture of the stone, I had another fit of four or five hours' pains: but, blessed be He for it, not so violent as formerly. Immediately after it was over, I found a great cold upon me, which had kept me in a manner confined ever since, and rendered me so tender I can scarce abide the air; so that I have been forced to make use of my servants to get me observations for determining the place of Saturn at this last opposition. My clerk performed his part but very indifferently at first; but, with use, is become pretty expert: and, causing him to observe the transits of many stars that passed the meridian near the same height with Saturn, I have determined his places as follows:—

* * * * * * * *

Parker's ephemeris, or Street's tables, are 40 minutes too fast; my French but 4: yet they err as much in latitude where Street agrees. Kepler's tables err about 34 minutes. These errors are caused partly by Kepler and Wing making the eccentricity too small, and the aphelion to move too slow. Bullialdus makes the greatest equation 6° 37½′, and the aphelion to move above 3 degrees in 100 years; which will represent his present motions very well: and, with rendering his mean motion something less, will answer the observations of 40 years past pretty well; but not Tycho's nor Walter's: and, consequently, the planet's motion is liable to secular inequalities, which only time can discover. I can tell you no good news about my labors. Sir I. Newton's spite, and my late illness, has put all to a stand. I am always mindful of you, glad to hear of your health and welfare, and ready to serve you when any good occasions offer.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 134.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 2, 1707-8.

Your thoughts concerning the restitution of the planets' motions are just: they act one on another, and since their actions are as the squares of their distances reciprocally, it will be difficult, and require a great deal of consideration, to disentangle them, and find what the effects of their actions have been in several ages. For certainly they must cause secular inequalities in the superiors: and though the inferiors, being less in bulk, cannot have so great effects on each other, yet approaching each other much nearer, their effects must be sensible and perplexed. I think I feel them both in Mars and Venus; and then our Earth, that moves betwixt them, must be involved with the same.

And for ♃'s satellites, I doubt not but their motions are all liable to inequalities; but my age and infirmities suffer me not to examine them as I would. All I can do is, to lay in a good stock of observations, as I have done, for the primary planets; whereby posterity may be enabled to proceed where I am forced to leave off, through the envy of ill men, lest I should impoverish my nearest relations, whom I am bound for justice and conscience to take care of, since they are in no capacity to provide for themselves.

Last night I caused ♃ to be observed: his ☌ to the sun happened on the 5th instant. I intend to examine the observations this evening and cause them to be repeated the next clear nights. I have now the oppositions of 32 years, or more, through God's blessing, carefully and exactly observed: which is a blessing that never was granted by him to any of my profession before me. I have a catalogue of the fixed stars four times as large as Tycho's, and twice as numerous as Hevelius's. I think I may now be allowed to sit down and praise the wise Creator of the Heavens for his favours to me, and leave those, who have more strength and health, to go on and derive further praises to Him from the result of my labors. And though the progress of the press be stopped at present, yet I doubt not it will go on again in a short time, till all be published. But, if I must be used as I have been, and my works not more carefully printed, it will be best to stay where we are, till God remove the envious, and send us more sincere and honest persons to deal with, and manage it.

The little gout I feel in my ankles makes me unable to walk much, or exercise; yet still, I bless God for it, I have but little pain. I can walk down to the town sometimes; and better up the hill, than on even ground: my mind is vigorous and I am still urging forward. Lately I took Ptolemy's lunar system into consideration. His inequalities are menstrual: so they are in Longomontanus or Longsberg's theory, in Bullialdus', Wing's, and Street's; but in the Alphonsine tables they are annual, as they are in Copernicus and Horrox. Kepler uses both ways, but most commonly that of Longomontanus, who affirms them equipollent. I was concerned to find how they came to be so; and, after some little time, found how it was; and how Longsberg, Bullialdus, Wing, and Street, came to make the moon approach nearer the earth in the octants and quadratures, than in the syzigies in the same anomaly, when she really removes farther of. This I intend to impart to you in my next letter. In the mean time, I hope you will not forget to observe the satellite eclipses, when they come conveniently to be observed in the evenings, and impart them to me as you used to do. 1707-8, Feb. 12, in the evening, my wife coming in, told me a bright star was very near the moon. I saw it was Venus; and with the seven-foot glass observed her covered at $6^h\ 55\frac{1}{2}'$. All the country hereabouts were amazed at the sight; and I doubt not but yours in Yorkshire were as

much concerned at the necessary prodigy. If Squire Bickerstaff's predictions, a witty banter on our astrologers, have reached you, your people might conclude, as some did in Essex, that it was a confirmation of them.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 135.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, April 19, 1708.

I have yours of the 16th past, to which I intended to have sent you a speedier answer; but was prevented with a change of my affairs, that you will not be displeased to hear of. Soon after yours arrived, Sir I. Newton sent to me, to bring up what papers I had ready for the press to London. I carried a copy of a part of the catalogue with the second volume of observations, the calculated places of the planets, done by you and Mr. Witty, and some other things that might intimate to him that he had all along hindered the work; though I did not think fit expressly to affirm it. We came to an agreement; he would have the second volume into his hands; the first copy of the catalogue, to have the magnitudes of the stars inserted, which were not in it; and agreed to pay me £125, for £200 and more I have disbursed within three years past: and we must go on anew. I accepted the condition; have filled up the catalogue, as far as my own is perfected, received the monies, and now I intend to present you with £10 for your pains in calculating of Prosthaphereses for me, and other service you have done me, for which I think I had not gratified you sufficiently before.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 136.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 13, 1708.

I am going to the constellations of Cassiopeia and Cepheus, with such helps as this place affords me, in order to complete the catalogue: but, Sir I. Newton is so false and froward a person, I know not how I shall proceed. But this I know, that nothing shall happen without the will of God; and what he wills shall be always for the best: which consideration is the main support of, Sir, your affectionate obliged friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 137.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 3, 1708.

Knowing that Sir Isaac Newton would be very urgent to have the catalogue of the fixed stars completed, I set myself, not long since, to finish those of Hevelius's sextant, which I have finished,

and Hercules: this last proved a tough piece of work: some stars wanted to be observed again; others to be examined and re-stated. In this last month I have gone through with him anew; and in a day or two I hope to have him complete. Sir Isaac Newton now calls for all my things as they are; whereby he would seem modestly to intimate that they are incomplete. To prevent his design from taking effect, I am forced to leave the work I was upon, to go on with the catalogue. The Great Bear will find me a great deal of employment; but the work is not insuperable; and, when that is over, there remains nothing that will give me much trouble. This is the reason why I have delayed writing to you so long, and not sent you what I designed about the lunar theory: and, for this reason, I must desire you to respite me a little longer. You see I forget you not; and, God sparing me health, I intend to be out of your debt with my first leisure.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 138.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 23, 1708.

Not only the ill weather has hindered me from observing the satellite eclipses this year, but my weakness shall I call it, or age. For I cannot now as formerly move quick to view the planets or manage my instruments, but am apt on every change of my position to tumble; which makes me leave this work for my young people: and my present servant is not very fit for it, for he takes no great delight in it. As long as you are capable of observing them, without injury to yourself, or the like danger, I should be glad to have them from you. But this is a small return for yours; you expect something else from me, and I shall gratify you now I have a little better leisure, than when I wrote to you.

* * * * * * * *

They [the Academy of Science at Paris] tell us of a new star seen following the tail of Hydra: I have such an one in my maps and catalogue, and I remember I wondered, when I observed it, that I saw it not in Bayer, for it was a fourth-light star at least. They say it (probably) makes its returns in two years, as that in the Whale's breast does in eleven months; if so, this may be the reason why neither Tycho nor Bayer have it. I shall look for it when that part of the heavens becomes visible in the evenings, and give you an account of it.

But, that which I am most pleased with, is that they tell us that M. La Hire has made such good tables of Mercury, that, by the help of them, he has found him upon the meridian, and since observed him frequently there. The first time he observed him on it was Oct. 12, 1699; but, he adds, that sometimes he could not find him, though he was farther removed from the sun than he was at other times when he had seen him; and hence draws an excuse for the fifth satellite of Saturn being sometimes visible, at others not. I have an observation of yours in my notes, 1689 or 1690, of Mercury seen on the meridian, with his meridional zenith distance. I am sorry you did not prosecute this planet then; my eyes are so much decayed, I fear it will be in vain to seek him now, otherwise my glasses are not much, I believe, inferior to La Hire's; my instrument is almost two foot more radius. I have tried to get La Hire's tables from Holland, but as yet I cannot.

I am going into Surrey next week, and shall be absent about five weeks: in the mean time you

may direct your letters hither, as usually: my niece, whom I leave in the house, will send them to me, and convey my answers to you as usually.

As for the progress of my works, I can say nothing at present. Sir Isaac Newton endeavors, cunningly, to have thrown all the delays, he has caused, upon me: but I have showed they were not caused by me. I have not charged him with them: but, if he can find nobody else to fix them on, he must bear them himself, and shuffle off, with his usual pretences and tricks, as he can. I never met with his fellow, for such cunning; and I hope in God I never shall again. However, I am satisfied it lies not in his power to prevent what God wills.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 139.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Novem. 22, 1708.

Finding nothing in Sir I. Newton but contrived delays to hinder my work from going on, I resolved however to proceed as I could; and therefore, during my stay in the country, set on the constellation of the greater Bear: but the work went on but untowardly there, by reason both of cross affairs, and the want of my original observations. Since my return, I have determined the right ascensions and distances from the pole of all the stars I had observed, to my own satisfaction. This is the fourth time they have passed under my hands. At present, their longitudes and latitudes are calculating by Isaac Woolferman, my domestic servant; and one Mr. Ryley, a pupil of J. Hodgson's, who lives in the town, repeats them. I have above 220 stars in this constellation, which will be broken into Hevelius's Asterion and Chara, his Tyger, Leo Minor, &c., as soon as the calculations are finished: for I think it most convenient to follow his model, rather than to breed confusion by making any new constellations of my own.

In the mean time, I have had much disturbance by a cold, the common distemper of the past month, and my lameness, proceeding from something like the gout; which, nevertheless, has not hindered me much from proceeding in my design. The constellation of Draco is under my hands: this contains about 80 stars already observed. I have laid a good ground to proceed upon, and made a good entrance; if God continue my health, I doubt not but to have it finished before Christmas: after which, I have only the lesser Bear to manage, with Hevelius's Moneceros, and the Southern Fish: the two last have passed once under my hands already, and I foresee will give me little trouble. The lesser Bear will not be difficult, for I have distances determined ready for these stars; so that I hope, with God's assistance, to finish all by Lady Day next.

You see the present posture of my work. You will fear the decease of his Royal Highness may hinder the progress of the press. I hope not at all: 'tis at a full stop by Sir I. Newton's practices. The dropt sheet is printed, but the printer has not yet sent me a proof of the next, which is necessarily to be reprinted, though I was promised it two months ago. As soon as I get it, I shall send you all the sheets you want, except I hear they will go on with six sheets more I have put into Mr. Hudson's hands, to be added to this volume, containing the planets' places calculated by you and Mr. Witty, from distances *a fixis* observed with the sextant: which if they go on to do, I shall detain these in my hands till these are finished; if not, I will send them as they are, as soon as I can.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 140.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 24, 1708-9.

I have got finished all my calculations of the stars' longitudes and latitudes for my catalogue this week: but it will require some time and labor to insert the variations of the longitudes in them, and to correct some stars that have not been well determined as they stand at present. I reckon that I have the places of 3000, within a few, under or over, determined very exactly. I have near 600 visible places of the moon, calculated from the observations by yourself, Mr. Witty, Mr. Ryley, and I. Woolferman: the last was my domestic servant, left me last month, and is gone, with my consent, a conductor of the train of artillery, to Port Mahon, with an allowance of 2*s.* 6*d.* per diem and a gunner's fee besides.

The places of Saturn, Jupiter, and Mars, I have all along derived from the observations as I took them: so those lie ready for transcription. And now, I bless God for it, I have all things in good readiness for the edition of my works; but I commit all to the ordering of his Providence: and, since Sir Isaac Newton has put a full stop to the press, shall not urge it forward again, till I see a good fund settled, and secured, to carry it on, without any danger of impediment, or obstruction from him, or any of his tools.

I suppose you have heard that Dr. Gregory is dead. Mr. Caswell, my friend, is chosen to succeed him in the Astronomy Professorship at Oxford. Mr. Keile put in for it. Mr. Halley did all he could to serve him, that he might marry his daughter; but his vile character caused some sober persons concerned to urge Mr. Caswell to accept it; who resigned his Divinity Beadle's place, worth more than £200 per annum, for this, worth about £120. The good man thinks this enough for him, and is well enough pleased with his change: and I think him very wise in what he has done.

Mr. Newton, the Mathematical Master at Christ's Hospital, has resigned; that is, is turned out for insufficiency: and James Hodgson succeeds him, and has been in that school ever since Christmas; and I hope will discharge his duty faithfully as he ought: I am sure he wants no endowments.

I am sorry to hear that you have not your health; this winter I have enjoyed mine better than usually, by reason that I kept more within doors, and did not expose myself to the cold so frequently as formerly. I bless God for this, and would advise you, as your years advance, abate your diet, avoid cold, use moderate exercise, and you will find, through his blessing, better effects of it than you could expect from physic.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 141.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, June 13, 1709.

I should beg your pardon for my long silence, but that I know you will easily excuse me, when I acquaint you that, ever since I received your last, I have been very busy in collecting the right ascensions and distances of the moon and planets from the pole, got by the help of the meridional arc; and determining the requisites for finding their places from them.

I have finished those for the moon, to the end of the year 1705; and of the planets to the be-

ginning of the present. And now the work of calculation lies in the hands of Mr. Riley and my servant; who, I hope, will finish it before I go into Surrey, which will not be this three weeks yet; because the rainy weather makes the harvest backwarder than usually. I bless God for this good success, and my health continued; though I grow lamer than I was, and I doubt not but his Providence will afford me a happy opportunity of publishing all. However, Sir I. Newton does all he can to hinder me: he is sensible all the blame of these delays is justly laid at his door: to remove it, he endeavors to transfer it to the Office of the Ordnance; but they are sensible of his cunning. I am so to carry myself, as not to decline the patronage of my very good friends at the Tower; but to embrace it, and at the same time to let them see how my old acquaintance would use them, and has endeavored to rob them of their due. But this business will not come on till the Master of the Ordnance, the Duke of Marlborough, returns from Flanders: so that you are not to despair of seeing my volumes completely printed, though I meet with obstacles and delays. Sir I. Newton tells some people there is no need of printing the observations made with the mural arc, but only the tables for the use of our seamen. This is a cunning suggestion to spoil the work. These must be printed before the catalogue passes out of my hands: they are copied in 175 sheets: the constellations must next be drawn and engraved; and then I shall part with the catalogue and planets' places, derived from the observations, but not willingly before. To do otherwise would be to set the cart before the horses; which he has always endeavored to get done, either in order utterly to stop and spoil the work, or to give me such vexation and trouble as should make me throw it up. This is his way of promoting it.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 142.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

Burstow, Aug. 19, 1709.

I have almost completed all the calculations of the planets' places, derived from my observations made at Greenwich from 1676 to 1709; but, of the moon, only to 1705 completed. The visible places of the moon, determined by you, Mr. Witty, and my servant, are about 1000; of the planets near as many; of the fixed stars about 3000; which is such a stock as the world never saw before, and will be a sure foundation for the theories. So far has God blessed me, and I doubt not but he will afford me an opportunity to see them published, since he has afforded me strength and means to carry the work on to such perfection.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 143.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Octob. 25, 1709.

This comes to inform you that, on Thursday last, my servant delivered to Mr. Stanfield, at Mr. Knap's, all the printed sheets of my *Historia Cœlestis*, from page 101 to the conclusion; except the reprinted copy of the first sheet of the *Maculæ*, which I could not get printed, nor so much as

set my eyes upon. This is Sir I. Newton's return for all my obliging civilities and kindnesses, of which you have sometimes, though many years ago, been witness. He is now removing to Chelsea, and has been lately much talked of; but not much to his advantage. Our society is ruined by his close, politic, and cunning forecast; I fear past retrieving, for our Doctor's Transactions have been twice burlesqued publicly; and now we have had none published I think this four months. I have corrected the errors of the press, in the copy I send you, but to the beginning of the observations of solar spots: the rest you may correct by the errata, which I have caused my servant to transcribe on the void half sheet of this letter. You will excuse me for this long delay of the performance of my promise caused only by Mr. Hudson's carelessly losing those sheets I put into his hands to send you: which has forced me to break the only entire spare set I have by me. I do not promise myself much good from the hopes I was put into lately; nor am I but very little concerned about printing the rest: now I am ready for all events. God's will be done.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 144.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Dec. 1, 1709.

SIR,

I am heartily glad to find by yours, of the 4th past, that you have received the remaining printed sheets of my envied *Historia Cœlestis*. I was much concerned that I could not send you them sooner; and not easy till I had put them into Mr. Stanfield's hands, to prevent another miscarriage. You are often concerned in them, but much more in what remains. Besides, you have had so great a share in them, by making the mural arc, and calculating the planets' places from the observations made with the sextant and it, that I can never forget to serve you when it lies in my power, nor in the least neglect you. You deal hardly with me, when you write that I threaten you shall hear seldom from me; for I desire to hear often from you, but am loth to put you to the expense of my answers to yours, when my letters shall bring you nothing that is worth the postage. Assure yourself I rejoice both when I have a letter from you, or meet with anything that may furnish me with matter for one to you.

I told you some time ago I had finished all my calculations of the stars' places for my catalogue, and that they are double the number of Hevelius's, and treble of Tycho's; how much they are exacter you know, and can judge better than any person I am acquainted with: besides, I also acquainted you some time since, that the lunar calculated places would be about 1000. They are now finished to the end of the year 1705.

I am now setting one Mr. Ryley, a very ingenious young man that lives in Greenwich, and is ready at numbers, to calculate the moon's visible places from my new tables, to the first 100 observed. The true places have been already calculated by the tables, by Mr. Witty, and Woolferman: my present servant, Joseph Crosthwait, a Cumberland youth, is to repeat Ryley's calculations, or rather work against him; for I do not use to let him see another's calculations till he has finished his own, to be sure of his work.

When this is done, we shall see very easily how near the tables agree with the heavens, and whether the new inequalities introduced into the lunar system are of mine or not, and, probably, what further emendations are to be applied. I shall not get them into this work till the latter end

of the next week. I have by me a nonagesimary table, calculated by you, which supposes the latitude 51° 28′ 10″, as I used it when you lived with me. I have since determined it better, 51° 28′ 30″; it will cost me some little labor to correct the table for this alteration: for use, it may stand as it is; but I must make it, however, fit this alteration. I shall give you a small table for the correction, when I have finished it; and if you think fit, and can take pleasure in comparing the tables with your observations, I shall pick out another hundred, to find you employment at your spare hours.

The places of the superior planets are all derived from the observations to the end of the year 1709, these are about 1000 in number; that is about twice as many as we have from all the astronomers that have been before: for Tycho has given but a few places derived from his observations. Gassendi and Horrox's are scarce to be depended upon, because taken only with a forestaff; and Gassendi gives no places derived from his. Hevelius designed to have given the places deduced from his, but there are none of them come to our hands. Besides, Tycho's and Hevelius's catalogues are faulty and imperfect, by reason they assumed the obliquities of the ecliptic too big; and the plain sights, wherewith they observed, rendered these places doubtful: we go on surer grounds, who have used prospective sights, a just obliquity, and a full catalogue. I am questioning whether I ought not to lay aside the old observations of Hipparchus, which Ptolemy has given us; and correct the motions of the sun, by comparing Bernard Walter's solar observations with my own. He lived 200 years ago, and was very diligent and careful; and perhaps he may be relied on to 4 or 5 minutes: whereas, if we compare Ptolemy with each other, it will be evident he has erred above half a degree, perhaps a whole one, or an entire day, in determining an equinox. Which makes some learned men think that he has wrested either his own, or Hipparchus's observations, to make them agree to his year of 365 days, 5^h 55′: which is 6 minutes too big per year; and, in 240 years, will make a whole day's error. I have heard Mr. Street complaining of him, and intimating that Ptolemy had no exact account of the Asian years and months, or that he did not well understand them: I am sometimes inclined to be of the same mind; but when I read in Pliny that Hipparchus made ephemerides (by which word I understand only calendars), for 600 years, it makes me suspend my opinion, till, by comparing some of my own observations with Walter's and Albategni's, or some other way, I may be able to determine. You see, hereby, whereabouts we are: I have only laid a better foundation for astronomy than those that have gone before me. If God spares me health, you see which way I intend to move. I commit all things to his direction, pray for your health, and doubt not of your assistance whenever necessary for, Sir, your real friend and servant,

JOHN FLAMSTEED.

[Extracted from MSS, vol. 33, pages 81 and 82.]

No. 145.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Feb. 11, 1709-10.

SIR,

Yours of the 6th past found me busy in considering the motions of the planet Saturn's opposition to the sun I have observed him at, in the last days of December. Mr. Bossley, whom I have sometimes mentioned to you, as employed by me, wrote me a letter, at the same time, wherein he told me that his former attempts to represent my observations of this planet's motions having

failed, he had tried anew, and sent me the result of his determinations; which I have since examined and find them not sufficient. The French having endeavored to do the like, it gave me occasion to examine theirs, which I find worse than his. It was not till yesterday, that I determined what was to be done in the business; but I am not yet fully resolved: only thus much, I think I foresee that I shall be able to answer all the good observations of the last 200 years, within six or seven minutes; and the old one that was made almost 2000 years ago tolerably well. And this much I dare assert from my work:

1. That the aphelion of Saturn moves much swifter forward, than it has usually been thought, *inter fixas.*

2. The node recedes *inter fixas.*

3. The greatest equations are not much different from Kepler's.

4. The inclination of his orbit about $1\frac{1}{2}$ minutes less than he or Bullialdus makes it.

I hope, next week, to get new tables ready for my calculator to compute his places by; to make a more limited and exact correction. I must add more, that his mean motion is much slower than Kepler and Bullialdus make it. As soon as I have done with this planet, I intend to proceed to Jupiter; for whom I have some tables also made formerly by Mr. Bossley, which will be useful to me. This work has hindered me from proceeding to get my great catalogue transcribed: but that is labor only fit for my young man. So that 'tis no loss of time; but an advantage it will certainly be to the public, to proceed with what has fallen under my hands, till God affords me an opportunity to go on with the proper work, which I commit wholly, as I do all my affairs, to his good Providence: with whose disposal, let them be what He pleases, I shall ever be very well pleased.

You now see the reason why my return to yours has been so long delayed; and I hope excuse me for it, and will pardon me if I should hereafter be as long before I answer some I receive from you: for I shall not be very easy till I have got this planet's motions off my hands, and perhaps I may at the same time fall on Jupiter's, who I hope will not give me so much trouble; but whom I shall more desire to bring to rules, because the motions of his satellites require the true knowledge of his.

I must add concerning Saturn that, whereas Sir I. Newton suggested to me that all the planets increased in their bulk continually, by an accession of matter from the tails of comets passing near them, and resolutions of matter from the ether about them, this now seems not probable. Mr. Halley had told him that the motions of Saturn were slower, this last 100 years, much than formerly. I have tables of Saturn by me, of his making, presented to Sir J. Moore, wherein he makes Saturn's motion in 100 years 26 minutes slower than 'tis in the *Caroline Tables.* Now, if the planets grow slower in their motions, they must consequently remove farther from the sun, and there is no reason for their removing farther from the sun except they increase in bulk and weight: but I do not find that Saturn moves any slower now than he did almost 2000 years ago. Which makes me think our earth, and the other planets, have gained little or nothing from the tails of comets, and that the fumes from them have filled our orbit from the sun as far as the orb of Venus with that matter, which causes the light we see, in the moonless nights, about the time of the vernal equinox; of which M. Fatio has given an account.

Whilst I have been upon Saturn, I have found a period that may be of very good use to our ephemeridists: 'tis 206 years, precise; after which, he returns to the same place very near in the ecliptic, at the same distance from the sun.

A period of 83 years reduces Jupiter nearly in like manner. I hope you are provided to observe

the eclipse of the sun, that happens on Friday next the 17th instant; I shall desire to see your observations of it: what I gain shall be sent you as soon as I can get leisure to put it in order, God sending health to, Sir, your friend to serve you, JOHN FLAMSTEED, M.R.

My knees and ankles grow very weak, so that I cannot get into the stage-coach without help, when I go to London.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 146.) *The Account of Sir Isaac Newton of the expenses of printing the Astronomical Observations of Mr. John Flamsteed, by order of his Royal Highness the Prince.*

Charge.	£.	s.	d.	*Discharge.*	£.	s.	d.
Received of the Treasurer of his Royal Highness	375	0	0	Paid to Awnsham Churchill, bookseller, for printing 98 sheets, at £1 14*s.* per sheet . . .	166	12	0
				Paid to Mr. John Flamsteed, 26th March, 1708, in part for his charge and trouble in preparing papers for the first and second volume of his observations and correcting the press . . .	125	0	0
				Paid to Mr. Machin for examining Mr. Flamsteed's copy by his minute-books, and also for repeating and correcting his calculations	30	0	0
				Paid to Mr. Churchill for his extraordinary charge in printing marginal notes in 60 of the above 98 sheets	6	0	0
				Paid Mr. Churchill for his charge in altering two sheets of specimens of the work in the beginning .	2	5	0
				Paid Mr. Churchill for his loss in providing certain sorts of stamps and rules for the whole work, which will be of no further use to him	20	0	0
				Balance due to his Royal Highness the Prince's Administrators .	25	3	0
	£375	0	0		£375	0	0

April 8th, 1710. This account was examined and approved by us,

Is. NEWTON. T. ROBARTES. CHR. WREN.

Jurat, 17° die Aprilis, 1710, coram THOS. BURY.

[Copied from a MS, in the possession of Dawson Turner, Esq.]

No. 147.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 14, 1710.

Since my last to you I have been frequently employed in correcting the motions of Saturn, Jupiter, and Mars: as for Saturn, I find I cannot make any numbers, as yet, that will represent all my own observations nearly; but they will be too slow in Bernard Walter's by 10 or 15 minutes, and about 8 minutes, in Hevelius, of time too fast; which intimates a secular intention and remission of his mean motion, and another inequality arising from his position in respect of Jupiter. About six years hence he will come to his mean longitude again; and observations made of his place, two or three years before and after 1716, will probably show us more of these inequalities, than we can conclude from any observations made since Tycho's time. In the mean time I am pleased to find that I can represent all his observations, within five or six minutes, my own nearer within three or four, and those betwixt us, of Hevelius, within seven or eight.

But my success in Jupiter is not so good: he intends and remits his motion strangely. From the year 1664 to 1676, it seems to agree with Mr. Bossley's mean motions: from 1676 to 1688 it retards about 12 minutes: from 1688 to 1699 it accelerates one minute: from 1704 to 1710 'tis retarded 10 minutes; yet this I am pleased with, that the errors are never more than eight minutes; whereas Kepler's Tables sometimes are faulty about 16. Kepler's numbers err in Mars sometimes near half a degree: but in the mean motions a correction of about eight minutes will take this away: so that I hope he will cause me less trouble than either Jupiter or Saturn has done. And I doubt not of as good success with Venus, in whose motions I find near the same fault; but I have not got my observations of her yet calculated, by the Tables, to see how much correction she will require.

I never yet saw Mercury in the sun: some of the French mathematicians have: I shall leave this planet to them and posterity; who, by the help of my new catalogue, will easily find his places. I have neither health sufficient for this planet, nor can I stand, as is requisite, to make the necessary observations of him, being now within a month of 64 years old complete.

You know what I have done towards rectifying the motions of the moon. I advance towards her as my age permits me. This winter I may perhaps do something more towards a further correction of them; and if I find it needful to require your help, you need not doubt but I shall impart to you all that is requisite for the thorough understanding of what you shall be upon; for I have ever dealt candidly with all men, and from you I never did, or will, or need I hope to, conceal anything I have under my hands.

The greatest error of my Tables in the moon I find to be, as in Jupiter and Mars, about eight minutes.

Sir I. Newton has put our Royal Society into great disorder by his partiality for E. Halley and Dr. Sloane, upon a small and inconsiderable occasion: so that they have broke up some few weeks before their time.* Dr. Harris has lost all his reputation by actions not fit for me to tell you. The French Academy affords us nothing of late: the ill-success of their public affairs, I fear, has ill

* This probably alludes to a dispute in the Council of the Royal Society between Dr. Woodward and Dr. Sloane. Dr. Woodward was requested by the Council to make an apology; and, on his refusal, was ejected from the Council. He afterwards moved the Court of Queen's Bench to be restored. See the MS Minutes of the *Council* of the Royal Society of the dates of March 29, May 3, May 24, and June 17, 1710. F. B.

influences upon them. We are at present under apprehension here, but I doubt not that good Providence that has hitherto watched over and guarded this nation, will still defend us; and turn all to good. That God Almighty may, and evermore, bless you with content and peace, and a long enjoyment of your health, is the hearty prayer for you of, Sir, your real and affectionate friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 148.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Sept. 20, 1710.

I have not finished Saturn yet, nor can well till I see how his places will be represented by my new numbers, five or six years hence. I must expect the same for Jupiter. I cannot bring my numbers nearer in Mars than within five or six minutes of my own observations; though they will agree better, I think, with Tycho's: but then I cannot think his exact more than within that quantity. On the whole, I plainly perceive there is a cause that vitiates their motions; and it seems to be their mutual gravitations on each other, and pretty regular. So that I do not doubt but to solve them: for, their gravitations being reciprocally as the squares of their intermutual distances, and directly as the bulk of their bodies, it will not be difficult to determine how much they draw the remoter planet inward towards the sun, or force the nearer from him. Now, their distances being as the cube-roots of their revolutions squared, it will be found how much their revolutions are accelerated or retarded, and consequently how much they move swifter, or slower, on this cause. Carry on the thought, and you will see that the place of the aphelion will be altered by the same cause; but to find in what proportion, will require much thought and pains. I only find it is so in Mars; and to represent two observations made at his conjunction to the sun, but four years distance (and abundantly confirmed by others taken both before and after them), it will be requisite to alter the place of his aphelion near half a degree: whereas the mean motion will alter it but about four minutes in that time. This is the consequence of Kepler's doctrine of magnetical fibres, improved by Sir Christopher Wren and prosecuted by Sir I. Newton: and I think I can lay some claim to a part of it; for I asserted it in a letter I wrote to Mr. Crompton, of Cambridge, about the great comet, in February, 1680-1.* You lived with me in 1684 and 1685, and I hope took a copy of it: which if you did, you will exceedingly oblige me by letting me know it, and sending me a transcript of it and the figure. For though I inserted the substance of that letter into my lectures at Gresham College, which I have by me, yet I am very desirous to get a copy of that letter, because I have two letters of a friend in my hands that relate directly to it. You need not copy the figure of the comet's path, for I have kept that.

[Copied from the *original* letter in the possession of Mrs. Giles.]

* This letter is printed in the *General Dictionary*, under the article "Newton." See the note in page 51. F. B.

No. 149.) *First draft of Mr. Flamsteed's Petition to Queen Anne.*

Dec. 29, 1710.

To the Queen's Most Excellent Majesty. The humble petition of John Flamsteed, her Majesty's Astronomer at the Royal Observatory, in Greenwich Park, humbly sheweth,

That your Majesty having signified by a letter from your Secretary, Mr. St. John, dated the 12th instant, that it was your Majesty's pleasure that I should admit the President of the Royal Society, together with such others that the Council of the said Royal Society shall think fit to join with them, to be constant visitors of the said Observatory, to give me directions for making such observations as they shall think necessary, and for purchasing such instruments as belong to me, and that I should yearly give them an account of such observations as I have made;

I humbly crave leave to represent to your Majesty, that the Observatory having been built by the order and directions of your Royal Uncle King Charles II., in the years 1675 and 1676, it was left to my discretion to continue and prosecute my observations in such manner as I thought fit.

That the instruments with which it is furnished are all of them either such as were given to me by Sir Jonas Moore, or purchased and built at my own cost.

That all the necessary observations have been made by me without directions from any person, there being very few persons in your Majesty's dominions that were fit to advise or direct me in my business.

That I have observed the places of the fixed stars and planets, according to my general instructions in your Royal Uncle's warrant.

That his Highness, your deceased consort the Prince, having seen my charts of the constellations, and having had the printing of my works recommended to him by the Professors and Fellows of the Royal Society, out of his affection to liberal arts and sciences, and a desire to promote those which are most useful to the nation, was pleased to order them to be printed, and to assign £1200 for the expense.

That the 1st volume, containing the Observations made from 1676 to 1689 is printed, but where disposed of your petitioner knows not.

That the 2nd volume, containing all my Observations from 1689 to 1705, fair copied in 175 sheets of large paper, was put into Sir Isaac Newton's hands March 24, 1707-8; and that soon after your petitioner received of him £125, in part of more than £173 it has cost him in calculators and copiers; and is now in his keeping for aught he knows.

That during the time the 1st volume was in the press, he often delayed its progress without any reason given, or on light and frivolous pretences, and after he had got the 2nd into his hands, he absolutely stopped it, and always showed an aversion to its progress.

That since that time I have finished the catalogue of 3000 fixed stars, which I have by me ready to be transcribed: that I have moreover by me such a number of the places of the planets derived from my observations by myself and such assistants as I have hired at my own expense, as no age before has seen.

That I have made further advances than 'tis proper to mention here, and might have presented your Majesty with the whole work perfected before this time, if his Royal Highness's noble intentions had not been prevented, and my endeavors continually obstructed by those who ought, and whose duty I conceived it was, to have seconded and promoted both.

That in 35 years I have spent in the service of your Majesty and your predecessors, I have expended a large sum, more than my appointments, in making instruments, and necessary assistance, and educated more than 100 brave youths that have passed into the public service.

That I may not have the President of the Royal Society, nor any of their Council set over me as visitors, nor suffered to prescribe to me what observations to make, since they know little of my business, and will but incommode me in my progress, and obstruct me, as some of them have done formerly; but [that] such of the nobility or gentry that are skilful in mathematics, together with the principal officers of your Majesty's Ordnance, that have been founders of my studies, may have the inspection and care of the Observatory.

And that your Majesty of your Royal goodness would please to order that the rest of the monies assigned by his Royal Highness for the carrying on and printing the Observations made at your Majesty's Observatory, and engraving the plates of the constellations and other requisite charges, may be employed to that purpose; that so I may see a work so useful to the nation, and so much desired and wanted by all ingenious persons of all nations, perfected and presented to your Majesty, and by you to them. For its accomplishment no expense nor pains my circumstances will bear has been spared, nor shall be, so long as the blessing of God gives life and health to me; and your petitioner shall ever pray for your Majesty's health, prosperity, and long life.

[Copied from the *original*, in MSS, vol. 35, page 93.]

No. 150.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Jan. 23, 1710-11.

December 15th last, I received a letter from the Secretary of State, that signified to me that it was her Majesty's pleasure, for the improvement of astronomy, to appoint the President, Vice-President, and such others as the Council of the Royal Society should think fit, to be the constant visitors of the Observatory; that they should see her Majesty's instruments repaired; and purchase those that were mine; and that I should every year give them an account of what observations I made; and make such as they appointed. A like letter was sent to the office of the Ordnance, and one to the Royal Society. It happens very unluckily for the procurer of these letters (you know who he is), that all the instruments in the Observatory are either absolutely given to me specially, and not to the Observatory, by Sir Jonas Moore, or else built at my own expense and charge: and I have neither any need nor desire to sell them; so that part of the letter fails. As for the other, Dr. H. Sloane is the sole Vice-President: the Council, I am apt to think, consists of persons not less ingenuous than he; so that I am in little pain about this visitation. But to obviate the inconveniency, that might emerge from this letter, God has raised me some friends that I hope will give the Queen a true state both of the Royal Observatory and Royal Society; and doubt not but in a little time the latter and its President and Vice-President will be ashamed of their attempt.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 151.) *Letter from Dr. Arbuthnott to Mr. Flamsteed.*

London, March 14, 1710-11.

Sir,

Her Majesty having commanded me to take care that the *Historia Cœlestis*, which was begun by his Royal Highness's order, and carried on at his charge, should be finished as soon as possible, and that it should appear in a dress suitable to the honour of such a patron, I should fail in my duty if I did not acquaint you that there remain several things to be performed on your part towards the perfection of so useful a work; and particularly what retards us at present is, the want of your most accurate catalogue of the fixed stars, which the world has so long wished to see. The copy you have hitherto delivered is imperfect, wanting the six northern constellations of Draco, Ursa Major and Minor, Cepheus, Cassiopea, and Hercules; Draco and Ursa Minor wholly, and the rest without longitudes, and latitudes, and differences: so that for want thereof they are disabled to proceed on the edition. Therefore, I desire you will deliver into my hands, as soon as possible, a perfect copy of your catalogue of the fixed stars, and you shall have a receipt, in due form, upon the delivery of it; and I can assure you there shall no pains be wanting that both the catalogue and the rest of the work be published in as creditable a manner as is fit for so useful a work. I am the more fully persuaded you will comply with so reasonable a request, because of the regard you have for the memory of the Prince, as well as for your own reputation, both which are interested somewhat in this performance. I expect your answer by the bearer, or as soon as you can; being, with all respect,

Your most humble servant,

Jo. Arbuthnott.

[Copied from the *original* MSS, vol. 35, page 89.]

No. 152.) *Letter from Mr. Flamsteed to Dr. Arbuthnott.*

The Observatory, March 23, 1710-11.

Sir,

'Tis no small satisfaction to me to find by yours of the 14th instant, received this week by Mr. Hunt, that her Majesty is pleased the *Historia Britannica Cœlestis*, that was begun to be printed at the charge of his Royal Highness, should be published as soon as possibly it can; and appear in a dress worthy so great and excellent a patron. It has been always my endeavor, and is still the same, that it should do so: in order to it, as soon as I found the press at a full stop, I carried on the large catalogue of the fixed stars with all the diligence, speed, and care I could; and completed it as far as I thought would be necessary, till it should come to be printed. I had no sooner done this, but the good providence of God (that has hitherto conducted all my labors, and, I doubt not, will do so to a happy conclusion) afforded me an occasion of carrying them much beyond those bounds which I had first proposed to myself, or could reasonably hope. The great differences I found, this and some foregoing years, betwixt the planets' places in the heavens, derived from my observations, and their places calculated by the best numbers, with a small intervening accident, put me upon forming new tables for one of the superior planets. My success herein carried me on to a second and third. I have now the fourth under my hands, and a large stock of materials ready for the rest. By what I have done, I have found wherein the faults of the common numbers be, and how they are to be limited and altered; but a great deal more help is requisite, and must be procured, to calculate the new tables, and the planets' places therefrom, to render the

work complete, worthy of the British nation, the name it bears, her Majesty's patronage, and to commend the memory of his Royal Highness to posterity. In order to which, it is very necessary that I should have a few hours' discourse with you, if possibly you can, at the Observatory; where I can show you the result of my endeavors, and we might consider together how to carry on the work, and keep it free from such hinderances and delays as have formerly retarded the progress of it. I will draw up some short notes for this purpose, against you come to dine with me; but if your necessary affairs and attendance will not allow you to afford me that favor, please to let me know, by a short note, at what hour and day in the week, except Monday morning or Saturday afternoon, I may find you at best leisure. I will wait upon you, and discourse more fully with you concerning this business, and I shall esteem it a favor done to, Sir, your obliged and humble servant,

JOHN FLAMSTEED, M.R.

[Extracted from MSS, vol. 33, page 87.]

No. 153.) *Letter from Dr. Arbuthnott to Mr. Flamsteed.*

London, March 26, 1711.

SIR,

I received yours, and am extremely glad at any improvement so noble a science as astronomy can receive; and shall be willing, as far as lies in my power, to give my helping hand towards publishing those Observations and tables mentioned in your letter: but that being beyond my commission (which was only to oversee the publishing of the Observations which were given in to the referees before his Royal Highness's death), I cannot at present say anything more to it than that, when these are printed, I shall be ready to solicit her Majesty that these may be published as an Appendix to the work. What I desired in my letter was, that you would be pleased to deliver to me those constellations that are wanting in the Catalogue you have already delivered, or such of them as you have complete. If you have nothing more to add to the catalogue, let me know so much by a line, and I shall order the press to proceed with what we have. I beg your positive answer to this, for the press at present stands still, and I am complained of for delays. I shall be ready to wait on you anywhere in town, and at any hour, only sending me a note in the morning or night before.

I am, with all respect, Sir, your most humble servant,

JO. ARBUTHNOTT.

[Copied from the *original* in MSS, vol. 35, page 97.]

No. 154.) *Letter from Mr. Flamsteed to Dr. Arbuthnott.*

The Observatory, March 28, 1711.

SIR,

I am obliged to you for the favor of yours received this morning by the bearer, and the more because it expresses your good will so fully to her Majesty's Observatory. A small touch of the gout, that came upon me last Sunday, kept me at home; but I thank God that I have now no pain, so that I hope, nevertheless, that I may be in London by the stage-coach to-morrow, by

11 o'clock or soon after, where, if the occasions permit, you will find me at Garraway's Coffee-house; and I shall inform you of the state of my work, it being too long to be told you in a letter, for an answer to which the messenger stays. I am, with all due respect and hearty thanks,

Your most obliged humble servant,

JOHN FLAMSTEED, M.R.

[Copied from the *original* draft in MSS, vol. 35, page 99.]

No. 155.) *Letter from Dr. Arbuthnott to Mr. Flamsteed.*

London, April 6, 1711.

SIR,

I send you what is left of the duplicate catalogue in Sir Isaac Newton's hands, the rest having been long since delivered to your kinsman Mr. Hodgson. I hope you will find me what is wanting, with all speed; being I am called upon to send it, which makes me the more importunate.

This is from, Sir, your most humble servant,

JO. ARBUTHNOTT.

[*Note, written on the letter, by Mr. Flamsteed.*]

Came to Greenwich, Tuesday, April 10, *manè*, when I was going to London, where I stayed till Saturday noon the 15th, and was to wait on the Doctor twice; Friday, 13th, twice; but found him both times absent.

[Copied from the *original* in MSS, vol. 35, page 101.]

No. 156.) *Letter from Dr. Arbuthnott to Mr. Flamsteed.*

London, April 16, 1711.

SIR,

I think I undertook, and promised to you, that your catalogue of the fixed stars should be correctly printed, and that I would take the blame upon me if it was not: how much was printed at that time I really did not know, but I will be faithful to my promise. You complain that, by the alteration of Ptolemy's names, all the ancient observations will be rendered useless: as to that, I can answer that Ptolemy's names are religiously adhered to as far as is consistent with the order that is observed in the Britannic catalogue, which differs very much from Ptolemy's. And to please you, everywhere *preceding* and *following*, *north* and *south*, *upper* and *lower*, are put instead of right and left: for you know you were not well satisfied with Ptolemy's postures, and to remove the objection I have compared one of the constellations mentioned, with Ptolemy's catalogue, and will undertake immediately to find any star of Ptolemy in your catalogue; so that I cannot see how the ancient observations are rendered useless. You know Ptolemy's order, and number too, differ much from yours; but the inferences are as plain as can be. As to some alterations in the numbers of your catalogue, they are plainly corrected in this; and which to be sure you will stand to, because they are slips of the pen, or computation in the copy, which you gave us, and which this was printed from. But that I may still proceed with all candor in this matter, I beg still, that you would find

me the constellations that are wanting, that they may be inserted; and if after the catalogue is printed, you do not agree to the corrections, upon a just representation of the exceptions, your own shall be printed just as it stands, and you shall correct it yourself. If you will not agree to this, to find me the constellations that are wanting, we must be contented with what we have, and be at the pains to compute them from your observations, which is a little hard, considering that you can supply them, and have promised so to do. I can say no more on this matter, but beg your answer as soon as possible, being, with all respect, Sir, your most humble servant,

JO. ARBUTHNOTT.

[Copied from the *original* in MSS, vol. 35, page 103.]

No. 157.) *Letter from Mr. Flamsteed to Dr. Arbuthnott.*

April 19, 1711.

SIR,

I met with yours of the 6th instant at the Observatory when I returned from London, on Saturday last, and with it, the copy of my catalogue of the fixed stars for the six latter [signs] of the zodiac; the rest you tell me was left long since with Mr. Hodgson: he assures me they never came to his hands, and I am as sure they never came to mine. On the left-hand side of this copy, I had caused Ptolemy's Greek names to be wrote against the stars to which they belong in my catalogue. This, I am apt to believe, is the true reason that part is detained, and you are told it was returned; for had you seen them, you would have seen what an outrageous fault Dr. Halley has committed in altering the Ptolemaic names in my catalogue. I lent a fair Ptolemy to Sir I. Newton; I believe 'tis still in his hands: if you please, you may do well to send for it, and collate my translation, and Dr. Halley's with it, for your own satisfaction.

In yours of the 16th instant, you acknowledge your *promise that my catalogue should be correctly printed.* I have seen, as yet, only the first and third sheets of it; in the first, I have noted more than forty alterations and deviations from my copy, and as many in the third, with which, I am apt to think, you had not been acquainted. I could wish you would order all the sheets printed off to be sent me, that I might give you all the faults made in my works by this confident person, all together. You tell me that *Ptolemy's names are religiously adhered to.* I fear you write only by hearsay; for if you please to compare his Greek and my translation with Dr. Halley's, you will find everywhere notorious differences; his names being sometimes contrary to Ptolemy's, and sometimes not *approaching sense.* You add, *as far as is consistent with the order of my catalogue;* this, I fear, is only on hearsay again: for the order of my catalogue obliged me not to make many alterations of Ptolemy's names, nor any that were anyways considerable, as you will see, if you compare my Latin with his Greek names. But you proceed, and say, that to *please me, everywhere preceding and following, north and south, upper and lower are put, instead of right and left;* this, Sir, is what I complain of, though but a part of it.

Ulug Beig follows Ptolemy strictly in his Arab catalogue; so does Copernicus, Clavius, and Tycho (saving that Tycho makes small alterations, when he had not got Ptolemy's stars observed). Hevelius follows Ptolemy too, most commonly, and rarely deviates from his nomenclature, but upon some mistake; Kepler and Bulialdus copy Tycho: so do the catalogues I have seen printed in French and Spanish. Now I believe Ulug Beig, Copernicus, Tycho, &c., to have been as wise and as

candid persons as any that have lived since, and therefore I adhere religiously to them. They use the words *dexter* and *sinister*, *right* and *left*, continually in their catalogues; though in their observations they sometimes use *preceding* and *following*, as I have done in mine. I desire always to follow their expressions, where there is no need to depart from them; and shall retain them in all my works, without any regard to the whimsies of any bold and confident innovator.

I have now spent 35 years in [the] composing and work of my catalogue, which may, in time, be published for the use of her Majesty's subjects, and ingenious men all the world over. I have endured long and painful distempers by my night watches and day labors. I have spent a large sum of money above my appointment, out of my own estate, to complete my catalogue, and finish my astronomical works under my hands. Do not tease me with banter, by telling me that these alterations are made *to please me*, when you are sensible nothing can be more displeasing nor injurious, than to be told so.

Make my case your own, and tell me ingenuously and sincerely, were you in my circumstances, and had been at all my labor, charge, and trouble, would you like to have your labors surreptitiously forced out of your hands, conveyed into the hands of your declared, profligate enemies, printed without your consent, and spoiled, as mine are, in the impression? Would you suffer your enemies to make themselves judges of what they really understand not? Would you not withdraw your copy out of their hands, trust no more in theirs, and publish your own works rather at your own expense, than see them spoiled, and yourself laughed at, for suffering it?

I see no way to prevent the evil consequences of Dr. Halley's conduct, but this. I have caused my servant to take a new copy of my catalogue, of which I shall cause as much to [be] printed off as Dr. Halley has spoiled; and take care of the correction of the press myself, provided you will allow me the naming of the printer, and that all the last proof sheets may be sent to Greenwich, at my charge, by the penny post, and not printed off till I have seen a proof without faults; after which, I will proceed to print the remaining part of the catalogue as fast as my health, and the small help I have, will suffer me. But if you like not this, I shall print it alone, at my own charge, on better paper, and with fairer types than those your present printer uses; for I cannot bear to see my own labors thus spoiled, to the dishonor of the nation, Queen, and people.

If Dr. Halley proceed, it will be a reflection on the President of the Royal Society; and yourself will suffer in your reputation, for encouraging one, of whom the wisest of his companions used to say, that the only *way to have any business spoiled effectually, was to trust it to his management.*

But I hope better things of you, and that you will endeavor to make me easy after all my long, painful, and chargeable labors, by affording me your assistance, as occasion shall serve, whereby you will ever oblige, Sir, your humble servant and sincere friend,

JOHN FLAMSTEED.

P.S. I forgot to tell you, that whereas Dr. Halley pretends that he has corrected faults in my catalogue, by his own calculation, I fear he has rather made some new: for all the calculations on which my catalogue is built, were wrought twice by different persons, at a great distance from each other, and sometimes oftener, so that there is little room left for suspicion; 'tis a plausible pretence: but he mistakes, if he insinuates me guilty of any such fault. I have taken sufficient care to prevent it, and will answer for all the faults in my catalogue; except those made by himself, and the printer.

[Extracted from MSS, vol. 33, page 95.]

No. 158.) *Letter from Dr. Arbuthnott to Mr. Flamsteed.*

London, April 21, 1711.

SIR,

I told you nothing in my letter, as to the catalogue that I sent, but what was told me: and Sir Isaac Newton does stand to it that Mr. Hodgson did take away some of the catalogue, how much he cannot precisely tell, but that was all that was left; so that matter they may clear between them. I think I told you in my letter that I had compared some part of Ptolemy's catalogue with the translation in the edition of the Britannic Catalogue, and found them to agree, bating the deductions that I had mentioned: and it seems a little hard to say, after that, that my information was only upon hearsay; for I made use of your book that is in Sir Isaac Newton's hands. And I do still say, that the exceptions you make are so far from being just, that they rather make the thing better: for *north and south, upper and lower, preceding* and *following*, are more safely understood than right and left, and never can make any confusion; since anybody can find a star of Ptolemy's catalogue in yours, and *vice versâ*, as far as Ptolemy's reaches. Indeed, if it be true that the alterations Dr. Halley has made in your numbers are erroneous, that is a fault, to remedy which I told you, if you would consent that the catalogue should be once completed, if you would not stand to those corrections, yours should be printed entirely according to your own copy. But I own I am much mistaken if, when the numbers differ from yours, they are erroneous: and I do declare, if it were my own case, I should be glad the greatest enemy I had should correct my numbers or my writings in any thing before they appeared in public, and I should think it still better than the correction of the most complaisant friend. I can answer for myself that I have no design to rob you of the fruits of your labor, but to make the catalogue correct, so as it may be fit to appear in public; and if you would have given in a complete one, it should have been done long ago: but since you are not pleased to do so, I will not delay any longer, but take the same method to make out the rest of the catalogue that you have done; which is, to employ people to calculate from the observations what is wanting. And why we should not succeed as well in this piece of journey-work I cannot imagine; and if, after all is ended, you do not like the performance, you shall be free to print your own. I promised to send you a copy of the sheet before the catalogue is published, and so I will; and whether you send me the remaining part of the catalogue or not I will keep my promise. But I cannot but say it is a little hard that, when you can so easily supply what is wanting, you will not so far gratify those concerned as to let it be printed first in this manner; and then it shall be reprinted, changed, or altered which way you please. I shall give you no further trouble in this matter, being, Sir, your most humble servant,

JO. ARBUTHNOTT.

[Copied from the *original* in MSS, vol. 35, page 107.]

No. 159.) *Letter from Mr. Flamsteed to Dr. Arbuthnott.*

The Observatory, April 24, 1711.

SIR,

Yours of the 21st I received yesterday. In answer to it I send you here included a specimen of three names of stars in Aries from Ptolemy's great catalogue, and of 12 in Taurus; with my rendering of them and Dr. Halley's: say you which is the juster and more fit to be used.

I esteem Ulug Beig, Copernicus, Clavius, Tycho, and Kepler, with Bulialdus, every whit as wise, judicious, and skilful men, and as modest as Dr. Halley. They use the same expressions that I do; to depart from them is to make confusion in the catalogues without cause or end: and, therefore, I cannot depart from them, nor suffer the names I have used, after my *long and chargeable labors, to be altered by one that had no share in the pains or expense.* I desire to have no difference with you: permit me, I beseech you, to print my catalogue at my own charge: I shall do it, God assisting me, with all the expedition I can. I shall retain the old names as they have been used this fifteen hundred years, and finish it with all the care and diligence possible; after which I shall present it to her Majesty, I hope with your approbation and assistance. And if then Dr. Halley dares be so bold as to alter anything in it, he may. I shall leave the world to judge whether he or I have been the more candid and ingenuous.

And if he can amend or correct any of my numbers, I shall take it kindly, and be obliged to him for it: but he must be very careful, and repeat all his calculations, for mine were all wrought twice over, first by a hired kinsman of his, that lived 120 miles from London, and afterwards by my servants (or other hired help) here, and collated and corrected by myself. I pray God keep and direct you, and am ever, Sir,

Your very humble servant,

JOHN FLAMSTEED.

[*Memorandum, written by Flamsteed.*] In a postscript I desired him to peruse my letter to Sir C. Wren, of which I gave him a copy at Garraway's, and particularly the last paragraph, whereby he would be satisfied that I had done all that lay in my power to expedite my work, and had taken great care of the catalogue of the fixed stars.

[Extracted from MSS, vol. 33, page 98.]

No. 160.) *Letter from Dr. Arbuthnott to Mr. Flamsteed.*

St. James's, May 15, 1711.

SIR,

I send you enclosed Dr. Halley's answer to your paper; and I expect with the same candor your reply. I have looked over the printed catalogue, and Ptolemy, and am pretty sure he is in the right, and that your amanuensis is mistaken. If there are any exceptions to his corrections of your numbers, I beg you would send them, for I have no other design than to have the catalogue correct. If Ptolemy is exactly to be followed, I should desire to be informed why you, in some cases, transfer the stars which Ptolemy places in one constellation to another; I believe this cannot be done without breaking in upon Ptolemy's nomenclature. I beg pardon for this trouble, being, with all respect, Sir, your most humble servant,

JO. ARBUTHNOTT.

[*Note written on the letter by Mr. Flamsteed.*]

My last letter to Dr. Arbuthnott was dated April the 24th, three weeks before the date of his answer.

[Copied from the *original* in MSS, vol. 35, page 111.]

No. 161.) *Dr. Halley's Justification of his version of Ptolemy.*

[Inclosed in the preceding letter.]

Star.	Ptolemy.	Versio J. Flamstedii in Catalogo.	Dr. E. Halley in opere cuso.
Aries.			
11 ϱ	ὁ ἐν τῷ ὀπισθομήρῳ	in femore posteriori	in femore vel clune borea
12 σ	ὁ ὑπὸ τὴν ἀγκύλην	sub poplite	austrina
13 ξ	ὁ ἐπὶ τοῦ ὀπισθίου ἀκρόποδος	in extremitate pedis posterioris	in extremo pede anteriori
Taurus.			
5 e	ὁ τυτος ἑπόμενος ἐπὶ τῆς δεξιᾶς ὠμοπλάτης	quæ sequitur in dextri humeri spatula	sequens in armo præcedente
8 ν	ὁ ἐπὶ τοῦ δεξιοῦ σφυροῦ	in suffragine dextra	in talo pedis præcedentis
24 ω	τῶν ἐν τῷ τραχήλῳ δύο μικρῶν ὁ πρόηγούμενος	duarum in collo parvularum præcedens	duarum ad genam præcedens
25 ω	ὁ ἑπόμενος αὐτῶν	sequens	sequens duarum parvularum in collo
26 χ	τοῦ ἐν τῷ αὐχένι τετραπλεύρου τῆς προηγουμένης πλευρᾶς ὁ νοτιώτατος	in quadrilatero cervicis præcedentis lateris australis	præcedens mediar: in □° colli. Bayero *p*, non χ
27 φ	ὁ βορειότερος τῆς προηγουμένης πλευρᾶς	borea præcedentis lateris □ in cervice	australis quadrilateri colli. Bayero ψ, non φ
28 p	τῆς ἑπομένης πλευρᾶς ὁ νοτιώτερος	australis sequentis lateris □ in cervice	australis quadrilateri colli χ, non *p*, Bayero
29 χ	ὁ βορειότερος τῆς ἑπομένης πλευρᾶς	borea sequentis lateris □	sequens mediarum in □° colli. φ non χ, Bayero
22 υ	τῶν ἐν τῷ βορίῳ ὠτίῳ δύο σύγγυγος ὁ βορειότερος	contiguarum duarum in aure borea borealior	borealium in aure præcedens
23 z	ὁ νοτιώτερος αὐτῶν	contiguarum in aure borea australior	borea australium in aure
10 d	ὁ ἐπὶ τοῦ ἀριστεροῦ πήχεως	in sinistro cubito	in sequenti crure

11 ρ. It is exactly according to Mr. Flamsteed's copy, "in femore vel clune borea:" and surely "in femore posteriori," as he would have it, is scarcely sense; there being no "femur anterius." ἐν τῷ ὀπισθομήρῳ means "in posteriori femoris parte," if he please to consider it better.

12 σ. This star is called by Tycho "in genu sinistro," and by me "in sequente genu posteriori," and not "austrina," as he falsely quotes it.

13 ξ. This star is made by Tycho, Hevelius, and all the moderns, to belong to the Whale, by a liberty I have no where taken; but this I call, with Ptolemy, "in extremo pede posteriori, alias μ Ceti." That I call "in extremo pede anteriori," (which is ξ of Bayer,) is neither in Ptolemy nor Tycho; so that here again he quotes false.†

5 e. Here he wilfully mistakes again; for, Ptolemy's 5th of Taurus is Bayer's *t*; *e* is only in Tycho. His amanuensis, or rather himself, ought to write Latin, and have called it "has sequens

[*Note written on the paper by Flamsteed.*]

† 'Tis Halley is impudent: he ought not to alter my names.

μ Ceti Ptolem:	= ♓ 5° 20′	Lat. 6° 20′ aust.
13 Arietis	= ♓ 7 40	Lat. 5 15.

Nulla hoc in loco fixa; sed hæc nequit esse Ceti μ.

in dextra scapula," and not "spatula," as it is both here and in the copy I have.* This star Tycho calls "in dextro armo," which he will allow to be good authority: and it having no name in my copy, I was left at liberty to use that word.

8 *ν*. I affirm it to have been a fault in all those that have rendered ἐπὶ τῦ σφυρῦ "in suffragine." That word signifies the hock or joint, that bends backwards in the hinder leg of a beast; and belongs not to the fore leg: ἐπὶ τῦ σφυρῦ is "in the fetlock joint," above the hoof; which in Latin is "in talo:" and this very star is called, in Ulug Beig, "in talo dextro." In this I own I have changed the word "suffrago," but for a more proper, as you may well judge.†

24 *ω*. This star Mr. Flamsteed mistakes for A Tauri, which I call "in medio colli," agreeable to Ptolemy. That which I call "ad genam," is neither in Ptolemy nor Tycho, and is properly called "ad genam" by Hevelius, from its place in the figure: and I hope he will acquiesce in Hevelius's judgment in this case.

25 *ω*. This 25th of Taurus, which he calls only "sequens," and should have been "harum sequens," requires a further explication as to what it follows, because of six other stars intervening. So that, if I have called it "sequens duarum parvularum in collo," I humbly hope 'tis no great offence.

26 χ —— 29 χ. Here I observe Bayer's marks are all falsified;‡ I suppose not without design: but, as to what I have done in the names of the four stars "in quadrilatero colli Tauri," I know not what can be objected; unless that instead of his "in cervice," I use "colli:" which I did, as being the shorter word. Nor does the description differ from Ptolemy's, unless that I have endeavoured to avoid the repetition of the words "præcedentis" and "sequentis lateris quadrilatero," too long for our narrow column.

22 *ν*, and 23 *κ*, are two double stars, which I have named with great caution to signify that duplicity, without receding from Ptolemy's designation. I have indeed omitted to say "in aure borea," according to the copy: for Tycho has done the same; the word "borea" abounding, because the other ear is not mentioned, perhaps as being hid behind the head of the Bull.

10 *d*. "In sinistro cubito" was never yet said, in Latin, of a quadruped.§ The word πῆχος signifies, in man, the space between the elbow and wrist; and, in a beast, the analogous part between the knee and fore foot: and, therefore, I have rendered it "in crure." Tycho, whom he pretends so much to follow, calls the same star "in suffragine sinistro:" and Ulug Beig has it "in radio sive focili sinistro;" that is, in the fore leg or shank. As to Hevelius, he has made very bold with the figures of the constellations, and names of the stars (as will appear in this of Taurus), that I cannot but wonder he should be reckoned amongst those that follow Ptolemy nearer than I have done.

[*Notes written on the paper by Flamsteed.*]

* Versio "spatula." I follow Ptolemy.

† No: *talus* for a man, *suffrago* for a beast.

‡ Not one, in my *originals*. It may be so, if he has altered them.

§ See Trapezuntius.

[Copied from the *original* in MSS, vol. 35, p. 113.]

No. 162.) *Letter from Mr. Flamsteed to Dr. Arbuthnott.*

The Observatory, May 23, 1711.

SIR,

Yours of the 15th brought with it Dr. Halley's justification as he calls it (but very improperly) of himself: for any one, that can compare his version with Ptolemy's Greek, will see easily at first his version is not to be justified. The true title due to it is his *recrimination;* but certainly he is the most unfortunate person I have ever met with; for, instead of excusing his faults by it, he adds more to them, and heightens them: there is not one paragraph in his paper wherein he is not grossly mistaken, and (which is peculiar unhappiness) ever worst where he takes most upon him, and seems most confident. All the designed mistakes and falsities he so rudely charges me with, are all entirely the mistakes and falsities of himself, and his excuses trifling; as I shall easily prove to you, whenever you please to oblige me with your company for an hour or two at the Observatory, where there are books, maps, and papers fit to do it by, with which probably you are not furnished. I am, and ever was, as desirous as you can be to have my own catalogue of the fixed stars correctly printed, that I may prevent it from being spoiled; and, therefore, I am resolved to take no notice of Dr. Halley at present. I regard him not so I may but satisfy you, and a friend or two more, that all his malicious suggestions are insignificant. I pray God convert him, and preserve the love of truth and sincerity in you, and bless every one of us in our own particular lawful business and affairs.

I am, Sir, always your sincere friend and humble servant,

JOHN FLAMSTEED.

[The subjoined paper immediately follows the above letter in the MS. book. F. B.*]

11 ρ. 'Tis not so: there are three stars at ρ; of which, that which I call "borea" is "borea ad ρ." The "austrina" is not "austrina in femore," but "austrina ad ρ:" the third precedes these two, and is still the southern-most. I follow the Greek exactly: it may be as I have turned it, more probably than as he would.

12 σ. He mistakes the "media ad ρ" for the "austrina in femore."

13 ξ. This star is neither in Tycho, nor Hevelius, nor any of the moderns that I know of. *Clamat Melicerta.* Ptolemy places

ξ Arietis, in ♈ 15° 0′, with 5° 15′ south latitude,
μ Ceti, ♈ 12° 40′ 6° 20′,

therefore they cannot be the same star, as he positively makes them. There is no other star in Aries that has near 5° south latitude, but one of these at ξ; which makes me suspect there may be an error in Ptolemy: and that, instead of the longitude being put $\iota\varepsilon$, it ought to be simply ι, or $\iota\alpha$; and then all things agree very well.

5 *e*. 'Tis his own mistake again, for Ptolemy has not the star marked *t* by Bayer; but Ptolemy's fifth of Taurus is most certainly marked *e* by Bayer, and Ptolemy has it as well as Tycho: but Dr. Halley, I fear, is distracted, or cares not what he writes. Trapezuntius renders it "spathula," I think more properly than "scapula." I hold to Ptolemy's descriptions and names, with his leave.

* These notes are a commentary on the preceding notes of Dr. Halley; and were probably either enclosed in the above letter to Dr. Arbuthnott, or conveyed to him afterwards. F. B.

8 ν. Whether this be more inconsiderately, or impudently, said, let those who read it judge. The hinder parts both of the Bull and Pegasus, in all the charts of the constellations I have yet seen, are cut off and invisible; and the forelegs are only seen: either, therefore, the $\sigma\phi\upsilon\rho o\varsigma$ was in the forelegs of the Bull, or Dr. Halley understands Greek better than he did himself. The 18th and 20th of Pegasus are $\grave{\epsilon}\nu$ $\sigma\phi\upsilon\rho\tilde{\varphi}$; and both are in the forelegs: so are the 23rd and 25th of Sagittarius. Blush, Doctor!

24 ω. No, Dr. Halley mistakes. A Tauri has north latitude, and comes to the meridian before the two at ω, which have south latitude both in Ptolemy and Tycho. Hevelius sometimes varies from Ptolemy, without any good cause: I stick to the ancients.

25 ω. 'Tis in my catalogue "duarum parvularum in collo sequens ad ω." He need not pretend to be witty on this occasion; but if he allows this to be "sequens ad ω," I have not mistaken it for A Tauri as he would insinuate. 'Tis a malicious, frivolous objection of his own.

26 χ ——29 χ. The numbers of these stars in Ptolemy are copied exactly. My man is mistaken in Bayer's letters; but 'tis a fault that is wholly insignificant: and Dr. Halley did not make [it] his business to find faults where he has no cause. For Bayer's letters are true set in my catalogue; and Ptolemy's names are very express, and determine the stars exactly. His no ways answers the Greek: and let him say what he will, he can never vindicate them to any reader that understands the Greek, and is skilful in these things. There can be no *design* in my servant's mistake (as he maliciously insinuates), nor, if it were my own; since the letters (Bayer's) are true placed in my catalogue, as well as the numbers.

22 ν and 23 $\varkappa$. I think my translation preserves Ptolemy's words, and designs the double stars, at ν and $\varkappa$, as well as his: and therefore he had no reason to alter or recede from it.

10 *d*. Trapezuntius translates it, as I do, "in sinistro cubito;" which determines the place of the star to that joint in a horse, which answers to the "cubitus" in a man: whereas "crus" is sometimes in beasts taken for the whole leg and thigh together. But, if he allows Dr. Hyde's putting "talus" for the fetlock of a beast, why does he not approve of Trapezuntius's using "cubitus" for the joint above it in the same leg? If he had said "in sinistro crure," instead of "in sequente crure," I had taken no further notice of him: but I must now ask why he declines the uses of "dexter" and "sinister," and puts "sequens" and "precedens" in the room of them. 'Tis to excuse the great mistake he committed in his two planispheres, published about 30 years ago, where he draws all the constellations, as Bayer does, with their backs towards us (except Andromeda, wherein, too, he follows Bayer), and thereby makes those legs, arms, and sides to be the *left*, which in Ptolemy's descriptions, and in all since (even Tycho and Hevelius) are the *right* legs, arms, sides, &c. And 'tis to excuse this blunder, and authorize it, he would change the names I have used (and have so good authority for) for those which are never used by any but Bayer; whom none but himself is so silly or blockish as to follow. But there is abundant malice in this conduct of his: an ingenuous man would have scorned to take the publishing of my work out of my own hands, without my consent or knowledge: an ingenuous man would never have endeavoured to conceal the printed sheets from me, nor refused me the sight of them when I desired it. God forgive him.

[Extracted from MSS, vol. 33, pages 99—104.]

No. 163.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 15, 1711.

Sir,

My servant is so much your friend, he will not suffer me to let your letters lie by me without returns: this morning he calls upon me to write to you; and I find 'tis time, for the date of your last is March 8, 1710, to which I shall give you now an answer, with some further news concerning myself and my works.

March the 19th last, I received a letter from Dr. Arbuthnott, one of the Queen's physicians, signifying that the copy of a part of my catalogue, which had been delivered into Sir I. Newton's hands at his desire, sealed up, was now in the Doctor's, who desired that I would give him four constellations that were wanting in it, with the variations, &c. for the rest. For, when the part of my catalogue was put into Sir I. Newton's hands (March the 15th, 1705) these constellations were not begun, and the rest imperfect: which, though Sir Isaac knew very well, he still persisted to have the keeping of it in his hands, sealed up; that, as he said, he might have all things in his power. He would not suffer any of my sheets to be printed, till he had gained this point: and I was forced to yield it, that he might not pretend and say the Prince would have printed my works, and I hindered it myself.

How the press went on, and Sir I. Newton hindered its progress, by continual shuffles and tricks, you have been informed formerly. I shall only tell you more, that the press had wrought off 98 sheets of the first volume on Octob. 21, 1707; that we met on March 20, 1707-8, and then Sir Isaac had opened the catalogue, and desired me to insert the magnitudes of the stars to their places; for they had not always been inserted in it: and of £173 I had disbursed, ordered £125 to be paid me; which, with some trouble, I got some time after. But, at the same time he got a second and more complete copy of the ecliptical constellations, then showed him, into his hands; of which Dr. Halley returned but the six latter signs into Mr. Hodgson's hands, about a month ago: the other half is lost, or Dr. Halley detains it with Dr. Arbuthnott's privity; for they are both of one church.

March the 25th last past, I was informed by a friend that my catalogue was in the press, and some sheets of it printed off: on the 29th I met Dr. Arbuthnott at Garraway's, who affirmed there was not a sheet printed. But, April the 2nd, I got the printed first sheet; and, soon after, the third, wherein I found that many of the names I used, which were translated from Ptolemy, and the same in sense with the Arab translations of Gauricus, Copernicus, Clavius, Tycho, Kepler, Bullialdus, and Hevelius, were altered: instead of *dexter* and *sinister*, were put *antecedens* and *consequens*; for *Bor.* and *Aust.* *superior* and *inferior*; some names made nonsense; some stars omitted; others inserted in improper places: and I learnt further that Dr. Halley looked after the press, and was the author of all this confusion. Till I knew this, I was willing to have filled up the copy of the catalogue: but, perceiving hereby that Halley was minding to spoil the work, and with more views than one or two, I sent Dr. Arbuthnott an account of his villainous outrage, and desired he would permit me to print my own catalogue at my own charge. Immediately hereupon I set my servant to copy the catalogue anew, and got about 10 sheets transcribed to lie ready: but, upon a review, found it necessary to copy them over again, which will be done with 10 days' pains at any time, and will have a many stars inserted that were not observed when the first imperfect copy was made, and the Hevelian constellations added. I find no impediment but James Hodgson's great

business in London: but the hot weather comes on: I hope he will have less in the vacation, and then I may have his full help.

I intend to get a constellation or two engraved, and perhaps more: for the good Providence, that has hitherto governed my works, seems to be providing me proper help; and this action of Halley's has exposed him to all the town, and they forbear not to say he is impudent, f—— and k——. Sir Isaac turns off the blame cunningly on Arbuthnott; and Halley is very willing to take it all on himself, to oblige his master, who made him 15 years ago Comptroller of the Mint at Chester: but every one sees his craft, and loathes him for it.

It will cost me near a hundred pounds, I fear, to print only my catalogue and preface: but I thank God he has given it me, and I shall not grudge to employ it in this business, for His glory and my country's service. His wisdom is beyond the wisdom of man. He has turned the cunning of the aforementioned crafty persons into foolishness; and I doubt not but, though I am feeble and lame through the gout in my knees and ankles, he will give me strength to publish both this, and what more I design during my life. I am no further solicitous but to show myself, Sir, ever your friend, to love and serve you,

JOHN FLAMSTEED, M.R.

P.S. Pray let me hear from you sometimes, though I should be too busy to return you an answer in the time you expect. When I have printed a few sheets, I will send you them. Dr. Arbuthnott is a countryman, and was a great friend, of the deceased Dr. Gregory.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 164.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

May 24, 1711.

The President in the chair. The President ordered that Mr. Flamsteed be desired to make an observation of the future eclipse of the sun, next July; and give it to the Royal Society, in compliance with her Majesty's letter to that purpose.

[Extracted from the *Journal* Book of the Royal Society.]

No. 165.) *Letter from the Royal Society to Mr. Flamsteed.*

Crane Court, May 30, 1711.

SIR,

By virtue of Her Majesty's letter to us directed, dated the 12th day of December, 1710, we do hereby order and direct you to observe the eclipses of the sun and moon this year, and particularly that of the sun of July the 4th ensuing; and we desire you to send such your observations to us, at our meeting at the house of the Royal Society, in Crane Court, in Fleet Street.

We are, your humble servants,

I^S. NEWTON, P.R.S.
HANS SLOANE, S.R. Secr.
R^D. MEAD.

[Copied from the *original* in MSS, vol. 35, page 115.]

No. 166.) *Letter from Dr. Halley to Mr. Flamsteed.*

London, June 23, 1711.

REVEREND SIR,

Though I am credibly informed that these sheets have been, from time to time, sent you from the press, yet, lest it should be otherwise, I have now sent you the catalogue of the fixed stars intended to be prefixed to your book; having spared no pains to make it as complete and correct as I could, by help of the Observations you have given us, made before the year 1706. I desire you to find all the real faults you can, not as believing there are none, but being willing to have a work of this kind as perfect as possible: and if you signify what's amiss, the errors shall be noted, or the sheet reprinted, if the case require it. Pray govern your passion, and when you have seen and considered what I have done for you, you may perhaps think I deserve at your hands a much better treatment than you for a long time have been pleased to bestow on

Your quondam friend, and not yet profligate enemy (as you call me),

EDM. HALLEY.

[Copied from the *original* in MSS, vol. 35, page 119.]

No. 167.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Sept. 20, 1711.

Mr. Halley has spoiled my catalogue in printing it; and thereby put me to the trouble of reprinting it. He is doing the same by some of my observations, and Sir Isaac Newton furnishes him perfidiously with materials. He has given him the places of the moon I imparted to him in the three large synopses (that you have seen here), which he publishes the moon's observed places from (as I find by one of his sheets I have got from the press): though they were communicated to him on this condition, precisely and expressly, that he should not impart them to any one: and this reason given him for it, that they were determined by the help of a small catalogue of the fixed stars I had rectified (by the help of such observations as I had got with the sextant) to the beginning of the year 1686; and that I intended, when I had finished the great catalogue I was then (1694) entering upon, I would correct and calculate them anew, with such others as I should gain afterwards. But Sir Isaac Newton is too great a person to be a slave to his word, and Dr. Halley is resolved to spoil everything that falls within the reach of his fingers. But of this I must give you a fuller account hereafter: at present I have other business to recommend to you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 168.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, December 22, 1711.

When I look on the date of your last, Nov. 2, I reprove myself for having so long neglected an useful and obliging friend: but, when I have used that name, a new reflection comes into my mind, and I conclude a real friend (and such an one I well esteem) that cannot but excuse me when I have told him the cause of it; and showed that it will turn to his advantage (satisfaction I mean). For

other advantages, to such as prosecute these sciences, the heavens scarce ever afford their inspectors till their earthly observatories are demolished. The wise providence of Heaven has indeed been liberal to me, in bestowing on me such a competency as hinders me not from enjoying the continually innocent delights of my studies, and blesses me daily with more content and pleasure in them. In the mean time, my estate (though I am no niggard in disposing the blessings of Heaven) improves daily: you are in the same circumstances; few are so happy as we: what have we to do, but to return our daily thanks to the indulgent Builder of the Heavens, and endeavor to render each other easy, by excusing mutually those faults of which we can assign a reason?

When I sent you the seven first printed sheets of my catalogue, I had not examined my books of observation to see if none observed were omitted. My man, having observed Saturn and some stars with him, that were not in my catalogue, with a small accident that happened to me about a month before, put me in mind to search my book, and collect such as I had noted not inserted, or that might not have been then noted. With some pains I found a pretty number, which I design to add to it in their proper places: this made me stop the progress of the press soon after. I had also another reason: the printer, I had employed, is a person over-stocked with business, but not with types; the work went very slowly on in his hands: I have hearkened out another that is well furnished with letter and figure, and wants employment. My servant, and another calculator, are hard at work; and I hope, ere the holidays are over, to have all the calculations finished, to begin to print before Candlemas, and to get as many sheets from the press in four weeks, as I did formerly in months. I design also to print the small catalogue which contains only the right ascensions of the stars in time, and their distances from the vertex of this place, with them; as also the tables, whereby the variations were inserted, and some other things that will be acceptable to you; of which I intend to send you the sheets as they come from the press; that if you think anything ought to be added, or advertised concerning them, I may have timely notice, and your advice about it.

I have had another contest with the President of the Royal Society, who had formed a plot to make my instruments theirs; and sent for me to a Committee, where only himself and two physicians (Dr. Sloane, and another as little skilful as himself) were present. The President ran himself into a great heat, and very indecent passion. I had resolved aforehand his kn—sh talk should not move me: showed him that all the instruments in the Observatory were my own; the mural arch and voluble quadrant having been made at my own charge, the rest purchased with my own money, except the sextant and two clocks, which were given me by Sir Jonas Moore, with Mr. Towneley's micrometer, his gift, some years before I came to Greenwich. This nettled him: for he has got a letter from the Secretary of State for the Royal Society to be visitors of the Observatory; and he said "*as good have no observatory as no instruments.*" I complained then of my catalogue being printed by Raymer, without my knowledge, and that I was *robbed of the fruits of my labors.* At this he fired, and called me all the ill names, puppy, &c., that he could think of. All I returned was, I put him in mind of his passion, desired him to govern it, and keep his temper: this made him rage worse: and he told me how much I had received from the Government in 36 years I had served. I asked what he had done for the £500 per annum that he had received ever since he settled in London. This made him calmer: but finding him going to burst out again, I only told him my catalogue, half finished, was delivered into his hands, on his own request, sealed up. He could not deny it, but said Dr. Arbuthnott had procured the Queen's order for opening it. This, I am persuaded, was false; or it was got after it had been opened. I said

nothing to him in return; but, with a little more spirit than I had hitherto showed, told them that God (who was seldom spoke of with due reverence in that meeting) had hitherto prospered all my labors, and I doubted not would do so to a happy conclusion; took my leave and left them. Dr. Sloane had said nothing all this while; the other Doctor told me I was proud, and insulted the President, and ran into the same passion with the President. At my going out, I called to Dr. Sloane, told him he had behaved himself civilly, and thanked him for it. I saw Raymer after, drank a dish of coffee with him, and told him, still calmly, of the villainy of his conduct, and called it *blockish*. Since then they let me be quiet; but how long they will do so I know not, nor am I solicitous: but I trouble you with a tedious relation.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 169.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Feb. 14, 1711-12.

A friend is come in to dine with me: I have time to tell you no more but that I shall increase my catalogue a twelfth part, or with near 300 stars. Joseph, with Mr. Ryley, a neighbour, is calculating their places from their A. R., and distances à polo, determined by myself: and as for Sir I. Newton's endeavors, I hope they are at an end. I am got, I bless God for it, to the further end of my work; nor do I doubt of finishing what I have proposed to myself, through the assistance of his good providence. May that ever direct and keep you! and, Sir, I am ever yours, to serve you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 170.) *Draft of Mr. Flamsteed's Petition and Remonstrance to Queen Anne.*

April 16, 1712.

His Royal Highness the Prince, out of the respect he had for ingenious and useful arts, took upon him to defray the charge of printing Mr. Flamsteed's *Historia Cœlestis*, which was to contain (with some others) all the Observations made at the Observatory in Greenwich Park, as also a catalogue of the fixed stars derived from them, and large drafts of the constellations, agreeable to the descriptions of the ancients. The Referees, having received some of the Observations, would not consent that the press should begin, except a copy of one-half or two-thirds of the catalogue (which, at that time, was neither perfect nor collated) were put into their hands, as a pledge for securing the delivery of the remaining part of the Observations, then not ready, but likely to be soon after. Mr. Flamsteed being unwilling to part with it, one of them proposed to have it put into his hands, sealed up, to which Mr. Flamsteed consented, and a part of the catalogue was accordingly put into his hands in March, 1705-6; after which the press set to work on the first volume, containing about 100 sheets, but proceeded very slowly, not, be assured, by any fault of Mr. Flamsteed's, as you will find by the copy of a letter accompanying this. Some time after, Mr. Flamsteed was told that the copy of the catalogue was opened and unsealed, which he could scarce believe.

March 1707-8, a further supplement to the catalogue was put into the Referee's hands (but not

yet perfect, for the Observations requisite to complete it could not yet be obtained), with the Observations made with the mural arch, in 175 sheets of paper.

In March 1710-11, Mr. Flamsteed was told the catalogue was in the press, and received a letter from one of the Referees, desiring him to send the places of the stars in the six northern constellations to finish it. At the same time he had unexpectedly the 1st and 3rd sheets put into his hands, wherein he was amazed to see how boldly the editor had presumed, without cause, to alter the names that he had employed, and taken from Ptolemy, whom all before us have followed for above 1500 years. That in many places, instead of correcting faults, he had committed many that were inexcusable; and that many stars were wanting which Mr. Flamsteed had inserted in his catalogue. For these reasons, that the whole might not be spoiled, he was forced to decline complying with what was desired of him; and, therefore, having now completed his catalogue, examined and corrected it, he designs to publish it with all convenient expedition, and requests that her Majesty would be pleased to allow it; and that no encouragement may be given to those who are putting out one, printed from a surreptitious and imperfect copy, in his name.

[Copied from the *original* in MSS, vol. 35, page 131.]

No. 171.) *Letter from the Royal Society to Mr. Flamsteed.*

Crane Court, July 3, 1712.

SIR,

The Committee of the Royal Society appointed for that purpose, finding you have not delivered to them a copy of your last year's observations, according to the direction of her Majesty's most gracious letter of the 12th of December, 1710, do hereby demand your compliance therewith: and now, the six months allowed you being elapsed, let you know that they expect you will send your observations to the Society's House, in Crane Court, according to the meaning of the said letter.* We are, your most humble servants,

I^S. NEWTON, P.R.S.
HANS SLOANE.
R. MEAD.
EDM. HALLEY.
ABR. HILL.

[Copied from the *original* in MSS, vol. 35, page 129.]

* The following memorandum is made, in red ink, at the bottom of the copy of this order, preserved in the *Letter* Book of the Royal Society: viz.

"Memorandum, This is an exact copy of a letter which I delivered to Mr. Flamsteed (July 4, 1712), at the "Royal Observatory: who returned this answer, that he would cause a copy to be taken of his last year's observa- "tions, and send them accordingly to the Royal Society by Michaelmas next. J. THORPE."

From this it is evident that the Royal Society wished to be regular and circumspect in all their proceedings relative to their new office of *visitors*. F. B.

No. 172.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 5, 1712.

Sir,

I heartily beg your pardon for having so long forborne to write to you. I have had a great deal more business of late than formerly. My wife's father died about Michaelmas 12 months, whereby some estate fell to her and her sister in London. I have been obliged to pay his debts, change a tenant, repair his houses, and thereby to waste a great deal of time in London. I hope all my trouble on that account will be over in a few weeks; and then I shall be more punctual, God sparing me life and health.

In the meantime the copy of my enlarged catalogue of the fixed stars is in the press: 3 sheets are wrought off, and I expect a proof of the 4th this evening.

Two of your acquaintance have been here lately: one about 7 weeks ago, the other a month. I promised the first to write to you, but was prevented by business: the 2nd promised to call for a letter which I wrote, but he never came for it. I was glad to hear of your health by both, and pray God continue it. Modest Mr. Caswell is dead: Mr. Keil gives out that he has his place—*at quam dissimilis homo!* It was reported some time since, that if he attained this preferment, he should marry Raymer's daughter. Raymer and he are both of the same principles; and 'tis pity two houses should be troubled with them. But the Archbishop of Canterbury's secretary was here last night, and assures me the professorship is not disposed of; so that I hope it will fall on one Mr. Whitesides, who is a very modest good man, and I hope will fill it, and discharge his duty as he ought.

Thursday night last Joseph observed Jupiter, and followed my directions so well, that I have his place determined by four different stars: 1712, July 3, at $12^h\ 47'$ in ♒ $3°\ 40'\ 20''$ with $0°\ 39'\ 20''$ latitude; 12 minutes forwarder than Parker's ephemeris.

This planet is now retrograde towards 3 stars in the head of ♑, $\pi\ \rho$ and o. You may do well to observe his place by them with your micrometer and 7 foot glasses. He will come afterwards up to σ in the same constellation, and to a measurable distance from it.

But, moreover, he is near that place in his orbit, where the node of his satellites is; and their eclipses have their greatest duration; and I could wish you would forecast to observe some of the calculated emersions when they become visible after the opposition. And that you would calculate their eclipses for one year more; after which I believe I shall solicit you no farther for them: for I have not strength to stand to observe them. I pray God keep you: and am always, Sir, your affectionate friend and obliged to serve you,

John Flamsteed, M.R.

P. S. I intend to send you the reprinted sheets of my catalogue before I go to Burstow.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 173.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Nov. 25, 1712.

Kind Sir,

You will easily pardon me for writing seldomer to you than formerly, when I have told you that age comes upon me fast, and that I write with pain, which yet is alleviated when

I consider that 'tis to a friend, who takes no less pleasure in my studies than I do myself. I can still gratify such a friend, and perhaps 'tis the greatest ('tis not the least) part of the pleasure I have now in my studies, that I can still do it. I sent you some time since, by Mr. Stanfield, 7 or 8 sheets of my catalogue; I have now 6 or 7 more wrought off, lying by me, to be sent as you shall direct; and hope, by that time your return reaches hither, I may have the whole: so that the next conveyance will complete what you have, and bring you a perfect catalogue, enlarged and corrected carefully: which I must entreat you to keep safe, and not to suffer any friend, or pretended friend, to have it to peruse; lest they copy it, as Raymer very impudently told me he would whenever I published it.

For he has printed it already, from an imperfect and incomplete copy, that I was forced to trust in the hands of Sir I. Newton, which he very treacherously broke open, though it was, at his own desire, sealed up, and so delivered into his hands. I intend also to add a preface to it: Raymer having wrote one to have put before his edition of my work, which I cannot get a sight of, though I have got his copy of my catalogue printed on such paper as will scarce last half an age. He has boasted all over the town what numerous faults he found in it, and showed my copy amongst his impious associates at Child's, all marked where he pretends to have corrected me. But, having examined his edition, I find that his pretended corrections are mistakes and rash errors of his own (to call them by no worse name): for where he could find none, he has presumed to make a great many together, as very unhappily for him 'twill be proved, as well by observed distances as by transits. This you may affirm boldly; and I am apt to think 'tis for this reason that I cannot get a sight of his planets' places derived from my observations, which I have by me ready for the press, 1000 places of the moon, and as many of the other planets.

I have caused my servant lately to observe both Saturn and Mars, which he has done very carefully; and upon examining his observations I find the planets' places as copied on the back page of this letter.

You will find, by comparing these observed places with the ephemerides, that Saturn is slower than the Caroline by 38 minutes, and Mars swifter by 6′ or 7′. The Sun was some degrees past the quartile of Saturn; in whom the error will be greater at the opposition, probably as big as it was last year. Of Mars I dare pronounce nothing at present, but suspect the same thing.

Last week I had my picture drawn by a good hand, who has done it very well, and like. I purpose to have it engraved on copper for a half sheet, as large as those of my catalogue. I shall not have it done very speedily; but, when it shall be, some copies shall be sent you for yourself and friends, that when you look on it, you may remember, Sir, your affectionate friend and servant,

I pray God keep you. John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 174.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Dec. 9, 1712.

Sir,

I wrote to you about 12 days ago, and sent you some observations of Saturn and Mars made lately, that differ considerably from the *Caroline Tables*. That letter went hence Saturday was sevennight, and I doubt not has safely reached your hands. The Tuesday after, I received one from you, dated November 29, wherein you complain of my slackness in answering yours, not without

some seeming cause I confess; but to let you see you had no just one, I must inform you something further, than I have yet done, of my present and past circumstances. I bless God for it, I have enjoyed my health this year as well as one of 66 complete can expect. Before I went into the country, a lease of a house in St. Paul's Church-yard came out; I was forced to repair it for my wife and sister and a new tenant. After my return, another of theirs was in the same circumstances, and I repaired it for an old one. The repairs of these houses have cost me above £150, and many journeys to London, where my catalogue was then in the press. Betwixt these I had very little time to spare; and of that, friends and visits, as usually here, devoured so much, I had not what I desired to bestow on my remoter acquaintances. I hope, therefore, you will pardon me for having forgot to acknowledge the receipt of the satellite eclipses you calculated for me; I thought I had done it before. Since I did not, accept my hearty thanks now: I shall cause my servant to take care to observe such of them as he can conveniently, and shall send you his observations.

Now my repairs are finished, the catalogue printed off, I desire to know how I may send you the sheets you want to complete your copy, and how many sheets you have received, by your next. I do not design to stop here: I order my solar tables to be copied, to print next after; and am thinking of a large preface into which I shall insert my subsidiary tables, whereby the stars' longitudes and latitudes were derived from their determined right ascensions and distances à polo, when I will likewise acquaint the world with your tables of prosthaphereses; but I fear they are too long to be printed on my pages.

I will add no more at present, but that I pray God continue your health; shall be glad to hear of it at your leisure; and, having punished you a great deal for your suspicions, I am nevertheless, and ever will be, Sir, your affectionate friend and servant,

JOHN FLAMSTEED, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 175.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Jan. 17, 1712-13.

I am no longer solicitous about Sir I. Newton's behaviour, nor Raymer's: they will be always the same, whatever they pretend, and I am still the same. I doubt not but the same good Providence that has ordered all my affairs, orders this now for the best; I will not think myself wiser than God, and I submit entirely to his will.

* * * * * * * * *

I have calculated the places of Saturn both from Jan. 19, 1712, when Saturn was in opposition of the Sun last year, when I found the error 6′ 38″: and for this last, 1713, Jan. 25th, 12′ 35″, when it was 8′ 51″, and find the error the same in 1654 (that is, 59 years ago). If I live a few years longer, or you after me, we may probably see a return of the errors that are found in my numbers, compared with Minheer Hevelius's observations. For if they arise from gravitation, those causes make a near return in 59 years.

Pray take care of your health: let me hear of you sometimes, and assure yourself I am always desirous to make you easy, and oblige you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 176.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Jan. 24, 1712-13.

Sir,

This letter you will think scarcely worth the postage, for I do not: but I could not forbear writing to you, to prevent any suspicions that I neglected you, because I have not sent you the remaining part of my catalogue. Dr. Halley had the confidence to tell me, in my own house, that he would print it anew, as I think I told you in one of my last letters. Since then, some of his associates have said the same thing, which makes me delay my intended publication; and, in the meantime, I have caused my solar tables to be copied, to be added to it, and am writing a preface to it, that shall prevent all their small designs. In the meantime I must desire you to keep what you have very close; you may let any friend see it, but suffer not a line of it to be copied. And if you can, secure the rest that is printed, so that, in case of your sudden decease, it may be returned safe into my hands or Mr. Hudson's. I shall nevertheless send you what I designed, for I shall esteem anything in your hands as safe as in my own.

On Monday last, Signior Blanchini, a Roman (who answered a letter of mine to Signior Champini, since a cardinal, and dead 28 years agone), came hither to see me: he presented me with the French *Connoissance des Tems* for this year. It shows all the planets' places, with their southings, risings, or settings, each fifth day of the year; but from tables that are yet very faulty, and err 12 or 14 minutes in Saturn, and as much in Jupiter; the rest I shall examine ere long. He has given me also the figures of the constellations, on a globe borne by an Atlas, standing now in the Farnesian Gardens. 'Tis remarkable that the places of the constellations answer to Ptolemy's age: the figures are poor; whereas, when I drew the constellations, I could not but take notice that the forms that would fit them showed that the designers were excellent draughtsmen. He has also presented me with his *Problema Pascalis*: 'tis a piece I should look upon as trifling if it had not in it some Roman observations of the sun's meridional heights, compared with B. Walter's, that may be of use to me. What more I learn by his conversation shall be imparted to you when God gives me another occasion to write to you. I am, Sir, your affectionate friend and servant,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 177.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

Feb. 26, 1712-13.

Ordered, that a letter be sent to Mr. Flamsteed, to communicate his last year's observations to the Society; and that the Committee formerly appointed for that business be summoned to meet next Thursday, at half an hour after three o'clock, to draw up the said letter.

[Extracted from the *Journal* Book of the Royal Society.]

No. 178.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 5, 1712-13.

The Committee appointed at the last meeting to draw up a letter to be sent to Mr. Flamsteed, to send to the Society his last year's observations, in obedience to her Majesty's letter for that purpose, presented the said letter, which was signed and delivered to Mr. Hunt, to be accordingly delivered to Mr. Flamsteed.

[Extracted from the *Journal* Book of the Royal Society.]

The following is the letter above mentioned.

Crane Court, March 5, 1712-13.

SIR,

The Committee of the Royal Society appointed for that purpose, do hereby give you notice, that they expect that you will in due time send them a copy of your observations made the last year, and ending with December, 1712, to the Society's house in Crane Court, according to the directions of her Majesty's most gracious letter of the 12th of December, 1710.

We are your humble servants,

IS. NEWTON.
F. ROBARTES.
HANS SLOANE.
ABR. HILL.
EDM. HALLEY.

[Copied from the *original* in MSS, vol. 35, page 121.]

No. 179.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

June 25, 1713.

The President in the chair. A small bundle of papers being sent by Mr. Flamsteed, with this superscription, "I desire that these observations, made at Greenwich, anno 1712, may not be "opened but in a full meeting of the Royal Society, myself being present. JOHN FLAMSTEED." They were delivered to Mr. Waller, to be locked up till the next meeting.

[Extracted from the *Journal* Book of the Royal Society.]

No. 180.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

July 2, 1713.

The President absent: Dr. Sloane Vice-President. The papers from Mr. Flamsteed, delivered the last meeting to the Secretary, to be locked up, were ordered to be opened by Mr. Waller, which was accordingly done at the Board, in the presence of the Society, and the title was read, viz. "Observationes cœlestes Britannicæ Regiæ, Grenovici, in Observatorio Regio habitæ, Anno 1712." Dr. Halley having some time looked upon them, they were again delivered to the Secretary, Mr. Waller, to be locked up till further order: which was done.

[Extracted from the *Journal* Book of the Royal Society.]

No. 181.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

July 23, 1713.

The President in the Chair. Mr. Flamsteed's astronomical papers were, by order of the Society, delivered by Mr. Waller to Dr. Halley to consider of them, and to prepare those observations for the Transactions.

[Extracted from the *Journal* Book of the Royal Society.]

No. 182.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 16, 1713.

Sir,

I fear you think it long ere you hear from me: I assure you I am, I pray God for it, in good health; and though I grow daily feebler, yet I have strength enough to carry on my business strenuously, though I am fallen upon the most difficult part of it. I have picked out six dozen of lunar observations from my store; 36 of which were taken with the sextant in the years 1677, 78, 79, and as many answering them, at 18 years' and about 10 days' distance, with the meridional arc. You know that, in this space of time, the moon returns to near the same distance from the sun, in the same anomaly, and distance from the node. So I shall see hereby, whether the inequalities return, or not, in it; and at the same time what corrections the equations will require. I employ, after my usual manner, two persons, my own servant and Mr. Ryley, to make the requisite calculations of the moon's true and apparent places from my tables, in order to compare them with those determined by you and the deceased Mr. Witty; or those done by Mr. Ryley and my servants. For, as you well know, a single calculator is not to be relied on in this work; and I do not rely on any calculation of my own, except it be repeated or confirmed by good circumstances.

I have now frequent occasion to make use of that nonagesimary table you calculated for me when you were first my domestic. I supposed then the latitude of this place 51° 28′ 10″, as I had determined it by the sextant in the year 1676; but you know that, since then, in the year 1690, it was determined by the mural arc, 51° 28′ 30″, which being a fixed instrument is rather to be relied on, and so I now constantly use it. This change will make some alteration in the table; but the effect will be altogether insensible in the calculation of the parallaxes of longitude, and therefore I do not trouble my calculators to make a new table at present: they shall do it when I have better leisure for them.

I have forgot whether you have a copy of my lunar tables or not, but I think you have; pray ascertain me in your answer.

I have had pretty good success with the superior planets. I doubt not, but through God's blessing, I may have as good with the moon, especially if your eyes hold good, that you can lend me your assistance; for, after all Raymer's boast of his treacherous patron's performances, I am sure we shall find much greater corrections necessary than they dare own. 'Tis pain to me to write: you will therefore excuse a short letter from your assured friend and servant,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 183.) *Letter from the Royal Society to Mr. Flamsteed.*

London, July 30, 1713.

Sir,

I thought it proper to give you notice, that there is a Committee appointed by the Royal Society to give you a visit on Saturday, in the afternoon, in order to see what repairs or conveniences are wanting for your house or Observatory; which, I presume, you may not take amiss from,

Sir, your very humble servant to command, J. Machin.

[Copied from the *original* in MSS, vol. 35, page 147.]

No. 184.) *Letter from Mr. A. Sharp to Mr. Flamsteed.*

Horton, August 1, 1713.

Sir,

Your kind letter of the 16th past has dispelled all my fears, and given good encouragement to hope for a seasonable accomplishment of your laudable designs, which I perceive you are still prosecuting with as much vigour as your circumstances will permit. If you have succeeded so well in the superior planets, as you are pleased to intimate, you have made a greater progress in your business than I apprehended; and have made a large step towards the perfecting of what you have undertaken; wherein others, notwithstanding all their specious pretences and loud boasts, have miscarried or failed. If you meet with but the same success in the moon (which I look upon as your most difficult province), the greatest and most arduous part of your work will be in a good measure dispatched. However, there is already sufficient to silence the unreasonable clamors of querulous and unthinking persons, who are too ready to pass unjust censures upon your having been so long in so considerable a post, and produced little or nothing: as some, I have lately met with, had the confidence to allege; whom I immediately convinced of the unreasonableness of such a suggestion, both by informing them what you had already done, and showing what you were pleased to communicate; allowing me that liberty which I only made use of for your just vindication. On this account it would be a very great satisfaction, both to myself and others of your real friends, if you would please to permit, what you have already printed, to be published: which (no doubt) would be a more universal and effectual vindication of your great industry and abilities, than all that your friends are capable of doing. I hope the preface to your catalogue, which some time ago you hinted you had in hand, is all ready, or will shortly be printed; which you told me would prevent the sinister designs of your antagonist. When it is ready, I hope you will be pleased to allow me the sight and enjoyment of it, as you have already of what is more valuable: and question not you will be mindful of your promise of some of the prints of your own effigies, when a fit opportunity offers. In the mean time, desire you will please to let me know in your next, whether you think fit to depend on me for the calculation of the eclipses of Jupiter's satellites for the next year, or employ your servant to do it, as you intimated you intended: though I cannot set about it yet, since I shall be taken up with other business which will unavoidably hinder for some time; yet, upon notice of your pleasure, shall readily undertake it: and you may be assured nothing shall be wanting for your satisfaction or advantage, that is in the power of,

Sir, your most obliged friend and humble servant, Abr. Sharp.

[Copied from the *original* in MSS, vol. 34.]

No. 185.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

August 24, 1713.

A letter was read, and approved of by the Council, to be sent to the officers of the Ordnance, viz.

To the Honorable the Principal Officers of Her Majesty's Office of Ordnance.

GENTLEMEN,

Her Majesty, having, last winter was two years, constituted the Royal Society visitors of the Royal Observatory at Greenwich, was pleased a few days since, in pursuance of that her order, voluntarily with her own mouth, to give fresh commands to our President, that he, and the rest of the gentlemen of the Society, should take care of Mr. Flamsteed's Observatory. Whereupon the Society sent thither some of their members to view the same, and they have reported the state thereof, in respect of the instruments, and what repairs they need; and that some of them are not the Queen's, nor capable of being made sufficiently fit for use. The Observatory being supported and repaired by your Office, we, the President and Council of the Society, take the liberty to lay these things before you, being ready, on our part, to do what in us lies for the putting her Majesty's commands in execution in the best manner: and this tending to make the Observatory more useful and creditable, we pray the favor of your answer, and remain,

Gentlemen, your most humble servants.

From our house, in Crane Court, in Fleet Street, 24th Aug. 1713.

[Extracted from the *Council* Book of the Royal Society.]

No. 186.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, October 31, 1713.

SIR,

I am now three months your debtor, for which you will pardon me when I tell you, I have been forced to a suit in law, with a Peer of Scotland, about a trust which fell to my wife; and of which I hope I am now discharged by a decree in Chancery: that my journey into the country has been more troublesome, and my stay there six weeks, which is something longer than ordinary: and that I had there the moon under my hands, and having examined 36 pages of observations made at 18 years and 10 days distance from each other, I find that, at the end of this period, when the moon returns to the same anomaly, in the same distance nearly from the sun, there are nearly the same errors.

And that Sir Isaac Newton's sixth equation is not allowed by the heavens. He has lately published his *Principia* anew, wherein he makes this equation ablative where it was formerly to be added, and to be added where it was subductive; and has altered his seventh, so as in part to destroy it. But, I have not yet examined how this will answer; for I have 112 more observations under my man's, and Mr. Ryley's hands, in good forwardness. Mr. Ryley has almost gone through his calculations; Joseph has about half of all his done; so that I hope I may have them all, to compare together and correct, in a month or six weeks: after which I hope I may give you more Tables, not only of Saturn, Jupiter, Mars, and Venus, but much better than those you have a copy of, for the moon.

I think, from what I have got already, that not only the sixth, but some other of the small equations, will be laid aside yet; but I dare not affirm anything positively till my double calculated 180 vouchers are ready. The places of the apogee and node require amendment; and perhaps their equations will need correction.

The inclinations of the limit are also faulty. I shall be glad if the new correction Sir I. Newton or his servant E. Halley has given would hold. I will try, and you shall know the result; but I want three or four more pair of hands, such as yours, to carry on the work, and am heartily sorry that you are removed so far from me, that I cannot readily make use of you for it. Sir I. Newton still continues his designs upon me, under pretence of taking care of the Observatory, and hinders me all he can: but, I thank God for it, hitherto without success. Lately he was for making me new instruments, which I want not: by the way, he has given me occasion to prove that all the instruments in the house are my own; and I have good evidence that Sir J. Moore gave me the sextant and clocks, and that they are at my own disposal: but I hope I shall not long be troubled with him. I think his new *Principia* worse than the old, save in the moon; and there he is fuller, but not so positive, and seems to refer much to be determined by observations. The book is really worth about 7*s.* or 8*s.*: it costs 4*s.* 4*d.* a-piece, printing and paper. Dr. Bentley puts the price 18*s.*; and so much mine cost me. I am told he sent Sir I. Newton half-a-dozen, and made him pay 18*s.* a piece for them. Perhaps this was contrivance: possibly it is not true.

Last night, after a long intermission, I caused my servant to observe the emersion of the fourth satellite. The calculation from my Tables made it at $7^h\ 36'$; my man saw it emerging very small at $6^h\ 55'$ by the clock, which was then about two minutes too fast.

If you calculate the satellite eclipses for the next year, pray make no alterations in the motions: for I reckon that the errors observed last year are diminishing, and proceed from a cause that depends on the distance of the planet from Saturn.

1713, Aug. 9,	at 14^h	$45'$,	Jupiter, by obs. in	♓	$9°\ 25'\ 54''$	lat. aust.	$1°\ 25'\ 10''$
10,	14	41		♓	9 18 17		1 25 30
Oct. 29,	7	23		♓	3 22 46		1 18 30

You will see hereby that Parker's ephemeris errs $12'$ in the planet's place; the French as much. I pray God keep you in health: I am better of my gout, at present, than usual, I praise Him for it; but my hand shakes, and I write so ill, that I am afraid you can scarce read it. But I can still tell you I am, in all sincerity,

Yours, to serve you,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 187.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Dec. 8, 1713.

Sir,

Yours of the 20th past, that brought me the satellite eclipses for the next year, I have received, and thank you for them. As my man observes them, you shall have the observations sent you: we have seen none here since those my last brought you; it having been constant foggy, moist, and cloudy weather, such as I remember not to have continued so long since I came hither, at this time of the year; rather have usually had winds and storms.

My man is gone to Burstow; and this week at his return will have much business, otherwise you should have had rules sent you for calculating the new variations that Sir Isaac Newton applies for the correction of my numbers, and the Horroxian theory of the moon. I have translated them; and they lie ready for his transcribing when he returns.

In the mean time I am seeing how they will better the business; and in a fortnight I hope I may acquaint you.

On St. Andrew's day, Dr. Sloane laid down his Secretaryship of the Royal Society: but either he, or another, had so managed the business, that Sir Isaac Newton had like to have been left out of the Presidency. There were high and furious debates. Dr. Halley is Secretary in Dr. Sloane's room; and Dr. Keill is brought into the Council. Sir I. Newton sees now that he is understood.

I would not have put you to charge of postage for so slight a letter as this, but that I know you earnestly expect one: and that you may see I can never be unmindful of you, or unthankful for what you impart to me. I thank God for it, I enjoy my health better than formerly; the gout is gone off, and I have no pain; but am feebler than I was, as my increasing years require. I pray for yours, and that we may both keep our journey's end continually in our mind; and am,

Sir, ever yours, affectionately,

John Flamsteed, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 188.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

December 24, 1713.

The President in the Chair. A letter from Dr. Arbuthnott to Dr. Sloane was read, enclosing a copy of a letter from Mr. Secretary St. John, by her Majesty's order, to the Board of Ordnance, December 12, 1710, relating to the Royal Society's taking a particular care of her Majesty's Royal Observatory, and the instruments thereto belonging. Ordered, that a letter from the Society to the Board of Ordnance be drawn up, and that Dr. Halley attend the President in order thereto.

[Extracted from the *Journal* Book of the Royal Society.]

No. 189.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

February 11, 1713-14.

The President in the Chair. It was ordered that a letter be written to Mr. Flamsteed, for the last year's Observations; and that he be desired to meet the Society, at one of their next meetings, to consider of the repairs of his house and instruments; and what may be necessary to be provided anew for the use of the Observatory.

[Extracted from the *Journal* Book of the Royal Society.]

No. 190.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

February 18, 1713-14.

The President ordered that a letter be written to Mr. Flamsteed, to send the Society his Observations of the year 1713; and that he sign them himself, as his own, and not his servant, as the last year. And that he be directed to observe the moon as often as occasion shall offer.

The President read the following letter of my Lord Bolingbroke to the office of Ordnance, as follows:

Whitehall, 12th Dec. 1710.

GENTLEMEN,

I send you enclosed, by the Queen's command, a copy of her Majesty's letter to the Royal Society, appointing the President, and in his absence the Vice-President, together with such others as the Council of the said Royal Society shall think fit to join with them, to be constant visitors of the Royal Observatory at Greenwich: and I am, at the same time, to signify her Majesty's pleasure to you, that you do receive and take notice of such representations as the said visitors shall think fit to make to your Board concerning her Majesty's instruments at any time remaining in the said Observatory: and that you order them to be repaired, erected, or changed, as there shall be occasion: and if any instruments be now there which do not belong to her Majesty, you are to give necessary directions for purchasing the same. Her Majesty is likewise pleased to direct that you should have regard to any complaints the said visitors may make to you of the behaviour of her Majesty's Astronomer and Keeper of the said Observatory, in the execution of his office.

I am, Gentlemen, your most humble servant,

H. ST. JOHN.

At the same time the following draught of a letter to the office of Ordnance was read and approved:

GENTLEMEN,

Her Majesty having authorized our President or Vice-President, and such of the Council as the Royal Society shall nominate, to be visitors of the Royal Observatory at Greenwich; and we, the said visitors, understanding that a letter dated 12th December, 1710, was then by her Majesty's order, sent to your Board by the Right Hon. Lord Viscount Bolingbroke, Principal Secretary of State, signifying her Majesty's pleasure that you do receive and take notice of such representations as the said visitors should make to your Board, concerning her Majesty's instruments in the said Observatory, and that you should order them to be repaired, erected, or changed, as there shall be occasion, or purchased for the Observatory, if any be there which do not belong to her Majesty; and her Majesty having sent to the Royal Society fresh orders to take care of the said Observatory, we, the visitors aforesaid, take the liberty to represent to you,

That in the great room of the Observatory, up one pair of stairs, there are two clocks, with inscriptions upon them, signifying that Sir Jonas caused them to be made. These two clocks, or two others as good, are requisite for that room.

That in the said room there is a brass quadrant of four feet radius, for observing the altitudes of the sun and stars. It is not well divided. Either this, or a new one of the same size, is requisite for that room.

That in the same room there is wanting also a good telescope of about eight feet radius, furnished with a good micrometer; and another good telescope of about sixteen feet radius.

That in the garden, there being a shed with a sextant, a wall quadrant, and a clock therein, the shed should be removed six or eight yards farther from the brow of the hill, that the ground may not sink under it. And the western wall should be thick and firm, with a broad foundation, that it may not warp; because the wall quadrant is to be fixed upon it. The sextant is grown rusty, and should be cleaned, and there should be a new wall quadrant made, the old one being much wore by long usage, and belonging, as we hear, to Mr. Flamsteed. The clock is also said to be Mr. Flamsteed's, and a better clock would be more useful.

If you please to give order to an able workman to repair these instruments, and make new ones where they are wanting, and to another workman to take care of removing the shed in the garden, some of us will go with them to Greenwich, and show them what is wanting to be done, and give the best advice they can for doing every thing after the best manner.

We are, Gentlemen, your most humble servants.

It was upon ballot ordered, that a Committee named to inspect the Observatory at Greenwich, in the Council of December 14th, be a standing Committee of Visitors for the same: and likewise that Mr. James Pound be added to the said Committee.

[Extracted from the *Council* Book of the Royal Society.]

No. 191.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

February 25, 1713-14.

The President in the Chair. A letter was ordered to be drawn up, to be sent to Mr. Flamsteed, to remind him of his sending his astronomical observations for the year past, to the Society, in obedience to her Majesty's commands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 192.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 4, 1713-14.

The President absent: no Vice-President in the chair. A letter, ordered the last meeting to be sent to Mr. Flamsteed, was accordingly brought in by Mr. Waller; which was read, and ordered to be delivered to Mr. Flamsteed by Mr. Thomas.

[Extracted from the *Journal* Book of the Royal Society.]

The following is the Letter above alluded to.

Crane Court, March 4, 1713-14.

Reverend Sir,

Her Majesty having been graciously pleased by her letter to the President and Council of the Royal Society, dated December 10th, 1710, to authorize and empower the said Society to demand and receive yearly such astronomical observations as shall from time to time be made by her Majesty's Astronomer in her Royal Observatory at Greenwich; as was done for the year 1712;

Sir, I am now directed and appointed by the said Royal Society to acquaint you, that they expect your compliance thereto, by sending them such astronomical observations as you shall have made in the year 1713, within the time prefixed by her Majesty's said letter; and that you should sign with your own hand those observations, as a circumstance that may render them the more valuable and authentic.

Sir, it is also recommended to your observation for the future, to observe the moon in or near her quadratures, as often as it may conveniently be done.

We are now endeavouring what in us lies to get the house, observatory, and the instruments at Greenwich well fixed and fitted for use: and we should be glad to know your own opinion as to what may be proper to be done upon that account.

I am, Reverend Sir, your most humble servant,

RIC. WALLER, S. R. Secr.

[Copied from the *original* in MSS, vol. 35, page 125.]

No. 193.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 11, 1713-14.

The President in the Chair. Mr. Thomas reported that he had delivered the letter into the hands of Mr. Flamsteed, who assured him that he would be ready to comply with the Society's demands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 194.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 20, 1713-14.

SIR,

You were used, when I forbore to write to you for more than a month, to call upon me with a letter. Having received none now from you since that of Nov. 20, which brought me the satellite eclipses of the year current (for which I think I returned you a letter of thanks), I begin to suspect that you do not enjoy your health. I send this, therefore, to inquire of your welfare, and to assure you I shall always continue a just esteem for one that has served me so friendly as you have done; and endeavor to discharge myself of some part at least of the many obligations you have laid upon me, by reciprocal services, when Heaven affords me an opportunity. Since my last to you I have observed Saturn in opposition to the sun; and find him at least 43 minutes slower than the *Caroline Tables*.

But my chief employment hath been on the moon. I told you that the heavens rejected that equation of Sir I. Newton, which Gregory and Whiston called his sixth: I had then compared but 72 of my observations with the tables; now I have examined above 100 more. I find them all firm in the same, and seventh, too: and whereas Sir Isaac Newton has in his new book (pages 424 and 425) thrown off his, and introduced one of near the same bigness, but always of a contrary denomination, and a bigger in the room of the seventh, if I reject them both, the numbers will agree something better with the heavens than if I retain them. So that I have determined to lay these crotchets of Sir I. Newton's wholly aside: and I think, if you purchase not the new edition of

That in the garden, there being a shed with a sextant, a wall quadrant, and a clock therein, the shed should be removed six or eight yards farther from the brow of the hill, that the ground may not sink under it. And the western wall should be thick and firm, with a broad foundation, that it may not warp; because the wall quadrant is to be fixed upon it. The sextant is grown rusty, and should be cleaned, and there should be a new wall quadrant made, the old one being much wore by long usage, and belonging, as we hear, to Mr. Flamsteed. The clock is also said to be Mr. Flamsteed's, and a better clock would be more useful.

If you please to give order to an able workman to repair these instruments, and make new ones where they are wanting, and to another workman to take care of removing the shed in the garden, some of us will go with them to Greenwich, and show them what is wanting to be done, and give the best advice they can for doing every thing after the best manner.

We are, Gentlemen, your most humble servants.

It was upon ballot ordered, that a Committee named to inspect the Observatory at Greenwich, in the Council of December 14th, be a standing Committee of Visitors for the same: and likewise that Mr. James Pound be added to the said Committee.

[Extracted from the *Council* Book of the Royal Society.]

No. 191.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

February 25, 1713-14.

The President in the Chair. A letter was ordered to be drawn up, to be sent to Mr. Flamsteed, to remind him of his sending his astronomical observations for the year past, to the Society, in obedience to her Majesty's commands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 192.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 4, 1713-14.

The President absent: no Vice-President in the chair. A letter, ordered the last meeting to be sent to Mr. Flamsteed, was accordingly brought in by Mr. Waller; which was read, and ordered to be delivered to Mr. Flamsteed by Mr. Thomas.

[Extracted from the *Journal* Book of the Royal Society.]

The following is the Letter above alluded to.

Crane Court, March 4, 1713-14.

Reverend Sir,

Her Majesty having been graciously pleased by her letter to the President and Council of the Royal Society, dated December 10th, 1710, to authorize and empower the said Society to demand and receive yearly such astronomical observations as shall from time to time be made by her Majesty's Astronomer in her Royal Observatory at Greenwich; as was done for the year 1712;

Sir, I am now directed and appointed by the said Royal Society to acquaint you, that they expect your compliance thereto, by sending them such astronomical observations as you shall have made in the year 1713, within the time prefixed by her Majesty's said letter; and that you should sign with your own hand those observations, as a circumstance that may render them the more valuable and authentic.

Sir, it is also recommended to your observation for the future, to observe the moon in or near her quadratures, as often as it may conveniently be done.

We are now endeavouring what in us lies to get the house, observatory, and the instruments at Greenwich well fixed and fitted for use: and we should be glad to know your own opinion as to what may be proper to be done upon that account.

I am, Reverend Sir, your most humble servant,

RIC. WALLER, S. R. Secr.

[Copied from the *original* in MSS, vol. 35, page 125.]

No. 193.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 11, 1713-14.

The President in the Chair. Mr. Thomas reported that he had delivered the letter into the hands of Mr. Flamsteed, who assured him that he would be ready to comply with the Society's demands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 194.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 20, 1713-14.

SIR,

You were used, when I forbore to write to you for more than a month, to call upon me with a letter. Having received none now from you since that of Nov. 20, which brought me the satellite eclipses of the year current (for which I think I returned you a letter of thanks), I begin to suspect that you do not enjoy your health. I send this, therefore, to inquire of your welfare, and to assure you I shall always continue a just esteem for one that has served me so friendly as you have done; and endeavor to discharge myself of some part at least of the many obligations you have laid upon me, by reciprocal services, when Heaven affords me an opportunity. Since my last to you I have observed Saturn in opposition to the sun; and find him at least 43 minutes slower than the *Caroline Tables*.

But my chief employment hath been on the moon. I told you that the heavens rejected that equation of Sir I. Newton, which Gregory and Whiston called his sixth: I had then compared but 72 of my observations with the tables; now I have examined above 100 more. I find them all firm in the same, and seventh, too: and whereas Sir Isaac Newton has in his new book (pages 424 and 425) thrown off his, and introduced one of near the same bigness, but always of a contrary denomination, and a bigger in the room of the seventh, if I reject them both, the numbers will agree something better with the heavens than if I retain them. So that I have determined to lay these crotchets of Sir I. Newton's wholly aside: and I think, if you purchase not the new edition of

That in the garden, there being a shed with a sextant, a wall quadrant, and a clock therein, the shed should be removed six or eight yards farther from the brow of the hill, that the ground may not sink under it. And the western wall should be thick and firm, with a broad foundation, that it may not warp; because the wall quadrant is to be fixed upon it. The sextant is grown rusty, and should be cleaned, and there should be a new wall quadrant made, the old one being much wore by long usage, and belonging, as we hear, to Mr. Flamsteed. The clock is also said to be Mr. Flamsteed's, and a better clock would be more useful.

If you please to give order to an able workman to repair these instruments, and make new ones where they are wanting, and to another workman to take care of removing the shed in the garden, some of us will go with them to Greenwich, and show them what is wanting to be done, and give the best advice they can for doing every thing after the best manner.

We are, Gentlemen, your most humble servants.

It was upon ballot ordered, that a Committee named to inspect the Observatory at Greenwich, in the Council of December 14th, be a standing Committee of Visitors for the same: and likewise that Mr. James Pound be added to the said Committee.

[Extracted from the *Council* Book of the Royal Society.]

No. 191.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

February 25, 1713-14.

The President in the Chair. A letter was ordered to be drawn up, to be sent to Mr. Flamsteed, to remind him of his sending his astronomical observations for the year past, to the Society, in obedience to her Majesty's commands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 192.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 4, 1713-14.

The President absent: no Vice-President in the chair. A letter, ordered the last meeting to be sent to Mr. Flamsteed, was accordingly brought in by Mr. Waller; which was read, and ordered to be delivered to Mr. Flamsteed by Mr. Thomas.

[Extracted from the *Journal* Book of the Royal Society.]

The following is the Letter above alluded to.

Crane Court, March 4, 1713-14.

REVEREND SIR,

Her Majesty having been graciously pleased by her letter to the President and Council of the Royal Society, dated December 10th, 1710, to authorize and empower the said Society to demand and receive yearly such astronomical observations as shall from time to time be made by her Majesty's Astronomer in her Royal Observatory at Greenwich; as was done for the year 1712;

Sir, I am now directed and appointed by the said Royal Society to acquaint you, that they expect your compliance thereto, by sending them such astronomical observations as you shall have made in the year 1713, within the time prefixed by her Majesty's said letter; and that you should sign with your own hand those observations, as a circumstance that may render them the more valuable and authentic.

Sir, it is also recommended to your observation for the future, to observe the moon in or near her quadratures, as often as it may conveniently be done.

We are now endeavouring what in us lies to get the house, observatory, and the instruments at Greenwich well fixed and fitted for use: and we should be glad to know your own opinion as to what may be proper to be done upon that account.

I am, Reverend Sir, your most humble servant,

RIC. WALLER, S. R. Secr.

[Copied from the *original* in MSS, vol. 35, page 125.]

No. 193.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 11, 1713-14.

The President in the Chair. Mr. Thomas reported that he had delivered the letter into the hands of Mr. Flamsteed, who assured him that he would be ready to comply with the Society's demands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 194.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 20, 1713-14.

SIR,

You were used, when I forbore to write to you for more than a month, to call upon me with a letter. Having received none now from you since that of Nov. 20, which brought me the satellite eclipses of the year current (for which I think I returned you a letter of thanks), I begin to suspect that you do not enjoy your health. I send this, therefore, to inquire of your welfare, and to assure you I shall always continue a just esteem for one that has served me so friendly as you have done; and endeavor to discharge myself of some part at least of the many obligations you have laid upon me, by reciprocal services, when Heaven affords me an opportunity. Since my last to you I have observed Saturn in opposition to the sun; and find him at least 43 minutes slower than the *Caroline Tables*.

But my chief employment hath been on the moon. I told you that the heavens rejected that equation of Sir I. Newton, which Gregory and Whiston called his sixth: I had then compared but 72 of my observations with the tables; now I have examined above 100 more. I find them all firm in the same, and seventh, too: and whereas Sir Isaac Newton has in his new book (pages 424 and 425) thrown off his, and introduced one of near the same bigness, but always of a contrary denomination, and a bigger in the room of the seventh, if I reject them both, the numbers will agree something better with the heavens than if I retain them. So that I have determined to lay these crotchets of Sir I. Newton's wholly aside: and I think, if you purchase not the new edition of

his book, you will be at least 17*s.* a saver by it; for I know not whether, after that these two pages aforementioned in the margin are taken away, all the alterations and additions be worth 12*d.* I suspect, now, that the equations of the apogee are too big, and the radix of it is put too forward; but dare say nothing of it positively, till I have sorted my calculations into proper classes, which may evince the truth of this conjecture, or correct it: of which I hope I shall not long be ignorant, for my long suit I hope will be at an end this day, or Monday next. I thank God heartily for it, I enjoy my health, though Raymer gives out that I am in a declining condition. My gout is not very troublesome; it only makes me keep home more than formerly; and thereby I gain more leisure to prosecute my endeavors, which I have great hopes will be very successful, through God's blessing on them. I have received accounts of some few satellite eclipses observed last year by Mr. Pound, at Wanstead; Mr. Gray, at Canterbury; and Mr. Durham, at Upminster; which I intend you a transcript of, in my next, when I learn by your answer that this is come safe to your hands.

Pray take care to keep up my printed Catalogue: you are the only person that has a copy of it. I have not trusted James Hodgson with one, for fear that Raymer should wheedle him out of it. I know he would part with one of his ears to get one: pray therefore be very cautious how you let it be seen; and whenever you find yourself ill, commit it, sealed up, into the hands of some trusty friend, who, if the worst should happen, may immediately send it back with a letter directing me where to meet with it.

I intend to publish what Tables I have by me, for the sun and other planets, together with it. And for the maps, I have hopes to order matters so they shall not be lost: though my special friends have done all that lies in their power to hinder both them, and the Observations, from which they and the Catalogues are derived, from being published: but I hope that good Providence I have always depended upon will prevent them. May the same ever preserve you and direct you.

I am, your very much obliged friend and servant,

John Flamsteed, M.R.

You will excuse my ill hand, my man is not at home to copy this letter; as probably he must my next to you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 195.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, August 31, 1714.

Whilst I was at Burstow the Queen died; and I employed my spare hours in calculating the moon's places by my new numbers, to the middle of about 20 lunar eclipses observed by myself and the French in 40 years past; and I find I can represent them within four minutes and a quarter of longitude. But this does not satisfy me: there are still some circumstances in her motions that have not been considered, which I am getting upon as soon as I can get help and leisure; for my servant and Mr. Ryley are not enough. At present I shall only acquaint you, 1st. That I reject Sir Isaac Newton's first equation of the apogee; though I fear I shall be forced to introduce another. 2. That I add 1′ 30″ to the moon's mean motions. 3. That I diminish the greatest equations of the apogee, &c. 4. That I neglect the alterations of the variations; and 5. Allow a small equation that extends itself even to the quadratures.

When I see the effect of my thoughts concerning a new equation that depends on causes not yet thought of by our great pretender, I shall acquaint you of it, and what I have above only pointed at.

I shall now cause the eclipses of Jupiter's satellites to be carefully observed. I grow weak, and must have another servant to attend me when I walk abroad; and then I shall have an attendant for my man Joseph, when I would have him to observe them. I would therefore desire you to calculate the eclipses of the satellites for the next year, and thereby you much oblige, Sir,

Your affectionate friend and servant.

We reckon that the King is, by this day, in Holland, or will be to-morrow: he is to come ashore at Greenwich, to reside two nights in the Queen's house, which is fitted up for him, and make his entry into London in his coaches. God send a happy arrival, and reign: for his accession to the Crown has dissipated much of our fears, and he is impatiently expected. J. F.

This day's post tells us he sets out to-day from Hanover.

I had forgot to tell you that my servant acquainted me that Dr. Thoresby was here, in my absence; perhaps it was he informed you that Raymer sets up for a finder of the longitude. 'Tis more than I hear of; but, like enough, that boaster may do so among his clans. Pray give the Doctor my service, and tell him I am sorry I had not the happiness to be at home when he came here: and let me know the ground of this report from him.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 196.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 23, 1714.

I have got Jupiter twice observed of late; first the very night of the opposition, and since again on the 9th instant. By both these observations he is forwarder than Parker's ephemeris by 15′ at least, and his latitude is at least 10′ more southerly. I doubt not but the *Caroline Tables* err 15′ in longitude; but the error in latitude is certainly some mistake of Parker's in his calculation, and 'tis probable runs through his whole year.

You may be easy as to my concerns for the future. That good Providence, whereon I trusted, has been pleased so to dispose affairs, that now all those that would do any injury to the Observatory have ruined their own credit, and our friends are advanced. I bless God for it, and I have taken an easy step, that, I trust, has defeated all the designs of Sir I. Newton. I think now of engraving a capital, and closing piece, for my Catalogue; and proceed to my planetary numbers. But my age, except God bless me with unexpected strength, will scarce permit me to go through with the work as I designed it. I shall, however, as he blesses me, do my endeavor, leaving the events to his good Providence, to which you are ever recommended by, Sir, yours ever,

John Flamsteed, M.R.*

[Copied from the *original* letter in the possession of Mrs. Giles.]

* This letter is signed by Flamsteed, but written by another hand. The original draught, in Flamsteed's handwriting, is at the Royal Observatory at Greenwich, and bound up in MSS, vol. 37.

No. 197.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Nov. 30, 1714.

Parker's ephemerides for 1715 came out last week: in it he tells us that he has corrected the motions of the 2 superiors, Saturn and Jupiter, by the directions of that *filius cœli*, Dr. Halley. Now, by comparing the places of Saturn and Jupiter, December 31, in this year's ephemeris with those of January 1, I find that he has subtracted 44 from Saturn's place by the *Caroline Tables*, which is the utmost error at Saturn's last opposition to the sun, and added 6 minutes only to Jupiter, who, in this very month, was 14′ or 15′ faster than the *Caroline Tables*. So that this, I fear, is but a very slovenly correction: and if he has allowed the same constantly (as I shall see ere long whether he has or not) 'tis a very silly one. But the latitude of Jupiter, which I told you has been this year false in him 10 minutes, is now made agreeable to the Caroline numbers.

I am obliged by a friend to take his son under my care; so that now I shall have an assistant and companion for my servant, when he is to sit up; so that I shall cause the satellite eclipses to be frequently observed, and the observations to be constantly imparted to you.

My servant is gone to Burstow upon affairs; I am therefore constrained to write to you with my own hand, not without pain and trouble; though, I thank God for it, I continue still free from the torments of the *stone*: my gout pains are supportable. May the good providence of Heaven preserve you from those distempers, that we may still enjoy the pleasure of our friendly correspondence, which is more than all to, Sir, your affectionate friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 198.) *Extract of a Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Feb. 1, 1714-15.

Mr. Halley's very sorry abstract of my Observations was printed in the Philosophical Transactions without my leave or knowledge. I was forced to impart them to them, by an order of the late Queen's, procured by Dr. Arbuthnott, and sent me in a letter from the late Secretary, Mr. St. John, now Lord Bolingbroke; and I desired that they might not be printed without my knowledge. But Sir I. Newton, to do all the mischief that lay in his power, and the most spitefully, permitted Halley to abstract them. How he has done it you may see by comparing the observations of [the] lunar eclipse January 12, 1712, with the observations themselves, which I sent you in a letter soon after. He gives the middle of the eclipse at 7^h 34′, but how it was got he says not; you must take it on his word. That account, I gave you, gives the times of 10 or 12 equal phases whence it was derived: not one word of these in him, nor of the shadow passing over any spots, though always noted for good uses by Hen. Cassini, nay, even himself, in the eclipses he has observed: so that the copy of observations is not anywise mine, but a pitiful, injudicious abstract of his, an impudent and malicious one throughout, and no ways to be trusted to.

But you tell me they are improved to some good use: that, I am sure, is more than was ever designed by the thievish editor. His and Sir I. Newton's designs are, by these abstracts, to prevent and obstruct the publication of the whole: he has robbed them of all their certainty; and, by giving them the name of my Observations, made me, or endeavored to make my Observations, appear as little

worth as his own are, and as little to be desired. I wait for an opportunity to publish some few years of them myself, which I am preparing for. But for this thief, I shall never seek any revenge, or to disparage him for what he has done; for this is contrary to my temper, the religion of a Christian. I forgive him; he is now known so well, that his tricks do him the injury he designs me; and he begins, I hope, to be sensible of it.

But what you say, he has first spread here; and I doubt not but has been brought you into Yorkshire. I would desire you to let me know what Royal Society people you converse with, and what account they give you of me, that I may clear myself to you, and stop the mouths of others, amongst whom he has spread his lying suggestions. As for the correction of Parker's ephemeris, I shall be able to tell you more ere long, for we have now some likelihood of clear nights, which we have scarcely had for a month past.

I have got another observation of Jupiter lately, but I have not yet had time to examine it. When the elections are over, you may hear. I have one thing more to desire of you, and I will not doubt but that you take care of it; that is, not to suffer any copy of it to be taken, nor to lend it to any one, lest a copy of it should be taken, or itself transmitted into Halley's hands, who had the impudence to tell me that he would copy it, and print it; whereas he knows not how it is made, nor how the true place of any one star in it was first gained.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 199.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.**

The Observatory, April 23, 1715.

I was told by Mr. Whiston yesterday that Dr. Halley has wrote to you to give him your observations of the late solar eclipse. I hope you are wiser than to do it before you have imparted them to me; and then you may refer him to me for an account of them. You remember how he served you about the quadrature of the circle: after such usage you ought to be very cautious how you trust him.

* * * * * * * * *

What our Society people did I neither know nor care. I am very apt to think they had no instruments, in order, for taking the sun's heights.

By these observations I am satisfied the moon's latitude was less, by a minute, than I made it in the figure I sent you; and that the total eclipse reached near to Deal or Dover: whereas Dr. Halley makes all East Kent free of them. I owe you some observations of Saturn and Mars, but have leisure to add no more, but that I expect to hear from you how much of the sun remained uncovered at Horton.

[Copied from the *original* letter in the possession of Mrs. Giles.]

* The original draught of this letter, in Flamsteed's hand-writing, is at Greenwich, and bound up in MSS, vol. 37.

No. 200.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

June 30, 1715.

Sir,

You were pleased, when you were here last about two years ago, to tell me that you would restore my MSS in your hands. I have sent Mr. Hodgson to receive them of you, and he will return the note you gave for them. I doubt not but you will herein oblige,

Sir, your humble servant,

John Flamsteed.

[Extracted from MSS, vol. 33, page 107.]

No. 201.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 9, 1715.

Sir,

You are concerned that you hear not from me: I am as much, that I cannot find leisure so often, nor write to you with so much ease as formerly. Assure yourself I have always a due regard to you, and respect for you; but I am now near 69 years old complete, and I doubt not but you will excuse me if I cannot express myself so well, or readily as formerly.

A young man, a surveyor in your neighbourhood, was here some months ago; and lately Mr. Thoresby has been twice here: but it was not my good fortune to be at home, though I saw and discoursed the former, and had a good account by him of your health, which I pray God long to continue. If he, or any friend of yours, comes again to London, you may do well to advise me of it by a letter, that I may direct them where to meet me there, or when to find me at home; for I much regret my loss of Dr. Thoresby's company. I would not have you think I value the post-charge of a letter; I should rejoice to have one from you, though it should tell me no more but that you are in health.

I doubt not but you have heard that the Lord Halifax is dead of a violent fever. If common fame speak true, he died worth £150,000; out of which he gave Mrs. Barton, Sir I. Newton's niece, for her *excellent conversation*, a curious house, £5000, with lands, jewels, plate, money, and household furniture, to the value of £20,000 or more. Sir I. Newton loses his support in him; and, having been in with Lord Oxford, Bolingbroke, and Dr. Arbuthnott, is not now looked upon as he was formerly.

I sent last week for my manuscripts. My man brought me but two of them; the third was in Dr. Halley's hands, who is loth to part with it; but Sir I. Newton, I doubt not, will force him. After which, Sir I. Newton will have still in his hands all the observations made with the mural arc from 1689 to 1713 complete; which I shall recall as soon as I have got back the book that Dr. Halley detains.

I believe I have now an interest in some of the prime officers at Court, that will not suffer me to be used as I have been formerly. I shall recall the MSS of my Observations from 1689 to 1705 complete; and know how he has disposed of £1200 of Prince George's money, whereof I never received but £125. I shall not deal proudly with him, nor call him names, as he did me; God forgive him: but I shall use him gently and calmly, till I have got what he has of mine in his power out of his hands. God has blessed me hitherto; all that has happened, I doubt not, was by

the order of his good Providence, and for the best. I will attend him patiently, till my hopes are turned into certainties.

I have begun to revise my *Doctrine of the Sphere*, in order to reprint it with new Lunar Tables, that shall agree with the heavens within five minutes; and leave it to posterity to bring them nearer, from the Observations I shall bequeath to them for that purpose. You expect my promised observations of the superior planets: I give them on the next page.

I have not had time, as yet, to compare these with my own Tables: you may compare them with Parker's ephemeris; and you will find that, for all his pretended emendations, he is still out in Saturn 13 minutes, in Mars 8 minutes. 'Tis pain to me to write: I add therefore that I desire you to give my humble services to Mr. Thoresby, and that I am, ever,

Sir, yours, to love and serve you,

JOHN FLAMSTEED, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 202.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 29, 1715.

I have yours of the 23rd instant. I am hastening into Surrey next week, and therefore answer it so soon, lest I should be hindered by any accidental business. As for my own business with Sir I. Newton, 'tis not yet over; but you know on whom only I have trusted, and therefore I fear nothing, nor need you be solicitous for me. Whilst I am in the country I shall look over the Lunar Tables again, and try to bring them yet a little nearer the observations; and at my return, God sparing me health, my servant shall copy them for the press, and they shall begin to print: but not before everything is ready for them, for I love to call upon the printers rather than be called upon by them. You may command copies for yourself, and two or three friends, if God blesses me with life to finish it.

I have put one of the maps of the constellations into the engraver's hands to be engraved; but I foresee it must lie still till my return.

I find no reason for making the recess of the equinoxes more than 50″ a year, or a degree in 72 years. If I alter my opinion I shall let you know it, and my reasons for doing it.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 203.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 11, 1715.

Yours of the 24th past brought, unexpected, the eclipses of ♃ satellites for the next year 1716. I thank you heartily for them. I was newly returned from Burstow, where I had resided five weeks, very uneasily, by reason I found my curate had signalized himself in such a manner for High Church, that I was forced to dismiss him. I have not yet got another, and find it very difficult to get one that is not tainted with the same principles. This has caused me some journeys extraordinary to London, and I fear will do more, and is the reason why you receive my thanks so late.

The observations of the solar eclipse being printed in the Transactions of the Royal Society, I

have now deduced mine from the first notes. I remember not whether I gave them to you, or not; and therefore I present you with them here.

* * * * * * * * *

Dr. Halley makes all these phases to have happened some few seconds later than they were noted here; whereas Crane Court lies about half a minute of time westwards of us, and all the times ought to have been near as much later there than here: so that I fear there was an error in his quadrant of about four minutes; and it showed the sun's heights so much too great. But Mr. Whiston told me that their clocks were set by Mr. Tompion's meridian line, which I gave him many years ago; and I suspect he never allowed for the difference of meridians betwixt the Observatory and his house; and this probably is the cause why the times taken at Crane Court agree so nearly to those taken here.

Mr. Pound's quadrant, I fear, had the same fault with that at Crane Court; or the Doctor made his times to agree with Mr. Pound's. As for the Doctor's rants or twits, I never did, nor ever shall, take any notice of them, till he ceases to think himself a wit, or becomes an honest and grateful man.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 204.) *Letter from Mr. Flamsteed to* ———.*

The Observatory, October 24, 1715.

Sir,

I thank you for yours. I intend, God willing, to be in London on Wednesday next, and at Garraway's about 2 o'clock; where I should be glad, if any occasions of yours call you into London, to meet you, that we may take care to obviate those designs of Sir I. Newton which will bring shame both on himself and the Royal Society. In the meantime I hope you take care to inform the Duke how unworthily, nay, treacherously, I am dealt with by Sir I. Newton.

I wonder that he should so impudently pretend to dispose of the printed copies of my works, *i. e.* the printed Observations: they cost him not an hour's labor or watching, nor was he at one penny expense for making them; but besides my daily labor and watchings, when he was asleep in his warm bed, it had cost me above £2000 out of my own pocket, when I showed them to the Prince, in instruments and necessary assistance. Which when I told the Prince at Greenwich, and that I was going on to get the maps of the constellations engraved at my own charge (as they had been drawn), he was pleased to tell me that there was no need of that; and when after, by an accident, my works were recommended to him by the Royal Society, he ordered £1200 for printing them and engraving the maps, designing thereby to gratify me in that for my great pains and expense. But Sir I. Newton very ungratefully set himself to hinder the work by several tricks and artifices, in which he made much use of E. Halley, encouraged the printer to commit faults, gave me all the trouble he could; yet, in spite of all, 'tis better printed than books of the same kind usually are, through my continual care; only one sheet must be reprinted, which they took care to have misprinted, when I was absent in Surrey in 1706.

I must tell you, also, that Sir I. Newton caused me further to disburse in having assistants to copy the Observations, and fit the work for the press, about £173; stopt the press without assigning any

* The direction not extant. F. B.

the order of his good Providence, and for the best. I will attend him patiently, till my hopes are turned into certainties.

I have begun to revise my *Doctrine of the Sphere*, in order to reprint it with new Lunar Tables, that shall agree with the heavens within five minutes; and leave it to posterity to bring them nearer, from the Observations I shall bequeath to them for that purpose. You expect my promised observations of the superior planets: I give them on the next page.

I have not had time, as yet, to compare these with my own Tables: you may compare them with Parker's ephemeris; and you will find that, for all his pretended emendations, he is still out in Saturn 13 minutes, in Mars 8 minutes. 'Tis pain to me to write: I add therefore that I desire you to give my humble services to Mr. Thoresby, and that I am, ever,

Sir, yours, to love and serve you,

JOHN FLAMSTEED, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 202.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 29, 1715.

I have yours of the 23rd instant. I am hastening into Surrey next week, and therefore answer it so soon, lest I should be hindered by any accidental business. As for my own business with Sir I. Newton, 'tis not yet over; but you know on whom only I have trusted, and therefore I fear nothing, nor need you be solicitous for me. Whilst I am in the country I shall look over the Lunar Tables again, and try to bring them yet a little nearer the observations; and at my return, God sparing me health, my servant shall copy them for the press, and they shall begin to print: but not before everything is ready for them, for I love to call upon the printers rather than be called upon by them. You may command copies for yourself, and two or three friends, if God blesses me with life to finish it.

I have put one of the maps of the constellations into the engraver's hands to be engraved; but I foresee it must lie still till my return.

I find no reason for making the recess of the equinoxes more than 50″ a year, or a degree in 72 years. If I alter my opinion I shall let you know it, and my reasons for doing it.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 203.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 11, 1715.

Yours of the 24th past brought, unexpected, the eclipses of ♃ satellites for the next year 1716. I thank you heartily for them. I was newly returned from Burstow, where I had resided five weeks, very uneasily, by reason I found my curate had signalized himself in such a manner for High Church, that I was forced to dismiss him. I have not yet got another, and find it very difficult to get one that is not tainted with the same principles. This has caused me some journeys extraordinary to London, and I fear will do more, and is the reason why you receive my thanks so late.

The observations of the solar eclipse being printed in the Transactions of the Royal Society, I

That in the garden, there being a shed with a sextant, a wall quadrant, and a clock therein, the shed should be removed six or eight yards farther from the brow of the hill, that the ground may not sink under it. And the western wall should be thick and firm, with a broad foundation, that it may not warp; because the wall quadrant is to be fixed upon it. The sextant is grown rusty, and should be cleaned, and there should be a new wall quadrant made, the old one being much wore by long usage, and belonging, as we hear, to Mr. Flamsteed. The clock is also said to be Mr. Flamsteed's, and a better clock would be more useful.

If you please to give order to an able workman to repair these instruments, and make new ones where they are wanting, and to another workman to take care of removing the shed in the garden, some of us will go with them to Greenwich, and show them what is wanting to be done, and give the best advice they can for doing every thing after the best manner.

We are, Gentlemen, your most humble servants.

It was upon ballot ordered, that a Committee named to inspect the Observatory at Greenwich, in the Council of December 14th, be a standing Committee of Visitors for the same: and likewise that Mr. James Pound be added to the said Committee.

[Extracted from the *Council* Book of the Royal Society.]

No. 191.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

February 25, 1713-14.

The President in the Chair. A letter was ordered to be drawn up, to be sent to Mr. Flamsteed, to remind him of his sending his astronomical observations for the year past, to the Society, in obedience to her Majesty's commands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 192.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 4, 1713-14.

The President absent: no Vice-President in the chair. A letter, ordered the last meeting to be sent to Mr. Flamsteed, was accordingly brought in by Mr. Waller; which was read, and ordered to be delivered to Mr. Flamsteed by Mr. Thomas.

[Extracted from the *Journal* Book of the Royal Society.]

The following is the Letter above alluded to.

Crane Court, March 4, 1713-14.

REVEREND SIR,

Her Majesty having been graciously pleased by her letter to the President and Council of the Royal Society, dated December 10th, 1710, to authorize and empower the said Society to demand and receive yearly such astronomical observations as shall from time to time be made by her Majesty's Astronomer in her Royal Observatory at Greenwich; as was done for the year 1712;

Sir, I am now directed and appointed by the said Royal Society to acquaint you, that they expect your compliance thereto, by sending them such astronomical observations as you shall have made in the year 1713, within the time prefixed by her Majesty's said letter; and that you should sign with your own hand those observations, as a circumstance that may render them the more valuable and authentic.

Sir, it is also recommended to your observation for the future, to observe the moon in or near her quadratures, as often as it may conveniently be done.

We are now endeavouring what in us lies to get the house, observatory, and the instruments at Greenwich well fixed and fitted for use: and we should be glad to know your own opinion as to what may be proper to be done upon that account.

I am, Reverend Sir, your most humble servant,

RIC. WALLER, S. R. Secr.

[Copied from the *original* in MSS, vol. 35, page 125.]

No. 193.) *Proceedings of the Royal Society relative to Mr. Flamsteed.*

March 11, 1713-14.

The President in the Chair. Mr. Thomas reported that he had delivered the letter into the hands of Mr. Flamsteed, who assured him that he would be ready to comply with the Society's demands.

[Extracted from the *Journal* Book of the Royal Society.]

No. 194.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 20, 1713-14.

SIR,

You were used, when I forbore to write to you for more than a month, to call upon me with a letter. Having received none now from you since that of Nov. 20, which brought me the satellite eclipses of the year current (for which I think I returned you a letter of thanks), I begin to suspect that you do not enjoy your health. I send this, therefore, to inquire of your welfare, and to assure you I shall always continue a just esteem for one that has served me so friendly as you have done; and endeavor to discharge myself of some part at least of the many obligations you have laid upon me, by reciprocal services, when Heaven affords me an opportunity. Since my last to you I have observed Saturn in opposition to the sun; and find him at least 43 minutes slower than the *Caroline Tables*.

But my chief employment hath been on the moon. I told you that the heavens rejected that equation of Sir I. Newton, which Gregory and Whiston called his sixth: I had then compared but 72 of my observations with the tables; now I have examined above 100 more. I find them all firm in the same, and seventh, too: and whereas Sir Isaac Newton has in his new book (pages 424 and 425) thrown off his, and introduced one of near the same bigness, but always of a contrary denomination, and a bigger in the room of the seventh, if I reject them both, the numbers will agree something better with the heavens than if I retain them. So that I have determined to lay these crotchets of Sir I. Newton's wholly aside: and I think, if you purchase not the new edition of

deal of trouble, yet they designed to have begged the rest of the copies for him. I told you in mine of October 24 last, that when I dismissed my calculators at Midsummer, 1706, I had been at £173 charge for their wages and accommodations, and some expense in attending the work at the press; and that on March 20, 1707-8, the referees met me in London, and having seen the observations made with the mural arc (175 sheets) offered to pay me £125 of my bill, provided I would then leave them in their hands: which, to secure some part of my expense, I was forced to do, on promise that the press should go on to print them. It was some months after ere I could get the £125; and I am apt to think, had it not been for Dr. Arbuthnott, I should never have received it. I gave a receipt for it at the bottom of the bill I had drawn up of my charges: hereby you will see that this money was so far from being a gratuity, that it was only part of a just debt, and that there is still £48 and above due to me on *that* account. As for trouble I gave *them* (the pluralities of himself) it was none but what Sir Isaac Newton occasioned by his own extremely perverse behaviour; which some of his referees could not but take notice of at that very time. As to the disposal of the books, the representation, you tell me, says the Queen designed to give them,

1. To public libraries.
2. To professors and eminent mathematicians at home and abroad.
3. To the nobility who have libraries of note.
4. To the Prince's friends, Mr. Compton, Mr. Clark, and their friends.
5. To the Referees and their friends.

When these are served, Sir Isaac thinks he has made so many fast friends; but at whose cost does he do it? the pains were mine, the expense, the Prince's or the public. Yet I must look upon it as a favour if he vouchsafe to beg the rest for me, when all persons were furnished that would buy them: this is a spiteful and malicious contrivance; but there is a worse still remains. Of the £714 12*s.* disbursed on the edition, he accounts what he paid Halley and Machin for their pains bestowed in *spoiling it*: for the catalogue is absolutely spoiled, [and] the abstracts of my Observations are very sorrily done, so that it will be a shame to our nation to have them seen in any public library. Whether Sir I. Newton suffered these to be done out of malice or ignorance, he ought to pay his workmen and tools out of his own pocket, and not to charge his follies on the public.

I doubt whether you will intimate what I have now told you to his Grace or no; but I shall speak of it upon all proper occasions. Sir, I should be glad to see you here, and a short account of what successes you meet with will be very welcome to, Sir, your much obliged friend and servant,

JOHN FLAMSTEED.

[Copied from the *original* draught, in MSS, vol. 35, page 135.]

No. 209.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 13, 1715-16.

From the year 1675 to 1711, my new tables of Saturn seldom or never err more from the heavens than three minutes: but, since the year 1711, the motion in the heavens has retarded gradually; so that last year, 1715, February 27, his observed place was 14 minutes slower than the tables gave it. I have not yet calculated his places from the tables for the last observations; but shall do it ere long. I hope my next will bring you some news that will be more acceptable to you;

though to me, it will bring both charge and trouble. The first I do not much value; and I hope that God, who hitherto blessed me with sufficient strength to bear my long and difficult labors, will enable me to go through with them.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 210.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 29, 1715-16.

God has in some measure granted me what I desired. Sir Isaac Newton had contrived to dispose of the printed volumes of my Observations in such manner that they should have been spread all over Europe as *his* gift to libraries and ingenious persons; with Dr. Halley's copy of my spoiled Catalogue of the fixed stars, and a malicious preface of Halley's that was wrote, without my knowledge, to it; as also his abstracts of the planetary observations taken with the mural arc, of which I trusted a copy into his hands to be printed March 20, 1707-8. I was fully informed of his intent; and, therefore, making my application by proper persons, got his Majesty's order to have 300 copies of them delivered unto me: and last night my man brought them down to the Observatory; though Mr. Churchill was by agreement to print but 400. Sir Isaac Newton has sent three copies into Italy, some say to the Pope; one to the King of France; one to Mons. Torcy, and Des Marets each one; 10 to the Royal Academy of Paris; and about 40 to the Exchequer; of which I am told the French Envoy has had 17. So that there is 9 or 10 left in the Exchequer, and 39 in Mr. Churchill's hands, which I am endeavoring to get into my own hands, that I may hinder any more of the false catalogues from going abroad, or his very sorry abstracts, which I intend to sacrifice to TRUTH, as soon as I can get leisure; saving some few that I intend to bestow on you, and such friends as you, that are hearty lovers of truth, that you may keep them by you as evidences of the malice of godless persons, and of the candor and sincerity of the friend that writes to you, and conveys them into your hands: for I will not say I make you a present of that which is so odious of itself, and will be detested by every ingenuous man. Pray let me know in your next how I may send you this precious parcel.

After the holidays, the press shall go on again, with Mr. Gascoigne's, Mr. Crabtree's, and my own observations at Derby (God sparing me life and health) and yours, and Mr. Witty's places of the planets calculated from the sextant observations. Perhaps we may go on with both these parts at once, for greater expedition. I would not therefore, as yet, bind up the true catalogue and observations together, because I am preparing prefaces, and something more, to go along with them.

* * * * * * * * *

I bless that good Providence, that never fails me, for its assisting me at last. I commend you to the protection of the same, and am always, Sir, your real and obliged friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 211.) *Letter from Mr. Flamsteed to Sir Isaac Newton.*

The Observatory, April 23, 1716.

SIR,

Pray return me by the bearer (my servant, Josepth Crosthwait) my 4to MS of night-notes, from November 1678 to February 1684, which it seems were not at hand when you returned those of the preceding and following years.

With the same I desire you to return also the 175 MS sheets of observations, made with my mural arc, which were trusted into your hands, March 20, 1707-8; together with so much of my catalogue as was delivered into your hands, sealed up at your own request, with my *original copies* of what is printed, which have not been yet returned.

And if with them you send me the copper plates belonging to what is printed, you will oblige me, and prevent further trouble from Your humble servant,

JOHN FLAMSTEED.

My man has your receipt for the 4to book of night notes, which, on your delivery of the book to him, he will return, and give you receipts for what other things you send per him.

[Extracted from MSS, vol. 33, page 108.]

No. 212.) *Extracts of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 8, 1716.

I shall take up with me some copies of the corrupted Catalogue, &c. to be stitched up for you and some friends: and I hope, the week after, to leave one with the carrier for you, directed as you order. I committed them to the fire about a fortnight ago. If Sir I. Newton would be sensible of it, I have done both him and Dr. Halley a very great kindness.

* * * * * * * * *

Yesterday I sent an attorney to Sir I. Newton, to demand an appearance for detaining one of my books of observations, with 175 MS sheets of observations made with the mural arc; but he would not be seen. So a note was left for him by the attorney. You shall have an account of all our proceedings: I am not fond of war.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 213.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, June 2, 1716.

On Wednesday last, in the evening, my man delivered to Mr. Hall, a Bradford carrier, my Catalogue of the fixed stars, as it is corrupted and spoiled by Dr. Halley. All the faults are marked in it, with lines under them; the stars that are false placed are marked in the margin. When you compare them with my own catalogue (whereof I gave you a copy about three years ago), you may perhaps find more errors than I have noted. If you do, pray keep a list of them, and let me have a copy of it in good time. With it, in the same cover, are bound up his sorry abstracts of the

planetary observations, taken with my mural arc; wherein he numbers the stars according to his own account, but no ways conformable to my own catalogue. He is as lazy and slothful as he is corrupt. With my lunar observations he gives her true places and latitudes, which are copied from the three large synopses that I imparted to Sir Isaac Newton, *under this condition that he should not impart them to anybody, without my leave.* Yet so true to his word, and so candid is the Knight, that he immediately imparted it to Halley; who has printed them as far as they reach: and afterwards thrust in the moon's places from the ephemerides, or rather, I believe, from the margin of my book of observations, which is now in his hands; for the *lazy and malicious thief* would scarce be at the pains to gather them himself from the almanacs.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 214.) *Extracts of a letter from Mr. A. Sharp to Mr. Flamsteed.*

Horton, June 11, 1716.

Yours, both of the 8th past, and 2nd instant, received; but thought it not proper to answer either till I could give you an account of the receipt of the books, which came to hand on Saturday last; for which, return you hearty thanks. Hereby am rendered more sensible of the scope of their design, which, undoubtedly was, out of envy, to wrest the honor and reputation of all your labors from you, and ascribe it to themselves, as far as with any pretence of reason they can; and would have the world obliged entirely to them for this edition of your works; since, in the preface, your delay in publishing your works is invidiously misrepresented, and *their* performance in forcing them out of your hands so extravagantly magnified. And with what an air of ostentation doth he represent his labor in calculating the six constellations next the north pole; as if all you had done were nothing comparable thereto. Yet I perceive, by the lines drawn through them, (having not yet had time to examine or compare them with yours,) that the greatest part of them are false. I shall be in some measure restless and in pain till I see your *vindication*, which I question not but you will do effectually; so as all things relating hereto will be set in a true light, and the learned world undeceived, whom they have attempted unworthily to impose upon, had you not seasonably prevented it by seizing these papers.* I hope, ere long, when your genuine works are made public, not only all whom these persons have endeavored to impose upon will be disabused, but themselves will be exposed to just censure; and the reputation, which they have attempted to defraud you of, and ambitiously aspired at for themselves, will be turned to their greater disgrace and confusion.

* In a subsequent letter (dated December 1, 1716), Mr. Sharp says, "The particular account you are pleased to "give me, of the matter contained in your preface is exceedingly acceptable: everything you mention being so "necessary, pertinent, and useful, as will render it very valuable; and convince the world of the real difference of "your *true*, and that *surreptitious* edition of your catalogue by Dr. Halley; who, in his preface, seems not so much "to aim at the public benefit, as to magnify his own performances, and put a slight upon, and undervalue yours. "But what you propose to write will set all matters in a true light; and effectually (though perhaps not directly) "discover the impertinency and mismanagement of those who, in seeking to expose yours, have rendered themselves obnoxious to the greater disgrace." There was no person so capable of judging of the respective merit of the two works, here alluded to, as Mr. Abraham Sharp, who had himself borne so prominent a part, not only in making instruments and observations, but also in computing the results, and in forming the maps: one who was, in fact, conversant with every branch of practical astronomy. F. B.

'Tis with great satisfaction that I understand that more of your works are going to the press. Nothing can so effectually contribute to your vindication, and the preventing and wiping off all their censures they would fix upon you, as the public appearance of *your own original uncorrupted performances:* which if the untimely appearance of this *spurious* piece do but hasten, 'tis the greatest benefit that can be expected from it.

* * * * * * * *

The appearance of Providence on your behalf preventing the designs of those who have endeavored to prejudice and circumvent you, gives good encouragement to hope that all your laudable designs will, in due time, be issued to your abundant satisfaction and advantage: than which, no earthly thing can be more desirable and pleasing to, Sir, your most obliged friend and humble servant.

[Copied from the *original* letter in the Royal Observatory, MSS, vol. 34.]

No. 215.) *Letter from the Royal Society to Mr. Flamsteed.*

Crane Court, Fleet Street, June 7, 1716.

SIR,

I am commanded by the President, Council, and Fellows of the Royal Society, to put you in mind that you are in arrears to them a copy of your Astronomical Observations for the year 1714; and that those of 1715, ending with December last, are now become due to them: both of which they require you to send them on or before Midsummer Day next, as you are obliged to do by her late Majesty's orders, which constitute them perpetual visitors of the Royal Observatory, and entitle them to the copies they now demand. Your humble servant,

EDM. HALLEY, R.S. Sec.

By order of the Society.

[Copied from the *original* in MSS, vol. 35, page 145.]

No. 216.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, August 25, 1716.

KIND SIR,

When I looked upon the date of your last letter, this morning, I found, to my great surprise, I had owed you an answer now this ten weeks. I must beg your pardon for it. I have now completed the 70th year of my age: a little business now lies heavier upon me, than a great deal did formerly. I have the press at work: they have dispatched me about eight or nine sheets. My parsonage-house in the country is, part of it, plucked down and rebuilding; and I have had a small but beneficial distemper, which, I thank God, is gone off pretty easily, and carried off my small gout pains with it. But I find my strength impairs daily, so that I can now walk but once a day to church on Sundays. My memory and reason continue still; so as that I have but little cause to complain, I bless God for it: for this is the greatest comfort of life, and I doubt not of its continuance to my death. I praise God for this enjoyment, and pray for it daily.

I shall send you my printed sheets, when I have got them all from the press, and the figures engraved for them.

Mr. Pound observed the fourth satellite, on the 17th instant: he promised, when I was with him on Wednesday last, that he would send me his observation. What he told me, as near as I remember, was this, that he observed with a 15-foot glass, and that the satellite was not eclipsed wholly, but deeply immersed in the penumbra: for, at $12^h\ 45'$, it shone pretty clear; at $12^h\ 55'$, it was very small, as it had been for some minutes before, and continued for some minutes after; so that he could but just see it: but at $13^h\ 15'$ it shone out very clear. I shall crave leave to correct these times, if I find his letter differs from them.

Newton has put my 175 sheets into Halley's keeping: this is the height of trick, ingratitude, and baseness. But I never expected any better from him, since he gave my catalogue into Halley's hands. I can bear it: God forgive all his falseness.

Yesterday Mr. Booth was here to see me: he gives you his service. I have not time to add more, but that I am, ever, Sir, your sincere friend and servant,

JOHN FLAMSTEED, M.R.

P.S. Mr. Pound is Rector of Wanstead, in Essex, seven or eight miles hence, N. by E.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 217.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Sept. 29, 1716.

I am busy now writing a preface to my catalogue; I have filled 48 pages already, yet have not dispatched above the half of what I have to say on that subject; but I have the whole in my view, and matter ready by me to complete it; but I write it in English, and must have it turned into Latin, which will require some time. Dr. Halley could dispatch all his scandal in eight pages: he is a worthless fellow, and I shall dispatch all my answer to it in less than so many lines. I hope you have compared my catalogue with the thief's transcript, and are sensible of the difference of them.

I have been talking with a man, last night and this morning, about printing the tables I last received; and we think it will be best to print them on the same paper that the Catalogue and Observations are printed; and that the title page shall own you for its author. But, because we print but 60 lines in a page, and the breadth would be too little, we think that the tenths of seconds may be omitted, both in the arcs and their differences; which being done, the page will be handsomely filled, and fit to bind up with the catalogue.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 218.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Nov. [1716.]

On Thursday last my man delivered to Mr. Oldswort, the carrier, a packet directed to Mr. Benjamin Bartlet: it contains the printed places of the planets, deduced by you and Mr. Witty, from my observations made with the sextant, in 6 sheets, to be added to that volume, as you will find by the numbering of the pages; and 10 sheets more containing Mr. Crabtree's and Mr. Gascoigne's Obser-

vations, which were made at Derby. These are to follow my preface to that volume of Observations' for which I have the materials ready by me, though I have not yet begun to form them into order; the preface to my catalogue having taken up all my time hitherto. I have wrote more than 60 pages of it: yet want a many to comprehend all I have to say on the subject. I have given an account in it of Hipparchus, his armillary sphere, and their bigness; the error of Ptolemy in pretending to correct them; of his parallactic rulers; of the ancient observations of the Arabs, and their catalogues, down to Ulugh Beigh, with the obliquity of the ecliptic determined by them; of Barnard Walter, and Copernicus, the Landgrave's, Tycho's, Ricioli's, and Hevelius's, not forgetting Lansberg's by the way, nor De Mouton. I have also showed how I rectified both the sun's motions without stating the latitude of the Observatory, refractions, or parallaxes. Thus far I have gone; I have still the rectification of about 40 principal fixed stars, that were the foundation for the rest, to account for: then the method of prosthaphereses I employed in calculating their places in longitude and latitude to insert, with an account of your tables; and then, afterwards, a small tract of the parallaxes of the fixed stars, the annual variations of the right ascension and distances from the pole, and a short description of the catalogue. Dr. Halley has nothing of these, but has wrote a preface only filled with lies and false suggestions against me. God forgive him; I do. But I shall surely let the world know his falsehood and unfitness, both of him and his master, for such an employment; though it is not absolutely necessary: for their late actions in my business have so informed the world of their inabilities, and they have so far ruined their own ill-gotten reputation, that I shall need to say but little in my own vindication.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 219.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Dec. 28, 1716.

I have rebuilt three quarters of my parsonage-house at Burstow, at about £120 charge: so that 'tis now the best in the country. I have had occasion to send my man frequently thither; my age renders me feeble, and I have lately felt a new distemper, a small swimming in my head, upon any hasty bending of my body, which I hope may be curable when the spring brings on warmer weather. At present I take nothing for it, because it is not very troublesome. But you expect something from me more than complaints of these infirmities that always attend years. I praise God for the health I enjoy, and pray that yours may be continued many years.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 220.) *Letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, May 2, 1717.

When I read the date of your last letter, January 31, 1716-17, it makes me a little ashamed of myself for having so long delayed an answer to it; but I doubt not you will excuse me, when I have told you that I have set my man to copy the observations made with the mural arc (which were trusted into Sir I. Newton's hands in 175 sheets of paper, to be printed, and which he unjustly

has retained these nine years) to be copied anew from my first night-notes. He has already transcribed 100 sheets; and I reckon that, before he has done, he will fill about 140 sheets; for he writes much closer than Mr. Weston did, and fairer; so that I shall give you about 12 years' observations more than there are in those sheets Sir I. Newton got into his hands by trick and fraud, and detains by violence. I hasten to get this copy finished; after which I shall begin to print some of the first years, in order to go through with all. But my infirmities increase, and whether God will spare me life and health, he only knows: I will do what I can that they may be published in my life-time. His will is mine; for 'tis best. I am heartily glad to find your health continue. I can still, I praise God for it, walk from my door to the Blackheath gate and back, with a little resting at some benches I have caused to be set up betwixt them. But I found myself so tired with getting up the hill when I return from church, that at last I have bought a sedan, and am carried thither in state on Sunday mornings, and back: I hope I may employ it in the afternoons, though I have not hitherto, by reason of the weather is too cold for me; but you expect things of another nature from me, and I shall afford them to you.

I have caused my man to observe Saturn diligently at his last opposition to the sun: he had him both on the 20th, 21st, 30th, and 31st of March; and his observations agree very well together. I shall give you his places from those of 21, 30, and 31.

1717.	A. R.	N. P. D.	Longitude.	Lat. North.
March 21	196° 18′ 0″	93° 57′ 0″	♎ 16° 32′ 24″	2° 30′ 31″
—— 30	195 40 10	93 40 55	15 50 50	2 46 33
—— 31	195 35 40	93 39 20	15 46 11	2 46 37

Calculating the place of Saturn from my tables, which agreed tolerably well with his places observed for 30 years together, I find that on March 21, at 12^h 19′, his place by them will be ♎ 16° 48′ 35″: so that the planet is 16′ 11″ slower in the heavens than my numbers make him; the latitudes, by the tables, being only 40″ bigger than it ought to be.

Two revolutions ago, that is, 58 years before this observation, in the year 1658, the same tables erred the same way only 7 minutes; nine or ten years ago, that is, 1707 and 1708, they agreed with the heavens; and 59 or 60 years before, they also agreed with the heavens. So that the motion of Saturn seems slower in ten years now, betwixt 1707 and 1717, than it was betwixt 1649 and 1659 by ten minutes; so that my hopes that there might be a restitution of the inequality in Saturn and Jupiter, after 2 revolutions of Saturn and 5 of Jupiter, are vanished; and the doctrine of gravitation and its effects are not as yet so perfectly understood as we imagined.

I shall cause my man to observe Mars at his opposition to the sun, in the latter end of this month; how he shall be found to agree with my numbers you shall be informed, God sparing me health.

I forgot to insert in its proper place that we have an ephemeris published this year (as there was one for the last, which I have not seen) by one Mr. Kingsly, that gives the place of Saturn, March 21, 12^h in ♎ 17° 15′, that is, 43 minutes forwarder than he really was: and his latitude but 2′ 26″, full 20 minutes less than it was.

Parker's ephemeris makes the place of it then ♎ 16° 27½′, about 5 minutes too slow; and his latitude 2° 46′, agreeing well with the observation.

The Duke of Marlborough has hired the house that was the Earl of Derby's, as I remember, when you lived with me. He walks feebly; but his memory is as good as ever; and his servants tell us he never was touched in that part, whatever reports the Jacobites have spread to the contrary. He sometimes rides on horseback in the park, or heath; sometimes in his coach; likes the air of this place so well that he intends to spend a part of his summer here, and 'tis hoped will recover his strength.

We talk here of paying off, and laying up, a dozen ships, and of disbanding 14,000 men; whence you will conclude that the King is not much concerned for the Swede's fleet or forces.

Pray let me hear from you sometimes; you will thereby very much oblige me. I pray God keep you in health, and am ever, your affectionate friend and servant,

JOHN FLAMSTEED, M.R.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 221.) *Extract of a letter from Mr. A. Sharp to Mr. Flamsteed.*

Horton, May 28, 1717.

After so long an interval of silence, your kind obliging letter of the 2nd instant has dissipated my fears and revived my hopes; since, notwithstanding the increase of the decays of nature and infirmities of age, your health is, by divine goodness, continued; [as well as] capacity of acting and inspecting your own affairs, and taking so great care and pains for retrieving the damage you have sustained by the fraud and violence of a malicious envious person, who cannot be supposed to have any other design in detaining your manuscripts so long than, to his power, to hinder the publishing thereof; and thereby offering, both to you and the public, the greatest injury and injustice he is capable of doing, without the least pretext of advantage to himself. But it is with the greatest satisfaction that I understand that so much, by your renewed care, is already done towards the repairing your loss: so that there appears a great probability that this hinderance and delay, instead of proving any detriment to the public, will redound much to its advantage in the producing twelve years' observations more than were included in the former, now out of your power. I hope you will see the publication of all, and that it pleases God still to prolong your life and health in so good measure for this very purpose: that you may see the invidious designs of those, who malign and oppose you, effectually baffled and defeated. I should be heartily glad to hear that your preface or introduction to your works or catalogue is printed; therein I hope to see, though perhaps not a designed and direct, yet a satisfactory and effectual *vindication* of yourself from all the calumnies and aspersions they have cast upon you.*

[Copied from the *original* letter in the Royal Observatory, MSS, vol. 34.]

* Notwithstanding these earnest wishes of Mr. Sharp, repeated in various letters, the *vindication* was not printed with Flamsteed's works: and it is now, for the first time, made public. F. B.

No. 222.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, July 2, 1717.*

As for my affairs, I thank God for it, they go pretty well; my man has prepared 60 sheets of observations, made with the mural arc, for the press; they will take in all to the end of the year 1694, that is, the first 5 years: the following, to the end of the present year, may make about 80 sheets more. I have discoursed with a printer, and have proposed about them: this week I shall talk with another; and I hope by the next to conclude. I fear I must print it all at my own charge; and I should go on cheerfully but that a late affair, I fear, may affect me. I shall see in a little time how things will go.

In the meantime I must tell you that Sir I. Newton still keeps the 175 sheets of MS in his hands, though they can be of no use to him now. They may be of use to me to collate with my new and more ample copies, in which I have omitted a many observations that are in the 175 sheets, especially solar, that could be of little use; and some lunar, where no convenient stars could be observed that night, either before or after the moon. These I would strike out in the 175 sheets, and collate both them and my new copy with my originals at once together; and this I am the more desirous to do, because that first copy was surprised out of my hands by the Knight, before I had leisure to examine and collate it as I designed.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 223.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Oct. 4, 1717.

Two days ago my man delivered 26 printed sheets of my observations, made with the mural arc, to one of Mr. Eastman's servants, to be delivered to Mr. Stanfield, that it might be packed up with his goods and sent you. I received the first sheet from the press on the 1st of August last; so that betwixt that day and last Wednesday I have got 26 sheets printed; so that I have printed three sheets a week, one week with another: whereas when Sir Isaac Newton concerned himself with the edition of the first volume of Observations, I could get but about one sheet and a third a week, and that very carelessly done, by his encouragement of the printer. These three last weeks I have got six sheets a week: but I fear I shall not get the like dispatch for the future, by reason of short days and cold weather coming fast upon us. Sir Isaac would never suffer the press to work off more than one or two a week, and still he keeps the 175 sheets in his hands, on purpose to hinder me now, as he did then: but I trust in God 'twill be no more in his power. It puts me to about £200 charge; but that I value not much; but he thinks he has altogether hindered the engraving of the maps. I hope not; if God sends me health to finish this printing work, I shall take care for them too; and he must bear the blame of having been the cause they were not begun 12 years ago, as I proposed in my printed estimate, whereof I think you have a copy or two. This you may tell Dr. Thoresby; and you may add, if you please, that (I thank God for it) I enjoy my

* The original draught of this letter is given in Flamsteed's Letter-book (MSS. vol. 33, page 110) in the library of the Royal Observatory. F. B.

health very well for my years; and doubt not but through his blessing, I may live to convince the world that those who make the most noise at Crane Court* are worthless, idle, malicious people, and a dishonor to the meeters there.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 224.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, January 25, 1717-18.

This day, I hope, finishes five alphabets, or 115 sheets, of my observations made with the mural arc. I would have had them home a fortnight ago; but ice in the river, and ill weather, hindered me then. To-morrow it will be a fortnight since I got a small cold, which brought my old cruel distemper, the stone, upon me. I had some severe fits of sharp pain; but, taking the Spa waters, with other usual remedies, the distemper is removed: I thank God for it; and not so only, but my strength is better; I can walk about the house without my cane, find my head clearer, and I talk much easier; the phlegm, which corrupted my voice, being also removed. I hope to get all the printed sheets here by the latter end of the next week; and within a fortnight they will be on their journey towards you, God sparing me life and health.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 225.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, March 1, 1717 18.

My late illness must excuse me for not having sent you printed sheets, according to my promise. I had a second and a third attack of the stone; but, I thank God, they went off upon using proper remedies, and I am now pretty well, though not free from the apprehensions of a return. In the mean time, the press has gone on, though not so well as formerly: yet so that I have now 132 sheets complete in my hands, which contain all the observations till April, 1710; and my man reckons that seven or eight more will reach to the present time. I desire you, therefore, bear with me now for three weeks or a month longer; and I shall send you what I promised you, and an addition for interest. The planets' places, derived from the observations, will make about 12 sheets more. A table for turning the revolves of the screw into degrees, minutes, and seconds, must also be inserted, with the subsidiary tables, in which you will be concerned: but I mention these at present, only that you may see I do not forget you. As soon as we get the observations out of the press I shall consult you about them.

[Copied from the *original* letter in the possession of Mrs. Giles.]

* The initials C. C. only are inserted in the original. And I take this opportunity of remarking (what ought to have been previously noted), that the initials only of Sir Isaac Newton's and Dr. Halley's names are frequently inserted in the manuscript: but as there can be no doubt about the parties intended to be represented thereby, I have not scrupled in all cases to insert the names *at full length*. F. B.

No. 226.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, April 5, 1718.

The table for turning the revolves of the screw and cents into degrees, minutes, and seconds, is now in the press. I intend to print that for the sextant, to accompany the observations taken with it, and the places of the planets derived from the observations taken with the mural arc, with my subsidiary tables, which will conclude the second volume. Your tables of Prostaphereses, and for finding the longitudes and latitudes of the stars, or planets, from their given right ascensions and distances from the pole, will most properly come in to be added to the catalogue; the paper on which 'tis printed being some little larger than that whereon the last observations are printed: besides, the catalogue itself being but a thin volume, they will serve to render it more agreeable to the two preceding, and more acceptable to the ingenious purchaser. I would therefore desire you to write something of the use of these tables, as short as you please, with examples. I shall turn what you write into Latin, and print it with them, if the good God, who has brought my labors thus near a conclusion, bestow life and health upon me for finishing of them. I want some clear nights to get Saturn again observed in opposition to the sun, which he is now near. You shall have what the heavens favor me with in the next, God willing.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 227.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Sept. 13, 1718.

Though I have used all my endeavors to hasten the press, yet I cannot get above one sheet per week wrought off; and I have but five sheets finished by me: the last concludes the Prostaphereses. My man is gone to London to correct the sixth sheet, or half a sheet, this day: they tell me that no printer in town has figure enough to print a whole sheet. So we must drudge on till we have finished these tables slowly: we shall go on faster with what remains.

I have nothing to say to what Dr. Halley has done amongst the Hyades. When they become conveniently observable, after Christmas, Joseph has a mind to examine his performance; and then, probably, you may hear more of them.

Nor shall I say anything about the satellites of Saturn. I fear that performance is a mistake of my friend; but Mr. Derham got a prebendary of Windsor, by the pretence of using the 80-foot glass of the Society; though some people are of opinion *he never saw through it;* and perhaps Mr. Pound thinks he may make a like advantage by it. As to his performance, I have just the same opinion of it that you have; but I love peace. He has been often my guest; and therefore I am silent.

I have voided another small stone yesterday, and without much pain. I am now complete 72 years of age, and entered on my 73rd. I thank God I have my health well, for my years; and I doubt not but he will continue it, that I may finish what I have under my hands.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 228.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.**

The Observatory, June 18, 1719.

I am very glad to hear that your tables are in such forwardness. The press goes on with my catalogue, in which some errors of the edition in your hands will be corrected; some few stars added; and a catalogue of all that lie within 10 degrees of the ecliptic disposed according to the longitude, for the sake of the moon and planets' appulses to them. So that Dr. Halley will have no need to excerp small catalogues from the great one.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 229.) *Extract of a letter from Mr. Flamsteed to Mr. A. Sharp.*

The Observatory, Nov. 24, 1719.

The last sheet of the tables of latitude and declinations will go into the press to-morrow: my preface in a month's time, God sparing me life and health, though with pains. And after them, the planets' places: these fill, at least, twelve sheets: how many the like prolegomenes, or prefaces, will fill, I know not: for, I shall enlarge on some necessary things, and shorten those that are less needful. Parker's Almanac, for the next year 1720, is out, without the planets' places in it. I know not the reason; but you know the man; thence you may guess it.

Dr. Halley has showed his new tables at the Temple Coffee-house: but I am told, by one that dwells in London, they are not yet finished. Mr. Pound has furnished him with my corrections of Mars and Venus. La Hire's Mercury is more correct than Street's; but Saturn and Jupiter will find work for those that come after us. I long to see what he does with them. Long health and happiness is ever wished by, Sir, your affectionate friend and servant.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 230.) *Letter from Mr. Crosthwait to Mr. A. Sharp.*

The Observatory, Jan. 2, 1719-20.

SIR,

Knowing that a very useful and friendly correspondence has for many years been carried on between you and that great and good man, Mr. Flamsteed, I think it a duty incumbent upon me to let Mr. Sharp be timely informed of his death. He was taken ill on Sunday last, about a quarter past twelve at night, and continued to vomit up everything he took till Thursday night; when, about 38 minutes past 9, it pleased God to take him. I shall always lament the loss the public will have of so valuable a man.

Your tables are all printed except two pages, which shall be finished in a little time. If there were any papers of yours in Mr. Flamsteed's hands, that you desire may be sent you, be pleased to let me know, and it shall be done. In the mean time I am, with the greatest respect,

Sir, your most humble servant,

JOSEPH CROSTHWAIT.

P.S. Please to let me know how a funeral ring may be sent you. J. C.

[Copied from the *original* letter in the possession of Mrs. Giles.]

* The original draught of this letter is preserved at the Royal Observatory; and is bound up in MSS, vol. 37.

No. 231.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

The Observatory, Jan. 30, 1719-20.

SIR,

I return my hearty thanks for yours of the 9th instant, to which I would long before now have returned an answer, could I have given you an account in what posture Mr. Flamsteed had left his affairs; but, as that was not in my power till now, I hope you will pardon my long silence.

He has by will given Mrs. Flamsteed about £120 per annum, in Exchequer and South Sea Stock, during her life: and this, after her death, to be equally divided between John Flamsteed's children and Mr. Hodgson's. He has besides given Mrs. Flamsteed about £50 per annum, which came by her, to be solely at her disposal. This is all he has disposed of: for, as to his ready money (which may amount to about £350), books, manuscripts, and printed copies of his works, he refers to a schedule annexed to his will; but, none being found, I presume this must go between Mrs. Flamsteed and Mrs. Hodgson, who are executrixes. This is what made him so uneasy, the last day of his life; when, though he was very sensible (and so continued till the last), yet his speech failed him so very much, that he could not express what he wanted. He often called for me, and would gladly have said something to me, but was not able, though he could call for me by name; and continued so to do till the last moment. You will see, by this, that he has not left me in a capacity to serve him, notwithstanding he has often told me he would: but, this I impute to his not being sensible of his near approach, till it was too late: but, the love, honor, and esteem I have, and shall always, for his memory and everything that belongs to him, will not permit me to leave Greenwich or London, before I hope the three volumes are finished.

His preface was never any of it printed. What you mention to have seen, must be a part of what Dr. Halley wrote to his catalogue; which I think you have; for his concludes with an account of the latitude of the Observatory, and its difference of longitude from Paris. Mr. Flamsteed's preface is all wrote in English; but, as to the alterations, he meant little more, except the putting of it into a different method. For 'tis now a general preface to all the volumes, and was designed to go along with the catalogue: and he intended to have separated it, and to have put what related to every volume, with the volume it belonged to. The planets' places, derived from the observations made with the mural arc, are ready for the press; and the observations are printed to July, 1717. Mr. Flamsteed's effigies are not yet engraved: but I hope it will not be long before it will; and then you may depend upon half a dozen, with six copies of your Tables of Declinations: for I have communicated to Mrs. Flamsteed what Mr. Flamsteed, to my knowledge, had always promised you: and she told me that this, or anything else that he intended for you, shall readily be complied with. She is willing to print your quadrature, with your tables of declination; I must therefore desire that you please to inform me, whether you would have it printed in Latin or English, and what title you would have before it. The constellations are all left undone, except the map of Orion; but if proper help could be easily obtained, I doubt not but a way might be found out to finish them likewise. The maps must all be drawn again; and the stars laid down upon them from the new catalogue: but how this can be done I know not; for it will take up my whole time to attend, transcribe for, and correct the press. A further account of these affairs, and whatever progress is made therein, shall, with your leave, from time to time be communicated to you, by,

Sir, your most faithful humble servant,

JOSEPH CROSTHWAIT.

I hope, as often as you think I can serve you in this or any other affair, you will please to favor me with your commands, without being solicitous about postage; for that shall always be defrayed with the greatest alacrity; and do assure you, if mine are not troublesome to you, yours never will be to J. Crosthwait.

P.S. Dr. Halley succeeds Mr. Flamsteed here; and Mr. Pound at Burstow. 'Tis surprising to me that a person, so notoriously disaffected to the present Government, should find encouragement at this time.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 232.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

London, March 15, 1719-20.

Yours of the 9th ultimo received; and hope you will pardon my long silence, which has been occasioned by the trouble we have met with from Dr. Halley, who has been perpetually calling upon Mrs. Flamsteed to remove. He gave us but a few days' time to do it in; which occasioned such confusion amongst Mr. Flamsteed's papers, that I could not find a perfect copy of the last impression of his catalogue, which, I presume, you have not yet seen. I left the Observatory the 7th instant; and he at the same time took possession. Since the receipt of yours, we have removed all the instruments mentioned in your letter; but the Office of Ordnance demand the sextant and the two clocks, besides several books; and insist that Sir Jonas gave them to the *house*, and not to the *person:* so the executrixes must either give them up, or go to law with the Crown. Mrs. Flamsteed gives you her hearty thanks for the account you have so generously, of your own accord, given about the instruments. What you mention about the sextant is what Mr. Flamsteed has left under his hand in writing: but this, I am afraid, will not be sufficient, considering they have the Crown to dispute with.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 233) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, May 6, 1720.

Your tables of right ascension and declination have been of great use in rectifying the variations; for the person who transcribed Mr. Flamsteed's tables of variations had committed a mistake, which runs through the whole catalogue. Most of the stars, in the constellations near the poles of the ecliptic and equator, were calculated by Mr. Flamsteed, and which I am apt to believe Dr. Halley had, along with the catalogue, formerly put into the hands of Sir Isaac Newton. What you mention to have done, in some of the circumpolar stars, in order to perpetuate their rectification, would be a vast labor to perform in so great a number; and is what I despair of ever seeing brought to perfection, except undertaken by yourself. If you please to compare the last impression of the catalogue with the first, you will find in the zodiac, I believe, about 60 stars' places that have been re-calculated; besides some added, and some left out, where we found the same stars repeated in two different constellations. I should be glad if you would please to let me know,

I hope, as often as you think I can serve you in this or any other affair, you will please to favor me with your commands, without being solicitous about postage; for that shall always be defrayed with the greatest alacrity; and do assure you, if mine are not troublesome to you, yours never will be to J. Crosthwait.

P.S. Dr. Halley succeeds Mr. Flamsteed here; and Mr. Pound at Burstow. 'Tis surprising to me that a person, so notoriously disaffected to the present Government, should find encouragement at this time.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 232.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

London, March 15, 1719-20.

Yours of the 9th ultimo received; and hope you will pardon my long silence, which has been occasioned by the trouble we have met with from Dr. Halley, who has been perpetually calling upon Mrs. Flamsteed to remove. He gave us but a few days' time to do it in; which occasioned such confusion amongst Mr. Flamsteed's papers, that I could not find a perfect copy of the last impression of his catalogue, which, I presume, you have not yet seen. I left the Observatory the 7th instant; and he at the same time took possession. Since the receipt of yours, we have removed all the instruments mentioned in your letter; but the Office of Ordnance demand the sextant and the two clocks, besides several books; and insist that Sir Jonas gave them to the *house*, and not to the *person:* so the executrixes must either give them up, or go to law with the Crown. Mrs. Flamsteed gives you her hearty thanks for the account you have so generously, of your own accord, given about the instruments. What you mention about the sextant is what Mr. Flamsteed has left under his hand in writing: but this, I am afraid, will not be sufficient, considering they have the Crown to dispute with.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 233.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, May 6, 1720.

Your tables of right ascension and declination have been of great use in rectifying the variations; for the person who transcribed Mr. Flamsteed's tables of variations had committed a mistake, which runs through the whole catalogue. Most of the stars, in the constellations near the poles of the ecliptic and equator, were calculated by Mr. Flamsteed, and which I am apt to believe Dr. Halley had, along with the catalogue, formerly put into the hands of Sir Isaac Newton. What you mention to have done, in some of the circumpolar stars, in order to perpetuate their rectification, would be a vast labor to perform in so great a number; and is what I despair of ever seeing brought to perfection, except undertaken by yourself. If you please to compare the last impression of the catalogue with the first, you will find in the zodiac, I believe, about 60 stars' places that have been re-calculated; besides some added, and some left out, where we found the same stars repeated in two different constellations. I should be glad if you would please to let me know,

before I send your tables of declination, whether you have ever had what Mr. Flamsteed entitles *Appendix Tabularum*, &c.

Being in London on Friday last, I met with Mr. Pound, who invited me to Wanstead with him, to make an observation with his long glass; but the only inducement that made me accept of his offer, was the hopes of seeing the five satellites of Saturn, or at least some of them. But when I came there, he told me the night was too light to see any of them: however, he showed me Jupiter, which I could perceive very distinctly; so that I believe the glass is good: but then the motion of the air, the shaking of the pole, &c., renders it very difficult to trace the object, and makes me conclude that not many good observations can be made with a glass of 123 feet long in the open air. However, it has, in some measure, answered his designs, it having been the only means by which he and his kinsman have obtained two good livings. He also showed me Dr. Halley's lunar tables (not yet published); but I cannot find they will give the moon's place so near the observed as Mr. Flamsteed's. He has left out two or three small equations, as Mr. Flamsteed has done, and altered the precepts a little for the easier (as he says) obtaining her place. But this is purely to evade an Act of Parliament, because Mr. Flamsteed's are entered in the Hall Book of the Company of Stationers. Dr. Halley has not yet got into the Observatory. Sir I. Newton is going to print another edition of his *Principia*, being displeased with the last; and pretends to make several alterations and emendations.

Mrs. Flamsteed is resolved to have all the maps of the constellations of the zodiac engraved and drawn according to Mr. Flamsteed's design; but these ought to be ready against the time the printing is finished. But it will be impossible for me to draw the charts, and lay down the stars, besides transcribing the preface, attending the press, &c., without some attention; and we can depend upon but very little from Mr. Hodgson

Mrs. Flamsteed would be very glad, and much obliged, if you would undertake this part of the work, and will allow you any recompense that you please to demand for your trouble.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 234.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, May 18, 1720.

Sir,

For yours of the 10th instant I esteem myself exceedingly obliged, and return you my most hearty thanks. I believe you conjecture right about Dr. Halley; for some of his greatest admirers (that I have met with) cannot help saying that he will make a sinecure of it. I think what I heard him give for a reason some years ago, viz. that no person ought to enjoy that place after he was 60 years of age, may now very justly be returned upon him, who is almost 64 complete.

Mr. Pound uses no tube to his long glass; the object glass is fixed in a tin tube of about 18 inches long, upon one side of which, near the end, there is a piece of wire fixed, and to it a small cord is fastened, which (of 123 feet long) reaches to another tin tube of about 12 inches long, which contains the eye-glass, with a wire upon it as the other. I should have informed you that there is a moulding fitted to one side of the pole; and the tin tube, which contains the object-glass, is fitted to a piece of wood which has another moulding agreeable to this. But the object glass is so fitted that it turns round to the object by holding the eye-glass in your hand, and stretching the above-men-

tioned cord very tight. But I observe the weather lengthens or shortens the cord considerably; and he himself could not help owning that this alone rendered the finding of an object very difficult. But 'tis still more difficult to keep the object in the glass, when you have found it: for, by moving to and fro upon uneven ground, you frequently lose the object, being obliged to move and direct the eye-glass with one hand, and a rest to lean upon with the other. The object-glass is drawn up and down the pole with a pulley.

Mr. Flamsteed was entirely of your opinion with respect to the satellites of Saturn; and for these two or three years past had no great value for Mr. Pound. For he frequently said, what he pretended to do with the long glass was to amuse the world in order to get preferment by it, as Mr. Derham had done; who I believe never saw an object through it, because he has given it under his hand to the Royal Society that it is 126 feet long. I am heartily concerned for the weakness of your eyes, which is an irreparable loss to Mr. Flamsteed's designs, in drawing the lines and divisions upon the maps; for if your sight will not permit you to undertake this, I shall despair of ever seeing the stars' places correctly laid down. Mr. Ab. Ryley, from whom we should have had much assistance, died about six days before Mr. Flamsteed; so that there is not one person left in this town capable of reading a proof sheet of Mr. Flamsteed's works. I have been constrained, ever since, to make shift with a school-boy of eleven years old, which makes the work very uneasy to me; and had it not been for the love and honor I bear to Mr. Flamsteed's memory (knowing how many potent enemies he has left behind, and how few friends capable of serving him in these affairs), I had before this time left Greenwich, and should have had a due regard to my own future support; but this I have refused upon his account. What success I may meet with after these affairs are finished, I know not; but if nothing happens, I shall then retire into the North and there end my days. After Mr. Flamsteed's death, the storekeeper of his Majesty's office of Ordnance Stores at Portsmouth (the only friend I have left in the world), offered me a clerk's place with him. If you will undertake to draw the lines and divisions, and set off the stars, the effigies can be drawn here by some good hand afterwards, versed in drawing animal and human shapes; and if you please, the instruments you made Mr. Flamsteed for drawing the lines, with one of the maps and the scale for setting off the stars, shall be sent you next week by the Bradford carrier, directed to Mr. Benjamin Bartlett, by, Sir, your most obliged humble servant,

Joseph Crosthwait.

Mrs. Flamsteed is very willing that hemispheres should be drawn of all the constellations as you propose; and leaves it to you to rectify the right ascensions and declinations to what year you please. But she would not have them published till the books are ready. J. C.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 235.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, June 4, 1720.

Sir,

Yours of the 20th of May brought the most acceptable news of your kind offer to lay down the stars, and draw the lines and divisions of all the maps of the constellations of the zodiac. When the world shall know that these were done by the hands of Mr. Sharp, it will make Mr. Flamsteed's

works more valuable, as well as more useful. I sent forward, the 3rd, by Mr. F. Poull (for so he spells his name), the carrier, a box directed for Mr. Bartlett, as usual, in which you will find 18 sheets of large imperial paper, together with the instruments, scales, &c., which Mr. Flamsteed made use of when he laid off the stars, drew the lines, &c., though perhaps these may not be of much use to you; if not, they may be easily sent back with the maps.

I have likewise sent you three maps of the constellations of Virgo, Sagittarius, and Aquarius, which were drawn according to Mr. Flamsteed's first design; as also the same three maps, drawn as he intended to have had them published, which is after his last design. By which you will easily perceive that he has taken in more than he has in the other three; and that with a view purely to embellish and set off this part of the work (could he have had due encouragement) to the greater advantage. But notwithstanding he met with none, but from his own private fortune, yet I am sure, had it pleased God to have spared his life, he would have found means to have drawn not only the zodiac, but all the other constellations, after the manner of the last three afore-mentioned. To draw them so large will advance the price of the engraving each plate to ten pounds, besides printing and paper. However, Mrs. Flamsteed is resolved, cost what it will, to have them all engraved according to Mr. Flamsteed's last design; she therefore requests the favor that you will please to draw them after the three last-mentioned maps. These three are all he had done of the zodiac of this size; but if the other nine, of the same size with the three first mentioned, will be of any use, they shall be sent you upon the first notice. You will find, pinned to the inside of the uppermost map, a small piece of paper, upon which are engraved the magnitudes of the stars according to Mr. Flamsteed's order. I have sent six sets of each sheet of your tables of declination; the printed sheets at present are in so much confusion upon our removing hither, that I cannot come at those sheets which have not yet been sent you; but as soon as ever I have time to put them in proper order, I shall take care to send you the *Appendix Tabularum*, &c. All the sheets hereafter shall be sent you as soon as printed.

I have not yet got all the preface transcribed, for 'tis very long; but as soon as I have, a skilful person will be employed to translate it; and whilst this is doing, I shall set about transcribing and printing the catalogues of Ptolemy, Tycho, Hevelius, &c., in a proper order, according to Mr. Flamsteed's design. I am, with hearty prayers that your invaluable life may be long continued, Sir, your most obliged humble servant,

Joseph Crosthwait.

Please, as soon as you have finished two or three of the constellations, to send them; and that no time may be lost, a proper person shall immediately be employed (as for my part I never had any genius that way) to draw the images.

Mrs. Flamsteed would have paid the carriage for the box, but was afraid the same care would not be taken if she did. She will take care, however, by some way or other, to defray all the expense you shall at any time be at upon her account. J. C.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 236.) *Extracts of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, July 16, 1720.

At my return from Holland I received yours of the 14th of last month, and am sorry to find thereby that the things I sent are like to be of so little use; but you are at full liberty to alter and correct anything that has been sent. Who made the scale I know not; none of the maps were drawn since I lived with Mr. Flamsteed: but by one I found the other day, drawn upon pasteboard, it seems to have been done by Mr. Hodgson. The two maps were torn before they came from Greenwich, and I should have pasted them to have preserved the figures, had I not thought that the stars' places were not laid down according to the printed catalogue you now have; and many faults have been discovered since the time that they were drawn. Besides, I am afraid that due care has not been taken in drawing the lines and divisions: and if the catalogue had been the same, I should not have depended upon the persons that drew the maps and laid down the stars: they were always too negligent to be trusted in doings of this kind. Of this Mr. Flamsteed was sufficiently convinced; for which reason, had he lived, he would have had them all drawn anew.

* * * * * * * * *

Dr. Halley has borrowed a quadrant of the Royal Society; but 'tis so ill made that he cannot use it. He would now quit his claim to the two clocks, and everything else, if Mrs. Flamsteed would part from the sextant. She hopes, if he, or any other person should write to you for an account of what you know of the instruments, as she has some reason to believe they will, that you be pleased not to satisfy them.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 237.) *Letter from Mrs. Flamsteed to Mr. A. Sharp.*

Greenwich, Aug. 15, 1720.

SIR,

Had I not been under the greatest grief possible, for the death of Mr. Flamsteed, which made me incapable of almost every thing, I had sooner acknowledged the great obligation I am under to you, for undertaking so laborious a work as drawing the lines, divisions, and laying down the stars.

If your friendship to poor Mr. Flamsteed during his life, and regard to his memory now dead, had not prevailed with you to undertake so troublesome a work, I know not what I should have done as to that part of it; for which no other person's judgment, nor no hand, was so qualified: because you exceed all others, as much in fidelity as you do in accuracy.

I must add, that as this is the greatest instance possible of your friendship to Mr. Flamsteed, I am sorry to say I can only give you my most hearty thanks for it: for, it is as much above my making a proper return, as it is beyond what I could have hoped for. I design to put those six constellations you sent, into the hands of a person to draw as soon as possible.

The preface is sent to a learned gentleman to translate into Latin: the rest Mr. Crosthwait goes on with, as fast as he can; and I believe will keep pace with the printer, and engravers, and others concerned.

I am, Sir, with the greatest gratitude, your most humble servant,

MARGARET FLAMSTEED.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 238.) *Extracts of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich Aug. 20, 1720.

Yours of the 2nd instant received, and am really surprised to find thereby that you have been able (notwithstanding your advanced age) to perform so laborious a work in so short a time. 'Tis what could not have been expected from any other hand. I am heartily sorry to find there are so many repetitions of the same stars in different constellations; and, what is still worse, that they commonly differ so much as one minute. Had they agreed, it would have added to the reputation of Mr. Flamsteed's works, and been a good proof of the accuracy of his observations and calculations. How this could happen, I know not, but am sure the fault was not in the instrument; for though it was a little out of the plane of the meridian, when the constellations of the zodiac were observed, yet if due care were taken in counting the clock, and numbering the distances from the vertex upon the limb, and these corrected by making proper allowances for the errors occasioned thereby, and the sinking of the wall, all things would agree as well as when the instrument was first made use of. This I have often experienced by many observations of the same stars; therefore these differences must be owing to the negligence of those Mr. Flamsteed intrusted to make observations, when he could not attend himself. I must entreat the favor that you will please to give me an account of what repetitions you have met with, that the stars may be yet expunged (if possible) out of one of the constellations: though I think when any stars appear in the zodiac and in other constellations, *those in the former may be best relied upon.* It will be much better to have fewer stars in the catalogue than to put in more than can be found in the heavens. To do this, as you justly observe, would give Dr. Halley sufficient ground for detraction: for, to find faults in his predecessor's works is what he was chiefly recommended to the Observatory for; and this part he will perform with the greatest pleasure and satisfaction.

* * * * * * * * *

Dr. Halley has not yet got any instruments, besides the quadrant I formerly mentioned; and I am now in more hopes than ever that he will not be able to get the sextant from Mrs. Flamsteed: for, notwithstanding she has long been threatened with a law-suit, there is not any as yet commenced against her, neither do I think there ever will.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 239.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, October 8, 1720.

SIR,

Yours of the 30th of August, and 20th of September, are both come safe to hand, and also the six last maps; for which, and the former, I return you my most hearty thanks, and do assure you that Mrs. Flamsteed will, in a little time, endeavor to make you some amends for the great care and pains you have taken in laying down the stars, &c.; but 'tis such an instance of your esteem and friendship for Mr. Flamsteed, that it will be impossible to make an adequate return. I am very much obliged for the table, you sent, of the stars repeated in the catalogue; and shall, in order to rectify this mistake, reprint the pages in which they are found: for I know of no other way to do it effectually. This will occasion some expense; but Mrs. Flamsteed is a woman of so

good a spirit, and has Mr. Flamsteed's reputation so much at heart, that she grudges no expense upon that account; and I am sure there is nothing in my power shall be wanting to preserve the same, and to disappoint Dr. Halley from making those little advantages to himself which I know he proposes. What you mention, in your last, about Halley's catalogue, is very true ;* but he now lays the blame upon Hevelius's catalogue, and says it was owing to the errors of his stars' places that his catalogue was faulty; and that he intends, ere long, to correct it by Mr. Flamsteed's.

I am now preparing for the press the catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, the Landgrave of Hesse, Hevelius, and a small one made by the French. I hope to set the printer about these very speedily; and, whilst these are printing, shall proceed with the designers and engravers with all possible expedition. Senex is so much a tool of Dr. Halley's, and affronted Mr. Flamsteed so much in his life-time, by engraving the *Zodiacus Stellatus*, and putting his own name to it, in order to screen Dr. Halley from the law, that I am afraid he is not to be trusted. Besides, he is reputed the very worst engraver in London; and is never reckoned amongst the persons famous in that way. We are much perplexed about a person fit to translate the preface; having employed several, and can find none that do it as it ought to be, for want of understanding the science; so that but very little progress is made in it, being obliged to let it pass through two different hands, to correct and amend the faults committed by the translator. I have lately been looking over Mr. Flamsteed's papers, to see if I could find whether the variations of the northern constellations (near the poles of the ecliptic and equator) were calculated; or whether they were made from his tables of variations, which I am afraid they were (though he told me they had been done by calculation), because I cannot find them in any of his books of calculations. All the rest I rectified by your tables; but where the differences are very great, they ought to have been calculated. The Office of Ordnance, I believe, rather than go to law, are willing to quit all claim to the clocks, &c., and to allow Mrs. Flamsteed something for the sextant, in order to put a stop to the noise he [Dr. Halley] makes, that he can do nothing for want of this instrument, and thereby to get rid of him; for I am confident he has no friends amongst them at this time. A further account of this, and whatever is done further in Mr. Flamsteed's affairs, shall from time to time be communicated to you, by Sir, your most obliged humble servant,

JOSEPH CROSTHWAIT.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 240.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Nov. 19, 1720.

As to drawing the images [of the constellations] I am confident they will be well done; and to this I am sure you will consent, when I tell you that the famous Sir James Thornhill has undertaken this part; purely out of gratitude for favors formerly received from Mr. Flamsteed. The preface is now translating by a gentleman who lives in London; but I have at last persuaded him to come down and be at Mrs. Flamsteed's whilst he is about it: which I think is the most effectual way to have it well done. Because here he may have such helps from Mr. Flamsteed's books as he could

* Dr. Halley's catalogue of the *southern* stars. F. B.

not otherwise be furnished with; and by this means we are sure to prevent the sheets being handed about before they are made public. What I told you in my last, concerning Dr. Halley's catalogue, I had from Mr. Pound.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 241.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Dec. 10, 1720.

Since my last, Mrs. Flamsteed has had some offer made her in favor of Mr. Flamsteed's works, but particularly about engraving all the maps (necessary) of the constellations in Mr. Flamsteed's catalogue: but as they propose this to be done in about a year's time, I am afraid it will be impossible to comply with this favor, except you will be so kind as to draw at your leisure the rest of the charts of the constellations. Without you undertake this, the thing cannot be done in the time; for I do assure you, not one person upon earth has given the least assistance to this work besides yourself; for which Mrs. Flamsteed is willing to gratify you to the utmost, in whatever way you shall choose, besides making due acknowledgments in the preface for the part you have so generously and readily undertaken, in order to complete Mr. Flamsteed's works. If you have time to proceed with the rest of the maps, I hope there will be no need to draw a particular map for every constellation; because wherever all the stars of two or three constellations (and the whole figures can be drawn) will come into the same map, there will be no occasion to draw particular maps for such.

We hear nothing further, as yet, of Dr. Halley and the Office of Ordnance; but I believe, in a little time, we shall; for I am informed the Attorney-General has been consulted by them in this affair, and has given his opinion against the office: because it does not appear, by their books, that they ever either repaired, or made, any instruments at their expense. But, however, he says he thinks it proper to prefer a bill in the Court of Exchequer, to oblige Mrs. Flamsteed to set forth what title she has to them; but he further declares in his opinion (which I have seen by means of a friend), that if Mrs. Flamsteed can prove that Mr. Flamsteed repaired the instruments, it will be a strong presumption that the property is in her.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 242.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Dec. 31, 1720.

You are at full liberty to order and draw the maps as you shall see most convenient, and to lay down no more than the principal stars of those that fall within other maps. As to what you take notice of, about drawing the maps near the pole, I submit it entirely to you; only I beg leave to inform you that Mr. Flamsteed, in giving account of the maps, and the method of doing them, seems to suppose them all to be done the same way: but that part of the preface which relates to this affair, I will transcribe and send you in my next. I think it will be better, in drawing Hydra, to enlarge the map than to contract the scale; but this, and everything else, is submitted to you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 243.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, March 25, 1721.

Sir James Thornhill is very obliging, and gives the maps all the despatch that can be expected, he having already finished most of those you sent. Four are now in the hands of the engraver, none of which are yet finished; that of *Taurus* is the nearest; though what he has done does not please me, there being several things in it that must be amended. But as I am not so good a judge as you are, I have sent you as good a proof as could be made by his press; and must intreat the favor that you will please to cast your eye upon it, and give me notice of what you find amiss; because your opinion in this matter must, and shall, determine the engraver in doing the rest. I am obliged to examine all the maps, not to see whether you have committed any mistakes, but to see whether the person, who touches in the outlines of the images with a pencil, has so drawn them that each star, which has a name in the catalogue, may fall in the image according to the description there given: and this I thought I had better do before they went to Sir James, than to give him the trouble of altering anything after he should have put his last hand to it. And, indeed, this makes him finish and shade the images much faster, when he is told, beforehand, that the outlines are true; for then all that he has to do is to give the image a free posture, and to shade it.

I think, in a former letter, I told you that the Attorney-General had given his opinion against the Board of Ordnance; but at the same time he advised a bill to be preferred against Mr. Flamsteed's executrixes, to oblige them to declare what they knew of this affair. They have since preferred a bill accordingly, to which Mrs. Flamsteed and Mr. and Mrs. Hodgson have given an answer. But, whether the Board, upon this, will drop the thing, I cannot tell: but this we shall know next term. I am entirely satisfied with the reasons you give, about drawing the maps near the pole, that it will be next to impossible to draw them according to Mr. Flamsteed's projection; nay, I find that the small maps he drew, of all the constellations 30° from it, are done by the common stereographic projection.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 244.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, April 21, 1721.

SIR,

Yours of the 4th instant received, and have since been with Sir James Thornhill, to whom I communicated so much of the contents of your letter as related to the map of *Taurus:* after which, he told me he owned the stars were not well engraved, but he would take care they should be all amended: but, as to the manner of doing the images, he said he ordered Mr. Vandergucht, jun. (who, I perceive, is a favorite of his) to etch them: and he added that one picture, well etched, was worth ten engraved. But, before we parted, he told me he would view Hevelius's; and afterwards consider of a method of doing them as well or better than anything of that kind had yet been done. I hope to see him again to-morrow, and then to agree with him whether they must be engraved, or etched.

That proof I sent you of *Taurus*, you may remember, I told you was not finished. The figures were not so much shaded as Sir James intended; and as to the Greek letters, they were purposely

omitted, that I might put them in with red ink upon the proof. I have procured the plate of the forms of the stars, from the engraver who engraved it for Mr. Flamsteed; and shall take care that all the faults you mention be rectified. I have caused Mr. Vandergucht to take out all the figures, both on the top and in the side of the map; they being, in my opinion, as ill performed as any of the rest: he not having engraved them near so well as you have writ them. This he owns, and says he would take it as a particular favor if you would be pleased to write, upon your next letter, the following numbers, 5, 10, 20, 30, &c., so far as 360 degrees, about the same size you think he ought to engrave them: he having never seen any figures so well performed in all his life.

Sir James Thornhill informs me that, as to the figure of Medusa's head, it is an error committed by all our painters. It ought to be as he has drawn it: for he drew it by the copy of an original he procured from Rome not long ago; but says, if the other is better liked, he will still alter it. I am very sorry I did not inform you before, that I had much rather the scale for the map of *Hydra* should be contracted; for I cannot think it will be worth the while to be at the expense of two plates for that one map: but I am afraid, by what you say in your last, that this will come too late to prevent it. I should be very glad that, in all the maps, you would do everything according to your own judgment: for I am very sure no person judges better than yourself. There are about three sheets of the observations to print; which, as soon as finished, shall be sent you, with the pictures you desire. I should be glad to know whether you had ever the map of Greenwich Park, the plan of the Observatory, with the different prospects of it, sent: if you have not, I will send them, as soon as some few alterations are made in the plate of the Park.

Dr. Halley, I am informed, goes for Oxford next week, and intends to tarry there about a fortnight. As to his being writing against Mr. Flamsteed, I hear nothing of it here, neither can I as yet see any grounds for his so doing; because not one of Mr. Flamsteed's volumes, as printed by himself, has been as yet put into the hands of any person whatsoever. Nay, Mr. Flamsteed was so cautious in his life-time, that he never would trust Mr. Hodgson with one: and I am sure, since his decease, not one sheet has been delivered out. What he may intend, after they are published, I know not: but I hope, with your assistance, we shall be able to prevent him. Mr. Mollyneux has a mind to purchase the mural arc, and the quadrant that used to stand in the great room.

Mrs. Flamsteed gives in her answer to the Office of Ordnance's bill on Wednesday next: after which, we shall soon see what turn that affair will take.

I am, Sir, your most obliged humble servant,

JOSEPH CROSTHWAIT.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 245.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

London, June 1, 1721.

Sir James Thornhill and Mr. Vandergucht, jun. dined with Mrs. Flamsteed about a fortnight ago; at which time he promised to see all the maps well done. He gives a very great character of Mr. Vandergucht, and says he is the best engraver of history that ever was in England. Dr. Halley makes no overtures for obtaining the mural arc; nay, he is so far from thinking of that instrument, that he has pulled down part of the meridional wall upon which it was fixed. He

has built a little boarded shed, between the study and the summer-house, and has fixed a stone in the ground, which stands about four feet high: what he intends to fix upon it I cannot yet learn; but as yet he has done nothing, neither has he anybody to assist him; and he bears such a very bad character, that I believe he may make observations by himself. He has lately made me an offer (which sufficiently shows his intention), by a friend of mine in the Office of Ordnance, which I rejected; so that I believe he is now convinced that 'tis not in his power to get me into any measures that may be prejudicial to anything that belongs to Mr. Flamsteed.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 246.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, June 24, 1721.

Mr. Vandergucht proceeds with the maps; and Mr. Vertue has promised to undertake to do some of them, that they may be the sooner finished. I must therefore request the favor that you will please to send up what maps you have by you that are already done, that Sir James may prepare them for him. We hear nothing more of Dr. Halley, or the Board of Ordnance; therefore I am in hopes the law-suit is now at an end; I am more inclined to be of this opinion, because I am informed he has lost most of his interest with the Board officers.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 247.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, July 22, 1721.

I have communicated your proposal to Mrs. Flamsteed, and she likes it very well; and desires that you will proceed accordingly. I took the same opportunity to hint to her, that it would be necessary that you should be gratified for the great expense of time and labor which you had spent in drawing the maps, &c. She told me that it was what she always intended; and should be glad to do it in such a way as might be most acceptable to you. I am very glad to find you have made so good a progress in calculating Dr. Halley's stars anew from his distances: I am really surprised he has not done it himself, after so many years; 'tis what I know he has often promised, but believe 'tis what he'll never perform.

I presume it will be necessary to print his catalogue after you have finished it. Next week I hope to get all the images traced upon the maps, with a black lead pencil, ready for Sir James Thornhill; after which he will quickly perform his part; and he has engaged the engraver to finish all by Christmas next. The preface part of it has been in several hands to translate, but not one of them have answered expectation: this is what has delayed it so long, and has given me a great deal of trouble and uneasiness. However, I hope it will now be done to satisfaction; for finding that none of the persons employed were equal to the work, I some time ago went over to Wanstead; and before I came away prevailed upon Mr. Pound to undertake it, who I take to be a person well qualified for such a work. As soon as ever the maps are put into the hands of the engraver the press shall be set to work; and then should be glad of your assistance about the catalogue. I am afraid it must be all reprinted.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 248.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Sept. 13, 1721.

Mr. Pound was here last week, and informs me that Senex is engraving of Planispheres, and intends to publish it as done from the British Catalogue. This, he says, he was about, some time before you wrote to him: I should be glad, by some means, to frustrate him in this. I think very speedily to set about reprinting the catalogue; for I find, in sketching out the images for Sir James Thornhill, which I am forced to do myself, that a great many stars must be put in other constellations, different to what they are already in the catalogue; besides several other errors which must be rectified. I should be glad to know if you have anything more to communicate about it, before I set the press to work.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 249.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Oct. 28, 1721.

Sir,

The press is now at work with the rest of Mr. Flamsteed's works, and I hope it will not any more be stopped till the whole shall be finished, which, I doubt not, may be accomplished by March next; notwithstanding there are a great many sheets to print, besides reprinting the catalogue. I therefore intreat the favor that you will be pleased, as soon as you can conveniently, to send me the rest of the repetitions, &c., that you have discovered in the catalogue; and if you know of any errors in Hevelius, I should be glad to have an account of them. Mr. Pound has been here lately; some time during which we compared all the quotations in Mr. Flamsteed's preface with the authors themselves. This, you know, was very necessary to be done, for fear of mistakes: and as most of those authors are very scarce, he could not do it effectually anywhere but here; all those books being amongst Mr. Flamsteed's. There are about 12 sheets already translated, and I think the whole may contain about 30 such, the rest of which he has promised to finish with all possible expedition. I hear nothing further of Dr. Halley or the Office of Ordnance: the solicitor belonging to the office tells me the Board will proceed no further against Mrs. Flamsteed about the instruments. I am, Sir, your most obliged humble servant,

Joseph Crosthwait.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 250.) *Extracts of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Dec. 28, 1721.

Ptolemy and Ulugh Beigh's catalogues are already printed, and the press is now upon Tycho's and Hevelius's; after which it will be proper to print Dr. Halley's, of whose stars' places I have often heard Mr. Flamsteed complain, as also of his distances; but of any particulars I do not remember. Mrs. Flamsteed has been absent at the Bishop of Chester's, which hindered me from writing sooner, being desirous first to inspect the Doctor's catalogue, to see if he had not wrote down, in the margin of it,

what faults he had discovered; which I could not be satisfied in till her return, it being locked up with the rest of the books.

* * * * * * * *

The maps are very near all finished ready for the engravers; after which more hands will be employed. There are three now at work, of which Mr. Vertue is one; he has undertaken six. More hands had been now at work had Sir James Thornhill performed his promise; but as he did not, we were obliged to get another person to draw and wash most of them. As soon as the catalogues are all printed, one or two, if you desire it, shall be sent you.

* * * * * * * *

Mr. Molineux has not purchased the instruments; and Dr. Halley has converted the sextant and quadrant houses into a pigeon-house.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 251.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Jan. 27, 1721-22.

Sir,

I am much concerned to find, by yours of the 2nd instant, that you had entertained the least suspicion of being forgotten or slighted by me, though there had been nothing more for you to do. I can assure you, with the greatest sincerity, that I shall for ever (though I am sure I shall have no share in the profits) retain a grateful remembrance of the generous and kind assistance you have given towards completing Mr. Flamsteed's works, and shall be ready at all times hereafter, so long as life endures, when in my power, to return you gratitude: and the memory of the ingenuous, disinterested Mr. Sharp will always, by me, be had in the greatest esteem, next to that of my deceased and good friend, Mr. Flamsteed.

I have lately received a letter from Mr. Pound about a difficulty he lay under in giving an account of an error in the divisions of the mural arc, of 15″, which Mr. Flamsteed has allowed for in determining the latitude of the Observatory, though he has no where assigned a reason for such allowance.

For finding the latitude of the Observatory by the observed distances of the pole-star from the vertex the latter end of 1689.*

	Greatest.			Least.		
Its distance observed from the vertex .	40°	50′	45″	36°	8′	0″
The error of the instrument . .	+ 0	1	10		1	10
The fault of the divisions . .	+ 0	0	15		0	15
Refractions	+ 0	0	41		0	36
Zenith distance correct .	40	52	51	36	10	1
	36	10	1			
Their difference . . .	4	42	50			
The half difference . . .	2	21	25			
Compt. of latitude . . .	38	31	26			
Hence the latitude of the Observatory .	51	28	34, or roundly,	51	28	30

* See the *Prolegomena* to the 3rd vol. of the *Historia Cœlestis*, pages 114 and 115. F. B.

For finding the obliquity of the Ecliptic.

1690, Dec. 12. The sun's upper limb from the vertex	75°	11′	30″	correct	75°	10′	0″
lower limb . .	74	38	45		74	37	15
					149	47	15
The sun's centre from the vertex . . .					74	53	37½
Error of the divisions				+	0	0	20
Refraction				+	0	3	0
The sun's centre from the vertex correct .					74	56	57
The latitude					51	28	30
The sun's declination . . .					23	28	27

The sun was then in ♑, 1° 39′: so that his declination was less than that of the tropic, by 0′ 37″; which, added to the declination observed, gives the obliquity of the ecliptic 23° 29′ 4″.

Again, 1691, June 11, the sun's upper limb observed from the vertex 27° 45′ 30″

lower limb . .	28	16	55
The sum . .	56	2	25
The centre of the sun	28	1	12
Error of the instrument . . .	—	2	20
Second error	+	0	15
Refraction	+	0	28
Sun's correct distance from the vertex . .	27	59	35
Equator from the vertex . . .	51	28	30
The sun's greatest declination . . .	23	28	55
Or .	23	29	0

The sun's place at this time was ♋ 0° 5′: so that his declination was not 1″ less than it was in the very solstitial colure. Though Mr. Flamsteed has not mentioned what this error was, I presume he either forgot it when he wrote the preface, or thought it so small as not worth regarding. However, to satisfy Mr. Pound in this matter, I searched among some other papers, and at last met with the following reason for this allowance, which I have sent him: whether it be right, you can best judge. "I "find (says Mr. Flamsteed) by comparing my observations of the sun's meridional distances from "the vertex, at the solstices, and the latitude thence deduced, and compared with the latitude found "by the pole star, that some [such] fault has been committed as requires the allowance of about "one quarter of a minute, to be added to all the zenith distances observed: which might happen "by the stretching of the feet, or bending of the beam compasses, when the points of 60 and 30 "degrees were laid off. And that this must be applied in all the measures taken, whether the "stars past the meridian to the north or south of our vertex: or rather 20″, when above 40° south "or north; 10″ when less. When, therefore, these observations come to be applied, either 15″ must be "deducted from the errors which are always to be subtracted from the zenith distances in the southern "part of the arc; or the zenith distances correct by the simple errors, must be augmented 15″."

The post being just going out, what I have further to add must be deferred to another opportunity. I am, Sir, your most obliged humble servant, JOSEPH CROSTHWAIT.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 252. *Letter from Mr. A. Sharp to Mr. J. Crosthwait.*

Horton, Feb. 2, 1721-22.

Sir,

I return you my thanks for your kind and generous account of my assistance, which you may be assured shall ever be *acknowledged* in anything within the compass of my ability.

But I can give you little or no satisfaction about Mr. Pound's scruple about an error Mr. Flamsteed makes allowance for in the division of the mural arc. I can assure you there was not the least mention of any such thing during my abode with Mr. Flamsteed, which was more than a year after it was finished. In which time almost all the stars, except those constellations near the north pole, were observed *satisfactorily ;* indeed whenever Mr. Flamsteed gave me the least help thereto in any of his *labors*. This I know, that all the care that could possibly be was taken in setting off the fundamental division, viz. 60°, equal to the radius. Wherein I had not only Mr. Flamsteed's advice and inspection, but assistance ; which was absolutely necessary, being a matter of very great consequence and difficulty, since the instrument was fixed against the wall in a perpendicular posture: therefore requiring the hands of two persons to apply the beam compass with an extent of near seven feet thereto. Considering which, 'tis not strange that, notwithstanding all the care that could be used, so small an error might be committed ; since a wooden beam of so great a length might easily and imperceptibly yield as much, if not more, than it amounts to, especially in the perpendicular. But you know there was not the least suspicion of any such thing entertained then, nor during my residence there, that I had the least notice of; nor did Mr. Flamsteed mention anything of it about three years after, when I returned from Portsmouth, and observed together with his *servant* at times by the mural arc most of the circumpolar stars, which you will find in his waste red book, about February or March, 1693-4.

When I was with him before, about 1690 or 1691, I remember I observed the two bright stars in the Dove, or Columba, though I do not find them amongst his printed observations. I presume Mr. Flamsteed judged they were not to be depended upon, because of the great refraction, they not appearing to the naked eye, but found out by the telescope, in the midst of the thick atmosphere, *seeming but* a degree above the horizon. Would gladly know whether you have ever met with, or seen these observations: they might be of some use to me in comparing with Dr. Halley his place of those two stars.

I have inclosed the greater part of Dr. Halley's catalogue of the southern constellations, as I have calculated them from his distances, and Mr. Flamsteed's stars' true places in right ascension, polar distance, longitude and latitude, to the year 1726 ; which is just half a degree beyond Mr. Flamsteed's catalogue. The design whereof was to render the planisphere useful at present, and to some future year ; that they may not only complete Mr. Flamsteed's work, but that they may be saleable, and *doubt* not may yield you a considerable benefit. Both the planispheres are now ready ; but, before I send, desire you to give me some further assurance by *post, or otherwise*, (since I hear now of your - - - - - - which will be equivalent) that you will not fail to get them - when engraven : and when printed send me, at least, three of every *sheet* as they are ; they will serve my occasion as well as if they were printed, else I shall have reason enough to grudge so *great* a labor, much exceeding half a dozen of the maps, which, if they are not printed, will be entirely lost unless I keep *these*.

My design in sending you the inclosed catalogue is to desire you to add the *variations* of right

ascension and polar distance thereto (which you can easily do out of the printed table which I calculated) that they may be put into the same order as Mr. Flamsteed's are. In the meantime I intend to calculate the *variation* for all the rest, namely 80, which fall so near the antarctic pole that the *variation* cannot with due exactness and certainty be had from the table. When this is done, the *fitting* of the stars of both hemispheres will be agreeable to each other, and equally useful. Your proposal, some time ago, to reduce my calculated right ascensions and polar distances of the southern stars, to longitude and latitude, encouraged me to put this latter table under you. Which, if you do, as I do not, you will undertake they will get the sooner ready for the press. I have prefixed no preface to this catalogue, referring that to your *hands*. Suppose this must be in Latin, as yours is.

Some account how your business proceeds, and how Dr. Halley manages since he resigned his office of Secretary of the Royal Society, will be acceptable to, Sir, your friend and servant,

A. SHARP.

P.S. You will find, in the inclosed, all those stars omitted which are in Mr. Flamsteed's catalogue; which it was to no purpose to insert (though Hevelius has done so), much less to take the pains to calculate them from Dr. Halley's distances, being much better done in Mr. Flamsteed's. The rest of the catalogue, which consists of about 80 stars, as soon as I have calculated the *variation*, shall be transmitted in a letter to you, by A. S.

[Decyphered by C. Babbage, Esq., from the *original* letter (in short hand) in the possession of Mrs. Giles.*]

No. 253.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, March 6, 1721-22.

SIR,

Since I received yours, I have added the variations to the catalogue you sent, but have not yet had time to repeat them; but hope I may, some time this week. The engravers proceed so slowly with the maps, that I know not when they will be finished: for we cannot get one map done in two months from each; and again, there are others whose prices are so extravagant, that 'tis impossible to comply with them. Those who do them at the most reasonable rate have no less than £10 10*s.* for their labor, and the plate costs, when cheapest, above £1 10*s.*; so that I do not see that the engraving, plate, paper, and rolling-press, besides expenses in attending them, can cost less than £20 each plate: which large expense begins, not a little, to shock Mrs. Flamsteed and Mr. Hodgson; however, I hope they will proceed till the whole shall be finished. I, some time ago, sent over one of the maps to Holland, by a particular friend, who showed it to several Dutch engravers, and they offered to do the whole in six months, and to find the copper-plates to each, at £6 10*s.* per map; which is but half what they will cost here: and, rather than they should not all be engraved, I shall offer to go over there to agree for doing the rest. As to what you propose about the planispheres, I do promise that they shall be engraved: and when printed, that what

* See some remarks on the decyphering of this letter in the subsequent *Introduction to the British Catalogue*. And I would here state that the words, printed in italics, are such as I do not consider to be rigidly and literally decyphered, although they evidently approximate to the sense of the passage; and that the words omitted are such as cannot, at present, be satisfactorily made out. F. B.

No. 252. *Letter from Mr. A. Sharp to Mr. J. Crosthwait.*

Horton, Feb. 2, 1721-22.

Sir,

I return you my thanks for your kind and generous account of my assistance, which you may be assured shall ever be *acknowledged* in anything within the compass of my ability.

But I can give you little or no satisfaction about Mr. Pound's scruple about an error Mr. Flamsteed makes allowance for in the division of the mural arc. I can assure you there was not the least mention of any such thing during my abode with Mr. Flamsteed, which was more than a year after it was finished. In which time almost all the stars, except those constellations near the north pole, were observed *satisfactorily;* indeed whenever Mr. Flamsteed gave me the least help thereto in any of his *labors*. This I know, that all the care that could possibly be was taken in setting off the fundamental division, viz. 60°, equal to the radius. Wherein I had not only Mr. Flamsteed's advice and inspection, but assistance; which was absolutely necessary, being a matter of very great consequence and difficulty, since the instrument was fixed against the wall in a perpendicular posture: therefore requiring the hands of two persons to apply the beam compass with an extent of near seven feet thereto. Considering which, 'tis not strange that, notwithstanding all the care that could be used, so small an error might be committed; since a wooden beam of so great a length might easily and imperceptibly yield as much, if not more, than it amounts to, especially in the perpendicular. But you know there was not the least suspicion of any such thing entertained then, nor during my residence there, that I had the least notice of; nor did Mr. Flamsteed mention anything of it about three years after, when I returned from Portsmouth, and observed together with his *servant* at times by the mural arc most of the circumpolar stars, which you will find in his waste red book, about February or March, 1693-4.

When I was with him before, about 1690 or 1691, I remember I observed the two bright stars in the Dove, or Columba, though I do not find them amongst his printed observations. I presume Mr. Flamsteed judged they were not to be depended upon, because of the great refraction, they not appearing to the naked eye, but found out by the telescope, in the midst of the thick atmosphere, *seeming but* a degree above the horizon. Would gladly know whether you have ever met with, or seen these observations: they might be of some use to me in comparing with Dr. Halley his place of those two stars.

I have inclosed the greater part of Dr. Halley's catalogue of the southern constellations, as I have calculated them from his distances, and Mr. Flamsteed's stars' true places in right ascension, polar distance, longitude and latitude, to the year 1726; which is just half a degree beyond Mr. Flamsteed's catalogue. The design whereof was to render the planisphere useful at present, and to some future year; that they may not only complete Mr. Flamsteed's work, but that they may be saleable, and *doubt* not may yield you a considerable benefit. Both the planispheres are now ready; but, before I send, desire you to give me some further assurance by *post, or otherwise*, (since I hear now of your - - - - - - which will be equivalent) that you will not fail to get them - when engraven: and when printed send me, at least, three of every *sheet* as they are; they will serve my occasion as well as if they were printed, else I shall have reason enough to grudge so *great* a labor, much exceeding half a dozen of the maps, which, if they are not printed, will be entirely lost unless I keep *these*.

My design in sending you the inclosed catalogue is to desire you to add the *variations* of

No. 255.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, June 26, 1722.

On the 16th of April last I sailed for Holland, (of which I gave you notice by a letter dated that day,) and met with a long and dangerous passage; not arriving at Rotterdam till the Sunday morning following. I spent all the week after there, inquiring after engravers; but could not meet with one there capable of doing the maps to satisfaction; and, having a recommendation to a merchant at Amsterdam, and being informed that there were several persons there, who were well versed in works of that nature, I left Rotterdam and reached Amsterdam the 29th. And after staying there a fortnight, and a great deal of trouble and expense, for want of knowing the Dutch language, I agreed with two engravers jointly, who have undertaken the whole, for 75 guilders each map, the plates included; which is £6 16*s.* 3*d.* English money: which is much cheaper than they could be done here; and I hope they will be better performed, if the persons answer the character given them in that place, one being esteemed an excellent map engraver, and the other as good for history. The merchant, to whom I was recommended, has offered to pay the money there, as they proceed; and to transmit the plates hither, from time to time, as they finish them. Without such a friend, it would have been very difficult to have got them from thence hither: he is a paper-merchant, and supplies the stationer in London, of whom Mr. Flamsteed bought all his paper, and of whom Mrs. Flamsteed takes all the paper she wants: so it will be easy for her and Mr. Hodgson to pay the money to the stationer. After my return from Amsterdam, I waited five weeks at Rotterdam for a passage; and at last had the good fortune to come home in the same yacht that I went over in. This has saved Mrs. Flamsteed £5 in her pocket; I having a friend on board, who is steward of her, that obtained my passage free. What reward for this, and all my fatigue and trouble (besides loss of time, which my circumstances can ill admit of), I am to meet with from Mrs. Flamsteed and Mr. Hodgson, I know not; having never made any bargain with them, nor never as yet received anything from them.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 256.) *Extracts of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, July 28, 1722.

Notwithstanding the moderate price of the Dutch engravers, Vandergucht still insists upon £30 for the maps of Taurus and Leo. I have complained of this usage to Sir James Thornhill, he being the person that set him to work. He says he thinks 10 guineas per map sufficient; I told him this should be immediately paid him to prevent any further disputes; though it was more than he deserved, having performed his part so ill. I have not seen Vandergucht since; but intend to offer him this, some time next week. There has nothing been done since I went for Holland; and I am afraid Mr. Pound has not made that progress in the preface that might have been expected after so long a time. I have been twice with him, and get no answer but " he will do it as soon as he can." This kind of trifling makes me entertain some jealousy lest Dr. Halley and Sir I. Newton (Mr. Flamsteed's old enemies) should have been tampering with him, in order to retard the work. I wish we had it out of his hands; however, he has only a copy, and not the original. I think to go again to

him next week to have his final answer, that I may be sure in what time he will finish it; for 'tis now time to let the world know what Mr. Flamsteed has left behind him, and when they will be published. I had put the press to work again this week, but I was subpœnaed down to the assizes at Rochester upon the account of a riot.

* * * * * * * *

Dr. Halley, I am informed, has given an account of a new star he has observed near the heels of Virgo, of the 5th magnitude, which, he says, was not observed by Mr. Flamsteed. I am apt to think he is mistaken; for a star of that bigness, with but 2° 54½′ south latitude, could not escape him. I rather think 'tis x, which has 2° 55′ 40″ north latitude.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 257.) *Extract of a letter from Mr. Crosthwait to Mr. A. Sharp.*

Greenwich, Sept. 6, 1722.

I am extremely obliged for the kind offer you made to draw the two maps of Taurus and Leo again; but there will now be no occasion to give you that trouble, for Mr. Vandergucht, in my absence, sent two servants with the two copper-plates to Mrs. Flamsteed's; after which I went to him, and, with great difficulty, prevailed upon him to take five-and-twenty guineas for the engraving the two plates, the copper included. I have been several times after Mr. Pound, but always receive for answer that he is in Berkshire, but where I cannot learn: therefore know not what to resolve on about the preface. I am told he will return this week; if he does, I am determined to have his final answer. I wrote to him before he went from Wansted, as did likewise Mrs. Flamsteed; but he has not as yet thought either of us worthy of an answer, neither has he acknowledged the receipt of the four last sheets of the preface that I sent him. The press now goes on apace, and I hope nothing will hinder or any more stop it till the whole is finished. Whether Mr. Pound translates the preface into Latin or not, I am very desirous that you should see it in English exactly as Mr. Flamsteed left it before it be printed; but if he should go on with the translation, it will then be impossible to part with the original; but, however, I'll try at my leisure hours if I cannot transcribe a copy of it for you. In the meantime there is one favor (after the many you have already granted) I must ask of you, which I hope, upon Mr. Flamsteed's account, you will not deny; which is, that you would be pleased to write some account of the maps and planispheres. And as the whole were drawn by you, there is no person so fit to do it; and when you are (if you please to do it) doing this, perhaps you may think it proper to say something of Dr. Halley's catalogue.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 258.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

London, Oct. 13, 1722.

Mrs. Flamsteed has received a letter from Mr. Pound, in which, instead of informing her when he should be able to finish the preface, he told her he had made but little progress in it, and could not fix any time when he should; he likewise made a demand of several valuable books, as also a

copy of all Mr. Flamsteed's printed sheets; the books he said he would return. Upon this unexpected demand, and his making several other trifling excuses, Mrs. Flamsteed and Mr. Hodgson agreed to send a special messenger to him, with a letter to desire him to return the copy of the preface, and what books he had of theirs; which request he complied with. So they are now in Mr. Hodgson's hands, who says that one of the grammar-masters of their Hospital is translating them; which, I am afraid, will still retard the publication much longer than I expected.

Dr. Halley is very angry, I am informed, that I will not show him your catalogue of the southern stars; and boasts that he served you very much in correcting your book. I only told the person that he should not see it, till published, without your consent; and that I thought he had had time enough in forty years to have done it anew himself. The maps are not yet arrived from Amsterdam; when they do, they, with those done by Vandergucht, shall be sent you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 259.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Dec. 15, 1722.

The reason that induced me to print the old catalogues, was because it was what Mr. Flamsteed always desired and intended; though I think they are of little use, except to gratify the curiosity of those who have not the catalogues of those authors.

Dr. Halley shall not see one line of the southern catalogue before 'tis published; and, indeed, I think it ought not now to go by the name of his catalogue, since you have been at the pains to calculate all anew, and to a different year: for we are beholden to him only for the observations, and to you for the catalogue. And, therefore, I think the title ought to be, "A Catalogue of some "of the Southern fixed Stars not visible in our Hemisphere, calculated from Dr. Halley's "Distances, and Mr. Flamsteed's Stars' Places, and fitted to the Year 1726, by yourself." This, or something like it, I hope you will approve of; or any other title that you please to order, shall be complied with.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 260.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Jan. 12, 1722-23.

As soon as the planets' places are all printed (which will not be very long), I intend to print your catalogue; but whether Mrs. Flamsteed and Mr. Hodgson will agree to let Mr. Flamsteed's catalogue be reprinted, I cannot yet tell; they seem unwilling because of the expense. There are none of the maps as yet arrived from Holland, though I have, for a good while, expected four: the merchant with whom I left them having wrote to his correspondent in London that the engraver had been with him, and had nearly finished them. I intend to write to him on Tuesday next about them. Senex proceeds with the hemispheres; and, I am informed, he says, after Mr. Flamsteed's works are published, he is to do another set for you. I intend to send you the rest of the preface by John Hall, or John Holdsworth, the first opportunity.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 261.) *Extracts of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, June 8, 1723.

I have long and impatiently expected the arrival of the proofs of the maps from Holland; and instead of all that I left, I have at last received one, viz. the map of Gemini, which is but very indifferently performed, though I received a letter from thence some time ago, in which I was informed they would be exceeding well performed. But finding the contrary, I have returned the proof to Amsterdam, and have noted the faults with red ink, in order to have them corrected: so that, as yet, I have not one to send you, but hope I may at the return of the King's yacht from Holland.

* * * * * * * *

The preface is now in the press, and about eight sheets of it are ready printed off. I am afraid it will not be very well translated; therefore, to excuse Mr. Flamsteed, the world must be informed in what condition he left it. In the paper, which I sent you with an account of Mr. Flamsteed's works, there is mention made of tables of the sun's place and declination; but, upon examination, I find those who calculated them have done them so carelessly, that they are not fit to go abroad with Mr. Flamsteed's works. The preface goes on but slowly, and the works cannot now be published till the King's return from Hanover; therefore I intend to reprint some sheets of the catalogue, while the press proceeds with the rest of the preface. I must intreat the favor, if you have wrote anything to be added to the preface, that you will please to send it with all convenient speed.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 262.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

London, Aug. 24, 1723.

The preface has been out of my hands ever since I sent you the copy of it. For, at that time, Mr. Hodgson undertook to get it translated by one of their grammar-masters, who proceeded so far in it, that, as I told you, eight sheets were printed off; yet, notwithstanding, upon showing it to Mr. Whiston and Dr. Jurin, they both declared that he had mistaken Mr. Flamsteed's meaning, and had besides used improper Latin; so that it is now again at a stand, and the whole must be retranslated, but by whom I cannot yet tell. Mrs. Flamsteed and Mr. Hodgson would willingly give ten shillings a sheet (printed sheets), could they find a proper person to do it. If you know of any such person near you, I should be glad, for I now despair of finding one here.

On Wednesday last arrived one of the plates from Holland, which I have long expected, otherwise you should have had more sheets sent ere now; but was unwilling to trouble you till I had one or more maps to send for your approbation.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 263.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Sept. 22, 1723.

A few days before the receipt of yours of the 30th of August, a relation of Mrs. Flamsteed's recommended a person to translate Mr. Flamsteed's preface. I picked a sheet out of the middle of

it, and sent it him to translate as a specimen, which he has performed and sent; and I have since waited on Mr. Whiston with it for his approbation. He told me it was excellent Latin, and found no fault, and has promised to revise all the rest; so I hope it will at last be well done. The gentleman's name is Anderson; he lives in St. James's, and is a presbyterian minister.

I received yesterday a proof of the map of Aquarius from Amsterdam, which I shall examine, and return if I find any faults.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 264.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Nov. 30, 1723.

I returned the proof of the map of Aquarius to Amsterdam, there being some omissions of small stars, which I put in with red ink upon it. I every day expect the return of the plate, of which, when it comes, I shall take care to send you one, with such other sheets as I can come at, that you still want. They proceed so very slow in Holland with the maps, that I know not what to do about the hemispheres, having not yet sent them over. I had some thoughts of getting them done in London, but know not how to trust Senex; and the other engravers are so extravagant in their prices, that there is no dealing with them. I think to try Mr. Bowen and Mr. Vertue; the former was bred to a map engraver, and I am in hopes he may be brought to reasonable terms.

When I send your next parcel I shall send you another catalogue or two, if you require it, to supply the place of that which you must necessarily have spoiled. Mr. Anderson designs to send part of the preface next week to the press; so I hope it will not be long before the whole may be finished. After which, Mrs. Flamsteed and Mr. Hodgson intend to publish the three volumes by themselves, and to publish the maps alone, as soon after as they can be got ready.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 265.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Feb. 10, 1723-24.

Sir,

In a little time after I received yours, I also received advice that another copper-plate was shipped at Amsterdam, and have been under very great uneasiness ever since for fear she had been cast away, she having sailed from Amsterdam the same day that the King sailed from Helvoet Sluys. But she is, after having been in very great danger, safely arrived in the river. I hope to get the plate out of her some day this week; and then shall get two or three taken off, one of which ahall be sent you, with such other sheets as I can get at, and that you still want to complete your volumes. The preface has been in the press some time, but they proceed but very slowly, having hitherto not been able to work off more than one sheet per week: what is done of it, when I send the map, shall come along with it. Dr. Halley, I am informed, has got an order (by the favor and interest of the Lord Chancellor Parker) on the Board of Ordnance for £500, to be by him laid out in such instruments as he shall think proper. I hope, after he gets it, that he will either make new instruments, or

purchase Mrs. Flamsteed's; the latter, I presume, he may like best, because he may then put some of the money in his own pocket. Dr. Halley and Mr. Pound pretend to have seen the late comet; though Mr. Pound himself told me, after it disappeared, that he could not be sure (it appeared so very small) whether it was one or not. Notwithstanding this, I am now informed that he pretends, nay, has actually delivered in its place to the Royal Society to seconds: it was seen near some small stars in the Dolphin. I had not the good fortune to see it: for, where I live, I have no convenience nor place to make use of a telescope; and as I never converse with Dr. Halley, did not care to go near him upon that or any other account. I am, Sir, your most obliged humble servant,

Joseph Crosthwait.

I hear Mount is going to reprint your tables. Yours, J. C.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 266.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, May 18, 1724.

I have lately received a proof of the map of Cetus from Amsterdam; it seems to be better done than the map of Aquarius, though I am afraid they are neither of them engraved by the same hand as the first. I have wrote to the merchant who made the bargain, to complain of their being ill done; and hope, as he is a person of a considerable interest there, to have them better done for the future. I have met with a young man in the Minories by Tower-hill, just come out of his time, that I am informed graves very well, and wants business; I have seen him, and have agreed to let him do one map, which he promises to perform as well as the first that came from Holland, and at the same price. If he does, he shall be kept constantly at work till the whole be finished; which may prove a great advantage to a young beginner, as well as be a service to Mrs. Flamsteed. There was a scheme came out of the late eclipse by Dr. Halley, and another by Mr. Whiston: the Doctor published the times of this eclipse, and the limits of the shadow, upon that which he published in 1715: but, by comparing them together he has made the time later in this by near two minutes than he did in that; but for what reason he has made this difference I have not heard, though he pretends to do both by periods.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 267.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, August 1, 1724.

Nutting, the engraver, is dead; so that I am afraid both the copper-plate and the drawing are lost; for the landlord seized and sold his goods; and who had the plate and drawing, I cannot as yet learn. Dr. Halley has lately built a new meridian wall of stone; and, as I am informed, has a new quadrant a making, of the same radius with Mr. Flamsteed's arc. I presume his building this wall of stone is in order to find fault with Mr. Flamsteed for having one of brick, which was more liable to warp; and its being built so near the precipice of the hill caused it to sink, the hill

being washed from it. Yet, for both these accidents, you know Mr. Flamsteed has made annually proper allowances. What he designs to do, I am satisfied is nothing but to cavil at, and undervalue, Mr. Flamsteed's performances; not to rectify any mistake, nor to corroborate and confirm what has been already done. I find a great deal of trouble in dispatching the preface; being obliged to go three times a week to London, either to wait upon the translator or printer, in order to expedite it as much as possible: he sometimes complaining of the printer, and they as often of him, for delaying it. We have got 15 sheets worked off, and hope we may get two or three more by the 14th: however, what is done by that time shall be sent you.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 268.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

London, Nov. 14, 1724.

Sir,

Notwithstanding I gave you notice, so long ago, that I had received advice that the plate was shipped at Amsterdam, the ship did not arrive in the river till last week; but her long stay, before she sailed, has made some amends: for instead of one plate, I have received two, and have likewise got another engraved here, at the same price, since I wrote to you. They are all three at the rolling-press, for some proofs to be taken off; when they are taken I shall send you one of each, with six sheets of imperial paper, and so much of the preface as is already printed. I shall take care to send the above-mentioned, with what else is ready, and can be come at, by the proper carrier; of which you shall have timely notice, from,

Sir, your most obliged humble servant, Joseph Crosthwait.

P.S. The map, in which *Navis* was the principal figure, is entirely lost.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 269.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, December 26, 1724.

Sir,

For three weeks last past I have attended on Mr. Child, the rolling-press man, for two proofs of the two maps that came last from Holland, and have been as often disappointed, both of them and some other sheets of the third volumes already printed; there being four or five small designs adapted to the top of each title-page. These, as well as the figures of the sextant, are still unprinted off, though he has as often promised as I have seen him to hasten them. We have likewise met with another delay from Mr. Anderson lately; who, after he had agreed for £1 1*s.* for translating and correcting every printed sheet, and after he had received £10 10*s.* of the money, he sent to let Mrs. Flamsteed know, except she would give him £2 for every sheet, he would proceed no further in it, it not answering his expectation. After a stop of three weeks, and some further promises, he now proceeds again, and promises to finish by the latter end of February next; and I hope, before the same time, to get the errata ready, and the designs rolled off, and the figures of

the sextant and mural arc. On Thursday last I sent forward (directed as usual) by Mr. Marsden, the Bradford carrier, 21 sheets of the preface, and six sheets of large imperial paper, and one dirty proof of the map done in London, of which I crave your opinion; the man being willing to proceed at £7 each; but we shall not give him another till I hear from you. I had not troubled you with this last parcel, till all had been finished that you still want, but only to assure you that all shall be sent as soon as ever completed, by, Sir, your most obliged humble servant,

JOSEPH CROSTHWAIT.

P S. Mr. Pound is dead; and I think Dr. Halley is much broke of late.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 270.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, April 17, 1725.

In my last I think I told you, that all Mr. Flamsteed's works would be entirely finished by the end of February last: but the translator of the preface, notwithstanding all his promises, has still two sheets to print. He not having performed his part in time, has been the reason that I have not sooner put you to the expense and trouble of a letter: but I can now assure you that, God willing, nothing can hinder everything from being printed by the 30th of this instant. After which every (immediately) thing that you want shall be sent you; though, in the mean time, I should take it as a favor if you would be pleased to let me have a particular account of what you have received of each volume.

Mrs. Flamsteed and Mr. Hodgson have determined not to let Mr. Flamsteed's letter to Dr. Wallis go along with the preface, though I am sure he intended it should; for what reason I know not. I doubt not but you have perused, and fully considered, what Mr. Flamsteed has therein offered, because in a letter of yours, Sept. 1719, you seem to press him to make it more public, by letting it go in the preface.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 271.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, June 19, 1725.

SIR,

Being just now very busy in sorting Mr. Flamsteed's books for the booksellers, I have only time to inform you, that we did not receive all from the printer, and rolling-press, till late on Thursday at night; and that yesterday I went to London (on purpose) and delivered, I hope, all that you want to complete your three volumes, to Jos. Holdsworth, Bradford carrier, and directed them as usual. The errata to the third volume is not yet come from Mr. Anderson: I have sent you the preface complete; so if you please to draw the map of Navis, &c., you may, if you think fit, return the sheets of the Latin preface I formerly sent. In the top of the first sheet of your cata-

logue, for "distant a polo B," it should have been "A:" but this you can amend with your pen better than I. There is another material fault in the title of one of your tables that were first printed; which, if you have not discovered, I shall send you in my next, as well as an account of the maps, and everything else; which account you may speedily expect from,

Your most obliged humble servant, Joseph Crosthwait.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 272.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, July 24, 1725.

Sir,

Yours of the 30th of June came safe to hand, and I am sorry to find thereby that you apprehend I have picked all the torn sheets, in order to patch up a fresh catalogue for you: when, on the contrary, in all the parcels I have sent, I have constantly endeavored to send the best I could find in the bundle opened for that purpose. If you please to review the catalogue formerly sent you, you will see the reason why so many half-sheets are cancelled: the following half-sheets and sheets have been re-printed; viz., sheets B, E, P, R; the sheets H and M; the half-sheets fol. 15, 43, 51, 55: so, when I send you the other catalogue, you must expect to see some more cancelled.

I hope, in the title to the Catalogue of the Southern fixed Stars, I have not disobliged Mr. Sharp; and as for Dr. Halley, I regard him not. As for the maps, I thought I had sent you proofs of six; viz. of the four done in Holland, and of the two done by Vandergucht. The other done here, no other proof has as yet been taken off, besides those I sent you: and the plates not being in my custody, I cannot command them when I please.

Mrs. Flamsteed (by the advice of Mr. Hodgson) and Mr. Hodgson have put a total stop to the engraving any more of the maps, ever since before Christmas: not being willing, for some time, to advance any more money about them: and whether ever they will be at the charge of doing them all, I cannot yet inform you. However, I have pressed the doing of the two hemispheres, as absolutely necessary to be immediately finished: nay, I went so far as to tell Mrs. Flamsteed, that had it not been on Mr. Flamsteed's and her account, you might have had them done before now, and might have raised considerable advantage by them. The northern hemisphere is now in the hands of the person that did the map here; and he has promised to exceed that, by much, in the goodness of the work. Dr. Halley has got a quadrant of eight feet radius made; and Mr. Graham, the clock-maker, is now a dividing of it in the great room.

The three volumes of Mr. Flamsteed's works are sold for eight guineas in sheets; and the allowance to booksellers is one set of books in seven. I wish they may sell: I advised the selling them at six guineas in sheets, but that was thought too low a price. I have some thoughts of going into Cumberland very speedily, to see an ancient mother, whom I have not seen in seventeen years; and if I should happen to go or return through Yorkshire, perhaps I may make bold to trouble you with a visit. If any booksellers of your acquaintance can dispose of some sets of the books, please to give notice as soon as possible (that I may acquaint Mrs. Flamsteed with it) to

Sir, your most obliged humble servant, Joseph Crosthwait.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 273.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Nov. 20, 1725.

Since my return I have been examining and comparing the single maps and the hemispheres together, and find they are not drawn agreeable the one to the other. The single maps are drawn to represent the images to us as they really appear on the concave side of the globe; and the hemispheres are done after the manner of Hevelius, &c., on the convex side. I wish they could still be done otherwise: I hope 'tis not yet too late.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 274.) *Letter from Mr. Crosthwait to Mr. A. Sharp.*

Greenwich, Jan 25, 1725-26.

SIR,

I am very sorry to find by yours that the dizziness of your head rather increases than abates: 'tis what Mr. Flamsteed, about your age, frequently complained of; and the more when he sat down to read or write any considerable time. And I doubt not but the increase of yours is owing to your hard studies, which you ought not to think of pursuing with the same application as heretofore. As for the planispheres, I should never have thought of troubling you about them any further; for I would have had them sold separate from the other maps: and my pressing this, and urging them being drawn different from the particular maps, and the stars being rectified to 1726, put Mrs. Flamsteed and Mr. Hodgson upon me to try if you would undertake to do them anew. The northern hemisphere is already engraved; a proof of which (as soon as I can get one), and two or three other maps, I shall send for your approbation: I have seen but one dirty proof yet of the hemisphere. I acquainted Mrs. Flamsteed and Mr. Hodgson with my sending you Bishop Burnet's History; and expected, knowing how much they were indebted to you, that they would have readily made you a present of it; but they never once offered it, though 'tis such a trifle. This I did to try what may be expected from some people when they have got their business done.

I am much obliged to you for the kind offer you are pleased to make of the small quadrant; 'tis what will be highly acceptable to me, and the more as it is of your own making. If you please, at your leisure, to send the map along with the quadrant, you will still more oblige your most obliged humble servant,

JOSEPH CROSTHWAIT.

P.S. The proposals for doing the maps by subscription were done in my absence. I know not of any of the maps that are to be left out: all that you drew must be engraved.

You need not give yourself any trouble about paying for Burnet's History, for I always designed to make you a present of them, if they did not; therefore I hope you will please to accept of them from J. C.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 275.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, March 12, 1725-26.

Mrs. Flamsteed being absent in London has prevented me sending you such proofs of the maps as she has by her; but she being expected home this evening without fail, I shall send forward some of them on Thursday next, directed and marked as usual, without further advice. I know little or nothing of Dr. Halley, except that he, underhand, hinders the sale of Mr. Flamsteed's books as much as he can: but this is no more than what I always expected. I am informed by one, whom he takes for his friend, that he is very angry with me, and says he never expected they would have been published: and being disappointed in this, I presume he will spend the remainder of his time in endeavoring to find faults. There is a new edition of Sir Isaac's *Principia* coming out by a gentleman of Cambridge.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 276.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, October 15, 1726.

A little while after the receipt of your last, our engraver, Mr. Mynde, took a country journey without giving us any notice of it; where he continued above a month: which gave me no small uneasiness, for I was afraid his bad circumstances had obliged him to go off. And what confirmed me the more in this opinion was, the orders he had left at his lodgings, not to let me know that he was absent; but as often as I came there, to tell me he was gone out about business. However, he is returned; and I have got proofs of three maps, viz. of Hercules, Andromeda, and Pisces. I got them of him but on Thursday last.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 277.) *Extract of a letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Sept. 23, 1727.

On Thursday last I delivered to Sam. Hagas, Bradford carrier, six books of your *Geometry Improved*, together with five maps, viz. Bootes, Cepheus, Cygnus, Aquila, and Hydra; directed to Mr. Benjamin Bartlett, as usual. The maps are no more than imperfect proof sheets, therefore you may use them as such; they being sent only to show you what is already done: for Mrs. Flamsteed has not suffered any besides to be yet taken off. But, I believe, after her return from Windsor, where she now is, which will be about the 15th of next month, the plates will all be sent to the press; and then you may depend upon having the number so often promised.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 278.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Dec. 6, 1727.

Sir,

Being wearied out by the dilatoriness of our present engraver, and believing that his finishing the southern hemisphere, with what he has besides, might prove a work of near another year, the thoughts of this put me upon trying, amongst the trade, to see if I could find a proper person that would do it at his terms. I applied myself to Mr. Vandergucht, and agreed with him something cheaper; and I am sure he is capable of performing it much better (this I did but a few days after the receipt of your last) than Mr. Mynde. For 'twas he that did the maps of Taurus and Leo, which I think are much the best done of any; but his price, at that time, was very extravagant, or else he had done them all. I had troubled you with this account before now, had I been able to write; but it has pleased God to visit me with a violent fit of sickness, which has confined me to my bed and a room for these seventeen days last past. I am now pretty well recovered, but am very low and weak, and am not yet able to get down stairs.

Last night I received a letter from Mr. Vandergucht, giving me notice that he has engraved all the lines and stars in the southern hemisphere, and that he can proceed no further till I can be with him to assist him in tracing in the images: this I shall comply with as soon as ever I am able. I hope you will excuse this trouble given by, Sir, your most obliged humble servant,

Joseph Crosthwait.

P.S. Mr. Hodgson informs me that Mr. Machin is going to publish a new theory of the moon: Mr. Whiston is now sure of the longitude, consequently of the reward, as he imagines.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 279.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Aug. 8, 1728.

Sir,

I doubt not but you have long impatiently expected the maps; but printers' promises I find are not to be depended upon: for though I have never failed attending twice a week, yet we have still two plates to take off; which I hope I shall get done next week, and then you shall soon have your number sent you. You formerly sent me an account of what you thought proper to be printed before the maps. I should be glad to know if you have anything more which you would have added to it: if you have, I should be glad to have it before your other is printed off. Dr. Halley has got an additional salary of one hundred pounds per annum, payable out of the Navy Office, yet keeps no assistant. This Mr. Molineux got done for him a little before his death. I hope this will find you in good health, as it leaves, Sir, your most obliged humble servant,

Joseph Crosthwait.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 280.) *Letter from Mr. J. Crosthwait to Mr. A. Sharp.*

Greenwich, Aug. 29, 1730.

Sir,

'Tis now a long time since I did myself the pleasure to write to Mr. Sharp: the only reason and excuse I can make is, that I have met with nothing to communicate worth either your expense or trouble. I hear of nothing new in mathematics. Dr. Halley, Mr. Machin, and Mr. Whiston, are all endeavoring to find the longitude in order to obtain £20,000. You and I have labored for Mr. Flamsteed for about ten years; and our reward, so often promised, is at last befallen us. Mrs. Flamsteed died the 29th ult., and has given Mr. Hodgson's son (a few legacies excepted) all from her own relations; and to you nor I not one farthing. For all my time spent, and all my own expenses in attending the printing and maps, I never had any allowance, besides losing two places which were offered me; one in the Ordnance Office of £80 per annum, which I refused at her request, in order to help complete Mr. Flamsteed's works. What has induced her to act so dishonestly by us at last, except it was that she had no further occasion, I cannot apprehend. Could Mr. Flamsteed have foreseen her gratitude, I am confident he would not have left it in her power; neither should you nor I; for we ought to have made a bargain with her first, but 'tis now too late. Young Hodgson informs me he has a ring at your service. If you please to accept it, let me know to whom you would have it delivered, and it shall be given to the person by, Sir, your most obliged humble servant,

Joseph Crosthwait.

[Copied from the *original* letter in the possession of Mrs. Giles.]

No. 281.) *Letter from Mrs. Flamsteed to the Vice-Chancellor of Oxford.*

[In the Bodleian Library at Oxford there is a copy of Halley's edition of the *Historia Cœlestis* in 1712, presented by Sir Robert Walpole, in which is written the following memorandum: viz. "Exemplar hoc *Historiæ Cœlestis*, quod in thesauraria Regia adservabatur, et cum paucis aliis, "vitaverat *iram et ignem Flamstedianum*, Bibliotheca Bodleiana debet honorabili admodum viro "Roberto Walpole, Scaccarii Cancellario, &c. Non. Maii MDCCXXV." And the following letter, from Mrs. Flamsteed to the Vice-Chancellor of the University, is likewise pasted in the book.]

Greenwich, March the 22nd, 1726.

Reverend Sir,

I had the honor of yours, dated Nov. the 7th, in which you were pleased to mention the favourable acceptance of three volumes of the *Historia Cœlestis*, transmitted by the late Lord Bishop of Chester. I have been since told that there remains in your public library one volume, printed in the year 1712, which passes as the genuine work of Mr. Flamsteed's. I most humbly intreat that you will please to order that single volume to be removed out of your public library, the greatest part of which is nothing more than an erroneous abridgment of Mr. Flamsteed's works; he not being concerned in the printing any more of that book than 97 sheets; the rest being done without his knowledge or consent: which 97 sheets, upon examination and comparing, will be found all that agrees with those three volumes which had the honor to be received by the University.

I must further add that if that single volume had been fit to have seen the light, Mr. Flamsteed had never been at the trouble and expense to have printed his own works, without any allowance for so chargeable an undertaking.

I beg your pardon, Reverend Sir, for giving you this trouble; and I persuade myself you will easily excuse me, when you consider that I am under an obligation not only to do justice to the memory of Mr. Flamsteed, but also to prevent the world's being imposed upon by a false impression.

I am, with great respect, Reverend Sir, your most humble servant,

MARGARET FLAMSTEED.

To the Reverend Dr. Mather, Vice-Chancellor of Oxford, at Corpus-Christi College, Oxford.

PART II.

FLAMSTEED'S *British Catalogue*, corrected and enlarged: with an *Introduction*, by the Editor, describing the mode in which it was originally constructed, and the method pursued in making the present corrections, and additions.

TO WHICH ARE ANNEXED,

Various *Notes*, at the end, explanatory of the several alterations now made in the Catalogue; together with a few useful *Tables*, amongst which are some intended to assist those who may be disposed to examine more minutely the original Observations.

Introduction to the BRITISH CATALOGUE.

THE BRITISH CATALOGUE of Flamsteed is one of the proudest productions of the Royal Observatory at Greenwich, considering the age in which it appeared: for it should be always borne in mind that he commenced his labors under a variety of new circumstances, and under great and manifold disadvantages. And if some errors are discoverable in his works, they should not be wholly imputed either to his own negligence or to that of his computers, but greatly to the various difficulties with which he had, all through life, to contend. "He walked in an "almost untrodden path, being one of the first who made use of telescopic sights "for these purposes:* and, for the first thirteen years, measured the relative dis- "tances of the stars from each other in every oblique direction, with a sextant. "This sextant, indeed, was affixed to a polar axis, by which the observations "themselves were rendered more convenient, but which called for a very tedious "and intricate trigonometrical calculation to reduce them to right ascension and "north polar distance. When afterwards, in the year 1689, he set up his seven- "feet mural arc, it was not such as we now have, but one contrived and divided "partly by himself, without any help but the strength of his own genius, to rectify "or allow for those errors of collimation, want of parallelism in his telescope to the "plane of the instrument, or of truth in the limb itself, the accuracy of which is "so necessary to a perfect observation. Neither was the effect of refraction "sufficiently ascertained in his days, much less the aberration of light, the "nutation of the earth's axis, and those little equations which are now so well "known to be requisite to the making of a right deduction. All these matters "deserve to be taken into account: and when two adjoining stars are found to "disagree, it should be considered that they may have been deduced from obser- "vations made in very different ways, totally independent of each other; so that "it is really more surprising that there should be found usually so great an "agreement with subsequent observations, than that there should be discovered "some differences." †

* His distinguished contemporary, Hevelius, refused to adopt the use of the telescope; and maintained that he could observe as accurately without the aid of such an instrument as he could with its assistance: a circumstance which does not speak much in favor either of the precision or the wants of the age.

† Wollaston's *Specimen of a General Astronomical Catalogue*: folio, 1789. Preface.

At the time when Flamsteed commenced his astronomical career, the only catalogue in general use was that of Tycho Brahé, published at the commencement of the 17th century, and containing somewhere about 1000 stars; whose positions could not have been very accurately determined, since the observations were made with the naked eye, and with instruments coarsely divided.* Without

* The publication of a *correct* and *complete* catalogue of the stars observed by Tycho would be very desirable, as I fear there is no such extant: for I am informed by Professor Rigaud, that he has recently discovered, in the Bodleian Library at Oxford, an *original* MS of Tycho Brahé's Catalogue, which is marked, apparently in Tycho's own handwriting, "ppria manu." And, on referring, at my request, to this MS catalogue for the elucidation of some obscure points, Professor Rigaud met with several discordances from the printed copies. He was also kind enough to communicate to me the following remarks relative to this manuscript.

"Tycho Brahé gave, in his *Progymnasmata*, the longitudes and latitudes of 777 stars: and Flamsteed conceived (*Proleg.* page 91) that Kepler had extended the catalogue to 1000, from Tycho's observations. But I find that the extension was made from some copy of this catalogue: at least there is not an exception of a dozen stars. Kepler, however, has introduced some alterations: and Flamsteed has made up the catalogue, which he has given us, from these two sources. As far as the *Progymnasmata* go, he has followed it without adopting Kepler's alterations; and all the additions are, as entirely, copied from the Rudolphine Tables. But, it is singular that he has not begun to introduce these additions till he comes to *Ursa Major:* so that he entirely omits 42 stars, which Tycho had added to the zodiacal constellations, and to that of *Ursa Minor*. This is the more remarkable because, in the British Catalogue, there is a column marked 'Tycho:' which thus contains references to stars that are not to be found in Flamsteed's edition of Tycho's catalogue.

"There is another omission that Flamsteed has made (in common with Kepler), which may deserve attention. Tycho, in his *Progymnasmata*, has marked a number of the stars thus (:), when they were a little exceeding the magnitude which he had assigned to them: and with (·), when they were a little less. This probably was not much attended to in Flamsteed's time; but it may assist us, at the present day, in tracing out the variation which may have taken place in the apparent magnitude of particular stars. Whatever may be thought of an allowance to be made for the imperfection of instruments, Tycho's estimate of the relative magnitudes of what he observed seems to have admitted of great precision. I do not mean that what he might have called of the 4th or 5th magnitude, would be exactly so called by modern astronomers; but if we find 4 · 4 4: affixed to stars, of which the first should now be found larger than the third, there could be little doubt but that some change has taken place in them."

It were much to be wished that some astronomers would take up this interesting branch of inquiry; for the elucidation of which this newly discovered MS of Tycho's catalogue would afford them much information. Sir W. Herschel accomplished a great deal in this matter; but an additional interest would be given to the subject by comparing his remarks with those of Tycho. It appears, from Gassendi's life of Tycho, that he had sent different MS catalogues to distinguished persons, at various times, and in different stages of improvement: and probably the Oxford MS may be one of these, for Professor Rigaud informs me that it originally belonged to a Venetian nobleman.

clocks, without telescopes, without micrometers, without any previous knowledge of the correct motions of the heavenly bodies, without any of those aids, indeed, which facilitate the observations of the modern astronomer, Tycho attempted by new and untried methods to perfect his catalogue. In order to deduce the longitude of a few principal and fundamental stars, he conceived (or revived) the happy idea of observing by *day* the position of Venus with respect to the sun, and by *night* her position with respect to such of the above-mentioned stars as might be then visible.

" Lorsque Vénus avait une hauteur assez considérable pour ne pas craindre les " refractions, il en prenait la distance au soleil avec un sextant, qui lui donnait " les minutes, et même quelques fractions. Ce sextant avait une alidade fixe, " au moyen de laquelle un observateur visait à Vénus, à travers les pinnules: " un second observateur regardait l'ombre du soleil sur une alidade mobile. " Outre la distance réciproque, on mesurait aussi les deux hauteurs, et quelque- " fois les azimuts. On ne negligait ni les déclinations aux armilles équatoriales, " ni les hauteurs méridiennes, qu'on mesurait au quart de cercle. La nuit dès " que le soleil était plongé sous l'horizon, et permettait de voir les étoiles, on se " hâtait de les comparer à Vénus. On prenait de nouveau, et toujours avec le " même soin, des distances, des hauteurs, des azimuts, des déclinations, et des " hauteurs méridiennes. On tenait compte des petits mouvemens: on avait ainsi " les ascensions droites, et les déclinaisons; et enfin les longitudes et les lati- " tudes. C'est ainsi que Tycho détermina quelques étoiles brillantes, en prit les " distances reciproques, et les rapporta toutes à la luisante du Bélier, qu'il crut, " avec raison, préférable à la petite étoile voisine, dont Copernic s'était servi." *

It was with such elements as these, and with a catalogue of stars determined in this manner,† that the physical astronomer had, at that time, to contend, in his investigations relative to the motions of the sun, the moon, and the planets: for, it is well known that, without a correct catalogue of stars, his labor is but a

* Delambre's *Hist. de l'Astron. Moderne*, vol. 1, page 177.

† The catalogue of Hevelius was not published till the year 1690; and was not to be procured in England till many years after, and then with much trouble: so that Tycho's might be considered as the only one known here at this interesting period. In a letter from Dr. Wallis to Mr. Waller, dated June 25, 1695, he says, that he hears that Hevelius's catalogue of stars has been abroad for a year or more; but has not heard of any coming to England, except one that Dr. Gregory got from Holland. And in a subsequent letter, dated August 27, in the same year, he repeats the same complaint. These facts are essential to be known, in order to acquire a true knowledge of the history and state of the science at that period.

vain and almost fruitless attempt at minute accuracy, since the whole planetary system is involved in equal, if not greater uncertainty.* But Flamsteed's observations, by a fortunate combination of circumstances, commenced a new and a brilliant era: for it happened that, at that period, the powerful mind of Newton was directed to this subject; a mutual and a friendly intercourse then existed between these two distinguished characters; and thus the first observations that could lay any claim to accuracy, were at once brought in aid of those deep researches in which our illustrious geometer was then engaged. The first edition of the *Principia* bears testimony of the assistance afforded by Flamsteed to Newton in these inquiries; although the former considers that the acknowledgment is not so ample as it ought to have been.†

The principal instrument, with which Flamsteed made his observations (at least, those from which the *British Catalogue* is deduced), was the mural arc divided by Mr. Abraham Sharp; and which is amply described in the *Prolegomena* to the third volume of the *Historia Cœlestis*. The observations themselves, in the order in which they were made, are recorded in the second volume of the same work. Flamsteed's mode of reducing his observations was very different from that pursued at the present day. Fortunately the original computation-book (MSS, vol. 23,) in which the results of the major part of his calculations are given, still exists, the whole very neatly and legibly written by Flamsteed himself; and we are thus enabled to follow him through the various steps of his process, and thus detect the existence and the source of any error. It may perhaps interest and instruct the reader if a specimen of his mode of pro-

* See numerous instances of the errors of the planetary ephemerides, quoted by Flamsteed in the preceding *History* of his own life; and in the *Appendix*.

† The second edition (1713) should indeed have contained a much more full acknowledgment of Flamsteed's assistance; since, in the interval, he had furnished Newton with a vast mass of additional information relative to various astronomical subjects, as is evident from the preceding history of his own life. But the quarrel between Newton and Flamsteed seems to have been at its height at the time that edition appeared: and it was not likely that, under such circumstances, a more favorable notice would be taken of Flamsteed; more especially after the expression of Newton's sentiments relative to a former case of the kind. See page 166. In fact, we find that, in the second edition of the *Principia*, the allusions to Flamsteed's name are less frequent than in the first; and that several passages in the first edition, acknowledging the assistance received from Flamsteed, are carefully omitted in the second. See the first edition of the *Principia*, pages 402, 421, 429, 491, 494, &c. Moreover, in the *Theoria Lunæ*, there is not a single allusion made to Flamsteed; although it is evident, from the preceding history, that Newton was indebted to him for most of the facts there stated.

ceeding were here given: I shall therefore make no apology for inserting the following extract from the computation-book above-mentioned, (MSS, vol. 23, page 43), which gives the arrangement and result of the observations of the stars in *Gemini*, made on February 12, 1696. I have numbered the columns at the top, for the sake of a convenient reference in the explanation; but the headings are such as they are designated by Flamsteed.

February 12, 1696 { Revolutio = 23^h 55^m 55^s / Reductio = 22 19 6

1	2	3	4	5	6	7	8	9	10	11
Star.	Tempora per Plan.	Transitus per Merid.	Distantia a Vertice.	Adde Ref.	Dist. Vert. correcta	Distantia a Polo	Culmin. hor. ⊙	Differ. transit.	Adde	Recta Ascensio
	h m s	h m s	° ′ ″	″	° ′ ″	° ′ ″	h m s	h m s	s	h m s
H	7 25 37	7 26 15	28 13 40	30	28 14 10	66 45 10	5 45 21	— 18 51	3	5 45 18
	31 12	31 50	28 21 15	30	28 21 45	66 52 45	50 56	14 16	2	50 54
η	36 27	37 5	28 54 55	30	28 55 25	66 26 25	56 11	8 1	1	56 10
μ	44 28	45 6	28 50 30	30	28 51 0	67 22 0	6 4 12	0	0	6 4 12
			30 32 10	30	30 32 40	69 3 40				
	49 42	50 20	30 50 5	30	30 50 35	69 21 35	9 26	+ 5 14	1	9 27
ν	50 45	51 23	31 6 20	30	31 6 50	69 37 50	10 30	6 18	1	10 31
	54 25	55 2	33 30 30	35	33 31 5	72 2 5	14 8	9 56	1	14 9
γ	59 58	8 0 34	34 51 5	35	34 51 40	73 22 40	19 40	15 28	2	19 42
	8 5 19	6 1	22 14 40	20	22 15 0	60 46 0	25 27	21 15	3	25 30
	12 8	12 44	34 56 40	40	34 57 20	73 28 20	31 50	27 38	4	31 54
	19 50	20 30	25 1 50	25	25 2 15	63 33 15	39 36	35 24	6	39 42
			25 9 40	25	25 10 5	63 41 5				
ω	23 30	24 9	26 51 50	25	26 52 15	65 23 15	43 15	39 3	7	43 22
	26 46	27 24	28 24 55	30	28 25 25	66 56 25	46 30	42 18	7	46 37
τ	31 30	32 12	20 46 20	20	20 46 40	59 17 40	51 18	47 6	8	51 26
	33 42	34 20	26 52 35	30	26 53 5	65 24 5	53 26	49 14	8	53 34
	35 38	36 14	34 50 10	40	34 50 50	73 21 50	55 20	51 8	9	55 29
λ	40 19½	40 56	34 25 5	35	34 25 40	72 56 40	7 0 2	56 50	10	7 1 12
δ	41 40	42 18	28 58 0	30	28 58 30	67 29 30	1 24	57 12	10	1 34
	43 42	44 20	30 29 10	30	30 29 40	69 0 40	3 26	59 14	10	3 36
			27 56 40	30	27 57 10	66 28 10				
ι			23 6 25	20	23 6 45	61 37 45				
	50 6	50 47	22 46 0	20	22 46 20	61 17 20	19 53	1 15 41	13	20 6
	50 34	51 15	22 58 50	20	22 59 10	61 30 10	20 21	16 9	13	20 34

The *first* column contains the name of the star observed. The *second* contains the time, as shown by the clock, at which the star passed the wire of the instrument: these values are taken from the observation-book.* The *third* is the same hour of the clock, corrected for the error of the instrument in azimuth, as described by Flamsteed in his *Prolegomena*, pages 133 and 140.† Passing over the next four columns, for the present (which relate to the declination), we come to the *eighth* column, the values in which are obtained by adding the constant $22^h\ 19^m\ 6^s$ (which he calls the *reduction* for the day‡) to the respective times contained in the third column. The *ninth* column contains the difference between the time of transit of each star, thus reduced, and his *determining star* for this day; which, in the present case, he has assumed to be μ *Geminorum*. The *tenth* column denotes the proportionate value which is to be applied to each observation, on account of the rate of the clock, which, it appears, made only $23^h\ 55^m\ 55^s$ on that day. The *last* column contains the final results, thus corrected; and are the values reserved for insertion in the British Catalogue, subject to a slight correction from further investigation, which has sometimes been applied in the case of the principal stars. This completes the reduction in right ascension.

Reverting now to the polar distances, we find that the values in the *fourth* column, are copied from the corrected zenith distances in the observation-book: these do not, in the present instance, exactly correspond with the printed copy in the *Historia Cœlestis*, inasmuch as they all exceed those values by $10''$; which arises from Flamsteed's having, at the time he made these reductions, assumed the error of his instrument to be only $5'\ 50''$, instead of $6'\ 0''$, as in the printed copy. The *fifth* column contains the amount of refraction, computed from Flamsteed's table, to the nearest five seconds. The *sixth* contains merely the sum of the values in the two preceding columns. The *seventh* contains the north polar distances, or the values in the preceding column, added to the co-latitude of the place (which, at the time he made these reductions, he

* See his *Historia Cœlestis*, vol. 2, page 291.

† See the principle on which this correction is made in page 374.

‡ This *reduction* (as well as the rate of the clock) is given for each day of observation; but Flamsteed has not stated the method of deducing it. It appears to be such a quantity as, added to the time of transit, *per meridiem*, of his determining star, shall be equal to the mean right ascension of that star on January 1, 1690. By which means he seems to have intended to obviate the necessity of any further correction for precession in the other stars. For he always assumes the same mean right ascension for his determining star, however distant in point of time the observations may have been made: that is, whether they were made in 1690, 1700, or any subsequent year.

assumed equal to 38° 31′ 0″ *) ; and are the final results reserved for insertion in the *British Catalogue*, subject to correction from further investigations, and also (which he did not consider requisite in the case of right ascensions) for the effect of precession. These results are generally assumed to the nearest 5″ only.

It is in this manner that Flamsteed has proceeded with all such observations as he intended to reduce : and the computation-book above-mentioned, contains nearly the whole of the stars which form the *British Catalogue* thus reduced, and afterwards arranged, in subsequent parts of the book, in the order of right ascension, according to the constellations. I have assumed the example of this day's work, as it affords me an opportunity of pointing out several material errors which are committed in the calculations: and also of showing how I have been enabled to detect others of a like kind. In the first place, it will be seen that the star which was observed at $8^h\ 5^m\ 19^s$ (which has been supposed to be 29 *Geminorum*) differs only $20^m\ 55^s$ from the transit of μ, instead of $21^m\ 15^s$, as stated in the ninth column; consequently the right ascension of that star is 20^s in error, and should be diminished 5′ : whereby it is recognised as 28 *Geminorum ;* and the star called 29 *Geminorum* is thus shown not to exist. Secondly, it will be seen also that the time of the transit of λ differs only $55^m\ 50^s$, from that of μ, instead of $56^m\ 50^s$, as stated in the ninth column; consequently the right ascension of that star, as here given, is erroneous 15′. Thirdly, the difference in the time of transit of the last two stars in the above list (which have been supposed to be 72 and 73 *Geminorum*) is erroneous, each of them 10^m : and consequently their right ascensions also are wrongly deduced; and, when corrected, will be found to agree with 64 and 65 *Geminorum*, and the stars called 72 and 73 *Geminorum* are thus shown not to exist.† Fourthly, it will be seen that the zenith distances of the three stars that passed at $8^h\ 12^m\ 8^s$, the next after $8^h\ 43^m\ 42^s$, and at $8^h\ 50^m\ 34^s$, are incorrectly copied out: one of them 2′, and the others 1′ in error. It would appear also that the zenith distance of the star that passed at $8^h\ 33^m\ 42^s$ has been copied out 2° wrong; but there is a

* This was the value at first assumed by Flamsteed; but he afterwards adopted 38° 31′ 30″, and corrected the final results accordingly.

† There is an error of 1^m in the difference in the time of transit of the second star in the above list, which passed at $7^h\ 31^m\ 12^s$; but this does not appear to have extended farther, and, consequently does not influence the results. There is also an error of 1^s in the time of culmination of ν, which will cause a corresponding error in the deduced right ascension; but this being so small a quantity in comparison with the other errors, I have not noticed it in the text.

mistake in the original entry. It was at first transcribed correctly, and afterwards altered with the pen by Flamsteed: it is the printed copy which is in error. (See the Note to No. 1005 of the present Catalogue.)

It may be easily seen, from these examples, that in this mode of conducting the process there may be many other sources of error besides those of the pen: for it is scarcely to be supposed that the deviation of the mural arc, in azimuth, was constant; nor, indeed, that the precise quantity of that deviation was at all times known. The table which Flamsteed used for this correction (and which is alluded to in his *Prolegomena*, page 133), is given in MSS, vol. 39, page 8, and is as follows:

Zen. dist.	Add.	Zen. dist.	Add.	Zen. dist.	Add.
6°	32ˢ	40°	33ˢ	56°	25ˢ
18	37	42	32	58	24
28	39	44	31	60	23
30	38	46	30	61	22
32	37	48	29	68	22
34	36	50	28	75	22
36	35	52	27	80	17
38	34	54	26	85	15

This table was constructed by Flamsteed in the year 1690, from observations made on Feb. 14, June 2, and Sept. 15; the zenith distances of certain stars being taken with the mural arc, and their intermutual distances from other known stars, with the sextant; and their right ascensions thus deduced, and compared with their times of transit.* It does not appear to have been verified or altered at any subsequent period; and was therefore used by him in all his reductions (see page 58): although he is not always very careful as to the precise second. In the next place, his mode of avoiding the necessity of computing the precession in right ascension, must frequently have led to erroneous results. Added to all which, it may be doubted whether by such modes of proceeding he acquired sufficient data for determining the rate of those clocks with which the Observatory was at that time furnished. Then, with respect to the polar distances, it is known that they are all mixed up with the error in the position of his instrument arising from the sinking of one end of the wall to which it was attached; the exact amount of which it would require more investigation to

* See a synopsis of the stars observed and the results, in MSS vol. 39, at the end of the book.

determine than could possibly be given to the subject by Flamsteed; neither had he the means of doing it with precision. (See his letter to Sir Isaac Newton, in the *Appendix*, No. 52, where his method is fully detailed.)

I have already remarked, and it may be easily seen, that Flamsteed's mode of daily reduction was very different from that pursued at present, where the proper corrections are deduced from the totality of observations made within the given period. On the contrary, he divided his work into as many parts as there were constellations; and reduced each part separately. Thus, for instance, taking the observations made on June 30, 1690, we find that, in his computation-book above alluded to, he has collected together all the observations of the stars in *Serpentarius*, and has deduced the right ascensions from ζ, which he has chosen for his determining star in this case; and whose right ascension is assumed equal to $16^h\ 20^m\ 8^s$. Then, in another part of his book, he collects all the observations of the stars in *Hercules*, made on the same day; whose right ascensions he deduces from that of α, assumed equal to $17^h\ 0^m\ 34^s$. And so on with the remaining constellations, *Scorpius*, *Serpens*, *Aquila*, and *Capricornus*, observed on that day; each forming a separate class of reductions. In these computations he does not appear to be very particular in the choice of his determining star: thus, in the cases above mentioned, he assumes ζ *Serpentarii* as the determining star for all the stars observed on that day in that constellation: whilst, in the stars in *Serpens*, observed on the same day, he has taken η *Serpentarii*; and, in the stars in *Aquila*, observed also on the same day, he has taken α *Serpentarii* as his determining star. But what is more singular, he has not always assumed the same value for the right ascension of his determining star, even when used on the same day: thus, in the collection of stars in the constellation *Serpens*, on the day above mentioned, he has taken, as I have already stated, ζ *Serpentarii* for his determining star, whose right ascension he here assumes equal to $16^h\ 20^m\ 10^s$; differing 2^s from the value assumed on the same day, when determining the stars in *Serpentarius* above mentioned. These are not solitary instances; and many cases of similar anomalies may be adduced to show that Flamsteed was not very particular in these minute corrections: neither, indeed, was it looked for at that period. It, therefore, is not surprising that we occasionally meet with anomalies and discordances in the catalogue, even in the same stars: an instance or two of which I shall here mention. The star, called 69 *Piscium*, is the same as 40 *Andromedæ*; and, consequently, their position in the heavens ought to be the same. But there is a difference of $30''$ in their right ascensions: which has arisen from the former having been

deduced from γ *Pegasi*, as its determining star; and the latter, from α *Andromedæ* at two different periods. Again, 6 *Libræ* is the same star as 14 *Hydræ contin.*: but there is a difference of 1′ in their right ascensions; which has arisen from the former having been deduced from β *Libræ*, as its determining star; and the latter from a *doubtful* observation compared with α *Virginis*, at two different periods.*

In another respect, also, his mode of proceeding was different from that adopted at the present day. For he does not appear to have taken the *mean* of several observations for a more correct result: since we find that, where more than one observation of a star has been reduced, he has generally assumed that result which seemed to him most satisfactory at the time, without any regard to the rest. Neither, in fact, did he reduce the whole (nor anything like the whole) of his observations: many days' work having been wholly omitted in his computation-book. And, moreover, many of the results, which have been actually computed in that book, have not been inserted in any of his MS catalogues; either from inadvertence, or from some suspicion of their accuracy. The reader will find many instances of this kind adduced in the *Notes*. So that, in fact, the *British Catalogue*, even corrected and enlarged as it now is, does not present a rigidly correct and faithful result of the observations; and can only be considered as an *index* and guide to those who may be disposed to examine more minutely the position of any particular stars, or to search into any branch of astronomy (such as the lunar or planetary motions) connected therewith. Thus, although there are 163 observations of η *Geminorum*, only 4 of those observations have been reduced by Flamsteed; and he has taken the result of the first reduction as the correct value. Again, there are 124 observations of γ *Geminorum*, yet only 2 of those have been reduced by Flamsteed; and he has here also taken the result of the first reduction as the correct value, although there is a difference of 5′ between the two. These are not singular cases, but the same method is pursued throughout the whole of his work. He seems to have been more solicitous about increasing the *number* of his stars in order to complete his catalogue, than anxious to reach those refinements in the art of *reduction* which have rendered modern observations of so much value, but which were neither known, nor even suspected, at that period. In fact, the age was not ripe for those improvements which have since been introduced; and it is necessary to recall to our recollection the state of astronomy at that period, in order to have a

* See *Mr. Crosthwait's Letter to Mr. A. Sharp*, on this subject, in the *Appendix*, No. 238, page 339.

fair estimate of Flamsteed's labors. The positions of even the principal stars were at that time not accurately known: and the best catalogues, which were those of Tycho and Hevelius, erred several *minutes* in the positions of many that were visible to the naked eye. Flamsteed, therefore, accomplished a great and desirable object in reducing those errors to *seconds* only; and in enlarging the field of inquiry by discovering new stars with the aid of the telescope, first used for such purposes about this time: and which new discoveries, being directed principally in the line of the zodiac, would form so many normal points of comparison for the planets, whose motions, at that period, were but imperfectly known. Yet, although he himself has not given us the full result of his labors, his original observations fortunately still remain; and the fruit may yet be gathered by some industrious labourer in this vineyard of science, and with much greater accuracy than could possibly have been done in Flamsteed's time. Nevertheless, if Flamsteed had not persevered in publishing at his own expense his observations, *in the order in which they were made*, the world would have been deprived of this advantage; and we should now have had to deplore the miserable counsel that dictated a different line of conduct.

Considering, therefore, that a new and a wide field has thus been opened to the future astronomer by the introduction of the telescope, it becomes peculiarly necessary that the first recorded results obtained by its means should be placed upon a firm and trustworthy basis: since those results may be appealed to, some centuries hence, for various astronomical purposes, or for the elucidation of points not hitherto dreamt of.* The catalogue of Ptolemy, after a lapse of 2000 years, is often referred to at the present day, for the purpose of obtaining a comparative view of the heavens in ancient and modern times; and it may sometimes be doubted whether the presumed discordances it contains have arisen from an actual alteration in the state of the heavens, or from errors which have unavoidably occurred in transcribing, from time to time, so many copies of the work. Tycho Brahé endeavoured to remedy this defect by undertaking a new survey of the heavens on Ptolemy's model, and placing on record the positions of all the principal stars. But, notwithstanding all the advantages arising from the invention of printing, there is already too much reason to fear

* One instance, amongst many, I will here mention. On February 27, 1715, the planet *Uranus* was observed by Flamsteed to be very near the star 80 *Leonis*; and thus an opportunity is afforded of determining the place of the planet at that time. But the right ascension of 80 *Leonis* in the *British Catalogue* is erroneous, on account of a mistake in the original entry from which Flamsteed derived its place, and which I have corrected in the note to that star, No. 1611. Thus all *doubt* about the identity and position of the star which formerly existed is now removed.

that his catalogue, even as published in his own days, is in many places incorrect; so that, in the case of any new, or missing, or variable star, we are unable to come to any precise and satisfactory conclusion. Under these circumstances, I think, there can be no question about the propriety of investigating the accuracy of that new and splendid catalogue which Flamsteed has left us (a catalogue nearly four times more numerous than those of any of his predecessors,) and of enlarging it by the addition of such stars as have been inadvertently omitted: whereby it may be appealed to, with more confidence, in after ages, and not fall into that disrepute and neglect to which, by its numerous errors, it appeared to be fast hastening, and from which it is here my object to redeem it.*

Flamsteed has given, in the *Prolegomena* to the third volume of his *Historia Cœlestis*, pages 144—147, a list of 45 stars, whose right ascensions he has deduced from direct comparisons with the sun, and three of which (β *Orionis*, α *Canis Minoris*, and β *Geminorum*) had been observed at both equinoxes: and the inference to be drawn from what he has there stated is, that the right ascensions of all those stars have exclusively served as zero points for the direct determination of the right ascensions of the other stars in the *British Catalogue*. I cannot find, however, any trace of the application of this method (in the way here supposed) in any of the MS volumes: neither does it appear that more than one-half of those stars have in any case been assumed by him as *determining stars* in his computation-book (MSS, vol. 23): whilst a much larger number, even of second-rate stars, have been employed by him for that purpose. Indeed the right ascensions, in the *British Catalogue*, of such of the first-mentioned stars as he has assumed, frequently differ from the values deduced in the manner above alluded to, and have been obtained by a totally different method. Thus, in reducing the observations made on January 4, 1690, he does not make use of α *Tauri* (one of his fundamental stars) for *direct* comparison; but in his computation-book, MSS, vol. 23, page 2, at the very commencement of his labors, he deduces the place of α *Arietis* by means of distances taken with the sextant. For, he says, "Antequam reliquarum fixarum ordinationem aggredior, "statuendæ sunt ascensiones rectæ fixarum in capite *Arietis*, e distantiis à polo "arcu murali anno 1690, et à Pallillicio [α *Tauri*] sextante, necnon à transi-"tibus observatis." And in the computation, which he has subjoined, he has assumed the position of α *Tauri*, for the epoch 1690, as AR = 64° 32′ 15″ and

* Sir Wm. Herschel remarks, in reference to this subject, that he "cannot help regretting that "a work, to which every astronomer has been taught to look up as the first authority, should have "been sent to the press with so many errors that we hardly know how far to give our confidence to "what is laid down in it." See *Phil. Trans.* for 1796.

D = + 15° 51′ 10″; which although differing only 5″ in right ascension from the value in the *British Catalogue*, yet varies therefrom 50″ in declination. From these values, and the assumed declination of *α Arietis*, (= + 21° 59′ 0″) and its observed distance from *α Tauri* (= 35° 32′ 0″) Flamsteed has determined the right ascension of *α Arietis* to be 27° 26′ 35″; differing only 5″ from the value given in the *British Catalogue*. Again, in reducing the stars observed in the constellation *Taurus*, on the same day (Jan. 4, 1690) he does not, even in this case, make use of *α Tauri* for his determining star; but computes, by a trigonometrical process, the right ascension of *o Tauri* from distances observed with the sextant, betwixt it and *γ Geminorum*; and in the computation which he has subjoined (in page 18 of the same volume) he has assumed the position of *γ Geminorum* for the epoch 1690, as AR = 94° 56′ 40″ and D = + 16° 37′ 30″; which although agreeing in right ascension with the value in the *British Catalogue*, yet varies therefrom 45″ in declination. From these values, and the assumed declination of *o Tauri* (= + 7° 55′ 0″) and its observed distance from *γ Geminorum* (= 47° 29′ 50″) he has determined the right ascension of *o Tauri* to be 47° 2′ 15″; differing 15″ from the value given in the *British Catalogue*. From the right ascension of *o Tauri* thus deduced, he determines the right ascension of all the other stars in that constellation, observed on the same day: and indeed most of the stars in the constellation *Taurus* have been deduced from the position of *o Tauri*, thus determined, without any further reference to the fundamental stars deduced from a direct comparison with the sun.

In fact, Flamsteed's method of deducing the right ascensions of his determining stars, appears to have been first to take the distances between a certain number of principal stars (generally 7) situate in favorable positions, all round the heavens, in the manner alluded to in his letter to the Bishop of Salisbury, in page 120 of the preceding history; the accuracy of which operation was ascertained by finding whether the differences of right ascension deduced therefrom made up the exact quantity of 360°; and afterwards to make use of these stars for determining the positions of others, in the manner above mentioned. We find in MSS, vol. 53, pages 163—176, several *rounds* of differences between various stars thus computed: and, in page 177, is a list of the differences of right ascension (deduced therefrom) of a great number of principal stars for the epoch 1686; which were probably used by him in forming the approximate catalogue which he made, about that period. And in MSS, vol. 62 A, we find many of these distances employed for a similar purpose in deducing the right ascensions

not only of his determining stars above mentioned, but also of other stars inserted in the *British Catalogue;* preferring these results, in some cases, to those obtained with the mural arc.

As the computation-book above mentioned is in excellent condition, and contains almost the whole of the stars in the *British Catalogue* (I believe nearly *every star* contained in Halley's edition of 1712, except in a few of the northern constellations, as hereafter mentioned), it has enabled me to correct many discordances and errors which I have discovered in the printed copy; and to trace the origin and cause of such mistakes. In the pursuit of this inquiry, I have also discovered that several days' observations, recorded in the original MS volumes, have been wholly omitted in the second volume of the *Historia Cœlestis;* and that some stars, which were supposed not to have been observed by Flamsteed, have thus been brought to light. See the *notes* to Nos. 70, 256, 293, 698, 1603, 2107, 2220, 3125, &c. of the present catalogue. Probably some others, of which we cannot at present discover any recorded observations, may accidentally turn up in the same manner, at some future time.

After the places of the stars had thus been deduced by Flamsteed, some of them appear to have been subsequently altered, probably from corrections which he thought proper afterwards to apply; but the source of which corrections I have not, in all cases, been able to discover. They never, however, amount to a quantity of any considerable magnitude: generally to a few seconds only. In some cases he has altered the right ascension of his determining star, but without applying such correction to the other stars which depend on the position of that determining star: and, consequently, some anomalies and discordances have arisen from this source. See the *notes* to Nos. 1378, 2476, &c., in the present catalogue. In other cases there appears to be an error in the determining star, which consequently affects the places of all those which depend on it. See the *notes* to Nos. 1265, 1633, 2179, 2413, &c. In many instances we trace him in the process of attempting to verify the right ascensions deduced from the *transits,* by the computed right ascensions deduced by means of *distances:* and, in one of those cases, not finding an exact agreement, we see that he has unfortunately discarded the correct result, and retained the inaccurate one. See the *note* to No. 454 in the present catalogue.

All the computations above alluded to were made by Flamsteed himself, and afterwards arranged into catalogues with his own hand. The calculations made by other persons, to which he alludes in the history of his life (see page 64, and other places), were the conversion of these results into longitudes and latitudes: a work, at that time, considered highly necessary and useful; but which

is of little importance at the present day: and Flamsteed might have spared himself much time, labor, and expense, if he had wholly neglected this portion of his work. It is, indeed, somewhat extraordinary, that he should have taken so much pains to obtain accuracy in the computation of the longitudes and latitudes of his stars; and at the same time should have neglected to revise the computations of the right ascensions and declinations. But the former were the result of a long and tedious trigonometrical calculation, and therefore might be fairly suspected to be liable to error: whilst the latter, being obtained by a very simple arithmetical process, were confided in, unfortunately, without suspicion of any important mistakes.

I have already stated that the computations of nearly the whole of the stars in the *British Catalogue* are contained in MSS, vol. 23, above mentioned; and which computations appear, from the marginal notes in the said book, to have been commenced on September 27, 1695.* The whole of the zodiacal constellations were finished before the end of July in the following year: and the major part of the remaining constellations before the end of the year 1700. Some additional computations appear to have been made after that time, to which I shall presently allude. On the publication of Halley's edition in 1712, he seems to have again examined all his observations up to that period, and made additions to his catalogue by the introduction of such stars as either had escaped him in his first examination, or had been observed subsequent thereto. These supplementary stars are for the most part contained in other MS volumes, which I shall mention in the sequel, and appear to be the most imperfectly reduced of the whole; probably owing not only to Flamsteed's vexations at that period, which rendered him less capable of minute accuracy and attention, but also to the haste with which they were made, in order to counteract, as speedily as possible, the effect of Halley's spurious edition.

But although the major part of the stars, observed by the mural arc, were reduced in so short a space of time by Flamsteed in the computation-book above mentioned, yet (as he has remarked in the history of his life, pages 65 and 96) there were several of the northern constellations which, having been at first but imperfectly observed, were deferred till a later period. These subsequent computations are contained in MSS, vol. 26, parts A and B: and they relate principally to the circumpolar stars, together with some others which were omitted in

* Flamsteed was in the habit of annexing, in the margin of his book, the precise date when the subject therein was written (sometimes even so minutely, as to mention the very *hour*), and this frequently repeated, during the progress of a long paper.

the first review of the subject. In the year 1712, he appears to have collected together, in part C of the same volume, page 32, &c., a list of *Addenda;* consisting of such other stars as, on a final review, he had omitted in his former collections, as above mentioned. The elements of reduction are not so evident in this list as in the others; and the reader cannot always be sure of the *source* of any manifest error.* The reductions of the stars in the constellation *Hercules* are contained in MSS, vol. 24, pages 140 and 141: which volume appears to have been used, at first, for the purpose of making an approximate catalogue; and is evidently the book mentioned in his letter to Sir Isaac Newton in page 130.

Besides these computations, which are all that I can discover relative to the observations made with the mural arc, there are others connected with the observations made with the sextant; undertaken for the purpose either of confirming the results of the former, or of supplying the positions of a few stars that had been observed only with the latter instrument. These embrace a troublesome trigonometrical calculation, and are consequently less numerous: they are contained in the several MS volumes mentioned in the note.† The following are some of the stars which are known to have been thus determined and retained in the *British Catalogue;* viz. 24 and 38 *Persei,* 99 and 103 *Tauri,* 76 *Geminorum,* 66 *Leonis,* 12 and 13 *Draconis,* and 3 *Cassiopeæ,* together with several of the stars in *Ursa Major* and *Ursa Minor.* But many others have been partially computed in the same manner, as already noticed.

In the observations of the zenith distances of the stars, we sometimes find a disagreement in the reading of the divisions of the mural arc, *per lineas diagonales,* and those *per strias cochleæ.* It was Flamsteed's universal practice, in such cases, to assume the former as the correct reading (see page 127); but it

* There is a duplicate of this list of *Addenda,* sewed into the same book, at the end. I would also here remark that there are duplicate computations of some of the other stars, in MSS, vol. 62, C and D, which have probably been made by Mr. J. Hodgson. They are, I believe, identical with the computations of the same stars, in MSS, vol. 23. In all these reductions, I allude only to the computation of right ascension and declination: that of longitude and latitude was a work of much greater time, labor, and expense. Many of these latter computations, however, still exist.

† The computations of right ascension and declination, from the observations made *per distantias,* are principally contained in the following MSS volumes: viz. *Cygnus* and *Hercules* in vol. 24; *Draco* and *Ursa Minor* in vol. 26 A; *Ursa Major* in vol. 26 C; sundry constellations in vols. 51—56; further computations in *Draco* and *Ursa Major* in vol. 61 E; and further sundry constellations in vol. 62 A, B, C. A few detached computations of the like kind are occasionally to be met with in some of the other MS volumes; but the above contain the principal collections.

appears, from a comparison of the places of such observations with modern catalogues, that the latter reading is sometimes the most correct. Various instances of this kind will be found in the *notes* to the present catalogue.

Considering, therefore, the several circuitous and different modes which Flamsteed was obliged to adopt in order to obtain his results, and the length of time during which the computations were carried on, which is in itself destructive of any system of uniformity, it is not at all surprising that we should meet with anomalies, when the whole came to be collected and arranged in one general catalogue. It is indeed too true that astronomers have long lamented that the *British Catalogue* should contain such numerous discordances as have been pointed out by various authors: but whether these have arisen from errors of observation or of the pen, has been frequently a matter of doubt and discussion. Many stars have been supposed to be lost, because they cannot now be found in the places assigned by Flamsteed: some have been mistaken for other and different stars by the modern astronomer:* whilst not a few have had a proper

* Amongst the several mistakes of this kind that have been made, I shall enumerate the following; which will be quite sufficient to show the confusion and uncertainty that has hitherto existed. Baron Zach states (*Monat. Corres.* vol. ix.) that the star observed at Mannheim by M. Barry, whose position for 1800 is AR = $1^h\ 33^m$, and D = + 22° 5′ 44″, as given in his catalogue of zodiacal stars, page cxiv, is 108 *Piscium*; also that the star No. 846 in the same catalogue is 19 *Virginis*; moreover that the star in Piazzi's catalogue XIX. 347 is 62 *Draconis*: yet none of these stars exist, and the public are only misled by Flamsteed's numbers being annexed in such ambiguous cases. He has likewise supposed that No. 960 in his catalogue is 91 *Virginis*, although it differs upwards of 18° in declination from Flamsteed's star. He also considers that the introduction of 101 *Virginis* into the *British Catalogue* has arisen from an error in computing its right ascension; for that if 30′ be added thereto it will agree with 20 *Bootis*: but the right ascension is correct, and the error has arisen from a mistake of 1° in the declination. He has also supposed that 23 *Sagittarii* is the same as Piazzi XVIII. 81: Flamsteed's star however is neither in Piazzi's nor in any other catalogue; but Professor Airy has been good enough to look out for it, at my request, and finds that its position accords with that given in the present catalogue. Sir Wm. Herschel has considered that 12 *Sagittarii* is the same as Piazzi XVII. 366: but this latter star is 11 *Sagittarii*, and Piazzi did not observe 12 *Sagittarii*. The following misnomers also occur in Piazzi's catalogue, some of which have been transferred likewise into Bradley's catalogue: viz. 38 *Persei* is III. 123 not 85; 18 *Aurigæ* is V. 27 not 26; 7 *Lyncis* is VI. 115 not 123; 22 ~~*Hydræ et*~~ *Crateris* is XI. 115 not 117; 35 *Draconis* is XVII. 380 not 370; 18 *Sagittarii* is XVIII. 52 not 33; 24 *Sagittarii* is XVIII. 105 not 99; 9 *Lyncis* is VI. 123; ~~*51 Camelopardi is the double star VII. 159, and not the star so designated by Bessel*~~; 29 *Sextantis* is X. 86, which both Piazzi and Bessel have supposed to be 28 *Sextantis*; 56 *Draconis* is XIX. 38; and *comes* 19 *Cygni* is XIX. 304, which Piazzi has supposed to be 19 *Cygni* itself. I would further remark that Lalande applies 80 *Aquarii* to Piazzi XXII. 254; whilst Piazzi considers it to be XXII. 279: neither of them however agreeing with the position as given in the present catalogue. These are not (neither have they ever been

motion assigned to them which they do not possess: and thus great confusion and uncertainty have been inadvertently introduced into a science, which in other respects may justly boast of its extraordinary accuracy and precision. These discordances have too frequently, but very unjustly, been attributed to errors of observation; arising either from the inexpertness of the observer, or the imperfection of his instruments. Whereas it will be found, by an examination of the above-mentioned computation-books, that nearly the whole of those errors are the result of arithmetical mistakes in the calculations, which those books enable us to rectify: and we have thus the means of restoring not only the *British Catalogue* to its originally intended accuracy, but also the character of Flamsteed to that high rank, to which it is, by his extraordinary labors, so justly entitled. His observations, although not equal to those made in more modern times, possess an interest and importance from their very antiquity, which will always render them valuable to the practical and physical astronomer. The *British Catalogue* itself (imperfect as Flamsteed left it) has been made the foundation, and has probably been the *cause*, of all subsequent Catalogues;* and its nomenclature is universally adopted by astronomers of all nations. But, Flamsteed was harassed and annoyed in the latter part of his life, and worn down by infirmities which had stuck to him from his infancy; and therefore had not the spirit, nor indeed had he the adequate means, for revising his computations, or for reducing the whole of his observations: since nearly 500 stars are now known to have been observed by him that are not inserted in the *British Catalogue*.† It is, however, rather a matter of astonish-

supposed to be) errors of the press, but the deliberate result of the attempts of the respective authors to reconcile the discordant cases in the *British Catalogue:* and are sufficient to show the inconvenience and impropriety of definitely annexing Flamsteed's number to a star, whose identity is not well ascertained. All the doubts relative to the above-mentioned stars are now removed by the publication of the present corrected and enlarged catalogue.

* Bradley's labors at the Royal Observatory, in this department of the science, consist almost wholly of a re-observation of the stars in Flamsteed's catalogue. He caused those stars to be reduced to the year 1744, and arranged in the order of right ascension, as a sort of working catalogue for his own use; which book still exists in the library of the Royal Observatory. Very few other stars have been observed by Bradley, except such as occasionally entered the field of his telescope whilst he was watching for those of Flamsteed. We are thus indebted to Flamsteed for the subsequent labors of Bradley: for had not Flamsteed led the way, there is much doubt whether Bradley (seeing that he merely followed Flamsteed's steps) would have pursued a similar independent course. Bradley's catalogue contains 3222 stars; and the present enlarged catalogue 3310.

† See Miss Herschel's catalogue of these stars: and my paper on the same subject, in *Mem. Ast. Soc.* vol. 4.

ment that he accomplished so much, considering his slender means, his weak frame, and the vexations which he constantly experienced.

I have already remarked that after Flamsteed had made the computations, agreeably to the method detailed in page 371, he arranged the results, in the order of right ascension, as far as the stars in each constellation were concerned, in another part of the book; classing together, and thus keeping separate and distinct, the several stars belonging to each constellation. These results were afterwards fairly copied out, as a general catalogue, into another large folio book (MSS, vol. 25), which is perhaps the first, in the order of time, of the many MS catalogues found amongst Flamsteed's papers.* I have not been able to ascertain the precise date when this catalogue was first transcribed: but I think there is sufficient internal evidence to show that it was in, or prior to, the year 1699; and it probably formed the ground-work of the MS catalogue, which was deposited (sealed up) in the hands of Sir Isaac Newton, as related by Flamsteed in the history of his life. (See pages 81 and 82.) However that may be, it cannot materially differ from that catalogue; and I make the remark here in order to show that the statement made by Dr. Halley, in the preface to his edition of 1712, is not strictly correct. For he says, "Halleius itaque, "acceptis chartis Grenovicensibus, ante omnia, catalogum fixarum, totius operis "partem longe præcipuam, cum observationibus ipsis *summa cura contulit et* "*recensuit;* nec raro scriptoris vel supputatoris vitio admissos in fixarum locis "*errores correxit* et emendavit, et *hiatus haud paucos* supplevit. Speciatim "vero in zodiaci signis stellas *omnes*, quarum usus frequentior, *accurato calculo* "*instituto ad examen rigidè* revocavit." Now, although it may be somewhat difficult to decide exactly as to the *corrections* and *emendations* which Halley claims to have made; yet, from a careful examination and comparison of Halley's edition, with the MS copy above mentioned, I suspect that they are not so numerous as he would wish to imply; and it is even known that some of his alterations are erroneous. I do not here allude to differences of a few *seconds* only, which frequently occur; because, in the first place, errors to such a trifling amount were not regarded at that day; and secondly, Halley had not the means of determining the places of any of the stars to such minuteness. The corrections, if any, must have amounted to several *minutes*, in order to have been considered worth mentioning. As to his having instituted an accurate calculation and rigid examination of *all* the zodiacal stars, the assertion is disproved by the

* I would exclude from this remark the *approximate* catalogue mentioned in page 382, and which is contained in MSS, vol. 24.

fact that numerous stars are inserted in his edition of the catalogue, whose places are now known to have been erroneously calculated by Flamsteed, and some of which do not even exist. (See the lists B and C, alluded to in page 393.) It was Flamsteed's conviction of such vain boasting that induced him to denounce this preface with so much censure in his letter to Mr. Sharp; and to which sentiments Mr. Sharp responded with equal feeling. See pages 322 and 323. With respect to the "hiatus haud paucos," it should be recollected that Flamsteed always avowed that there were several other stars whose positions he had computed, and more especially in some of the northern constellations, which he stated himself ready to supply if the Referees would accede to his mode of publishing the catalogue; that is, of deferring it till after the printing of the observations from which they were deduced. See page 96, and the correspondence between him and Dr. Arbuthnott, in the *Appendix*, No. 151—162. Dr. Halley states that the reduction of the observations of these northern stars cost "a great deal of trigonometrical calculation:" but this could apply only to a few that were observed with the sextant, and not to the mass of them which were observed with the mural arc.* In those northern constellations therefore we certainly find more discordances between the editions of Halley and Flamsteed, than in any of the other constellations: and Halley may so far lay claim to the merit of having filled up "hiatus haud paucos," inasmuch as he has introduced some stars that Flamsteed had omitted; whilst Flamsteed, on the other hand, has inserted others that had been overlooked by Halley. The whole passage however is written in a spirit unworthy of Halley's high reputation; and coupled with the equivocal motive for taking the publication of the work out of Flamsteed's hands,† as well as with the alleged cause of so many typographical errors and of the omission of some of the copper-plates,‡ conveys a very

* See Mr. Sharp's opinion upon this subject in his letter in the *Appendix*, No. 214.

† "Flamsteedii autem oculis in nascentia indies syderum phenomena intentis, et in etate jam "provecta minus acutis, Edmundo Halleio LL.D. Geometriæ professori Saviliano, atque in astrono- "micis probe exercitato, quod ad reliquam editionem deerat maturandi et ad umbilicum perducendi, "datum est negotium." The reader would scarcely suspect that, when this paragraph was written, Flamsteed was in the height of his zeal for the publication of his works: and fully capable then, and for many years afterwards, of attending to all the necessary details. See the letters in the *Appendix*, Nos. 162, 320, &c.

‡ "Tanto autem et tam preclaro munere donatum minime offendat nævulus unus aut alter: vel "quod in libro primo tot reperiantur errata typographica, quibus excusandis *sudantis præli festi-* "*natio* vix satis esse videtur; vel quod ordo figurarum in observationibus lunaribus, pag. 194, "a fig. 77 incipiat; unde cuilibet pronum esset suspicari nonnullas schematum tabulas in præce- "dentibus desiderari: quod tamen aliter se habet. Utriusque *causas* rectius novit ipse Flam-

unmerited reflection and censure on Flamsteed's abilities and labors: and it cannot be wondered at that Flamsteed should so feel it, and resent it accordingly.

In making the present revision and correction of the *British Catalogue*, I wish it to be fully understood that I have not attempted the re-computation of the original observations, according to the present precise and accurate modes of reduction, nor the investigation of any of the instrumental errors: for, however desirable such an object may be (and I do not despair of its being accomplished at some future time, since those observations have much intrinsic merit) yet I have neither leisure nor inclination for so laborious a task.* All that I have professed to do, and all that the reader must expect, is the enlargement of the catalogue, by including *all* the stars that can be deduced from Flamsteed's observations; and the clearing it of all its known numerical errors: in fact, to render it, in this respect, such as Flamsteed himself intended it should have been; and such as he himself would eventually have published it, had he not been thwarted in his original plan, and had he been furnished with the requisite assistance.

The mode which I have pursued to accomplish this end, has been, in the first place, to reduce the whole of Bradley's catalogue to the epoch of 1690, by means of the following formula, taken from Bessel's *Astronomiæ Fundamenta*, page 136:

$$B - 65\left[\pi - \cdot 722\,(p - \pi)\right]$$

where B denotes the position of the star in Bradley's catalogue, p the precession in 1800, and π the precession in 1755; all taken from Bessel's work abovementioned. This being accomplished, I was enabled, by a comparison, to see

" steedius." Dr. Halley knew the *cause*, as well as, if not better than, Flamsteed: and we cannot but smile at the " sudantis prœli *festinatio*," after reading the preceding history of Flamsteed's life.

* M. Argelander has stated (in a Thesis, entitled *De Observationibus Astronomicis à Flamsteedio institutis Dissertatio*, April 12, 1822) that the probable error of observation by the mural arc, in the *right ascension* of a star computed from a comparison with n stars, situate nearly in the same parallel of declination, is equal to

$$0^s,3263\sqrt{\frac{n+1}{n}}$$

And that the probable error in the *declination* of a star, computed from a comparison with n stars, is equal to

$$5'',20\sqrt{\frac{n+1}{n}}$$

which includes the probable error in the division, *per lineas diagonales*, deduced by him equal to 4",041. The fact I believe to be, that Flamsteed's observations are more accurate than have generally been imagined: and indeed much more so than he himself supposed them capable of proving to be.

where the most material errors occurred in the *British Catalogue:* and by examining the computation-book above-mentioned I have in most cases succeeded in correcting them.* Such stars as I could not find in Bradley's catalogue, I have afterwards looked for in Piazzi's, and compared the positions therewith: and if not to be found there, I have then had recourse to Lalande's *Histoire Céleste*, and the two volumes of the *Memoires de l'Academie des Sciences*, for 1789 and 1790:† and lastly to the *Zones* of M. Bessel. In one or other of these works I have generally been able to identify the smaller stars observed by Flamsteed, and which had hitherto been the subject of much doubt. Some few, indeed, still exist, which have baffled all my efforts at explanation; and I must leave them, under the hope that the sources of error may be happily discovered by some more fortunate individual. See the Tables C and D, at the end, to which I shall presently allude.

With respect to the additional stars which I have here introduced, they are such as have been already collected together, and arranged by me, in a supplementary catalogue printed in the *Memoirs of the Astronomical Society*, vol. iv., page 129, entitled "A Catalogue of Flamsteed's inedited Stars." Since that paper however was printed, and since the present manuscripts of Flamsteed have come to light, I have seen cause to alter some of my opinions relative to the identity of certain stars inserted in that catalogue; as will be evident from a comparison of the notes here appended to the respective stars. Much of the obscurity respecting the identity, and even the existence, of some of the stars has been cleared away; and the major part of them are now satisfactorily accounted for. I must not omit, however, to repeat my acknowledgment of the great assistance I have derived in the progress of this inquiry, from the previous labors of the late Sir William Herschel, and of his sister Miss Herschel; who have done much towards clearing up the difficulties which attend this subject. And if I have occasionally differed from them in the result of any inquiry, I trust it is upon such grounds as they themselves would have assented to, had they fortunately possessed the same means of information, which have since been accidentally placed in my possession, by the recent discovery of Flamsteed's manu-

* In order to preserve the results of these several computations and comparisons with Bradley's catalogue, I have inserted, in the sixth and eighth columns of the present catalogue, the difference between Flamsteed's value, and the value as thus deduced from Bradley's catalogue; as I shall more fully explain in the sequel.

† These two volumes of the *Memoires* contain most of the circumpolar stars not inserted in the *Histoire Céleste*, and may be considered as a supplement to that work.

scripts.* To M. Argelander of Abo I am also much indebted for the very able remarks contained in a paper inserted in the *Astronomische Nachrichten*, Nos. 226 and 227; and which may be considered as a sort of commentary on my own paper just alluded to. That distinguished astronomer will see that many of his shrewd and happy suggestions are verified by Flamsteed's MS papers and writings; and that I have here taken advantage of many of his remarks, to correct some of my former opinions, in the memoir above-mentioned.†

The corrections of the catalogue being thus made, and the stars omitted by Flamsteed having been inserted, I have arranged the whole in one general catalogue, in the order of right ascension, as the most convenient for modern reference. The following is the statement of the contents of the several columns. The *first* contains the number of the stars in the catalogue in numerical order, for the sake of reference; the asterisks which are annexed to any number indicating that there is a *note* to such a star at the end of the catalogue: the *second* contains the nomenclature of the stars, as designated by Flamsteed's numbers; to which are occasionally attached Bayer's letters; those stars to which no number is prefixed, are such as are now, for the first time, introduced into the *British Catalogue:* the *fourth* contains the magnitude: the *fifth*, the right ascension in arc for the epoch 1690: the *sixth*, the difference, or quantity, to be applied to such right ascension, in order to obtain the value of Bradley's catalogue reduced to the same epoch, in the manner above-mentioned:‡ the *seventh* contains the declination for the same epoch (which I have considered more convenient than the north polar distance, for comparison with Bradley's catalogue:) the *eighth* contains the difference, or quantity,‡ to be applied (regard

* See Miss Herschel's *Catalogue of Stars taken from Mr. Flamsteed's Observations*, &c. folio, 1798; and Sir William Herschel's several papers in the *Phil. Trans.* for 1796, 1797, and 1799. We cannot but lament that, at the very time these two distinguished characters were pursuing this important inquiry, the manuscripts of Flamsteed were mouldering (*literally so*) on the shelves of the Royal Observatory: whereby the world has been deprived of the benefit of much of their critical acumen, and superior judgment on these matters.

† I have also referred to, and examined, the *Explication* prefixed to the copy of the *British Catalogue* inserted in Lalande's *Ephémérides des Mouvements Célestes*, for 1785—1792; wherein mention is made of numerous errors in the catalogue. And the result of that examination is, that the errors which he has noted are those *which he has himself made in his own reprint of the catalogue.*

‡ Some of these differences, when they have been too large for convenient insertion in the columns, or when they have not been accurately ascertained, have been transferred to the *Notes* at the end, with a reference thereto; and the sign prefixed to the asterisk denotes the manner in which such difference is to be applied.

being had to the *sign*, which is always to be used algebraically) to such declination, in order to obtain the value in Bradley's catalogue reduced to the same epoch: the *last* contains the number of observations of the star; which, however, it must be understood, is no guide as to the number employed in the reduction. (See page 376.) For this last column, the public are indebted to the industry of Miss Herschel; who has, in the work above alluded to, given a reference to every observation of every star in the *British Catalogue*. Where any star has been now inserted, for the first time, I have myself given, in the *note* thereto, the reference to the observation: whereby the reader may be enabled to judge for himself whether my conjectures respecting the identity of such star be justly founded.

On examining this catalogue, the reader will see that the greatest differences from Bradley's results occur in the right ascensions; and that the differences in declination amount, in general, to a few seconds only. This might be expected from the principles and construction of the instrument employed in making the observations for right ascension; and from the mode pursued by Flamsteed in deducing the results therefrom. And though it may still be a question whether we shall ever be able to reconcile all the anomalies which occasionally present themselves on this subject, yet much may be done to clear away the present difficulties. The correction of the declinations appears to be a much more easy task: the index error of the instrument can now be much better ascertained than at the time of Flamsteed; and the refraction can be more correctly applied by taking a mean height for the barometer, and thermometer for each month in the year. The latitude of the Observatory is also now more correctly known.

I shall here take the opportunity of adverting to a passage in the *Prolegomena* to the *Historia Cœlestis*, page 114, (and which occurs again at page 115 and 117) where Flamsteed alludes to the *error of division* in his mural arc. The circumstance is incidentally mentioned, without any explanation whatever; and as it does not occur in any other of his computations, and as no further notice is taken of it in his printed works, much doubt and inquiry have been exercised on this subject without effect. Its amount is stated, in four places, at 15″; and once, at 20″. Fortunately there exists, amongst the original MS papers of Mr. A. Sharp (now in the possession of Mrs. Giles) a letter from Mr. Crosthwait which alludes to this very subject; and in which the question is referred to Mr. Sharp (who divided the instrument) as to his knowledge of the existence of such an error. The answer of Mr. Sharp is, agreeably to his

invariable custom, written in *short hand* at the back of the letter: and my object now was to get this answer (if possible) decyphered. I applied to several gentlemen conversant with the art of stenography, but without effect; since the characters used by Mr. Sharp were not such as are explained in any of the books of the present day. On accidentally mentioning the subject to Charles Babbage, Esq. (Lucasian Professor of Mathematics at Cambridge) he very kindly offered to make the attempt; and I am happy to say with complete success. For, by a laborious and minute examination and comparison of all the parts, he at length obtained the key to the alphabet; and has thus enabled us to know Mr. Sharp's opinion on this interesting subject. Mr. Crosthwait's letter, and Mr. Babbage's decyphered answer thereto, are given in the *Appendix*, Nos. 251 and 252, page 346: and although a few of the words are still involved in obscurity, yet they fortunately do not bear materially on the point in question. It would appear, from the reference in Mr. Crosthwait's letter to some computations of Flamsteed (which still exist in MSS, vol. 39, pages 116 and 127*) that the supposed error was detected by Flamsteed from some discordances in the results of observations of the zenith distances of the sun at the two solstices, and of the pole star at its upper and lower culmination: but, on a revision of those computations, it will be found that the discordances have arisen almost wholly from the errors in the table of refraction used by Flamsteed; and that when Bradley's refractions are adopted, those discordances nearly vanish. So that, indeed, there is no just ground at present for supposing that the arc was erroneously divided: and other investigations are requisite for determining the fact.

I shall not here enter into a statement or account of the several errors and discordances which I have discovered in the *British Catalogue;* nor into the various alterations that I have introduced; as those will best appear from the various *notes* at the end of the catalogue, where each particular case is separately and distinctly considered. And I would here remark, once for all, that I have not made any alteration (however slight) in the *British Catalogue*, without a statement of the same, and the reasons for such alteration. I would further observe once more, that wherever an asterisk is annexed to the number in the first column of the catalogue, there is a corresponding *note* at the end, explanatory of some circumstance connected with such star. I shall however take the opportunity of stating, in this place, some general facts and remarks, that could not so conveniently be inserted in the body of those notes.

* See the results of these computations in page 186 of the preceding history.

I have in all cases preserved Flamsteed's numbers, for the several stars inserted in the *British Catalogue:* for although that order is occasionally deranged by the correction of the errors which have been discovered, yet I have not thought it right or proper to disturb the nomenclature so universally adopted. Thus, although the position of the very first star in the *British Catalogue* (1 *Arietis*) is erroneously deduced, and ought to have been placed between 4 and 5 *Arietis;* yet I have still continued to designate it by its well-known number, and have merely altered its relative position in the catalogue. Again, *Polaris* is now the second star in *Ursa Minor*, instead of being the first. And again, the position of 1 *Sagittarii* is also erroneously deduced, and should have been placed between 11 and 12 *Sagittarii.* The *rejection* also of certain non-existing and duplicate stars would derange the notation. But, to alter all these numbers at the present day, on this account, would lead to endless confusion; and I have therefore retained the *original* number of each star in the catalogue. Other cases of a like kind might be adduced, which would confirm the propriety of not making any alteration in this respect. In fact, we find that Flamsteed's notation is already and will continue to be further deranged, by the mere precession of the equinoxes.

The number of stars in the *British Catalogue*, as published by Flamsteed, is 2935: but as 22 of those are duplicates (or synonymous) this number should be reduced to 2913. Out of these, however, there are at least 61 that do not (nor ever did) exist: whence the correct number will be only 2852. The number in the present catalogue is 3310, amongst which there are no synonymes: consequently there is an increase of 458 additional stars.* The synonymes in Flamsteed's edition of the catalogue are given in Table A, at the end of the *notes:* the first column containing the names of the stars, as retained in the present catalogue; and the second, the synonymes. The insertion of such duplicate stars was evidently an oversight of the editors; as Flamsteed endeavoured to guard against it as much as possible. And Mr. Crosthwait, in one of his letters to Mr. Sharp (see page 339) alludes also to the same subject. It was however difficult wholly to avoid it, in the manner the catalogue was arranged. In some of the MS catalogues (of which there are several, in various stages of their progress, amongst Flamsteed's MSS at the Royal Observatory) we see occasionally that a star has been struck out of a certain constellation, with a note attached thereto that it belongs to some other. This star has sometimes

* Out of these additional stars not more than about 200 are to be found in Piazzi's catalogue: the remainder have been identified by a minute examination of the works alluded to in page 388.

been omitted to be inserted in such new place; and at other times both positions have been inadvertently retained: thus, in the one case, increasing the number of omitted stars; and in the other, producing a synonyme.

I have already alluded above to certain stars, which have hitherto formed part of the *British Catalogue*, but which, from Flamsteed's own computations, are now found not to exist. The total number of such stars, whose non-existence may be determined in a similar manner, is 61; and are given in Table B, at the end of the *notes*. These stars have consequently been totally excluded from the present catalogue: and the numbers annexed to the same respectively, in the above list, refer to the *notes* at the end of the catalogue, where the reason for their exclusion is fully detailed. The non-existence of those few stars, to which an asterisk is annexed, is in some measure founded on conjecture: but, as to the rest, there can be little or no doubt about them. The star, to which no number is annexed (viz. 34 *Tauri*) is supposed to be the planet *Uranus*.* It was observed by Flamsteed on Decem. 13, 1690, at $9^h\ 41^m\ 49^s$. There are five other observations of this planet in the *Historia Cœlestis*, viz. on March 22, 1712, at $9^h\ 35^m\ 19^s$; on Feb. 21, 1715, at $12^h\ 27^m\ 1^s$; on the following day, at $12^h\ 22^m\ 59^s$; on Feb. 27, at $12^h\ 1^m\ 42^s$; and on April 18, following, at $8^h\ 50^m\ 44^s$. M. Argelander is also of opinion that the star No. 1647 in the present catalogue is deduced from an observation of this planet. Perhaps also some of the stars in the next mentioned list (C) may be one or other of the new planets.

There is another class of stars, however, which form part of the present catalogue, and of which the observations appear to be accurately recorded, but which still cannot now be found in the heavens. These amount to about 23 in number, and are given in Table C, at the end of the *notes*. It cannot be supposed that so many stars have actually vanished from our system: and the only probable explanation, that can be offered, is either that there has been some error in the original observations, or some inaccuracy in recording them (but, of which we shall now perhaps ever remain ignorant): or that they may relate to some of the new planets, that accidentally entered the field of the telescope in the course of observation: or again, that they may be stars varying from time to time in magnitude, and perhaps occasionally disappearing. That stars, of this latter class, exist, there can be no question; and that some of the stars in the *British Catalogue may* be of this kind, would appear probable from the cir-

* See *Astronomisches Jahrbuch*, for 1789, page 202.

cumstance that Sir W. Herschel states (in his fourth catalogue of the comparative brightness of stars, inserted in the *Phil. Trans.* for 1799, page 143) that he could not discover 9 *Tauri;* and that M. Lalande could not find 14 *Draconis.* Moreover, Piazzi says that he could not find 3 *Arietis.* Yet all these stars are known to exist; and in the places originally described.

But the most remarkable class of stars are those which, although inserted in the *British Catalogue,* neither exist, nor have been observed (as far as I can ascertain) by Flamsteed; and the difficulty is to account for their insertion. These stars however are but few, amounting only to about 10 in number, and are given in Table D, at the end of the *notes.* I have taken some pains to inquire into this singular circumstance; but I am unable to throw much light on it. Some of them, I suspect, are introduced through errors of computation; as I have remarked in the notes appended to them: but, as to the rest, I cannot discover the least clue to the cause of their introduction; nor any trace of the computations. Many of those, which Miss Herschel considered as lost stars, have been found to have been introduced into the *British Catalogue,* from such errors as those just mentioned: but these still remain unexplained.

Amongst the stars in the *British Catalogue,* there are several to which the north polar distance only is attached, the right ascensions being wholly omitted. These amount to about 29 in number, and are given in Table E, at the end of the *notes.* The omission of the right ascension has in some cases been unavoidable; as, where the transit of the star across the wire has not been observed. In other cases, it has arisen from inadvertence; as, where a complete observation has been made and recorded. Halley, in his edition of 1712, has supplied some of these deficiencies: and such cases are marked with an asterisk annexed to the numbers, in the above list. In the present catalogue I have inserted either Halley's values above alluded to, or an assumed *approximate* value deduced from modern observations: referring however to the *notes* for a statement of such fact.

As it may be desirable to see, at one view, the whole of the alterations in the positions of the stars, which I have made in the *British Catalogue,* I shall give a list of the several corrections, in Table F, at the end of the *notes.* The numbers, prefixed to the stars in that list, denote the corresponding numbers in the present catalogue, and consequently refer to the *notes* at the end, where the authority for each correction is fully stated. In a few of the cases, the reasons adduced are wholly, or in part, founded on conjecture: and to such stars I have, in the above list, annexed an asterisk, for the sake of distinction; in order that the reader may more fully satisfy himself on such doubtful occasions, and

examine more minutely into the circumstances, if he should think proper. That some of these mistakes are errors of the press, I think there can be no doubt: but, on the whole, there are fewer errors of this kind than might reasonably have been expected. See the *notes* to Nos. 2131, 2403, 2414, 2505, &c.

After all the corrections above-mentioned, however, are made, there still remains a portion of the stars whose positions, as they now exist in the present catalogue, are far from being satisfactory: but whose discordances may perhaps yield to future investigations, or to probable conjectures. I have drawn out all these cases in two separate lists, with the design of presenting them in one view to the reader, and thus more closely to draw his attention to the subject. The first list, inserted in Table G, at the end of the *notes*, contains all those stars in the present catalogue (with the exception of those that are supposed not to exist) whose difference in right ascension amounts to $4'$ and upwards: amongst which it will be seen that many of the discordances may be reconciled by supposing an error of 1^m in the time of transit (a mistake very easily made); and that others depend on the inaccuracy of the determining star. The second list, inserted in Table H, at the end of the *notes*, contains all those stars whose difference in declination amounts to $1'$ and upwards: amongst which it will be seen that more than one-third of the discordances arise from the difference in the refraction adopted: so that, after all, the discordances that would exist, if the observations were rigidly and accurately reduced, are probably very few, and of no great amount.

It is proper to make some remarks here respecting *Bayer's letters*, by which the principal stars in the *British Catalogue* are designated. It is well known that such stars were, by the ancient astronomers, for the most part denoted and identified by a very verbose description, corresponding with their position in some fictitious or imaginary figure in the heavens: whilst some indeed were called by a specific and definite name. This plan was pursued by Ptolemy, and has been adopted, even down to the time of Flamsteed, by most succeeding astronomers. But, such a verbal description of the places of the stars (limited even as they then were) was liable to great confusion, since the figure itself was not always well defined or understood: it therefore occurred to Bayer that much of this inconvenience might be removed, if the stars in each constellation, visible to the naked eye (which were all that were then known), were denoted by the letters of the alphabet, in the order of their magnitudes; those which were of the greatest magnitude being denoted by the first letters, and so on successively to the end of the alphabet. This was a great improvement on the former mode of designa-

tion, as it at once indicated the *class* to which any particular star in a given constellation might be assigned: and although there might be some uncertainty as to the precise magnitude indicated by any particular letter, and although the same letters would not always indicate the same magnitude, when used in different constellations, yet with respect to any given constellation, it gave a tolerably clear idea of the class, to which any star belonged: and, by the help of maps, their positions were pretty well authenticated.

Bayer began with the Greek alphabet; and if the known stars in the given constellation exceeded the number of letters in that alphabet, he then took up the Roman alphabet as far as was required.* These two alphabets fully answered his purpose: for he did not meet with any constellation where it was necessary to extend the notation beyond the second alphabet. Flamsteed proposed to follow Bayer, by affixing to the respective stars in each constellation, the corresponding letters in Bayer's maps: at the same time however preserving also in many cases the verbose descriptions and the proper names of the principal stars, agreeably to the custom of his predecessors. On these latter points he was rigidly austere, as may be seen by the anathema pronounced by him (in his *Prolegomena*, page 161) on all such as should deviate from that practice.† In comparing the stars in the *British Catalogue*, with those in Bayer's maps, I have met with several errors, which I have here corrected. These errors have arisen sometimes from the printer having mistaken Flamsteed's letters, which are frequently obscurely written:‡ whilst in other cases Flamsteed appears to have taken the wrong letters from Bayer's maps.§ However this may be, I have here

* The word Roman is here used in contradistinction to the Greek; and in fact the Roman character was the one actually adopted by Bayer: but modern astronomers prefer the *Italic* letters, as being more easily distinguishable in print. Bayer never used any *capital* letters, except the letter A; which he has invariably adopted, both in his letter-press and on his maps, whenever he entered on the second alphabet.

† The following is the passage alluded to. "Tertia [columna] verò stellarum indicat nomina "secundum Ptolemæum; quæ quidem nomina mordicùs tenenda et conservanda duxi: quando- "quidem non solùm Arabes et Persæ, in suis stellarum catalogis, et suarum observationum historiis, "sed et Germani, Itali, Galli, Hispani, Lusitani, atque nostrates Britanni, iisdem usi sunt nomi- "nibus: atque, si aliter fecissent, veterum observationes intellectu admodum difficiles reddidissent. "Veteribus igitur adhæresco: ac innovatores ignaros, arrogantes, ab omnibus integerrimis, et peri- "tissimis astronomiæ professoribus posteris *castigandos* relinquo et *puniendos*."

‡ Thus, 65 *Piscium* is *i* not *ι*; 52 *Andromedæ* is χ not λ; 67 *Eridani* is β not *h*; 62 *Geminorum* is ρ not *s*; 15 *Scorpii* is ψ not χ; 45 *Herculis* is *l* not *e*. But see the *notes* to Nos. 82, 193, 667, 710, 1041, 1572, 1847, 2214, 2313, 2372, 2400, and perhaps some others.

§ Thus, 49 *Andromedæ* is not ξ; 50 *Andromedæ* is not ν; 43 *Cassiopeæ* is not *c*; 56 *Ceti* is

restored them all to Bayer's original stars, as far as the same could be identified; conceiving this to be much better than that the error should be perpetuated. Much confusion and inconvenience have already arisen in many of these discordant cases: and if only a few corrections were made, others would necessarily arise, as one error will generally be found to involve another. I have therefore considered it better to revise the whole, and to restore Bayer's letters in every case to their proper stars; or to such stars as may most nearly approach the positions intended to be laid down by Bayer.

But, besides these letters of Bayer, Flamsteed has frequently introduced new ones (and in some cases, *duplicates*) of his own. This, however, I have reason to believe was only done, as a temporary measure for convenient reference:* and had he lived to revise his catalogue himself, when it was finally published, I have no doubt but that he would have reconsidered and amended the subject; or probably have omitted such new letters altogether. For as it was Bayer's object that the order of magnitudes should, as nearly as possible, follow the order of the letters, it is evident that the introduction of such new letters would, in most cases, be at variance with this great and advantageous principle. Thus, for the sake of an example, let us take the case of 1, 6, and 12 *Aquilæ*, which Flamsteed has (without reference to Bayer) respectively designated by the letters *m*, *l*, and *i*: and which, according to Bayer's system of notation, would be consi-

not *v*; 55 *Cassiopeæ* is not *ι*; 6 *Persei* is not *h*; 58 *Tauri* is not *h*; 27 *Orionis* is not *ρ*; 57 *Cancri* is not *ι*; 5 *Ophiuchi* is not *g*; 106 *Aquarii* is not A. But see the *notes* to Nos. 159, 174, 184, 190, 237, 257, 261, 538, 710, 837, 1245, 1252, 1654, 2245, 3251, and perhaps some others.

* See the group of 6 stars, situate under the feet of *Cassiopea*, in Flamsteed's maps, designated by the letters *c*, *d*, *e*, *f*, *g*, *h*: also the group of 6 stars between *Aquila* and *Ophiuchus*, designated by the letters *i*, *k*, *l*, *m*, *n*, *o*: also the two groups in *Pegasus*, one consisting of 3 stars, designated by the letters *e*, *f*, *g*, and the other consisting of 4 stars designated by the letters *l*, *m*, *n*, *p*: also the group of 5 stars in *Cygnus* designated by the letters *h*, *i*, *k*, *l*, *m*: also the group of 5 stars in *Ophiuchus*, designated by the letters *n*, *o*, *p*, *q*, *r*: also the group of 4 stars, near Medusa's head, in *Perseus*, designated by the letters *p*, *q*, *r*, *s*: also the group of 3 stars in *Gemini*, designated by the letters *p*, *q*, *r*: also the group of 3 stars near the tail of *Cetus*, designated by the letters *f*, *g*, *h*: also the group of stars forming the *Pleiades*, designated by the letters *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, *l*, *m*, *p*, *s*. In all these, and some others of a like kind that might be adduced, I consider that Flamsteed had inserted the letters in his MS maps, for a temporary purpose only, whilst he was in the course of verifying the positions of the stars (similar to the plan adopted by Newton in his *Principia*, for showing the path of the comet of 1680, and by many modern astronomers when unknown stars are referred to): and that such letters have been inadvertently and improperly retained by his editors. I have therefore, for the reasons stated in the text, in all cases rejected them, when they do not accord with Bayer.

dered as only of the 6th magnitude; since *h* is the last letter which he uses in that constellation. They are however all of the 4th magnitude; and if Bayer's principle were followed, ought to have been inserted after the letter μ. Again, 70 *Ophiuchi* is designated by the letter *p*, in the *British Catalogue;* and therefore (according to Bayer's principle) might be supposed to be a star of very small magnitude; certainly not greater than the 6th. But it is a star of the 4th magnitude; and therefore ought to class with λ and μ. As the introduction of such new letters, therefore, vitiates the whole of Bayer's principle of notation, I have in all cases rejected them; and retain none but those adopted by Bayer himself.

A more striking instance, however, of the perversion of Bayer's principle of notation may be seen in the method which has been adopted, in the *British Catalogue*, in designating the stars in the constellation *Coma Berenices*. This constellation is not inserted amongst Bayer's maps: and therefore the whole of it is new ground to Flamsteed, who has paid no attention whatever to the leading feature of Bayer's method. For in the first place he does not use any Greek letters: and secondly, the letters which he does use, are not chosen or adapted with any regard to the magnitude of the stars; and are applied only to a small cluster, in the middle of the constellation. They seem introduced (as I have before stated) for the sake of some temporary convenience: and as they are so completely at variance with the principles laid down by Bayer, I have rejected the whole of them; being fully convinced that they never would have been sanctioned by Flamsteed, had he lived to see the final correction and publication of his catalogue.

Sometimes there is a doubt, as in the case of two near stars of equal (or nearly equal) magnitude, to which star Bayer's letter should be applied. When such instances occur Flamsteed has annexed the letter to each of them, and affixed the numerals 1 and 2, according to their order of right-ascension. Thus, in the case of $\varkappa$ *Tauri*, the two stars are designated as $\varkappa^1$ and $\varkappa^2$; although there is only one star denoted by that letter in Bayer's map. This is justifiable, since it cannot now be ascertained as to which of the two in particular Bayer meant to affix that letter: and probably their joint effect might have produced the appearance of *one* star to his eye. Other cases of this kind occasionally occur; and as no inconvenience or confusion can arise from this method, I have preserved Flamsteed's mode of designation. But, where the two stars differ much from each other in magnitude, and are clearly distinguishable, such a mode of notation may lead to some confusion, as it evidently vitiates the leading principle of Bayer's method. Thus, in the case of μ *Cancri*, where one of the stars is of the

5th, and the other only of the 7th magnitude, and which Flamsteed calls μ^1 and μ^2, I have rejected the letter as applied to the first of these stars, and retained it only in the latter; as there can be no question as to which of these two was intended to be so distinguished by Bayer: and by designating the former by the letter μ, a false estimate of its magnitude is held out to the world. The same remarks may be applied to several others: but it is needless to multiply examples of this kind; since they must be obvious to every one conversant with this subject. Flamsteed has indeed too frequently broken through this leading principle of Bayer's method, by adding numerals (in the order of right ascension) to Bayer's letters, without any regard to the magnitude of the stars in question; and sometimes even in defiance of Bayer's express notation. Thus, let us take the case of 2, 4, and 6 *Virginis:* the former (which is of the 5th magnitude) is called by Bayer ξ; and the two latter, (which are of the 6th magnitude) A^1 and A^2. But Flamsteed, on account of the *proximity* of the first two stars, without any regard to their *magnitudes*, has called them ξ^1 and ξ^2; and denoted the latter only by A. These errors I have corrected in the present catalogue. In some instances an innovation appears to have been made without due consideration: thus ξ *Geminorum* is properly 31 *Geminorum* in the *British Catalogue*, and is so called by Flamsteed: but he has also designated 30 *Geminorum* (a star of smaller magnitude) by that letter, overlooking entirely 32 *Geminorum*, which is marked as of equal magnitude and much nearer Bayer's star. All such discordances are also corrected in the present catalogue.

There are four *clusters* of stars in Bayer's maps, distinguished by a *single* letter only, which appear to have been partly overlooked by Flamsteed: these are τ *Serpentis* consisting of 8 stars; τ *Eridani* consisting of 9 stars; υ *Eridani* consisting of 7 stars; and π *Orionis* consisting of 6 stars. In all these cases Flamsteed has supposed that Bayer intended to denote only *two* stars in each of those constellations:* which has probably arisen from his having only the *maps*

* The group of 10 stars, designated by Bayer as ψ *Aurigæ*, has been wholly overlooked by Flamsteed, as there is no star designated by that letter in the *British Catalogue*: probably from the difficulty of identifying the particular stars. In fact I have not been able to satisfy myself on this point, and I must leave the case as it is. The stars in question are the group lying between 90°—100° right ascension, and 38°—50° north declination. Some of them may be identified; but unless the whole be satisfactorily made out, it would only introduce confusion to apply Bayer's letters to a portion of them. Fortunately the stars are of small magnitude; and whether the letter be applied or not, is a matter of no great moment. I would here also remark that some difference of opinion formerly existed as to the identity of the 8 stars forming the cluster τ *Serpentis*: some astronomers conceiving that 33 *Serpentis* ought to be included, and 22 *Serpentis*

of Bayer, *without the letter-press* printed at the back; as in such case, the mistake might easily have occurred. Perhaps this circumstance may also have given rise to other deviations from Bayer's method.* In all these instances I have restored Bayer's letter, annexing the numerals in the usual manner:† but it may be proper to make a few additional remarks in the case of υ *Eridani*. Only four of the 7 stars, so designated by Bayer, were observed by Flamsteed, on account of their great southern declination; and only two of them to which he has annexed any letter, which are called by him υ^1 and υ^2. But they are, in fact, υ^6 and υ^7 of Bayer; and the other two stars are υ^4 and υ^5, and must be restored to their proper order: otherwise when the stars in the southern hemisphere come to be joined with those in the northern hemisphere, in one general catalogue (as must, sooner or later, be the case) another source of discordance will arise, which had better be obviated at once.‡ I am aware that some confusion is likely to arise, at first, from these various alterations; which have not been made without due reflection, nor without consulting those who are well versed in the subject. But, I trust, if any such confusion is experienced, that it will soon wear away, and that the alterations here adopted will eventually tend to the convenience of the practical astronomer.

It has been too much the practice, of late years, to increase the number of

omitted. But as the star, supposed to be 33 *Serpentis*, does not exist, there can now be no doubt on the subject.

* The copy of Bayer's maps, which belonged to the late Mr. A. Sharp, who had the final arrangement of Flamsteed's maps, is now in the possession of Mrs. Giles, and does *not* contain the letter-press at the back of the maps. There are many copies of this imperfect edition in existence: they bear the same date (1603), and appear to be printed from the same plates, as the perfect edition.

† The usual manner of annexing the numerals is according to the order of the right ascensions of the stars: but, in a few cases, it would seem that Bayer intended a different arrangement. Thus the stars, forming the two series denoted by π *Eridani*, and ψ *Aurigæ*, appear to be reckoned in the order of their north polar distances: whilst those denoted by υ *Eridani* seem as if reckoned contrary to the order of their right ascensions. These few doubtful instances, however, ought not to invalidate the general rule adopted by astronomers.

‡ Already some confusion of this kind has been introduced by the inattention of Lacaille to Bayer's letters and method. Thus 41 *Eridani* (which is the fourth of the series of stars designated by the letter υ in Bayer's map) is called ξ; which letter is affixed by Bayer to a star situate in a very different part of the constellation: again 43 *Eridani* (which is the fifth of the above series) is called *d*: whilst the first, second, and third of the above series, are respectively called *h*, *f*, and *g*. Numerous other cases may also be met with: and it is hoped that this subject will engage the attention of those modern astronomers who are about to publish new catalogues of the stars in the southern hemisphere, from their own observations; otherwise this confusion may be inadvertently increased, and perhaps rendered perpetual.

letters by which the stars are denoted: " a custom more honored in the breach " than the observance;" since much confusion has thereby been introduced, which otherwise would not have occurred. Piazzi led the way, by adding a variety of letters, both large and small, without any system or regularity: but it has since been carried to a most inconvenient, and even absurd, length by Bode, who has, in his great catalogue, exhausted two or three alphabets on some of the constellations, without the prospect of its leading to any advantage. Other astronomers have introduced a practice of designating stars, contiguous to any of Bayer's known stars, by numerals, according to the order of their right ascension; without any regard to their similarity of magnitude, which is the very essence of Bayer's notation. Thus we meet with α^1 *Libræ*, α^2 *Ceti*, β^1 *Capricorni*, and some others, which can have no pretensions to be classed with the stars designated by those letters in Bayer's maps.* Indeed it would have been much better had Bayer himself limited his notation to a few of the first letters of the Greek alphabet, so as to have excluded all stars below the 4th or 5th magnitude; since the smaller stars were very likely, especially in his day, to be mistaken one for the other: even as we now find to be the case when we attempt to identify some of his stars. As a much more convenient and certain mode of designating the stars, by means of a numerical arrangement in the order of their right ascension, is now universally adopted, astronomers ought to discountenance any further innovation on Bayer's method; and perhaps if they were to agree even to discard or disuse his notation altogether, in stars below the 4th or 5th magnitude, as above hinted at, it might tend to simplify and improve the subject. This however is a matter in which each practical astronomer will use his own discretion.†

It does not exactly appear, from Bayer's work, how he obtained the positions of the stars which he has inserted in his maps. Tycho was the only authority in his day; and even the errors of Tycho would thus be perpetuated, if Bayer did not survey the heavens himself, and lay down his maps from actual observation. That some mistakes, arising from this source, have been committed, is evident

* As it is certainly very convenient to adopt some sort of nomenclature by which the proximity and order of right ascension of a small star, close to any one of Flamsteed's stars, might be designated, I would suggest the propriety of adopting Piazzi's method of notation, by annexing the letters *pr* or *sq* (according as the small star is preceding or following) instead of figures, which are too apt to mislead.

† The late Sir Wm. Herschel, in one of his papers inserted in the *Phil. Trans.* (1796, page 181) says that he discarded the *letters* entirely, and used only *numbers*, in order to prevent confusion in his references.

from an inspection of the position of the stars in the left leg * of *Ophiuchus*: where a cluster of stars is placed on the *north* of the ecliptic, which, in fact, are situate to the *south* of that line. This error has arisen from Bayer having too implicitly followed the printed copy of Tycho's catalogue of the stars in the constellation *Ophiuchus*, all of which are therein stated to have *north* latitude, and are accordingly so printed likewise by Flamsteed in his *Historia Cœlestis*. But, I suspect that all the stars in *Ophiuchus*, from the 26th to the 32nd, both inclusive, in Tycho's catalogue, as edited by Flamsteed, have *south* latitude; otherwise they will not agree with the actual state of the heavens; nor indeed can they be all identified even on this supposition: and I have consequently been obliged to leave most of them as I found them.† Other discordances also, apparently arising from the imperfection of the catalogues used by Bayer, are noticed in the sequel: see more particularly the *notes* to Nos. 63, 1498, 1888, &c.

Part of the confusion in the application of Bayer's letters has arisen from a want of attention in drawing the outlines of the constellations on the maps; whereby it has sometimes happened that the stars which are placed by Bayer in one constellation, are by Flamsteed (or rather, by his editors) retained in another. Thus, on account of the right foot of *Andromeda* being too far extended, the star called φ *Persei* is brought within the limits of the former constellation: and thereby, in the *British Catalogue*, the constellation *Andromeda* appears to have two stars denoted by the letter φ: but the second of the two stars so designated ought to have been transferred to its proper constellation, *Perseus*; and doubtless would have been so, had Flamsteed lived to complete the maps. In one instance, that of σ *Cancri*, he or his editors have completely mistaken the three stars, designated by that letter, in Bayer's map. Other cases of inaccurate delineation and arrangement will present themselves on a more minute examination of the maps and the catalogue, all of which I have here corrected.‡ Similar anomalies also occur even where no letter is concerned: single

* In the left *foot*, according to Flamsteed.

† On mentioning my difficulties to Professor Rigaud, he was good enough to examine the MS copy of Tycho's catalogue in the Bodleian Library at Oxford, alluded to in page 368: and he informs me that the 31st and 32nd stars are marked as having *south* latitude (viz. 7° 10′ and 4° 20′); but that all the rest are, as in Flamsteed's edition, situate in *north* latitude. See also Kepler's remarks on some of these stars at the end of his *Tabulæ Rudolphinæ*.

‡ Thus, 51 *Andromedæ*, which is called ν by Flamsteed, is υ *Persei*; 6 *Cancri*, which is called χ by Flamsteed, is χ *Geminorum*; 15 *Cancri*, which is called ψ by Flamsteed, is ψ *Geminorum*; and so perhaps with some others.

stars belonging to one constellation stand insulated, in the maps, in the midst of other constellations, to which (according to the *British Catalogue*) they do not belong; whilst also, in many instances, the stars near the boundaries of two contiguous constellations are so intermixed, that it is difficult to discriminate one from the other. Some of these discordances I have collected together for the information of the reader, and inserted in the note below, in order that the present subject should not be interrupted.* Whether it would be desirable or expedient to remodel the whole of the *British Catalogue*, as at present corrected and enlarged, and to arrange the stars, belonging to the several constellations, more in unison with correct drawings of their outlines on the maps, can only be properly considered in conjunction with other alterations to which I shall presently allude. Had Flamsteed lived to have finished the maps, or had they appeared (as he wished) at the same time with the catalogue, these errors would doubtless have been discovered and corrected. In making a survey of the heavens, so entirely new as that of Flamsteed's, he was often obliged, in the actual course of his observations, to annex to any new star passing through the field of the telescope, the name of the constellation at random, leaving it to be corrected at a more convenient and leisure season. Owing to the vexations and obstructions which he met with, he died before this portion of his work could be fully completed: and although Mr. A. Sharp and Mr. Crosthwait bestowed great

* With respect to misplacements of the stars, we find 16 *Trianguli* improperly inserted in the constellation *Aries;* 101 *Virginis* improperly inserted in *Bootes;* 6 and 49 *Leonis Minoris* in *Leo;* 51 *Ceti* in *Pisces;* 54 *Ceti* in *Aries;* 50 *Camelopardi* in *Lynx;* 32, 33, and 34 *Ophiuchi* in the head of *Hercules;* and so perhaps of many others. In some cases the anomaly is evident on inspection, as where the misplaced star is *close* to another in a different constellation: thus, 23 *Piscium* is close to 83 *Pegasi*, and should have been assumed as one of the stars in the latter constellation: in like manner 76 *Orionis* is close to 8 *Monocerotis*, 1 *Sagittæ* is close to 1 *Vulpeculæ*, and 25 *Leonis Minoris* is close to 34 *Ursa Majoris;* and therefore they all require a similar alteration in their arrangement. In many cases the boundaries of two contiguous constellations on the maps run too much one within the other, and thereby cause considerable difficulty in identifying the stars. Thus 1 *Sagittarii* is intermixed with the stars in *Ophiuchus;* 1 *Aquarii* with those in *Aquila;* 29—33 *Scorpii* with those of *Ophiuchus;* 3 *Hydræ* with those of *Monoceros;* 37, 39, and 34 *Lyncis* with those of *Ursa Major;* 19 and 46 *Ursæ Majoris* with those of *Lynx;* 25 *Leonis Minoris* with those of *Ursa Major;* 1 *Canum Venaticorum* with those of *Ursa Major;* 40, 41, 69 and 73—80 *Draconis* with those of *Cepheus;* 45—49 *Serpentis* with those of *Hercules;* 4 *Aquilæ* with those of *Serpens;* 3 *Cygni* with those of *Vulpecula;* and perhaps some others. But the most singular anomaly is the case of 13 *Navis*, which is situate nearly 20° from any part of that constellation; and has moreover the whole breadth of the constellation *Monoceros* intervening. It ought in fact to be called 15 *Canis Minoris*, to which constellation it more properly belongs.

pains in getting out the maps, yet the finishing touch of the classification of the stars into their proper constellations could not, for various reasons, be effectually accomplished. And it appears, from the correspondence between these two gentlemen, that the expense of reprinting the catalogue with *their* emendations, at length weighed seriously with Mrs. Flamsteed, and prevented its execution. Considering therefore all the adverse circumstances attending the case, we ought to be thankful that so much has been transmitted to us, even in the state in which it now exists. See the letters of Mr. Crosthwait in the *Appendix*, Nos. 243, 260, 272, &c. In order that the reader may see at one view the several alterations which I have made in the application of Bayer's letters, as well as a list of the new ones and duplicates introduced by Flamsteed, which I have discarded, I shall insert the whole of them in Table I, at the end of the *notes*. The *alterations* which have been made are legitimate corrections, and therefore require no apology: the propriety of *discarding* the new letters introduced by Flamsteed must rest on the decision of the public.

It is not necessary for me here to enter into the propriety or expediency of making a total revision and amendment of Bayer's method of designating the principal stars, so as to include those of considerable magnitude which he has omitted, and to exclude such as are of inferior magnitude, and therefore liable to be confused one with another: or whether it would be desirable to extend his plan to other constellations than those of Ptolemy, viz. to *Coma Berenices* re-introduced by Tycho, and to the 8 constellations introduced by Hevelius.* Such a measure, if considered desirable (and there can be no question of its convenience and advantage, if too much confusion would not be created thereby), will doubtless be taken up by others more competent to decide on its general propriety, and more likely to secure its general adoption, if determined upon. That Bayer's plan was imperfectly executed at first, is too notorious (and that it should have been so much so, is somewhat surprising at the present day), since several stars of the 4th and 5th, and some even of the 3rd magnitudes, are wholly omitted in his maps; and only a small portion of those of the 6th magnitude retained.† Moreover the southern hemisphere was not sufficiently well known at that period to warrant a special nomenclature: and although astronomers may justly hesitate about reforming the nomenclature of the stars

* *Monoceros*, *Sextans*, *Camelopardus*, *Lynx*, *Leo Minor*, *Canes Venaticorum*, *Vulpecula*, and *Lacerta*.

† Delambre has justly remarked that no man ever acquired immortal fame at so little sacrifice or trouble, as Bayer.

in the northern hemisphere, it well becomes those distinguished astronomers, who are now about to publish new catalogues of the southern stars, to consider whether their nomenclature can be placed on such a footing as will destroy the anomalies and discordances that at present exist.

With respect to the magnitudes of the stars, I have compared every star in the *British Catalogue* with the corresponding one in Piazzi's: and wherever I found a considerable discordance, and in numerous other cases, I have had recourse to the *original* observations. If the error has been manifest, the requisite alteration has been made; but always with a note of the same. In other (irreconcileable) cases of considerable discordance with modern observers, I have merely noted the fact: since the star may possibly be included in the class of variable stars; the extent and number of which are not sufficiently known at the present day. But where there appears to be a difference of only one magnitude, between the values given in the *British Catalogue*, and those of modern astronomers (of which there are perhaps about 500 cases), I have not thought it necessary to notice the circumstance: for, in Flamsteed's time this subject was not sufficiently attended to; and he has frequently adopted the magnitude recorded against the observation from which the star was reduced, without comparing it with other observations, and taking the mean. In the case of the principal stars, he has not noted their magnitudes in his observations, but appears to have assumed such as had been agreed on by his predecessors. The magnitudes, as now corrected, will be found to be much more in accordance with modern observations, than in the *British Catalogue*, where the subject does not seem to have been minutely attended to by the editors. And I would here remark that, in the *printed* copy of the Observations, the magnitudes are very frequently omitted, and oftentimes erroneous; and that in no case is the *half* magnitude noted: so that if, at any future time, it should be considered desirable to investigate this subject more fully, it will be necessary to have recourse to the *original manuscripts*.*

In the notation of the magnitudes I have introduced an alteration which I trust will be found convenient and useful. It has hitherto been the practice to designate the magnitude of a star, which is supposed to be nearly a mean between two other given magnitudes, by noting down the two extreme magnitudes, and inserting a dot between them. Thus, a star which is above the

* I would also observe that on consulting the original MSS, the reader will occasionally find the letters *p* and *c* annexed to the magnitudes. The former (I apprehend) stands for *pusilla*; and the latter, for *circiter*. This is not noticed in any of his printed works.

fourth magnitude, but not so great as the third, is designated as 3·4m: and in some instances we find that, when the observer has conceived that it was rather nearer to one of the fourth magnitude than to one of the third, the order of the figures has been reversed, and written thus (4·3m); which latter expression means I presume the same as the character 4· used by Tycho, as mentioned in page 368. If it were required to designate a magnitude rather less than the third, and yet not so small as 3·4, Tycho would annex two large dots to the figure 3, thus (3:); whilst Flamsteed would annex the letter *p* thereto, thus (3 *p*).* But this vague and indefinite system of notation ought not to exist at the present day: and it would be very desirable to introduce a more convenient and general mode of designating the precise magnitude of a star, as far as the same can be ascertained with any tolerable accuracy.† Even the term ***magnitude*** is incorrect; since the expression has no relation to the size of the object, but merely to its brilliancy: unless indeed we suppose that the largest stars are the nearest to us in position. Sir Wm. Herschel has entered into this subject at great length in his several papers "On the comparative brightness of the Stars," inserted in the *Phil. Trans.* for 1796, 1797, and 1799, but with a somewhat different view from that in which I am now considering it; and he has there introduced several new conventional marks for expressing minute differences of magnitude, and at the same time laments the insufficiency of the usual mode of notation. But surely there can be no greater difficulty in designating the apparent magnitude (or brilliancy) of the stars, than there is in designating or expressing magnitudes of any other kind: and there is no good reason why a ***whole*** magnitude (or what may be conceived, by any individual astronomer, to be a whole magnitude) should not be divided into as many fractional parts as may be found convenient or distinguishable. As the decimal system of notation, however, may be liable to be mistaken for, and confused with the ancient method, I have preferred the adoption of the common ***fraction***. In the present catalogue therefore, wherever Flamsteed has used 3·4, I have adopted $3\frac{1}{2}$; and wherever he has used 4·3, I have adopted $3\frac{3}{4}$: and thus with other cases where the author intended to express some fractional part of a ***whole*** mag-

* See the preceding note.

† Formerly the fractional parts of a second, in transit observations, used to be designated by certain conventional characters, under the impression that the second of time could not be accurately divided, in such observations. But this has at length given way to the more perfect system of decimal notation; and with how much advantage to the science the practical astronomer well knows.

nitude. Even these *quarter* magnitudes might be again divided, and we might thus have $3\frac{1}{8}$, $3\frac{3}{8}$, &c.: and although there may frequently be a doubt about the limits or degrees of magnitudes, yet with reference to any particular star as a standard, we have thus the means of recording in a concise manner all the apparent shades of difference, without the necessity of using so much circumlocution, or such a vague system of notation, as that which has been hitherto pursued.

Although (as I have already stated) I have neither time nor inclination to enter on the arduous task of attempting to reduce the observations of Flamsteed, agreeably to the refined modes of modern computation, yet I shall here endeavor to render such slight service, as I am able, to those who may devote themselves to this inquiry, or who may be desirous of examining any particular portion of such observations. These helps will consist of four tables, inserted at the end of this volume, which will enable the computer to investigate and ascertain the errors in the position and division of the mural arc, to determine the amount of precession, aberration, and nutation on any given day, and to obtain a more correct (though I still fear only an approximate) value for the refraction at various times of the year. I shall here give the description and use of these several tables.

Table K gives the position, for the epoch 1690, of all such principal stars in the *British Catalogue*, and of such of the minor ones, as have been observed by Flamsteed *ten* times and upwards, and as have also been since observed by Bradley and Pond. These positions are deduced agreeably to the following formula, given by M. Bessel in his *Fundamenta Astronomiæ*, page 136: viz.*

$$B - (P - B) \times \frac{13}{15} + (p - \pi) \times 101{\cdot}1$$

where P denotes the position of the star in Mr. Pond's last catalogue of 1112 stars, and the rest of the quantities as in page 387 By means of the stars thus reduced, amounting to 348 in number, the computer is almost sure to find, on any given day, one good determining star at least (if not more) as a normal point of comparison for his investigations. The annual precession for the same epoch, annexed to each star, has been determined by means of the following formula, given also by M. Bessel in the work above-mentioned: viz.

$$\pi - (p - \pi) \times 1{\cdot}444$$

* The position of *Polaris* is an exception: the right ascension and declination of this star have been deduced from the formula in page 306 of the same work.

And the annual proper motion of the star (which is inserted in a separate collateral column) is obtained by means of the following formula, deduced also from the same work of M. Bessel: viz.

$$\frac{1}{75}(P - B) - \frac{1}{3}(2p + \pi)$$

The amount of the annual variation will consequently be obtained by taking the *algebraical* sum of these two last-mentioned quantities.

Table L contains the sun's true longitude, deduced from Delambre's tables, for every 10th day of the year, on the supposition that its true longitude on January 1, 1690, at *midnight*, at Greenwich, *old style*, and according to the civil mode of reckoning,* is exactly 292° 0′ 0″. The values for any intermediate day may be readily obtained by allowing about 1° for each day's variation. These values are expressed in degrees, and decimal parts of a degree. In a collateral column I have given the corresponding number of days elapsed *since* midnight of January 1st; and, in the last column, have given the corresponding decimal fraction of the year for such elapsed portion.

Table M contains, first, the correction which should be applied to the sun's longitude in Table K in order to obtain the accurate value for any given year therein expressed; with a view of correcting (when so required) the error arising from the assumption of the sun's longitude being exactly 292°, which in fact is never the case. Secondly, the mean longitude of the moon's node on January 1st, at *midnight*, at Greenwich, *old style*, in the several years therein stated. At the bottom of this table is given the daily variation in the place of the moon's node.

By means of these three tables, the amount of the annual variation of any of the stars, for any given day, may be readily determined: as likewise the arguments for entering the tables of aberration and nutation.

Table N is inserted for the purpose of computing the refractions more accurately than by the method adopted by Flamsteed. It contains the mean height of the thermometer, for the several months of the year, deduced from the mean result of 10 years' observations made at the Royal Observatory, from 1818 to 1827; and the mean height of the barometer for the whole of such period. The

* That is, on Jan. 1st, at 12^h, I have preferred *midnight* to *noon*, because the major part of the observations were made nearer to the former hour than to the latter. The values were correctly computed for every 30th day; and the intermediate quantities obtained by interpolation. The results are sufficiently near, for the purpose intended.

mode pursued in forming this table was by taking out, from the printed volumes of the Observations, the height of the barometer and the exterior thermometer, on each day, as near to the hour of 9 o'clock in the evening as the same were registered. I apprehend that no great error can arise from the adoption of this table, especially if the star be not very near the horizon. The first numerical column shows the highest mean height for any given month for some *one* year out of the 10 years; and the second column shows the lowest mean height: the last column shows the general mean height for any given month, deduced from the mean of the whole 10 years; and is the value which should be used in seeking the amount of refraction from the tables. The mean height of the barometer for one year never differed more than ·05 from the general mean of the 10 years; and which has been found equal to 29·85 inches.

I shall close this Introduction with the following general remarks on the *British Catalogue*, suggested by the preceding history. That when the original copy of the catalogue was delivered into the hands of Sir Isaac Newton, it was expressly stated by Flamsteed and distinctly understood by all parties, that it was in many respects incomplete. That, during the time it was in Sir Isaac Newton's possession, Flamsteed, had not only increased the number of observations very considerably, the result of which he was willing to communicate; but had also discovered some errors in his former computations, which he was desirous of correcting. That the refusal of the Referees to print the catalogue in the manner originally proposed and assented to, and their clandestine publication of it with Dr. Halley's additions, annoyed him so much, that he declined having any further communication with them. That he then hastened to print his own catalogue, improved with his subsequent corrections, and the addition of several stars subsequently observed; in order to counteract, as speedily as possible, the effect of Halley's spurious edition. That the catalogue, thus hastily got up by Flamsteed, was at first circulated amongst his private friends only; and then, very sparingly: but has since been published by his editors (with the exception of a few re-printed sheets), in the third volume of the *Historia Cœlestis*, under the title of the *British Catalogue;* the expense of revising, re-modelling, and re-printing the whole being objected to by the executors. That, owing to these unfavorable circumstances, it has been deprived of many of those improvements and corrections contemplated by Flamsteed in the final arrangement; such as a more accurate classification of the stars under their proper constellations, the discarding of synonymes, the proper application of Bayer's letters, the correct statement of the relative magnitudes, and various

other improvements that would doubtless have rendered its state more perfect than that in which he left it; and which could not be effected so well as by the author himself. That, after Flamsteed's decease, these subjects occupied the deep attention and care of Mr. Sharp and Mr. Crosthwait, who could only partially accomplish their intended improvements, but that it is nevertheless to them, and to them alone, that the world is indebted for the final publication of the *British Catalogue*, as printed in the *Historia Cœlestis*: which therefore may, in some measure, be considered as a *posthumous* work.

F. B.

THE BRITISH CATALOGUE,

CORRECTED AND ENLARGED.

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1	36 Piscium	6	0° 9′ 40″	+ 6° 30′ 45″	+ 29,1	+ 13,3	5
2	37 Piscium	6	0 10 0	+12 10 55	+147,5		1
3	24 Andromedæ θ	4½	0 12 30	+36 57 10	+180,5	+ 18,7	1
4*	Piscium		0 15 —	+ 2 31 25	— *		1
5	38 Piscium	7	0 22 0	+ 7 8 25	+ 18,5	+ 20,4	2
6*	39 Piscium	8	0 25 30	+14 36 10	+109,3	+ 17,5	2
7	25 Andromedæ σ	5	0 31 0	+35 3 50	+173,0	— 1,0	1
8	26 Andromedæ	6	0 37 30	+42 4 25	+ 29,2	— 25,2	1
9	8 Ceti ι	3	0 54 0	—10 33 0	+ 25,4	+ 15,8	10
10	40 Piscium	6	0 58 0	+14 31 35	+101,4	+ 3,9	2
11	41 Piscium d	6	1 10 10	+ 6 27 45	+ 4,1	+ 11,4	9
12	27 Andromedæ ρ	5	1 11 0	+36 14 50	+184,3	+ 3,6	1
13	42 Piscium	6	1 33 45	+11 45 30	+174,6	0,0	1
14	9 Ceti	6	1 44 40	—13 56 10	+ 13,5	— 1,7	3
15	12 Cassiopeæ	6	2 1 0	+60 7 10	— 31,0	— 37,6	1
16	43 Piscium	6	2 7 30	+12 35 35	+131,9	+ 3,2	1
17*	Ceti		2 10 20	— 3 56 10			1
18	44 Piscium	6	2 22 45	+ 0 13 10	+ 13,9	0,0	7
19	45 Piscium	6	2 26 0	+ 5 58 35	+ 24,6	— 10,4	1
20*	Piscium	6¾	2 30 —	+18 26 15			1
21	10 Ceti	6	2 41 40	— 1 46 25	— 28,1	+ 13,0	6
22*	Andromedæ	5	2 55 30	+42 41 10			1
23*	46 Piscium	7	2 58 0	+17 47 55	+ 44,3	— 12,4	2
24	47 Piscium	6	2 58 30	+16 10 15	+ 58,3	+ 2,0	7
25	48 Piscium	6	3 2 0	+14 43 30	+ 35,8	+ 3,9	1
26	28 Andromedæ	6	3 26 30	+28 1 35	+103,2	+ 38,0	4
27	11 Ceti	6	3 30 30	— 2 50 10	+ 7,7	+ 15,1	1
28	13 Cassiopeæ	6	3 32 30	+64 48 15		— 7,6	1
29	12 Ceti	6	3 33 0	— 5 40 45	+ 25,2	+ 14,6	6
30*	Piscium		3 36 30	+ 3 8 50			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
31	49 Piscium	6	3° 41′ 15″	+14° 19′ 5″	— 1,9	+ 4,5	1
32	14 Cassiopeæ λ	5	3 43 30	+52 48 50	+ 32,0	— 30,8	3
33	15 Cassiopeæ κ	4	3 58 0	+61 13 20	—104,5	— 25,4	1
34*	Andromedæ		4 0 —	+31 51 20			1
35	51 Piscium	6	4 4 30	+ 5 14 25	+131,0	— 7,2	3
36*	52 Piscium	6	4 6 0	+18 35 25	+ 25,2	— 34,3	3
37	16 Cassiopeæ	6	4 13 0	+65 2 10	— 47,3	— 6,2	1
38	13 Ceti	6	4 49 30	— 5 18 35	+ 33,7	+ 16,2	7
39*	14 Ceti	6	4 54 0	— 2 12 50	+ 59,2		4
40*	Piscium	6	4 57 —	+11 30 30			1
41*	17 Cassiopeæ ζ	4	5 0 0	+52 11 30	— 37,0	— 26,1	8
42*	Andromedæ	6	5 1 15	+42 46 50			1
43	29 Andromedæ π	4½	5 4 0	+32 0 0	+168,2	+ 23,5	4
44*	Andromedæ	6	5 4 —	+33 40 10			1
45	53 Piscium	7	5 9 0	+13 32 0	+ 91,1	— 48,5	4
46*	Ursæ Minoris		5 15 0	+87 20 30			4
47*	Piscium	7	5 24 0	+ 1 25 25			3
48	30 Andromedæ ε	4	5 32 40	+27 36 45	+ 78,6	+ 18,6	4
49*	15 Ceti	7	5 34 0	— 2 13 20	— 16,6	+ 30,3	6
50	31 Andromedæ δ	3	5 41 30	+29 8 50	+104,0	+ 35,3	5
51*	Piscium		5 46 50	+ 1 23 35			1
52	18 Cassiopeæ α	3	5 49 0	+54 50 15	— 47,1	— 27,1	3
53*	54 Piscium	6	5 50 15	+19 35 5	— 59,5	— 69,9	1
54*	55 Piscium	6	5 54 10	+19 45 5	+ 74,7	— 74,4	5
55	32 Andromedæ	6	6 7 0	+37 44 35	+ 2,1	+ 25,5	1
56	19 Cassiopeæ ξ	6	6 13 45	+48 48 20	+ 84,7	— 0,2	1
57*	Piscium		6 18 25	+ 7 38 55			1
58*	33 Andromedæ	Neb.	6 28 0	+39 34 35			4
59	20 Cassiopeæ π	6	6 36 0	+45 19 30	+102,6	— 15,6	1
60*	21 Cassiopeæ	6	6 39 0	+73 16 40	—509,5	+ 26,1	1
61	22 Cassiopeæ ο	6	6 55 0	+46 35 30	+ 4,1	— 43,1	1
62*	16 Ceti β	3	6 59 50	—19 41 40	+ 33,8	+ 0,8	7
63*	17 Ceti ϕ¹	5	7 8 30	—12 19 15	— 17,7	+ 52,2	7
64*	23 Cassiopeæ	6	7 11 30	+73 8 20	—746,5	+ 24,5	1
65*	2 Ursæ Minoris	4½	7 24 —	+84 34 30	+ *	— 9,7	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
66	18 Ceti	6	7° 28′ 20″	−14° 34′ 5″	+ 12″,4	− 6″,2	3
67	57 Piscium	6	7 34 45	+13 47 15	+ 66,6	− 40,0	3
68	24 Cassiopeæ η	4	7 40 30	+56 9 50	+119,4	− 48,0	2
69*	58 Piscium	6	7 42 30	+10 16 30	+ 75,2	− 4,1	5
70*	59 Piscium	6	7 44 30	+17 53 10	− 25,2	− 33,4	1
71	34 Andromedæ ζ	4	7 45 0	+22 33 55	− 7,8	+ 23,9	3
72*	60 Piscium	6	7 51 0	+ 5 2 40	+ 4,2	− 12,3	3
73	25 Cassiopeæ ν	5	7 51 0	+49 16 30	+102,6	− 19,5	1
74	61 Piscium	7	7 52 30	+19 14 0	+ 64,1	− 30,9	2
75*	Piscium		8 3 —	+ 3 40 10			1
76	62 Piscium	6	8 4 0	+ 5 37 10	− 3,0	− 9,4	10
77	63 Piscium δ	4	8 10 0	+ 5 53 5	− 14,1	+ 15,0	47
78	64 Piscium	6	8 10 0	+15 14 25	+ 87,1	+ 55,2	2
79	35 Andromedæ ν	4	8 11 0	+39 22 55	+146,5	− 0,4	4
80*	1 Ursæ Minoris α	3	8 17 30	+87 38 40	+664,8	− 12,6	87
81*	Piscium	6	8 18 —	+27 1 30			1
82*	65 Piscium i	6	8 19 10	+26 0 40	+ 76,3	+ 8,4	3
83*	19 Ceti ϕ^3	6	8 39 20	−12 19 35	− 35,4	+ 3,0	5
84*	Piscium	7	8 49 —	+ 1 42 —			2
85*	26 Cassiopeæ υ^1	7	9 14 30	+57 17 30	− 15,5		1
86	20 Ceti	6	9 18 20	− 2 50 20	− 28,7	+ 8,5	15
87	66 Piscium	6	9 32 30	+17 30 0	+ 83,6	− 7,3	2
88*	27 Cassiopeæ γ	3	9 35 30	+59 2 0	+ 16,3	− 21,8	2
89*	28 Cassiopeæ υ^2	6	9 38 30	+57 30 20			1
90*	36 Andromedæ	6	9 38 30	+21 56 40	− 81,2	− 13,7	2
91*	21 Ceti	6	9 40 0	−10 25 35	− 8,1	− 6,0	2
92*	67 Piscium k	6	9 49 45	+25 31 0		+ 10,1	2
93*	37 Andromedæ μ	3½	9 52 30	+36 48 25	+206,0	+ 6,0	5
94	22 Ceti ϕ^3	5	10 7 30	−12 57 25	− 20,1	+ 9,6	3
95*	38 Andromedæ η	4½	10 13 0	+21 44 25	−109,2	− 23,6	4
96	68 Piscium h^1	6	10 16 0	+27 18 10	+103,7	+ 16,1	2
97*	23 Ceti ϕ^4	6	10 48 20	−13 3 45	− 14,0	+ 0,2	3
98*	39 Andromedæ	6	11 25 0	+39 40 15	− 1,5	− 12,1	1
99*	69 Piscium σ^1	5	11 29 30	+30 7 55	+ 9,3	− 11,9	5
100*	70 Piscium	6	11 31 40	+ 6 15 10	− 57,5		1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					☊R.	D.	
			° ′ ″	° ′ ″	″	″	
101	71 Piscium ε	4	11 43 30	+ 6 12 20	— 8,4	+ 22,8	61
102	25 Ceti	6	11 53 0	— 6 30 5	—143,2	— 16,7	4
103*	Piscium	6	11 56 45	+27 59 50			1
104*	30 Cassiopeæ μ	5	11 59 0	+53 23 40	+ *	— *	1
105*	26 Ceti	6	12 0 0	— 0 18 30	— 86,1	+ 10,7	1
106	72 Piscium	6	12 11 10	+13 16 0	+ 46,2	+ 10,5	3
107*	73 Piscium	6	12 12 50	+ 3 58 5	+ 3,1	+ 55,1	12
108	74 Piscium ψ^1	5	12 16 15	+19 47 45	+ 64,6	+ 23,2	4
109*	76 Piscium σ^2	5	12 18 40	+30 30 35	+ 20,0	+ 3,4	2
110	77 Piscium	6	12 25 40	+ 3 15 5	+105,9	— 24,4	3
111	27 Ceti	6	12 31 20	—11 38 45	+ 0,6	— 11,0	2
112	31 Cassiopeæ	6	12 32 30	+67 7 0	+135,8	— 10,7	4
113*	75 Piscium	6	12 33 30	+11 17 10	+ 75,3	— 8,6	2
114*	41 Andromedæ	5	12 37 0	+42 17 10	— 28,5	— 32,6	1
115	28 Ceti	6	12 38 10	—11 30 50	— 1,9	+ 10,4	4
116	78 Piscium	6	12 45 0	+30 20 45	+ 20,7	+ 0,9	2
117	79 Piscium ψ^2	6	12 50 10	+19 4 35	+ 69,1	+ 3,6	3
118*	42 Andromedæ ϕ	5	12 57 0	+45 34 55	— 50,7	— 21,3	1
119*	Cassiopeæ	6	12 58 —	+62 32 45			2
120*	32 Cassiopeæ	6	13 0 0	+63 22 0	+ 9,8	— 38,9	5
121	29 Ceti	6	13 1 30	+ 0 21 35	— 39,1	— 15,7	1
122*	30 Ceti	6	13 3 0	—11 27 25	— 0,7	+ 10,6	4
123	43 Andromedæ β	2	13 5 0	+33 57 20	+187,1	+ 24,0	5
124	80 Piscium e	5	13 6 30	+ 3 59 45	— 5,4	— 0,3	23
125	33 Cassiopeæ δ	4	13 8 30	+53 28 30	+ 3,8	+ 43,8	1
126	44 Andromedæ	6	13 14 0	+40 25 50	— 40,3	— 36,2	1
127	31 Ceti η	3	13 15 30	—11 50 35	— 14,6	+ 15,0	7
128	81 Piscium ψ^3	6	13 19 10	+17 59 30		+ 8,0	2
129*	Piscium	6½	13 26 40	+14 0 45			2
130	45 Andromedæ	5½	13 29 0	+36 4 15	— 14,7	— 33,7	1
131	82 Piscium g	6	13 31 15	+29 45 30	+ 58,2	+ 18,0	4
132*	83 Piscium τ	5	13 39 30	+28 25 35	+ 80,0	+ 15,4	5
133	32 Ceti	6	13 40 0	—10 33 55	— 6,4	— 2,0	2
134	33 Ceti	6	13 40 30	+ 0 47 5	— 50,0	— 2,0	1
135	84 Piscium χ	5	13 42 10	+19 22 13	+ 68,8	+ 13,5	5

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
136*	Piscium		13° 48′ 20″	+30° 24′ 55″	″	″	1
137	34 Ceti	6	14 2 10	− 3 54 30	−147,0	− 3,1	1
138	35 Ceti	6	14 10 40	+ 0 49 25	− 64,1		1
139	85 Piscium ϕ	5	14 15 10	+22 55 45	+ 12,9	− 0,9	2
140	36 Ceti	6	14 18 0	− 8 26 35	− 2,1	+ 11,8	1
141*	Cassiopeæ	7	14 22 —	+66 9 35			1
142*	86 Piscium ξ	5	14 23 30	+ 5 55 15	+ 26,1	+ 9,4	40
143*	Piscium		14 24 0	+ 5 55 20	+ 19,5	+ 14,3	2
144	87 Piscium	7	14 25 20	+14 28 50	+ 43,0	− 3,5	2
145*	Ceti	7	14 35 30	− 9 16 25			1
146	88 Piscium	6½	14 39 0	+ 5 20 5	+ 30,0	+ 30,4	9
147*	37 Ceti	6	14 42 0	− 9 36 25	+ 11,3	+ 40,4	6
148*	38 Ceti	6	14 46 0	− 2 37 20	− 36,9	− 68,4	1
149*	Ceti		14 54 —	−17 26 15			1
150*	35 Cassiopeæ	7	15 11 0	+63 1 40	+153,4	− 43,4	1
151	39 Ceti	6	15 13 30	− 4 8 40	− 12,0	− 3,2	1
152*	34 Cassiopeæ ϕ	6	15 14 30	+56 35 30	− 18,1	− 18,6	2
153	40 Ceti	6	15 17 0	− 3 54 50	+ 25,3	− 17,0	2
154	89 Piscium *f*	6	15 28 0	+ 1 57 55	− 17,0	+ 14,3	16
155*	41 Ceti	7	15 32 0	− 9 18 30	+ 12,4	+ 1,5	1
156	90 Piscium υ	5	15 36 50	+25 37 5	+ 83,1	+ 8,3	2
157	91 Piscium *l*	6	16 0 30	+27 6 10	+ 66,9	+ 1,4	3
158	42 Ceti	6	16 0 45	− 2 9 5	− 57,5	+ 3,7	2
159*	46 Andromedæ ξ	4½	16 5 0	+43 53 20	− 17,2	+ 4,2	2
160	36 Cassiopeæ ψ	5½	16 10 0	+66 30 0	+ 20,7	− 20,4	4
161	37 Cassiopeæ δ	3	16 30 0	+58 37 0	− 11,3	− 38,6	3
162	47 Andromedæ	6	16 32 0	+36 5 5	− 12,2	− 12,4	1
163	43 Ceti	6	16 42 0	− 2 5 15	− 34,0	+ 11,6	2
164	92 Piscium	7	16 47 30	+16 11 15	+103,2	+ 1,1	2
165	44 Ceti	6	17 4 50	− 9 37 55	+146,0	− 12,8	2
166	45 Ceti θ	3	17 8 0	− 9 48 15	+ 2,6	+ 13,7	9
167*	38 Cassiopeæ *A*	6	17 15 0	+68 37 0	− 44,5	+111,4	1
168*	48 Andromedæ ω	5	17 21 0	+43 47 20	− 26,4	− 3,8	2
169	93 Piscium ρ	5	17 24 0	+17 32 20	+ 92,7	+ 17,6	4
170*	94 Piscium	5	17 30 10	+17 36 50	+ 31,2	+ 13,6	4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
171	46 Ceti	5	17° 36′ 0″	−16° 13′ 25″	− 16,3	− 7,0	2
172	47 Ceti	6	17 54 0	−14 40 55	− 29,2	+ 4,1	2
173	95 Piscium	7	17 54 10	+ 3 44 45	+ 7,7	− 25,0	2
174*	49 Andromedæ *A*	5	17 57 30	+45 23 15	− 77,0	+ 11,2	2
175	96 Piscium	6½	18 13 30	+ 5 40 10	+ 21,1	+ 32,9	2
176	97 Piscium	6½	18 17 40	+16 44 5	+ 56,6		2
177	98 Piscium μ	5	18 29 30	+ 4 31 35	+ 42,9	+ 23,9	18
178	39 Cassiopeæ χ	6	18 31 30	+57 37 50	− 35,8	− 29,9	2
179*	48 Ceti	6	18 40 0	−23 14 45	+ 51,0		3
180*	40 Cassiopeæ	6	18 41 0	+71 26 30	− 18,6	− 18,9	2
181	99 Piscium η	4	18 44 30	+13 43 40	+ 6,9	+ 15,5	38
182*	Piscium		19 0 45	+10 17 5			1
183	100 Piscium	6	19 36 30	+10 57 30	+ 29,5	− 11,8	1
184*	50 Andromedæ	5¾	19 42 0	+39 50 10	− 17,8	− 20,6	3
185*	Ceti		19 44 —	− 0 39 0			1
186	101 Piscium	6	19 48 0	+13 3 40	+ 67,1	− 3,8	4
187*	51 Andromedæ	3½	19 48 40	+47 2 40	− 17,1	− 25,2	4
188	49 Ceti	6	19 52 30	−17 16 45	− 1,7	− 1,6	2
189*	42 Cassiopeæ	6	19 57 30	+69 1 50	− 99,4	+ 5,8	5
190*	43 Cassiopeæ ω	6	20 1 0	+66 26 50	+ 7,6	+ 17,2	2
191*	102 Piscium π	5	20 10 40	+10 32 15	+ 15,5	+ 11,7	23
192*	50 Ceti	6	20 13 30	−16 59 45	− 28,7	− 17,2	2
193*	52 Andromedæ χ	6	20 15 0	+42 47 50	− 23,5	− 21,3	1
194	53 Andromedæ τ	5	20 37 30	+38 59 20	− 29,9	+ 1,5	2
195	103 Piscium	7¾	20 38 30	+15 1 40	+ 61,4	+ 26,2	2
196	104 Piscium	6½	20 40 0	+12 40 45	+ 55,3	+ 58,8	3
197	44 Cassiopeæ	6	20 42 30	+58 58 20		− 20,3	1
198	105 Piscium	6½	20 45 0	+14 48 30	+ 41,1	+ 27,2	3
199*	Andromedæ υ	4	20 47 0	+41 2 30			1
200*	1 Trianguli	6	21 3 0	+27 56 10	+ •		1
201*	54 Andromedæ	4	21 8 40	+49 6 50	− 64,8	− 24,6	5
202*	106 Piscium ν	5	21 20 10	+ 3 53 55	+ 2,2	+ 15,7	23
203*	Cassiopeæ	6	21 26 —	+62 18 20			1
204*	2 Arietis	6	21 25 45	+18 44 35	+ 21,0	− 41,1	7
205*	109 Piscium	6½	22 1 0	+18 31 0	+ 22,8	− 7,3	4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					ÆR	D.	
206	110 Piscium o	5	22° 16′ 30″	+ 7° 34′ 35″	− 12″,0	+ 19″,3	19
207	52 Ceti τ	3½	22 25 20	−17 35 0	−130,0	+ 48,8	2
208*	3 Arietis	6	22 26 15	+15 50 25	+ 96,0	+ 6,6	3
209*	4 Arietis	6¾	22 51 15	+15 23 5	+ 35,7	+ 27,0	6
210	1 Persei	6	23 0 30	+53 35 40	− 58,3	− 13,7	1
211	45 Cassiopeæ ε	3	23 7 0	+62 7 40	+141,9	− 35,5	2
212*	46 Cassiopeæ	6	23 10 0	+67 8 10	− 13,5	− 2,3	3
213*	2 Persei g	6	23 12 30	+49 14 15	− 98,5	+ 3,4	1
214*	1 Arietis	6¾	23 16 0	+20 42 45			1
215	53 Ceti χ	5	23 35 0	−12 14 25	+ 28,7	+ 7,6	2
216*	54 Ceti	6	23 37 0	+ 9 29 35	+ 13,8	− 11,1	1
217	55 Andromedæ	Neb.	23 44 0	+39 10 45	− 37,8	− 0,4	1
218	2 Trianguli α	4	23 50 30	+28 3 0	+157,8	− 21,4	5
219*	47 Cassiopeæ	6½	24 0 0	+75 45 30	−203,8	− 13,7	1
220	55 Ceti ζ	3	24 3 0	−11 53 15	− 17,3	+ 17,3	3
221	5 Arietis γ	4	24 8 30	+17 45 15	+ 47,2	− 5,8	71
222*	Cassiopeæ		24 19 10	+63 5 50			1
223*	48 Cassiopeæ	5	24 20 0	+69 23 10	− 2,7	− 43,8	3
224	111 Piscium ξ	6	24 23 30	+ 1 38 0	− 9,1	+ 29,0	10
225	6 Arietis β	3	24 23 30	+19 16 5	+ 51,6	+ 10,2	75
226	49 Cassiopeæ	6	24 24 0	+74 35 10	−236,3	+ 8,8	2
227	56 Andromedæ	6	24 29 0	+35 42 25	+ 9,2	+ 9,7	1
228*	50 Cassiopeæ	4½	24 29 0	+70 53 40	+ 6,2	− 13,3	3
229*	7 Arietis	7	24 39 45	+22 2 15	+ 23,5	− 1,6	4
230	3 Persei	6½	24 48 0	+47 40 0	− 63,8	+ 2,2	1
231*	51 Cassiopeæ		24 55 0	+73 4 0	−615,6	− 17,0	1
232*	52 Cassiopeæ	7	25 4 0	+63 22 40	+ 73,9	− 3,4	1
233	8 Arietis ι	6	25 7 0	+16 16 45	+ 34,3	+ 17,7	12
234*	53 Cassiopeæ	7	25 7 30	+62 52 40	+ 98,4	− 46,1	1
235	9 Arietis λ	5	25 11 0	+22 3 35	+ 21,3	+ 13,7	14
236	4 Persei	6	25 31 0	+52 58 0	− 60,3	− 9,0	1
237*	56 Ceti	6	25 32 30	−24 3 25	− 11,4	− 3,0	1
238*	58 Ceti	7	25 34 30	− 3 36 0		+ 27,3	1
239*	Arietis	6	25 36 20	+19 31 35			1
240*	Arietis		25 42 20	+10 46 15			2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR	D.	
241*	54 Cassiopeæ	7	25° 52′ 0″	+70° 3′ 20″	— 77,1	+ 13,8	1
242	112 Piscium	6½	26 1 0	+ 1 35 25	+ 17,6	+ 1,0	12
243	3 Trianguli ε	6	26 13 0	+31 46 15	+120,3	— 11,3	2
244	57 Andromedæ γ	2½	26 17 30	+40 48 55	— 73,2	+ 9,5	4
245*	57 Ceti	6	26 18 0	—22 20 45	— 8,4	— 7,3	1
246*	59 Ceti υ	4½	26 21 0	—22 35 35	+ 10,5	— 19,8	2
247*	113 Piscium α	3	26 30 45	+ 1 14 40	+ 2,0	+ 9,6	27
248	10 Arietis	6½	26 32 30	+24 24 55	+ 19,5	+ 19,3	4
249*	Piscium		26 44 10	+ 1 49 45			1
250	60 Ceti	6	26 50 15	— 1 23 45	— 4,7	+ 38,4	2
251	61 Ceti	7	26 59 50	— 1 51 30	— 4,7	+ 42,0	2
252*	11 Arietis	6	27 19 30	+24 11 45	+ 67,1	+ 27,9	5
253	12 Arietis κ	5¾	27 19 30	+21 8 55	+ 24,0	— 2,4	4
254*	13 Arietis α	2	27 26 30	+21 58 15	+ 50,0	+ 2,6	77
255	58 Andromedæ	6	27 30 0	+36 21 40	— 21,7	+ 12,0	1
256*	5 Persei h	6	27 39 0	+56 9 20	—265,8	— 10,5	3
257*	55 Cassiopeæ	6	27 40 0	+65 2 10	+116,0	+ 11,6	1
258	4 Trianguli β	4	27 46 30	+33 30 0	+152,0	— 12,9	4
259	14 Arietis	6	27 57 30	+24 26 45	+ 63,8	+ 14,7	8
260	59 Andromedæ	6	28 4 30	+37 33 0	— 34,3	+ 6,3	1
261*	6 Persei	6	28 20 0	+49 36 0	— 9,1	— 16,6	2
262*	5 Trianguli	7	28 20 0	+30 2 30	+101,1	— 0,6	2
263	62 Ceti	6½	28 22 0	— 3 49 40	+ 28,3	+ 29,9	2
264	15 Arietis	6	28 22 30	+18 1 15	+ 40,3	— 20,4	4
265*	16 Arietis	7	28 24 45	+24 27 20	+ 28,6	— 14,2	4
266	60 Andromedæ b	6	28 31 0	+42 45 10	— 57,6	— 3,5	1
267*	6 Trianguli	6	28 36 20	+28 49 40	+ 84,6	— 9,4	2
268	64 Ceti	6	28 45 45	+ 7 6 0	— 8,3	— 18,6	5
269	17 Arietis η	6	28 52 30	+19 43 35	+ 55,3	+ 17,8	13
270*	63 Ceti	6	28 58 30	— 3 18 45	— 8,1	+ 32,8	2
271*	18 Arietis	7	28 58 45	+18 26 45			1
272	19 Arietis	6½	29 1 30	+13 47 45	+124,3	+ 33,9	2
273*	65 Ceti ξ¹	6	29 9 0	+ 7 22 20	+ 30,8	— 4,6	11
274*	7 Persei χ	6¾	29 9 30	+56 3 0	+ 21,2	— 1,0	2
275*	8 Persei	7	29 12 0	+56 26 15	—294,0	— 16,9	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
276	66 Ceti	6	29° 14′ 15″	− 3° 52′ 5″	+ 55,1	+ 9,6	2
277*	7 Trianguli	6	29 24 10	+31 53 30	+128,4	+ 2,4	4
278*	61 Andromedæ	6	29 31 0	+55 40 40		− 16,6	1
279*	Andromedæ		29 33 0	+55 43 —			1
280	20 Arietis	6	29 31 30	+24 18 55	+ 46,9	+ 14,7	3
281	21 Arietis	7	29 31 30	+23 34 30	+119,3	+ 10,8	2
282	8 Trianguli δ	5	29 32 10	+32 46 55	+226,2	− 23,6	4
283	9 Trianguli γ	4	29 43 30	+32 23 30	+127,9	− 16,8	5
284*	62 Andromedæ c	6	29 55 0	+45 55 15	−130,8	+ 6,5	1
285	63 Andromedæ	6	30 13 0	+48 41 55	−120,5	+ 8,8	2
286*	10 Trianguli	6	30 13 45	+27 11 20	+179,7	− 7,1	4
287	22 Arietis β	$5\frac{3}{4}$	30 14 0	+18 26 30	+ 39,4	+ 6,2	14
288	9 Persei i	6	30 18 30	+54 24 30	−109,0	− 41,2	2
289	67 Ceti	6	30 22 0	− 7 52 40	+ 83,5	+ 16,0	3
290*	23 Arietis	7	30 29 30	+18 14 35	+ 35,3	− 2,6	5
291	68 Ceti o	$2\frac{3}{4}$	30 55 30	− 4 24 35	+ 25,2	− 9,0	7
292*	Cassiopeæ	5	30 59 30	+65 58 50	−227,8	− 25,9	2
293*	10 Persei	7	31 0 30	+55 11 10	−244,7	− 50,5	1
294	64 Andromedæ	6	31 4 30	+48 34 10	−176,1	+ 4,4	4
295	65 Andromedæ	5	31 21 30	+48 50 35	−180,5	+ 12,8	4
296	69 Ceti	6	31 32 0	− 1 3 10	− 28,0	+ 36,8	2
297	70 Ceti	6	31 36 0	− 2 19 40	+ 50,0	+ 30,6	4
298*	Arietis	$6\frac{1}{2}$	31 37 30	+ 9 24 50			3
299	66 Andromedæ	$6\frac{1}{2}$	31 53 30	+49 9 35	−187,6	− 27,6	3
300*	24 Arietis ξ	6	32 1 30	+ 9 11 45	+154,7	− 45,7	21
301*	11 Trianguli	7	32 16 30	+30 22 45	+126,9	+ 11,0	1
302	71 Ceti	6	32 19 45	− 4 12 40	+ 23,2	+ 16,1	4
303*	12 Trianguli	6	32 31 0	+28 15 45	+ 54,4	− 18,1	3
304*	Arietis		32 31 50	+ 9 9 5	+ 74,2	− 18,5	2
305	13 Trianguli	7	32 40 0	+28 30 40	+ 77,5	+ 4,0	1
306*	Cassiopeæ	6	32 40 45	+71 25 0	− *	− 10,8	1
307	25 Arietis	7	32 42 30	+ 8 47 35	+101,2	+ 6,1	7
308	72 Ceti ρ	4	32 44 30	−13 42 45	+ 25,6	+ 8,7	2
309	73 Ceti ξ^2	$4\frac{3}{4}$	32 55 30	+ 7 2 25	+ 39,5	+ 21,9	14
310*	Ceti		33 16 5	+ 8 9 10			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
311	26 Arietis	6½	33° 20′ 0″	+18° 27′ 0″	+ 26″,3	+ 7″,5	4
312	27 Arietis	6½	33 26 0	+16 18 5	+ 62,8	+ 15,8	4
313	14 Trianguli	6	33 27 0	+34 44 30	−398,9	+ 10,0	1
314*	28 Arietis	6	33 33 20	+18 28 40	— *		1
315	29 Arietis	6½	33 59 15	+13 37 45	+ 49,9	+ 29,8	8
316	75 Ceti	5½	34 6 15	− 2 26 20	+ 2,8	+ 40,9	4
317	15 Trianguli	7	34 13 45	+33 18 20	+188,5	− 1,5	1
318	76 Ceti σ	4	34 21 0	−16 37 35	+ 5,9	− 15,4	3
319*	30 Arietis	6	34 48 0	+23 16 30	− 8,9		4
320	77 Ceti	6	34 51 0	− 9 14 35	+ 45,0	+ 20,2	2
321	78 Ceti ν	4½	34 54 30	+ 4 13 40	+ 31,3	− 45,4	2
322	31 Arietis	5½	34 55 30	+11 4 20	+111,5	+ 12,3	11
323*	79 Ceti	8	34 56 45	− 4 54 50	+ 32,5	+ 8,2	2
324	80 Ceti	6	35 11 0	− 9 12 40	+ 32,1	+ 24,9	2
325	32 Arietis ν	6	35 19 30	+20 35 15	+ 13,9	+ 20,5	14
326	11 Persei	7	35 21 30	+53 45 0	−198,4	− 8,6	1
327	81 Ceti	6	35 31 30	− 4 46 10	+ 16,6	+ 21,5	3
328*	Ceti		35 36 20	+ 9 16 5			1
329	33 Arietis	5	35 39 30	+25 41 55	+ 53,5	− 41,9	5
330*	12 Persei	6	35 43 7	+38 51 0	− 3,4	− 4,7	1
331	13 Persei θ	4	35 49 0	+47 53 15	+ 59,5	− 17,7	4
332	82 Ceti δ	3	35 55 0	− 1 2 25	− 27,4	+ 25,4	11
333	14 Persei	6	36 5 0	+42 57 45	−191,6	− 54,7	1
334	83 Ceti ε	3	36 9 30	−13 12 35	− 24,0	− 22,6	3
335	34 Arietis μ	6	36 14 15	+18 39 45	+ 33,5	− 2,0	13
336	35 Arietis	4	36 19 30	+26 21 35	+ 78,6	− 3,8	5
337	84 Ceti	6	36 20 45	− 2 3 0	+ 40,2	+ 30,9	4
338*	Ceti		36 21 5	+ 9 12 40	− 10,5		2
339*	Arietis		36 27 —	+24 17 55			1
340	85 Ceti	6	36 28 30	+ 9 24 20	− 33,3	− 44,8	3
341*	Persei	8	36 41 50	+55 42 50			1
342	36 Arietis	7	36 45 45	+16 25 5	+ 51,1	+ 18,6	4
343*	Persei	6	36 45 50	+55 45 15			1
344	86 Ceti γ	3	36 49 30	+ 1 53 55	− 19,0	+ 10,3	11
345	37 Arietis ο	6	36 51 45	+13 58 5	+ 88,9	+ 12,6	20

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
346*	38 Arietis	6	37° 1′ 0″	+11° 6′ 35″	+ 75,5	+ 10,3	4
347	87 Ceti μ	4	37 3 20	+ 8 46 15	+ 47,7	+ 25,4	12
348*	15 Persei η	4	37 8 0	+54 34 30			1
349	89 Ceti π	$3\frac{3}{4}$	37 21 0	−15 11 45	− 5,3	0,0	3
350	39 Arietis	4	37 22 0	+27 55 45	+114,5	− 7,7	3
351	16 Trianguli	7	37 39 30	+23 51 55	+202,2	− 1,5	2
352*	1 Eridani τ^1	4	37 39 50	−19 54 25	+ 18,7	+ 2,3	3
353*	Ceti	$5\frac{1}{2}$	37 46 —	+ 3 22 35	− *		1
354*	16 Persei	4	37 46 30	+37 0 30	+ 96,8	− 3,2	4
355	40 Arietis	6	37 48 45	+16 57 25	0,0	+ 20,0	2
356	41 Arietis	3	37 56 45	+25 56 55	+ 74,2	+ 7,5	6
357	42 Arietis π	6	38 0 50	+16 8 25	+ 23,3	+ 18,8	28
358*	17 Persei	$5\frac{1}{2}$	38 6 30	+33 45 20	+123,3	− 13,7	2
359*	18 Persei τ	5	38 8 0	+51 27 30	+ 49,5	− 2,7	6
360*	20 Persei	6	38 34 0	+37 2 30	+ 63,2	− 5,6	3
361	43 Arietis σ	6	38 35 45	+13 46 5	+ 71,5	+ 28,7	23
362	2 Eridani τ^2	4	39 15 0	−22 18 15	− 9,6	+ 5,1	1
363*	44 Arietis ρ^1	$6\frac{1}{2}$	39 23 45	+16 26 35	+ 48,0	+ 4,0	4
364*	45 Arietis ρ^2	$6\frac{1}{2}$	39 36 30	+17 2 25	+ 40,3	+ 18,2	14
365*	21 Persei	5	39 39 0	+30 39 20	+ 10,1	− 8,5	2
366*	46 Arietis ρ^3	$6\frac{1}{2}$	39 44 45	+16 45 5	+ 55,6	+ 5,0	16
367*	Persei	6	39 45 —	+51 4 35			1
368*	22 Persei π	5	39 46 0	+38 23 30	+ 50,7	− 15,8	4
369*	Ceti	6	39 55 10	+ 7 6 5	+ 34,6	+ 9,8	1
370*	Cassiopeæ	5	39 58 15	+73 8 45		+ 34,7	1
371*	24 Persei	$4\frac{1}{2}$	40 0 0	+33 55 10			2
372	47 Arietis	$6\frac{1}{2}$	40 6 0	+19 23 25	+ 54,2	+ 12,2	4
373*	Arietis		40 18 10	+20 20 35	+ 4,4		1
374	3 Eridani η	3	40 20 0	−10 9 35	− 13,3	+ 7,1	1
375	48 Arietis ε	5	40 23 30	+20 3 45	+ 13,0	− 26,8	24
376	23 Persei γ	3	40 39 0	+52 15 40	+ 46,9	− 27,7	3
377*	Persei	7	40 40 55	+31 8 50			1
378*	Persei k	6	40 41 25	+55 27 0			1
379*	91 Ceti λ	5	40 46 50	+ 7 38 15	+ 31,9	+ 18,9	13
380*	50 Arietis	7	40 47 0	+16 44 5	+ 51,3	+ 34,9	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
381	4 Eridani	6	40° 53′ 30″	−25° 7′ 45″	+ 61″,3	+ 3″,3	1
382*	49 Arietis	6	40 56 45	+25 11 35	− 25,4	+ 40,4	3
383	5 Eridani	6	41 2 45	− 3 43 35	− 95,2	− 0,8	1
384	51 Arietis	7	41 3 0	+25 21 35		+ 18,7	3
385	6 Eridani	6	41 3 45	−24 52 25	+ 67,2	+ 9,2	1
386	7 Eridani	6	41 19 45	− 4 8 5	− 41,3	+ 8,0	1
387	25 Persei ρ	4	41 21 30	+37 36 20	+ 60,7	− 13,1	4
388*	8 Eridani ρ^1	6	41 29 20	− 8 54 30	+ 9,2	− 14,3	1
389	92 Ceti α	2	41 31 50	+ 2 50 25	+ 5,8	+ 14,6	21
390*	93 Ceti	7	41 33 0	+ 3 5 35	+ 9,2	+ 31,4	9
391*	Persei ι	4	41 43 0	+48 23 30			2
392	52 Arietis	6	41 50 45	+24 0 15	− 6,4	+ 43,8	4
393*	9 Eridani ρ^2	5	41 53 20	− 8 56 10	− 26,0	+ 15,9	1
394	26 Persei β	2½	42 2 15	+39 43 10	+ 34,2	+ 20,1	5
395	27 Persei $\varkappa$	4¾	42 11 0	+43 39 10	+ 80,2	− 37,0	1
396*	11 Eridani τ^3	3½	42 12 0	−24 51 40	− 64,6	− 12,3	1
397*	10 Eridani ρ^3	5	42 16 30	− 8 51 10	− 11,5	+ 47,1	2
398	53 Arietis	7	42 30 30	+16 38 45	+ 26,2	+ 23,9	5
399	54 Arietis	6½	42 42 15	+17 33 45	+ 54,4	+ 39,1	3
400	55 Arietis	7	42 46 45	+27 51 10	− 25,1	+ 24,5	3
401	28 Persei ω	5	42 50 30	+38 23 40	+ 77,3	+ 11,3	3
402*	Arietis		42 55 —	+19 32 5			1
403*	56 Arietis	6½	43 28 30	+26 3 15	− 12,3	+ 0,1	2
404	57 Arietis δ	4	43 29 15	+18 31 10	+ 48,2	+ 6,4	20
405*	Arietis		43 29 45	+18 9 20	− *		1
406	29 Persei	6	44 12 30	+49 2 50	− 16,6	− 7,1	1
407	30 Persei	6	44 15 0	+42 51 10	+126,9	− 20,3	1
408	94 Ceti	6	44 15 20	− 2 23 30	− 12,6	+ 21,3	1
409	58 Arietis ζ	5	44 17 0	+19 51 15	+ 38,0	+ 25,9	13
410*	31 Persei ι	5½	44 19 30	+48 55 30	+ 28,5	− 15,5	1
411	12 Eridani	3	44 43 30	−30 13 35	+ 39,0	+ 16,3	2
412	32 Persei l	6	45 11 0	+42 10 10	+123,9	+ 3,3	2
413	13 Eridani ζ	3	45 12 20	−10 0 5	− 7,4	+ 12,1	3
414	59 Arietis	7	45 23 30	+25 55 20	− 34,8	− 29,6	2
415	14 Eridani	6	45 24 20	−10 19 25			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					℞.	D.	
			° ′ ″	° ′ ″	″	″	
416*	Arietis	6½	45 25 50	+27 53 0			1
417	60 Arietis	7	45 32 15	+24 30 35	+ 9,9	+ 0,2	4
418	33 Persei α	2½	45 37 0	+48 43 5	— 16,6	— 8,2	5
419	95 Ceti	6	45 38 45	— 2 5 55	— 8,8	+ 32,9	2
420*	96 Ceti $\varkappa^1$	5	45 47 0	+ 2 11 55	+ 37,2	+ 29,7	3
421	61 Arietis τ^1	7	45 51 0	+19 59 15	+ 23,0	+ 29,6	10
422	62 Arietis	6	45 56 0	+26 27 15	— 28,7	+ 17,2	3
423*	15 Eridani	6	46 9 50	—23 40 5	+ 24,8	— 6,6	1
424*	97 Ceti $\varkappa^2$	5½	46 14 0	+ 2 31 20	+ 19,5	+ 23,4	2
425*	63 Arietis τ^2	6	46 15 0	+19 35 25	+ 4,7	+ 30,4	10
426*	16 Eridani τ^4	4	46 26 0	—22 54 45	+ 18,4	+ 1,9	2
427	64 Arietis	6	46 31 15	+23 35 35	+ 24,2	— 9,7	3
428	65 Arietis	7	46 38 30	+19 39 35	+ 78,1	+ 32,3	3
429	34 Persei	6	46 51 30	+48 22 55	+ 16,5	+ 38,4	1
430	1 Tauri o	4	47 2 0	+ 7 54 15	+ 48,5	+ 0,8	15
431	35 Persei σ	5	47 14 30	+46 53 10	— 17,7	— 10,7	1
432	2 Tauri ξ	4	47 35 30	+ 8 36 55	+ 63,8	+ 12,8	18
433	66 Arietis	7	47 36 0	+21 41 55	+ 15,0	+ 2,2	3
434	36 Persei	6½	47 47 30	+44 58 10	+ 2,2	— 24,4	1
435	4 Tauri s	6	48 22 10	+10 14 15	+ 44,8	+ 7,5	8
436*	5 Tauri f	5	48 27 0	+11 50 15	+ 25,9	+ 13,2	13
437	37 Persei ψ	5	48 41 0	+47 7 20	— 39,3	— 15,8	2
438	17 Eridani	4½	48 49 0	— 6 10 10	+ 8,1	+ 6,0	3
439	6 Tauri t	6	48 57 40	+ 8 17 25	+ 41,5	+ 6,2	5
440	7 Tauri	6	49 2 0	+23 23 20	+ 44,7	— 2,5	2
441	18 Eridani ε	3½	49 35 30	—10 32 5	— 72,0	+ 10,4	3
442*	9 Tauri	6	49 41 40	+22 8 55	+ 50,6	+ 3,9	2
443*	19 Eridani τ^5	4	50 2 0	—22 41 55	— 6,3	— 5,5	1
444*	Tauri	6	50 14 0	— 0 27 55	— 18,5	+ 23,8	1
445*	10 Tauri	4½	50 16 30	— 0 38 15	— 18,6	+ 57,4	3
446	39 Persei δ	3	50 16 30	+46 44 40	— 20,2	+ 23,9	2
447	20 Eridani	5½	50 33 50	—18 31 5	— 54,8	— 14,5	1
448	11 Tauri	6	50 34 40	+24 17 15	+ 42,1	+ 5,8	1
449*	40 Persei o	6	50 42 50	+32 55 35	+ 3,5	+ 15,0	1
450	21 Eridani	6	50 56 15	— 6 38 50	+ 4,8	— 26,2	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
451	12 Tauri	6	50° 56′ 30″	+ 2° 0′ 45″	− 11″,2	+ 12″,1	1
452	41 Persei ν	4	51 4 30	+41 33 40	+ 11,7	− 16,1	3
453	13 Tauri	6	51 7 20	+18 40 20	+ 30,7	− 4,5	4
454*	38 Persei o	4	51 15 15	+31 16 10			2
455	22 Eridani	5½	51 20 0	− 6 14 15	− 3,3	− 21,7	2
456	14 Tauri	6	51 29 0	+18 39 0	− 42,1	− 11,6	4
457*	16 Tauri	7	51 37 30	+23 16 50	− 25,9	− 16,2	8
458*	17 Tauri	5	51 38 40	+23 5 55	− 11,6	+ 5,1	24
459*	18 Tauri	7	51 41 40	+23 49 35	+ 1,2		4
460*	19 Tauri	5	51 43 10	+23 27 35	− 20,3	− 14,3	11
461*	20 Tauri	6	51 52 30	+23 21 50	− 19,3	− 13,4	14
462*	21 Tauri	6½	51 53 30	+23 32 35	− 27,8	+ 15,1	7
463*	22 Tauri	7	51 55 30	+23 31 15	− 16,2	+ 2,0	7
464*	23 Tauri	5	52 0 30	+22 56 15	− 12,6	+ 23,1	13
465	23 Eridani δ	3½	52 6 30	−10 50 15	− 7,9	+ 28,6	2
466	24 Eridani	5	52 12 40	− 2 10 25	− 32,5	+ 2,6	1
467*	24 Tauri	7	52 15 30	+23 6 50	− 10,7		3
468	25 Tauri η	3	52 17 30	+23 6 15	− 11,1	+ 13,4	36
469	25 Eridani	6	52 17 30	− 1 18 40	− 24,0	+ 24,5	1
470	29 Tauri u^1	6	52 18 30	+ 5 2 35	+ 21,4	+ 8,9	2
471*	42 Persei n	6	52 30 45	+32 5 30	+ 66,9	+ 30,6	1
472*	26 Tauri	7½	52 39 10	+22 51 50	+ 19,4		1
473*	27 Tauri	6	52 42 30	+23 3 45	− 6,8	+ 15,4	33
474*	28 Tauri	7½	52 42 40	+23 8 55	− 3,4	+ 6,4	14
475	30 Tauri e	5	52 49 10	+10 9 0	+ 54,9	+ 14,6	2
476*	Tauri		52 51 10	+22 42 15			2
477*	26 Eridani π	5	52 53 0	−13 5 55	− 6,1	− 14,7	2
478*	27 Eridani τ^6	4	53 24 0	−24 11 25	− 77,4	− 44,6	1
479*	Persei		53 28 30	+46 55 0			1
480	43 Persei A	5	53 29 30	+49 45 20	−147,8	− 31,8	3
481*	28 Eridani τ^7	5¾	53 36 0	−24 51 35	− 42,0	− 7,9	1
482	44 Persei ζ	3	53 41 0	+30 55 30	+ 24,9	− 9,5	11
483*	31 Tauri u^5	6	53 52 30	+ 5 33 55	+ 29,8	+ 12,5	2
484	45 Persei ε	3	54 18 0	+39 4 30	+ 9,7	− 22,1	7
485	29 Eridani	6½	54 18 20	− 6 1 0	− 15,0	− 33,2	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					ℛ.	D.	
486*	30 Eridani	5½	54° 21′ 20″	− 6° 19′ 5″	− 4″,5	− 4″,4	2
487	32 Tauri	6	54 39 10	+21 32 35			1
488	33 Tauri	7	54 41 0	+22 13 55	+ 29,2		1
489	32 Eridani	4½	54 41 30	− 3 54 20	− 3,6	+ 5,2	2
490	46 Persei ξ	5	54 45 0	+34 51 25	− 11,6	+ 2,3	2
491*	33 Eridani τ^3	4½	55 8 30	−25 33 15	− 23,0	− 11,7	1
492*	Tauri		55 8 40	+ 5 6 15			1
493	35 Tauri λ	4	55 52 40	+11 34 25	+ 51,3	+ 13,1	9
494	34 Eridani γ	2	55 54 0	−14 25 5	− 1,3	− 19,8	4
495*	47 Persei λ	4	55 56 0	+49 27 50	− 22,1	− 15,9	4
496	36 Tauri	7	56 28 10	+23 12 50	+ 26,4	− 5,6	3
497	35 Eridani	5	56 28 30	− 2 27 30	− 26,7	+ 22,8	1
498	48 Persei c	5	56 35 30	+46 50 35	+ 5,8	− 26,8	4
499	37 Tauri A^1	5	56 36 40	+21 11 30	+ 13,0	+ 9,5	8
500	38 Tauri ν	4	56 40 40	+ 5 5 15	+ 5,0	+ 22,8	4
501*	36 Eridani τ^2	4½	56 42 0	−24 55 15	− 61,2	− 9,6	2
502*	39 Tauri A^2	6	56 46 0	+21 8 0	+ 10,2	− 11,9	2
503	40 Tauri	7	56 51 0	+ 4 32 30	− 32,2	+ 10,0	3
504	41 Tauri	6	56 54 40	+26 42 55	+ 27,6	+ 24,1	1
505	49 Persei	6½	56 57 30	+36 52 0	+ 53,2	− 9,3	1
506	42 Tauri ψ	5	56 57 40	+28 7 5	+ 78,8	+ 11,6	4
507	50 Persei	6½	57 0 30	+37 11 0	+ 57,7	− 14,8	1
508*	Persei	6	57 44 40	+48 15 25			1
509*	43 Tauri ω^1	6	57 47 40	+18 45 10	+ 10,1	− 10,5	5
510	44 Tauri p	6	58 0 0	+25 37 35	+ 35,6	+ 8,4	5
511	51 Persei μ	4	58 5 30	+47 34 35	− 32,6	− 16,7	4
512*	Persei	6	58 11 —	+52 47 5			1
513	52 Persei f	5	58 29 0	+39 39 10	+ 2,7	− 4,6	3
514	45 Tauri	7	58 43 30	+ 4 40 15	− 4,9	+ 31,9	1
515*	Persei b^1	5	58 47 45	+49 29 10			3
516	37 Eridani	6	58 49 30	− 7 45 55	− 10,8	− 7,0	1
517*	Persei	6	59 3 0	+49 14 40			2
518*	Tauri	7	59 11 10	+ 8 3 25	+ 3,2		1
519*	38 Eridani o	4	59 11 40	− 7 40 45	− 5,1	+ 4,9	2
520	46 Tauri	7	59 13 30	+ 6 52 50	+ 10,9	+ 23,8	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
521*	Persei b^2	5	59° 14′ 30″	+50° 7′ 30″	″	″	2
522*	Camelopardi	6	59 15 —	+55 43 15			1
523*	47 Tauri	5½	59 17 10	+ 8 25 55	— 10,0	+ 29,3	2
524	48 Tauri	7	59 33 10	+14 34 25	+ 29,8	+ 40,6	1
525*	49 Tauri μ	5½	59 40 30	+ 8 4 35	+ 49,3	+ 6,1	3
526	50 Tauri ω^2	6	59 47 0	+19 46 15	+ 29,3	+ 8,8	9
527	53 Persei d	6	59 49 30	+45 42 45	+ 7,0	— 17,4	4
528	39 Eridani A	5	59 56 0	—11 3 35	— 50,6	— 7,8	3
529	51 Tauri	7	60 2 0	+20 46 35	— 11,5	+ 10,9	3
530	54 Persei	6	60 6 30	+33 46 10	— 38,9	+ 13,0	1
531*	40 Eridani d	5	60 15 20	— 8 10 15	—146,2	—207,2	2
532	52 Tauri ϕ	5	60 18 0	+26 33 45	+162,6	+ 6,5	2
533	53 Tauri	7	60 18 10	+20 20 55	+ 25,8	+ 5,9	1
534*	56 Tauri	7	60 20 10	+20 59 5	— 5,6	— 6,8	3
535	54 Tauri γ	3	60 32 40	+14 50 5	+ 39,2	+ 14,5	29
536*	55 Tauri	7	60 32 45	+15 44 5	+ 52,2		1
537*	57 Tauri h	6½	60 37 40	+13 15 0	+ 55,7	— 8,1	1
538*	58 Tauri	7	60 45 10	+14 18 15	+ 76,0	+ 28,9	11
539	59 Tauri χ	5	60 56 30	+24 51 0	+ 30,9	+ 18,6	4
540*	60 Tauri	7	61 7 30	+13 18 30	+142,1	— 15,0	1
541	55 Persei	6	61 7 30	+33 21 50	— 16,8	+ 8,5	1
542	56 Persei	7	61 10 0	+33 12 0	— 27,2	— 4,7	1
543	61 Tauri δ^1	4	61 16 10	+16 46 15	+ 48,7	+ 12,4	24
544	62 Tauri	7	61 20 45	+23 32 15	— 0,7	— 3,3	1
545	63 Tauri	6	61 25 0	+16 1 0	+ 31,5	— 14,5	2
546*	41 Eridani v^4	3¾	61 31 30	—34 34 5	+ 88,2		1
547*	64 Tauri δ^2	4	61 34 20	+16 40 55	+ 6,9	+ 7,9	10
548*	Tauri		61 43 30	+18 17 35			1
549	65 Tauri κ^1	5	61 44 10	+21 32 25	+ 32,8	+ 5,6	9
550	66 Tauri τ	5	61 44 40	+ 8 41 45	+ 22,0	+ 20,3	2
551*	67 Tauri κ^2	5	61 45 0	+21 26 50	+ 30,0	+ 2,8	3
552*	68 Tauri δ^3	4½	61 54 0	+17 10 40	+ 22,7	— 4,1	16
553	1 Camelopardi	6	61 56 30	+53 11 0	— 67,0	— 10,2	2
554	69 Tauri v^1	5	61 57 10	+22 4 0	+ 26,0	+ 2,1	8
555	70 Tauri	7	61 59 10	+15 11 5	+ 46,9	+ 21,9	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
556*	42 Eridani ξ	6	62° 4′ 20″	− 4° 29′ 45″	−131,9	− 10,6	2
557	71 Tauri	7	62 11 30	+14 52 5	− 11,9	+ 19,3	10
558*	72 Tauri υ^2	6	62 12 0	+22 15 15	+ 23,2	+ 1,5	4
559	73 Tauri π	5	62 16 40	+13 57 55	+ 36,6	+ 23,2	4
560	74 Tauri ε	3½	62 38 0	+18 27 15	+ 49,5	− 14,1	36
561	75 Tauri	7	62 41	+15 37 15	+ 12,1	+ 19,0	1
562	76 Tauri	7	62 42 40	+14 0 25	+ 42,5	+ 13,2	4
563	77 Tauri θ^1	5	62 43 10	+15 13 35	+ 57,1	+ 21,8	20
564	78 Tauri θ^2	5	62 44 40	+15 8 5	+ 45,7	+ 24,5	21
565*	79 Tauri b	5	62 52 0	+12 18 55	+ 57,3	+ 18,3	2
566	57 Persei m	6	62 57 30	+42 20 30	− 82,4	+ 36,4	3
567*	43 Eridani υ^3	5	63 5 0	−34 44 35			1
568	80 Tauri	7	63 7 10	+14 55 30	+ 45,6	− 22,9	3
569	44 Eridani	5½	63 10 0	+ 0 39 0	−104,7	+ 17,5	1
570*	Tauri	6	63 12 30	+15 28 25		+ 16,3	6
571*	81 Tauri	7	63 14 40	+14 58 55	+ 45,5	− 20,7	2
572	83 Tauri	7	63 17 30	+13 0 15	+ 61,0	+ 19,9	1
573*	Tauri	6	63 17 40	+15 26 15			1
574	84 Tauri	7	63 23 20	+14 23 35	+ 29,0	+ 6,4	1
575	85 Tauri	7	63 32 30	+15 8 30	+ 53,5	+ 10,9	1
576	58 Persei e	5	63 51 0	+40 35 0	− 56,4	− 20,4	4
577	2 Camelopardi	5	63 55 30	+52 48 40	− 82,4	− 27,5	2
578	3 Camelopardi	6	63 56 0	+52 25 0	− 34,7	− 42,4	1
579	45 Eridani	5½	64 2 30	− 0 45 0	− 96,8	+ 15,4	1
580	86 Tauri ρ	5	64 4 0	+14 8 45	+ 44,1	+ 19,9	11
581	87 Tauri α	1	64 32 20	+15 50 20	+ 35,4	+ 6,1	125
582	88 Tauri d	5	64 39 30	+ 9 28 45	+ 41,7	+ 17,7	2
583	46 Eridani	5	64 42 45	− 7 25 40	− 16,0	+ 14,3	2
584*	47 Eridani	5	64 49 45	− 8 55 5	− 19,8	+ 12,4	1
585*	89 Tauri	7	65 6 0	+15 21 55	+ 66,3	+ 14,5	3
586	90 Tauri c^1	5	65 12 30	+11 50 45	+ 47,8	+ 8,7	4
587*	48 Eridani ν	4	65 13 0	− 4 2 5	− 1,3	+ 44,7	1
588	59 Persei	6	65 16 30	+42 43 50	− 51,9	− 29,7	1
589	49 Eridani	5½	65 17 50	+ 0 19 40	+ 90,1	+ 19,9	1
590*	50 Eridani υ^6	4	65 19 0	−30 24 45	+ 83,5	− 51,6	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
591	91 Tauri σ^1	6	65° 22′ 0″	+15° 8′ 20″	+ 40,4	+ 26,0	18
592*	92 Tauri σ^2	6	65 23 30	+15 15 25	+ 33,3	+ 15,2	19
593	51 Eridani c	4	65 30 50	− 3 7 55	− 26,7	+ 3,1	1
594	4 Camelopardi	6	65 39 0	+56 9 20	−174,2	− 44,1	2
595*	93 Tauri c^2	6	65 41 40	+11 32 35	+ 59,8	+ 18,1	2
596*	9 Camelopardi	4½	65 52 15	+65 45 15			1
597*	52 Eridani v^7	3	65 52 30	−31 13 10	+ 12,8	− 21,2	1
598	94 Tauri τ	5	65 55 10	+22 19 10	+ 23,6	− 4,8	14
599	53 Eridani	3½	66 0 30	−14 56 25	− 26,1	− 20,3	3
600*	Eridani		66 8 0	−12 46 0			1
601*	95 Tauri	6½	66 10 10	+23 27 30	−133,1	− 6,1	1
602*	Eridani		66 16 50	−14 59 0	− 12,6	− 44,4	1
603	54 Eridani	3½	66 44 0	−20 18 0	− 23,7	+ 3,8	2
604	55 Eridani	6	67 11 20	− 9 24 45	− 9,5	− 1,2	1
605*	1 Aurigæ	5	67 17 30	+36 54 0	− 25,4	− 29,1	2
606*	56 Eridani	6	67 18 30	− 9 7 10	− 5,5	+ 6,6	1
607	57 Eridani μ	4	67 31 0	− 3 51 10	− 33,0	− 26,6	1
608	5 Camelopardi	6	67 31 10	+54 41 30	−161,7	− 17,3	1
609	6 Camelopardi	6	67 53 40	+55 16 20	−118,3	− 28,2	1
610*	2 Aurigæ	5½	68 0 0	+36 8 0	− 25,9	− 13,0	2
611*	96 Tauri	6	68 0 15	+15 24 5	+ 31,3	− *	1
612	7 Camelopardi	5	68 10 30	+53 12 0	− 89,7	− 12,8	2
613*	1 Orionis π	4	68 15 15	+ 6 23 20	+ 57,3	− 29,8	5
614	97 Tauri i	6	68 19 0	+18 16 5	+ 30,8	+ 1,0	4
615*	2 Orionis π^2	4	68 25 0	+ 8 19 35	+ 75,6	+ 4,2	1
616	58 Eridani	5½	68 26 30	−17 32 10	− 46,1	+ 13,8	1
617	59 Eridani	6	68 40 0	−16 54 30	− 44,5	− 12,7	1
618*	3 Orionis π^3	4	68 40 45	+ 5 2 5	+ 13,2	+ 4,5	3
619	4 Orionis o^1	4½	68 45 0	+13 41 15	+ 40,3	+ 13,9	2
620	8 Camelopardi	7	68 49 30	+52 37 50		− 36,9	1
621	10 Camelopardi	4¾	69 0 0	+59 55 50	+ 58,3	− 31,1	1
622	60 Eridani	6	69 4 30	−16 47 10	− 46,5	− 9,8	1
623	3 Aurigæ ι	4	69 14 0	+32 37 50	+ 18,1	− 16,2	7
624	5 Orionis	6	69 18 40	+ 1 57 20	+ 5,6	+ 6,9	1
625*	Tauri	6	69 23 —	+24 4 5			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					ℛ	D.	
626	6 Orionis g	6	69° 23′ 30″	+10° 51′ 45″	+ 55,4	+ 58,7	2
627	61 Eridani ω	5	69 25 30	− 6 0 45	− 7,8	+ 19,1	2
628*	7 Orionis π^4	6	69 27 15	+ 9 36 45	+ 53,0	+ 9,4	1
629*	8 Orionis π^5	4	69 32 0	+ 1 53 50	+ 4,7	− 9,6	7
630*	4 Aurigæ	5	69 34 40	+37 22 20	− 4,0	− 11,2	2
631	9 Orionis o^2	4½	69 44 0	+12 58 40	+ 42,5	+ 14,9	2
632*	99 Tauri	6	69 46 —	+23 26 —			
633	5 Aurigæ	6	69 46 30	+38 53 0	− 9,9	− 36,8	1
634	6 Aurigæ	6	69 47 0	+39 8 55	− 16,4	− 52,4	1
635	98 Tauri k	6	69 48 30	+24 31 25	+ 12,5	+ 6,9	2
636*	11 Camelopardi	5½	69 52 30	+58 28 50	− 96,7	− 23,0	2
637	12 Camelopardi	6	69 53 30	+58 31 50	−123,4	− 21,8	1
638	7 Aurigæ ε	4	69 57 30	+43 19 0	− 3,2	− 21,1	3
639*	100 Tauri	6	70 2 0	+15 50 5			1
640	8 Aurigæ ζ	4	70 14 0	+40 34 50	− 24,3	− 37,8	5
641	62 Eridani b	6	70 17 10	− 5 41 40	+ 16,2	− 14,1	2
642*	101 Tauri	6	70 29 50	+15 33 40	+ 7,8	− *	1
643*	10 Orionis π^6	4½	70 37 30	+ 1 11 40	+ 9,4	+ 17,4	2
644*	9 Aurigæ	5¾	70 41 30	+51 7 40	−204,4	− 14,7	3
645*	102 Tauri ι	4	71 9 30	+21 5 55	− 9,3	+ 10,9	13
646	10 Aurigæ η	4	71 12 30	+40 46 10	+ 32,4	− 24,0	6
647*	14 Camelopardi	7	71 14 40	+62 14 45	− 78,8	− 27,5	1
648	63 Eridani	6	71 18 30	−10 45 25	− 35,9	+ 5,3	1
649*	Camelopardi	6	71 21 35	+53 15 10			1
650	64 Eridani	6	71 24 0	−13 1 50	− 43,1	− 3,3	1
651	65 Eridani ψ	5	71 36 50	− 7 39 55	− 17,3	+ 0,4	2
652*	11 Orionis	5	71 43 0	+14 55 35	+ 29,6	+ 7,2	3
653*	Leporis		72 3 —	−20 32 30			1
654*	104 Tauri m	6	72 17 30	+18 11 0	+ 39,0	+ 4,3	8
655*	103 Tauri	6	72 19 —	+23 49 —			
656	105 Tauri	6	72 21 30	+21 14 50	+ 3,5	+ 5,9	3
657	106 Tauri l	6	72 22 40	+19 57 30	− 7,0	+ 20,4	3
658*	1 Leporis	6	72 24 30	−23 16 5	+ 48,1	− 9,1	1
659*	107 Tauri	8	72 39 30	+19 24 35	− 36,6	+ 6,0	3
660*	13 Orionis	6	72 41 0	+ 9 3 0	− 41,9	− 17,9	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					ÆR.	D.	
661	14 Orionis *i*	5	72° 44′ 30″	+ 8° 3′ 55″	+ 63,7″	— 47,8″	2
662*	66 Eridani	6	72 51 50	— 5 6 35	+ 6,4	+ 4,8	1
663*	15 Orionis	5	72 59 30	+15 9 5	+ 27,6	+ 19,7	2
664	11 Aurigæ *μ*	5	73 5 0	+38 4 0	— 40,2	— 10,9	1
665	16 Orionis *h*	6	73 5 30	+ 9 23 20	— 47,4	+ 1,3	1
666	2 Leporis *ε*	4	73 7 0	—22 49 10	— 94,2	+ 1,1	2
667*	67 Eridani *β*	3	73 9 30	— 5 31 20	— 3,9	— 14,4	6
668	15 Camelopardi	6	73 14 0	+57 43 40			2
669	68 Eridani	6	73 21 30	— 4 53 35	— 28,9	— 11,7	1
670	12 Aurigæ	6	73 25 0	+46 1 10	— 78,8	— 9,7	1
671	13 Aurigæ *α*	1	73 28 0	+45 37 55	+ 17,2	— 48,2	13
672*	Tauri	6	73 29 20	+15 37 50			1
673	69 Eridani *λ*	4	73 35 0	— 9 11 15	— 0,4	+ 0,1	3
674	14 Aurigæ	5	73 49 30	+32 16 30	— 22,6	+ 14,6	2
675*	108 Tauri	7	74 13 10	+21 53 55	— 4,5	— 3,0	9
676	17 Orionis *ρ*	4½	74 16 0	+ 2 26 50	+ 41,4	+ 19,7	2
677	16 Camelopardi	6	74 17 30	+57 11 10		— 34,5	2
678*	15 Aurigæ *λ*	5	74 21 0	+39 46 55	+ 49,5	— 87,2	1
679	3 Leporis *i*	5	74 29 0	—12 17 5	— 65,2	+ 25,4	2
680	16 Aurigæ	6	74 29 0	+32 59 50	— 31,8	— 9,7	3
681	17 Aurigæ	6¾	74 30 30	+33 22 30			1
682*	Aurigæ	6	74 40 30	+33 21 30	— 10,7	+ 34,0	1
683	4 Leporis *κ*	5	74 45 0	—13 20 25	— 57,8	— 9,1	2
684	8 Orionis	6	74 45 0	+10 57 0		— 1,9	2
685*	18 Aurigæ	8	74 45 30	+33 36 20		+ 4,8	1
686	5 Leporis *μ*	4	74 46 20	—16 36 15	— 46,2	— 10,3	2
687	19 Orionis *β*	1	74 55 0	— 8 36 0	— 3,0	+ 13,3	16
688	19 Aurigæ	6	74 55 20	+33 35 5	— 44,5	— 5,7	2
689	20 Aurigæ *ρ*	6	74 59 30	+41 26 30	— 32,0	— 5,7	1
690	109 Tauri *n*	6	75 10 40	+21 43 35	— 16,9	— 2,9	19
691*	17 Camelopardi	6	75 16 —	+62 44 10		— 3,5	1
692	20 Orionis *τ*	4	75 38 0	— 7 13 5	+ 34,7	+ 5,6	5
693	21 Orionis	6	75 45 10	+ 2 13 55	+ 27,6	+ 7,7	3
694	21 Aurigæ *σ*	5½	75 55 0	+37 2 40	— 21,5	— 5,0	1
695	22 Aurigæ	6	75 58 30	+28 35 50	— 93,2	— 20,9	4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
696	6 Leporis λ	4½	76° 20′ 30″	−13° 31′ 55″	− 48,0	+ 0,6	3
697	7 Leporis ν	5½	76 25 0	−12 40 15	− 43,8	− 5,7	2
698*	Orionis		76 25 10	− 0 46 —			1
699	110 Tauri	7	76 25 40	+16 21 25	+ 45,1	+ 15,7	3
700*	22 Orionis o	5	76 28 40	− 0 43 50	+ 43,6	+ 10,9	4
701	18 Camelopardi	6	76 33 0	+56 56 30	− 60,3	− 37,8	1
702	111 Tauri	6½	76 35 0	+17 3 30	+ 49,4	− 30,2	5
703	23 Orionis m	6	76 37 50	+ 3 11 55	+ 37,1	+ 26,5	2
704*	112 Tauri β	2	76 40 0	+28 17 55	+ 65,5	− 16,2	49
705	24 Aurigæ φ	5½	76 48 0	+34 9 20	− 47,3	+ 15,1	2
706	19 Camelopardi	6	76 54 0	+63 53 30		− 56,5	1
707	113 Tauri	6	77 2 40	+16 22 15	+ 30,0	+ 31,9	4
708	24 Orionis γ	2	77 8 20	+ 6 1 45	− 25,1	− 5,9	13
709*	25 Orionis ψ¹	5	77 10 0	+ 1 31 5	+ 17,9	+ 17,0	4
710*	27 Orionis p	6	77 10 30	− 1 13 55	+ 37,6	+ 25,0	4
711	28 Orionis η	3	77 13 40	− 2 43 25	+ 4,2	+ 10,3	7
712	29 Orionis e	5	77 14 30	− 8 7 55	+ 56,9	+ 2,9	1
713	114 Tauri o	5	77 15 40	+21 37 25	+ 7,0	+ 6,4	18
714*	115 Tauri	6	77 16 10	+17 38 40	+ 40,7	+ 19,3	3
715	8 Leporis	6	77 21 20	−14 15 15	− 72,4	+ 0,4	1
716	116 Tauri	6½	77 28 30	+15 34 20	+ 71,2	− 17,5	2
717	117 Tauri	7	77 30 40	+16 56 0			4
718	118 Tauri	6	77 33 10	+24 51 0	+ 19,2	+ 3,2	4
719	30 Orionis ψ²	5	77 39 0	+ 2 46 55	+ 19,6	+ 18,6	4
720*	20 Camelopardi	7	77 50 30	+56 14 0		− 41,9	1
721	21 Camelopardi	6½	78 0 0	+61 41 15		+ 22,9	2
722	25 Aurigæ χ	5½	78 9 0	+31 54 40	− 8,8	+ 3,0	4
723	22 Camelopardi	7½	78 15 30	+56 7 25		− 32,9	1
724*	119 Tauri	6	78 30 40	+18 18 55	+ 23,6	+ 10,2	9
725	31 Orionis	6	78 31 30	− 1 22 35	− 77,0	+ 1,1	1
726	32 Orionis A	5	78 32 30	+ 5 40 5	+ 51,4	+ 4,8	1
727*	Tauri	7	78 33 55	+16 46 35			2
728	33 Orionis n¹	6	78 44 10	+ 3 0 45	+ 50,3	+ 12,6	3
729	9 Leporis β	3	78 46 10	−21 2 35	− 88,8	+ 7,0	3
730*	23 Camelopardi	6	78 48 15	+61 14 55	+475,2	+ 6,0	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
731	120 Tauri	7	78° 50′ 30″	+18° 16′ 5″	+ 18,4	+ 17,3	6
732	34 Orionis δ	2	79 2 30	— 0 34 25	+ 20,9	+ 21,9	10
733*	35 Orionis	7	79 4 10	+14 2 20	+ 32,0	+ 18,9	7
734	121 Tauri	6	79 8 0	+23 47 5	+ 18,0	+ 4,7	4
735*	36 Orionis υ	4	79 13 30	— 7 33 45	+ 46,7	— 19,0	1
736*	24 Camelopardi	6	79 14 0	+56 21 40	+ *	— 24,4	3
737	37 Orionis ϕ^1	5	79 26 30	+ 9 14 5	+ 57,1	+ 9,0	1
738*	38 Orionis n^2	6	79 29 0	+ 3 30 25	+ 47,1	+ 26,3	3
739	10 Leporis	6	79 30 0	—21 7 45	—108,7	+ 6,4	1
740	39 Orionis λ	4	79 30 50	+ 9 40 45	+ 32,9	+ 17,9	1
741	25 Camelopardi	7½	79 37 30	+54 39 15		— 16,8	2
742	26 Aurigæ	6	79 42 0	+30 15 15	— 28,7	+ 13,7	2
743*	122 Tauri	7	79 46 0	+16 47 35	+ 32,5	+ 35,4	6
744	123 Tauri ζ	3	79 47 10	+20 54 20	+ 2,9	+ 5,3	69
745	11 Leporis α	3	79 47 30	—18 4 45	— 84,1	+ 2,6	3
746	40 Orionis ϕ^2	5	79 57 30	+ 9 4 40	+ 67,2	— 11,4	1
747*	Tauri		79 58 20	+29 0 0			1
748*	41 Orionis θ^1	6	79 59 50	— 5 37 25	+ 69,7	— 33,8	2
749	42 Orionis c	5	80 1 0	— 5 4 45	+ 43,8	— 5,5	3
750*	43 Orionis θ^2	4	80 1 40	— 5 38 55	+ 60,4	— 35,4	3
751	44 Orionis ι	3½	80 3 30	— 6 9 5	+ 48,1	+ 1,3	2
752*	45 Orionis	7	80 5 10	— 5 5 55	+ 38,5	+ 7,1	3
753	46 Orionis ε	2	80 7 10	— 1 26 45	+ 25,0	+ 23,9	14
754*	125 Tauri	5½	80 7 40	+25 40 5	+ 40,8	+ 23,0	4
755	26 Camelopardi	5½	80 8 30	+55 55 30		— 18,2	2
756*	27 Camelopardi	5½	80 — —	+56 22 35			1
757	28 Camelopardi	6½	80 11 15	+56 44 0		— 17,8	1
758	27 Aurigæ o	6	80 31 30	+49 38 10	—142,4	— 16,8	3
759*	Columbæ	6	80 31 40	—30 4 15			1
760	47 Orionis ω	5	80 42 0	+ 3 53 55	+ 33,7	+ 17,9	1
761	48 Orionis σ	4	80 47 40	— 2 49 25	+ 25,8	+ 19,8	10
762	126 Tauri	6	80 51 0	+16 19 15	+ 3,6	+ 24,0	9
763	49 Orionis d	5	80 59 15	— 7 25 25	— 41,3	— 2,5	1
764	29 Camelopardi	5½	81 4 0	+56 45 5		— 8,1	1
765*	Orionis		81 6 25	+23 1 0			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
766*	127 Tauri	7	81° 10′ 0″	+18° 47′ 40″	+ 16,2	− 35,6	5
767	30 Camelopardi	6	81 17 0	+58 48 40	− 93,8	− 27,7	1
768	50 Orionis ζ	2	81 17 30	− 2 8 45	− 30,1	− 0,1	22
769*	Columbæ	5	81 27 40	−28 53 30			1
770	51 Orionis *b*	5	81 38 10	+ 1 17 5	− 76,1	− 2,3	1
771	128 Tauri	6	81 44 15	+15 54 15	+ 92,1	+ 5,8	1
772	28 Aurigæ	7	81 50 30	+39 22 25	− 22,2	− 11,6	1
773	31 Camelopardi	5	81 51 0	+59 45 0	−118,6	− 8,5	3
774*	29 Aurigæ τ	6	81 56 30	+39 1 45	− 32,4	− 28,7	3
775*	Columbæ		82 9 —	−32 48 0			1
776*	Columbæ α		82 9 35	−34 14 40			1
777	129 Tauri	6	82 13 40	+15 38 45	+ 37,4	+ 36,7	6
778*	30 Aurigæ ξ	5	82 15 30	+55 34 10		+ 2,2	5
779	130 Tauri	6	82 20 10	+17 33 55	+ 37,9	+ 6,2	9
780	12 Leporis	6	82 20 20	−22 33 20	−131,4	− 4,4	1
781	131 Tauri	6	82 23 30	+14 19 30	− 13,9	+ 14,7	4
782*	Orionis		82 28 —	+20 42 50			1
783	31 Aurigæ υ	6	82 29 30	+37 9 55	− 30,0	− 13,4	2
784*	132 Tauri	5¼	82 29 40	+24 24 50	+ 33,4	+ 3,1	7
785	32 Aurigæ ν	5	82 30 30	+39 0 40	+ 3,6	− 30,3	2
786	133 Tauri	6	82 32 0	+13 44 5	+ 17,2	+ 25,9	4
787	52 Orionis	6	82 49 40	+ 6 17 55	+ 48,0	+ 20,3	1
788*	Columbæ		82 50 35	−34 49 20			1
789	13 Leporis γ	3½	82 55 30	−22 34 50	−149,4	− 28,0	2
790	33 Camelopardi	7	82 56 30	+55 32 50			1
791*	134 Tauri	6	83 1 30	+12 30 5	+ 46,4	+ 25,2	4
792*	Camelopardi		83 3 45	+55 48 —			1
793	135 Tauri	6	83 12 30	+14 9 55	+ 29,9	+ 15,9	4
794	14 Leporis ζ	4	83 14 0	−14 58 15	− 8,1	− 8,1	2
795	53 Orionis κ	3	83 16 15	− 9 49 15	− 11,7	+ 15,7	9
796	136 Tauri	5	83 27 0	+27 29 15	+ 59,9	+ 11,2	6
797*	34 Camelopardi	6	83 30 0	+55 14 10		− 29,8	1
798	33 Aurigæ δ	4	83 32 30	+54 12 5		− 27,6	5
799*	Aurigæ	7½	83 36 15	+54 27 15		− 3,8	1
800*	137 Tauri	5	83 40 0	+14 2 45	+100,0	+ 9,6	3

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR	D.	
801	54 Orionis χ^1	5	84° 0′ 50″	+20° 10′ 15″	— 19,5	+ 1,2	10
802	56 Orionis	6	84 5 0	+ 1 43 20	+ 22,5	+ 58,3	1
803	55 Orionis	6	84 5 10	— 7 38 25	+ 66,5	+ 4,8	1
804	57 Orionis χ^2	5	84 9 30	+19 38 15	— 20,9	+ 18,2	3
805	34 Aurigæ β	2	84 12 30	+44 52 0		— 27,8	9
806	35 Aurigæ π	6	84 15 0	+45 51 40			4
807	36 Aurigæ	6	84 24 0	+47 49 30	—101,7	— 8,1	1
808*	Aurigæ		84 25 15	+44 31 10			1
809	15 Leporis δ	$3\frac{3}{4}$	84 31 40	—20 56 30	— 79,9	— 30,5	2
810	58 Orionis α	1	84 35 30	+ 7 18 15	+ 36,9	+ 10,5	36
811	37 Aurigæ θ	4	84 39 0	+37 8 25	+ 6,0	— 10,5	6
812	139 Tauri	6	84 40 40	+25 51 45	+ 59,4	+ 15,5	4
813*	Orionis	7	84 52 0	+ 9 25 0			1
814*	35 Camelopardi	$5\frac{1}{2}$	84 58 —	+51 31 25		— 20,8	1
815*	Columbæ β	4	85 3 40	—35 52 50			1
816*	38 Aurigæ	$6\frac{1}{2}$	85 15 30	+42 52 10	— 48,8	— 24,6	1
817*	Orionis	6	85 20 50	+12 43 50			2
818*	140 Tauri	$7\frac{1}{2}$	85 25 0	+22 49 55		+ 5,4	1
819	36 Camelopardi	6	85 27 15	+65 42 5		— 1,7	1
820*	Columbæ λ	5	85 30 10	—33 52 25			1
821	59 Orionis	6	85 34 15	+ 1 45 45	+ 35,1	+ 10,6	2
822	16 Leporis η	4	85 35 20	—14 15 25	— 52,7	— 1,7	2
823	37 Camelopardi	$5\frac{1}{2}$	85 42 0	+58 54 40		— 17,5	2
824	39 Aurigæ	$6\frac{1}{2}$	85 42 30	+42 57 15	— 55,9	— 34,9	1
825	60 Orionis	6	85 42 50	+ 0 28 45	+ 39,1	+ 19,0	2
826	141 Tauri	6	85 44 30	+22 20 45	+ 22,0	— 4,4	1
827*	39 Camelopardi	$6\frac{1}{2}$	85 47 —	+60 26 40		— 38,1	1
828*	Aurigæ	*Tel.*	85 49 10	+38 32 0			1
829	38 Camelopardi	7	85 55 30	+59 9 10			2
830*	Aurigæ	*Tel.*	85 56 25	+37 55 30			1
831	1 Monocerotis	6	86 3 0	— 9 26 40	+ 85,0	— 3,6	1
832	2 Monocerotis	6	86 4 0	— 9 37 0	+ 87,9	— 3,3	1
833*	64 Orionis χ^3	6	86 16 40	+19 38 45		+ 10,4	3
834*	Camelopardi	4	86 17 10	+69 20 20			1
835	40 Aurigæ	6	86 18 0	+38 27 40	+ 27,2		4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
836	61 Orionis μ	4	86° 19′ 30″	+ 9° 36′ 5″	+ 39,5		6
837*	1 Geminorum	5	86 19 30	+23 14 5	— 8,4	— 15,4	97
838*	62 Orionis χ^4	6	86 23 10	+20 5 45	— 18,9	+ 12,4	3
839	3 Monocerotis	6	86 47 30	—10 38 10	+ 97,5	— 11,4	2
840*	2 Geminorum	7	86 58 10	+23 37 5	+ 43,1	+ 4,8	4
841	40 Camelopardi	6½	86 58 30	+60 1 10		— 19,2	2
842	41 Aurigæ	6	87 0 0	+48 43 10		— 17,8	1
843*	63 Orionis	6	87 5 45	+ 5 23 25			1
844	66 Orionis	6	87 8 45	+ 4 7 55	+ 23,1	+ 11,7	1
845	1 Lyncis	5½	87 22 30	+61 32 40	—163,6	— 4,8	3
846	67 Orionis ν	4½	87 28 0	+14 45 25	+ 14,3	+ 11,9	3
847*	3 Geminorum	6½	87 43 30	+23 6 55	+ 9,2	+ 5,8	8
848	17 Leporis	6	87 48 20	—16 29 50	— 54,8	— 3,3	1
849	4 Geminorum	7	87 55 30	+23 0 5	+ 1,6	+ 19,1	5
850	18 Leporis θ	4	88 4 30	—14 56 30	—148,8	— 3,3	1
851	2 Lyncis	4	88 6 0	+59 3 0	—136,4	+ 15,4	5
852	5 Geminorum	7	88 8 10	+24 26 20	— 9,3	+ 2,4	3
853	6 Geminorum	7	88 22 45	+22 55 55	+ 14,1	— 3,9	3
854	68 Orionis	6	88 25 0	+19 48 15	— 1,1	+ 30,4	1
855*	3 Lyncis	6	88 25 45	+61 49 35	— *	+ 1,8	1
856	4 Monocerotis	6	88 26 40	—11 8 10		— 2,8	1
857	69 Orionis f^1	6	88 32 30	+16 9 5	+ 21,4	+ 12,1	3
858	70 Orionis ξ	4½	88 33 30	+14 13 45	+ 78,3	+ 17,6	5
859	19 Leporis	6	88 35 0	—19 9 55	— 97,9	+ 7,9	1
860	42 Aurigæ	6	88 38 30	+46 28 15	— 86,3	— 3,7	1
861	4 Lyncis	6	88 41 0	+59 26 20		— 14,8	5
862	43 Aurigæ	6	88 49 0	+46 25 20	— 93,4	— 7,2	1
863	44 Aurigæ $\varkappa$	4½	88 54 30	+29 33 35	— 11,4	— 6,5	2
864*	Monocerotis	5	89 — —	— 6 25 40			1
865	7 Geminorum η	4½	89 2 20	+22 32 50	+ 10,1	+ 7,4	163
866	71 Orionis	6	89 9 30	+19 12 50	— 25,6		1
867*	45 Aurigæ	6	89 12 30	+53 31 50	— *	— 10,9	1
868	8 Geminorum	7½	89 20 30	+24 1 15	+ 18,9	+ 6,4	4
869	72 Orionis f^2	6	89 23 0	+16 11 25	+ 12,7	+ 7,2	3
870	9 Geminorum	7	89 30 30	+23 47 45	+ 36,5	+ 6,4	3

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
871	73 Orionis k^1	6	89° 34′ 40″	+12° 35′ 45″	+ 27″,6	+ 30″,8	3
872*	Lyncis	6	89 39 30	+58 32 0			2
873*	Lyncis	6	89 43 15	+58 42 0			3
874*	74 Orionis k^2	6	89 45 10	+12 18 30	+ 33,8	+ 33,6	3
875	5 Monocerotis	4½	89 55 0	− 6 13 0	+ 71,0		2
876*	5 Lyncis	6	89 57 30	+58 31 30		− 24,4	5
877	75 Orionis l	6	90 0 0	+10 0 35	+ 25,3	+ 7,5	2
878	10 Geminorum	8	90 0 10	+23 40 25	+ 11,8	+ 6,0	2
879*	Lyncis	6	90 4 —	+56 23 30			1
880	11 Geminorum	8	90 6 10	+23 32 40	+ 39,0	− 2,7	2
881	12 Geminorum	8	90 7 10	+23 20 55		+ 4,0	1
882	46 Aurigæ	5	90 17 30	+49 23 20	−146,3	− 8,8	1
883	6 Monocerotis	6½	90 44 0	−10 38 50	+ 83,2	− 1,6	1
884	6 Lyncis	6½	90 59 0	+58 19 30	−120,7	− 31,1	2
885	13 Geminorum μ	3	91 3 0	+22 37 25	+ 8,5	+ 0,2	193
886	7 Monocerotis	6	91 10 30	− 7 43 50	+ 79,7	− 0,3	2
887*	47 Aurigæ	6	91 44 30	+46 49 25		+ 4,3	1
888	14 Geminorum	7½	91 46 0	+21 46 5	+ 38,3	+ 6,2	4
889	8 Monocerotis	4	91 51 0	+ 4 42 30	− 50,3	+ 2,2	4
890*	76 Orionis	6	91 52 30	+ 5 23 0			
891	1 Canis Maj. ζ	3	92 8 40	−29 56 40	−137,0	− 38,1	2
892	48 Aurigæ	6	92 9 30	+30 37 40		+ 23,5	1
893*	7 Lyncis	6½	92 10 30	+55 31 20			1
894*	2 Canis Maj. β	2	92 16 30	−17 50 10	− 36,8	− 0,7	8
895*	15 Geminorum	7	92 19 30	+20 55 45	− 1,4	+ 10,9	4
896	8 Lyncis	6½	92 20 0	+61 40 50		− 15,1	1
897*	Lyncis	6	92 20 45	+58 17 50			1
898	16 Geminorum	7	92 22 30	+20 37 45	+ 5,0	+ 28,6	6
899*	9 Lyncis	7	92 28 0	+56 34 20		− 33,2	1
900	10 Lyncis	6½	92 31 15	+61 40 25		− 41,9	2
901	18 Geminorum ν	4	92 38 0	+20 21 35	+ 20,1	+ 7,3	103
902*	3 Canis Majoris	4	92 43 10	−33 17 35	− 78,4	− 53,8	2
903	11 Lyncis	6	92 49 30	+57 2 50		− 23,1	1
904*	77 Orionis	6	92 52 0	+ 0 27 0	−134,2	− 18,4	1
905	78 Orionis	6	92 53 45	− 0 7 20	−155,1	− 27,0	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
906*	9 Monocerotis	6	92° 55′ 0″	— 4° 12′ 50″	— 0″,7	+ 14″,1	1
907*	Geminorum	8	92 56 —	+22 42 25			1
908	41 Camelopardi	7	92 58 0	+62 7 50		— 42,6	1
909	10 Monocerotis	6	93 9 30	— 4 36 20	+ 20,5	— 13,5	2
910	19 Geminorum	7	93 25 45	+16 4 5	+ 67,8	+ 25,8	3
911*	11 Monocerotis	4	93 26 0	— 6 52 10	+ 58,4	— 13,6	2
912	20 Geminorum	7½	93 32 30	+17 57 15	+ 44,0	— 3,8	4
913*	21 Geminorum	6½	93 48 30	+17 57 40	— *	— 12,9	1
914*	Monocerotis	4	93 54 —	+ 5 7 25			1
915	49 Aurigæ	5½	93 55 30	+28 12 35	— 30,0	+ 18,2	4
916*	12 Monocerotis	6½	93 59 0	+ 5 2 45	— 57,3	— 30,8	3
917*	13 Monocerotis	5	94 3 0	+ 7 31 10	— 51,3	— 4,0	3
918	22 Geminorum	7	94 4 40	+19 37 5	+ 43,4	+ 11,5	3
919	50 Aurigæ	5½	94 17 30	+42 42 35	— 2,6	— 16,3	2
920	51 Aurigæ	5½	94 17 50	+39 36 30	— 32,7	— 1,3	1
921	52 Aurigæ	5	94 18 40	+40 6 40	— 34,0	+ 11,1	1
922*	Lyncis	7	94 21 45	+59 40 45		+ 10,7	1
923*	23 Geminorum	7	94 30 40	+17 0 0	+ 33,9	+ 53,4	1
924	14 Monocerotis	5½	94 31 0	+ 7 46 10		+ 7,2	2
925	53 Aurigæ	6	94 41 0	+29 11 55	— 18,4	+ 5,3	2
926*	42 Camelopardi	4½	94 42 0	+67 49 55	— *	+ 0,7	1
927*	12 Lyncis	5½	94 43 0	+59 41 35		— 24,6	4
928	4 Canis Maj. ξ^1	5	94 44 30	—23 13 25	— 13,8	— 12,9	1
929	24 Geminorum γ	2½	94 56 40	+16 36 45	+ 23,1	+ 21,0	124
930	54 Aurigæ	6	95 0 10	+28 28 55	— 17,7	+ 22,3	3
931*	43 Camelopardi	4½	95 7 30	+69 9 35	— *	+ 18,1	2
932	55 Aurigæ	5	95 8 30	+44 46 10	— 78,4	— 15,9	1
933*	13 Lyncis	6	95 19 10	+57 25 30	— *	— 1,2	2
934	25 Geminorum	7	95 26 0	+28 26 15	+ 57,8	— 12,1	2
935	5 Canis Maj. ξ^2	5	95 31 30	—22 45 5	— 26,9	— 1,2	1
936	6 Canis Maj. ν^1	5	95 43 0	—18 26 5	— 43,6	— 13,8	2
937	7 Canis Maj. ν^2	5	95 48 0	—19 1 25	— 19,2	— 11,6	3
938*	15 Monocerotis	5½	96 0 30	+10 8 30	—112,9	— 5,9	3
939*	57 Aurigæ	6	96 1 30	+49 3 40	—112,6	— 9,4	1
940	26 Geminorum	5	96 4 15	+17 54 5	+ 57,2	+ 0,7	6

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
941	8 Canis Maj. ν^a	5	96° 4′ 20″	−18° 0′ 10″	− 22,″7	− 4,″7	4
942*	56 Aurigæ	6	96 5 0	+43 50 5	+ 17,5	− 6,6	2
943	27 Geminorum ε	3	96 12 30	+25 23 15	+ 9,4	+ 9,5	26
944*	28 Geminorum	6	96 16 45	+29 13 50	− 18,9	+ 15,5	4
945	14 Lyncis	5	96 27 30	+59 44 55	−187,9	− 6,9	4
946*	30 Geminorum	6	96 37 0	+13 29 35	+ 30,2	+ 12,2	24
947	31 Geminorum ξ	$4\frac{3}{4}$	96 57 30	+13 11 5	+ 37,8	− 4,8	55
948*	32 Geminorum	6	97 7 0	+12 58 20	+ 17,3	− 0,4	1
949	58 Aurigæ	$4\frac{1}{2}$	97 12 30	+42 5 40		− 20,7	1
950	16 Monocerotis	6	97 25 40	+ 8 52 20	− 71,4	+ 3,1	1
951	15 Lyncis	5	97 36 30	+58 45 50	−118,8	− 11,1	5
952	17 Monocerotis	5	97 38 30	+ 8 19 50		− 3,2	2
953*	9 Canis Maj. α	1	97 52 30	−16 19 15	− 45,9	− 92,2	47
954*	59 Aurigæ	6	97 54 30	+39 11 15		− 5,3	2
955*	18 Monocerotis	5	97 56 0	+ 2 42 40		+ 2,2	2
956	33 Geminorum	$6\frac{1}{2}$	97 57 40	+16 30 25	+117,8	+ 9,1	4
957	60 Aurigæ	6	97 58 0	+38 46 25		− 17,3	2
958	34 Geminorum θ	4	98 4 30	+34 17 0	+ 22,6	0,0	6
959	61 Aurigæ	6	98 9 40	+38 49 10		+ 41,8	1
960	11 Canis Majoris	5	98 10 50	−14 7 40	− 3,6	− 2,7	1
961	35 Geminorum	6	98 12 0	+13 43 25	+104,4	+ 7,5	2
962	36 Geminorum d	6	98 14 0	+22 4 40	+ 14,9	+ 7,6	8
963*	Geminorum		98 14 —	+23 55 25			2
964*	10 Canis Majoris	6	98 20 0	−30 47 10	− *		1
965*	12 Canis Majoris	6	98 25 0	−20 43 30	+ 78,2	+ 44,1	2
966	16 Lyncis	6	98 45 20	+45 26 10	− 78,6	+ 15,8	2
967	37 Geminorum	6	99 3 30	+25 42 45	− 7,5	+ 15,0	2
968	38 Geminorum e	6	99 16 0	+13 31 5	+ 80,2	+ 32,0	8
969*	13 Canis Maj. $\varkappa$	5	99 27 0	−32 10 50	+424,2	+ 5,2	1
970	62 Aurigæ	$6\frac{1}{2}$	99 28 0	+38 25 25	+ 6,6	− 1,8	2
971*	Lyncis	5	99 40 5	+60 11 50			1
972	39 Geminorum	$6\frac{1}{2}$	99 55 0	+26 26 15	− 19,2	+ 19,4	4
973	14 Canis Maj. θ	5	99 56 50	−11 40 10	− 5,7	− 58,6	1
974*	15 Canis Majoris	6	100 1 30	−19 52 30	+ 48,2	+ 1,4	2
975	40 Geminorum	6	100 4 30	+26 17 15	+ 0,2	+ 0,2	5

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
976	16 Canis Maj. o^1	5	100° 18′ 40″	−23° 49′ 35″	+ 29,9	− 1,8	1
977*	17 Canis Majoris	6	100 24 30	−20 2 30	+143,6	− 4,3	2
978*	18 Canis Maj. μ	4	100 29 10	−13 40 40	− 30,7	+ 2,8	1
979*	17 Lyncis	7	100 30 0	+61 12 30	−295,0	+ 9,4	2
980*	19 Canis Majoris	6	100 33 0	−19 46 20	− 7,2	− 4,6	3
981*	20 Canis Maj. ι	4	100 35 30	−16 41 10	− 41,6	− 1,9	2
982*	41 Geminorum	6	100 36 0	+16 27 5	+ 31,1	+ 40,3	2
983	42 Geminorum ω	6	100 52 0	+24 36 55	+ 27,5	− 15,6	9
984*	Geminorum	6	100 54 —	+11 0 40			1
985	43 Geminorum ζ	$3\frac{1}{2}$	101 25 0	+20 58 15	+ 29,0	+ 34,1	23
986	21 Canis Maj. ε	$2\frac{3}{4}$	101 34 50	−28 34 30	+116,3	− 17,7	6
987*	Geminorum	$5\frac{1}{2}$	101 35 15	+11 21 53			3
988*	44 Geminorum	$6\frac{1}{2}$	101 39 0	+23 3 15	+ 15,7	+ 5,4	7
989*	Canis Majoris		101 41 35	−28 34 —			1
990	19 Monocerotis	5	101 52 30	− 3 49 30	+ 19,8	− 10,3	2
991*	Lyncis	7	102 6 0	+60 14 30			1
992	18 Lyncis	6	102 13 15	+60 7 0	−221,9	+ 10,7	3
993	22 Canis Majoris	4	102 18 45	−27 31 5	+122,2	− 8,2	3
994*	Lyncis	6	102 22 55	+50 13 20	— *		1
995	23 Canis Maj. γ	3	102 26 0	−15 12 25	+ 3,0	− 9,2	3
996	24 Canis Maj. o^2	$4\frac{3}{4}$	102 31 0	−23 24 40	+ 19,9	− 1,2	4
997	63 Aurigæ	$4\frac{1}{2}$	102 33 20	+39 46 20	+ 43,1	+ 6,2	1
998*	45 Geminorum	6	102 37 30	+16 22 35	+ 62,9	+ 14,9	14
999	46 Geminorum τ	5	102 52 45	+30 42 10	−139,1	+ 5,6	2
1000	44 Camelopardi	6	102 53 30	+59 24 30		− 13,8	1
1001	45 Camelopardi	7	103 0 30	+59 37 10		− 10,9	1
1002*	47 Geminorum	6	103 0 45	+27 19 5	+ 63,0	+ 3,6	2
1003	46 Camelopardi	7	103 10 45	+59 45 0		− 5,0	1
1004*	Lyncis	6	103 11 30	+52 38 20			1
1005*	48 Geminorum	6	103 23 0	+24 36 15	+ 31,2	− 16,0	7
1006	49 Geminorum	7	103 26 10	+26 12 55	− 19,2	+ 17,3	1
1007*	Geminorum	7	103 27 5	+17 26 30			1
1008	20 Monocerotis	6	103 42 0	− 3 47 20	+ 24,8	+ 7,9	2
1009	47 Camelopardi	6	103 44 0	+60 25 —		− 20,1	1
1010*	Lyncis	5	103 45 0	+49 57 0			4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1011*	51 Geminorum	$5\frac{3}{4}$	103° 52′ 30″	+16° 38′ 15″	+ 42,2	+ 11,5	8
1012*	21 Monocerotis	6	103 53 20	+ 0 10 10	— 12,7	+ 5,2	2
1013*	52 Geminorum	$6\frac{3}{4}$	103 55 0	+25 22 35	+ 38,3	— 0,5	4
1014	25 Canis Maj. δ	$2\frac{1}{2}$	103 55 20	—25 55 45	+ 95,7	— 9,2	7
1015	22 Monocerotis	$4\frac{1}{2}$	104 0 30	— 0 1 0	— 4,8	— 3,8	2
1016	64 Aurigæ	5	104 6 0	+41 23 0	+ 1,5	— 4,3	1
1017*	53 Geminorum	$6\frac{1}{2}$	104 9 0	+28 22 55	— 44,0	+ 27,9	1
1018	23 Monocerotis	$6\frac{1}{2}$	104 21 30	+ 0 13 20			1
1019*	19 Lyncis	5	104 28 0	+55 48 20	— *	— 4,2	2
1020*	20 Lyncis	6	104 38 0	+50 40 30		— 5,6	1
1021	24 Monocerotis	6	104 51 30	+ 0 20 10	+ 16,3	+ 11,4	1
1022	26 Canis Majoris	7	104 51 40	—25 27 10	+ 73,6	— 3,3	1
1023	54 Geminorum λ	5	105 3 30	+17 3 10	+ 12,7	+ 11,9	31
1024	65 Aurigæ	5	105 16 30	+37 17 30	+135,9	+ 5,6	1
1025*	27 Canis Majoris	7	105 23 10	—25 50 40	+ 73,5	— 15,9	1
1026*	55 Geminorum δ	3	105 23 30	+22 30 30	+ 2,8	+ 0,4	43
1027*	28 Canis Majoris	$3\frac{1}{2}$	105 32 30	—26 16 35	+ 68,9	+ 38,3	3
1028	66 Aurigæ	5	105 39 0	+41 13 10	— 8,6	— 9,1	1
1029	21 Lyncis	5	105 49 0	+49 45 30	— 64,7	+ 42,4	4
1030*	56 Geminorum	$6\frac{1}{2}$	105 54 0	+20 59 5	+ 13,4	— 2,1	9
1031	57 Geminorum A	$5\frac{1}{2}$	106 8 0	+25 36 0	— 5,0	+ 0,9	8
1032	58 Geminorum	$7\frac{1}{2}$	106 12 0	+23 30 5	+ 3,0	— 15,6	4
1033	59 Geminorum	6	106 18 0	+28 11 25	+ 21,6	— 1,9	3
1034	29 Canis Majoris	5	106 25 30	—24 1 20	+ 66,2	— 4,8	2
1035	30 Canis Majoris	5	106 26 50	—24 24 45	+ 63,9	— 25,6	2
1036	22 Lyncis	6	106 35 0	+50 14 30		+ 48,4	2
1037	60 Geminorum ι	$4\frac{1}{2}$	106 36 10	+28 22 10	— 0,4	— 10,5	7
1038	1 Canis Minoris	$6\frac{3}{4}$	106 55 40	+12 13 45	+ 15,2	+ 21,1	1
1039*	61 Geminorum	$6\frac{1}{2}$	107 9 30	+20 50 5	+ 0,1	— 5,0	5
1040	2 Canis Min. ε	6	107 9 40	+ 9 50 35	+ 39,0	+ 16,0	2
1041*	62 Geminorum ϱ	5	107 16 40	+32 21 15	+ 16,8	+ 7,5	5
1042*	63 Geminorum	6	107 19 30	+22 1 55	— 3,0	+ 3,8	10
1043*	Geminorum	7	107 20 —	+28 8 10			1
1044*	64 Geminorum b^1	6	107 30 0	+28 42 30	— 22,3	+ 6,5	4
1045	3 Canis Min. β	3	107 34 10	+ 8 52 25	+ 36,3	+ 0,9	12

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1046*	65 Geminorum b^2	6	107° 37′ 20″	+28° 30′ 35″	— 12,5	— 1,8	3
1047	4 Canis Min. γ	6	107 48 30	+ 9 30 35		+ 13,1	3
1048*	5 Canis Min. η	6	107 50 0	+ 7 31 5	+ 12,3	+ 54,6	2
1049	31 Canis Maj. η	$2\frac{3}{4}$	107 56 0	−28 43 30	+101,0	— 8,2	5
1050*	6 Canis Minoris	6	108 7 30	+12 36 15	+ 16,4	+ 8,3	1
1051*	48 Camelopardi	6	108 16 0	+60 12 30	— 18,4	— 30,8	2
1052*	66 Geminorum α	1	108 41 0	+32 31 5	+ 0,9	+ 1,2	45
1053*	23 Lyncis	6	108 41 40	+57 43 35		+ 13,2	1
1054*	67 Geminorum	$7\frac{1}{2}$	108 54 40	+16 15 15		+ 27,9	2
1055*	68 Geminorum	6	108 57 45	+16 26 55	+ 31,5	+ 9,3	6
1056	7 Canis Min. δ^1	6	180 58 40	+ 2 31 45	+ 7,8	+ 15,0	2
1057	24 Lyncis	5	109 9 0	+59 22 5	— 67,0	+ 19,9	5
1058	69 Geminorum υ	5	109 12 0	+27 32 10	— 35,3	+ 6,7	8
1059*	8 Canis Min. δ^2	6	109 13 30	+ 3 54 35	+ 24,1	+ 16,1	2
1060*	9 Canis Min. δ^3	6	109 28 10	+ 4 0 5	+101,8	+ 15,7	1
1061	70 Geminorum	5	109 30 0	+35 41 45	+ 72,9	+ 1,3	1
1062*	49 Camelopardi	5	109 37 30	+63 31 0	— *	— 18,7	1
1063	71 Geminorum o	5	109 41 0	+35 14 55	+107,1		2
1064*	Canis Minoris		110 0 20	+ 6 30 25			1
1065	50 Camelopardi	6	110 4 15	+51 6 10	+130,0	+ 24,9	1
1066*	51 Camelopardi	5	110 10 10	+66 8 15	— *	— 40,9	1
1067	74 Geminorum f	6	110 22 30	+18 20 5	+ 34,0	+ 13,3	8
1068	25 Monocerotis	6	110 27 30	— 3 27 20	+ 21,9	+ 3,0	2
1069	10 Canis Min. α	$1\frac{1}{2}$	110 45 20	+ 5 58 45	— 18,4	— 54,5	52
1070*	Canis Minoris		110 54 10	+ 5 54 5	+ 24,9	+ 19,3	4
1071	75 Geminorum σ	5	110 58 45	+29 35 15	— 33,5	— 9,2	9
1072*	76 Geminorum c	6	111 18 0	+26 29 30	— 46,3	— 47,6	Δ
1073	77 Geminorum $\varkappa$	$4\frac{1}{2}$	111 25 0	+25 5 40	+ 10,8	+ 10,2	18
1074*	Navis $\varkappa$		111 32 30	−26 7 15			3
1075*	Canis Majoris	6	111 33 15	−14 35 5		— *	1
1076	78 Geminorum β	2	111 34 0	+28 43 55	— 28,9	— 4,4	71
1077	26 Monocerotis	$4\frac{1}{2}$	111 35 0	— 8 51 40	+ 89,2	— 4,5	4
1078*	79 Geminorum	7	111 43 30	+21 1 5	+ 13,6	+ 1,5	4
1079	80 Geminorum π	5	111 50 0	+34 7 45	+ 91,9	+ 3,8	1
1080	81 Geminorum g	6	112 1 40	+19 13 25	+ 16,7	+ 0,3	10

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1081*	11 Canis Minoris	6	112° 16′ 40″	+11° 28′ 55″	+ 57,9	+ 7,0	1
1082*	82 Geminorum	6	112 30 0	+23 51 55	— 24,9	+ 0,7	1
1083*	Geminorum	7	112 41 —	+28 56 5			1
1084*	Lyncis	6	112 45 —	+33 58 0			1
1085*	1 Navis σ	6	112 47 10	−27 41 30	−105,3	— 38,8	1
1086	2 Navis	6	112 48 10	−13 58 5	+ 15,8	— 11,0	2
1087*	52 Camelopardi	5½	112 48 30	+57 16 0	−152,6	— 17,9	1
1088	3 Navis τ	4½	112 49 40	−28 14 45	+ 56,5		1
1089*	Navis	6	112 53 50	−23 58 50			2
1090*	4 Navis	6	112 55 0	−13 50 25	+ 5,6	— 7,3	2
1091	25 Lyncis	6	112 57 0	+48 7 50	— 15,5	+ 25,6	2
1092*	26 Lyncis	6	113 0 0	+48 19 30	— 20,1	— 26,0	2
1093*	5 Navis	6	113 21 0	−11 27 40	+ 17,7		1
1094*	Navis		113 27 —	−21 48 20			1
1095	83 Geminorum φ	5	113 37 0	+27 31 30	— 7,0	— 1,2	8
1096*	84 Geminorum	5	113 39 15	+23 5 30	— 56,1	— 6,7	1
1097	53 Camelopardi	6	113 46 45	+61 6 45	−204,6	— 2,1	1
1098*	12 Canis Minoris	5½	113 47 40	+13 18 55			
1099*	Navis ϑ	4½	113 47 40	−25 12 5			2
1100	13 Canis Min. ζ	5	113 54 10	+ 2 31 10	— 2,6	+ 9,9	1
1101	6 Navis	5	113 55 30	−16 28 10	— 11,0	— 11,6	2
1102	7 Navis ξ	3½	114 3 30	−24 7 0	+ 26,1	+ 17,2	5
1103*	8 Navis	5½	114 17 40	−12 3 35	+ 5,9	+ 1,9	1
1104*	54 Camelopardi	6	114 19 45	+58 4 45			1
1105*	9 Navis	5	114 21 0	−13 6 20	+ 13,1	— 29,5	2
1106*	85 Geminorum	6	114 23 0	+20 39 55	— 7,1	— 16,4	14
1107*	10 Navis	6	114 30 0	−14 4 40	+ 43,7	— 11,9	1
1108*	Canis Minoris		114 42 15	+ 9 38 40			1
1109	1 Cancri	6	114 49 30	+16 34 15	+ 40,7	+ 25,5	5
1110*	Canis Minoris		115 8 5	+ 9 25 45			1
1111*	Cancri	6½	115 11 40	+17 18 45			2
1112*	55 Camelopardi	5	115 20 —	+69 18 50			1
1113	2 Cancri ω¹	6	115 31 30	+26 11 55	+ 11,6	+ 4,1	8
1114	14 Canis Minoris	6	115 33 45	+ 3 0 55	— 7,4	+ 14,2	1
1115	3 Cancri	6	115 44 0	+18 6 55	+ 42,8	+ 41,5	6

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1116*	4 Cancri ω^2	6	115° 44′ 20″	+25° 53′ 45″	″	+ 19″,4	2
1117*	11 Navis *e*	4½	115 53 10	−22 5 0	− 7,9	+ 2,9	4
1118	5 Cancri	6	115 56 45	+17 16 15	+ 10,3	− 1,3	6
1119*	27 Monocerotis	5	116 3 30	− 2 52 20	+ 3,6	+ 10,6	5
1120*	6 Cancri	5	116 6 0	+28 37 5	+ 3,7	+ 10,9	6
1121	27 Lyncis	5	116 17 0	+52 20 50	−190,3	+ 5,5	2
1122	28 Monocerotis	5	116 21 30	− 0 34 10	+ 9,4	+ 7,2	5
1123*	7 Cancri	8	116 23 0	+22 53 45	− 46,3	+ 12,4	2
1124	12 Navis	6	116 27 10	−22 29 45	− 2,6	− 5,3	2
1125	13 Navis	4	116 31 0	+ 3 8 15	+ 45,3	+ 52,3	2
1126*	Cancri		116 38 0	+28 22 5			2
1127*	28 Lyncis	7	116 41 0	+44 6 25	− *	− 16,1	1
1128*	Cancri	6	116 46 —	+19 40 45			1
1129*	Navis	5	116 56 —	−17 34 50			1
1130	8 Cancri	6	116 57 0	+13 57 35	− 31,1	+ 8,7	3
1131	9 Cancri μ^1	7	116 58 45	+23 28 35	− 31,9	+ 13,8	4
1132*	Camelopardi		117 18 15	+57 — ?			1
1133*	10 Cancri μ^2	6½	117 22 30	+22 26 25	− 31,5	+ 1,7	14
1134*	11 Cancri	7½	117 26 40	+28 20 35	+ 12,8	− 7,3	2
1135*	56 Camelopardi	6	117 27 15	+61 15 40	−421,8	− 0,3	1
1136*	Navis	6	117 38 30	−16 47 40			2
1137	14 Navis	6	117 44 50	−18 52 25	− 53,1	− 23,6	1
1138	12 Cancri	6	117 48 15	+14 29 55	+113,8	+ 24,8	2
1139*	Lyncis	6	117 48 30	+60 4 40	−210,8		1
1140*	13 Cancri ψ^1	7	117 51 0	+26 42 55	+ 11,4	+ 0,5	2
1141*	14 Cancri ψ^2	7	117 55 30	+26 24 30	+ 4,3	− 24,9	6
1142*	29 Lyncis	6	117 59 30	+60 27 50	−205,3	+ 7,4	4
1143*	Navis	6	118 0 —	−19 42 30			1
1144*	57 Camelopardi	5	118 0 40	+63 24 25	−147,7	− 2,0	1
1145	29 Monocerotis	6	118 15 0	− 2 7 15	+ 0,4	+ 19,9	3
1146*	Cancri	7	118 23 10	+15 30 5			1
1147*	15 Cancri	6	118 27 45	+30 32 15	− 4,0	+ 23,3	4
1148	15 Navis	3	118 34 40	−23 26 25	+ 28,8	+ 5,6	3
1149*	Cancri	6	118 35 2	+10 42 40			2
1150	16 Cancri ζ	5½	118 35 30	+18 32 25	+ 18,5	+ 2,7	26

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1151*	58 Camelopardi	6	118° 45′ 0″	+58° 39′ 45″	+ 81,3	− 26,3	3
1152*	Cancri	7	118 45 15	+14 53 5			1
1153	16 Navis	5	118 47 40	−18 21 50	+ 11,0	− 15,0	4
1154*	Navis	6	118 48 30	−15 22 10			2
1155*	17 Navis	6	118 49 0	−12 21 30			0
1156	18 Navis	6	119 4 30	−12 54 50	− 23,6	− 15,0	1
1157*	Cancri		119 5 50	+18 33 0			3
1158*	Lyncis	7	119 8 —	+60 5 15			1
1159*	Lyncis	6½	119 8 —	+61 33 15			1
1160*	19 Navis	5½	119 11 0	−12 2 10	− 7,3	− 6,3	3
1161*	Cancri	7	119 15 55	+13 56 45			1
1162	20 Navis	5½	119 47 0	−14 52 55	− 44,1	− 9,4	3
1163	17 Cancri β	3¾	119 55 0	+10 6 15	+ 1,0	+ 0,9	34
1164	18 Cancri χ	6	120 17 0	+28 10 50	+ 13,7	− 20,2	5
1165	31 Lyncis	5	120 22 0	+44 8 0	− 14,4	+ 10,8	3
1166*	Cancri	5	120 22 50	+21 40 45			1
1167	19 Cancri λ	6	120 30 40	+24 57 20	− 9,1	+ 15,4	10
1168*	21 Navis	6	120 47 30	−15 20 50	+ 4,4	− 30,7	1
1169	1 Ursæ Maj. o	4½	121 2 30	+61 41 30	− 70,8	+ 27,4	6
1170	20 Cancri d^1	6	121 23 15	+19 16 55	+ 4,7	+ 28,0	16
1171*	2 Ursæ Maj. A	5	121 38 0	+66 8 20	−222,9	+ 16,2	3
1172*	21 Cancri	6	121 43 15	+11 36 0	+ 41,5	− 15,9	3
1173*	22 Cancri ϕ^1	6½	121 52 50	+28 52 25	+ 1,3	− 1,0	3
1174	23 Cancri ϕ^2	6	121 59 15	+27 54 10	+ 12,0	+ 22,5	5
1175	24 Cancri υ^1	6	122 2 45	+25 30 50	− 30,6	+ 2,5	5
1176	25 Cancri d^2	6	122 3 45	+18 2 0	− 28,7	− 17,5	8
1177	22 Navis	6	122 4 0	−12 5 30	− 55,3	+ 7,2	1
1178*	1 Hydræ	6	122 16 0	− 2 47 30	+ 28,8	+ 54,2	4
1179	27 Cancri	6	122 23 0	+13 37 35	+ 9,5	+ 50,9	4
1180	30 Monocerotis	6	122 32 0	− 2 55 50	+ 17,2	+ 10,9	9
1181*	28 Cancri υ^2	6½	122 32 30	+25 7 40	− 6,6	+ 27,5	8
1182*	2 Hydræ	6	122 44 0	− 3 0 30	+ 14,8	+ 25,2	4
1183*	Cancri	6	122 47 40	+25 20 15			1
1184	29 Cancri	6½	122 49 0	+15 11 10	+ 9,6	+ 55,2	6
1185*	3 Ursæ Majoris	6	122 50 30	+66 2 0	−224,6	+ 14,4	3

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR	D.	
1186*	4 Ursæ Maj. π	5	123° 11′ 0″	+65° 21′ 20″	−151″,8	+ 1″,2	3
1187*	30 Cancri υ^3	6	123 16 30	+25 5 0	— 1,3	+ 20,5	6
1188	32 Lyncis	7	123 22 0	+37 26 20		+ 30,0	3
1189	31 Cancri θ	5¾	123 28 15	+19 6 20	— 22,8	— 0,4	16
1190*	Ursæ Majoris		123 36 15	+61 1 40			1
1191*	32 Cancri	7½	123 39 15	+25 6 0	— 14,5	+ 5,0	5
1192	33 Lyncis	6	123 40 0	+37 26 0	+ 20,3	+ 30,4	3
1193	33 Cancri η	6½	123 41 15	+21 27 25	— 31,7	+ 1,8	20
1194*	34 Cancri	6	123 55 40	+11 4 20	+ 40,7	+ 37,7	3
1195*	35 Cancri	7	124 21 30	+20 37 15	+ 6,5	— 3,4	5
1196*	Cancri	7	124 28 45	20 — —			1
1197*	Cancri	7	124 37 —	+29 20 15			1
1198*	Ursæ Majoris	6	— — —	+50 6 —			1
1199*	Ursæ Majoris	6	— — —	+50 1 —			1
1200	1 Leonis Minoris	7	124 42 40	+33 51 30	— 5,9	— 47,3	1
1201	34 Lyncis	6	124 53 0	+46 52 30	−119,5	+ 21,1	1
1202	36 Cancri c^1	6	125 4 0	+10 41 15	— 11,9	+ 44,3	2
1203	3 Hydræ	6	125 4 15	— 6 57 5	+ 42,2	+ 22,7	1
1204*	2 Leonis Minoris	6	125 7 0	+33 33 45	−324,9	+ 17,4	1
1205*	Cancri		125 11 50	+20 18 50			1
1206*	3 Leonis Minoris	6	125 12 30	+33 47 30	−309,0	— 33,1	1
1207	4 Hydræ δ	4	125 17 45	+ 6 44 55	+ 25,5	+ 11,6	5
1208*	37 Cancri c^2	6½	125 18 30	+10 36 45	+ 21,1	+ 42,2	2
1209*	4 Leonis Minoris	7	125 20 0	+33 0 35	−324,4	— 39,1	1
1210*	Ursæ Majoris		125 25 45	+67 47 40			1
1211*	38 Cancri	8	125 27 45	+20 49 45		+ 22,0	4
1212*	Cancri f		125 30 30	+20 46 10		— *	1
1213*	39 Cancri	6	125 33 30	+21 4 30	— 16,6	— 31,6	1
1214*	40 Cancri	6	125 34 45	+21 1 50	— 16,8	— 4,8	2
1215	5 Hydræ σ	5	125 37 30	+ 4 23 45	+ 22,5	+ 6,1	3
1216*	Cancri		125 37 45	+21 — —	— 10,6		1
1217*	41 Cancri ε	7	125 40 0	+20 36 20		— 0,8	4
1218*	42 Cancri	7½	125 42 45	+20 46 45		+ 8,6	1
1219*	Cancri		125 45 10	+20 38 45		— 8,0	2
1220*	Ursæ Majoris		125 50 35	+56 4 35			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1221	43 Cancri γ	4	126° 19′ 0″	+22° 32′ 50″	+ 3″,6	− 2″,9	28
1222	6 Hydræ	6	126 19 15	−11 24 30	+ 43,6	+ 1,3	1
1223*	44 Cancri	8	126 21 45	+19 13 30	+ 6,9		1
1224	45 Cancri A^1	6	126 31 0	+13 44 45	+ 8,6	+ 47,9	5
1225*	46 Cancri σ^1	6	126 33 30	+31 46 55	+ 3,3	+ 70,4	1
1226	7 Hydræ η	4	126 44 45	+ 4 28 30	+ 11,6	+ 18,5	4
1227	47 Cancri δ	4	126 45 10	+19 15 25	− 5,9	− 4,4	35
1228	9 Hydræ	6	126 50 30	−14 51 50		+ 12,8	3
1229*	5 Ursæ Maj. b	6	126 52 15	+63 4 15	−108,9	− 4,3	1
1230	48 Cancri ι	5	126 57 30	+29 51 20	+ 5,9	+ 5,1	5
1231	49 Cancri b	6	126 57 50	+11 9 40	+ 26,6	+ 35,9	4
1232*	7 Ursæ Majoris	6	127 — —	+61 59 0			1
1233*	31 Monocerotis	5	127 6 0	− 6 8 50	+ 36,5	+ 0,5	4
1234*	10 Hydræ	5	127 8 15	+ 6 45 45	+ 20,6	+ 33,9	1
1235	6 Ursæ Majoris	5	127 25 0	+65 44 10	−178,2	+ 6,9	2
1236*	Ursæ Majoris	6	127 25 —	+66 39 10			1
1237*	Ursæ Majoris	6	127 25 —	+67 9 50			1
1238	50 Cancri A^2	6	127 28 20	+13 12 5	+ 5,1	+ 47,2	2
1239	11 Hydræ ε	4	127 34 15	+ 7 31 15	+ 24,9	+ 9,3	5
1240*	35 Lyncis	6	127 44 10	+44 49 50	+ 7,4	+ 39,3	4
1241*	Hydræ	6	127 48 —	− 0 48 10			1
1242*	5 Leonis Minoris	7	127 53 0	+34 24 0	−310,3	+ 19,4	1
1243	12 Hydræ	6	127 55 30	−12 27 5	+ 27,1	+ 31,2	1
1244	13 Hydræ ρ	5	127 59 15	+ 6 56 55	+ 27,8	+ 9,7	5
1245*	51 Cancri	6	128 19 30	+33 35 55		+ 3,3	5
1246*	52 Cancri	7	128 25 15	+17 7 30	+ 76,2	− 13,7	1
1247	53 Cancri ρ^1	6	128 25 20	+29 23 5	+ 45,5	+ 8,3	3
1248	54 Cancri	7	128 25 30	+16 27 10	+ 0,7	+ 58,1	6
1249	14 Hydræ	5½	128 26 15	− 2 19 55	+ 20,3	+ 33,5	2
1250*	55 Cancri ρ^2	6	128 30 20	+29 28 45	− 20,5	− 11,6	4
1251	8 Ursæ Maj. ρ	5	128 31 0	+68 46 40	−190,5	+ 28,7	3
1252*	57 Cancri σ^2	5½	128 48 0	+31 43 5	+ 26,2	− 1,4	2
1253*	Hydræ	6	128 59 10	+ 6 28 30			1
1254*	15 Hydræ	6	129 4 30	− 6 2 55	+ 25,9	+ 14,9	4
1255*	58 Cancri ρ^3	6	129 15 15	+29 4 35	− 16,0	− 2,1	3

No.	STAR.	Mag.	Right Ascens. 1690.			Declination. 1690.			Difference from Bradley.		No. of Obs.
									AR.	D.	
			°	′	″	°	′	″	″	″	
1256*	59 Cancri	5½	129	25	0	+34	3	55	+ 22,5	+ 5,0	4
1257*	9 Ursæ Maj. ι	4	129	28	0	+49	13	10	—143,4	— 9,6	6
1258	16 Hydræ ζ	4	129	44	0	+ 7	5	40	+ 27,9	+ 5,0	8
1259*	60 Cancri	6	129	44	0	+12	46	40	+ 17,1	+ 4,8	11
1260*	61 Cancri σ^3	6	129	45	10	+31	23	15	+ 26,4	+ 9,6	2
1261*	62 Cancri o^1	6	129	58	0	+16	27	50	+ 38,9	+ 57,1	14
1262*	63 Cancri o^2	6	130	3	20	+16	43	35	+ 4,2	+ 47,9	13
1263	17 Hydræ	6	130	4	0	— 6	49	10	+ 28,5	+ 19,4	2
1264*	10 Ursæ Majoris	4	130	5	0	+42	57	30	— 25,8	+ 40,4	5
1265*	64 Cancri	6	130	5	20	+33	35	10	+ 26,2	+ 3,6	4
1266*	11 Ursæ Maj. σ^4	6	130	10	0	+68	4	0	—556,0	+ 3,7	2
1267*	Cancri		130	13	5	+10	33	15			1
1268	65 Cancri α	4	130	22	0	+13	1	25	+ 20,6	+ 10,2	26
1269*	66 Cancri	6	130	34	0	+33	25	30	+ 10,9	+ 10,9	5
1270	12 Ursæ Maj. κ	4	130	35	0	+48	20	30	— 86,6	+ 8,8	5
1271*	13 Ursæ Maj. σ^5	5	130	39	0	+68	20	30	—161,8	+ 2,5	3
1272*	67 Cancri	6½	130	48	30	+29	5	5	0,0	+ 12,8	2
1273*	Ursæ Majoris	6	130	53	5	+52	1	25			1
1274*	68 Cancri	8	131	5	0	+18	16	10	— 58,2	— 19,0	1
1275	69 Cancri ν	6	131	7	0	+25	38	5	+ 58,4	+ 18,2	6
1276*	14 Ursæ Maj. τ	5	131	13	0	+64	44	10	— 25,3	— 29,3	3
1277*	Hydræ	6	131	16	25	+ 6	49	5			2
1278*	Hydræ	6	131	20	50	+ 6	49	55			1
1279*	70 Cancri	6½	131	23	45	+29	5	25	+ 6,9	+ 4,3	2
1280*	Cancri	8	131	39	5	+25	47	55			1
1281*	Lyncis	5½	131	40	30	+39	38	45			3
1282	15 Ursæ Maj. *f*	5	131	42	0	+52	49	10	— 84,2	— 10,6	4
1283*	Ursæ Maj.	7	131	47	—	+74	11	—			1
1284*	Ursæ Maj.		131	56	5	+62	54	0			1
1285*	71 Cancri	7	132	4	20	+18	35	50	+ 10,1		1
1286	72 Cancri τ	6½	132	19	0	+30	51	50	+ 6,2	+ 13,5	3
1287*	16 Ursæ Maj. *c*	5	132	20	40	+62	39	30	— 24,8	— 4,5	2
1288	18 Hydræ ω	6	132	23	45	+ 6	17	55	+ 33,6	+ 15,3	3
1289	75 Cancri	6½	132	37	15	+27	52	35	— 34,3	— 9,4	3
1290*	76 Cancri κ	6½	132	43	30	+11	52	55	+ 8,8	+ 14,1	11

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1291	77 Cancri ξ	5½	132° 51′ 40″	+23° 15′ 55″	+ 14″,5	+ 9″,9	5
1292*	78 Cancri	7½	132 54 20	+18 42 0	— 24,3	— 22,5	2
1293	79 Cancri	8	133 7 15	+23 13 20	— 23,4	+ 7,8	3
1294	17 Ursæ Majoris	5	133 9 30	+57 59 40	—129,1	— 26,5	2
1295	36 Lyncis	5½	133 21 15	+44 27 50	— 78,0	— 13,0	2
1296	19 Hydræ	6	133 22 0	— 7 22 10	+ 53,9	+ 25,8	2
1297*	18 Ursæ Maj. *e*	5	133 24 0	+55 16 10	— 31,6	— 77,8	1
1298	20 Hydræ	6	133 35 15	— 7 33 55	+ 72,7	+ 32,2	1
1299	80 Cancri	7	133 37 20	+19 17 0	— 13,5	— 0,6	3
1300*	81 Cancri π^1	7	133 49 30	+16 12 55	— 58,7	+ 15,6	12
1301*	Hydræ	7	134 0 0	+ 5 6 20			2
1302	19 Ursæ Majoris	6	134 1 0	+35 53 45	— 14,3	— 52,7	1
1303	20 Ursæ Majoris	7	134 7 0	+61 2 30	—137,8	+ 27,6	1
1304	21 Hydræ	6	134 15 45	— 5 52 20	+ 63,0	+ 25,7	1
1305*	82 Cancri π^2	6	134 31 0	+16 11 35	— 8,3	+ 14,8	24
1306	22 Hydræ θ	4	134 33 45	+ 3 36 15	— 29,0	— 52,9	6
1307*	Ursæ Majoris	6	134 40 —	+57 58 40			1
1308	37 Lyncis	6	134 45 30	+52 31 0	— 97,8	+ 44,0	1
1309*	38 Lyncis	4	134 50 0	+38 4 40	+ 65,9	+ 0,5	4
1310*	Cancri		135 14 25	+12 45 40			3
1311*	39 Lyncis	6	135 18 10	+50 49 20	— *	+ 20,0	1
1312	23 Hydræ	6	135 18 30	— 5 5 40	+ 60,8	+ 31,6	1
1313	24 Hydræ	6	135 22 0	— 7 29 0	+ 31,0	+ 23,7	3
1314	83 Cancri	6	135 23 45	+18 59 5	+ 22,1	+ 15,3	2
1315*	40 Lyncis	4	135 30 0	+35 40 10	+ 17,7	+ 10,6	9
1316*	25 Hydræ	5	135 37 45	—10 41 25	+ 45,4	+ 16,9	1
1317*	21 Ursæ Majoris	6½	135 53 0	+55 18 50	—179,1	— 1,9	1
1318	26 Hydræ	6	136 11 45	—10 41 55	+ 52,2	+ 24,0	3
1319*	Hydræ	7	136 17 45	— 8 20 0	+ 30,4	+ 48,6	1
1320*	22 Ursæ Majoris	7	136 18 0	+73 31 25	— *	+ 34,2	2
1321	27 Hydræ	6	136 19 45	— 8 16 30	+ 26,1	+ 31,8	5
1322	6 Leonis Minoris	6	136 20 0	+26 29 0	+143,1	— 18,5	1
1323*	1 Leonis κ	5	136 37 0	+27 29 5	+ 33,3	+ 6,3	6
1324*	23 Ursæ Maj. *h*	4	136 38 30	+64 22 0	— 1,4	+ 39,1	3
1325*	24 Ursæ Maj. *d*	4½	136 39 0	+71 9 10	—400,6	— 8,3	3

No.	STAR.	Mag	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1326*	Ursæ Majoris		136° 52′ 30″	+57° 4′ 0″	″	″	1
1327	41 Lyncis	6	137 4 0	+46 55 0	−126,7	+ 31,8	2
1328	28 Hydræ *A*	6	137 28 7	− 3 48 35	+ 15,4	+ 14,3	3
1329*	2 Leonis *ω*	6	137 57 0	+10 22 15	+ 17,9	+ 35,4	5
1330	3 Leonis	6	137 58 30	+ 9 30 35	+ 41,9	+ 14,4	2
1331	7 Leonis Minoris	6	137 59 0	+34 59 15		+ 2,4	1
1332*	25 Ursæ Maj. *θ*	3½	137 59 0	+53 3 20	−184,1	− 24,9	6
1333	29 Hydræ	6	138 0 15	− 7 54 0	+ 38,4	− 8,3	1
1334	30 Hydræ *α*	2	138 4 52	− 7 20 25	+ 24,2	+ 6,4	12
1335*	Hydræ	6	138 5 25	− 4 44 35			2
1336	8 Leonis Minoris	5	138 9 0	+36 26 30	− 62,5	+ 1,9	2
1337	31 Hydræ τ^1	5	138 21 0	− 1 26 55	+ 9,6	+ 35,0	3
1338*	26 Ursæ Majoris	5½	138 21 0	+53 23 50	−107,9	− 4,4	6
1339*	Hydræ	6	138 25 30	− 0 52 30			2
1340*	27 Ursæ Majoris	6	138 28 15	+73 36 5	−550,8	+ 55,0	1
1341	4 Leonis *λ*	4	138 28 40	+24 18 5	+ 34,3	+ 18,5	7
1342	9 Leonis Minoris	6	138 36 20	+37 50 0	− 77,5	− 9,0	2
1343*	Ursæ Majoris	7	138 45 —	+70 35 45			1
1344	10 Leonis Minoris	4½	138 45 45	+37 44 25	+ 31,5	+ 12,4	4
1345*	5 Leonis *ξ*	5½	138 47 30	+12 37 45	+ 16,5	+ 57,5	18
1346	6 Leonis *h*	6	138 49 20	+11 3 0	+ 8,2	+ 24,2	9
1347*	Leonis Minoris	6	138 54 —	+40 57 45			1
1348*	32 Hydræ τ^2	6	139 2 30	+ 0 8 55	− 9,3	+ 35,9	3
1349*	Hydræ	6	139 10 25	+ 3 12 40			2
1350	11 Leonis Minoris	6	139 13 15	+37 10 35	− 22,4	+ 13,0	3
1351	7 Leonis	6	139 42 30	+15 43 35	+ 42,2	+ 40,9	2
1352*	42 Lyncis	6	139 43 15	+41 35 40	− 48,5	+ 30,3	5
1353	33 Hydræ	6	139 45 15	− 4 34 20	+ 49,1	+ 60,6	1
1354	8 Leonis	6	139 58 30	+17 47 50	− 19,6	+ 12,5	2
1355	9 Leonis	6	140 1 30	+26 1 55	− 96,4	+ 16,4	1
1356*	Leonis Minoris	6	140 9 25	+36 36 25			1
1357*	10 Leonis	5	140 11 30	+ 8 11 35	+ 37,0	+ 27,8	6
1358	11 Leonis	6	140 15 0	+15 42 55	+ 42,1	+ 17,2	1
1359*	Leonis Minoris	6	140 17 40	+36 42 20			1
1360	12 Leonis	7½	140 20 30	+26 43 55	− 75,8	+ 17,4	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
1361*	28 Ursæ Majoris	5	140° 24′ 0″	+65° 3′ 0″	− 91,2	− 29,8	1
1362	2 Sextantis	5	140 32 30	+ 6 1 35	+ 69,4	− 7,6	2
1363*	Leonis		140 35 45	+14 41 25			1
1364*	34 Hydræ	6	140 39 30	− 8 3 20	+ 23,2	+ 6,2	3
1365	43 Lyncis	6¾	140 40 15	+41 8 30	− 57,8	− 1,1	4
1366*	Ursæ Majoris	5	140 50 35	+64 40 0	+ 6,6		1
1367	13 Leonis	6	140 57 20	+27 17 35	−101,3	+ 15,1	2
1368*	13 Leonis Minoris	6	140 59 30	+36 28 40		+ 14,2	1
1369	35 Hydræ *i*	4	141 0 0	+ 0 13 50	+ 8,7	+ 36,0	2
1370	44 Lyncis	5½	141 4 15	+58 31 45	− 86,8	− 32,3	1
1371*	Leonis		141 5 45	+31 30 —			1
1372	14 Leonis *o*	3½	141 8 20	+11 16 15	− 1,1	+ 24.1	28
1373*	37 Hydræ	6	141 10 —	− 9 11 30		+ 3,2	1
1374*	Hydræ	6	141 18 0	− 9 23 15	+ 19,8	+ 1,0	1
1375	15 Leonis *f*	6	141 18 0	+31 22 15	+ 58,9	+ 1,6	1
1376*	38 Hydræ *κ*	4½	141 19 20	−12 56 30	+145,3	− 24,9	1
1377	16 Leonis *ψ*	6	141 42 10	+15 24 15	− 14,3	+ 40,8	18
1378*	14 Leonis Minoris	6	141 44 0	+46 31 40	−226,7	− 15,2	1
1379	17 Leonis *ε*	3	142 2 0	+25 10 15	+ 26,4	+ 20,0	9
1380	29 Ursæ Maj. *υ*	4	142 9 0	+60 28 20	−115,3	− 33,5	4
1381*	15 Leonis Minoris	6	142 11 0	+47 26 5	−317,5	+ 1,8	1
1382	18 Leonis	6	142 24 30	+13 12 15	+ 5,7	+ 33,8	8
1383*	Sextantis	6	142 25 5	+ 8 5 58			3
1384	19 Leonis	7	142 40 0	+12 57 55	+ 46,9	+ 46,7	4
1385	30 Ursæ Maj. *φ*	5	142 41 0	+55 29 20	− 49,5	− 17,1	4
1386*	16 Leonis Minoris	6	142 46 40	+41 3 10	− 89,5	− 17,6	1
1387	20 Leonis	6	143 5 0	+22 35 35	+ 63,7	+ 25,4	2
1388*	3 Sextantis	6	143 8 0	− 5 50 0	+251,5	+ 17,3	1
1389*	17 Leonis Minoris	7	143 22 0	+39 20 10	− 51,5	+ 21,9	1
1390	21 Leonis	7	143 31 15	+13 15 35	+ 10,1	+ 25,6	1
1391*	22 Leonis *g*	6	143 32 30	+25 50 0	+ 21,5	+ 5,5	3
1392	23 Leonis	6	143 33 30	+14 29 25	− 24,5	+ 9,2	3
1393	4 Sextantis	6	143 33 30	+ 5 46 15	+ 85,9	− 2,8	1
1394*	5 Sextantis	6	143 44 30	− 5 57 5	+ *		1
1395	24 Leonis *μ*	3½	143 45 0	+27 26 55	+ 23,2	− 24,5	5

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1396*	Leonis Minoris	7	143° 45′ — ″	+36° 24′ 55″	″	″	1
1397	31 Ursæ Majoris	6	143 47 30	+51 15 15	+ 38,8	+ 7,3	2
1398	6 Sextantis	6	143 52 30	− 2 48 50	+ 95,0	+ 3,9	1
1399	7 Sextantis	6	144 1 0	+ 3 52 30	+ 96,9	+ 15,6	2
1400	39 Hydræ υ^1	5	144 9 15	−13 24 45	− 29,6	− 3,2	2
1401	8 Sextantis	6	144 18 0	− 6 40 0	− 39,5	− 1,9	3
1402*	Sextantis	6	144 19 30	+ 7 23 40			4
1403	9 Sextantis	6	144 26 30	+ 6 22 55	+100,3	+ 7,5	1
1404*	Leonis Minoris	7	144 32 —	+46 52 0			1
1405*	Ursæ Majoris	6	144 32 45	+58 16 30			2
1406	18 Leonis Minoris	6	144 33 30	+33 49 40	− 21,8	+ 6,6	1
1407	19 Leonis Minoris	5½	144 38 30	+42 30 15	− 56,6	+ 5,2	6
1408*	10 Sextantis	6	144 58 30	+10 22 50	+ 45,5	− 2,2	6
1409	26 Leonis	7	145 19 0	+16 40 25	+ 14,2	+ 13,1	1
1410*	27 Leonis ν	5½	145 22 20	+13 53 35	+ 5,7	+ 29,9	24
1411*	11 Sextantis	5½	145 24 10	+ 9 46 20	+ 51,8	− 1,2	9
1412*	Leonis Minoris	7	145 24 25	+31 5 55			1
1413	20 Leonis Minoris	6	145 45 0	+33 25 0	− 26,7	+ 3,3	5
1414*	Ursæ Majoris	6	145 48 0	+55 22 10	+ *		1
1415*	12 Sextantis	6	145 52 30	+ 4 50 40			1
1416	29 Leonis π	4	145 56 15	+ 9 30 15	+ 37,0	+ 20,4	22
1417*	Leonis Minoris	7	145 58 15	+32 59 45			1
1418*	Sextantis		146 7 —	+10 24 50			1
1419*	Leonis	5	146 23 25	+23 24 10			4
1420*	Leonis Minoris	7	146 26 35	+34 6 50			1
1421*	Leonis Minoris	7	146 30 35	+33 55 30			1
1422*	Sextantis		146 41 —	+ 9 42 0			1
1423*	Leonis Minoris	7	146 50 40	+36 28 20			1
1424	13 Sextantis	6	146 59 40	+ 4 41 20	+ 69,0	− 3,4	3
1425*	Sextantis	6½	147 7 10	+ 6 29 32			2
1426*	Leonis	6	147 12 10	+17 14 22			2
1427	21 Leonis Minoris	5	147 14 30	+36 43 45	+ 32,7	+ 13,7	5
1428	40 Hydræ υ^2	5	147 31 0	−11 35 0	− 29,3	+ 7,1	1
1429*	Leonis Minoris	6	147 31 —	+33 6 45			1
1430	30 Leonis η	3½	147 35 15	+18 14 55	+ 19,5	+ 17,6	52

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1431	14 Sextantis	6	147° 37′ 0″	+ 7° 6′ 20″	+ 74″,8	− 11″,0	3
1432	31 Leonis *A*	5	147 51 0	+11 29 25	+ 5,6	+ 18,9	9
1433	32 Leonis α	1	147 57 0	+13 27 15	− 1,5	+ 28,9	133
1434	15 Sextantis	4	147 59 30	+ 1 7 40		− 14,0	2
1435	16 Sextantis	6	148 14 30	+ 7 40 30	+ 3,9	− 14,4	3
1436	33 Leonis	6½	148 29 0	+17 12 15			1
1437*	17 Sextantis	6	148 38 30	− 6 54 10	+140,4	− 6,2	3
1438	34 Leonis	7	148 43 30	+14 51 30	+ 12,8	+ 9,3	6
1439*	Hydræ		148 44 30	−11 18 30			1
1440	32 Ursæ Majoris	5	148 46 15	+66 37 15	−164,5	+ 28,5	1
1441*	41 Hydræ λ	4	148 52 0	−10 50 35	+ 5,5	+ 5,4	3
1442	18 Sextantis	6	148 53 0	− 6 54 20	− 10,9	− 10,3	3
1443*	Sextantis	6	148 58 —	− 5 48 30		+ 1,9	2
1444	19 Sextantis	6	149 8 30	+ 6 7 50	+ 72,1	− 11,1	3
1445*	Leonis Minoris	5	149 10 —	+38 54 15			1
1446*	Ursæ Majoris	3	149 11 —	+70 16 50			1
1447	22 Leonis Minoris	6½	149 17 30	+32 59 20	+ 7,4	− 9,9	3
1448*	20 Sextantis	6½	149 32 30	− 5 52 10	+135,3	+ 3,3	3
1449*	33 Ursæ Maj. λ	3½	149 34 0	+44 26 30	− 71,1	− 4,3	2
1450	23 Leonis Minoris	5½	149 37 30	+30 49 45	+ 9,0	+ 20,5	4
1451	21 Sextantis	6	149 39 40	− 6 28 10	+ 45,7	− 15,5	2
1452	24 Leonis Minoris	6	149 43 0	+30 12 40	− 76,1	+ 3,8	2
1453	35 Leonis	6	149 49 45	+25 1 40	− 41,0	− 8,4	6
1454*	36 Leonis ζ	3	149 50 10	+24 56 10	+ 18,2	+ 19,0	25
1455*	37 Leonis	6	149 59 15	+15 14 45	+ 29,5	+ 33,5	7
1456	39 Leonis	6	150 2 30	+24 38 5	− 82,2	+ 17,7	2
1457*	Ursæ Majoris	4	150 20 0	+67 6 0		+ 35,1	1
1458	22 Sextantis	6	150 33 45	− 6 32 30	− 28,4	+ 13,5	6
1459	25 Leonis Minoris	6	150 37 0	+43 23 0	−146,4	+ 10,9	1
1460	40 Leonis	6	150 41 20	+21 1 15	+ 17,7	+ 1,3	13
1461	41 Leonis γ	2	150 41 50	+21 23 5	+ 41,8	+ 10,6	67
1462*	Ursæ Majoris	6	150 54 0	+42 46 25	− 43,0		1
1463*	34 Ursæ Maj. μ	3	150 56 0	+43 1 40	− 61,0	+ 40,5	4
1464*	Leonis	6	151 3 —	+ 7 57 55			1
1465*	23 Sextantis	6	151 13 10	+ 3 49 30	+113,7	+ 20,6	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1466	26 Leonis Minoris	6	151° 15′ 0″	+36° 45′ 25″	+ 0″,5	+ 22″,4	2
1467*	42 Leonis	6	151 16 30	+16 30 25	+ 11,0	+ 47,7	7
1468	27 Leonis Minoris	6	151 18 0	+35 27 0	— 76,7	+ 16,6	3
1469*	Leonis	7	151 27 10	+10 31 5			1
1470*	Leonis Minoris	6	151 29 15	+31 10 5			1
1471*	28 Leonis Minoris	6	151 32 30	+35 15 45	+ 21,2	+ 20,3	4
1472	43 Leonis	6	151 39 15	+ 8 5 30	+124,2	+ 20,2	3
1473	24 Sextantis	6	151 51 30	+ 0 38 50	+150,0	+ 11,0	2
1474	35 Ursæ Majoris	6	151 52 30	+67 11 0			1
1475	25 Sextantis	6	151 54 30	— 2 31 30	+120,1	+ 3,0	2
1476*	Sextantis	6	151 55 45	— 2 5 25			1
1477	29 Leonis Minoris	6	151 57 0	+36 58 25	— 72,6	+ 38,7	1
1478	30 Leonis Minoris	$4\frac{3}{4}$	151 59 30	+35 20 55	+ 32,2	+ 25,8	4
1479	44 Leonis	$5\frac{1}{2}$	152 12 40	+10 20 15			8
1480	31 Leonis Minoris	5	152 26 15	+38 16 20	+ 40,9	+ 10,8	5
1481	36 Ursæ Majoris	5	152 36 15	+57 33 0	— 5,1	— 6,5	3
1482*	26 Sextantis	6	152 39 45	+ 0 33 45	+111,2	+ 34,7	2
1483	27 Sextantis	6	152 43 10	— 2 49 40	+213,6	+ 3,2	1
1484	42 Hydræ μ	4	152 47 15	—15 16 20	— 32,6	+ 8,4	4
1485	45 Leonis	6	152 48 30	+11 19 15	+ 4,3	+ 16,9	6
1486*	28 Sextantis	5	152 52 15	— 1 10 25	+ *		1
1487	32 Leonis Minoris	6	152 57 0	+40 29 20	+ 41,8	+ 15,0	2
1488*	Leonis	7	153 2 15	+15 53 50			1
1489*	Leonis		153 11 —	+11 42 50			1
1490*	Sextantis	5	153 13 0	— 2 10 20	+150,2		1
1491*	29 Sextantis	5	153 23 45	— 1 10 30	+126,0	+ 25,0	2
1492*	Sextantis	6	153 26 30	+ 3 4 0			1
1493*	33 Leonis Minoris	$5\frac{1}{2}$	153 31 0	+33 57 0	+ 59,5	+ 11,0	3
1494	30 Sextantis	5	153 36 30	+ 0 56 10	— 1,1	+ 2,0	4
1495	31 Sextantis	6	153 38 0	+ 3 44 0	— 38,4	— 29,7	1
1496	37 Ursæ Majoris	5	153 43 10	+58 39 55	— 43,6	— 15,1	2
1497*	Leonis Minoris	6	153 45 30	+41 59 55			1
1498*	46 Leonis *i*	6	153 53 30	+15 42 15	+ 22,0	+ 30,6	9
1499*	34 Leonis Minoris	6	153 54 0	+36 33 40	+ 45,5	+ 25,0	4
1500	32 Sextantis	6	154 1 10	+ 6 13 15	+ 94,6	+ 2,2	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
1501	47 Leonis ρ	4	154° 6′ 40″	+10° 52′ 50″	+ 3,4″	+ 20,6″	39
1502*	43 Hydræ ϕ^1	5	154 25 15	−15 22 5			1
1503	35 Leonis Minoris	5½	154 36 0	+37 54 50	+ 23,0	+ 10,8	2
1504	48 Leonis	6	154 38 50	+ 8 31 45	+ 0,9	+ 22,2	8
1505	49 Leonis	6	154 41 10	+10 14 5	− 9,6	+ 7,4	7
1506*	Ursæ Majoris	6	154 46 —	+70 3 5			1
1507	44 Hydræ	6	154 49 45	−22 9 55	− 4,1	+ 15,9	1
1508	36 Leonis Minoris	6	155 1 0	+35 40 0	+ 78,7	+ 13,1	2
1509*	Hydræ	6	155 3 45	−21 35 50	+ 1,1	+ 26,4	1
1510*	Ursæ Majoris	6	155 4 —	+70 40 30			1
1511*	38 Ursæ Majoris	5	155 5 15	+67 19 20	−265,0	− 8,0	2
1512*	37 Leonis Minoris	5¼	155 16 45	+33 34 0	+ 31,4	+ 14,0	6
1513*	1 Crateris ϕ^2	6	155 18 30	−14 45 0	− 57,9	− 12,5	1
1514*	38 Leonis Minoris	6	155 20 0	+39 30 20	− 94,6	+ 10,6	1
1515	50 Leonis	6¾	155 34 10	+17 42 55	− 41,9	+ 39,1	2
1516*	39 Leonis Minoris	6	155 46 0	+29 7 10	+ 20,3	+ 19,3	2
1517*	2 Crateris ϕ^3	5	155 53 30	−15 16 45	− 58,9	− 6,1	3
1518	39 Ursæ Majoris	6	155 58 0	+58 48 35	− 70,3	− 3,2	2
1519*	Leonis Minoris	6	156 13 25	+33 18 15			1
1520*	Ursæ Majoris	5	156 18 30	+47 49 15			2
1521	33 Sextantis	6	156 22 30	− 0 7 40	+105,3	− 1,4	3
1522*	40 Leonis Minoris	6	156 30 0	+27 56 15	−142,1	+ 0,7	5
1523	40 Ursæ Majoris	6	156 34 15	+58 31 45		+ 9,7	1
1524	41 Leonis Minoris	5	156 36 45	+24 47 35	+ 18,2	+ 14,5	4
1525	34 Sextantis	6	156 39 30	+ 5 11 30	− 37,8	− 7,5	1
1526	41 Ursæ Majoris	6½	156 40 30	+58 59 35	−117,7	− 34,0	1
1527	35 Sextantis	6	156 49 0	+ 6 21 30	− 26,9	+ 6,0	1
1528	42 Leonis Minoris	4½	157 7 30	+32 17 50	+ 10,6	+ 8,8	5
1529	36 Sextantis	6	157 16 10	+ 4 6 10	+ 80,2	+ 4,3	1
1530	51 Leonis *m*	6	157 25 0	+20 30 10	− 14,4	+ 30,3	2
1531	37 Sextantis	6	157 27 0	+ 7 59 40	+114,4	− 4,8	5
1532*	52 Leonis *k*	6	157 28 45	+15 48 35	+ 26,1	+ 25,1	7
1533	38 Sextantis	6	157 46 30	+ 7 58 10	+ 64,5	− 3,4	4
1534	43 Leonis Minoris	6	157 53 30	+31 1 45	+158,5	+ 45,0	1
1535	42 Ursæ Majoris	5½	157 55 0	+60 57 10	−144,7	− 10,5	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1536	43 Ursæ Majoris	6	157° 56′ 0″	+58° 12′ 15″	— 4″,8	+ 15″,7	1
1537*	3 Crateris	6	157 56 15	—15 40 30	— 31,6	— 2,1	4
1538	44 Leonis Minoris	6	158 8 30	+29 35 45	+173,3	+ 6,4	2
1539	53 Leonis *l*	6	158 13 45	+12 9 45	+ 7,3	+ 32,9	20
1540*	39 Sextantis	7	158 20 40	— 7 28 20	+ 39,6	— 2,7	1
1541	40 Sextantis	6	158 21 20	— 2 24 0	+130,4	+ 8,0	3
1542*	4 Crateris	4	158 35 30	—14 35 13	+ 3,9	+ 25,6	3
1543	44 Ursæ Majoris	6	158 37 30	+56 13 20	— 17,7	— 13,9	1
1544	41 Sextantis	6	158 39 0	— 7 16 0	+147,1	— 2,6	3
1545	45 Leonis Minoris	6	158 55 30	+29 29 45			1
1546	46 Leonis Minoris	4½	158 56 45	+35 52 20	+ 65,1	— 13,3	5
1547*	5 Crateris	6	158 59 30	—16 42 30	— 3,0	+ 23,7	1
1548	45 Ursæ Maj. *ω*	4½	159 0 0	+44 49 50	— 43,1	— 14,9	3
1549*	Sextantis	6	159 4 —	+ 2 38 40			1
1550	47 Leonis Minoris	6	159 24 30	+35 40 20	— 26,9	+ 14,3	2
1551*	48 Leonis Minoris	6	159 25 10	+27 7 55	+ 90,3	— 12,7	1
1552	46 Ursæ Majoris	6	159 30 45	+35 9 0		— 2,4	4
1553*	6 Crateris	6	159 35 45	—18 29 0	— 8,8	— 18,3	2
1554	54 Leonis	4½	159 41 0	+26 23 5	— 2,9	+ 20,6	4
1555	49 Leonis Minoris	6	159 47 0	+19 47 30	+195,4	+ 4,2	1
1556	50 Leonis Minoris	6	159 53 0	+27 8 30	+117,7	+ 7,7	2
1557	55 Leonis	5¾	159 55 50	+ 2 22 5	+ 29,6	+ 37,1	4
1558	56 Leonis	6½	159 58 0	+ 7 49 25	+ 32,5	+ 14,1	7
1559	57 Leonis	6	160 3 30	+ 2 3 45	+ 24,2	+ 43,6	1
1560	47 Ursæ Majoris	6	160 27 30	+42 4 10	+ 73,2	+ 18,0	1
1561	48 Ursæ Maj. *β*	2	160 42 30	+58 2 0	— 16,6	— 5,2	6
1562	49 Ursæ Majoris	6	160 50 0	+40 51 45	— 24,9	+ 0,1	2
1563*	50 Ursæ Maj. *α*	1½	161 2 30	+63 25 10	— 16,7	— 29,8	6
1564	58 Leonis *d*	5¾	161 7 50	+ 5 15 55	+ 14,4	+ 20,5	15
1565	59 Leonis *c*	5	161 9 10	+ 7 45 5	+ 36,5	+ 15,4	9
1566	7 Crateris *α*	4	161 12 0	—16 39 20	—112,5	— 2,4	3
1567	60 Leonis *b*	5	161 25 30	+21 50 0	+ 12,4	— 1,7	6
1568*	61 Leonis *p*′	5	161 27 50	— 0 50 5	+143,8	+ 28,5	3
1569*	51 Ursæ Majoris	6¼	161 46 15	+39 53 50	+ 44,0	+ 14,7	2
1570*	8 Crateris	6	161 50 0	—25 8 50	+120,1	— 67,9	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1571*	Leonis		161° 53′ —	+ 9° 14′ 10″	″	″	1
1572*	62 Leonis p^2	6	161 55 40	+ 1 38 55	+ 21,8	+ 35,7	4
1573	51 Leonis Minoris	6	162 7 0	+26 52 0	+119,1		1
1574	63 Leonis χ	4½	162 14 30	+ 8 59 45	+ 8,7	+ 19,7	24
1575	52 Leonis Minoris	5½	162 33 30	+27 12 0	+ 87,6	+ 11,1	3
1576*	9 Crateris	5	162 36 15	−25 37 50	+ 26,4	+ 4,6	1
1577	64 Leonis	6	162 44 45	+24 59 0	+ 7,5	+ 22,7	3
1578*	65 Leonis p^3	6	162 46 0	+ 3 37 15	— 9,0	+ 21,8	8
1579*	66 Leonis p^4	6	162 56 20	+ 0 20 15	+ *	— 0,9	Δ
1580*	67 Leonis	6	163 1 45	+26 18 55	— 9,5	+ 39,9	5
1581	52 Ursæ Maj. ψ	3½	163 2 0	+46 9 55	— 84,3	+ 19,8	4
1582*	10 Crateris	6	163 26 30	−26 24 55	+ 23,3	+ 21,1	1
1583	11 Crateris β	3½	164 7 45	−21 8 45	— 48,4	+ 8,1	2
1584*	Leonis		164 19 25	+21 50 —			1
1585	68 Leonis δ	2½	164 22 40	+22 12 25	+ 48,1	+ 15,7	19
1586*	69 Leonis p^5	5½	164 28 0	+ 1 36 5	+ 13,4	+ 27,1	5
1587*	Leonis	6	164 28 35	+ 9 44 40			3
1588	70 Leonis θ	3	164 28 40	+17 6 25	+ 8,8	+ 23,0	12
1589*	Leonis		164 29 20	+ 3 55 30			1
1590	72 Leonis	5	164 39 15	+24 46 5	+ 23,4	+ 29,6	2
1591	73 Leonis n	6	164 53 45	+14 58 55	+ 21,6	+ 29,1	3
1592*	Ursæ Majoris	6	165 6 20	+51 9 15			1
1593	74 Leonis ϕ	4	165 13 30	— 1 58 25	+ 0,2	+ 28,5	7
1594	75 Leonis	6	165 19 30	+ 3 41 55	+ 30,2	+ 24,0	6
1595*	53 Ursæ Maj. ξ	4	165 23 0	+33 15 40	— 25,2	— 23,2	5
1596	54 Ursæ Maj. ν	4	165 24 0	+34 46 5	+ 13,0	+ 34,4	3
1597	55 Ursæ Majoris	5	165 30 45	+39 52 30	+ 32,4	+ 7,3	1
1598*	76 Leonis	6	165 44 45	+ 3 20 5	+ 10,4	+ 24,5	6
1599*	Draconis	6	165 56 25	+66 1 30			1
1600	12 Crateris δ	4	165 58 15	−13 6 50	— 13,9	+ 36,3	1
1601	77 Leonis σ	4½	166 16 0	+ 7 42 45	+ 56,2	+ 30,0	22
1602	56 Ursæ Majoris	6	166 25 30	+45 10 30	— 60,4	+ 5,1	1
1603*	71 Leonis	6	166 33 30	+19 7 25			1
1604	78 Leonis ι	4	166 55 30	+12 13 10	+ 42,4	+ 33,7	18
1605*	13 Crateris λ	5½	167 1 0	−17 5 5	— 43,9	+ 4,3	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1606	79 Leonis	5½	167° 1′ 45″	+ 3° 5′ 35″	+ 7″,5	+ 36″,0	6
1607	14 Crateris ε	4	167 14 30	− 9 10 5	+ 12,4	+ 9,4	1
1608	81 Leonis	6	167 20 45	+18 8 45		+ 29,7	2
1609	15 Crateris γ	4	167 22 0	−15 59 10	− 27,1	− 8,4	1
1610*	82 Leonis	7½	167 25 30	+ 4 59 35	+ 11,4	+ 28,3	1
1611*	80 Leonis	6	167 27 45	+ 5 33 15	+ 34,8	+ 22,3	2
1612*	Leonis		167 33 45	+10 21 15			1
1613	83 Leonis	8	167 44 0	+ 4 41 35	+ 70,3	+ 24,7	1
1614	16 Crateris κ	5	167 53 30	−10 39 50	+ 6,1	+ 17,8	1
1615	84 Leonis τ	4	167 59 30	+ 4 32 55	+ 21,0	+ 31,7	14
1616	57 Ursæ Majoris	6	168 4 0	+41 2 45	− 35,8	− 31,7	2
1617	1 Draconis λ	3½	168 7 40	+71 2 0	− 92,9	+ 9,4	2
1618	85 Leonis	6	168 22 30	+17 6 45	− 2,2	+ 24,1	4
1619	58 Ursæ Majoris	6	168 26 45	+44 52 30	−188,5	− 14,9	1
1620*	Draconis	6	168 31 30	+62 47 15			1
1621	86 Leonis	6	168 34 0	+20 6 25	− 18,3	+ 17,0	4
1622	87 Leonis e	4½	168 37 15	− 1 18 5	+ 3,6	+ 9,5	8
1623	88 Leonis	6	168 56 0	+16 4 35	− 49,3	+ 20,6	2
1624	17 Crateris	6	169 12 45	−27 33 30	+121,8	− 26,3	1
1625	2 Draconis	6	169 20 30	+71 2 20	− 44,8	+ 2,2	2
1626	18 Crateris	6	169 21 0	−29 22 20	+225,4	− 29,8	1
1627*	19 Crateris	4	169 23 0	−30 8 10	+263,1	− 43,2	1
1628	89 Leonis	6	169 37 0	+ 4 46 15	+ 12,3	+ 16,4	6
1629	90 Leonis	6	169 37 15	+18 30 15	+ 40,4	+ 1,6	4
1630*	20 Crateris	6	169 48 30	−31 10 50		+ 5,9	1
1631*	Ursæ Majoris	6	170 0 —	+29 29 50			1
1632	21 Crateris θ	4	170 14 15	− 8 5 55	+ 26,8	+ 23,0	1
1633*	22 Crateris	7	170 14 45	−31 50 55		− 30,4	1
1634	91 Leonis υ	4	170 16 0	+ 0 52 25	+ 14,3	+ 38,2	16
1635	23 Crateris	6	170 21 45	−31 15 45			1
1636	59 Ursæ Majoris	6	170 25 30	+45 20 0	− 45,5	+ 24,1	1
1637	60 Ursæ Majoris	6	170 27 45	+48 32 30	−115,2	+ 23,5	1
1638	1 Virginis ω	6	170 36 50	+ 9 50 25	+ 2,6	+ 23,1	12
1639	24 Crateris ι	5	170 44 30	−11 30 15	+ 3,2	+ 24,4	1
1640*	Ursæ Majoris	6	170 51 0	+59 41 30			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1641	61 Ursæ Majoris	6	171° 8′ 15″	+35° 57′ 20″	+ 56,7″	— 46,3″	1
1642	3 Draconis	6	171 8 30	+68 26 50	+132,1	+ 33,4	1
1643*	25 Crateris	5	171 8 45	−33 0 40	+301,8	— 71,1	1
1644	92 Leonis	6	171 9 0	+23 3 55	+ 0,6	+ 17,3	3
1645	62 Ursæ Majoris	6	171 19 0	+33 27 10	+ 36,0	+ 21,7	1
1646	26 Crateris	6	171 31 0	−30 46 0		— 63,9	1
1647*	Leonis	7	171 56 0	+ 4 19 5			1
1648	27 Crateris ζ	4	172 17 0	−16 38 10	— 19,7	+ 17,9	1
1649	2 Virginis ξ	5	172 19 40	+ 9 58 5	— 14,7	+ 35,0	12
1650	63 Ursæ Maj. χ	4	172 24 0	+49 29 10	−111,5	+ 35,6	3
1651	3 Virginis ν	5	172 28 40	+ 8 15 25	— 3,2	+ 13,6	19
1652*	Ursæ Majoris	6	172 33 30	+57 21 30			1
1653	93 Leonis	4	172 59 0	+21 55 55	— 0,4	+ 25,9	2
1654*	4 Virginis A^1	6	172 59 50	+ 9 57 30	— 19,5	+ 24,3	8
1655*	Leonis		173 10 0	+15 59 40	— 11,2	+ 55,3	2
1656*	Leonis		173 17 50	+ 6 54 30			1
1657	94 Leonis β	1½	173 18 10	+16 17 35	— 29,6	+ 25,4	34
1658*	Leonis	6	173 19 —	+17 58 5			1
1659	5 Virginis β	3	173 38 0	+ 3 29 55	+ 63,3	+ 22,4	41
1660*	28 Crateris	4	174 16 0	−32 10 20	+259,3	— 45,0	1
1661*	64 Ursæ Maj. γ	2	174 20 30	+55 25 0	— 52,5	+ 2,5	5
1662*	Ursæ Majoris	7	174 24 —	+42 38 50			1
1663*	65 Ursæ Majoris	7	174 42 45	+48 11 50	— 45,2	+ 12,5	2
1664*	6 Virginis A^2	6	174 47 0	+10 9 45	+ 1,2	+ 15,5	3
1665	95 Leonis o	6	174 55 10	+17 21 50	+ 17,5	+ 21,5	6
1666	66 Ursæ Majoris	6	174 55 45	+58 19 20	−174,2	+ 2,0	2
1667	29 Crateris	6	174 57 45	−26 44 40	+129,1	— 25,0	1
1668*	30 Crateris η	4½	175 4 30	−15 25 40	— 27,5	+ 4,1	1
1669	7 Virginis b	5½	176 1 0	+ 5 22 15	+ 0,5	+ 37,8	23
1670	8 Virginis π	5	176 14 20	+ 8 20 5	+ 23,3	+ 26,7	17
1671	31 Crateris	5½	176 16 30	−17 56 10	— 30,0	+ 6,5	1
1672	1 Comæ Ber.	7	176 28 0	+23 49 15	— 54,0	+ 1,2	2
1673	67 Ursæ Majoris	6	176 34 0	+44 45 50	— 60,0	+ 12,2	3
1674	2 Comæ Ber.	6	177 6 0	+23 11 20	— 38,9	— 10,7	3
1675	9 Virginis o	5	177 21 10	+10 26 55	— 21,5	+ 29,4	13

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
1676	1 Corvi α	4	178° 7′ 30″	−23° 0′ 10″	+ 14″,8	+ 12″,7	4
1677*	10 Virginis	6	178 27 30	+ 3 37 45	− 16,0	+ 29,1	15
1678*	11 Virginis	6	178 33 20	+ 7 31 35	+ 18,7	+ 21,2	7
1679	2 Corvi ε	4	178 34 7	−20 53 50	− 21,3	+ 11,7	5
1680*	3 Comæ Ber.	6	178 40 15	+18 32 15	+ 6,0	− 7,7	1
1681*	Comæ Ber.	6	178 43 5	+29 0 30			1
1682	3 Corvi	6	178 47 20	−21 52 35		+ 6,9	1
1683	4 Comæ Ber.	6	179 0 0	+27 36 0	+ 55,1	− 4,7	1
1684	68 Ursæ Majoris	7	179 3 0	+58 47 10	−115,6	− 15,4	1
1685	5 Comæ Ber.	6	179 6 20	+22 16 0	− 72,4	+ 11,7	4
1686*	Virginis		179 11 10	+ 3 58 55			1
1687*	12 Virginis	6½	179 25 0	+11 58 35	− 36,8	+ 46,2	5
1688*	Virginis		179 32 10	+ 3 59 30			2
1689	1 Can. Venat.	6	179 50 0	+55 9 50		− 7,9	1
1690	4 Corvi γ	3	179 59 22	−15 49 10	− 37,6	+ 4,8	5
1691*	69 Ursæ Maj. δ	2½	180 0 0	+58 45 40	−116,6	− 9,2	5
1692	6 Comæ Ber.	5	180 4 0	+16 37 0	− 34,1	+ 34,6	1
1693*	Draconis	6	180 5 20	+71 55 35			1
1694*	2 Can. Venat.	6	180 7 45	+42 23 10	− 39,9	+ 6,4	1
1695*	7 Comæ Ber.	4½	180 10 0	+25 40 15	− 59,2	+ 0,3	3
1696*	13 Virginis	6	180 42 30	+ 0 55 50	− 29,4	+ 30,4	17
1697*	Comæ Ber.	7	180 50 30	+27 44 15			1
1698	14 Virginis	6	180 50 45	− 7 10 55		− 23,8	2
1699	8 Comæ Ber.	7	180 55 0	+24 45 35	− 48,8	+ 0,5	1
1700	9 Comæ Ber.	6	180 58 0	+29 53 35	+ 18,0	− 6,5	1
1701	15 Virginis γ	3	181 1 0	+ 1 2 55	− 6,6	+ 36,5	53
1702	10 Comæ Ber.	6	181 2 0	+30 11 15		+ 9,2	1
1703*	Virginis	7½	181 2 50	− 7 11 45			1
1704	3 Can. Venat.	6	181 7 0	+50 42 40	− 80,1	− 10,0	1
1705*	16 Virginis c	6½	181 8 40	+ 5 2 5	+ 12,2	+ 22,2	7
1706	5 Corvi ζ	5	181 8 45	−20 29 35	− 3,3	+ 11,8	3
1707*	Corvi	6	181 14 30	−11 51 0			1
1708	11 Comæ Ber.	4½	181 15 30	+19 30 25	− 12,2	+ 13,8	1
1709*	Comæ Ber.	7	181 25 10	+27 43 45			1
1710	70 Ursæ Majoris	6	181 25 30	+59 35 50	− 73,4	− 18,8	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
1711*	Comæ Ber.	7	181° 26′ 10″	+28° 21′ 25″	″	″	1
1712	17 Virginis	6	181 41 40	+ 7 1 45	— 12,0	+ 11,6	4
1713*	12 Comæ Ber.	5	181 43 0	+27 34 20	— 0,6	— 9,7	4
1714	6 Corvi	6	181 49 0	—23 7 5	+ 25,0	+ 6,2	2
1715*	Comæ Ber.	7	182 6 45	+27 34 35			1
1716	4 Can. Venat.	6	182 7 15	+44 15 55	— 66,3	— 3,9	1
1717*	13 Comæ Ber.	4½	182 10 30	+27 49 20	+ 2,4	— 1,4	4
1718	5 Can. Venat.	6	182 13 0	+53 16 50	— 59,6	+ 12,0	2
1719	71 Ursæ Majoris	7	182 32 15	+58 30 15		— 13,3	1
1720	6 Can. Venat.	5	182 37 30	+40 44 35	— 28,5	— 3,0	1
1721*	14 Comæ Ber.	4½	182 42 30	+28 59 15	+ 5,2	+ 8,6	4
1722*	15 Comæ Ber.	4½	182 51 0	+29 59 30	+ 19,8	+ 11,6	2
1723*	16 Comæ Ber.	4½	182 51 30	+28 32 50	— 8,4	— 2,9	4
1724*	72 Ursæ Majoris	7	182 54 30	+56 53 0		— 14,4	1
1725	73 Ursæ Majoris	6	183 11 30	+57 26 30	—104,8	— 28,2	1
1726*	Comæ Ber.	6	183 17 15	+27 56 25			1
1727*	Comæ Ber.	7	183 18 30	+27 36 50	— 23,4		1
1728*	17 Comæ Ber.	6	183 21 0	+27 37 50	— 25,4	+ 9,5	2
1729*	18 Comæ Ber.	6	183 28 6	+25 49 35	+ 19,1	+ 4,0	3
1730	7 Corvi δ	3	183 28 45	—14 47 10	— 50,0	— 3,2	5
1731	20 Comæ Ber.	6	183 31 0	+22 37 10	+ 31,1	— 9,1	2
1722	74 Ursæ Majoris	6	183 50 30	+60 7 15	— 92,2	— 11,3	2
1733*	75 Ursæ Majoris	6	183 51 —	+60 29 15			1
1734*	7 Can. Venat.	7	183 51 0	+53 15 20	— *	— 10,4	1
1735*	21 Comæ Ber.	5	183 52 30	+26 17 5	— 20,3	+ 3,3	2
1736	8 Corvi η	5	184 1 30	—14 28 35	+ 26,4	+ 4,6	4
1737	4 Draconis	6	184 3 30	+70 54 55		+ 29,1	1
1738	20 Virginis	6	184 21 0	+11 59 55	— 46,9	+ 50,0	1
1739*	21 Virginis q	6	184 27 0	— 7 44 25	+ 18,4	+ 12,8	15
1740*	Comæ Ber.	7	184 31 30	+26 9 35			1
1741	22 Comæ Ber.	7	184 32 0	+25 59 40	— 51,2	+ 17,3	2
1742	9 Corvi β	3	184 33 0	—21 40 55	+ 2,1	+ 15,1	4
1743	8 Can. Venat.	4½	184 44 15	+43 2 50	+ 45,6	+ 20,1	1
1744*	23 Comæ Ber.	4	184 51 0	+24 20 5			3
1745	24 Comæ Ber.	5	184 53 30	+20 5 15	— 14,4	+ 8,2	3

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1746	5 Draconis $\varkappa$	3	184° 56′ 20″	+71° 29′ 55″	+180,5	+ 15,6	1
1747*	25 Virginis f	6	185 13 20	— 4 7 45	— 27,9	+ 41,9	5
1748*	22 Virginis	6	185 15 10	+ 9 54 0			
1749	6 Draconis	6	185 20 20	+71 44 5		+ 3,4	1
1750	25 Comæ Ber.	6	185 21 0	+18 48 25	+ 0,5	— 12,0	2
1751*	23 Virginis	6	185 24 10	+ 9 40 30			
1752*	24 Virginis	6	185 25 40	+ 7 16 5			
1753	26 Virginis χ	5	185 49 40	— 6 17 25	— 24,6	+ 25,7	18
1754	26 Comæ Ber.	5	185 54 40	+22 46 35	+ 12,1	— 9,7	3
1755	9 Can. Venat.	6½	185 57 0	+42 35 25	— 47,2	— 14,6	1
1756	27 Virginis	6	186 30 0	+12 7 25	— 50,0	+ 40,1	1
1757	28 Virginis	6	186 30 0	— 5 48 0	— 24,5	+ 36,4	1
1758	29 Virginis γ	3	186 30 10	+ 0 14 55	— 68,3	+ 36,6	73
1759	30 Virginis ρ	5	186 33 10	+11 56 25	— 10,7	+ 34,2	2
1760	31 Virginis d^1	6	186 33 40	+ 8 30 25	— 0,1	+ 28,3	5
1761*	76 Ursæ Majoris	6	186 56 45	+64 25 30	+ 31,2	— 13,6	2
1762*	Virginis	8	186 56 50	+ 0 7 41			5
1763	32 Virginis d^2	6	187 29 20	+ 9 22 15	— 3,3	+ 21,1	5
1764	10 Can. Venat.	6	187 33 30	+40 58 0	— 28,5	+ 20,3	1
1765	33 Virginis	6½	187 40 0	+11 16 15	— 19,6	+ 8,3	1
1766	27 Comæ Ber.	5	187 47 30	+18 16 35			3
1767	34 Virginis	6	187 54 40	+13 38 55	— 23,4	+ 42,9	2
1768	35 Virginis	6	188 1 30	+ 5 16 15	— 12,4	+ 9,4	1
1769	28 Comæ Ber.	6	188 11 0	+15 15 0	— 31,1	+ 13,3	1
1770*	36 Virginis	6	188 21 0	+15 49 5	— 37,3	+ 16,9	5
1771	30 Comæ Ber.	6	188 31 30	+29 14 35	+ 29,6	+ 19,5	1
1772	11 Can. Venat.	6	188 36 15	+50 10 0		— 7,1	1
1773	7 Draconis	6	188 37 20	+68 29 30		— 10,1	3
1774*	Ursæ Majoris	6	188 42 45	+62 1 20			1
1775*	Can. Venat.	6	188 50 45	+39 12 40			1
1776	37 Virginis	6	188 58 0	+ 4 45 30	— 16,9	— 27,7	2
1777*	31 Comæ Ber.	5½	189 7 30	+29 14 15	+ 45,7	— 4,8	2
1778	32 Comæ Ber.	7	189 11 30	+18 46 15	+ 4,8	— 8,0	2
1779	33 Comæ Ber.	7	189 14 10	+18 48 15	— 9,8	+ 3,4	1
1780	38 Virginis	6	189 21 20	— 1 51 15	— 88,0	— 18,7	5

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley. AR.	Difference from Bradley. D.	No. of Obs.
			h m s	° ′ ″	″	″	
1781	39 Virginis	6	12 36 0	− 7 22 30			1
1782*	35 Comæ Ber.	4½	12 36 0	+22 56 25		− 3,2	4
1783	40 Virginis ψ	5	12 36 20	− 7 51 5	+ 21,4	+ 19,4	5
1784	41 Virginis	6	12 36 40	+14 6 5		+ 38,0	1
1785*	42 Virginis	6	12 37 20	+ 9 30 30			
1786	43 Virginis δ	3	12 40 0	+ 5 5 10	− 26,9	+ 16,1	55
1787*	77 Ursæ Maj. ε	3	12 40 30	+57 39 50	−154,4	− 45,1	3
1788*	12 Can. Venat.	2½	12 41 15	+40 0 0	+ 13,1	+ 7,6	4
1789	8 Draconis	6	12 41 20	+67 7 35		+ 6,2	2
1790	36 Comæ Ber.	5	12 41 30	+19 5 15	+ 4,7	+ 10,0	4
1791*	44 Virginis k	6	12 42 40	− 2 8 25	+ 9,3	+ 38,3	5
1792	46 Virginis	6	12 43 10	− 1 41 10	− 66,4	− 16,9	3
1793*	37 Comæ Ber.	5½	12 43 30	+32 28 0	+ 77,0	− 1,6	3
1794	38 Comæ Ber.	6	12 43 40	+18 48 40	+ 12,2	− 24,3	3
1795*	Comæ Ber.	6	12 43 45	+22 56 50			1
1796	47 Virginis ε	3	12 44 10	+12 37 35	− 18,0	+ 33,3	49
1797*	78 Ursæ Majoris	6	12 44 30	+58 3 10	− 49,2		1
1798	9 Draconis	6	12 44 30	+68 16 20	+167,1	+ 18,0	2
1799	48 Virginis	6	12 45 30	− 1 58 30	− 55,4	− 38,2	3
1800	14 Can. Venat.	5	12 46 0	+37 28 20	+ 36,7	− 12,9	2
1801	39 Comæ Ber.	5	12 46 20	+22 50 0	− 6,8	− 23,9	2
1802	40 Comæ Ber.	6	12 46 30	+24 17 20	+ 27,8	− 0,5	1
1803*	49 Virginis g	5	12 46 30	− 9 4 45	+ 12,1	+ 26,9	8
1804	41 Comæ Ber.	4¾	12 47 0	+29 17 50	+ 1,7	+ 3,7	3
1805*	Virginis	6½	12 47 30	− 7 18 25			3
1806*	45 Hydræ ψ	5	12 47 40	−21 27 20	− 37,6	+ 24,5	2
1807*	Comæ Ber.	6	12 48 0	+29 13 40	+ 19,7	− 0,7	1
1808*	50 Virginis	6	12 48 30	− 8 41 5	+ 14,6	+ 75,3	8
1809	51 Virginis θ	4	12 48 40	− 3 52 35	+ 27,6	+ 12,6	20
1810*	Comæ Ber.		12 49 0	+18 30 15			1
1811	42 Comæ Ber.	4½	12 49 0	+19 10 35	− 5,0	+ 22,8	3
1812*	Can. Venat.	6	12 49 20	+39 5 15			1
1813	15 Can. Venat.	5¾	12 49 30	+40 11 45	− 61,9	+ 0,9	1
1814*	52 Virginis	6	12 49 30	− 8 30 30			
1815	53 Virginis	4½	12 49 40	−14 31 5	+ 40,7	− 3,5	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1816*	16 Can. Venat.	7	193° 56′ 45″	+40° 22′ 45″	— 54″,6	+ 21″,6	1
1817	17 Can. Venat.	6	193 57 0	+40 9 35	— 47,7	— 7,0	1
1818	54 Virginis	6	194 13 30	—17 9 50	+ 55,8	— 15,3	1
1819*	18 Can. Venat.	6	194 19 30	+42 27 10	+ 21,5	— 2,6	1
1820*	43 Comæ Ber.	5½	194 19 30	+29 27 30	+ 1,8	+ 63,6	4
1821*	55 Virginis	6	194 33 0	—18 16 50	— *	— 22,0	1
1822*	56 Virginis	8	194 39 0	— 8 43 0	— 33,9	+ 12,0	1
1823	57 Virginis	6	194 47 30	—18 16 40	+146,9	— 20,3	1
1824*	Can. Venat.	5½	194 54 10	+41 48 40			2
1825*	Centauri	5	194 56 55	—29 48 25			1
1826	59 Virginis *e*	6½	195 19 40	+11 3 20	+ 55,3	+ 15,9	1
1827*	58 Virginis	6	195 19 50	— 8 53 0	— 55,2	— 57,4	1
1828	19 Can. Venat.	7	195 23 0	+42 30 20	— 15,4	— 5,7	2
1829*	Virginis	8	195 24 10	— 7 5 30			1
1830*	Virginis		195 27 30	+15 18 20			1
1831	60 Virginis *σ*	5	195 29 30	+ 7 6 35	+ 0,8	+ 22,4	4
1832*	61 Virginis	4½	195 32 45	—16 34 25	+ 4,1	— 72,1	1
1833*	46 Hydræ *γ*	3½	195 33 15	—21 31 50	— 37,5	+ 24,2	6
1834*	Virginis		195 33 45	+15 23 55			1
1835	20 Can. Venat.	6	195 54 30	+42 13 25	— 60,2	— 26,0	3
1836	62 Virginis	6	196 2 0	— 9 38 50	— 25,7	— 53,0	1
1837*	21 Can. Venat.	6	196 15 0	+51 19 20	— 31,0	+ 7,6	3
1838*	22 Can. Venat.	6	196 — —	+53 13 0			
1839	63 Virginis	6	196 36 0	—16 5 30	+ 85,1	— 20,6	1
1840	23 Can. Venat.	7	196 36 45	+41 47 20	— 84,8	+ 3,0	1
1841*	Virginis	6	196 37 —	+ 3 43 15			1
1842	64 Virginis	6	196 37 50	+ 6 47 5	+ 20,2	+ 32,9	3
1843*	Virginis	6	196 48 —	+15 45 55			1
1844	65 Virginis	6	196 49 50	— 3 16 40	— 22,7	— 40,1	1
1845	66 Virginis	6½	197 6 0	— 3 32 0	+ 67,5	+ 9,8	4
1846	67 Virginis *α*	1	197 14 0	— 9 32 0	— 9,1	+ 14,5	108
1847*	68 Virginis *i*	6	197 36 30	—11 4 45	— 28,9	— 2,1	1
1848	69 Virginis	5½	197 44 15	—14 21 0	+ 33,8	— 2,2	1
1849*	79 Ursæ Maj. *ζ*	3	197 51 40	+56 33 40	— 70,8	— 20,3	6
1850*	80 Ursæ Maj. *g*	5	198 13 0	+56 37 30	—121,2	— 37,0	5

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1851*	70 Virginis	6	198° 19′ 30″	+15° 26′ 35″	— 41,7	— 19,2	3
1852*	Virginis		198 23 30	+ 5 29 20			1
1853*	71 Virginis	6	198 28 45	+12 26 5	— 49,1	+ 17,1	2
1854*	72 Virginis l^1	$6\frac{1}{2}$	198 34 45	— 4 52 5	— 7,3	+ 45,4	6
1855	73 Virginis	6	198 50 15	—17 6 45	+ 56,0	— 9,7	1
1856*	74 Virginis l^2	6	198 58 50	— 4 38 35	— 30,6	+ 6,6	16
1857*	Bootis	6	199 3 —	+25 50 5			1
1858	75 Virginis	6	199 5 15	—13 44 50	+ 15,2		2
1859	76 Virginis h	6	199 10 40	— 8 33 25	— 9,6	+ 13,7	9
1860*	Ursæ Majoris	5	199 18 45	+61 33 40			1
1861	77 Virginis	7	199 19 20	— 6 0 50		— 3,4	2
1862*	Bootis		199 33 25	+25 57 20			1
1863	78 Virginis	6	199 37 0	+ 5 15 35	+ 3,2	+ 22,8	4
1864*	79 Virginis ζ	4	199 44 0	+ 0 59 40	— 19,2	+ 36,6	31
1865*	80 Virginis	6	199 51 30	— 3 48 35	+ 10,6	+ 36,6	10
1866*	Can. Venat.	5	200 14 0	+38 46 25			1
1867*	81 Virginis	$7\frac{1}{2}$	200 22 20	— 6 16 35	— 80,4		1
1868	24 Can. Venat.	$5\frac{1}{2}$	200 27 0	+50 36 55	— 82,7	— 1,2	1
1869	81 Ursæ Majoris	$5\frac{1}{2}$	200 33 45	+56 56 40	—111,4	+ 17,5	1
1870*	Bootis	7	200 51 38	+24 7 5			1
1871*	25 Can. Venat.	5	200 53 15	+37 53 0			1
1872*	Bootis	6	201 1 10	+12 18 50			2
1873	82 Virginis m	6	201 21 0	— 7 7 35	— 4,4	+ 21,9	21
1874	1 Bootis	6	201 28 0	+21 32 30	— 7,1	— 7,4	4
1875*	Bootis	7	201 28 15	+21 36 5			1
1876*	2 Bootis	6	201 35 15	+24 4 40	+ 10,0	+ 13,7	3
1877	84 Virginis o	6	201 52 30	+ 5 7 5	— 8,7	+ 14,2	9
1878*	82 Ursæ Majoris	6	201 57 0	+54 30 30	— *	— 18,4	1
1879*	83 Virginis	6	201 58 25	—14 36 15	— 36,9	+ 8,6	3
1880	1 Centauri i	$4\frac{1}{2}$	202 1 45	—31 27 15	+ 57,5	— 15,1	2
1881	85 Virginis	6	202 15 20	—14 11 45	— 46,6	+ 13,7	4
1882	83 Ursæ Majoris	6	202 15 30	+56 15 50	—109,6	— 2,6	2
1883*	Bootis	5	202 15 45	+24 16 40			2
1884	86 Virginis n	6	202 22 50	—10 51 35	— 28,1	+ 16,1	7
1885	87 Virginis	6	202 40 20	—16 17 35	— 29,3	+ 12,1	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
			° ′ ″	° ′ ″	″	″	
1886*	Virginis		202 43 15	— 8 8 35			1
1887	2 Centauri *g*	4½	202 53 10	−32 52 55	+ 85,6	+ 0,6	3
1888*	88 Virginis *n*	6	203 2 15	— 5 16 35	— 11,9	+ 15,9	2
1889	3 Bootis	6	203 4 0	+27 16 20	+ 39,6	+ 0,3	2
1890*	Bootis	6½	203 5 0	+32 28 15			3
1891	4 Bootis *τ*	4	203 7 30	+19 0 45	— 5,2	+ 23,4	5
1892*	Can. Venat.	7	203 13 0	+40 4 30			1
1893	89 Virginis	5½	203 17 30	−16 34 55	— 51,0	+ 36,6	3
1894*	Can. Venat.	7	203 24 30	+40 6 30			1
1895	3 Centauri *k*	4½	203 30 0	−31 26 15	+ 70,8	+ 6,3	3
1896*	Can. Venat.	7	203 36 5	+32 57 10			1
1897	5 Bootis *υ*	4	203 38 0	+17 21 0	— 8,5	+ 11,3	5
1898*	Can. Venat.	6	203 40 30	+32 45 10			3
1899	84 Ursæ Majoris	6	203 45 15	+56 0 0		— 14,1	1
1900*	6 Bootis *e*	5½	203 45 30	+22 49 0	+ 12,0	+ 11,4	1
1901*	Bootis	6	203 49 35	+20 11 10			1
1902	85 Ursæ Maj. *η*	3	203 50 0	+50 53 0	— 66,9	— 31,5	7
1903	4 Centauri *h*	4½	203 51 30	−30 22 35	+ 70,3	+ 3,4	3
1904*	Can. Venat.	7	204 20 55	+36 19 10			1
1905*	Can. Venat.	7	204 21 40	+36 12 10			1
1906*	Bootis		204 24 15	+18 16 30			1
1907*	Can. Venat.	6	204 31 25	+35 59 15			1
1908	7 Bootis	7	204 36 0	+19 28 30	+ 0,4	+ 13,2	3
1909	90 Virginis *p*	6	204 42 50	+ 0 2 5	— 37,4	+ 22,9	12
1910*	91 Virginis	6	204 43 0	+ 2 34 51			1
1911*	Bootis	5	204 46 55	+30 10 25			1
1912	8 Bootis *η*	3	204 58 40	+19 58 20	+ 6,4	— 34,8	13
1913	92 Virginis	6	205 10 50	+ 2 34 55	— 8,3	+ 14,5	5
1914	47 Hydræ	6	205 18 45	−23 26 20	— 10,4	+ 3,7	2
1915	9 Bootis	5	205 36 0	+29 1 30	+ 0,6	+ 13,3	3
1916	86 Ursæ Majoris	6	205 36 30	+55 16 0		+ 5,2	1
1917*	Can. Venat.		205 36 35	+33 32 40			1
1918*	10 Draconis *i*	5	205 37 0	+66 16 0	— 81,3	— 1,5	1
1919*	48 Hydræ	6	205 41 15	−23 28 45	— 5,3	+ 7,9	1
1920*	10 Bootis	7	206 1 30	+23 13 30	+ 14,4	+ 4,6	3

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1921	93 Virginis τ	5	206° 28′ 40″	+ 3° 3′ 25″	— 2″,5	+ 29″,9	11
1922*	Bootis	6	206 30 25	+23 48 0			1
1923*	11 Bootis	6¾	206 46 30	+28 53 13	— 0,7	+ 55,4	3
1924*	Bootis	7	206 54 30	+33 4 0			2
1925*	Virginis	7½	207 6 15	—14 49 35			1
1926*	5 Centauri θ	2½	207 8 0	—34 48 35	+ 26,1	— 67,9	1
1927*	Bootis	7	207 12 55	+33 9 20			2
1928*	49 Hydræ π	4	207 13 15	—25 10 45	— 34,5	+ 38,1	3
1929	94 Virginis	6	207 29 45	— 7 23 25	— 34,1	+ 0,4	4
1930	95 Virginis	6	207 36 0	— 7 49 5	— 23,8	+ 16,9	10
1931*	Virginis	7½	207 36 5	—14 41 5			1
1932*	Bootis	5	207 54 5	+52 28 45			2
1933*	96 Virginis	6	208 7 30	— 8 51 45	+ 53,5	+ 67,5	2
1934*	Bootis	7	208 11 0	+32 20 20			1
1935*	50 Hydræ	6	208 46 30	—25 46 40	+ 28,8	— 3,1	2
1936*	Bootis	6	208 56 25	+45 19 5			2
1937*	11 Draconis α	3½	209 0 0	+65 51 58	+ 15,9	+ 12,1	2
1938*	97 Virginis	7	209 1 30	— 8 25 45	+ 4,3	+ 32,6	1
1939	12 Bootis d	5	209 3 0	+26 34 20	+ 46,1	+ 21,7	2
1940*	98 Virginis $\varkappa$	4	209 6 50	— 8 48 45	— 28,6	+ 32,8	26
1941*	Virginis		209 9 —	+ 3 51 45			1
1942	13 Bootis	6	209 12 30	+50 56 0	—147,3	+ 25,7	2
1943*	Hydræ	6	209 18 30	—27 48 30			1
1944*	Bootis	6	209 26 25	+33 45 10			1
1945	14 Bootis	6	209 47 30	+14 25 40	+ 5,5	+ 17,8	4
1946	15 Bootis	6	209 55 10	+11 34 35	+ 2,3	+ 7,9	5
1947	99 Virginis ι	4	209 57 30	— 4 30 5	— 18,6	— 21,6	12
1948*	Bootis	7	210 2 0	+30 33 10			1
1949*	Bootis		210 21 45	+20 22 30	+ 50,0	— 0,5	1
1950*	16 Bootis α	1	210 22 30	+20 49 0	— 42,8	—134,2	42
1951	100 Virginis λ	4	210 36 40	—11 55 25	— 31,0	+ 12,6	28
1952*	17 Bootis $\varkappa$	4	210 38 0	+53 15 0	—158,3	+ 20,1	4
1953*	101 Virginis	6	210 39 30	+16 42 50			1
1954	102 Virginis υ^1	5	210 55 30	— 0 49 5	— 84,5	+ 25,8	6
1955*	Bootis	7	210 56 55	+41 10 28			2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
1956	18 Bootis	6	211° 4′ 0″	+14° 26′ 50″	+ 22″,2	+ 25″,6	3
1957	19 Bootis λ	4	211 9 30	+47 31 20	— 70,4	+ 34,4	2
1958	20 Bootis	5	211 16 0	+17 44 10	+ 15,4	+ 45,1	2
1959*	Bootis *A*	6	211 18 5	+36 56 0			3
1960	21 Bootis *i*	4	211 20 0	+52 48 50	—163,3	+ 3,1	4
1961*	51 Hydræ	5	211 20 0	—26 18 20	+ 10,5	— 3,6	1
1962*	Bootis	6	211 22 —	+53 59 55			1
1963*	3 Ursæ Minoris	6	211 21 30	+76 3 5	— *		1
1964*	103 Virginis v^2	5	211 31 0	— 0 33 25	— 19,1	+ 33,9	4
1965*	Bootis	6	211 36 30	+52 45 20	—112,6		1
1966	2 Libræ	7	211 42 45	—10 16 5	— 27,8	— 23,3	1
1967*	Bootis	6	211 56 50	+53 28 30			1
1968*	Bootis	4½	212 2 35	+ 9 51 55			3
1969*	Bootis		212 11 0	+ 7 14 10			1
1970*	Bootis	7	212 11 35	+ 9 40 0			1
1971*	Bootis	5	212 13 5	+ 7 14 35			1
1972*	Bootis	6	212 16 20	+ 9 30 0			1
1973	52 Hydræ	5	212 31 45	—28 4 35	+ 33,7	+ 18,0	1
1974*	4 Ursæ Minoris	5	212 46 30	+78 59 20	—189,0	+ 52,0	3
1975	104 Virginis	6	212 50 20	— 4 41 10	—151,5	— 46,6	1
1976*	Bootis		212 56 —	+38 37 10			1
1977*	22 Bootis *f*	6	213 0 30	+20 37 40	+ 5,9	+ 52,3	2
1978	105 Virginis φ	4	213 4 30	— 0 49 10	— 31,3	+ 18,8	9
1979	106 Virginis	6	213 8 30	— 5 28 30	—146,4	— 35,6	1
1980*	Bootis	6	213 11 55	+39 48 20			1
1981*	Virginis	6	213 30 —	+ 2 13 30			1
1982	23 Bootis θ	4	213 42 0	+53 17 50	—198,0	— 16,5	2
1983*	Bootis	6	213 51 0	+37 35 30			1
1984*	Bootis	6	214 8 20	+33 11 5			1
1985	24 Bootis *g*	6½	214 29 0	+51 14 20	—115,6	+ 35,4	2
1986	25 Bootis ρ	4	214 36 30	+31 45 20	+ 20,0	— 3,3	2
1987*	26 Bootis	7	214 36 30	+23 38 10	+ 15,1	+ 40,6	1
1988*	Bootis	6	214 38 40	+28 3 45			2
1989	27 Bootis γ	3	214 53 0	+39 40 35	+ 26,6	+ 35,6	4
1990	28 Bootis σ	5	215 17 30	+31 6 20	+ 23,8	+ 34,8	4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
1991*	Bootis	6	215° 17′ —	+33° 54′ 20″	″	″	1
1992*	Bootis	6	215 17 —	+38 20 25			1
1993*	Bootis	6	215 23 —	+37 59 45			1
1994	3 Libræ	6	215 23 0	−23 40 0			1
1995*	Libræ		215 36 55	− 9 11 0			2
1996*	Bootis	6	215 56 —	+50 44 10			1
1997*	4 Libræ	6	216 21 0	−23 39 5	+ 29,4	+ 13,7	3
1998	29 Bootis π	3¾	216 32 10	+17 46 20	+ 29,5	− 3,1	2
1999*	30 Bootis ζ	3	216 34 30	+15 4 40	+ 64,8	+ 12,6	3
2000	31 Bootis	5	216 36 30	+ 9 30 10	+ 12,2	+ 36,8	1
2001*	Bootis	6	216 38 45	+44 59 10			1
2002	32 Bootis	6	216 41 30	+13 0 10		+ 53,9	2
2003	107 Virginis μ	4	216 42 0	− 4 17 35	− 10,8	+ 12,2	16
2004*	33 Bootis	6	216 50 0	+45 45 30	− 40,5	+ 13,2	2
2005*	54 Hydræ	5½	217 2 30	−24 6 0	+ 3,6	− 11,9	1
2006*	5 Ursæ Minoris	4	217 15 0	+77 3 40	−154,8	+ 47,9	3
2007	5 Libræ	6	217 15 40	−14 6 40	− 86,6	− 49,5	1
2008	55 Hydræ	6	217 21 45	−24 17 50	+ 16,4	+ 16,2	1
2009	108 Virginis	6	217 26 0	+ 2 2 35		+ 29,9	4
2010	56 Hydræ	6	217 26 0	−24 45 25	+ 41,5	− 2,5	1
2011	34 Bootis	6	217 26 45	+27 51 40	+ 11,7	+ 21,9	2
2012	57 Hydræ	7	217 28 0	−25 18 45		− 17,1	1
2013*	Bootis	6	217 31 20	+38 5 10			1
2014	109 Virginis	4	217 38 30	+ 3 12 35	+ 39,1	+ 54,2	6
2015	35 Bootis o	4½	217 41 30	+18 18 0	+ 18,7	− 1,5	3
2016*	36 Bootis ε	3	217 51 0	+28 23 55	+ 43,6	+ 20,5	5
2017*	Bootis	6	217 54 45	+41 46 45			2
2018*	Virginis	6	217 55 50	+ 3 20 50			1
2019*	6 Libræ	5	218 2 15	−26 38 20	+ 48,4	− 5,0	2
2020	7 Libræ μ	5	218 7 0	−12 50 15	− 60,5	+ 29,5	15
2021*	Bootis	6	218 17 15	+46 30 40			1
2022	8 Libræ	6	218 25 0	−14 40 45	− 44,2	− 5,3	22
2023	9 Libræ α	2	218 28 10	−14 43 35	− 66,7	+ 0,4	50
2024*	Bootis	6	218 29 —	+43 42 0			1
2025*	10 Libræ	6	218 38 25	−17 2 45	− 3,0	− 11,8	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2026	11 Libræ	6	218° 45′ 0″	− 0° 59′ 25″	+ 4″,2	+ 29″,0	2
2027	12 Libræ	6	219 6 30	−23 20 50	+ 7,4	+ 13,4	3
2028*	Bootis	5½	219 13 30	+39 6 50			2
2029	37 Bootis ξ	4	219 16 0	+20 24 20	+ 42,6	+ 14,7	2
2030*	Bootis	6	219 18 40	+29 54 20			2
2031	13 Libræ ξ^1	6	219 24 45	−10 36 35	− 24,8	+ 19,0	14
2032	38 Bootis *h*	6	219 34 0	+47 25 30	− 5,2	+ 2,8	5
2033*	Bootis	5	219 34 20	+38 35 5			1
2034	39 Bootis	6	219 47 30	+50 0 40	− 19,6	+ 17,8	3
2035	14 Libræ	6	219 52 45	−24 9 55			1
2036*	Bootis	7	220 — —	+47 52 55			1
2037*	15 Libræ ξ^2	6	220 1 40	−10 8 25	− 80,5	+ 41,6	12
2038	59 Hydræ	6	220 6 0	−26 22 55	+ 25,2	+ 4,9	1
2039	16 Libræ	5½	220 16 0	− 3 3 55	− 22,7	+ 23,6	3
2040	17 Libræ	7	220 22 30	− 9 52 45	− 1,1	− 4,9	5
2041	1 Serpentis	7	220 25 45	+ 1 5 40	− 5,7	+ 50,6	1
2042	18 Libræ	6	220 32 45	− 9 52 35	+ 1,9	+ 28,8	9
2043	60 Hydræ	6½	220 56 15	−26 48 0	+ 11,1	− 1,6	1
2044*	Bootis	6½	221 5 20	+42 24 35			2
2045	19 Libræ δ	4½	221 8 0	− 7 16 15	− 56,9	+ 41,9	11
2046*	6 Ursæ Minoris	7	221 10 30	+73 15 30		+ 17,3	1
2047	2 Serpentis	7	221 30 0	+ 1 6 10	− 13,7	+ 39,2	1
2048*	20 Libræ	3	221 31 0	−24 1 45	− 29,8	− 12,9	10
2049*	Bootis	6	221 31 25	+50 53 30			1
2050*	110 Virginis	4½	221 49 20	+ 3 19 35	− 12,9	+ 39,1	4
2051*	Bootis	6	221 54 15	+40 53 40			3
2052	40 Bootis	6½	221 56 0	+40 31 40		− 41,7	3
2053	41 Bootis ω	5	222 7 30	+26 14 55	+ 34,4	+ 29,1	3
2054	21 Libræ ν^1	5	222 22 0	−15 2 5	− 44,3	+ 39,3	14
2055	22 Libræ ν^2	6	222 24 45	−15 15 35	− 49,4	+ 23,7	6
2056*	Bootis	6	222 29 20	+48 30 5			2
2057	42 Bootis β	3	222 35 0	+41 37 50	− 59,3	+ 14,1	4
2058*	Bootis	6	222 42 0	+36 26 10			2
2059	43 Bootis ψ	5	222 47 30	+28 10 20	− 8,7	+ 29,0	3
2060*	Bootis	5½	223 4 20	+45 52 0			2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					R.	D.	
2061*	7 Ursæ Min. β	3	223° 5′ 30″	+75° 24′ 40″	—196,3	+ 40,4	3
2062*	44 Bootis i	5	223 22 0	+48 52 30	+ 61,9	+ 18,0	3
2063	45 Bootis c	5	223 24 0	+26 5 35	+104,6	+ 20,5	3
2064*	24 Libræ ι^1	5	223 40 30	—18 35 15	— 54,7	+ 3,1	15
2065*	Libræ	7	223 41 30	—17 54 15			1
2066	46 Bootis b	6	223 45 0	+27 30 25	+ 22,9	+ 24,7	1
2067*	47 Bootis k	6	223 48 30	+49 21 50	— 55,8		4
2068*	23 Libræ	7	223 52 20	—24 6 35			1
2069	1 Lupi	5	223 55 0	—30 19 25	+ 93,9	— 7,4	1
2070*	25 Libræ ι^2	6	223 57 10	—18 27 0	— 55,6	+ 5,8	7
2071	26 Libræ	6	224 18 10	—16 34 0	— 48,7	— 43,3	2
2072*	Bootis	6	224 30 50	+34 16 25			1
2073*	Bootis	6	224 30 —	+30 25 40			1
2074*	2 Lupi δ	4½	224 45 0	—28 58 25	+ 90,5	+ 4,7	3
2075	3 Serpentis	6½	224 57 20	+ 6 6 25	— 11,5	+ 47,4	1
2076	4 Serpentis	6	225 1 0	+ 1 32 15	— 0,8	+ 42,7	2
2077*	8 Ursæ Minoris	6	225 2 0	+76 7 40			1
2078	27 Libræ β	2	225 6 30	— 8 12 45	— 44,7	+ 15,8	39
2079*	9 Ursæ Minoris	7	225 9 0	+72 57 45	— *		2
2080	48 Bootis χ	5	225 22 30	+30 19 50	+ 28,8	+ 32,3	4
2081*	Lupi	4	225 34 15	—35 5 0			1
2082	49 Bootis δ	3	225 46 30	+34 29 30	— 73,8	+ 3,8	3
2083	28 Libræ	6	225 51 40	—16 59 20	— 37,7	— 39,3	1
2084*	Lupi	6	225 53 0	—35 42 0			1
2085	5 Serpentis	6	225 53 0	+ 2 57 25	+ 41,0	+ 9,4	4
2086	29 Libræ o^1	7	225 57 40	—14 24 0	— 63,2	+ 8,7	7
2087*	Bootis	5	226 3 30	+21 44 30			2
2088	6 Serpentis	6	226 19 30	+ 1 51 50	— 3,1	+ 25,7	3
2089*	30 Libræ o^2	6	226 28 30	—13 58 45	— 71,1	— 52,5	2
2090	1 Coronæ Bor. o	6	226 50 0	+30 46 0	— 10,6	— 7,7	4
2091	31 Libræ ε	4	226 52 30	— 9 10 40	— 36,8	— 2,2	8
2092*	7 Serpentis	7	226 56 45	+13 41 40	— 44,7	+ 41,5	1
2093	8 Serpentis	7	226 57 0	+ 0 6 20	— 0,2	+ 27,1	2
2094*	10 Ursæ Minoris	7	226 57 0	+75 3 50	— *		1
2095	50 Bootis	5	227 22 30	+34 4 0	— 89,9	+ 3,3	2

No.	STAR	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					℞.	D.	
2096	2 Coronæ Bor. η	5	227° 35′ 30″	+31° 25′ 30″	+ 48,7″	+ 13,6″	4
2097*	32 Libræ ζ^1	6	227 44 0	−15 36 15	− 72,1	+ 9,3	8
2098*	9 Serpentis τ^1	6	227 52 30	+16 32 10	− 54,3	+ 33,7	1
2099*	33 Libræ ζ^2	7	228 3 15	−16 20 15	− 84,8	+ 2,7	1
2100*	51 Bootis μ	4	228 13 0	+38 28 55	−260,5	+ 24,1	1
2101	10 Serpentis	6	228 15 30	+ 2 56 15	− 15,4	+ 43,6	3
2102*	34 Libræ ζ^3	6	228 19 15	−15 30 20	− 12,3	− 19,1	7
2103	3 Coronæ Bor. β	4	228 45 0	+30 11 55	+ 36,1	+ 10,5	9
2104*	35 Libræ ζ^4	6	228 53 0	−15 45 50	− 43,2	− 9,7	9
2105	36 Libræ	6	228 59 30	−26 57 55	− 14,5	− 1,3	3
2106	11 Serpentis	6	229 15 45	− 0 6 45	− 7,6	+ 35,8	2
2107*	12 Ursæ Minoris	7	229 18 —	+72 19 40			1
2108*	37 Libræ	5½	229 19 30	− 8 58 10	+ 22,6	− 9,7	3
2109	11 Ursæ Minoris	5	229 26 0	+72 56 10		+ 44,3	2
2110*	Libræ		229 26 0	− 8 6 40			1
2111*	12 Serpentis τ^2	7	229 29 30	+17 7 40	− 59,2	+ 28,6	2
2112*	12 Draconis ι	3	229 29 40	+60 2 0	− *	+116,1	Δ
2113	38 Libræ γ	3½	229 34 30	−13 43 15	− 31,1	− 0,3	22
2114*	39 Libræ	4	229 35 0	−27 4 20	− 11,2	+ 7,3	3
2115*	40 Libræ	4	229 55 30	−28 43 10	− 21,5	− 0,4	4
2116	52 Bootis ν^1	6	229 56 45	+41 54 30	+ 23,8	+ 2,5	4
2117*	Serpentis	7	229 57 20	+18 12 20			1
2118	13 Serpentis δ	3	230 0 15	+11 35 40	+ 1,4	+ 35,2	6
2119	3 Lupi γ	5½	230 1 15	−33 21 5		− 27,6	1
2120*	Coronæ Bor.		230 6 10	+28 0 40			1
2121	4 Coronæ Bor. θ	4½	230 6 30	+32 25 40	+ 13,6		1
2122*	53 Bootis ν^2	6	230 10 30	+41 58 30	−128,8	− 6,3	5
2123	14 Serpentis A^1	6	230 10 45	+ 0 29 25	− 15,6	+ 32,9	1
2124	41 Libræ	6	230 18 0	−18 14 25	− 41,6	− 19,9	11
2125*	13 Ursæ Min. γ	3	230 24 0	+72 55 40		+ 28,1	3
2126	5 Coronæ Bor. α	2½	230 24 0	+27 46 40	− 10,1	+ 17,1	9
2127*	15 Serpentis τ^3	6	230 24 0	+18 43 5	− 86,9	− 9,5	1
2128	16 Serpentis	7	230 24 15	+11 4 0			1
2129	42 Libræ	6	230 31 40	−22 46 20	− 52,7	− 2,4	2
2130*	17 Serpentis τ^4	6½	230 33 0	+16 8 20	− 59,1	+ 56,9	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2131*	18 Serpentis τ^5	6	230° 34′ 30″	+17° 10′ 10″	— 62,9	″	1
2132	4 Lupi	5½	230 45 15	−33 39 45	+ 53,5	— 39,6	1
2133*	14 Ursæ Minoris	7	230 55 —	+74 32 40			1
2134	6 Coronæ Bor. μ	5	231 0 0	+40 3 0	— 98,3	+ 46,8	1
2135	43 Libræ $\varkappa$	4	231 3 40	−18 38 20	— 68,6	— 1,3	14
2136	44 Libræ η	4	231 41 40	−14 38 55	— 70,5	— 5,4	14
2137*	54 Bootis ϕ	6	231 42 30	+41 23 0	−107,9	+ 11,6	4
2138*	19 Serpentis τ^6	6	231 42 30	+17 2 50	— 64,3	+ 19,4	2
2139*	20 Serpentis χ	6	231 48 45	+13 51 30	— 13,1	+ 49,2	1
2140*	7 Coronæ Bor. ξ	4	231 56 30	+37 39 50			4
2141	21 Serpentis ι	5	231 57 0	+20 41 15	— 43,8	+ 30,0	1
2142*	22 Serpentis τ^7	6	232 0 15	+19 28 15	— 26,3	+ 36,1	4
2143*	Herculis	6	232 0 —	+47 39 45			1
2144	23 Serpentis ψ	6	232 7 20	+ 3 32 25	— 10,0	— 6,8	4
2145	24 Serpentis α	2	232 15 15	+ 7 25 40	+ 23,4	+ 19,3	15
2146	8 Coronæ Bor. γ	4	232 26 0	+27 18 15	— 3,1	+ 4,0	1
2147	25 Serpentis A^2	6	232 31 45	— 0 48 25	— 10,6	+ 27,3	3
2148*	26 Serpentis τ^8	6	232 41 15	+18 15 50	— 94,9	+ 14,8	1
2149*	5 Lupi λ	5	232 49 15	−32 37 0		— 89,2	1
2150	27 Serpentis λ	4	232 51 15	+ 8 21 5	— 2,1	+ 8,9	7
2151	9 Coronæ Bor. π	5	232 57 0	+33 31 10	— 9,7	— 0,7	3
2152	28 Serpentis β	3	232 59 30	+16 24 40	— 51,5	+ 33,7	8
2153	1 Scorpii b	6	233 7 20	−24 46 10	— 46,5	— 1,2	5
2154*	29 Serpentis	5½	233 8 10	+16 31 15	— 45,5	— 0,3	1
2155	30 Serpentis	6	233 11 15	— 2 50 40	+ 7,5	+ 36,2	1
2156*	31 Serpentis υ	6	233 14 30	+15 5 15			1
2157	32 Serpentis μ	4	233 22 30	— 2 27 30	— 14,3	+ 37,0	6
2158*	34 Serpentis ω	6	233 41 30	+ 3 9 55	— 90,6	+ 34,8	5
2159	35 Serpentis $\varkappa$	4	233 42 40	+19 7 5	+ 34,1	+ 29,5	2
2160	2 Scorpii A	5	233 47 0	−24 21 45	— 30,3	— 0,6	7
2161	36 Serpentis b	6	233 47 30	— 2 7 30	— 7,0	+ 24,3	4
2162	37 Serpentis ε	3	233 51 15	+ 5 26 25	— 8,2	+ 14,6	6
2163	45 Libræ λ	4	233 52 0	−19 12 5	— 40,5	— 4,0	15
2164*	3 Scorpii	7	234 2 30	−24 17 5	— 19,8		5
2165	46 Libræ θ	4	234 4 30	−15 46 55	— 43,4	+ 9,4	19

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2166*	10 Coronæ Bor. δ	4	234° 9′ 0″	+27° 2′ 45″	+ 9″,3	− 8″,3	1
2167	4 Scorpii	6	234 14 15	−25 18 45	− 79,1		2
2168	47 Libræ	6	234 18 0	−18 25 10	− 26,4	− 35,3	2
2169*	38 Serpentis ρ	5	234 25 30	+21 55 35	− 30,1	+ 44,1	1
2170	5 Scorpii ρ	4	234 27 10	−28 16 0	+ 38,0	− 5,9	4
2171*	39 Serpentis	6½	234 42 15	+14 11 5	− 31,6	+ 7,8	1
2172	11 Coronæ Bor. κ	5	234 54 0	+36 38 30	− 24,2	− 14,3	2
2173*	Scorpii	7	234 56 10	−20 3 10			1
2174	40 Serpentis	7	234 56 30	+ 9 31 30	− 37,6	+ 3,9	1
2175	6 Scorpii π	3	235 3 30	−25 10 50	− 33,8	− 2,3	20
2176*	48 Libræ	4	235 13 15	−13 20 35	+ 12,6	− 17,0	4
2177*	Serpentis	6	235 14 40	+21 14 30			1
2178*	Libræ		235 20 40	−12 40 40			1
2179*	15 Ursæ Min. θ	5	235 27 15	+78 20 10		+ 93,3	1
2180*	1 Herculis χ	6	235 29 0	+43 20 45	+ 77,4	+ 28,6	5
2181	7 Scorpii δ	3	235 32 30	−21 42 10	− 69,2	+ 8,6	50
2182	41 Serpentis γ	3	235 33 0	+16 41 40	− 12,9	− 50,0	7
2183*	Serpentis ϕ	7	235 44 55	+15 19 5			1
2184	49 Libræ	6	235 45 15	−15 34 35	− 52,1	− 44,3	3
2185	2 Herculis	6	236 2 30	+44 4 0		− 6,6	2
2186	50 Libræ	6	236 4 0	− 7 30 25	−141,4	+ 32,1	1
2187	12 Coronæ Bor. λ	5	236 7 30	+38 52 15	+ 30,0	− 11,9	2
2188	13 Coronæ Bor. ε	4½	236 11 30	+27 48 0	− 5,4	+ 6,8	1
2189	4 Herculis	6	236 18 0	+43 29 35		− 6,7	2
2190*	3 Herculis	6	236 22 30	+ 5 19 50	+ 18,4	− 3,8	1
2191*	42 Serpentis	6	236 33 15	− 8 8 35			
2192*	51 Libræ	4½	236 50 30	−10 28 55	+ 11,8	+ 2,1	3
2193*	5 Herculis r	6	236 51 0	+18 42 25	− 60,8	+ 3,3	4
2194	8 Scorpii β	2	236 53 30	−18 55 10	− 69,5	+ 6,1	72
2195	43 Serpentis	6	237 7 15	+ 5 52 25	− 14,0	+ 2,2	1
2196	9 Scorpii ω^1	5	237 12 20	−19 47 20	− 65,9	− 4,3	11
2197	44 Serpentis π'	4	237 15 20	+23 41 0	− 49,8	+ 37,5	2
2198	14 Coronæ Bor. ι	5½	237 15 30	+30 44 20		+ 25,9	2
2199	15 Coronæ Bor. ρ	6	237 18 0	+34 15 40		− 38,1	3
2200*	10 Scorpii ω^2	5	237 21 0	−19 59 30	− 83,2	− 0,5	13

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2201	11 Scorpii	6	237° 36′ 0″	−11° 52′ 30″	− 15,3	+ 4,6	3
2202	45 Serpentis	6	238 13 0	+10 45 5	− 7,4	+ 9,6	5
2203*	6 Herculis υ	5	238 18 30	+46 55 40	− 49,4	− 36,4	3
2204	12 Scorpii c^1	6	238 18 30	−27 34 5	− 22,0	− 1,8	2
2205	46 Serpentis	6	238 20 0	+10 56 35	+ 19,6	− 8,4	2
2206	13 Scorpii c^2	6	238 20 15	−27 5 5	− 47,3	+ 18,9	3
2207	47 Serpentis	6	238 23 30	+ 9 23 30	− 16,7	+ 1,1	2
2208*	Serpentis	6	238 25 —	+ 9 28 25			1
2209*	7 Herculis κ	5	238 30 30	+17 54 10	+ 72,0	+ 4,0	11
2210	14 Scorpii ν	4	238 32 0	−18 36 55	− 73,6	− 1,4	27
2211*	8 Herculis q	6	238 41 30	+18 3 35	+ 55,0	− 4,8	5
2212*	15 Scorpii ψ	5	238 46 0	− 9 13 40	+ 41,0	+ 17,3	6
2213*	16 Scorpii	6	238 51 30	− 7 42 45	− 42,0	+ 13,5	1
2214*	13 Draconis θ	3	239 2 20	+59 23 0		+103,2	Δ
2215*	16 Ursæ Min. ζ	4	239 4 30	+78 42 20		+ 63,6	2
2216*	17 Scorpii χ	6	239 12 0	−11 0 35	− 36,5	+ 4,3	2
2217*	Scorpii		239 15 30	−17 43 0			1
2218*	48 Serpentis	6	239 22 40	+17 29 5	− 34,0	+ 45,3	3
2219	16 Coronæ Bor. τ	6	239 26 0	+37 17 45	− 74,6	+ 49,0	1
2220*	18 Ursæ Minoris	6	239 29 —	+80 56 0			1
2221	9 Herculis	6	239 29 30	+ 5 51 15	− 14,0	− 22,8	2
2222	1 Ophiuchi δ	3	239 32 0	− 2 52 0	+ 11,1	+ 13,2	12
2223	10 Herculis	5	239 37 30	+24 20 30	− 35,9	− 55,4	1
2224*	18 Scorpii	7	239 42 45	− 7 31 5	− 2,1	− 5,8	2
2225*	49 Serpentis	6	239 44 0	+14 22 50	− 29,2	− 6,1	1
2226	11 Herculis φ	6	239 48 0	+45 46 30		− 17,9	3
2227*	Scorpii	5½	239 49 0	−27 46 15			
2228*	12 Herculis	7	239 55 30	+ 8 40 15	+ 27,2	+ 12,4	1
2229*	13 Herculis p	5½	240 6 0	+12 18 0			1
2230	14 Herculis	7	240 7 30	+44 40 20		− 19,6	1
2231	15 Herculis	6	240 21 0	+12 12 50	+ 25,1	+ 50,9	1
2232	16 Herculis	6	240 25 30	+19 37 20	+ 53,5	− 10,0	2
2233	2 Ophiuchi ε	3½	240 29 50	− 3 54 15	− 14,1	+ 18,5	11
2234	19 Scorpii	6	240 31 45	−23 23 25	− 41,9	+ 41,1	2
2235	20 Scorpii σ	5	240 37 30	−24 48 5	− 61,5	− 20,4	22

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2236*	17 Herculis	6	240° 46′ 0″	+23° 55′ 30″	″	— 8,8	1
2237	17 Coronæ Bor. σ	6	240 47 0	+34 40 10	— 58,0	— 7,6	3
2238*	17 Ursæ Minoris	7	240 51 30	+76 26 5	— *	— 4,8	1
2239	18 Coronæ Bor. υ	6	241 5 0	+29 56 45	— 45,9	— 3,0	1
2240	18 Herculis	7	241 10 30	+24 23 35	+ 21,9	+ 0,5	1
2241	19 Herculis	6	241 23 30	+26 41 0	+ 1,8	— 11,0	1
2242	4 Ophiuchi ψ	5	241 31 20	−19 16 10	— 51,0	— 5,6	11
2243	50 Serpentis σ	5	241 36 30	+ 1 47 0	— 28,6	+ 39,0	7
2244*	Herculis	6	241 39 25	+21 54 25			1
2245*	5 Ophiuchi ϱ	5	241 47 30	−22 41 30	— 82,0	+ 1,5	4
2246	20 Herculis γ	3	242 3 0	+19 54 25	+ 58,9	+ 21,0	6
2247*	21 Herculis o	6	242 15 45	+ 7 42 20	+ 58,4	— 21,0	2
2248	7 Ophiuchi χ	6	242 18 15	−17 42 30	— 81,7	— 13,8	9
2249	19 Coronæ Bor. ξ	5	242 30 30	+31 38 20		+ 7,0	5
2250	21 Scorpii α	1	242 37 30	−25 41 55	— 17,0	— 6,0	45
2251	22 Herculis τ	4	242 40 0	+47 4 50		— 23,6	1
2252	20 Coronæ Bor. ν^1	5	242 41 45	+34 33 15			5
2253*	21 Coronæ Bor. ν^2	5	242 43 45	+34 27 0			4
2254*	3 Ophiuchi υ	5	242 45 30	— 7 38 55			3
2255*	Ophiuchi	5	242 45 50	— 6 50 42			2
2256*	23 Herculis	6	242 47 30	+33 5 10	— 95,3	— 8,8	2
2257*	24 Herculis ω	6	242 48 0	+14 46 30		+ 10,3	3
2258*	Serpentis	6	242 49 10	+ 3 4 20			1
2259	22 Scorpii	$5\frac{1}{2}$	242 52 15	−24 23 10	— 27,7	— 15,8	3
2260	8 Ophiuchi φ	4	243 22 45	−15 53 45	— 60,7	— 1,6	9
2261*	Herculis	7	243 27 —	+16 4 0			1
2262	9 Ophiuchi ω	5	243 29 30	−20 45 35	−120,7	— 1,3	4
2263*	Herculis	7	243 32 20	+16 9 20			2
2264	25 Herculis	5	243 36 30	+38 7 10	— 46,0	+ 19,8	1
2265	10 Ophiuchi λ	4	243 49 30	+ 2 41 20	+ 8,4	+ 29,3	8
2266*	26 Herculis	7	244 4 30	+33 24 50	— 94,0	— 0,6	1
2267	23 Scorpii τ	4	244 10 0	−27 31 30	— 0,1	— 11,5	28
2268*	27 Herculis β	3	244 12 20	+22 11 45	+ 81,1	+ 0,1	5
2269*	Herculis s	6	244 16 —	+21 10 50			1
2270*	Herculis	7	244 17 —	+12 8 25			1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
2271*	28 Herculis *n*	6	244° 19′ 30″	+ 6° 13′ 10″	+ 67″,8	— 5″,9	3
2272*	Herculis		244 28 —	+22 53 45			1
2273	29 Herculis *h*	4	244 29 25	+12 10 55	+138,0	+ 14,8	8
2274	30 Herculis *g*	5	244 39 0	+42 35 15	—104,4	— 7,3	4
2275*	Herculis	6	244 43 —	+11 3 15			1
2276*	14 Draconis *η*	3	244 56 0	+62 14 0		— 25,4	1
2277	31 Herculis	7	244 59 30	+34 12 0	— 61,0	+ 15,6	1
2278	12 Ophiuchi	6	245 1 30	— 1 37 45	+ 47,5	— 2,8	2
2279	13 Ophiuchi *ζ*	3	245 1 55	— 9 54 20	+ 9,9	+ 24,3	25
2280	19 Ursæ Minoris	5	245 8 0	+76 37 30		+ 46,5	2
2281	20 Ursæ Minoris	6	245 15 30	+75 57 0		+ 41,6	2
2282	32 Herculis	6	245 22 30	+31 10 30	— 63,8		1
2283*	33 Herculis	6	245 24 0	+ 7 46 30	+210,8	— 11,6	2
2284*	34 Herculis	6	245 29 30	+49 39 10	— *	+ 5,7	2
2285*	Herculis	7	245 44 10	+38 44 40			1
2286*	Herculis	5	245 50 5	+14 21 15			1
2287*	24 Scorpii	5½	245 56 15	—17 6 25	— 41,3	+ 25,2	6
2288*	Scorpii	7	245 57 30	—19 17 35		+ 22,1	3
2289	35 Herculis *σ*	4	246 4 0	+43 5 30	—121,6	+ 32,3	3
2290*	36 Herculis m^1	7	246 17 0	+ 4 49 55	+ 84,1	+ 56,3	1
2291*	37 Herculis m^2	6	246 17 45	+ 4 51 30	+ 93,1	+ 5,5	3
2292*	14 Ophiuchi	6	246 29 45	+ 1 48 10	+ 51,1	+ 33,8	3
2293	38 Herculis	6	246 32 30	+ 5 30 30	+ 45,6	— 9,1	1
2294*	Herculis	5	246 35 30	+13 2 15			2
2295*	15 Ophiuchi	6	246 39 45	—22 33 0		— 51,8	1
2296*	21 Ursæ Min. *η*	5	246 49 45	+76 26 20		+ 50,8	1
2297	25 Scorpii	6	247 0 0	—24 55 20	— 52,3		1
2298*	15 Draconis *A*	4	247 7 30	+69 26 10	+ *	+ 7,0	1
2299*	Herculis		247 10 5	+25 28 40			1
2300	39 Herculis	5	247 15 0	+27 32 40	+ 55,1	— 18,6	1
2301*	16 Draconis	5	247 22 30	+53 32 30	— *	— 1,9	1
2302*	17 Draconis	5	247 23 20	+53 34 5	— *	— 8,3	1
2303	40 Herculis *ζ*	3	247 23 30	+32 11 30	+ 6,7	+ 22,1	3
2304	16 Ophiuchi	6	247 26 0	+ 1 37 25	+ 35,7	+ 11,7	1
2305	41 Herculis	6	247 27 45	+ 6 43 0	+ 64,6		1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2306	26 Scorpii ε	3	247° 32′ 30″	−33° 40′ 45″	− 39″,6	− 22″,8	4
2307*	42 Herculis	5	247 39 15	+49 33 20		0,0	3
2308*	43 Herculis *i*	5½	247 43 15	+ 9 11 0	+ 88,0	− 1,4	4
2309	18 Ophiuchi	6½	247 45 0	−24 2 55			1
2310*	Herculis	6	247 51 30	+16 21 10			2
2311	19 Ophiuchi	6	247 53 15	+ 2 39 20	+ 16,2	+ 18,7	1
2312	44 Herculis η	3	248 2 30	+39 32 20	+115,3	− 12,7	2
2313*	45 Herculis *l*	5	248 7 0	+ 5 50 20	+ 70,0	− 1,5	5
2314	46 Herculis	7	248 9 30	+28 57 20	+121,1		1
2315	20 Ophiuchi	5½	248 9 45	−10 11 55	+ 81,4	+ 11,3	1
2316*	Herculis	7	248 22 30	+11 42 40			2
2317	47 Herculis *k*	5	248 48 0	+ 7 49 15	+ 94,7	− 8,9	5
2318*	Herculis	6	248 49 30	+13 50 30			2
2319	21 Ophiuchi	6	248 55 15	+ 1 46 55	+ 36,0	− 0,4	3
2320	22 Ophiuchi	7	249 3 45	−22 57 25	− 67,2		1
2321	27 Scorpii	6	249 16 0	−32 43 10			1
2322	48 Herculis	6	249 17 0	+30 31 40	+ 69,8	− 4,4	1
2323*	23 Ophiuchi	5½	249 30 15	− 5 36 40	+ 41,7		2
2324	49 Herculis	6	249 31 0	+15 31 30	− 65,5	+ 12,6	2
2325*	24 Ophiuchi	7	249 33 45	−22 36 45	− 75,9		1
2326	50 Herculis	5	249 37 30	+30 21 30	+ 66,6	+ 20,6	1
2327	18 Draconis *g*	5	249 41 20	+65 10 45	+125,9	+ 8,8	1
2328	51 Herculis	5	249 45 0	+25 12 20	− 63,2	+ 8,7	3
2329	25 Ophiuchi ι	4	249 49 30	+10 42 10	+ 59,1	+ 28,0	14
2330*	Herculis	8	249 51 20	+15 56 30			1
2331	52 Herculis	5½	250 5 30	+46 32 35	−147,6	+ 6,0	3
2332	53 Herculis	5	250 17 10	+32 14 40	+ 78,2	− 6,6	1
2333	26 Ophiuchi	6	250 19 30	−24 27 10	− 46,8	− 63,6	1
2334	54 Herculis	5	250 25 0	+18 57 50	+ 85,6	− 8,3	5
2335*	55 Herculis	5	250 28 30	+18 56 20			
2336	56 Herculis	6	250 33 20	+26 16 0	+135,6	− 22,7	1
2337	27 Ophiuchi κ	4	250 44 30	+ 9 53 0	+ 33,0	+ 29,7	17
2338	29 Ophiuchi	6	250 59 10	−18 22 0	−157,3	− 67,3	1
2339	57 Herculis	6	251 10 0	+25 51 40	+127,8	+ 4,2	1
2340	30 Ophiuchi	6	251 10 30	− 3 43 15	+ 43,6	+ 5,2	5

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2341*	Herculis	6½	251° 11′ 5″	+15° 56′ 55″	″	″	2
2342*	28 Ophiuchi	6	251 14 0	−25 12 35			1
2343*	Ophiuchi		251 19 30	+ 7 5 10			1
2344*	31 Ophiuchi	6	251 26 0	−25 9 20	− 21,8		1
2345*	Herculis	6	251 52 30	+15 26 30			2
2346	28 Scorpii	6	251 58 40	−21 4 25	−153,0	− 59,0	1
2347	58 Herculis ε	3	252 5 30	+31 24 20	+ 71,5	+ 22,4	3
2348*	32 Ophiuchi	6	252 15 15	+14 33 50	− 55,3	+ 26,0	2
2349	33 Ophiuchi	6	252 22 15	+14 4 20	− 41,5	+ 25,1	1
2350	34 Ophiuchi	6	252 25 45	+14 2 10	+ 13,9	+ 35,5	1
2351	59 Herculis *d*	6	252 31 30	+34 2 40	+ 77,3	− 0,5	1
2352*	Ophiuchi	6	252 34 55	−17 9 30			1
2353*	60 Herculis	5¼	252 46 0	+13 11 55	− 33,7	+ 8,4	5
2354*	61 Herculis *c*	6	253 4 30	+35 52 40		+ 1,7	1
2355	35 Ophiuchi η	3	253 10 0	−15 18 0	− 17,3	+ 14,7	35
2356*	Scorpii		253 15 —	−26 36 20			1
2357*	19 Draconis h^1	5	253 34 30	+65 37 0	+164,8	− 11,0	1
2358	20 Draconis h^2	6	253 42 40	+65 31 10		− 16,3	1
2359*	Ophiuchi	6	253 44 40	+10 29 0			1
2360	29 Scorpii	6	253 45 0	−26 33 45	− 6,3		1
2361	62 Herculis	6	253 54 0	+24 55 10	+152,6	+ 5,0	1
2362*	36 Ophiuchi *A*	5¾	254 4 10	−26 5 50	+ 24,9	− 85,1	12
2363*	30 Scorpii	6	254 19 0	−26 3 15		− 62,3	1
2364	37 Ophiuchi	6	254 27 0	+11 0 0	+ 67,7	− 14,0	4
2365*	63 Herculis	6	254 31 0	+24 39 0		− 8,5	1
2366*	31 Scorpii	6½	254 37 15	−26 14 35			1
2367	39 Ophiuchi	6	254 46 30	−23 53 55	+ 46,7	− 11,9	6
2368	21 Draconis μ	4¾	254 47 0	+54 54 10	−168,5	− 23,5	1
2369	64 Herculis α	3	255 8 30	+14 46 20	− 28,9	+ 24,8	15
2370*	41 Ophiuchi	4½	255 8 45	− 0 4 0	−126,1	+ 33,6	3
2371	65 Herculis δ	4	255 33 0	+25 13 35	+111,2	+ 23,7	8
2372*	40 Ophiuchi ξ	4	255 37 30	−20 44 10	− 12,9	− 5,6	9
2373*	32 Scorpii	7	255 41 45	−24 32 50	− 46,3	+ 4,9	1
2374*	42 Ophiuchi θ	3¾	255 44 15	−24 38 20	+ 62,3	− 13,5	16
2375*	53 Serpentis ν	4	255 51 0	−12 29 30	+ 28,4	+ 9,2	8

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2376*	43 Ophiuchi	6	255° 57′ 0″	−27° 47′ 25″	+ 91″,9	− 12″,3	4
2377*	66 Herculis	6	256 1 30	+11 14 20	− 28,6	− 15,7	6
2378	67 Herculis π	3½	256 2 30	+37 11 15	+ 91,9	− 9,6	2
2379	68 Herculis u	5	256 28 0	+33 27 50	+ 28,7	− 6,5	3
2380	33 Scorpii	7	256 33 45	−23 54 35	− 42,0	− 9,3	1
2381	69 Herculis e	4½	256 44 0	+37 38 40	+ 57,6	− 4,4	1
2382*	44 Ophiuchi b	5	256 51 40	−23 50 15	+ 33,4	− 26,1	16
2383*	45 Ophiuchi d	6	256 52 0	−29 31 50		− 26,5	2
2384*	70 Herculis	4½	257 0 0	+24 50 20	+136,7	+ 2,4	4
2385*	22 Draconis ζ	4	257 2 30	+66 5 50	−162,3	+ 6,7	3
2386*	72 Herculis w	6	257 14 0	+32 53 45	+148,2	− 71,7	1
2387	47 Ophiuchi	6	257 24 45	−12 11 20	+ 18,8	− 27,8	1
2388	34 Scorpii v	4	257 26 15	−36 59 15	− 2,0	− 26,8	1
2389*	Ophiuchi		257 32 50	− 4 47 0			1
2390*	Herculis		257 37 35	+ 7 54 40			1
2391	49 Ophiuchi σ	5	257 47 15	+ 4 26 40	+ 4,8	+ 12,8	3
2392	73 Herculis	6	257 47 30	+23 16 45		+ 2,9	1
2393	74 Herculis	6	257 56 0	+46 33 25	−101,5	+ 39,3	3
2394*	50 Ophiuchi c^1	7	258 0 15	−23 32 35			1
2395*	51 Ophiuchi c^2	6	258 8 40	−23 40 55	− 35,4	+ 24,2	14
2396*	35 Scorpii λ	3	258 8 45	−36 49 15	+ 24,3	− 20,9	2
2397	75 Herculis ρ	4	258 15 0	+37 28 0	− 0,9	− 32,4	1
2398	52 Ophiuchi	6	259 11 0	−21 46 45	− 31,3	− 27,5	5
2399	76 Herculis λ	4½	259 31 30	+26 22 50	+119,7	− 18,4	1
2400*	77 Herculis x	6	259 41 15	+48 32 0	−179,2	+ 28,4	3
2401*	Herculis	6	259 48 —	+31 25 0			1
2402	78 Herculis	6½	259 52 45	+28 40 0	+138,7	− 15,8	1
2403*	22 Ursæ Min. ε	4	259 53 0	+82 27 15	−170,3	+ 47,2	1
2404*	53 Ophiuchi f	6	259 57 15	+ 9 50 10		− 10,5	3
2405	55 Serpentis ξ	4	259 58 0	−15 9 30	− 2,6	− 5,7	13
2406*	Serpentis		259 58 —	−15 20 10			5
2407	54 Ophiuchi	6	260 1 15	+13 24 30	+ 30,8	+ 2,9	1
2408	2 Sagittarii	6	260 4 0	−21 41 0	−104,9	− 2,0	2
2409	55 Ophiuchi α	2	260 7 45	+12 48 45	+ 55,2	+ 16,9	26
2410	56 Ophiuchi	6	260 11 45	+13 22 45			1

No.	STAR.		Mag.	Right Ascens. 1690.			Declination. 1690.			Difference from Bradley.		No. of Obs.
										AR.	D.	
				°	′	″	°	′	″	″	″	
2411*	57 Ophiuchi	μ	5½	260	15	0	− 7	53	50	+ 18,7	+ 33,1	2
2412	23 Draconis	β	2½	260	56	0	+52	33	10	−249,2	− 14,9	7
2413*	56 Serpentis	o	5	261	3	0	−12	38	35	−164,9	− 85,7	10
2414*	79 Herculis		6	261	9	45	+24	31	25	+105,4	+ 9,6	1
2415*	58 Ophiuchi		6	261	12	0	−21	29	15	+ 69,4	− 1,4	11
2416*	24 Draconis	ν^1	5½	261	34	45	+55	25	5	−177,9	− 19,7	5
2417*	25 Draconis	ν^2	5½	261	36	15	+55	24	15	−187,0	− 11,4	5
2418*	3 Sagittarii		6	262	1	15	−27	39	55	+ 3,7	+ 8,2	7
2419	60 Ophiuchi	β	3	262	2	30	+ 4	43	55	+ 7,5	+ 21,4	18
2420	82 Herculis	y	6	262	11	0	+48	47	0	−165,9	+ 16,0	4
2421*	61 Ophiuchi		6	262	15	0	+ 2	45	20	+ 17,7	− 8,7	1
2422	83 Herculis		7	262	27	45	+24	45	20	− 65,7	− 30,8	1
2423	85 Herculis	i	4	262	40	0	+46	11	55		− 19,7	5
2424*	84 Herculis		7	262	40	45	+24	29	30	− 62,7	+ 13,0	1
2425*	26 Draconis		6	262	55	0	+62	7	20			1
2426*	62 Ophiuchi	γ	4	263	5	30	+ 2	51	25	+ 4,6	+ 17,9	18
2427	27 Draconis	f	5	263	16	0	+68	19	20	+197,8	+ 35,6	2
2428	86 Herculis	μ	4	263	34	0	+27	55	50	+ 53,1	− 43,1	1
2429*	Ophiuchi		7	263	54	5	+ 9	59	0			1
2430	63 Ophiuchi		5	263	58	15	−24	46	14	− 34,8		2
2431	87 Herculis		6	264	1	15	+25	45	25	+166,5	− 0,1	1
2432	28 Draconis	ω	4	264	37	0	+68	53	30	+325,8	+ 40,9	4
2433*	Herculis		7	264	42	0	+47	45	15			1
2434*	Serpentis		6	265	7	40	− 4	0	50			3
2435*	4 Sagittarii		6½	265	14	0	−23	44	25	− 39,7	− 0,4	7
2436	29 Draconis		6	265	18	0	+74	23	25		+ 27,0	2
2437*	5 Sagittarii		7	265	18	30	−24	12	40	− 26,1		1
2438	30 Draconis		6	265	27	0	+50	52	35	−103,1	+ 3,3	1
2439	64 Ophiuchi	ν	4	265	29	16	− 9	42	0	+ 23,7	+ 19,8	13
2440	88 Herculis	z	6	265	32	30	+48	29	45	−181,5	+ 9,3	2
2441*	65 Ophiuchi		6	265	34	15	−17	56	15			1
2442	89 Herculis		6	265	40	30	+26	8	5	+206,1	− 14,3	1
2443	90 Herculis	f	6	265	47	0	+40	5	40	+ 83,7	− 7,1	1
2444*	Sagittarii			265	47	15	−22	43	20			1
2445	6 Sagittarii		7	265	50	45	−17	5	10	+ 11,7	− 48,6	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
2446*	7 Sagittarii	6	265° 58′ 30″	−24° 13′ 45″	− 17″,4	− 8″,7	5
2447*	57 Serpentis ζ	5½	266 2 0	− 3 37 40	+ 2,6	− 5,0	3
2448*	9 Sagittarii	7	266 13 30	−24 18 40	− 10,2	− 24,4	2
2449*	66 Ophiuchi	4½	266 13 30	+ 4 25 25	+ 27,0	+ 5,7	4
2450*	67 Ophiuchi	4	266 16 45	+ 2 58 45	+ 14,7	+ 25,0	5
2451	91 Herculis θ	4	266 22 30	+37 19 20	+128,0	− 6,2	3
2452	92 Herculis ξ	4	266 24 0	+29 18 50		− 8,9	5
2453	10 Sagittarii γ	3	266 28 0	−30 22 35	+ 35,2	− 14,9	7
2454*	93 Herculis	5	266 30 0	+16 47 10	+246,9	+ 57,2	1
2455*	68 Ophiuchi	4	266 30 30	+ 1 20 55	+ 11,2	+ 11,7	3
2456	69 Ophiuchi τ	5	266 32 50	− 8 8 30	+ 27,3	+ 12,6	6
2457	94 Herculis ν	5	266 38 30	+30 14 40	+ 81,6	+ 1,0	4
2458*	31 Draconis ψ¹	4¼	266 58 30	+72 17 35	−306,1	− 39,8	4
2459*	Draconis	7	266 59 0	+72 17 55	−307,7	− 29,3	1
2460*	95 Herculis	4	267 2 0	+21 38 0	+213,2	− 6,8	4
2461	32 Draconis ξ	3	267 5 0	+56 56 30	−118,1	− 18,7	3
2462	96 Herculis	5	267 13 0	+20 52 0	+254,8	− 4,6	3
2463*	97 Herculis	5½	267 20 30	+22 57 40	+178,7	− 26,5	1
2464*	33 Draconis γ	2	267 23 0	+51 32 50	− 99,4	− 13,5	5
2465*	70 Ophiuchi	4	267 26 30	+ 2 36 25	+ 57,5	− 51,6	7
2466*	11 Sagittarii	7	267 35 15	−23 59 0			2
2467*	71 Ophiuchi	6	268 6 45	+ 8 43 45	+ 44,9	+ 12,4	4
2468*	72 Ophiuchi	4	268 9 35	+ 9 33 15	+ 22,1	+ 15,5	5
2469	98 Herculis	5	268 11 0	+22 13 20	+231,8	+ 3,6	4
2470*	1 Sagittarii	6	268 13 0	−23 42 50	− 38,6		4
2471*	Sagittarii		268 14 40	−19 52 0			1
2472*	73 Ophiuchi	6	268 32 30	+ 3 58 45	+ 1,9	+ 2,6	1
2473*	12 Sagittarii	7	268 34 15	−23 8 10			1
2474	99 Herculis b	5	268 47 30	+30 33 0	+ 90,0	− 6,1	2
2475*	100 Herculis	6	268 48 10	+26 5 10	+119,3	− 10,5	2
2476*	101 Herculis	5	268 48 20	+20 1 50	+282,5	− 3,0	3
2477	13 Sagittarii μ	4	268 49 30	−21 5 50	− 55,4	+ 16,4	22
2478	102 Herculis	4½	268 50 0	+20 48 30	+169,1	− 32,0	2
2479	103 Herculis o	4	268 51 0	+28 45 0	+ 58,9	+ 2,2	4
2480	14 Sagittarii	7	268 56 0	−21 45 0	− 75,5	+ 5,0	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
2481*	15 Sagittarii	6	269° 12′ 0″	−20° 46′ 25″	− 56″,7	+ 0″,1	15
2482	16 Sagittarii	7	269 12 30	−20 26 0	− 49,2	+ 2,6	2
2483	17 Sagittarii	7	269 33 10	−20 36 0	− 69,4	+ 4,9	1
2484	104 Herculis *A*	4½	270 2 0	+31 21 30	+101,7	− 2,2	3
2485*	34 Draconis ψ^2	5¼	270 12 0	+72 0 50	−185,1	+ 27,8	4
2486	19 Sagittarii δ	3	270 16 15	−29 54 20	+ 67,1	− 14,5	15
2487*	35 Draconis	6	270 40 0	+76 58 5	+645,9	+ 18,1	1
2488	20 Sagittarii ε	3	270 54 15	−34 28 45	− 21,4	− 8,6	5
2489*	18 Sagittarii	7	271 15 10	−30 53 40			1
2490	58 Serpentis η	3	271 19 40	− 2 56 40	− 57,5	− 27,6	5
2491*	74 Ophiuchi	6	271 20 20	+ 3 16 0	+ 43,5	+ 40,8	2
2492*	105 Herculis	5	271 34 10	+24 21 0	+141,2	− 1,8	1
2493	21 Sagittarii	6	271 44 15	−20 39 45	− 49,2	+ 3,4	11
2494	106 Herculis	5½	271 48 0	+21 52 0	− 1,3	− 17,7	4
2495	107 Herculis *t*	6	272 11 45	+28 45 45	+141,6	− 30,8	4
2496	22 Sagittarii λ	4	272 12 45	−25 32 5	− 12,0	− 43,8	29
2497*	108 Herculis	6	272 13 0	+29 44 40	+146,0	− 6,6	1
2498*	1 Lyræ $\varkappa$	5	272 15 30	+35 57 25	− 20,4	− 12,7	1
2499	109 Herculis	4	272 37 45	+21 39 50	+ 0,9	− 20,6	5
2500	59 Serpentis *d*	6	272 51 20	+ 0 2 45	− 58,2	+ 19,8	1
2501*	23 Sagittarii	7	272 54 30	−23 24 0			2
2502	36 Draconis	6	273 2 0	+64 18 0	+ 33,7	− 7,8	2
2503	60 Serpentis *c*	6	273 23 50	− 2 9 0	− 24,2	+ 15,8	1
2504	2 Lyræ μ	6	273 30 0	+39 22 0	+ 28,6	− 12,1	1
2505*	24 Sagittarii	7	273 43 45	−24 12 15	+ 33,3	− 39,6	2
2506	25 Sagittarii	7	273 56 30	−24 23 50	−181,2	− 49,7	1
2507	61 Serpentis *e*	6	274 0 0	− 1 11 20	− 47,1	+ 23,1	1
2508	37 Draconis	6	274 18 0	+68 38 30	−111,5	− 12,4	1
2509*	1 Aquilæ	4	274 34 20	− 8 25 25	+ 49,6	− 0,8	4
2510*	38 Draconis	6	274 41 20	+68 37 10	+ 21,2	− 23,0	1
2511	39 Draconis *b*	5	274 51 30	+58 38 0	− 47,6	+ 1,8	2
2512*	Sagittarii		274 54 45	−23 42 40	+ 0,7	− 39,1	1
2513*	40 Draconis	5	275 40 0	+79 54 5	+376,2	+ 16,4	1
2514*	41 Draconis	5	275 42 0	+79 54 10	+360,8		1
2515	26 Sagittarii	6	275 46 10	−24 4 0	−118,1	− 30,1	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2516*	42 Draconis	6	276° 10′ 0″	+65° 22′ 30″	+386,1″	— 3,7″	1
2517	43 Draconis φ	5	276 15 30	+71 9 10	+ 97,4	+ 36,5	2
2518*	2 Aquilæ	5	276 19 40	— 9 18 0	— 9,0	— 22,0	3
2519	27 Sagittarii φ	5	276 33 45	—27 15 35	+ 23,4	— 1,3	25
2520*	44 Draconis χ	4	276 35 0	+72 34 50	+347,6	— 7,5	2
2521	3 Lyræ α	1	276 36 0	+38 31 35	+ 63,4	+ 0,1	12
2522*	3 Aquilæ	5	276 39 25	— 8 32 35	+ 10,4	+ 13,9	2
2523*	45 Draconis d	5	276 51 30	+56 49 10		+ 9,6	1
2524	28 Sagittarii	7	276 56 15	—22 40 0	— 98,1	— 9,4	3
2525	4 Aquilæ	5	277 17 30	+ 1 46 40	+ 22,3	+ 21,3	2
2526	5 Aquilæ	6	277 38 0	— 1 15 25	— 51,9	+ 32,1	1
2527*	6 Aquilæ	4	277 42 0	— 5 2 35	— 67,5	+ 16,4	3
2528*	29 Sagittarii	6	277 49 45	—20 37 50	— 48,8	+ 1,6	7
2529	30 Sagittarii	6	278 3 15	—22 27 55	— 20,3	— 21,1	3
2530	110 Herculis	4½	278 4 30	+20 16 30	+ 26,5	+ 8,4	1
2531	111 Herculis	4	278 18 45	+17 52 35	+ 87,0	— 14,1	1
2532	31 Sagittarii	6	278 23 30	—22 13 55	—111,7	— 28,1	5
2533	4 Lyræ ε	5	278 30 0	+39 22 25	+ 75,8	— 6,3	2
2534	5 Lyræ	6	278 30 20	+39 18 50	+ 85,0	0,0	2
2535	6 Lyræ ζ	5	278 30 40	+37 18 40	+ 60,3	— 12,5	5
2536	7 Lyræ	5	278 31 10	+37 17 45	+ 58,1	+ 4,8	1
2537	7 Aquilæ	6	278 42 45	— 3 35 0	— 18,4	+ 16,8	1
2538	8 Aquilæ	6	278 46 5	— 3 38 40	+ 13,9	+ 16,3	1
2539	32 Sagittarii ν¹	5	278 52 0	—23 4 45	— 22,5	— 0,8	21
2540	33 Sagittarii	6	278 53 0	—21 41 40	— 67,1	+ 1,5	4
2541	34 Sagittarii σ	3¾	279 0 0	—26 37 25	+ 23,6	— 37,1	36
2542	35 Sagittarii ν²	5	279 6 0	—23 0 35	— 25,4	— 9,7	21
2543	46 Draconis c	5	279 11 0	+55 14 25	— 94,8	+ 2,8	4
2544*	Sagittarii	8	279 18 50	—23 30 50			2
2545	8 Lyræ ν¹	6	279 34 0	+32 29 20	— 22,8	— 18,1	1
2546*	9 Lyræ ν²	6	279 35 0	+32 13 30	— 13,1	— 12,6	1
2547	10 Lyræ β	3	279 40 0	+33 2 10	— 16,8	— 16,9	2
2548*	36 Sagittarii ξ¹	5	279 45 0	—21 1 5	— 83,4	+ 8,7	31
2549	112 Herculis	5	279 45 30	+21 5 0	+ 8,6	+ 2,3	1
2550*	37 Sagittarii ξ²	4	279 49 45	—21 28 5	— 83,6	0,0	73

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2551*	62 Serpentis	6	280° 5′ 30″	+ 6° 15′ 40″	— 15,6	+ 8,6	1
2552*	9 Aquilæ	4¾	280 6 40	— 6 12 40	+ 15,7	+ 9,9	1
2553*	63 Serpentis θ	3	280 12 30	+ 3 50 0	— 12,7	+ 14,9	2
2554	13 Herculis	5	280 25 10	+22 17 10	— 4,2	— 7,3	1
2555	64 Serpentis	6	280 25 30	+ 2 9 55	— 12,4	+ 3,7	2
2556	38 Sagittarii ζ	3	280 41 15	—30 16 15	+ 97,2	— 9,5	19
2557	11 Lyræ δ¹	4½	280 42 30	+36 35 40	+ 68,3	+ 58,6	1
2558	12 Lyræ δ²	4	280 54 30	+36 32 0	+ 36,5	— 8,2	2
2559	10 Aquilæ	6	281 8 15	+13 31 10	+ 10,5	+ 16,3	2
2560	11 Aquilæ	6	281 12 45	+13 14 25	— 15,8	+ 7,0	2
2561*	12 Aquilæ	5	281 17 0	— 6 8 5	— 8,8	0,0	1
2562	13 Aquilæ ε	3½	281 23 10	+14 40 45	+ 13,9	+ 7,0	6
2563*	13 Lyræ	6	281 27 30	+43 34 0	+ 69,8		2
2564*	Sagittarii		281 27 50	—21 56 20			2
2565	39 Sagittarii o	4	281 32 15	—22 8 45	— 49,8	— 18,3	82
2566	14 Aquilæ g	6	281 39 0	— 4 6 30	— 19,7	— 0,5	2
2567	47 Draconis o	4	281 39 0	+59 1 20	+ 18,9	— 2,5	4
2568	14 Lyræ γ	3	281 50 0	+32 17 50	+ 23,2	— 16,3	5
2569	40 Sagittarii τ	4	281 52 30	—28 4 15	+ 48,5	— 33,2	19
2570	15 Lyræ λ	6	282 5 10	+31 44 5	— 13,7	+ 22,3	5
2571	15 Aquilæ h	6	282 9 0	— 4 27 15	— 6,3	+ 2,0	2
2572	16 Aquilæ λ	3	282 28 0	— 5 15 35	— 66,0	+ 0,9	2
2573*	Sagittarii		282 38 10	—21?			1
2574	17 Aquilæ ζ	3	282 47 10	+13 26 0	+ 27,0	+ 6,2	10
2575	41 Sagittarii π	4	282 50 45	—21 28 5	— 68,9	— 15,3	98
2576	48 Draconis	6	282 52 0	+57 24 45	+ 18,9	+ 10,9	1
2577*	Draconis	5	283 1 15	+73 45 5			1
2578	18 Aquilæ	6	283 5 15	+10 37 35	+ 38,0	+ 9,0	3
2579*	16 Lyræ	6	283 9 0	+46 31 15			1
2580*	Lyræ	6	283 17 35	+31 17 55			1
2581	19 Aquilæ	6	283 26 30	+ 5 37 25	+ 50,3	— 9,5	2
2582	49 Draconis	6	283 40 0	+55 13 40	— 80,5	+ 6,9	1
2583*	50 Draconis	4½	283 43 0	+75 2 30	+ *	+ 14,4	2
2584*	17 Lyræ	6	283 55 30	+32 2 35	+ 24,8	— 5,4	1
2585	20 Aquilæ	5½	283 58 30	— 8 25 15	— 40,5	+ 14,3	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
2586	18 Lyræ ι	5	284° 3′ 10″	+35° 38′ 40″	+ 31″,5	— 14″,4	4
2587	42 Sagittarii ψ	5	284 7 45	−25 44 35	— 10,7	— 8,7	13
2588*	51 Draconis	5½	284 29 30	+52 56 10	— 15,8	+ 7,2	1
2589	52 Draconis υ	4½	284 30 30	+70 52 0	+ 14,8	+ 27,8	3
2590	21 Aquilæ	5	284 30 50	+ 1 47 55	+ 22,0	+ 18,6	2
2591*	Vulpeculæ	6	284 49 45	+21 3 35			2
2592	43 Sagittarii d	6	284 53 0	−19 27 35	— 54,6	— 6,2	9
2593*	19 Lyræ	6	284 58 40	+30 47 35	— 19,3	+ 4,9	1
2594	22 Aquilæ	6	285 17 0	+ 4 19 35	+ 38,2	+ 4,5	3
2595	1 Sagittæ	6	285 28 45	+20 43 0	+ 24,1	+ 19,7	2
2596*	23 Aquilæ	5	285 41 25	+ 0 33 30	+ 8,0	+ 10,5	3
2597	1 Vulpeculæ	5	285 43 15	+20 52 30	+ 12,8	— 2,8	3
2598	24 Aquilæ	7	285 44 30	− 0 11 20	+ 21,2	+ 8,6	1
2599	20 Lyræ η	6	285 48 0	+38 38 25	+ 13,9	— 12,3	3
2600	25 Aquilæ ω	6	285 48 25	+11 4 15	+ 39,5	+ 1,1	1
2601	44 Sagittarii ρ^1	5	285 55 45	−18 23 10	— 45,6	+ 1,5	14
2602*	45 Sagittarii ρ^2	6	285 56 45	−18 50 15	— 33,0	— 17,3	6
2603	46 Sagittarii υ	6	285 56 45	−16 29 25	+151,1	— 11,8	2
2604	26 Aquilæ f	6	285 59 30	− 5 57 40	+ 28,3	+ 23,7	2
2605	27 Aquilæ d	6	286 8 50	− 1 26 10	+ 4,7	+ 22,3	2
2606*	2 Vulpeculæ	6	286 9 35	+22 30 0			2
2607	28 Aquilæ A	6	286 17 25	+11 50 0	+ 33,1	+ 11,4	1
2608*	29 Aquilæ	7	286 19 —	+10 59 40			1
2609	21 Lyræ ϑ	6	286 24 0	+37 36 35	+ 19,2	— 8,3	3
2610	53 Draconis	5	286 26 30	+56 20 55	+ 29,1	— 9,2	1
2611*	47 Sagittarii χ^1	6	286 35 30	−25 3 35	+ 11,7	— 22,6	5
2612*	48 Sagittarii χ^2	8	286 37 30	−24 58 5			1
2613*	49 Sagittarii χ^3	6	286 39 40	−24 31 15	+ 43,8	— 16,7	4
2614	50 Sagittarii	6	286 57 30	−22 20 35	— 28,5	— 11,3	1
2615	54 Draconis	5	287 8 0	+57 11 15	−156,6	— 17,4	1
2616	55 Draconis	6	287 11 0	+65 28 25	−194,1	— 33,7	1
2617	30 Aquilæ δ	3	287 27 40	+ 2 31 45	+ 36,9	+ 20,1	9
2618	1 Cygni ϰ	4	287 28 30	+52 49 15	+ 32,5	— 17,1	3
2619	3 Vulpeculæ	6	287 31 30	+25 41 45	+ 56,3	+ 6,1	5
2620	31 Aquilæ b	6	287 34 15	+11 18 50	— 37,2	+ 48,5	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2621	2 Sagittæ	6	287° 35′ 45″	+16° 21′ 45″	+ 64,9	+ 11,2	1
2622*	Sagittarii		287 39 10	−27 33 5			1
2623	32 Aquilæ ν	5	287 39 45	− 0 14 45	+ 5,8	+ 12,1	2
2624	3 Sagittæ	6	287 42 0	+16 22 35	+ 28,9	+ 21,3	1
2625*	23 Ursæ Min. δ	3	287 43 0	+86 24 50	−735,0	+ 19,5	1
2626*	2 Cygni	5	287 56 30	+29 2 35	+131,4	+ 0,8	4
2627*	4 Vulpeculæ	6	287 57 35	+19 13 15	+ 48,4	+ 2,0	2
2628	57 Draconis δ	3½	288 5 0	+67 7 0	+ 38,7	+ 7,4	5
2629*	3 Cygni	6	288 8 30	+24 22 55	+ 31,0	− 35,4	1
2630*	5 Vulpeculæ	6	288 10 0	+19 30 45	+ 23,6	+ 0,6	3
2631	35 Aquilæ c	6	288 19 45	+ 1 21 10	+ 21,2	+ 2,6	2
2632	36 Aquilæ e	6	288 36 30	− 3 24 0	+ 6,0	+ 9,5	1
2633	4 Cygni	6	288 42 30	+35 43 30	+150,6	− 9,6	1
2634	6 Vulpeculæ	4	288 56 30	+24 4 10	+ 37,8	− 14,4	4
2635*	7 Vulpeculæ	6	288 57 15	+19 40 10	+ 13,6	+ 6,6	3
2636*	8 Vulpeculæ	6	288 59 20	+24 9 35	+ 63,2		3
2637*	Vulpeculæ	6	288 59 45	+19 17 55			2
2638	51 Sagittarii h^1	6	289 18 0	−25 21 15	− 35,9	− 3,0	2
2639*	52 Sagittarii h^2	5	289 27 20	−25 31 20	− 17,0	− 7,1	2
2640	37 Aquilæ k	6	289 30 25	−11 11 55	− 6,8	+ 6,5	2
2641	6 Cygni β	3½	289 32 10	+27 20 20	+ 78,2	− 4,6	11
2642*	Cygni	6	289 32 55	+27 20 30	+ 65,7	+ 3,5	2
2643*	Sagittarii	7	289 35 25	−19 29 45			1
2644	38 Aquilæ μ	4	289 43 30	+ 6 45 10	+ 54,6	− 0,3	2
2645*	Sagittarii	6	289 45 50	−18 52 50			1
2646	58 Draconis π	4	289 47 30	+65 7 20		+ 1,2	4
2647*	7 Cygni ι^1	6	289 59 0	+51 43 5	− 84,3	− 39,9	1
2648*	59 Draconis	6	290 1 20	+76 1 0	+ 1,9	− 1,8	3
2649*	8 Cygni	6	290 1 30	+33 50 15	+145,0	− 63,6	2
2650	39 Aquilæ x	3½	290 2 45	− 7 40 45	+ 13,5	+ 4,3	3
2651*	41 Aquilæ ι	4	290 9 45	− 1 56 15	+ 20,0	+ 1,0	5
2652	9 Vulpeculæ	6	290 14 45	+19 7 35	− 5,5	+ 4,1	4
2653	60 Draconis τ	4½	290 15 30	+72 45 30	+136,9	+ 36,8	4
2654	53 Sagittarii	6	290 17 30	−24 5 25	− 15,7	− 4,7	2
2655	42 Aquilæ	6	290 19 15	− 5 18 0	+ 50,4	− 6,4	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2656*	Sagittarii	6	290° 21′ 45″	−24° 5′ 25″	— 8,9	— 20,3	1
2657*	10 Cygni i^3	6	290 27 30	+51 5 35	+ 46,4	— 2,1	3
2658*	9 Cygni	6	290 37 —	+28 48 45		— 31,2	1
2659*	24 Ursæ Minoris	6½	290 39 0	+86 45 30	−810,3	+ 15,1	1
2660	54 Sagittarii e^1	6	290 45 30	−16 57 35	— 84,7	— 16,0	2
2661	4 Sagittæ ε	5	290 48 0	+15 47 45	+ 55,8	+ 11,3	1
2662	44 Aquilæ σ	5	290 57 50	+ 4 43 30	+ 33,3	+ 4,9	2
2663	11 Cygni	6	291 8 0	+36 17 5	+127,5	— 10,4	1
2664*	Cygni	6	291 8 —	+41 45 45			1
2665	45 Aquilæ	6	291 11 0	− 1 18 35	+ 11,0	+ 28,8	2
2666*	55 Sagittarii e^2	6	291 12 0	−16 48 20	— 33,7	— 14,9	4
2667*	Sagittarii		291 27 20	−16 13 0			1
2668	5 Sagittæ α	4	291 33 10	+17 19 55	+ 31,4	+ 0,4	4
2669	12 Cygni φ	5	291 45 30	+29 28 5	+100,4	— 4,0	4
2670	6 Sagittæ β	4	291 46 30	+16 47 10	+ 28,5	+ 10,5	4
2671	46 Aquilæ	6	291 55 0	+11 30 10	+ 7,3	— 16,5	2
2672	47 Aquilæ χ	6	291 59 20	+11 7 50	+ 24,6	— 5,0	2
2673	13 Cygni θ	4	292 1 30	+49 31 35	+ 20,0	+ 4,3	2
2674*	56 Sagittarii f	6	292 3 45	−20 27 55	— 17,6	— 5,1	13
2675	14 Cygni	6	292 20 0	+42 7 55	+ 22,0	— 26,6	1
2676	48 Aquilæ ψ	6	292 31 40	+12 35 40	+ 34,0	— 8,6	2
2677	49 Aquilæ υ	6	292 38 10	+ 6 53 20	+ 38,0	+ 27,2	1
2678	10 Vulpeculæ	6	292 41 30	+25 3 20	+ 63,8	+ 14,2	2
2679	50 Aquilæ γ	3	292 52 20	+ 9 53 25	+ 28,7	+ 3,2	43
2680*	61 Draconis σ	4½	293 12 30	+69 7 45	+120,3	— 96,8	5
2681	15 Cygni	6	293 15 0	+36 37 55	+ 99,8	— 3,9	1
2682*	16 Cygni c	6	293 23 0	+49 49 35	+ 23,5	— 16,6	1
2683	7 Sagittæ δ	4½	293 23 20	+17 48 0	+ 7,0	+ 1,3	4
2684	51 Aquilæ	5	293 25 30	−11 30 55	— 2,3	+ 16,5	1
2685	52 Aquilæ π	6	293 31 0	+11 4 40	+ 26,8	+ 0,3	2
2686	57 Sagittarii	6	293 33 15	−19 47 25	— 76,0	— 7,5	4
2687*	17 Cygni χ	5	293 38 30	+33 2 5	+ 99,4	— 36,9	1
2688*	11 Vulpeculæ		293 43 —	+26 35 —			
2689	18 Cygni δ	3½	293 48 0	+44 24 15	+ 84,2	— 22,4	2
	8 Sagittæ ζ	6	293 48 0	+18 23 50	+ 18,7	— 4,3	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2691	53 Aquilæ α	$1\frac{1}{2}$	293° 54′ 0″	+ 8° 5′ 5″	+ 85,1	+ 22,1	63
2692	54 Aquilæ o	$5\frac{3}{4}$	294 2 0	+ 9 40 25	+ 57,4	— 8,4	2
2693	55 Aquilæ η	$3\frac{1}{2}$	294 10 0	+ 0 14 30	+ 6,2	+ 16,5	8
2694	58 Sagittarii ω	5	294 11 15	−27 4 30	+ 51,2	— 9,2	8
2695*	Aquilæ	6	294 16 —	— 3 52 35			1
2696	56 Aquilæ	5	294 19 0	— 9 20 40	+ 28,0	+ 10,8	1
2697	12 Vulpeculæ	5	294 25 30	+21 51 5	+ 26,1	+ 0,5	1
2698	57 Aquilæ	6	294 27 15	— 9 0 0	+ 8,5	+ 6,8	1
2699	59 Sagittarii b	5	294 27 15	−27 56 30	+ 42,3	— 25,5	1
2700*	9 Sagittæ	$6\frac{1}{2}$	294 38 0	+17 54 30	+ 7,6	— 9,7	1
2701	58 Aquilæ	6	294 42 15	— 0 30 10	+ 39,6	+ 7,5	1
2702	59 Aquilæ ξ	5	294 47 30	+ 7 41 40	+ 55,2	— 10,7	2
2703*	19 Cygni	6	294 52 0	+37 57 10	+113,5	— 8,1	1
2704*	60 Sagittarii A	5	294 59 30	−26 59 5	+ 40,8	— 23,5	5
2705	60 Aquilæ β	$3\frac{1}{2}$	295 1 0	+ 5 40 5	+ 16,3	— 37,9	51
2706*	13 Vulpeculæ	$4\frac{1}{2}$	295 3 45	+23 18 5	+ 40,0	— 3,3	1
2707	61 Sagittarii g	6	295 5 45	−16 15 45	— 42,8	— 51,9	4
2708*	Vulpeculæ	6	295 19 45	+23 32 5	+ 47,7	+ 5,4	1
2709	61 Aquilæ φ	6	295 22 30	+10 37 55	+ 52,9	+ 5,0	2
2710	10 Sagittæ	6	295 28 45	+15 50 30	+ 25,8	+ 8,6	1
2711*	20 Cygni d	$5\frac{1}{2}$	295 42 30	+52 13 35	+ 9,8	— 32,2	1
2712	62 Sagittarii c	6	295 52 15	−28 31 30	+ 42,6	— 14,3	4
2713	11 Sagittæ	6	295 54 45	+15 58 55	+ 33,9	+ 11,8	1
2714	21 Cygni η	6	296 9 0	+34 17 15	+ 77,7	— 10,3	3
2715	22 Cygni	6	296 9 0	+37 40 25	+175,4	+ 49,7	2
2716	63 Sagittarii	6	296 9 15	−14 27 20	— 31,5	— 7,1	1
2717	12 Sagittæ γ	4	296 14 20	+18 40 55	+ 23,7	— 6,4	3
2718	14 Vulpeculæ	5	296 28 0	+22 17 20	+ 10,2	— 8,6	2
2719*	13 Sagittæ	6	296 30 5	+16 41 45	+ 26,2	+ 12,2	1
2720	23 Cygni	6	296 44 30	+56 44 0	— 64,7	— 28,4	1
2721*	24 Cygni ψ	5	296 53 15	+51 37 40	+ 44,3	+ 16,1	1
2722	64 Sagittarii	6	296 59 45	−12 26 25	— 39,3	+ 4,7	2
2723	65 Sagittarii	6	297 3 0	−13 29 50	— 23,4	— 28,7	1
2724*	15 Vulpeculæ	$4\frac{1}{2}$	297 4 0	+26 55 30	+ 76,6	— 5,1	2
2725	62 Aquilæ	6	297 5 45	— 1 32 25	+ 19,0	— 3,4	4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					℞.	D.	
2726	25 Cygni	6	297° 6′ 0″	+36° 13′ 15″	+145,4″	— 16,1″	2
2727	16 Vulpeculæ	5	297 12 30	+24 5 45	+ 51,3	+ 13,9	1
2728	63 Aquilæ τ	6	297 14 10	+ 6 26 0	+ 40,2	+ 10,9	1
2729	63 Draconis ε	5½	297 16 30	+69 28 25	— 73,7	+ 16,7	5
2730*	14 Sagittæ	6	297 19 30	+15 11 10	+ 19,1	+ 19,1	2
2731*	15 Sagittæ	6	297 32 10	+16 15 30	+ 10,8	— 23,2	1
2732	16 Sagittæ η	6	297 50 50	+19 7 50	+ 26,1	+ 9,9	2
2733*	64 Aquilæ	6	298 0 0	— 1 32 20	— 15,5	+ 8,0	3
2734*	26 Cygni e	6	298 8 30	+49 16 5	+ 18,1	— 26,4	1
2735	17 Vulpeculæ	4½	298 23 0	+22 45 0	+ 40,6	— 2,0	2
2736*	1 Capricorni ξ^1	6	298 41 30	—13 16 15	— 12,2	— 17,4	3
2737	27 Cygni b^1	5	298 42 0	+35 9 0	+ 38,8	— 56,7	1
2738	2 Capricorni ξ^2	6	298 47 45	—13 29 5	— 40,6	— 25,3	9
2739	65 Aquilæ θ	3	298 49 20	— 1 42 35	+ 6,6	— 12,5	41
2740	17 Sagittæ θ	6	299 3 45	+20 1 0	+ 22,8	+ 28,1	1
2741	66 Aquilæ	5½	299 18 0	— 1 54 25	+ 6,1	+ 8,9	8
2742	18 Vulpeculæ	5¾	299 23 30	+26 0 40	+ 60,2	+ 6,8	3
2743	28 Cygni b^2	5	299 26 30	+35 57 20	+145,4	— 18,2	2
2744	64 Draconis e	5½	299 36 0	+63 57 50	—274,7	— 14,0	3
2745	19 Vulpeculæ	6	299 42 0	+25 54 40	+ 47,8	+ 2,8	3
2746	20 Vulpeculæ	5½	299 44 30	+25 34 50	+ 52,4	+ 0,2	3
2747*	65 Draconis	7	299 45 30	+63 45 50	—206,3	+ 14,2	2
2748	3 Capricorni	6	299 48 30	—13 14 55	— 46,8	— 5,6	3
2749	4 Capricorni	6	299 56 45	—22 43 40	— 25,9	— 0,3	2
2750	67 Aquilæ ρ	5	299 58 50	+14 16 40	+ 15,4	+ 21,3	1
2751	5 Capricorni α^1	4	300 7 0	—13 25 35	— 30,6	— 12,4	55
2752	66 Draconis	6	300 11 30	+61 6 30	—177,0	— 1,9	2
2753	6 Capricorni α^2	3	300 12 45	—13 27 55	— 23,5	— 14,2	60
2754	21 Vulpeculæ	5½	300 21 10	+27 47 10	+ 97,4	— 17,3	2
2755	67 Draconis ρ	5	300 21 30	+66 59 40	—154,3	— 3,0	7
2756*	7 Capricorni σ	6	300 22 30	—20 2 45	— 42,3	— 10,3	15
2757	22 Vulpeculæ	5	300 31 10	+22 35 20	+ 46,0	+ 2,1	1
2758*	Vulpeculæ	5	300 32 —	+24 40 15			1
2759	18 Sagittæ	6	300 40 45	+20 40 15	+ 1,8	+ 18,6	1
2760	29 Cygni b^3	6	300 42 0	+35 53 10	+132,8	— 17,9	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2761	23 Vulpeculæ	4½	300° 42′ 45″	+26° 53′ 40″	+ 75,8	− 16,8	2
2762*	Capricorni	8	300 50 30	−15 43 20	− 39,7	− 20,0	2
2763	8 Capricorni ν	6	300 52 0	−13 41 40	− 29,0	− 14,5	3
2764	24 Vulpeculæ	5	300 52 15	+23 44 45	+ 42,6	− 8,1	1
2765*	30 Cygni o^1	4½	300 52 30	+45 54 10	+ 54,8	− 15,5	3
2766	9 Capricorni β	3	300 54 0	−15 43 15	− 38,8	− 12,6	60
2767	31 Cygni o^2	5	300 57 0	+45 49 40	+ 69,2	− 25,2	3
2768	32 Cygni	5½	301 27 30	+46 47 10	+ 54,7	− 15,5	2
2769*	33 Cygni	5	301 32 —	+55 38 40		− 5,6	1
2770*	34 Cygni	6	301 34 30	+37 6 0	+ 68,2	− 27,7	1
2771*	68 Draconis	6	301 37 0	+61 8 55	− 13,1	+ 13,9	2
2772*	35 Cygni	6	301 39 30	+34 2 20	+111,4	− 5,1	2
2773*	36 Cygni	6	301 42 0	+36 3 10	+ 67,1	+ 12,7	1
2774*	69 Draconis	6	301 44 30	+75 35 50	+596,5	+ 3,7	1
2775*	Aquilæ		301 52 —	+ 4 22 55			1
2776	25 Vulpeculæ	6	302 10 30	+23 29 10	+ 37,6	− 6,1	1
2777*	10 Capricorni π		302 23 30	−19 11 15	− 36,4	− 17,2	29
2778*	37 Cygni γ	3	302 45 13	+39 17 20	+ 92,1	− 8,2	10
2779	11 Capricorni ρ	6	302 47 30	−18 47 45	− 33,9	− 25,6	31
2780*	Capricorni	8	302 49 15	−18 50 40	+ 20,5		1
2781*	39 Cygni	6	302 50 30	+31 12 50	+119,6	+ 0,6	1
2782*	12 Capricorni σ		303 1 40	−19 34 5	− 28,9	− 24,5	8
2783*	68 Aquilæ	6	303 2 0	− 4 21 0	+ 22,7	+ 2,7	2
2784*	69 Aquilæ	5	303 21 15	− 3 53 5	+ 11,4	+ 1,5	2
2785	71 Draconis	6	303 35 0	+61 17 10	+ 38,1	− 5,8	2
2786*	Capricorni	6	303 50 —	−10 53 15			1
2787	1 Delphini	6	303 51 30	+ 9 53 5	+ 22,1	+ 10,3	1
2788	40 Cygni	6	304 0 30	+37 27 0	+ 58,7	− 27,4	1
2789*	41 Cygni	4	304 10 0	+29 21 40	+ 70,4	− 6,8	4
2790*	Cephei		304 21 15	+76 52 —			1
2791	42 Cygni	6	304 22 0	+35 26 50	+ 58,6	− 14,0	1
2792*	43 Cygni ω^1	5	304 23 30	+48 22 50	+ 9,5	− 18,5	2
2793*	72 Draconis	6	304 23 50	+61 17 0			1
2794*	1 Cephei κ	5	304 30 0	+76 45 25	+465,6	+ 3,6	6
2795	2 Delphini ε	3	304 35 20	+10 16 40	+ 44,0	+ 4,5	7

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					ℛ.	D.	
2796	44 Cygni	6	304° 47′ 30″	+35° 55′ 5″	+ 56,4	− 13,1	1
2797	3 Delphini η	6	304 48 45	+11 59 25	+ 40,3	+ 14,5	5
2798*	13 Capricorni τ^1	6	304 59 30	−16 10 35	− 34,6	− 42,4	1
2799	45 Cygni ω^2	5	305 6 30	+47 56 0	+ 29,8	− 18,1	2
2800*	70 Aquilæ	5	305 7 45	− 3 35 40	+ 41,7	+ 6,3	2
2801	4 Delphini ζ	5	305 12 30	+13 37 35	− 9,2	+ 26,6	1
2802*	46 Cygni ω^3	5	305 26 30	+48 11 40	− 11,8	− 6,9	2
2803*	47 Cygni	6	305 27 30	+34 12 45	+ 27,5	+ 3,0	4
2804	14 Capricorni τ^2	6	305 28 40	−15 59 55	− 16,9	− 34,0	11
2805*	71 Aquilæ	4	305 34 45	− 2 9 35	− 3,1	+ 8,7	3
2806	15 Capricorni υ	6	305 35 40	−19 11 15	− 25,6	− 31,8	11
2807	26 Vulpeculæ	6	305 42 30	+24 50 0	+ 29,2	+ 0,9	1
2808	5 Delphini ι	6	305 44 30	+10 19 20	+ 20,8	+ 11,3	1
2809	6 Delphini β	3	305 45 0	+13 33 25	+ 23,4	− 43,7	11
2810*	Capricorni	6	305 46 —	−17 11 20			1
2811	1 Aquarii	6	305 53 0	− 0 34 35	− 17,7	+ 15,0	2
2812	27 Vulpeculæ	5	305 57 30	+25 24 30	+ 29,2	+ 1,1	1
2813	2 Cephei θ	5	306 0 30	+61 58 0	+252,4	− 15,5	6
2814	7 Delphini κ	6	306 0 30	+ 9 1 20	+ 53,6	+ 7,9	1
2815	8 Delphini θ	6	306 1 0	+12 15 5	+ 35,8	+ 15,5	2
2816*	29 Vulpeculæ	5	306 10 —	+20 8 20		+ 5,5	1
2817	48 Cygni	6	306 13 0	+30 30 35	+ 79,1		1
2818*	Cygni		306 14 —	+30 27 45			1
2819	28 Vulpeculæ	6	306 16 15	+23 3 30	− 37,5	− 13,9	2
2820*	9 Delphini α	3	306 18 0	+14 50 30	+ 39,9	+ 16,2	8
2821	10 Delphini	6	306 41 15	+13 30 25	+ 7,0	+ 3,6	1
2822	16 Capricorni ψ	5	306 54 40	−26 20 20	+ 13,6	− 47,1	3
2823	17 Capricorni	6	307 2 10	−22 36 20	− 15,7	− 3,0	4
2824	49 Cygni	6	307 6 0	+31 13 35	+ 99,9	+ 8,5	1
2825	11 Delphini δ	3½	307 14 15	+13 59 5	+ 35,1	+ 17,6	12
2826*	50 Cygni α	2	307 40 30	+44 12 0	+162,3	− 25,7	5
2827*	Capricorni	6	307 42 15	−19 17 35			1
2828	2 Aquarii ε	4¾	307 43 0	−10 36 5	− 4,4	+ 6,6	17
2829	3 Aquarii	5	307 50 10	− 6 7 55	+ 9,4	− 1,1	2
2830	30 Vulpeculæ	6	307 51 0	+24 11 15	+ 65,2	− 13,9	30

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2831	12 Delphini γ	3	308° 3′ 30″	+15° 1′ 45″	+ 42″,3	+ 5″,7	13
2832	13 Delphini	5	308 6 0	+ 4 53 55	+ 38,9	— 2,6	1
2833	51 Cygni	6	308 10 0	+49 15 10	— 10,7	— 27,2	1
2834*	52 Cygni	6	308 11 40	+29 36 40	+ 92,6	+ 3,1	1
2835	18 Capricorni ω	6	308 17 30	—28 2 5	+ 59,1	— 30,0	3
2836	53 Cygni ε	3	308 24 0	+32 50 10	+104,9	+ 9,2	2
2837	14 Delphini	6	308 38 15	+ 6 44 5	+ 49,5	+ 22,4	1
2838*	15 Delphini	6	308 43 —	+11 24 50		+ 7,1	1
2839	4 Aquarii	6	308 44 30	— 6 45 20	+ 9,4	+ 4,2	1
2840	54 Cygni λ	4	308 49 0	+35 22 40	+ 82,2	— 18,9	1
2841	73 Draconis	5½	308 49 0	+73 52 30	— 56,2	+ 34,8	3
2842*	5 Aquarii	6	308 55 0	— 6 38 25	+ 53,8	+ 4,4	2
2843	6 Aquarii μ	4½	308 58 40	—10 7 5	— 8,3	+ 8,9	17
2844*	Capricorni		309 16 —	—19 55 45			1
2845	19 Capricorni	6½	309 19 0	—19 3 45	— 45,2	— 13,4	8
2846*	Cephei	6	309 19 5	+56 29 40			1
2847	55 Cygni	6	309 34 30	+44 59 5	+ 82,0	— 5,2	1
2848*	3 Cephei η	4	309 39 0	+60 38 55	+301,2	+ 46,3	5
2849*	4 Cephei	6	309 41 30	+65 32 30	+380,2	— 8,6	2
2850*	31 Vulpeculæ	6	309 42 0	+25 57 30	+ 64,6	+ 6,5	1
2851	56 Cygni	6	309 45 20	+42 55 5	+ 69,9	— 13,0	1
2852	7 Aquarii	6	310 1 15	—10 51 5	+ 23,2	— 14,7	3
2853	16 Delphini	6	310 11 0	+11 24 20	+108,5	+ 19,5	1
2854	17 Delphini	6	310 11 0	+12 33 40	+144,3	+ 13,4	1
2855*	32 Vulpeculæ	5	310 19 30	+26 54 25	+ 58,6	— 15,8	1
2856	20 Capricorni	6½	310 29 40	—20 12 5	— 51,0	— 11,8	4
2857	1 Piscis Aust.	5	310 31 20	—33 25 30	+ 68,8	— 31,6	1
2858	57 Cygni	6	310 33 50	+43 14 25	+ 42,7	— 25,7	1
2859	8 Aquarii	6½	310 42 45	—14 13 35	— 17,6	+ 1,6	1
2860	1 Equulei	5	310 50 30	+ 3 7 30	+180,5	+ 17,9	2
2861*	21 Capricorni	6	310 51 0	—18 42 15	— 41,4	— 15,3	4
2862	18 Delphini	6	310 51 15	+ 9 40 35	+ 75,7	— 20,9	1
2863	9 Aquarii	6	311 0 10	—14 42 30	— 5,4	— 10,5	1
2864	10 Aquarii	6	311 1 15	— 6 39 30	+ 41,6	+ 8,0	1
2865	11 Aquarii	6	311 2 40	— 5 53 35	+ 34,9	— 26,6	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2866	33 Vulpeculæ	6	311° 5′ 45″	+21° 9′ 0″	+ 34,3	+ 6,7	1
2867*	Aquarii		311 13 20	+ 6 19 5			1
2868	58 Cygni ν	4	311 23 30	+40 0 0	+ 65,9	— 24,2	3
2869	74 Draconis	6	311 24 0	+79 58 0	— 74,9	+ 42,0	2
2870	75 Draconis	6	311 29 0	+80 19 10	—277,6	+ 28,3	2
2871	22 Capricorni η	5	311 41 30	—21 2 50	— 66,3	— 3,8	20
2872	2 Equulei	6	311 43 15	+ 5 59 5	+ 33,7	+ 15,5	1
2873*	2 Piscis Aust.	6	311 49 20	—33 32 40	+ 47,0	— 5,0	1
2874	12 Aquarii	6	311 54 40	— 7 0 55	+ 4,7	— 21,6	1
2875	23 Capricorni θ	5	312 7 50	—18 25 50	— 48,4	— 14,3	29
2876	24 Capricorni A	6	312 13 10	—26 12 15	+ 30,0	— 34,5	5
2877	3 Equulei	6	312 16 30	+ 4 17 45	+ 28,1	+ 12,5	2
2878	59 Cygni f^1	5½	312 18 20	+46 20 5	+ 77,6	— 17,9	1
2879	4 Equulei	6	312 30 45	+ 4 45 30	+ 20,9	— 0,9	2
2880	60 Cygni	6	312 34 40	+44 57 35	+ 66,4	— 7,1	2
2881	25 Capricorni χ	6	312 41 30	—22 24 15	— 30,9	— 19,2	5
2882*	26 Capricorni	6	312 53 40	—21 24 35	— 39,1	— 22,7	2
2883*	27 Capricorni	6	312 57 0	—21 45 55	— 33,2	— 25,4	4
2884	13 Aquarii ν	5	313 10 10	—12 35 45	— 2,8	— 7,3	21
2885*	61 Cygni	6	313 14 30	+37 15 35	+ *	+ *	1
2886	62 Cygni ξ	4	313 24 45	+42 43 0	+ 27,1	— 22,6	2
2887*	3 Piscis Aust.	6	313 42 —	—28 50 40		— 33,7	1
2888	5 Equulei γ	4	313 48 20	+ 8 54 20	+ 40,2	+ 5,7	5
2889	6 Equulei	6	313 50 55	+ 8 48 30	+ 57,4	+ 10,3	5
2890	63 Cygni f^2	6	313 58 13	+46 25 40	+ 59,7	— 24,4	1
2891	28 Capricorni φ	6	314 29 40	—21 54 10	— 51,0	— 21,9	7
2892	29 Capricorni	6	314 39 0	—16 25 20	— 56,7	— 29,5	10
2893	4 Piscis Aust.	4½	314 44 30	—33 25 20	+ 54,4	— 47,1	2
2894*	7 Equulei δ	4	314 49 50	+ 8 47 25	+ 55,0	— 71,2	6
2895	14 Aquarii	6	314 54 30	—10 28 20	— 9,6	— 13,9	2
2896	64 Cygni ζ	3	314 55 45	+28 58 40	+ 47,2	— 3,3	1
2897	8 Equulei α	4	315 4 30	+ 3 59 20	+ 20,0	+ 6,3	6
2898	30 Capricorni	6	315 8 30	—19 15 5	— 51,7	— 11,1	4
2899	31 Capricorni	7	315 13 40	—18 44 5	— 49,1	+ 3,5	2
2900	15 Aquarii	6	315 28 10	— 5 47 5	+ 16,5	— 33,1	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2901*	Capricorni	6	315° 30′ 10″	−17° 26′ 50″	″	″	1
2902	65 Cygni τ	4	315 36 0	+36 44 45	+ 53,4	+ 13,8	2
2903	76 Draconis	5	315 38 0	+81 20 30	−212,0	+ 17,3	1
2904	16 Aquarii	6	316 12 0	− 5 51 5	− 5,9	+ 10,3	2
2905	32 Capricorni ι	5	316 14 30	−18 7 15	− 36,2	− 19,9	28
2906	66 Cygni υ	5	316 17 0	+33 37 0	+ 62,0	− 1,2	1
2907	67 Cygni σ	4	316 18 20	+38 7 10	+ 44,1	− 13,3	2
2908	9 Equulei	6	316 25 45	+ 6 3 50	+ 25,4	+ 6,4	3
2909	17 Aquarii	6	316 34 15	−10 36 35	− 0,2	− 15,8	3
2910	33 Capricorni	6	316 38 30	−22 8 15	− 50,1	− 20,8	3
2911*	68 Cygni A	6	316 43 0	+42 40 5	+ 57,0		1
2912*	34 Vulpeculæ	6	316 45 30	+22 34 30			1
2913	18 Aquarii	6	316 48 10	−14 10 30	− 0,5	− 19,0	3
2914	10 Equulei β	4	316 52 25	+ 5 30 35	+ 7,6	+ 5,7	3
2915*	1 Pegasi	4	316 55 15	+18 30 15	+ 77,4	− 6,8	3
2916	19 Aquarii	6	317 7 40	−11 2 15	+ 10,5	− 24,6	5
2917	20 Aquarii	6	317 10 0	− 4 42 0	+ 10,4	− 8,2	1
2918	34 Capricorni ζ	5	317 13 45	−23 43 5	− 25,6	− 24,2	11
2919	21 Aquarii	6	317 16 0	− 4 51 30	+ 21,1	− 11,9	2
2920	35 Capricorni	6	317 24 30	−22 29 45	− 35,1	− 44,7	3
2921*	5 Piscis Aust.	6	317 34 0	−32 32 10	+142,6		1
2922	77 Draconis	5	317 39 30	+76 51 10	+161,4	+ 26,0	1
2923	5 Cephei α	3	317 43 30	+61 17 40	+244,2	− 31,0	3
2924	36 Capricorni b	6	317 45 10	−23 7 5	− 27,1	− 38,1	6
2925*	Pegasi		317 58 —	+18 4 —			1
2926	6 Cephei	6	318 9 40	+63 34 30	+208,9	− 25,1	2
2927*	69 Cygni	6	318 17 0	+35 21 10	+ 18,6	− 16,4	2
2928*	6 Piscis Aust.	6	318 20 0	−35 15 50	+ 19,4	−115,8	2
2929	35 Vulpeculæ	6	318 29 30	+26 16 45	+ 59,6	+ 3,8	1
2930	70 Cygni	6	318 40 0	+35 47 40			2
2931	22 Aquarii β	3	318 48 0	− 6 54 15	+ 8,8	− 20,6	58
2932*	2 Pegasi	6	318 58 45	+22 18 5	+ 8,9	+ 2,7	1
2933	37 Capricorni	6	319 21 10	−21 26 15	− 41,1	− 3,1	7
2934*	38 Capricorni	7	319 21 45	−21 35 35	− 43,4	− 28,3	2
2935	7 Piscis Aust.	6	319 29 45	−34 23 20	+ 88,6		1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2936*	71 Cygni g	6	319° 30′ 0″	+45° 12′ 10″	+ 49,7	− 35,8	1
2937	8 Piscis Aust.	4½	319 30 30	−27 31 0	+ 81,2	− 34,6	1
2938	39 Capricorni ε	4	319 55 30	−20 49 25	− 40,8	− 16,5	13
2939	23 Aquarii ξ	6	320 18 10	− 9 13 5	+ 11,0	− 6,4	23
2940	7 Cephei	6	320 20 30	+65 28 20	+300,0	− 23,6	2
2941*	Aquarii		320 26 —	− 1 44 35			1
2942	72 Cygni	6	320 31 0	+37 10 10	+ 86,0	− 17,8	1
2943	3 Pegasi	6	320 33 0	+ 5 15 0	+ 74,9	− 4,6	1
2944	73 Cygni ρ	4	320 35 0	+44 14 50	+ 23,9	− 37,8	1
2945	40 Capricorni γ	4	320 43 30	−18 1 45	− 30,5	− 29,3	47
2946	4 Pegasi	6	320 44 0	+ 4 23 40	+ 72,6	+ 7.5	2
2947	5 Pegasi	6½	320 48 15	+17 56 40	+ 53,4	+ 2 0	1
2948	24 Aquarii	6	320 54 50	− 1 25 0	− 94,1	− 41,8	1
2949*	25 Aquarii d	6	320 57 10	+ 0 52 15	− 0,4	+ 2,2	3
2950	8 Cephei β	3	321 3 0	+69 12 25	+259,4	+ 0,9	3
2951	41 Capricorni	6	321 4 10	−24 37 45	+ 5,3	− 41,9	2
2952	74 Cygni	6	321 7 0	+39 2 30	+ 88,6	− 8,1	1
2953*	Pegasi	6	321 7 50	+18 53 0			1
2954*	42 Capricorni	6	321 10 10	−15 24 0	− 40,4	− 39,7	5
2955	43 Capricorni $\varkappa$	5	321 20 0	−20 14 55	− 34,6	− 18,1	5
2956*	44 Capricorni	6	321 32 10	−15 47 5	− 35,7	− 26,7	4
2957	9 Piscis Aust. ι	4	321 34 30	−34 23 50	+ 52,4	− 62,6	2
2958	26 Aquarii	6	321 35 30	− 0 6 25	− 22,3	+ 14,6	2
2959	7 Pegasi	6	321 40 45	+ 4 17 20	+ 22,8	+ 6,0	1
2960	45 Capricorni	6	321 46 10	−16 8 5	− 37,1		4
2961	75 Cygni	6	322 0 0	+41 53 25	+ 61,3	− 21,4	1
2962*	46 Capricorni c^1	6	322 6 40	−10 28 55	− 4,1	− 3,0	6
2963	8 Pegasi ε	3	322 13 40	+ 8 28 25	+ 45,8	+ 7,3	27
2964	76 Cygni	6	322 16 0	+39 25 0	+ 68,8	− 7,9	1
2965*	9 Cephei	6	322 19 0	+60 42 20	+299,0	− 37,2	1
2966	10 Piscis Aust. θ	4	322 20 30	−32 17 40	+ 49,9	− 45,6	2
2967*	47 Capricorni c^2	6	322 25 40	−10 40 50	− 11,9	− 4,8	5
2968*	9 Pegasi	4½	322 26 45	+15 56 50	+ 54,3	+ 4,0	3
2969	48 Capricorni λ	5	322 27 0	−12 46 5	+ 6,6	− 12,9	17
2970	77 Cygni	6	322 28 0	+39 40 45	+ 83,5	+ 1,6	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
2971*	49 Capricorni δ	3	322° 29′ 0″	−17° 29′ 45″	− 35,1	− 69,5	49
2972	50 Capricorni	6	322 29 10	−13 5 25	+ 5,4	− 20,6	1
2973*	78 Cygni μ	5	322 34 0	+27 21 20	+ 67,4	+ 6,7	1
2974	10 Pegasi κ	4	322 38 45	+24 14 5		+ 19,4	2
2975*	79 Cygni	6	322 39 15	+36 53 5	+ 35,7		1
2976*	Cygni	6	322 39 20	+39 45 25	+ 64,1	− 9,4	1
2977	80 Cygni π^1	4	322 47 0	+49 47 40	− 3,2	− 14,7	1
2978*	12 Pegasi	6	322 52 30	+21 32 20	+299,1	+ 2,7	1
2979*	27 Aquarii	6	322 52 30	+ 1 16 30	+ 0,7	− 4,9	4
2980*	Capricorni		323 13 50	−14 8 30			2
2981	13 Pegasi	6	323 50 0	+15 51 45	+ 73,1	+ 4,2	5
2982	81 Cygni π^2	5	323 51 0	+47 53 45	− 10,3	− 16,8	1
2983	14 Pegasi	6	324 2 0	+28 44 40	+ 32,3	+ 16,2	1
2984*	10 Cephei ν	5	324 5 0	+59 42 20	+167,5	− 9,5	3
2985	51 Capricorni μ	5	324 5 30	−14 59 5	− 3,0	− 15,2	31
2986*	Aquarii		324 5 50	−11 59 50			1
2987	11 Cephei	5	324 14 30	+69 53 10	+245,8	+ 23,5	2
2988*	Aquarii		324 15 0	−11 44 45			1
2989	12 Cephei	7	324 29 30	+59 15 50	+297,2	+ 9,5	1
2990*	Pegasi	6	324 36 —	+ 5 26 0			1
2991	15 Pegasi	6	324 39 45	+27 21 25	+ 36,1	+ 10,0	1
2992	16 Pegasi	6	324 44 0	+24 28 50	+ 48,2	+ 17,5	3
2993	78 Draconis	5	324 47 0	+70 53 50	− 44,7	+ 23,0	2
2994*	11 Piscis Aust.	6	325 24 45	−29 4 20			1
2995	17 Pegasi	6	325 26 35	+10 37 15	+ 49,8	+ 11,0	2
2996	12 Piscis Aust. η	5	325 42 30	−29 54 20	+ 81,0	− 44,5	2
2997*	13 Cephei μ	6	326 7 —	+55 9 50			1
2998*	18 Pegasi	6	326 8 45	+ 5 16 0	+ 56,4	− 15,3	3
2999	28 Aquarii	6	326 18 15	− 0 51 0	− 6,5	− 47,8	3
3000	29 Aquarii	6	326 22 0	−18 26 0	− 41,7	− 14,5	1
3001	19 Pegasi	6	326 25 30	+ 6 47 55	+ 62,8	− 40,0	1
3002	20 Pegasi	6	326 29 0	+11 39 0	+ 59,8	+ 12,5	1
3003*	13 Piscis Aust.	6	326 35 15	−31 22 10	+ 58,4	− 93,4	1
3004*	Piscis Aust.	4	326 43 —	−28 19 0			1
3005	30 Aquarii	6	326 46 45	− 8 0 0	−161,3	+ 2,5	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3006*	31 Aquarii o	5	326° 49′ 0″	− 3° 37′ 50″	− 4,8	− 4,7	8
3007	21 Pegasi	5	327 0 30	+ 9 53 40	+ 76,5	+ 49,5	2
3008	32 Aquarii	6	327 12 45	− 2 23 5	− 19,9	− 6,1	4
3009	79 Draconis	7	327 16 0	+72 14 35	−124,9	− 5,3	1
3010	33 Aquarii ι	4	327 25 10	−15 20 55	− 26,8	− 18,1	16
3011	34 Aquarii α	3	327 28 0	− 1 48 20	− 14,9	− 2,4	46
3012	22 Pegasi ν	5	327 29 45	+ 3 33 20	+ 56,8	+ 33,2	6
3013*	14 Piscis Aust. μ	4½	327 31 40	−34 27 40	+ 55,6	− 64,9	2
3014*	Aquarii		327 41 20	−11 56 35			2
3015*	Aquarii		327 49 25	−11 33 55			1
3016	14 Cephei	6	327 51 0	+56 31 4	+281,0	− 0,9	1
3017	23 Pegasi	6	327 52 0	+27 28 30	+102,2	− 1,5	1
3018	15 Piscis Aust.	5½	327 57 0	−34 1 50	+ 79,6	− 65,3	2
3019	35 Aquarii	5¾	327 59 30	−20 0 50	− 44,0	− 7,5	3
3020*	Aquarii		328 0 15	−14 47 30			1
3021	24 Pegasi ι	4	328 8 20	+23 51 0	+ 73,9	− 3,7	7
3022*	36 Aquarii	7	328 15 10	− 9 41 30	+ 82,9		2
3023*	Aquarii	7	328 16 —	− 9 0 25			1
3024	25 Pegasi	6½	328 20 15	+20 11 50	− 41,5	+ 45,3	1
3025*	15 Cephei	6¾	328 23 0	+58 19 40	+262,7	− 14,6	1
3026*	37 Aquarii e^1	6	328 29 30	−12 19 10	− 7,3	− 24,1	3
3027*	38 Aquarii e^2	6	328 30 30	−13 3 50	− 23,7	− 19,8	26
3028	16 Cephei	5½	328 34 30	+71 42 35	+291,7	− 6,7	2
3029	26 Pegasi θ	4	328 37 30	+ 4 41 15	+ 68,3	+ 14,7	14
3030*	17 Cephei ξ	5	328 38 30	+63 8 0	+ *	− 13,0	1
3031*	18 Cephei		328 39 —	+61 37 0			1
3032	19 Cephei	6	328 51 0	+60 47 20	+175,7	− 17,1	2
3033	20 Cephei	6	328 51 30	+61 17 40	+179,9	− 30,6	1
3034*	27 Pegasi π^1	5	328 52 30	+31 40 25	+ 27,7	− 4,0	2
3035*	39 Aquarii	7	328 55 30	−15 42 5	− 27,5	− 0,1	3
3036	28 Pegasi	6½	328 57 30	+19 28 10	+ 44,1	+ 7,2	1
3037	29 Pegasi π^2	4½	329 3 15	+31 40 40	+ 44,4	− 19,6	5
3038*	16 Piscis Aust. λ	4½	329 8 40	−29 16 30	+ 54,4	− 89,6	2
3039	40 Aquarii	7½	329 11 45	−13 25 0	+ 36,8	− 81,7	1
3040	41 Aquarii	6	329 16 30	−22 35 45	+ 4,5	− 6,4	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3041	21 Cephei ζ	4½	329° 58′ 30″	+56° 41′ 15″	+237,2	− 5,4	2
3042*	42 Aquarii	6	330 2 45	−14 21 15	− 24,5	− 12,7	12
3043	43 Aquarii θ	4	330 6 30	− 9 18 30	+ 15,7	− 0,6	23
3044*	Lacertæ	5	330 7 40	+38 11 50			1
3045	22 Cephei λ	6	330 11 30	+57 54 10	+239,9	− 17,9	1
3046	44 Aquarii	6	330 13 10	− 6 54 35	+ 20,5	− 26,0	2
3047	45 Aquarii	6	330 35 30	−14 49 0	− 31,8	− 78,4	2
3048	1 Lacertæ	5	330 36 15	+36 13 20	+ 92,6	− 8,4	3
3049	23 Cephei ε	4	330 51 0	+55 30 50	+313,5	− 7,6	3
3050	24 Cephei	5½	330 52 30	+70 49 15	+230,2	+ 1,1	2
3051	46 Aquarii ρ	5½	330 57 40	− 9 21 25	+ 12,1	− 9,1	21
3052	80 Draconis	6	331 2 0	+70 35 10		+ 19,4	1
3053	47 Aquarii	5½	331 7 0	−23 7 55	− 10,4	− 9,7	2
3054	30 Pegasi	6	331 12 15	+ 4 14 45	+ 37,5	+ 13,5	3
3055	48 Aquarii γ	3	331 24 40	− 2 56 5	− 6,7	+ 9,4	25
3056	49 Aquarii	5	331 32 0	−26 17 45	+ 20,8	− 54,6	1
3057	31 Pegasi	4½	331 33 25	+10 39 25	+ 40,2	+ 12,3	3
3058	32 Pegasi	6	331 44 50	+26 46 55	+ 63,0	+ 9,3	2
3059*	Aquarii		331 49 30	− 8 44 10			1
3060*	50 Aquarii	6	331 57 30	−15 4 50	− 28,6	− 8,0	3
3061	25 Cephei	7	331 58 30	+61 15 55	+246,7	− 11,9	1
3062	51 Aquarii	6	331 59 0	− 6 23 5	+ 12,6	− 12,4	1
3063	2 Lacertæ	5	332 4 0	+44 59 45	+ 28,2	− 24,2	3
3064	33 Pegasi	6½	332 10 45	+19 17 35	+ 73,2	+ 14,2	1
3065	52 Aquarii π	5	332 21 40	− 0 10 45	− 4,9	+ 1,4	16
3066	53 Aquarii	6	332 25 45	−18 17 45	− 5,9	− 19,0	2
3067	54 Aquarii	6	332 33 40	−12 46 20	− 50,8	− 55,0	1
3068	34 Pegasi	6	332 42 0	+ 2 49 35	+ 57,3	+ 13,5	2
3069	3 Lacertæ	4½	332 52 0	+50 41 40	− 14,6	− 32,2	2
3070	4 Lacertæ	5	333 1 0	+47 55 45	− 30,3	− 39,8	1
3071	35 Pegasi	6	333 1 40	+ 3 9 15	+ 56,1	− 7,6	2
3072	55 Aquarii ζ	4	333 13 0	− 1 35 30	+ 7,3	+ 8,1	29
3073*	Piscis Aust.	4½	333 24 —	−27 38 10			1
3074	36 Pegasi	6½	333 24 20	+ 7 34 0	+ 47,6	− 18,4	3
3075*	56 Aquarii	6	333 25 0	−16 9 0	− 35,1	− 16,7	3

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
3076*	17 Piscis Aust. β	3	333° 25′ 15″	−33° 53′ 20″	+ 72,9	−103,4	1
3077	57 Aquarii σ	5	333 33 15	−12 14 5	− 11,4	− 56,3	31
3078	37 Pegasi	6	333 33 55	+ 2 52 15	+ 29,5	− 2,4	2
3079	58 Aquarii	6	333 48 30	−12 28 25	− 10,4	− 18,0	8
3080	38 Pegasi	6	333 57 15	+30 59 50	+ 84,8	+ 8,0	1
3081	5 Lacertæ	4½	334 10 40	+46 8 25	− 12,7	− 26,5	1
3082	26 Cephei	6	334 13 15	+63 34 0	+274,0	− 19,1	1
3083	6 Lacertæ	5	334 16 10	+41 33 0	+101,1	− 10,8	2
3084	27 Cephei δ	4½	334 22 30	+56 51 0	+226,4	− 36,0	2
3085*	39 Pegasi	6½	334 24 0	+18 39 5	+ 52,4	− 12,3	1
3086	59 Aquarii υ	5	334 25 30	−22 16 20	− 15,2	− 35,9	2
3087	60 Aquarii	6	334 31 0	− 3 9 30	− 6,4	+ 10,0	3
3088	7 Lacertæ	4	334 39 30	+48 42 30	− 7,4	− 23,4	2
3089	61 Aquarii	6	334 47 30	−19 2 25	− 92,7	− 18,0	2
3090	62 Aquarii η	4	334 51 30	− 1 42 10	− 7,9	+ 5,7	21
3091*	Aquarii	5	334 55 15	−25 33 40			1
3092	63 Aquarii κ	5	335 25 10	− 5 48 45	− 0,7	− 7,6	19
3093	8 Lacertæ	6	335 32 0	+38 2 35	+ 21,8	− 0,8	2
3094*	64 Aquarii	6	335 43 40	−11 37 10	+ 2,3	− 22,5	1
3095	18 Piscis Aust. ε	3½	335 50 10	−28 38 0	+ 67,0	− 39,3	2
3096	40 Pegasi	6	335 58 0	+17 56 0	+ 36,7	− 13,3	1
3097*	28 Cephei	6	335 58 —	+77 12 10		+ 15,2	1
3098	9 Lacertæ	6	336 9 10	+49 58 0	+ 96,9	− 45,3	1
3099	41 Pegasi	6½	336 11 30	+18 4 55	+ 30,7	− 7,4	1
3100	19 Piscis Aust.	5	336 14 15	−30 57 10	+ 67,7		1
3101	10 Lacertæ	6	336 20 30	+37 27 15	+ 54,4	− 19,0	1
3102	42 Pegasi ζ	3	336 29 10	+ 9 13 50	+ 73,9	− 15,8	12
3103*	Pegasi	7	336 38 55	+12 54 30			1
3104*	29 Cephei ϱ	6	336 41 0	+77 13 45	− 23,3	+ 22,6	1
3105*	65 Aquarii	6	336 41 20	−11 42 15	− 96,4	− 29,3	1
3106*	Aquarii		336 42 0	− 9 55 15			2
3107	66 Aquarii g^1	6	336 43 10	−20 26 0	− 34,3	− 5,5	5
3108	11 Lacertæ	5	336 44 30	+42 41 0	+ 47,4	− 47,7	1
3109	67 Aquarii	6	336 45 30	− 8 33 45	− 2,7	− 35,0	3
3110	43 Pegasi ο	5	336 47 30	+27 42 0	−113,5	+ 6,7	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3111	30 Cephei	6	336° 52′ 0″	+61° 59′ 10″	+258,6	— 13,9	1
3112	12 Lacertæ	6	336 54 30	+38 37 15		— 11,3	1
3113	31 Cephei	6	336 59 0	+72 2 15	— 21,5	+ 10,8	1
3114	20 Piscis Aust.	6	337 5 30	—26 50 20	+ 85,3	— 52,3	1
3115	44 Pegasi η	3	337 7 0	+28 36 45	+ 57,9	— 1,6	3
3116	13 Lacertæ	6	337 34 30	+40 12 20	+ 47,4	— 6,9	3
3117*	45 Pegasi	$6\frac{1}{2}$	337 35 45	+17 44 35	+ 37,0	+ 8,3	1
3118*	68 Aquarii g^2	6	337 43 0	—21 12 45	— 31,8	— 25,9	4
3119	46 Pegasi ξ	5	337 47 45	+10 35 25	+ 58,2	— 12,9	9
3120	69 Aquarii τ^1	5	337 49 0	—15 41 5	— 48,8	+ 27,5	4
3121	47 Pegasi λ	4	337 53 40	+21 56 30	+ 66,4	+ 16,8	5
3122	70 Aquarii	6	338 2 40	—12 10 45	— 20,2	— 1,0	7
3123*	71 Aquarii τ^2	$5\frac{3}{4}$	338 17 40	—15 12 25	— 39,1	— 34,5	10
3124*	21 Piscis Aust.	6	338 31 20	—31 9 0	+ 55,7	— 58,3	1
3125*	72 Aquarii	6	338 38 40	— 8 55 45			1
3126	48 Pegasi μ	4	338 45 35	+22 57 55	+ 59,3	+ 38,2	9
3127*	22 Piscis Aust. γ	5	338 46 0	—34 29 20	+ 93,0	— 65,3	1
3128	73 Aquarii λ	4	339 6 10	— 9 13 20	+ 10,0	+ 23,3	35
3129	14 Lacertæ	6	339 6 30	+40 19 30	+ 35,9	— 7,4	1
3130	49 Pegasi σ	6	339 9 45	+ 8 11 40	+ 85,1	+ 16,4	4
3131	74 Aquarii	6	339 17 0	—13 14 55	— 12,2	— 12,0	6
3132	75 Aquarii	7	339 26 0	—13 48 20	+ 0,7	— 69,0	1
3133*	Piscium		339 31 50	+ 0 11 35			1
3134	15 Lacertæ	5	339 32 0	+41 41 0	+ 22,4	— 25,0	1
3135	76 Aquarii δ	3	339 33 30	—17 27 35	— 79,1	+ 6,7	14
3136	77 Aquarii	6	339 35 0	—17 53 45	— 36,6	— 32,7	4
3137	78 Aquarii	6	339 36 0	— 8 50 25	+ 3,9	— 1,1	4
3138	32 Cephei ι	4	339 37 30	+64 34 45	+229,1	— 10,3	2
3139	23 Piscis Aust. δ	5	339 38 30	—34 10 0	+ 84,2	— 59,8	1
3140	1 Piscium	7	339 46 20	— 0 34 55	+ 33,1	+ 24,9	1
3141	50 Pegasi ρ	6	339 53 45	+ 7 10 20	+ 55,2	+ 5,7	3
3142*	24 Piscis Aust. α	1	340 5 0	—31 14 45	+101,3	— 34,1	7
3143*	Piscium		340 26 10	+ 2 9 55			1
3144	51 Pegasi	6	340 33 15	+19 7 0	+ 67,9	+ 6,3	3
3145	16 Lacertæ	6	340 34 30	+39 58 0	+ 35,6	— 27,5	1

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3146*	Andromedæ	6	340° 44′ 20″	+48° 5′ 40″	″	″	1
3147*	Andromedæ	6	340 52 15	+47 2 20			1
3148	2 Piscium	6	340 53 10	— 0 41 15	+ 27,9	+ 20,0	1
3149*	52 Pegasi	6	340 54 45	+10 4 45	+ 58,3	+ 9,6	1
3150*	80 Aquarii	7	341 7 30	— 6 22 35			1
3151*	3 Piscium	6	341 11 0	— 1 28 35	+ 0,6	+ 32,3	1
3152	81 Aquarii	7	341 19 0	— 8 42 0	— 7,3	— 53,6	2
3153	82 Aquarii	7	341 37 0	— 8 13 0	— 40,5	— 40,6	2
3154*	1 Andromedæ o	3½	341 55 30	+40 40 30	+ 54,4	— 20,0	3
3155	4 Piscium β	5	342 1 40	+ 2 9 25	— 4,6	+ 15,1	27
3156	2 Andromedæ	6	342 6 0	+41 6 30	+ 41,1	— 30,5	2
3157	53 Pegasi β	2	342 11 25	+26 24 20	+ 58,1	+ 27,4	11
3158*	83 Aquarii h^1	6	342 14 40	— 9 21 10	+ 4,2	— 13,9	14
3159*	84 Aquarii h^2	7	342 17 0	— 9 24 35	+ 19,5	— 27,0	2
3160	54 Pegasi α	2	342 19 25	+13 32 15	+ 56,5	+ 29,9	35
3161*	85 Aquarii h^3	7¼	342 25 40	— 9 35 40	+ 10,0	— 19,1	4
3162	86 Aquarii c^1	6	342 29 30	—25 24 40	— 8,1	+ 13,8	2
3163	3 Andromedæ	6	342 36 10	+48 23 0	+ 1,1	— 15,8	2
3164*	87 Aquarii h^4	7½	342 45 40	— 9 21 0	+ 5,8	— 27,4	2
3165*	55 Pegasi	5	342 50 15	+ 7 45 0	+ 53,3	— 20,4	3
3166	56 Pegasi	5½	342 59 50	+23 48 10	+ 74,3	+ 2,0	2
3167	5 Piscium A	6	343 11 40	+ 0 26 35	+ 37,0	+ 31,1	6
3168	88 Aquarii c^2	4	343 13 10	—22 50 55	— 17,8	+ 13,2	2
3169	89 Aquarii c^3	5½	343 19 30	—24 7 45	— 6,4	+ 4,0	2
3170	4 Andromedæ	6	343 24 0	+44 44 0	— 3,8	— 46,5	1
3171*	1 Cassiopeæ	6	343 25 0	+57 45 40	— 5,3	— 32,1	1
3172	5 Andromedæ	6	343 27 30	+47 37 35	— 12,1	— 30,5	3
3173*	57 Pegasi	6	343 27 40	+ 7 0 15	+ 49,1	+ 6,5	2
3174*	58 Pegasi	6	343 35 50	+ 8 8 45	+ 41,5	+ 16,8	2
3175*	59 Pegasi	5¾	344 1 0	+ 7 2 40	+ 33,7	+ 3,7	3
3176	6 Andromedæ	6½	344 2 30	+41 53 55	+ 51,2	— 55,6	1
3177	2 Cassiopeæ	7	344 7 30	+57 40 0	+116,7	— 27,0	1
3178	60 Pegasi	6	344 11 30	+25 10 5	+ 52,2	+ 41,1	1
3179	33 Cephei π	5	344 27 0	+73 42 45	+334,3	+ 14,4	1
3180	90 Aquarii φ	5	344 33 45	— 7 42 20	+ 3,4	— 34,6	16

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					Æ.	D.	
3181	7 Andromedæ	5½	344° 37′ 30″	+47° 43′ 50″	— 8″,0	— 30″,2	2
3182*	Aquarii	6½	344 38 35	— 7 37 0			1
3183*	Aquarii		344 54 5	— 5 10 25			1
3184	91 Aquarii ψ^1	5	344 54 30	—10 45 50	+ 14,8	— 16,5	16
3185	61 Pegasi	6	345 10 15	+26 33 40	+ 19,3	+ 15,7	1
3186	92 Aquarii χ	6	345 11 10	— 9 24 25	+ 13,6	— 8,6	9
3187	6 Piscium γ	4	345 16 10	+ 1 35 15	+ 73,6	+ 36,6	40
3188	93 Aquarii ψ^2	5	345 26 30	—10 51 45	+ 0,6	— 20,2	16
3189	94 Aquarii	6	345 42 0	—15 8 5	— 8,1	— 13,9	1
3190	95 Aquarii ψ^3	5	345 42 10	—11 17 35	+ 3,3	— 14,9	14
3191*	96 Aquarii	6½	345 49 40	— 6 49 20	+ 14,5	+ 39,8	2
3192	8 Andromedæ	6	345 53 0	+47 20 0	— 15,1	— 17,3	2
3193	9 Andromedæ	6	345 55 30	+40 5 20	+ 84,6	— 6,4	1
3194*	Piscium		346 2 25	— 5 36 0			2
3195	7 Piscium b	5½	346 8 40	+ 3 41 35	— 6,1	+ 8,4	13
3196	10 Andromedæ	6½	346 17 30	+40 23 15	+ 59,9	+ 1,0	1
3197*	11 Andromedæ	6	346 18 30	+46 56 15	— 32,9	— 16,5	2
3198	62 Pegasi τ	6	346 19 40	+22 2 40	+ 42,1	+ 22,6	4
3199*	Piscium		346 19 55	— 6 21 35			1
3200	63 Pegasi	6	346 25 0	+28 43 40	+ 61,0	+ 3,1	1
3201*	34 Cephei o	5	346 28 40	+66 25 20	+178,7	— 3,9	1
3202*	Andromedæ	6	346 29 —	+42 26 10			1
3203	12 Andromedæ	6	346 29 15	+36 30 0	+ 83,4	+ 8,7	1
3204*	97 Aquarii	6	346 36 15	—16 43 30	— 41,1	— 29,5	2
3205	98 Aquarii b^1	5	346 40 10	—21 46 50	— 48,2	— 23,4	2
3206	64 Pegasi	6	346 42 0	+30 7 0	+ 60,4	+ 15,5	1
3207	65 Pegasi	6	346 49 0	+19 8 15	+ 26,2	— 7,8	1
3208	66 Pegasi	6	346 51 20	+10 37 10	+ 48,3	+ 8,7	3
3209*	Piscium	6	346 54 35	— 1 25 15			3
3210*	67 Pegasi	6½	347 25 15	+30 41 35	+ 71,2	— 17,5	1
3211	99 Aquarii b^2	5	347 26 10	—22 20 20	— 37,6	+ 12,9	2
3212	68 Pegasi υ	6	347 28 40	+21 42 5	+ 64,9	+ 12,0	4
3213*	3 Cassiopeæ	6	347 37 0	+56 58 30			Δ
3214	8 Piscium $\varkappa$	5	347 45 40	— 0 26 25	+ 6,0	+ 14,3	38
3215*	4 Cassiopeæ	5	347 49 30	+60 35 30	— 22,3	— 20,1	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3216*	9 Piscium	6¾	347° 50′ 15″	− 0° 35′ 5″	+ 24″,4	+ 36″,9	15
3217	10 Piscium θ	5	348 3 30	+ 4 40 45	+ 18,3	+ 9,3	7
3218	13 Andromedæ	6	348 4 0	+41 13 0	+ 31,8	− 25,4	3
3219	69 Pegasi	6	348 4 20	+23 28 5	+ 48,1	+ 6,9	2
3220*	70 Pegasi	5½	348 21 35	+11 3 5	+ 71,9	+ 19,7	3
3221	11 Piscium	6	348 22 40	− 3 30 5	+ 23,2	+ 34,3	3
3222	12 Piscium	6	348 24 15	− 2 44 25	− 7,3	+ 13,8	2
3223*	100 Aquarii b^3	6½	348 50 40	−23 4 0	+ 7,5	− 27,7	1
3224*	Cassiopeæ	6	348 58 20	+56 51 5			2
3225	13 Piscium	6	349 1 0	− 2 48 5	− 9,6	+ 32,7	5
3226	14 Andromedæ	6	349 1 30	+37 32 30	+ 79,3	− 17,1	1
3227*	101 Aquarii b^4	5	349 15 30	−22 36 50	− 32,0	− 28,4	4
3228*	71 Pegasi	6	349 29 30	+20 47 15	+ 33,6	+ 20,4	5
3229	14 Piscium	6	349 33 0	− 2 58 5	+ 19,0	+ 48,5	11
3230	72 Pegasi	6	349 39 7	+29 37 10	+ 56,3	− 3,2	2
3231	73 Pegasi	6	349 50 10	+31 47 20	+ 54,3	− 7,5	2
3232	15 Andromedæ	6	349 53 0	+38 32 15	+ 39,3	− 24,3	1
3233	15 Piscium	6	349 55 0	− 0 24 5	− 29,0	+ 25,3	2
3234	16 Piscium	6	350 8 40	+ 0 22 45	− 7,8	+ 31,7	8
3235	74 Pegasi	7	350 28 0	+15 6 40	+139,4	+ 11,1	1
3236*	75 Pegasi	6	350 34 20	+16 40 50	+119,5	+ 25,7	4
3237*	16 Andromedæ λ	4	350 37 30	+44 47 35	+ 49,5	− 64,6	2
3238*	17 Andromedæ ι	4	350 45 30	+41 33 45	+ 34,5	− 25,9	2
3239*	102 Aquarii ω^1	5	350 55 45	−15 56 5	− 35,3	+ 7,0	5
3240	17 Piscium ι	6	351 0 20	+ 3 57 15	+ 25,5	− 41,1	12
3241*	Pegasi	6	351 0 50	+ 7 57 50			1
3242	18 Andromedæ	6	351 4 30	+48 45 50	− 18,0	− 18,6	3
3243	19 Andromedæ κ	4	351 18 45	+42 37 55	+ 36,9	− 40,6	1
3244	103 Aquarii A^1	5	351 22 30	−19 43 55	− 44,8	− 18,8	1
3245*	104 Aquarii A^2	5	351 25 15	−19 31 15	− 49,8	− 39,2	3
3246	18 Piscium λ	5	351 33 20	+ 0 4 15	+ 6,5	+ 16,6	41
3247	105 Aquarii ω^2	5	351 40 0	−16 14 45	− 42,7	− 38,9	5
3248	76 Pegasi	6	351 45 0	+14 37 5	+ 45,8	+ 2,9	2
3249*	35 Cephei γ	3	351 49 0	+75 54 35	−248,2	− 7,8	1
3250	77 Pegasi	6	351 53 0	+ 8 36 50	+ 86,8	+ 1,3	2

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3251*	106 Aquarii i^1	5	352° 2′ 10″	−19° 59′ 45″	− 57″,9	+ 7″,0	5
3252	78 Pegasi	5½	352 5 45	+27 38 40	+ 95,5	+ 9,4	2
3253*	107 Aquarii i^2	6	352 28 40	−20 23 50	− 26,2	− 7,1	5
3254	19 Piscium	5	352 38 40	+ 1 45 55	− 11,0	+ 15,8	9
3255	20 Andromedæ ψ	5½	352 42 0	+44 42 40	+ 24,3	− 32,1	1
3256*	Piscium		352 58 —	+ 2 30 25			1
3257*	Piscium		352 59 —	+ 2 27 25			1
3258*	Piscium		352 59 10	− 0 29 40			1
3259	20 Piscium	5½	353 0 10	− 4 28 35	+ 4,2	− 18,9	8
3260	5 Cassiopeæ τ	5	353 1 30	+56 56 30	+ 32,2	− 46,9	3
3261*	Piscium		353 6 30	+ 6 31 30			3
3262	21 Piscium	6	353 24 0	− 0 38 55	− 3,8	+ 22,2	5
3263	79 Pegasi	6	353 29 45	+27 7 0	+ 69,9	+ 10,2	2
3264	6 Cassiopeæ	6	353 31 0	+60 30 10	− 59,3	− 31,8	2
3265*	108 Aquarii	6	353 50 30	−20 37 55	− 54,9	+ 4,5	3
3266	80 Pegasi	6	353 51 45	+ 7 35 40	+104,9	+ 4,9	1
3267	22 Piscium	6	354 1 30	+ 1 12 15	+ 11,8	+ 18,4	5
3268	23 Piscium	6	354 10 0	+19 57 0	+ 41,7	− 3,1	1
3269	81 Pegasi φ	6	354 10 45	+17 23 50	+ 56,1	+ 9,2	3
3270	82 Pegasi	6	354 11 15	+ 9 13 15	+ 83,8	+ 13,8	2
3271*	83 Pegasi	6	354 14 0	+20 1 0	+ 48,5	+ 18,5	2
3272	24 Piscium	6	354 15 30	− 4 52 0	− 23,3	− 33,2	9
3273	25 Piscium	6	354 18 20	+ 0 22 25	− 1,1	− 17,8	1
3274	7 Cassiopeæ ρ	6	354 46 30	+55 47 0	+ 26,9	− 26,2	4
3275	26 Piscium	6	354 49 0	+ 5 21 0	+ 24,6	− 5,3	5
3276*	Piscium		354 57 35	+ 6 29 45			1
3277	84 Pegasi ψ	6	355 30 0	+23 25 5	+ 35,4	+ 3,0	5
3278*	1 Ceti	7	355 35 30	−17 33 55	+ 10,2	− 25,2	3
3279	27 Piscium	5	355 42 10	− 5 16 35	− 7,2		16
3280	28 Piscium ω	5	355 51 0	+ 5 8 40	+ 26,9	+ 5,3	20
3281	8 Cassiopeæ σ	6	355 52 0	+54 2 20	+ 44,6	− 31,8	4
3282*	Piscium		355 56 15	− 7 37 5			1
3283*	Pegasi		356 8 35	+25 11 45		+ 0,6	1
3284	29 Piscium	5	356 29 10	− 4 45 15	− 0,5	− 4,7	12
3285*	85 Pegasi	6	356 30 15	+25 26 25	+106,6	− 60,4	4

No.	STAR.	Mag.	Right Ascens. 1690.	Declination. 1690.	Difference from Bradley.		No. of Obs.
					AR.	D.	
3286	30 Piscium	5	356° 30′ 30″	− 7° 44′ 25″	+ 23″,2	+ 10″,4	7
3287*	Piscium	7	356 32 25	− 4 29 50	+ 0,1		1
3288	31 Piscium c^1	6	356 38 40	+ 7 13 45	− 10,8	+ 9,0	5
3289*	32 Piscium c^2	5½	356 39 40	+ 6 45 40	+ 27,8	+ 6,5	4
3290*	2 Ceti	4½	356 57 30	−19 3 55	− 5,7	+ 12,2	5
3291*	Ceti		357 5 —	−18 14 55			1
3292	9 Cassiopeæ	6	357 9 0	+60 34 0	− 30,2	− 18,6	1
3293	3 Ceti	6	357 9 30	−12 14 10	− 12,8	+ 3,8	4
3294*	33 Piscium	5	357 22 0	− 7 26 55	+ 1,2	+ 35,4	13
3295	86 Pegasi	5½	357 26 15	+11 40 10	+ 79,1	+ 3,0	1
3296	10 Cassiopeæ	6	357 41 0	+62 28 50	− 63,9	− 38,3	1
3297*	21 Andromedæ α	2	358 6 0	+27 22 15	+ 66,0	+ 14,5	10
3298*	4 Ceti	6	357 58 10	− 4 16 30	− 11,7	− 2,5	1
3299*	5 Ceti	6	358 5 10	− 4 10 25	− 2,4	− 1,3	1
3300	11 Cassiopeæ β	2¾	358 14 0	+57 26 40	+ 51,1	− 30,1	4
3301*	87 Pegasi	6	358 16 0	+16 29 0	+ 52,1	+ 14,4	1
3302	34 Piscium	6	358 32 0	+ 9 24 45	+ 17,6	+ 25,9	5
3303	22 Andromedæ	5	358 35 30	+44 21 30	+ 10,1	− 44,5	1
3304*	6 Ceti	6	358 52 0	−17 10 35	− 11,7	+ 1,0	5
3305	88 Pegasi γ	2	359 19 30	+13 27 5	+ 23,8	+ 25,6	44
3306	23 Andromedæ	6	359 23 30	+39 19 40	− 12,2	− 31,5	2
3307	89 Pegasi χ	6	359 38 50	+18 28 0	+ 46,9	+ 51,0	1
3308*	7 Ceti	5	359 43 0	−20 39 20	− 12,4	+ 6,1	4
3309	35 Piscium	6	359 45 40	+ 7 5 45	+ 14,5	+ 5,8	2
3310*	Ceti	6	359 55 —	−11 18 0			1

NOTES.

4. — *Piscium.* This star was observed by Flamsteed on Oct. 23, 1702, at $9^h\,25^m\,24^s$; but the *minutes* are marked as doubtful in the printed copy. On examining the *original* entry (MSS, vol. 7,) I find that the minutes had been originally entered 29: but the 9 has been subsequently altered to 5, and the mark of uncertainty placed against it. I cannot discover any star that will exactly correspond with this observation. M. Argelander has suggested that if we read $9^h\,24^m\,24^s$, instead of the value above mentioned, the observation will agree with Lalande in his *Hist. Cél.* page 118, at $0^h\,4^m\,51^s,5$: and that two observations of it at Abo, give its position for 1830, $\mathcal{R} = 0^h\,7^m\,13^s,95$ and $D = +\,3^\circ\,18'\,23'',6$, mag. $7\frac{1}{2}$. If this be the star, the right ascension in the present catalogue should be diminished 15′; and it would then stand at the head of the list.

6. 39 *Piscium.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entries, it is designated as the 8th; which I have therefore adopted.

17. — *Ceti.* Observed by Flamsteed on Oct. 21, 1702, at $9^h\,39^m\,56^s$: It is Piazzi O. 60 = 7 Mayer.

20. — *Piscium.* Observed by Flamsteed on Nov. 30, 1697, after $7^h\,0^m\,15^s$. The zenith distance indicates it to be Piazzi O. 66; and it was observed also by Bradley.

22. — *Andromedæ.* Observed by Flamsteed on Oct. 31, 1691, at $9^h\,11^m\,20^s$; and was (together with No. 42 of the present catalogue, observed on the same day) regularly reduced by him in MSS, vol. 23, page 283: where a note is written against them that they belong to the constellation *Cassiopea:* in which place they were afterwards neglected to be inserted. The star is Piazzi O. 74: and it is inserted in Halley's edition of 1712, from which the position in the present catalogue is taken.

23. 46 *Piscium.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the *original* entries it is designated as the 7th; which I have therefore adopted.

30. — *Piscium.* Observed by Flamsteed on Dec. 11, 1692, at $6^h\,16^m\,39^s$. It is the star observed by Lalande in *Hist. Cél.* page 118, at $0^h\,19^m\,28^s,5$, mag. 7: and is No. 6 in my catalogue of Flamsteed's inedited stars, corrected by the remarks of M. Argelander. In the *original* entry (MSS, vol. 5,) it is noted to have been observed by Mrs. Flamsteed.

34. — *Andromedæ.* Observed by Flamsteed on Nov. 19, 1693, at $7^h\,57\frac{1}{2}^m$; but the time is noted as uncertain. The zenith distance indicates it to be the star observed by Lalande in *Hist. Cél.* page 349, at $0^h\,21^m\,21^s$, and in page 389 at $0^h\,20^m\,28^s,5$, mag. 6; and which preceded π *Andromedæ* $5^m\,24^s$. This is somewhat greater than the difference given by Flamsteed: but as the time of his observation is doubtful, it is probable that this is the star.

36. 52 *Piscium.* The star 50 *Piscium,* whose position in the *British Catalogue* is $\mathcal{R} = 3^\circ\,50'\,45''$ $D = +\,18^\circ\,35'\,5''$, does not exist. The star thus designated was observed by Flamsteed on

Nov. 30, 1697, at $7^h\ 6^m\ 10^s$: but the *minute* in the time of transit is marked as doubtful: and if we read $7^h\ 7^m\ 10^s$ instead of $7^h\ 6^m\ 10^s$, the observation will agree with 52 *Piscium*. It is not inserted in Halley's edition of 1712; which shows that its position was not then considered as accurately determined: and it is struck out of the synopsis in MSS, vol. 25, page 69, as if the error had been detected. The declination of 52 *Piscium* in the *British Catalogue*, is $+\ 18°\ 36'\ 55''$: it has been deduced by Flamsteed, in MSS, vol. 23, pages 153 and 154, from the observations of Dec. 17 and 18, 1692. And he has inserted in the *British Catalogue* the declination as there deduced, without any of the subsequent corrections which he has applied to the other stars; such as $10''$ for the revised correction of the instrument, $30''$ for the error in the assumed latitude of the place, and about $1'$ for the precession. These being applied, the declination will be as stated in the present catalogue.

39. 14 *Ceti*. This star is 14 *Mayer*, and was observed by him, by Bradley, and Piazzi; and its position is well authenticated. The declination, as given by Flamsteed in the *British Catalogue*, is $-5°\ 12'\ 50''$; but Sir W. Herschel has rightly conjectured that an error of $3°$ has been made in this quantity: for on examining the original computation-books of Mr. Crosthwait, MSS, vol. 64 F (page 31) and Mr. Ryley, vol. 65 E (page 30) I find the N.P.D. stated in *each* of them to be $92°\ 12'\ 50''$; which was deduced from the observation made on Sept. 11, 1704, at $12^h\ 18^m\ 6^s$, and Oct. 19, 1704, at $10^h\ 0^m\ 38^s$, as appears by MSS, vol. 26 C, pages 33 and 34. Besides the two observations of this star, quoted by Miss Herschel, viz. in pages 466 and 485 of the *Historia Cœlestis*, there is another in page 182 (which however does not appear to have been extracted for computation by Flamsteed), and also a doubtful one in page 212; both erroneously called 15 *Ceti* by Miss Herschel. The longitude and latitude of this star, as computed by Mr. Crosthwait and Mr. Ryley, are deduced from the declination here given: so that it must afterwards have been recomputed, from the erroneous declination, for the *Historia Cœlestis*.

40. — *Piscium*. Observed by Flamsteed on Nov. 30, 1697, after $7^h\ 11^m\ 25^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 39, at $0^h\ 25^m\ 19^s$, 5, mag. 7; for it was observed *after* its transit over the meridian; and in the original MS entry it is called the companion to 53 *Piscium*.

41. 17 *Cassiopeæ* ξ. The declination of this star in the *British Catalogue* is $+\ 52°\ 12'\ 10''$; which has been deduced from the observation made on Dec. 3, 1691, at $7^h\ 0^m\ 49^s$; as appears from MSS, vol. 26 B, page 36, compared with vol. 23, page 361. But the annual precession for two years seems to have been omitted. If we take the mean of the three reductions, in the volume last quoted, we should reduce the above value $40''$; which I have accordingly done.

42. — *Andromedæ*. See the note to No. 22 of this catalogue. The present star is Piazzi O. 124. It is inserted in Halley's edition of 1712: from which the position in the present catalogue is taken.

44. — *Andromedæ*. Observed by Flamsteed on Nov. 19, 1693, after $8^h\ 1^m\ 46^s$. It is Piazzi O. 128.

46. — *Ursæ Minoris*. Besides the references given by Miss Herschel, this star will be found to have been observed by Flamsteed on Dec. 9, 1689, at $6^h\ 41^m\ 35^s$, and again on Sept. 19, 1693, at $12^h\ 1^m\ 30^s$. It is given in Halley's edition of 1712, from which work the present position is taken. It is Piazzi O. 177.

47. — *Piscium.* There are three observations of this star by Flamsteed in Oct. 1702, although it is not inserted in the *British Catalogue;* viz. on the 20th, at $9^h\ 56^m\ 23^s$, on the 21st at $9^h\ 52^m\ 51^s$, and on the 23rd at $9^h\ 45^m\ 52^s$. It is Piazzi O. 131.

49. 15 *Ceti.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entry it is designated as the 7th; which I have therefore retained.

51. — *Piscium.* Observed by Flamsteed on Nov. 15, 1702, at $8^h\ 21^m\ 12^s$. Miss Herschel has supposed it to be the same star as No. 47 in this catalogue: but it is Piazzi O. 137.

53. 54 *Piscium.* There is an annual proper motion of this star, in declination of $-\ 0'',28$, which will reduce the difference in declination to $51'',5$.

54. 55 *Piscium.* The star 56 *Piscium*, whose position in the *British Catalogue* is $R = 5°\ 55'\ 45''$, $D = +19°\ 42'\ 45''$, does not exist. The star thus designated was observed by Flamsteed on Dec. 1 and 3, 1697; the observations of the former of which days are not printed in the second volume of the *Historia Cœlestis.* But the observations in question belong to 55 *Piscium:* as do all the others which Miss Herschel has supposed to refer to that star. See MSS, vol. 23, page 437. In MSS, vol. 27 A (which contains fragments of the *British Catalogue*) 55 *Piscium* is erased, and 56 *Piscium* retained; and the mean of the right ascensions assumed as the correct one. And it is remarkable that Halley, in his edition of 1712, has also taken this mean of the two stars 55 and 56 *Piscium:* as if he was aware that only one of them existed; or probably it was so given to him by Flamsteed.

57. — *Piscium.* Observed by Flamsteed on Oct. 26, 1699, at $9^h\ 50^m\ 30^s$. It is the star observed by Lalande in *Hist. Cél.* page 127, at $0^h\ 30^m\ 45^s$, mag. 7.

58. 33 *Andromedæ.* This star was not observed by Bradley or Piazzi; but was observed by Lalande in *Hist. Cél.* page 477, at $0^h\ 31^m\ 49^s$.

60. 21 *Cassiopeæ.* The right ascension of this star must be diminished $8'\ 29'',5$ in order to correspond with Bradley's observations. It was observed by Flamsteed on Oct. 11, 1693, at $10^h\ 42^m\ 30^s$, and regularly reduced by him in MSS, vol. 23, page 363. I am unable to account for the discordancy, unless it arose from the inexpertness of the assistant, S. C., who (as I find from a MS note in the original entry in MSS, vol. 5) made the observations this night for the *first time:* and it will be found that many of the right ascensions, deduced from the observations of that evening, are very discordant. He probably made an error of 30^s: and that we ought to read $10^h\ 42^m\ 0^s$, instead of $10^h\ 42^m\ 30^s$.

62. 16 *Ceti* β. This star is marked as of the 2nd magnitude, in the original entry: and Piazzi makes it $2\frac{1}{2}$.

63. 17 *Ceti* ϕ^1. I would here remark that the four stars in the *British Catalogue*, designated by the letter ϕ, do not correspond with the quadrilateral figure formed by the stars so called in Bayer's map; and whose positions appear to be taken from Ptolemy's catalogue. I have however retained that letter, as I cannot find any other stars that correspond better.

64. 23 *Cassiopeæ.* The right ascension of this star must be diminished $12'\ 26'',5$, in order to correspond with Bradley's observations. It was observed by Flamsteed on Oct. 11, 1693, at $10^h\ 44^m\ 40^s$: but the time is marked as only approximate; yet Flamsteed has deduced the right ascension therefrom, as if it were the correct value. See MSS, vol. 23, page 363, also the note to No. 60 above.

65. 2 *Ursæ Minoris.* This is the star which corresponds with No. 15 and 19 in my list of Flamsteed's inserted stars; and which I imagined were two different and distinct stars. M. Argelander has very justly observed that the transits of such stars, so near the pole, do not probably take place exactly in the order in which they are put down by Flamsteed; because the zenith distances are oftentimes taken *after* they have passed the meridian, and thus on account of their slow motion a doubt frequently occurs. An error of this kind has arisen in the present case, as I shall now proceed to show. The star denominated 2 *Ursæ Minoris* by Flamsteed, and whose position as given in the *British Catalogue*, is $\mathcal{R} = 9^\circ\ 39'\ 30''$ and $D = +\ 86^\circ\ 33'\ 30''$, has been deduced from the observation made on Septem. 19, 1693, at $12^h\ 19\frac{1}{2}^m$; as may be seen by an inspection of MSS, vol. 23, page 430. In this computation Flamsteed has supposed that the star in question passed the meridian $5^m\ 38^s$ *after* Polaris (the time $12^h\ 19\frac{1}{2}^m$, as in the original MS, being erroneously printed $12^h\ 19\frac{1}{2}^m$ in the *Historia Cœlestis*): but this was not the case: for on referring to the *original* entry of the observations of this same star, on Dec. 19 following, I find that it passed the meridian at $6^h\ 1^m\ 40^s$, or $3^m\ 22^s$ *before* Polaris: a fact which has been omitted in the printed copy. With respect to the declination, I find, on reference to Flamsteed's computation in the MS volume above mentioned, that he has committed two errors: in the first place he has copied out the zenith distance as $35^\circ\ 6'$ instead of $33^\circ\ 6'$; and in the next place he has made another mistake of $1'$ in reducing the distance from the zenith to that from the pole. Deducting therefore 2° and adding $1'$, we have the declination as here given; and the position then nearly agrees with that given by Halley in his edition of 1712. It is Piazzi O. 220 = (43 Hev.) *Cephei*. The right ascension, however, must be increased nearly 1° in order to agree with modern observations.

69. 58 *Piscium.* This star is marked as of the 7th magnitude, in the *British Catalogue:* but in the *original* entries it is only once so called, whilst it is twice designated as the 6th. I have therefore altered it.

70. 59 *Piscium.* Observed by Flamsteed on Dec. 1, 1697, at $7^h\ 17^m\ 57^s$: but the observations of that day are not printed in the *Historia Cœlestis.*

72. 60 *Piscium.* The declination of this star in the *British Catalogue* is $+5^\circ\ 3'\ 40''$: which has been deduced by Flamsteed from the observations made on Sept. 4, 1693, at $13^h\ 4^m\ 40^s$. But in reducing it to the epoch of 1690, he has made an error of $1'$, which I have here corrected.

75. — *Piscium.* Observed by Flamsteed on Nov. 15, 1705, at $8^h\ 27^m\ 45^s,5$. It is Piazzi O. 189 = 20 Mayer.

80. 1 *Ursæ Minoris α.* The right ascension of this star must be increased $11'\ 4'',8$ in order to correspond with Bradley's observations, reduced agreeably to Bessel's formula. There are several computations of its position, scattered through various volumes of the MSS; but the final one appears to be that which is to be found in MSS vol. 24, page 127.

81. — *Piscium.* Observed by Flamsteed on Nov. 30, 1697, at $7^h\ 24^m\ 6^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 573, at $0^h\ 38^m\ 35^s,3$, mag. 7.

82. 65 *Piscium i.* This star is denoted by the Greek letter ι in Flamsteed; which is also given to 17 *Piscium.* This is an error of the press; as Bayer (whom Flamsteed professed to follow) calls it *i*: and Flamsteed himself denotes it by *i* in the MS copies. I have therefore corrected it.

83. 19 *Ceti* ϕ^2. This star is marked as of the 5th magnitude in the *British Catalogue:* but in the

original entries it is twice designated as the 6th, and once as $5\frac{1}{2}$; I have therefore altered it. See the Note to No. 63.

84. — *Piscium.* Observed by Flamsteed on Dec. 3, 1714, at $7^h\ 14^m\ 1^s$; and again on Dec. 7, at $6^h\ 54^m\ 30^s$. It is Piazzi O. 207 = 23 Mayer; and was observed also by Bradley.

85. 26 *Cassiopeæ* υ^1. The right ascension of this star in the *British Catalogue* is $9^\circ\ 29'\ 30''$: but I shall show that there is an error of 1^m in time; and that we should deduct $15'$ from the above value. It was observed on Dec. 2, 1691, at $7^h\ 22^m\ 52^s$, as appears by the printed copy of the *Historia Cœlestis*. But, on referring to the original MS entry, there is written in the margin "*certè* $21'\ 52''$;" and Flamsteed had made the alteration in part of his computation of this star in MSS, vol. 23, pages 360 and 361; but had not continued it through the whole process. I have here corrected it. The true position is given in Halley's edition of 1712.

88. 27 *Cassiopeæ* γ. The declination of this star in the *British Catalogue* is $+59^\circ\ 2'\ 40''$; which has been deduced from the observation made on Dec. 2, 1691, after $7^h\ 23^m\ 29^s$; as may be seen in MSS, vol. 23, page 361. But it appears that 2 years' precession has been omitted: I have therefore deducted $40''$ from the above value.

89. 28 *Cassiopeæ* υ^2. There is only one star designated by the letter υ, in Bayer's map: but, as it may be doubted whether such letter belongs to 26 *Cassiopeæ*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

90. 36 *Andromedæ*. The declination of this star in the *British Catalogue* is $+21^\circ\ 57'\ 40''$. It was observed by Flamsteed on Nov. 17, 1693, at $8^h\ 27^m\ 8^s$; and regularly reduced by him in MSS, vol. 23, page 287. But in applying the annual precession he appears to have made an error of $1'$, which I have here corrected.

91. 21 *Ceti*. This star is omitted in Flamsteed's map of *Cetus*.

92. 67 *Piscium k*. In the observation of this star by Flamsteed, on Dec. 21, 1689, at $5^h\ 50^m\ 49^s$, it is stated that it has a companion following it, to the south: which is probably the same as that observed by Lalande in *Hist. Cél.*, page 27, at $0^h\ 46^m\ 0^s{,}3$, mag. $7\frac{1}{2}$.

93. 37 *Andromedæ* μ. The declination of this star in the *British Catalogue* is $+36^\circ\ 47'\ 25''$: but there is an error in copying out the minutes, as it should be 48 instead of 47. See MSS, vol. 23, page 388; and vol. 25, page 119, where the source of the error is manifest. I have here made the correction; which agrees with the value given in Halley's edition of 1712.

95. 38 *Andromedæ* η. The declination of this star in the *British Catalogue* is $+21^\circ\ 45'\ 25''$. It has been deduced by Flamsteed from the observation made on Nov. 17, 1693, after $8^h\ 27^m\ 8^s$; as appears by MSS, vol. 23, page 287, compared with vol. 26 B, page 21. But in applying the annual precession for 4 years he has made an error of $1'$, which I have here corrected.

97. 23 *Ceti* ϕ^4. This star is marked as of the 5th magnitude, in the *British Catalogue:* but it is always designated as the 6th in the observation book; and which I have therefore retained. See the Note to No. 63.

98. 39 *Andromedæ*. The declination of this star in the *British Catalogue* is $+39^\circ\ 41'\ 15''$. It was observed by Flamsteed on Oct. 31, 1691, at $9^h\ 45^m\ 11^s$; and reduced by him in MSS, vol. 23, page 283. But in the steps of the process he has made an error of $1'$, which I have here corrected.

99. 69 *Piscium* σ^1. This is the same star as 40 *Andromedæ*. The position of 69 *Piscium* is stated by Flamsteed to be $\mathcal{R} = 11^\circ\ 29'\ 0''$, $D = +30^\circ\ 7'\ 40''$: and the position of 40 *Andromedæ* to be $\mathcal{R} = 11^\circ\ 30'\ 0''$, $D = +30^\circ\ 8'\ 15''$. I have taken the mean of the two, for the correct value.

100. 70 *Piscium*. The declination of this star in the *British Catalogue* is $+6^\circ\ 13'\ 10''$: but on comparing the zenith distance with that of δ and ε *Piscium*, observed on the same day, it will be seen that an error of $2'$ has been made, which I have here corrected.

103. — *Piscium*. Observed by Flamsteed on Dec. 3, 1697, at $7^h\ 27^m\ 22^s$; but I do not find that he has any where reduced this observation. It is the star observed by Lalande in *Hist. Cél.* page 573, at $0^h\ 53^m\ 40^s{,}5$, mag. 6.

104. 30 *Cassiopeæ* μ. This star exists, but I cannot find a perfect observation of it by Flamsteed. The position, as given in the present catalogue, is taken from Halley's edition of 1712: but the same value is also to be found in MSS, vol. 23, page 382. Flamsteed's position is imperfect, inasmuch as the right ascension is not given: it was deduced from the observation made on Dec. 2, 1691. The star is there called ϑ: but, in MSS, vol. 23, page 361, Flamsteed has erased ϑ with the pen, and inserted μ instead thereof; and in page 382 of the same book, where he has collected the stars of this constellation in the order of right ascension, this same star is inserted as μ, whereas the observation properly belongs to ϑ. I apprehend that neither the right ascension nor declination can be depended upon. It was observed also, by Flamsteed, with the sextant.

105. 26 *Ceti*. Miss Herschel says there is no observation of this star by Flamsteed: but it was observed by him on Aug. 24, 1704, at $13^h\ 49^m\ 39^s$; and reduced by him in MSS, vol. 26 C, page 33.

107. 73 *Piscium*. One of the observations of this star, viz. that made on Nov. 3, 1703, at $9^h\ 26^m\ 53^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 33; and in deducing the right ascension he has, from some cause or another, committed an error of about $3^m\ 40^s = 55'$ in space. Whence has arisen the introduction of 24 *Ceti* into the *British Catalogue*; whose position as there given is $\mathcal{R} = 11^\circ\ 17'\ 30''$, and $D = +3^\circ\ 59'\ 5''$; but which consequently does not exist.

109. 76 *Piscium* σ^2. There is only one star designated by the letter σ, in Bayer's map: but, as it may be doubted whether such letter belongs to 69 *Piscium*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

113. 75 *Piscium*. The right ascension of this star in the *British Catalogue* is $12^\circ\ 18'\ 30''$. It was observed by Flamsteed on Nov. 29, 1692, at $7^h\ 42^m\ 55^s$, and Oct. 11, 1697, at $11^h\ 8^m\ 22^s$: and both of them have been reduced by him. On examining the steps of the process in MSS, vol. 23, page 153, I find he has made a mistake of 1^m in the time of this star, in the first observation; and that $15'$ in space should consequently be added to the value above stated. In the second observation he has deduced the value correctly (page 436): but conceiving that the first value was accurate, he has expressed a doubt in the margin, and deducted 1^m from the result. I have here corrected these errors.

114. 41 *Andromedæ*. The letter *d* is annexed to this star in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

118. 42 *Andromedæ* ϕ. The declination of this star in the *British Catalogue* is + 45° 35′ 55″; which is deduced from the observations made on Oct. 31, 1691, at $9^h\ 51^m\ 16^s$; and which has been regularly reduced by Flamsteed in MSS, vol. 23, page 283. But in applying the annual precession he has made an error of 1′, which I have here corrected.

119. — *Cassiopeæ*. Observed by Flamsteed on December 2, 1691, after $7^h\ 31^m\ 1^s$, and again on Dec. 5, 1693, after $7^h\ 24^m$: the time in both cases being uncertain. It is inserted in Halley's edition of 1712; and has been observed by Bradley, where the right ascension in his catalogue should be 13° 57′ 59″,7 instead of 13° 59′ 48″,8, as erroneously printed.

120. 32 *Cassiopeæ*. The star, designated by Flamsteed as 29 *Cassiopeæ*, does not exist in the place pointed out by him, viz. $\mathcal{R}$ = 11° 29′ 0″, D = + 63° 22′ 0″. It is the same star as 32 *Cassiopeæ*. There are two observations of it: one on Dec. 2, 1691, at $7^h\ 31^m\ 1^s$; the other on Dec. 5, 1693, at $7^h\ 24^m$. In both cases the star No. 119 of the present catalogue was observed at the same time; which identifies the star in question. The observation of 1691 is that from which Flamsteed has deduced the position of 29 *Cassiopeæ*; but it has been remarked by Miss Herschel that there is probably an error of 6^m in noting down the time; and that $7^h\ 31^m\ 1^s$, should be $7^h\ 37^m\ 1^s$. This would reconcile the two observations: and although there is no appearance of such error in the original entry in the *books*, yet as it is a mistake very likely to occur in transcribing it from the slate, or the *first* notes, I have adopted her solution of the difficulty.

122. 30 *Ceti*. The declination of this star in the *British Catalogue* is — 11° 26′ 5″: but there is evidently an error of about 1½′, by comparing its zenith distance with that of other stars observed on the same day. In Halley's edition of 1712 it is — 11° 27′ 25″, which is much more near to the true value; and which I have therefore adopted.

129. — *Piscium*. Observed by Flamsteed on Dec. 17, 1691, at $6^h\ 33^m\ 30^s$, and Nov. 29, 1692, at $7^h\ 46^m\ 26^s$; and both observations have been reduced by him. But in the first of them he has made a mistake of 10° in deducing the north polar distance from the zenith distance, as may be seen in MSS, vol. 23, page 153: and he probably rejected them on account of their discordancy. It is Piazzi O. 311.

132. 83 *Piscium* τ. The declination of this star in the *British Catalogue* is + 28° 24′ 25″. Although there are 5 observations of it, yet its position is deduced from that made on Dec. 21, 1689, at $6^h\ 6^m\ 3^s$: and in copying out the zenith distance into the computation book, MSS, vol. 23, page 149, he has copied out the *un*corrected zenith distance; whereby an error of 1′ 10″ has been made in the declination, which I have here corrected.

136. — *Piscium*. Observed by Flamsteed on Nov. 30, 1697, at $7^h\ 46^m\ 34^s$; and has been regularly reduced by him. Why it was not inserted in his catalogue I am unable to discover. It was observed by Lalande in *Hist. Cél.* page 350, at $1^h\ 2^m\ 21^s,0$, mag. 9.

141. — *Cassiopeæ*. Observed by Flamsteed on Dec. 5, 1693, at $7^h\ 29^m\ 36^s$: and has since been observed by Lalande in *Hist. Cél.* page 375, at $1^h\ 4^m\ 3^s,5$, mag. 8.

142. 86 *Piscium* ζ. This star is stated, in the *British Catalogue*, to be of the 4th magnitude: but in the original observations it is no where stated to be more than the 5th; and in one place it is marked as the 6th, but afterwards altered to the 5th, which I have retained. Piazzi makes

it of the 6th. In Bayer's map it is placed as if situate in *north* latitude; which I apprehend has arisen from an error in Tycho's catalogue.

143. — *Piscium.* Observed by Flamsteed on Oct. 9, 1695, at $11^h\,22^m\,29^s$, and Nov. 13, 1700, at $8^h\,59^m\,30^s$. It is Piazzi I. 17; and has been also observed by Bradley and Mayer.

145. — *Ceti.* Observed by Flamsteed on Dec. 10, 1699, at $7^h\,7^m\,8^s$. It is the star observed by Lalande in *Hist. Cél.* page 247, at $1^h\,3^m\,44^s$, mag. $8\frac{1}{2}$.

147. 37 *Ceti.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but, in the *original* entries, it is twice designated as $5\frac{1}{2}$, and once as the 7th; I have therefore taken the mean.

148. 38 *Ceti.* There is an annual proper motion of this star, in declination, of $+\,0'',27$: but this will increase the difference of declination to $-\,86'',0$. I cannot account for so great a discordance.

149. — *Ceti.* Observed by Flamsteed on Dec. 10, 1699, after $7^h\,7^m\,34^s$, and is probably the same as that observed by Lalande, in *Hist. Cél.* page 250, at $1^h\,4^m\,58^s,6$, mag. 8.

150. 35 *Cassiopeæ.* The position of this star in the *British Catalogue* is $\mathcal{R} = 15°\,26'\,0''$, and $D = +\,63°\,21'\,40''$. It was observed by Flamsteed on Oct. 11, 1693, at $11^h\,17^m\,0^s$ *circiter;* but supposing this to be the time, or nearly the time of transit, it is evident, on comparing it with the transit of other stars observed on the same day, that an error of 1^m has been made in the computation; which I have here corrected by deducting 15′ from the above value. With respect to the declination, I find, on referring to MSS, vol. 26 B, page 13, that an error of 20′ has been made either by the printer or the transcriber, which I have also rectified.

152. 34 *Cassiopeæ* ϕ. This star has not any letter attached to it, in the *British Catalogue:* but it is ϕ in Bayer's map, which I have here annexed. In the original entry of the observation of this star on Dec. 2, 1691, the time of transit is marked as *vitiosa.*

155. 41 *Ceti.* This star is marked of the 6th magnitude, in the *British Catalogue:* but, in the *original* entry, it is designated as the 7th; which I have therefore adopted.

159. 46 *Andromedæ* ξ. This star has no letter attached to it in the *British Catalogue:* but it is ξ in Bayer's map; which letter Flamsteed has erroneously affixed to 49 *Andromedæ.* I have restored the correct reading.

167. 38 *Cassiopeæ A.* I cannot account for the great difference in the declination of this star, as I do not detect any considerable error. In Bayer's map it is designated by the letter *A*, which Flamsteed has omitted in the *British Catalogue:* I have therefore restored it here.

168. 48 *Andromedæ* ω. This star has no letter attached to it in the *British Catalogue:* but it is ω in Bayer's map, which I have here annexed.

170. 94 *Piscium.* This star is marked by Piazzi as of the $6\frac{1}{2}$ magnitude only.

174. 49 *Andromedæ A.* This star is called ξ in the *British Catalogue:* but that letter properly belongs to 46 *Andromedæ.* See No. 159 above. It is in fact denoted by *A* in Bayer's map, which I have here restored.

179. 48 *Ceti.* There are three observations of this star by Flamsteed: but that on Dec. 14, 1691, at $7^h\,6^m\,2^s$, has been erroneously reduced by him in right ascension in MSS, vol. 23, page 175,

inasmuch as he made an error not only of one whole hour (equal to 15° 2′ 30″ according to the rate of his clock at that time) but also of 1ᵐ 46ˢ in another part of the process: the difference being equal to 14° 36′ 0″. Whereby he has considered it to be a totally different star, viz. 74 *Ceti*: whose position in the *British Catalogue* is Æ = 33° 13′ 45″ and D = — 23° 14′ 35″, but which consequently does not exist.

180. 40 *Cassiopeæ*. This star has the letter *h* annexed to it, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

182. — *Piscium*. Observed by Flamsteed on October 31, 1705, at 10ʰ 12ᵐ 53ˢ; and has since been observed by Lalande in *Hist. Cél.* page 204, at 1ʰ 21ᵐ 44ˢ, 5, mag. 7.

184. 50 *Andromedæ*. This star is marked as *υ* in the *British Catalogue*: but the star, so designated in Bayer's map, is No. 199 of the present catalogue: I have therefore rejected it here.

185. — *Ceti*. Observed by Flamsteed on Aug. 28, 1704, at 14ʰ 5ᵐ 50ˢ; and has since been observed by Lalande in *Hist. Cél.* page 119, at 1ʰ 23ᵐ 41ˢ, 5, mag. 7½: where M. Argelander thinks that an error is made in recording the transits of the star, by the wires; and that the readings give the 1st and 2nd wires instead of the 2nd and 3rd. Five observations made by him at Abo give the position of this star for 1830 Æ = 1ʰ 26ᵐ 3ˢ, 38 and D = + 0° 5′ 1″, 8.

187. 51 *Andromedæ*. This star is called *ν* in the *British Catalogue;* which letter has also been affixed to 50 *Andromedæ*: and it is called R² by Piazzi. It is in fact *υ Persei*: and I have therefore rejected the letter here. In the *British Catalogue* it is also said to be of the 5th magnitude: but it is nowhere so stated in the *original* observations. On the contrary it is marked as of the 3½ magnitude in the observation of October 31, 1691, at 10ʰ 18ᵐ 42ˢ, 5; which corresponds with Piazzi; and which I have therefore retained. In the printed copy of the *Historia Cœlestis* on November 20, 1693, at 8ʰ 52ᵐ 49ˢ, it is marked as of the 5th magnitude: but here he has evidently mistaken the star; and on referring to the *original*, I find it marked as of the 6th.

189. 42 *Cassiopeæ*. The star designated as 41 *Cassiopeæ*, whose position in the *British Catalogue* is Æ = 19° 56′ 0″, and D = + 71° 2′ 10″, does not exist. The star here alluded to was observed by Flamsteed on Dec. 16, 1691, at 7ʰ 3ᵐ 29ˢ; and again on Dec. 5, 1693, at 7ʰ 51ᵐ 45ˢ. These are both 42 *Cassiopeæ*: but Flamsteed, in copying out the zenith distance of the first observation, has made a mistake of 2°; as may be seen by referring to MSS, vol. 23, pages 362 and 363: and which has led him into the error of supposing this to be a different star from that observed in 1693. The present star has the letter *g* annexed to it, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

190. 43 *Cassiopeæ ω*. This star is called *c* by Flamsteed in the *British Catalogue*: but it is designated as *ω* by Bayer: and in one of the MS catalogues belonging to Flamsteed, I find a query, in pencil, whether it ought not to be so denominated. I have restored the correct reading.

191. 102 *Piscium π*. In the observation of this star by Flamsteed, on Nov. 28, 1689, at 8ʰ 17ᵐ 57ˢ, he says that it has a companion following it, to the south, and distant 5′: which is Piazzi I. 128.

192. 50 *Ceti*. The declination of this star in the *British Catalogue* is — 16° 59′ 0″. The observation from which it has been deduced was made on Dec. 10, 1691, at 7ʰ 27ᵐ 41ˢ; as may be

seen in MSS, vol. 23, page 175. But the result is evidently erroneous, as its zenith distance differed only 17′ from that of 49 *Ceti* observed on the same day. In fact, it appears that certain corrections have been applied to the place of this latter star, which have been omitted in the present star. I have therefore increased the declination 45″.

193. 52 *Andromedæ* χ. This star is denoted by the Greek letter λ in the *British Catalogue*: but 16 *Andromedæ* has been already expressed by that letter. Bayer calls it χ: and as Flamsteed professed to follow Bayer, I have examined all the MSS catalogues belonging to Flamsteed, and find that it is universally called χ. I presume therefore that it is a typographical error, and have altered it accordingly.

199. — *Andromedæ* υ. Observed by Flamsteed on Nov. 2. 1691, at $10^h\ 14^m\ 51^s$; and was regularly reduced by him, with the other stars observed on that day. It is inserted in Halley's edition of 1712, from which the position in this catalogue is taken. It is Piazzi I. 142 = (41 Hev.) *Andromedæ*: and is the star designated by the letter υ in Bayer's map; which I have here retained.

200. 1 *Trianguli*. Observed by Flamsteed on Nov. 9, 1695, at $9^h\ 46^m$: but the time is noted to the nearest minute only. Nevertheless Flamsteed has considered this as the correct time of transit; and reduced it accordingly in MSS, vol. 23, page 269. Its right ascension ought to be increased about 32^s (=8′ in arc), in order to make it correspond with Piazzi I. 148; which star it undoubtedly is. In the *British Catalogue*, this star, as well as 11 *Trianguli*, is designated by the letter *d*; but, as no such letter exists in Bayer's map, I have rejected it in each place.

201. 54 *Andromedæ*. This is called ϕ in the *British Catalogue*: but that letter has already been affixed by Flamsteed to 42 *Andromedæ*. It is in fact ϕ *Persei*: and I have therefore rejected the letter here.

202. 106 *Piscium* ν. This is the same as 51 *Ceti*: whose right ascension and declination, however, as given in the *British Catalogue*, exceed 106 *Piscium*, by 5″.

203. — *Cassiopeæ*. Observed by Flamsteed on Dec. 2, 1691, at $8^h\ 10^m\ 48^s$. It is Piazzi I. 159.

204. 2 *Arietis*. This is the same as 107 *Piscium*. In the *British Catalogue*, however, it exceeds that star 35″ in right ascension, and 5″ in declination. It is of the 6th magnitude in the *original* entries, which I have therefore retained.

205. 109 *Piscium*. The star designated by Flamsteed as 108 *Piscium* does not exist; its position in the *British Catalogue* is $\mathcal{R} = 22^\circ\ 0'\ 30''$, and $D = +\ 21^\circ\ 31'\ 50''$; that is, about 3° more north than 109 *Piscium*. It was observed on Oct. 11, 1697, at $11^h\ 46^m\ 5^s$, and its observed zenith distance *per lineas diagonales* is there stated to be $30^\circ\ 1'\ 5''$; which Flamsteed has presumed to be the true value, and reduced it accordingly. But *per strias cochleæ* this zenith distance ought to be increased 3°; and the star then proves to be 109 *Piscium*. In the *British Catalogue*, this star is said to be of the 8th magnitude; it being so marked in the observation of Nov. 29, 1692, at $8^h\ 20^m\ 37^s$. But in the *original* entry of Oct. 11, 1697, at $11^h\ 46^m\ 5^s$, it is marked as of the $6\frac{1}{2}$ magnitude, which corresponds with Piazzi; and which I have therefore retained.

208. 3 *Arietis*. The declination of this star in the *British Catalogue* is $+15^\circ\ 49'\ 5''$; which is

deduced from the observation made on Oct. 31, 1705, at $10^h\ 26^m\ 42^s$; as may be seen in MSS, vol. 26 C, page 34. But in the steps of the process he has made two errors, amounting to $1'\ 20''$; and which I have added to the value above stated. Piazzi states (Note to No. 174) that he could not discover this star: but it was seen by Bradley, Lalande, and Bessel.

209. 4 *Arietis.* In the observation of this star by Flamsteed on Nov. 27, 1690, at $8^h\ 36^m\ 12^s$, he states that it has a small companion to the north. Probably Piazzi I. 174.

212. 46 *Cassiopeæ.* The letter *d* is annexed to this star in the *British Catalogue*, and also to 4 *Cassiopeæ:* but, as there is no such star in Bayer's map, I have rejected it in both places.

213. 2 *Persei g.* I am not satisfied with the designation of this star, as it does not well accord with the star so called in Bayer's map.

214. 1 *Arietis.* The right ascension of this star in the *British Catalogue* is $20^\circ\ 46'\ 0''$: it was observed on Nov. 18, 1695, at $9^h\ 18^m\ 16^s$; but it is evident, on comparing the observation of this star with those of γ, β and λ *Arietis* made on the same day, that an error of 10^m in time has been made in the reduction. This is confirmed by an inspection of the MSS, vol. 23, page 8, where the error is manifest. I have therefore added $2^\circ\ 30'$ to Flamsteed's value; and it will then agree with Piazzi I. 179.

216. 54 *Ceti.* The right ascension of this star in the *British Catalogue* is $23^\circ\ 52'\ 0''$. It was observed by Flamsteed on Sept. 19, 1704, at $13^h\ 2^m\ 7^s$; and reduced by him in MSS, vol. 26 C, page 33: but, by comparing its time of transit with that of other stars observed on the same day, it is evident that he has made an error of 1^m. I have therefore deducted $15'$ from the value above given; and it will now correspond with modern observations.

219. 47 *Cassiopeæ.* This star is marked, in the *British Catalogue*, as of the 5th magnitude: which is erroneous. There is but one observation of it, which was made on Dec. 16, 1691, at $7^h\ 19^m\ 51^s$; and in the *original* entry it was first marked as of the 6th magnitude, and afterwards altered to the 7th; which figure has been evidently mistaken for a 5. I have therefore restored the correct value; which will agree with modern observations.

222. — *Cassiopeæ.* Observed by Flamsteed on Nov. 24, 1708, at $9^h\ 5^m\ 0^s$; but it does not appear to have been reduced by him. It is the same as that observed by Lalande in *Hist. Cel.* page 378, at $1^h\ 44^m\ 25^s{,}8$, mag. $6\frac{1}{2}$.

223. 48 *Cassiopeæ.* The declination of this star in the *British Catalogue* is $+69^\circ\ 23'\ 55''$; which is deduced from the observation made on Dec. 16, 1691, after $7^h\ 21^m\ 24^s$: as may be seen in MSS, vol. 26 B, page 36, compared with vol. 23, page 363. But in copying out the polar distance from this latter volume he has made an error of $45''$, which I have here corrected. The letter *e* is annexed to this star in the *British Catalogue*, and also to 1 *Cassiopeæ:* but as there is no such letter in Bayer's map I have rejected it in both places.

228. 50 *Cassiopeæ.* This star is designated by the letter *f*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

229. 7 *Arietis.* There is no magnitude annexed to this star, in the *British Catalogue:* but in the *original* entries, it is once designated as the 8th, once as the 7th, and once as the 6th; I have therefore taken the mean.

231. 51 *Cassiopeæ.* Observed by Flamsteed on Dec. 16, 1691, at $7^h\ 23^m\ 30^s$; and not at $8^h\ 23^m\ 30^s$,

as Miss Herschel imagines. The zenith distance also is, by Flamsteed, reckoned *per lineas diagonales*, and not *per strias cochleæ*, as may be seen in MSS, vol. 26 B, page 34. The right ascension, in order to agree with Bradley's determination, should be diminished 10′ 15″,6: but I cannot account for the discordance, unless it be an error of 40ˢ in the time of transit. The other observation on Dec. 5, 1693, which in Miss Herschel's opinion belongs to this star, is that of No. 370 in the present catalogue. There is no magnitude annexed to this star, in the original entry: but Piazzi considers it to be of the 8th.

232. 52 *Cassiopeæ*. } These two stars were observed by Flamsteed on Oct. 11, 1693; and have
234. 53 *Cassiopeæ*. } been regularly reduced by him, as may be seen in MSS, vol. 26 B, pages 13 and 34. I apprehend that in transcribing the zenith distances, or from some other cause, their north polar distances have been interchanged, in the *British Catalogue*. In fact, the original entry is confused, and stands thus:—

h	m	s		°	′	″		
11	48	35	. . .	15	36	40	353·85	6
	56	22	. .	11	25	35	256·85	6
	56	8	. . .	11	51	20	268·45	7
	59	28	. .	18	31	50	420·15	7

By which it would appear that the times of transit only ought to have been interchanged, and not the zenith distances. I have therefore rectified the errors. The relative positions of these two stars are correctly given in Flamsteed's maps.

237. 56 *Ceti*. This star is marked, in the *British Catalogue*, as of the 4th magnitude; which is erroneous. There is but one observation of it, by Flamsteed, which was on Nov. 30, 1689, at $8^h\ 33^m\ 14^s$; where it is said to be the *least* of the two stars, which he had considered to be designated by the letter ν: and consequently it should be of *less* magnitude than 59 *Ceti*; which, in the *original* entry, is marked as of the 5th magnitude. I have therefore altered Flamsteed's value to the 6th, which corresponds with Piazzi. In the *British Catalogue* this star is marked as υ^1: but the star designated by that letter in Bayer's map, is 59 *Ceti*. I have therefore rejected it here.

238. 58 *Ceti*. The right ascension of this star in the *British Catalogue* is 26° 19′ 30″. It was observed on Nov. 21, 1698, at $9^h\ 17^m\ 9^s$: and, on examining the steps of the process, by which this value has been obtained in MSS, vol. 23, page 177, it appears that Flamsteed has made a mistake of 3 minutes in time. We must therefore deduct 45′ from the value given by him: which I have accordingly done. This star is marked as of the 6th magnitude in the *British Catalogue*: but in the *original* entry it is designated as the 7th; which I have therefore retained.

239. — *Arietis*. Observed by Flamsteed on Nov. 28, 1693, at $8^h\ 39^m\ 51^s,5$: it is Piazzi I. 222.

240. — *Arietis*. Observed by Flamsteed on Dec. 18, 1691, at $7^h\ 18^m\ 38^s$; and again on Dec. 24, at $6^h\ 55^m\ 21^s$: and although the two observations differ 7ˢ in right ascension, there can be no doubt but that this star is Piazzi I. 223 = 68 Mayer. In the original MS entry there is the mark of uncertainty placed against the time of transit of the second observation.

241. 54 *Cassiopeæ*. This star is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entry it is designated as the 7th; which I have here retained.

245. 57 *Ceti.* This star is marked, in the *British Catalogue*, as of the 5th magnitude: but in the *original* entry of the only observation that was made of it, on Nov. 30, 1689, at $8^h\ 36^m\ 16^s$, it is distinctly marked "6^m lucis vel minor." I have therefore altered it.

246. 59 *Ceti υ.* Flamsteed has marked this star as υ^2: but there is only one star designated by that letter in Bayer's map, which is evidently this star. In the *original* entry of the observation of this star on Dec. 10, 1699, at $7^h\ 53^m\ 43^s$, it is marked as of the 5th magnitude: but I have not made any alteration here.

247. 113 *Piscium α.* This star is marked by Piazzi as of the 5th magnitude only.

249. — *Piscium.* Observed by Flamsteed on Nov. 15, 1705, at $9^h\ 42^m\ 20^s$, 5: it was not observed by Lalande; but it may be found in Bessel's *Zone* 130 at $1^h\ 54^m\ 0^s$, 80, mag. 7.

252. 11 *Arietis.* In the observations of this star by Flamsteed on Nov. 27, 1690, at $8^h\ 54^m\ 1^s$, it is stated that it has a small companion, following it. This accompanying star is also mentioned by Piazzi in his note to I. 255; and it was observed by Lalande in *Hist. Cél.* page 30, at $1^h\ 54^m\ 53^s$. Bradley, Piazzi, and Lalande, make it *precede* 11 *Arietis*: but Flamsteed says it *follows* that star.

254. 13 *Arietis α.* This star is twice called of the 2nd magnitude, in the *printed* copy of the *Historia Cœlestis*: but there is no denomination annexed in the *original* entries. Piazzi says it is of the 3rd magnitude.

256. 5 *Persei h.* Observed by Flamsteed on Jan. 14, 1694, at $5^h\ 30^m\ 56^s$; and regularly reduced in MSS, vol. 23, page 293: but the observations of that day are not printed in the *Historia Cœlestis.* There is no letter annexed to this star in the *British Catalogue;* but it is *h* in Bayer's map, which Flamsteed has erroneously annexed to 6 *Persei.* There are also two other imperfect observations of it, in the *original* MSS: one on Dec. 19, 1693, after $7^h\ 18^m\ 52^s$, where the zenith distance is given *per strias cochleæ* = 105·39: and another on Jan. 4, 1694, at $6^h\ 10^m\ 32^s$, where the *original* MS entry stands thus: viz.,

h	m	s		°	′	″
6	5	7	*Arietis α* . . .			
	10	32	*Persei* exit. . . .	4	40	0
	11	58	. . . .			
	12	5	. . . .			
10	10	55½	*Geminorum η* . .	28	59	0

257. 55 *Cassiopeæ.* This star is not the *ι* in Bayer's map, which belongs to Piazzi II. 72; a star of the 4th magnitude, which was not observed by Flamsteed.

261. 6 *Persei.* The letter *h* is annexed to this star in the *British Catalogue:* but that letter belongs to 5 *Persei.* I have therefore rejected it here.

262. 5 *Trianguli.* The right ascension of this star in the *British Catalogue* is 28° 35′ 0″: it was observed by Flamsteed on Nov. 9, 1695, at $10^h\ 15^m\ 4^s$; and reduced by him in MSS, vol. 23, page 269; where it will be seen that Flamsteed has erroneously copied out the time of transit as $10^h\ 16^m\ 4^s$; and that he has consequently made the right ascension too great by 15′; which I have here corrected. Its right ascension has also been deduced *per distantias*, in MSS, vol. 62 D, page 26; which confirms the correction here made. Flamsteed has not reduced the second observation made on Dec. 13, 1695.

265. 16 *Arietis.* This star is marked, in the *British Catalogue*, as of the 8th magnitude: but in the *original* entries it is marked once as the 8th and once as the 6th. I have therefore assumed it to be of the 7th magnitude: which will accord with Piazzi.

267. 6 *Trianguli.* The declination of this star in the *British Catalogue* is + 28° 50′ 40″. The observation from which it has been deduced was made on Dec. 31, 1689, at $7^h\ 5^m\ 41^s$; as may be seen in MSS, vol. 23, page 269, compared with page 330. But in the steps of the process he has made an error of 1′ which I have here corrected. In the *British Catalogue* this star is designated by the letter *ι*: but as there is no such letter in Bayer's map, I have here rejected it.

270. 63 *Ceti.* The right ascension of this star in the *British Catalogue* is 28° 43′ 30″: but an error of 1^m in time has been made in the reduction. There are two observations of it by Flamsteed: one on Nov. 21, 1698, at $9^h\ 30^m\ 43^s$, and the other on Nov. 25 in the same year, at $9^h\ 13^m\ 14^s$. On examining the original MS entry, I find that the first observation was originally entered $9^h\ 20^m\ 43^s$, but the 20 has been altered with the pen to 30: and the following note is made in the margin, "Query $29^m\ 43^s$," and Flamsteed has so assumed it in his computations: See MSS, vol. 23, pages 176 and 177. This alteration was afterwards made, as I imagine, from discovering the discordance in the two observations: but it is evident, on comparing the observation of this star with that of *o Ceti* made on the same day, that the error is in the observation of Nov. 25th; and that the time $9^h\ 13^m\ 14^s$, should be $9^h\ 14^m\ 14^s$. I have therefore added 15′ to Flamsteed's value.

271. 18 *Arietis.* Observed by Flamsteed on Oct. 25, 1695, at $11^h\ 19^m\ 7^s$: and regularly reduced by him in MSS, vol. 23, page 6: but I cannot find any star that will correspond with the declination here given. In the MS volume above mentioned it is marked as doubtful: and on referring to the *original* MS entry (in MSS, vol. 6) it appears as if the zenith distance had been altered with the pen; but I cannot ascertain the original figures: and it is evident that Flamsteed suspected some discordancy, as the reading *per strias cochleæ* is not inserted in the margin, agreeably to his usual practice. If we suppose an error of 6′ in the zenith distance, and that the declination were diminished to that amount, the star will correspond with Piazzi II. 12.

273. 65 *Ceti* ξ^1. This is supposed by Flamsteed to be the first of the two stars designated by the letter ξ, in Bayer's map: but it may be doubted whether such letter belongs to this star, or to 64 *Ceti.*

274. 7 *Persei* χ.
275. 8 *Persei.*

} Besides the observation of each of these stars, indicated by Miss Herschel, there is another of each of them on Jan. 4, 1694, at $6^h\ 11^m\ 58^s$ and $6^h\ 12^m\ 5^s$; which, on account of the zenith distance not being given, have been supposed to belong to the constellation *Aries:* since Flamsteed appears to have been observing in that constellation at that time. But, on referring to the original MS entry (a copy of which is given above to the note to No. 256 of this catalogue), it will be seen that Flamsteed had turned his telescope towards the constellation *Perseus:* and there can be no question but that these were the stars thus recorded. It is remarkable that Flamsteed makes 8 *Persei* to *follow* 7 *Persei* about 10′; but all the subsequent astronomers make it *precede* that star about the same quantity. And, in order that 8 *Persei* should agree with modern observations, its right ascension ought to be diminished nearly 5′.

277. 7 *Trianguli.* This star is designated by the letter η in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

278. 61 *Andromedæ.* The declination of this star in the *British Catalogue* is + 47° 22′ 0″. It was observed by Flamsteed on Nov. 19, 1693, at $9^h\ 39^m\ 4^s$; and regularly reduced by him in MSS, vol. 23, page 287. There is no star, however, to be found, corresponding to the position there deduced: nor with the next observed star which followed it 8ˢ afterwards. But if we suppose that the observation was made to the *north* of the zenith, this star will agree nearly with Piazzi II. 35; and the next observed star, with Piazzi II. 36. There is no authority for such a reading, in the original MSS; but, in no other way can the two observations be reconciled. I have therefore ventured to make the alteration: its right ascension however is too great by 5′. Both Piazzi and Lalande say that this star is lost: but Sir William Herschel states that it is in its place as given in the *British Catalogue.* See *Phil. Trans.* for 1797.

279. — *Andromedæ.* Observed by Flamsteed on Nov. 19, 1693, at $9^h\ 39^m\ 12^s$: but not noticed by Miss Herschel. In the original MS entry (MSS, vol. 5) it has the word "altera" set against it: which, as I conceive, implies that it was near to the star observed immediately preceding. I have therefore ventured to consider it as Piazzi II. 36. See the preceding note.

284. 62 *Andromedæ c.* The position of this star does not very well accord with the star designated by the letter *c* in Bayer's map: but there is no other that comes nearer to it.

286. 10 *Trianguli.* This star is designated by the letter *a* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

290. 23 *Arietis.* This star is called θ^2 by Flamsteed: but Bayer has only one star designated by that letter, in his maps, which is 22 *Arietis.* I have therefore rejected the letter here, on account of the difference in magnitude.

292. — *Cassiopeæ.* Observed by Flamsteed on Dec. 5, 1693, at $8^h\ 36^m\ 2^s$; and on Nov. 24, 1708, at $9^h\ 32^m\ 15^s$. It is Piazzi II. 72 = (35 Hev.) *Cassiopeæ.* Halley has inserted it in his edition of 1712, whose values I have assumed in the present catalogue; and I do not know why it was afterwards rejected. It was observed by Bradley. The right ascension does not well accord with modern observations.

293. 10 *Persei.* Observed by Flamsteed on Jan. 14, 1694, at $5^h\ 44^m\ 21^s$, as appears by MSS, vol. 23, page 293. But the observations of that day are not printed in the *Historia Cœlestis.*

298. — *Arietis.* Observed by Flamsteed on Dec. 11, 1690, at $8^h\ 12^m\ 58^s$, and reduced by him with the other stars observed on that day, in MSS, vol. 23, page 5; but in making the reduction he copied out the degrees of zenith distance 41° instead of 42°, whereby an error of 1° was committed in the declination. The star was again observed by him on Oct. 27, 1695, at $11^h\ 21^m\ 34^s$, where the zenith distance is correctly reduced; but as the time is expressed as uncertain, I presume that Flamsteed doubted whether it was the same star; and consequently rejected it, as I do not find it in any of the various MSS catalogues of the zodiacal stars. The observation on Jan. 14, 1696, was not reduced by Flamsteed. The star is not Piazzi II. 83, as mentioned in my catalogue of Flamsteed's omitted stars; but it is the star which was observed by Lalande in *Hist. Cél.* page 41, at $2^h\ 11^m\ 50^s$, 5, mag. 7½, as noted by M. Argelander.

300. 24 *Arietis* ξ. This star appears to be the same as that which is also designated as ψ *Ceti*, in Bayer's map of this latter constellation.

301. 11 *Trianguli.* This star, as well as 1 *Trianguli*, is designated by the letter *d* in the *British Catalogue*: but see the note to No. 200, above.

303. 12 *Trianguli.* The declination of this star in the *British Catalogue* is + 28° 16′ 45″. The observation from which it was deduced, was made on Dec. 21, 1689, at $7^h\ 21^m\ 16^s$; as may be seen in MSS, vol. 23, page 269, compared with page 330. But in copying out the zenith distance Flamsteed has made an error of 1′, which I have here corrected. In the *British Catalogue* this star is designated by the letter *c*: but as there is no such letter in Bayer's map, I have here rejected it.

304. — *Arietis.* Observed by Flamsteed on Dec. 23, 1704, at $7^h\ 13^m\ 57^s$; and again on Nov. 7, 1714, at $10^h\ 38^m\ 17^s$. It is Piazzi II. 85; and was observed also by Bradley.

306. — *Cassiopeæ.* Observed by Flamsteed on Dec. 5, 1693, at $8^h\ 42^m\ 46^s$: and the position here given is taken from Halley's edition of 1712. Modern observations show that the above value ought to be diminished 1^m, and that we should read $8^h\ 41^m\ 46^s$: but there is no authority for such a correction, in the *original* MSS. With the alteration here suggested, its $\mathcal{R}$ would be 32° 25′ 40″; and it would then agree with Piazzi II. 97 = (36 Hev.) *Cassiopeæ*, which has been also observed by Bradley.

310. — *Ceti.* Observed by Flamsteed on Dec. 18, 1699, at $7^h\ 48^m\ 52^s$: it is the same as that observed by Lalande in the *Hist. Cél.* page 41, at $2^h\ 18^m\ 41^s$, 3, mag. 7.

314. 28 *Arietis.* Observed by Flamsteed on Dec. 10, 1692, at $8^h\ 19^m\ 58^s$: an observation which Miss Herschel considers as belonging to 26 *Arietis*; but it will not agree therewith unless we suppose an error of at least 1^m in the time of transit; and that $8^h\ 19^m\ 58^s$ should be $8^h\ 18^m\ 58^s$. And as there is no star that can be found in any catalogue that will correspond with the position given in the *British Catalogue*, the conjecture is very probable.

319. 30 *Arietis.* In the observation of this star by Flamsteed, on Nov. 25, 1690, at $9^h\ 31^m\ 32^s$, he says that it is the brightest of two stars which are near together, and almost on the same parallel. The companion here alluded to, is Piazzi II. 128, and was observed by Bradley. The present star is marked as of the 7th magnitude in the *British Catalogue*: but, in the *original* entries, it is always designated as the 6th; which I have therefore adopted.

323. 79 *Ceti.* This star is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entry it is designated as the 8th, which I have therefore adopted. Piazzi calls it the 7th.

328. — *Ceti.* Observed by Flamsteed on Jan. 7, 1701, at $6^h\ 26^m\ 48^s$. It is Piazzi II. 140.

330. 12 *Persei.* Observed by Flamsteed on Dec. 19, 1693, after $7^h\ 49^m\ 50^s$, but the time of transit is not noted. It appears however from MSS, vol. 25, page 122, under the constellation *Perseus*, that the position of this star was determined by means of *distances*. In the *British Catalogue* this star is designated by the letter *q*: but, as there is no such letter in Bayer's map, I have here rejected it.

338. — *Ceti.* Observed by Flamsteed on Jan. 7, 1701, at $6^h\ 29^m\ 46^s$, and again on Dec. 5, 1711, at $8^h\ 58^m\ 34^s$. It is Piazzi II. 155; and was also observed by Bradley.

339. — *Arietis.* Observed by Flamsteed on Nov. 18, 1695, after $10^h\ 10^m\ 25^s$, when it passed north

of 16 *Trianguli* about 26′. It is probably the star observed by Lalande in *Hist. Cél.* page 30, at $2^h\ 31^m\ 44^s{,}5$, mag. $6\frac{1}{2}$.

341. — *Persei.* Observed by Flamsteed on Jan. 18, 1694, at $5^h\ 53^m\ 24^s$: and by Lalande in *Hist. Cél.* page 371, at $2^h\ 33^m\ 59^s$, mag. 7.

343. — *Persei.* Observed by Flamsteed on Jan. 18, 1694, at $5^h\ 53^m\ 40^s$: and by Lalande in *Hist. Cél.* page 371, at $2^h\ 34^m\ 14^s{,}9$, mag. 7.

346. 38 *Arietis.* This is the same as 88 *Ceti*, whose position in the *British Catalogue* is $\text{Æ} = 37° 4′ 0″$, and $\text{D} = +11° 7′ 0″$. It is marked as of the 7th magnitude, in the *British Catalogue;* but in the *original* entries, it is once marked as $5\frac{1}{2}$, and once as $6\frac{1}{2}$; I have therefore ventured to alter it to the 6th, which will more nearly agree with modern observations.

348. 15 *Persei* η. The declination of this star in the *British Catalogue* is $+48° 27′ 40″$, and it was observed by Flamsteed on Jan. 17, 1693, at $5^h\ 46^m\ 1^s$. There is no star that corresponds with this declination: and on referring to Halley's edition of 1712, I find the declination to be that which I have assumed in the present catalogue. And if we suppose that Flamsteed observed the star to the *north* of the zenith (instead of the *south*, as stated in the Observation-book) we shall have the corrected zenith distance equal to $3° 6′ 50″$ *north*, instead of $2° 59′ 50″$ *south;* the difference in declination ($=6° 6′ 40″$) being nearly the difference between Flamsteed's value, and that given by Halley. It thus becomes Piazzi II. 179$=\eta$ (9 Hev.) *Persei.* In the *British Catalogue* it is stated to be of the 6th magnitude: but in Halley's edition it is called the 4th. On consulting the *original* entry in the MS book, I find that it is there also noted as the 4th; which I have here adopted.

352. 1 *Eridani* τ^1. This is the same star as 90 *Ceti*, whose position in the *British Catalogue* is $\text{Æ} = 37° 40′ 30″$, and $\text{D} = -19° 54′ 30″$. I shall here take the opportunity of remarking that there are nine stars designated by the letter τ, in Bayer's map of *Eridanus*, whilst Flamsteed has only two; namely, the present star and 2 *Eridani.* The remaining seven are respectively 11, 16, 19, 27, 28, 33, and 36 *Eridani;* to which I have restored the letters, as indicated by Bayer. See the Introduction, page 399.

353. — *Ceti.* Observed by Flamsteed on Jan. 12, 1703, at $6^h\ 17^m\ 0^s$; the time is marked "circiter;" and it appears to have been too great by nearly 2^m. It is probably Piazzi II. 171.

354. 16 *Persei.* This star, as well as 20 *Persei*, is designated by the letter *p* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have rejected it in each place.

358. 17 *Persei.* This star is designated by the letter *r*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

359. 18 *Persei* τ. One of the observations of this star was made by Flamsteed on Jan. 16, 1693, at $5^h\ 55^m\ 16^s$; but it was *post transitum.* In reducing this observation in MSS, vol. 26 B, pages 16 and 23, Flamsteed has assumed that value as the correct time of transit, and thus increased the right ascension: which error has led to the introduction of another star into the *British Catalogue*, 19 *Persei*, whose position as there given is $\text{Æ} = 38° 32′ 30″$ and $\text{D} = +51° 27′ 35″$; but which consequently does not exist.

360. 20 *Persei.* This star is designated by the letter *p* in the *British Catalogue;* but see the note to No. 354 above.

363. 44 *Arietis* ρ^1 }
364. 45 *Arietis* ρ^2 } There is only one star designated by the letter ρ, in Bayer's map: but as it may be doubted to which of these three it ought to be applied, Flamsteed has annexed the letter to each; which I have here retained. The first of
366. 46 *Arietis* ρ^3 }

these stars (44 *Arietis*) is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entries, it is so designated only once; whilst it is once called of the 7th, and another time, as of the 6½. I have therefore adopted this latter value.

365. 21 *Persei*. This star is marked, in the *British Catalogue*, as of the 4½ magnitude. But in the *original* entries, it is once so called, and at the other time it is marked as the 6th. I have adopted the present reading, as a mean between the two.

367. — *Persei*. Observed by Flamsteed on Jan. 18, 1694, after $5^h\ 59^m\ 8^s$; but the time is not noted. It is probably Piazzi II. 220.

368. 22 *Persei* π. This star is marked, in the *British Catalogue*, as of the 4th magnitude: but, in the *original* entries, it is once marked as 4½, and once as 5½: I have therefore taken the mean of the two.

369. — *Ceti*. Observed by Flamsteed on Jan. 7, 1694, at $6^h\ 44^m\ 21^s$: it is Piazzi II. 215, and was observed also by Bradley.

370. — *Cassiopeæ*. Observed by Flamsteed on Decem. 5, 1693, at $9^h\ 11^m\ 50^s$: it is Piazzi II. 237 = (37 Hev.) *Cassiopeæ*, and was observed also by Bradley. Miss Herschel has supposed this to be the same star as No. 231 of this catalogue.

371. 24 *Persei*. The position of this star in the *British Catalogue* is $\mathcal{R} = 40°\ 47'\ 38''$ and $D = +\ 34°\ 50'\ 50''$. It was observed by Flamsteed on Dec. 19, 1693, at $8^h\ 5^m\ 21^s$; and again on Jan. 16, 1696, at $6^h\ 10^m\ 5^s$: the former observation has been regularly reduced by him in MSS, vol. 23, page 293; but the result is omitted to be inserted in the list of stars in the constellation *Perseus* in page 392 of the same volume. It is however given in Halley's edition of 1712; from which the position in the present catalogue is taken. In the *British Catalogue* this correct position is rejected, and the erroneous one, above mentioned, is inserted: which (from a note I find made in MSS, vol. 25, page 122) has been deduced *per distantias*. The only observations however of this star are recorded in the first volume of the *Historia Cœlestis*, page 90: and Mr. Henderson, who has been good enough to make the trigonometrical calculation at my request, finds that it corresponds almost exactly with the value given by Halley. The star in question is Piazzi II. 221, and not 227, as supposed by him and by Bessel. The position given in the *British Catalogue* has been erroneously deduced by Flamsteed, through a transposition of the logarithms of the sine and cosine of the arc $46°\ 12'\ 39''$; as may be seen in MSS, vol. 56, page 23, where the original computation is to be found. In the *British Catalogue* this star is designated by the letter *s*: but, as there is no such letter in Bayer's map, I have here rejected it.

373. — *Arietis*. Observed by Flamsteed on Jan. 14, 1692, at $6^h\ 14^m\ 31^s$; and imperfectly on three other days. It has subsequently been observed by Bradley, and is No. 414 in his catalogue.

377. — *Persei*. Observed by Flamsteed on Dec. 5, 1711, at $9^h\ 16^m\ 46^s$. M. Argelander says that it is the double star 336 in Struve's great catalogue; and that two observations of it at Abo give its position for 1830, $\mathcal{R} = 2^h\ 51^m\ 7^s{,}43$ and $D = +31°\ 44'\ 3''{,}2$.

378. — *Persei k*. Observed by Flamsteed on Jan. 18, 1694, at $6^h\ 9^m\ 14^s$. It is inserted in Hal-

ley's edition of 1712; and is Piazzi II. 236=(15 Hev.) *Persei*. It is designated by the letter *k* in Bayer's map, which I have here retained.

379. 91 *Ceti* λ. This star is marked, in the *British Catalogue*, as of the 4th magnitude: but it is nowhere so designated in the *original* entries. On the contrary, it is twice marked as of the 5th magnitude; which I have here retained.

380. 50 *Arietis*. The right ascension of this star in the *British Catalogue* is 41° 2′ 0″: it was observed by Flamsteed on Oct. 27, 1695, at $11^h\ 58^m\ 2^s$; and reduced by him in MSS, vol. 23, page 6. But on examining the steps of the process, it will be seen that Flamsteed has made a mistake of 1^m in the reduction. I have therefore deducted 15′ from the above value, and assumed the right ascension as given in the present catalogue. I ought here to remark, that there is another observation of this star on Jan. 13, 1696, after $6^h\ 21^m\ 9^s$, and which Miss Herschel has considered to be a different star, No. 436, in her catalogue. But it may be seen that the zenith distance in the last column is wrong 3°; and that when this is rectified it will agree with this star.

382. 49 *Arietis*. This star is marked as of the 7th magnitude, in the *British Catalogue*: but in the *original* entries it is once designated as the 5th, once as the 6th, and once as the 7th. I have therefore taken the mean. Flamsteed remarks that this star and 51 *Arietis* appear as *one* to the naked eye.

388. 8 *Eridani* ρ^1. There is only one star designated by the letter ρ in Bayer's map: but, as it may be doubted whether such letter may belong to this star, or to 9 or 10 *Eridani*, Flamsteed has annexed it to each; which I have therefore retained: although I think the letter belongs more properly to the latter star.

390. 93 *Ceti*. This star is marked as of the 6th magnitude in the *British Catalogue*: but, in the *original* entry it is designated as the 7th, which I have therefore retained.

391. *Persei* ι. Observed by Flamsteed on Jan. 16, 1693, at $6^h\ 9^m$ *circiter*; and again on Jan. 18, 1694, at $6^h\ 13^m\ 26^s$. The former observation has been reduced by Flamsteed, in MSS, vol. 23, page 289; and inserted in his list of stars in the constellation *Perseus*, in page 392 of the same volume. But the latter observation has not been reduced. The star is inserted in Halley's edition of 1712, from which the position in this catalogue is taken. It is Piazzi II. 253=(18 Hev.) *Persei*: and is designated by the letter ι in Bayer's map; which I have here retained.

393. 9 *Eridani* ρ^2. There is only one star designated by the letter ρ in Bayer's map: but, as it may be doubted whether such letter may belong to this star, or to 8 or 10 *Eridani*, Flamsteed has annexed it to each, which I have therefore retained; although I think the letter belongs more properly to the latter star.

396. 11 *Eridani* τ^3. The right ascension of this star in the *British Catalogue* is 42° 23′ 0″. There is only one observation of it by Flamsteed, with the mural circle; viz. on Dec. 10, 1691, after $8^h\ 55^m\ 39^s$: but the time of transit is not given. I find, by inspection of MSS, vol. 62 A, page 105, that the right ascension has been deduced from the observation *per distantias*, made on Jan. 28, 1682, at $7^h\ 20^m$: where the distance between this star and β *Canis Majoris* is stated to be 46° 54′ 20″, with the mark of uncertainty placed against the minutes. In the MS book above-mentioned, Flamsteed has copied out the distance 46° 44′ 20″, by which an erroneous right ascension has been deduced. But if we restore the original distance, the right ascension will

come out 42° 12′ 0″, which agrees more nearly with modern observations, and which I have therefore adopted. The whole, however, is but an approximate method. There is no letter annexed to it in the *British Catalogue*: but it is the third of the group designated by the letter τ in Bayer's map. See the note to No. 352.

397. 10 *Eridani* ρ^3. This star is stated to be of the 4th magnitude: but in the *original* entries it is designated as the 5th; which I have therefore retained. See further in the Notes to No. 388 and 393.

402. — *Arietis*. Observed by Flamsteed on Nov. 26, 1695, at $10^h\ 3^m\ 53^s$; the time being doubtful. It is Piazzi II. 261 = 98 Mayer.

403. 56 *Arietis*. The declination of this star in the *British Catalogue* is + 26° 1′ 15″. It was observed by Flamsteed on Nov. 18, 1695, at $10^h\ 38^m\ 50^s$; and again on Dec. 4, at $9^h\ 30^m\ 37^s$: both of which are regularly and correctly reduced by him in MSS, vol. 23, page 8. But, in copying it out, in his list of stars in the constellation *Aries*, in page 50 of the same volume, he appears to have confused it with 59 *Arietis*: and in page 53 has recomputed it with an erroneous north polar distance. It is evident, by comparing its zenith distance with other stars observed on the same day, that an error of about 2′ has been made, which I have here corrected. In the first observation above alluded to, Flamsteed remarks that it has two bright stars preceding it: which are probably those observed by Lalande in *Hist. Cél.* page 200, at $2^h\ 59^m\ 1^s{,}5$ and at $2^h\ 59^m\ 34^s{,}5$: the former of which was observed also by Bradley.

405. — *Arietis*. Observed by Flamsteed on Nov. 19, 1706, at $10^h\ 33^m\ 11^s$. I cannot find any star corresponding with it, unless we suppose that Flamsteed has made an error of 2^m in inserting the time: but there is no authority for such a supposition, in the original entry. The letter δ, however, is there marked as doubtful; and I cannot agree with M. Argelander that this was the star observed. If we suppose that $10^h\ 33^m\ 11^s$ should be $10^h\ 31^m\ 11^s$, the observation will agree with Piazzi II. 264 = 99 Mayer.

410. 31 *Persei* ι. There is no letter annexed to this star, in the *British Catalogue*: but it is designated as ι in Bayer's map; and I have therefore adopted it.

416. — *Arietis*. Observed by Flamsteed on Nov. 18, 1695, at $10^h\ 46^m\ 40^s$; and supposed by him to be a star in *Perseus*; which was probably the reason it was not reduced with the other stars in *Aries* on that day. It is Piazzi III. 32.

420. 96 *Ceti* κ^1. In the observation of this star by Flamsteed, on Jan. 14, 1692, at $6^h\ 36^m\ 31^s$, it is stated that there are two stars below it and preceding it; one of the 5th magnitude, and the other of the 6th. But I cannot find any two stars that will correspond with this description. The present star (96 *Ceti*) appears to be the one which is also designated as *g Tauri*, in Bayer's map of that constellation. See, however, the Note to No. 424.

423. 15 *Eridani*. The declination of this star in the *British Catalogue* is — 23° 39′ 5″. It was observed by Flamsteed on Dec. 10, 1691, at $9^h\ 11^m\ 11^s$; and reduced by him in MSS, vol. 23, page 179. But in copying out the corrected value in the list of stars in *Eridanus*, in page 198 of the same volume, he has made a mistake of 1′, which I have here corrected.

424. 97 *Ceti* κ^2. There is only one star designated by the letter κ, in Bayer's map: but, as it might be doubted whether such letter belongs to 96 *Ceti*, or to the present star, Flamsteed has applied it to each, which I have therefore retained. It is the same which is designated as

g Tauri in Bayer's map of that constellation. I would also remark that in the *British Catalogue* it is marked as of the 4th magnitude: but in the original entry it is stated to be *less* than 96 *Ceti;* and I have therefore adopted that reading.

425. 63 *Arietis* τ^3. Bayer has only one star designated by the letter τ, in his map: but as it may be doubted whether it belongs to this star or to 61 *Arietis*, Flamsteed has annexed the letter to each; which I have here retained.

426. 16 *Eridani* τ^4. There is no letter annexed to this star in the *British Catalogue:* but it is the fourth of the group of nine stars designated by the letter τ in Bayer's map; which I have therefore restored. See the note to No. 352.

436. 5 *Tauri f.* This star is omitted in Flamsteed's map of *Taurus.*

442. 9 *Tauri.* Sir William Herschel says "this star is lost;" and M. Lalande says that it is not to be found. See page 394. It is, however, still in its place. Probably it is a variable star.

443. 19 *Eridani* τ^5. There is no letter annexed to this star in the *British Catalogue:* but it is the fifth of the group of nine stars designated by the letter τ in Bayer's map; which I have therefore restored. See the note to No. 352.

444. — *Tauri.* Observed by Flamsteed, on Jan. 14, 1692, at $6^h\ 54^m\ 15^s$: it is Piazzi III. 98 = 114 Mayer, and has been also observed by Bradley. It is called by Flamsteed, in the original MS entry, the companion to 10 *Tauri:* but, in the printed copy, the words *comes hujus* are erroneously annexed to the following star.

445. 10 *Tauri.* The declination of this star has been deduced by Flamsteed from the observation made on Jan. 4, 1690, at $7^h\ 30^m\ 29^s$: but the reading *per strias cochleæ* differs about 1′ from that *per lineas diagonales.* The two subsequent observations by Flamsteed, as well as those of modern astronomers, show that the former is the correct reading; and that the declination should be diminished about that quantity: but I have not made any alteration.

449. 40 *Persei o.* Observed by Flamsteed on Dec. 5, 1711, at $9^h\ 56^m\ 58^s$: but the position here given (which is the same as in the *British Catalogue*) has been deduced *per distantias.* See MSS, vol. 25, page 122: where there is also another value given to this star; viz. $\text{Æ} = 50°\ 44'\ 0''$ and $D = +\ 32°\ 56'\ 20''$; which I apprehend to have been deduced from the observation just mentioned. This is the star called *parvula supra o* in Flamsteed's *Historia Cœlestis*, vol. I. page 90; and is in fact the star designated as *o* in Bayer's map. See the note to No. 454. Flamsteed has designated 38 and 40 *Persei* by the letters o^1 and o^2; but the former is the Greek ομικρον, and the latter the English o.

454. 38 *Persei o.* Observed by Flamsteed on Feb. 5, 1690, at $5^h\ 17^m$; and again on Dec. 5, 1711, at $9^h\ 59^m\ 1^s$: in both of which observations it is called *o*: and its proper position is inserted in Halley's edition of 1712; which, however, was deduced from *distances*, made with the sextant, from *α Arietis* and *α Tauri*, as given in the first volume of the *Historia Cœlestis*, page 90, at $9^h\ 12^m$ and $9^h\ 44^m$. See MSS, vol. 25, page 122. It is Piazzi III. 123 = (31 Hev.) *Persei*, and not III. 85, as supposed by him, and by Bessel. The position here given is taken from the MS volume above stated. In the edition of the *British Catalogue* by Flamsteed, the place of the star is materially altered: the present position is erased, and a new one introduced; viz. $\text{Æ} = 49°\ 17'\ 35''$ and $D = +\ 31°\ 2'\ 9''$: but no such star exists. The error has arisen from a mistake in the trigonometrical calculation, the original of which still exists (MSS, vol. 56, page

23); and where it will be seen that Flamsteed has taken out the logarithmic sine of 19° 36′ 40″ equal to 9·585266 instead of 9·525866. When this error is corrected, the result will agree with the position here given. In consequence of this restoration of the original and correct values, the present star will be the star designated as *o* in Bayer's map; and which was supposed to have been omitted. It may be proper here to remark that the position of this star is also erroneously laid down in Flamsteed's chart; and does not accord with either of the positions here alluded to. As this is one of the stars with which the comet of 1680 was compared (see Newton's *Principia*, lib. 3) it is the more necessary to attend to the remarks here made.

457. 16 *Tauri*. The star which is called *n* 15 *Tauri* does not exist, neither is there any observation of such a star in the *Historia Cœlestis*. Flamsteed observed on Jan. 10, 1690, at $7^h\ 9^m\ 2^s$, a star which he denominates *n* (but which is in fact 16 *Tauri*), and, in copying this out for computation, in the MSS, vol. 23, page 21, he has inadvertently annexed the zenith distance of *α Arietis*, observed on the same day; and this has been the cause of the error. I would here remark that the letter *g* annexed to this star in the *British Catalogue* is not the *g* in Bayer's map (which is a different star, viz. No. 420 of this catalogue), but one of the series of additional letters which Flamsteed has introduced for the purpose of designating the Pleiades: I have therefore rejected it here.

458. 17 *Tauri*. This star has the letter *b* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 79 *Tauri*. I have therefore rejected it here.

459. 18 *Tauri*. This star has the letter *m* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 104 *Tauri*. I have therefore rejected it here.

460. 19 *Tauri*. This star has the letter *e* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 30 *Tauri*. I have therefore rejected it here.

461. 20 *Tauri*. This star has the letter *c* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 90 or 93 *Tauri*. I have therefore rejected it here.

462. 21 *Tauri*. This star has the letter *k* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 98 *Tauri*. I have therefore rejected it here.

463. 22 *Tauri*. This star has the letter *l* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 106 or 107 *Tauri*. I have therefore rejected it here.

464. 23 *Tauri*. This star has the letter *d* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 88 *Tauri*. I have therefore rejected it here.

467. 24 *Tauri*. This star has the letter *p* annexed to it in the *British Catalogue*: but this is not the star designated by that letter in Bayer's map; which is 44 *Tauri*. I have therefore rejected it here.

471. 42 *Persei n.* The position of this star in the *British Catalogue* is $\mathcal{R} = 52^\circ\ 28'\ 59''$, and $D = +32^\circ\ 18'\ 25''$; which is erroneously deduced by means of *distances* from *α Arietis* and *α Tauri* as given in the first volume of the *Historia Cœlestis*, page 90. The original computation still exists (MSS, vol. 56, page 24), and the source of the error is manifest in one of the steps of the process. When this is corrected, the result will agree with the value given by Halley in his edition of 1712; from which the position in the present catalogue is taken. There is an observation of it with the mural arc, on Dec. 5, 1711, at $10^h\ 4^m\ 5^s$; which confirms the result here given.

472. 26 *Tauri.* This star has the letter *s* annexed to it in the *British Catalogue:* but this is not the star designated by that letter in Bayer's map; which is 4 *Tauri.* I have therefore rejected it here.

473. 27 *Tauri.* This star has the letter *f* annexed to it in the *British Catalogue:* but this is not the star designated by that letter in Bayer's map; which is 5 *Tauri.* I have therefore rejected it here.

474. 28 *Tauri.* This star has the letter *h* annexed to it in the *British Catalogue:* but this is not the star designated by that letter in Bayer's map; which is 58 *Tauri.* I have therefore rejected it here.

476. — *Tauri.* Observed by Flamsteed on Feb. 4, 1691, at $5^h\ 45^m\ 56^s$, and again in $\mathcal{R}$ only on Feb. 3, 1693, at $5^h\ 41^m\ 40^s$. The former is copied out for reduction in MSS, vol. 23, page 25; but, in so doing, an error has been committed of $6'$ in the zenith distance. I know not for what reason it was omitted to be inserted in the catalogue, unless it were the suspicion of some error. It is Piazzi III. 163.

477. 26 *Eridani π.* This star is marked as of the 4th magnitude, in the *British Catalogue:* but in the *original* entry, it is designated as the 5th; which I have therefore retained.

478. 27 *Eridani* τ^6. This star has no letter annexed to it in the *British Catalogue:* but it is the sixth of the group of nine stars designated by the letter τ in Bayer's map; and I have therefore restored it. See the note to No. 352.

479. — *Persei.* Observed by Flamsteed on Jan. 16, 1693, at $6^h\ 54^m\ 45^s$; and has been regularly reduced by him in MSS, vol. 23, page 289. It is inserted in Halley's edition of 1712, from which the position in the present catalogue is taken. It is Piazzi III. 186; which is erroneously called 46 *Persei.*

481. 28 *Eridani* τ^7. This star has no letter annexed to it in the *British Catalogue:* but it is the seventh of the group of nine stars designated by the letter τ in Bayer's map; and I have therefore restored it. See the note to No. 352.

483. 31 *Tauri* u^3. Bayer has only one star designated by the letter *u*, in his map: but, as it may be doubted whether such letter belongs to 29 *Tauri*, or to the present star, Flamsteed has annexed that letter to each of them; which I have here retained.

486. 30 *Eridani.* The star 31 *Eridani*, whose position in the *British Catalogue* is $\mathcal{R} = 54^\circ\ 36'\ 20''$, and $D = -6^\circ\ 14'\ 15''$, does not exist; neither is there any observation of it by Flamsteed. There are two observations of 30 *Eridani* in the *Historia Cœlestis*, the first of which is correctly reduced: but in the process of the reduction of the observation made on Jan. 14, 1692, at

$7^h\ 10^m\ 43^s$, in MSS, vol. 23, page 181, Flamsteed has made a mistake of 1^m in time. I would also remark that the zenith distance *per strias cochleæ* is the correct value, and not that *per diagonales*, which is corrected in one of the MS copies. This makes an error of 5′ in the declination: and the two errors united, form the difference in question; and clearly show that the observations belong to one and the same star.

491. 33 *Eridani* τ^8. There is no letter annexed to this star in the *British Catalogue:* but it is the eighth of the group of nine stars designated by the letter τ in Bayer's map; and I have therefore restored it here. See the note to No. 352.

492. — *Tauri.* Observed by Flamsteed on Jan. 21, 1715, at $6^h\ 47^m\ 42^s$. It is Piazzi III. 203.

495. 47 *Persei* λ. This star is certainly designated as of the 4th magnitude, in one of the observations: but in the *original* entry of the observation on Jan. 16, 1693, it is noted that *c* (48 *Persei*) is *brighter* than λ. Piazzi designates it as of the 6th magnitude.

501. 36 *Eridani* τ^9. There is no letter annexed to this star in the *British Catalogue:* but it is the last of the group of nine stars designated by the letter τ in Bayer's map; and I have therefore restored it here. See the note to No. 352. This star is designated as of the 4th magnitude, in the *British Catalogue:* but in the *original* entries it is said to be $4\frac{1}{2}$; which I have here adopted.

502. 39 *Tauri* ^2A. There is only one star designated by the letter *A*, in Bayer's map: but as it may be doubted whether such letter belongs to this star, or to 37 *Tauri*, I have annexed the letter to each.

508. — *Persei.* Observed by Flamsteed on Jan. 20, 1696, at $7^h\ 4^m\ 54^s$: and by Lalande in *Hist. Cél.* page 376, at $3^h\ 58^m\ 30^s,8$, mag. $8\frac{1}{2}$: and also in the *Mem. de l'Acad.* for 1790, page 374, at $3^h\ 51^m\ 18^s,5$. Four observations of it by M. Argelander give its position for 1830, $AR = 4^h\ 1^m\ 9^s,22$, and $D = +48°\ 38'\ 53'',6$; the magnitude varying from $7\frac{1}{2}$ to $6\frac{1}{2}$.

509. 43 *Tauri* ω^1. There is only one star designated by the letter ω in the *British Catalogue*, which, in my opinion, belongs to 50 *Tauri*, and not to this star: but I have retained them both.

512. — *Persei.* Observed by Flamsteed on Jan. 25, 1696, after $6^h\ 42^m\ 20^s$. It is Piazzi IV. 7 = (12 Hev.) *Camelopardi.* It is inserted in Halley's edition of 1712, where its position is $AR = 58°\ 11'\ 0''$, and $D = +52°\ 31'\ 20''$.

515. — *Persei* b^1. Observed by Flamsteed on Feb. 5, 1690, at $5^h\ 46^m\ 39^s$; and again on Jan. 20, 1696, at $7^h\ 9^m\ 7^s$. It is Piazzi IV. 18 = (41 Hev.) *Persei:* and is inserted in Halley's edition of 1712; from which the position in the present catalogue is taken. There is also another observation by Flamsteed on Jan. 25, 1696, at $6^h\ 45^m\ 11^s$, which I think there can be no doubt belongs to this star. That observation is stated to have been made to the *north* of the zenith: but no star can be found in such position. If, however, we suppose, with M. Argelander, that the observation was made to the *south* of the zenith, (and, as Flamsteed appears to have been re-observing some of the stars which he had observed on the 20th of the same month, this is extremely probable,) the star will then turn out to be the same as that observed on the two above-mentioned days. It is No. 155 in Miss Herschel's catalogue, and No. 77 in my list of Flamsteed's inedited stars: which I had there incorrectly conjectured to be Piazzi III. 257. In

Bayer's map it is designated by the letter *b*, which I have here retained: although it may be doubted whether such letter belongs to this star, or to No. 521 in the present catalogue. I have therefore called this b^1; and the other, b^2.

517. — *Persei.* Observed by Flamsteed on Feb. 5, 1690, at $5^h\ 47^m\ 40^s$; and on Jan. 20, 1696, at $7^h\ 10^m\ 6^s$. M. Argelander states it to be found in the *Mem. de l'Acad.* for 1790, page 374, at $4^h\ 3^m\ 35^s,5$: mag. $5\frac{1}{2}$. It is inserted in Halley's edition of 1712; from which the present position is taken.

518. — *Tauri.* Observed by Flamsteed on Oct. 27, 1695, at $13^h\ 11^m\ 19^s$. The zenith distance *per strias cochleæ* is the correct value, which differs 10′ from that *per lineas diagonales.* This correction being made, the star is Piazzi IV. 13; and was observed also by Bradley.

519. 38 *Eridani o.* This star is marked as of the $3\frac{1}{2}$ magnitude, in the *British Catalogue*: but, in the *original* entry, it is designated as the 4th; which I have here retained.

521. — *Persei* b^2. Observed by Flamsteed on Feb. 5, 1690, at $5^h\ 48^m\ 26^s$; and again on Jan. 20, 1696, at $7^h\ 10^m\ 59^s$. M. Argelander states it to be found in the *Mem. de l'Acad.* for 1790, page 374, at $4^h\ 4^m\ 25^s$, mag. 6. It is inserted in Halley's edition of 1712; from which the present position is taken. See the note to No. 515. above.

522. — *Camelopardi.* Observed by Flamsteed on Jan. 25, 1696, after $6^h\ 45^m\ 11^s$. The reading *per strias cochleæ* disagrees with that *per lineas diagonales*, 2° 56′. In the *original* entry, the latter is marked as doubtful: but in the MS copy, they are both so marked. I have adopted the reading *per lineas diagonales*, because it will then agree with the star observed by Lalande in the *Hist. Cél.* page 371, at $4^h\ 4^m\ 58^s$, mag. 7. But if we adopt the reading *per strias cochleæ*, the declination will be 52° 47′ 15″, and thus agreeing more nearly with the other stars that Flamsteed was then observing. I cannot find, however, any star that will accord with such a position.

523. 47 *Tauri.* This star is marked as of the 7th magnitude, in the *British Catalogue*: but in the *original* entry of Oct. 27, 1695, it is said to be of the $5\frac{1}{2}$ magnitude; which I have here retained.

525. 49 *Tauri* μ. This star is marked as of the 4th magnitude, *in the British Catalogue*: but in the *original* entry of Oct. 27, 1695, it is said to be of the $5\frac{1}{2}$ magnitude; which I have here retained.

531. 40 *Eridani d.* There is an annual proper motion of this star, in declination, of − 3″,391: which will make the difference in declination only + 13″,2. There is also a considerable proper motion in right ascension.

534. 56 *Tauri.* There are three observations of this star by Flamsteed: but the only one reduced by him is that made on Jan. 6, 1690, at $8^h\ 1^m\ 39^s$: and from which he has made the $AR = 60°\ 35'\ 10''$. On examining MSS, vol. 23, page 19, where the steps of the process are put down, I find that Flamsteed has made a mistake of 1^m in time. I have therefore deducted 15′ from the value in the *British Catalogue.*

536. 55 *Tauri.* Observed by Flamsteed on Jan. 19, 1712, at $7^h\ 21^m\ 16^s$. The declination in the *British Catalogue* is + 15° 52′ 5″: but on comparing the observed zenith distance with that of the other stars on that day (excepting 63 *Tauri* immediately following), it will be found

that an error of 8′ has been committed, which I have here corrected. I would here remark that the zenith distance of 63 *Tauri, per strias cochleæ*, is the correct reading; although Miss Herschel does not so consider it.

537. 57 *Tauri h.* There is no letter annexed to this star in the *British Catalogue:* but it is designated by the letter *h* in Bayer's map, which Flamsteed has erroneously annexed to 58 *Tauri.*

538. 58 *Tauri.* This star is designated by the letter *h* in the *British Catalogue:* but that letter, in Bayer's map, more properly belongs to 57 *Tauri.* I have therefore rejected it here.

540. 60 *Tauri.* Observed by Flamsteed on Dec. 31, 1698, at $8^h\ 43^m\ 50^s$. The right ascension in the *British Catalogue* is 61° 3′ 20″; but it is evident on comparing the difference in the time of transit, with *α Tauri*, that an error of about 4′ in space has been made; and that the true Æ deduced correctly would more nearly accord with the value in Halley's edition of 1712, which I have therefore adopted. On examining MSS, vol. 23, page 32, I find that Flamsteed had originally inserted another star (58 *Tauri*, erroneously deduced from the observation made on Feb. 2, 1691, at $6^h\ 23^m\ 53^s$), whose Æ he made 61° 0′ 30″: and perhaps after he had made the observation above stated, he took the mean of the two for the proper value. The error is still considerable.

546. 41 *Eridani* υ^4. There is no letter annexed to this star, in the *British Catalogue:* but it is the fourth in the series of stars designated by the letter *υ*, in Bayer's map; which I have therefore retained. See the *Introduction*, page 400.

547. 64 *Tauri* δ^2. There is only one star designated as *δ Tauri*, in Bayer's map: but as the present star may also lay claim to that appellation, I have retained Flamsteed's notation.

548. — *Tauri.* Observed by Flamsteed on Nov. 28, 1715, at $11^h\ 7^m\ 35^s$: but I cannot find it in any catalogue. Six observations of it by M. Argelander make its position for 1830 Æ $= 4^h\ 15^m\ 3^s{,}3$, and D $= +18°\ 38'\ 39''{,}4$; mag. 6.

551. 67 *Tauri* κ^2. There is only one star designated as *κ Tauri*, in Bayer's map: but, as there may be a doubt whether the present star may not also lay claim to that appellation, I have retained Flamsteed's notation. The magnitude of the star, however, differs much from that given by Piazzi, which is only 6½: I cannot find any recorded observation of it.

552. 68 *Tauri* δ^3. This star is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entries, it is designated once as the 4th, and once as the 5th; I have therefore taken the mean.

556. 42 *Eridani ξ.* This star is marked as of the 3½ magnitude, in the *British Catalogue:* which is erroneous, since in the *original* entry it is denominated only as of the 6th, which I have here retained; and which agrees with Piazzi.

558. 72 *Tauri* υ^2. There is only one star designated as *υ Tauri*, in Bayer's map: but, as the present star may also lay claim to that appellation, I have retained Flamsteed's notation.

565. 79 *Tauri b.* This star is erroneously marked with the letter *h* in Flamsteed's map.

567. 43 *Eridani* υ^5. There is no letter annexed to this star, in the *British Catalogue:* but it is the

fifth in the series of stars, designated by the letter υ in Bayer's map; which I have therefore retained. See the *Introduction*, page 400.

570. — *Tauri.* There are six observations of this star by Flamsteed; only two of which (Dec. 12, 1690, and Feb. 2, 1691) appear to have been reduced by him. It is Piazzi IV. 99 = 160 Mayer, and has also been observed by Bradley. It is inserted in Halley's edition of 1712, from which the position in this catalogue is taken: and I know not why it was afterwards rejected. I would here remark that modern observations indicate an error in the entry of the observation of this and the following star, on Oct. 29, 1691, page 137 of the second volume of the *Historia Cœlestis.* It appears that the zenith distances are transposed: that is, they are placed against the wrong stars, and that the reading ought to be as follows: viz.

160 Mayer =	13	19	41	.	36	1	10	.	816.41	.	35	58	40
81 Tauri =	13	19	48	.	36	31	20	.	827.79	.	36	28	50

and in no other way can the observations be reconciled to the present state of the heavens. I ought to remark, however, that there is no appearance of such an error in the original MS entry; which is exactly as it stands in the printed copy: yet I think such a mistake is very probable to have occurred in transcribing; and I have therefore computed the values accordingly.

571. 81 *Tauri.* There are two observations of this star, by Flamsteed: the first of which, on Feb. 4, 1691, at $6^h\ 27^m\ 30^s$, has been erroneously copied out in north polar distance by him; as may be seen in MSS, vol. 23, by comparing the values in pages 25 and 33. He therefore considered it another star, 82 *Tauri*, whose position in the *British Catalogue* is $\mathcal{R} = 63^\circ\ 14'\ 40''$, and $D = +14^\circ\ 23'\ 30''$: but which consequently does not exist. The other observation of this star is on Oct. 29, 1691, at $13^h\ 19^m\ 48^s$, and not at $13^h\ 19^m\ 41^s$, as supposed by Miss Herschel. See the preceding note.

573. — *Tauri.* Observed by Flamsteed on Oct. 29, 1691, at $13^h\ 19^m\ 53^s$; which, compared with α *Tauri*, observed on the same evening, will show that this is Piazzi IV. 102 = 162 Mayer.

584. 47 *Eridani.* This star is marked as of the 4th magnitude, in the *British Catalogue*: but, in the *original* entry, it is designated as the 5th; which I have therefore retained.

585. 89 *Tauri.* Besides the two observations of this star, indicated by Miss Herschel, there is another on Feb. 2, 1691, at $6^h\ 41^m\ 12^s$, which is imperfectly recorded in the second volume of the *Historia Cœlestis*, page 90; and which led me to suspect that it was another star, No. 87 in my list of Flamsteed's inedited stars. But on referring to the *original* MS entry, I find that there were *three* observations of Palilicium in zenith distance on that day, the last of which, in the first and last columns, is erroneous 9′, and should be the same as the two preceding. And moreover, that the zenith distance which should be opposite to $6^h\ 41^m\ 12^s$ is altogether omitted. The original is very confused, inasmuch as the wrong zenith distances are placed against these two stars: it stands thus,

6.38.57	Palilicium	35	39	0
	—— post trans. repet. . .	35	39	5
::41.12	ad Austrum . . .	35	39	5
	Palilicium egreditur . . .	36	7	35

Now it is evident that the zenith distances in these last two lines should be transposed: and Flamsteed himself has so considered it when he reduced the stars, as may be seen in MSS, vol. 23, page 23, where this star is evidently shown to be 89 *Tauri.*

587. 48 *Eridani* ν. The declination of this star in the *British Catalogue* is $-4^\circ\ 0'\ 5''$. It was observed by Flamsteed on Dec. 9, 1691, at $10^h\ 31^m\ 7^s$: but the reading of the zenith distance *per strias cochleæ* differs $2'$ from that *per lineas diagonales.* Flamsteed has assumed the latter: but the former appears to be the correct reading. I have therefore added $2'$ to the above value.

590. 50 *Eridani* v^6. This star is called v^1 in the *British Catalogue:* but it is the sixth in the series of stars designated by the letter v in Bayer's map; and I have therefore altered the notation. See the *Introduction,* page 400.

592. 92 *Tauri* σ^3. There is only one star designated as σ *Tauri* in Bayer's map: but, as the present star may also lay claim to that appellation, I have retained Flamsteed's notation.

595. 93 *Tauri* c^3. There is only one star designated as c *Tauri* in Bayer's map: but, as the present star may also lay claim to that appellation, I have retained Flamsteed's notation.

596. 9 *Camelopardi.* Observed by Flamsteed Jan. 25, 1696, the second star after $7^h\ 5^m\ 44^s$; but the time is not stated; and consequently the right ascension is not given in the *British Catalogue.* It was considered by Flamsteed at first as belonging to *Ursa Major;* and is inserted in that constellation by Halley, in his edition of 1712; from which, the right ascension in the present catalogue is taken. It is Piazzi IV. 176 = (17 Hev.) *Camelopardi.*

597. 52 *Eridani* v^7. This star is marked as v^8 in the *British Catalogue:* but it is the seventh and last in the series of stars designated by the letter v in Bayer's map: and I have therefore altered the notation. See the *Introduction,* page 400.

600. — *Eridani.* Observed by Flamsteed on Feb. 2, 1701, at $6^h\ 40^m\ 58^s$. It is Piazzi IV. 154.

601. 95 *Tauri.* The right ascension of this star in the *British Catalogue* is $66^\circ\ 20'\ 10''$. It was observed by Flamsteed on Feb. 10, 1707, at $6^h\ 9^m\ 35^s$: and reduced by him in MSS, vol. 26 C, page 34. The reading *per strias cochleæ* differs $26'$ from that *per lineas diagonales;* but the latter appears to be the true one. If the transit of this star be compared with that of α *Tauri* on the same evening, it will be seen that the right ascension given by Flamsteed is full $10'$ greater than it ought to be. The MS book containing the *original* entry is lost: and the observations of that day are not inserted in the *copy.* In the MS copy of the *British Catalogue* (MSS, vol. 27 C) the right ascension appears to have been originally $66^\circ\ 10'\ 10''$; and to have been subsequently altered with the pen. I have therefore restored the original and correct value.

602. — *Eridani.* Observed by Flamsteed on Feb. 2, 1701, at $6^h\ 41^m\ 33^s$. It is Piazzi IV. 157, and has been observed also by Bradley.

605. 1 *Aurigæ.* This star is designated by the letter f, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

606. 56 *Eridani.* Observed by Flamsteed on Jan. 14, 1692, at $8^h\ 2^m\ 25^s$. The declination in the *British Catalogue* is $-9^\circ\ 2'\ 10''$: but the reading *per strias cochleæ* differs $5'$ from that *per lineas diagonales.* The former is the true one: and although Flamsteed has assumed the latter in his reduction in MSS, vol. 23, page 181; yet in the original MS entry he appears (from the subsequent insertion of some figures) to have been afterwards aware of his error:

I have therefore added 5′ to Flamsteed's declination: and it now accords very well with Bradley.

610. 2 *Aurigæ.* This star is designated by the letter *g*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

611. 96 *Tauri.* Observed by Flamsteed on Jan. 1, 1700, at $9^h\ 11^m\ 59^s$. In order to correspond with modern observations, the declination should be diminished 5′: but I have examined the original MS entry (MSS, vol. 7), and do not find any trace of error. The two subsequent observations on that day, which are supposed to relate to No. 100 and 101 *Tauri*, are not reconcilable with the present position of the heavens: as No. 100 does not exist; and the star which is supposed to be No. 101 differs 9′ in right ascension from Flamsteed's value.

613. 1 *Orionis* π. This star has no letter annexed to it in the *British Catalogue:* but it is the first of the cluster of six stars designated by the letter π in Bayer's map. See the Introduction, page 399.

615. 2 *Orionis* π^2. This star is called π^1 in the *British Catalogue:* but it is the second of the cluster of six stars designated by the letter π in Bayer's map. See the Introduction, page 399.

618. 3 *Orionis* π^3. This star has no letter annexed to it in the *British Catalogue:* but it is the third of the cluster of six stars designated by the letter π in Bayer's map. See the Introduction, page 399.

625. — *Tauri.* Observed by Flamsteed on Feb. 14, 1696, after $6^h\ 5^m\ 36^s$; but the exact time of transit is not noted. I cannot find this star in any catalogue: but M. Argelander states that it has been observed at Abo, and that its position for 1830 is $\mathrm{AR} = 4^h\ 45^m\ 54^s{,}75$ and $\mathrm{D} = +\ 24^\circ\ 18'\ 47''{,}4$.

628. 7 *Orionis* π^4. This is called π^3 in the *British Catalogue:* but it is the fourth of the cluster of six stars designated by the letter π in Bayer's map. See the Introduction, page 399.

629. 8 *Orionis* π^5. This star has the letter *z* annexed to it in the *British Catalogue:* but there is no such letter in Bayer's map. It is, in fact, the fifth of the cluster of six stars designated by the letter π in Bayer's map; which I have therefore retained. See the Introduction, page 399.

630. 4 *Aurigæ.* This star is designated by the letter ω, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

632. 99 *Tauri.* The position of this star in the *British Catalogue* is $\mathrm{AR} = 69^\circ\ 55'\ 45''$ and $\mathrm{D} = +\ 23^\circ\ 2'\ 55''$. There is no observation of it with the mural arc: but in MSS, vol. 25, page 39, it is stated by Flamsteed to have been deduced by means of *distances* from α *Geminorum* and γ *Orionis:* and in the original entry of the observations made with the sextant on Dec. 14th and 17th, 1677, there is a diagram of the three stars forming the triangle there alluded to. These stars are 98, 99, and 103 *Tauri.* The position of the first of these stars is correct, and was subsequently observed with the mural arc: but the positions of the two others are erroneous both in right ascension and declination. And that these errors have arisen from some mistake in the computation is manifested by repeating the trigonometrical process, from the data given by Flamsteed: whereby it will be found that the position of this star corresponds exactly with Piazzi IV. 243 = 179 Mayer. I have given only the *approximate* values for the epoch 1690. The three stars, alluded to by Flamsteed, as forming the triangle, are denoted by the numbers

10, 11, and 12, in his *Historia Cœlestis*, vol. 1, page 6: which correspond respectively with 99, 98, and 103 *Tauri*. It is singular that there should be a star very near the point in which Flamsteed has erroneously placed this star; and whose position is given by M. Argelander.

636. 11 *Camelopardi*. This star is marked as of the 5th magnitude in the *British Catalogue*: but although it is once so called in the observations, yet, at another time, it is stated to be of the 6th. I have therefore taken the mean. Piazzi makes it 6½.

639. 100 *Tauri*. Observed by Flamsteed on Jan. 1, 1700, at $9^h\ 20^m\ 5^s$: but I cannot find a star in any catalogue that corresponds with this description. See the note to No. 611 of this catalogue. The zenith distance is marked as doubtful in the original MS entry.

642. 101 *Tauri*. Observed by Flamsteed on Jan. 1, 1700, at $9^h\ 21^m\ 55^s$. In order to correspond with more modern observations, the declination should be diminished at least 9′: but I have examined the original MS entry, (MSS, vol. 7,) and do not find any trace of error. See the Note to No. 611 of this catalogue.

643. 10 *Orionis* π^4. This star has no letter annexed to it in the *British Catalogue*: but it is the last of the cluster of six stars designated by the letter π in Bayer's map; and I have therefore retained that appellation. See the Introduction, page 399.

644. 9 *Aurigæ*. One of the observations of this star was made by Flamsteed on Jan. 20, 1696, at $7^h\ 56^m\ 28^s$; where it is stated to have been observed to the *north* of the zenith. This observation has been reduced by Flamsteed accordingly in MSS, vol. 23, page 375, and has given rise to the introduction of 13 *Camelopardi* into the *British Catalogue*, whose position there given is $\mathcal{R} = 70^\circ\ 38'\ 30''$ and $D = +\ 51^\circ\ 59'\ 20''$: but no such star exists. It has therefore been suggested by Miss Herschel that probably the observation was made to the *south* of the zenith; in which case it would agree with 9 *Aurigæ*. And although there is nothing in the original MS entry (MSS, vol. 6) to warrant this alteration, yet I have ventured to adopt it; since in no other way can the discordance be reconciled. I would remark that in the list of stars in *Ursa Major* (amongst which this star was originally classed) in MSS, vol. 25, the right ascension is marked as doubtful.

645. 102 *Tauri* ι. One of the observations of this star, viz. that made on Oct. 1, 1704, at $15^h\ 29^m\ 27^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 34: and by some unaccountable mistake he has made an error of upwards of 1½ hour in the right ascension. Whence has arisen the introduction of 3 *Tauri* into the *British Catalogue;* whose position as there given is $\mathcal{R} = 48^\circ\ 16'\ 10''$ and $D = +\ 21^\circ\ 4'\ 30''$: but which consequently does not exist.

647. 14 *Camelopardi*. This star is marked as of the 5th magnitude, in the *British Catalogue*: but in the *original* entry, it is designated as the 7th; which I have therefore retained.

649. — *Camelopardi*. Observed by Flamsteed on Jan. 20, 1696, at $7^h\ 59^m\ 20^s$: and regularly reduced by him in MSS, vol. 23, page 375. It is inserted in Halley's edition of 1712, under the constellation of *Ursa Major:* and was observed by Lalande in *Hist. Cél.* page 374, at $4^h\ 53^m\ 23^s{,}4$. I know not why it was afterwards rejected.

652. 11 *Orionis*. This star, as well as 15 *Orionis*, has the letter *y* annexed to it in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have rejected it in both places.

653. — *Leporis*. Observed by Flamsteed on Feb. 13, 1691, at $6^h\ 4^m\ 46^s$: but it is not reduced by

him, with the other stars on that day. The right ascension appears to be doubtful, as it was observed *post transitum*. On referring to the *original* MS entry, (MSS, vol. 5,) I find much confusion in the figures denoting the time of transit. It appears that the time of transit of the subsequent star, viz. $6^h\ 27^m\ 46\frac{1}{2}^s$ had been *originally* entered in its stead: the 27 is completely obliterated, and the figure 4 inserted in the margin: the seconds, instead of being 46 as in the printed copy, appear to me to have been altered to 11. On the whole, the transit cannot be depended upon; and I suspect, with M. Argelander, that the time of observation should have been recorded $6^h\ 24^m\ 46^s$: in which case it would agree with Piazzi IV. 285; and I have therefore adopted that reading.

654. 104 *Tauri m.* One of the observations of this star, viz. that made on Oct. 1, 1704, at $15^h\ 33^m\ 52^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 34: and, by some unaccountable mistake, he has made an error of upwards of $1\frac{1}{2}$ hour in the right ascension. Whence has arisen the introduction of 8 *Tauri* into the *British Catalogue*: whose position as there given is $AR = 49^\circ\ 22'\ 30''$ and $D = +\ 18^\circ\ 9'\ 30''$; but which consequently does not exist. The present star is designated once as of the 5th magnitude, and once of the 6th.

655. 103 *Tauri.* The position of this star in the *British Catalogue* is $AR = 72^\circ\ 4'\ 48''$ and $D = +\ 24^\circ\ 11'\ 5''$. There is no observation of it with the mural arc: but in MSS, vol. 25, page 39, it is stated by Flamsteed to have been deduced by means of *distances* from *α Geminorum* to *γ Orionis*: and in the original entry of the observations made with the sextant on Dec. 14th and 17th, 1677, there is a diagram of the three stars forming the triangle there alluded to. See the note to No. 632 above. By repeating the trigonometrical process from the data given by Flamsteed, it will be found that some mistake has been committed, whereby its right ascension and declination have been erroneously deduced: and when the computation is correctly made, this star will be found to be Piazzi IV. 295 = 152 Lacaille. I have given only the *approximate* values, for the epoch of the catalogue. It was observed by Bradley.

658. 1 *Leporis.* This star is designated as of the 9th magnitude in the *British Catalogue*: but I apprehend that this is a typographical error; as it is stated to be the 6th, in the *original* entry.

659. 107 *Tauri.* This star is called l^a, in the *British Catalogue*: but, as there is only one star designated by that letter, in Bayer's map, I have here rejected it, on account of the difference of magnitude. It is also stated to be of the 6th magnitude: but, in the *original* entries, it is once styled *parvula*, and once as of the 8th magnitude. I have therefore altered it.

660. 13 *Orionis.* Observed by Flamsteed on Feb. 2, 1701, at $7^h\ 7^m\ 19^s$; and agreeably to a remark made by him, was probably reduced by Mr. Hodgson; from which the position in the *British Catalogue* is taken: but I cannot discover that those computations now exist. It was also reduced in MSS, vol. 26 C, page 32: but in so doing, an error of $2'$ was made in the right ascension, and $10'$ in the declination. Whence arose the introduction, into the *British Catalogue*, of 12 *Orionis*: whose position, there given, is $AR = 72^\circ\ 39'\ 0''$, and $D = +\ 8^\circ\ 53'\ 10''$; but which does not exist.

662. 66 *Eridani.* The right ascension of this star in the *British Catalogue* is $72^\circ\ 31'\ 50''$. It was observed by Flamsteed on Dec. 9, 1691, at $11^h\ 1^m\ 37^s$: but on comparing the time of its transit with that of other stars on the same day, it is evident that there is an error of $20'$ in the right ascension. And on referring to MSS, vol. 23, page 202, I find that the true value is

there stated as in the present catalogue: so that the error must have been committed by the transcriber or printer.

663. 15 *Orionis.* See the note to No. 652, above.

667. 67 *Eridani* β. In the *British Catalogue* this star is called *h:* but this is an error of the press; as it is denoted by the letter β by Bayer, whom Flamsteed professed to follow: and it is also so marked in the MS copies. I have therefore restored the true reading. I would here remark that in Flamsteed's MSS the letter *h* is with difficulty distinguished from the letter β.

672. — *Tauri.* Observed by Flamsteed on Feb. 13, 1696, at $6^h\ 30^m\ 14^s$: and partly reduced by him in MSS, vol. 23, page 27. It is Piazzi V. 1.

675. 108 *Tauri.* This star is omitted in Flamsteed's map of *Taurus.*

678. 15 *Aurigæ* λ. The observations of Bradley and Piazzi confirm the proper motion of this star in declination: and which, being assumed equal to $-\ 0'',65$, will make the difference from Bradley equal to $-\ 44'',9$ only.

682. — *Aurigæ.* Observed by Flamsteed on Feb. 3, 1693, at $7^h\ 8^m\ 41^s$. It is Piazzi V. 26, erroneously called by him 18 *Aurigæ*, which is the next star, No. 27. It was observed also by Bradley.

685. 18 *Aurigæ.* This star is Piazzi V. 27; and not No. 26, as erroneously stated by him.

691. 17 *Camelopardi.* There is no right ascension annexed to this star in the *British Catalogue.* It was observed by Flamsteed on Jan. 25, 1696, at $7^h\ 52^m$ nearly: but the time is only approximately given. I have therefore inserted an approximate right ascension corresponding with modern observations.

698. — *Orionis.* Observed by Flamsteed on Sept. 19, 1690, at $16^h\ 41^m\ 56^s$. Thus says the *original* MS entry, which is not in the printed *copy.* See the next note. It is Piazzi V. 58.

700. 22 *Orionis o.* In the observation of this star on Sept. 19, 1690, at $16^h\ 42^m\ 10^s$, the reading *per strias cochleæ* differs $2'$ from that *per lineas diagonales:* but the former is the correct reading, and the zenith distance should be increased $2'$: and has been so assumed by Flamsteed in his reduction of this star in MSS, vol. 23, page 185. I would here remark that in the *original* MS entry there is the following note, which has not been printed in the *Historia Cœlestis:* viz., "Comes transit $41^m\ 56^s$ dist: $8'$ ad austrum," which is the star No. 698 of the present catalogue, and which has been observed by Bradley, Piazzi, and Lalande. The present star is not denoted by any letter in the *British Catalogue:* but it is *o Orionis* in Bayer's map. I have therefore retained it.

704. 112 *Tauri* β. This is the same star as 23 *Aurigæ* γ.

709. 25 *Orionis* ψ^1. One of the observations of this star was made by Flamsteed on Jan. 15, 1700, at $8^h\ 44^m\ 26^s$: and in copying out the zenith distance, for the purpose of deducing the declination, he has inadvertently written $50°\ 24'\ 5''$; whereby an error of $20'$ has been made. See MSS, vol. 26 C, page 32. This has given rise to the introduction of 26 *Orionis* into the *British Catalogue;* whose position there given is $\mathcal{R} = 77°\ 10'\ 20''$, and $D = +\ 1°\ 11'\ 25''$; but which consequently does not exist. There is only one star designated by the letter ψ, in Bayer's map, which evidently belongs to 30 *Orionis:* Flamsteed however has annexed that

letter to both the stars; and, in order to prevent any confusion by the alteration, I have here retained it in each case.

710. 27 *Orionis p.* This star is called ρ^2 in the *British Catalogue;* but it is in fact *p* in Bayer's map; and I have therefore restored the correct reading.

714. 115 *Tauri.* This star is marked as of the $7\frac{1}{2}$ magnitude in the *British Catalogue:* which is erroneous, as it is designated as of the 6th, in the only observations where the magnitude is noted. I have therefore altered it.

720. 20 *Camelopardi.* The declination of this star in the *British Catalogue* is + 56° 14′ 50″. It was observed by Flamsteed on Jan. 26, 1696, at $8^h\ 17^m\ 37^s$; and regularly reduced by him in MSS, vol. 23, page 377. But in reducing the result to the epoch of the catalogue he appears to have applied the amount of the precession (= 25″) with a wrong sign. I have therefore deducted 50″ from the above value.

724. 119 *Tauri.* This star is marked as of the 7th magnitude in the *British Catalogue:* but it is nowhere called less than the 6th in the observation book; and once it is stated to be $5\frac{1}{2}$. I have therefore altered it.

727. — *Tauri.* Observed by Flamsteed on Feb. 8, 1691, after $7^h\ 9^m\ 11^s$; and again on Feb. 14, 1696, at $6^h\ 46^m\ 28^s$. The latter observation has been regularly reduced by Flamsteed, in MSS, vol. 23, page 28; and I do not know why it was omitted in the MS list of the stars in *Taurus,* inserted in page 33 of the same book. M. Argelander has shown that it is the double star (distant 12″) which is to be found in Bessel's Zones, 330, 338, and 340, at $5^h\ 21^m\ 48^s$, and + 16° 56′.

730. 23 *Camelopardi.* The right ascension of this star should be increased 7′ 55″,2 in order to correspond with Bradley's observation. In Halley's edition of 1712, the right ascension is 79° 2′ 20″; which is much nearer the value originally deduced by Flamsteed: see MSS, vol. 23, pages 375 and 381. But in the 1st Appendix to that volume (MSS, vol. 26 A) he has revised all the calculations of the circumpolar stars, and made considerable alterations in their positions.

733. 35 *Orionis.* The right ascension of this star in the *British Catalogue* is 79° 7′ 50″: but it is marked as doubtful. In Halley's edition of 1712 it is inserted in the constellation *Taurus;* and the right ascension there given is 79° 4′ 10″: and this is the value which I find annexed to it, in all the MS catalogues, under the constellation *Taurus.* I know not why it was altered unless by an error of the transcriber: and on this supposition I have restored the original and correct value. In the *British Catalogue* it is marked as of the 6th magnitude: but in the *original* entries, it is always designated as the 7th; which I have therefore retained. It was never noted as of the 5th, as erroneously printed.

735. 36 *Orionis v.* This star is said by Flamsteed to have many telescopic stars near it.

736. 24 *Camelopardi.* The right ascension of this star should be increased nearly 12′ in order to correspond with more modern observations. In Halley's edition of 1712, the right ascension is 79° 15′ 0″. See the note to No. 730. Besides the 2 observations of this star indicated by Miss Herschel, there is another on Jan. 25, 1696, after $8^h\ 10^m\ 32^s$; and which I had considered as a different one (No. 98) in my list of Flamsteed's inedited stars. But, on referring

to the *original* MS entry I find written in the margin 4. 48. 10, which indicates the zenith distance of the star, and identifies it with the present one, as conjectured by M. Argelander.

738. 38 *Orionis* n^2. This star is not designated by any letter in the *British Catalogue:* but it is the second of the two stars denoted by the letter *n* in Bayer's map. I have therefore retained it.

743. 122 *Tauri*. This star is omitted in Flamsteed's map of *Taurus*.

747. — *Tauri*. Observed by Flamsteed on Jan. 20, 1710, at $7^h\ 50^m\ 43^s$. It is the same star as that observed by Lalande, in *Hist. Cél.* page 143, at $5^h\ 27^m\ 6^s{,}7$, mag. 6: and M. Argelander says that 7 observations of it at Abo give its position for 1830, $AR = 5^h\ 28^m\ 30^s{,}02$, and $D = +29°\ 6'\ 30''{,}7$.

748. 41 *Orionis* θ^1. This star is called θ^1 by Flamsteed: but there is only one star designated by that letter in Bayer's maps, which is 43 *Orionis:* and it ought therefore to be rejected, on account of its difference in magnitude. But, I apprehend that the magnitude of 43 *Orionis* is erroneously stated in the *British Catalogue;* and I have therefore retained the letter here.

750. 43 *Orionis* θ^2. I cannot find any memorandum of the magnitude of this star, in the original entries. Piazzi says it is of the 6th only. There is only one star designated by the letter θ in Bayer's map; but as it may be doubted whether that letter belongs to this star or to 41 *Orionis*, Flamsteed has annexed it to each, which I have here retained.

752. 45 *Orionis*. This star is called c^2 in the *British Catalogue:* but, as there is only one star designated by that letter in Bayer's map, I have here rejected it, on account of the difference of magnitude. In the *British Catalogue*, it is also stated to be of the 5th magnitude: but on referring to the *original* entry of the observation of Jan. 8, 1694, at $9^h\ 21^m\ 9^s$, I find that the printed copy is erroneous, and that the star should be designated as of the 7th magnitude. I have therefore altered it.

754. 125 *Tauri*. This star is marked of the 3rd magnitude in the *British Catalogue:* but in the *original* observations it is once called of the 5th, and once of the 6th magnitude. I have therefore taken the mean.

756. 27 *Camelopardi*. The declination of this star in the *British Catalogue* is $+56°\ 52'\ 35''$: it was observed by Flamsteed on Jan. 22, 1696, after $8^h\ 26^m\ 41^s$; and the north polar distance regularly reduced by him, as appears by MSS, vol. 23, page 377: but, in copying it out for the catalogue, an error of $30'$ has been made, which I have here corrected. I cannot however find any star that will correspond with the observation: but M. Argelander supposes that it may be 24 *Camelopardi*, which had been observed on the same evening; and, continuing in the field of the telescope, was thus inadvertently re-observed and taken for *another* star.

759. — *Columbæ*. Observed by Flamsteed on Feb. 11, 1691, at $7^h\ 5^m\ 15^s$. M. Argelander says that it is to be found in Lacaille's *Cœlum Australe*, page 121, and is the 8th star in the 6th column, which enters at $5^h\ 21^m\ 25^s$ and exits at $5^h\ 26^m\ 8^s$. See the note to No. 776 of this catalogue.

765. — *Orionis*. Observed by Flamsteed on Jan. 25, 1713, at $8^h\ 9^m\ 33^s$. It is Piazzi V. 192.

766. 127 *Tauri*. This star is marked as of the 6th magnitude in the *British Catalogue:* but nowhere (either in the printed or manuscript observations) is it denoted otherwise than as of the 7th: which I have therefore adopted.

769. — *Columbæ.* Observed by Flamsteed on Feb. 11, 1691, at $7^h\ 8^m\ 58^s$. It is Piazzi V. 183, and No. 427 in Lacaille's *Cœlum Australe.* See the note to No. 776 of this catalogue.

774. 29 *Aurigæ* τ. This star is marked as of the 5th magnitude in the *British Catalogue*: but in the only *original* entry where the magnitude is recorded, it is called of the 6th; which I have therefore adopted.

775. — *Columbæ.* Observed by Flamsteed on Feb. 11, 1691, after $7^h\ 11^m\ 46^s$. It is Piazzi V. 197, and No. 433 in the catalogue in Lacaille's *Cœlum Australe.* See the next note.

776. — *Columbæ* α. Observed by Flamsteed on Feb. 11, 1691, at $7^h\ 11^m\ 46^s$, where it is designated by the letter α. It is Piazzi V. 196, and No. 434 in Lacaille's *Cœlum Australe.* It is inserted in Halley's edition of 1712, in the constellation of *Canis Major*, and where its position is $AR = 82^\circ\ 7'\ 30''$, and $D = -34^\circ\ 15'\ 10''$. The 7 stars in the constellation *Columba* which are given in this catalogue (viz. No. 759, 769, 775, 776, 788, 815, and 820) have all been regularly reduced by Flamsteed in MSS, vol. 23, page 191: and I have assumed the declinations which he has there deduced. The right ascensions have been determined by taking the differences between their transit and that of ζ *Canis Majoris*, which was the determining star assumed by him in these reductions.

778. 30 *Aurigæ* ξ. This is the same star as 32 *Camelopardi*: the position of which in the *British Catalogue* is $AR = 82^\circ\ 16'\ 0''$, and $D = +55^\circ\ 34'\ 30''$: and which is correctly stated to be of the 5th magnitude.

782. — *Orionis.* Observed by Flamsteed on Feb. 10, 1691, after $7^h\ 16^m\ 29^s{,}5$. It is Piazzi V. 222 = 218 Mayer.

784. 132 *Tauri.* This star is marked as of the 4th magnitude, in the *British Catalogue*: but, in the *original* entries, it is once only designated as the 4th, and twice as the 6th: I have therefore taken the mean.

788. — *Columbæ.* Observed by Flamsteed on Feb. 11, 1691, at $7^h\ 14^m\ 30^s$. It is Piazzi V. 211, and No. 440 in the catalogue in Lacaille's *Cœlum Australe.* See the note to No. 776 above.

791. 134 *Tauri.* In the observation of this star in the *Historia Cœlestis*, vol. 2, page 292, it is said to have a bright companion preceding it. Probably one of the stars given in Lalande's *Hist. Cél.* page 311.

792. — *Camelopardi.* Observed by Flamsteed on Jan. 22, 1696, at $8^h\ 38^m\ 27^s$. The zenith distance is not given: but it is evidently the star observed by Lalande in *Mem. de l'Acad.* for 1790, page 377, at $5^h\ 41^m\ 4^s{,}5$, mag. 8; and which passed $15'\ 22''$ to the north of 33 *Camelopardi.*

797. 34 *Camelopardi.* The declination of this star in the *British Catalogue* is $+55^\circ\ 15'\ 0''$. It was observed by Flamsteed on Jan. 22, 1696, at $8^h\ 40^m\ 10^s$; and regularly reduced by him in MSS, vol. 23, page 377. But in reducing it to the epoch of 1690, he appears to have applied the precession ($= 25''$) with a wrong sign. I have therefore deducted $50''$ from the above value.

799. — *Aurigæ.* Observed by Flamsteed on Jan. 20, 1696, at $8^h\ 48^m\ 9^s$, and regularly reduced by him in MSS, vol. 23, page 375. I know not why it was omitted in the *British Catalogue.* It is probably No. 854 in Bessel's catalogue of Bradley's stars.

800. 137 *Tauri.* In the reduction of the observation of this star, which was made on Feb. 8, 1691, at $7^h\ 29^m\ 52^s$, Flamsteed has made an error of 13′ in the declination: whereby he has considered it to be another star, 138 *Tauri*, whose position in the *British Catalogue* is $\mathcal{R} = 83°\ 41'\ 0''$, and $D = +\ 13°\ 49'\ 35''$, but which does not exist. It may be proper to state here that in that observation the zenith distance *per strias cochleæ* is the correct reading, which differs 10′ from that *per lineas diagonales* assumed by Flamsteed in his reduction; and that the further error of 3′ is made in the arithmetical computation. See MSS, vol. 23, page 27: and compare it with the deduced zenith distance in the original MS entry.

808. — *Aurigæ.* Observed by Flamsteed on Feb. 20, 1692, at $6^h\ 47^m\ 4^s$; and regularly reduced by him in MSS, vol. 23, page 295. I know not why it was omitted in the *British Catalogue.* I cannot find it in any catalogue; but M. Argelander says that he has observed it 6 times, and that its position for 1830 is $\mathcal{R} = 5^h\ 47^m\ 54^s{,}00$, and $D = +\ 44°\ 34'\ 11''{,}6$; mag. $6\frac{1}{2}$.

813. — *Orionis.* Observed by Flamsteed on Feb. 8, 1691, at $7^h\ 34^m\ 37^s$, but the time is doubtful. It was regularly reduced by Flamsteed in MSS, vol. 23, page 187, but omitted in his list of stars in *Orion* in page 206 of the same volume. It is the star observed by Lalande in the *Hist. Cél.* page 206, at $5^h\ 45^m\ 14^s{,}5$, mag. $7\frac{1}{2}$.

814. 35 *Camelopardi.* The right ascension of this star is not given in the *British Catalogue.* It was observed on Jan. 22, 1696, the second after $8^h\ 40^m\ 10^s$: but the time is not stated. I have given the approximate value corresponding with modern observations.

815. — *Columbæ* β. Observed by Flamsteed on Feb. 11, 1691, at $7^h\ 23^m\ 23^s{,}5$. It is Piazzi V. 267 (where it is designated by the letter β, which I have here retained), and No. 452 in the catalogue in Lacaille's *Cœlum Australe.* It is inserted in Halley's edition of 1712, where its position is $\mathcal{R} = 85°\ 2'\ 0''$, and $D = -\ 35°\ 53'\ 30''$. See the note to No. 776 above.

816. 38 *Aurigæ.* Observed by Flamsteed on Feb. 14, 1693, at $7^h\ 8^m\ 54^s$. The declination in the *British Catalogue* is $+\ 42°\ 57'\ 10''$: but this is too great by 5′, if compared with Bradley's observations. On examining the original MS entry, I find that the minutes in the zenith distance were originally entered 39, but that Flamsteed has altered it with the pen to 34, as in the printed copy. There can be no doubt however that the original entry is the correct one. See also the MS *copy* of the same observation; where a similar alteration has been made. Flamsteed remarks that this star has another which precedes it, 30′ distant: but it is not stated whether that distance is measured in time or in space; in right ascension or declination. Probably it is the star observed by Lalande in *Hist. Cél.* page 142, at $5^h\ 48^m\ 7^s{,}5$, mag. 9.

817. — *Orionis.* Observed by Flamsteed on Feb. 8, 1691, at $7^h\ 36^m\ 32^s$; and again in zenith distance on Feb. 13, 1696, the third after $7^h\ 11^m\ 0^s$. The former observation was regularly reduced by Flamsteed in MSS, vol. 23, page 187; but omitted in the list of stars in *Orion* in page 206 of the same volume. It has been observed twice by Bessel as of the 7th magnitude; Zone 56, at $5^h\ 48^m\ 54^s{,}30$, and 146, at $5^h\ 48^m\ 55^s{,}44$.

818. 140 *Tauri.* This star is marked as of the 6th magnitude: but in the *original* entry of the observation it is designated as $7\frac{1}{2}$; which I have therefore adopted.

820. — *Columbæ* λ. Observed by Flamsteed on Feb. 11, 1691, at $7^h\ 25^m\ 9^s$. It is Piazzi V. 276, and No. 453 in the catalogue in Lacaille's *Cœlum Australe.* See the note to No. 776 above.

827. 39 *Camelopardi.* There is no right ascension annexed to this star in the *British Catalogue.* It was observed by Flamsteed on Jan. 20, 1696, after $8^h\ 57^m\ 35^s$: but the time is not stated. I have given the approximate right ascension corresponding with modern observations.

828. — *Aurigæ.* Observed by Flamsteed on Feb. 20, 1692, at $6^h\ 52^m\ 40^s$. It is the star observed by Lalande in *Hist. Cél.* page 208, at $5^h\ 50^m\ 40^s{,}8$, mag. $7\frac{1}{2}$.

830. — *Aurigæ.* Observed by Flamsteed on Feb. 20, 1692, at $6^h\ 53^m\ 9^s$. It is Piazzi V. 301; and has been observed also by Lalande in *Hist. Cél.* page 208, at $5^h\ 51^m\ 8^s{,}5$, mag. 7.

833. 64 *Orionis* χ^3. The right ascension of this star in the *British Catalogue* is $87°\ 1'\ 40''$. It was observed by Flamsteed on Jan. 6, 1690, at $9^h\ 45^m\ 7^s$; again on Feb. 7, 1691, at $7^h\ 53^m\ 5^s{,}5$; and again on Feb. 13, 1696, at $7^h\ 21^m\ 21^s$. It may be proper to remark that the time of the second observation here mentioned should be $7^h\ 50^m\ 5^s{,}5$. This correction has been made by Miss Herschel; and on referring to the *original* MS entry I find that the *original entry* has been altered, and that 53 is placed in the margin. It is evident however that the time of the transit of this star, and that immediately following it on the same day, are both too great by 3^m. The comparisons of all the above observations show that Flamsteed's right ascension of this star must be diminished $45'$; and it will then accord with the value in Halley's edition of 1712. It is Piazzi V. 304. It thus becomes χ^3 *Orionis:* and the star called χ^3 by Flamsteed is now χ^4.

834. — *Camelopardi.* Observed by Flamsteed on Jan. 25, 1696, at $8^h\ 35^m\ 26^s$. It is Piazzi V. 335 = (22 Hev.) *Camelopardi.*

837. 1 *Geminorum.* This star has the letter H affixed to it in the *British Catalogue:* but it does not form any part of the constellation *Gemini*, in Bayer's map; and the letter is merely used there as a note of reference to an unformed star. I have therefore rejected it.

838. 62 *Orionis* χ^4. Observed by Flamsteed on Jan. 6, 1690, at $9^h\ 45^m\ 32^s$; again on Feb. 7, 1691, at $7^h\ 53^m\ 30^s{,}5$; and again on Feb. 13, 1696, at $7^h\ 21^m\ 43^s{,}5$. The second observation has been reduced by Flamsteed; but in consequence of the error of 3^m alluded to in the note to No. 833 of this catalogue, he has considered it as a different star from the present one, differing $45'$ in right ascension from it: viz., 65 *Orionis*, whose position in the *British Catalogue* is $\mathrm{AR} = 87°\ 8'\ 15''$, and $\mathrm{D} = +\ 20°\ 5'\ 50''$, but which does not exist. The present star is called χ^3 by Flamsteed: but, in consequence of the error just alluded to, and those noticed in the note to No. 833, it becomes χ^4. It may be proper here to state that there are only two stars denoted by the letter χ in Bayer's map: but as it may be doubted which of the two (54 or 57 *Orionis*) formed the first; and which of the two (62 or 64 *Orionis*) formed the second of such stars, I have, after the example of Flamsteed, designated the whole of them by that letter.

840. 2 *Geminorum.* This star is marked as of the 8th magnitude in the *British Catalogue:* but it is only once so called in the observation book; whilst it is twice called of the 7th, which I have here adopted.

843. 63 *Orionis.* The right ascension of this star in the *British Catalogue* is $86°\ 31'\ 30''$; which is erroneous. It was observed by Flamsteed on Feb. 6, 1691, at $7^h\ 56^m\ 35^s$, and regularly and correctly reduced by him, as appears by MSS, vol. 23, page 187. But it is not inserted in his list of stars in *Orion*, in page 206 of the same volume; although the incorrect value is

there inserted, as also in Halley's edition of 1712. I cannot discover how this singular mistake could have happened: but I have rectified it in the present catalogue. It is Piazzi V. 321.

847. 3 *Geminorum.* This star is marked as of the 8th magnitude in the *British Catalogue:* but it is only once so called in the observation book; whilst it is twice called of the 7th, and twice of the 6th. I have therefore taken the mean of the two latter.

855. 3 *Lyncis.* The right ascension of this star must be reduced above 5′ in order to correspond with modern observations. There is evidently an error of 2′ in the reduction, as it followed 1 *Lyncis* 1° 1′ 15″ according to Flamsteed's own computation in MSS, vol. 23, page 377.

864. — *Monocerotis.* Observed by Flamsteed on Feb. 2, 1701, after $8^h\ 12^m\ 56$; but the exact time of transit is not given. The reading *per strias cochleæ* differs 21′ from that *per lineas diagonales:* but as Flamsteed has assumed the latter, in his reduction of this star, in MSS, vol. 23, page 220, I have also adopted it. If the former be assumed, the declination would be — 6° 4′ 40″. On neither supposition however can I find any star that will accord with the observation. M. Argelander has suggested that if we suppose an error in the register, and that the zenith distance were 58° 7′ 0″ [1317.42], the observation would agree with the star observed by Lalande in *Hist. Cél.* page 264, at $6^h\ 1^m\ 43^s{,}3$, mag. 6. But there is nothing in the *original* MS entry to warrant this alteration.

867. 45 *Aurigæ.* There is only one observation of this star, by Flamsteed, which took place on Feb. 14, 1693, at $7^h\ 24^m\ 40^s$: but although the time is marked as doubtful, it has been assumed as correct, by Flamsteed, in his computation of the right ascension in MSS, vol. 23, page 299. In fact, it should be diminished about 15^s (=3′ 45″) in order to accord with modern observations.

872. — *Lyncis.* Observed by Flamsteed on Jan. 20, 1696, at $9^h\ 12^m\ 27^s$; and again on Jan. 22, at $9^h\ 4^m\ 46^s$; both of which are regularly reduced by him, as a star in *Ursa Major*, in MSS, vol. 23, pages 375 and 377; and are inserted in Halley's edition of 1712, in that constellation. It is Piazzi VI. 55.

873. — *Lyncis.* Observed by Flamsteed on Jan. 20, 1696, at $9^h\ 12^m\ 54^s$ (which probably ought to be $9^h\ 12^m\ 44^s$) and again on Jan. 22, 1696, at $9^h\ 5^m\ 1^s$: both of which are regularly reduced by him as a star in *Ursa Major*, in MSS, vol. 23, pages 375 and 377; and are inserted in Halley's edition of 1712. It is Piazzi VI. 57.

874. 74 *Orionis* k^2. There is only one star designated by the letter *k*, in Bayer's map: but, as there may be a doubt whether it belongs to 73 *Orionis*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

876. 5 *Lyncis.* In the observation of this star on Jan. 13, 1696, at $9^h\ 41^m\ 50^s$, it is said to have some companions preceding it.

879. — *Lyncis.* Observed by Flamsteed on Jan. 20, 1696, after $9^h\ 13^m\ 42^s$: but the time is not given. M. Argelander says that it is No. 10 *Lyncis* in Bode's great catalogue; and that it is to be found in the *Mem. de l'Acad.* for 1790, page 377, at $6^h\ 9^m\ 11^s{,}8$, mag. 6.

887. 47 *Aurigæ.* Observed by Flamsteed on Feb. 14, 1693, at $7^h\ 34^m\ 46^s$: and he has the following remark annexed to the observation, viz. "Altera hanc præcedit 40′ ad boream distans, "duæ sequuntur 30′ ab hac, et ab invicem dissitæ." M. Argelander thinks that the first star

here mentioned is the one observed by Lalande in *Hist. Cél.* page 377, at $6^h\ 12^m\ 24^s$: and that the last two are in the same work, page 383, at $6^h\ 17^m\ 12^s,5$ and $6^h\ 17^m\ 14^s$ respectively.

890. 76 *Orionis.* This star does not exist, neither can I find any observation of it by Flamsteed. Perhaps it arose from some inaccurate computation either of the declination of 8 *Monocerotis*, or of the right ascension of 63 *Orionis.*

893. 7 *Lyncis.* Observed by Flamsteed on Jan. 20, 1696, at $9^h\ 22^m\ 31^s$: the time is marked as doubtful, but it is not far from the truth, if at all. The declination given in the *British Catalogue* is $+56°\,31'\,20''$: but the zenith distance *per strias cochleæ* differs 1° from that *per lineas diagonales.* Flamsteed has assumed the latter as the correct reading, in his reduction of the star in MSS, vol. 23, page 377: but in the *copy* of the entry of the observation (MSS, vol. 16, page 95) there is a marginal note that implies that he had discovered his error. I have therefore assumed the observed zenith distance as $3°\,57'\,0''$, and consequently deducted 1° from the declination as given by Flamsteed. The star is then Piazzi VI. 115, and not VI. 123 as supposed by Piazzi and Bessel.

894. 2 *Canis Majoris* β. The declination of this star in the *British Catalogue* is $-17°\,49'\,10''$. There are several observations of it by Flamsteed: but those, from which the declination has been deduced, are Feb. 14, 1690, after $5^h\ 56^m\ 9^s$, and March 4, 1693, at $6^h\ 29^m\ 5^s$; both of which are reduced in MSS, vol. 23, pages 219 and 221; and the value there deduced is $-17°\,50'\,10''$: which is also the value in Halley's edition of 1712. Why it was afterwards altered I am not able to ascertain: but as there appears to be no good reason for it, I have restored the original value.

895. 15 *Geminorum.* In the reduction of one of the observations of this star, viz., that made on Feb. 7, 1690, at $7^h\ 51^m\ 46^s$, Flamsteed has made an error of 1^m in right ascension, and of 2° in declination. See MSS, vol. 23, page 37. Whereby he considered it to be another star which is inserted in Halley's edition of 1712, as having $\mathcal{R} = 92°\,4'\,0''$, and $D = +22°\,55'\,45''$. There is another observation of this star on Feb. 10, 1696, at $7^h\ 57^m\ 40^s$; which is supposed by Miss Herschel to refer to another star, 17 *Geminorum*, whose position in the *British Catalogue* is $\mathcal{R} = 92°\,35'\,0''$, and $D = +20°\,55'\,45''$; but which does not exist: its introduction being owing to an error of 1^m in the time of transit. In the first MS catalogue (in MSS, vol. 25) this star is struck out with the pen, as if Flamsteed had discovered his error: but it was afterwards inadvertently re-introduced into another MS catalogue.

897. — *Lyncis.* Observed by Flamsteed on Feb. 16, 1704, at $7^h\ 29^m\ 54^s$. M. Argelander remarks that it is to be found in the *Mem. de l'Acad.* for 1790, page 379, at $6^h\ 18^m\ 4^s,2$, mag. 6.

899. 9 *Lyncis.* Observed by Flamsteed on Jan. 20, 1696, at $9^h\ 23^m\ 41^s$; and has since been observed by Bradley and Piazzi, who both call it 7 *Lyncis;* which is however another star, No. 893 of this catalogue.

902. 3 *Canis Majoris.* The right ascension of this star in the *British Catalogue* is $92°\,45'\,40''$. It was observed by Flamsteed on Feb. 11, 1691, at $7^h\ 53^m\ 58^s,5$; and again on Feb. 17, 1702, at $7^h\ 28^m\ 55^s$. The former observation has been reduced by him in MSS, vol. 23, page 219: and appears to be the one from which the $\mathcal{R}$ in the *British Catalogue* is deduced. But, it will be seen that Flamsteed has made an error of 10^s in the steps of the process: and I have therefore deducted $2'\,30''$ from the above value. The other observation has been reduced in

MSS, vol. 26 A, page 26: but in extracting the time of transit, Flamsteed has made an error of 1^m. It is designated by the letter λ in the *British Catalogue;* but it is not the star so called by Bayer; nor did Flamsteed observe that star. I have therefore rejected it here.

904. 77 *Orionis.* Observed by Flamsteed on Jan. 7, 1694, at $10^h\ 15^m\ 29^s$. In the *British Catalogue* the declination is $+\ 0°\ 23'\ 0''$: but on comparing it with the zenith distance of the other stars observed on that day, it will be seen that an error of $4'$ has been made in the reduction, which I have added to Flamsteed's value. It may be proper here to state that the corrected zenith distance of 78 *Orionis* (erroneously placed *before* it in the observations of that day in the *Historia Cœlestis*) should be $51°\ 35'\ 10''$, instead of $51°\ 34'\ 10''$.

906. 9 *Monocerotis.* This star is marked as of the 5th magnitude in the *British Catalogue:* which is erroneous, as in the *original* entry it is designated as the 6th, which I have here retained.

907. — *Geminorum.* Observed by Flamsteed on Feb. 19, 1696, after $7^h\ 22^m\ 8^s$: and has since been observed by Lalande in *Hist. Cél.* page 272, at $6^h\ 17^m\ 46^s{,}5$, mag. $7\frac{1}{2}$.

911. 11 *Monocerotis.* This star is marked as of the 5th magnitude in the *British Catalogue;* which does not correspond with the *original* entry, where it is designated as the 4th: and which I have consequently retained. Piazzi, however, says that it is only $6\frac{1}{2}$.

913. 21 *Geminorum.* This star does not exist, in the position here given. The observation, from which it has been deduced by Flamsteed, was made on Feb. 19, 1696, at $7^h\ 26^m\ 50^s$; as may be seen by MSS, vol. 23, page 43. Most of the modern astronomers have supposed it to be Piazzi VI. 135; or the second of the two stars forming the double star 20 *Geminorum.* But this is on the assumption that Flamsteed has made an error of 1^m in recording the time; and that $7^h\ 26^m\ 50^s$ ought to be read $7^h\ 25^m\ 50^s$. Although this is very probable, (a similar mistake having *certainly* been twice committed on the same day with two previous stars,) yet there is nothing in the original MS entry to warrant the alteration. It is the only solution, however, of the difficulty.

914. — *Monocerotis.* Observed by Flamsteed on Feb. 17, 1690, after $7^h\ 32^m\ 49^s{,}5$. M. Argelander thinks that it is Herschel VII. 2; and observed by Bessel in Zone 61, at $6^h\ 22^m\ 10^s{,}33$, mag. 8.

916. 12 *Monocerotis.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but in the *original* entries, it is twice designated as $6\frac{1}{2}$, and once as *parvula.* I have therefore altered it.

917. 13 *Monocerotis.* This star is marked as of the 4th magnitude, in the *British Catalogue:* but, in the *original* entries, it is once designated as the 5th, and once as $4\frac{1}{2}$: I have therefore altered it.

922. — *Lyncis.* Observed by Flamsteed on Feb. 16, 1704, at $7^h\ 38^m\ 2^s$. It is Piazzi V. 174; and has also been observed by Bradley.

923. 23 *Geminorum.* Observed by Flamsteed on Feb. 7, 1690, at $8^h\ 0^m\ 34^s$. The declination in the *British Catalogue* is $+\ 17°\ 10'\ 0''$: it should be remarked, however, that the zenith distance *per strias cochleæ* differs $10'$ from that *per lineas diagonales.* Flamsteed has deduced the value from the latter; but the former is the correct reading: and I have accordingly deducted $10'$ from Flamsteed's value. I would also remark that Flamsteed states that at 15^s preceding the transit of this star, two other stars passed, one above and the other below 23

Geminorum. One of these was probably Piazzi VI. 157; and the other, M. Argelander believes to be a star in Harding's chart, probably observed by him. In the *British Catalogue* the present star is said to be of the *fifth* magnitude: but in the observation it is stated to be of the *seventh;* which accords more nearly with modern observations, and which I have therefore retained.

926. 42 *Camelopardi.* The right ascension of this star ought to be diminished nearly 4′, in order to accord with modern observations.

927. 12 *Lyncis.* This star is marked as of the 7th magnitude, in the *British Catalogue:* but in the original entries, it is twice designated as the 5th, and twice as the 6th; I have therefore taken the mean.

931. 43 *Camelopardi.* The right ascension of this star ought to be diminished about 7′, in order to accord with modern observations.

933. 13 *Lyncis.* The right ascension of this star ought to be diminished about 15′, in order to accord with modern observations. Probably an error of 1^m in time has been made in the time of transit, on Feb. 16, 1704, at $7^h\ 41^m\ 51^s$; which M. Argelander thinks should be $7^h\ 41^m\ 1^s$.

938. 15 *Monocerotis.* This star is marked as of the 4th magnitude in the *British Catalogue:* which is erroneous. Out of the 3 observations, it is once called of the 5th and twice of the 5½ magnitude, in the *original* entries. I have therefore adopted the latter.

939. 57 *Aurigæ.* The right ascension of this star in the *British Catalogue* is 95° 56′ 30″. It was observed on Feb. 14, 1693, at $7^h\ 51^m\ 53^s$: but in copying out these figures into the computation book, MSS, vol. 23, page 299, Flamsteed has inadvertently assumed the time $7^h\ 51^m\ 33^s$; whereby an error of 20^s has been committed in the deduced right ascension. I have therefore added 5′ to his value: and the right ascension will then accord with that given by Halley in his edition of 1712.

942. 56 *Aurigæ.* The right ascension of this star, in the *British Catalogue*, is 95° 50′ 0″. The only observation of the time of its transit, was made on Feb. 17, 1693, at $7^h\ 40^m\ 47^s$; which is copied out for computation in MSS, vol. 23, page 299. But in one of the steps of the process Flamsteed has made an error of 1^m: I have therefore added 15′ to the right ascension as given by him. It will then accord with the value in Halley's edition of 1712.

944. 28 *Geminorum.* The star 29 *Geminorum* does not exist. The observation from which the false position was deduced, was made on Feb. 12, 1696, at $8^h\ 5^m\ 19^s$: but in the process of computation in MSS, vol. 23, page 43, an error of $+\ 20^s$ has been made in the resulting value. When this is corrected, the star will agree with 28 *Geminorum.* Miss Herschel has stated (page 134) that there are 2 other observations that will agree with 29 *Geminorum:* viz., on Feb. 10, 1706, at $7^h\ 56^m\ 21^s$; and on Feb. 18, 1707, at $7^h\ 40^m\ 35^s$. The first of these will differ several minutes according as we determine the position of the star from γ or from ϑ *Geminorum:* and indeed the whole of the observations at the latter end of that day, are so anomalous that no dependence can be placed on them. With respect to the second of the above observations, it will be seen that it is nearly as discordant, since the difference in the time of transit between γ *Geminorum* and this star is 7^s less than in the former observation. I cannot refer to the *original* MSS observations of these dates, as they are unfortunately lost: and it is

remarkable that in the MS *copy* (MSS, vol. 17) the observations of Feb. 18 and 19, 1707, are wholly omitted.

946. 30 *Geminorum.* This star is called ξ^1 by Flamsteed: but Bayer has only one star designated by that letter in his map, which is 31 *Geminorum.* I have therefore rejected it here on account of the difference in magnitude.

948. 32 *Geminorum.* This star is designated as *parvula* in the observation book: and modern observations show it to be of the 8th magnitude only.

953. 9 *Canis Majoris* α. The observations of Bradley and Piazzi confirm the proper motion of this star in declination: and which being assumed equal to — 1″,25, will make the difference from Bradley's value equal to — 10″,0 only.

954. 59 *Aurigæ.* Flamsteed remarks, in his first observation of this star, *Historia Cœlestis*, page 190, that another star follows it, "*Æque clara 6′ ab hac.*" The star, here alluded to, is probably 61 *Aurigæ:* another observation of which occurs in the same page.

955. 18 *Monocerotis.* This star is marked as of the 4th magnitude, in the *British Catalogue:* but, in the *original* entries, it is once designated as the 5th, and once as $5\frac{1}{2}$; I have therefore altered it.

963. — *Geminorum.* Observed by Flamsteed on Feb. 7, 1690, at $8^h\ 15^m$; and again on Feb. 7, 1691, after $8^h\ 37^m\ 48^s$. It is the star observed by Lalande in *Hist. Cél.* page 316, at $6^h\ 39^m\ 55^s$, mag. 6.

964. 10 *Canis Majoris.* The right ascension of this star should be diminished above 8′ in order to correspond with modern observations. It was observed by Flamsteed on Feb. 17, 1702, at $7^h\ 50^m\ 48^s$; and its reduction will be found in MSS, vol. 26 A, page 26. It will be there seen that the determining star for that day's observation was ε *Canis Majoris;* consequently the present star ought to have preceded this determining star 3° 29′ 45″, according to the rate of Flamsteed's clock. But such is not the case in the catalogue: nor will the observation, thus deduced, agree with the present state of the heavens. There is evidently some confusion in the registering of the time of transit, or in the subsequent computation; as this star appears to have preceded 12 *Canis Majoris* by $1^m\ 26^s = 22′\ 15″$, according to Flamsteed's assumed rate in the MS volume above mentioned. If the position of this star be deduced from λ *Canis Majoris*, observed on the same day, its right ascension would be 98° 13′ 0″; thus agreeing more nearly with the present state of the heavens: and 13 *Canis Majoris* would be 99° 37′ 0″. But in such case the intermediate star 12 *Canis Majoris* would be 98° 35′ 0″; which differs 10′ from its value in the present catalogue. We have therefore only a choice of difficulties; and I have not ventured on any alteration. The present star is called κ^1 by Flamsteed: but there is only one star designated by that letter in Bayer's map, which is 13 *Canis Majoris.* I have therefore rejected it here, on account of the difference of magnitude.

965. 12 *Canis Majoris.* In the observation of this star on Feb. 16, 1702, at $7^h\ 56^m\ 6^s$, Flamsteed remarks that it has a small cluster of stars (or nebula) preceding it. In the *British Catalogue*, this star is denoted by the letter ρ: but, as there is no such letter in Bayer's map, I have rejected it here.

969. 13 *Canis Majoris* κ. The right ascension of this star must be increased 7′ 4″,3 in order to

correspond with Bradley's observations. In Halley's edition of 1712 the right ascension is 99° 35′ 30″: which is much nearer the correct value. See the note to No. 964 above.

971. — *Lyncis.* Observed by Flamsteed on Jan. 25, 1696, at $9^h\ 28^m\ 31^s$; and regularly reduced by him in MSS, vol. 23, page 381. It is inserted in Halley's edition of 1712; and I know not why it was afterwards omitted. It is Piazzi VI. 293; who says it is of the $7\frac{1}{2}$ magnitude.

974. 15 *Canis Majoris.* The declination of this star in the *British Catalogue* is — 19° 53′ 30″. But it is evident, from a computation of the values of the zenith distances observed, that an error of 1′ has been made in the declination. Halley's edition of 1712 makes the declination — 19° 52′ 50″: and I suspect that this is nearer to the true value. Why it was afterwards altered I cannot ascertain. I have however ventured to make the alteration above-mentioned. In the *British Catalogue*, this star, as well as 17 and 19 *Canis Majoris*, are designated by the letter π: but, as there is no such letter in Bayer's map, I have rejected it in each case.

977. 17 *Canis Majoris.* The declination of this star in the *British Catalogue* is —20° 3′ 30″. But it is evident, on a computation of the values of the zenith distances observed, that an error of 1′ has been made in the declination. Halley's edition of 1712 makes the declination —20° 2′ 30″: and this I suspect to be the true value, and have accordingly restored it. Why it was afterwards altered I cannot ascertain; unless it be a typographical error. This star is called π^3 in the *British Catalogue:* but see the note to No. 974 above.

978. 18 *Canis Majoris* μ. The magnitude of this star is not noted in the observation book: and modern observations show it to be of the $5\frac{1}{2}$ magnitude only.

979. 17 *Lyncis.* Miss Herschel has supposed that the observation made by Flamsteed on Jan. 25, 1696, after $9^h\ 28^m\ 31^s$, refers to a different star from that made on Feb. 16, 1704, at $8^h\ 2^m\ 28^s$. But, I think there can be no doubt that they both relate to the same star.

980. 19 *Canis Majoris.* The declination of this star in the *British Catalogue* is —19° 47′ 20″. But it is evident, on a computation of the values of the zenith distances observed, that an error of 1′ has been made in the computation. Halley's edition of 1712 makes the declination —19° 46′ 30″: and I suspect that this is nearer to its true value. Why it was afterwards altered I cannot ascertain; but I have ventured to make the alteration above-mentioned. This star is called π^5 in the *British Catalogue:* but see the note to No. 974 above.

981. 20 *Canis Majoris* ι. The declination of this star in the *British Catalogue* is — 16° 42′ 10″. But it is evident, on a computation of the values of the zenith distances observed, that an error of 1′ has been made in the computation. Halley's edition of 1712 makes the declination — 16° 41′ 5″: and I suspect this is nearer to its true value. Why it was afterwards altered I cannot ascertain: but I have ventured to make the alteration above-mentioned.

982. 41 *Geminorum.* The declination of this star in the *British Catalogue* is + 16° 23′ 5″; which is probably deduced from the observation of Feb. 8, 1701, at $8^h\ 35^m\ 38^s$. But the zenith distance *per strias cochleæ* differs 4′ from that *per lineas diagonales.* Flamsteed has taken the latter as the true reading: but a comparison of the observation made on Sept. 29, 1704, at $17^h\ 33^m\ 54^s$, and the result of modern observations, show that the former is the correct reading. I have therefore added 4′ to Flamsteed's value.

984. — *Geminorum.* Observed by Flamsteed on Feb. 19, 1696, after $7^h\ 48^m\ 36^s{,}5$. Owing to an

error in Miss Herschel's catalogue, the declination of this star, in my list of Flamsteed's inedited stars, was erroneous. It is the star observed by Lalande in *Hist. Cél.* page 210, at $6^h\ 49^m\ 56^s{,}5$, mag. $7\frac{1}{2}$.

987. — *Geminorum.* Observed by Flamsteed on Feb. 17, 1690, at $8^h\ 11^m\ 44^s$; again on Feb. 14, 1696, after $8^h\ 17^m\ 39^s$; and again on Feb. 19, 1696, at $7^h\ 57^m\ 52^s$: but the first only has been reduced by him, which gives the position as in the present catalogue. It is the star observed by Lalande in *Hist. Cél.* page 312, at $6^h\ 52^m\ 37^s$, mag. 6; and in Bessel's *Zone* 148, at $6^h\ 53^m\ 31^s{,}85$, mag. 7.

988. 44 *Geminorum.* One of the observations of this star, viz. that made on Feb. 22, 1705, at $7^h\ 44^m\ 51^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 34: and in deducing the right ascension he has made an unaccountable mistake of upwards of 21°. Whence has arisen the introduction into the *British Catalogue* of 124 *Tauri*: whose position, as there given, is $\text{Æ} = 80°\ 3'\ 0''$, and $D = +\ 23°\ 2'\ 35''$; but which consequently does not exist. The present star is called ω^3 by Flamsteed: but there is only one star designated by that letter in Bayer's map, which is 42 *Geminorum*; I have therefore rejected it here.

989. — *Canis Majoris.* Observed by Flamsteed on Oct. 5, 1691, at $17^h\ 24^m\ 54^s$. It is Piazzi VI. 307.

991. — *Lyncis.* Observed by Flamsteed on Jan. 25, 1696, at $9^h\ 38^m\ 10^s$, and regularly reduced by him, in MSS, vol. 23, page 381; whence the position in the present catalogue is obtained. It is Piazzi VI. 339.

994. — *Lyncis.* Observed by Flamsteed on March 4, 1703, at $7^h\ 19^m\ 0^s$. The book containing the *original* MS entry is lost; but in the MS *copy* (MSS, vol. 16) the observation is marked as doubtful, although not printed so in the *Historia Cœlestis*. It is probably Piazzi VI. 331: but, if so, the right ascension in the present catalogue must be reduced about 20′, in order to correspond therewith. M. Argelander remarks that there is no proper motion to account for this difference; as his observations of Piazzi's star give its position for 1830, $\text{Æ} = 6^h\ 58^m\ 55^s{,}72$, and $D = +\ 50°\ 3'\ 26''{,}6$.

998. 45 *Geminorum.* This star is designated by the letter *o*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

1002. 47 *Geminorum.* In the observation of this star on Feb. 22, 1690, at $7^h\ 57^m\ 19^s$, Flamsteed remarks that it has a cluster of stars near it. This is VIII. 40 of Sir William Herschel's first catalogue of Nebulæ, and clusters of stars.

1004. — *Lyncis.* Observed by Flamsteed on Jan. 23, 1696, at $9^h\ 55^m\ 12^s$. It is to be found in the *Mem. de l'Acad.* for 1790, at $7^h\ 1^m\ 4^s{,}2$, mag. 6.

1005. 48 *Geminorum.* In the printed observation of this star, by Flamsteed, on Feb. 12, 1696, at $8^h\ 33^m\ 42^s$, the zenith distance *per lineas diagonales* is $28°\ 58'\ 25''$; but the reading *per strias cochleæ* is not given: and agreeably to this reading Flamsteed had originally deduced the declination of the star in MSS, vol 23, page 43. On referring to the *original* MS entry (MSS, vol. 6) I find that the reading *per strias cochleæ* is there given 611.10, which would make the zenith distance only $26°\ 58'\ 25''$: and although this is not altered in the *original* entry, yet in the MS *copy* (MSS, vol. 16), and also in MSS, vol. 23, above quoted, it is altered with

the pen by Flamsteed. It thus becomes 48 *Geminorum*, as suggested by Burckhardt. In the *British Catalogue*, this star is designated by the letter *m*: but, as there is no such letter in Bayer's map, I have here rejected it.

1007. — *Geminorum.* Observed by Flamsteed on March 8, 1695, at $7^h\ 1^m\ 51^s$: an observation which Miss Herschel has supposed to belong to 50 *Geminorum;* but that is not the case. See the note to No. 1011 below. I cannot find any star that will correspond with the position here given. In my list of Flamsteed's inedited stars, I have suggested that it might be Piazzi VI. 346; but on reconsidering and re-examining the subject, I am not now of that opinion.

1010. — *Lyncis.* Observed by Flamsteed on Jan. 13, 1696, after $10^h\ 39^m\ 16^s$; again on Jan. 23, 1696, after $9^h\ 55^m\ 12^s$; again on March 4, 1703, at $7^h\ 24^m\ 26^s$; and again on Feb. 15, 1704, at $8^h\ 22^m\ 0^s$. Miss Herschel has supposed these observations to belong to two different stars; and I was also of the same opinion in my notes thereon, in my list of Flamsteed's inedited stars, Nos. 149 and 150. But I think M. Argelander's suggestion is the most probable, that they belong to one and the same star; since there is no other star in the heavens that occurs on the same parallel, and so near together. It is the star observed by Lalande in *Hist. Cél.* page 383, at $7^h\ 2^m\ 8^s{,}5$, mag. 5.

1011. 51 *Geminorum.* The star 50 *Geminorum* does not exist. Its position in Flamsteed's catalogue is $AR = 103^\circ\ 38'\ 0''$, and $D = +\ 15^\circ\ 38'\ 5''$; and which has been erroneously deduced from the observation of the present star, 51 *Geminorum*, made on Jan. 23, 1690, at $9^h\ 43^m\ 13^s$: in the first place, by making an error of 1^m in the right ascension, and secondly by making an error of 1° in the declination: as may be seen, by following the steps of the computation in MSS, vol. 23, page 37. When these are corrected, the star will agree with 51 *Geminorum.*

1012. 21 *Monocerotis.* This star is marked as of the 5th magnitude in the *British Catalogue:* which is erroneous, as in both the observations it is stated to be of the 6th; which I have here adopted.

1013. 52 *Geminorum.* This star is designated by the letter *n*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

1017. 53 *Geminorum.* The declination of this star in the *British Catalogue* is $+\ 28^\circ\ 20'\ 55''$. It was observed by Flamsteed on Jan. 29, 1710, at $9^h\ 27^m\ 20^s$; and a comparison of its zenith distance with that of 48 and 55 *Geminorum*, observed on the same day, shows that an error of $2'$ has been made in the declination; which I have here corrected.

1019. 19 *Lyncis.* The right ascension of this star must be diminished nearly $7'$, in order to correspond with modern observations. It has been deduced by Flamsteed, in MSS, vol. 23, page 373, from 11 *Ursæ Majoris;* which, itself, must be diminished. It is a double star (H. II. 27). The right ascension in Halley's edition of 1712 is $104^\circ\ 19'\ 50''$. There is another observation of it (besides those indicated by Miss Herschel) on Jan. 23, 1696, the second after $9^h\ 55^m\ 12^s$, the zenith distance of which is erroneous. For the observation was made to the *north* of the zenith, and not to the *south*, as erroneously stated in the *printed* copy. See the *original* MS entry, in MSS, vol. 6, page 174. And it is in this manner that its declination has been deduced by Flamsteed in MSS, vol. 23, page 377. The error in the printed copy has given rise to the supposition of another and a different star; viz. No. 440 in Miss

Herschel's catalogue, and No. 151 in my list of Flamsteed's inedited stars: but which does not exist. For, the proper correction being made, the star will be found to be 19 *Lyncis*.

1020. 20 *Lyncis*. The declination of this star in the *British Catalogue* is + 50° 41′ 30″. It was observed by Flamsteed on Feb. 16, 1704, at $8^h\ 18^m\ 26^s$: and on deducing the declination from the zenith distance, it will be seen that an error of 1′ has been made, which I have here corrected. Modern observations show this star to be of the 7½ magnitude.

1025. 27 *Canis Majoris*. The magnitude of this star is not recorded in the observation book: and modern observations make it of the 4½ magnitude.

1026. 55 *Geminorum* δ. The magnitude of this star is not recorded in the *original* observation books: and modern observations make it only of the 4½ magnitude.

1027. 28 *Canis Majoris*. This star is marked as of the 5th magnitude in the *British Catalogue*: but, in the *original* entries, the magnitude is only once noted, and then designated as 3½. Modern observations make it of the 6th: I apprehend therefore either that it is a variable star, or that some error has been made by Flamsteed.

1030. 56 *Geminorum*. This star is designated by the letter *q*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

1039. 61 *Geminorum*. This star is designated by the letter *r* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it. It is marked as of the 6th magnitude in the *British Catalogue*: but in the *original* observations, it is only marked so once, whereas it is twice marked as of the 7th. I have therefore taken the mean.

1041. 62 *Geminorum* ρ. This star is designated by the letter *s* in the *British Catalogue*; but this is evidently a typographical error: as there is no star so designated in Bayer's map. I have therefore restored the correct reading.

1042. 63 *Geminorum*. This star is designated by the letter *p* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

1043. — *Geminorum*. Observed by Flamsteed on Feb. 19, 1696, after $8^h\ 21^m\ 55^s$. M. Argelander supposes that it is the star observed by Lalande in *Hist. Cél.* page 144, at $7^h\ 16^m\ 14^s{,}5$; in which case it must have been observed by Flamsteed after its transit over the meridian; since it appears by his observation to have passed *after* the transit of 65 *Geminorum*: whereas Lalande makes it pass *before* that star. On referring to the *original* MS entry (MSS, vol. 6) there is a remark which appears to confirm this.

1044. 64 *Geminorum* b^1. In the reduction of the observation of this star, made on Feb. 12, 1696, at $8^h\ 50^m\ 6^s$, Flamsteed has made an error of 10^m in deducing the difference in the time of transit. (See MSS, vol. 23, page 43.) Whereby he has increased its right ascension 2° 30′, and considered it as another and a different star, namely, 72 *Geminorum*, whose position in the *British Catalogue* is $\mathcal{R}$ = 110° 1′ 30″, and D = + 28° 42′ 15″: but which consequently does not exist.

1046. 65 *Geminorum* b^2. In the reduction of the observation of this star, made on Feb. 12, 1696, at $8^h\ 50^m\ 34^s$, Flamsteed has made the same error as mentioned in the preceding note. Whereby he considered this star as another and a different star, namely 73 *Geminorum*, whose position in the *British Catalogue* is $\mathcal{R}$ = 110° 8′ 30″, and D = + 28° 29′ 25″: but which consequently does not exist. He has also made an error of 1′ in copying out the zenith distance. It

should be here observed, that there is only one star designated by the letter *b*, in Bayer's map: but as there may be a doubt whether such letter belongs to 64 *Geminorum*, or to the present star, Flamsteed has annexed it to each, which I have here retained.

1048. 5 *Canis Minoris* η. In the observation of this star on Feb. 18, 1690, at $8^h\ 32^m\ 36^s$, Flamsteed remarks that it has a companion south following: probably the star observed by Lalande in *Hist. Cél.* page 44, at $7^h\ 18^m\ 11^s{,}5$; mag. 8.

1050. 6 *Canis Minoris*. This star is designated by the letter o in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

1051. 48 *Camelopardi*. In the *original* entries this star is once denoted as of the 6th, and once as of the $6\frac{1}{2}$ magnitude.

1052. 66 *Geminorum* α. The magnitude of this star is only once noticed in the observations; and where it is stated to be of the 1st magnitude. Piazzi makes it of the 3rd.

1053. 23 *Lyncis*. This star is marked as of the 7th magnitude, in the *British Catalogue:* but in the observations, it is designated as the 6th; which I have therefore retained.

1055. 68 *Geminorum*. This star is designated by the letter *k* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

1059. 8 *Canis Minoris* δ^2. }
1060. 9 *Canis Minoris* δ^3. } There are only two stars designated by the letter δ, in Bayer's map: but as it is doubtful which was intended as the second of the two stars (or rather, whether the united appearance of 8 and 9 *Canis Majoris* did not constitute that which Bayer has considered as the second of the two), Flamsteed has included all three; and designated the whole by that letter, and which I have here retained.

1062. 49 *Camelopardi*. Observed by Flamsteed on Jan. 23, 1696, at $10^h\ 20^m\ 40^s$: but the time is marked as doubtful: and the right ascension ought to be diminished about $7'$ in order that it may correspond with modern observations.

1064. — *Canis Minoris*. Observed by Flamsteed on Oct. 5, 1691, at $17^h\ 57^m\ 53^s$. It is Piazzi VII. 150.

1066. 51 *Camelopardi*. The right ascension of this star must be diminished full 1°, in order to correspond with modern observations. In the MS copy the time of transit is marked as doubtful. See the list of Errata, referring to page 383.

1070. — *Canis Minoris*. Observed by Flamsteed on Oct. 4, 1691, at $18^h\ 6^m\ 40^s$, according to the *printed* copy; but in the *original* MS entry (MSS, vol. 5) there is a marginal note in Flamsteed's hand-writing, which shows that it should be $18^h\ 5^m\ 40^s$: again on Oct. 7, 1691, at $17^h\ 53^m\ 2^s$, where there are two errors in the *printed* copy, inasmuch as the transit of Procyon is wholly omitted, and its zenith distance placed against its companion. The *original* MS entry stands thus:

17^h	52^m	25^s	Procyon transit . . .	— — —	
53	2		Comes Procyonis transit . .	— — —	
55	$6\frac{1}{2}$		Exit Procyon . . .	45°	$31'$ $0''$

Again on Dec. 24, 1691, at $12^h\ 35^m\ 19^s{,}5$; where the word "sup." (although so written in the original MS entry) should probably be "sub." And again on Jan. 22, 1692, at

$10^h\ 26^m\ 0^s$. The comparison of all these observations shows the star to be Piazzi VII. 170 = 307 Mayer.

1072. 76 *Geminorum c.* There is no observation of this star by Flamsteed, with the mural arc: but it was several times observed with the sextant: from which its position has probably been reduced.

1074 — *Navis* ×. Observed by Flamsteed on March 4, 1693, at $7^h\ 46^m$; again on Feb. 16, 1709, at $8^h\ 46^m\ 43^s$; and again on the following day, at $8^h\ 42^m\ 38^s$. It is inserted in Halley's edition of 1712, from which the position, here given, is taken: and it is also inserted in Flamsteed's maps. It is the double star Piazzi VII. 175 and 177; and is denoted by the letter × in Bayer's map.

1075. — *Canis Majoris.* Observed by Flamsteed on Feb. 16, 1702, at $8^h\ 48^m\ 33^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 278, at $7^h\ 30^m\ 36^s{,}5$, and page 280, at $7^h\ 31^m\ 3^s{,}5$, mag. $5\frac{1}{2}$: which, however, exceeds it 5′ in declination.

1078. 79 *Geminorum.* This star is omitted in Flamsteed's map of *Gemini.*

1081. 11 *Canis Minoris.* This star is designated by the letter π in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

1082. 82 *Geminorum.* The declination of this star in the *British Catalogue* is + 23° 50′ 55″. But it is evident, on a computation of the value of the zenith distance observed, that an error of 1′ has been made in the declination, which I have here corrected.

1083. — *Geminorum.* Observed by Flamsteed on Feb. 19, 1696, the second after $8^h\ 37^m\ 40^s$: but the time of transit is not given. It is probably the star observed by Lalande in *Hist. Cél.* page 144, at $7^h\ 37^m\ 33^s$, mag. $7\frac{1}{2}$.

1084. — *Lyncis.* Observed by Flamsteed on Feb. 19, 1696, after $8^h\ 37^m\ 40^s$: but the time of transit is not given. It is probably Piazzi VII. 215.

1085. 1 *Navis* σ. Observed by Flamsteed on March 4, 1693, at $7^h\ 50^m\ 47^s$, and reduced by him in MSS, vol. 23, page 223: but he has made a mistake of 2° in copying out the zenith distance, and hence has deduced the declination, given in the *British Catalogue,* to be — 25° 41′ 30″. I have therefore added 2° to this quantity; and it will then appear that 1 *Navis* is Piazzi VII. 200 = Lacaille No. 677 = Bradley No. 1118: and the same as No. 161 in my list of Flamsteed's inedited stars. It is denoted by the letter σ in Bayer's map, which I have here adopted.

1087. 52 *Camelopardi.* This star is marked as of the 5th magnitude in the *British Catalogue:* but in the *original* entry it is designated as $5\frac{1}{2}$, which I have here retained. Modern observations make it of the $7\frac{1}{2}$ magnitude.

1089. — *Navis.* Observed by Flamsteed on Feb. 16, 1709, at $8^h\ 52^m\ 9^s$, and again on the following day at $8^h\ 48^m\ 7^s$. It is the star observed by Lalande in *Hist. Cél.* page 468, at $7^h\ 35^m\ 25^s{,}5$, mag. 6, and not Bradley No. 1130, as stated in my list of Flamsteed's inedited stars, through a mistake in the computation.

1090. 4 *Navis.* In the printed observation of this star on Feb. 16, 1702, at $8^h\ 54^m\ 1^s$, Flamsteed remarks that it has a companion preceding it. This remark however is not to be found in the MS copy (MSS, vol. 16).

1092. 26 *Lyncis.* This star is marked as of the 5th magnitude, in the *British Catalogue*: but, in the observations, it is designated as the 6th, which I have here retained.

1093. 5 *Navis.* Observed by Flamsteed on Feb. 16, 1702, at $8^h\ 55^m\ 45^s$. The declination in the *British Catalogue* is $-\ 11°\ 37'\ 40''$: but, on comparing the zenith distance with that of other stars observed on the same day, it will be seen that Flamsteed has made an error of $10'$. I have therefore deducted this value from the declination given by him; and it will now correspond with modern observations.

1094. — *Navis.* Observed by Flamsteed on March 16, 1692, at $7^h\ 12^m$; but the exact time is uncertain. I cannot find it in any catalogue: but M. Argelander states that its position for 1830, deduced from 7 observations, is $\mathcal{R} = 7^h\ 39^m\ 54^s{,}12$, and $D = -\ 22°\ 6'\ 21''$.

1096. 84 *Geminorum.* The magnitude of this star is not recorded in the original entry of the observation: and modern observations make it of the $7\frac{1}{2}$ magnitude.

1098. 12 *Canis Minoris.* I cannot find any observation of this star in the *Historia Cœlestis*: neither does it exist in any catalogue. It is interlined in the list of stars in the constellation *Gemini*, in MSS, vol. 25, page 43: but no reference is made to the observation. This is the only trace of it that I can find in the MSS.

1099. — *Navis o.* Observed by Flamsteed on March 16, 1692, after $7^h\ 14^m\ 22^s$; but the time is not given: and again on March 4, 1693, at $7_h\ 54^m\ 58^s$, which is regularly reduced by him in MSS, vol. 23, page 223. It is given in Halley's edition of 1712, from which the position in the present catalogue is taken: and it is also inserted in Flamsteed's maps. It is Piazzi VII. 220: and is denoted by the letter *o* in Bayer's map; which I have here adopted.

1103. 8 *Navis.* Piazzi makes this star to be of the 7th magnitude.

1104. 54 *Camelopardi.* The declination of this star in the *British Catalogue* is $+\ 56°\ 4'\ 45''$. It was observed by Flamsteed on Jan. 23, 1696, at $10^h\ 39^m\ 18^s$: and by comparing the reading of the zenith distance *per lineas diagonales* with that *per strias cochleæ*, it will be seen that there is a difference of $2°$; and that the former ought to be $6°\ 29'\ 40''$. That this is the true value is evident from the circumstance that no star exists corresponding with the recorded reading: the star called 54 *Camelopardi* by Piazzi and Lalande not agreeing therewith in any particular. Nor was the right star observed by either of those astronomers. It was however observed by Mr. Groombridge, who has (in his catalogue not yet published) applied the name of this star to the proper observation. I have therefore corrected Flamsteed's error.

1105. 9 *Navis.* This star is marked as of the 4th magnitude, in the *British Catalogue*: but, in the *original* entries, it is designated once as the 4th and once as the 6th; I have therefore taken the mean.

1106. 85 *Geminorum.* This star is designated by the letter *l* in the *British Catalogue*: but, as there is no such star in Bayer's map, I have rejected it here.

1107. 10 *Navis.* The declination of this star, in the *British Catalogue*, is $-\ 14°\ 2'\ 40''$. It was observed by Flamsteed on Feb. 16, 1702, at $9^h\ 0^m\ 22^s$: and a comparison of its zenith distance with other stars observed on the same day, shows that an error of $2'$ has been made in the declination; which I have here corrected.

1108. — *Canis Minoris.* Observed by Flamsteed on Feb. 2, 1701, at $9^h\ 54^m\ 56^s$: but the time is uncertain. It is Piazzi VII. 249 = (10 Hev.) *Canis Minoris.*

1110. — *Canis Minoris.* Observed by Flamsteed on Feb. 2, 1701, at $9^h\ 56^m\ 39^s$. It is Piazzi VII. 258.

1111. — *Cancri.* Observed by Flamsteed on March 16, 1695, at $7^h\ 20^m\ 30^s$; and again imperfectly on Feb. 14, 1696, after $9^h\ 14^m\ 44^s$. It is Piazzi VII. 261.

1112. 55 *Camelopardi.* There is no right ascension annexed to this star in the *British Catalogue.* It was observed by Flamsteed on Jan. 25, 1696, at $10^h\ 32^m$; but the time is only approximative. In Halley's edition of 1712 he has given the right ascension equal to 115° 33′: but I have assumed an approximate value more agreeable to Flamsteed's observation.

1116. 4 *Cancri* ω^2. There is only one star designated by the letter ω in Bayer's map: but, as it may be doubted whether this belongs to 2 *Cancri*, or to the present star, Flamsteed has annexed it to each, which I have here retained.

1117. 11 *Navis e.* This star is marked as of the 4th magnitude in the *British Catalogue*: but, in the *original* entry, it is designated as $4\frac{1}{2}$; which I have here retained.

1119. 27 *Monocerotis.* Piazzi makes the magnitude of this star as $6\frac{1}{2}$ only.

1120. 6 *Cancri.* In the *British Catalogue* this star is called χ: but it is χ *Geminorum*, according to Bayer. I have therefore omitted the letter here.

1123. 7 *Cancri.* The declination of this star in the *British Catalogue* is +22° 52′ 45″. The observation from which it has been deduced was made by Flamsteed on March 7, 1696, at $8^h\ 2^m\ 17^s$; and regularly reduced by him in MSS, vol. 23, page 177; where the declination is stated to be + 22° 53′ 30″. Why it was afterwards altered I am unable to ascertain: but as it is evident that an error of 1′ has been made, I have here corrected the above value.

1126. — *Cancri.* Observed by Flamsteed, on Feb. 7, 1690, after $9^h\ 19^m\ 45^s$; and again on March 8, 1692, at $7^h\ 52^m\ 40^s$. It was observed by Lalande in *Hist. Cél.* page 144, at $7^h\ 53^m\ 21^s{,}5$, mag. 7.

1127. 28 *Lyncis.* There is only one observation of this star by Flamsteed, which was on Feb. 15, 1704, at $9^h\ 10^m$: but the exact time is uncertain. The right ascension in the present catalogue ought to be reduced above 20′, in order to correspond with modern observations.

1128. — *Cancri.* Observed by Flamsteed on Feb. 14, 1696, after $9^h\ 19^m\ 40^s$, apparently between the passage of 8 and 9 *Cancri.* M. Argelander thinks that it is the star observed by Lalande in *Hist. Cél.* page 219, at $7^h\ 53^m\ 19^s{,}7$, and in page 254, at $7^h\ 53^m\ 20^s{,}6$; mag. 6. In which case it must have passed the meridian of Greenwich 45^s *before* 8 *Cancri.*

1129. — *Navis.* Observed by Flamsteed on Feb. 19, 1690, at $9^h\ 5^m\ 0^s$. M. Argelander states that it is No. 77 *Offic. Typog.* in Bode's great catalogue, and that it is the star mentioned by Bessel in his paper on the comet of 1811; and that 6 observations of it at Abo give its position for 1830 $\mathrm{A\!R} = 7^h\ 52^m\ 14^s{,}79$, and D = − 17° 56′ 16″,3.

1132. — *Camelopardi.* Observed by Flamsteed on Jan. 23, 1696, at $10^h\ 51^m\ 12^s$; but the zenith distance is not given. It is probably Piazzi VII. 311: as the right ascension will correspond thereto, and the declination is probably betwixt that of 54 and 56 *Camelopardi*, between the transits of which stars it was observed.

1133. 10 *Cancri* μ^2. This star is marked as of the 5th magnitude, in the *British Catalogue:* but in the *original* entries it is designated once only as of the 5th, once as $6\frac{1}{2}$, and once as 7th. I have taken the mean. I would also remark, that there is only one star, in Bayer's map, designated by the letter μ; but as it may be doubted whether it ought to be annexed to this star or to 9 *Cancri*, Flamsteed has affixed it to each; which I have here adopted.

1134. 11 *Cancri.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entry, it is designated as $7\frac{1}{2}$; which I have therefore retained.

1135. 56 *Camelopardi.* The right ascension of this star must be diminished $7'\ 1'',8$, in order to correspond with Bradley's observations.

1136. — *Navis.* Observed by Flamsteed on Feb. 16, 1709, at $9^h\ 11^m\ 23^s$, and again on the following day at $9^h\ 7^m\ 22^s$. It is not in any catalogue: but M. Argelander states that 7 observations of it at Abo give its position for 1830, $\mathcal{R} = 7^h\ 56^m\ 51^s,81$, and $D = -17°\ 11'\ 19'',8$.

1139. — *Lyncis.* Observed by Flamsteed on Feb. 16, 1704, at $9^h\ 11^m\ 30^s$. It is No. 1169 in Bessel's edition of Bradley's catalogue.

1140. 13 *Cancri* ψ^1. } There is only one star designated by the letter ψ in Bayer's map: but as it
1141. 14 *Cancri* ψ^2. } may be doubted to which of these two it ought to be applied, Flamsteed has annexed it to each; which I have here retained. I would also remark that in the *British Catalogue*, the first of these stars is stated to be of the $6\frac{1}{2}$ and the latter of the 4th magnitude. But, in the *original* entries, they are both stated to be of the 7th magnitude, which I have here adopted. At the same time, Flamsteed remarks that 14 *Cancri* is the brightest of the two: but not so bright as 15 *Cancri.*

1142. 29 *Lyncis.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but, in the *original* entries, it is designated twice as the 6th, and once as $5\frac{1}{2}$; I have therefore taken the former value.

1143. — *Navis.* Observed by Flamsteed on March 16, 1692, the second after $7^h\ 14^m\ 22^s$. The time of transit is not given: but in the original MS entry (MSS, vol. 5) it is said to have passed the meridian 2^m before the next following star, 15 *Navis.* It is probably the star observed by Lalande in *Hist. Cél.* page 283, at $7^h\ 57^m\ 46^s,6$, mag. 6.

1144. 57 *Camelopardi.* The right ascension of this star in the *British Catalogue* is $117°\ 45'\ 30''$. It was observed by Flamsteed on Jan. 25, 1696, at $10^h\ 41^m\ 54^s$; and regularly reduced by him in MSS, vol. 23, page 381. By comparing the time of transit with that of other stars observed on the same day, it appears that an error of 1^m has been made: and it may here be remarked that the value in Halley's edition of 1712 is $118°\ 0'\ 40''$; which appears to be the correct value. I know not why it was afterwards altered: but I have here restored the correct reading. There is no magnitude affixed to the observation: and that assumed by Flamsteed differs from Piazzi, who makes it $6\frac{1}{2}$.

1146. — *Cancri.* Observed by Flamsteed on Feb. 19, 1696, at $9^h\ 4^m\ 49^s$; and regularly reduced in MSS, vol. 23, page 45. It is inserted in his list of stars in the constellation *Gemini*, in MSS, vol. 25, page 43; but is afterwards struck out with the pen, with a note that it belongs to the constellation *Cancer:* in which constellation it is inserted in Halley's edition of 1712. It is the star observed by Lalande in *Hist. Cél.* page 52, at $7^h\ 59^m\ 2^s,5$, mag. 7.

1147. 15 *Cancri.* This star is designated as ψ^3 *Cancri* in the *British Catalogue:* but it is not, from its position, entitled to that appellation, neither is such a reading authorized by Bayer; who in fact places it in *Gemini*, and calls it ψ *Geminorum*, whence the origin of the error. In the *British Catalogue* it is marked as of the 5th magnitude: but the *original* entries designate it always as the 6th; which I have therefore adopted.

1149. — *Cancri.* Observed by Flamsteed on March 7, 1696, after $8^h\ 11^m\ 9^s$; and again on March 14, at $7^h\ 43^m\ 36^s$. It is Piazzi VIII. 3 = 328 Mayer. In the latter observation Flamsteed remarks (in the *original* MS entry) that it has a bright one before it about 45′ distant; which is probably Piazzi VII. 322.

1151. 58 *Camelopardi.* There is no right ascension to this star in the *British Catalogue:* but it is the same star as 30 *Lyncis*, whose right ascension is that which is here given. The declination however of 30 *Lyncis* is + 58° 37′ 5″. In Halley's edition of 1712 the declination is + 58° 38′ 55″.

1152. — *Cancri.* Observed by Flamsteed on Feb. 19, 1696, at $9^h\ 6^m\ 17^s$: but the observations of that day are not reduced by him. There is a star in Halley's edition of 1712, viz. $\mathcal{R}$ = 118° 43′ 30″ and D = + 15° 2′ 30″, which I presume was meant for this star. The present star was observed by Lalande in *Hist. Cél.* page 52, at $8^h\ 0^m\ 28^s$, mag. 7½.

1154. — *Navis.* Observed by Flamsteed on Feb. 19, 1690, at $9^h\ 13^m$; and again on Feb. 16, 1702, at $9^h\ 17^m\ 33^s$. It is inserted in Halley's edition of 1712, from which the position in the present catalogue is taken. It was observed by Lalande in *Hist. Cél.* page 280, at $7^h\ 59^m\ 40^s{,}2$, mag. 6.

1155. 17 *Navis.* This star does not exist, neither can I find any observation of it in the *Historia Cœlestis.* Miss Herschel thinks that its introduction into the *British Catalogue* may have arisen from an error in computing the preceding star (No. 1154 of this catalogue): but from some alterations in the computations of 20 *Navis*, in MSS, vol. 23, page 223, I think it very probable that it was originally deduced erroneously from the observation of that star made on Feb. 19, 1690, at $9^h\ 16^m\ 21^s$.

1157. — *Cancri.* Observed by Flamsteed on March 16, 1700, at $7^h\ 28^m\ 47^s$; again on the following day, at $7^h\ 24^m\ 48^s$; and again on March 19, at $7^h\ 22^m\ 50^s$, which should probably be $7^h\ 21^m\ 50^s$. It is Piazzi VIII. 14 = 329 Mayer.

1158. — *Lyncis.* Observed by Flamsteed on Jan. 23, 1696, at $10^h\ 58^m\ 39^s$: but the exact time is not known. I cannot find the star in any catalogue: but M. Argelander says that the observations at Abo give its position for 1830 $\mathcal{R} = 8^h\ 2^m\ 51^s{,}04$ and D = + 59° 41′ 55″,6. In the *original* MS entry it is said to have been observed 2^m after it had passed the meridian.

1159. — *Lyncis.* Observed by Flamsteed on Jan. 23, 1696, the second after $10^h\ 58^m\ 39^s$. It is Piazzi VIII. 30.

1160. 19 *Navis.* This star is designated as of the 4½ magnitude in the *British Catalogue:* but it is only once so called in the *original* entries; whereas it is twice denominated as 5½; which I have here retained.

1161. — *Cancri.* Observed by Flamsteed on Feb. 19, 1696, at $9^h\ 8^m\ 19^s{,}5$; and afterwards by Lalande in *Hist. Cél.* page 216, at $8^h\ 3^m\ 22^s$, mag. 7½. It is inserted in Halley's edition of 1712.

1166. — *Cancri.* Observed by Flamsteed on March 7, 1693, at $8^h\ 10^m\ 37^s$. It is Piazzi VIII. 42; and is called *étoile singulière* by Lalande in *Hist. Cél.* page 211.

1168. 21 *Navis.* The declination of this star in the *British Catalogue* is — 15° 2′ 50″: but this is an error of the press, as it ought to be — 15° 20′ 50″. It was observed by Flamsteed on Feb. 17, 1702, at $9^h\ 21^m\ 32^s$: and the comparison of the zenith distance with that of other stars observed on the same day, indicates the error: which is confirmed by an examination of the original MS catalogue in MSS, vol. 25, from which most of the others appear to have been copied, where the correct value is given as above stated; and which I have here restored.

1171. 2 *Ursæ Majoris A.* The right ascension of this star is deduced *per distantias*: as may be seen in MSS, vol. 26 C, page 10.

1172. 21 *Cancri.* The right ascension of this star in the *British Catalogue* is 121° 40′ 0″. There are three observations of it by Flamsteed, two only of which have been reduced: viz. one on March 12, 1691, at $7^h\ 59^m\ 33^s$, and the other on March 7, 1696, at $8^h\ 23^m\ 38^s$. The former is regularly and correctly reduced by him in MSS, vol. 23, page 77; but in the latter he has made an error of 20^s (= 5′ 0″): and he appears to have taken the mean as the true value. By correcting this error, however, we should have the right ascension equal to 121° 43′ 15″; which is the value assumed in the present catalogue. The declination in the *British Catalogue* is + 11° 34′ 25″; and it appears to be regularly deduced by Flamsteed from the observations above stated; but in copying it out in the list of stars in the constellation *Cancer*, in page 68 of the same volume, he does not appear to have made any allowance for the annual precession. On recomputing the two observations agreeably to Flamsteed's method, I obtain the value given in this catalogue.

1173. 22 *Cancri* φ^1. One of the observations of this star was made by Flamsteed on March 7, 1696, at $8^h\ 25^m\ 18^s$; but which should be $8^h\ 24^m\ 18^s$. Owing to this error Flamsteed has considered it as another and a different star, viz. 26 *Cancri*, whose position in the *British Catalogue* is $\mathcal{R}$ = 122° 9′ 30″ and D = + 28° 51′ 8″: but which does not exist. See MSS, vol. 23, page 77, where this mistake is manifest: also the original MS entry (MSS, vol. 6) where the figures denoting the minutes appear to have been altered.

1178. 1 *Hydræ.* This star is marked as of the 4th magnitude in the *British Catalogue*: but in all the *original* entries it is designated as the 6th; which I have here retained.

1181. 28 *Cancri* v^3. In the observation of this star on March 14, 1696, at $7^h\ 59^m\ 28^s{,}5$, Flamsteed remarks that it has a companion 20′ more northerly. See the note to No. 1183 below.

1182. 2 *Hydræ.* This star is marked as of the 4th magnitude in the *British Catalogue*: but in the *original* entries, it is twice designated as the 6th, and once as $6\frac{1}{2}$. I have therefore altered it here.

1183. — *Cancri.* Observed by Flamsteed on March 14, 1696, at $8^h\ 0^m\ 29^s$. M. Argelander thinks that it is the same star as that mentioned in the note to 1181: but this star was also observed at the same time. It is Piazzi VIII. 79 = 341 Mayer.

1185. 3 *Ursæ Majoris.*
1186. 4 *Ursæ Majoris* π. } The right ascensions of these stars have been deduced *per distantias* in MSS, vol. 26 C, page 10; and the results appear to be more correct than those which are here retained by Flamsteed. In the *British Catalogue*, each of

these stars is denoted by the letter π: but there is only one star designated by that letter in Bayer's map, which is the latter of the two: the other ought more properly to be taken in connexion with 2 *Ursæ Majoris*, and be called A^2. I have therefore rejected the letter from the first of these stars. In the *British Catalogue*, No. 1185 is said to be the brightest: but, in the *original* entries, the reverse is stated to be the case. I have therefore altered them here.

1187. 30 *Cancri* υ^3. Besides the 5 observations of this star indicated by Miss Herschel, there is another on March 18, 1696, which passed after $7^h\ 46^m\ 30^s$, and which she has supposed to be another star, namely, No. 463 in her catalogue and No. 191 in my list of Flamsteed's inedited stars. But, on referring to the original MS entry (MSS, vol. 6) I find that the zenith distance, *per strias cochleæ*, should be 600·28 instead of 660·28, and that the corresponding reading *per lineas diagonales* should be 26° 29′ 55″, as stated in the margin of the *original* MS entry. Whence it will appear that the star is 30 *Cancri*. There are only two stars designated by the letter υ in Bayer's map, but as it may be doubted whether the second belongs to 28 *Cancri*, or to this star, Flamsteed has annexed the letter to both: which I have here retained.

1190. — *Ursæ Majoris*. Observed by Flamsteed on March 11, 1703, at $8^h\ 19^m\ 38^s$. There is no star that will exactly correspond with the position here given: but if we suppose with M. Argelander that there is an error of 1^m in the time of transit, and that it should be $8^h\ 18^m\ 38^s$, the observation will agree with No. 23 *Ursæ Majoris* in Bode's great catalogue; which was observed by Lalande in *Hist. Cél.* page 382, at $8^h\ 21^m\ 15^s{,}5$; and in *Mem. de l'Acad.* for 1790, page 381, at $8^h\ 22^m\ 5^s$, mag. 7. And 6 observations of it at Abo give its position for 1830, $\mathrm{AR} = 8^h\ 25^m\ 15^s{,}60$, and $\mathrm{D} = +\ 60°\ 31'\ 30''{,}7$. In such case the right ascension in this catalogue should be diminished 15′.

1191. 32 *Cancri*. This star is called υ^4 by Flamsteed: but there are only 2 stars designated by that letter in Bayer's map, to which Flamsteed has added a third (30 *Cancri*). I have therefore rejected the letter here.

1194. 34 *Cancri*. In the observation of this star by Flamsteed on March 7, 1696, at $8^h\ 32^m\ 27^s{,}5$, he states that it has a companion to the south: probably the star observed by Lalande in *Hist. Cél.* page 211, at $8^h\ 21^m\ 4^s$, mag. $6\frac{1}{2}$.

1195. 35 *Cancri*. In the observation of this star by Flamsteed on March 14, 1696, at $8^h\ 6^m\ 43^s$, it is stated that it has a companion to the north: probably Piazzi VIII. 104.

1196. — *Cancri*. Observed by Flamsteed on Feb. 7, 1690, at $10^h\ 0^m\ 2^s$: but the declination is not given. M. Argelander is of opinion that it is Piazzi VIII. 104 = 349 Mayer: perhaps the star mentioned in the preceding note.

1197. — *Cancri*. Observed by Flamsteed on March 7, 1696, the second after $8^h\ 32^m\ 27^s{,}5$: but the time of transit is not given. M. Argelander is of opinion that it is the star mentioned in Bessel's Zone, 349, at $8^h\ 26^m\ 12^s{,}50$; again in 350, at $8^h\ 26^m\ 10^s{,}64$; and again in 352, at $8^h\ 26^m\ 3^s{,}08$; with $+\ 28°\ 53'\ 55''7$, mag. 7.

1198. — *Ursæ Majoris*. Observed by Flamsteed on Jan. 23, 1696, the third after $11^h\ 13^m\ 34^s$: but the time of transit is not given; and the zenith distance only approximately. I cannot discover any star that will correspond thereto. In my list of Flamsteed's inedited stars I have

suggested that it might be Piazzi VIII. 131: but I am not now of that opinion; nor do I think that it accords with either of the stars in Lalande's *Hist. Cél.* page 383.

1199. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, the fourth after $11^h\ 13^m\ 34^s$: but the time of transit is not given; and the zenith distance only approximately. I cannot discover any star that will correspond thereto. See the preceding note; where the remarks apply also to this star.

1204. 2 *Leonis Minoris.* There is only one observation of 2, 3, 4, and 5 *Leonis Minoris;* each of which was made on Feb. 15, 1704, at $9^h\ 42^m\ 58^s$, at $9^h\ 43^m\ 20^s$, at $9^h\ 43^m\ 50^s$, and at $9^h\ 54^m\ 0^s$ respectively. But owing to some error in the computation, the right ascension of all of them has been deduced about 5′ too great, to agree with modern observations. The comparison of the times of transit with that of 48 *Cancri* on the same day, will show that some error of this kind has been committed. The right ascension of this star must be diminished 5′ 24″,9 in order to accord with Bradley's.

1205. — *Cancri.* Observed by Flamsteed on March 10, 1698, at $8^h\ 28^m\ 34^s$. I cannot find the star in any catalogue, nor does it exist agreeably to the position here given. The declination will agree with Piazzi VIII. 112; but the right ascension differs about 12′.

1206. 3 *Leonis Minoris.* Observed by Flamsteed on Feb. 15, 1704, at $9^h\ 43^m\ 20^s$. The right ascension must be diminished 5′ 9″,0 in order to correspond with Bradley's. See the note to No. 1204 above.

1208. 37 *Cancri* c^3. There is only one star designated by the letter *c*, in Bayer's map: but, as it may be doubted whether such letter belongs to 36 *Cancri*, or to the present star, Flamsteed has annexed it to each of them, which I have here retained. In the *British Catalogue* it is marked as of the 6th magnitude: but, in the *original* observations, it is once marked as the 6th, and once as the 7th. I have therefore adopted the mean.

1209. 4 *Leonis Minoris.* Observed by Flamsteed on Feb. 15, 1704, at $9^h\ 43^m\ 50^s$. The right ascension must be diminished 5′ 24″,4 in order to correspond with Bradley's. See the note to No. 1204 above.

1210. — *Ursæ Majoris.* Observed by Flamsteed on March 11, 1703, at $8^h\ 26^m\ 55^s$. It is Piazzi VIII. 137.

1211. 38 *Cancri.* This star is designated by the letter *o* in the *British Catalogue:* but there is no such letter in Bayer's map. I have therefore rejected it.

1212. — *Cancri f.* Observed by Flamsteed on March 18, 1696, at $7^h\ 58^m\ 16^s$. I cannot find any star that will exactly correspond with it. The right ascension agrees with Piazzi VIII. 124: but the declination is too great by 10′. M. Argelander imagines that there has been some confusion in the original entry; and on looking into it (MSS, vol. 6) I find this to be the case: for it stands thus,

h	m	s		°	′	″
7	58	5	*o*	30	44	30
	58	16	*f*	30	48	5
	58	49	ε	30	58	45
	58	52	ε	30	58	45
	59	9	*b*	30	56	20
	59	23	*b*	—	—	—

The first ε is altered with the pen: but the two *b*'s remain. I apprehend that the entry should have been as follows: viz.

h	m	s					°	′	″
7	58	5	.	.	.	*o*	30	44	30
	58	16	.	.	.	*f*	30	58	45
	58	49							
	58	52	.	.	.	ε	30	58	45
	59	9	.	.	.		30	48	5
	59	23	.	.	.	*b*	30	56	20

1213. 39 *Cancri.*
1214. 40 *Cancri.* There is only one observation in zenith distance of each of these stars, which was on Feb. 28, 1702, at $8^h\ 59^m\ 44^s$ and $8^h\ 59^m\ 49^s$: and it appears that some error has been made in noting down the zenith distances, which should evidently be transposed. Consequently the declinations of these stars must be transposed also, which I have accordingly done: but at the same time I ought to remark that the original MSS are correctly printed, and that there is nothing to authorize the alteration, but the comparison of modern observations.

1216. — *Cancri.* Observed by Flamsteed on Feb. 17, 1690, at $9^h\ 47^m\ 30^s$: it has no zenith distance annexed; but it is most probably situate in *Præsepe*, in which case it is Piazzi VIII. 129 = Mayer 359. Miss Herschel has supposed it to be 40 *Cancri*: but the time of transit will not agree with that star. See the note to No. 1214.

1217. 41 *Cancri* ε. The declination of this star, in the *British Catalogue*, is + 20° 34′ 45″: but this does not accord with the two reductions made by Flamsteed in MSS, vol. 23, pages 73 and 77. In fact, if we reduce the observation of Feb. 14, 1690, at $9^h\ 59^m\ 53^s$, and of March 7, 1696, at $8^h\ 39^m\ 20^s$, agreeably to Flamsteed's method, the declination will come out + 20° 36′ 20″: which is the value I have adopted in the present catalogue. This star is called ε by Flamsteed: but Bayer intended, by this letter, to denote the whole nebulous appearance which he calls *Præsepe*.

1218. 42 *Cancri.* This star has the letter *c* annexed to it in the *British Catalogue*: but that letter has already been applied by Flamsteed to 36 and 37 *Cancri*, as indicated by Bayer. I have therefore rejected the letter here.

1219. — *Cancri.* Observed by Flamsteed on Feb. 17, 1690, at $9^h\ 48^m\ 3^s$, and again on March 18, 1696, at $7^h\ 59^m\ 9^s$. There is some confusion in the time of the two transits; but the star is probably Piazzi VIII. 134 = 362 Mayer. It is inserted by Flamsteed in his catalogue of the stars in the constellation *Gemini*, in MSS, vol. 25, page 45. See the note to No. 1212 above.

1220. — *Ursæ Majoris.* Observed by Flamsteed on March 11, 1703, at $8^h\ 28^m\ 34^s$. I cannot find any star that answers to the position here given. M. Argelander has suggested that if we read $8^h\ 34^m\ 28^s$, instead of $8^h\ 28^m\ 34^s$, the observation would agree with the star observed by Lalande in *Hist. Cél.* page 384, at $8^h\ 36^m\ 47^s{,}3$, mag. 7½.

1223. 44 *Cancri.* This star is marked as of the 6th magnitude in the *British Catalogue*: but in the *original* entry it is designated as the 8th: which I have here retained.

1225. 46 *Cancri* σ^1. Observed by Flamsteed on Feb. 18, 1619, at $9^h\ 47^m\ 10^s$, and regularly

reduced by him in MSS, vol. 23, page 73. I cannot account for the great difference of declination. In the *British Catalogue* it has no letter annexed to it: but it is the first of the three stars designated by the letter σ in Bayer's map: the two others being 57 and 61 *Cancri*. The three stars assumed by Flamsteed are 51, 59, and 64 *Cancri;* which are erroneous, and do not agree with the positions laid down by Bayer: I have therefore corrected them.

1229. 5 *Ursæ Majoris b.* The letter *b* is annexed to 7 *Ursæ Majoris*, in the *British Catalogue;* but that letter evidently belongs to this star. In the *British Catalogue* this star is marked as of the 5th magnitude; but, in the *original* entry, it is designated as the 6th; which I have here retained.

1232. 7 *Ursæ Majoris.* The right ascension of this star in the *British Catalogue* is 127° 46′ 30″. It was observed by Flamsteed on Jan. 23, 1696, the second after $11^h\ 34^m\ 34^s$: but the observation is imperfect, as the time of transit is not given. In order to obtain the right ascension, Flamsteed has made use of the observation of a star which he calls *b*, and which he supposed to be the same star, made with the sextant on Oct. 21, 1681, as given in the second volume of the *Historia Cœlestis*, page 99; and the original computation is to be seen in MSS, vol. 26 C, page 7, and a repetition of it in MSS, vol. 61 E, page 56. But the star there observed was not this star, but 5 *Ursæ Majoris* (No. 1229 of the present catalogue): consequently the right ascension so deduced is erroneous; and Flamsteed himself appears to have suspected some mistake, as in page 9 of the first of the above-mentioned MS volumes he has placed the mark of uncertainty against the name of this very star, and calls it "the *companion* of *b*." The right ascension therefore is undetermined and undeterminable. It evidently is not so great as that given in the *British Catalogue*, as it passed the meridian before the transit of 6 *Ursæ Majoris*. I cannot find any star in any catalogue that will correspond with the zenith distance observed by Flamsteed; and I therefore consider that the star does not exist.

1233. 31 *Monocerotis.* This star is designated as of the 4th magnitude, in the *British Catalogue:* but, in the *original* entries it is twice noticed as of the 5th only; which I have therefore retained. It still differs considerably from Piazzi; who states it to be of the 7th.

1234. 10 *Hydræ.* The magnitude of this star is not noticed in the *original* entry of the observation. Piazzi says it is only of the 7th.

1236. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, the first after $11^h\ 32^m\ 51^s$. It is not to be found in any catalogue: but M. Argelander states that six observations of it at Abo, give its position for 1830, $\mathrm{A}R = 8^h\ 41^m\ 57^s{,}74$, and $\mathrm{D} = +\ 66°\ 9'\ 52''{.}0$.

1237. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, the second after $11^h\ 32^m\ 51^s$. It occurs twice in Lalande's list, in the *Mém. de l'Acad.* for 1790; viz. in page 385, at $8^h\ 40^m\ 11^s{,}8$, and in page 386, at $8^h\ 40^m\ 12^s{,}5$; mag. $8\frac{1}{2}$.

1240. 35 *Lyncis.* I would here remark that Miss Herschel calls this star 36 *Lyncis*, and has also increased by unity the numbers of all the remaining stars in this constellation. This however is erroneous. The star which originally occupied the place of 36 *Lyncis* (but which is struck out in the printed copy of the *British Catalogue*) was 2 *Leonis Minoris*, as may be seen in MSS, vol. 27 C, page 46, which is the manuscript used by the printer: and it was evidently struck out during the progress of the printing; the figure 7 (denoting its magnitude) being left at the end, exactly as in the printed copy.

1241. — *Hydræ.* Observed by Flamsteed on March 9, 1691, after $8^h\ 36^m\ 37^s$. The time is not specified; but the star is probably Piazzi VIII. 167. In the *original* MS entry (MSS, vol. 5) it is stated to have passed the meridian a little *before* the preceding star 13 *Hydræ.*

1242. 5 *Leonis Minoris.* Observed by Flamsteed on Feb. 15, 1704, at $9^h\ 54^m\ 0^s$. The right ascension must be diminished $5'\ 10'',3$ in order to correspond with Bradley's observations. See the note to No. 1204 above.

1245. 51 *Cancri.* This star is called σ^1 by Flamsteed: but he has mistaken the stars designated by the letter σ in Bayer's map. They are 46, 57, and 61 *Cancri:* and not 51, 59, 64, and 66 *Cancri,* as he has supposed. I have here corrected all these errors. See the note to No. 1225 above.

1246. 52 *Cancri.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entry it is designated as the 7th; which I have therefore adopted.

1250. 55 *Cancri* ρ^3. In the reduction of one of the observations of this star (March 7, 1696, at $8^h\ 50^m\ 46^s$) Flamsteed has made an error of 1^m in taking out the difference between the time of its transit, and that of γ *Geminorum* (his determining star) on the same day: as may be seen in MSS, vol. 23, page 77. Whence has arisen the introduction of 56 *Cancri* into the *British Catalogue,* whose position there given in $\mathcal{R} = 128°\ 47'\ 30''$ and $D = +\ 29°\ 27'\ 35''$: but which does not exist. There are only two stars designated by the letter ρ, in Bayer's map: and I have no doubt but that 53 *Cancri* and the present star formed the appearance which Bayer took for the first of these stars; and that 58 *Cancri* was the second of such stars. I have therefore designated these stars respectively ρ^1, ρ^2, ρ^3. The star called ρ^3 by Flamsteed does not exist: and those called by him ρ^3 and ρ^4, are not entitled to that appellation.

1252. 57 *Cancri* σ^2. This star is called ι^2 by Flamsteed: but there is only one star designated by that letter in Bayer's map, which is 48 *Cancri.* I have therefore rejected it here. It is in fact the second of the three stars designated as σ by Bayer; which I have therefore restored. See the note to No. 1225.

1253. — *Hydræ.* Observed by Flamsteed on March 13, 1702, at $8^h\ 22^m\ 32^s$. It does not appear to have been observed by Bradley or Piazzi: but it is in Bessel's Zone 59, at $8^h\ 42^m\ 47^s,06$.

1254. 15 *Hydræ.* In the reduction of one of the observations of this star (March 14, 1692, at $8^h\ 25^m\ 28^s$) Flamsteed has made an error of 9^m, in taking out the difference between the time of its transit, and that of α *Canis Majoris* (his determining star) on the same day: as may be seen in MSS, vol. 23, page 229. Whence has arisen the introduction of 8 *Hydræ* into the *British Catalogue,* whose position there given is $\mathcal{R} = 126°\ 49'\ 0''$ and $D = -\ 5°\ 3'\ 0''$; but which does not exist.

1255. — *Cancri* ρ^3. This star is called ρ^4 by Flamsteed: but for the reasons stated in the note to No. 1250, the order of notation must be altered; and it now becomes ρ^3.

1256. 59 *Cancri.* This star is designated as σ^3 in the *British Catalogue:* but Flamsteed has mistaken the stars denoted by the letter σ in Bayer's map. See the note to No. 1225.

1257. 9 *Ursæ Majoris* ι. The right ascension of this star has been deduced *per distantias:* as may be seen in MSS, vol. 26 C, page 9.

1259. 60 *Cancri.* This star is called α^1 in the *British Catalogue:* but as there is only one star designated by the letter α in Bayer's map, I have rejected it here, on account of the difference in magnitude. It is marked as of the $4\frac{1}{2}$ magnitude in the *British Catalogue;* but in the only observation where the magnitude is noticed, it is designated in the *original* entry as of the 6th; which I have therefore retained.

1260. 61 *Cancri* σ^3. This star has no letter annexed to it in the *British Catalogue*: but it is the third of the three stars designated by σ in Bayer's map. I have therefore inserted it. See the note to No. 1245.

1261. 62 *Cancri* o^1. One of the observations of this star, viz. that made on Dec. 30, 1711, at $13^h\ 29^m\ 25^s$, was reduced by Flamsteed in MSS, vol. 26 C, page 34: and in the process of reduction he has made an error of 10^m in the right ascension. Which has given rise to the introduction of 73 *Cancri* into the *British Catalogue;* whose position, there given, is $\mathcal{R} = 132^\circ\ 27'\ 0''$ and $D = +\ 16^\circ\ 29'\ 55''$; but which does not exist. See the next note.

1262. 63 *Cancri* o^2. Besides the observations of this star, indicated by Miss Herschel, there appears to have been another made by Flamsteed on Dec. 30, 1711, after $13^h\ 29^m\ 25^s$. See MSS, vol. 26 C, page 34, where the observation is reduced. But in the process of reduction, Flamsteed has committed two errors: for he has made a mistake of 10^m in the right ascension (as in the preceding star); and has also made a mistake of 1° in the declination. Which has given rise to the introduction of 74 *Cancri* into the *British Catalogue;* whose position, there given, is $\mathcal{R} = 132^\circ\ 32'\ 0''$, and $D = +\ 15^\circ\ 44'\ 50''$; but which does not exist. The observation, here alluded to, is not printed in the *Historia Cœlestis*, nor is it to be found in the *copy* of the manuscript observations (MSS, vol. 17): the book containing the *original* observations is not in existence.

1264. 10 *Ursæ Majoris.* The right ascension of this star has been deduced *per distantias:* as may be seen in MSS, vol. 26 C, page 9. In the *British Catalogue*, it has the letter *n* annexed to it: but as there is no such letter in Bayer's map, I have here rejected it.

1265. 64 *Cancri.* This star is designated as σ^3 in the *British Catalogue:* but Flamsteed has mistaken the stars denoted by the letter σ in Bayer's map. See the note to No. 1225.

1266. 11 *Ursæ Majoris* σ^1. The right ascension of this star should be diminished $9'\ 16'',0$, in order to correspond with Bradley's observations. As this star is made the *determining* star on the two days on which it was observed (March 14, 1691, and Jan. 13, 1696), it is evident that other stars will be partially affected by this error. The position was deduced *per distantias*, as may be seen in MSS, vol. 26 C, page 7. It may be proper to state here that the magnitude in the *British Catalogue* is marked as the 5th; but, in the original entry, it is designated as the 6th, which I have therefore retained.

1267. — *Cancri.* Observed by Flamsteed on March 15, 1714, at $8^h\ 17^m\ 5^s$. It was observed by Lalande, in *Hist. Cél.* page 324, at $8^h\ 46^m\ 15^s,3$, mag. 6.

1269. 66 *Cancri.* This star is designated as σ^4 in the *British Catalogue*: but Flamsteed has mistaken the stars denoted by the letter σ in Bayer's map. See the note to No. 1225.

1271. 13 *Ursæ Majoris* σ^4. There is only one star designated by the letter σ, in Bayer's map: but as there may be a doubt whether such letter belongs to 11 *Ursæ Majoris*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

1272. 67 *Cancri.* This star is called ρ^3 by Flamsteed: but as there are only two stars designated by that letter in Bayer's map, I have rejected the letter here. See the note to No. 1250 above.

1273. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, at $11^h\ 45^m\ 16^s$. It is not to be found in any catalogue: but M. Argelander states that six observations of it at Abo give its position for 1830, $R = 8^h\ 53^m\ 18^s,19$, and $D = +\ 51^\circ\ 29'\ 40'',5$. The declination agrees very well, but the right ascension differs 10^s, unless the star has a proper motion. On examining the original MS entry (MSS, vol. 6), I find that the time is expressed as doubtful: and was originally entered $11^h\ 40^m\ 16^s$.

1274. 68 *Cancri.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the observation-book it is designated as the 8th; which I have therefore retained.

1276. 14 *Ursæ Majoris* τ. The right ascension of this star was deduced *per distantias:* as may be seen in MSS, vol 26 C, page 9.

1277. — *Hydræ.* Observed by Flamsteed on March 13, 1702, at $8^h\ 31^m\ 39^s$; and March 17, 1702, at $8^h\ 20^m\ 47^s$. It is Piazzi VIII. 233.

1278. — *Hydræ.* Observed by Flamsteed on March 13, 1702, at $8^h\ 31^m\ 57^s$. It is Piazzi VIII. 236.

1279. 70 *Cancri.* This star is called ρ^6 by Flamsteed; but as there are only two stars designated by that letter in Bayer's map, I have rejected the letter here. See the note to No. 1250 above.

1280. — *Cancri.* Observed by Flamsteed on March 14, 1696, at $8^h\ 35^m\ 52^s$. It was observed by Lalande in *Hist. Cél.* page 148, at $8^h\ 52^m\ 55^s$, mag. 8.

1281. — *Lyncis.* Observed by Flamsteed on March 12, 1691, at $8^h\ 39^m\ 0^s$; again on Feb. 21, 1694, at $9^h\ 51^m\ 45^s$; again on Feb. 15, 1704, at $10^h\ 9^m\ 32^s$; and again on the following day, at $10^h\ 5^m\ 57^s$. It was inserted in Halley's edition of 1712; and I know not why it was afterwards omitted, as it appears to have been regularly reduced by Flamsteed himself: not only from the observations made with the mural arc, but also *per distantias;* as may be seen in MSS, vol. 26 C, pages 7 and 9: where however the right ascension is $131^\circ\ 39'\ 30''$. It is Piazzi VIII. 245 = (17 Hev.) *Lyncis.* In the observation of Feb. 21, it is stated that the star has two contiguous ones to the south: these are probably the stars in the *Hist. Cél.* page 320, at $8^h\ 52^m\ 26^s,5$, and $8^h\ 53^m\ 57^s,3$.

1283. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 25, 1696, at $11^h\ 37^m\ 0^s$: but both the time and the zenith distance are uncertain, and expressed to the nearest minute only. It is probably No. 52 *Ursæ Majoris* in Bode's great catalogue; and the star observed by Lalande in *Hist. Cél.* page 365, at $20^h\ 54^m\ 9^s,2$, mag. 6.

1284. — *Ursæ Majoris.* Observed by Flamsteed on March 11, 1703, at $8^h\ 52^m\ 52^s$. It is the double star No. 53 *Ursæ Majoris* in Bode's great catalogue; and is mentioned by Piazzi in the note to VIII. 261.

1285. 71 *Cancri.* The right ascension of this star in the *British Catalogue* is $131^\circ\ 55'\ 30''$: but there is no star that will accord with such position. It was observed by Flamsteed on Jan. 27, 1712, at $11^h\ 18^m\ 28^s$; and reduced by him in MSS, vol. 26 C, page 34: but the steps of the

process are not there detailed. I apprehend that there is an error in recording the observation in the book; and that it should stand as follows: viz.

h	m	s		°	′	″
11	18	28	Saturnus intus			
	19	2	Cancri	33	10	30
	21	19	Saturnus transit . . .	33	3	50

For, it appears, from the observations of the preceding and following days, that the time employed by Saturn, from entering the field of view of the telescope, to the wire in the centre, was $2^m\ 51^s$: which would reconcile the whole; since the right ascension would then be as in the present catalogue. I have therefore adopted the above solution of the difficulty.

1287. 16 *Ursæ Majoris c.* The right ascension of this star has been deduced *per distantias:* as may be seen in MSS, vol. 26 C, page 11.

1290. 76 *Cancri* $\varkappa$. This star is marked as of the $4\frac{1}{2}$ magnitude, in the *British Catalogue:* but I can find only one mention of it in the *original* entries, where it is designated as of the $6\frac{1}{2}$. Piazzi's value is between the two, viz. $5\frac{1}{2}$.

1292. 78 *Cancri.* The right ascension of this star, in the *British Catalogue*, is $132^\circ\ 56'\ 20''$: but there appears to be an error of $2'$ in the reduction. It was observed by Flamsteed on Nov. 22, 1711, at $6^h\ 18^m\ 18^s$, and again on Jan. 27, 1712, at $11^h\ 22^m\ 22^s$: both of which are reduced in MSS, vol. 26 C, page 34. If we take the *mean* difference in the time of transit between this star and that immediately following it on those days (80 *Cancri*), we shall find that the right ascension given by Flamsteed ought to be reduced $2'$, which is accordingly done in the present catalogue. The magnitude is marked, in the *British Catalogue*, as the 6th: but in the observations, it is once designated as the 8th, and once as the 7th; I have therefore taken the mean.

1297. 18 *Ursæ Majoris e.* I cannot account for this great difference in declination: as there does not appear to be any error in the computations. The right ascension has been deduced *per distantias:* as may be seen in MSS, vol. 26 C, page 9.

1300. 81 *Cancri* π^1. Flamsteed has marked this star with the letter π in the *British Catalogue:* but I think that letter more properly belongs to 82 *Cancri.* As there may be a doubt however on this subject, I have annexed the letter to each.

1301. — *Hydræ.* Observed by Flamsteed on March 13, 1702, at $8^h\ 42^m\ 29^s$; and again on March 17, at $8^h\ 31^m\ 36^s$. It has since been observed by Lalande in *Hist. Cél.* page 324, at $9^h\ 1^m\ 3^s{,}2$, mag. 6.

1305. 82 *Cancri* π^2. There is no letter annexed to this star in the *British Catalogue:* but, in my opinion, it is the star designated by the letter π in Bayer's map, which letter Flamsteed has placed against 81 *Cancri.* As there may however be a doubt on this subject, I have annexed the letter to each.

1307. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, after $11^h\ 58^m\ 23^s$; but the time is not given. It is No. 65 in Bode's great catalogue; and may be found in the *Mém. de l'Acad.* for 1790, page 379, at $9^h\ 6^m\ 18^s{,}5$, mag. $5\frac{1}{2}$.

1309. 38 *Lyncis.* The right ascension of this star appears to have been deduced *per distantias:* see MSS, vol. 26 C, pages 7 and 9.

1310. — *Cancri.* Observed by Flamsteed on March 19, 1700, at $8^h\ 26^m\ 9^s,5$; and on the following day at $8^h\ 22^m\ 11^s$; and again on Feb. 5, 1713, at $11^h\ 6^m\ 42^s$. It is Piazzi IX. 35 = 398 Mayer. In reducing the first of these observations in MSS, vol. 26 C, page 32, Flamsteed has made an error of 10° in right ascension: which has given rise to the introduction of 25 *Leonis* into the *British Catalogue*, whose position there given is $\mathcal{R} = 145°\ 14'\ 30''$ and $D = +12°\ 46'\ 35''$; but which consequently does not exist.

1311. 39 *Lyncis.* The right ascension of this star must be diminished above $3'\frac{1}{2}$, in order to correspond with modern observations. There appears to be some error in the reduction of other of the stars observed on Feb. 16, 1704.

1315. 40 *Lyncis.* The right ascension of this star appears to have been deduced *per distantias:* see MSS, vol. 26 C, pages 7 and 9.

1316. 25 *Hydræ.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entry, it is designated as the 5th; which I have therefore retained. Piazzi considers it, however, to be of the 7th.

1317. 21 *Ursæ Majoris.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entry, it is designated as $6\frac{1}{2}$; which I have here retained. Piazzi says it is of the 8th.

1319. — *Hydræ.* Observed by Flamsteed on March 13, 1702, at $8^h\ 51^m\ 34^s$. It is Piazzi IX. 56; and was observed also by Bradley.

1320. 22 *Ursæ Majoris.* The right ascension of this star should be diminished about $9\frac{1}{2}$ minutes in order to correspond with modern observations. See the note to No. 1266 of this catalogue.

1323. 1 *Leonis* ϰ. This star is marked as of the 4th magnitude, in the *British Catalogue:* but in the *original* observations, it is no otherwise designated than as the 5th; which I have therefore adopted.

1324. 23 *Ursæ Majoris h.* The right ascension of this star was deduced *per distantias:* as may be seen in MSS, vol. 26 C, pages 8 and 9.

1325. 24 *Ursæ Majoris d.* The right ascension of this star should be diminished $6'\ 40'',6$ in order that it may be compared with Bradley's observations. See the note to No. 1266 of this catalogue. The right ascension *per distantias* (which has been computed in MSS, vol. 26 C, page 9) is only $136°\ 35'\ 0''$.

1326. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, at $12^h\ 9^m\ 7^s$, and regularly reduced by him; as appears by MSS, vol. 23, page 379. It is inserted in Halley's edition of 1712; and I know not why it was afterwards rejected. It is No. 74 *Ursæ Majoris* in Bode's large catalogue: and is to be found also in *Mém. de l'Acad.* for 1790, page 378, at $9^h\ 14^m\ 25^s$, mag. $6\frac{1}{2}$.

1329. 2 *Leonis* ω. This star is marked as of the 5th magnitude, in the *British Catalogue:* but in the *original* entries it is always designated as the 6th; which I have therefore adopted.

1332. 25 *Ursæ Majoris* θ. The right ascension of this star was determined *per distantias:* as may be seen in MSS, vol. 26 C, page 8.

1335. — *Hydræ.* Observed by Flamsteed on March 9, 1691, after $9^h\ 17^m\ 0^s$; and again on the 19th, at $8^h\ 41^m\ 53^s$: and was regularly reduced by Flamsteed, in MSS, vol. 23, pages 225, 227. Why it was afterwards omitted in the collection of the stars in *Hydra* in page 272 of the same volume, I have not been able to ascertain. It is the star observed by Lalande in *Hist. Cél.* page 321, at $9^h\ 18^m\ 45^s{,}7$: which should be $9^h\ 17^m\ 45^s{,}7$, mag. 6.

1338. 26 *Ursæ Majoris.* The right ascension of this star has been deduced *per distantias*: as may be seen in MSS, vol. 26 C, page 8.

1339. — *Hydræ.* Observed by Flamsteed on March 13, 1702, after $8^h\ 58^m\ 42^s$; and again on the 21st, at $8^h\ 34^m\ 2^s$. It is Piazzi IX. 96.

1340. 27 *Ursæ Majoris.* The right ascension of this star must be diminished $9'\ 10''{,}8$ in order to correspond with Bradley's observations. It was observed by Flamsteed on Jan. 25, 1696, at $12^h\ 3^m\ 52^s$; and the determining star was μ *Persei*, which passed above 5 hours before it.

1343. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 25, 1696, after $12^h\ 3^m\ 52^s$; but the time is not given. It is No. 89 in Bode's large catalogue, and was observed (*sub polo*) by Lalande in *Hist. Cél.* page 366, at $21^h\ 23^m\ 39^s$, mag. $6\frac{1}{2}$.

1345. 5 *Leonis* ξ. This star is marked as of the 4th magnitude, in the *British Catalogue*: but, in the *original* entries, it is once designated as the 5th, and once as the 6th: I have therefore taken the mean.

1347. — *Leonis Minoris.* Observed by Flamsteed on Feb. 21, 1694, after $10^h\ 21^m\ 31^s$; but, in the original MS entry (MSS, vol. 6), it is said to have passed the meridian a little *before* the preceding star, 11 *Leonis Minoris*. It is Piazzi IX. 115.

1348. 32 *Hydræ* τ^4. This star is marked as of the 5th magnitude, in the *British Catalogue*: but, in the *original* entry, it is designated as the 6th; which I have therefore retained.

1349. — *Hydræ.* Observed by Flamsteed on March 13, 1702, the second after $8^h\ 58^m\ 42^s$; and again on the 21st, at $8^h\ 37^m\ 1^s$. It is Piazzi IX. 114.

1352. 42 *Lyncis.* Besides the 4 observations of this star, indicated by Miss Herschel, there is another on March 17, 1702, at $8^h\ 55^m\ 4^s$. The zenith distance *per lineas diagonales* differs $4^\circ\ 55''$ from that *per strias cochleæ*: and that the latter is the correct reading is evident from the fact, that no star exists in the position indicated by the former reading. This error has given rise to the insertion of 12 *Leonis Minoris*, whose place was deduced from the observation above alluded to, and whose position in the *British Catalogue* is $\mathcal{R} = 139^\circ\ 45'\ 20''$, and $D = +\ 36^\circ\ 40'\ 55''$: but which does not exist.

1356. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $8^h\ 56^m\ 40^s$. It is Piazzi IX. 133. See M. Argelander's correction of my former note on this star in my list of Flamsteed's inedited stars.

1357. 10 *Leonis.* This is the same star as 1 *Sextantis*, whose position in the *British Catalogue* is $\mathcal{R} = 140^\circ\ 10'\ 40''$, and $D = +\ 8^\circ\ 12'\ 10''$.

1359. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $8^h\ 57^m\ 13^s$. It is Piazzi IX. 137.

1361. 28 *Ursæ Majoris*. The position of this star accords better with the star designated by the letter *h* in Bayer's map, than 23 *Ursæ Majoris*: but the magnitude of the star is against such a supposition: for in Piazzi it is marked only as the 7th.

1363. — *Leonis*. Observed by Flamsteed on March 2, 1713, at $10^h\ 2^m\ 5^s$. It is Piazzi IX. 141, = 413 Mayer.

1364. 34 *Hydræ*. Besides the two observations of this star indicated by Miss Herschel, there is another on March 19, 1691, at $8^h\ 54^m\ 7^s$, which Flamsteed has considered as another star, 36 *Hydræ*, whose position in the *British Catalogue* is $AR = 141°\ 9'\ 30''$ and $D = -8°\ 3'\ 15''$: but which does not exist. Flamsteed has deduced its right ascension (MSS, vol. 23, page 227) as if it had been observed on the meridian; but it was *post transitum*. It is not inserted in Halley's edition of 1712.

1366. — *Ursæ Majoris*. Observed by Flamsteed on Jan. 23, 1696, at $12^h\ 25^m\ 25^s$; and was regularly reduced by Flamsteed in MSS, vol. 23, page 379. It is inserted in Halley's edition of 1712; and I know not why it was afterwards rejected. It is the star given in the *Mém. de l'Acad.* for 1790, page 383, at $9^h\ 31^m\ 27^s{,}9$, mag. $6\frac{1}{2}$.

1368. 13 *Leonis Minoris*. Observed by Flamsteed on March 17, 1702, at $9^h\ 0^m\ 0^s$. The right ascension in the *British Catalogue* is $141°\ 2'\ 0''$: but on comparing the time of transit with that of the following star (18 *Leonis Minoris*) on that day, it will be seen that Flamsteed has made a mistake of 10^s in time, or $2'\ 30''$ in arc; which I have here corrected; and it will now agree more nearly with modern observations.

1371. — *Leonis*. Observed by Flamsteed on March 25, 1690, at $8^h\ 27^m\ 35^s$. It is Piazzi IX. 155.

1373. 37 *Hydræ*. Observed by Flamsteed on March 19, 1691. The time of transit set down against it is $8^h\ 54^m\ 40^s$: but this evidently belongs to the following star; and consequently its correct right ascension cannot be ascertained from that observation. See the following note.

1374. — *Hydræ*. Observed by Flamsteed on March 19, 1691. The time of transit is not set down against it: but it is evident that the time set down against the preceding star 37 *Hydræ* (viz. $8^h\ 54^m\ 40^s$) belongs to this star: and consequently that the right ascension in the *British Catalogue* which is set down against 37 *Hydræ*, should be placed against this star; which I have accordingly done. It is Piazzi IX. 152, and was observed also by Bradley. Both these stars (viz. No. 1373 and 1374) are reduced by Flamsteed in MSS, vol. 23, page 227.

1376. 38 *Hydræ* x. Observed by Flamsteed on March 13, 1702, at $9^h\ 11^m\ 44^s$; and regularly reduced by him in MSS, vol. 23, page 231; α *Hydræ* being assumed as the determining star. But, from the value which he has there deduced, the right ascension ought to be increased nearly $2'$; for he has afterwards altered the right ascension of his determining star, without altering those stars which depended thereon. It is not inserted in his list of stars in the constellation *Hydra*, in page 274 of the same volume.

1378. 14 *Leonis Minoris*. Observed by Flamsteed on March 18, 1703, at $9^h\ 1^m\ 15^s$. Neither this star, nor 15, nor 16 *Leonis Minoris* agrees well in right ascension. There is a fundamental error of $1'\ 30''$ in the right ascension of these three stars, and also in 17 *Leonis Minoris*. For it appears by MSS, vol. 26 A, page 28, that the determining star was 19 *Leonis Minoris*, whose right ascension was assumed $144°\ 40'\ 0''$. In the *British Catalogue* he has altered this

…in *Mém. de l'Acad.* for 1790, page 380,

…ar is the same as 25 *Leonis*: but this cannot …rences which she has given to the observation …storia Cœlestis, of which I shall make mention …rly indicate and agree with 10 *Sextantis*. It is …nstellation *Leo*. See the note to No. 1310 above.

…he 4½ magnitude in the *British Catalogue*: but, in …nated, and twice as the 6th: I have therefore taken

…t this star is the same as 28 *Leonis*: but this cannot …rly 30′. All the references which she has given to …the *Historia Cœlestis*, of which I shall make mention …2) clearly indicate and agree with 11 *Sextantis*. It is …in the constellation *Leo*. The position of 28 *Leonis* in the …45″, and D = + 9° 46′ 0″: but this star does not exist. …r in computing the observation of 11 *Sextantis* on March 10, …it is stated to have passed the meridian nearly at the same …inutes and seconds which were inserted in the original entry …he pen; but in such a careless manner, that they might easily …h in fact is the value which Flamsteed has transcribed into his …23, page 81) and calculated the results accordingly. This has …of the star 28 *Leonis* above mentioned, which consequently does

Observed by Flamsteed on Feb. 21, 1694, at 10ʰ 46ᵐ 11ˢ. It is

Observed by Flamsteed on March 17, 1694, at 9ʰ 19ᵐ 9ˢ. In my list …ted stars, I have supposed this to be Piazzi IX. 229: it will not, how- …with that star in right ascension unless we suppose that it has a consider- …(equal at least to 4″ per annum); or that Flamsteed has made an error of …n the time of transit; and that 9ʰ 19ᵐ 9ˢ should be 9ʰ 19ᵐ 39ˢ.

Observed by Flamsteed on Feb. 21, 1702, at 10ʰ 46ᵐ 49ˢ. The declination …*Catalogue* is + 5° 50′ 40″; but on examining the steps of the process in MSS, …224, it will be seen that Flamsteed has made an error of 1°. I have therefore …value, and the position of the star will now agree with modern observations. It …in Halley's edition of 1712; and is Piazzi IX. 223.

…*nis Minoris*. Observed by Flamsteed on March 17, 1702, at 9ʰ 19ᵐ 56ˢ: and by …in *Hist. Cél.* page 150, at 9ʰ 50ᵐ 14ˢ, mag. 7½.

…*extantis*. Observed by Flamsteed on March 11, 1702, at 9ʰ 38ᵐ 57ˢ: but as no other …ere observed on that day (except No. 1423 of this catalogue) it is difficult to deduce the right ascension. It is probably the star observed by Lalande in *Hist. Cél.* page 324,

right ascension to 144° 38′ 30″; but without correcting the places of the other stars from which they were deduced.

1381. 15 *Leonis Minoris.* See the preceding note.

1383. — *Sextantis.* There are three distinct observations of this star by Flamsteed: viz. on Feb. 21, 1702, after $10^h\ 30^m\ 13^s$; again on March 20, at $8^h\ 53^m\ 52^s$; and again on the following day, at $8^h\ 50^m\ 8^s$. I cannot find any observation of it in Bradley, Piazzi, Lalande, or Bessel: but M. Argelander says that it is No. 10 *Sextantis* in Bode's great catalogue, 6th magnitude, observed by Koch; and that six observations of it at Abo give its position for 1830, $\mathcal{R} = 9^h\ 37^m\ 11^s,6$ and D = + 7° 29′ 23″,3.

1386. 16 *Leonis Minoris.* See the note to No. 1378 above.

1388. 3 *Sextantis.* Observed by Flamsteed on Feb. 28, 1702, at $10^h\ 9^m\ 45^s$. The determining star, assumed for the right ascension in MSS, vol. 23, page 224, was 1 *Sextantis* (= 10 *Leonis*): so that if an error was made in the time of transit of that star, it would affect the right ascension not only of this star, but also that of 5 *Sextantis*.

1389. 17 *Leonis Minoris.* Observed by Flamsteed on March 18, 1703, at $9^h\ 7^m\ 46^s$. The declination in the *British Catalogue* is +39° 10′ 0″: but on examining the steps of the process, from which it is deduced in MSS, vol. 26 A, page 28, it appears that Flamsteed has not corrected for the error of the instrument, but has assumed 12° 21′ 40″, as the true zenith distance instead of 12° 11′ 30″. We must therefore increase the declination by 10′ 10″, which I have accordingly done. See the note to No. 1378 above. In the *British Catalogue*, this star is marked as of the 6th magnitude: but in the observations, it is designated as the 7th; which I have therefore retained.

1391. 22 *Leonis g.* In the observation of this star by Flamsteed on March 25, 1690, at $8^h\ 37^m\ 21^s$, he remarks that it has a small star preceding it to the north. This is probably the star alluded to by Piazzi in the note to his IX. 190.

1394. 5 *Sextantis.* Observed by Flamsteed on Feb. 28, 1702, at $10^h\ 12^m\ 11^s$. The right ascension ought to be increased about 4′, in order to correspond with modern observations: but see the note to No. 1388 *supra.*

1396. — *Leonis Minoris.* Observed by Flamsteed on March 18, 1703, after $9^h\ 7^m\ 46^s$. It is the star observed by Lalande in *Hist. Cél.* page 323, at $9^h\ 40^m\ 58^s,2$, mag. 6½.

1402. *Sextantis.* Observed by Flamsteed on Feb. 21, 1702, after $10^h\ 41^m\ 6^s$; again on March 9, at $9^h\ 39^m\ 44^s$; again on March 20, at $9^h\ 1^m\ 28^s$; and again on the following day at $8^h\ 57^m\ 44^s$. It is the star observed by Lalande in *Hist. Cél.* page 226, at $9^h\ 43^m\ 30^s,5$, mag. 6.

1404. — *Leonis Minoris.* Observed by Flamsteed on March 18, 1703, after $9^h\ 12^m\ 57^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 352, at $9^h\ 45^m\ 10^s,5$, mag. 7.

1405. — *Ursæ Majoris.* Observed by Flamsteed on March 17, 1694, at $9^h\ 14^m\ 12^s$; and again on Jan. 23, 1696, after $12^h\ 32^m\ 25^s,5$. Both these observations have been reduced in MSS, vol. 23, pages 369 and 379: and the result is given in Halley's edition of 1712. Why it was afterwards rejected I am unable to ascertain. It is not Piazzi IX. 211, as suggested by me in my list of Flamsteed's inedited stars; but (as M. Argelander justly remarks) it is No. 118

Ursæ Majoris in Bode's great catalogue: and occurs in *Mém. de l'Acad.* for 1790, page 380, at $9^h\ 45^m\ 23^s{,}7$, mag. $5\frac{1}{2}$.

1408. 10 *Sextantis.* Miss Herschel says that this star is the same as 25 *Leonis*: but this cannot be, as their positions do not agree. All the references which she has given to the observation of 25 *Leonis* (except that to page 436 of the *Historia Cœlestis*, of which I shall make mention in the sequel, see the note to No. 1418) clearly indicate and agree with 10 *Sextantis*. It is inserted in Halley's edition of 1712, in the constellation *Leo*. See the note to No. 1310 above.

1410. 27 *Leonis* ν. This star is marked as of the $4\frac{1}{2}$ magnitude in the *British Catalogue*: but, in the *original* entries, it is only once so designated, and twice as the 6th: I have therefore taken the mean.

1411. 11 *Sextantis.* Miss Herschel says that this star is the same as 28 *Leonis*: but this cannot be, as their right ascensions differ nearly 30′. All the references which she has given to 28 *Leonis* (except that to page 436 of the *Historia Cœlestis*, of which I shall make mention in the sequel, see the note to No. 1422) clearly indicate and agree with 11 *Sextantis*. It is inserted in Halley's edition of 1712 in the constellation *Leo*. The position of 28 *Leonis* in the *British Catalogue* is $AR = 145°\ 53'\ 45''$, and $D = +\ 9°\ 46'\ 0''$: but this star does not exist. Its introduction arose from an error in computing the observation of 11 *Sextantis* on March 10, 1691. In the observation book it is stated to have passed the meridian nearly at the same time with ν *Leonis*, and the minutes and seconds which were inserted in the original entry (MSS, vol. 5) are erased with the pen; but in such a careless manner, that they might easily be mistaken for $44^m\ 0^s$: which in fact is the value which Flamsteed has transcribed into his computing book (MSS, vol. 23, page 81) and calculated the results accordingly. This has given rise to the introduction of the star 28 *Leonis* above mentioned, which consequently does not exist.

1412. — *Leonis Minoris.* Observed by Flamsteed on Feb. 21, 1694, at $10^h\ 46^m\ 11^s$. It is Piazzi IX. 221.

1414. — *Ursæ Majoris.* Observed by Flamsteed on March 17, 1694, at $9^h\ 19^m\ 9^s$. In my list of Flamsteed's inedited stars, I have supposed this to be Piazzi IX. 229: it will not, however, strictly agree with that star in right ascension unless we suppose that it has a considerable proper motion (equal at least to 4″ per annum); or that Flamsteed has made an error of 30^s in noting down the time of transit; and that $9^h\ 19^m\ 9^s$ should be $9^h\ 19^m\ 39^s$.

1415. 12 *Sextantis.* Observed by Flamsteed on Feb. 21, 1702, at $10^h\ 46^m\ 49^s$. The declination in the *British Catalogue* is $+\ 5°\ 50'\ 40''$; but on examining the steps of the process in MSS, vol. 23, page 224, it will be seen that Flamsteed has made an error of 1°. I have therefore deducted this value, and the position of the star will now agree with modern observations. It is inserted in Halley's edition of 1712; and is Piazzi IX. 223.

1417. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $9^h\ 19^m\ 56^s$: and by Lalande in *Hist. Cél.* page 150, at $9^h\ 50^m\ 14^s$, mag. $7\frac{1}{2}$.

1418. — *Sextantis.* Observed by Flamsteed on March 11, 1702, at $9^h\ 38^m\ 57^s$: but as no other stars were observed on that day (except No. 1423 of this catalogue) it is difficult to deduce the correct right ascension. It is probably the star observed by Lalande in *Hist. Cél.* page 324,

at $9^h\ 50^m\ 13^s,3$. I conceive that Flamsteed was desirous of observing some stars to compare with Venus on that day; and that those two were the only ones that presented themselves.

1419. — *Leonis.* Observed by Flamsteed on March 27, 1690, after $8^h\ 38^m\ 37^s$; again on March 25, 1696, after $8^h\ 53^m\ 23^s$; again on April 6, 1709, at $8^h\ 8^m\ 3^s$; and again on April 8, at $7^h\ 59^m\ 23^s$. It is Piazzi IX. 230 = (15 Hev.) *Leonis.*

1420. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $9^h\ 21^m\ 49^s$: and by Lalande, in *Hist. Cél.* page 210, at $9^h\ 52^m\ 10^s,3$, mag. $7\frac{1}{2}$.

1421. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $9^h\ 22^m\ 5^s$: and by Lalande, in *Hist. Cél.* page 210, at $9^h\ 52^m\ 28^s$, mag. $7\frac{1}{2}$.

1422. — *Sextantis.* Observed by Flamsteed on March 11, 1702, at $9^h\ 41^m\ 13^s$: but, for the reasons stated in the note to No. 1418 above, it is difficult to deduce the correct right ascension. It is probably the star observed by Lalande in *Hist. Cél.* page 324, at $9^h\ 52^m\ 2^s,7$, mag. 8. Miss Herschel has supposed that this star and No. 1418 of the present catalogue, are No. 10 and 11 *Sextantis:* but neither the difference in right ascension nor in declination will agree with this supposition; as may be seen by the following comparison.

		Difference in R.		Difference in Dec.		
		m	s	°	′	″
Nos. 10 and 11 *Sextantis*	Flamsteed	1	42,7	0	36	30
	Bradley	1	42,5	0	36	34
Nos. 1418 and 1422 *hujus*	Flamsteed	2	16,0	0	42	50
	Lalande	2	17,5	0	43	1

I have therefore considered them as distinct and separate stars.

1423. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $9^h\ 23^m\ 25^s$: and by Lalande, in *Hist. Cél.* page 60, at $9^h\ 52^m\ 57^s,5$, mag. $7\frac{1}{2}$.

1425. — *Sextantis.* Observed by Flamsteed on Feb. 21, 1702, at $10^h\ 51^m\ 45^s$; and again on March 9, 1702, at $9^h\ 50^m\ 51^s$. It is the star observed by Lalande in *Hist. Cél.* page 327, at $9^h\ 54^m\ 23^s,2$, mag. $6\frac{1}{2}$.

1426. — *Leonis.* Observed by Flamsteed on March 6, 1696, at $10^h\ 9^m\ 28^s$; and again on the following day, at $10^h\ 5^m\ 21^s$: but the two observations will not agree in right ascension, unless we suppose an error of 1^m in noting down the time of the first observation; and that we read $10^h\ 8^m\ 28^s$, instead of $10^h\ 9^m\ 28^s$. No such error, however, is discoverable in the original MSS: but as Flamsteed was observing the same stars on those two days, for the purpose of comparing them with *Mars*, I consider the conjecture very plausible: and with this correction the star is Piazzi IX. 240 = 431 Mayer.

1429. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, after $9^h\ 26^m\ 4^s$: and supposed by Miss Herschel to be 22 *Leonis Minoris.* It is however Piazzi IX. 246.

1437. 17 *Sextantis.* The right ascension of this star, in the early MS catalogue, is 148° 41′ 15″: and this is the value inserted in Halley's edition of 1712. Why it was afterwards altered I cannot imagine, as all the observations agree very well together, and show that the former reading is the most correct.

1439. — *Hydræ.* Observed by Flamsteed on March 19, 1691, after $9^h\ 24^m\ 54^s$; where it is called *Comes* λ *Hydræ.* The time of transit is not given: but both the right ascension and declination are given in Halley's edition of 1712; from which I have copied them. It is the star observed by Lalande in *Hist. Cél.* page 328, at $10^h\ 0^m\ 21^s{,}5$, mag. 6.

1441. 41 *Hydræ* λ. In the observation of this star on April 3, 1690, at $8^h\ 20^m\ 15^s$, it is stated that it has, besides the companion, No. 1439 of this catalogue, another companion to the north. This last-mentioned star is probably the one observed by Lalande in *Hist. Cél.* page 326, at $9^h\ 59^m\ 21^s{,}5$, mag. $6\frac{1}{2}$.

1443. — *Sextantis.* Observed by Flamsteed on Feb. 28, 1702, after $10^h\ 32^m\ 24^s$; and again on March 9, 1702, after $9^h\ 58^m\ 55^s$. It is Piazzi X. 6.

1445 — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, the second after $9^h\ 26^m\ 4^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 56, at $9^h\ 58^m\ 23^s{,}3$, mag. 6; but the declination does not very well agree.

1446. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, after $13^h\ 3^m\ 17^s$. M. Argelander supposes it to be Piazzi X. 26; which, by the great difference of precession in $\mathcal{R}$ would be in the field of the telescope at the time of transit of No. 1457 below.

1448. 20 *Sextantis.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entries it is once designated as the 7th, once as $6\frac{1}{2}$, and once as 6th. I have therefore altered the reading.

1449. 33 *Ursæ Majoris* λ. The right ascension of this star has been deduced *per distantias;* as may be seen in MSS, vol. 26 C, page 6.

1454. 36 *Leonis* ζ. The magnitude of this star is designated, in the *original* entries, once as the 4th, and once as $3\frac{1}{2}$. Piazzi makes it of the $4\frac{1}{2}$ magnitude.

1455. 37 *Leonis.* In the observation of this star, on March 10, 1691, at $10^h\ 0^m\ 19^s$, there is an error of 6° in the zenith distance; as may be readily ascertained by a comparison of the values *per lineas diagonales* and those *per strias cochleæ:* the latter of which is the correct reading. This error is corrected in the margin of the original MS entry (MSS, vol. 5): but Flamsteed has copied out the erroneous reading into his computation book; as may be seen in MSS, vol. 23, page 81. Hence has arisen the introduction of 38 *Leonis* into the *British Catalogue,* whose position there given is $\mathcal{R} = 150°\ 0'\ 0''$ and $D = +\ 21°\ 14'\ 50''$; but which consequently does not exist.

1457. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, at $13^h\ 3^m\ 17^s$; and regularly reduced by him in MSS, vol. 23, page 379. It is inserted in Halley's edition of 1712; where the position slightly differs from the present one. It is Piazzi X. 42 = (30 Hev.) *Ursæ Majoris.*

1462. — *Ursæ Majoris.* Observed by Flamsteed on March 17, 1702, at $9^h\ 39^m\ 37^s$: it is Piazzi X. 44. In the printed copy of the *Historia Cœlestis,* it is stated that this star has a companion preceding it: but in the original MS entry (MSS, vol. 7) this remark belongs to the next following star, No. 1471 of this catalogue. Upon the supposition that the printed copy is the correct reading, M. Argelander considers the accompanying star to be that which was observed by Lalande in *Hist. Cél.* page 8, at $10^h\ 6^m\ 59^s$; mag. 8.

1463. 34 *Ursæ Majoris* μ. The right ascension of this star was deduced *per distantias*: as may be seen in MSS, vol. 26 C, page 8.

1464. — *Leonis*. Observed by Flamsteed on Feb. 28, 1696, after $10^h\ 47^m\ 33^s$. M. Argelander thinks that it is probably the double star H. II. 43 = Struve 1426 = the star in Bessel's Zone 69, at $10^h\ 10^m\ 14^s{,}51$; if so, it passed the meridian on that day *before* the preceding star 42 *Leonis*.

1465. 23 *Sextantis*. This star is marked as of the 5th magnitude, in the *British Catalogue*: but in the *original* entries it is designated as the 6th; which I have therefore adopted.

1467. 42 *Leonis*. In the observation of this star by Flamsteed on March 25, 1690, at $9^h\ 8^m\ 15^s$, it is stated that it has a companion following it, to the north; which is probably the star observed by Lalande in *Hist. Cél.* page 222, at $10^h\ 12^m\ 16^s{,}4$, mag. 7.

1469. — *Leonis*. Observed by Flamsteed on Nov. 7, 1712, at $18^h\ 23^m\ 11^s$. It is Piazzi X. 51 = 445 Mayer.

1470. — *Leonis Minoris*. Observed by Flamsteed on Feb. 16, 1704, at $11^h\ 24^m\ 59^s$. It is Piazzi X. 53.

1471. 28 *Leonis Minoris*. In the original MS entry of the observation of this star on March 17, 1702, at $9^h\ 42^m\ 9^s$ (MSS, vol. 7), Flamsteed remarks that it has a companion preceding it. See the note to No. 1462 above.

1476. — *Sextantis*. Observed by Flamsteed on March 9, 1702, at $10^h\ 10^m\ 2^s$; and by Lalande, in *Hist. Cél.* page 277, at $10^h\ 12^m\ 48^s$, mag. $6\frac{1}{2}$.

1482. 26 *Sextantis*. Flamsteed has reduced only one of the observations of this star, viz. that on Feb. 21, 1702, at $11^h\ 13^m\ 53^s$: and in so doing he has as usual taken the reading of the zenith distance *per lineas diagonales* as the correct one, which differs 5′ from that *per strias cochleæ*. But that the latter is the correct reading may be seen from the observation made on March 9, 1702, at $10^h\ 12^m\ 58^s$. The declination in the *British Catalogue* is + 0° 38′ 45″: but I have deducted 5′ from this value, and it will then agree with modern observations.

1486. 28 *Sextantis*. Observed by Flamsteed on Feb. 28, 1702, at $10^h\ 48^m\ 36^s$; and regularly reduced by him in MSS, vol. 23, page 224. I cannot find any star that will correspond with it: and I suspect that an error of 2^m has been committed in making the original entry; and that $10^h\ 48^m\ 36^s$ should be $10^h\ 50^m\ 36^s$: in which case it would agree with 29 *Sextantis*. On examining the original MS entry, I find that the figure 4 had been originally a 5, but the other figures remain as when first entered. It is not Piazzi X. 86.

1488. — *Leonis*. Observed by Flamsteed on April 5, 1701, at $8^h\ 36^m\ 40^s$. It is Piazzi X. 83 = 449 Mayer.

1489. — *Leonis*. Observed by Flamsteed on April 3, 1690, after $8^h\ 35^m\ 45^s$. It is Piazzi X. 85 = 450 Mayer.

1490. — *Sextantis*. Observed by Flamsteed on March 9, 1702, at $10^h\ 15^m\ 11^s$. It is Bradley, No. 1456.

1491. 29 *Sextantis.* Observed by Flamsteed on Feb. 21, 1702, at $11^h\ 16^m\ 49^s$; and again on March 9, 1702, at $10^h\ 15^m\ 54^s$. The former only was reduced by him in MSS, vol. 23, page 224: but on examining the steps of the process, he has committed an error of 1° in the zenith distance, whereby he has made the declination in the *British Catalogue* = − 0° 10′ 30″, instead of − 1° 10′ 30″, as stated in the present catalogue, which is the correct value. It is Piazzi X. 86; which he and Bessel have erroneously styled 28 *Sextantis.*

1492. — *Sextantis.* Observed by Flamsteed on March 8, 1702, at $10^h\ 19^m\ 55^s$: and by Lalande in *Hist. Cèl.* page 275, at $10^h\ 18^m\ 50^s{,}4$, mag. 7.

1493. 33 *Leonis Minoris.* This star is marked as of the 4½ magnitude, in the *British Catalogue*: but, in the *original* entries, it is first designated as the 6th, then as 5½, and lastly as 4th. I have taken the mean. Perhaps it is a variable star.

1497. — *Leonis Minoris.* Observed by Flamsteed on Feb. 5, 1690, at $6^h\ 6^m\ 44^s$; and regularly reduced by him in MSS, vol. 23, page 289. It is also inserted in Halley's edition of 1712, in the constellation of *Ursa Major*; from which the present position is taken: and I know not why it was afterwards rejected. It is the star observed by Lalande in *Hist. Cèl.* page 8, at $10^h\ 20^m\ 6^s{,}2$, mag. 5.

1498. 46 *Leonis i.* Although this star is called *i* in the *British Catalogue*, and here retained, yet it is not in the position of the star designated by that letter in Bayer's map: which, in fact, accords more nearly with 34 *Leonis.* But it is evident, on comparing Ptolemy's catalogue with that of Tycho, that there is some confusion in the latter, which it is difficult to rectify at the present day. I have not therefore made any alteration.

1499. 34 *Leonis Minoris.* This star is marked as of the 4½ magnitude, in the *British Catalogue*: but in the *original* entries, it is first designated as the 7th, then as the 6th, two days afterwards as 4½, and lastly as the 6th again: I have therefore taken the mean.

1502. 43 *Hydræ* ϕ^1. This star is called ϕ^1 by Flamsteed: but there is only one star designated by that letter in Bayer's map; but as it may be doubted whether such letter belongs to 1 *Crateris*, 2 *Crateris*, or to this star, Flamsteed has annexed it to each; which I have here retained.

1506. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, the second after $13^h\ 22^m\ 18^s$. It is the star No. 168 *Ursæ Majoris*, in Bode's great catalogue; and was observed by Lalande in *Mém. de l'Acad.* for 1790, page 386, at $10^h\ 25^m\ 26^s$, mag. 5½.

1509. — *Hydræ.* Observed by Flamsteed on April 6, 1691, at $8^h\ 42^m\ 50^s$, and regularly reduced by him in MSS, vol. 23, page 227: but is omitted in the collected list of the stars in the constellation *Hydra*, in page 274 of the same volume. It was observed by Bradley; and is No. 1472 in his catalogue.

1510. — *Ursæ Majoris.* Observed by Flamsteed on Jan. 23, 1696, after $13^h\ 22^m\ 18^s$. It is Piazzi X. 126 = (35 Hev.) *Ursæ Majoris.*

1511. 38 *Ursæ Majoris.* The right ascension of this star in the *British Catalogue* is 155° 47′ 40″: but it is evident, on comparing the two observations of this star, with the time of transit of other stars on the same days, that an error of 3^m has been committed. The right ascension here adopted, is taken from Halley's edition of 1712: which agrees with the computations

made by Flamsteed himself in MSS, vol. 23, pages 369 and 379; and vol. 26 B, page 15. Why it was afterwards altered I have not been able to ascertain.

1512. 37 *Leonis Minoris.* This star is marked as of the 3rd magnitude, in the *British Catalogue:* but in the *original* entries it is designated three times as the 6th, once as the 4th, and once as $4\frac{1}{2}$, but nowhere greater. I have taken the mean of the whole.

1513. 1 *Crateris* φ^2. This star is called φ^2 by Flamsteed: but there is only one star designated by that letter in Bayer's map. Yet as it may be doubted whether such letter was intended for 43 *Hydræ*, 2 *Crateris*, or for this star, Flamsteed has annexed that letter to each; which I have here retained.

1514. 38 *Leonis Minoris.* The right ascension of this star in the *British Catalogue* is 155° 22′ 30″. It was observed on March 17, 1702, at $9^h\ 57^m\ 12^s$; and reduced by Flamsteed in MSS, vol. 23, page 82: but in the steps of the process he has made an error of 10^s; whereby the right ascension in that catalogue is 2′ 30″ too great; and which I have here deducted.

1516. 39 *Leonis Minoris.* In the observation of this star on March 11, 1703, at $10^h\ 26^m\ 37^s$, Flamsteed remarks that it has a companion following it; which is probably the star observed by Lalande in *Hist. Cél.* page 63, at $10^h\ 29^m\ 30^s{,}5$, mag. 8. In the manner in which the note is printed in the *Historia Cœlestis*, vol. 2, page 457, it would appear that it belonged to the preceding star, 36 *Leonis Minoris:* but, on referring to the MSS copy (MSS, vol. 16), I find that it refers to the present star.

1517. 2 *Crateris* φ^2. This star perhaps is strictly φ *Hydræ* in Bayer's map: but see the notes to Nos. 1502 and 1513.

1519. — *Leonis Minoris.* Observed by Flamsteed on March 17, 1702, at $10^h\ 0^m\ 35^s$. It is Piazzi X. 131.

1520. — *Ursæ Majoris.* Observed by Flamsteed on April 19, 1694, at $8^h\ 1\frac{1}{2}^m$; and again on the following day, at $7^h\ 57^m\ 34^s$. It is Piazzi X. 135 = (36 Hev.) *Ursæ Majoris.*

1522. 40 *Leonis Minoris.* The declination of this star in the *British Catalogue* is + 27° 53′ 15″. It is deduced by Flamsteed from the observation made March 15, 1702, at $10^h\ 4^m\ 33^s$, as appears by MSS, vol. 23, page 82; where the declination is stated to be 3′ less than in the *British Catalogue:* and such also is the value given in Halley's edition of 1712, where this star is inserted in the constellation *Leo.* Why it was afterwards altered I have not been able to ascertain: but as it is evident that an error of 3′ has been committed, I have here corrected it. Probably it was an error of the press.

1532. 52 *Leonis k.* This star is designated in the *British Catalogue* by the capital letter K; which is erroneous, as Bayer never uses the *capital* letter, except to the first letter of the Roman alphabet.

1537. 3 *Crateris.* The letter *b* annexed by Flamsteed to this star, and to Nos. 1547 and 1553 of the present catalogue, is so far erroneous, that it ought strictly to be *b Hydræ.* Bayer's stars in *Crater* do not exceed 11.

1540. 39 *Sextantis.* The position of this star in the *British Catalogue* is $\mathcal{R}$ = 158° 18′ 10″, and D = − 7° 23′ 20″. It was observed by Flamsteed on March 8, 1702, at $10^h\ 39^m\ 30^s$: but in

reducing it, he has made two mistakes; as may be seen in MSS, vol. 23, page 226. In the first place, he has copied out the time of transit 10ˢ too small; and secondly, he has made an error of 5′ in the zenith distance. Both these errors are here corrected, and its position will now correspond with modern observations.

1542. 4 *Crateris.* This star is called *ν* in the *British Catalogue:* but it is *ν Hydræ* in Bayer's map; his stars in *Crater* do not exceed 11.

1547. 5 *Crateris.* Observed by Flamsteed on March 19, 1691, at $10^h\ 5^m\ 16^s$. The declination in the *British Catalogue* is $-16°\ 51'\ 45''$: but the reading *per lineas diagonales* differs $9'\ 15''$ from that *per strias cochleæ.* Flamsteed assumed the former as the correct reading, and deduced the declination accordingly: but from some additional figures inserted by him in MSS, vol. 23, page 227, it is evident that he considered that an error of about 10′ had been committed: whence it would appear that the reading *per strias cochleæ* is the correct reading. I have therefore deducted $9'\ 15''$ from the value above stated; and it will then agree with modern observations. See the note to No. 1537.

1549. — *Sextantis.* Observed by Flamsteed on March 3, 1696, after $10^h\ 50^m\ 22^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 275, at $10^h\ 41^m\ 21^s$, mag. 6.

1551. 48 *Leonis Minoris.* The declination in the *British Catalogue* is $+\ 27°\ 4'\ 5''$. It was observed by Flamsteed on March 17, 1702, at $10^h\ 13^m\ 31^s$; and regularly reduced by him, in MSS, vol. 23, page 82: and the declination is there correctly stated to be $+\ 27°\ 7'\ 55''$, as I have given it. It is again reduced by him, but incorrectly, in MSS, vol. 26 A, page 28: which has led to the error in his catalogue.

1553. 6 *Crateris.* See the note to No. 1537.

1563. 50 *Ursæ Majoris α.* The right ascension of this star was deduced *per distantias:* see MSS, vol. 26 C, page 9, and vol. 62 D, pages 20 and 22.

1568. 61 *Leonis* p^1. This star has no letter annexed to it in the *British Catalogue:* but I believe it to be the first of that group of 5 stars, designated by the letter *p* in Bayer's map; and I have therefore inserted it. I am aware that there may be some doubt with respect to *this* star, as the position does not exactly coincide with that of Bayer: but I think that the error is in Bayer's map: and there is no other star that will correspond with it. The other four stars agree very well with Bayer. See the note to No. 1572.

1569. 51 *Ursæ Majoris.* This star is marked as of the 7th magnitude in the *British Catalogue:* but in the *original* entries it is so designated once, and as $5\frac{1}{2}$ once; I have therefore taken the mean.

1570. 8 *Crateris.* I cannot account for the great difference in the declination of this star. By the observation it was $4°\ 0'\ 30''$ south of 11 *Crateris:* allowance being made for the difference of refraction according to Flamsteed's tables. This would reduce the difference. In the *British Catalogue* it has the Greek letter *ι* annexed to it: but the star so designated by Bayer is 24 *Crateris;* and the present star is not noticed by him. I have therefore rejected the letter here, as it would only confuse; more especially as 35 *Hydræ* is also designated by this letter.

1571. — *Leonis.* Observed by Flamsteed on April 15, 1690, after $7^h\ 46^m\ 51^s$. It is the star observed by Lalande in *Hist. Cél.* page 226, at $10^h\ 53^m\ 35^s{,}4$, mag. $7\frac{1}{2}$.

1572. 62 *Leonis* p^3. This star is called *g* in the *British Catalogue*: but that letter has been already affixed to 22 *Leonis*, to which it properly belongs. Bayer (whom Flamsteed professed to follow), calls it *p*; at least it is one of the group of five stars to which he applied that letter; and I have therefore restored it. The other four stars which form the group are 61, 65, 66, and 69 *Leonis*.

1576. 9 *Crateris*. In the *original* MS entry of the observation of this star, on April 6, 1691, at $9^h\ 12^m\ 57^s$, it is stated that it has a smaller star following it to the north. This companion is Piazzi X. 240. The present star is χ *Hydræ* in Bayer's map: his stars in *Crater* do not exceed 11.

1578. 65 *Leonis* p^3. This star has no letter annexed to it in the *British Catalogue*: but it is the third of the group of 5 stars designated by the letter *p* in Bayer's map; and I have therefore retained it. See the note to No. 1572.

1579. 66 *Leonis* p^4. This star was not observed by the mural arc. In the MS catalogues of the Zodiacal stars, and in the collection of stars in the constellation *Leo*, in MSS, vol. 23, page 102, the position of this star is said to have been obtained *per distantias*. I have not yet been able, however, to discover the observation from which it has been deduced. The right ascension must be increased nearly 25′ in order to correspond with the star which is supposed by Bradley and Piazzi to be the one intended by Flamsteed. In the *British Catalogue* there is no letter annexed to this star: but it is the fourth in the group of five stars designated by the letter *p* in Bayer's map; I have therefore inserted it. See the note to No. 1572.

1580. 67 *Leonis*. This is the same star as 53 *Leonis Minoris*.

1582. 10 *Crateris*. The declination of this star in the *British Catalogue* is $-26°\ 27'\ 35''$: this however is nearly 3′ greater than it ought to be. On examining the original MS entry, I find that the reading *per strias cochleæ* was at first entered 1765.68; but that it has been altered to 1766.58. I have no doubt that the former is the correct reading; and I have therefore deducted 2′ 40″ from Flamsteed's value; and it will now more nearly agree with modern observations.

1584. — *Leonis*. Observed by Flamsteed on April 15, 1690, at $8^h\ 41^m\ 15^s$. It is Piazzi XI. 9 = 469 Mayer.

1586. 69 *Leonis* p^5. This star has no letter annexed to it in the *British Catalogue*: but it is the last of the group of 5 stars designated by the letter *p* in Bayer's map; and I have therefore retained it. See the note to No. 1572.

1587. — *Leonis*. Observed by Flamsteed on April 7, 1697, at $9^h\ 11^m\ 55^s$; again on Jan. 31, 1714, at $13^h\ 25^m\ 47^s$; and again on Feb. 15, 1714, at $12^h\ 22^m\ 43^s$. It is Piazzi XI. 12.

1589. — *Leonis*. Observed by Flamsteed on March 30, 1702, at $9^h\ 45^m\ 26^s$. It is the star observed by Lalande in *Hist. Cél.* page 325, at $11^h\ 2^m\ 54^s{,}5$, mag. 6.

1592. — *Ursæ Majoris*. Observed by Flamsteed on April 16, 1694, at $8^h\ 48^m$ nearly. It is probably Piazzi XI. 19.

1595. 53 *Ursæ Majoris* ξ. The right ascension of this star has been deduced *per distantias*: as may be seen in MSS, vol. 26 C, page 8.

1598. 76 *Leonis*. This star is marked as of the 7th magnitude in the *British Catalogue*: but in

the *original* entries it is so designated only once, whilst it is three times called 6th; which I have therefore adopted.

1599. — *Draconis.* Observed by Flamsteed on April 12, 1694, at $5^h\ 7^m$ nearly. It is probably Piazzi XI. 43.

1603. 71 *Leonis.* Observed by Flamsteed on April 8, 1691, at $9^h\ 13^m\ 23^s$, or at $9^h\ 20^m\ 57^s$: but this observation, as well as the 4 immediately following it, are left out in the printed copy of the *Historia Cœlestis.* In the original MS entry (MSS, vol. 5) the time of transit was originally written $9^h\ 13^m\ 23^s$; but this is altered to $9^h\ 20^m\ 57^s$; which is afterwards erased, and the former value again written in the margin: from which the right ascension in the *British Catalogue* ($= 164°\ 39'\ 0''$) has been deduced, as may be seen in MSS, vol. 23, page 83. I suspect however that $9^h\ 20^m\ 57^s$ is the correct reading: in which case it will correspond with the star observed by Lalande in *Hist. Cél.* p. 332, at $11^h\ 12^m\ 5^s$, mag. $6\frac{1}{2}$. In the MS catalogue in MSS, vol. 25, page 51, the right ascension is $166°\ 33'\ 30''$; which I have here restored.

1605. 13 *Crateris* λ. The declination of this star in the *British Catalogue* is $-17°\ 7'\ 5''$. It was observed by Flamsteed on April 6, 1691, at $9^h\ 30^m\ 31^s$; and regularly and correctly reduced by him in MSS, vol. 23, page 229: but in copying out the stars in this constellation in page 274 of the same volume, he has made an error of $2'$; which I have here corrected.

1610. 82 *Leonis.* Observed by Flamsteed on March 25, 1690, at $10^h\ 12^m\ 46^s$. The declination in the *British Catalogue* is $+\ 5°\ 4'\ 35''$: but it should be remarked, that the reading *per strias cochleæ* differs $5'$ from that *per lineas diagonales.* Flamsteed has assumed the latter as the correct reading, but it is evident, from modern observations, that the former is the accurate one. I have therefore deducted $5'$ from the above value.

1611. 80 *Leonis.* The right ascension of this star in the *British Catalogue* is $167°\ 5'\ 30''$; which has been deduced by Flamsteed, in MSS, vol. 23, page 87, from the observation made by him on Feb. 28, 1696, at $11^h\ 51^m\ 39^s$: but this time of transit will not correspond with the position of any star in that parallel of declination. On referring to the *original* MS entry, in MSS, vol. 6, I find that, besides the time above stated, there is also inserted $11^h\ 52^m\ 12^s$, against which there is no zenith distance inserted. These figures, I know not for what reason, are omitted in the printed copy of the *Historia Cœlestis.* I apprehend therefore that an error has been made in the original entry; and that this omitted time of transit should have been inserted opposite to the zenith distance $46°\ 1'\ 40''$: in which case the recorded time of transit ($11^h\ 51^m\ 39^s$) would probably refer to the passage of some other star in *Leo.* There is another observation of this star by Flamsteed on Feb. 27, 1715, at $11^h\ 51^m\ 15^s$: from which the position in the present catalogue is taken: and which, by a comparison of *d Leonis*, which was observed on the same days, confirms the view I have taken of the subject; and which is the more important, as the star was very near the planet *Uranus* at that period.

1612. — *Leonis.* Observed by Flamsteed on March 28, 1691, at $10^h\ 6^m\ 42^s$. It is Piazzi XI. 69.

1620. — *Draconis.* Observed by Flamsteed on April 12, 1694, at $9^h\ 17^m\ 18^s$: and regularly reduced by him in MSS, vol. 23, page 371. It is inserted in Halley's edition of 1712, under the constellation of *Ursa Minor*, where the right ascension is stated to be $168°\ 40'\ 0''$; and I know not why it was afterwards rejected. It is No. 273 *Ursæ Majoris* in Bode's great catalogue; and may be found also in *Mém. de l'Acad.* for 1790, page 382, at $11^h\ 20^m\ 27^s{,}6$, mag. 5.

1627. 19 *Crateris.* This is ξ *Hydræ* in Bayer's map: his stars in *Crater* do not exceed 11.

1630. 20 *Crateris.* It appears from a comparison of Mr. Henderson's observation of this star, at the Cape of Good Hope, with Bradley's value, that it has a proper motion, in declination, of + 0″,81: which, being applied to Flamsteed's value, would make the difference equal to — 46″,7. It appears likewise to have a proper motion in right ascension of about the same amount.

1631. — *Ursæ Majoris.* Observed by Flamsteed on Feb. 21, 1696, after $12^h\ 25^m\ 57^s$: in the *original* MS entry (MSS, vol. 6), however, it is stated that it passed the meridian 2^m before that star (59 *Ursæ Majoris*). It therefore appears to be Piazzi XI. 111.

1633. 22 *Crateris.* Observed by Flamsteed on Feb. 17, 1702, at $12^h\ 38^m\ 45^s$. The declination in the *British Catalogue* is — 32° 50′ 55″, but it is evident, on a comparison with the zenith distances of other stars observed on that day, that an error of 1° has been committed. I have therefore deducted that quantity from the above value, and it will then agree with modern observations. It is Piazzi XI. 115 = 1024 La Caille; and not No. 117 in Piazzi's catalogue, as he has erroneously supposed. There appears to have been some fundamental error in the right ascension of the determining star for those observed in the constellation *Crater* on this day, as they differ considerably from modern results. See 8, 17, 18, 19, 20, 22, 23, 25, 26, 28, and 29 *Crateris:* all of which have been observed only once by Flamsteed, and on this day.

1640. — *Ursæ Majoris.* Observed by Flamsteed on March 17, 1694, at $10^h\ 58^m\ 59^s$; and regularly reduced by him in MSS, vol. 23, page 371. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken. Why it was afterwards rejected, I am unable to ascertain. It is No. 293 *Ursæ Majoris* in Bode's great catalogue: and occurs twice in the *Mém. de l'Acad.* for 1790: viz. page 380, at $11^h\ 28^m\ 57^s$, mag. 6; and page 381 at $11^h\ 29^m\ 1^s,4$, mag. 7½.

1643. 25 *Crateris.* This is o *Hydræ* in Bayer's map: his stars in *Crater* do not exceed 11.

1647. — *Leonis.* Observed by Flamsteed on Dec. 3, 1714, at $18^h\ 4^m\ 32^s$: in the *original* MS entry it is marked of the 7th magnitude. I cannot find any star that will correspond with this observation. M. Argelander thinks that it may be *Uranus*, whose position on that day, at $17^h\ 54^m\ 57^s$, mean time at Greenwich, was $11^h\ 29^m\ 1^s,94$ (= 172° 15′ 29″) and D = + 4° 11′ 6″,5: but the great difference in the declination is against this supposition.

1652. — *Ursæ Majoris.* Observed by Flamsteed on March 17, 1694, at $11^h\ 5^m\ 48^s$; and regularly reduced by him in MSS, vol. 23, page 371. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken. Why it was afterwards rejected I am unable to ascertain. It is No. 312 *Ursæ Majoris* in Bode's great catalogue; and is also in *Mém. de l'Acad.* for 1790, page 380, at $11^h\ 35^m\ 43^s,9$, mag. 5.

1654. 4 *Virginis* A^1. This star is designated by the letter ξ, and called ξ^3, in the *British Catalogue:* but it is evidently the first of the two stars indicated by the letter *A*, in Bayer's map. I have therefore restored the correct reading. See the Introduction, page 399.

1655. — *Leonis.* Observed by Flamsteed on April 25, 1690, at $8^h\ 35^m\ 35^s$; and again on April 24, 1692, at $8^h\ 44^m\ 4^s$. It is Piazzi XI. 160; and was observed also by Bradley.

1656. — *Leonis.* Observed by Flamsteed on March 3, 1713, at $11^h\ 59^m\ 8^s$. It is the star observed by Lalande in *Hist. Cél.* page 229, at $11^h\ 39^m\ 10^s,5$, mag. 6.

1658. — *Leonis.* Observed by Flamsteed on April 8, 1691, after $9^h\ 47^m\ 54^s$: but in the *original* MS entry it is said to have passed *eodem momento cum cauda Leonis*, and is stated to be of the 6th magnitude. It is No. 476 *Leonis* in Bode's great catalogue, observed by Messier.

1660. 28 *Crateris.* The letter β is annexed to this star: but it must be understood that it is β *Hydræ* both in Flamsteed's and in Bayer's map. The star properly called β *Crateris*, is No. 1583 of the present catalogue.

1661. 64 *Ursæ Majoris* γ. The right ascension of this star was deduced *per distantias*: as may be seen in MSS, vol. 26 C, page 8.

1662. — *Ursæ Majoris.* Observed by Flamsteed on April 16, 1694, after $9^h\ 26^m\ 24^s$: but probably passed *before* that time. It is the star observed by Lalande in *Hist. Cél.* page 9, at $11^h\ 42^m\ 3^s{,}3$, mag. 7.

1663. 65 *Ursæ Majoris.* In my comparison of this star with that observed by Bradley, I have taken that which is designated by Flamsteed's number in Bessel's catalogue: but I would remark that it would correspond equally well with the anonymous star immediately following.

1664. 6 *Virginis* A^2. This star is called *A* by Flamsteed: but it is the second of the two stars so indicated by Bayer. I have therefore called it A^2.

1668. 30 *Crateris* η. This star is marked as of the 4th magnitude in the *British Catalogue*: but in the *original* entry it is designated as $4\frac{1}{2}$; which I have therefore adopted. Still, it differs considerably from Piazzi.

1677. 10 *Virginis.* This star has the letter *r* annexed to it in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

1678. 11 *Virginis.* This star has the letter *s* annexed to it in the *British Catalogue*: but as there is no such letter in Bayer's map, I have here rejected it.

1680. 3 *Comæ Berenices.* The right ascension of this star in the *British Catalogue* is $178°\ 43'\ 30''$. It was observed by Flamsteed on April 16, 1692, at $9^h\ 35^m\ 47^s$; and reduced by him in MSS, vol. 23, page 301. But in the last step of the process he has made an error of 13^s in time: I have therefore deducted $3'\ 15''$ from the value in the *British Catalogue*; and it will now agree with modern observations.

1681. — *Comæ Berenices.* Observed by Flamsteed on April 11, 1692, at $9^h\ 54^m\ 26^s$; and reduced by him (but erroneously) in MSS, vol. 23, page 300. It is omitted however by him in his list of stars in that constellation in page 402 of the same volume. It is Piazzi XII. 3.

1686. — *Virginis.* Observed by Flamsteed on April 6, 1708, at $10^h\ 14^m\ 17^s$: but I cannot find any star in any catalogue that will answer to this observation. Miss Herschel supposes that there is an error in recording the time; and that it ought to be $10^h\ 15^m\ 41^s$; in which case it would agree with No. 1688 in this catalogue: but there is no authority for this in the MS entry (MSS, vol. 17); which stands exactly as it is printed in the *Historia Cœlestis.* It appears however extremely probable that this is the *same star* as that which had been observed on the two preceding evenings of observations for a comparison with Jupiter.

1687. 12 *Virginis.* This star has the letter *t* annexed to it in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

1688. — *Virginis.* Observed by Flamsteed on April 2, 1708, at $10^h\ 30^m\ 51^s$; and again on

April 5, at $10^h\ 19^m\ 27^s$. It is Piazzi XII. 16. None of the observations made by Flamsteed on March 30 and 31, and on April 2 and 5, 1708, although in the *printed* copy, are in the MS *copy* of Flamsteed's observations (MSS, vol. 17). The *original* book of entry is lost.

1691. 69 *Ursæ Majoris δ.* The declination of this star in the *British Catalogue* is $+ 58^\circ\ 46'\ 40''$; which has been deduced from the whole of the observations, which are reduced in MSS, vol. 23, pages 365—373. But on reducing them to the epoch of 1690, it is evident that Flamsteed has made an error of $1'$, which I have here corrected. The right ascension has been deduced *per distantias:* as may be seen in MSS, vol. 26 C, page 8.

1693. — *Draconis.* Observed by Flamsteed on April 12, 1694, at about $10^h\ 3\frac{1}{2}^m$: but the time is doubtful. M. Argelander thinks that it is the star observed by Lalande in *Mém. de l'Acad.* for 1790, page 387, at $12^h\ 4^m\ 56^s$, mag. 6; and again in page 388, at $12^h\ 4^m\ 55^s{,}6$, mag. $5\frac{1}{2}$.

1694. 2 *Canum Venaticorum.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but, in the *original* entry, it is designated as the 6th; which I have therefore retained.

1695. 7 *Comæ Berenices.* This star is designated by the letter *h*, in the *British Catalogue:* but see the *Introduction*, page 398.

1696. 13 *Virginis.* This star has the letter *n* annexed to it in the *British Catalogue:* but its position does not, in any way, accord with the star so designated in Bayer's map; which appears to have been introduced from the star called "In genu sinistro" in Tycho's catalogue. See the note to No. 1888.

1697. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 19^m\ 28^s$. It is Piazzi XII. 39.

1703. — *Virginis.* Observed by Flamsteed on April 21, 1712, at $9^h\ 23^m\ 17^s$. It is Piazzi XII. 35.

1705. 16 *Virginis c.* This star appears to be variable in magnitude. Bayer and Hevelius make it of the 6th magnitude; Piazzi of the $5\frac{1}{2}$; Mayer of the 5th; and Bradley of the 3rd. In the *British Catalogue*, it is marked as of the $3\frac{1}{2}$ magnitude: but in the *original* entries the magnitude is only once noted, and then as of the $6\frac{1}{2}$ only, and not as the 5th, as erroneously printed.

1707. — *Corvi.* Observed by Flamsteed on April 6, 1691, at $10^h\ 27^m\ 18^s$, and regularly reduced by him in MSS, vol. 23, page 229. It is inserted in Halley's edition of 1712, from which the position in the present catalogue is taken. Why it was afterwards rejected, I cannot ascertain. It is Piazzi XII. 54.

1709. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 21^m\ 47^s$. I cannot discover any star that will correspond with this observation, unless we suppose an error of 1^m in the time of transit; and that it ought to be $9^h\ 20^m\ 47^s$. In this case it will correspond with Piazzi XII. 52. See the note to No. 1711 below.

1711. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 21^m\ 51^s$. I cannot discover any star that will correspond with this observation, unless we suppose an error of 1^m in the time of transit, and that it ought to be $9^h\ 20^m\ 51^s$. In this case it will agree with the star observed by Lalande in *Hist. Cél.* page 64, at $12^h\ 9^m\ 15^s{,}5$, mag. 6. See the note to No. 1709 above.

1713. 12 *Comæ Berenices.* This star is designated by the letter *e* in the *British Catalogue:* but see the *Introduction*, page 398.

1715. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 24^m\ 33^s$. It is Piazzi XII. 68.

1717. 13 *Comæ Berenices.*
1721. 14 *Comæ Berenices.*
1722. 15 *Comæ Berenices.*
1723. 16 *Comæ Berenices.*

These stars are designated by the letters *f*, *b*, *c*, *a*, respectively, in the *British Catalogue*: but see the *Introduction*, page 398.

1724. 72 *Ursæ Majoris.* The right ascension of this star in the *British Catalogue* is $182^\circ\ 39'\ 15''$: it was observed by Flamsteed on April 19, 1694, at $9^h\ 47^m\ 41^s$; and regularly reduced by him in MSS, vol. 23, page 371; but was not included with the rest in his collection of stars in the constellation of *Ursa Major*. On examining the differences in the time of transit of this star and others that passed on the same day, in MSS, vol. 23, above-mentioned, it will be seen that an error of $15'\ 15''$ has been committed, which I have deducted from the value in the *British Catalogue*: and it will now agree with modern observations.

1726. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 29^m\ 13^s$. It is the star observed by Lalande in *Hist. Cél.* page 64, at $12^h\ 17^m\ 38^s{,}5$, mag. 6.

1727. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 29^m\ 18^s$. It is Piazzi XII. 96.

1728. 17 *Comæ Berenices.* This star is designated by the letter *d*, in the *British Catalogue*: but see the *Introduction*, page 398. It is likewise marked as of the $4\frac{1}{2}$ magnitude: but in the *original* entries it is both times designated as the 6th; which I have therefore adopted.

1729. 18 *Comæ Berenices.* Besides the observations of this star, indicated by Miss Herschel, there is another in the original MS book, on April 8, 1692, at $10^h\ 19^m\ 46^s$; the observations of which day are not printed in the *Historia Cœlestis*, on account of some irregularity in the clock. They have however been computed by Flamsteed in MSS, vol. 23, page 300: and in reducing this star he has erroneously copied out the zenith distance 3° too small; which has given rise to the star 19 *Comæ Berenices* inserted in the *British Catalogue*, whose position, as there stated, is $\mathrm{AR} = 183^\circ\ 29'\ 0''$, and $\mathrm{D} = +\ 28^\circ\ 49'\ 40''$; but which consequently does not exist. The present star is marked as of the 5th magnitude, in the *British Catalogue*: but, in the *original* entries, it is each time designated as the 6th; which I have therefore retained.

1733. 75 *Ursæ Majoris.* The right ascension of this star is not given in the *British Catalogue*: but I have assumed an approximate value corresponding with modern observations.

1734. 7 *Canum Venaticorum.* The right ascension of this star must be diminished upwards of $3\frac{1}{2}'$ in order to correspond with modern observations.

1735. 21 *Comæ Berenices.* This star is designated by the letter *g*, in the *British Catalogue*: but see the *Introduction*, page 398.

1739. 21 *Virginis q.* Bessel has erroneously called this 22 *Virginis*.

1740. — *Comæ Berenices.* Observed by Flamsteed on April 24, 1703, at $9^h\ 34\ 7^s$. It is the star observed by Lalande in *Hist. Cél.* page 65, at $12^h\ 22^m\ 33^s{,}5$, mag. 7.

1744. 23 *Comæ Berenices.* This star is designated by the letter *k*, in the *British Catalogue*: but see the *Introduction*, page 398.

1747. 25 *Virginis f.* The position of this star in the *British Catalogue* is $\mathcal{R} = 185° 31' 20''$, and $D = -4° 8' 45''$. There are 5 observations of it by Flamsteed, but I cannot find more than one of them (May 4, 1693, at $8^h 58^m$) that has been reduced by him, in MSS, vol. 23, page 97: and where only the N. P. D. is given; the time of transit being doubtful. In Halley's edition of 1712, the right ascension is $185° 16' 20''$; which accords more nearly with modern observations. But it is evident, on comparing the transits of this star with those of χ and γ *Virginis* on Dec. 13, 1698, and March 5, 1718, that an error of at least 18′ has been committed in deducing the right ascension; and which quantity I have deducted from the value given by Flamsteed. In the MS volume above-mentioned, page 108, the right ascension is said to have been deduced *per distantias;* and the declination is erroneous 1′, which I have here corrected. Probably it has been an error of the press, arising from a transposition of the figures denoting the minutes, and that 31 has been printed for 13.

1748. 22 *Virginis.*
1751. 23 *Virginis.*
1752. 24 *Virginis.*

These stars do not exist: neither can I find any observation of them in the *Historia Cœlestis.* I apprehend however that the present positions have been deduced from an erroneous computation of the three stars (Nos. 1968, 1970, and 1971 of this catalogue) which were observed on April 25, 1693, at $11^h 15^m 4^s$, $11^h 15^m 40^s$, and $11^h 15^m 46^s$. The *differences* in right ascension agree exactly: and there is not much discordance in the declinations; especially if we suppose an error of 1′ in 23 *Virginis*, and read $9° 41' 30''$, instead of $9° 40' 30''$. It is singular that all the stars in *Bootes*, observed on that day, are reduced in MSS, vol. 23, pages 335 and 337, except these three: and I find, in the *original* observation book (MSS, vol. 5), a marginal note by Flamsteed, that these three stars belong to the constellation *Virgo*, which is probably the reason why they were omitted; and perhaps, in computing their places afterwards, some unaccountable error was made in the right ascension of the determining star, which would reconcile the whole discordance.

1761. 76 *Ursæ Majoris.* The right ascension of this star in the *British Catalogue* is $186° 41' 45''$. It was observed twice by Flamsteed: viz. on April 19, 1694, at $10^h 4^m 6^s$; and again on April 27, at $9^h 32^m 42^s$: and both observations are reduced by him in MSS, vol. 23, page 373: and the results agree with the value given by him in his catalogue. The first reduction however is marked as doubtful; and the second has a note against it that the value is 1^m too little. In fact, by comparing the time of transit with that of other stars on the same days, it will be seen that such an error has been committed. I have therefore added 15′ to Flamsteed's value.

1762. — *Virginis.* There are 5 observations of this star by Flamsteed: viz. on March 13, 1698, at $12^h 16^m 56^s,5$; again on March 22, at $11^h 47^m 41^s$; again on March 23, at $11^h 44^m 32^s$; again on March 6, 1716, at $12^h 38^m 23^s$; and again on March 5, 1718, at $12^h 46^m 16^s$. It is the star observed by Lalande in *Hist. Cél.* page 333, at $12^h 33^m 25^s,6$, mag. 6.

1770. 36 *Virginis.* This is the same star as 29 *Comæ Berenices.*

1774. — *Ursæ Majoris.* Observed by Flamsteed on April 19, 1694, at $10^h 11^m 10^s$; and regularly reduced by him in MSS, vol. 23, page 373. Why it was not included in the *British Catalogue* I cannot ascertain. M. Argelander states that it is the star in *Mém. de l'Acad.* for 1790, page 383, at $12^h 39^m 35^s$, mag. 6.

1775. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $10^h 22^m 43^s$; and regularly reduced by him in MSS, vol. 23, page 370: it is also inserted in Halley's edition of

1712. Why it was afterwards rejected I am not able to ascertain. It is the star observed by Lalande in *Hist. Cél.* page 58, at $12^h\ 40^m\ 23^s,2$, mag. 6.

1777. 31 *Comæ Berenices.* This star is marked as of the $4\frac{1}{2}$ magnitude, in the *British Catalogue:* but, in the *original* entries, it is designated once as the 6th, and once as the 5th. I have therefore taken the mean.

1782. 35 *Comæ Berenices.* In the reduction of one of the observations of this star (March 9, 1692, at $12^h\ 40^m\ 45^s$) Flamsteed has made an error of 6° of declination in the steps of the process, as may be seen in MSS, vol. 23, page 404. Whence has arisen the erroneous insertion in the *British Catalogue* of 34 *Comæ Berenices*, whose position there given is $\mathcal{R} = 189°\ 30'\ 0''$, and $D = +\ 28°\ 56'\ 30''$; but which does not exist.

1785. 42 *Virginis.* I cannot find any observation of this star in Flamsteed's *Historia Cœlestis;* nor can I discover that such a star exists. There is a star of the 7th magnitude in Zach's catalogue, in the first volume of his *Tabulæ Speciales*, which is called 42 *Virginis;* and which was seen both by him at Seeberg and by MM. Barry and Henry at Manheim. The declination of this star agrees very well with the *British Catalogue:* but the right ascension differs upwards of 3′. It is remarkable however that this star also is not now to be found. For although Professor Airy has at my request looked out for it, and found a star agreeing with that of Baron Zach in right ascension, yet the declination differs more than 4′ from that given by MM. Barry and Henry; and is moreover only of the 10th magnitude. Neither Piazzi, Lalande, nor Bessel has any star that can be mistaken for it.

1787. 77 *Ursæ Majoris* ε. The right ascension of this star appears to have been deduced *per distantias:* see MSS, vol. 26 C, page 8.

1788. 12 *Canum Venaticorum.* Besides the 3 observations of this star indicated by Miss Herschel, there is another on March 13, 1694, at $12^h\ 31^m\ 17^s,5$: which, owing to some error of the transcriber, has been erroneously printed in the *Historia Cœlestis;* and has given rise to the supposition of another and a different star, viz. No. 119 of Miss Herschel's catalogue, and No. 311 in my list of Flamsteed's inedited stars. On referring to the original MS entry (MSS, vol. 6), this star is distinctly called *Cor Caroli;* and it is also distinctly stated that the observation was made to the *south* of the zenith; and it is likewise stated to be of the 3rd magnitude. In the *Apographa* (MSS, vol. 16) it is transcribed erroneously as *Ursæ Maj. informis infra caudam.*

1791. 44 *Virginis k.* One of the observations of this star, viz. that on April 9, 1701, at $10^h\ 54^m\ 0^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 33: and the result accords nearly with former computations; and has been used by Mr. Crosthwait in MSS, vol. 64 F, page 35, and by Mr. Ryley in MSS, vol. 65 E, page 34, in the computation of the longitude and latitude of that star. But, in copying it out for the catalogue, an error of 50′ in declination appears to have been made: which has given rise to the introduction of 45 *Virginis* into the *British Catalogue;* whose position as there given is $\mathcal{R} = 190°\ 57'\ 10''$, and $D = -\ 2°\ 57'\ 40''$; but which does not exist.

1793. 37 *Comæ Berenices.* This is the same star as 13 *Canum Venaticorum;* which Flamsteed however designates as of the $4\frac{1}{2}$ magnitude.

1795. — *Comæ Berenices.* Observed by Flamsteed on April 27, 1692, at $9^h\ 44^m\ 22^s$. It is the star observed by Lalande in *Hist. Cél.* page 68, at $12^h\ 50^m\ 46^s,5$, mag. $7\frac{1}{2}$.

1797. 78 *Ursæ Majoris.* Observed by Flamsteed on March 13, 1694, after $12^h\ 36^m\ 40^s$; but the time is not given in the printed copy; nor is there any right ascension set against this star in the *British Catalogue.* In the original MS entry (MSS, vol. 6) it is stated to have passed *cum ε Virginis:* but this could not be accurately known, on account of the difference in the zenith distance. In Halley's edition of 1712, the right ascension is given $= 191°\ 50'\ 30''$, which I have assumed in the present catalogue; and which is not far from the truth.

1803. 49 *Virginis g.* This star is called *g* by Flamsteed, and I have retained that letter: but it does not accord well with the position of the star in Bayer's map. There is no other however that can be properly substituted for it: and probably Bayer's position of the star is erroneous.

1805. — *Virginis.* There are 3 observations of this star by Flamsteed: viz. on May 12, 1693, at $8^h\ 58^m\ 31^s$; again on April 21, 1712, at $10^h\ 12^m\ 10^s$; and again on April 24, at $10^h\ 0^m\ 22^s$. The former of these only has been reduced by Flamsteed, in MSS, vol. 23, page 97: but in copying out the result in his list of stars for this constellation, in page 106 of the same volume, he has committed an error both in the right ascension and declination. There is moreover, in that observation, a difference of $10'$ between the reading *per strias cochleæ* and that *per lineas diagonales.* The star is inserted in his catalogue in MSS, vol. 25, page 53; and in Halley's edition of 1712. It occurs in Lalande's *Hist. Cél.* page 154, at $12^h\ 57^m\ 33^s$, mag. 6.

1806. 45 *Hydræ ψ.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the observations it is designated as the 5th; which I have here retained.

1807. — *Comæ Berenices.* Observed by Flamsteed on April 27, 1692, at $9^h\ 50^m\ 55^s,5$. It is the star observed by Lalande in *Hist. Cél.* page 334, at $12^h\ 58^m\ 25^s$, mag. 6. It was also observed by Bradley.

1808. 50 *Virginis.* The position of this star is deduced from 4 observations, which are all reduced in MSS, vol. 23, pages 93, &c.: and the declination deduced therefrom is stated, in page 106 of the same volume, to be $-8°\ 40'\ 20''$. Why it was afterwards altered, I am unable to ascertain: nor can I make the declination so great as is here stated.

1810. — *Comæ Berenices.* Observed by Flamsteed on April 16, 1692, at $10^h\ 35^m\ 47^s$; and regularly reduced by him in MSS, vol. 23, page 301; but omitted to be inserted by him in his list of stars in that constellation, in page 404 of the same volume. It appears, from Flamsteed's observation, to have passed 9^s *after* 42 *Comæ Berenices:* but there is no star that answers to such time of transit. M. Argelander supposes that an error has been committed in noting down the original entry; and that the time of transit of this star and of 42 *Comæ Berenices* ought to be transposed: in which case it will have passed 9^s *before* that star, and thus agree with Piazzi XII. 283. I have adopted this suggestion, although there is no indication of such an error in the original MS entry.

1812. — *Canum Venaticorum.* Observed by Flamsteed on April 17, 1694, after $10^h\ 34^m\ 43^s$. It is the star in Lalande's *Hist. Cél.* page 57, at $12^h\ 59^m\ 33^s,5$, mag. 6. In the original MS entry (MSS, vol. 6) it is said to have passed at the same time with the subsequent stars there mentioned; viz. Nos. 15, 16, 17 *Canum Venaticorum;* which agrees very well with the fact.

1814. 52 *Virginis.* I cannot find any observation of this star in Flamsteed's *Historia Cœlestis:* nor can I discover that such a star exists.

1816. 16 *Canum Venaticorum.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the observations it is designated as the 7th; which I have here retained.

1819. 18 *Canum Venaticorum.* The right ascension of this star in the *British Catalogue* is 194° 4′ 30″. It was observed by Flamsteed on April 27, 1694, at $10^h\ 1^m\ 45^s$; and regularly reduced by him in MSS, vol. 23, page 373, from which that value was taken; and which has been inserted also by Halley in his edition of 1712. But there is a marginal note in the said MS volume stating that the values there reduced are 1^m too small. I have therefore added 15′ to Flamsteed's right ascension, and it will now agree with modern observations. In the *original* entry of the observation of this star it is designated as of the 6th magnitude; and not of the 7th, as in the printed copy. It differs much from the value assigned to it by Piazzi, which is of the 8th.

1820. 43 *Comæ Berenices.* There appears to be a proper motion of this star, in declination, of + 0″,96; which will reduce the difference of declination to + 1″,2.

1821. 55 *Virginis.* Observed by Flamsteed on Feb. 17, 1702, after $14^h\ 14^m\ 37^s$; the time of transit is not noted; but in the original MS entry (MSS, vol. 7) it is stated to be *post transitum.* It is inserted in Halley's edition of 1712: but how the right ascension has been obtained is not stated. It is evidently about 8′ too great: but I have not made any alteration.

1822. 56 *Virginis.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entry of the observation, it is designated as the 8th; which I have here retained.

1824. — *Canum Venaticorum.* Observed by Flamsteed on April 17, 1694, at $10^h\ 43^m\ 7^s$; and again, April 27, at $10^h\ 4^m\ 1^s$: both of which were regularly reduced by him in MSS, vol. 23, pages 371 and 373; and the results were inserted in Halley's edition of 1712. Why this star was afterwards rejected, I am unable to ascertain. It is Piazzi XIII. 27 = (11 Hev.) *Can. Venat.*

1825. — *Centauri.* Observed by Flamsteed on May 4, 1692, at $9^h\ 32^m\ 22^s$: and regularly reduced by him in MSS, vol. 23, page 231. It is inserted in Halley's edition of 1712; from which the position in the present catalogue is taken: and I know not why it was afterwards rejected. It is Piazzi XIII. 31.

1827. 58 *Virginis.* The declination of this star in the *British Catalogue* is — 8° 41′ 40″. It was observed by Flamsteed on April 10, 1697, at $11^h\ 3^m\ 6^s$: and by comparing the zenith distance with that of other stars observed on the same day, it will be seen that an error of about 11′ 20″ has been committed; which I have added to Flamsteed's declination; and it will now more nearly agree with modern observations.

1829. — *Virginis.* Observed by Flamsteed on April 24, 1712, at $10^h\ 9^m\ 31^s$. It is the star observed by Lalande in *Hist. Cél.* page 154, at $13^h\ 6^m\ 43^s$, mag. 7½.

1830. — *Virginis.* Observed by Flamsteed on April 24, 1703, at $10^h\ 17^m\ 38^s$. It is Piazzi XIII. 41.

1832. 61 *Virginis.* There appears to be a proper motion of this star, in declination, of — 1″,060; which will reduce the difference of declination to — 3″,2.

1833. 46 *Hydræ* γ. This star is marked as of the 3rd magnitude, in the *British Catalogue:* but in the *original* entries it is designated twice as the 3rd, once as the 4th, and once as $3\frac{1}{2}$: the latter of which I have assumed in the present catalogue.

1834. — *Virginis.* Observed by Flamsteed on April 24, 1703, at $10^h\ 18^m\ 3^s$. It is Piazzi XIII. 43.

1837. 21 *Canum Venaticorum.* Besides the 2 observations of this star, indicated by Miss Herschel, there is another on March 13, 1694, at $12^h\ 54^m\ 53^s$; which by some mistake of the transcriber has given rise to the supposition that another star was intended; viz. No. 120 of Miss Herschel's catalogue, and No. 322 of my list of Flamsteed's inedited stars. But on referring to the original MS entry (MSS, vol. 6) I find that it is not stated that the observation was made to the *north* of the zenith: and in MSS, vol. 23, page 369, where the observation is copied out for the purpose of being reduced, it is distinctly stated that it was made to the *south.* The observation therefore refers to this star. See also MSS, vol. 26 A, page 1.

1838. 22 *Canum Venaticorum.* I cannot find any observation of this star in Flamsteed's *Historia Cœlestis;* neither can I discover that such a star exists. The first trace of it, which I can find, is its insertion in the early MS Catalogue in MSS, vol. 26 B, page 30.

1841. — *Virginis.* Observed by Flamsteed on April 28, 1693, at $10^h\ 2^m$: but the exact time is not given. M. Argelander states that it is the star in Bessel's zone No. 77, at $13^h\ 12^m\ 19^s,54$, mag. $6\frac{1}{2}$. It is in Halley's edition of 1712; from which the approximate position in the present catalogue is taken: it was probably rejected by Flamsteed on account of its uncertainty.

1843. — *Virginis.* Observed by Flamsteed on April 24, 1703, at $10^h\ 23^m$. It is the star observed by Lalande, in *Hist. Cél.* page 73, at $13^h\ 11^m\ 49^s$, mag. 8.

1847. 68 *Virginis i.* The declination of this star in the *British Catalogue* is — 11° 3′ 45″. It was observed by Flamsteed, on April 29, 1690, at $9^h\ 56^m\ 33^s$; and reduced by him in MSS, vol. 23, page 93. But in copying out the zenith distance he has made an error of 1′; which, although corrected in that page, still remains in the list of stars in the constellation *Virgo*, in page 108 of the same volume. I have therefore added 1′ to the above value. In the printed copy of the *British Catalogue* this star is denoted by the Greek letter ι; which letter is also annexed to 99 *Virginis.* On consulting the MS catalogues, I find it universally denoted by the English letter *i:* and as this corresponds with Bayer, I have restored the original and correct reading. In the *British Catalogue* this star is marked as of the 4th magnitude: but in the original entry it is designated as the 6th; which I have here retained.

1849. 79 *Ursæ Majoris* ζ. The right ascension of this star in the *British Catalogue* is 197° 22′ 0″: but it is evident that some error has been committed in the reduction. From a comparison of the 5 corresponding observations of 80 *Ursæ Majoris* indicated by Miss Herschel, it is manifest that it preceded that star only about $1^m\ 20^s$: and therefore that its right ascension ought to be increased about half a degree. In fact, the right ascension was originally deduced by Flamsteed *per distantias;* as may be seen in MSS, vol. 26 C, page 10: and the value there given is 197° 51′ 40″, which I have assumed in the present catalogue. Why Flamsteed afterwards altered this value I have not been able to ascertain.

1850. 80 *Ursæ Majoris g.* The right ascension of this star is deduced *per distantias:* as may be seen in MSS, vol. 26 C, page 10.

1851. 70 *Virginis.* The observation of this star on May 4, 1701, at $9^h\ 50^m\ 10^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 33: and in the process of reduction he has made an error of 1 hour in the time. Whence has arisen the introduction of 18 *Virginis* into the *British Catalogue*, whose position there given is $\mathcal{R} = 183°\ 17'\ 0''$ and $D = +\ 15°\ 26'\ 15''$: but which consequently does not exist.

1852. — *Virginis.* Observed by Flamsteed on May 2, 1704, at $9^h\ 52^m\ 0^s$. It is the star observed by Lalande in *Hist. Cél.* page 336, at $13^h\ 19^m\ 16^s$, mag. 7.

1853. 71 *Virginis.* In the observation of this star on May 4, 1701, at $9^h\ 50^m\ 45^s$, in MSS, vol. 26 C, page 33, Flamsteed has made an error of 1 hour in the process of reduction. Which has given rise to the introduction of 19 *Virginis* into the *British Catalogue:* whose position there given is $\mathcal{R} = 183°\ 25'\ 45''$ and $D = +\ 12°\ 25'\ 35''$; but which consequently does not exist.

1854. 72 *Virginis l'.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the *original* entries, it is designated twice as of the 7th, and only once as of the 6th. I have therefore altered the reading.

1856. 74 *Virginis l².* There is only one star designated by the letter *l* in Bayer's map: but as it may be doubted whether he intended 72 or 74 *Virginis*, Flamsteed has annexed that letter to each of them; which I have here retained. See the note to No. 1865.

1857. — *Bootis.* Observed by Flamsteed on April 24, 1703, at $10^h\ 32^m$. In the MS copy of the entry (MSS, vol. 16) it is said to be *double.* It is the star observed by Lalande in *Hist. Cél.* page 471, at $13^h\ 21^m\ 0^s{,}5$, mag. 8; and its companion is the star immediately preceding it.

1860. — *Ursæ Majoris.* Observed by Flamsteed on April 19, 1694, at $10^h\ 53^m\ 1^s$; and regularly reduced by him in MSS, vol. 23, page 373. It is inserted in Halley's edition of 1712: from which, the position in the present catalogue is taken; and I know not why it was afterwards rejected. It is Piazzi XIII. 110 = (69 Hev.) *Ursæ Majoris.*

1862. — *Bootis.* Observed by Flamsteed on April 24, 1703, at $10^h\ 33^m\ 59^s$. It is the star observed by Lalande in *Hist. Cél.* page 471, at $13^h\ 22^m\ 57^s{,}5$, mag. $6\frac{1}{2}$.

1864. 79 *Virginis ζ.* This star is called of the 6th magnitude in the *British Catalogue:* but it is now of the 4th; and is so designated in the observation of April 27 and May 9, 1692, and April 25, 1693; whilst on the 28th it is called of the 3rd magnitude. I have here restored the correct magnitude.

1865. 80 *Virginis.* Flamsteed has denoted this star as *l²* in the *British Catalogue:* but there is only one star designated by that letter in Bayer's map, which is either 72 or 74 *Virginis.* I have therefore rejected it here.

1866. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 9^m\ 8^s$; and regularly reduced by him in MSS, vol. 23, page 370. The right ascension, as deduced from that observation, would be $200°\ 29'\ 0''$: but there is no star that will correspond therewith. I suspect therefore that Flamsteed has made an error of 1^m in recording the time; and that $11^h\ 9^m\ 8^s$ should be $11^h\ 8^m\ 8^s$. In which case the observation will then agree with Piazzi XIII. 136 = (19 Hev.) *Canum Venaticorum:* and I have adopted this explanation, although there is nothing in the original MS entry to warrant it.

1867. 81 *Virginis.* This star is called of the 6th magnitude in the *British Catalogue*: but in the *original* entry it is designated as the 7½; which I have here retained.

1870. — *Bootis.* Observed by Flamsteed on April 24, 1703, at $10^h\ 39^m\ 11^s$. It is Piazzi XIII. 150.

1871. 25 *Canum Venaticorum.* The declination of this star in the *British Catalogue* is + 47° 53′ 0″. It was observed by Flamsteed on April 16, 1694, at $11^h\ 10^m\ 51^s$; and regularly reduced by him, in MSS, vol. 23, page 370. We also find 2 other reductions of it in MSS, vol. 26 A, page 3, and vol. 62 D, page 34. But in copying it for the catalogue in MSS, vol. 25 (which is the earliest *general* catalogue that I can discover), an error of 10° has been made, which I have here corrected. It will now correspond with the star observed by Lalande in *Hist. Cél.* page 164, at $13^h\ 27^m\ 46^s$, mag. 6.

1872. — *Bootis.* Observed by Flamsteed on April 25, 1693, at $10^h\ 31^m\ 19^s$; and again on May 14, 1704, at $9^h\ 18^m\ 5^s$: the former of which is regularly reduced by him in MSS, vol. 23, page 335; but there is a note against it, in the margin, that the star belongs to the constellation *Virgo*, which (I presume) is the reason why it was omitted in this constellation. It is the star observed by Lalande in *Hist. Cél.* page 469, at $13^h\ 29^m\ 16^s{,}6$, mag. 6.

1875. — *Bootis.* Observed by Flamsteed on March 31, 1701, at $12^h\ 9^m\ 18^s$. It is Piazzi XIII. 161.

1876. 2 *Bootis.* In the observation of this star on April 3, 1701, at $11^h\ 58^m\ 12^s$, Flamsteed remarks that it has a small companion preceding it. Perhaps No. 1870 of this catalogue.

1878. 82 *Ursæ Majoris.* Observed by Flamsteed on April 19, 1694, after $11^h\ 4^m\ 51^s$; but no time is given. In the earliest MS catalogue the right ascension is noted "*circiter:*" but this is omitted in the subsequent ones. The right ascension must be diminished at least 5′ in order to correspond with modern observations.

1879. 83 *Virginis.* The right ascension of this star in the *British Catalogue* is 201° 38′ 25″. There are 3 observations of it by Flamsteed: but only one of them (May 2, 1692, at $10^h\ 4^m\ 7^s$) appears to have been reduced, in MSS, vol. 23, page 95; and where an error of 20′ has evidently been made in converting the right ascension in time into degrees, minutes, and seconds; and which I have here corrected.

1883. — *Bootis.* Observed by Flamsteed on March 31, 1701, at $12^h\ 12^m\ 34^s$; and again on April 3, 1701, at $12^h\ 0^m\ 56^s$: but the first observation only has been reduced by him, in MSS, vol. 23, page 332. It is however omitted in his list of stars in the constellation *Bootes*, in page 406 of the same volume. I cannot find the star in any catalogue: but M. Argelander states that 6 observations of it at Abo, give its position for 1830, $\mathcal{R} = 13^h\ 35^m\ 43^s{,}08$; and D = + 23° 33′ 40″,0.

1886. — *Virginis.* Observed by Flamsteed on March 9, 1718, at $13^h\ 33^m\ 32^s$. It is the star observed by Lalande in *Hist. Cél.* page 154, at $13^h\ 36^m\ 5^s{,}5$, mag. 6½.

1888. 88 *Virginis n.* In the observation of this star on April 29, 1690, at $10^h\ 18^m\ 11^s$, Flamsteed remarks that it has a companion preceding it, to the north: probably Piazzi XIII. 192. The present star has no letter annexed to it in the *British Catalogue;* but it appears to be the nearest to the position of the star that is designated by the letter *n* in Bayer's map: there being

much confusion in the place of the star, supposed to be that star, in Ptolemy, Ulugh Beigh, and Tycho. I have therefore retained it here.

1890. — *Bootis.* Observed by Flamsteed on March 31, 1701, after $12^h\ 15^m\ 49^s$; again on April 3, 1701, at $12^h\ 4^m\ 10^s$; and again on May 15, 1702, at $9^h\ 28^m\ 25^s$. It is Piazzi XIII. 195.

1892. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 20^m\ 1^s$; and regularly reduced by him in MSS, vol. 23, page 370. The star is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken. I know not why it was afterwards rejected. It is the star observed by Lalande in *Hist. Cél.* page 339, at $13^h\ 38^m\ 20^s{,}5$, mag. $6\frac{1}{2}$.

1894. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 20^m\ 46^s$; and regularly reduced by him in MSS, vol. 23, page 370. The star is also inserted in Halley's edition of 1712, from which the position in the present catalogue is taken. I know not why it was afterwards rejected. It is the star observed by Lalande in *Hist. Cél.* page 339, at $13^h\ 38^m\ 28^s$, mag. 6.

1896. — *Canum Venaticorum.* Observed by Flamsteed on April 3, 1701, at $12^h\ 6^m\ 14^s$; and regularly reduced by him in MSS, vol. 23, page 334. It is to be found in Bessel's zone, No. 413, at $13^h\ 41^m\ 9^s{,}74$, mag. 8.

1898. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 21^m\ 51^s$; again on March 31, 1701, at $12^h\ 18^m\ 11^s$; and again on April 3, at $12^h\ 6^m\ 33^s$. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken. It is to be found in Bessel's zone No. 413, at $13^h\ 41^m\ 27^s{,}27$, mag. 7.

1900. 6 *Bootis e.* Flamsteed has erroneously annexed the letter *e* to 10 *Bootis*, instead of this star, to which it properly belongs.

1901. — *Bootis.* Observed by Flamsteed on May 15, 1702, at $9^h\ 31^m\ 18^s$. It is the star observed by Lalande in *Hist. Cél.* page 71, at $13^h\ 40^m\ 26^s{,}5$, mag. 7.

1904. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 24^m\ 33^s$. It is the star observed by Lalande in *Hist. Cél.* page 61, at $13^h\ 41^m\ 19^s$, mag. 7.

1905. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 24^m\ 36^s$. It is the star observed by Lalande in *Hist. Cél.* page 61, at $13^h\ 41^m\ 23^s$, mag. $5\frac{1}{2}$.

1906. — *Bootis.* Observed by Flamsteed on March 31, 1701, at $12^h\ 20^m\ 58^s$. It is Piazzi XIII. 228.

1907. — *Canum Venaticorum.* Observed by Flamsteed on April 16, 1694, at $11^h\ 25^m\ 15^s$. It is the star observed by Lalande, in *Hist. Cél.* page 61, at $13^h\ 42^m\ 2^s{,}5$.

1910. 91 *Virginis.* Observed by Flamsteed on May 13, 1703, at $9^h\ 41^m\ 10^s$: and reduced by him in MSS, vol. 26 C, page 33. But I suspect that there is an error of 2^m in recording the time of transit; and that 41^m should be 43^m. Unfortunately the original MS entry does not exist: but, in the MS transcript (MSS, vol. 16) it is as in the printed copy. If we adopt this supposed reading, the star will correspond with 92 *Virginis*, and consequently its position in the *British Catalogue* as here given should be cancelled: as no such star exists.

1911. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 10^m\ 59^s$; and regularly reduced

by him in MSS, vol. 23, page 334; but omitted in his list of the stars in *Bootes* in page 406 of the same volume. It is Piazzi XIII. 235.

1917. — *Canum Venaticorum.* Observed by Flamsteed on April 3, 1701, at $12^h\ 14^m\ 17^s$: and regularly reduced by him in MSS, vol. 23, page 334. It is the star observed by Lalande in *Hist. Cél.* page 162, at $13^h\ 46^m\ 33^s$, mag. 7.

1918. 10 *Draconis i.* This is the same star as 87 *Ursæ Majoris.*

1919. 48 *Hydræ.* The right ascension of this star in the *British Catalogue* is 205° 54′ 0″. It was observed by Flamsteed on April 28, 1711, at $10^h\ 40^m\ 28^s$, and reduced by him in MSS, vol. 26 C, page 34. On comparing the time of transit with that of 47 *Hydræ*, which immediately preceded it, it will be seen that some gross error has been committed in the reduction; as the right ascension cannot be more than 205° 41′ 15″, which is the value I have adopted in the present catalogue.

1920. 10 *Bootis.* The declination of this star in the *British Catalogue* is +23° 12′ 30″: which is deduced from the observation made on April 25, 1693, at $10^h\ 51^m\ 4^s$: and which has been reduced by him in MSS, vol. 23, page 335. But whilst he has corrected the declination of all the other stars, observed on that day, for precession (see page 406 of the same volume), he has left this uncorrected. I have therefore added 1′ to the value above given. Flamsteed has annexed the letter *e* to this star; but it will not accord with the star so designated in Bayer's map, either in position or magnitude. See the Note to No. 1900.

1922. — *Bootis.* Observed by Flamsteed on May 13, 1704, at $9^h\ 43^m\ 28^s$: but I cannot find any star that will correspond thereto. M. Argelander has suggested that if the observation were made to the *north* of the zenith, it would agree with the star observed by Lalande in *Hist. Cél.* page 390, at $13^h\ 43^m\ 37^s$, mag. 5½; but there is nothing in the MS entry to warrant this reading.

1923. 11 *Bootis.* In the observation of this star on March 31, 1701, at $12^h\ 30^m\ 33^s$, Flamsteed remarks that it has a companion preceding it; which M. Argelander supposes to be the star observed by Lalande in *Hist. Cél.* page 335, at $13^h\ 51^m\ 4^s$, mag. 8.

1924. — *Bootis.* Observed by Flamsteed on March 31, 1701, after $12^h\ 30^m\ 33^s$; and again on April 3, at $12^h\ 19^m\ 28^s$, both of which have been regularly reduced by him in MSS, vol. 23, pages 332 and 334, but not inserted in his list of stars in the constellation *Bootes*, in page 406 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 69, at $13^h\ 51^m\ 26^s{,}3$, mag. 7.

1925. 49 *Hydræ π.* The right ascension of this star in the *British Catalogue* is 207° 4′ 30″. There are 3 observations of it by Flamsteed; the first of which however (May 3, 1692, after $10^h\ 18^m\ 2^s$) appears to be the only one completely reduced by him, in MSS, vol. 23, page 231; but as the exact time is not given, the right ascension, in his list of stars in page 276 in the same volume, is given only approximately = 207°. The observation on April 28, 1711, at $10^h\ 46^m\ 29^s$, reduced in MSS, vol. 26 C, page 34, shows that an error of more than 8′ has been committed. The star is inserted in Halley's edition of 1712; from which the right ascension in the present catalogue is taken.

1926. — *Virginis.* Observed by Flamsteed on April 24, 1712, at $10^h\ 56^m\ 11^s$. It is the star observed by Lalande in *Hist. Cél.* page 160, at $13^h\ 53^m\ 40^s{,}5$, mag. 7½.

1926. 5 *Centauri* θ. There appears to be an annual proper motion of this star, in declination of — 0″,820; which will reduce the difference of declination to —14″,6.

1927. — *Bootis.* Observed by Flamsteed on March 31, 1701, at $12^h\ 32^m\ 19^s$; and again on April 3, at $12^h\ 20^m\ 41^s$; both of which have been regularly reduced by him in MSS, vol. 23, pages 332 and 334, but not inserted in his list of stars in the constellation *Bootes*, in page 406 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 69, at $13^h\ 52^m\ 38^s,5$, mag. 7.

1931. — *Virginis.* Observed by Flamsteed on April 24, 1712, at $10^h\ 58^m\ 10^s$. It is Piazzi XIII. 300=561 Mayer.

1932. — *Bootis.* Observed by Flamsteed on March 31, 1701, after $12^h\ 36^m\ 10^s$; and again on April 3, at $12^h\ 23^m\ 30^s$. It is Piazzi XIII. 296.

1933. 96 *Virginis.* If we compare the difference in the zenith distance of this star and κ *Virginis*, observed on the same days, it will appear that an error of 30″ has been made in the declination: which would reduce the difference from Bradley's value to +37″,5. In the *British Catalogue* this star is marked as of the 5th magnitude: but in the observations it is designated as of the 6th; which I have here retained.

1934. — *Bootis.* Observed by Flamsteed on March 31, 1701, at $12^h\ 36^m\ 10^s$: and regularly reduced by him in MSS, vol. 23, page 332; but not included in his list of stars in the constellation *Bootes* in page 406 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 69, at $13^h\ 56^m\ 31^s,3$, mag. 7½.

1935. 50 *Hydræ.* The observation of this star made on May 19, 1691, at $9^h\ 35^m\ 37^s$, has been reduced by Flamsteed in MSS, vol, 23, page 119: and in turning the right ascension in time, into the right ascension in space, he has made an error of 1°, whence has arisen the introduction of 1 *Libræ* into the *British Catalogue:* whose position as there given is $\mathcal{R}=209°\ 46'\ 30''$, and $D=-25°\ 46'\ 45''$; but which consequently does not exist.

1936. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 27^m\ 23^s$; and again on May 14, 1704, at $9^h\ 50^m\ 1^s$; the first of which has been regularly reduced by him in MSS, vol. 23, page 334; but not inserted in his list of stars in the constellation *Bootes*, in page 406 of the same volume. It is Piazzi XIII. 316.

1937. 11 *Draconis* α. This star is marked as of the 2nd magnitude in the *British Catalogue:* but in the *original* entries it is designated once as the 3rd and once as the 4th. I have therefore taken the mean.

1938. 97 *Virginis.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the *original* entry it is designated as the 7th; which I have therefore retained.

1940. 98 *Virginis* κ. In the observation of this star on April 29, 1690, at $10^h\ 42^m\ 26^s$, it is stated by Flamsteed that it has a companion preceding it, at 45′ distant.

1941. — *Virginis.* Observed by Flamsteed on April 29, 1690, after $10^h\ 42^m\ 26^s$. It is probably Piazzi XIV. 12.

1943. — *Hydræ.* Observed by Flamsteed on May 3, 1692, at $10^h\ 34^m$. It is No. 387 *Hydræ* in Bode's great catalogue, observed by Vidal: and is to be found also in Lacaille's *Cœlum Australe.*

1944. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 29^m\ 34^s$; and regularly reduced by him in MSS, vol. 23, page 334; but not included in his list of stars in the constellation *Bootes* in page 406 of the same volume. It is Piazzi XIV. 16.

1948. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 31^m\ 56^s$; and regularly reduced by him in MSS, vol. 23, page 334; but not inserted by him in his list of stars in the constellation *Bootes* in page 406 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 165, at $14^h\ 4^m\ 17^s$, mag. $7\frac{1}{2}$.

1949. — *Bootis.* Observed by Flamsteed on Feb. 14, 1690, at $15^h\ 37^m\ 49^s$. It is the star observed by Lalande in *Hist. Cél.* page 71, at $14^h\ 6^m\ 33^s$, mag. 7. It was also observed by Bradley, and is No. 1848 in his catalogue. Owing to the proper motion of *Arcturus*, it now passes *after* that star.

1950. 16 *Bootis* α. There appears to be an annual proper motion of this star, in declination, of $-1''{,}945$; which will reduce the difference of declination to $-7''{,}8$.

1952. 17 *Bootis* κ. In the *original* entries of the observations of this star, it is designated once as of the 3rd magnitude, once as the 5th, and once as $4\frac{1}{2}$.

1953. 101 *Virginis*. The declination of this star in the *British Catalogue* is $+\ 17^\circ\ 42'\ 50''$: but no star, corresponding thereto, is to be found in Flamsteed's *Historia Cœlestis*, or in the heavens. It is the star observed by Flamsteed on April 25, 1693, at $11^h\ 9^m\ 34^s$; and he has made an error of 1° in the process of reducing the declination, which I have here corrected. It is the star observed by Lalande in *Hist. Cél.* page 74, at $14^h\ 7^m\ 45^s{,}7$, mag. $6\frac{1}{2}$.

1955. — *Bootis.* Observed by Flamsteed on March 31, 1701, the first after $12^h\ 48^m\ 17^s$; and again on April 3, at $12^h\ 35^m\ 35^s$; the latter of which was regularly reduced by him in MSS, vol. 23, page 334; but not inserted in his list of the stars in the constellation *Bootes*, in page 406 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 289, at $14^h\ 7^m\ 25^s{,}5$, mag. 7; and page 9, at $14^h\ 7^m\ 31^s$: in which last observation the 3rd wire should be read, instead of the 2nd.

1959. — *Bootis A.* Observed by Flamsteed on March 31, 1701, at $12^h\ 48^m\ 17^s$; again on May 13, 1704, at $10^h\ 3^m\ 0^s$; and again on the following day, at $9^h\ 59^m\ 12^s$; the first of which only has been reduced by him in MSS, vol. 23, page 332, but not inserted in his list of stars in the constellation *Bootes* in page 406 of the same volume. It is Piazzi XIV. 45 = (16 Hev.) *Bootis*. It is designated by the letter *A* in Bayer's map; which I have here retained.

1961. 51 *Hydræ*. The declination of this star in the *British Catalogue* is $-\ 26^\circ\ 28'\ 20''$. It was observed by Flamsteed on May 3, 1692, at $10^h\ 42^m\ 4^s$; and regularly and correctly reduced by him in MSS, vol. 23, page 231: but in copying it into the list of stars in the constellation *Hydra*, in page 276 in the same volume, he has made an error of $10'$ in the declination, which I have here corrected.

1962. — *Bootis.* Observed by Flamsteed on April 3, 1701, after $12^h\ 39^m\ 41^s$. It is the star mentioned in the *Mém. de l'Acad.* for 1790, page 378, at $14^h\ 10^m\ 6^s$, mag. 7.

1963. 3 *Ursæ Minoris*. The right ascension of this star in the *British Catalogue* is $211^\circ\ 24'\ 0''$. It was observed by Flamsteed on April 10, 1704, at $12^h\ 9^m\ 10^s$; and reduced by him in MSS, vol. 26 A, page 25. But in the steps of the process he has made a mistake of 10^s; I have

therefore deducted 2′ 30″ from the above value. The right ascension however must be diminished nearly 10′ more, in order to correspond with modern observations.

1964. 103 *Virginis* v^s. There is only one star designated by the letter *v*, in Bayer's map: but as it may be doubted whether such letter belongs to 102 *Virginis*, or to the present star, Flamsteed has annexed it to each, which I have here retained.

1965. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 38^m\ 20^s$; and regularly reduced by him in MSS, vol. 23, page 334; but not inserted in the list of stars in the constellation *Bootes*, in page 406 of the same volume. It is Piazzi XIV. 50. It was also observed by Bradley, and is No. 1856 in his catalogue.

1967. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 39^m\ 41^s$; and regularly reduced by him in MSS, vol. 23, page 334; but not inserted in his list of stars in the constellation *Bootes*, in page 406 of the same volume. It is to be found in *Mém. de l'Acad.* for 1790, page 377, at $14^h\ 11^m\ 51^s{,}2$, mag. $7\frac{1}{2}$.

1968. — *Bootis.* Observed by Flamsteed on April 25, 1693, at $11^h\ 15^m\ 4^s$; again on May 13, 1704, at $10^h\ 5^m\ 32^s$; and again on the next day at $10^h\ 1^m\ 45^s{,}5$. It is Piazzi XIV. 69 = (18 Hev.) *Bootis.*

1969. — *Bootis.* Observed by Flamsteed on May 14, 1704, at $10^h\ 2^m\ 19^s$. It is supposed by Miss Herschel to be the same as No. 1971 of the present catalogue: but it is probably Piazzi XIV. 71.

1970. — *Bootis.* Observed by Flamsteed on April 25, 1693, at $11^h\ 15^m\ 40^s$. It is Piazzi XIV. 72.

1971. — *Bootis.* Observed by Flamsteed on April 25, 1693, at $11^h\ 15^m\ 46^s$. It is Piazzi XIV. 73 = (19 Hev.) *Bootis.*

1972. — *Bootis.* Observed by Flamsteed on May 13, 1704, at $10^h\ 6^m\ 28^s$. It is Piazzi XIV. 75.

1974. 4 *Ursæ Minoris.* The right ascension of this star in the *British Catalogue* is 213° 46′ 30″. There are 3 observations of it by Flamsteed: but that, from which he deduced that right ascension, was made on April 10, 1704, at $12^h\ 15^m\ 15^s$; as may be seen in MSS, vol. 26 A, page 25, and C, page 21. And it will then be found that the degrees should be 212 instead of 213: I have therefore deducted 1° from the right ascension in the *British Catalogue.* I would further remark that the right ascension as deduced *per distantias* in MSS, vol. 26 A, page 22, is 212° 46′ 40″: and that in Halley's edition of 1712, it is 212° 47′ 0″. This star is designated by the letter *b*, in the *British Catalogue:* but as there is no such letter in Bayer's map, I have here rejected it.

1976. — *Bootis.* Observed by Flamsteed on April 3, 1701, the second after $12^h\ 39^m\ 41^s$. It was observed by Lalande in *Hist. Cél.* page 164, at $14^h\ 15^m\ 26^s{,}5$, mag. 7.

1977. 22 *Bootis f.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but in the observations it is designated as the 6th; which I have therefore retained.

1980. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 44^m\ 36^s$. It is the star observed by Lalande in *Hist. Cél.* page 9, at $14^h\ 16^m\ 0^s$, mag. 6.

1981. — *Virginis.* Observed by Flamsteed on April 29, 1690, at $10^h\ 58^m\ 9^s$. It is Piazzi XIV. 96.

1983. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 47^m\ 12^s$. It is the star observed by Lalande in *Hist. Cél.* page 164, at $14^h\ 19^m\ 13^s{,}7$, mag. $6\frac{1}{2}$.

1984. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 48^m\ 21^s$. It is Piazzi XIV. 103.

1987. 26 *Bootis.* This star is omitted in Flamsteed's map of *Bootes.*

1988. — *Bootis.* Observed by Flamsteed on March 31, 1701, after $13^h\ 1^m\ 24^s$; and again on April 3, at $12^h\ 50^m\ 22^s$. It is the star observed by Lalande in *Hist. Cél.* page 335, at $14^h\ 23^m\ 35^s$, mag. 6.

1991. — *Bootis.* Observed by Flamsteed on April 3, 1701, the first after $12^h\ 52^m\ 57^s$. It is the star observed by Lalande in *Hist. Cél.* page 162, at $14^h\ 24^m\ 56^s{,}5$, mag. 7.

1992. — *Bootis.* Observed by Flamsteed on April 3, 1701, the second after $12^h\ 52^m\ 57^s$. It is the star observed by Lalande in *Hist. Cél.* page 164, at $14^h\ 24^m\ 59^s{,}5$, mag. $7\frac{1}{2}$.

1993. — *Bootis.* Observed by Flamsteed on April 3, 1701, the third after $12^h\ 52^m\ 57^s$, It is Piazzi XIV. 128.

1995. — *Librœ.* Observed by Flamsteed on April 18, 1715, at $11^h\ 58^m\ 44^s$; and again on the following day at $11^h\ 54^m\ 55^s$. It is Piazzi XIV. 137 = 577 Mayer.

1996. — *Bootis.* Observed by Flamsteed on April 3, 1701, after $12^h\ 58^m\ 21^s$. I cannot find the star in any catalogue: but M. Argelander says that 8 observations of it at Abo make its position for 1830, $\mathcal{R} = 14^h\ 28^m\ 42^s{,}89$ and $D = +\ 50^\circ\ 6'\ 46''{,}3$.

1997. 4 *Librœ.* This is the same star as 53 *Hydræ:* whose position in the *British Catalogue* is $\mathcal{R} = 216^\circ\ 23'\ 30''$ and $D = -\ 23^\circ\ 38'\ 50''$. But that right ascension is erroneous, and should be the same as 4 *Librœ;* as may be seen in MSS, vol. 32, page 231.

1999. 30 *Bootis* ζ. In the observation of this star on May 27, 1692, at $9^h\ 27^m\ 52^s$, Flamsteed remarks that it has a companion preceding it to the south: which is probably Piazzi XIV. 145.

2001. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $12^h\ 58^m\ 21^s$. It is the star observed by Lalande in *Hist. Cél.* page 8, at $14^h\ 29^m\ 28^s{,}3$, mag. 5.

2004. 33 *Bootis.* This star is called h^1 by Flamsteed: but there is only one star designated by that letter in Bayer's map, which is 38 *Bootis.* I have therefore rejected it here. The present star is in Bayer's map; but without any letter annexed.

2005. 54 *Hydræ.* The declination of this star in the *British Catalogue* is $-\ 24^\circ\ 10'\ 45''$; and it is deduced from the observation made on May 3, 1692, at $11^h\ 4^m\ 50^s$. Flamsteed has taken the reading *per lineas diagonales* as the measure of the zenith distance: but a comparison with modern observations shows that the reading *per strias cochleæ* is, in this case, the most correct: and as it differs $4'\ 45''$ from the former, I have deducted that value from the declination given by Flamsteed.

2006. 5 *Ursæ Minoris.* This star is designated by the letter *a*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2013. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $13^h\ 1^m\ 51^s$. It is the star observed by Lalande in *Hist. Cél* page 164, at $14^h\ 33^m\ 49^s{,}5$, mag. 7.

2016. 36 *Bootis* ε. Besides the 4 observations of this star, indicated by Miss Herschel, there is another on April 6, 1691, at $12^h\ 4^m\ 53^s$: and which she has supposed to be another and a

different star, No. 48 in her catalogue, and No. 397 in my list of Flamsteed's inedited stars. But, on examining the original MS entry (MSS, vol. 5) I think the cause of the error is manifest. In the first place it is there stated that all the transits of the stars, after the passage of Polaris, till the transit of the moon on that day, were made by a Mr. Carter; and we find that all the stars so observed by him, were stars of considerable magnitude: and it is distinctly stated that the star in question was *ε Bootis*, although this is not mentioned in the printed copy. Secondly, there is evidently an error in the *hour* of the first two observations made by him, which should be $12^h\ 23^m\ 18\frac{1}{2}^s$ and $13^h\ 4^m\ 53^s$. It is true that this latter value will not, even with this correction, correspond with *ε Bootis*: but we may readily imagine, with M. Argelander, that the figures should be inverted, and that we may read $13^h\ 53^m\ 4^s$; or that some other error in recording the time has been made by a novice in the art of observing.

2017. — *Bootis.* Observed by Flamsteed on April 3, 1701, after $13^h\ 1^m\ 51^s$; and again on May 16, 1702, at $10^h\ 23^m\ 52^s$. It is the star observed by Lalande in *Hist. Cél.* page 161, at $14^h\ 35^m\ 16^s{,}4$, mag. $6\frac{1}{2}$.

2018. — *Virginis.* Observed by Flamsteed on May 13, 1704, at $10^h\ 29^m\ 2^s$; and again on the following day at $10^h\ 25^m\ 14^s$. It is Piazzi XIV. 180.

2019. 6 *Libræ.* This is the same star as 58 *Hydræ*: whose position in the *British Catalogue* is $\mathcal{R} = 218°\ 1'\ 15''$ and $D = -\ 26°\ 37'\ 50''$.

2021. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $13^h\ 4^m\ 54^s$. It is Piazzi XIV. 179.

2024. — *Bootis.* Observed by Flamsteed on April 3, 1701, after $13^h\ 4^m\ 54^s$. It is Piazzi XIV. 182.

2025. 10 *Libræ.* The declination of this star in the *British Catalogue* is $-\ 17°\ 1'\ 45''$. It was observed by Flamsteed on May 15, 1700, at $10^h\ 21^m\ 55^s$: and on comparing its zenith distance with that of other stars observed on the same day, it will be seen that an error of $1'$ has been made; which I have here corrected.

2028. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $13^h\ 8^m\ 37^s$; and again on May 16, 1702, at $10^h\ 29^m\ 2^s$. It is the star observed by Lalande in *Hist. Cél.* page 9, at $14^h\ 39^m\ 58^s$, and page 289, at $14^h\ 40^m\ 26^s{,}5$, mag. 6.

2030. — *Bootis.* Observed by Flamsteed on May 13, 1704, at $10^h\ 34^m\ 30^s$; and again on the following day at $10^h\ 30^m\ 47^s$. It is Piazzi XIV. 193 = (32 Hev.) *Bootis.*

2033. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $13^h\ 9^m\ 56^s$. It is the star observed by Lalande in *Hist. Cél.* page 164, at $14^h\ 42^m\ 25^s{,}5$, mag. 6.

2036. — *Bootis.* Observed by Flamsteed on May 27, 1692, after $9^h\ 39^m\ 34^s$. M. Argelander thinks it is the star observed by Lalande in *Hist. Cél.* page 353, at $14^h\ 47^m\ 52^s{,}2$, mag. 8. In the original MS entry (MSS, vol. 5) it is said to transit *paulo post præcedentem* (i. e. after 38 *Bootis*).

2037. 15 *Libræ* ξ^3. There is only one star designated by the letter ξ, in Bayer's map: but, as it may be doubtful whether such letter belongs to 13 *Libræ*, or to the present star, Flamsteed has annexed it to each, and which I have here retained.

2044. — *Bootis.* Observed by Flamsteed on March 31, 1701, the first after $13^h\ 29^m\ 34^s$; and again on April 3, at $13^h\ 15^m\ 59^s$. M. Argelander says that it is the star observed by Lalande in *Hist. Cél.* page 161, at $14^h\ 47^m\ 44^s{,}5$, mag. $7\frac{1}{2}$: where the zenith distance should be

6° 51′ 6″ instead of 6° 59′ 6″: and that it is also in Bessel's zone No. 472, at $14^h\,49^m\,35^s{,}08$. It was likewise observed by M. Argelander himself, who makes its position for 1830, $\mathcal{R} = 14^h\,49^m\,35^s{,}08$ and D = + 41° 49′ 31″,3.

2046. 6 *Ursæ Minoris.* The declination of this star in the *British Catalogue* is + 73° 14′ 30″. It was observed by Flamsteed on April 10, 1704, at $12^h\,48^m\,16^s$; and reduced by him in MSS, vol. 23, page 430. But in the steps of the process he has made a mistake of 1′, which I have here corrected.

2048. 20 *Libræ.* This star is called γ in the *British Catalogue:* which letter is also affixed to 38 *Libræ*, to which it properly belongs. The present star is the γ *Scorpii* of Bayer; and therefore I have rejected the letter here.

2049. — *Bootis.* Observed by Flamsteed on March 31, 1701, at $13^h\,29^m\,34^s$. It is Piazzi XIV. 235.

2050. 110 *Virginis.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entries, it is designated once as the 4th, once as $4\frac{1}{2}$, and once as the 5th: I have therefore taken the mean.

2051. — *Bootis.* Observed by Flamsteed on March 31, 1701, the second after $13^h\,29^m\,34^s$; again on April 3, at $13^h\,19^m\,14^s$; and again on May 16, 1702, at $10^h\,39^m\,42^s$. It is the star observed by Lalande in *Hist. Cél.* page 9, at $14^h\,50^m\,28^s$, mag. 7.

2056. — *Bootis.* Observed by Flamsteed on March 31, 1701, the third after $13^h\,29^m\,34^s$; and again on April 3, at $13^h\,21^m\,49^s$. It is the star observed by Lalande in *Mém. de l'Acad.* for 1790, page 375, at $14^h\,53^m\,27^s$, mag. 7; and again in page 377, at $14^h\,53^m\,32^s{,}6$, mag. 6.

2058. — *Bootis.* Observed by Flamsteed on May 13, 1704, at $10^h\,48^m\,43^s$; and again on the next day, at $10^h\,44^m\,54^s{,}5$. It is Piazzi XIV. 263.

2060. — *Bootis.* Observed by Flamsteed on April 3, 1701, after $13^h\,26^m\,54^s$; and again on May 16, 1702, at $10^h\,44^m\,18^s$. It is the star observed by Lalande in *Hist. Cél.* page 353, at $14^h\,56^m\,24^s$, mag. 7.

2061. 7 *Ursæ Minoris* β. The right ascension of this star has been determined *per distantias:* and the computation is to be seen in MSS, vol. 26 A, page 22.

2062. 44 *Bootis i.* The declination of this star in the *British Catalogue* is + 48° 51′ 30″; which is deduced from the observation made by Flamsteed on May 5, 1692, at $11^h\,21^m\,13^s$: but in the steps of the process he has made great confusion in the figures; and although he has ultimately obtained the above value, it is evident that an error of 1′ has been made, which I have here corrected. In the *British Catalogue*, this star is not denoted by any letter: but Bayer calls it *i:* and on referring to the earliest MS catalogue, in MSS, vol. 25, I find that it is there so designated; and I have therefore restored the letter. Flamsteed has marked it as of the 6th magnitude: but in the observation book it is designated always as the 5th; which I have here adopted.

2064. 24 *Libræ* i^1. This star is marked as of the $3\frac{1}{2}$ magnitude in the *British Catalogue:* but in the *original* entries it is designated twice as the 5th, and once as the 6th. I have therefore altered the reading.

2065. — *Libræ.* Observed by Flamsteed on May 5, 1691, at $11^h\,27^m\,48^s$: and regularly reduced

by him in MSS, vol. 23, page 111; but omitted in his list of stars in the constellation *Libra*, in page 128 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 342, at $15^h\ 1^m\ 30^s{,}5$, mag. 7.

2067. 47 *Bootis k.* The right ascension is not affixed to this star in the *British Catalogue*, although there are 4 observations of it by Flamsteed; all of which he has reduced in MSS, vol. 23, page 332–335: but has omitted to insert in his list of stars in the constellation *Bootes*, in page 406 of the same volume; probably on account of an error of 1^m in the computation of the first observation. The right ascension however is given by Halley in his edition of 1712; from which the value in the present catalogue is taken. There is no letter annexed to this star in the *British Catalogue*: but it is designated by the letter *k* in Bayer's maps. I have therefore annexed that letter here.

2068. 23 *Libræ.* The right ascension of this star in the *British Catalogue* is $222°\ 25'\ 0''$. It was observed by Flamsteed on May 19, 1711, at $10^h\ 31^m\ 36^s$; and reduced by him in MSS, vol. 26 C, page 34: but on comparing its time of transit with that of other stars observed on the same day it is evident that an error of nearly 6^m has been committed, which I have here corrected. It is Piazzi XV. 5; which he has considered to be a star in *Scorpio.*

2070. 25 *Libræ* ι^2. There is only one star designated by the letter ι in Bayer's map; but as it may be doubted whether such letter belonged to this star, or to 24 *Libræ*, Flamsteed has annexed it to each; which I have here retained.

2072. — *Bootis.* Observed by Flamsteed on April 3, 1701, at $13^h\ 29^m\ 44^s$. It is the star observed by Lalande in *Hist. Cél.* page 162, at $15^h\ 1^m\ 48^s$, mag. $7\frac{1}{2}$.

2073. — *Bootis.* Observed by Flamsteed on April 3, 1701, after $13^h\ 29^m\ 44^s$. It is the star observed by Lalande in *Hist. Cél.* page 166, at $15^h\ 2^m\ 16^s{,}3$, mag. $7\frac{1}{2}$.

2074. 2 *Lupi δ.* This star is marked as of the $5\frac{1}{2}$ magnitude, in the *British Catalogue*: but, in the *original* entries it is designated once as the 4th, and twice as the 5th; I have therefore altered it.

2077. 8 *Ursæ Minoris.* The position of this star in the *British Catalogue* is $\mathcal{R} = 224°\ 47'\ 0''$, and $D = +\ 76°\ 57'\ 40''$. It was observed by Flamsteed on April 10, 1704, at $13^h\ 3^m\ 43^s$; and reduced by him in MSS, vol. 26 A, page 25. But, in the steps of the process he has committed two errors. In the first place he has made a mistake of 1^m in the right ascension; and I have therefore added $15'$ to the value above stated. Secondly, he has made a mistake of $50'$ in the declination, which I have deducted from the above value. These corrections being made, the star is Piazzi XIV. 283. The right ascension, however, (like most of the stars observed on the same day), is still too great by about $10'$.

2079. 9 *Ursæ Minoris.* The right ascension of this star must be diminished about $6'$ in order to correspond with modern observations.

2081. — *Lupi.* Observed by Flamsteed on May 3, 1692, at $11^h\ 38^m\ 53^s$; and regularly reduced by him in MSS, vol. 23, page 121. It is also inserted in Halley's edition of 1712; from which, the position in the present catalogue is taken. It is Piazzi XV. 34; which he calls ϕ^1 *Lupi*; although there is no star designated by that letter in Bayer's map.

2084. — *Lupi.* Observed by Flamsteed on May 3, 1692, at $11^h\ 40^m\ 7^s$; and regularly reduced by

him in MSS, vol. 23, page 121. It is also inserted in Halley's edition of 1712; from which, the position in the present catalogue is taken. It is Piazzi XV. 42; which he calls ϕ^2 *Lupi*. See the Note to No. 2081.

2087. — *Bootis.* Observed by Flamsteed on May 13, 1704, at $11^h\ 2^m\ 7^s$; and again on the next day at $10^h\ 58^m\ 18^s$. It is Piazzi XV. 36 = (44 Hev.) *Bootis.*

2089. 30 *Libræ* o^2. There is only one star designated by the letter o in Bayer's map: but as it may be doubted whether that letter belongs to 29 *Libræ*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

2092. 7 *Serpentis.* The declination of this star in the *British Catalogue* is $+13°\ 40'\ 40''$. It was observed by Flamsteed on May 16, 1693, at $10^h\ 57^m\ 54^s$: and reduced by him in MSS, vol. 23, page 235. But in the steps of the process he has made an error of $1'$, which I have here corrected.

2094. 10 *Ursæ Minoris.* The right ascension of this star (like most of those observed on the same day, viz. April 10, 1704) must be diminished about $10'$, in order to correspond with modern observations.

2097. 32 *Libræ* ζ^1. This star is called ζ^1 by Flamsteed. See the note to No. 2104, below.

2098. 9 *Serpentis* τ^1. This star has no letter annexed to it in the *British Catalogue:* but it is the first of the group of 8 stars designated by the letter τ in Bayer's map; I have therefore retained it here. See the *Introduction*, page 399.

2099. 33 *Libræ* ζ^2. The declination of this star in the *British Catalogue* is $-16°\ 10'\ 15''$. It was observed by Flamsteed on May 9, 1691, at $11^h\ 29^m\ 47^s$; and the declination deduced by him in MSS, vol. 23, page 113, is from the reading *per lineas diagonales.* But this differs $10'$ from that *per strias cochleæ*, which appears, from modern observations, to be the correct reading. I have therefore added $10'$ to Flamsteed's value. This star is called ζ^2 by Flamsteed. See the note to No. 2104, below.

2100. 51 *Bootis* μ. Observed by Flamsteed on May 5, 1692, at $11^h\ 40^m\ 19^s$; and regularly reduced by him in MSS, vol. 23, page 333, where the right ascension is deduced equal to $228°\ 9'\ 45''$. But in the list of stars in the constellation *Bootes*, in page 408 of the same volume, this value is altered to $228°\ 13'\ 0''$ without any apparent reason. The former is evidently the more correct value, as the present right ascension must be diminished $4'\ 20'',5$ in order to correspond with Bradley's observations.

2102. 34 *Libræ* ζ^3. This star is called ζ^3 by Flamsteed. See the note to No. 2104.

2104. 35 *Libræ* ζ^4. There is only one star designated by the letter ζ in Bayer's map: but, as it may be doubted whether that belongs to 32, 33, 34 *Libræ*, or to the present star, (or perhaps to the whole collectively) Flamsteed has annexed the letter to each; which I have here retained. The present star is marked as of the 4th magnitude, in the *British Catalogue:* but it is no where so designated in the observations: on the contrary, it is twice stated to be of the 6th, in the *original* entries, which I have therefore here adopted.

2107. 12 *Ursæ Minoris.* The right ascension of this star is not given in the *British Catalogue*, neither is there any observation of it recorded in the printed copy of the *Historia Cœlestis.* I find however by MSS, vol. 23, page 430, that it was observed on April 10, 1704, between

$13^h\ 21^m\ 9^s$ and $13^h\ 25^m\ 4^s$. The time of transit is not recorded: but the uncorrected zenith distance is stated to be $20°\ 47'\ 40''$. I have assumed an approximate value for the right ascension, corresponding with modern observations.

2108. 37 *Libræ*. This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entries it is twice designated as $5\frac{1}{2}$; which I have here adopted.

2110. — *Libræ*. Observed by Flamsteed on May 20, 1690, at $10^h\ 45^m\ 45^s$; and regularly reduced by him in MSS, vol. 23, page 111; but omitted to be inserted in his list of stars in the constellation *Libra* in page 128 of the same volume. It is given in Halley's edition of 1712: from which, the position in the present catalogue is taken. It is the star observed by Lalande in *Hist. Cél.* page 288, at $15^h\ 22^m\ 53^s{,}4$, mag. 6.

2111. 12 *Serpentis* τ^3. In the observation of this star on May 16, 1693, at $11^h\ 8^m\ 1^s$, Flamsteed remarks that it has a brighter companion preceding it, about 30′ north. This is probably Piazzi XV. 89. The present star is called τ^1 in the *British Catalogue:* but it is the second of the group of 8 stars designated by the letter τ in Bayer's map; I have therefore altered Flamsteed's notation. See the *Introduction*, page 399.

2112. 12 *Draconis* ι. The right ascension of this star must be diminished nearly 9′ in order to correspond with modern observations. It appears also that the declination is very discordant. It was not observed with the mural arc: but its right ascension was determined by means of *distances* from δ *Cygni* given in the *Historia Cœlestis*, vol. 1, page 72. I cannot discern how the declination was determined; as the first star called ι *Draconis* in the *Historia Cœlestis*, vol. 1, page 72, is ι *Herculis*.

2114. 39 *Libræ*. } These are the two stars designated by the letter o in Bayer's map of *Scorpio:*
2115. 40 *Libræ*. } but which Flamsteed has inserted in the constellation *Libra*.

2117. — *Serpentis*. Observed by Flamsteed on May 30, 1693, at $10^h\ 9^m\ 19^s$; and regularly reduced by him in MSS, vol. 23, page 237: but omitted to be inserted in the list of stars in the constellation *Serpens* in page 302 of the same volume. It is Piazzi XV. 114.

2120. — *Coronæ Borealis*. Observed by Flamsteed on June 4, 1691, at $9^h\ 52^m\ 45^s$: I cannot find however any star that will correspond thereto. M. Argelander supposes that there may be an error in registering the reading *per strias cochleæ:* and that the reading should be 432·06, instead of 532·06: in which case it would agree with θ *Coronæ Borealis*. In the original MS entry (MSS, vol. 5) it is stated that Mr. Clowes *alone* made the observation, at the same time correcting the clock. So that it is not unlikely that some error has been committed.

2122. 53 *Bootis* ν^2. There is only one star designated by the letter ν in Bayer's map: but as it may be doubted whether it belongs to this star or to 52 *Bootis*, Flamsteed has annexed the letter to each; and which I have therefore adopted.

2125. 13 *Ursæ Minoris* γ. The right ascension of this star has been determined *per distantias:* and the computation may be seen in MSS, vol. 26 A, page 22.

2127. 15 *Serpentis* τ^6. This star has no letter annexed to it in the *British Catalogue:* but it is the third of the group of 8 stars designated by the letter τ in Bayer's map: I have therefore annexed the corresponding letter. See the *Introduction*, page 399.

2130. 17 *Serpentis* τ^4. There is no letter annexed to this star in the *British Catalogue*: but it is the fourth of the group of 8 stars, designated as τ in Bayer's map; I have therefore annexed the corresponding letter. See the *Introduction*, page 399.

2131. 18 *Serpentis* τ^5. The declination of this star in the *British Catalogue* is $+17^\circ\ 19'\ 10''$. It was observed by Flamsteed on May 16, 1693, at $11^h\ 12^m\ 21^s$; and correctly reduced by him in MSS, vol. 23, page 235. It is also correctly inserted in the list of stars in the constellation *Serpens*, in page 302 of the same volume: the declination being $+17^\circ\ 10'\ 10''$: and also in the MS copy of the *British Catalogue*, (MSS, vol. 27 B,) which was used by the printer. There can be no doubt therefore that the error in the *British Catalogue* is a typographical error; which I have here corrected. In the *British Catalogue* it is called τ^6; but it is the fifth of the group of 8 stars designated by the letter τ in Bayer's map: I have therefore restored the correct reading. See the *Introduction*, page 399.

2133. 14 *Ursæ Minoris*. There is no right ascension annexed to this star in the *British Catalogue*: but I have inserted an approximate value, that accords nearly with modern observations.

2137. 54 *Bootis* φ. This star is also designated as ψ *Herculis* in Bayer's map.

2138. 19 *Serpentis* τ^6. This star is called τ^8 in the *British Catalogue*: but as it is the sixth of the group of 8 stars designated by the letter τ in Bayer's map, I have changed the number. See the *Introduction*, page 399.

2139. 20 *Serpentis* χ. The declination of this star in the *British Catalogue* is $+13^\circ\ 50'\ 30''$. It was observed by Flamsteed on April 25, 1693, at $12^h\ 33^m\ 55^s$: but in reducing the zenith distance, in MSS, vol. 23, page 233, he has made an error of $1'$, which I have here corrected.

2140. 7 *Coronæ Borealis* ξ. The right ascension of this star in the *British Catalogue* is $231^\circ\ 59'\ 0''$. It was observed by Flamsteed on May 5, 1692, at $11^h\ 55^m\ 13^s$: but in reducing the right ascension in MSS, vol. 23, page 339, he has made an error of 10^s, which I have here corrected, by deducting $2'\ 30''$ from the value above stated.

2142. 22 *Serpentis* τ^7. There is no letter annexed to this star in the *British Catalogue*: but it is the seventh in the group of 8 stars, designated by the letter τ in Bayer's map. I have therefore inserted it here. See the *Introduction*, page 399.

2143. — *Herculis*. Observed by Flamsteed on June 1, 1702, at $10^h\ 23^m\ 0^s$, just as it was going out of the field of the telescope. It is Piazzi XV. 153.

2148. 26 *Serpentis* τ^8. This star has no letter annexed to it in the *British Catalogue*: but it is the last in the group of 8 stars, designated by the letter τ in Bayer's map. I have therefore inserted it. See the *Introduction*, page 399.

2149. 5 *Lupi* λ. Observed by Flamsteed on May 3, 1692, at $12^h\ 7^m\ 51^s$; and reduced by him in MSS, vol. 23, page 121, where the *apparent* declination comes out equal to $-32^\circ\ 37'\ 45''$. It is evident therefore that some error has been committed in reducing this value to the epoch of 1690; as the precession could not amount to $45''$.

2154. 29 *Serpentis*. The declination of this star in the *British Catalogue* is $+16^\circ\ 28'\ 45''$. It was observed by Flamsteed on April 12, 1703, at $13^h\ 34^m\ 34^s$: and on comparing its zenith distance with that of other stars observed on the same day, it will be seen that an error of $2'\ 30''$ has been made; which I have here corrected. There is no recorded observation of its

magnitude; and it has evidently been mistaken for some other; as Piazzi makes it of the 8th only.

2156. 31 *Serpentis* υ. In the observation of this star on April 25, 1693, at $12^h\ 39^m\ 35^s$, Flamsteed remarks that it has a small bright companion following it: probably Piazzi XV. 176.

2158. 34 *Serpentis* ω. In the observation of this star on May 17, 1693, at $11^h\ 19^m\ 52^s$, there is an error of 20° in the reading of the zenith distance *per lineas diagonales;* as may be readily ascertained by comparing it with the reading *per strias cochleæ*. In the original MS entry, this error is detected and corrected in the margin: but in MSS, vol. 23, page 235, the north polar distance is deduced from the incorrect reading, and which has given rise to the star 33 *Serpentis*, whose position in the *British Catalogue* is 233° 24′ 45″, and D = + 17° 33′ 10″; but which consequently does not exist.

2164. 3 *Scorpii*. This star is called A^2 by Flamsteed: but there is only one star designated by that letter in Bayer's map, which is 2 *Scorpii*. I have therefore rejected it here on account of the difference of magnitude.

2166. 10 *Coronæ Borealis* δ. The declination of this star in the *British Catalogue* is +27° 7′ 45″. It was observed by Flamsteed on May 27, 1692, at $10^h\ 37^m\ 51^s$; and was reduced by him in MSS, vol. 23, page 339. But in copying out the zenith distance from the observing book, he has made a mistake of 5′, which I have here corrected.

2169. 38 *Serpentis* ρ. This star is marked as of the $3\frac{1}{2}$ magnitude in the *British Catalogue:* but in the *original* entry it is designated as of the 5th; which I have therefore adopted.

2171. 39 *Serpentis*. This star is marked as of the 6th magnitude in the *British Catalogue:* but in the *original* entry it is designated as of the $6\frac{1}{2}$; which I have therefore retained.

2173. — *Scorpii*. Observed by Flamsteed on May 6, 1696, at $12^h\ 8^m\ 10^s$. It is the star observed by Lalande in *Hist. Cél.* page 343, at $15^h\ 46^m\ 10^s$, mag. 6.

2176. 48 *Libræ*. This star is designated as ψ in the *British Catalogue:* but there is no star so designated in Bayer's map; I have therefore rejected it here.

2177. — *Serpentis*. Observed by Flamsteed on April 25, 1693, at $12^h\ 47^m\ 34^s$; and regularly reduced by him in MSS, vol. 23, page 233. It is Piazzi XV. 212.

2178. — *Libræ*. Observed by Flamsteed on April 21, 1700, at $13^h\ 2^m\ 6^s$. It is called ψ *Libræ* in the printed copy of the *Historia Cœlestis;* but in the *original* MS entry (MSS, vol. 7) this error is corrected. We find also there a correction of the zenith distance; and that 64° 17′ 20″, 1457·87, should be 64° 7′ 20″ and 1453·87: so that M. Argelander's conjectures on this subject are fully confirmed. It is probably the star observed by Lalande in *Hist. Cél.* page 346, at $15^h\ 48^m\ 0^s{,}5$, mag. $7\frac{1}{2}$; and which, as M. Argelander remarks, occurs twice in Bessel's Zones, Nos. 246 and 249.

2179. 15 *Ursæ Minoris* θ. The position of this star in the *British Catalogue* is $\mathcal{R}$ = 235° 17′ 30″, and D = + 78° 10′ 10″. It was observed by Flamsteed on April 10, 1704, at $13^h\ 45^m\ 49^s$; and reduced by him in MSS, vol. 26 A, page 25. But in deducing the results he has made two errors. For he has assumed this star as his determining star for the right ascension of other stars observed on the same day; and taken its right ascension equal to $15^h\ 41^m\ 9^s + 40^s =$

$15^h\ 41^m\ 49^s = 235°\ 27'\ 15''$; and in Halley's edition of 1712, it is 235° 24′ 50″. It is evident that its right ascension ought not to have been altered without altering all the others that have been determined by it: which would produce great confusion. I have therefore restored the original value; and it will now more nearly accord with modern observations. In the computation of the declination, Flamsteed has made an error of 10′, which I have also corrected. Still there is a greater difference in declination than there ought to be. The above-mentioned right ascension, of $15^h\ 41^m\ 9^s$, has been deduced *per distantias;* as may be seen in MSS, vol. 26 A, page 22; but Flamsteed has there used the erroneous declination.

2180. 1 *Herculis* χ. In Bayer's map, χ *Herculis* is designated as a star of the 4th magnitude: perhaps it was the united appearance of this star and 4 *Herculis*, which gave rise to such a description.

2183. — *Serpentis* ϕ. Observed by Flamsteed on April 25, 1693, at $12^h\ 49^m\ 36^s$: and regularly reduced by him in MSS, vol. 23, page 233. It is Piazzi XV. 222. It is designated by the letter ϕ in Bayer's map, and also in Flamsteed's observation; which I have here retained.

2190. 3 *Herculis.* The position of this star in the *British Catalogue* is $\mathcal{R} = 236°\ 15'\ 0''$, and $D = +\ 5°\ 20'\ 50''$. It was observed by Flamsteed on April 8, 1703, at $13^h\ 57^m\ 42^s$; and reduced by him in MSS, vol. 23, page 343. But, in copying out the time of transit, he has inadvertently written $13^h\ 57^m\ 12^s$: so that the above right ascension must be increased 7′ 30″; which is accordingly done in the present catalogue. With respect to the declination it appears, by comparing its zenith distance with that of the preceding and following stars observed on the same day, that an error of about 1′ has been made; which I have also here corrected. This star is marked as of the 5th magnitude, in the *British Catalogue:* but in the observation book, it is the 6th; which I have retained.

2191. 42 *Serpentis.* I cannot find any observation of this star in the *Historia Cœlestis:* nor can I discover that such a star exists. Its introduction into the *British Catalogue* may have arisen from an error of 2^h in the computation of 69 *Ophiuchi:* but I cannot find any trace of it in the MSS.

2192. 51 *Libræ.* This star is called ξ in the *British Catalogue:* but that letter has been already affixed to 13 and 15 *Libræ.* It is the ξ *Scorpii* of Bayer; and consequently ought not to have been introduced into this constellation. I have therefore rejected the letter here.

2193. 5 *Herculis r.* This star is said to be of the 3rd magnitude in the *British Catalogue:* but in the MS observations it is said to be of the 6th: which accords with modern observations. I have therefore corrected it.

2200. 10 *Scorpii* ω^2. There is but one star designated by the letter ω in Bayer's map: but, as it may be doubtful whether such letter belongs to 9 *Scorpii*, or to the present star, Flamsteed has annexed it to each; which I have therefore retained.

2203. 6 *Herculis* υ. The right ascension of this star in the *British Catalogue* is 238° 21′ 0″: but this is erroneous. It was deduced from the observation made on June 2, 1702, at $10^h\ 40^m\ 29^s$; and was regularly reduced in MSS, vol. 23, page 346; where the right ascension is as stated in the present catalogue; and which agrees more nearly with modern observations.

2208. — *Serpentis.* Observed by Flamsteed on April 8, 1703, the second after $14^h\ 5^m\ 33^s$, and

reduced by him in MSS, vol. 26 C, page 33. It is Piazzi XV. 283; and was also observed by Bradley.

2209. 7 *Herculis* κ. } These positions of these stars do not accord with those in Bayer's map, inas-
2211. 8 *Herculis q.* } much as κ is there represented as the most northerly of the two. The latter star is marked as of the 5½ magnitude, in the *British Catalogue:* but, in the original entries, it is designated twice as the 6th, twice as the 7th, and only once as the 5th; I have therefore taken the mean.

2212. 15 *Scorpii* ψ. This star is designated by the letter χ in the *British Catalogue:* but it ought to have been denoted by the letter ψ; and it is so designated in one of the MS catalogues. It is evidently therefore a typographical error, and I have accordingly altered it. The letter χ properly belongs to 17 *Scorpii.*

2213. 16 *Scorpii.* The declination of this star in the *British Catalogue* is — 7° 37′ 45″. It was observed by Flamsteed on May 16, 1693, at $11^h\ 45^m\ 35^s$: and reduced by him in MSS, vol. 23, page 121. Flamsteed has taken the reading *per lineas diagonales* as the correct reading of the zenith distance; which differs at least 5′ from that *per strias cochleæ.* But the latter is the more correct: and I have therefore added 5′ to the declination above stated.

2214. 13 *Draconis* θ. There is no observation of this star, by Flamsteed, with the mural arc: but its right ascension has been deduced by means of *distances* from δ *Cygni;* as given in the *Historia Cœlestis,* vol. 1, page 72. I cannot find how the declination has been deduced. It differs much from Bradley's observations: but there is a proper motion of + 0″,32 which will diminish this difference to + 82″,4.

2215. 16 *Ursæ Minoris* ζ. The two observations of this star confirm the great difference of declination: but this depends on the accuracy of the index error of the instrument.

2216. 17 *Scorpii* χ. This star has no letter annexed to it in the *British Catalogue:* but it is designated as χ in Bayer's map; and I have therefore inserted it. See the note to No. 2212.

2217. — *Scorpii.* Observed by Flamsteed on June 1, 1699, at $10^h\ 43^m\ 18^s$. It is the star observed by Lalande in *Hist. Cél.* page 342, at $16^h\ 3^m\ 17^s{,}4$, mag. 6½.

2218. 48 *Serpentis.* This star more properly belongs to the constellation *Hercules,* than to that of *Serpens.*

2220. 18 *Ursæ Minoris.* The right ascension of this star is not given in the *British Catalogue,* neither is there any observation of it recorded in the printed copy of the *Historia Cœlestis.* I find however by the computation book, MSS, vol. 23, page 430, that it was observed on April 10, 1704, between $14^h\ 6^m\ 50^s$ and $14^h\ 23^m\ 52^s$; but the time of transit is not inserted. The corrected zenith distance is stated to be 29° 24′ 45″: and the corresponding declination deduced by Flamsteed, should be 80° 56′ 0″, and not 80° 57′ 0″ as stated in the *British Catalogue,* which I have here corrected. It appears that this star was observed about 5^m after its transit over the meridian: which might easily happen in a star so near the pole. I have assumed an approximate right ascension corresponding to modern observations. The book containing the original MS entry is lost: and there is no trace of the observation in the MS copy (MSS, vol. 17).

2224. 18 *Scorpii.* This star is marked as of the 4th magnitude, in the *British Catalogue:* but

in the *original* entry, it is designated only as the 7th, which I have retained. Piazzi makes it to be the 5th: and I cannot account for the discordance, unless it be a variable star.

2225. 49 *Serpentis.* The right ascension of this star in the *British Catalogue* is 239° 29′ 0″. It was observed by Flamsteed on April 8, 1703, at $14^h\ 10^m\ 8^s$: but no star exists corresponding to that observation. If we suppose, however, that he has made a mistake of 1^m in recording the time of transit, and that $14^h\ 10^m\ 8^s$ should have been $14^h\ 11^m\ 8^s$, it will then agree with the star that is generally assumed to be 49 *Serpentis.* Although there is nothing in the MS entry to warrant this alteration, I have yet ventured to make it.

2227. — *Scorpii.* This star is not in the *British Catalogue*, neither can I find any observation of it by Flamsteed. It is however given in Halley's edition of 1712, and therefore I made no doubt but that I should discover some notice amongst Flamsteed's manuscripts. After much search I found it interlined in a fragment of a catalogue of the zodiacal stars (MSS, vol. 27 A): but no reference as to the observation from which it was deduced. It is Piazzi XVI. 31.

2228. 12 *Herculis.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the observation book, it is designated as the 7th; which I have adopted.

2229. 13 *Herculis p.* This star has no letter annexed to it in the *British Catalogue:* but as it is designated by the letter *p* in Bayer's map, I have here retained it.

2236. 17 *Herculis.* The declination of this star in the *British Catalogue* is + 23° 57′ 30″. It was observed by Flamsteed on May 16, 1702, at $11^h\ 54^m\ 50^s$: and by comparing its zenith distance with that of other stars observed on the same day, it is evident that an error of 2′ has been made, which I have here corrected.

2238. 17 *Ursæ Minoris.* The right ascension of this star must be diminished about 5′ in order to correspond with modern observations.

2244. — *Herculis.* Observed by Flamsteed on May 11, 1702, at $12^h\ 17^m\ 56^s$. It is the star observed by Lalande in *Hist. Cél.* page 468, at $16^h\ 10^m\ 25^s$, mag. 6.

2245. 5 *Ophiuchi* ρ. This star is called *g*, in the *British Catalogue:* but, there is no such letter in Bayer's map. It is in fact ρ, which Flamsteed has applied erroneously to 40 *Ophiuchi;* but which I have here restored to the proper star. The error originated with Bayer, who has placed the star in *north* latitude, instead of *south.*

2247. 21 *Herculis o.* In the reduction of the observation of this star made on May 22, 1693, at $11^h\ 30^m\ 38^s$, in MSS, vol. 23, page 243, the results seem to be correctly computed. But in copying out the polar distances of the stars in the constellation *Ophiuchus*, in page 306 of the same volume, Flamsteed has inadvertently annexed the polar distance of the preceding star (9 *Herculis*): whence has arisen the introduction of the star 6 *Ophiuchi* into the *British Catalogue;* whose position as there given is $\mathcal{R}$ = 242° 15′ 50″, and D = + 5° 50′ 25″, but which consequently does not exist.

2253. 21 *Coronæ Borealis* ν^2. The position of this star in the *British Catalogue* is $\mathcal{R}$ = 249° 49′ 30″ and D = + 34° 7′ 45″. This false position has been deduced by Flamsteed from the observation on May 5, 1692, at $12^h\ 38^m\ 8^s$; which he has reduced in MSS, vol. 23, page 339. But from a singular confusion of the figures, which cannot well be explained, but which is easily discovered on inspection, he has deduced the above erroneous result. The value in the present

catalogue is deduced from the 4 comparisons of observations with 20 *Coronæ Borealis*, indicated by Miss Herschel. I would here remark, that there is only one star indicated by the letter *r*, in Bayer's map: but, as there might be a doubt whether it belonged to this, or the preceding star, Flamsteed has annexed it to each, which I have therefore retained.

2254. 3 *Ophiuchi v.* The right ascension of this star in the *British Catalogue* is 241° 30′ 30″; which is deduced by Flamsteed in MSS, vol. 23, page 243, from the observation of May 28, 1693, at $11^h\ 3^m\ 41^s$. But the two subsequent observations of this star, indicated by Miss Herschel, clearly show that an error of 5^m has been made in the time of transit; and that it should have been $11^h\ 8^m\ 41^s$. With this correction, all the observations agree: as well as the reduction *per distantias* in MSS, vol. 62 D, page 6. I have therefore added 1° 15′ to the above value. It is Piazzi XVI. 83 = (6 Hev.) *Ophiuchi.*

2255. — *Ophiuchi.* Observed by Flamsteed on May 10, 1702, at $12^h\ 26^m\ 10^s$; and again on the following day, at $12^h\ 22^m\ 11^s$. It is the star observed by Lalande in *Hist. Cél.* page 290, at $16^h\ 16^m\ 11^s,6$.

2256. 23 *Herculis.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but, in the *original* entry, it is designated as the 6th; which I have retained.

2257. 24 *Herculis ω.* This is the same star as 51 *Serpentis*, whose position in the *British Catalogue* is Æ = 242° 47′ 45″, and D = + 14° 46′ 15″.

2258. — *Serpentis.* Observed by Flamsteed on June 2, 1693, at $10^h\ 48^m\ 50^s$: the time however is uncertain. It is the star observed by Lalande in *Hist. Cél.* page 291, at $16^h\ 16^m\ 30^s,5$, mag. 6.

2261. — *Herculis.* Observed by Flamsteed on April 7, 1703, the first after $14^h\ 26^m\ 52^s$. It is the star observed by Lalande in *Hist. Cél.* page 81, at $16^h\ 18^m\ 29^s$, mag. 7½.

2263. — *Herculis.* Observed by Flamsteed on April 7, 1703, the second after $14^h\ 26^m\ 52^s$; and again on the next day, at $14^h\ 26^m\ 33^s$. It is the star observed by Lalande in *Hist. Cél.* page 81, at $16^h\ 18^m\ 48^s,5$, mag. 7½.

2266. 26 *Herculis.* This star is marked as of the 6½ magnitude in the *British Catalogue:* but in the *original* entry, it is designated only as the 7th; which I have retained.

2268. 27 *Herculis β.* The declination of this star in the *British Catalogue* is +22° 9′ 55″: which has been deduced from the observation of June 2, 1702, at $11^h\ 3^m\ 55^s$; and which has been regularly reduced by Flamsteed, in MSS, vol. 23, page 346. But in copying it out for the *British Catalogue* he does not appear to have made any allowance for the 12½ years' precession. I have therefore added 1′ 50″ to the above value.

2269. — *Herculis s.* Observed by Flamsteed on June 27, 1708, after $9^h\ 9^m\ 57^s$; but the time is not mentioned. Unfortunately the original MS entry is lost; and in the transcript, it is altogether omitted. Flamsteed has made the reduction of it in MSS, vol. 23, page 344; and there the time of transit is stated at $9^h\ 10^m\ 15^s$; still however marked as doubtful. It is evident that it must have passed soon after the transit of *β Herculis:* and I think there can be no doubt but that it is the star observed by Lalande, in *Hist. Cél.* page 468, at $16^h\ 20^m\ 52^s,6$, mag. 5; and thus the conjectures of M. Argelander are fully verified. It is denoted by the letter *s* in Bayer's map; which I have here retained.

2270. — *Herculis.* Observed by Flamsteed on April 12, 1703, after $14^h\ 12^m\ 25^s$. It is the star observed by Lalande in *Hist. Cél.* page 84, at $16^h\ 22^m\ 23^s$, mag. $8\frac{1}{2}$.

2271. 28 *Herculis n.* This is the same star as 11 *Ophiuchi;* whose position in the *British Catalogue* is $\mathcal{R} = 244°\ 19'\ 45''$ and $D = +6°\ 12'\ 25''$. It is designated by the letter *n* in Bayer's map; which I have here retained.

2272. — *Herculis.* Observed by Flamsteed on May 6, 1691, $12^h\ 46^m\ 40^s$; but in the original MS entry (MSS, vol. 5) there is a marginal note that it should be $12^h\ 45^m\ 40^s$: in which case the time would agree with β *Herculis;* but the declination would be very discordant. It is difficult to decide what star was actually observed: but I am inclined to think, with M. Argelander, that it is the star observed by Lalande in *Hist. Cél.* page 348, at $16^h\ 22^m\ 57^s{,}4$, mag. 7; and again in page 469, at $16^h\ 22^m\ 0^s{,}6$, mag. $5\frac{1}{2}$.

2275. — *Herculis.* Observed by Flamsteed on April 7, 1703, the fourth after $14^h\ 26^m\ 52^s$. It is the star observed by Lalande in *Hist. Cél.* page 84, at $16^h\ 24^m\ 5^s$, mag. 7.

2276. 14 *Draconis* η. M. Lalande states that he could not find this star: but it certainly exists.

2283. 33 *Herculis.* The right ascension of this star in Halley's edition of 1712 is $245°\ 26'\ 0''$; which corresponds more nearly with the two observations of Flamsteed, than the value here given.

2284. 34 *Herculis.* The right ascension of this star in Halley's edition of 1712 is $245°\ 24'\ 0''$; which more nearly corresponds with the two observations of Flamsteed. In fact the right ascension here given must be reduced above 5′, in order to correspond with modern observations.

2285. — *Herculis.* Observed by Flamsteed on June 2, 1702, at $11^h\ 10^m\ 0^s$. It is Piazzi XVI. 127.

2286. — *Herculis.* Observed by Flamsteed on April 7, 1703, at $14^h\ 39^m\ 14^s$. It is Piazzi XVI. 136.

2287. 24 *Scorpii.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entry it is designated as $5\frac{1}{2}$; which I have here adopted.

2288. — *Scorpii.* Observed by Flamsteed on June 26, 1692, at $9^h\ 22^m\ 58^s$; again on May 16, 1693, after $12^h\ 13^m\ 50^s$; and again on May 28, 1693, after $11^h\ 21^m\ 18^s$. It is Piazzi XVI. 145.

2290. 36 *Herculis* m^1. This star is marked as of the 6th magnitude in the *British Catalogue:* but in the *original* entry it is designated as of the 7th; which I have here adopted.

2291. 37 *Herculis* m^2. There is only one star designated by the letter *m* in Bayer's map; but, as there may be a doubt whether such letter belongs to this or the preceding star, Flamsteed has annexed it to each; which I have therefore retained.

2292. 14 *Ophiuchi.* In the observation of this star on May 22, 1693, at $11^h\ 47\frac{1}{2}^m$, Flamsteed remarks that it has some bright stars near it: probably those observed by Lalande in *Hist. Cél.* page 291: and one of which was also observed by Bradley.

2294. — *Herculis.* Observed by Flamsteed on April 7, 1703, at $14^h\ 42^m\ 7^s$; and again on the next day at $14^h\ 38^m\ 35^s$: and regularly reduced by him in MSS, vol. 23, pages 340 and 342. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken: and I know not why it was afterwards rejected. It is Piazzi XVI. 154.

2295. 15 *Ophiuchi.* The right ascension of this star in the *British Catalogue* is 246° 54′ 45″. It was observed by Flamsteed on June 3, 1711, at $11^h\,0^m\,3^s$; and reduced by him in MSS, vol. 26 C, page 34. But in reducing it, he has evidently committed an error of 1^m in the time; as may be seen by comparing it with the transit of other stars observed on the same day. I have therefore deducted 15′ from the value above given.

2296. 21 *Ursæ Minoris* η. The right ascension of this star in the *British Catalogue* is 246° 20′ 0″. It was observed by Flamsteed on April 10, 1704, at $14^h\,30^m\,39^s$; and has been assumed by him as the determining star, on that day, for several of the stars observed. See MSS, vol. 26 A, page 25. The right ascension, which he has there assumed, is that which I have given in this catalogue; and which is nearly the same as that given in Halley's edition of 1712; and which corresponds more nearly with modern observations than that inserted in the *British Catalogue.* Why it was afterwards altered I have not been able to ascertain: but it is evident there is no ground for such an alteration. And, as the right ascension of all the other stars which are determined therefrom, ought also to be altered (which would produce great confusion) I have restored the original value: being convinced that the alteration has arisen from some error. The right ascension here adopted has been deduced *per distantias:* as may be seen in MSS, vol. 26 A, page 23.

2298. 15 *Draconis A.* The right ascension of this star is deduced *per distantias,* as may be seen in MSS, vol. 26 A, page 13: but it must be increased nearly 5′ in order to agree with modern observations. There are also *two* reductions of the observation of it with the mural arc: one in the same volume, page 19, from ν *Draconis,* observed on the same day; and the other in vol. 26 B, page 3, from β *Draconis;* where the time of transit of β is taken $9^h\,57^m\,59^s$ instead of $9^h\,56^m\,59^s$. Both of these reductions (after the correction of the error just alluded to) give the right ascension more accurately than in the *British Catalogue.*

2299. — *Herculis.* Observed by Flamsteed on May 14, 1704, at $12^h\,21^m\,51^s$. It is the star observed by Lalande in *Hist. Cél.* page 169, at $16^h\,31^m\,33^s{,}5$, mag. 6.

2301. 16 *Draconis.* The right ascension of this star must be diminished 9′ 0″,5 in order to correspond with Bradley's observations. It was observed on July 1, 1703, at $9^h\,3^m\,36^s$; and, together with 17 *Draconis,* preceded No. 2299 of the present catalogue. In MSS, vol. 26 A, page 18, where these stars are extracted for the purpose of reduction, there is the following note, "Tempora transituum vitiosissime notata à T. Westono; vide protocollum observationum." On reference however to the protocol (MSS, vol. 16) I do not find any notice of the fact alluded to. In MSS, vol. 26 B, page 3, these stars are reduced in the usual manner, without any remarks; there they *precede* 15 *Draconis.* They are not inserted in Halley's edition of 1712.

2302. 17 *Draconis.* The right ascension of this star must be diminished 8′ 58″,5 in order to correspond with Bradley's observations. It was observed on July 1, 1703, at $9^h\,3^m\,39^s$: see the preceding note. But, besides this observation of the star, there is another on June 2, 1702, after $11^h\,17^m\,38^s$: which, owing to the negligence of the transcriber, has been erroneously printed; and has thus given rise to the supposition of another and a different star, No. 486 in Miss Herschel's catalogue, and No. 441 in my list of Flamsteed's inedited stars. In the copy of the MS entry (MSS, vol. 16) the observation is stated to have been made to the *north:* and there is a remark added, that it is the mean of two nearly contiguous stars, whose transit took place a

little *before* the preceding star (42 *Herculis*). These two stars were undoubtedly 16 and 17 *Draconis*: and thus M. Argelander's conjectures have been fully confirmed.

2307. 42 *Herculis*. In the original MS entry of the observation of this star on June 2, 1702, at $11^h\ 17^m\ 38^s$, Flamsteed remarks that it has a companion preceding it to the south; which is probably Piazzi XVI. 161.

2308. 43 *Herculis i*. This is the same star as 17 *Ophiuchi*; whose position in the *British Catalogue* is $R = 247° 43' 45''$ and $D = +9° 10' 50''$.

2310. — *Herculis*. Observed by Flamsteed on April 7, 1703, at $14^h\ 47^m\ 10^s$; and again on the next day, at $14^h\ 43^m\ 33^s$: and regularly reduced by him in MSS, vol. 23, pages 340 and 342. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken: and I know not why it was afterwards rejected. It is the star observed by Lalande, in *Hist. Cél.* page 81, at $16^h\ 35^m\ 50^s$.

2313. 45 *Herculis l*. This star is called *e* in the *British Catalogue*; which letter is also annexed to 69 *Herculis*: but it appears to be a typographical error. For it is designated as *l* by Bayer, whom Flamsteed professed to follow; and it appears to be so marked in the MS catalogues. I have therefore restored the correct reading.

2316. — *Herculis*. Observed by Flamsteed on April 7, 1703, at $14^h\ 49^m\ 12^s$; and again on the next day, at $14^h\ 45^m\ 39^s$: and regularly reduced by him in MSS, vol. 23, pages 340 and 342. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken: and I know not why it was afterwards rejected. It is the star observed by Lalande, in *Hist. Cél.* page 84, at $16^h\ 38^m\ 14^s{,}3$, mag. $7\frac{1}{2}$.

2318. — *Herculis*. Observed by Flamsteed on April 7, 1703, at $14^h\ 51^m\ 0^s$; and again on the next day, at $14^h\ 47^m\ 25^s$: and regularly reduced by him in MSS, vol. 23, pages 340 and 342. It is also inserted in Halley's edition of 1712; from which the position in the present catalogue is taken: and I know not why it was afterwards rejected. It is the star observed by Lalande, in *Hist. Cél.* page 83, at $16^h\ 39^m\ 52^s{,}5$, mag. 6.

2323. 23 *Ophiuchi*. This star is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entry it is designated as $5\frac{1}{2}$; which I have here retained.

2325. 24 *Ophiuchi*. The declination of this star in the *British Catalogue* is $-22° 46' 45''$. It was observed by Flamsteed on June 3, 1711, at $11^h\ 11^m\ 37^s$: and on comparing the zenith distance with that of other stars observed on the same day, it will be seen that an error of $10'$ has been committed; which I have here corrected.

2330. — *Herculis*. Observed by Flamsteed on April 7, 1703, at $14^h\ 55^m\ 2^s$: and regularly reduced by him in MSS, vol. 23, page 340. M. Argelander says that it is No. 171 *Herculis*, in Bode's great catalogue; observed by Lalande, but not to be found recorded in his *Hist. Cél.* It is marked as of the 8th magnitude, in the MS entry (MSS, vol. 16.)

2335. 55 *Herculis*. I cannot find any other observation of this star than the imperfect one on April 8, 1703, immediately after the transit of 54 *Herculis*, at $14^h\ 53^m\ 59^s$. But no notice is taken of this observation by Flamsteed, in his reduction of the stars observed on that day, either in MSS, vol. 23, page 342, or in MSS, vol. 24, page 141; and as, in the MS entry (MSS, vol. 16) the two observations are united by a circumflex, I apprehend he considered

that they belonged to the same star. The references given by Miss Herschel to this star, evidently belong to 54 *Herculis*.

2341. — *Herculis*. Observed by Flamsteed on April 7, 1703, at $15^h\ 0^m\ 19^s$; and again on the next day at $14^h\ 56^m\ 48^s$. It is the star observed by Lalande in *Hist. Cél.* page 81, at $16^h\ 49^m\ 6^s$.

2342. 28 *Ophiuchi*. The right ascension of this star in the *British Catalogue* is 250° 59′ 0″. It was observed by Flamsteed on June 3, 1707, at $11^h\ 22^m\ 42^s$, and reduced in MSS, vol. 26 C, page 34; but in reducing it and the next following star (31 *Ophiuchi*) it is evident, by comparing their time of transit with η or A *Ophiuchi*, observed on the same day, that an error of 1^m has been committed, which I have here corrected by adding 15′ to the above value.

2343. — *Ophiuchi*. Observed by Flamsteed on May 13, 1704, at $12^h\ 42^m\ 17^s$. It is the star observed by Lalande in *Hist. Cél.* page 89, at $16^h\ 50^m\ 14^s{,}5$ mag. $6\frac{1}{2}$.

2344. 31 *Ophiuchi*. The right ascension of this star in the *British Catalogue* is 251° 11′ 0″. It was observed by Flamsteed on June 3, 1707, at $11^h\ 23^m\ 30^s$: but in reducing it he has evidently committed an error of 1^m, as I have already remarked in the note to No. 2342 above. I have therefore added 15′ to the above value.

2345. — *Herculis*. Observed by Flamsteed on April 7, 1703, after $15^h\ 0^m\ 19^s$; and again on the next day, at $14^h\ 59^m\ 37^s$: and was regularly reduced by him in MSS, vol. 23, pages 340 and 342. It was also inserted in Halley's edition of 1712; from which, the position in the present catalogue is taken: and I know not why it was afterwards rejected. It is the star observed by Lalande in *Hist. Cél.* page 81, at $16^h\ 51^m\ 58^s{,}5$, mag. 6.

2348. 32 *Ophiuchi*. The right ascension of this star in the *British Catalogue* is 252° 5′ 15″. There are two observations of it by Flamsteed: viz. one on May 5, 1692, at $13^h\ 15^m\ 48^s$; and the other on April 12, 1703, at $14^h\ 51^m\ 8^s$. The former is regularly reduced by him in MSS, vol. 23, page 241, and is the one from which the value in the *British Catalogue* is taken: but it is evident that an error of 40^s has been made in noting down the time of transit; and that $13^h\ 15^m\ 48^s$ should be $13^h\ 16^m\ 28^s$: and consequently that 10′ should have been added to the above value, which I have accordingly done in the present catalogue. In the reduction of the second observation in MSS, vol. 26 C, page 33, Flamsteed appears to have taken α *Ophiuchi* as the determining star for his right ascension, instead of α *Herculis*; whereby an error of about 5° has been committed: and which has given rise to the introduction of 46 *Ophiuchi* into the *British Catalogue*, whose position there given is $\mathcal{R}$=257° 14′ 0″ and D=+14° 33′ 40″, but which does not exist.

2352. — *Ophiuchi*. Observed by Flamsteed on June 30, 1690, at $9^h\ 30^m\ 31^s$; and regularly reduced by him in MSS, vol. 23, page 239; but not included in his list of stars in the constellation *Ophiuchus*, in page 306 of the same volume. It is Piazzi XVI. 297=674 Mayer.

2353. 60 *Herculis*. One of the observations of this star, viz. that made on April 12, 1703, at $14^h\ 53^m\ 10^s$, has been reduced by Flamsteed in MSS, vol. 26 C, page 33: but in so doing he has assumed α *Ophiuchi* as his determining star for the right ascension, instead of α *Herculis*. Whereby an error of about 5° has been committed; and which has given rise to the introduction of 48 *Ophiuchi* into the *British Catalogue*; whose position there given is $\mathcal{R}$=257° 44′ 30″, and D=+13° 11′ 30″; but which consequently does not exist. The present star is marked

as of the 6th magnitude, in the *British Catalogue*: but, in the *original* entries, it is designated once only as the 6th, once as the 5th, and once as $4\frac{1}{2}$; I have therefore taken the mean.

2354. 61 *Herculis c.* The right ascension of this star in the *British Catalogue* is 253° 34′ 30″. It was observed by Flamsteed on May 16, 1702, at $12^h\,44^m\,6^s$; and reduced by him in MSS, vol. 23, page 345. But in the steps of the process he has made an error of 2^m: I have therefore deducted 30′ from the above value, and it will now correspond with modern observations.

2356. — *Scorpii.* Observed by Flamsteed on June 5, 1690, after $11^h\,1^m\,50^s$. It is probably Piazzi XVI. 311; which is erroneously called 29 *Scorpii* by Lalande, in *Hist. Cél.* page 567.

2357. 19 *Draconis h*[1]. There is no letter annexed to this star in the *British Catalogue;* but it is, in fact, the star designated by the letter *h* in Bayer's map, which Flamsteed has annexed to the following star. As however there may be a doubt on the subject, I have annexed the letter to each.

2359. — *Ophiuchi.* Observed by Flamsteed on May 14, 1704, at $12^h\,48^m\,13^s$. It is the star observed by Lalande in *Hist. Cél.* page 86, at $16^h\,59^m\,46^s{,}5$, mag. 7.

2362. 36 *Ophiuchi A.*
2363. 30 *Scorpii.*
} The observations of Bradley and Piazzi confirm the proper motion of these stars. The first of them is designated by the letter *A*: but its position does not accord with the star so called in Bayer's map: nor can I find any star that will agree therewith. I have however preserved Bayer's designation, as it is now well known by that letter, and is not likely to create any confusion. Both the stars ought to have been included, by Flamsteed, in the constellation *Ophiuchus.*

2365. 63 *Herculis.* The position of this star in the *British Catalogue* is $\mathrm{AR} = 254°\,16'\,0''$, and $\mathrm{D} = +\,27°\,39'\,0''$. It was observed by Flamsteed on May 16, 1702, at $12^h\,49^m\,51^s$: and reduced by him in MSS, vol. 23, page 345. But in deducing the result, he has committed two mistakes: for, in the first place, he has made an error of 2^m in taking out the difference in the time of transit of his determining star; which causes an error of 30′ in the right ascension: and secondly, he has made an error of 3° in copying out the polar distance in his list of stars in the constellation *Hercules*, in MSS, vol. 24, page 104. Both these errors are corrected in the present catalogue. It is Piazzi XVII. 11; and was also observed by Bradley.

2366. 31 *Scorpii.* This is the same star as 38 *Ophiuchi.* In the observation of this star by Flamsteed, on June 5, 1690, at $11^h\,23^m\,7^s$, he remarks that 3 or 4 telescopic stars follow it, at 30′ distant.

2370. 41 *Ophiuchi.* The right ascension of this star in the *British Catalogue* is 255° 38′ 45″. There are 3 observations of it by Flamsteed; and the right ascension in the *British Catalogue* has been deduced from that made on June 21, 1704, at $10^h\,18^m\,16^s$, as appears by MSS, vol. 26 C, page 33. But the time of transit is erroneous 2^m, as is evident by comparing the intervals of 37 and 41 *Ophiuchi* on the following day, and on May 13th preceding. I have therefore deducted 30′ from the right ascension above-mentioned. I apprehend that the entry should stand thus:

h	m	s		°	′	″
10	16	14	α Herculis . . .	36	53	0
			Transit cum precedente .	51	42	40
10	18	16	65 Herculis . . .	26	25	40

This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the *original* entries it is once designated as the 4th, and twice as the 5th; I have therefore altered it.

2372. 40 *Ophiuchi* ξ. This star is designated by the letter ρ, in the *British Catalogue:* but this is erroneous, inasmuch as that letter is applied, in Bayer's map, to 5 *Ophiuchi;* which Flamsteed calls *g*. The present star is the ξ of Bayer; which I have here restored.

2373. 32 *Scorpii*. This star is marked as of the 6th magnitude, in the *British Catalogue:* but in the observation book it is designated as the 7th; which I have therefore retained.

2374. 42 *Ophiuchi* θ. I have called this star θ, after Flamsteed: but it does not accord with the position of the star so designated in Bayer's map, which has *north* latitude. I apprehend however that the error is Bayer's. See the *Introduction*, page 402.

2375. 53 *Serpentis* ν. In the reduction of one of the observations of this star (June 16, 1690, at $10^h\ 43^m\ 19^s$) Flamsteed has committed an error by taking the right ascension of his determining star, on that day, to be η *Hercules*, instead of η *Ophiuchi:* as may be seen in MSS, vol. 23, page 233. Whence has arisen the introduction of 52 *Serpentis* into the *British Catalogue:* whose position there given is $\mathcal{R} = 250°\ 56'\ 15''$, and $D = -12°\ 29'\ 40''$; but which does not exist.

2376. 43 *Ophiuchi*. This star is marked as of the $4\frac{1}{2}$ magnitude, in the *British Catalogue:* but in the *original* entry it is twice designated as the 6th, and once (not as a star of the 4th magnitude, as in the printed copy, but) as a telescopic star. I have therefore altered the value.

2377. 66 *Herculis*. The declination of this star in the *British Catalogue* is $+\ 11°\ 10'\ 20''$. It has been correctly deduced by Flamsteed from the observation made on June 30, 1690, at $9^h\ 43^m\ 58^s$: as may be seen in MSS, vol. 23, page 341. But in copying it out in the list of stars in the constellation *Hercules*, in MSS, vol. 24, page 104, he has made a mistake of $4'$, which I have here corrected. The correct declination may also be seen in MSS, vol. 26 C, page 33; where it is also deduced from another observation. In the *British Catalogue* the letter ω is affixed to this star: but that letter has been already annexed to 24 *Herculis;* to which it properly belongs. In Bayer's map (which Flamsteed professed to follow) it does not exist as a star in *this* constellation. I have therefore rejected the letter here. It is the same star as *e Ophiuchi* in Bayer's map.

2382. 44 *Ophiuchi b*. This star is designated by a capital *B*, in the *British Catalogue:* but, as Bayer never uses the capital letters, except in the letter *A*, I have restored the correct reading. It is also marked as of the $4\frac{1}{2}$ magnitude: but, in the observation book, it is designated thrice as the 5th, and once as the 6th; I have therefore altered it. See the *Introduction*, page 402.

2383. 45 *Ophiuchi d*. This star has no letter annexed to it in the *British Catalogue:* but it is denoted by *d* in Bayer's map; which letter Flamsteed has erroneously affixed to 58 *Ophiuchi*. I have therefore corrected the error.

2384. 70 *Herculis*. The observation of this star by Flamsteed on June 3, 1702, at $11^h\ 51^m\ 16^s$, has been regularly reduced by him in MSS, vol. 23, page 346: but in copying it out in the list of stars in the constellation *Hercules*, in MSS, vol. 24, page 104, he has made an error, in the right ascension, of 24^s; which has given rise to the introduction of 71 *Herculis* into the *British Catalogue;* whose position, as there given, is $\mathcal{R} = 257°\ 6'\ 0''$, and $D = +24°\ 50'\ 20''$: but which consequently does not exist. The present star is marked as of the 4th magnitude,

in the *British Catalogue*: but, in the *original* observations, it is designated once as the 4th and once as the 5th. I have therefore taken the mean.

2385. 22 *Draconis* ζ. This star is marked as of the 2nd magnitude, in the *British Catalogue*: but, in the *original* entries, it is once designated as the 3rd, and once as the 5th; I have therefore taken the mean. Piazzi makes it to be the 3rd.

2386. 72 *Herculis* ω. There appears to be an annual proper motion of this star, in declination, of — 0″,960; which will reduce the difference in declination to — 9″,3.

2389. — *Ophiuchi*. Observed by Flamsteed on May 10, 1702, at $13^h\ 25^m\ 9^s$. It is Piazzi XVII. 99 = (27 Hev.) *Ophiuchi*.

2390. — *Herculis*. Observed by Flamsteed on May 13, 1704, at $13^h\ 8^m$: but the time is doubtful. The observation is not inserted in the MS *copy*; and the *original* is lost. It is the star observed by Lalande in *Hist. Cél.* page 88, at $17^h\ 16^m\ 11^s,5$, mag. 6.

2394. 50 *Ophiuchi* c^1. } These stars are denoted by the letter *e* in the *British Catalogue*: but this
2395. 51 *Ophiuchi* c^2. } is evidently a misprint; as they are denoted by *c* in Bayer's map; which I have here restored; although I am not quite satisfied with their accordance to Bayer. See the *Introduction*, page 402.

2396. 35 *Scorpii* λ. This star has not any letter affixed to it in the *British Catalogue*: but it ought to have been denoted by the letter λ, as we find in some of the MS catalogues; and as it is designated by Bayer. I have therefore inserted it.

2400. 77 *Herculis x*. This star is called κ in the *British Catalogue*: but that letter has been already affixed to 7 *Herculis*, to which it properly belongs. It is *x* in Bayer, whom Flamsteed professed to follow; and I have therefore restored the correct reading. It was probably an error of the press.

2401. — *Herculis*. Observed by Flamsteed on June 19, 1693, at $10^h\ 48^m\ 57^s$. It is Piazzi XVII. 143.

2403. 22 *Ursæ Minoris* ε. The right ascension of this star in the *British Catalogue* is 259° 33′ 0″. It was observed by Flamsteed on April 11, 1704, at $15^h\ 20^m\ 24^s$; and extracted for computation in MSS, vol. 26 A, page 25. The right ascension there assumed is 259° 53′ 0″: and in Halley's edition of 1712 it was 259° 55′ 10″. I apprehend therefore that there is a typographical error in the *British Catalogue*, and I have therefore restored the original value; which will now more nearly correspond with modern observations. The right ascension here adopted has been deduced *per distantias*; as may be seen in MSS, vol. 26 A, page 23.

2404. 53 *Ophiuchi f*. This star has no letter annexed to it in the *British Catalogue*: but it is designated by the letter *f* in Bayer's map; which I have therefore retained.

2406. — *Serpentis*. Observed by Flamsteed on June 8, 1691, at $11^h\ 37^m\ 16^s,5$; again on April 10, 1692, at $15^h\ 23^m\ 17^s$; again on April 12, 1692, at $15^h\ 14^m\ 0^s$; again on July 4, 1701, at $9^h\ 49^m\ 15^s$; and again on July 12, 1703, at $9^h\ 17^m\ 8^s$: and is inserted in his MS catalogue, in MSS, vol. 27 B, in the constellation *Sagittarius*. It is Piazzi XVII. 156 = 691 Mayer.

2411. 57 *Ophiuchi* μ. The right ascension of this star in the *British Catalogue* is 260° 30′ 0″. There are two observations of it by Flamsteed: one on April 12, 1692, at $15^h\ 15^m\ 7^s$; and the other on May 10, 1702, at $13^h\ 36^m\ 55^s$. The former appears to have furnished the right ascen-

sion which is given by Halley in his edition of 1712; viz. 260° 18′ 15″: whilst the latter seems to have been the authority from which the right ascension in the *British Catalogue* is deduced. But there is every appearance that an error of 1ᵐ has been made in entering the time of transit; and that 13ʰ 36ᵐ 55ˢ should be 13ʰ 35ᵐ 55ˢ. I have therefore deducted 15′ from the first value. In the *British Catalogue* this star is marked as of the 4th magnitude: but, in the *original* entry, it is designated as 5½ only; which I have therefore retained.

2413. 56 *Serpentis* o. In the reduction of one of the observations of this star (June 16, 1690, at 11ʰ 3ᵐ 51ˢ) Flamsteed has committed an error by taking the right ascension of his determining star, on that day, to be η *Herculis*, instead of η *Ophiuchi*; as may be seen in MSS, vol. 23, page 233. Whence has arisen the introduction of 54 *Serpentis* into the *British Catalogue*, whose position there given is $\mathcal{R} = 256° 5′ 15″$, and $D = -12° 40′ 10″$; but which does not exist.

2414. 79 *Herculis*. The right ascension of this star in the *British Catalogue* is 261° 6′ 45″. It was observed by Flamsteed on June 19, 1693, at 10ʰ 54ᵐ 4ˢ; and regularly reduced by him in MSS, vol. 23, page 341: and also correctly copied out in his list of stars in the constellation *Hercules* in MSS, vol. 24, page 104, as stated in this catalogue. I presume therefore that the above is a typographical error, which I have here corrected.

2415. 58 *Ophiuchi*. This star is designated by the letter *D*, in the *British Catalogue*: but it does not accord with the position of that star in Bayer's map; the letter should, in fact, be annexed to 45 *Ophiuchi*. I have therefore rejected it here.

2416. 24 *Draconis* ν^1. } The observations of these stars on July 1, 1690, at 10ʰ 1ᵐ 45ˢ, and 10ʰ
2417. 25 *Draconis* ν^2. } 1ᵐ 50ˢ, have been reduced by Flamsteed in MSS, vol. 23, page 341, as if they had been observed to the *south* of the zenith. Whence has arisen the introduction of the two stars 80 and 81 *Herculis*, whose positions, in the *British Catalogue*, are respectively $\mathcal{R} = 261° 32′ 15″$, $D = +47° 34′ 50″$, and $\mathcal{R} = 261° 33′ 30″$, $D = +47° 34′ 30″$; but which consequently do not exist. The present stars are both marked, in the *British Catalogue*, of the 4th magnitude: but, in the *original* entries, they are each designated once as the 5th and once as the 6th; I have therefore altered them.

2418. 3 *Sagittarii*. One of the observations of this star by Flamsteed was made on June 5, 1690, at 11ʰ 52ᵐ 41ˢ; and has been reduced by him in MSS, vol. 23, page 245. But in deducing the north polar distance he has made an error of 6°, which has given rise to the introduction of 59 *Ophiuchi* into the *British Catalogue*, whose position as there given is $\mathcal{R} = 262° 1′ 15″$, and $D = -21° 40′ 40″$; but which consequently does not exist. The present star has the letter *p* annexed to it in the *British Catalogue*: but as there is no such letter in Bayer's map, I have here rejected it.

2421. 61 *Ophiuchi*. The declination of this star in the *British Catalogue* is + 2° 46′ 20″. It was observed by Flamsteed on June 3, 1691, at 12ʰ 4ᵐ 59ˢ; and regularly reduced by him in MSS, vol. 23, page 241. But he has assumed the reading *per lineas diagonales* as the correct reading; which however differs 1′ from that *per strias cochleæ*. Modern observations show that the latter reading is the more correct; and I have accordingly added 1′ to the above value.

2424. 84 *Herculis*. The right ascension of this star in the *British Catalogue* is 262° 35′ 30″. It was observed by Flamsteed on June 19, 1693, 10ʰ 59ᵐ 56ˢ; and reduced by him in MSS, vol.

23, page 342: but an error is easily discoverable in the steps of the process. As this star passed 52ˢ after 83 *Herculis*, I have added 13′ to the right ascension of that star, in order to obtain the value in the present catalogue. But both of them must be diminished above 1′ in order to correspond with modern observations.

2425. 26 *Draconis*. The declination of this star in the *British Catalogue* is + 62° 2′ 20″. It was observed by Flamsteed on July 9, 1703, at $9^h\ 41^m\ 49^s$: and the zenith distance recorded in the MS entry (MSS, vol. 16) is 10° 22′ 30″, and not 10° 27′ 30″, as in the printed copy: and this is the value assumed by Flamsteed in his reduction of the star in MSS, vol. 26 B, page 4. The reading *per strias cochleæ* differs however 5′ from this value: and modern observations show that this is the most correct. I have therefore added 5′ to the above value; and the declination will now agree with modern observations.

2426. 62 *Ophiuchi* γ. This star is marked as of the 3rd magnitude, in the *British Catalogue*: but, in the *original* entries, it is always designated as the 4th; which I have therefore adopted.

2429. — *Ophiuchi*. Observed by Flamsteed on May 13, 1704, at $13^h\ 32^m\ 19^s$. It is the star observed by Lalande in *Hist. Cél.* page 86, at $17^h\ 40^m\ 14^s$, mag. 6½.

2433. — *Herculis*. Observed by Flamsteed on May 14, 1704, at $13^h\ 32^m\ 41^s$. It is the star observed by Lalande in *Hist. Cél.* page 353, at $17^h\ 42^m\ 9^s{,}3$, mag. 7.

2434. — *Serpentis*. Observed by Flamsteed on June 23, 1699, at $10^h\ 54^m\ 21^s$; again on June 22, 1702, at $11^h\ 1^m\ 4^s$; and again on the next day, at $10^h\ 57^m\ 17^s$. It is Piazzi XVII. 293.

2435. 4 *Sagittarii*. This star has the letter *b* annexed to it in the *British Catalogue*: but as that letter is also annexed to 59 *Sagittarii* (to which star, according to Bayer, it properly belongs) I have rejected it here.

2437. 5 *Sagittarii*. This star has the letter *i* annexed to it in the *British Catalogue*: but as there is no such letter in Bayer's map, I have here rejected it.

2441. 65 *Ophiuchi*. Observed by Flamsteed on May 6, 1691, at $14^h\ 10^m\ 58^s$; and regularly reduced by him in MSS, vol. 23, page 241. But no such star is now to be found. It is neither Piazzi XVII. 308, nor XVI. 251; as conjectured by that astronomer. Professor Airy has been kind enough to look for this star, at my request; but has not been able to discover it.

2444. — *Sagittarii*. Observed by Flamsteed on June 5, 1715, at $12^h\ 7^m\ 37^s$. It is Piazzi XVII. 312 = 709 Mayer.

2446. 7 *Sagittarii*. This star has the letter *a* annexed to it in the *British Catalogue*: but as that letter is also annexed to 60 *Sagittarii* (to which star, according to Bayer, it properly belongs) I have rejected it here.

2447. 57 *Serpentis* ζ. This star is marked as of the 3rd magnitude in the *British Catalogue*: but in the *original* entry it is designated as of the 5½; which I have here retained.

2448. 9 *Sagittarii*. In the reduction of the observation of this star, made on June 23, 1705, at $10^h\ 48^m\ 37^s$, in MSS, vol. 26 C, page 34, Flamsteed has made an error of 5° in declination. Whence has arisen the introduction of 8 *Sagittarii* into the *British Catalogue*; whose position, as there given, is $\mathcal{R}$ = 266° 13′ 30″, and D = — 19° 17′ 50″; but which consequently does not exist.

2449. 66 *Ophiuchi.* The declination of this star in Bessel's *Fundamenta Astronomiæ* should be + 4° 24′ 14″,2. The letter *n* is annexed to it in the *British Catalogue:* but as there is no such letter in Bayer's map, I have here rejected it.

2450. 67 *Ophiuchi.* This star is designated as *o* in the *British Catalogue:* but there is no such star in Bayer's map. I have therefore rejected it here.

2454. 93 *Herculis.* The right ascension of this star should be increased 4′ 6″,9 in order to correspond with Bradley's observations. The right ascension in Halley's edition of 1712 is 266° 34′ 30″; which is perhaps much nearer the truth than the present value. The declination is marked as doubtful in the *British Catalogue:* probably an error of 1′ has been made.

2455. 68 *Ophiuchi.* This star is designated by the letter *K*, in the *British Catalogue:* but as there is no such letter in Bayer's map, I have here rejected it.

2458. 31 *Draconis* ψ^1. The right ascension of this star must be diminished 5′ 6″,1 in order to correspond with Bradley's observations. Besides the 3 observations indicated by Miss Herschel, there was another (both of this and the next following star) made by Flamsteed on July 3, 1703, at $10^h\ 21^m\ 31^s$, which is not inserted in the printed copy of the *Historia Cœlestis*, but is to be found partially entered in the MS *copy* of that day's observations (MSS, vol. 16). The *original* book of entry is unfortunately lost: but the observations in question are fully copied out for reduction in MSS, vol. 26 A, page 19, and 26 B, page 4; and the results are given in 26 C, page 19. But in this last step of the process Flamsteed has made an error of 2^h: whence has arisen the introduction into the *British Catalogue* of 62 *Draconis;* whose position as there given is $\mathcal{R} = 296°\ 48'\ 0''$ and $D = +\ 72°\ 17'\ 50''$; but which consequently does not exist. This star is marked as of the 7th magnitude, in the *British Catalogue:* but in the *original* entries it is designated once as $4\frac{1}{2}$, once as $3\frac{1}{2}$, and once as the 5th: I have therefore taken the mean.

2459. — *Draconis.* Observed by Flamsteed on July 9, 1703, at $9^h\ 58^m\ 12^s$. It is Piazzi XVII. 287: and like the preceding star, its right ascension must be diminished 5′ 7″,7 in order to correspond with Bradley's observations.

2460. 95 *Herculis.* In the observation of this star on June 19, 1693, the first after $11^h\ 15^m\ 51^s$, there is an error in recording the zenith distance; which has led to the supposition that another and a different star was observed: viz. No. 103 of Miss Herschel's catalogue, and No. 459 of my list of Flamsteed's inedited stars. In the original MS entry (MSS, vol. 5) the reading *per strias cochleæ* is altered from 477·70 to 677·70; and in the margin we find 29° 54′ 15″ instead of 20° 54′ 15″. The whole is now accordant, and corresponds with this star: and the conjectures of M. Argelander are here fully confirmed.

2463. 97 *Herculis.* The right ascension of this star in the *British Catalogue* is 267° 14′ 30″. It was observed by Flamsteed on June 27, 1708, at $10^h\ 42^m\ 23^s$: and as it passed $1^m\ 14^s$ after 95 *Herculis*, its right ascension ought to be at least 267° 20′ 30″; which is the value I have stated in the present catalogue. But the whole of the right ascensions of the stars observed on that day appear to require correction.

2464. 33 *Draconis* γ. Besides the 4 observations of this star indicated by Miss Herschel, there is another on July 1, 1703, at $10^h\ 23^m\ 35^s$: which, on account of an error in recording the zenith distances, has given rise to the supposition that another and a different star was intended: viz.

No. 405 of Miss Herschel's catalogue, and No. 461 of my list of Flamsteed's inedited stars. The original MS entry is unfortunately lost: but in the MS transcript it is distinctly stated that this star is γ *Draconis*, that the star which precedes it is ξ, and that the star which follows it is *c*. All these stars were observed to the *north* of the zenith: and the zenith distances set down against ξ and *c* should be transposed; and the time of transit of the latter should be $11^h\ 11^m\ 0^s$, instead of $11^h\ 10^m\ 0^s$. It is agreeably to this alteration that the positions of these stars are deduced in MSS, vol. 26 A, page 18: where we find the following marginal note: "tempora transituum vitiosissime notata a T. Wolferman, Vide protocollum observationum." In the protocol book (MSS, vol. 16) however we find no remark of the kind. The following is the corrected reading of page 462 of the *Historia Cœlestis*; where it will be seen that an alteration of 10^s is made in the time of transit of the first two stars: viz.

h	m	s			°	′	″
10	22	35	Draconis	Bor. ξ	5	17	15
10	23	45	„	Bor. γ	0	6	20
11	11	0	„	Bor. *c*	3	36	20

These alterations fully confirm the shrewd and appropriate remarks of M. Argelander.

2465. 70 *Ophiuchi*. This star is designated by the letter *p*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2466. 11 *Sagittarii*. The position of this star in the *British Catalogue* is $AR = 266° 35' 15''$ and $D = -23° 8' 10''$. It was observed by Flamsteed on May 26, 1711, at $12^h\ 59'\ 8''$; and again on the next day, at $12^h\ 55^m\ 18^s$: both of which are reduced by him in MSS, vol. 26 C, page 34, and both of them erroneously. For in the first observation he has made an error of 1° in the right ascension: and in copying out the result for the catalogue has inadvertently assumed the declination belonging to No. 2473 of this catalogue: viz. $-23° 8' 10''$. These two errors being corrected, the star is Piazzi XVII. 366.

2467. 71 *Ophiuchi*. } These stars are designated by the letter S, in the *British Catalogue*: but,
2468. 72 *Ophiuchi*. } as there is no such letter in Bayer's map, I have rejected it in both cases. This letter appears to have been borrowed from Tycho's catalogue. The last of these two stars is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entries, it is once designated as the 5th, and twice as the 4th; which latter I have adopted here.

2470. 1 *Sagittarii*. The position of this star in the *British Catalogue* is $AR = 258° 13' 0''$ and $D = -25° 42' 50''$. It was observed by Flamsteed on July 15, 1692, at $9^h\ 31^m\ 17^s$; again on May 9, 1699, at $14^h\ 12^m\ 24^s$; again on May 26, 1711, at $13^h\ 1^m\ 37^s$; and again on the next day at $12^h\ 57^m\ 46^s$. But the observation of May 26th is the only one that has been reduced: see MSS, vol. 26 C, page 34. In the process of reduction, two errors have been committed: viz. 10° in right ascension, and 2° in declination. These errors being corrected, the star will correspond with Piazzi XVII. 386.

2471. — *Sagittarii*. Observed by Flamsteed on June 5, 1715, at $12^h\ 17^m\ 25^s$. It is the star observed by Lalande in *Hist. Cél.* page 172, at $17^h\ 59^m\ 2^s{,}5$, mag. 7. The reading *per lineas diagonales* differs 3′ from the reading *per strias cochleæ*; but the former is the correct one.

2472. 73 *Ophiuchi*. The declination of this star in the *British Catalogue* is $+ 3° 57' 45''$. It was observed by Flamsteed on June 30, 1690, at $10^h\ 34^m\ 1^s$; and reduced by him in MSS,

vol. 23, page 241. But in copying out the zenith distance he has made an error of 1′, which I have here corrected. This star is designated by the letter *q*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2473. 12 *Sagittarii.* The position of this star in the *British Catalogue* is $\mathcal{R} = 268° 35' 45''$, and $D = -24° 0' 10''$. It was observed by Flamsteed on May 27, 1711, at $12^h 59^m 15^s$; and reduced by him in MSS, vol. 26 C, page 34. But in the process of reduction he has committed two errors: viz. one by assuming the right ascension 1′ 30″ too great; and the other by assuming the declination 52′ too great. These errors being corrected, the star is the same as that observed by Lalande in *Hist. Cél.* page 173, at $18^h 0^m 30^s,5$; and again in page 566, at $18^h 0^m 27^s,5$, mag. 7½.

2475. 100 *Herculis.* This star is called *i* by Flamsteed: but that letter has already been applied by him to 43 *Herculis.* As this star, although in Bayer's map, has no letter there annexed to it, I have omitted it here.

2476. 101 *Herculis.* The right ascension of this star must be increased 4′, 42″,5 in order to correspond with modern observations. There is evidently some error in copying out the list of the stars in the constellation *Hercules* from MSS, vol. 24, page 104: as it appears that some of the values have been corrected, whilst this remains the same. It passed 2^s *before* 102 *Herculis;* and therefore it ought not to differ in right ascension more than 30″ from that star.

2481. 15 *Sagittarii.* This star is called μ^2 by Flamsteed: but there is only one star designated by that letter in Bayer's map, which is 13 *Sagittarii.* I have therefore rejected it here, on account of the difference in magnitude.

2485. 34 *Draconis* ψ^2. There is only one star designated by the letter ψ, in Bayer's map: but as it may be doubted whether such letter ought to be annexed to the present star, or to 31 *Draconis,* Flamsteed has affixed it to each: which I have here retained. In the *British Catalogue* it is marked as of the 4½ magnitude: but in the *original* entries it is so designated only once, and at another time as of the 6th: I have assumed the mean. Piazzi makes it of the 6th only.

2487. 35 *Draconis.* The declination of this star in the *British Catalogue* is + 77° 3′ 5″. It was observed by Flamsteed on July 9, 1703, at $10^h 14^m 32^s$, and reduced by him in MSS, vol. 26 B, page 4. The reading of the zenith distance in the MS entry (MSS, vol. 16) is 25° 23′ 50″ and not 25° 18′ 50″ as in the printed copy: and this is the value assumed by Flamsteed in his reduction of the star in the MS volume above mentioned. The reading *per strias cochleæ* differs however 5′ from this value: and modern observations show that this is the most correct. I have therefore deducted that quantity from the above value, and the star will then be Piazzi XVII. 380, and not 370 as assumed by him. With respect to the right ascension, I would remark that it has been deduced by Flamsteed in the MS volume above mentioned, from that of 28 *Draconis:* but in MSS, vol. 26 C, page 18, another method appears to have been pursued; the clue to which I cannot discover. The star was observed by Bradley: and it appears that the right ascension must be increased 10′ 45″,9 in order to correspond with his observations.

2489. 18 *Sagittarii.* Observed by Flamsteed on June 3, 1691, at $12^h 41^m 5^s$: where the time is marked as doubtful. It is not reduced by him in MSS, vol. 23, but Mr. Hodgson has reduced

it in MSS, vol. 62 C, page 59; and makes its right ascension to be 59′ *more* than 19 *Sagittarii*. The star is inserted in Halley's edition of 1712; where the right ascension is stated to be 271° 15′ 0″. I have therefore ventured to make the correction of 1°. It is Piazzi XVIII. 52; and not 33, as he has erroneously supposed.

2491. 74 *Ophiuchi*. This star is designated by the letter *r*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2492. 105 *Herculis*. The right ascension of this star in the *British Catalogue* is 271° 19′ 10″. It was observed by Flamsteed on July 1, 1702, at $10^h\ 53^m\ 40^s$; and reduced by him in MSS, vol. 23, page 340: but in deducing the right ascension he has made a mistake of 1^m. I have therefore added 15^s to the above value; and it will now more nearly accord with modern observations.

2497. 108 *Herculis*. The declination of this star in the *British Catalogue* is + 29° 45′ 40″. It was observed by Flamsteed on July 4, 1690, at $10^h\ 32^m\ 14^s$; and was reduced by him in MSS, vol. 23, page 341. But in the steps of the process it will be seen that he has made an error of 1′, which I have here corrected.

2498. 1 *Lyræ* $\varkappa$. This star has a letter annexed to it in the *British Catalogue*, which may be taken either for $\varkappa$, χ, or x: but, as it should be the former of these letters, I have here retained it.

2501. 23 *Sagittarii*. The position of this star in the *British Catalogue* is $\mathcal{R}$ = 272° 55′ 30″, and D = − 24° 4′ 0″. It was observed by Flamsteed on May 26, 1711, at $13^h\ 20^m\ 23^s$; and again on the next day, at $13^h\ 16^m\ 33^s$. The first observation was reduced by him in MSS, vol. 26 C, page 34, and in the process of reduction two errors have been committed: viz. 1′ in the right ascension, and 40′ in the declination. I cannot find the star in any catalogue: but Prof. Airy, at my request, has been kind enough to look out for it, and finds not only that the star exists, but that it agrees very well with Flamsteed's position.

2505. 24 *Sagittarii*. The right ascension of this star in the *British Catalogue* is 273° 34′ 45″. It was observed by Flamsteed on May 26, 1711, at $13^h\ 23^m\ 40^s$; and again on the next day, at $13^h\ 19^m\ 50^s$: the former of which has been reduced by him in MSS, vol. 26 C, page 34. Both observations agree very well together, and show that it followed $7^m\ 57^s$ after 21 *Sagittarii*. Its right ascension therefore ought to be 273° 43′ 45″: and I apprehend that it is a typographical error arising from a transposition of the figures indicating the minutes. It is Piazzi XVIII. 105; and not 99 as supposed by him. It was observed by Bradley.

2509. 1 *Aquilæ*. This star is designated by the letter *m*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2510. 38 *Draconis*. The declination of this star in the *British Catalogue* is + 68° 32′ 10″. It was observed by Flamsteed on July 3, 1703, at $10^h\ 52^m\ 14^s$; and the declination is deduced by him from the reading *per lineas diagonales*, which differs 5′ from the reading *per strias cochleæ*. But modern observations show that the latter is the more correct; and I have therefore added 5′ to the above value.

2512. — *Sagittarii*. Observed by Flamsteed on July 1, 1699, at $11^h\ 2^m\ 11^s$. It is Piazzi XVIII. 129 = 740 Mayer.

2513. 40 *Draconis.* The right ascension of this star is deduced by Flamsteed from that of 28 *Draconis;* and must be increased 6′ 16″,2 in order to correspond with Bradley's observations.

2514. 41 *Draconis.* The declination of this star in the *British Catalogue* is + 79° 57′ 10″. It was observed by Flamsteed on July 9, 1703, at $10^h\ 34^m\ 55^s$: and by comparing its zenith distance with that of the stars observed on the same day, it will be seen that an error of 3′ has been made; which I have here corrected. It was observed by Bradley: and the right ascension here given must be increased 6′ 0″,8 in order to agree with his observations.

2516. 42 *Draconis.* The right ascension of this star must be increased 6′ 26″,1 in order to correspond with Bradley's observations. In MSS, vol. 26 B, page 2, it is deduced from that of *β Draconis*, observed on the same day: but in vol. 26 C, page 18, another mode is adopted; the clue to which I cannot discover.

2518. 2 *Aquilæ.* This star is designated by the letter *o* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have rejected it here.

2520. 44 *Draconis χ.* The right ascension of this star must be increased 5′ 47″,6 in order to correspond with Bradley's observations. See the note to No. 2516 above; which applies in like manner to this star.

2522. 3 *Aquilæ.* This star is designated by the letter *n* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2523. 45 *Draconis d.* The right ascension of this star in the *British Catalogue* is 277° 7′ 30″; which is stated in MSS, vol. 26 C, page 18, to have been obtained *per distantias.* But there are two good observations of it with the mural arc, which have been reduced by Flamsteed in MSS, vol. 26 B, pages 1 and 2, (in the latter of which there is an error of 1^m,) and which make the right ascension 276° 51′ 30″, as stated in Halley's edition of 1712. I have therefore restored the former value; which will now more nearly correspond with modern observations. The computation of the observation *per distantias* is in MSS, vol. 26 A, page 14; and the result is there given 277° 10′ 54″.

2527. 6 *Aquilæ.* This star is designated by the letter *l* in the *British Catalogue:* but as there is no such letter in Bayer's map, I have here rejected it. The magnitude (which should be 3½ according to the MS) differs considerably from Piazzi's value, which is 5½.

2528. 29 *Sagittarii.* Besides the 6 observations of this star, indicated by Miss Herschel, there is another on June 5, 1690, at $12^h\ 51^m\ 41^s$. On referring to the original MS entry (MSS, vol. 4) I find that the time of transit should be $12^h\ 55^m\ 41^s$. This error of the press has given rise to the supposition that another and a different star was intended: viz. No. 14 of Miss Herschel's catalogue, and No. 469 of my list of Flamsteed's inedited stars; but which consequently does not exist. M. Argelander's remarks on this star are here also confirmed.

2544. — *Sagittarii.* Observed by Flamsteed on June 5, 1690, at $13^h\ 1^m\ 37^s$; and again on July 21, 1693, at $9^h\ 52^m\ 39^s$: both of which are regularly and correctly reduced by him in MSS, vol. 23, pages 125 and 127; and inserted in his list of stars in the constellation *Sagittarius* in page 134 of the same volume. I know not why it was excluded from the *British Catalogue.* It is Piazzi XVIII. 225.

2546. 9 *Lyræ* ν^2. There is but one star designated by the letter *ν*, in Bayer's map: but as it may

be doubted whether such letter belongs to this or the preceding star, Flamsteed has annexed it to each, which I have here retained.

2548. 36 *Sagittarii* ξ^1 } There is only one star designated by the letter ξ, in Bayer's map: but, as
2550. 37 *Sagittarii* ξ^2 } it may be doubted whether such letter belongs to 36 or 37 *Sagittarii*, Flamsteed has annexed it to each, which I have here retained. In the *British Catalogue*, 37 *Sagittarii* is marked as of the 6th magnitude (that is, *less* than 36 *Sagittarii*) but in the *original* entries it is stated to be *greater*, and of the 4th; which value I have here adopted.

2551. 62 *Serpentis*. The declination of this star in the *British Catalogue* is $+6^\circ\ 14'\ 40''$. It was observed by Flamsteed on June 30, 1690, at $11^h\ 20^m\ 3^s$; and reduced by him in MSS, vol. 23, page 233. But in the steps of the process he has made an error of $1'$, which I have here corrected.

2552. 9 *Aquilæ*. This star is designated by the letter *k* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2553. 63 *Serpentis* θ. I have no doubt but that this is the θ of Bayer, although the position of the star does not exactly accord with that in his map.

2561. 12 *Aquilæ*. This star is designated by the letter *i* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it. It is also marked as of the 4th magnitude: but in the *original* entry it is designated as the 5th; which I have here retained.

2563. 13 *Lyræ*. This star has the letter π annexed to it in the *British Catalogue*: but as there is no such letter in Bayer's map, I have here rejected it.

2564. — *Sagittarii*. Observed by Flamsteed on June 29, 1700, at $11^h\ 17^m\ 26^s$; and again on the next day, at $11^h\ 13^m\ 7^s$. It is the star observed by Lalande in *Hist. Cél.* page 173, at $18^h\ 52^m\ 0^s$, mag. 8.

2573. — *Sagittarii*. Observed by Flamsteed on August 12, 1692, at $8^h\ 44^m\ 0^s$. It is supposed by M. Argelander to be the star observed by Lalande in *Hist. Cél.* page 171, at $18^h\ 56^m\ 39^s{,}5$, mag. 7. This will certainly agree with the time of transit: but in the original MS entry (MSS, vol. 5) it is called *comes* π; and the star here alluded to differs upwards of $2^\circ\ 4'$ from π. I rather think that it is the star in *Hist. Cél.* page 173, at $18^h\ 56^m\ 18^s$, mag. 6: and that Flamsteed has recorded the time of transit erroneously.

2577. — *Draconis*. Observed by Flamsteed on July 31, 1692, at $9^h\ 29^m\ 33^s$, and regularly reduced by him in MSS, vol. 26 B, page 2. It is also inserted in Halley's edition of 1712; but with an erroneous position. It should be remarked that the reading *per lineas diagonales* differs $8'$ from that *per strias cochleæ*: Flamsteed has assumed the former reading, but no star can be found corresponding thereto. I have assumed however that the latter is the correct reading; and the star will then agree *nearly* with No. 216 *Draconis* in Bode's great catalogue: which has been observed by Lalande in *Hist. Cél.* page 359, at $18^h\ 49^m\ 53^s$, mag. 6; and three times in the *Mém. de l'Acad.* for 1789. I would remark that, in the original entry, the zenith distance is marked as doubtful. If therefore we read $22^\circ\ 10'\ 15''$, as suggested by M. Argelander, the whole of the discordance will vanish: since the declination would then be $+\ 73^\circ\ 42'\ 5''$.

2579. 16 *Lyræ*. The declination of this star in the *British Catalogue* is $+\ 37^\circ\ 31'\ 15''$. It was observed by Flamsteed on July 4, 1690, at $11^h\ 15^m\ 40^s$; and regularly reduced by him in

MSS, vol. 23, page 347. But in copying it out in the list of stars in the constellation *Lyra*, in page 416 of the same volume, he has made an error of 9°, which I have here corrected. The declination is correctly given in Halley's edition of 1712, under the constellation *Cygnus*, This star has the letter ρ annexed to it in the *British Catalogue*: but there is no such letter in Bayer's map. I have therefore omitted it.

2580. — *Lyræ.* Observed by Flamsteed on July 26, 1690, at $9^h\ 49^m\ 28^s$; and regularly reduced by him in MSS, vol. 23, page 347, but not inserted in his list of the stars in the constellation *Lyra*, in page 416 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 20, at $18^h\ 56^m\ 53^s{,}5$, mag. 6.

2583. 50 *Draconis.* The right ascension of this star must be increased $18'\ 35''{,}7$ in order to correspond with Bradley's observations. The two observations by Flamsteed are regularly reduced in MSS, vol. 26 B, pages 2 and 4; but they are very discordant: and again, in another manner (the clue to which I cannot discover), in vol. 26 C, page 18. I cannot find how the present right ascension has been deduced.

2584. 17 *Lyræ.* The declination of this star in the *British Catalogue* is $+\ 32°\ 3'\ 35''$. It was observed by Flamsteed on July 26, 1690, at $9^h\ 52^m\ 0^s$; and regularly reduced by him in MSS, vol. 23, page 347. But the reading *per lineas diagonales* differs $1'$ from that *per strias cochleæ*. Flamsteed has assumed the former as the correct reading: but modern observations show that the latter is the true one. I have therefore subtracted $1'$ from the above value.

2588. 51 *Draconis.* The right ascension of this star in the *British Catalogue* is $284°\ 20'\ 30''$: but I suspect that this is an error of the press, and that the minutes should be 29. It was observed by Flamsteed on July 29, 1692, at $9^h\ 41^m\ 55^s$; and though the time is marked as doubtful, I apprehend it is correct. But Flamsteed has made an error of 50^s in copying it out for computation in MSS, vol. 26 B, page 1: and in vol. 26 C, page 18, he has given another and a different value, which he says was obtained *per distantias;* and which is nearly the same as that inserted in Halley's edition of 1712. The value which I have adopted is that which is shown by a comparison of its time of transit, with that of 53 *Draconis* on the same day; and which agrees with modern observations.

2591. — *Vulpeculæ.* Observed by Flamsteed on Aug. 13, 1702, at $8^h\ 54^m\ 7^s$; and again on Aug. 23, at $8^h\ 16^m\ 52^s$: both of which observations are regularly reduced by him in MSS, vol. 23, page 354. It is the star observed by Lalande in *Hist. Cél.* page 105, at $19^h\ 3^m\ 38^s$, mag. $6\frac{1}{2}$.

2593. 19 *Lyræ.* The declination of this star in the *British Catalogue* is $+\ 38°\ 47'\ 35''$. It was observed by Flamsteed on July 26, 1690, at $9^h\ 56^m\ 12^s$; and regularly reduced by him in MSS, vol. 23, page 347: but, in copying it out for the press, an error of 8° has been made, which I have here corrected. It is accurately printed in Halley's edition of 1712: and has been observed by Bradley. This error has given rise to the supposition that another and a different star was intended: viz. No. 18 of Miss Herschel's catalogue, and No. 476 of my list of Flamsteed's inedited stars; which consequently does not exist.

2596. 23 *Aquilæ.* The declination of this star in the *British Catalogue* is $+\ 0°\ 32'\ 30''$. It has been regularly reduced by Flamsteed in MSS, vol. 23, page 247: but in copying out the result, in his list of stars in the constellation *Aquila*, in page 312 of the same volume, he has made an error of $1'$, which I have here corrected. In the *British Catalogue* this star is marked as

of the 7th magnitude: but, in the *original* entries, it is designated as the 5th; which I have here retained.

2602. 45 *Sagittarii* ρ^2. There is only one star designated as ρ, in Bayer's map: but, as it may be doubtful whether such letter ought to belong to 44 *Sagittarii*, or the present star (or indeed whether their joint appearance might not have been considered as one star), Flamsteed has annexed the letter to each; which I have therefore retained.

2606. 2 *Vulpeculæ*. Observed by Flamsteed on Aug. 13, 1702, after $8^h\ 57^m\ 41^s$; and again on Aug. 23, at $8^h\ 22^m\ 10^s$: both of which are reduced by him in MSS, vol. 23, page 354; and there can be no doubt but that this is the star intended to have been inserted as 2 *Vulpeculæ* in the *British Catalogue;* but which is left blank, the longitude and latitude only being given as deduced from Hevelius's catalogue. It is the star observed by Lalande in *Hist. Cél.* page 28, at $19^h\ 8^m\ 51^s{,}5$, mag. 5.

2608. 29 *Aquilæ*. Observed by Flamsteed on June 30, 1690, after $11^h\ 44^m\ 46^s$. The time of transit is not stated; but he remarks that it passed almost at the same moment as 28 *Aquilæ*. It was observed by Bradley. The right ascension is not given in the *British Catalogue:* but I have assumed an approximate value, corresponding with modern observations. This star is called ω^2 in the *British Catalogue:* but there is only one star designated by that letter in Bayer's map; which is 25 *Aquilæ*. I have therefore rejected it here, on account of the difference of magnitude.

2611. 47 *Sagittarii* χ^1.
2612. 48 *Sagittarii* χ^2.
2613. 49 *Sagittarii* χ^3.

There is only one star designated by the letter χ, in Bayer's map: but, as it may be doubted whether the impression of such star might not have been caused by the united light of the three stars here quoted, Flamsteed has annexed such letter to each of them; which I have here retained. The first of these stars is marked as of the 5th magnitude, in the *British Catalogue:* but, in the *original* entries, it is designated as the 6th. The second of them is also marked of the 5th; but in the *original* entry it is called *telescopica*, and designated as the 8th. I have therefore made the requisite alterations.

2622. — *Sagittarii*. Observed by Flamsteed on June 28, 1702, at $12^h\ 8^m\ 27^s$. It is Piazzi XIX. 126.

2625. 23 *Ursæ Minoris* δ. This star was observed only once by Flamsteed, viz. on June 3, 1691, at $13^h\ 45^m\ 35^s$; and then at the special request of Sir Christopher Wren, as appears by a note made in the margin of the book containing the original entry (MSS, vol. 5). The right ascension assumed by him, which has been deduced *per distantias*, as may be seen in MSS, vol. 26 A, page 23, must be diminished $12'\ 15''{,}0$ in order to correspond with Bradley's observations. But no great accuracy can be expected in stars thus computed, and so near the pole. In Halley's edition of 1712, the right ascension is $287^\circ\ 35'\ 0''$.

2626. 2 *Cygni*. Besides the three observations of this star indicated by Miss Herschel, there is another made by Flamsteed on Sept. 5, 1690, after $7^h\ 40^m$. The time of transit is not given. but Flamsteed has reduced the zenith distance in MSS, vol. 23, page 351; and in so doing has made an error of $6'$ in copying out the observation. In the list of stars in the constellation *Cygnus*, in page 418 of the same volume, he has inserted the result of this computation *after* 4 *Cygni;* which has given rise to the supposition that another and a different star was intended:

viz. 5 *Cygni*, whose right ascension in the *British Catalogue* is undetermined, and whose declination is there stated to be + 29° 8′ 5″: but which does not exist. Both Bradley and Piazzi have given the name of 5 *Cygni* to a star that will not agree with this observation.

2627. 4 *Vulpeculæ*. The declination of this star in the *British Catalogue* is + 19° 12′ 15″: which was deduced by Flamsteed in MSS, vol. 23, page 253, from the observation made on Aug. 20, 1690, at $8^h\ 35^m\ 24^s$. But it will be seen that the reading *per lineas diagonales* differs 1′ from that *per strias cochleæ*: and that the latter is the correct reading, may be ascertained from a comparison of the observation of the same star made on Aug. 23, at $8^h\ 23^m\ 53^s$. I have therefore added 1′ to the above value.

2629. 3 *Cygni*. The declination of this star in the *British Catalogue* is + 24° 36′ 55″. It was observed by Flamsteed on Sept. 20, 1690, at $6^h\ 46^m\ 26^s$; which was reduced by him in MSS, vol. 23, page 351. But in copying out the corrected zenith distance, he has made an error of 5′; and, in transcribing, another error of 9′: both of which I have here rectified: and it will now agree with modern observations.

2630. 5 *Vulpeculæ*. In the observation of this star on Aug. 20, 1690, at $8^h\ 36^m\ 14^s$, Flamsteed remarks that it has a cluster of stars about it. This is Herschel VIII. 21.

2635. 7 *Vulpeculæ*. This star is marked as of the 5th magnitude, in the *British Catalogue*, but in the *original* entries, it is always designated as the 6th; which I have therefore adopted.

2636. 8 *Vulpeculæ*. The declination of this star in the *British Catalogue* is + 24° 39′ 25″. There are 3 observations of it by Flamsteed; all of which are correctly reduced by him in MSS, vol. 23, pages 351, 353, and 355. But in copying out the results in page 418 of the same volume, he has made an error of 30′ which I have here rectified. The value is correctly given in Halley's edition of 1712; and I know not why it was afterwards altered.

2637. — *Vulpeculæ*. Observed by Flamsteed on Aug. 13, 1702, at $9^h\ 8^m\ 39^s$; and again on Aug. 23, at $8^h\ 30^m\ 23^s$. It is Piazzi XIX. 128.

2639. 52 *Sagittarii* h^2. There is only one star designated by the letter *h*, in Bayer's map: but, as it may be doubted whether such letter belongs to 51 *Sagittarii*, or the present star, Flamsteed has annexed it to each, which I have here retained. In the *British Catalogue* it is marked as of the 6th magnitude: but in the *original* entry it is designated as the 5th; which I have therefore adopted.

2642. — *Cygni*. Observed by Flamsteed on June 15, 1691, at $13^h\ 9^m\ 49^s$; and again on Aug. 13, 1702, at $9^h\ 13^m\ 6^s$. It is Piazzi XIX. 162; and was also observed by Bradley.

2643. — *Sagittarii*. Observed by Flamsteed on Sept. 9, 1700, at $7^h\ 26^m\ 48^s$. It is Piazzi XIX. 176 = 793 Mayer.

2645. — *Sagittarii*. Observed by Flamsteed on Sept. 9, 1700, at $7^h\ 27^m\ 30^s$. It is Piazzi XIX. 180 = 794 Mayer.

2647. 7 *Cygni* ι^1. This star has no letter annexed to it in the *British Catalogue*: but since, in conjunction with 10 *Cygni*, it may be assumed as the star to which Bayer annexed the letter ι, in his map, I have designated this as ι^1.

2648. 59 *Draconis*. One of the observations of this star, viz. that made on July 9, 1703, has been erroneously reduced by Flamsteed in MSS, vol. 26 A, page 19, and 26 B, page 4; by his

having assumed the time of transit as $11^h\ 23^m\ 10^s$, instead of $11^h\ 31^m\ 3^s$ as in the printed copy. Whence has arisen the introduction of 56 *Draconis* into the *British Catalogue;* whose position there given is $\mathcal{R} = 287°\ 52'\ 30''$ and $D = +\ 76°\ 1'\ 5''$; but which does not exist. Unfortunately the original MS. entry is not in existence: but in the transcript (MSS, vol. 16) it is exactly as in the printed copy. So that I presume that the error was afterwards discovered, and found to be the same as the present one. Consequently Piazzi XIX. 38 is *not* 56 *Draconis*, as erroneously supposed by that astronomer.

2649. 8 *Cygni*. There are two observations of this star; both of which Flamsteed has reduced in MSS, vol. 23, pages 351 and 355. But he has assumed the declination, deduced from the former, as the most correct, in his list of stars in the constellation *Cygnus*, in page 418 of the same volume. This differs however 55″ from the declination, deduced from the latter: which, according to modern observations, is the more correct of the two.

2651. 41 *Aquilæ ι*. Besides the 4 observations of this star indicated by Miss Herschel, there is another made by Flamsteed on Aug. 9, 1713, at $9^h\ 24^m\ 53^s$; and which she has considered to be another and a different star: viz. No. 358 of her catalogue, and No. 484 of my list of Flamsteed's inedited stars. The present star is marked as of the $3\frac{1}{2}$ magnitude in the *British Catalogue:* but in the *original* entry it is designated as the 4th only; which I have here retained.

2656. — *Sagittarii*. Observed by Flamsteed on June 30, 1691, at $12^h\ 2^m\ 52^s$: and regularly reduced by him in MSS, vol. 23, page 125; but omitted to be inserted in the list of stars in the constellation *Sagittarius*, in page 134 of the same volume. It is Piazzi XIX. 201 = 796 Mayer: and was observed by Bradley.

2657. 10 *Cygni ι*^{2}. This is the star which Flamsteed considers to be the *ι* in Bayer's map: but, see the note to No. 2647 above.

2658. 9 *Cygni*. The right ascension of this star is not given in the *British Catalogue:* but in MSS, vol. 23, page 418, it is approximately given as here stated. The magnitude likewise is omitted; which I have here supplied from the recorded observation.

2659. 24 *Ursæ Minoris*. The right ascension of this star in the *British Catalogue* is 288° 9′ 0″. It was observed by Flamsteed on June 3, 1691, at $13^h\ 57^m\ 20^s$; and reduced by him in MSS, vol. 26 A, page 25. But in deducing the right ascension he has made a mistake of 10^m. I have therefore added 2° 30′ to the above value. The right ascension in Halley's edition of 1712 is 290° 31′ 15″: so that the error must have been detected prior to the publication of that work. The right ascension however as it now stands, must be diminished 13′ 30″,3 in order to correspond with Bradley's observations: but this arises from the error of the determining star, viz. *δ Ursæ Minoris*.

2664. — *Cygni*. Observed by Flamsteed on Aug. 15, 1691, after $9^h\ 9^m\ 56^s$. It is probably the star observed by Lalande in *Hist. Cél.* page 237, at $19^h\ 27^m\ 22^s$,4, mag. 6.

2666. 55 *Sagittarii e*^{2}. There is only one star designated by the letter *e*, in Bayer's map: but as it may be doubted whether such letter belongs to 54 *Sagittarii*, or the present star, Flamsteed has annexed it to each, which I have therefore retained.

2667. — *Sagittarii*. Observed by Flamsteed on Aug. 9, 1713, at $9^h\ 30^m\ 0^s$. It is Piazzi XIX. 230 = 799 Mayer.

2674. 56 *Sagittarii f.* In the observation of this star on July 26, 1690, at $10^h\ 24^m\ 55^s$, Flamsteed remarks that it has a small companion following it to the south: which is probably the star observed by Lalande in *Hist. Cél.* page 173, at $19^h\ 35^m\ 3^s$, mag. 9.

2680. 61 *Draconis* σ. There appears to be an annual proper motion of this star, in declination, of $-1'',820$; which will reduce the difference in declination to $+\ 21'',5$.

2682. 16 *Cygni c.* See the note to No. 2734.

2687. 17 *Cygni* χ. M. Bessel does not think that this is the star, designated by the letter χ in Bayer's map. But there is no other star of that magnitude observed by Flamsteed or Piazzi that comes nearer to the position there indicated. I apprehend that the error is in Bayer, as there is no star in the catalogue either of Ptolemy or Tycho that will accord with the position he has given.

2688. 11 *Vulpeculæ.* This star is inserted in the *British Catalogue*, but without either right ascension or declination annexed: the longitude and latitude are however given, as if deduced from Hevelius. I have not been able to find any observation of it by Flamsteed, nor can I discover that such a star exists: the observation in Lalande's *Hist. Cél.* page 25, evidently does not belong to this star. In fact, it appears from the following account of it, given by Hevelius, that it no longer exists:—"Pariter illa nova sub capite *Cygni*, seu potius in aure "sinistra *Vulpeculæ*, uti in catalogo meo denominatur, cum primum anno 1670, mense Julio, "sub adspectum prodiret, tertiæ magnitudinis extitit: brevi autem post adeo diminuta ut, anno "1671, die 23 Augusti, penitus sit exstincta. Sed proximo anno 1672, die 8 Martii, rursus "affulsit, initio instar minutissimæ alicujus stellulæ, deinde vero ad tertiam magnitudinem "crevit, ac denuo sensim imminuta, sic ut altera vice anno 1672, die 25 Septembris penitus "evanuerit, ut ab eo tempore nunquam a quopiam sit conspecta: sicut ex Mach. mea Celesti, "ac epistola ad Oldenburgium elucet." Hevelius *Prodromus Astronomiæ*, page 122. Under the presumption, however, that it may be a *variable*, and not a *lost* star, I have preserved its recorded position, with a view of inducing astronomers to look out for it, from time to time.

2695. — *Aquilæ.* Observed by Flamsteed on July 25, 1690, after $10^h\ 37^m\ 16^s$. It is perhaps the star observed by Lalande in *Hist. Cél.* page 109, at $19^h\ 42^m\ 21^s,5$, mag. 5.

2700. 9 *Sagittæ.* This star is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entry it is designated as $6\frac{1}{2}$; which I have therefore adopted.

2703. 19 *Cygni.* In the observation of this star on Aug. 15, 1691, at $9^h\ 24^m\ 46^s$, Flamsteed remarks that it has a companion preceding it to the south: which is probably the star observed by Lalande in *Hist. Cél.* page 14, at $19^h\ 41^m\ 53^s$, mag. 6. If so, it was also observed by Bradley: and is the star (XIX. 304) which Piazzi has erroneously called 19 *Cygni.*

2704. 60 *Sagittarii A.* This star has the small letter *a* affixed to it in the *British Catalogue*: but Bayer always commenced the Roman alphabet, with a large, or capital, letter.

2706. 13 *Vulpeculæ.* This star is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entry it is designated as $4\frac{1}{2}$; which I have here retained.

2708. — *Vulpeculæ.* Observed by Flamsteed on Aug. 25, 1690, at $8^h\ 45^m\ 36^s$; and regularly reduced by him in MSS, vol. 23, page 349; but omitted to be inserted in his list of stars in the

constellation *Cygnus* in page 418 of the same volume. It is Piazzi XIX. 327; and was also observed by Bradley.

2711. 20 *Cygni d.* This star is not designated by any letter in the *British Catalogue:* but as it is *d* in Bayer's map, I have here restored that letter.

2719. 13 *Sagittæ.* The letter χ is annexed to this star in the *British Catalogue;* but as there is no such letter in Bayer's map, I have here rejected it.

2721. 24 *Cygni* ψ. The right ascension of this star is not given in the *British Catalogue.* It was observed by Flamsteed on Sept. 25, 1690, at $7^h\ 2\frac{1}{2}^m$ *circiter.* The right ascension is given in Halley's edition of 1712, from which the right ascension in the present catalogue is taken: and it agrees very well with modern observations.

2724. 15 *Vulpeculæ.* The right ascension of this star in the *British Catalogue* is 297° 24′ 0″. It was observed by Flamsteed on Aug. 25, 1690, at $8^h\ 52^m\ 31^s$; and again on Sept. 1, at $8^h\ 30^m\ 1^s{,}5$; and regularly reduced by him in MSS, vol. 23, page 349. It is also copied out correctly in his list of stars in the constellation *Cygnus* in page 418 of the same volume; viz. $\mathcal{R} = 297°\ 4'\ 0''$; and this likewise is the value inserted in Halley's edition of 1712. Why it was afterwards altered I have not been able to ascertain: but I have here restored the original and correct value.

2730. 14 *Sagittæ.* The letter *y* is annexed to this star in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2731. 15 *Sagittæ.* The letter *z* is annexed to this star in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2733. 64 *Aquilæ.* The declination of this star in the *British Catalogue* is — 1° 30′ 20″. It was observed by Flamsteed on June 5, 1690, at $14^h\ 16^m\ 4^s$; again on Sept. 16, 1690, at $7^h\ 40^m\ 40^s$; and again on June 26, 1691, at $12^h\ 49^m\ 10^s$; but the first and last only have been reduced by Flamsteed in MSS, vol. 23, pages 247 and 251, where the results are correctly given. But in copying them out in the list of stars in the constellation *Aquila,* in page 316 of the same volume, he has made an error of 2′; which I have here corrected.

2734. 26 *Cygni e.* This star is called c^3 in the *British Catalogue:* but it is *e* in Bayer's map, and I have therefore restored the correct reading. This is no second star denoted by *c* in those maps, and consequently 16 *Cygni* should not be designated as c^1, but merely as *c.*

2736. 1 *Capricorni* ξ^1. There is only one star designated by the letter ξ in Bayer's map: but as it may be doubted whether such letter belongs to 2 *Capricorni,* or to the present star, I have annexed it to both.

2747. 65 *Draconis.* This star is marked as of the $5\frac{1}{2}$ magnitude, in the *British Catalogue:* but in the recorded observations it is designated as the 7th; which I have here retained. There is only one star denoted by the letter *e* in Bayer's map, which is 64 *Draconis:* I have therefore rejected the letter here, on account of the difference of magnitude.

2756. 7 *Capricorni* σ. There is no magnitude affixed to this star, in the *British Catalogue:* but in one of the original entries, it is called the 6th; which I have here adopted.

2758. — *Vulpeculæ.* Observed by Flamsteed on Aug. 25, 1690, after $9^h\ 6^m\ 19^s$. It is the star observed by Lalande in *Hist. Cél.* page 26, at $20^h\ 6^m\ 22^s{,}5$, mag. 5.

2762. — *Capricorni.* Observed by Flamsteed on Aug. 6, 1690, at $10^h\ 16^m\ 23^s$; and again on Sept. 8, 1714, at $8^h\ 21^m\ 1^s$. It is Piazzi XX. 79; and was also observed by Bradley.

2765. 30 *Cygni* o^1. This star is marked as of the 4th magnitude, in the *British Catalogue:* but in the *original* entries, it is once so called, and once of the 5th only; I have therefore taken the mean. In one of the observations it is stated to be a little brighter than 31 *Cygni.*

2769. 33 *Cygni.* Observed by Flamsteed on Sept. 25, 1690, after $7^h\ 20^m\ 52^s,5$. In the original MS entry it has the mark *B* against it; which denotes that it was observed to the *north* of the zenith: but this is omittted in the printed copy. In consequence of this omission it has generally been supposed to belong to another and a different star: viz. No. 29 of Miss Herschel's catalogue, and No. 494 of my list of Flamsteed's inedited stars; but which consequently does not exist. It is correctly deduced in MSS, vol. 23, page 353. There is no right ascension set against this star in the *British Catalogue:* but I have annexed an approximate value, agreeing with modern observations. Neither is there any magnitude annexed; which I have however supplied from the *original* entry.

2770. 34 *Cygni.* This is supposed to be the star which shone out with so much lustre in the year 1600; and is called, in the old catalogues, *Stella nova.*

2771. 68 *Draconis.* In the observation of this star by Flamsteed on July 29, 1692, at $10^h\ 50^m\ 19^s$, he states that it has a companion 15′ distant. Probably the star observed by Lalande in *Mém. de l'Acad.* for 1789, page 213, at $20^h\ 5^m\ 48^s,3$, mag. 7.

2772. 35 *Cygni.* This star is designated by the letter *m* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2773. 36 *Cygni.* The declination of this star in the *British Catalogue* is + 36° 8′ 10″. It was observed by Flamsteed on Sept. 8, 1690, at $8^h\ 25^m\ 17^s$: and the declination is deduced, in MSS, vol. 23, page 351, from the reading *per lineas diagonales*, which differs 5′ from that *per strias cochleæ.* But it appears, from modern observations, that the latter is the correct reading, and I have therefore deducted 5′ from the above value.

2774. 69 *Draconis.* The declination of this star in the *British Catalogue* is + 75° 30′ 50″. It was observed by Flamsteed on July 9, 1703, at $12^h\ 18^m\ 30^s$, and reduced by him in MSS, vol. 26 A, page 19, and 26 B, page 4: but the results are discordant. In the MS entry (MSS, vol. 16) the zenith distance is 23° 53′ 45″: but the value printed in the *Historia Cœlestis* appears to be the correct reading: and by comparing it with that of 1 *Cephei*, which immediately follows on the same day, it will seem that an error of 5′ has been committed; which I have here corrected. I would further remark that the right ascension must be increased $9^s\ 56^s,5$, in order to correspond with Bradley's observations. This arises from the circumstance that the right ascension has been determined by Flamsteed in MSS, vol. 26 A, page 19, from comparison with that of 1 *Cephei*; which itself requires to be increased. The reduction in MSS, 26 B, page 4, comes more nearly to the correct value.

2775. — *Aquilæ.* Observed by Flamsteed on Aug. 25, 1690, after $9^h\ 12^m\ 54^s$. The time of transit is not given: but the star is probably that observed by Lalande in *Hist. Cél.* page 190, at $20^h\ 12^m\ 39^s,4$, mag. 6½.

2777. 10 *Capricorni* π. There is no magnitude annexed to this star in the *British Catalogue:* nor can I find any record of it in the observations. Piazzi makes it to be of the 5th.

2778. 37 *Cygni* γ. Besides the 9 observations of this star indicated by Miss Herschel, there is another made by Flamsteed on July 1, 1703, at $12^h\ 44^m\ 58^s$, which has commonly been supposed to belong to 70 *Draconis*, whose position in the *British Catalogue* is $\mathcal{R} = 302°\ 38'\ 30''$, and $D = +\ 64°\ 0'\ 30''$; but which does not exist. The original MS entry of this observation is not in existence; but in the MS copy (MSS, vol. 16) it appears that, at the close of that day's observations, Flamsteed had turned his telescope to the *south*: for, the last star, at $13^h\ 4^m\ 45^s$, is distinctly marked thus in the MS, although not so stated in the printed copy. I apprehend therefore that the star immediately preceding (which is the star in question) was also observed to the *south*. The whole discordance will then vanish, and the observation will apply to 37 *Cygni*.

2780. — *Capricorni*. Observed by Flamsteed on July 19, 1700, at $11^h\ 33^m\ 18^s$. It is Piazzi XX. 144 = 836 Mayer: and was also observed by Bradley.

2781. 39 *Cygni*. This star is designated by the letter *h* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2782. 12 *Capricorni* o. There is no magnitude annexed to this star, in the *British Catalogue*: nor can I find any record of it in the observations. Piazzi makes it to be of the 6th.

2783. 68 *Aquilæ*. In the reduction of one of the observations of this star (Aug. 20, 1690, at $9^h\ 35^m\ 45^s$) Flamsteed has made an error of $1^h\ 0^m\ 10^s$, in taking out the difference between the time of its transit and that of ζ *Aquilæ* (his determining star) on that day: as may be seen in MSS, vol. 23, page 251. Whence has arisen the erroneous introduction of 33 *Aquilæ* into the *British Catalogue*, whose position there given is $\mathcal{R} = 287°\ 59'\ 30''$, and $D = -\ 4°\ 20'\ 25''$: but which does not exist. See the following note.

2784. 69 *Aquilæ*. In the reduction of one of the observations of this star (Aug. 20, 1690, at $9^h\ 37^m\ 1^s$) Flamsteed has made an error of $1^h\ 0^m\ 10^s$, in taking out the difference between the time of its transit and that of ζ *Aquilæ* (his determining star) on that day: as may be seen in MSS, vol. 23, page 251. Whence has arisen the erroneous introduction of 34 *Aquilæ* into the *British Catalogue*, whose position there given is $\mathcal{R} = 288°\ 18'\ 45''$, and $D = -\ 3°\ 53'\ 0''$: but which does not exist. See the preceding note.

2786. — *Capricorni*. Observed by Flamsteed on Aug. 22, 1690, at $9^h\ 31\frac{1}{4}^m$, and inserted by him in his catalogue, in MSS, vol. 25, page 63. It is Piazzi XX. 174 = 842 Mayer.

2789. 41 *Cygni*. This star is designated by the letter *i* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2790. — *Cephei*. Observed by Flamsteed on July 26, 1692, at $11^h\ 14^m\ 30^s$; and noticed again on July 31, after $10^h\ 54^m\ 40^s$. It is Piazzi XX. 119. It is called by Flamsteed the companion of 1 *Cephei*.

2792. 43 *Cygni* ω^1. Observed by Flamsteed on Sept. 24, 1690, after $7^h\ 29^m\ 46^s$; and again on the next day, at $7^h\ 32^m\ 32^s$. It is evident that these two observations belong to the same star. In the printed copy, the reading of the zenith distance of the former observation *per strias cochleæ* is expressed as doubtful: but in the original MS entry (MSS, vol. 4) there is no such doubt expressed: and it is now evident that that reading is the most correct. Flamsteed however has assumed the reading *per lineas diagonales* as the correct one; and has, in MSS, vol.

23, page 353, deduced the declination accordingly. Whereby he has introduced the star 38 *Cygni* into the *British Catalogue*: whose position as there printed is without any right ascension, but with D = + 48° 33′ 0″; but which star does not exist.

2793. 72 *Draconis.* Miss Herschel says that there is no observation of this star by Flamsteed; but it was observed by him on July 31, 1692, at $10^h\ 53^m\ 3^s$; and partly reduced in MSS, vol. 23, page 250; an observation which Miss Herschel supposes to belong to 71 *Draconis.* It will be seen however that the interval between the present star and 68 *Draconis* is 3^m greater than the interval between 68 and 71 *Draconis* on July 29th. It was observed by Piazzi (XX. 162) but much diminished in magnitude: and also by Lalande in *Mém. de l'Acad.* for 1789, page 213, at $20^h\ 20^m\ 8^s$, mag.

2794. 1 *Cephei* ×. The right ascension of this star must be increased 7′ 45″,6, in order to correspond with Bradley's observations. It is derived partly from observations with the mural arc, and partly from observations *per distantias*; as may be seen in MSS, vol. 26, page 356, &c.: where several errors in the steps of the process are discoverable.

2798. 13 *Capricorni* τ^1. There is no letter annexed to this star in the *British Catalogue*: but, as it may be doubted whether 14 *Capricorni*, or this star, was intended to be designated by the letter τ in Bayer's map, I have annexed the letter to each.

2800. 70 *Aquilæ.* In the reduction of one of the observations of this star (Aug. 20, 1690, at $9^h\ 44^m\ 6^s$) Flamsteed has made an error of $1^h\ 0^m\ 10^s$ in taking out the difference between the time of its transit and that of ζ *Aquilæ* (his determining star) on the same day: as may be seen in MSS, vol. 23, page 251. Whence has arisen the erroneous introduction of 40 *Aquilæ* in the *British Catalogue*, whose position there given is $\mathcal{R}$ = 290° 5′ 15″, and D = − 3° 35′ 35″; but which does not exist. See the notes to Nos. 2783 and 2784 above.

2802. 46 *Cygni* ω^2. Bayer has only two stars in *Cygnus*, designated by the letter ω: but as it may be doubtful which of the three contiguous stars were intended, Flamsteed has applied that letter to all of them: which I have here retained.

2803. 47 *Cygni.* This star is designated by the letter *l* in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2805. 71 *Aquilæ.* In the reduction of one of the observations of this star (Aug. 20, 1690, at $9^h\ 45^m\ 51^s{,}5$) Flamsteed has made an error of $1^h\ 0^m\ 10^s$ in taking out the difference between the time of its transit and that of ζ *Aquilæ* (his determining star) on that day: as may be seen in MSS, vol. 23, page 251. Whence has arisen the erroneous introduction of 43 *Aquilæ* into the *British Catalogue*, whose position there given is $\mathcal{R}$ = 290° 32′ 15″, and D = − 2° 9′ 30″: but which does not exist. See the notes to Nos. 2783, 2784, and 2800.

2810. — *Capricorni.* Observed by Flamsteed on Aug. 22, 1690, after $9^h\ 38^m\ 19^s$. It is Piazzi XX. 240 = 850 Mayer.

2816. 29 *Vulpeculæ.* The right ascension of this star in the *British Catalogue* is 306° 18′ 0″. It was observed only once by Flamsteed; viz. on Sept. 23, 1690: but the time of transit is not given. In the *original* MS entry it is said to have passed the meridian about the same time as 27 *Vulpeculæ*: and modern observations show that it passed the meridian *before* 28 *Vulpeculæ*: which I have therefore adopted; but at the same time considering its right ascension as doubtful.

2818. — *Cygni.* Observed by Flamsteed on Sept. 1, 1690, after $9^h\ 6^m\ 31^s$. It is Piazzi XX. 243; and was also observed by Bradley.

2820. 9 *Delphini α.* In the observation of this star on July 26, 1690, at $11^h\ 21^m\ 26^s$, Flamsteed remarks that it has 2 companions, one very near, preceding it, and another more remote following it; both to the south. These are probably the stars observed by Lalande in *Hist. Cél.* page 116, at $20^h\ 29^m\ 21^s$, and at $20^h\ 30^m\ 36^s$.

2826. 50 *Cygni α.* Besides the 4 observations of this star indicated by Miss Herschel, there is another on July 1, 1703, at $13^h\ 4^m\ 45^s$: which in the MS entry is stated to have been observed to the *south* of the zenith; and is so reduced in MSS, vol. 26 B, page 4: but which is not so noted in the *printed* copy of the *Historia Cœlestis.* Owing to this error, the observation has been supposed to belong to another and a different star: viz. No. 310 of Miss Herschel's catalogue, and No. 503 of my list of Flamsteed's inedited stars; but which consequently does not exist.

2827. — *Capricorni.* Observed by Flamsteed on July 15, 1697, at $12^h\ 6^m\ 19^s$. It is the star observed by Lalande in *Hist. Cél.* page 177, at $20^h\ 36^m\ 38^s$, mag. 9.

2834. 52 *Cygni.* This star is designated by the letter *k* in the *British Catalogue*: but, as there is no such star in Bayer's map, I have here rejected it.

2838. 15 *Delphini.* The right ascension of this star in the *British Catalogue* is 308° 38′ 15″. It was observed by Flamsteed on Aug. 25, 1690, after $9^h\ 38^m\ 49^s$; at which time 14 *Delphini* passed the meridian. Flamsteed remarks that this star passed almost at the same moment: but the exact time is not given. In the reduction of this star in MSS, vol. 23, page 255, he has assumed that it passed *exactly* at the same moment; and consequently made its right ascension the same as that star: which is not correct. I have restored it to its proper place in the catalogue, but left the right ascension doubtful.

2842. 5 *Aquarii.* In the observation of this star on Aug. 20, 1690, at $9^h\ 59^m\ 17^s$, Flamsteed remarks that it has a companion preceding it: which is probably Piazzi XX. 340.

2844. — *Capricorni.* Observed by Flamsteed on Aug. 22, 1690, after $9^h\ 53^m\ 10^s$: and inserted in his catalogue in MSS, vol. 25, page 63. It is the star observed by Lalande in *Hist. Cél.* page 178, at $20^h\ 42^m\ 55^s{,}5$, mag. $7\frac{1}{2}$.

2846. — *Cephei.* Observed by Flamsteed on Sept. 28, 1692, at $7^h\ 40^m\ 41^s$. The reading *per lineas diagonales* differs full 1° from that *per strias cochleæ*: but if we assume the latter as the correct reading (as here adopted) the observation will agree with Piazzi XX. 332 = (6 Hev.) *Cephei.*

2848. 3 *Cephei η.* The right ascension of this star is deduced by Flamsteed from one observation only, compared with *α Cephei*: and it consequently partakes of the discordancy of that star.

2849. 4 *Cephei.* The right ascension of this star is deduced by Flamsteed from one observation only, compared with *β Cephei*: and it consequently partakes of the discordancy of that star.

2850. 31 *Vulpeculæ.* This star has the letter *r* annexed to it, in the *British Catalogue*: but, as even the constellation is not to be found in Bayer, I have here rejected it.

2855. 32 *Vulpeculæ.* This star is designated by the letter *q*, in the *British Catalogue*: but, as even the constellation is not to be found in Bayer, I have here rejected it.

2861. 21 *Capricorni.* In the observation of this star on Aug. 22, 1690, at $9^h\ 59^m\ 17^s$, Flamsteed remarks that it has 2 companions following it, at 25′ distant. Probably some of the stars observed by Lalande in *Hist. Cél.* page 177.

2867. — *Aquarii.* Observed by Flamsteed on Sept. 12, 1699, at $8^h\ 50^m\ 45^s$. It is the star observed by Lalande in *Hist. Cél.* page 188, at $20^h\ 50^m\ 4^s{,}8$, mag. 7.

2873. 2 *Piscis Austrini.* The right ascension of this star in the *British Catalogue* is 311° 34′ 20″. It was observed on Sept. 11, 1691, at $8^h\ 30^m\ 21^s$; and on comparing the time of transit with that of the preceding and subsequent star on the same day, it is evident that an error of 1^m has been committed in the reduction. I have therefore added 15′ to the above value.

2882. 26 *Capricorni.* This star is called χ^3 in the *British Catalogue:* but there is only one star designated by that letter in Bayer's map, which is 25 *Capricorni:* I have therefore rejected the letter here.

2883. 27 *Capricorni.* In the observation of this star on Aug. 22, 1690, at $10^h\ 7^m\ 40^s$, Flamsteed remarks that it has a companion preceding it, nearly in the same declination as 26 *Capricorni;* which is probably Piazzi XX. 454. This star is marked as χ^3 in the *British Catalogue:* but there is only one star designated by that letter in Bayer's map, which is 25 *Capricorni;* I have therefore rejected it here.

2885. 61 *Cygni.* The great proper motion of this star, both in right ascension and declination, must be taken into account in making comparisons of its position with modern observations.

2887. 3 *Piscis Austrini.* The right ascension of this star in the *British Catalogue* is 313° 34′ 0″; but it is marked as doubtful. There is but one observation of it, viz. on Sept. 25, 1690, at *about* $8^h\ 9\frac{1}{2}^m$; but this is uncertain, and modern observations show that it should have been nearer $8^h\ 10^m$. I have increased the above value therefore 8′; but still leaving the $\mathcal{R}$ uncertain.

2894. 7 *Equulei δ.* There appears to be an annual proper motion of this star, in declination, of — 0″,20; which will reduce the difference in declination to — 58″,1.

2901. — *Capricorni.* Observed by Flamsteed on Oct. 16, 1698, at $6^h\ 56^m\ 22^s$. It is Piazzi XXI. 66 = 877 Mayer.

2911. 68 *Cygni A.* The right ascension of this star is not given in the *British Catalogue*, and I know not for what reason, as there is a perfect observation of it on Sept. 24, 1690, at $8^h\ 25^m\ 27^s$. The right ascension is given in Halley's edition of 1712; from which the value in the present catalogue is taken. Flamsteed remarks that it has a telescopic star preceding it to the north; which is probably the star observed by Lalande in *Hist. Cél.* page 1, at $21^h\ 8^m\ 33^s{,}5$, mag. 6.

2912. 34 *Vulpeculæ.* Observed by Flamsteed on Sept. 5, 1690, at $9^h\ 34^m\ 40^s$: its right ascension in the *British Catalogue* is 314° 15′ 30″; but in taking out the difference between the time of its transit and that of φ *Cygni* (his determining star) on that day, he has made an error of 10^m, as may be seen in MSS, vol. 23, page 351. I have therefore added 2° 30′ to the above value, and it will then agree with modern observations. It was observed by Lalande in *Hist. Cél.* page 29, at $21^h\ 12^m\ 7^s$, mag. 5.

2915. 1 *Pegasi.* This star is designated by the letter *e* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

2921. 5 *Piscis Austrini.* Observed by Flamsteed on Sept. 25, 1690, at about $8^h\ 25\frac{1}{2}^m$: but the time is only approximately given: therefore the right ascension cannot be depended on; and indeed it is marked as doubtful in the *British Catalogue.*

2925. — *Pegasi.* Observed by Flamsteed on Oct. 9, 1691, at about $7^h\ 34^m\ 45^s$: but neither the time of transit nor the zenith distance is accurately given. It is the star observed by Lalande in *Hist. Cél.* page 188, at $21^h\ 16^m\ 33^s$, mag. 7.

2927. 69 *Cygni.* The right ascension of this star in the *British Catalogue* is $318°\ 2'\ 0''$. There are 2 observations of it by Flamsteed: one on Sept. 1, 1690, after $10^h\ 12^m\ 2^s$; and the other on Sept. 23, at $8^h\ 35^m\ 24^s$. The latter, which is the only perfect one, is regularly reduced in MSS, vol. 23, page 351: but in converting the time into degrees, in page 422 of the same volume, he has made an error of 15′, which I have here added to the above value.

2928. 6 *Piscis Austrini.* About 1′ of this difference in the declination may be accounted for, by the difference of the tables of refraction used by Flamsteed and Bessel.

2932. 2 *Pegasi.* This star is designated by the letter *f* in the *British Catalogue:* but, as there is no such star in Bayer's map, I have here rejected it. It is also designated as of the $4\frac{1}{2}$ magnitude: but, in the *original* entry, it is marked as the 6th; which I have here retained.

2934. 38 *Capricorni.* This star is marked as of the 6th magnitude in the *British Catalogue:* but in the *original* entries, it is once designated as the 7th, and once as *telescopica* and less than 37 *Capricorni.* I have therefore altered it.

2936. 71 *Cygni g.* In the observation of this star, on Sept. 24, 1690, at $8^h\ 36^m\ 33^s$, Flamsteed remarks that it has a companion preceding it to the north 40′: but it is not stated whether in Æ or Dec. M. Argelander thinks the star is Piazzi XXI. 140.

2941. — *Aquarii.* Observed by Flamsteed on Sept. 20, 1690, at about $8^h\ 55\frac{1}{2}^m$: and inserted in his catalogue in MSS, vol. 25, page 65. It is the star observed by Bessel in Zone 16, at $21^h\ 28^m\ 8^s{,}56$, mag. 7; and again in Zone 18, at $21^h\ 28^m\ 8^s{,}62$, mag. $6\frac{1}{2}$.

2949. 25 *Aquarii d.* This is the same star as 6 *Pegasi,* whose position in the *British Catalogue* is Æ = 320° 56′ 45″, and D = + 0° 52′ 0″.

2953. — *Pegasi.* Observed by Flamsteed on Oct. 12, 1691, at $7^h\ 34^m\ 39^s$: and regularly reduced by him in MSS, vol. 23, page 263; but omitted in his list of stars in the constellation *Pegasus* in page 324 of the same volume. It is the star observed by Lalande in *Hist. Cél.* page 32, at $21^h\ 30^m\ 52^s{,}5$, mag. $5\frac{1}{2}$.

2954. 42 *Capricorni.* }
2956. 44 *Capricorni.* } These two stars are designated as d^1 and d^2 in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected them.

2962. 46 *Capricorni* c^1. In the observation of this star on Aug. 20, 1690, at $10^h\ 51^m\ 53^s$, Flamsteed remarks that it has a small companion to the north: probably Piazzi XXI. 257.

2965. 9 *Cephei.* The right ascension of this star is deduced by Flamsteed from that of *α Cephei:* and consequently partakes of the discordance of that star.

2967. 47 *Capricorni* c^3. Bayer has three stars designated by the letter *c*, in his map: but I cannot discover which is the third; or rather which is the *first* of the three.

2968. 9 *Pegasi*. This star is designated by the letter *g*, in the *British Catalogue*: but, as there is no such letter in Bayer's map, I have here rejected it.

2971. 49 *Capricorni* δ. There appears to be an annual proper motion of this star, in declination, of $-0'',216$, which will reduce the difference of declination to $-55'',4$.

2973. 78 *Cygni* μ. There is only one observation of this star by Flamsteed, which was on Sept. 23, 1690, after $8^h\ 53^m\ 52^s$; but the time of transit is not given, and I cannot find how the right ascension has been deduced by him. In the printed copy of the *Historia Cœlestis* it is called of the 6th magnitude, and is said to have passed *paulo post precedentem:* but in the original MS entry (MSS, vol. 4) it is called of the 5th magnitude, and is said to have passed *paulo post priorem;* which may be supposed to apply to $\varkappa$ *Pegasi*. But neither of these readings will accord with the fact, as this star could not have passed the meridian *after* either 12 *Pegasi* or $\varkappa$ *Pegasi*. There is no letter annexed to this star in the *British Catalogue:* but as it is designated by the letter μ in Bayer's map, and also in the *Historia Cœlestis*, I have here inserted it. Flamsteed has designated it as of the $3\frac{1}{2}$ magnitude: but, on reference to the *original* entry, there are no such figures, and it appears to be marked as the 5th; which I have therefore adopted; and which agrees with Piazzi.

2975. 79 *Cygni*. There is no right ascension annexed to this star in the *British Catalogue:* I have therefore adopted that which is given by Halley, in his edition of 1712. It was observed by Bradley.

2976. — *Cygni*. Observed by Flamsteed on Sept. 1, 1690, at $10^h\ 12^m\ 2^s$; and regularly reduced by him in MSS, vol. 23, page 349; but omitted in his list of stars in the constellation *Cygnus* in page 424 of the same volume. It is Piazzi XXI. 265; and was also observed by Bradley.

2978. 12 *Pegasi*. Observed by Flamsteed on Sept. 23, 1690, at $8^h\ 53^m\ 52^s$; and correctly reduced by him in MSS, vol. 23, page 259. But the right ascension must be increased nearly $5'$ in order to correspond with modern observations.

2979. 27 *Aquarii*. This is the same star as 11 *Pegasi;* whose position in the *British Catalogue* is $\mathcal{R} = 322°\ 52'\ 20''$, and $D = +\ 1°\ 16'\ 10''$.

2980. — *Capricorni*. Observed by Flamsteed on Sept. 11, 1699, at $9^h\ 45^m\ 45^s$; and again on the following day, at $9^h\ 42^m\ 2^s$. It is Piazzi XXI. 291 = 902 Mayer.

2984. 10 *Cephei* ν. Flamsteed has erroneously annexed the letter ν to 15 *Cephei*, instead of this star, to which it properly belongs.

2986. — *Aquarii*. Observed by Flamsteed on Aug. 22, 1701, at $10^h\ 51^m\ 8^s$: and reduced by him in MSS, vol. 26 C, page 33. It is the star observed by Lalande in *Hist. Cél.* page 571, at $21^h\ 42^m\ 33^s,5$, mag. 7.

2988. — *Aquarii*. Observed by Flamsteed on Aug. 22, 1701, at $10^h\ 51^m\ 46^s$: and reduced by him in MSS, vol. 26 C, page 33. It is the star observed by Lalande, in *Hist. Cél.* page 571, at $21^h\ 43^m\ 10^s$, mag. 6.

2990. — *Pegasi*. Observed by Flamsteed on Oct. 16, 1691, at about $7^h\ 37^m$; but the time is not accurately given. It is regularly reduced by him in MSS, vol. 23, page 265; and inserted in

his list of stars in the constellation *Pegasus* in page 324 of the same volume. It is Piazzi XXI. 322.

2994. 11 *Piscis Austrini.* Observed by Flamsteed on Sept. 11, 1691, at $9^h\ 24^m\ 33^s$, and regularly and correctly reduced by him in MSS, vol. 23, page 144: and the proper result (as here given) is inserted in his list of stars in the constellation *Piscis Austrinus* in MSS, vol. 25. The declination in the *British Catalogue* is $-29°\ 54'\ 20''$; which is erroneous: and I cannot account for the error in any other way than by supposing that, in copying out the declinations for the press, the transcriber inadvertently took out the declination of the next following star in that constellation, viz. 12 *Piscis Austrini.* The proper correction being made, this star will be found to be the one observed by Lacaille, as recorded in his *Cœlum Australe*, page 125, which entered the rhomboid at $21^h\ 44^m\ 14^s$, and passed out at $21^h\ 46^m\ 29^s$, mag. 7.

2997. 13 *Cephei* μ. The right ascension of this star is not given in the *British Catalogue.* It was observed on Sept. 28, 1692, at about $8^h\ 47^m$: but the time of transit is only approximately noted; and I have therefore left the right ascension doubtful.

2998. 18 *Pegasi.* This star is marked as of the 5th magnitude, in the *British Catalogue:* but, in the *original* entries it is designated twice as the 6th, and once as the 7th: I have therefore altered it.

3003. 13 *Piscis Austrini.* There appears to be an annual proper motion of this star, in declination, of $+\ 0'',190$; but this tends to increase the difference in declination.

3004. — *Piscis Austrini.* Observed by Flamsteed on Sept. 11, 1691, after $9^h\ 29^m\ 14^s$. It is probably Piazzi XXI. 378.

3006. 31 *Aquarii* o. Besides the 7 observations of this star, indicated by Miss Herschel, there is another made by Flamsteed on Nov. 23, 1693, at $5^h\ 10^m\ 33^s$: which, owing to an error, which I shall presently explain, has been supposed by her to be another and a different star; viz. No. 105 of her catalogue, and No. 527 in my list of Flamsteed's inedited stars. The zenith distance, in the printed copy of the *Historia Cœlestis*, is $53°\ 8'\ 0''$ and 1204·64: but no such star can be found corresponding thereto. M. Argelander therefore suggested that the reading should be $55°\ 8'\ 0''$ and 1249·64: in which case, it would agree with 31 *Aquarii.* On referring to the original MS entry (MSS, vol. 5) I find that 1204·64 was originally entered 1249·64, and afterwards altered by the pen, on account (as I suppose) of its not agreeing with the reading *per lineas diagonales.* So that M. Argelander's suspicions are here again fully confirmed.

3013. 14 *Piscis Austrini* μ. This star is marked as of the 4th magnitude, in the *British Catalogue:* but, in the *original* entries it is once so called, and once of the 5th only; I have therefore taken the mean.

3014. — *Aquarii.* Observed by Flamsteed on Aug. 19, 1701, at $11^h\ 44^m\ 52^s,5$; and again on Aug. 22, at $11^h\ 5^m\ 29^s$: and reduced by him in MSS, vol. 26 C, page 33. It is the star observed by Lalande in *Hist. Cél.* page 571, at $21^h\ 56^m\ 54^s,5$, mag. $6\frac{1}{2}$.

3015. — *Aquarii.* Observed by Flamsteed on Aug. 22, 1701, at $11^h\ 6^m\ 0^s$; and reduced by him in MSS, vol. 26 C, page 33. It is the star observed by Lalande in *Hist. Cél.* page 572, at $21^h\ 57^m\ 16^s,3$, mag. $7\frac{1}{2}$.

3020. — *Aquarii.* Observed by Flamsteed on Oct. 17, 1699, at $7^h\ 53^m\ 0^s$; and is inserted in Halley's edition of 1712: but I cannot find that it ever was reduced by Flamsteed. It is the star observed by Lalande in *Hist. Cél.* page 181, at $21^h\ 57^m\ 32^s,5$, mag. $7\frac{1}{2}$.

3022. 36 *Aquarii.* This star is marked as of the 6th magnitude, in the *British Catalogue*: but in the *original* entries it is each time designated as the 7th; which I have therefore retained.

3023. — *Aquarii.* Observed by Flamsteed on Oct. 11, 1697, after $8^h\ 11^m\ 26^s$. It is the star observed by Lalande in *Hist. Cél.* page 197, at $21^h\ 58^m\ 51^s,5$, mag. 8; and was also observed by Bradley. M. Argelander states that 9 observations of it at Abo, give its position for 1830, $\text{Æ} = 22^h\ 0^m\ 31^s,30$ and $D = -8^\circ\ 21'\ 27'',4$.

3025. 15 *Cephei.* The letter ν is annexed to this star in the *British Catalogue;* but that letter belongs to 10 *Cephei.*

3026. 37 *Aquarii* e^1. Flamsteed has annexed the letter *e* to 38 *Aquarii*; but, in Bayer's map, the star designated by that letter has *north* latitude, as also has the star in the catalogues of Ptolemy and Tycho, whom Bayer followed. I have restored the letter to the proper star: but, lest any confusion should be created by this alteration, I have called this star e^1, and the next e^2.

3027. 38 *Aquarii* e^2. Flamsteed has considered this to be the star designated by the letter *e* in Bayer's map: but see the preceding note.

3030. 17 *Cephei* ξ. The right ascension of this star, like all the rest in *Cepheus* which have been deduced by Flamsteed from α *Cephei*, must be increased nearly 5′ in order to correspond with modern observations.

3031. 18 *Cephei.* The right ascension of this star is not given in the *British Catalogue:* but I have here inserted its approximate value, corresponding with Bradley's observations. Its *relative* value therefore will not accord with the other stars in *Cepheus.* The magnitude is not annexed to it in the *British Catalogue;* nor in Bradley's. It was not observed by Piazzi.

3034. 27 *Pegasi* π^1. This star is not designated by any letter in the *British Catalogue:* but as it may be doubted whether the letter π does not belong to this star as well as to 29 *Pegasi*, I have called it π^1.

3035. 39 *Aquarii.* This star is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entry it is designated as the 7th only; which I have adopted.

3038. 16 *Piscis Austrini* λ. There appears to be an annual proper motion of this star, in declination, of $+\ 0''155$; but this tends to increase the difference of declination. The magnitude annexed to the first observation of this star, in the *printed* copy, is not to be found in the *original* entry. Piazzi says it is of the 6th magnitude only.

3042. 42 *Aquarii.* This star is marked as of the 7th magnitude in the *British Catalogue:* but, in the *original* entries the magnitude is only once stated, and then designated as the 6th; which I have here retained.

3044. — *Lacertæ.* Observed by Flamsteed on Oct. 31, 1691, at $7^h\ 2^m\ 26^s$. It is Piazzi XXII. 36 = (1 Hev.) *Lacertæ.*

3059. — *Aquarii.* Observed by Flamsteed on Oct. 23, 1699, at $7^h\ 42^m\ 29^s$. It is Piazzi XXII. 81.

3060. 50 *Aquarii.* The declination of this star in the *British Catalogue* is — 15° 3′ 50″. Flamsteed has reduced the two observations of it, which were made in Sept. 1690, in MSS, vol. 23: and in his list of the stars in *Aquarius*, in page 162 of the same volume, he has assumed the latter of the two as the correct result: but on examining the steps of the process it will be seen that he has made an error of 1′ in transcribing the zenith distance, which I have here corrected.

3073. — *Piscis Austrini.* Observed by Flamsteed on Sept. 11, 1691, after $9^h\ 56^m\ 29^s$: and reduced by him in MSS, vol. 23, page 144: it is also inserted in Halley's edition of 1712. In the *original* MS entry it is stated to have passed the meridian on that day a little *before* the preceding star 17 *Piscis Austrini*: which clearly shows that it is Piazzi XXII. 118; where it is said to be of the 7th magnitude; and also denoted by the letter ξ, which I presume is erroneously inserted for the letter ζ.

3075. 56 *Aquarii.* This star is denoted by the letter *f* in the *British Catalogue*: but it is not the star designated by that letter in Bayer's map; and which appears to have been observed by Ptolemy and Ulug Beigh: nor can I find any star that will exactly accord with the position laid down by those authors.

3076. 17 *Piscis Austrini* β. About 1′ of this difference in declination may be accounted for, by the difference in the tables of refraction used by Flamsteed and Bessel.

3085. 39 *Pegasi.* In the observation of this star on Oct. 12, 1691, at $8^h\ 27^m\ 34^s$, Flamsteed remarks that it has a small companion, preceding it to the south, at 38′ distant. M. Argelander thinks that this is the star observed by Lalande in *Hist. Cél.* page 34, at $22^h\ 24^m\ 13^s{,}3$, mag. 7.

3091. — *Aquarii.* Observed by Flamsteed on Sept. 16, 1690, at about $10^h\ 8^m$; and reduced by him in MSS, vol. 23, page 147. It is probably the star observed by Lalande in *Hist. Cél.* page 570, at $22^h\ 26^m\ 3^s{,}5$, mag. 6: but, if so, the time given by Flamsteed is too great by about 1^m.

3094. 64 *Aquarii.* Observed by Flamsteed on Aug. 18, 1701, at $12^h\ 20^m\ 12^s$; and not on Oct. 30, 1700, at $7^h\ 30^m\ 39^s$, as indicated by Miss Herschel: which observation belongs to 65 *Aquarii.* The right ascension in the *British Catalogue* is 335° 28′ 40″: but in the process of reduction, an error of 1^m has been committed. See MSS, vol. 26 C, page 33. I have therefore added 15′ to the above value: and it will now accord with modern observations.

3097. 28 *Cephei.* The right ascension of this star is not given in the *British Catalogue*: but I have inserted its approximate value, corresponding with modern observations. Its *relative* value therefore will not accord with that of other stars in *Cepheus.*

3103. — *Pegasi.* Observed by Flamsteed on Sept. 15, 1693, at $10^h\ 18^m\ 6^s$. It is Piazzi XXII. 195.

3104. 29 *Cephei* ρ. Although there is an observation of this star with the mural arc, on Sept. 21, 1691, at $9^h\ 53^m\ 47^s$, yet Flamsteed has deduced the right ascension in the *British Catalogue* (=336° 27′ 0″) from *distances;* as may be seen in MSS, vol. 23, page 358. But this value will not accord with modern observations, nor with his own observation made with the mural circle. For by taking the difference in the time of its transit, with that of other stars observed on the same day, it will be seen that the right ascension above stated ought to be increased 14′; which I have accordingly done.

3105. 65 *Aquarii.* Observed by Flamsteed on Oct. 30, 1700, at $7^h\ 30^m\ 39^s$: an observation which Miss Herschel has supposed to belong to 64 *Aquarii.*

3106. — *Aquarii.* Observed by Flamsteed on Nov. 15, 1702, at $6^h\ 25^m\ 6^s$; and again on the next day at $6^h\ 15^m\ 45^s$. It is Piazzi XXII. 200 = 933 Mayer.

3117. 45 *Pegasi.* In the observation of this star on Oct. 12, 1691, at $8^h\ 40^m\ 19^s$, Flamsteed remarks that it has a companion following it, 10′ distant. This is probably the star alluded to by Piazzi in the note to his XXII. 212.

3118. 68 *Aquarii* g^2. There is only one star designated by the letter *g*, in Bayer's map: but as Flamsteed appears to have had a doubt whether such letter belongs to 66 *Aquarii*, or to the present star, he has annexed it to each, which I have retained, lest any confusion should be created: although in my opinion the first (or 66 *Aquarii*) is the star intended by Bayer.

3123. 71 *Aquarii* τ^4. There is only one star designated by the letter τ, in Bayer's map: but, as there may be a doubt whether such letter belongs to 69 *Aquarii*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

3124. 21 *Piscis Austrini.* The declination of this star in the *British Catalogue* is — 31° 8′ 0″: it was observed by Flamsteed on Sept. 25, 1690, at $9^h\ 49^m\ 9^s$, and reduced by him in MSS, vol. 23, page 144: but in one of the steps of the process he has made an error of 1′, which I have here corrected.

3125. 72 *Aquarii.* Observed by Flamsteed on August 19, 1700, at $12^h\ 0^m\ 24^s$: but the observations of that day are not printed in the *Historia Cœlestis.* The declination in the *British Catalogue* is — 5° 55′ 45″; but in reducing the observation in MSS, vol. 26 C, page 32, he has made an error of 3°, which I have here corrected. It is Piazzi XXII. 230 = 939 Mayer.

3127. 22 *Piscis Austrini* γ. There appears to be an annual proper motion of this star, in declination, of + 0″,136; but this tends to increase the difference in declination.

3133. — *Piscium.* Observed by Flamsteed on Sept. 22, 1705, at $10^h\ 2^m\ 5^s$: the observations of which day are not in the MS *copy;* and the *original* is unfortunately lost. It is the star observed by Lalande in *Hist. Cél.* page 118, at $22^h\ 43^m\ 16^s{,}7$, mag. 7½.

3142. 24 *Piscis Austrini* α. This is the same star as 79 *Aquarii.*

3143. — *Piscium.* Observed by Flamsteed on Dec. 2, 1706, at $5^h\ 28^m\ 26^s$. It is the star observed by Lalande in *Hist. Cél.* page 110, at $22^h\ 46^m\ 55^s{,}6$, mag. 6.

3146. — *Andromedæ.* Observed by Flamsteed on Nov. 2, 1691, at $7^h\ 35^m\ 6^s$. It is the star observed by Lalande in *Hist. Cél.* page 363, at $22^h\ 46^m\ 48^s{,}4$, mag. 5.

3147. — *Andromedæ.* Observed by Flamsteed on Nov. 2, 1691, at $7^h\ 35^m\ 37^s{,}5$. It is the star observed by Lalande in *Hist. Cél.* page 363, at $22^h\ 47^m\ 23^s{,}2$, mag. 6.

3149. 52 *Pegasi.* The declination of this star in the *British Catalogue* is + 10° 3′ 45″. It was observed by Flamsteed on Oct. 7, 1691, at $9^h\ 14^m\ 31^s$; and reduced by him in MSS, vol. 23, page 261. But in the steps of the process he has made an error of 1′, which I have here corrected.

3150. 80 *Aquarii.* The right ascension of this star in the *British Catalogue* is 340° 37′ 30″: it is deduced from the observation made on Aug. 12, 1706, at $12^h\ 46^m\ 18^s$ (as appears by MSS,

vol. 26 C, page 34). It is evident however that there is an error of 2^m in the reduction; and that 30′ should be added to the above value: which is accordingly done in the present catalogue. Still I cannot find any star that will correspond thereto: that which is called 80 *Aquarii* by Lalande in *Hist. Cél.* page 188, being Piazzi XXII. 254: and that which is so called by Piazzi, differing from the position here given.

3151. 3 *Piscium.* In the observation of this star on Sept. 16, 1690, at $10^h\ 32^m\ 57^s,5$, Flamsteed remarks that it has 2 companions following it. These are probably the 2 stars in Lalande's *Hist. Cél.* page 118, which immediately follow 3 *Piscium.*

3154. 1 *Andromedæ* o. The declination of this star in the *British Catalogue* is + 49° 40′ 30″: but all the observations show that an error of 9° has been committed, which I have here corrected. It is probably a typographical error.

3158. 83 *Aquarii* h^1.
3159. 84 *Aquarii* h^2.
3161. 85 *Aquarii* h^3.

There is only one star designated by the letter *h*, in Bayer's map: but, as some doubt may arise as to which star such letter may belong, (or rather, whether the letter may not have been applied to the cluster of 4 stars, formed by 83, 84, 85, and 87 *Aquarii*) Flamsteed has annexed the letter to each, which I have therefore retained. The last of the present stars is marked as of the 6th magnitude, in the *British Catalogue*: but, in the *original* entries, it is once designated as the 7th and once as 7½. I have therefore altered it.

3164. 87 *Aquarii* h^4. The position of this star, in the *British Catalogue*, is $\mathcal{R}$ = 342° 50′ 40″ and D = − 9° 19′ 55″: both of which are erroneous. It was observed by Flamsteed on Sept. 29, 1697, at $9^h\ 49^m\ 43^s$; and Oct. 11, at $9^h\ 9^m\ 21^s$: and on comparing the time of transit with that of 83 *Aquarii* on both those days, it will be seen that an error of 5′ has been made; which I have here corrected. It will also be seen by a similar comparison of zenith distances, that an error of 1′ 5″ has been made in the declination: both of which I have here corrected. In the *British Catalogue*, this star is marked as of the 6th magnitude: but, in the *original* entries it is once designated as the 7th, and once as the 8th: I have therefore taken the mean. See the notes to Nos. 3158, 3159, and 3161.

3165. 55 *Pegasi.* This star is designated by the letter *l*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3171. 1 *Cassiopeæ.* The letter *e* is annexed to this star in the *British Catalogue*, and also to 48 *Cassiopeæ:* but as there is no such letter in Bayer's map, I have rejected it in both places.

3173. 57 *Pegasi.* This star is designated by the letter *m*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3174. 58 *Pegasi.* This star is designated by the letter *n* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3175. 59 *Pegasi.* This star is designated by the letter *p*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3182. — *Aquarii.* Observed by Flamsteed on Nov. 4, 1714, at $7^h\ 35^m\ 11^s$. It is Piazzi XXIII. 2 = 954 Mayer.

3183. — *Aquarii.* Observed by Flamsteed on October 21, 1702, at $8^h\ 31^m\ 2^s$. It is the star observed by Lalande in *Hist. Cél.* page 187, at $23^h\ 4^m\ 36^s,5$, mag. 6.

3191. 96 *Aquarii*. The declination of this star in the *British Catalogue* is $-6°\ 40'\ 20''$: but on comparing it with other stars observed on the same day, it is evident that an error of 9′ has been committed, which I have here rectified.

3194. — *Piscium*. Observed by Flamsteed on October 21, 1702, at $8^h\ 35^m\ 37^s$; and again on the next day, at $8^h\ 32^m\ 5^s$. It is the star observed by Lalande in *Hist. Cél.* page 187, at $23^h\ 9^m\ 14^s$, mag. 7.

3197. 11 *Andromedæ*. In the observation of this star on Nov. 2, 1691, at $7^h\ 57^m\ 18^s$, Flamsteed remarks that it has a companion 15′ to the south: probably Piazzi XXIII. 51.

3199. — *Piscium*. Observed by Flamsteed on Oct. 21, 1702, at $8^h\ 36^m\ 47^s$. I cannot find it in any catalogue: but M. Argelander says that one observation of it made at Abo gives its position for 1830, $R = 23^h\ 12^m\ 35^s{,}64$ and $D = -5°\ 36'\ 5''{,}8$.

3201. 34 *Cephei o*. This star is marked as of the 5th magnitude in the *British Catalogue*: but in the *original* entry, it is designated as the 4th; which I have therefore adopted, although it makes the difference greater from Piazzi's value, which is the 7th.

3202. — *Andromedæ*. Observed by Flamsteed on Nov. 8, 1691, after $7^h\ 35^m\ 12^s$. The time is not given: but the star is probably that which was observed by Lalande in *Hist. Cél.* page 200, at $23^h\ 11^m\ 13^s{,}5$, mag. $6\frac{1}{2}$.

3204. 97 *Aquarii*. The declination of this star in the *British Catalogue* is $-16°\ 45'\ 0''$. Its position has been deduced by Flamsteed, in MSS, vol. 23, page 147, from the observation made on Sept. 16, 1690, at $10^h\ 54^m\ 40^s{,}5$: and the result is $-16°\ 43'\ 0''$, which has been erroneously copied into all the MS catalogues as $-16°\ 45'\ 0''$. To the above value however we must add 30″ for the error in the assumed latitude of the observatory, adopted in the reduction: and the result will be as given in the present catalogue.

3209. — *Piscium*. Observed by Flamsteed on Nov. 27, 1691, at $6^h\ 17^m\ 29^s$; again on Oct. 21, 1702, at $8^h\ 39^m\ 3^s$; and again on the next day, at $8^h\ 35^m\ 33^s$. It is Piazzi XXIII. 68=962 Mayer.

3210. 67 *Pegasi*. The declination of this star in the *British Catalogue* is $+30°\ 51'\ 35''$; but it is marked as doubtful. There is but one observation of it, on Nov. 25, 1690, at $6^h\ 22^m\ 28^s$; which is regularly reduced in MSS, vol. 23, page 261: but in deducing the declination Flamsteed has assumed the reading *per lineas diagonales* as the correct reading; whereas it appears, from modern observations, that the reading *per strias cochleæ* (which differs 10′ from the former) is the most correct. I have therefore deducted 10′ from the above value.

3213. 3 *Cassiopeæ*. There is no observation of this star in the 2nd volume of the *Historia Cœlestis*, the references given by Miss Herschel, belonging to another and a different star, No. 3224 of this catalogue. In MSS, vol. 26 B, page 36, the position is said to have been obtained *per distantias;* probably from the star which is called *supra τ*, in vol. 2, page 63. If so, I apprehend it must have been introduced through some mistake in the trigonometrical computation: as there is no star to be found corresponding with the position here given.

3215. 4 *Cassiopeæ*. The letter *d* is annexed to this star in the *British Catalogue*, and also to 46 *Cassiopeæ;* but as no such letter occurs in Bayer's map, I have rejected it in both places.

3216. 9 *Piscium*. This star is called x^s by Flamsteed: but there is only one star designated by

that letter in Bayer's map, which is 8 *Piscium*. I have therefore rejected it here, on account of the difference in magnitude.

3220. 70 *Pegasi*. This star is designated by the letter *q*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3223. 100 *Aquarii* b^2. This star is marked as of the 5th magnitude, in the *British Catalogue:* but in the *original* entry, there is no such value: on the contrary, it is called a telescopic star, and I have therefore assumed its magnitude at $6\frac{1}{2}$.

3224. — *Cassiopeæ*. Observed by Flamsteed on Dec. 3, 1691, at about $5^h\ 58^m$; and again on the next day, at $5^h\ 53^m\ 3^s$; and regularly reduced by him in MSS, vol. 23, pages 360 to 363. These observations are referred to, by Miss Herschel, as belonging to 3 *Cassiopeæ:* but they belong to Piazzi XXIII. 101 = (1 Hev.) *Cassiopeæ*. It is singular however that the result of these two observations should have been omitted in all his MS catalogues of the stars in this constellation, and that the doubtful star, 3 *Cassiopeæ*, should be inserted in all of them in its stead. This is the case even in MSS, vol. 23, page 382; where we should expect to find only the result of the above-mentioned observations.

3227. 101 *Aquarii* b^4. There are only 3 stars indicated by the letter *b* in Bayer's map; to which Flamsteed has here added a fourth: which I have retained, because it may be doubted whether the third star designated by Bayer belongs to this star, or to No. 3223.

3228. 71 *Pegasi*. This star is designated by the letter *y*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3236. 75 *Pegasi*. This star is designated by the letter *S*, in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3237. 16 *Andromedæ* λ. There appears to be an annual proper motion of this star, in declination, of $-0''{,}35$; which reduces the difference of declination to $-42''{,}1$.

3238. 17 *Andromedæ* ι. I do not find that the magnitude of this star is recorded in either of the two *original* entries: it differs materially from Piazzi's value, which is the 7th only.

3239. 102 *Aquarii* ω^1. Bayer's letter, which belongs to this star, has slipt out of the form, in printing the *British Catalogue*. I have here restored it.

3241. — *Pegasi*. Observed by Flamsteed on Oct. 7, 1691, at $9^h\ 54^m\ 51^s$; and regularly reduced by him in MSS, vol. 23, page 263; but not inserted in the list of stars in the constellation *Pegasus* in page 328 of the same volume. It is Piazzi XXIII. 146.

3245. 104 *Aquarii* A^2. The right ascension of this star is not given in the *British Catalogue*. There is only one observation of it in right ascension, viz. on Sept. 15, 1690, at $11^h\ 17^m\ 33^s$; which has been regularly reduced by Flamsteed in MSS, vol. 23, page 145, and the result inserted in his list of stars in the constellation *Aquarius* in page 164 of the same volume. The result is also inserted in Halley's edition of 1712; and I know not why it was afterwards omitted. It may be proper here to state that there is only one star designated by the letter *A*, in Bayer's map: but as it may be doubted whether such letter belongs to 103 *Aquarii* or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

3249. 35 *Cephei* γ. Although there is an observation of this star with the mural circle, yet its right ascension has been determined by Flamsteed *per distantias*. See MSS, vol. 23, page 358.

3251. 106 *Aquarii* i^1. } These stars are called A^3 and A^4, in the *British Catalogue*: there is how-
3253. 107 *Aquarii* i^2. } ever but one star designated by that letter in Bayer's map; and although there may be a doubt to which of two stars that letter ought to be applied (as already mentioned in the note to No. 3245) yet there can be no question as to *these* two stars being those which are designated, by Bayer, with the letter *i*. I have therefore corrected the error.

3256. — *Piscium.* Observed by Flamsteed on Nov. 29, 1697, the first after $5^h\ 59^m\ 8^s$. It is the star in Bessel's Zone No. 25, at $23^h\ 38^m\ 11^s,56$, mag. 8.

3257. — *Piscium.* Observed by Flamsteed on Nov. 29, 1697, the second after $5^h\ 59^m\ 8^s$. It is the star observed by Lalande, in *Hist. Cél.* page 118, at $23^h\ 37^m\ 38^s,5$, mag. 8; and also in Bessel's Zone, No. 25, at $23^h\ 38^m\ 15^s,00$, mag. $8\frac{1}{2}$.

3258. — *Piscium.* Observed by Flamsteed on July 19, 1698, at $14^h\ 56^m\ 8^s$. I cannot find any star that will correspond with this observation, unless we suppose an error in noting down the time of transit; and that $14^h\ 56^m\ 8^s$ should be $14^h\ 57^m\ 8^s$. In which case it is Piazzi XXIII. 193.

3261. — *Piscium.* Observed by Flamsteed on Oct. 20, 1702, at $9^h\ 7^m\ 22^s$; again on the next day, at $9^h\ 3^m\ 48^s$; and again on the following day, at $9^h\ 0^m\ 18^s$. It is the star observed by Lalande in *Hist. Cél.* page 127, at $23^h\ 37^m\ 52^s,5$, mag. 7.

3265. 108 *Aquarii.* This star is called A^5 by Flamsteed: but there is strictly only one star designated by that letter in Bayer's map. I have therefore rejected it here. See the notes to Nos. 3245 and 3251.

3271. 83 *Pegasi.* This star is designated by the letter *r* in the *British Catalogue:* but, as there is no such letter in Bayer's map, I have here rejected it.

3276. — *Piscium.* Observed by Flamsteed on Oct. 22, 1702, at $9^h\ 7^m\ 40^s$. It is the star observed by Lalande in *Hist. Cél.* page 127, at $23^h\ 45^m\ 17^s,5$, mag. 7.

3278. 1 *Ceti.* The right ascension of this star in the *British Catalogue* is $355°\ 20'\ 30''$. It has been deduced by Flamsteed from the observation made on Nov. 28, 1693, at $6^h\ 40^m\ 10^s$: but he has made an error of 1^m in the steps of the process, in MSS, vol. 23, page 177. I have therefore added 15′ to the above value. It is marked as of the 6th magnitude, in the *British Catalogue:* but, in the *original* entries, it is designated as the 7th; which I have therefore retained.

3282. — *Piscium.* Observed by Flamsteed on Nov. 14, 1705, at $7^h\ 43^m\ 7^s,5$. It is Piazzi XXIII. 249.

3283. — *Pegasi.* Observed by Flamsteed on Oct. 17, 1691, at $9^h\ 39^m\ 3^s$. It is Piazzi XXIII. 251.

3285. 85 *Pegasi.* There appears to be an annual proper motion of this star, in declination, of $-1''{,}08$; which reduces the difference of declination to $+\ 9''{,}8$.

3287. — *Piscium.* Observed by Flamsteed on Oct. 11, 1697, at $10^h\ 4^m\ 20^s$. It is Piazzi XXIII. 258 = 994 Mayer: and was also observed by Bradley.

3289. 32 *Piscium* c^2. There is only one star designated by the letter *c*, in Bayer's map: but, as is may be doubted whether such letter belongs to 31 *Piscium*, or to the present star, Flamsteed has annexed it to each, which I have therefore retained.

TABLE A.

Synonymous Stars.

(See page 392.)

69	Piscium	= 40	Andromedæ
106	———	= 51	Ceti
2	Arietis	=107	Piscium
38	——	= 88	Ceti
1	Eridani	= 90	——
112	Tauri	= 23	Aurigæ
30	Aurigæ	= 32	Camelopardi
58	Camelopardi	= 30	Lyncis
10	Leonis	= 1	Sextantis
67	——	= 53	Leonis Min.
36	Virginis	= 29	Comæ Ber.
31	Comæ Ber.	= 13	Canum Ven.
10	Draconis	= 87	Ursæ Maj.
4	Libræ	= 53	Hydræ
6	——	= 58	——
24	Herculis	= 51	Serpentis
28	———	= 11	Ophiuchi
43	———	= 17	———
31	Scorpii	= 38	———
25	Aquarii	= 6	Pegasi
27	———	= 11	——
24	Piscis Aust.	= 79	Aquarii

N.B. The left-hand column contains the names retained in the preceding catalogue.

TABLE B.

Stars not existing: being errors of computation.

(See page 393.)

No.	STAR.	No.	STAR.
36	50 Piscium	1352	12 Leonis Minoris
54	56 ———	1310	25 Leonis
205	108 ———	1411	28 ——
107	24 Ceti	1455	38 ——
179	74 ——	1851	18 Virginis
120*	29 Cassiopeæ	1853	19 ———
189	41 ——	1791	45 ———
359	19 Persei	1729	19 Comæ Bernices
644*	13 Camelopardi	1782	34 ———
645	3 Tauri	1935	1 Libræ
654	8 ——	2158	33 Serpentis
457	15 ——	2375	52 ———
	34 —— (*Uranus*)	2413	54 ———
571	82 ——	2247	6 Ophiuchi
988	124 ——	2348	46 ———
800	138 ——	2353	48 ———
486	31 Eridani	2418	59 ———
660	12 Orionis	2384	71 Herculis
709	26 ———	2416	80 ———
838	65 ———	2417	81 ———
895	17 Geminorum	2448	8 Sagittarii
944	29 ———	2648	56 Draconis
1011	50 ———	2458	62 ———
1044	72 ———	2778*	70 ———
1046	73 ———	2783	33 Aquilæ
1173	26 Cancri	2784	34 ——
1250	56 ——	2800	40 ——
1261	73 ——	2805	43 ——
1262	74 ——	2626	5 Cygni
1254	8 Hydræ	2792	38 ——
1364	36 ——		

TABLE C.

Stars observed, but not existing.

(See page 393.)

No.	STAR.
314*	28 Arietis
639	100 Tauri
756	27 Camelopardi
864	Monocerotis
913*	21 Geminorum
1007	Geminorum
1198	Ursæ Majoris
1199	Ursæ Majoris
1205	Cancri
1212	Cancri
1220	Ursæ Majoris
1232	7 Ursæ Majoris
1486*	28 Sextantis
1647	Leonis
1686	Virginis
1910*	91 Virginis
1922	Bootis
2120	Coronæ Borealis
2335	55 Herculis
2441	65 Ophiuchi
3150	80 Aquarii
3213	3 Cassiopeæ

N.B. The existence of the stars, to which an asterisk is annexed, may be reconciled by supposing an error in recording the *minute* in the time of transit.

TABLE D.

Stars not observed, nor existing.

(See page 394.)

No.	STAR.
890	76 Orionis
1098	12 Canis Minoris
1155	17 Navis
1748	22 Virginis
1751	23 ——
1752	24 ——
1814	52 ——
1838	22 Canum Venat.
2191	42 Serpentis
2688	11 Vulpeculæ

TABLE E.

Stars having no Right Ascension.

(See page 394.)

No.	STAR.
104*	30 Cassiopeæ
596*	9 Camelopardi
691	17 ———
756	27 ———
814	35 ———
827	39 ———
1112	55 ———
1151	58 ———
1733	75 Ursæ Majoris
1797*	78 ———
1838	22 Canum Venat.
2067*	47 Bootis
2107	12 Ursæ Minoris
2133	14 ———
2220	18 ———
2606	2 Vulpeculæ
2688	11 ——
2608	29 Aquilæ
	5 Cygni
2658	9 ——
2721*	24 ——
2769	33 ——
	38 ——
2911*	68 ——
2975*	79 ——
2997	13 Cephei
3031	18 ——
3097	28 ——
3245*	104 Aquarii

N.B. The two stars in *Vulpecula* are also deficient in declination. Nos. 1838, 2688, and the two stars in *Cygnus*, to which no number is annexed, do not exist.

TABLE F.

Corrections made in the British Catalogue.

(See page 394.)

No.	STAR.	Æ.	D.	No.	STAR.	Æ.	D.
		° ′ ″	° ′ ″			° ′ ″	° ′ ″
36	52 Piscium		− 0 1 30	348	15 Persei		+ 6 6 50
39	14 Ceti		+ 3 0 0	371	24 ——	− 0 47 38	− 0 55 40
41	17 Cassiopeæ		− 0 0 40	380	50 Arietis	− 0 15 0	
65	2 Urs. Min.	− 2 15 30	− 1 59 0	396	11 Eridani	− 0 11 0	
72	60 Piscium		− 0 1 0	403	56 Arietis		+ 0 2 0
85	26 Cassiopeæ	− 0 15 0		423	15 Eridani		− 0 1 0
88	27 ———		− 0 0 40	454	38 Persei	+ 1 57 40	+ 0 14 1
90	36 Andromedæ		− 0 1 0	471	42 ——	+ 0 1 46	− 0 12 55
93	37 ———		+ 0 1 0	534	56 Tauri	− 0 15 0	
95	38 ———		− 0 1 0	536	55 ——		− 0 8 0
98	39 ———		− 0 1 0	540	60 ——	+ 0 4 10	
99	69 Piscium	+ 0 0 30	+ 0 0 15	587	48 Eridani		− 0 2 0
100	70 ———		+ 0 2 0	596	9 Camelop.	Æ inserted.	
104	30 Cassiopeæ	Æ inserted.		601	95 Tauri	− 0 10 0	
113	75 Piscium	+ 0 15 0		606	56 Eridani		− 0 5 0
118	42 Andromedæ		− 0 1 0	632	99 Tauri	− 0 9 45	+ 0 23 5
122	30 Ceti		− 0 1 20	655	103 ——	+ 0 14 12	− 0 37 55
132	83 Piscium		+ 0 1 10	662	66 Eridani	+ 0 20 0	
150	35 Cassiopeæ	− 0 15 0	− 0 20 0	691	17 Camelop.	Æ inserted.	
192	50 Ceti		− 0 0 45	720	20 ———		− 0 0 50
208	3 Arietis		+ 0 1 20	733	35 Orionis	− 0 3 40	
214	1 ———	+ 2 30 0		756	27 Camelop.		− 0 30 0
216	54 Ceti	− 0 15 0		797	34 ———		− 0 0 50
223	48 Cassiopeæ		− 0 0 45	814	35 ———	Æ inserted.	
232	52 ———		+ 0 30 0	816	38 Aurigæ		− 0 5 0
234	53 ———		− 0 30 0	827	39 Camelop.	Æ inserted.	
238	58 Ceti	− 0 45 0		833	64 Orionis	− 0 45 0	
262	5 Trianguli	− 0 15 0		843	63 ———	+ 0 34 15	
267	6 ———		− 0 1 0	893	7 Lyncis		− 1 0 0
270	63 Ceti	+ 0 15 0		894	2 Canis Maj.		− 0 1 0
278	61 Andromedæ		+ 8 18 40	902	3 ———	− 2 30 0	
303	12 Trianguli		− 0 1 0	904	77 Orionis		+ 0 4 0

No.	STAR.	Æ.	D.	No.	STAR.	Æ.	D.
		° ′ ″	° ′ ″			° ′ ″	° ′ ″
923	23 Geminorum		— 0 10 0	1582	10 Crateris		+ 0 2 40
939	57 Aurigæ	+ 0 5 0		1603	71 Leonis	+ 1 54 30	
942	56 ———	+ 0 15 0		1605	13 Crateris		+ 0 2 0
974	15 Canis Maj.		+ 0 1 0	1610	82 Leonis		— 0 5 0
977	17 ———		+ 0 1 0	1611	80 ——	+ 0 22 15	
980	19 ———		+ 0 1 0	1633	22 Crateris		+ 1 0 0
981	20 ———		+ 0 1 0	1680	3 Comæ Ber.	— 0 3 15	
982	41 Geminorum		+ 0 4 0	1691	69 Ursæ Maj.		— 0 1 0
1017	53 ———		+ 0 2 0	1724	72 ———	+ 0 15 15	
1020	20 Lyncis		— 0 1 0	1733	75 ———	Æ inserted.	
1082	82 Geminorum		+ 0 1 0	1747	25 Virginis	— 0 18 0	+ 0 1 0
1085	1 Navis		— 2 0 0	1761	76 Ursæ Maj.	+ 0 15 0	
1093	5 ——		+ 0 10 0	1797	78 ———	Æ inserted.	
1104	54 Camelop.		+ 2 0 0	1819	18 Canum Ven.	+ 0 15 0	
1107	10 Navis		— 0 2 0	1827	58 Virginis		— 0 11 20
1112	55 Camelop.	Æ inserted.		1847	68 ———		— 0 1 0
1123	7 Cancri		+ 0 1 0	1849	79 Ursæ Maj.	+ 0 29 40	
1144	57 Camelop.	+ 0 15 10		1871	25 Canum Ven.		—10 0 0
1168	21 Navis		— 0 18 0	1879	83 Virginis	+ 0 20 0	
1172	21 Cancri	+ 0 3 15	+ 0 1 35	1919	48 Hydræ	— 0 12 45	
1213	39 ——		+ 0 2 40	1920	10 Bootis		+ 0 1 0
1214	40 ——		— 0 2 40	1928	49 Hydræ	+ 0 8 45	
1217	41 ——		+ 0 1 35	1953	101 Virginis		— 1 0 0
1232	7 Ursæ Maj.	— 0 46 30		1961	51 Hydræ		+ 0 10 0
1285	71 Cancri	+ 0 8 50		1963	3 Ursæ Min.	— 0 2 30	
1292	78 ——	— 0 2 0		1974	4 ———	— 1 0 0	
1368	13 Leonis Min.	— 0 2 30		2005	54 Hydræ		+ 0 4 45
1373	37 Hydræ	— 0 8 0		2025	10 Libræ		— 0 1 0
1389	17 Leonis Min.		+ 0 10 10	2046	6 Ursæ Min.		+ 0 1 0
1415	12 Sextantis		— 1 0 0	2062	44 Bootis		+ 0 1 0
1482	26 ———		— 0 5 0	2067	47 ——	Æ inserted.	
1491	29 ———		— 1 0 0	2068	23 Libræ	+ 1 27 20	
1511	38 Ursæ Maj.	— 0 42 25		2077	8 Ursæ Min.	+ 0 15 0	— 0 50 0
1514	38 Leonis Min.	— 0 2 30		2092	7 Serpentis		+ 0 1 0
1522	40 ———		+ 0 3 0	2099	33 Libræ		— 0 10 0
1540	39 Sextantis	+ 0 2 30	— 0 5 0	2107	12 Ursæ Min.	Æ inserted.	
1547	5 Crateris		+ 0 9 15	2131	18 Serpentis		— 0 9 0
1551	48 Leonis Min.		+ 0 3 50	2133	14 Ursæ Min.	Æ inserted.	

No.	STAR.	Æ.	D.	No.	STAR.	Æ.	D.
		° ′ ″	° ′ ″			° ′ ″	° ′ ″
2139	20 Serpentis		+ 0 1 0	2497	108 Herculis		— 0 1 0
2140	7 Cor. Bor.	— 0 2 30		2501	23 Sagittarii	— 0 1 0	+ 0 40 0
2154	29 Serpentis		+ 0 2 30	2505	24 ———	+ 0 9 0	
2166	10 Cor. Ber.		— 0 5 0	2510	38 Draconis		+ 0 5 0
2179	15 Ursæ Min.	+ 0 9 45	+ 0 10 0	2514	41 ———		— 0 3 0
2190	3 Herculis	+ 0 7 30	— 0 1 0	2523	45 ———	— 0 16 0	
2203	6 ———	— 0 2 30		2551	62 Serpentis		+ 0 1 0
2213	16 Scorpii		— 0 5 0	2579	16 Lyræ		+ 9. 0 0
2220	18 Ursæ Min.	Æ inserted.	— 0 1 0	2584	17 ——		— 0 1 0
2225*	49 Serpentis	+ 0 15 0		2588	51 Draconis	+ 0 9 0	
2236	17 Herculis		— 0 2 0	2593	19 Lyræ		— 8 0 0
2253	21 Cor. Bor.	— 7 5 45	+ 0 19 15	2596	23 Aquilæ		+ 0 1 0
2254	3 Ophiuchi	+ 1 15 0		2606	2 Vulpeculæ	Æ inserted.	D. inserted.
2268	27 Herculis		+ 0 1 50	2608	29 Aquilæ	Æ inserted.	
2295	15 Ophiuchi	— 0 15 0		2627	4 Vulpeculæ		+ 0 1 0
2296	21 Ursæ Min.	+ 0 29 45		2629	3 Cygni		— 0 14 0
2325	24 Ophiuchi		+ 0 10 0	2636	8 Vulpeculæ		— 0 30 0
2342	28 ———	+ 0 15 0		2658	9 Cygni	Æ inserted.	
2344	31 ———	+ 0 15 0		2659	24 Ursæ Min.	+ 2 30 0	
2348	32 ———	+ 0 10 0		2688	11 Vulpeculæ	Æ inserted.	D. inserted.
2354	61 Herculis	— 0 30 0		2721	24 Cygni	Æ inserted.	
2365	63 ———	+ 0 15 0	— 3 0 0	2724	15 Vulpeculæ	— 0 20 0	
2370	41 Ophiuchi	— 0 30 0		2733	64 Aquilæ		— 0 2 0
2377	66 Herculis		+ 0 4 0	2769	33 Cygni	Æ inserted.	
2403	22 Ursæ Min.	+ 0 20 0		2773	36 ——		— 0 5 0
2411	57 Ophiuchi	— 0 15 0		2774	69 Draconis		+ 0 5 0
2414	79 Herculis	+ 0 3 0		2816	29 Vulpeculæ	— 0 8 0	
2421	61 Ophiuchi		— 0 1 0	2838	15 Delphini	+ 0 4 45	
2424	84 Herculis	+ 0 5 15		2873	2 Piscis Aust.	+ 0 15 0	
2425	26 Draconis		+ 0 5 0	2887	3 ———	+ 0 8 0	
2463	97 Herculis	+ 0 6 0		2911	68 Cygni	Æ inserted.	
2466	11 Sagittarii	+ 1 0 0	— 0 50 50	2912	34 Vulpeculæ	+ 2 30 0	
2470	1 ———	+10 0 0	+ 2 0 0	2927	69 Cygni	+ 0 15 0	
2472	73 Ophiuchi		+ 0 1 0	2975	79 ——	Æ inserted.	
2473	12 Sagittarii	— 1 30 0	+ 0 52 0	2994	11 Piscis Aust.		+ 0 50 0
2487	35 Draconis		— 0 5 0	2997	13 Cephei	Æ inserted.	
2489	18 Sagittarii	+ 1 0 0		3031	18 ———	Æ inserted.	
2492	105 Herculis	+ 0 15 0		3060	50 Aquarii		— 0 1 0

No.	STAR.	R.	D.	No.	STAR.	R.	D.
		° ′ ″	° ′ ″			° ′ ″	° ′
3094	64 Aquarii	+ 0 15 0		3164	87 Aquarii	— 0 5 0	— 0 1
3097	28 Cephei	R inserted.		3191	96 ———		— 0 9
3104	29 ———	+ 0 14 0		3204	97 ———		+ 0 1
3124	21 Piscis Aust.		— 0 1 0	3210	67 Pegasi		— 0 10
3125	72 Aquarii		— 3 0 0	3245	104 Aquarii	R inserted.	
3149	52 Pegasi		+ 0 1 0	3278	1 Ceti	+ 0 15 0	
3150	80 Aquarii	+ 0 30 0		3298	4 ——	— 0 15 0	
3154	1 Andromedæ		— 9 0 0	3299	5 ——	— 0 15 0	

Stars whose differences in Right Ascension *amount to about 4′ and upwards.*

(See page 395.)

N.B. The Constellations are arranged alphabetically.

No.	STAR.	Difference.	Remarks.
		′ ″	
314	28 Arietis	−15 —	Conjectural error of 1^m: if so, it is 26 *Arietis*.
405	Arietis	−30 —	Conjectural error of 2^m: else the star does not exist.
867	45 Aurigæ	− 3 45,0	Approximate time of transit assumed, instead of the correct time.
2100	51 Bootis μ	− 4 20,5	Subsequently altered by Flamsteed, or his editors, without apparent reason.
730	23 Camelopardi	+ 7 55,2	
736	24 ———	+12 —	
926	42 ———	− 4 —	Deduced partly from 11 *Ursæ Majoris*, which is itself discordant.
931	43 ———	− 7 —	
1062	49 ———	− 7 —	Time of transit doubtful.
1135	56 ———	− 7 1,8	Subsequently altered by Flamsteed, without apparent reason.
964	10 Canis Majoris	− 8 —	Differ according to the determining star assumed for the reduction.
969	13 ——— κ	+ 7 4,3	
1734	7 Canum Venat.	− 3½ —	Probably the time of transit only approximate.
1866	———	−15 —	Conjectural error of 1^m: else the star does not exist.
60	21 Cassiopeæ	− 8 29,5	The time of transit probably erroneously recorded, through the inexpertness of the assistant.
64	23 ———	−12 26,5	
231	51 ———	−10 15,6	Probably an error of 40′ in the time of transit.
306	———	−15 —	Conjectural error of 1^m: else the star does not exist.
2794	1 Cephei κ	+ 7 45,6	Probably an error in the computation.
2813	2 —— θ	+ 4 12,4	Deduced from α *Cephei* which is itself discordant.
2848	3 —— η	+ 5 1,2	
2849	4 ——	+ 6 20,2	
2923	5 —— α	+ 4 4,2	
2940	7 ——	+ 5 0,0	
2950	8 —— β	+ 4 19,4	
2965	9 ——	+ 4 59,0	
2987	11 ——	+ 4 5,8	
2989	12 ——	+ 4 57,2	
3016	14 ——	+ 4 41,0	
3025	15 ——	+ 4 22,7	
3028	16 ——	+ 4 51,7	
3030	17 —— ξ	+ 5 —	
3041	21 —— ζ	+ 3 57,2	
3045	22 —— λ	+ 3 59,9	
3049	23 —— ε	+ 5 13,5	
3050	24 ——	+ 3 50,2	
3061	25 ——	+ 4 6,7	

No.	STAR.	Difference.	Remarks.
		′ ″	
3082	26 Cephei	+ 4 34,0	Deduced from *α Cephei*, which is itself discordant.
3084	27 δ	+ 3 46,4	
3111	30 ——	+ 4 18,6	
3138	32 —— ι	+ 3 49,1	
3179	33 —— π	+ 5 34,3	
3249	35 —— γ	— 4 8,2	Deduced *per distantias*, probably erroneously.
353	Ceti	—30 —	Conjectural error of 2ᵐ: else the star does not exist.
1711	Comæ Ber.	—15 —	Conjectural error of 1ᵐ: else the star does not exist.
1626	18 Crateris	+ 3 45,4	I cannot discover how the Æ has been deduced.
1627	19 ——	+ 4 23,1	
1643	25 ——	+ 5 1,8	
1660	28 ——	+ 4 19,3	
2112	12 Draconis ι	— 9 —	Deduced *per distantias*.
2298	15 —— A	+ 5 —	
2301	16 ——	— 9 0,5	"Tempora transituum vitiosissime notata."
2302	17 ——	— 8 58,5	
2412	23 —— β	— 4 9.2	Deduced *per distantias*.
2458	31 —— ψ¹	— 5 6,1	Probably the error of the determining stars, viz. *α Lyræ* and *ν³ Draconis*.
2459	——	— 5 7,7	
2432	28 —— ω	+ 5 25,8	Probably all depending on the same error, viz. that of the determining star, 28 *Draconis*.
2487	35 ——	+10 45,9	
2513	40 ——	+ 6 16,2	
2514	41 ——	+ 6 0,8	
2516	42 ——	+ 6 26,1	
2520	44 —— χ	+ 5 47,6	
2583	50 ——	+18 35,7	I cannot discover how the Æ has been deduced.
2744	64 —— e	— 4 34,7	Partly the error of the determining star, viz. *δ Draconis*.
2774	69 ——	+ 9 56,5	Error of the determining star, *κ Cephei*.
2870	75 ——	— 4 37,6	Partly the error of the determining star, viz. *ν³ Draconis*.
913	21 Geminorum	—15 —	Conjectural error of 1ᵐ: if so, it is 20 *Geminorum*.
2284	34 Herculis	— 5 —	Evidently some error in the computation.
2454	93 Herculis	+ 4 6,9	Subsequently altered by Flamsteed, or his editors, without apparent reason.
2460	95 ——	+ 3 33,2	
2462	96 ——	+ 4 14,8	I cannot account for this difference.
2476	101 ——	+ 4 42,5	Evidently some error in the computation.
579	66 Leonis	+25 —	Deduced *per distantias*: probably an error of computation.
594	2 Leonis Minoris	— 5 24,9	Probably some error in the clock, or determining star; as they were all observed on the same day.
596	3 ——	— 5 9,0	
598	4 ——	— 5 24,4	
602	5 ——	— 5 10,3	

No.	STAR.	Difference.	Remarks.
1368	13 Leonis Minoris	— 4 —	Probably the time of transit only approximate.
1381	15 ————	— 5 17,5	I cannot answer for this difference.
855	3 Lyncis	— 5 —	2′ of this is Flamsteed's error.
933	13 ——	—15 —	Conjectural error of 1^m.
979	17 ——	— 4 55,0	Probably an error in the assumed rate of the clock.
994	——	—20 —	Time of transit doubtful.
1019	19 ——	— 7 —	Error of the determining star, viz. 11 *Ursæ Majoris*.
1127	28 ——	—20 —	Time of transit doubtful.
1311	39 ——	— 3½ —	1′ of this is the error of the determining star, viz. 21 *Lyncis*.
2411	57 Ophiuchi μ	—15 —	Conjectural error of 1^m: else the star does not exist.
2978	12 Pegasi	+ 5 —	I cannot account for these differences.
256	5 Persei h	— 4 25,8	
275	8 ——	— 4 54,0	Probably an error in recording the observation.
4	Piscium	—15 —	Conjectural error of 1^m: else the stars do not exist.
3258	———	+15 —	
2225	49 Serpentis	+15 —	
1388	3 Sextantis	+ 4 11,5	See the note to this star.
1394	5 ———	+ 4 —	I cannot account for this difference.
1486	28 ———	+30 —	Conjectural error of 2^m: if so, it is 29 *Sextantis*.
200	1 Trianguli	+ 8 —	Aproximate time of transit assumed, instead of the correct time.
313	14 ———	— 6 38,9	Probably the time of transit only approximate.
1821	55 Virginis	— 8 —	The time of transit not recorded.
1910	91 ———	+30 —	Conjectural error of 2^m: if so, it is 92 *Virginis*.
1190	Ursæ Majoris	—15 —	Conjectural error of 1^m: else the star does not exist.
1266	11 ———— σ^1	— 9 16,0	The right ascension deduced *per distantias*.
1320	22 ————	— 9½ —	Deduced partly from 11 *Ursæ Majoris*, which is itself discordant.
1325	24 ———— d	— 6 40,6	
1340	27 ————	— 9 10,8	
1414	————	+ 8 —	Conjectural error of 30′: else the star does not exist.
1511	38 ————	— 4 25,0	The two results discordant.
1878	82 ————	— 5 —	The time of transit not recorded.
80	1 Ursæ Min. α	+11 4,8	No very great accordance expected.
1963	3 ————	— 10 —	All observed on the same day.
2079	9 ————	— 6 —	
2094	10 ————	—10 —	
2238	17 ————	— 5 —	
2625	23 ———— δ	—12 15,0	Deduced *per distantias*.
2659	24 ————	—13 30,3	Error of the determining star, viz. 23 *Ursæ Minoris*.

TABLE H.

Stars whose differences in Declination *amount to* 1′ *and upwards.*

(See page 395.)

N.B. The Stars are arranged according to their North Polar distance.

No.	STAR.	Difference.	Remarks.
2215	16 Ursæ Min. ζ	+ 1′ 3″,6	I cannot account for these differences.
2179	15 ——— θ	+ 1 33,3	
167	38 Cassiopeæ	+ 1 51,4	
2112	12 Draconis ι	+ 1 56,1	Determined *per distantias*: probably an erroneous star taken.
2214	13 ——— θ	+ 1 43,2	Determined *per distantias*.
1297	18 Ursæ Maj. e	— 1 17,8	I cannot account for this difference.
2649	8 Cygni	— 1 3,6	The two observations differ nearly 1′.
1225	46 Cancri	+ 1 10,4	I cannot account for this difference.
1212	Cancri f	—10 —	Conjectural error in the original entry.
54	55 Piscium	— 1 14,4	The reduction confused with that of 56 *Piscium*.
271	18 Arietis	— 6 —	Conjectural errors in the original entries, else the stars do not exist.
642	101 Tauri	— 9 —	
611	96 ——	— 5 —	
148	38 ——	— 1 8,4	I cannot account for these differences.
1353	33 Hydræ	+ 1 0,6	
1808	50 Virginis	+ 1 15,3	
1933	96 ———	+ 1 7,5	
2413	56 Serpentis	— 1 5,7	
3039	40 Aquarii	— 1 21,7	
3132	75 ———	— 1 9,0	
3047	45 ———	— 1 18,4	
1075	Canis Majoris	— 5 —	Conjectural error in the original entry.
2338	29 Ophiuchi	— 1 7,3	The greater part of these differences arises from the difference in the tables of refraction.
2333	26 ———	— 1 3,6	
1570	8 Crateris	— 1 7,9	
3038	16 Piscis Aust.	— 1 29,6	
1646	26 Crateris	— 1 3,9	
3003	13 Piscis Aust.	— 1 33,4	
2149	5 Lupi λ	— 1 29,2	
1643	25 Crateris	— 1 11,1	
3076	17 Piscis Aust. β	— 1 43,4	
3018	15 ———	— 1 5,3	
2957	9 ——— ι	— 1 2,6	
3013	14 ——— μ	— 1 33,4	
3127	22 ——— γ	— 1 5,3	
2928	6 ———	— 1 55,8	

(See page 404.)

STAR.	F.	B.	STAR.	F.	B.	STAR.	F.	B.	STAR.	F.	B.
46 Andromedæ	nil	ξ	13 Canis Maj.	κ^2	κ	52 Eridani	υ^2	υ^7	45 Orionis	c^2	nil
48 ———	nil	ω	1 Capricorni	nil	ξ^1	67 ———	h	β	62 ———	χ^3	χ^4
49 ———	ξ	A	2 ———	ξ	ξ^2	1 Geminorum	H	nil	64 ———	χ^4	χ^3
50 ———	υ	nil	13 ———	nil	τ^1	30 ———	ξ^1	nil	27 Pegasi	nil	π^1
51 ———	υ	nil	14 ———	τ	τ^2	31 ———	ξ^2	ξ	29 ——	π	π^2
52 ———	λ	χ	25 ———	χ^1	χ	42 ———	ω^1	ω	5 Persei	nil	h
54 ———	φ	nil	26 ———	χ^2	nil	44 ———	ω^2	nil	6 ——	h	nil
37 Aquarii	nil	e^1	27 ———	χ^3	nil	62 ———	s	ρ	15 ——	nil	η
38 ———	e	e^2	34 Cassiopeæ	nil	φ	13 Herculis	nil	p	31 ——	nil	ι
102 ———	nil	ω^1	38 ———	nil	A	28 ———	nil	n	8 Piscium	κ^1	κ
106 ———	A^3	i^1	43 ———	c	ω	45 ———	e	l	9 ———	κ^2	nil
107 ———	A^4	i^2	10 Cephei	nil	ν	66 ———	ω	nil	65 ———	ι	i
108 ———	A^5	nil	15 ———	ν	nil	77 ———	κ	x	15 Sagittarii	μ^2	nil
25 Aquilæ	ω^1	ω	56 Ceti	υ^1	nil	100 ———	i	nil	2 Scorpii	A^1	A
29 ———	ω^3	nil	59 ——	υ^2	υ	61 Leonis	nil	p^1	3 ———	A^2	nil
22 Arietis	θ^1	θ	3 Crateris	b^1	nil	62 ——	g	p^2	15 ———	χ	ψ
23 ———	θ^2	nil	4 ———	ν	nil	65 ——	nil	p^3	17 ———	nil	χ
6 Bootis	nil	e	5 ———	b^2	nil	66 ——	nil	p^4	35 ———	nil	λ
10 ———	e	nil	6 ———	b^3	nil	69 ——	nil	p^5	9 Serpentis	nil	τ^1
33 ———	h^1	nil	8 ———	ι	nil	20 Libræ	γ	nil	12 ———	τ^1	τ^2
38 ———	h^2	h	9 ———	χ	nil	51 ——	ξ	nil	15 ———	nil	τ^3
44 ———	nil	i	19 ———	ξ	nil	1 Lyræ	x	κ	17 ———	nil	τ^4
47 ———	nil	k	25 ———	o	nil	1 Navis	nil	σ	18 ———	τ^2	τ^5
6 Cancri	χ	nil	28 ———	β	nil	5 Ophiuchi	g	ρ	19 ———	τ^3	τ^6
15 ———	ψ^3	nil	7 Cygni	nil	ι^1	40 ———	ρ	ξ	22 ———	nil	τ^7
32 ———	υ^4	nil	10 ———	ι^1	ι^2	45 ———	nil	d	26 ———	nil	τ^8
42 ———	c	nil	16 ———	c^1	c	50 ———	e^1	c^1	37 Tauri	A	A^1
46 ———	nil	σ^1	20 ———	nil	d	51 ———	e^2	c^2	39 ——	nil	A^2
51 ———	σ^1	nil	26 ———	c^2	e	53 ———	nil	f	57 ——	nil	h
57 ———	ι^2	σ^2	78 ———	nil	μ	58 ———	D	nil	58 ——	h	nil
58 ———	ρ^4	ρ^3	19 Draconis	nil	h^1	1 Orionis	nil	π^1	106 ——	l^1	l
59 ———	σ^2	nil	20 ———	h	h^2	2 ———	π^1	π^2	107 ——	l^2	nil
60 ———	α^1	nil	11 Eridani	nil	τ^3	3 ———	nil	π^3	3 Ursæ Maj.	π^1	nil
61 ———	nil	σ^3	16 ———	nil	τ^4	7 ———	π^2	π^4	4 ———	π^2	π
64 ———	σ^3	nil	19 ———	nil	τ^5	8 ———	z	π^5	5 ———	nil	b
65 ———	α^2	α	27 ———	nil	τ^6	10 ———	nil	π^6	7 ———	b	nil
66 ———	σ^4	nil	28 ———	nil	τ^7	17 ———	ρ^1	ρ	4 Virginis	ξ	A^1
67 ———	ρ^5	nil	33 ———	nil	τ^8	22 ———	nil	o	6 ———	A	A^2
70 ———	ρ^6	nil	36 ———	nil	τ^9	27 ———	ρ^2	p	13 ———	n	nil
81 ———	π	π^1	41 ———	nil	υ^4	33 ———	n	n^1	68 ———	ι	i
82 ———	nil	π^2	43 ———	nil	υ^5	38 ———	nil	n^2	80 ———	l^3	nil
3 Canis Maj.	λ	nil	50 ———	υ^1	υ^6	42 ———	c^1	c	88 ———	nil	n
10 ———	κ^1	nil									

TABLE K.

List of 345 *Determining Stars.*

(See page 407.)

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
8 Ceti	ι	3	0 54 25,1	+45,91	0,00	− 10 32 42,8	+20,06	− 0,02
17 Cassiopeæ	ζ	4	4 59 16,5	48,21	+ 0,15	+ 52 11 3,2	19,98	+ 0,01
29 Andromedæ	π	4½	5 6 43,7	47,09	+ 0,10	+ 32 0 21,4	19,98	+ 0,03
18 Cassiopeæ	α	3	5 48 3,4	48,86	+ 0,20	+ 54 49 49,7	19,96	− 0,03
16 Ceti	β	3	7 0 5,4	45,10	+ 0,28	− 19 41 44,2	19,91	+ 0,07
63 Piscium	δ	4	8 9 37,5	46,27	+ 0,14	+ 5 53 20,4	19,86	− 0,01
1 Ursæ Min.	α	2½	8 28 34,8	117,74		+ 87 38 27,4	19,76	
20 Ceti		6	9 17 48,6	45,82	+ 0,04	− 2 50 12,5	19,80	+ 0,01
71 Piscium	ε	4	11 43 18,1	46,42	+ 0,06	+ 6 12 40,9	19,64	+ 0,02
80 Piscium	e	5	13 6 42,3	46,29	− 0,27	+ 3 59 56,5	19,54	− 0,19
86 Piscium	ζ	5	14 23 42,1	46,49	+ 0,23	+ 5 55 27,8	19,43	− 0,06
36 Cassiopeæ	ψ	5½	16 10 6,2	58,79	+ 0,35	+ 66 29 38,2	19,27	+ 0,01
37 Cassiopeæ	δ	3	16 29 7,1	55,29	+ 0,73	+ 58 36 24,6	19,24	− 0,06
45 Ceti	θ	3	17 8 3,8	44,96	− 0,02	− 9 47 49,4	19,17	− 0,19
98 Piscium	μ	5	18 29 50,5	46,48	+ 0,35	+ 4 32 9,0	19,02	− 0,17
99 Piscium	η	4	18 44 31,4	47,55	+ 0,10	+ 13 43 53,1	19,00	+ 0,02
102 Piscium	π	5	20 10 57,7	47,26	− 0,02	+ 10 32 22,7	18,83	+ 0,05
106 Piscium	ν	5	21 20 9,8	46,47	+ 0,05	+ 3 54 10,3	18,69	− 0,01
110 Piscium	ο	5	22 16 8,3	46,99	+ 0,16	+ 7 34 51,4	18,56	+ 0,03
5 Arietis	γ	4	24 9 9,3	48,60	+ 0,14	+ 17 45 6,9	18,30	+ 0,02
6 Arietis	β	3	24 24 13,1	48,87	+ 0,15	+ 19 16 20,6	18,27	− 0,10
8 Arietis	ι	6	25 7 26,7	48,47	+ 0,14	+ 16 17 3,0	18,16	− 0,02
57 Andromedæ	γ	2½	26 16 13,7	53,64	+ 0,09	+ 40 49 7,0	17,99	− 0,06
113 Piscium	α	3	26 30 38,3	46,17	+ 0,14	+ 1 14 48,9	17,95	− 0,01
13 Arietis	α	2	27 27 3,5	49,71	+ 0.28	+ 21 58 25,3	17,80	− 0,14
65 Ceti	ξ^1	6	29 9 33,5	47,24	− 0,03	+ 7 22 15,8	17,52	− 0,02
22 Arietis	θ	5¾	30 14 35,7	49,34	+ 0,08	+ 18 26 34,2	17,33	+ 0,01
73 Ceti	ξ^2	4¾	32 56 3,7	47,32	+ 0,10	+ 7 2 46,2	16,84	− 0,01
82 Ceti	δ	3	35 54 25,2	45,76	+ 0,12	− 1 1 58,7	16,25	− 0,03
86 Ceti	γ	3	36 49 15,8	+46,38	− 0,06	+ 1 54 15,0	+16,06	− 0,17

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
87 Ceti	μ	4	37 3 46,9	+47,84	+ 0,33	+ 8 46 42,7	+15,98	— 0,05
42 Arietis	π	6	38 1 9,3	49,55	+ 0,08	+ 16 8 41,5	15,82	+ 0,01
46 Arietis	ρ^3	6½	39 45 17,9	49,84	+ 0,37	+ 16 45 21,0	15,42	— 0,20
48 Arietis	ε	5	40 23 38,7	50,73	+ 0,09	+ 20 4 20,5	15,15	— 0,02
92 Ceti	α	2	41 31 54,1	46,64	+ 0,04	+ 2 50 45,4	15,02	— 0,11
26 Persei	β	2½	42 2 17,7	57,14	+ 0,53	+ 39 43 27,2	14,90	+ 0,01
57 Arietis	δ	4	43 29 48,2	50,60	+ 0,25	+ 18 31 14,6	14,55	0,00
58 Arietis	ζ	5	44 17 36,2	51,04	+ 0,05	+ 19 51 44,3	14,36	— 0,08
33 Persei	α	2½	45 36 36,0	62,30	+ 0,17	+ 48 42 58,9	14,04	— 0,07
63 Arietis	τ^3	6	46 15 6,1	51,13	0,00	+ 19 35 55,5	13,87	— 0,03
1 Tauri	o	4	47 2 47,7	48,02	+ 0,03	+ 7 54 19,6	13,67	— 0,09
2 Tauri	ξ	4	47 36 26,9	48,22	+ 0,12	+ 8 37 9,5	13,53	— 0,05
5 Tauri	f	5	48 27 20,6	49,13	+ 0,10	+ 11 50 28,5	13,30	— 0,03
39 Persei	δ	3	50 16 2,0	62,38	+ 0,16	+ 46 45 6,3	12,83	— 0,07
17 Tauri		5	51 38 25,6	52,69	+ 0,06	+ 23 6 2,6	12,45	— 0,06
19 Tauri		5	51 42 46,1	52,81	+ 0,08	+ 23 27 22,5	12,43	— 0,06
20 Tauri		6	51 52 6,5	52,79	+ 0,08	+ 23 21 39,5	12,39	— 0,08
23 Tauri		5	52 0 10,8	52,67	+ 0,12	+ 22 56 40,6	12,35	— 0,07
25 Tauri	η	3	52 17 15,8	52,75	+ 0,07	+ 23 6 30,7	12,27	— 0,07
27 Tauri		6	52 42 19,7	52,77	+ 0,07	+ 23 4 4,1	12,16	— 0,09
44 Persei	ζ	3	53 41 21,8	55,66	+ 0,09	+ 30 55 22,5	11,89	— 0,07
45 Persei	ε	3	54 18 10,1	59,16	+ 0,03	+ 39 4 8,6	11,71	— 0,04
34 Eridani	γ	2	55 53 52,4	41,71	+ 0,10	— 14 25 19,2	11,25	— 0,11
54 Tauri	γ	3	60 33 7,3	50,61	+ 0,20	+ 14 50 19,4	9,86	— 0,03
61 Tauri	δ^1	4	61 16 49,5	51,28	+ 0,16	+ 16 46 28,1	9,64	— 0,05
64 Tauri	δ^2	4	61 34 16,8	51,26	+ 0,17	+ 16 41 4,1	9,55	— 0,05
68 Tauri	δ^3	4½	61 54 11,2	51,45	+ 0,19	+ 17 10 37,5	9,45	— 0,03
74 Tauri	ε	3½	62 38 38,5	51,92	+ 0,18	+ 18 27 1,1	9,22	— 0,04
77 Tauri	θ^1	5	62 44 4,8	50,83	+ 0,05	+ 15 13 56,9	9,19	— 0,03
78 Tauri	θ^2	5	62 45 13,3	50,80	+ 0,20	+ 15 8 28,8	9,18	— 0,02
87 Tauri	α	1	64 32 47,2	51,12	+ 0,14	+ 15 50 35,6	8,62	— 0,18
94 Tauri	τ	5	65 55 30,5	53,50	+ 0,06	+ 22 19 5,4	8,19	— 0,04
3 Aurigæ	ι	4	69 15 8,0	57,99	— 0,75	+ 32 37 33,4	7,12	— 0,03
8 Orionis	π^5	4	69 32 1,9	46,62	+ 0,05	+ 1 53 40,5	7,01	— 0,03
102 Tauri	ι	4	71 9 12,1	+53,31	+ 0,14	+ 21 6 8,1	+ 6,48	— 0,07

STAR.	Mag.	Right Ascension.			Declination.		
		Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
		° ′ ″	″	″	° ′ ″	″	″
13 Aurigæ α	1	73 28 4,6	+65,65	+ 0,21	+ 45 37 33,3	+ 5,71	— 0,45
19 Orionis β	1	74 54 53,1	43,05	+ 0,06	— 8 35 46,9	5,22	— 0,03
112 Tauri β	2	76 40 57,3	56,49	+ 0,14	+ 28 17 50,0	4,62	— 0,21
24 Orionis γ	2	77 7 49,5	48,04	+ 0,09	+ 6 1 39,1	4,47	— 0,04
114 Tauri o	5	77 15 42,6	53,74	+ 0,08	+ 21 37 30,5	4,42	— 0,02
34 Orionis δ	2	79 2 45,0	45,79	+ 0,10	— 0 34 1,3	3,81	— 0,06
11 Leporis α	3	79 46 0,2	39,53	+ 0,09	— 18 4 43,6	3,56	— 0,01
123 Tauri ζ	3	79 47 8,7	53,52	+ 0,07	+ 20 54 26,0	3,55	— 0,05
46 Orionis ε	2	80 7 31,8	45,48	+ 0,05	— 1 26 21,5	3,44	— 0,03
48 Orionis σ	4	80 48 2,8	45,00	+ 0,05	— 2 49 5,5	3,21	— 0,03
50 Orionis ζ	2	81 16 53,2	45,24	+ 0,11	— 2 8 46,2	3,04	— 0,02
31 Camelopardi	5	81 49 2,4	80,04	0,00	+ 59 44 53,4	2,86	— 0,09
54 Orionis χ^1	5	84 0 40,6	53,31	— 0,15	+ 20 10 21,5	2,09	— 0,12
58 Orionis α	1	84 36 0,8	48,54	+ 0,10	+ 7 18 24,2	1,89	— 0,02
37 Aurigæ θ	4	84 39 8,6	61,11	+ 0,15	+ 37 8 22,6	1,87	— 0,17
1 Geminorum	5	86 19 18,7	54,58	+ 0,05	+ 23 13 55,9	1,29	— 0,14
2 Lyncis	4	88 3 34,9	79,43	+ 0,13	+ 59 3 15,0	0,68	— 0,05
7 Geminorum η	4½	89 2 29,9	54,31	+ 0,01	+ 22 32 57,3	+ 0,33	— 0,04
13 Geminorum μ	3	91 2 58,3	54,34	+ 0,16	+ 22 37 32,5	— 0,37	— 0,15
1 Canis Maj. ζ	3	92 6 18,7	34,43	+ 0,07	— 29 57 17,2	0,74	— 0,04
18 Geminorum ν	4	92 38 17,2	53,41	+ 0,05	+ 20 21 42,1	0,92	— 0,04
24 Geminorum γ	2½	94 56 57,6	51,95	+ 0,09	+ 16 37 8,0	1,73	— 0,07
27 Geminorum ε	3	96 12 34,1	55,44	+ 0,08	+ 25 23 24,5	2,17	— 0,04
31 Geminorum ξ	4¾	96 58 11,7	50,65	— 0,06	+ 13 11 12,4	2,44	— 0,23
9 Canis Maj. α	1	97 52 16,6	40,16	— 0,50	— 16 19 28,1	2,75	— 1,25
43 Geminorum ζ	3½	101 25 24,0	53,52	+ 0,07	+ 20 58 49,9	4,17	— 0,05
54 Geminorum λ	5	105 3 41,6	51,92	+ 0,01	+ 17 3 22,8	5,21	— 0,05
55 Geminorum δ	3	105 23 27,6	54,00	+ 0,07	+ 22 30 31,0	5,33	— 0,05
3 Canis Min. β	3	107 34 46,8	48,97	— 0,01	+ 8 52 28,5	6,06	— 0,07
66 Geminorum α	1	108 41 8,3	58,09	— 0,13	+ 32 31 9,7	6,43	— 0,09
10 Canis Min. α	1½	110 45 41,7	47,94	— 0,62	+ 5 58 57,6	7,11	— 1,07
77 Geminorum $\varkappa$	4½	111 25 11,2	54,73	— 0,02	+ 25 5 52,1	7,33	— 0,07
78 Geminorum β	2	111 34 13,5	56,93	— 0,67	+ 28 43 53,5	7,38	— 0,08
81 Geminorum g	6	112 1 58,0	52,46	— 0,03	+ 19 13 26,9	7,53	— 0,06
9 Cancri μ^1	7	116 58 12,2	+53,75	0,00	+ 23 28 49,1	— 9,09	— 0,04

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
17 Cancri	β	$3\frac{3}{4}$	119 55 0,7	+49,08	0,00	+ 10 6 18,4	−10,01	− 0,07
1 Ursæ Maj.	o	$4\frac{1}{2}$	121 1 30,8	77,91	− 0,26	+ 61 42 7,7	10,35	− 0,20
31 Cancri	θ	$5\frac{3}{4}$	123 27 54,0	51,78	− 0,02	+ 19 6 23,2	11,06	− 0,09
33 Cancri	η	$6\frac{1}{2}$	123 40 43,3	52,54	− 0,01	+ 21 27 29,7	11,13	− 0,08
43 Cancri	γ	4	126 19 9,3	52,69	− 0,10	+ 22 32 45,4	11,88	0,00
47 Cancri	δ	4	126 45 0,8	51,60	+ 0,04	+ 19 15 35,7	12,00	− 0,26
13 Hydræ	ρ	5	127 59 41,4	47,91	+ 0,02	+ 6 57 7,8	12,35	− 0,08
9 Ursæ Maj.	ι	4	129 26 16,2	63,94	− 0,66	+ 49 13 20,1	12,75	− 0,34
60 Cancri		6	129 44 16,1	49,48	+ 0,01	+ 12 46 45,6	12,82	− 0,04
16 Hydræ	ζ	4	129 44 28,8	47,90	− 0,02	+ 7 5 45,7	12,82	− 0,04
65 Cancri	α	4	130 22 15,8	49,51	+ 0,06	+ 13 1 36,8	12,99	− 0,05
12 Ursæ Maj.	$\varkappa$	4	130 33 34,4	63,11	− 0,07	+ 48 20 46,4	13,05	− 0,15
76 Cancri	$\varkappa$	$6\frac{1}{2}$	132 43 36,9	49,08	+ 0,02	+ 11 53 8,5	13,61	− 0,02
1 Leonis	$\varkappa$	5	136 37 34,2	53,14	− 0,04	+ 27 29 13,9	14,58	− 0,07
30 Hydræ	α	2	138 5 14,4	44,25	+ 0,03	− 7 20 21,9	14,93	+ 0,03
4 Leonis	λ	4	138 29 13,3	51,98	0,00	+ 24 18 24,7	15,02	− 0,04
5 Leonis	ξ	$5\frac{1}{2}$	138 47 50,8	48,94	− 0,08	+ 12 38 47,0	15,09	− 0,09
10 Leonis		5	140 12 9,5	47,83	− 0,05	+ 8 12 2,3	15,41	− 0,01
14 Leonis	o	$3\frac{1}{2}$	141 8 26,5	48,49	− 0,13	+ 11 16 41,6	15,62	− 0,06
24 Leonis	μ	$3\frac{1}{2}$	143 45 28,0	52,13	− 0,29	+ 27 26 34,1	16,18	− 0,07
27 Leonis	ν	$5\frac{1}{2}$	145 22 23,3	48,79	− 0,08	+ 13 54 5,0	16,51	− 0,02
29 Leonis	π	4	145 56 49,1	47,86	+ 0,03	+ 9 30 37,2	16,61	− 0,05
30 Leonis	η	$3\frac{1}{2}$	147 35 29,0	49,52	+ 0,02	+ 18 15 12,0	16,94	− 0,01
32 Leonis	α	1	147 57 12,4	48,52	− 0,23	+ 13 27 41,7	17,00	+ 0,02
33 Ursæ Maj.	λ	$3\frac{1}{2}$	149 32 59,1	55,94	− 0,20	+ 44 26 29,2	17,30	− 0,07
36 Leonis	ζ	3	149 50 25,8	50,66	+ 0,03	+ 24 56 26,9	17,35	+ 0,01
41 Leonis	γ	2	150 42 8,7	49,82	+ 0,34	+ 21 23 24,0	17,50	− 0,15
34 Ursæ Maj.	μ	3	150 54 59,7	55,07	− 0,05	+ 43 2 19,0	17,53	+ 0,01
47 Leonis	ρ	4	154 6 38,5	47,68	+ 0,07	+ 10 53 11,5	18,05	− 0,03
48 Ursæ Maj.	β	2	160 41 59,5	56,58	+ 0,14	+ 58 1 53,4	18,92	+ 0,01
50 Ursæ Maj.	α	$1\frac{1}{2}$	161 2 25,7	58,97	− 0,29	+ 63 24 46,1	18,98	− 0,10
58 Leonis	d	$5\frac{3}{4}$	161 8 1,1	46,57	+ 0,05	+ 5 16 16,4	18,98	− 0,03
63 Leonis	χ	$4\frac{1}{2}$	162 14 56,8	46,94	− 0,29	+ 9 0 9,7	19,12	− 0,08
68 Leonis	δ	$2\frac{1}{2}$	164 23 12,5	48,18	+ 0,23	+ 22 12 49,6	19,32	− 0,14
70 Leonis	θ	3	164 28 48,0	+47,63	0,00	+ 17 6 51,2	−19,33	− 0,06

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
77 Leonis	σ	4½	166 16 59,7	+46,62	— 0,06	+ 7 43 16,1	—19,49	— 0,02
78 Leonis	ι	4	166 55 58,2	46,96	+ 0,21	+ 12 13 46,3	19,54	— 0,08
84 Leonis	τ	4	167 59 54,5	46,31	— 0,06	+ 4 33 28,1	19,62	— 0,03
91 Leonis	υ	4	170 16 12,6	46,03	+ 0,06	+ 0 53 0,5	19,77	+ 0,04
2 Virginis	ξ	5	172 19 20,5	46,45	+ 0,07	+ 9 58 41,7	19,88	— 0,03
3 Virginis	ν	5	172 28 30,4	46,36	+ 0,09	+ 8 15 51,8	19,90	— 0,21
94 Leonis	β	1½	173 18 11,2	46,66	— 0,48	+ 16 18 6,3	19,92	— 0,09
5 Virginis	β	3	173 38 9,3	46,11	+ 0,83	+ 3 30 35,5	19,94	— 0,28
64 Ursæ Maj.	γ	2	174 19 22,8	48,84	+ 0,18	+ 55 25 2,7	19,96	— 0,01
8 Virginis	π	5	176 14 40,5	46,17	+ 0,04	+ 8 20 34,1	20,02	— 0,04
9 Virginis	o	5	177 20 57,6	46,08	— 0,14	+ 10 27 22,8	20,04	+ 0,03
1 Corvi	α	4	178 7 35,3	45,70	+ 0,16	— 22 59 53,0	20,05	— 0,04
69 Ursæ Maj.	δ	2½	179 57 45,5	45,98	+ 0,23	+ 58 45 34,3	20,06	— 0,05
13 Virginis		6	180 41 4,2	45,97	+ 0,86	+ 0 56 22,5	20,06	— 0,03
15 Virginis	η	3	181 0 56,7	45,97	— 0,05	+ 1 3 32,8	20,06	— 0,02
7 Corvi	δ	3	183 27 57,0	46,30	— 0,02	— 14 47 4,3	20,02	— 0,13
26 Virginis	χ	5	185 49 16,3	46,20	— 0,01	— 6 16 57,0	19,96	— 0,03
29 Virginis	γ	3	186 29 34,6	45,96	— 0,50	+ 0 15 34,5	19,93	— 0,04
40 Virginis	ψ	5	189 34 5,8	46,43	+ 0,10	— 7 50 43,8	19,78	— 0,02
43 Virginis	δ	3	189 59 59,5	45,66	— 0,40	+ 5 5 30,4	19,75	— 0,06
77 Ursæ Maj.	ε	3	190 3 38,3	40,44	+ 0,23	+ 57 39 9,1	19,75	— 0,06
12 Canum Ven.		2½	190 21 44,3	42,94	— 0,27	+ 40 0 3,9	19,73	+ 0,06
36 Comæ Ber.		5	190 53 33,3	44,66	+ 0,02	+ 19 5 21,6	19,70	+ 0,06
47 Virginis	ε	3	191 41 4,2	45,06	— 0,19	+ 12 38 6,2	19,64	+ 0,04
49 Virginis	g	5	192 55 35,5	46,69	+ 0,11	— 9 4 18,4	19,55	+ 0,01
51 Virginis	b	4	193 29 4,7	46,29	+ 0,05	— 3 52 20,5	19,51	— 0,02
46 Hydræ	γ	3½	195 32 28,5	48,09	+ 0,16	— 21 31 27,7	19,33	+ 0,07
67 Virginis	α	1	197 13 51,5	46,97	+ 0.01	— 9 31 43,8	19,16	— 0,02
79 Ursæ Maj.	ζ	3	197 50 10,4	36,67	+ 0,27	+ 56 33 21,3	19,09	— 0,03
74 Virginis	l^n	6	198 58 21,5	46,50	— 0,02	— 4 38 26,2	18,97	— 0,02
79 Virginis	ζ	4	199 43 54,0	45,86	— 0,20	+ 1 0 11,3	18,88	+ 0,09
82 Virginis	m	6	201 20 58,2	46,89	— 0,02	— 7 7 13,9	18,68	+ 0,03
85 Ursæ Maj.	η	3	203 49 1,5	36,02	— 0,14	+ 50 52 30,1	18,35	— 0,01
8 Bootis	η	3	204 58 44,6	42,90	+ 0,03	+ 19 58 7,8	18,18	— 0,33
93 Virginis	τ	5	206 28 33,3	+45,50	+ 0,07	+ 3 3 59,4	—17,95	— 0,05

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
11 Draconis	α	3½	209 0 23,9	+24,27	— 0,12	+ 65 52 11,4	—17,54	— 0,01
98 Virginis	$\varkappa$	4	209 6 13,7	47,49	+ 0,13	— 8 48 14,6	17,53	+ 0,06
99 Virginis	ι	4	209 57 5,0	46,76	+ 0,11	— 4 30 1,0	17,38	— 0,38
16 Bootis	α	1	210 23 1,6	42,12	— 1,14	+ 20 48 53,3	17,30	— 1,95
100 Virginis	λ	4	210 36 2,7	48,13	+ 0,11	— 11 55 15,9	17,26	+ 0,07
25 Bootis	ρ	4	214 36 54,6	38,92	— 0,07	+ 31 45 7,2	16,51	+ 0,16
107 Virginis	μ	4	216 41 36,8	46,87	+ 0,20	— 4 17 3,2	16,09	— 0,28
36 Bootis	ε	3	217 51 44,9	39,32	— 0,02	+ 28 24 14,8	15,83	+ 0,03
7 Libræ	μ	5	218 5 59,6	48,80	+ 0,02	— 12 49 46,3	15,79	+ 0,03
8 Libræ		6	218 24 18,9	49,24	— 0,03	— 14 40 47,3	15,72	— 0,03
9 Libræ	α	2	218 27 4,4	49,25	0,00	— 14 43 32,6	15,71	— 0,01
15 Libræ	ξ^2	6	220 0 16,1	48,28	+ 0,07	— 10 7 43,9	15,36	+ 0,03
19 Libræ	δ	4½	221 7 4,2	47,66	0,00	— 7 15 32,6	15,11	+ 0,02
20 Libræ		3	221 30 28,1	51,90	+ 0,06	— 24 2 0,8	15,00	+ 0,06
21 Libræ	ν^1	5	222 21 12,3	49,60	+ 0,07	— 15 1 26,0	14,82	+ 0,03
42 Bootis	β	3	222 34 0,6	33,92	0,00	+ 41 38 7,0	14,77	— 0,03
43 Bootis	ψ	5	222 47 31,8	+38,68	— 0,16	+ 28 10 48,9	14,72	+ 0,02
7 Ursæ Min.	β	3	223 2 21,4	— 6,62	+ 0,01	+ 75 25 23,4	14,66	— 0,05
24 Libræ	ι^1	5	223 39 32,3	+50,63	+ 0,07	— 18 35 11,6	14,51	+ 0,02
27 Libræ	β	2	225 5 48,2	48,03	— 0,03	— 8 12 28,7	14,26	+ 0,02
48 Bootis	χ	5	225 23 4,0	37,62	— 0,08	+ 30 20 19,4	14,09	+ 0,06
38 Libræ	γ	3½	229 33 51,5	49,71	+ 0,13	— 13 43 18,5	13,01	+ 0,08
13 Serpentis	δ	3	230 0 16,3	42,82	+ 0,01	+ 11 36 11,9	12,89	+ 0,08
5 Cor. Bor.	α	2½	230 23 38,2	37,84	+ 0,18	+ 27 47 0,8	12,79	— 0,04
43 Libræ	$\varkappa$	4	231 2 29,4	51,24	+ 0,05	— 18 38 16,7	12,61	— 0,04
44 Libræ	η	4	231 40 21,6	50,09	+ 0,14	— 14 38 58,7	12,44	0,00
24 Serpentis	α	2	232 15 24,5	43,91	+ 0,22	+ 7 25 54,1	12,28	+ 0,11
27 Serpentis	λ	4	232 51 21,2	43,63	— 0,12	+ 8 21 15,2	12,11	+ 0,01
28 Serpentis	β	3	232 58 29,9	41,26	+ 0,14	+ 16 25 13,3	12,08	+ 0,03
45 Libræ	λ	4	233 51 17,7	51,62	+ 0,04	— 19 12 9,0	11,83	+ 0,03
46 Libræ	θ	4	234 3 30,9	50,57	+ 0,26	— 15 46 57,1	11,77	+ 0,21
6 Scorpii	π	3	235 2 52,0	53,71	+ 0,08	— 25 10 53,2	11,50	+ 0,05
7 Scorpii	δ	3	235 31 17,4	52,56	+ 0,07	— 21 42 3,3	11,36	+ 0,06
41 Serpentis	γ	3	235 32 21,3	41,01	+ 0,41	+ 16 42 10,4	11,33	— 1,21
13 Cor. Bor.	ε	4½	236 11 23,6	+37,20	+ 0,02	+ 27 48 8,9	—11,16	— 0,01

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
8 Scorpii	β	2	236 52 16,6	+51,74	+ 0,08	− 18 55 4,8	−10,96	+ 0,05
9 Scorpii	ω^1	5	237 11 7,3	52,04	+ 0,12	− 19 47 25,7	10,87	+ 0,06
10 Scorpii	ω^2	5	237 19 28,7	52,12	+ 0,14	− 19 59 30,9	10,83	+ 0,04
14 Scorpii	ν	4	238 30 42,1	51,74	+ 0,08	− 18 36 57,7	10,48	+ 0,05
1 Ophiuchi	δ	3	239 32 12,4	46,84	− 0,01	− 2 51 41,0	10,17	− 0,06
2 Ophiuchi	ε	$3\frac{1}{2}$	240 29 28,1	47,17	+ 0,13	− 3 54 0,4	9,88	+ 0,09
20 Scorpii	σ	5	240 36 25,4	54,06	+ 0,07	− 24 48 30,1	9,85	+ 0,11
4 Ophiuchi	ψ	5	241 30 28,1	52,14	+ 0,03	− 19 16 14,1	9,55	− 0,01
21 Scorpii	α	1	242 37 10,3	54,55	+ 0,06	− 25 42 2,7	9,23	+ 0,06
10 Ophiuchi	λ	4	243 49 31,0	45,13	+ 0,12	+ 2 41 52,1	8,85	− 0,01
23 Scorpii	τ	4	244 9 55,1	55,39	+ 0,09	− 27 31 44,1	8,74	+ 0,07
27 Herculis	β	3	244 13 48,9	38,61	− 0,12	+ 22 11 43,6	8,72	+ 0,04
29 Herculis	h	4	244 31 53,2	42,07	− 0,15	+ 12 11 13,2	8,62	− 0,02
13 Ophiuchi	ζ	3	245 1 59,0	49,15	+ 0,10	− 9 53 59,3	8,47	+ 0,09
44 Herculis	η	3	248 4 19,2	30,62	+ 0,10	+ 39 32 11,0	7,49	− 0,04
25 Ophiuchi	ι	4	249 50 32,6	42,22	− 0,05	+ 10 42 39,2	6,91	+ 0,01
27 Ophiuchi	$\varkappa$	4	250 45 20,5	42,68	− 0,27	+ 9 53 26,6	6,61	+ 0,08
58 Herculis	ε	3	252 6 43,4	34,32	− 0,03	+ 31 24 38,0	6,16	+ 0,09
35 Ophiuchi	η	3	253 9 37,3	51,23	+ 0,12	− 15 17 55,1	5,81	+ 0,19
36 Ophiuchi	A	$5\frac{3}{4}$	254 5 5,8	55,43	− 0,46	− 26 6 4,5	5,50	− 1,05
21 Draconis	μ	$4\frac{3}{4}$	254 44 20,0	18,46	− 0,12	+ 54 53 44,1	5,28	+ 0,05
64 Herculis	α	3	255 7 59,3	40,87	+ 0,03	+ 14 46 40,5	5,15	+ 0,10
65 Herculis	δ	4	255 34 55,3	36,82	− 0,06	+ 25 14 6,9	4,99	− 0,10
42 Ophiuchi	θ	$3\frac{3}{4}$	255 45 15,2	54,90	+ 0,04	− 24 38 34,5	4,94	+ 0,06
44 Ophiuchi	b	5	256 52 8,6	54,61	+ 0,08	− 23 50 36,2	4,56	− 0,04
51 Ophiuchi	c^2	6	258 7 58,6	54,59	+ 0,10	− 23 40 30,9	4,13	+ 0,04
55 Serpentis	ξ	4	259 57 57,9	51,33	0,00	− 15 9 36,9	3,50	+ 0,05
55 Ophiuchi	α	2	260 8 31,6	41,48	+ 0,14	+ 12 49 13,4	3,43	− 0,15
23 Draconis	β	$2\frac{1}{2}$	260 51 50,6	20,13	+ 0,01	+ 52 32 53,7	3,15	+ 0,01
56 Serpentis	o	5	261 0 7,0	50,43	+ 0,13	− 12 39 59,1	3,14	+ 0,01
58 Ophiuchi		6	261 13 12,6	53,78	− 0,04	− 21 29 22,6	3,06	+ 0,13
60 Ophiuchi	β	3	262 2 36,9	44,33	+ 0,01	+ 4 44 3,2	2,78	+ 0,24
62 Ophiuchi	γ	4	263 5 36,7	44,98	− 0,03	+ 2 51 46,1	2,41	− 0,02
64 Ophiuchi	ν	4	265 29 35,8	49,40	+ 0,06	− 9 41 35,4	1,58	− 0,04
33 Draconis	γ	2	267 21 15,8	+20,75	+ 0,08	+ 51 32 39,4	− 0,93	− 0,03

			Right Ascension.			Declination.		
			...ed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			′ ″	″	″	° ′ ″	″	″
			...48 31,9	+53,72	+ 0,04	— 21 5 37,0	— 0,42	+ 0,09
			...51 54,8	34,98	+ 0,06	+ 28 45 0,3	0,40	+ 0,06
			...11 1,9	53,59	+ 0,02	— 20 46 28,9	— 0,29	+ 0,10
			...16 56,8	57.53	+ 0,07	— 29 54 32,8	+ 0,10	+ 0,02
			...12 32,7	55,56	+ 0,01	— 25 32 38,3	0,77	— 0,12
			...276 33 57,3	56,25	+ 0,17	— 27 15 38,2	2,29	+ 0,07
			276 36 42,8	30,12	+ 0,32	+ 38 31 15,6	2,31	+ 0,32
			278 51 35,9	54,43	+ 0,02	— 23 4 48,1	3,09	+ 0,07
			279 0 20,1	55,86	+ 0,10	— 26 38 1,1	3,14	+ 0,02
			279 5 25,8	54,39	+ 0,13	— 23 0 46,1	3,17	+ 0,07
			279 39 40,7	33,12	+ 0,04	+ 33 1 54,0	3,37	+ 0,01
			279 43 35,7	53,58	+ 0,01	— 21 0 58,0	3,39	+ 0,06
		4	279 48 17,3	53,75	+ 0,06	— 21 28 6,9	3,42	+ 0,07
		3	280 42 47,0	57,48	+ 0,07	— 30 16 26,6	3,73	+ 0,07
		3½	281 23 26,0	40,83	— 0,03	+ 14 40 56,9	3,96	— 0,05
		4	281 31 17,4	53,97	+ 0,12	— 22 9 3,5	4,01	+ 0,04
		3	281 50 21,9	33,57	+ 0,02	+ 32 17 32,3	4,12	+ 0,05
		4	281 53 19,8	56,45	— 0,03	— 28 4 33,8	4,14	— 0,18
	ζ	3	282 47 39,0	41,31	— 0,03	+ 13 26 8,7	4,44	— 0,01
	π	4	282 49 32,9	53,67	+ 0,04	+ 21 28 20,6	4,46	+ 0,04
	ψ	5	284 7 28,3	55,36	+ 0,08	— 25 44 45,7	4,90	+ 0,07
	ρ^1	5	285 54 55,9	52,39	+ 0,05	— 18 23 20,4	5,50	+ 0,13
	δ	3	287 27 58,7	45,13	+ 0,28	+ 2 31 56,3	6,02	+ 0,17
	δ	3½	288 5 16,6	0,81	+ 0,32	+ 67 7 2,6	6,23	+ 0,08
	β	3½	289 33 27,2	36,21	+ 0,02	+ 27 20 13,9	6,72	+ 0,05
...ne	ι	4	290 9 58,6	46,62	+ 0,10	— 1 56 15,7	6,92	+ 0,06
...	φ	5	291 47 9,0	35,46	+ 0,02	+ 29 27 55,4	7,44	+ 0,11
...tarii	f	6	292 3 38,3	52,92	— 0,14	— 20 27 59,3	7,53	+ 0,02
...ilæ	γ	3	292 52 42,5	42,76	+ 0,10	+ 9 53 28,4	7,80	+ 0,06
...ilæ	α	1½	293 54 46,1	43,37	+ 0,60	+ 8 5 1,0	8,13	+ 0,43
...ilæ	η	3½	294 10 5,1	45,90	+ 0,01	+ 0 14 48,3	8,22	0,00
...ilæ	β	3½	295 1 8,6	44,17	+ 0,12	+ 5 39 55,5	8,49	— 0,41
...eculæ		5	296 28 12,8	38,62	— 0,04	+ 22 17 10,4	8,96	+ 0,04
...ilæ	δ	3	298 49 19,6	46,50	+ 0,10	— 1 42 25,9	9,67	+ 0,08
...icorni	α^2	4	300 6 26,2	+50,12	+ 0,06	— 13 25 50,1	+10,06	+ 0,07

STAR.		Mag.	Right Ascension.			Declination.		
			Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
			° ′ ″	″	″	° ′ ″	″	″
6 Capricorni	α^2	3	300 12 18,9	+50,13	+ 0,09	− 13 28 12,0	+10,09	+ 0,08
7 Capricorni	σ	6	300 21 40,5	52,29	+ 0,10	− 20 2 58,8	10,14	+ 0,09
9 Capricorni	β	3	300 53 16,1	50,82	+ 0,07	− 15 43 32,3	10,30	+ 0,11
10 Capricorni	π		302 22 50,8	51,88	+ 0,05	− 19 11 36,5	10,75	+ 0,10
37 Cygni	γ	3	302 46 41,4	32,18	+ 0,06	+ 39 17 8,9	10,86	+ 0,06
11 Capricorni	ρ	6	302 46 55,9	51,72	− 0,01	− 18 48 17,6	10,86	+ 0,08
2 Delphini	ε	3	304 36 1,8	42,98	+ 0,03	+ 10 16 44,2	11,39	+ 0,03
15 Capricorni	υ	6	305 35 14,1	51,66	− 0,01	− 19 11 50,1	11,68	+ 0,08
6 Delphini	β	3	305 45 18,5	42,06	+ 0,07	+ 13 32 41,2	11,72	+ 0,03
11 Delphini	δ	$3\frac{1}{2}$	307 14 48,7	42,00	+ 0,02	+ 13 59 23,1	12,14	+ 0,02
2 Aquarii	ε	$4\frac{3}{4}$	307 42 50,0	48,95	+ 0,08	− 10 36 0,3	12,27	+ 0,06
50 Cygni	α	2	307 43 9,8	30,35	+ 0,04	+ 44 11 33,5	12,27	+ 0,03
12 Delphini	γ	3	308 4 13,2	41,74	− 0,01	+ 15 2 0,7	12,37	− 0,13
6 Aquarii	μ	$4\frac{1}{2}$	308 58 26,3	48,76	+ 0,07	− 10 6 56,2	12,62	+ 0,03
22 Capricorni	η	5	311 40 22,1	51,74	+ 0,01	− 21 2 58,1	13,34	+ 0,03
23 Capricorni	θ	5	312 6 51,4	50,94	+ 0,14	− 18 26 3,3	13,45	+ 0,01
13 Aquarii	ν	5	313 9 59,5	49,25	+ 0,11	− 12 35 53,6	13,73	+ 0,05
8 Equulei	α	4	315 4 41,2	44,99	+ 0,13	+ 3 59 29,8	14,21	− 0,03
32 Capricorni	ι	5	316 13 47,7	50,52	+ 0,08	− 18 7 39,6	14,49	+ 0,10
34 Capricorni	ζ	5	317 13 16,2	51,96	+ 0,03	− 23 43 33,1	14,73	+ 0,09
5 Cephei	α	3	317 47 10,9	21,38	+ 0,35	+ 61 17 8,4	14,86	+ 0,02
22 Aquarii	β	3	318 48 4,3	47,58	+ 0,06	− 6 54 38,4	15,09	+ 0,06
39 Capricorni	ε	4	319 54 43,9	50,89	+ 0,07	− 20 49 44,2	15,35	+ 0,07
23 Aquarii	ξ	6	320 18 12,2	48,06	+ 0,13	− 9 13 12,2	15,44	+ 0,03
40 Capricorni	γ	4	320 42 40,2	50,11	+ 0,28	− 18 2 16,9	15,53	+ 0,06
8 Cephei	β	3	321 7 17,4	12,83	0,00	+ 69 12 28,2	15,62	− 0,03
8 Pegasi	ε	3	322 14 18,0	44,15	+ 0,12	+ 8 28 30,0	15,86	+ 0,05
48 Capricorni	λ	5	322 27 1,6	48,75	+ 0,07	− 12 46 19,7	15,91	+ 0,05
49 Capricorni	δ	3	322 28 5,3	49,83	+ 0,29	− 17 30 39,3	15,91	− 0,21
51 Capricorni	μ	5	324 4 59,6	49,13	+ 0,41	− 14 59 19,8	16,40	+ 0,09
33 Aquarii	ι	4	327 24 38,1	48,94	+ 0,07	− 15 21 12,2	16,90	0,00
34 Aquarii	α	3	327 27 42,1	46,32	+ 0,04	− 1 48 24,4	16,91	+ 0,05
24 Pegasi	ι	4	328 9 8,5	41,30	+ 0,40	+ 23 50 52,7	17,04	+ 0,07
26 Pegasi	θ	4	328 38 13,3	45,12	+ 0,38	+ 4 41 24,5	17,13	+ 0,10
43 Aquarii	θ	4	330 6 35,1	+47 61	+ 0,16	− 9 18 31,1	+17,40	+ 0,02

STAR.	Mag.	Right Ascension.			Declination.		
		Computed for 1690.	Ann. Prec.	Prop. Mot.	Computed for 1690.	Ann. Prec.	Prop. Mot.
46 Aquarii ρ	$5\frac{1}{2}$	330° 57′ 50″,2	+47″,59	+ 0″,02	— 9° 21′ 36″,3	+17″,54	+ 0″,05
48 Aquarii γ	3	331 24 20,9	46,47	+ 0,19	— 2 55 59,2	17,61	+ 0,07
52 Aquarii π	5	332 21 31,4	46,00	+ 0,06	— 0 10 46,2	17,77	+ 0,06
55 Aquarii ζ	4	333 12 52,7	46,23	+ 0,22	— 1 35 27,5	17,91	+ 0,10
57 Aquarii σ	5	333 33 0,7	47,92	+ 0,03	— 12 15 8,4	17,96	+ 0,12
62 Aquarii η	4	334 51 13,4	46,23	+ 0,13	— 1 42 2,8	18,16	— 0,01
63 Aquarii $\varkappa$	6	335 25 10,4	46,82	— 0,02	— 5 48 48,0	18,24	— 0,06
42 Pegasi ζ	3	336 30 18,6	44,68	+ 0,08	+ 9 13 32,2	18,40	+ 0,04
71 Aquarii τ^2	$5\frac{3}{4}$	338 17 0,3	48,00	0,00	— 15 13 1,5	18,64	+ 0,04
73 Aquarii λ	4	339 6 20,5	47,14	— 0,02	— 9 13 1,2	18,74	+ 0,08
76 Aquarii δ	3	339 32 10,8	48,18	— 0,01	— 17 27 31,2	18,79	+ 0,06
24 Piscis Aust. α	1	340 6 15,9	50,12	+ 0,37	— 31 15 9,8	18,86	— 0,13
4 Piscium β	5	342 1 30,2	45,74	+ 0,08	+ 2 9 39,4	19,08	+ 0,02
53 Pegasi β	2	342 12 6,5	42,93	+ 0,27	+ 26 24 36,0	19,10	+ 0,18
83 Aquarii h^1	6	342 14 30,0	46,98	+ 0,21	— 9 21 27,2	19,10	+ 0,07
54 Pegasi α	2	342 20 13,6	44,51	+ 0,13	+ 13 32 43,3	19,11	+ 0,03
90 Aquarii ϕ	5	344 33 41,8	46,70	+ 0,10	— 7 42 44,3	19,34	— 0,15
91 Aquarii ψ^1	5	344 54 15,7	46,97	+ 0,44	— 10 46 7,5	19,37	+ 0,10
6 Piscium γ	4	345 16 32,7	45,83	+ 0,78	+ 1 35 50,1	19,40	+ 0,03
93 Aquarii ψ^2	5	345 26 22,7	46,94	+ 0,12	— 10 52 2,0	19,54	+ 0,02
95 Aquarii ψ^3	5	345 42 5,6	46,96	+ 0,11	— 11 17 54,0	19,44	+ 0,04
68 Pegasi υ	6	347 29 27,3	44,24	+ 0,28	+ 21 42 12,5	19,58	+ 0,07
8 Piscium $\varkappa^1$	5	347 45 34,9	46,01	+ 0,17	— 0 26 4,8	19,60	— 0,09
17 Piscium ι	6	351 0 15,2	45,76	+ 0,47	+ 3 57 1,2	19,81	— 0,42
103 Aquarii A^1	5	351 21 44,0	47,06	+ 0,01	— 19 44 11,8	19,83	— 0,03
104 Aquarii A^2	5	351 24 19,7	47,04	+ 0,07	— 19 32 3,2	19,84	+ 0,10
18 Piscium λ	5	351 33 30,1	45,97	— 0,05	+ 0 4 40,4	19,84	— 0,13
5 Cassiopeæ τ	5	353 1 56,2	42,25	+ 0,14	+ 56 55 38,3	19,91	+ 0,08
28 Piscium ω	5	355 51 12,2	45,84	+ 0,23	+ 5 8 51,0	20,01	— 0,09
29 Piscium	5	356 29 7,6	46,08	+ 0,06	— 4 45 12,5	20,02	+ 0,03
2 Ceti	$4\frac{1}{2}$	356 57 20,4	46,34	+ 0,05	— 19 3 46,5	20,03	+ 0,06
33 Piscium	5	357 21 50,7	46,09	+ 0,08	— 7 26 22,6	20,04	+ 0,05
21 Andromedæ α	2	358 6 52,9	45,63	+ 0,22	+ 27 22 37,7	20.05	— 0,13
11 Cassiopeæ β	$2\frac{3}{4}$	358 13 43,5	44,99	+ 1,09	+ 57 26 21,5	20,05	— 0,18
88 Pegasi γ	2	359 19 48,1	+45,92	+ 0,10	+ 13 27 29,9	+20,06	+ 0,01

TABLE L.

(See page 408.)

In leap years, enter the Table in January and February, with the given date *minus* unity, for the sun's longitude. And, for the days elapsed, add unity to the numbers in the column after those months.

Day. (*Old Style.*)		Sun's true Long.	Days elapsed since Jan. 1.	Fraction of the Year.
Jan.	1	292°·490	0	·000
	11	302·667	10	·028
	21	312·817	20	·055
	31	332·929	30	·083
Feb.	10	333·000	40	·110
	20	343·033	50	·137
March	2	352·999	60	·165
	12	2·900	70	·192
	22	12·767	80	·220
April	1	22·578	90	·247
	11	32·317	100	·274
	21	42·017	110	·302
May	1	51·675	120	·329
	11	61·283	130	·357
	21	70·867	140	·384
	31	80·424	150	·411
June	10	89·967	160	·439
	20	99·500	170	·466
	30	109·028	180	·494
July	10	118·567	190	·521
	20	128·133	200	·548
	30	137·731	210	·576
Aug.	9	147·350	220	·603
	19	157·017	230	·631
	29	166·745	240	·658
Sept.	8	176·517	250	·685
	18	186·333	260	·713
	28	196·222	270	·730
Oct.	8	206·150	280	·757
	18	216·167	290	·795
	28	226·192	300	·822
Nov.	7	236·283	310	·850
	17	246·417	320	·877
	27	256·580	330	·904
Dec.	7	266·750	340	·932
	17	276·950	350	·959
	27	287·149	360	·987
	37	297·317	370	·014

TABLE M.

(See page 408.)

Year. (*Old Style.*)		Correction for Sun's Long.	Moon's Node.
	1690	—·114	0°·165
	1691	—·353	340·837
B	1692	+·394	321·455
	1693	+·155	302·127
	1694	—·084	282·798
	1695	—·322	263·470
B	1696	+·424	244·089
	1697	+·186	224·760
	1698	—·053	205·432
	1699	—·292	186·103
B	1700	+·455	166·722
	1701	+·216	147·394
	1702	—·023	128·065
	1703	—·262	108·737
B	1704	+·485	89·355
	1705	+·246	70·027
	1706	+·007	50·699
	1707	—·232	31·370
B	1708	+·515	11·989
	1709	+·276	352·660
	1710	+·037	333·332
	1711	—·201	314·004
B	1712	+·546	294·622
	1713	+·307	275·294
	1714	+·068	255·965
	1715	—·171	236·637
B	1716	+·576	217·256
	1717	+·337	197·927
	1718	+·098	178·599
	1719	—·141	159·270
B	1720	+·606	139·889

Daily variation of the Moon's node = — ·05295.

TABLE N.

(See page 408.)

Mean Thermometer for each Month.

..	Highest mean.	Lowest mean.	General mean.
January . .	41°·8	33°·5	39°·0
February .	45·6	38·0	41·6
March . .	49·5	40·0	44·4
April . .	54·2	47·0	50·4
May . . .	60·4	51·6	56·2
June . .	67·0	57·7	62·8
July . . .	71·1	60·8	65·8
August . .	67·7	57·8	64·8
September .	64·1	57·0	60·4
October . .	56·0	48·0	52·4
November .	50·8	40·8	46·6
December .	46·1	38·1	41·4

The mean height of the Barometer, for the whole year, is 29·85 inches.

ADDENDA ET CORRIGENDA.

Extract of a letter from Mr. Flamsteed to Dr. Edward Bernard.

Observatory, Jan. 22, 1677-8.

WORTHY SIR,

Yours of the 20th past I received not till the 16th instant; and then, not knowing how to direct my answer so as it might come safe to your hands, I was forced to defer this return till I might learn it at London. In the mean time Mr. Bulliaw, having wrote to Mr. Sherburne to get him what observations of Mercury I had, he was pleased, in the conclusion of a letter to me on that occasion, to let me know that he had received a letter from you signifying that you designed to leave the *mathematical* for a *theological* employment, and that he thought I might be a fitting successor to your place. I had never so good an opinion of mine abilities as to think I could deserve so eminent a reward of my studies by them; but since you and he concur in the same opinion, I begin to think I may not want enough to serve it, if my other friends, who are here concerned, shall but be of the same mind, and lend me their assistance where it is requisite. But, my chief dependence is on yourself; for, being of another University, I am not acquainted with the customs of yours; nor know I which way to move to seek after it. But, if you will please to let me know what friends I ought to make, and to whom to apply myself, with such other directions as you may think convenient, some time before you remove, that I may not be prevented by others interposing, I shall take some pains to look after it: for it were both incivility and folly to neglect the motion of so great a kindness.

[Copied from the *original* in the Bodleian library, Oxford.]

Extract of a letter from Mr. Flamsteed to Dr. Edward Bernard.

Observatory, Feb. 8, 1677-8.

WORTHY SIR,

I am much obliged to you for the information you last gave me, and the friendship you have showed me (who am altogether otherways a stranger to you, except by the relation of our profession) in the motion of me to be the successor in a chair, which I much question whether I should be able to maintain with the credit it has gained by those who have possessed it. But, I am apt to think I have some friends here who are unwilling I should ever attain to that preferment, since you mention Mr. Halley in the postscript of your letter; I suppose by the intimation of Mr. Sherbourne, who on the receipt of one of yours wrote me word that he thought I might make you a fit successor: but when I went to visit him afterwards told me I could not hope of that favor, by reason I was not of your University, and that Mr. Halley, being of it, might reasonably expect it. I knew by whom he was influenced, but it concerns me to be silent. I therefore suppressed my just indignation at those who I thought had been concerned to seek my benefit as much as I had been for their credit; and resolved for the present to content myself with a place which I have furnished with instruments of my own contrivance, but full of trouble and no gains, till I see an opportunity of

removing to some [one] more advantageous, and where I may have a better air with lesser or fewer distempers. I am as weary of the place as you of yours: my inclinations are for an employment that may render me more useful in the world, and promote more glory to my Maker, which (as you well intimate) is the sole end of our lives, and to which I would divert all my labors. But, till he pleases to afford me such an opportunity, I am content with my present employment, which I manage much differently from those who went before me; and, if the love of my own methods persuade me not amiss, with more advantage.

Last week I received a letter from Mr. Halley, who tells me that, if the clouds (which are more frequent than he expected) prevent him not, he hopes to be at home by August next. But, I am apt to think he will make it Christmas ere he returns. He is very ingenious, as I found when he talked with me: and, his friends being wealthy, you may expect that advantage by a resignation to him, which it is scarce in my power to afford you.

I am of the same mind concerning the Royal Society as you. The President is changed*: Mr. Hooke fills all with discourse and mighty projects of invention and discoveries, but they are seldom seen; and, when they are, *parturiunt montes.*

[Copied from the *original* in the Bodleian library, Oxford.]

Letter from Dr. Wallis to Sir Isaac Newton.

Oxford, Jan. 9, 1698-9.

Sir,

I had lately an intimation from Dr. Gregory, as if it were a desire of yours, that (in a letter of Mr. Flamsteed's concerning the parallax of the earth's annual orb, which I am about to print) I would omit a paragraph wherein you are mentioned. It is wherein, when I had pressed his communicating to me and the public his observations concerning this matter, he excuseth his delay of gratifying me therein, from his diversion by other business, and (amongst the rest) this for one: "Contraxeram etiam cum Do. Newtono doctissimo tunc temporis in academia Cantabrigiensi Pro- "fessore necessitudinem cui lunæ loca ab observationibus meis ante habitis deducta 150 dederam "cum locis simul è tabulis meis ad earum tempora supputatis tum similium in posteriore prout "assequerer promissorum cum elementis calculi mei in ordine ad emendationem theoriæ lunaris "Horroccianæ enarrare spero cum [eorum?] successus censeas harum expectationi suæ pares "huic cum ingeniosis duobus viris (in Septentrionalibus Angliæ partibus) ut ipsis impertirem loca "duorum - - - - planetarum (Saturni et Jovis) in ordine ad restituendum illorum motus "quam rem ipsi tunc moliebantur." Upon which I wrote to Mr. Flamsteed to have his opinion in it: for I was not to alter his letter without his order.

I have since an answer from him to this purpose; that it was (he supposeth) rather a suggestion of Dr. Gregory's own, than any orders from you, to have it left out: for that you do readily acknowledge in all companies that you had from him such observations; and that upon his now writing to you on this occasion he hath received no answer from you to the contrary; and therefore he thinks that you do acquiesce as to the publishing that paragraph with the rest, and seems yet to adhere to it. For my part I am willing to serve you both, and not willing to displease either; and therefore

* Sir Joseph Williamson was elected President in November, 1677, in the room of Lord Brouncker. F. B.

desire that you will please to accommodate that business between yourselves; and I shall readily comply with what you two agree upon therein: and I desire I may have an account from you as soon as may be, because the press will quickly be ready for it.

I am, Sir, yours to serve you,

JOHN WALLIS.

I don't apprehend any prejudice to you in printing it, being merely true matter of fact: and it seems of concernment to him to satisfy the world (from this and other things mentioned) that he is not idle; though he be not yet in readiness to publish the whole of his observations (for which he is frequently called upon), it being a great work.

[Copied from the *original*, in the possession of the Earl of Portsmouth: and it may be proper to state that some of the Latin words, especially towards the latter part of the sentence, are not very legibly written, and consequently the construction not very distinct. I have not ventured to supply the punctuation.]

Letter from Mr. Flamsteed to Dr. Smith.

Observatory, Nov. 1, 1700; mane.

SIR,

I go into Surrey, God willing, to-morrow morning, and shall scarce return till Wednesday. If I get that day to London, I will meet you at Garraway's; but I fear I hardly can, and therefore make this quiet return to advise you not to expect me.

The discourses Mr. Halley has broached and fomented, relating to my not publishing my observations, signify little; for the world him, his principles and practices; and that all the dust he raises is only by the help of our young lewd gentlemen, whom he encourages in their vices, and they remunerate him by spreading his slanders. I have resolved to take no revenge of him, but (that I may not be wanting to myself) I have put a larger paper than I gave you, with some notes on it, into a great hand, who is as much satisfied with it as you with my lesser and incomplete letter, and will make good use of it. A courtier would have me print it: but there is no need, for Mr. Halley's reports are like to make him ashamed in a little time. His great friends are sensible of his behaviour, and begin to keep him at a distance. I would have him reform of himself, and make reparations if it might be; and therefore give him fair time: for I cannot mention the reflections he has made without exposing him too much; whereas I desire not to make him smart, but blush; or rather, not blush so much as repent and become a good and sincere man.

I was somewhat amazed at what you told me, at our parting, that it was reported I had *justified the murder of King Charles I.* I thank you for speaking it out, and desire you to take notice of the words, how maliciously they are laid together. He must certainly be a most wicked man that would *justify any murder whatsoever*, be the person murdered never so bad. If they had said I had *vindicated the trial or execution*, it had been enough; but that looked not so black as murder: and, when calumnies are to be spread by malicious people, always the most ugly and odious words are picked out to express them, and make them take the deeper. Sir, I am of opinion that neither the Rump Parliament, nor the High Commission Court, had any right to judge the King: and I esteem not only those who sat upon him, but the members of the Rump, to be as guilty of his murder as you do, and have said as much on several occasions. And must ever pity that unfortunate

good man and king, who was ruined (not, as is commonly thought, by a party of ill subjects in his own kingdom alone, but) by French contrivances that excited the rebellion, and by suggestions to his Queen, and her ascendant over him, kept him from following those good councils that were given him by the wisest of his peers, and the best of his clergy about him. This I have always said; and this some histories, lately published, make evident. I hope you are now satisfied. This I find has been spread amongst a party, and whispered to and fro, and obtained belief because I could not learn what it was, nor possibly obviate [it]. I met with another, to the same purpose, spread in the country; which I stopped by resenting it strongly. Pray spread my answer to you, amongst the same people; you will do justice, and oblige me.

I pray God to preserve you, and every good man, from the like villainous and malicious calumnies and slanders; and forgive all them that raised or spread them: I do it most heartily. At my return you shall have an appendix to my last letter, if God send life and health to, Sir, your affectionate friend,

JOHN FLAMSTEED, M.R.

[Copied from the *original* in the Bodleian library, Oxford.]

Page	Line	
33		In the first *Note*, fill up the blank with the figures 37 and 68.
34	25	For " 36 " read " 37."
55	10	The date of the decease of Flamsteed's father was March 8, 1688.
58	9	For " 1694," read " 1694-5."
58		Cancel the third *Note* in that page; and in lieu of it insert, " This synopsis is in MSS, vol. 16, page 40."
65		Newton's visit to the Observatory, alluded to in the second note, is more minutely recorded in MSS, vol. 16. See page xxxv. of the Preface.
81	Note ‡	There are also three other copies of these articles (all unexecuted) amongst the Newton MSS in the possession of the Earl of Portsmouth; slightly differing from each other.
86	28	It was paid to Mr. Hodgson, and his receipt for the same (dated April 12, 1708) is amongst the Newton MSS in the possession of the Earl of Portsmouth.
89	28	This letter was certainly communicated to Newton; for I saw the *original* in Flamsteed's handwriting amongst the Newton MSS in the possession of the Earl of Portsmouth.
95	11	The date, April 18, should probably be April 19; as the passage alluded to is to be found in that letter. See page 284.
101	5	from the bottom: for " the order of the dates," read " page 363."
105	14	For " vol. 35," read " vol. 33."
113	31	For " days " read " degrees."
115	15	For " Parkins " read " Perkins."
120		The calculations, on which the table there given is founded, are in MSS, vol. 51, page 335.
129	6	Amongst the Newton MSS in the possession of the Earl of Portsmouth, there is a copy of the formal certificate which Newton appears to have given to Gregory, as to his opinion of Gregory's talents as a mathematician. There are several letters of Dr. Gregory addressed to Newton, amongst those MSS: and I would here remark that the early ones are always signed " D. Gregor*ie*;" but that the latter ones are signed " D. Gregor*y*."

Page	Line	
130	33	The volume here alluded to is MSS, vol. 24.
136	11	For " 5° 66′," read " 5°·66." The last two figures being *decimals*.
142		The statements given on this page were written on the letter of June 29, 1695. See the remark in page 157.
145	17	For " 1664 " read " 1694."
148	14	Put a comma after the word " just."
163	9	For " T." Flamsteed, read " J." Flamsteed.
166		The letter No. 43, was not received by Flamsteed till *after* he had written the subsequent one (No. 44) to Dr. Wallis. The reader must bear this in mind when he reads this last-mentioned letter.
167	7	Since this letter was printed I have seen, amongst the Newton MSS in the possession of the Earl of Portsmouth, the very letter which Dr. Gregory addressed to Newton, dated Decem. 23, 1697, requesting him to use his influence to obtain for him the situation of tutor here alluded to. The letter is directed " To the much honored Sir Isaac Newton, &c."
208	28	Flamsteed's copy of Dr. Gregory's book, with his marginal notes thereon (some of which are written with the pencil only), is now the property, and in the Library of the *London Institution*.
210	26	The tables here alluded to are probably those in MSS, vol. 50 I.
212	18	The person, designated as Cap. Raymer in this and other places, was evidently Dr. Halley; and so avowed by Flamsteed in his letter to Mr. Sharp, dated August 5, 1703, in reply to his suspicion on the point.
276	Note.	The disorders in the Royal Society alluded to in the note here quoted, were occasioned by the disputes mentioned by Flamsteed: for since that note was printed, I have seen, amongst the Newton MSS in the possession of the Earl of Portsmouth, a letter addressed to Newton, dated March 28, 1710, (apparently by an anonymous writer,) calling on him to exert his authority to preserve better order, &c, in the meetings, and complaining of Sloane's conduct.
283	14	The catalogue of the 6 latter signs of the zodiac, here alluded to, is probably the fragment still preserved in MSS, vol. 27 A. See page lxvi.
293		The computations of the places of the moon, alluded to in No. 167, are those which are still extant in MSS, vols. 54 and 57. They were afterwards more correctly made in vol. 60.
311	last	For " 37 " read " 36."
313	last	For " 37 " read " 36."
321	6	For " 1715-16 " read " 1716."
327		In the table, inserted in this page, the hours and minutes have been inadvertently omitted: they should stand as follow:

	h	m
March 21 .	12	19
—— 30 .	11	47
—— 31 .	11	43

And in the last column, for " 2° 30′ 31″," read " 2° 46′ 25″."

332	last	For " vol. 37 " read " vol. 36."
359	15	For " sheets B, E, P, R," read " half-sheets B, E, P, R."
——	34	By a letter to Mr. Sharp, dated Novem. 20, 1725, it appears that the price of the 3 volumes was altered to *five* guineas: because the Dutch were going to reprint the whole, had not the price been reduced.
376	2	For " 14 *Hydræ contin.*" read " 58 *Hydræ*."
382	26	For " page 127 " read " pages 127 and 258."

Page	Line	
383	34	For " 22 *Hydræ et Crateris* " read " 22 *Crateris*."
—	36	Dele " 51 *Camelopardi* is the double star VII. 159, and not the star so designated by Bessel:" for Bessel is correct in his identification of this star.
398	19	The letters annexed to the stars in *Coma Berenices*, were evidently introduced for the purpose of identifying his *Sextant* Observations. See *Historia Cœlestis*, vol. 1, page 67.
404	11	I have not inserted in Table I. (page 655) the *new* letters introduced by Flamsteed, as the major part of them have been already pointed out in the Note in page 397. The *duplicates* however will be found in that Table.
405	last.	Add to the *Note*, " We also find the algebraic signs + and — annexed; but without any explanation of their force or meaning."
407	27	For " 348" read " 345."
408	17	For " Table K" read " Table L."
416		No. 203, is inadvertently placed out of the regular order of right ascension.
419		No. 279, is inadvertently placed out of the regular order of right ascension.
—		No. 281, for " 23° 34′ 30″ " read " 23° 34′ 40″."
427		No. 561, for " 62° 41′ " read " 62° 41′ 30″."
428		No. 596, for " 65° 45′ 15″ " read " 65° 45′ 55″."
—		No. 613, for " π " read " π^1."
430		No. 675, for " 21° 53′ 55″ " read " 21° 53′ 5″."
464		No. 1834, dele the letter *n*.
479		No. 2388, for " v " read " υ."
484		No. 2554, for " 13 Herculis " read " 113."
491		No. 2809, for " 13° 33′ 25″ " read " 13° 32′ 25″;" and for " —43″,7 " read " +16″,3."
505		No. 3297 is inadvertently placed out of the regular order of right ascension.
519	last	For " 5' " read " 5′."
536	19	For " to " read " and."
584	18	For " in the declinations " read " in the differences of declination."
592	33	For " 1925 " read " 1928:" and the note should then be placed in page 593, in the order of the numbers.
—	41	For " 1926 " read " 1925."
612	25	For " 2^m " read " 1^m."
—	26	For " 30′ " read " 15′."
647		Insert an asterisk against No. 278.
648		Insert an asterisk against No. 1285.

THE END

LONDON: Printed by WILLIAM CLOWES and SONS, Stamford Street.