XI. *Some Account of the Astronomical Observations made by Dr. Edmund Halley, at the Royal Observatory at Greenwich. By Francis Baily, Esq., V.P.R.S., and President of this Society.*

Read November 14, 1834.

On the decease of Mr. Flamsteed, which happened on December 31, 1719, Dr. Halley (who was then in the 64th year of his age) was appointed his successor as Astronomer Royal: a situation which he held till the time of his death in January 1742, a period of twenty-two years. The astronomical observations, which he made in that situation, have never yet been published; except the accounts of the three following phenomena which were communicated to the Royal Society, and printed in the *Philosophical Transactions:* namely, the solar eclipse on November 27, 1722, the transit of *Mercury* over the sun's disc on October 29, 1723, and the lunar eclipse on March 15, 1735–6. The rest exist in manuscript only, and have never yet been made public. They are contained in four small quarto volumes bound in vellum, deposited in the library of the Royal Observatory at Greenwich: and it has been a frequent subject of inquiry, both at home and abroad, as to the contents of these volumes, and the value of the observations. The first of these volumes consists of 278 numbered pages; but, as the pages 275 and 276 are repeated, there are actually 280 pages, containing the observations from October 1, 1721, to June 23, 1724, both inclusive; to which are added 8 pages (sewed in, at the end) containing the observations from June 24 to August 7 in the same year: so that the whole of this volume may be considered as consisting of 288 pages. The second volume consists of 176 pages, containing the observations from August 8, 1724, to March 19, 1727, both inclusive. The third volume consists of 346 pages, containing the observations from March 19, 1727, to September 30, 1734, both inclusive. And the fourth and last volume consists of only 60 written pages, containing the observations from October 7, 1734, to December 31, 1739, both inclusive. As this last book is not full, it would appear that Dr. Halley did not make

(or, at least, did not *record*) any observations during the last two years of his life.*

The manuscripts here alluded to, are very badly and sometimes rather confusedly written; especially in the early part of the series: there being numerous computations, and much extraneous matter written on the same page with the observations, intermixed with and occasionally obliterating the more important figures: so that they cannot be so readily consulted with that ease and convenience, nor with that clearness and distinctness, which are desirable in works of this kind. Added to which there is a constant risk of loss and damage by fire, or other accident, which ought not to exist in a document of this importance. In fact, a circumstance that occurred a short time since, shewed the danger to which these volumes might be occasionally exposed. Mr. HENDERSON had obtained the loan of them for the purpose of some inquiry in which he was engaged: and during the time they were in his possession, a fire broke out in the house in which he was resident: and although they were then fortunately secured, yet by some similar occurrence they might be exposed to the risk of being materially injured, or perhaps wholly destroyed.

Under these circumstances, and considering that there was no probability of getting the manuscripts printed, I drew up a statement urging the propriety and expediency of having, at least, a *fair manuscript copy* made of the original observations: which, at the same time that it would render these documents more easy to be consulted, would also render the loss of the originals a less serious evil. This statement was laid by Captain BEAUFORT before the Lords Commissioners of the Admiralty: who, desirous of promoting every object that may tend to the advancement of science, immediately ordered a transcript to be made; and the same was, on December 14th, 1832, presented by them to this Society. It is contained in one large folio volume, consisting of 518 written pages: and it is in consequence of this present that I have been induced to draw up the present Memoir, to which I beg leave to draw the attention of the Society.

* Professor RIGAUD informs me that he has a copy of a memorandum made by the late Dr. MASKELYNE, containing a catalogue of Dr. HALLEY's MSS. as they were sent to Greenwich by the Board of Longitude. It mentions the four volumes, quoted in the text, and adds, "N.B. Dr. HALLEY left a *fifth* volume of observations at his death, which do not appear in "this collection."

Shortly after Dr. HALLEY's appointment he communicated, at one of the meetings of the Royal Society (May 12, 1720), an account of a design which he had in view, to enlarge the British Catalogue of fixed stars, by inserting in the vacant spaces of the zodiac all such stars as are plainly visible through a telescope of five feet length; in order to make the method of finding the longitude at sea by the moon more practicable than it is at present. Dr. HALLEY then went on to describe his method; which is nearly the same as that adopted by LA CAILLE at the Cape of Good Hope. For he proposed to *fix* a telescope, having a net-work of squares, against a wall; and by taking the differences of the times of transit, and noting the parallels on which the stars move, he would fill up the vacant spaces with such stars as pass. And by fixing the telescope at different elevations he would ultimately accomplish his object. The President, Sir ISAAC NEWTON, remarked that he [Sir ISAAC] had founded his theory chiefly upon observations of the moon's place in the *conjunctions* and *oppositions* to the sun. But it would be necessary for the further correction of this theory to collect, first, all the errors of it in the *quadratures*, and afterwards what errors there are in the *octants*. For which end he proposed it, as an useful work, to frame an ephemeris of the moon's motion, from the theory, for eighteen years; in which period the errors return. And this would be a ready means to examine how much the theory may err from the observations made at any other time.*

I have already remarked, that the first recorded observation by Dr. HALLEY was on October 1, 1721, nearly two years after he was appointed Astronomer Royal. For, it should be noted that, at the time of FLAMSTEED's death, his executors claimed and actually took away all the astronomical instruments which he had in use there; the mural arc, the voluble quadrant, the clocks, &c.: every thing was removed. And although an attempt was at first made by the Government to detain them, and although a legal process was actually commenced in the Court of Exchequer, with a view to recover them, yet FLAMSTEED's right and title to them was at length clearly proved; since it was shewn that they had been made, erected and even *repaired* at his own

* See the MS. *Journal* book of the Royal Society: from which these statements are extracted. It does not appear however that either HALLEY's or NEWTON's plan was carried into effect.

expense. It therefore became necessary to procure new ones for Dr. HALLEY; which necessarily took up some time. What these new instruments were, and what was the state of the Royal Observatory at that time, I am enabled to state from a document which is preserved in the Minute-Books of the Council of the Royal Society.

For, it appears that, on May 12, 1726, Dr. HALLEY represented to the Council of that Society (who, together with such other persons as they might appoint, had been declared, by His Majesty's warrant, to be Visitors of the Royal Observatory*) that the sum of money advanced to him by Government, for the purchase of instruments, was nearly expended; and at the same time requested that those accounts might be audited. "Where-upon it was ordered that there be a Committee appointed to visit the state "of the Observatory at Greenwich, and inspect the accounts, on Monday "next, May the 16th: and that the following gentlemen constitute the said "Committee; viz. Dr. TAYLOR, Dr. STUKELEY, Mr. HADLEY, Mr. GRAHAM, "and Mr. FOLKES."

At the next meeting of the Council, on May 26th, "MARTIN FOLKES, Esq. "reported, in the name of the Committee appointed as Visitors of the Royal "Observatory at Greenwich, by an order dated the 12th of this month, that "they did, in pursuance of the said order, go down to Greenwich on Monday "last, and did there view the state of the Observatory, inspecting the several "works and instruments, erected or purchased by the present Professor; "and did also examine the several bills and vouchers for the moneys thereon "expended; and have also considered, to the best of their judgment, what "there yet remains to be done, in order to put the Observatory into that "condition which seems requisite to answer the intentions of it. And that "having agreed in one report, they appointed him to lay the same before "the Council. Which report was accordingly read, setting forth in general,

"1°. As to the state of the Observatory.

* This mode of visitation has recently been altered: and, on the accession of his present Majesty, he was pleased to issue a new warrant appointing the President of the Royal Society for the time being and five other fellows of that Society nominated by him, together with the President of the Royal Astronomical Society for the time being and five other fellows of that Society nominated by him, and the Savilian professor of astronomy at Oxford, and the Plumian professor of astronomy at Cambridge, to be the Visitors of the Royal Observatory; with certain powers for the execution of the office.

" That all the instruments now lodged or erected in the Royal Observa-
" tory, and belonging to it, were procured by the present Professor: those
" which were used by his predecessor being carried off and claimed by his
" executors.

" That there is a room adjoining to the west end of the house, newly
" erected, which serves as an observatory for taking the transits of objects
" on the meridian. It being furnished with a curious telescopic instrument
" of five-feet radius, fitted to an axis, and adjusted with screws to revolve
" in the plane of the meridian. And a plain week-clock, standing by it, for
" making the observations.

" That the great room in the observatory is furnished with a plain
" month-clock, and three very good telescopes: one of nine feet, another of
" sixteen, and the third of twenty-four feet in length. And also two very
" good micrometers of different forms.

" That in the garden, from off the south-east corner of the house, there
" is erected another building, being a room designed to hold two large mural
" quadrants of eight-feet radius, for observing the meridional altitudes of
" objects. One of which quadrants is to command that part of the meridian
" which lies to the south: and the other, that part which lies to the north.
" And in the middle of the said room is erected a firm stone-wall, lying
" north and south, being eleven feet high, nine feet long, and two feet thick;
" consisting of nine large stones cemented. To the east face of which wall
" is affixed the large quadrant, which is for taking the observations to the
" south; being entirely finished and fitted up for use.

" That they are informed that the materials for making the other
" quadrant are procured, and many of its parts formed: as the brass limb,
" the iron bars, and the tube for telescope, with some others.

" 2°. Next as to the bills and vouchers for the moneys expended in
" making these works and instruments.

" The said report sets forth, that they have examined the several parti-
" culars in the said bills, which, in the total, amount to the sum of
" £435. 10*s*. 4*d*.: and have had the opinion of able judges that the several
" particulars are reasonably rated; and are fully persuaded that the several
" sums have been truly laid out with great frugality and good husbandry.

" That it appears by the vouchers produced to them that, out of the sum
" of £500 which was issued from the Treasury, for fitting up His Majesty's

" Observatory at Greenwich, the sum of £40 was paid for taxes and fees.
" So that there remains only the sum of £24. 9*s*. 8*d*. balance in the hands
" of His Majesty's Observer.

" 3°. They offer it as their opinion, that the Observatory cannot be
" accounted sufficiently furnished with instruments, for serving all the pur-
" poses required, unless there be one large and substantial instrument,
" which is not confined to the plane of the meridian; but fitted for making
" observations in any other vertical. For the want of which, the Professor
" is at present obliged to make use of one which belongs to this Society.

" They further say, that they have formed a calculation, as near as they
" are able, of the expenses requisite for finishing the quadrant already
" begun; and also of the charges of an instrument of suitable bigness to
" make observations out of the meridian: and, in the whole, are of opinion
" that the expense will amount to the sum of £200 more than the balance
" now remaining.

" This report being considered, and the matter thereof debated,

" It was resolved, that in pursuance to the directions of her late
" Majesty's letter, dated 12th December, 1710, application be made to the
" Board of Ordnance, to procure an order for issuing a sum requisite to
" furnish the Observatory with proper instruments."

It appears therefore that for four years at least (or nearly six years from his original appointment), the only fixed instrument which Dr. HALLEY possessed was the five-feet transit instrument above mentioned.* With this he took a number of observations, principally of the moon and certain determining stars, sufficient to fill more than one of the small volumes above alluded to. But, these observations are of little importance to us at the present day (except that they give us the approximate place of the moon in right ascension), not only for want of their corresponding zenith distances, but more especially on account of the irregular and confused state of his clocks (to which I shall more particularly allude in the sequel): and which renders it almost impossible for us now to determine with accuracy the right ascensions of the bodies observed. Moreover, it is not always certain what instrument has been used in the observations; whether the transit instrument

* This is probably the instrument described by Dr. SMITH in his *Complete System of Opticks*, 1738, page 326; where, however, the telescope is said to be 5½ feet long. This instrument is now at Greenwich.

or one of the moveable telescopes: for as they are generally recorded in the same manner, it requires some attention to distinguish the particular cases. Thus, on December 22, 1721, we have the following observations recorded, amongst others, viz.

h	m	s	
9	27	35	Prima Hyadum γ transiit.
9	43	35	Aldebaran transiit.
19	23	49	Jovis centrum transiit.
19	30	12	β Scorpii transiit.
19	32	23	Jupiter, optimè, transiit.
19	38	45	β Scorpii, iterum transiit.

Again, on the following day, December 23, we have, amongst others,

	18	53	40	Prima trium sub pede ♏ transiit.
	18	55	40	Media transiit.
	18	59	15	Sequens earum, J. H. transiit.
	19	3	41	His australior, J. H. transiit.
	19	19	13	♀ transiit.
	19	23	10	☿ transiit.
repet.	19	27	45	♀ transiit.
repet.	19	31	42	☿ transiit.

Now, it is evident in the first case, that *Jupiter* and β *Scorpii*, and in the latter case that the two planets, ♀ and ☿, could not be observed *twice* with the transit instrument, on the same day: and the only doubt may be whether *all* the observations might not have been made with one of the moveable telescopes, and not with the transit instrument.

This confusion of instruments, in the records of the observations, is evident also from other circumstances. For, it would appear that the transit instrument was furnished with *three* parallel wires; the whole of which are sometimes recorded, although in general the centre one only was used.* Thus, on October 24, 1721, the transits of *Venus* and of *Mars* are taken over the 3 wires: and on the following day, the transit of δ *Tauri* is also taken over the 3 wires. But the intervals of the wires, when reduced to the equator, do not correspond with each other in these several transits. And

* We often meet with the expression *intrat ad filum, exiit ad filum;* which denotes the passage of the star across the *side* wires. Sometimes the star is observed just as it is going out of the field of the telescope, which is denoted by the term *exiit ex aperturâ.*

the discordancies are so great, not only here but in other parts of the volumes, as to lead to a suspicion that different instruments have been used in these different observations. Again, on December 20, 1721, we find the transit of *Mercury* recorded by its approach *ad diagonium primum*, its transit over the centre wire, and its approach *ad diagonium secundum:* and here the intervals are much smaller than in either of the preceding cases.* In other places, mention is made of a *triangular* micrometer placed in the focus of the telescope: thus on January 15, 1722, we find the transit of *Mars* thus recorded, viz.

h	m	s	
6	24	48	♂ intrat triangulum.
6	25	38	♂ transiit.
6	26	28	♂ exiit triangulo.

Probably the sides of this triangle were made of *plates* of metal, instead of wires: for we find the transit of *o Sagittarii* on April 30, 1724, thus recorded, viz.

h	m	s	
15	52	56	o intrat ad laminam.
15	53	48	o transiit.
15	54	41	o exiit.

Perhaps the parallel wires, as well as the triangular plates, were inserted in the same micrometer; for we find them both mentioned in the same observation. Thus, on August 19, 1726, we have the observation of β *Aurigæ* recorded as follows; viz.

h	m	s	
17	41	5	Humerus Aurigæ β exiit ad filum.
17	41	25	exiit ab alterâ parte laminæ.
17	42	57	exiit ex aperturâ.

Be this however as it may, it is evident that more than one instrument has been used in making the observations: and, although this is sometimes

* In the *Phil. Trans.*, Vol. 31, page 113, Dr. HALLEY alludes to his practice of using cross hairs placed in the focus of the telescope at half right angles, so fixed as that the first of two stars nearly on the same parallel may pass over the centre of the glass, and move exactly along one of the threads: whilst the interval of time between the transit thereof and that of the following star is exactly measured by a pendulum clock, whereby the time of transit and difference of declination may be deduced. This was doubtless one of the microscopes.

stated in the MS. volumes, yet, as it frequently occurs without such indication, great caution is necessary in attempting to deduce any accurate results from the recorded observations.

The number of clocks used by Dr. HALLEY, prior to the erection of the mural quadrant, which took place about the end of the year 1725, appears to have been two; which he designates as *horologium majus*, and *horologium minus*. I apprehend that the former was so called on account of its being placed in the *great* room of the observatory: whence we may infer that the latter (called, by way of distinction, *horologium minus*), was placed in the transit room. We may conclude, therefore, that when the *horologium minus* was used, the observation was made with the transit instrument: on the contrary, when the *horologium majus* was employed, there is reason to suppose that one of the moveable telescopes in the great room was the instrument used. As the names of the clocks so employed are occasionally mentioned, we may thus obtain some clue to the instrument. All the observations however are recorded alike, in the MS. books: and I cannot too often repeat that great care and attention are requisite in separating the several cases.

Besides these two clocks, we occasionally meet with the term *monitor:* but, whether this was applied to either of the two above mentioned, or, whether it was what is now called a *journey-man* clock, or merely an *alarum* clock, it is difficult to decide at the present day.

During the time that the meridional passage of the stars was taken with the transit instrument only, the time appears to have been noted by the *horologium minus:* but, after the erection of the mural quadrant (at the end of the year 1725), we find the term *horologium murale* introduced; which appears to have been another clock, as they are all three occasionally mentioned in the observations of the same day. This last-mentioned clock seems to have been generally used (as indeed its name implies), with the mural quadrant, unless, as is sometimes the case, otherwise expressed. In fact, many instances occur about this period where there exists much confusion as to the clock, and consequently (since they were each in a different room), as to the instrument employed: and the observations in such cases require a close examination. For, though Dr. HALLEY frequently mentions the clock used, yet he appears sometimes to have omitted it: which is the more to be regretted because all the observations with these different instruments and

different clocks, are entered alike in the journals, without any discriminating mark or index.

It does not appear that the pendulums of either of his clocks were compensated for the effect of heat and cold: nor that the clocks had any *going-spring* to prevent their stopping when in the act of being wound up. In fact, the numerous stoppages they experienced (either from this cause, or from their being suffered to run down from absence or neglect), together with the frequent alterations in the rate, made by Dr. Halley himself (circumstances which occur frequently two or three times in a week), render it extremely difficult to deduce any very accurate results from the transit observations at such periods. In one case (December 14, 1725), it would appear that the clock had stopped *during the observation:* and it will undoubtedly sound strange to the modern astronomer to be informed that one cause of the irregularity of the clocks arose from the bob of the pendulum striking against the sides of the clock case.* Dr. Maskelyne, in a letter (a copy of which was found in Dr. Halley's MS. books), observes that Dr. Halley "seems to have reckoned his clock a good one: for he, in one place, applies "the expression *minimè fallax* to it." I confess I have not been able to discover this passage, although I have carefully inspected the whole of the manuscript volumes three or four times: but, even if it should exist, there is perhaps no term that was ever so ill applied; since there are very few cases where any of his clocks could be depended on for many days together. It is true that on April 12, 1726, he remarks, "*motus horologii maximè quidem* "*equabilis; tardat nonnisi* $1\frac{1}{2}''$ *per diem,*" and again on May 23, 1730, he says "*patet murale* [*horologium*], *hac temperie aeris, cœli revolutiones accuratè* "*representare.*" But these cases are rather mentioned as exceptions than as the general rule: and we even find that in the latter instance he was obliged, within 10 days, to put back the clock one minute, and to shorten the pendulum 2^s per day.

Prior to the erection of the mural quadrant, Dr. Halley seems to have adopted the use of apparent time; or rather, to have noted the error of the clock at the time of the sun's transit, without much regard to its rate of going during the intervals: and it is not always evident (there being no *regular* astronomical observations), how the rate could be accurately deduced.

* See Sept. 14, 1722, and Jan. 16, 1726.

Soon after the erection of that instrument, he adjusted the *horologium murale* to sidereal time (December 16, 1725); and in the following year (October 1, 1726), he adjusted the *horologium minus* also to sidereal time: but, this latter clock was occasionally altered backwards and forwards, probably to suit some temporary purpose. These frequent interruptions, either by his own alterations, or by the stopping of the clock in winding up, or going down, render it difficult to deduce a constant and regular rate for any length of time: an inconvenience which is felt, even to the very end of his observations. And it is certainly somewhat singular that after an experience of twenty years at the Royal Observatory, and much practice in astronomical instruments prior thereto, he should not have managed to keep his clocks in better adjustment, as he must have been well aware of its great importance towards obtaining accurate results.*

The mural quadrant having been completed, it appears that the first observation made with it was on October 20, 1725: from which period, onwards, we generally find both the transits and zenith distances recorded, after the example of his predecessor FLAMSTEED: still, however, with some confusion of the clocks, and also, for some time at least, with some apparent confusion as to the instrument employed. For, it is not always manifested (when the transits only are recorded), *which* instrument, or *which* clock has been used for the observation.

This quadrant, which was of 8-feet radius, was made by GRAHAM; and is described in Dr. SMITH's *System of Optics* above-mentioned, page 332. The limb had a double division: the inner one being divided into degrees and 12th parts of a degree: and the outer one into 96 equal parts, each of which was subdivided into 16 equal parts; and, by means of a nonius, again subdivided into other 16 equal parts. Thus, the observations could be read off to

* The following are some of the alterations, made in the *daily* rate of his clocks, during the latter period of his life: viz.

1730.	Oct. 20	10 seconds per day.	1733.	May 27	10 seconds per day.
1731.	May 16	5	——	Oct. 22	10
——	Nov. 1	10	1734.	May 6	10
1732.	Feb. 29	7	1735.	Mar. 14	not stated.
——	April 11	*paucula secunda.*	——	Nov. 9	12
——	June 13	6	1736.	May 1	$13\frac{1}{2}$
——	Oct. 9	not stated.	1739.	May 27	15

13″; and, by estimation, even to a less quantity: but Dr. HALLEY generally set down the nearest reading of the nonius, although we sometimes (but not very frequently), see the $\frac{1}{2}$ nonius recorded. The observations, therefore, may be wrong 5″ or 6″ from this cause. The double readings of the limb, which are usually recorded in the manuscript observations, are a check on each other; and, at the beginning of the second volume of the original observations (or at page 140 of the MS. copy), there is a table of corresponding parts for the two divisions. In the present MS. copy the readings of the divisions in degrees, minutes, and seconds, are placed in the last column: whilst the readings by the division into 96 parts are placed in the preceding collateral column. This plan has generally been adopted by Dr. HALLEY, although in a few instances he appears to have deviated from it.

This quadrant, which was made of iron, still exists in the Royal Observatory at Greenwich; and was used till the year 1753, when it was redivided by BIRD, and turned towards the north. In this state and position it has since been successively used by BRADLEY, MASKELYNE, and POND, until the erection of the two mural circles. The division of the arc was found by BRADLEY to be erroneous − 16″ on the whole quadrant: so that a proportional allowance must be made for this error, in all the zenith distances taken by Dr. HALLEY.

Fortunately, the principal observations made by Dr. HALLEY, more especially during the time that he had the transit instrument only, appear to have been conducted for the purpose of determining the position of the moon, and occasionally that of the planets. With this object in view, he usually observed such stars as were nearly on the same parallel of declination as those bodies, and differing from them very little in right ascension. Such observations, therefore, furnish records for those astronomers who are desirous of determining the positions of the moon or planets at those periods. Dr. HALLEY remarks, in his paper on this subject, inserted in the *Philosophical Transactions*, No. 369, Vol. 31, page 209, that "Of all the celestial observations that have hitherto been made, none are so capable of perfect exactness as the near appulses of the moon and planets to the fixed stars. For, though the places of the stars have not as yet attained an ultimate precision, yet these sorts of observations are ever good, and the places of the planets ascertained in proportion to the correctness of the catalogues that may hereafter be made." And this opinion, with which I believe

he and many of his contemporaries were strongly impressed, is perhaps the best key to the course of observations which he pursued.*

But, with respect to any accurate information relative to the position of the fixed stars, afforded by the present volumes, I conceive that the right ascension and declination of such of them as are to be found in modern catalogues, could be better and more correctly ascertained by carrying back their present places, by means of precession, to the period required, than by deducing their positions directly from Dr. HALLEY's observations. And, with respect to such as have been observed by him for the first time, their places may be ascertained by their *differences* of right ascension and declination from other known stars. For, I apprehend it would be difficult (if not impossible), to deduce a correct and independent catalogue from the observations themselves: neither are the stars observed in sufficient number to warrant the expense and trouble of the attempt. It was unfortunately the opinion of Dr. HALLEY, or rather, perhaps, the opinion of the age in which he lived, that the frequent and constant observation of the fixed stars was but of secondary importance; especially after they had been once observed and formed into a catalogue: and that the whole force and strength of the observatory should be devoted to the motions of the sun, moon, and planets, by observing their course amongst those stars, and recording their distances from those that lay near their path; whereby the solar, lunar, and planetary tables might the more speedily be brought to perfection; an object at all times (but more especially at that period), of considerable importance. This appears evident not only from the papers communicated by Dr. HALLEY and others to the Royal Society about this period,† but also from the conduct of NEWTON and HALLEY in having directed that the observations of FLAMSTEED should be so published as to include only those parts relating to the moon and planets; and leaving the mass of his observations of the fixed stars wholly unnoticed. I allude here to Dr. HALLEY's edition of FLAMSTEED's observations, published in 1712. But this practice does not accord with the views of the modern astronomer, who considers that, in a public observatory at least, the two kinds of observation should go hand-in-hand together:

* See the Astronomical Observations communicated to the Royal Society by the Rev. Mr. POUND and others, in the years 1716 and 1718, in the *Phil. Trans.* of that period.

† See the volumes of the *Phil. Trans.* alluded to in the preceding note.

and that the fundamental points of astronomy should be fixed by an accurate and well-determined catalogue of stars, corrected and enlarged from time to time. FLAMSTEED set a noble example of this kind, which was adopted and pursued by BRADLEY: and it may be fairly stated, that had it not been for their exertions, practical astronomy would probably have been but in a miserable state, at the present day.

Dr. MASKELYNE remarks (in the letter alluded to in page 178), that soon after the erection of the mural quadrant, Dr. HALLEY "made observations "both of transits and zenith distances at the quadrant. There are 13 "observations of the sun taken between November 9th and December 18th, "1725, and 3 on August 16th, 20th, and 22nd, 1726, in which the transits "of one limb of the sun were observed at the transit instrument, and the "other at the mural arch; and the two clocks compared together. There "are also 25 observations of stars, &c. (August 16th, 17th, 18th, 19th, 1726), "at various altitudes; the transits being taken at the two instruments: the "observations will give the error of the plane of the quadrant; the greatest "quantity of which, that I see, is 7″. In many parts of the limb Dr. HALLEY "notes that the error is nothing at all.* He observed the sun's zenith "distances constantly at the two solstices. From the vernal equinox of "1726 to the autumnal of 1728, there are 96 observations of the sun at "both instruments near the equinoxes, which might serve to settle the "right ascensions of some of the principal stars, supposing their differences "of right ascension known, or found from his other observations, if possible, "or found elsewhere: for he did not take care to observe transits of the "same stars at the opposite equinoxes. The stars chiefly made use of are "*Aldebaran, α Orionis, Procyon, Sirius,* and *Arcturus.*"

It is said that Dr. HALLEY never made use of a plumb-line: if so, it will be necessary to compute the errors of the mural quadrant, from the observations. But, where the observations relate only to those stars which are near each other, and on the same parallel, no material error can arise.

The altitudes of the centre of the sun and moon were taken by estimation; probably, deduced from the apparent equality of the segments, at the moment of the transit of the limb; in the manner sometimes used by FLAMSTEED.

* See Nov. 21, 1725, April 21, 1728, Dec. 10, 1737, and perhaps some other places.

It was Dr. HALLEY's frequent practice, for a few days before and after the full moon, to observe the transit not only of the perfect enlightened limb, but also of the ragged edge: this latter is denominated *sectio lunæ* in the manuscript books.

I shall now allude to some of the most remarkable phenomena recorded by Dr. HALLEY, in these volumes.

There are many observations of the superior planets: but none of them very near the time of their opposition to the sun. There are also several observations of *Venus* and *Mercury:* but not a single observation of the eclipses of *Jupiter's* satellites.

I have found but one double observation of *Polaris,* above and below the pole, on the same day; viz. December 3, 1721. In fact, this star does not appear to have been very frequently observed at any time.

The following are the only occultations that I find recorded: viz.

1721.	Oct. 25.	ϑ^1 and ϑ^2 Tauri.	1724.	June 26.	Saturn.
1722.	Oct. 7.	χ Capricorni.	1726.	Aug. 28.	ε Capricorni.
1723.	Feb. 26.	Anon.	——	Oct. 4.	Anon.
——	Mar. 16.	Anon.	1738.	Dec. 12.	α Tauri.

The occultation of *Mars,* on January 7, 1726, could not be observed on account of bad weather: and Dr. HALLEY was also prevented from observing the same phenomenon, on October 7, 1736, by ill health. The latter, however, was seen at Oxford by Dr. BRADLEY; and the particulars are recorded by Dr. HALLEY.

That rare and singular phenomenon which occurred on May 17, 1737, namely, the occultation of *Mercury* by *Venus,* was not observed by Dr. HALLEY at the moment of obscuration, on account of a passing cloud, which hid the planets at that instant of time. The two planets, however, were seen approaching each other, till *Mercury* was distant from *Venus* only about a tenth or a twelfth part of her diameter; when clouds came on: and, on their clearing away about eight minutes afterwards, *Mercury* was totally obscured. The account of this phenomenon, by Dr. BEVIS, has been inserted in the *Philosophical Transactions* for 1737, page 394.

I find recorded, likewise, observations of the three following stars: namely, that which was supposed to have been occulted by *Jupiter* on January 11, 1717, as mentioned in the *Philosophical Transactions,* Vol. XXX.

No. 351, and which appears to be Piazzi V. 192: another by the moon, during a lunar eclipse on August 29, 1718, as recorded by Flamsteed, and which appears to be Piazzi XXIII. 96: and the third by *Saturn* on June 26, 1724, which appears to be 28 *Sagittarii*. These observations were made for the express purpose of identifying those stars: and, with the same view, he observed also the right ascensions of the stars near which passed the comets of 1680, 1707, and 1723; the last of which comets appears to have occulted one of the stars that lay in its path. But, as the zenith distances are not given, they are not so readily identified. Dr. Halley, however, has furnished us with the means of identifying the star mentioned by Galileo to be near *Jupiter* at the beginning of March 1610, new style; and which is remarkable as being, according to Dr. Halley, the very first observation of that kind that was made with the telescope.* The zenith distance is only approximately given; and it appears to have been nearly on the same parallel as 109 *Tauri*, which star it preceded about $12^m\ 38^s$: so that it was probably the star observed by Lalande in the *Histoire Céleste*, page 32, at $4^h\ 56^m\ 2^s{\cdot}5$, and again in page 36. Dr. Halley states also, in the paper just referred to, that he had identified the star which is recorded by Riccioli to have been observed near *Saturn* on July 3, 1662, new style: but I do not find any star, so designated, amongst his observations. The identity of these stars, however, may be the subject of some doubt; as it appears that Dr. Halley determined their probable position by computing the place where the planets ought to have been at that time; and which, owing to the inaccuracy of the tables, may not be strictly correct.

The transit of *Mercury* over the sun's disc, on October 29, 1723, was observed only at its commencement by Dr. Halley: and the same phenomenon, on October 31, 1736, was observed only at its termination. At the Royal Observatory at Paris, however, they were more fortunate in observing this last-mentioned transit; since both the ingress and egress were observed there: and it is somewhat singular (and which is the cause of my mentioning it here), that the Paris observations are entered by Dr. Halley, in his book, in the regular order of date, after his own observations of that day, and without any interlineation: the next day's observations (Nov. 1st) being also regularly recorded in their due order, without confusion or derange-

* See his paper in the *Phil. Trans.* No. 369, for September, October, and November, 1721.

ment. This has given rise to a suspicion that the MS. books, which are the subject of this paper, are not the *original* entries: since it would have been impossible for Dr. HALLEY to have received intelligence from Paris, prior to the following day, where the entries are continued without interruption. The probable conjecture, however, is that the records of the observations were first noted down on slates, or loose pieces of paper (as still practised in many observatories), and that they were afterwards entered in the book, at the leisure of the observer, when they had perhaps accumulated to an amount beyond the limits of prudence or safety. I have found some of these loose pieces of paper, between the leaves of the books, and containing observations (apparently in their original state), exactly corresponding with the entries in the books: which confirms this conjecture. I would also remark that many of the entries in the books, especially in the first volume, were first written in pencil, and afterwards traced over with ink; which would seem to indicate the authenticity of the record, were it not that here also we find transits entered out of their regular order.*

Amongst the observations there are 5 solar, and 4 lunar eclipses: viz.

Solar.	*Lunar.*
November 27, 1722.	February 2, 1728–9.
September 14, 1726.	July 28, 1729.
September 3, 1727.	November 20, 1732.
February 18, 1736–7.	March 15, 1735–6.
July 24, 1739.	

The accounts of the first and the last of these phenomena were, as I have already observed, communicated to the Royal Society; and have been published in the *Phil. Trans.* of their respective years.

* In Dr. HALLEY's account of this transit of Mercury, published in the *Phil. Trans.* he remarks that the transit, which took place on April 24, 1707, "escaped unseen by all the "astronomers of Europe, excepting singly M. ROEMER, at Copenhagen." But, since the present Memoir was read before the Society, I have discovered, amongst FLAMSTEED's manuscripts at the Royal Observatory at Greenwich, a letter written by Mr. ABRAHAM SHARP, wherein he states that he also observed it, at Little Horton in Yorkshire; but under very unfavourable circumstances. The sun rose in the midst of a cloud, which having cleared off he observed *Mercury* just leaving the sun's disc, about 10′ below (or to the south of) his horizontal diameter, at $4^h\ 52\frac{1}{2}^m$ on the morning of April 25th. But he says that he was taken so much by surprise that he had not time to make any accurate measures, either of time or position. The portion of the letter detailing the circumstances here alluded to is given at full length in the *Monthly Notices* of this Society for April 1835.

There are three recorded observations, with the transit instrument, of the transit of the two singular stars 36 *Ophiuchi* and 30 *Scorpii*. It is well known that these two stars, although distant from each other upwards of 13 minutes of space, have a common proper motion of considerable magnitude. FLAMSTEED has only one observation of 30 *Scorpii*; and BRADLEY did not observe it at all, in right ascension: and, moreover, the catalogue of PIAZZI does not indicate a difference in the position of the two stars that precisely accords with present determinations made with great precision and accuracy. These observations, therefore, by Dr. HALLEY are so far interesting and satisfactory, as they confirm the uniformity in the motion of the two stars. The first observation was on July 10th, 1722, where the difference in the time of transit was 53^s: the second on July 15th, where the difference was 52^s: and the third on July 7th, 1723, where the difference was again 53^s. The mean of these is $52^s,67$; which being multiplied by 15·041 (since Dr. HALLEY's clock was *then* adjusted to solar time), will give the difference of right ascension, in arc, equal to $13'\ 12'',16$. FLAMSTEED made the difference equal to 54 seconds of the solar clock. Probably the mean of the whole should be taken, as neither of these astronomers observed to less than a second of time, except in very few cases: so that we should have the mean equal to $53^s = 13'\ 17'',16$; which accords very well with the determination ($= 13'\ 11'',4$) more recently made by Captain SMYTH, at my request, as given in the fifth volume of the *Memoirs* of this Society, page 166.

In the margin of the MS. books, are generally set down the equated (or mean) time of the moon's limb passing the meridian, together with the right ascension of the moon's limb at that time, computed (as I presume), from such observation. These values are the same as those which are printed at the end of Dr. HALLEY's *Tabulæ Astronomicæ*, edition 1749. The sun's longitude and mean anomaly are also frequently recorded: and occasionally we find similar computations relative to some of the planets, whose transits have been observed.

In the original MS. of the observations we frequently find the letters C.B., I.F., I.H., and E.H., annexed to several of the stars. The first two, viz. C.B. (*Catalogus Britannicus*), and I.F. (*Johannes Flamsteedius*), denote that the star in question is to be found in FLAMSTEED's British Catalogue: but it must be understood that the edition, here referred to, is generally that of Dr. HALLEY in 1712, and that consequently the numbers of the stars will not always correspond with those published afterwards in the third volume

of the *Historia Celestis.* The letters I. H. denote those stars which are in the catalogue of HEVELIUS, and which Dr. HALLEY supposed, at the time of observation, had not been since observed: and the letters E. H. refer to such stars as Dr. HALLEY himself imagined he had observed for the first time. Of this latter class there are perhaps about 500 observations: many of these, however, refer to the same star; and some of them, when examined, are found to be stars that have been previously observed by HEVELIUS or FLAMSTEED. It is only on a more minute and special investigation that the *original* observations of any *new* stars can be accurately ascertained. Some of the observations (probably about 20, in all), have the words " Error, J.F." marked against them; which denotes the detection of some real or supposed error in FLAMSTEED's catalogue: but these have principally arisen from some mistake in the computations.

We find also, in about half a dozen places, references to a Manuscript: as if the star in question were to be found in such work. But I am unable to throw any light on such reference; unless, indeed, it be the manuscript copy of FLAMSTEED's observations, which, there is every reason to believe, had (according to the statement contained in my account of FLAMSTEED's life, read before this Society in November 1833), been detained, by Dr. HALLEY, in spite of FLAMSTEED's remonstrance. I find similar references to a Manuscript, in an imperfect printed copy of Dr. HALLEY's edition of FLAMSTEED's Observations, published in 1712. This copy was lent to me by Professor RIGAUD of Oxford: the references are in Dr. HALLEY's hand-writing, and were evidently made prior to the book having been bound. It is probably the very copy which he received from the printer, when he edited the work.

Upon the whole, I consider these MSS. as valuable principally for the observations of the moon and planets (and more especially for the moon), since they appear to have been conducted and pursued principally for that object: and Dr. HALLEY was, at that time, anxious to perfect and complete his lunar and planetary tables. But, the places of those bodies could be more accurately determined at the present day, from his observations, than at the time of Dr. HALLEY: since the elements of reduction are now much better known; Dr. HALLEY having ascertained the position of his *determining star* by the application of precession only, without regard to aberration, nutation, or proper motion. The same remark might also be applied to

the positions of the fixed stars. Yet, it would be interesting to know what *new* ones had been observed by Dr. HALLEY. I fear, however, that the result and advantage of this inquiry would scarcely repay the time and labour that it would necessarily involve.

I have already remarked that, with the exception of three communications to the Royal Society, none of the observations made by Dr. HALLEY had been made public. This had probably been the subject of public complaint: and it was at length brought under the notice of the Council of the Royal Society, by Sir ISAAC NEWTON, President, at a meeting held on March 2, 1726–7; which I find recorded in the Minute-book of the Society in the following words: viz.

" The President took occasion to observe that, by the late Queen's letter, " concerning the visitation of the Royal Observatory at Greenwich (which " letter he ordered to be read), they were, amongst other things, directed " to demand of the Royal Astronomer,* within six months after every year " was elapsed, a true and fair copy of the annual observations he shall have " made. And that whereas this precept had not, of late, been observed, it " might be of ill consequence to continue in the neglect of it; and therefore " he thought it proper to take this opportunity, now the Royal Astronomer* " was present, to put them in mind of the said precept.

" The Royal Astronomer* hereupon acquainted the Council that he had " a multitude of unpublished observations by him, especially on the Moon's " place and motion. But, there being many uses to be made of the said " observations for forming a method for better ascertaining the longitude of " places, and a great reward being appointed by Act of Parliament for " discovering such methods, *he had hitherto kept his observations in his own " custody, that he might have time to finish the theory he designs to build upon " them, before others might take the advantage of reaping the benefit of his " labours.* However, as there were many observations for settling the places " of the fixed stars, which have not been yet rightly settled, he would " deliver to the Society the observations *on that head,* by Michaelmas " next."

It is worthy of remark, that this was the last meeting of the Royal Society at which Sir ISAAC NEWTON was present: as he died on the 20th of the same

* *Sic* in MS.

month (eighteen days only after this meeting of the Council), in the 85th year of his age.

In the year 1731, Dr. HALLEY recalled the attention of the public to an opinion which he had promulgated, about twenty years previously, relative to a proposal for finding the longitude at sea, by means of the motions of the moon: and in a paper inserted in the *Philosophical Transactions* of that year, took occasion to advert to the number of observations of the moon that he had made at the Royal Observatory: which amounted, according to his statement (the accuracy of which I have no reason to suspect), to nearly fifteen hundred. The major part of these observations, however, were made with the transit instrument only: so that the declinations remained still to be satisfactorily adjusted. But, it may be amusing to us to know, and may also in some measure lead us to judge of the state of practical astronomy at that day to be informed, that he considered it a subject of boast and congratulation that, by means of those observations the lunar tables were then rendered so exact that he was "able to compute the true place of the moon "with certainty, within the compass of two minutes of her motion, during "the present year 1731; and so for the future:" and therefore that *this exactness* was a motive for suggesting it as a means for determining the longitude. The idea, however, was an excellent one: and the method of lunar distances, then in embryo, is now become one of the most important and valuable means of determining the longitude at sea.*

I cannot close this short account of the contents of the MS. volume in question, without noticing the valuable assistance of Dr. LEE, the Treasurer, and Lieut. RAPER, one of the Council of this Society, who have examined and compared the copy thus presented to the Society by the Lords Commissioners of the Admiralty, with the original manuscript books: a laborious task, which they have executed with great care and attention. In the performance of this troublesome, yet necessary, undertaking, they discovered numerous errors of the amanuensis; arising partly from his own inattention, and partly from the obscurity of the writing. These errors were, by them,

* In the same volume of the *Phil. Trans.* (page 109), Mr. HODGSON, who gave a collection of the eclipses of *Jupiter's* satellites for the year 1732, states, as an argument for the use of these phenomena in determining the longitude, that "even with telescopes of different powers "and lengths, the difference of time amounts to scarce one quarter of a minute."

marked with pencil in the margin; and I have since re-examined and corrected the whole of them with the pen, so that there is every reason to believe that the transcript is now a faithful and accurate copy of the original. The loss of the original volumes, therefore, now becomes of less importance, in case such an unfortunate event should ever occur: and the information contained in them can be circulated more widely, and with more ease and comfort to the reader, without the risk of a total loss of the observations themselves.