VIII. *Some Particulars respecting the principal Instruments at the Royal Observatory at Greenwich, in the time of Dr.* HALLEY. *By* S. P. RIGAUD, *Esq., Savilian Professor of Astronomy at Oxford.*

Read May 13, 1836.

THE careful and detailed account which has been recently drawn up by Mr. BAILY,* leaves very little hope of much benefit to be now derived from Dr. HALLEY'S observations. An authentic copy of them has, however, been deposited with the Royal Astronomical Society; and as men of science may be induced to consult them, a knowledge of the instruments with which they were made becomes a matter of some importance. Dr. BRADLEY'S best observations were made with those which BIRD at a later period constructed for him; but a general account of what he found at Greenwich in 1742, is given in the *Memoirs*† prefixed to his miscellaneous works. A fuller description of what strictly belonged to an earlier time, could not, with propriety, be introduced in that place. Hence no opportunity was afforded for printing several distinct notices, which were found among BRADLEY'S papers, and which referred to the state of the Royal Observatory while Dr. HALLEY had the superintendence of it. The information which they contain, with some illustration from other sources, is now therefore submitted to the Society. Considerable interest belongs to every thing connected with this great national establishment; and, for the epoch now under consideration, little comparatively was known of its history, until Mr. BAILY'S communication recalled our attention to the subject.

The Transit.

Immediately on FLAMSTEED'S death, which took place on the 31st of December, 1719, Dr. HALLEY was fixed upon for his successor; for there is

* Memoirs of the Royal Ast. Soc., vol. viii. p. 169.

† Page lxxii.

in the Bodleian library a letter from Dr. MEAD to HEARNE, dated January 9, 1719-20, in which he says, "I have been so happy to get FLAMSTEED'S place for Dr. HALLEY, by means of my Lord SUNDERLAND." The Lord Chancellor PARKER, afterwards Earl of MACCLESFIELD, was also one of his most powerful friends and patrons upon that occasion, and his official appointment was executed on the 9th of February.* He did not, however, take possession of the house till the 7th of March,† and on the 6th of May he had not "got into the observatory;"‡ for he found it "wholly unprovided of instruments, and indeed of every thing else that was movable."§ In this difficulty he borrowed a quadrant from the Royal Society; but it is said to have been so ill made, that he could not use it. He was desirous, in consequence, of procuring FLAMSTEED'S sextant from his widow;|| but the unhappy differences that had existed personally between him and his predecessor, were a bar to this negociation, and in October 1720, he was still unprovided with the means of carrying on his observations.¶ This situation, he says, with painful recollection, "was the more grievous to me on account of my advanced age, being then in my sixty-fourth year."**

The same disgraceful and injurious parsimony, which had left on FLAMSTEED the burden of providing what was necessary for the observatory, still continued to operate, and the claim was set up by government to the instruments which his widow had, in full right, taken away with her. The attorney-general gave his opinion against the board of ordnance on this occasion; "but, at the same time, he advised a bill to be preferred against Mr. FLAMSTEED'S executrices, to oblige them to declare what they knew of this affair."†† These circumstances must have created great delay; and, as we only learn that the defendants had given their answer in March 1721, it is probable that more time elapsed before this oppressive measure was abandoned. Mr. CROSTHWAIT, in a letter dated October 28, 1721, says, "I hear nothing further of Dr. HALLEY or the office of ordnance; the

* MS. Life of Halley in the observatory at Oxford. Circumstances seemed to indicate that it might be written by ISRAEL LYONS, but it is certainly of an earlier date.

† Account of Flamsteed, with his Correspondence and Catalogue, published by Mr. BAILY, p. 334.

‡ Ibid. p. 335.

§ Phil. Trans. vol. xxxvii. p. 192.

|| Account of Flamsteed, p. 338.

¶ Ibid. p. 340.

** Phil. Trans. vol. xxxvii. p. 192.

†† Account of Flamsteed, p. 342.

solicitor belonging to the office tells me, that the board will proceed no further against Mrs. FLAMSTEED for the instruments:"* some, however, had now been procured, since the first recorded observation was made on the 1st of October.†

LALANDE, in 1759, reprinted HALLEY's tables for the planets and comets: he prefixed a historical preface to the volume, and he says in it, "M. HALLEY se procura en 1721 une lunette de six pieds faite par M. HOOK, mobile dans le méridien sur un axe, avec laquelle il commença à observer tous les jours la lune à son passage au méridien."‡ In his *Astronomie* (1792) §, he repeats the same statement, with the single variation of altering the year to 1722; but this is a mistake: in a later part|| of the same work, he says, after speaking of RÖEMER's transit, "HALLEY fit faire, en 1721, un pareil instrument, que l'on conserve encore à Greenwich, mais dont on ne fait plus d'usage; l'axe est de fer, et la lunette a environ cinq pied de long." Dr. MASKELYNE, in the preface¶ to his *Observations*, calls it a five feet transit; and Mr. BAILY** has marked the discrepancy between this measure and that which Dr. SMITH has given in his *Optics*, sect. 841, but which only differs from the others in being more precise. As there could be no doubt about the instrument to which they alluded, the visitors of the Royal Observatory in 1726, LALANDE and MASKELYNE, satisfied themselves with giving the description in a certain number of feet, without thinking it necessary to enter into the particulars of odd inches; the exact†† length being 5 feet $6\frac{1}{2}$ inches, which only exceeds Dr. SMITH's numbers by half an inch. He himself, however, uses the same indefinite method of description in another respect, for he speaks of the transverse axis being "about an ell long," and it is 3 feet 6 inches. There is a memorandum made by BRADLEY, on the

* Account of Flamsteed, p. 345.

† Memoirs of Ast. Soc. vol. viii. p. 171.

‡ Page v.

§ Sect. 537.

|| Sect. 2388. LALANDE in this place seems to have forgotten that HOOKE died in 1703; but the date of his earlier publication (1759) was within the fair reach of tradition, especially when it is recollected that he was brought up under LE MONNIER, who not only was in correspondence with BRADLEY, but certainly visited him at Greenwich, and even observed there with this very instrument. Among the unpublished observations with the transit, several occur 1743, June 6 and 7, against which "M. MONNIER," "M. MON," are written.

¶ Page 1.

** Note, p. 171.

†† For this and the other measures of the parts of HALLEY's transit, I am indebted to Mr. TAYLOR, the late senior assistant at the Royal Observatory.

1st of August, 1743, in which he says, " this day I put a shorter eye-glass into the transit telescope; its focus is about $1\frac{1}{2}$ inch. So that the telescope magnifies now about 40 times." There will be no great violence in supposing, that 1,6 inches may be the quantity here spoken of, and then the focal length of the object-glass will come out 5 feet 4 inches; so that the sum of the distances will be 5 feet $5\frac{1}{2}$ inches, and allow for the tube extending a little beyond the further lens. It is to be regretted that no precise description has been found of the old eye-glass, so that we can only collect that HALLEY observed at his transit with a power of less than 40. The aperture of his telescope was only $1\frac{3}{4}$ inches, which allowed him but a small portion of light from the objects that were passing through the field of view.

HOOKE, as FLAMSTEED complains,* persuaded Sir JONAS MOORE to entrust him with the construction of a ten feet quadrant for Greenwich; but when it was set up, " inutile compertum est† . . . organum hoc debile nimis et tenue."‡ It is not therefore suprising, that he equally failed in giving firm and steady action to his transit. It must indeed be recollected, that it was probably one of the very first instruments of the kind which was made in England; and it is impossible to tell what may have been done to it in the twenty years during which, at least, it had existed before it came into HALLEY's possession. We cannot now ascertain whether it was or was not a part of the original construction, but SMITH speaks of the axis being " surrounded with light braces of brass, to keep it from springing." These were applied in the following manner: At 8 inches from one extremity of the axis a plate was fixed across it, to which, by means of metal rings, the telescope was attached; nearly from the ends of this plate four pieces arose, of about $8\frac{1}{2}$ inches in length, forming two triangles, above and below the tube; from the vertices of these two triangles, and from the parts where, at their bases, they made angles with the telescope, four bars extended to each end of the axis, and these bars, on the longer side, were tied together by diagonal pieces, which crossed each other on the axis. In the early days of machinery, the addition of these braces may have carried with them some promise of advantage, and HALLEY probably did not observe the evil which they occasioned; but BRADLEY, as soon as he examined this instrument, in 1742, found, that from every change of temperature, it was liable to the injurious

* Account, pp. 42, 43. † Histor. Cœl. Prolegomena, p. 106. ‡ Ibid. p. 108.

effect of unequal expansion* in them. There seems originally to have been a strong prejudice in favour of narrow openings for the meridian. RÖEMER† confines them to four inches, and BRADLEY, even for BIRD's transit, only ventured to increase his width to six:‡ the changes which he detected in HALLEY's instrument, made him probably afraid of allowing the sun's rays to fall unequally on the axis of his telescope. It is most likely that HALLEY's shutters were not broader; especially as there is no reason to believe that the room erected for his transit, at the west end of the house, was of any large dimensions.

There is a book, which BRADLEY used for making occasional calculations and rough entries of any thing, on which he happened to be occupied. The following are some of the memoranda which occur in it.

"1726, September 1.—I went to Greenwich; and Mr. GRAHAM and I adjusted the meridian telescope at right angles to the axis. Upon trial, we found it varied from the truth about $1\frac{1}{2}$ diameter of the thread, which amounted to not more than about $\frac{1}{8}$ of a minute;§ the line of collimation being so much towards the east. After the line of collimation was truly at right angles to the axis, we set [the] thread exactly to the mark made for a meridian on the park wall, near Admiral HOSIER's house, and then looking backwards, to the north, we found that the left side of the wire just touched the extreme point of the top of a house belonging to some hospital in Greenwich, leaving the said point to the left hand of the thread. It likewise cut a light in a garret window of some house in the town, so as to make the part that was to the right of the thread a small matter less than the other."||

* Misc. Works, p. 1. Professor WOODHOUSE, in the *Phil. Trans.* for 1827 (p. 144), gives an account of the action of the braces on the Cambridge transit. From the representation given by HORREBOW, RÖEMER appears to have made his telescope in two frusta of long cones, united at the centre by their bases. This might be inconvenient when the object-glass is of considerable aperture, but such a form would be less liable to flexure than a cylinder. BIRD left the axis of the transit, and the tubes of all the instruments that he made for Oxford, rough as they came from under the hammer: and he was right; for these surfaces are the densest parts of the metal, and by their being polished off much must be lost in the stiffness of the materials.

† Miscellanea Berolinensia, vol. iii. p. 278.

‡ Maskelyne's Gr. Obs. vol. ii. p. 160.

§ Of space, or 0,5 second in time.

|| Immediately after this paragraph, we find in the manuscript: "The difference betwixt the lengths of the ends of the axis, as measured by Mr. GRAHAM in June 1742, is $26\frac{1}{2}$ inches." There is some little difference in the writing from that of the part which precedes it. The lines

"September 2.—This day I went to Greenwich again; and Mr. GRAHAM and I set the meridian axis truly level, and then brought the mark to agree exactly to the wire plumb-line, when hung truly into the notch made for that purpose."

From hence it is clear, that HALLEY had got his transit adjusted, and before this time had fixed his meridian mark. BRADLEY, indeed, found occasion afterwards to alter it. In 1742, he says, "According to the observations taken August 14 and 15, *Capella* was about 3″ longer in passing the western arc than the eastern; and, as a difference of one minute in azimuth would cause a difference in the transits of this star of $12\frac{1}{4}''$,* it follows that the telescope, when set to the mark on the wall, points $\frac{1}{4}$ of a minute to the west of the true meridian. The observations taken August 3 and 4, make the error but about half as much." In the following January he made the necessary alteration in the position of the mark; but the fault was not originally in HALLEY's determination; for in June 1744, BRADLEY made the following memorandum: "The instrument now (when I look northward) points very near the same marks as it did when Mr. GRAHAM and I adjusted the line of collimation in the year 1726; but, as the southern mark has been made nearly two inches more easterly than it was before, it is certain one or other of the marks must have changed its position; and I judge, from the present circumstances, that 'tis the mark on the park wall that has altered, for I find the marks that I made on a chimney in Greenwich do exactly correspond with the same distant objects with which I formerly compared them. The same consequence may be likewise gathered from the transits of *Capella* above and below the pole; for, when the instrument was adjusted to the northern mark on the chimney (as it was till after I new-rectified the line of collimation on June 6), the star was the same time in moving through each semicircle; but after this last rectification, the star was longer, by 3″ or 4″, in passing the western semicircle, than it should have been had the southern mark by which it was rectified been in the true meridian."

are shorter; and though the colour of the ink is nearly the same, the note appears to have been only inserted in a place which had been left vacant at the bottom of the page. Each of the memoranda for September 1 and 2 were probably written at the time when they are dated; notes of observations in August 1726 come immediately before them, and they are followed by others of the 6th of September in the same year.

* This has been corrected in the margin to $10\frac{1}{4}$.

If there are any late observations taken by HALLEY with his transit, it may become a question, whether an allowance ought to be made for the error, which might be occasioned by this sinking of the wall. BRADLEY'S transit at Wansted, like HALLEY'S, had not the telescope originally in the middle of the axis, and he made the necessary provision of two meridian marks, at a proper distance from each other, so as to adjust his instrument on them when he inverted it. No mention has been found of any thing similar at Greenwich, and indeed there would have been little use in it. There is reason to believe, that BIRD was the first who contrived the apparatus with a strong perpendicular screw, for lifting the transit off its YY. The smaller transits of earlier days would have been lifted by hand, and to have taken hold of HALLEY'S would have thrown it immediately out of adjustment. It appears, also, that he had not himself fixed any cheque to the north; and it is possible, after all, that he did not frequently trouble himself about the adjustment to the meridian mark; indeed, there is something very extraordinary in the two friends going down to examine the position of the instruments for the astronomer royal, without there being any notice of his taking a share in the operation.

The use which was made, upon this occasion, of the plumb-line, is, unfortunately, not fully explained. Dr. MASKELYNE sometimes hung such a line in a notch at one end of his telescope, and, when the tube was set up perpendicular, examined its coïncidence with a mark on the other extremity: the braces, however, of HALLEY'S transit would have interfered with any experiment of this kind, as well as with RAMSDEN'S method, described by VINCE in his *Practical Astronomy.**

It may be observed, that, in setting the telescope, BRADLEY speaks of the wire or the thread without any epithet of distinction, which would imply that there was more than one perpendicular to the horizon. This, however, is not conclusive; and as Mr. BAILY† considers that there were three, it may be right to state the ground upon which, in another place,‡ I had previously ventured to express a different opinion.

In a letter written by FLAMSTEED to Dr. EDWARD BERNARD (Oct. 20, 1681), which is preserved in the Bodleian, he says, "crux unica filaris cuique tubo sufficit, pluribus non opus est." This certainly shews that more had then

* Sect. 81. † Page 175. ‡ Bradley, Misc. Works, p. li.

been thought of, but it as certainly shews that the highest authority of the day in practical astronomy was decidedly against the use of them. It is very possible that HOOKE may have satisfied himself with the same view; and, as the instrument came ready made into HALLEY'S hands, there is at least as much probability, *a priori,* in favour of the one supposition as of the other. BRADLEY'S* account, however, seems rather to indicate that the simpler construction was originally adopted; for, in stating what he found necessary to have done to the transit, before he made use of it in 1742, he says, "at the same time Mr. SISSON put in two other wires (15′ from the middle wire), to be made use of for taking of the transit of objects, in case clouds, &c. should hinder me doing it at the middle wire."† Now, if these side wires had only been put in to replace what had already been used by HALLEY, there could have been no occasion for giving any other reason for their insertion: and BRADLEY'S allusion to what might hinder himself from taking the central observation seems surely to imply that he was adopting a precaution, for which a provision had not been previously introduced into the telescope. It was one, likewise, to which he was accustomed; for there is the following memorandum which POUND had made respecting his own transit at Wansted:

"The angles between the perpendicular threads in the focus of the meridian instrument are,

"Between the 1st and 2d ..	24′ 26″,	that is, in time, 1′ 37″ 44‴	the sun being on the equator.
"Between the 2d and 3d ..	23 57	.... 1 35 48	

"1724. July, Aug., Sept."

POUND appears to have been very attentive to the rates of his clocks; and the book in which he entered his observations for that purpose shews that, to get his time, he began by observing the instant when a given object passed the prime vertical; but, notwithstanding the more rapid motion in altitude, this was a direction in which, for a considerable part of the year, he could not see the sun, and he subsequently reduced his azimuth to one-half or (as he more accurately determined the position of his instrument) to 46° 28½″. In this manner he seems uniformly to have gone on to the 14th of September, 1724, from whence till his death the transits are entered for the meridian passage. It may, therefore, be fairly concluded that this instrument was

* Bradley's Miscellaneous Works, p. 382. † Ibid.

erected in the summer of 1724, and, consequently, may have been constructed with improvements, which BRADLEY, who constantly used it after his uncle's death, was willing to introduce into HALLEY'S telescope, which was of a much earlier date.

Mr. BAILY says,* "On October 24, 1721, the transits of *Venus* and of *Mars* are taken over the three wires, and on the following day the transit of δ *Tauri* is also taken over the three wires. But the intervals of the wires, when reduced to the equator, do not correspond with each other in these several transits." Now, this fact would be very difficult to reconcile with a system of wires, which were all perpendicular to the horizon, and seems rather to point out that the observations were made with micrometrical wires, which HALLEY appears sometimes to have called diagonia, and which crossed each other, according to CASSINI'S† arrangement.

Assuming that the hours are those of solar time, the observations‡ of γ and α *Tauri*, on Dec. 22, 1721 (O.S.), may have been meridional; but *Jupiter* was then about $11^h\ 20^m$ beyond the latter in right ascension, and, consequently, could not have come up to the meridian till near 21^h. On the following day, *Venus* and *Mercury* were very near each other, with little more than 17^h of right ascension, so that they preceded the sun by about an hour and three quarters: consequently, they could not have been on the meridian between seven and eight o'clock in the morning.

The observation of *Mars* on Jan. 15, 1722, and of ο *Sagittarii* on April 30, 1724 (O.S.), may both of them have been meridional, and as, in August, 1726, HALLEY had begun§ to use sidereal time, β *Aurigæ* may have been observed by him, as it passed the meridian S.P. on the 26th. From these instances, therefore, it appears that he may have had the triangular|| wires

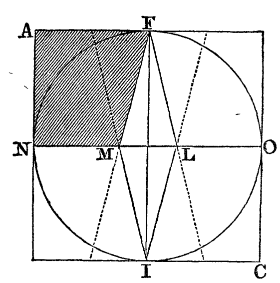
* Mem. Ast. Soc. vol. viii. p. 175.
† Ibid. p. 176, note.
‡ Ibid. pp. 175, 178.
§ Ibid. p. 179.

|| It is remarkable that HOOKE (as may be seen in his Posthumous Works, pp. 526, 527), as early as 1685, had not only devised this micrometrical arrangement of lines, but that also which has been known by the name of BRADLEY'S reticule: he does not, however, in his description of them, allude to their use for astronomical purposes. CASSINI, in the *Mém. de l'Académie* (1717, p. 262), gives an account of his endeavour to ascertain the parallax of the fixed stars by a telescope fixed in the meridian at the mean altitude of *Sirius*: a method exactly the same in principle, though much inferior in mechanical application, to that adopted by Mr. POND (*Phil. Trans.* 1817, p. 159). For this purpose he used his micrometer in 1714 (p. 264). LA CAILLE, who made great use of the reticules, describes, in his *Cœlum Australe* (p. vi.), his method of

in the focus of his transit. The transit which LE MONNIER describes* had, indeed, three parallel wires, as well as the other two at half right angles, but it was made at a much later date than HALLEY'S.

The Quadrant.

The Board of Ordnance, as early as October 1720, were willing "to allow Mrs. FLAMSTEED something for the sextant,"† sooner than leave HALLEY without the means of carrying on any observations. It is probable that, with the same object, the transit was purchased at no very considerable expense; but it was not sufficient for what the establishment required. MOLYNEUX wished to purchase FLAMSTEED'S mural arc, but the negociation failed,‡ and it seems to have been expected that HALLEY would have endeavoured to procure it.§ This instrument will remain for ever famous in the history of astronomy, for having been the foundation for a most valuable part of the *Historia Cœlestis;* but, although it took in the revolution of *Polaris,* HALLEY was desirous that the observations should be extended to other circumpolar stars. He, therefore, laid the plan of providing the observatory with two quadrants, and the second was actually begun (as we



constructing them. He inscribed a circle in a square on a plate of metal; he then drew the lines in the positions of proper intersections: "jam vero omne fere spatium in circulo NIOF, cui quadratum AC circumscribitur, comprehensum, lima corrodatur, ita ut solæ laminæ FL, LI, IM, spatiumque trapezoidale ANMF supersint; ad extremum protendantur et ad angulum rectum se mutuo intersecent fila FI, NO, eritque reticulus absolutus." The part of the plate AFMN was, also, left as a means of distinguishing whether the stars had passed above or below the horizontal wire. Though HALLEY'S cross lines were not drawn exactly in the same order, these particulars may explain his use of the word "lamina." With respect to the aperture of the telescope, BRADLEY made the same use that he did of it: for, having changed the wires of his transit at Wansted, he says (Aug. 23, 1729), "the two wires, that are fixed parallel to the middle one, are pretty near the same distance from it as those were before I made this alteration, but the exact distance of each must be found by observation; as also the edges of the brass aperture, which are likewise filed nearly parallel, in order to be made use of also for determining when an object that does not happen to be observed at the middle wire would have been there, &c."

* Histoire Céleste, p. lxxvii. † Account of Flamsteed, p. 340.

‡ Account of Flamsteed, pp. 343, 346. § Ibid. p. 343.

learn from the report in 1726 to the Royal Society), although the funds supplied by the government were insufficient for its completion. The more limited extent, however, of the instrument that he did procure, was, in some measure, compensated by the superior skill with which GRAHAM constructed and divided it.

FLAMSTEED'S pier was of brick, with beams of wood built into it, on which his mural arc was suspended; it was situated also near the declivity of the hill, which caused it gradually to sink.* For this alteration he ascertained an annual allowance, as a correction for the error which it occasioned;† but HALLEY saw that it was better to take it down (which he did in 1721),‡ and to build a new one of stone, which, being removed from the former position, might also give a better view of the northern horizon. On the eastern side of it GRAHAM'S quadrant was erected in the latter part of 1725, and was immediately brought into use. This famous instrument has been so often described, that it would be useless now to repeat any general account of it;§ but there are circumstances connected with its state, while in HALLEY'S keeping, which are probably as yet nearly, if not wholly unknown, and which, on the present occasion, may be worth recording.

After the adjustment of the transit on the 2d September, 1726, BRADLEY goes on to say, " We likewise set the mural instrument by the help of a wire plumb-line, so that the centre was truly perpendicular over the first division of the limb, and then brought the moveable plumb-line to agree exactly to the middle of the hole, at the intersection of the lines, which were made for a mark to know whether there was any alteration in the situation of the instrument: then we fastened the screws, &c. belonging to the moveable plumb-line.

" Memor.: the wire was free from the centre plate, near the thickness of a sixpence, and from the nearest part of the limb about half as much."

Mr. BAILY has mentioned|| the tradition of HALLEY'S never having used the plumb-line. GRAHAM appears to have made provision for two, one of

* Account of Flamsteed, p. 356. Flamsteed, Prolegom. p. 109.

† Ibid. p. 357. Prolegom. p. 132. ‡ Ibid. p. 343.

§ We learn from CROSTHWAIT'S letter (Account, p. 359), that, during the summer of 1725, GRAHAM divided the limb in the great room of the observatory.

|| Mem. Ast. Soc. vol. viii. p. 182.

which was to hang from the centre,* and the other (called moveable) to be attached to any part of the frame† which might be thought to be most convenient. BRADLEY'S expressions seem, however, to intimate, that these were not at first kept constantly in their places, but were hung on when the instrument was to be examined. This is a subject to which HALLEY possibly did not very frequently attend; but, in 1730, some of the fastenings having given way (as will be hereafter more fully explained), it could have required no very delicate test to ascertain that all was not right. In this difficulty he appears at last to have had recourse to BRADLEY, and he certainly could not have done better. Independent of other qualities, BRADLEY had the great advantage of being himself a workman: we find him cleaning his own clock, taking his telescope to pieces, changing the eye-glass when he found it desirable, and putting in new wires or altering the position of the old ones: and nothing escaped his attention.‡ The result of his examination will be seen from the following memoranda:

"June 28, 1734.—I examined Dr. HALLEY'S mural arch, and found that the centre was fallen towards the south so much, that the wire of the plummet, instead of bisecting the spot (that it was at first adjusted to), fell southward of it, so that the north edge of the wire touched the south edge

* Possibly, in the same manner as in MOLYNEUX'S zenith sector (BRADLEY'S Misc. Works, p. 95).

† Thus, 1742, Aug. 18, BRADLEY says, "I adjusted the quadrant with a plumb-line, and brought the centre to lie exactly over the beginning of the divisions; then, fixing another plumb-line in the notch at the top, I found that it exactly corresponded with the mark below, to which it had been adjusted by Mr. GRAHAM in 1726." Again, Sept. 11 of the same year, "I made a new point between the 19th and 20th of the outer divisions .... in order to adjust to it the moveable plumb-line, which I now keep constantly hanging from the notch above; the mark which was formerly made for that purpose by Mr. GRAHAM being somewhat too big. The old mark lies below, and a small matter nearer the line of the beginning of the divisions than the new one. N.B. The moveable plumb-line was made to correspond exactly with this new mark, when another plumb-line, hung from the centre of the quadrant, beat against the beginning of the divisions."

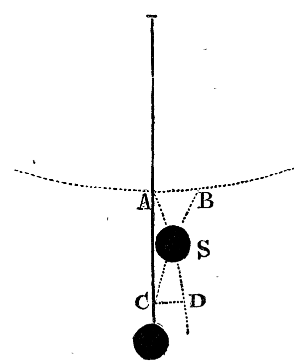
‡ An instance of this occurs while he was at Wansted in 1725; for we find, "Aug. 4. Memorand. In adjusting the telescope to the mark, the end of the axis must be always made to bear against the piece of brass fixed on the right hand." He had, therefore, observed the fact, which is so clearly shewn by Mr. BAILY in his Report on Captain FOSTER'S Pendulum Experiments (p. 31), and had guarded against the error, which might be produced by any variation of the parts of the axis that rested on the points of support.

of the spot; the difference of the present situation of the arch from what it was when last adjusted (which, I believe, was about a year after it was first set up), amounting, as far as I could estimate, to about 20″. The index now giving the zenith distances 20″ too small.

"The top of the wall is likewise fallen westward, or else the arch of the instrument (which seems more likely) is got further from the wall, near as much as the thickness of half-a-crown, for I found it necessary to force in the limb so much before the wire of the plummet could play freely.

"How much the quadrant is altered from the situation it had when first adjusted, may more certainly be found from these data (the forementioned error of 20″ being what I only guessed it to be by comparing it, as well as I could, without actually measuring the difference).

"At the time of this experiment the south side of the wire seemed to fall against the north edge of the small arc SA, where that arc cuts the outermost concentric circle AB. S being the spot where the two small arcs SA, SB intersect.



"July 26, 1735.—Mr. GRAHAM, Mr. MACHIN, and self, examined again the situation of Dr. HALLEY'S mural arch, and found, as before, that the wire of the plummet beat to the southward of the spot (and, I believe, very nearly at the same distance as in my last year's experiment), for the middle of the wire, at this time, did not beat so much to the southward as the middle of the small arc SA, where it intersects the concentric circle AB; though the south edge of the wire seemed within the north edge of the stroke SA. So that, if the former remark of the beating of the plummet was just, the wall or instrument must have gone this last year a little more to the southward at the top, but not more than the semidiameter of the wire, which scarce exceeds 2″ or 3″.

"But, in order to determine more exactly the present situation of the instrument, and to find how much it varies from what it was when first adjusted, we now observed the point C, where the plumb-line cuts the little arc CSB, below the spot S, and then Mr. GRAHAM, with a small beam compass and scale of equal parts, measured the distance CD between the two small arcs (at that distance from the spot S) where the plumb-line cut the arc CSB. This distance was found to be ·014 parts of an inch, which being the double error, we concluded that, at this time, the plumb-line falls $\frac{7}{1000}$ of an inch to the south of the spot, which amounts just to 15″. So that the zenith distances shewn by the index are 15″ too small at this time. I this day took the zenith distance of *Arcturus* as follows: By the inner division, 30° 53′ 12″; by the outward, 32° 15′ 2½″.

" From this experiment it appears that I have guessed the error too much by 7″ or 8″ in the former experiment."

HALLEY had now nearly completed his 79th year; and, after the labours which he had gone through, it is only wonderful that he could still apply himself to any active employment. He must have felt that the end of life was possibly at no great distance; and others with the same impression would, probably, abstain from any interference with the affairs of the observatory. We must not, therefore, be surprised at the quadrant remaining without repair, nor at its having suffered still more in the subsequent years, during which his existence was so greatly prolonged. The state in which this fine instrument was left at his death, has been described by BRADLEY in a paper, which is printed in his *Misc. Works*.* This was drawn up from memoranda that he had made when examining the instruments, on his becoming Astronomer Royal; and it may not be amiss, on the present occasion, to extract such of the original notes as bear immediately upon the question.

[July] " 5. This day I discovered that the screws, which held the brass cylinder (about which the telescope moves), were all broken, so that the cylinder itself moved with the telescope, which was now supported by means of the smaller part of the brass centre-piece, which turned in the hole of the square brass plate, that is screwed to the iron work. In order to judge whether this occasioned any alteration in the observed zenith distances, I made that night the following observations:

" 16ʰ 8ᵐ 18ˢ–	....	ζ Ophiuchi	....	65	9	0–	....	61° 27′ 40″
30 47	....	κ Ophiuchi	....	44	7	0–	....	41 39 25
40 55½	....	η Ophiuchi	....	71	4	4⅓	....	66 48 50

[July] " 6. This day I took off the telescope, and found the brass cylinder (about which it used to move) adhering so closely to the inside of the steel collar that is fixed to the telescope, that I could not move it, or get it out,

* Page 381.

which must have occasioned the breaking of the screws. I then took off the steel collar, and carried it, with the centre cylinder in it, to Mr. GRAHAM, who forcing out the brass cylinder, afterwards turned it a little smaller, to prevent the like accident for the future. The next day I examined the situation of the quadrant, by hanging a plumb-line near the centre pin, made for that purpose. And in order to judge how much the position of the quadrant was altered since it was adjusted by Mr. GRAHAM and myself in July 1726, I fixed a micrometer above the centre of the quadrant, in such a manner that a wire plumb-line, hanging from its moveable plate, might be brought (by turning the screw) to correspond exactly with the centre of the quadrant; then noting the division, that the index of the micrometer pointed at, I afterwards turned the screw till the plumb-line below came to the beginning of the divisions on the arc, and found that the index was moved $\frac{26}{40}$ of one revolution. By proceeding in the same manner, I afterwards found that 10′ on the limb of the quadrant corresponded to $11\frac{12}{40}$ revolutions of the index of the micrometer: from whence it follows that $\frac{26}{40}$ answers to $34\frac{1}{2}''$, which was, therefore, the error of the instrument at this time: the observed zenith distances being $34\frac{1}{2}''$ too small."

After shortly reciting the examinations, above stated, in 1734 and 1735, he then goes on to say, "In this last comparison I could easily perceive, that the plummet beat more southerly than in either of the former trials; and the instrument seems to have changed its situation about as much since the year 1734, as it had done in the like time of eight years before; and if we suppose that the alteration was pretty regular and proportional to the time, we may allow about 2″ per annum from the time it was first adjusted to the year 1742." He then mentions the observation which he had made of *Arcturus* in 1735.

"When I compared the plummet in 1734 with the spot, I at the same time took notice, that the arc of the quadrant at the bottom was got too far from the wall, so that I was obliged to force it inwards near $\frac{1}{10}$th of an inch before the line was free. This seemed to be owing to the brass cocks, that were fixed into the wall with cement, having given way and becoming loose, as I found two of them, near the lower part of the limb, were in 1742; for which reason I have now fixed them with lead instead of cement; for I find that the bottom part of the limb must be bent inwards with considerable force, before it can be made to correspond nearly with the plane of the other parts."

" July 9, 1742. The brass cylinder was this day fixed again at the centre with new screws, and the steel collar, as before, to the telescope.

h m s			° ′ ″
" 13 48 19½	Arcturus	32 15 12	30 55 22
16 7 35 cir. ..	ζ Ophiuchi	65 9 0—	61 27 40

[July] " 10.

" 13 48 0 cir. ..	Arcturus	32 15 12	30 55 22
16 7 22½	ζ Ophiuchi	65 8 15½	61 27 35
— 29 49+	κ Ophiuchi	44 6 15½	41 39 30

" N.B. From the zenith distances of κ and ζ *Ophiuchi*, taken this night, compared with those of July 5, it appears, that when the telescope turned upon the smaller part of the brass cylinder, the zenith distances were much the same as at present, and therefore, though the screws might have been broke long before it was discovered, no sensible error would be thereby occasioned in the observed zenith distances.

[July] " 20. By the zenith distances of η *Ophiuchi*, taken at the entrance and as it went out, it appears that the wire is not exactly horizontal; the zenith distances at the entrance being about 15″ greater than at the going out of the object. The like differences I observed in other stars."

" Upon August 4th, and 13th and 14th, I took the transits of above 60 different stars at different altitudes, both with the mural quadrant and the transit instrument; and, upon making proper allowance for the clocks, I found that the stars came sooner to the middle thread of the transit instrument than to that of the quadrant, as in the following table:

Zenith dist. of the object...	1½° N.	1° N.	0°	3° S.	6°	12°	15°	20°	25°	30°	33°	35°	40°	45°	50°	55°	60°	65°	70°	75°	80°	85°
Diff. in time between the transits at both inst^s. ...	31″	30″	27″	21″	15″	10″—	8″+	8″	7″+	6″	5″—	4″+	4″+	4½″	5″—	5″	5½″	6″—	6″—	6″	6½″	7″
Error of the transit inst...	...	...	...	...	...	0¼	...	0½	...	0½	...	...	0¾	...	0¾	...	1	1	1	1	1	1¼
Errors of the plane of the quadrant from the true meridian	31	30	27	21	15	10	8½	8½	8—	6½	6+	5—	5	5+	5½	5¾	6½	7—	7—	7	7½	8+

" The second column* in this table shews how many seconds the object came at the quadrant, after it had passed the transit instrument, which

* For the convenience of insertion, the columns of the table are here copied out in horizontal lines.

would be the true correction for the plane of the quadrant, if the transit instrument was truly adjusted to the meridian; but, by comparing the transits of *Capella* above and below the pole, it appeared that the telescope is directed about 10″ or 15″ in azimuth to the west of the south point. Upon this account, the object coming later to the transit instrument than to the true meridian, as in column 3d, these numbers, added to those of the preceding column, will give (as in the 3d [4th] column) the true difference in time between the objects coming on the meridian and to the thread of the telescope of the mural quadrant. Hence the numbers in the last column, being subtracted from the time of the observed transits at the quadrant, will give the true time of passing the meridian. These numbers will be the proper correction for such of Dr. Halley's observations as were made after the year 1730, when the limb of the instrument, towards the bottom, was visibly removed further from the wall than when it was first adjusted; the cocks at that time plainly appearing to have given way or yielded; but, however, this yielding in those cocks, near the bottom of the limb, might not sensibly affect the situation of the plane of the limb in other parts above 10 or 12 degrees from the zenith, and, therefore, as none of the other cocks had given way, it will be proper to make the same allowance for the errors of the plane, in the other parts, as were at this time found to be necessary (even from the first using of this instrument)."

Against the observations of the 18th of August, 1742, it is noted, that when " compared with those made on the 15th, they give the error of the quadrant but about 25″ or 30″, or somewhat less than what I had before collected it to be, by measuring with the micrometer." But to the observations of the 19th it is added, " this day I perceived that the mark did not correspond with the plumb-line, and, therefore, I suspect that the quadrant had given way a little, after I had adjusted the wire yesterday; but this evening, before I made these observations, I adjusted the mark to the plumb-line carefully, and the difference between the observed zenith distances this night and Aug. 15, being at a medium about 34″, shew that the error of the quadrant was as much as I had before found by means of the micrometer."

Lastly, Bradley concluded his examination of the quadrant on the 12th of September, and says at the end, " By comparing the zenith distances, taken this day, with those of Aug. 29, it appears that the line of collimation, as now rectified, corresponds well with what it was before any alteration was made to the telescope. Hence we may conclude that the line of collimation was not sensibly changed, from its first rectification by Mr. Graham in 1726, during the following sixteen years, and, therefore, in the later observations of Dr. Halley, the error of the zenith distances was about 30″, and, about the year 1734, only 15″, to be added to the observed zenith distances, as above related."

After the erection of Graham's quadrant, Halley is said to have made comparatively little use of his transit: this was certainly open to objection, but it must not be considered as a mere indolent neglect of his duty; it was rather a compliance with the general sentiment of the times. It appears, from the letter* that Röemer wrote to Liebnitz in 1700, that he himself looked to more than the simple instrument of which he was the inventor; the plan which he there details, is distinctly that of a transit circle, not of a mere transit. There were advantages in the method which he proposed, but the arts were not then sufficiently advanced† to work out such a combination, and Flamsteed's example led to the attempt of simplifying the observation, by confining it to the quadrant. It was not till Bradley's time that the right ascensions and zenith distances were systematically taken on the meridian, with different instruments, at Greenwich. Now, for transits, the perpendicular side-wires are of essential importance; and, if there had been any in the older instrument, it is probable, with Halley's object, that he would not have had them omitted in the new one; but the inference from Bradley's memoranda is against there having originally been any such construction for it. On the 29th of August, 1742, the telescope having been taken off, was sent to London " to have a new nozzle and some other alterations made to it. When it was there, Mr. Graham removed the nonius plate or index a little further from the centre, because it had been found

* Misc. Berol. vol. iii. p. 277. The structure of Röemer's " rota meridiana," may be found in Horrebow's Works, vol. iii. p. 157. From the drawing of it, in Table VIII., it may be seen, that, in the same manner as is now adopted, he contrived a couple of microscopes fixed to the wall, by which he might read off the division on the circle, which was attached to the horizontal axis of his telescope.

† Röemer created for himself another serious difficulty; for, apparently wishing, from an erroneous expectation of greater stability, that the axis should be supported by the walls of the building, he was desirous to have a room especially built for his instrument, six feet in breadth, twenty in height, and thirty, or at least twenty-five, in length. Misc. Berol. p. 277.

that in hot weather the index scarcely reached the outer divisions. . . . . . He has, likewise, put in two more wires parallel to and 15′ each from the middle perpendicular wire, in order to take the time of the transits at each, when the weather permits, or at either of the outer ones when any accident prevents the transit of the middle thread being observed." In this case, as for the transit instrument, it may be fairly argued, that the reason for the addition would be superfluous if there had previously been side wires, which had been used for this very same purpose; besides, if GRAHAM had originally provided his telescope with three wires, "two more" must have made up five, which certainly exceed the number which is ever indicated in any of BRADLEY's observations with it. There is also another circumstance, which leads to the same conclusion. The transit observations begin on the 25th of July; they contain no interruption, or any notice of alterations, subsequent to that date; and they are entered regularly at the first, second, and third wires. But this is not so with the quadrant: some of the observations made with it are as early as the 15th of June, but only with a single entry of time for the transit, and in the ruled paper only one column is drawn for that time till the 12th of September, when the telescope had been repaired and replaced. After that period, there are three columns for the passage at the "first," "second," and "third" threads, so that the times are registered for one or more of them, according as circumstances had been less or more favourable.

Methods of observing the Sun and Moon.

As soon as BRADLEY became Astronomer Royal, he adopted the uniform practice of observing the moon and sun's limbs; and the height or transit of the centre occurs but rarely in his Greenwich books. But, previous to that time, he seems only to have observed the sun for the rate of his clocks, and, like HALLEY, he was in the habit sometimes of taking, and almost* always of only recording, the time at which the centre of the disc passed the meridian. This entry, however, was generally, in his case, the mean result; thus, 1729, June 7, there is entered, "0^h 16^m 4^s, ☉'s centre very exact." But there is also added, "I took the passage of the sun's limbs

* There are some few exceptions. "1726, March 8,—0^h 6^m 26^s ☉'s 1st limb at the 1st thread. Hence the centre in the meridian at 9^m 18^s." "September 14,—23^h 54^m 23^s, the ☉'s last limb went off the last cross, hence was on the meridian at 23^h 50^m $8\frac{1}{2}^s$."

over each of the three threads in my meridian instrument, so that I had six different observations to determine the transit of the sun's centre." Again, among the miscellaneous rough calculations in the old writing-book, we find,

" 1725, Sept. 16, ..	42 49½	1 L. 1 T.	44 25½	
	44 25½	1 L. mer.	1 4½	[☉ S. D.]
	46 31	2 L. mer. inverted	45 30	
			9 29	[Eq. of time.]
			54 59	
			5 01"	

And answering to this, among the observations of the sun, there is merely entered for the same day, " 23^{h} 45^{m} 30^{s} cent. ☉;" and the difference of the clock from noon is marked as 5^{m} 1^{s}. The same calculation-book, however, affords proof of his sometimes also using the other method. Thus, for a day which is not noted, he has written,

" 3 43	1 Th. 1 L.
5 21	1 L. mer.
6 55	cent. mer.
7 30	
9 6"	

And again, " March 25, ..

0 2 44	1 L. 1 Th.
4 22	1 L. mer. : :
5 27+	cent. mer. : :
8 26½	2 L. 3 Th."

In the observation-book on March 25, 1726, there is simply entered, " 0^{h} 5^{m} $26\frac{1}{2}^{s}$ ☉ cent." It appears, therefore, that the practice of observing the transit of the sun's centre, as adopted by FLAMSTEED, was continued in the early part of the last century. The small* magnifying powers then used with the meridian instruments, may have given the opportunity of estimating, as Mr. BAILY suggests,† the equality of the segments of the disc, when divided by the central perpendicular wire; but BRADLEY sometimes

* BRADLEY has the following memoranda: " 1745, August 29. About the middle of this month I fitted a shorter eyeglass into the quadrant telescope (viz. one of 2 inches focus, the old one being about an inch longer)." Hence, if the focal distance of the object-glass is taken roughly at 8 feet, the magnifying power will have been in HALLEY's time about 32. " 1742, July 20. By the time of the transits of the two stars [κ and η *Ophiuchi*] through the aperture of the [quadrant] telescope, it appears to take in an angle of 65′ of a degree." It has been mentioned, that the magnifying power of HALLEY's transit was less than 40; possibly it was nearly the same as that of the quadrant.

† Mem. Ast. Soc. vol. viii. p. 182.

observed the sun's centre on one of the side wires. Thus, in the calculation-book, there occur,

" 1726, August 6, ..	1	18	1 L. 1 Th.
	2	27	cent. 1 Th.
	2	59	1 L. mer.
	4	37	1 L. 3 Th.
	5	9	2 L. mer."

We do not know the power nor the extent of field in the Wansted transit; but, as the first wire was distant from the middle by about 24′ of space, it may have been convenient, if not necessary, to have had recourse to some other method than that of bisecting the disc for observing the central transit on the side-wires. Now, Dr. Maskelyne points* out, that the zenith distance of the moon's centre may be taken by causing a very small segment of the enlightened limb, which just before, or just after its transit, is cut off by the vertical wire at the centre, to be bisected by the horizontal wire. An allowance, as he mentions, will thus become requisite for the alteration in declination;† but this would not be the case for the analogous method, when applied to the transit; for, if a small segment were taken from the upper or lower limb, above or below the horizontal wire, its bisection by the perpendicular wire would occur at the same time with the centre's passage of it. This might possibly afford for the sun a more probable estimate than that of the central bisection of the full orb.

The Clocks.

Röemer, in describing what he considered to be necessary for his observatory, speaks of "tria horologia ejusdem magnitudinis et fabricæ."‡

* Greenwich Obs. vol. i. preface, p. vii.

† Mr. Maskelyne says, "this nicety, I have been informed, was not attended to in Dr. Bradley's time." Such authority deserves great consideration, and the statement may be accurate; but it must in justice be added, that there is reason to presume that the circumstance was not overlooked by Bradley. The change is certainly less rapid in declination than in right ascension, and he had no instrument at Wansted for observing the moon's zenith distance; but, on the 26th of February, 1726, having lost the transit of the moon's preceding limb at the centre, he took it at the third wire; and when he enters the time of the limb's passing the meridian, he adds, "N.B. In this deduction, I have allowed for the ☽ 's motion."

‡ Misc. Ber. vol. iii. p. 278.

Now, they were all for a single instrument, which seems to indicate that each was, as it were, to be a check upon the others, so that the time might be more secure when taken from a mean of the whole. HORREBOW has a little variation, for he says,* "Ternis horologiis ad minimum opus erit in observatorio, quorum bina capsis inclusa sollicite servantur, atque in continuo motu conservantur: tertium ita suspendi debet, ut observator distincte crepitus vibrationum auribus percipiat, atque indicatos numeros minutorum secundorum ictu oculi dignoscere queat in eodem corporis et fere capitis situ, quo astrum per pinnacidium contuetur." He then refers to a plate, in which the two larger clocks are drawn, on one side, close to each other, and the hands are pointing to the same hour, so as to indicate that they were both either set to solar or sidereal time. This was possibly the arrangement at Greenwich, where HALLEY, as Mr. BAILY informs us,† had two clocks and a monitor. When BRADLEY, however, succeeded to the observatory, they were separated (if they had not been so before); for we have the following memorandum, 1742, July 23: "I raised the bob of the pendulum of the clock in the quadrant-room 10^s, and at the same time set it 18^m 18^s forwarder, so that I now suppose it will shew sidereal time very nearly, the pendulum being, as I judge, of a proper length for the season of the year. I set the clock in the room, where the transit instrument is, one second slower than the other." These two were wound up every month, of which regular notices are entered, and the third, which BRADLEY calls the smaller, appears to have gone a week. There is an expression, also, which favours the conjecture of HALLEY having, at least sometimes, used the "horologium minus" with his transit; for one of the larger clocks having been taken down in 1742, the time of the sun's passage the next day is given "by the little clock set up again in the transit-room." When the larger was brought back, we find "the little clock taken out of the transit-room, and set in the great room above stairs." There it appears to have been kept for occasional use; for when the other large clock was taken down in 1744, "that in the great room was put up with tbe quadrant." These clocks could not have had the advantage of a going-spring. The first contrivance, I believe, of this kind which came into any extensive use, was SHELTON'S bolt and shutter, in which the lever which opened the hole for the key,

* Opera, vol. iii. p. 27.

† Mem. of Ast. Soc. vol. viii. p. 177.

brought a force to press upon the train of wheels while the maintaining power was suspended by the clock being wound up. This was later than HALLEY'S time; and MOLYNEUX, who might well be supposed to have supplied himself with the best instruments, has entered,* in 1726, the addition that was to be made for the seconds, which he considered to be lost during the time of winding up his clock. It is not impossible that more than one was formerly thought necessary in an observatory, not only for the reason above suggested, but likewise to guard against the loss of time from any one of them stopping, or from the interruption now under consideration.

From BRADLEY'S expression of the pendulum being "of a proper length for the season," we may fairly infer that this at least (for the quadrant clock) had no compensation for variation of temperature; but there is more decided evidence on the subject. 1742, September 26, "The clock in the transit-room was taken down in order to have a new pendulum, contrived by bars of brass and steel, to remove in great measure the effect of heat and cold from retarding or accelerating its motion." And, 1744, August 18, "The great clock by the quadrant was taken down and sent to Mr. GRAHAM to have a new pendulum," which was likewise of the gridiron construction; for in the transit-book it is noted (1744, September 6), that "the quadrant clock has now the same sort of pendulum as transit." GRAHAM did not publish the account of his mercurial compensation till 1726, and HALLEY was undoubtedly provided with his clocks before that time.

* Bradley's Miscellaneous Works, p. 143.