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EARLY ASTRONOMICAL RESEARCHES OF JOHN FLAMSTEED

ERIC G. FORBES, University of Edinburgh

The name of John Flamsteed, England's first Astronomer Royal, will forever be associated with his major three-volume work *Historia coelestis britannica* (London, 1725), published over five years after his death thanks to the unpaid labours of two of his most devoted assistants, Abraham Sharp and Joseph Crosthwait. The first volume contains extracts from astronomical observations by William Gascoigne and William Crabtree made between 1638 and 1643, followed by Flamsteed's own observations at Derby prior to 1675 and those subsequently carried out at Greenwich from 1675 to 1689. It includes the right ascensions, north polar distances, celestial longitudes and latitudes of the Moon and planets, all deduced from measurements made with the famous sextant (Figure 1) which he was obliged to use before Sharp's mural quadrant became operational in 1689. The second volume contains observations with the latter instrument up to 1719, plus lunar and planetary positions computed from those data; while the third volume contains various star-catalogues, including his own of 2935 stars, together with various auxiliary tables. The work as a whole has been praised by William Cudworth,[1] the biographer of Abraham Sharp, as occupying the same place in practical astronomy as Isaac Newton's *Principia* in theoretical astronomy; and Francis Baily has described it over a century after its publication as "the proudest production (considering the period at which it was made) of the Royal Observatory at Greenwich".[2]

Baily's bracketed qualification is nevertheless extremely significant since, as he goes on to stress, the rates of Flamsteed's clocks were irregular because pendula had not yet been provided with temperature compensation, analytic theories of motion and methods of reduction had not yet been developed, the phenomena of aberration and nutation were still unknown, the identifications of stars were quite often erroneous, the values of astronomical refraction were uncertain, and atmospheric temperatures and pressures were never recorded. By modern standards, therefore, Flamsteed's forty-three years of labour at Greenwich yielded little data of lasting validity. The main importance of Flamsteed's pioneering work rather lay, as one of his later successors Sir George Biddell Airy recognized, in his observing methods, and the skilful manner in which he extracted information from them. It is these features which I wish to illustrate in this article, with particular reference to several little-known aspects of Flamsteed's early researches *not* incorporated in the large systematically-arranged treatise referred to above: his investigation at Derby in 1672 of the size of the solar parallax, his attempt to interpret the physical constitution of celestial bodies, his theory of cometary motion, and his researches on telescope optics. My own knowledge of these was acquired primarily from a study of his manuscript lectures in astronomy, delivered at Gresham College, London, from 1681 to 1684;[3] also, to a lesser extent, upon articles in the *Philosophical transactions of the Royal Society* and his correspondence with contemporaries such as Newton (much of which has still to be published).[4]

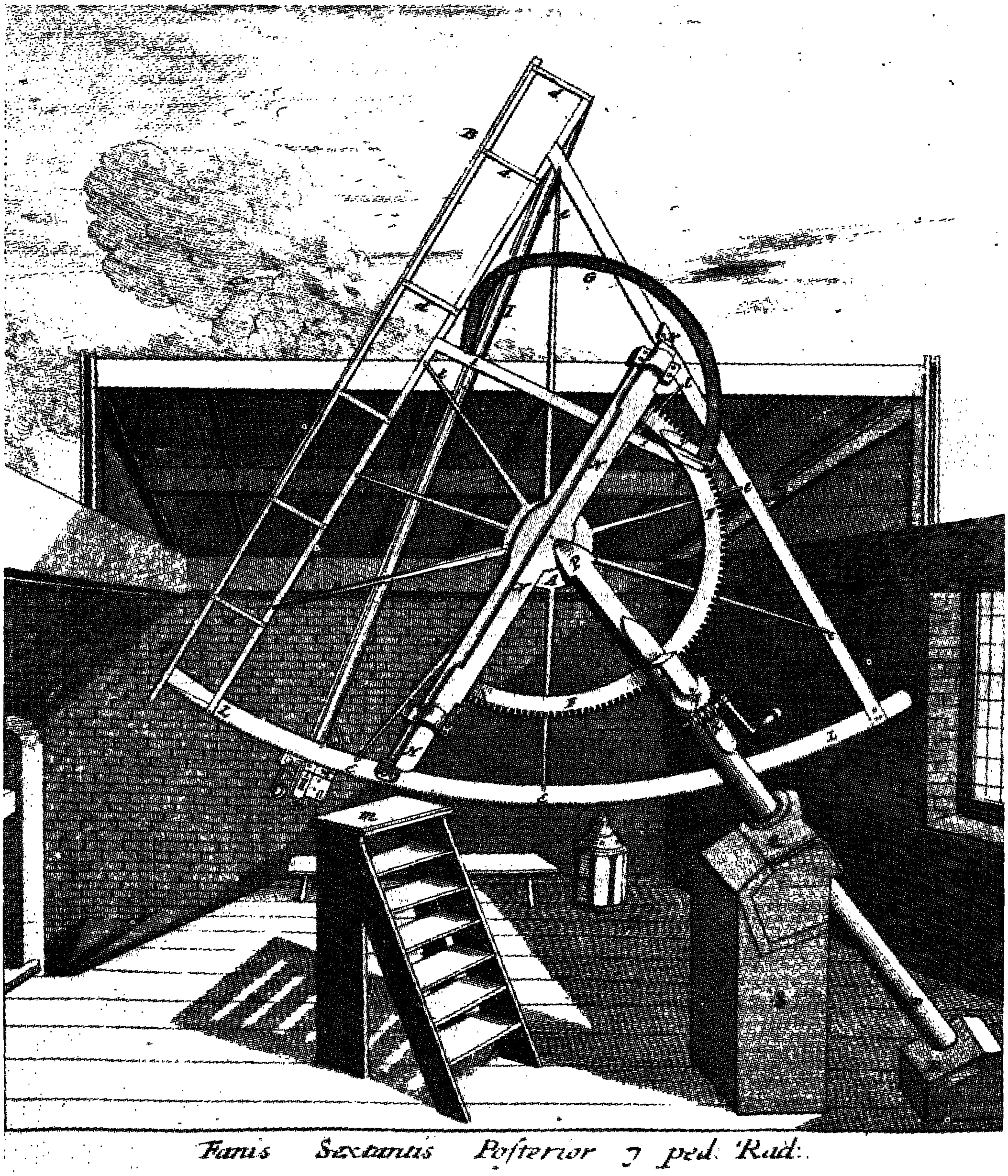

FIG. 1. Flamsteed's 7ft sextant, about 1676 (from the engraving by Francis Place).

Method for Finding the Solar Parallax

As early as 1666, at the age of twenty, Flamsteed was already expressing his discontent with the ancient method for finding the Earth's distance from the Sun attributed to Aristarchus. He read about this in the first volume of Giovanni Battista Riccioli's *Almagestum novum* (Bononiae, 1651)—a book containing a wealth of information on astronomical theories and methods to which he was particularly indebted—but criticised that Jesuit Father for wrongly defining the instant at which the Moon's disc appeared to be half-illuminated

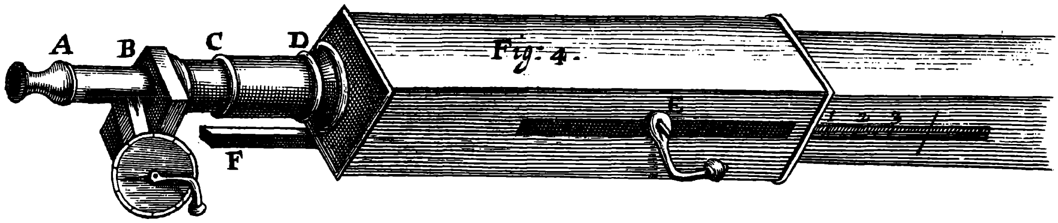

FIG. 2. Richard Towneley's screw-micrometer (from the drawing by Robert Hooke, *Philosophical transactions*, ii, no. 29 (1667)).

by the Sun.[5] Five years later, after he had begun to make his own telescopic observations at Derby, he also became aware that the irregularities in the Moon's surface made it difficult in practice to estimate accurately the apparent time at which this dichotomy occurred.[6] Thus he resolved to abandon this traditional method for finding the solar parallax, and began seeking an alternative which he found a year or so later in Johann Kepler's *Astronomia nova* (Pragae, 1609); namely, a method similar to one proposed by Tycho Brahe involving observations of the apparent angular distances between Mars and the Sun by day, and between Mars and neighbouring stars at night. The fundamental principle was that a comparison between the former quantity and the corresponding geocentric distance obtained from tables of planetary and solar ephemerides yielded the sum of Mars's and the Sun's parallaxes, while the night-time observations yielded Mars's parallax only; whence the solar parallax could be derived. Tycho himself, of course, lacked an accurate instrument for measuring Mars's changing position with respect to the stars, and was interested in the inverse problem of finding its parallax from the result for the sum of the two parallaxes (assuming the erroneous value of 3′ for the Sun's parallax).

Flamsteed was well aware that he had a double advantage over "The Noble Dane". He had, at his disposal, an astronomical telescope and a screw-micrometer (see Figure 2) that could be fitted into the focal plane of the eyepiece; with these, he could measure Mars's position with respect to the stars with the hitherto-unobtainable accuracy of almost 1″. The particular micrometer in his possession—a gift from his patron Sir Jonas Moore—had been made by a tenant of the Lancashire landowner Richard Towneley in accordance with a design by William Gascoigne found among the latter's unpublished correspondence with William Crabtree. A description and drawing of it were published by Robert Hooke in the *Philosophical transactions* for 1667[7] after Towneley himself, in a previous issue of that same periodical, had drawn attention to Gascoigne's priority over Adrian Auzout in regard to this type of instrument and explained how the micrometer could be applied to astronomical observations.[8]

The first suitable occasion for testing the viability of this new method came at the end of September 1672, when Mars happened to be near its perigee position and situated near the stars of the Pleiades group.[9] The observations made by Flamsteed may be summarized by the sequence of line-diagrams (Figure 3), *A* and *B* denoting two of the brightest stars in the Pleiades[10] and

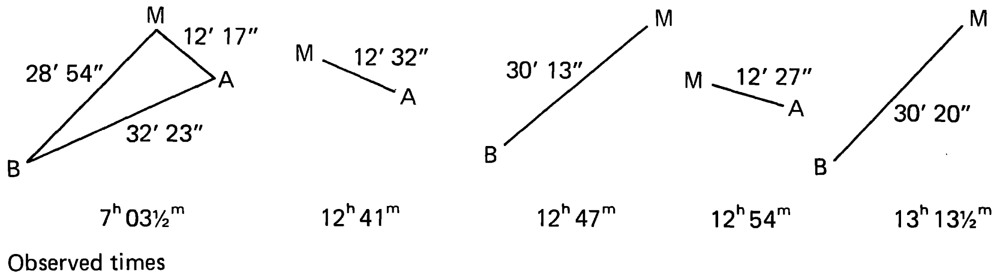

FIG. 3. Sequence of line-diagrams illustrating the results of Flamsteed's micrometric measurements of Mars's position (*M*) with respect to two bright stars (*A* and *B*) in the Pleiades on 26 September 1672 (Old Style).

M Mars's position relative to them at the apparent times indicated. The observed times were measured from local apparent noon at Derby on 26 September 1672, Old Style dating: thus the first observation was at $7^h03\frac{1}{2}^m$ p.m. on that day, and the next four were made shortly after midnight, at 41^m a.m., 47^m a.m., 54^m a.m., and $1^h13\frac{1}{2}^m$ a.m. respectively on the following day. The positions of the two stars do not vary; only the position of Mars, for which the celestial longitude and latitude at 7^h03^m p.m. were unambiguously fixed by the three (almost) simultaneous micrometric measurements of *AB*, *AM*, *BM*. In practice, Flamsteed's micrometric readings referred to the distance of each star from the more remote limb of the planet. He then subtracted Mars's semi-diameter, which he took to be 15″, in order to obtain the distance to the centre of its disc.

The measurements revealed that *AM* initially increased by 15″, then decreased by 5″ during the 13^m time-interval between the second and the fourth observations, from which Flamsteed deduced that it would decrease by a further $7\frac{1}{2}''$ during the $19\frac{1}{2}^m$ separating the fourth and final observations, implying a net decrease of $2\frac{1}{2}''$ over the entire 6^h10^m time-interval; thus $AM = 12'19\frac{1}{2}''$ at $1^h13\frac{1}{2}^m$ a.m. on 27 September 1672. Note that this result was obtained by linear interpolation. The corresponding distance $BM = 30'20''$, and the invariable distance $AB = 32'23''$, were known by direct measurement; thus the position of *M* was fixed, relative to those of the two stars, for the time of the final observation (Figure 4).

Flamsteed established the celestial coordinates of *A* and *B* two nights later from the reduction of independent altitude observations taken with his 3ft radius

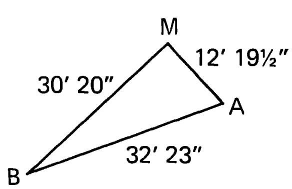

FIG. 4. Mars's position with respect to the two stars (*A* and *B*) of the Pleiades at $1^h13\frac{1}{2}^m$ on 27 September 1672.

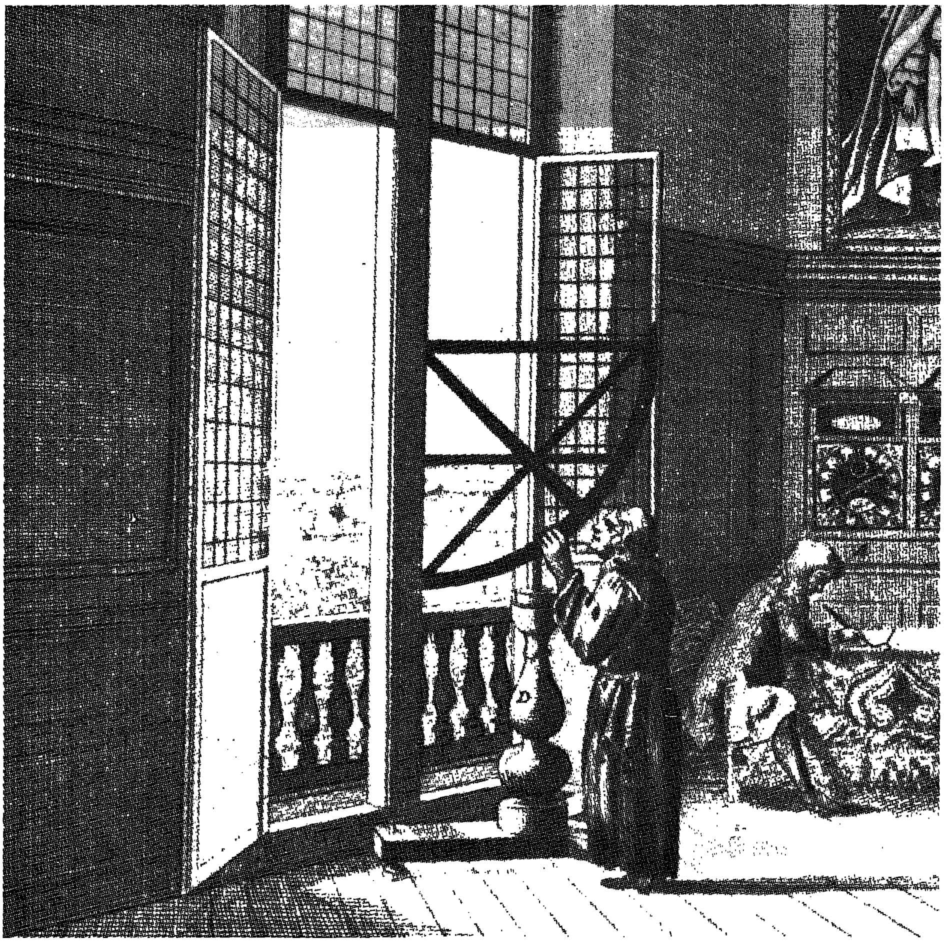

FIG. 5. Flamsteed's 3ft wooden quadrant, about 1676 (from the etching by Francis Place).

wooden quadrant (see Figure 5).[11] These values were found to differ by several minutes of arc from those catalogued in Tycho's *Historia coelestis* (Augustae Vindelicorum, 1666), after due allowance had been made for the effect of precession. With these data, Flamsteed was able to calculate the coordinates of Mars, and hence to deduce that this planet's change in longitude was $37\frac{1}{2}''$ retrograde, and in latitude $1'19\frac{1}{2}''$ decreasing, over the $6^{h}10^{m}$ interval between the initial and final observations. The true motion of Mars during the same period was found by linear interpolation from the appropriate geocentric co-ordinates of this planet tabulated in Joannes Hecker's *Ephemerides*[12] for 1672, although Flamsteed undoubtedly preferred to apply the theoretical corrections for the Sun's irregular motion in his own recently-composed tract "De Temporis Aequatione Diatriba"[13] in preference to Hecker's empirical estimates of this quantity. The figures of $20'$ retrograde in longitude, and $1'13\frac{1}{2}''$ decreasing in latitude, found in this manner, were both lower than those obtained from his observations: thus they appeared to justify his belief that the differences of $37\frac{1}{2}''-20'' = 17\frac{1}{2}''$ and $1'19\frac{1}{2}''-1'13\frac{1}{2}'' = 6'$ represent the sum of the diurnal parallaxes of the Sun and Mars in longitude and latitude respectively.

The next requirement was to determine which proportions of these values were inherent in the observation at $7^{h}03\frac{1}{2}^{m}$ p.m. on 26 September (Old Style), and at $1^{h}13\frac{1}{2}^{m}$ a.m. on 27 September (Old Style) respectively; for this was sufficient to fix the amount of Mars's parallax. Flamsteed calculated the parallactic angles using the traditional nonagesimal method, well known in his time. The two precepts which he then employed to determine Mars's parallaxes in longitude and altitude actually apply to the Sun, making no allowance for the lack of coincidence between the plane of the planet's orbit and the ecliptic. Errors arising on this account must, however, have been small compared with those incurred through his neglect in this instance of the phenomenon of atmospheric refraction at the rather low altitude of about 15°. Mars's horizontal parallax deduced from the parallax in this altitude was 22″, and the Sun's "almost 10″"—a remarkably accurate result which is a tribute both to the consistency of the method and the accuracy of his observations, with perhaps a certain amount of luck on his side!

Flamsteed repeated this investigation at Greenwich on three subsequent occasions between 1676 and 1681, taking the Sun–Mars distance both in the morning and in the evening with his new 6ft 9in. radius sextant which was equipped with telescopic sights. Following Henry Oldenburg's death in 1678, the *Philosophical transactions* ceased temporarily to exist, before being resumed under the auspices of the Royal Society in January 1683. In consequence Flamsteed was unable to use this organ of publication to report the golden opportunity that he and his young assistant Edmond Halley had had on 14 January 1679 of repeating the whole procedure with an even higher degree of precision. He was now paying particular attention to the effects of refraction and carefully selecting his comparison stars to assist in the elimination of this phenomenon; nevertheless, his result of about 10″ for the Sun's horizontal parallax was just the same as that "collected before from the Derby observations of the year 1672".[14] Another set of measurements using a different comparison star yielded the slightly higher value of 12″, which possibly accounts to some extent for his caution in announcing as his final conclusion that the solar parallax was *no more than* 15″.

Speculations on the Physical Constitution of Celestial Bodies

Flamsteed's belief that stars are similar in nature to the Sun was based upon his observations at Derby during 1672. Using a 14ft telescope with a micrometer eyepiece, he measured the angular diameters of Mercury and Sirius and concluded that both were about 14″.[15] Careful observations with his 3ft radius wooden quadrant of the angular distance between α Arietis and Aldebaran convinced him that there was no appreciable annual stellar parallax; thus, since it remained undetectable, it must be less than 1′. By provisionally assuming that the diameter of the Earth's orbit subtended exactly that angle at the distance of Sirius, and using his micrometric result that the diameter of the Sun's disc is about 1/220 of the Earth's orbital diameter, he deduced that Sirius would be 94 times greater in volume that the Sun. Taken together with his general observations that stars appear "less hairy" when seen through a good telescope and must be very bright considering their vast distances from the Earth, this rough calculation was sufficient to convince him that the comparison of Sirius with the Sun was a meaningful one.

Having thus established this important analogy, Flamsteed reasoned that, just as sun-spots obscure parts of the Sun, similar dark patches may exist on stars and cause them to appear periodically darker or brighter in a manner dependent upon their respective rates of axial rotation. The description of the nova in the neck of the Whale given by Johannes Hevelius's *Mercurius in sole visus* (Gedani, 1661–62) and subsequently reported as part of an account of Ismael Bullialdus's *Ad astronomos monita duo* (Parisis, 1667) in the first volume of Oldenburg's *Philosophical transactions*,[16] appeared to support this hypothesis. Bullialdus's purpose in writing that tract had been to incite astronomers to study this star (and the nebula in the girdle of Andromeda) to establish the period and amount of their observed variations in brightness. Another two novae situated in the neck and breast of the constellation Cygnus, the behaviour of which was described in the French *Journal des sçavans* for 22 June 1671,[17] appeared to exhibit periodic brightness fluctuations of ten months' and fourteen years' duration respectively.[18] Flamsteed regarded the longer period of the star's supposed axial rotation as an indication of its greater bulk. Other important sources consulted by Flamsteed in his search for further evidence included Riccioli's *Almagestum novum*, Hevelius's "Historiola Mirae Stellae",[19] Simon Marius's *Mundus Jovialis*, Galileo's *Sidereus nuncius*, and Huygens's *Systema Saturnium*.

The interpretation of these data was centred on the behaviour of the nova in the neck of the Whale, since that was the star for which the most reliable data had been collected. After dismissing the possibilities of the appearances being caused by one of its hemispheres being dark and the other bright, by a lucid point or broad spot on its surface, and by its inside being dark and its outside bright, Flamsteed concluded that the only rational explanation which would fit the facts was to suppose the star to be composed of a bright ball of lucid matter surrounded by a solid shell of dark matter. The rotation of a star having a gap in its dark outer shell could produce the brightness variation of novae in general, and there was no justification for rejecting *a priori* periods of axial rotation more than 330 times that of the Earth or 13 times that of the Sun. The Sun's own physical appearance could be explained by introducing the dynamical postulate that the outer matter is drawn through an orifice into the core and replaced by brighter matter from the inside, the reversibility of this process being the reason why certain stars known to the Ancients were no longer visible in the seventeenth century. Planets were considered to have been produced by this same mechanism, and thus to be identical in nature to stars. This enabled Flamsteed to cite the gradual diminution in brightness of Jupiter's satellites in support of his hypothesis that stars may be enveloped by a dark atmosphere. Comets were thought by him to result from the transformation from a planet-type to a star-type constitution due to the flow of luminous matter from the outer shell to the inner core.

A further consequence of the analogy between stars and the Sun was that stars may also be regarded as the central "Suns" of planetary systems, and comets could be regarded as the planets of some vortex whose central "Sun" is extinct. Although Flamsteed used the word "vortex", he did not accept the physical explanation of planetary motion in the third book of Descartes's *Principia philosophiae*, mainly because it failed to explain how the Sun can both attract and repel the planets while permitting them to retain their orbits. In his view, magnetism was the only physical agency capable of accounting for the elliptical form of their orbits. Fundamentally, therefore, it was Flamsteed's faith in his analogies and adherence to this physical principle which governed his belief that the lucid matter in the interiors of the planets and in the exteriors of the Sun must also possess such a property. Newton, not being bound by either of these presuppositions, felt himself under no obligation to admit the existence of magnetism in celestial matter. As soon as he had grasped the important fact that a central attractive force can produce an elliptical orbit, he felt free to accept gravity as the causal agency of planetary motion.

Theory of Cometary Motion

One of the leading authorities on both the physical constitution and motion of comets in Flamsteed's time was Hevelius who, in *Prodromus cometicus* (Gedani, 1666), favours the belief that their paths are conic sections with the Sun at one focus, as Kepler had shown to be the case for planets. However, in his *Cometographia* (Gedani, 1668), he espouses the alternative belief that the orbits were rectilinear. The reasons for this latter view are stated on pp. 807–8 of the third volume of the *Philosophical transactions*, for 19 October 1668—one of several accounts of cometary observations published in that periodical during the fifteen years prior to the appearance of the Comet of 1680. The method used by Flamsteed to calculate the celestial coordinates of this particular comet, the length and direction of its tail, and its node and path, is similar to that employed in his determination of the solar parallax inasmuch as Venus was used as an intermediary in the measurement of the angular distances between the comet and the bright star Aquila. The longitude and altitude of the nonagesimal point deduced from the known latitude of Greenwich, the obliquity of the ecliptic, and the hour-angle of the observation, gave the parallactic angle and the quantitative corrections to be applied for the parallax in both apparent celestial coordinates. Here, however, an appropriate correction for atmospheric refraction is also made before the geocentric longitudes and latitudes are obtained. Hecker's *Ephemerides* were used merely to interpolate the corresponding longitude of the Sun. Unfortunately, arithmetical errors in Flamsteed's reductions greatly weakened the significance of his results for the length and direction of the comet's tail, and falsified his conclusion that the latter was not coincident with the great circle arc between its head and the Sun.

The further application of the method to the determination of the elements of the comet's path required a repetition of the same observations on at least one other occasion. Flamsteed investigated the question of whether the comet's orbit was indeed a great circle arc by first repeating the whole process of calculation for each successive pair of observations obtained at Greenwich during December 1680 and January 1681, then for the first observation of 12 December with each of the other thirteen. The final results are recorded in his letter to Richard Towneley dated 22 March 1680/1.[20]

Such was Flamsteed's faith in the accuracy of his method, that he regarded the calculated angles between the comet's orbit and the ecliptic as evidence for a gradual movement of the comet away from the pole of the ecliptic which became less as the comet receded further from the Sun. Only at remote distances from our luminary did it appear that the motion was along a great circle arc. He believed, too, that a physical explanation was necessary to explain why the angle made by the axis of the comet's tail was always *ahead* of the great circle passing through its head and the Sun, as well as the less regular tendency for the node to regress and apparently to advance again with increasing distance from the Sun. One thing was certain: neither a Keplerian orbit nor a straight line could represent these data.

On the other hand, his observational results did enable him to assert that the comet observed by Jean Charles Gallet and Antonio Collio in Rome in November, and that observed by the Paris astronomers and himself in December, were identical. Both were of the same appearance, with the same side always facing along a common direction of motion. Moreover, the comet appeared to be attracted by the Sun and to move towards it, while in the east; and to be repelled by the Sun when in the west, after having been carried around by what Flamsteed called the solar vortex. In a letter to James Crompton in Cambridge, dated 3 January 1680/1,[21] Flamsteed conjectures that an unexpected shortening in the length of the comet's tail might either be due to the weakening repulsion from the Sun failing to push the material out of the atmosphere surrounding the head, or to the exhaustion of a hypothetical energy source within the comet that produced its blaze. He subsequently suggested to this same correspondent[22] that the head may instead be composed of a translucent liquid and that the tail curved slightly backward (*i.e.* towards the west), interpreting the comet's motion in terms of the same principles of vortex motion and magnetic action as Kepler had previously applied to planetary motion in the *Epitome astronomiae copernicanae* (Ulmae, 1618, 1620, 1621).

Newton's reaction to these ideas, which were communicated to him by Crompton, is most interesting.[23] He rejected solar magnetism on the ground that the Sun, being an extremely hot body, could not possess such a virtue. Supposing, however, that it did, then it ought to exhibit the directional property of a magnet and cause the comet to be constantly attracted towards its centre and never repelled. He rightly questioned the generality of the inference that comets verge from south to north, and expressed doubts that the head can be liquid. His belief that the comet's motion in December was too irregular to permit its identification with the comet seen in November, was, however, a result of his confusing the New Style dating used by Gallet with the Old Style dating currently in use in England. Flamsteed, in his next letter to Crompton,[24] corrected this misunderstanding and enclosed a diagram which revealed that the comet moved towards the Sun practically in a straight line, and away from it along a similar path. This seemed opposed to the idea that the comet's motion might be strongly accelerated, for which reason he felt unable to accept Newton's suggestion that the comet could swing round the Sun and back again rather than approach it and be repelled before reaching it. He disagreed, too, with Halley's opinion that the comet's motion was governed by the Sun's gravitational attraction, since he thought that the material of which the comet was composed would then be dissipated. Newton's own further reflections on this subject[25] led him to conclude that the best explanation was to postulate that the Sun did possess magnetism and that the comet's orbit was determined by the centripetal force of magnetic attraction and the centrifugal force produced by its motion along a curved path. Soon afterwards, he substituted universal gravitation for magnetism as the causal agency, doubtless because of his belief that hot bodies cannot be magnetic, and possibly because he accepted John Wallis's theory of the tides (1666)[26] which includes the earliest rational demonstration that a gravitational force can be operative across apparently empty space. As he demonstrated in the first Book of his *Principia* (1687),[27] if such a centrally-directed force (whether magnetic or gravitational) exists, it must vary its strength in accordance with the inverse-square of the distance between the two attracting bodies. Perhaps too little significance has hitherto been attached by historians of science to Flamsteed's influence on this particular phase in Newton's intellectual development.

The picture of Flamsteed which emerges from a close study of the observational and interpretative aspects of his work presented above, is that he was far too uncritical of his own methods and calculations. Discrepancies between his results and those of others arose, in his opinion, from errors in the latter. He had far more faith in a single measurement of an angular distance in the heavens with his micrometer, than in numerous self-consistent observations of the same quantity made by another observer with equally reliable instrumentation. Thus, when publishing the results of his first solar parallax observations in the *Philosophical transactions* for December 1672,[28] he could not refrain from remarking that Hevelius's lifelong labours on the rectification of errors in Tycho Brahe's measurements of the celestial coordinates of stars would be in vain because the former was not using telescopic sights fitted with a micrometer eyepiece.[29] This criticism elicited a reply from Hevelius in his *Machina coelestis pars prior* (Gedani, 1673);[35] but after Flamsteed repeated his objection in an open letter to Giovanni Domenico Cassini on the motions of Jupiter's satellites,[31] Hevelius protested to Oldenburg[32] that his observational method was much more reliable than Flamsteed seemed to imagine.

At this stage Robert Hooke, who was no friend of Flamsteed, attacked Hevelius's published defence of the conventional bare sights in *Animadversions on the first part of the Machina Coelestis of . . . Hevelius* (London, 1674). He, too, failed to acknowledge the high internal consistency of Hevelius's sextant observations. Spurred by the vindictive tone of this unexpected attack, Hevelius sent Flamsteed an eye-witness's testimony that his measurements were consistent to within $\pm 6''$ for stars of the fifth or lower magnitude classes, and asked him to come to his defence against Hooke until such time that the second part of the *Machina coelestis* would be published. In the hope of guaranteeing this favour, Hevelius sent Flamsteed his own observations of Mars's position with respect to the same stars in the Pleiades on which the latter's solar parallax investigation had been based.

After comparing Hevelius's measurements with his own, Flamsteed replied[33] to the effect that his correspondent must have incurred an error of $\pm 2'$ in both celestial coordinates of the stars; to which Hevelius retorted[34] that the discrepancies ought not to have been ascribed entirely to errors in *his* observations. Flamsteed's own method of reduction presupposed that Hecker's ephemerides and Tycho's star-positions were entirely free from error. Hecker's solar tables stood in need of correction by as much as 7′ and Flamsteed himself had stated that Tycho erred by over $\pm 5'$. Systematic errors could arise in sextant measurements of the Sun's meridian altitudes from several sources, irrespective of whether telescopic sights with micrometer eyepieces were applied to that instrument. Our recalculation of the celestial coordinates of the comet of 1680, the length and direction of its tail, and the node of its orbit, entirely bears out Hevelius's assessment by revealing the presence of errors in Flamsteed's numerical results as much as sixty times greater than those which he had been accusing Hevelius of incurring! Yet nothing that the latter could say or do, nor Halley's first-hand testimony in favour of Hevelius after a two-month stay in Danzig during 1679 for the very purpose of settling this dispute,[35] was destined to shake Flamsteed's exaggerated confidence in the perfection of his own data and methods.

Theory of Telescope Optics

The above-mentioned circumstances did, however, have an effect on Flamsteed: they made him aware of the need for an explicit and immediate vindication of the value of applying telescopic sights to levels, quadrants, and sextants. A brief description of the procedure for doing this had been published in Jean Picard's *La mesure de la Terre* (Paris, 1671). The high accuracy of Picard's results for the length of a degree measured by triangulation on the Earth's surface already constituted implicit evidence in favour of Flamsteed's view, but few copies of this treatise had reached England[36] and he himself had great difficulty in understanding the French language. What he felt was still lacking was a rational geometric proof of the principles of telescope optics, which would *demonstrate why* the conventional instrument embodying a pair of convex lenses is the most suitable one for astronomers to employ.

By far the most significant source of Flamsteed's knowledge of optics was the manuscript writings of William Gascoigne, the inventor of the screw micrometer, which he had acquired from his friend Richard Towneley whose uncle (Christopher) had obtained them some years previously from the widow of Gascoigne's correspondent William Crabtree and exchanged with this nephew for other documents of antiquarian interest. Writing to William Molyneux of Dublin on 29 May 1682,[37] Flamsteed openly acknowledged this debt, remarking that he had learned the basic principles of optics from Gascoigne's loose papers in less than two hours, having previously been misled by reading the works of authors who did not even know the correct law of refraction. Gascoigne's four principles, cited in a previous letter,[38] are: that the radius of a circle is at right-angles to the tangent; light rays are refracted towards the perpendicular in passing from a rarer to a denser medium, and vice versa; the amount of this refraction is proportional to the sine of the angle of incidence; and angles of incidence and reflection from a plane surface are equal.

It would seem that Gascoigne himself had written down these rules after having read Descartes's tract "La Dioptrique" appended to the *Discours de la* *méthode* (Leyde, 1637); at any rate, he chose Descartes's ratio of 3:2 as the refractive index of light through glass. Flamsteed adopted this same value as a simple but close approximation to the more precise ratio of 300:193—or 14:9 as the *mean* ratio for white light—which Newton's experiments on light and colour[39] had recently established; and used it to construct, in turn, the path of refracted rays through a block of glass, the segment of a sphere, and a plano-convex lens. He found that this lens had a greater focusing effect when the convex side faces towards the light-source. He next considered a double-convex spherical lens with surfaces of equal radii of curvature, and calculated the position of the focus of parallel rays through it. His major conclusions were: that no spherical lens will bring all parallel rays to a single focus, though others with hyperbolically-curved and elliptically-curved surfaces will; and that a plano-convex lens with its convex side facing the light-source is the best arrangement for providing a sharper focusing property and hence—in modern terminology—for obtaining a higher resolution. His claim to have been the first person to discover this latter property, and to distinguish clearly between the conditions required for obtaining higher magnification and higher resolution, is supported by Molyneux in a letter dated 1 August 1682.[40] These preliminary studies enabled Flamsteed to appreciate the fact that a telescope with two convex lenses, on account of its large magnification and the possibility of placing a micrometer or threads in the common focus, is the *only* kind for use in astronomical observations.

Flamsteed also appears to have learned the principles of catoptrics from reading Gascoigne's letter to Crabtree of 8 June 1642, which contained a description of Gascoigne's ox-eye experiments[41] which could have been inspired by Descartes's "La Dioptrique" or Claude Mydorge's *Prodromi catoptricorum et dioptricorum* (Paris, 1641). Gascoigne established that the refractive index of the vitreous humour was approximately the same as that of glass, but was unable to calculate this quantity for the other refracting surfaces because of their variable and complex curvature. Nevertheless, these very same features ensure that the eye is capable of accommodating itself to compensate the effect of colour aberration produced by an eyepiece with a high degree of curvature, and that the same phenomenon can be further reduced by using telescope objectives with a greater focal length. The eye itself acts as an additional lens in inverting the image of an object. Since the property of magnification depends upon the ratio of the focal lengths of the objective and eyepiece, whereas the distinctness depends only upon the focal length (or radius of curvature) of the objective, Flamsteed saw that no advantage would be gained through introducing more lenses into a telescope. Thus he preferred Newton's reflecting telescope, which had a parabolic mirror bringing all light-rays to a single focus in a short tube.[42]

Concerning the manner in which a telescope might best be applied to a quadrant, Flamsteed thought that the objective ought to be positioned over the pivotal point, the eyepiece on the outer edge of the limb, and the focal plane with cross-wires on the inner edge of the limb. In effect, therefore, the width of the limb was determined by the magnifying power of the telescope; and it had to be raised by a few millimetres above the plane of the instrument so that the plumb-line through the pivot would not be obstructed. The zero point on the limb was obtained by inverting the quadrant and bisecting the difference between the two points where the plumb-line crosses the outer limb. A level with telescopic sights, shaped like a carpenter's square, could be rectified in the same way. To avoid the effect of astronomical refraction, which is capable of producing 3′ differences or more between morning and midday observations, this procedure ought to be carried out in day-time only in serene and settled atmospheric conditions, or at night by sighting on stars near the zenith; the disadvantage of the latter alternative being that several nights' experiments would be required.

Flamsteed also investigated the properties of concave lenses in a similar manner, and found that it *is* possible to have a telescope with a concave eye-piece linked to a micrometer provided that the objective is plano-convex with the convex side facing the object being viewed. However, such an eyepiece (on which cross-threads could be placed, or a pair of intersecting lines cut on the glass surface with a diamond) had to be firmly fixed—in contrast to the convex eyepiece which was movable with the threads firmly fastened. To summarize: Flamsteed's original contributions to the theory of telescope optics, or the superstructure that he erected on the principles known to him from Gascoigne's writings, included the application of telescopic sights to astronomical instruments and the rectification of these instruments for the measurement of altitudes, the focusing properties of plano-concave and double-concave lenses and the effects of inverting them with regard to the direction of the light-source, and the comparison of the double-concave with a double-convex lens of both equal and unequal radii of curvature.

The year 1687, remembered in the annals of science as the year of publication of Newton's *Principia*, also marks the culmination of the early phase of Flamsteed's career at Greenwich. During the twelve years that had elapsed since he had taken up that appointment, the first Astronomer Royal had continued the micrometric measurements of solar, lunar and planetary diameters begun at Derby and, using new tubes with 16ft and 8ft focus objectives, made frequent telescopic observations of lunar occultations and small angular distances in the heavens before his telescopic sextant (Figure 1) was fitted for use. Apart from the numerous celestial observations which he made with that instrument, Flamsteed experimentally established his earlier theoretical demonstration of the Equation of Time,[43] provided a firmer observational basis for the theory of atmospheric refraction, and critically reviewed previous attempts to establish the value of the obliquity of the ecliptic. His belief in the constancy of the obliquity and in Copernicus's postulates of the Earth's diurnal and annual motions led him to conceive a new graphical representation of all these celestial appearances which he then applied to the calculation of the circumstances of eclipses and lunar occultations. This appeared under the title of "The Doctrine of the Sphere" in Jonas Moore's *A new systeme of the mathematicks* (London, 1681) along with his revised solar and lunar tables and additional tables for the calculation of eclipses. Its value as a method of rectifying the Sun's and Moon's ephemerides diminished about a century later when the errors in astronomical observations became reduced to only a few seconds of arc. Nevertheless, in spite of what Joseph Delambre has said to the contrary,[44] Flamsteed deserves credit for originality in devising it.

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3. E. G. Forbes (ed.), *The Gresham Lectures of John Flamsteed* (London, 1975).
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6. The first hint of this is found in the letter from Flamsteed to Oldenburg, 13 May 1671, published in A. Rupert Hall and Marie Boas Hall (eds), *The correspondence of Henry Oldenburg*, viii (Madison, Milwaukee and London, 1971), 47.
7. H. Oldenburg (ed.), *Philosophical transactions*, ii, no. 29 (1667), 541–4.
8. *Ibid.*, i, no. 25 (1667), 457–8.
9. Flamsteed's prognostication of this event was published in *ibid.*, no. 86 (1672), 5040–2, the observations and results being duly reported in *ibid.*, no. 89 (1672), 5118–24.
10. These are identifiable with the stars numbered 26 and 27 in the earlier of the two papers cited on p. 5042 of ref. 9 above.
11. *Ibid.* The right ascensions and declinations derived from Tycho's observations are stated as 11°44′00″, 3°58′0″S. and 12°12′0″, 4°10′30″S. respectively.
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21. An extract from this letter which omits Flamsteed's statement of his opinion of comets, their formation, motion, etc., is printed in Baily's *Account* (ref. 5), 123–4. The missing details are supplied in H. W. Turnbull (ed.), *The correspondence of Isaac Newton*, ii (Cambridge, 1960), 336–40.
22. Flamsteed to Crompton for Newton, 12 February 1680/1. This letter is no longer extant but some of its content can be inferred from Newton's reply (cf. ref. 23 below).
23. Newton to Flamsteed, 28 February 1680/1; published in Turnbull (ed.), *The correspondence of Isaac Newton*, ii, 340–7.
24. Flamsteed to Newton, 7 March 1680/1; *ibid.*, 348–56.
25. Newton to Crompton, April 1681; *ibid.*, 358–62.
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27. Isaac Newton, *Principia mathematica philosophiae naturalis* (Londini, 1687), Lib. i, Prop. XI, pp. 50–51.
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29. *Ibid.*, 5119.
30. Pp. 294 *et seq.*
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37. The Flamsteed–Molyneux correspondence, comprising 70 letters written during the decade prior to the publication of Molyneux's *Dioptrica nova* (Dublini, 1692), is preserved in the Southampton Civic Record Office, England, under the reference D/M 1/1. The particular letter to which we are here referring is on ff. 22 and 24.
38. Flamsteed to Molyneux, 11 April 1682; *ibid.*, ff. 14, 15, 19.
39. *Philosophical transactions*, vi, no. 80 (1672), 3075–87.
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