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The snare of simplicity: the Newton–Flamsteed correspondence revisited

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Abstract The correspondence in 1680 and 1681 between John Flamsteed and Isaac Newton on Flamsteed's theory of the comet of 1680 tells half the story. Related manuscripts reveal Newton was pursuing his own comprehensive line of inquiry based on principles that were the antithesis of Flamsteed's procedures. Following generally accepted views in England, Newton's work was marked by critical evaluation of data but marred by uncritical use of simple calculating techniques based on what might be termed Platonic archetypes of straightness. Flamsteed's intervention provided useful data and allowed Newton to seek additional information. Although Newton supposedly briefly considered a solution that vaguely resembled his parabolic approximation of the path of the comet determined 5 years later, the evidence Newton provided (based on simple hypotheses) did not support such a highly curved path or one in which the sun apart from the solar vortex exerted influence. Newton's work, including an alleged harmonic law of tails, was quietly abandoned in favor of other work. With new insight, Newton revived work on comets as fully gravitational objects immediately following Halley's visit in 1684. This little known side of the episode provides a fresh opportunity to examine Newton's sources and actual practice in developing a new line of inquiry. An appendix dating sections of Newton's Waste Book entries on comets is included.

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As to ye Hypoth. of Monsieur Mallemont, though it should not be true yet if it were to answer to ye Phænomena it would be very valuable by reason of its simplicity. Newton to Hooke (28 November 1679).

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1 Introduction

The capstone of the *Principia* (1686) was Isaac Newton's demonstration that the comet of 1680 was subject to the same laws of gravitational motion as the planets. Free of perturbations, the path of a comet, like that of a planet, would be an ellipse (Ruffner 2010). A parabolic path provided an approximate solution for the visible section near the sun. The small scale diagram of the parabola in modern editions is misleading. The importance Newton placed on the solution was illustrated by a two page fold-out diagram, the only large scale drawing in the *Principia*. The situation was entirely different during the actual appearances of the comet in 1680 and 1681. Newton had initiated a comprehensive study of comets predicated on a concept of straightness represented by the straight lines of Euclidean geometry and the great circles of celestial spherical trigonometry when Flamsteed intervened with a series of letters offering a completely different pattern of analysis. Some analysts, as shown in Sect. 16, see seeds of later work in Newton's suppressed responses to Flamsteed's ideas. Newton's efforts in 1681, however, are better understood as backward rather than forward looking. Newton abandoned the project, in part from the lack of accurate data and in part from the press of other interests.

2 Prelude to the Comet of 1680

Studies beginning with the Comet of 1577 negated the prevailing view of Aristotle that comets must be sub-lunar bodies without necessarily reviving the view of the Chaldeans and other ancients that comets are a kind of planet (Hellman 1944). The work of Tycho Brahe, Michael Maestlin, Horatio Grassi and others placing comets in the planetary realm did not settle the matter of a comet's path (Westman 2011). These early studies also engendered debates over the astrological significance of comets which were of little interest in English philosophical circles of Newton's day beyond the possibility of direct physical influence (Schechner 1997). Accordingly, this paper focuses on principles related to a comet's motion both apparent and real and associated issues about accuracy of the data.

The observed or apparent course of a comet is merely the projection onto the celestial sphere of its true path of whatever shape and distance from the earth (Heidarzadeh 2008). The idea that a comet appears to follow a great circle arc dates at least as far back as the comet of 1472 at a time when they were believed to be sub-lunar phenomena (Jervis 1985, pp. 114–119, 195–196). In 1588, Tycho found exquisite confirmation using different pairs of observations from the comet of 1577 that showed virtually identical positions of the ascending node (Brahe 1648, bk. 2, pp. 47, 55). Snell (1619, p. 19) adhered to the idea so strongly; he evidently believed that the calculation for a single pair of observations for the great comet of 1618 was sufficient to demonstrate apparent great circle motion. Further in the same volume (Snell 1619, p. 85) Christoph Rothmann using procedures similar to Tycho's showed that the inclination or obliquity at the ecliptic of the apparent path of the comet of 1585 varied a little more than 1° for different pairs of observations. Other examples revealing slight deviations from great circles were cited from time to time leading Flamsteed to suggest, in a May

1681 lecture, that the consensus had been that comets "moved nearly in the Arch of a great Circle" (Forbes 1975, p. 105). By way of correction, Flamsteed's investigations of the comet of 1680 showed variations in obliquity of more than 6° for different pairs of observations in December and January which he attributed to an increase of distance from the earth (Forbes 1975, p. 110). Ironically, if Snell had moved beyond the single calculation, he would have found similarly large deviations for the comet of 1618. Oblivious to Flamsteed's work, Newton also understood deviations to be an indication of distance but evidently thought the deviations were quite small.

As shown by Ruffner (1971), opinions about the true motion of comets divided sharply between those following Tycho (1648, bk. 2, pp. 98–99), who thought that their short lived path must be an imperfect arc of a circular or (later) an elliptical path mimicking the planets; those following Kepler (1619, pp. 3, 93–94), who thought that comets being ephemeral bodies must move in rectilinear paths amongst the planets; and those following Kepler (1983, part 3, arts. 119, 126–129), who contended that comets were dead stars that wander according to no known principle from vortex to vortex near their outer edges, well beyond the orbit of Saturn in the case of the solar vortex. Gassendi (1658, 1, p. 27) preferred to think of comets as everlasting bodies moving with uniform rectilinear motion through boundless space that begins nowhere and ends nowhere. Harking back to the ancients, Ward (1653, p. 21 *et seq.*) dismissed Descartes' idea with a passing mention going on to strongly oppose the views of Kepler and Gassendi. As aptly summarized by Plot (1677, p. 225),

> [Ward] who rather embracing the opinions of *Diogenes*, *Apollonius Myndius*, of the *Chaldeans*, and at length *Seneca*; That *Comets are perpetual stars and carryed about in a continued motion*; than of *Kepler*, who thought of them still produced de novo, quickly perishing again; or of *Gassendus*, who held indeed they might be *corpora aeterna*, but yet that they always moved in straight lines;
> [Ward] proposed this new theory of them, viz. that it is much more probable they might be carried round in *Circles* or *Ellipses* ... so great that the Comets are never visible to us, but when they come to the *Perige's* of those *Circles* or Ellipses, ... these vast *Orbs*, which by reason by of standing in an oblique, or perpendicular posture to the eye, he demonstrated might well seem to carry them in *straight lines*

Rather than travelling in orbits and merely seeming to be carried in straight lines, as Ward put it, the more generally accepted view of comets in mid-seventeenth century English philosophical circles was summarized by Streete (1661, p. 11),

> Comets do always appear unto us much nearer [than the Fixt-Stars], and amongst the Orbs of the Planets, they are generated of Planetary substance, but incompact and dissolvable, Illuminated (as the Planets) by the Sun, and according to the general consent of observations, their motions are (as Kepler defines them) in or near to right lines.

About the same year as Streete's book, perhaps in response to the comet of 1661, Christopher Wren, then Savilian Professor of Astronomy at Oxford, devised a scheme to determine the distance of a comet assuming it moved in a straight line with uniform speed and challenged Robert Hooke, John Wallis, and others to do the same.[1] Put to the test with the comet of 1664, Horrox (1678, p. 42) found the path to be somewhat curved but indicated nevertheless that the Wren method "did very near solve all the appearances preceding and subsequent." Unknown in England at the time, Huygens (1888–1950, 5, pp. 230, 266, 361, 388) berated Kepler for maintaining strict straight line motion but found the idea worked well for the comet of 1664 except near the end of visibility. Huygens found that straight lines worked less well for the comet of 1665 but for Huygens as well as Hooke, the general approach was to assume uniform straight line motion and adjust the path by adding slight curvature or speed changes to fit further observations. Flamsteed (1995–2002, 1, p. 552), on the other hand, noting similarities in the apparent paths, proposed an orbital period of about 12 years for the comets of 1665 and 1677.

Newton's earliest views on comets are not documented. Wide reading as a student touched only incidentally the full range of opinions about comets since antiquity. The appearance of the comet of 1664 lit Newton's interest sufficiently to make observations, the background reading for which, starting with Streete, the Snell/Rothmann volume, and Wing (1651), served as Newton's introduction to practical astronomy (McGuire and Martin Tamny 1983, p. 300). Newton's student essays and notes entitled *Quaestiones quaedam philosophiae* (Certain philosophical questions) included his observations of the comets of 1664 and 1665 and queries doubting Descartes' optical theory of tails and Descartes' idea that comets when in the solar vortex ride near its outer edge (CUL MS Add. 3996, ff. 93, 114–116). Newton's reading notes (CUL MS Add. 3958(B)1, ff. 9–13) included items from Thomas Sprat's *History of the Royal Society* (1667) and issues of the *Philosophical Transactions* between March 1664/1665 and April 1667 which treated most notably the dispute between Adrien Auzout and Johannes Hevelius over the accuracy of Hevelius' final observation of the comet of 1664. Newton offered no theories of his own during this period. As a student, as later, Newton was interested in data offered by others rather than their ideas. Nevertheless, Newton's treatment of comet data was consistent with Streete's view.

3 Two key mathematical techniques

About 1677, perhaps in association with the comet that appeared in April, Newton (1967–1981, 5, pp. 210–213) posed a problem: "To determine the position of a comet proceeding uniformly in a straight line ... from three observations of its course."[2] This

[1] According to Wallis, Wren issued the challenge in 1661 or 1662 (Horrox 1678, appendix, p. 1). The manuscript of Wallis' derivation which I have not seen (Bodleian Library Oxford MS Don. D. 45, ff. 283v-280r (reversed) carries the title "*Problema. Di Christophori Wren, mihi propositur, J. K. Joh. o. A°* 1665," the first part of which translates, "Problem, proposed to me by Dr. Christopher Wren." D. T. Whiteside conjectured long ago in a personal communication that the rest of the title probably refers to "*Pridie kalendarum Junii Anno 1665*" or "the day before the first day of June, 1665," which might refer to the actual date of the Wren's challenge but more likely to the date of the manuscript. Wallis' derivation is in Horrox (1678, appendix, pp. 1–9). Wren's solution is in Hooke (Horrox 1678, pp. 41–42, table 5, figure 19).

[2] Cometæ in linea recta ... uniformiter progeredientis positionem cursus ex tribus observtionibus deteminінare.

method, listed as Problem 16 in what Newton purported to be the ninth of ten Lucasian lectures for the academic year beginning October 1676, assumed a stationary observer and tracked the apparent motion of a comet across the celestial sphere. The method does treat an actual path as Whiteside thought (Newton 1967–1981, 5, pp. 211–212, n. 251). The main function of this three point method seems to have been Newton's earliest method for interpolating comet observations before adopting the method of differences.

The Wren/Wallis method for calculating an actual rectilinear path was adapted by Newton sometime between 1678 and 1680 and became the basis for what Newton alleged to be the second of eight Lucasian lectures in 1680–1681, listed as Problem 52 (Newton 1967–1981, 5, pp. 299–303): "From four observed positions of a comet crossing the sky with a uniform rectilinear motion, to gather its distance from the Earth, and its motion and fixed direction, supposing the Copernican hypothesis."[3] A preliminary title without proof, "On finding the distance of a comet in the Copernican system,"[4] was included with a full treatment of an equant (CUL MS Add. 3963.1 f. 1v). Whiteside (1964, p. 127, n. 37) believes the simultaneous interest in an equant predated Newton's correspondence with Hooke in December 1679 but solutions to mathematical problems were always of interest to Newton. Whatever the actual dates and circumstances of these mathematical exercises, documents that can be dated with reasonable confidence between January 1680/1681 and April 1681 indicate Newton was building a comprehensive research program on comets using the techniques of Problems 16 and 52, and related Platonic ideals of straightness.

4 The Comet of 1680 and Newton's revived interest

Foul weather at Cambridge, London, Paris and much of northwestern Europe obscured the comet of 1680 on all but a few days in November.[5] Few people throughout the region were able to see it let alone make measurements. In time, reports trickled in from the south of France, Italy, and Germany where conditions were better. Newton was probably tending his 'chymical' furnace and may have been totally oblivious to the comet. If Newton actually lectured on the Wren/Wallis method during the fall term of 1680, it was an abstract problem unrelated to the real world phenomena. When making inquiries later about what others observed in November, Newton had nothing of his own to add. But having spotted the tail on December 12, or more likely based oo those who had seen it, Newton's long standing interest in comets began to be revived (Schaffer 1987).

[3] E Cometæ motu uniformi rectilineo per Cœlum trajicientis locus quatuor observatis, distantiam a terra, motusque determinationem, in Hypothesi Copernicanæa colligere.

[4] De invention distantiæ Cometæ in Systemate Copernicæa.

[5] Hooke (in London) managed without instruments to obtain reasonable fixes of the comet's position on 2 days in November which were briefly discussed at the Royal Society (Birch 1756–1757, 4, pp. 57–58). Hooke's observations from late December to early February gathered details of the comet's physical appearances rather than its motion. Hooke presented this information in a lecture at the Royal Society in fall 1682 (Hooke 1705, pp. 150–159).

On December 15, the next clear night, Newton viewed the comet casually in company with two colleagues, a Mr. Bainbridge and John Ellis. Making no notes at the time, they later recalled the tail stuck up like an outstretched finger which they reckoned to be about 41° long. The head although nearly obscured by moon light appeared as a small star with the aid of a lens or small telescope but no position measurements were obtained (Newton 1959–1977, 2, p. 366).[6] This appearance undoubtedly sparked discussions among the few Fellows and Scholars who had witnessed the November event.

The next 2 weeks may have offered two or three opportunities to view the comet but Newton indicated he had not minded it then. He was busy preparing a critique of Thomas Burnet's *Telluris Theoria Sacra* (Sacred Theory of the Earth) in which Newton notably invoked mechanical properties of the solar vortex.[7] Newton also would have received a letter (the first ever) from Flamsteed dated 15 December 1680 sent by way of James Crompton in which Flamsteed boasted this "December" comet was the same comet that appeared in November, just as he had predicted.[8] Flamsteed included brief descriptions of the comet's tail and a hastily reduced position of the head on December 12. It is not known if Flamsteed's letter had any immediate effect on Newton—it would have been received at the peak of involvement with issues raised by Burnet. By December 29, however, Newton had become fully engaged in observations of the comet, recording arc distances from reference stars and making far more detailed notes of the tail than he had done in 1664.

Newton's observations extended from December 29, 1680 to March 9, 1680/1681 using a variety of techniques. A full examination of Newton's sources, practices, and results requires a separate study but some quick points can be noted. McGuire and Tamny (1983, p. 300) indicate Newton's descriptions of stars in his comet observations of 1664 and 1665 were stock phrases from Tycho's catalogue of stars as translated by Wing (1651). It also should be noted, these phrases, largely the same since the time of Ptolemy, mean little without corresponding depictions of the mythological images on a globe or star chart.[9] Based on Newton's description of stars in the constellation Cetus, he was not using the charts in Bayer (1603)Bayer's *Uranometria* (1603).[10] The

[6] While conditions also prevented Flamsteed from making measurements in relation to reference stars, he judged the tail to be 50° long (Flamsteed 1995–2002, 1, p. 747).

[7] Newton's initial response of Burnet's theory, now lost, was dated 24 December 1680. A relevant fragment concerning the vortex was subsequently quoted by Burnet (Newton 1959–1977, 2, p. 322).

[8] Flamsteed (at Greenwich) did not see the comet in November but based on reports he predicted the return of the comet in the evening after passing the sun. When the tail appeared in early December 1680 Flamsteed launched a systematic series of observations continuing into early February. As fast as he could make them, Flamsteed sent hastily reduced observations to various correspondents. Flamsteed routed a letter to Newton because he had been impressed by the telescope Newton sent to the Royal Society and assumed Newton would be interested. A few months later Flamsteed referred to Newton as the learned Professor of Astronomy at Cambridge (Forbes 1975, p. 113). For other recipients see Flamsteed (1995–2002, 1, pp. 747–763, 780–784).

[9] See Buchwald and Feingold (2013, pp 252–257) who also discuss the similar problems that Newton faced in the early 1700s when dealing with positions of the Colures in the constellations as described by Hipparchus.

[10] According to Harrison (1978) Newton owned an edition of *Uranometria* published without the guide in 1655 and a separate *Explicatio* published in 1640. Acquisition dates are not known. The unified edition is available on the Internet at lindahall/bayer.

use of an unidentified (and little noticed) celestial globe in describing the position of the second comet on April 1, 1665 as "being in or very neare ye Tropick wth longitude ♈ 4d or thereabouts by the Globe" (CUL MS Add. 3996, f. 116v) suggests it also was Newton's key to identifying the stars.[11] Tycho's catalog would have been used for making reductions of the raw observations and providing the stock phrases for the write-up, but Newton also included private notation as he would continue to do later.

Newton had not been involved with practical astronomy for 10 or 15 years when interest was revived by the comet of 1680.[12] He still would have needed a pictorial guide to identify all but a few major stars. Newton's star descriptions (CUL MS Add. 3965.14, ff. 614r-613r) for observations in December and January were his own and not the stock phrases in Tycho's catalog, except incidentally, such as Andromeda's head (α And) or Andromeda's right knee (φ And). The constellation Triangulus universally depicted as a simple triangle was traditionally described in terms of the apex and base whereas Newton described it in relation to the angles. Newton also identified a sixth magnitude star in Triangulus not found in any known prior source, making several cross references from it to the comet and the other stars. Newton termed this new star the 1st star and relabeled the two southern stars in the east angle the second and third stars according to their increasing longitude.[13] Newton's left/right description of the more typical mythological images rule out the use of Bayer's atlas. The mythological images on a star chart face the earth, while the images on a globe, with a few exceptions such as Andromeda, face outward reversing the left and right sides. Newton's descriptions of mythological figures were common sense readings of the outward facing images a celestial globe. Tycho's left leg of Pegasus [κ Peg] became Newton's "more northern star in the left foot of Pegasus," Tycho's star in the girdle of Cassiopeia [η Cas] became Newton's "middle star in the belly" (f. 614r),[14] and so forth. A close examination of the artistic details in the images on various globes might serve to establish the maker or at least eliminate certain ones. Mercator's globe (1551) can be ruled out by the lack of a depiction for a sixth magnitude star in the north fish,

[11] Buchwald and Feingold (2013, pp. 36–42) provide a detailed account of technical issues concerning Newton's observation of the comet of 1664 but fail to note Newton's use of a globe in this observation of the comet of 1665. Hall (1992, p. 91) notes that the duties of the Professor of Mathematics included instructing students in the use of globes and other mathematical instruments.

[12] For Newton's possible interest in astronomy around 1671 see CUL MS Add. 3985B, ff. 38r-40r, "Table of ye fixed Starrs for ye yeare 1671 of ye three first magnitudes." The table is in Newton's hand and includes about 225 first three magnitude stars, plus about fifty 4th magnitude stars, one 5th magnitude star and several "nebula." The coordinates most likely are from Tycho's catalog in Wing (1669) with 60′ longitude added for precession. It has slight differences from Kepler (1627) which may not matter in this case. Corresponding equatorial coordinates are added. Newton's descriptors are in English, freely translated in brief from Tycho's Latin. Wing (1651) was not likely Newton's source because it has many significant typographical errors and the English translations do not match Newton's descriptions. The purpose is not known. Around the same time Newton admitted he would have paid more attention to a dull star-like object if it had displayed a tail (*Philosophical Transactions* #81, 1672, pp. 4017–4018). See also Buchwald and Feingold (2013, pp. 271–272) who discuss the discrepancy in the precession allowed for the year 1671 but think the document might have been written later.

[13] The star probably was a 6th magnitude star designated eta Triangulus by Hevelius (1690) and zeta Triangulus by Flamsteed (1712). Newton's distances were none too accurate when compared with corresponding values calculated from the coordinates of Hevelius or Flamsteed.

[14] Borealiorem in pede sinistro Pegasi and mediam in ventre Cassiopeiae.

ore Pisces [σ Psc], used for Newton's crucial observation of January 11. Moreover, Newton noted a faint tail extended on January 11 up to the "small star in the left side of Perseus next to the girdle (or sword belt) where it ended"[15] suggesting he was guided by a pictorial representation of a clothed figure rather than Mercator's naked figure. Tycho designated the star the left elbow of Perseus [κ Per]. The Dutch globe maker Willem Blaeu or Blaue using Tycho's coordinates depicted it as a fourth magnitude star where the elbow touches the left side of Perseus at waist level atop a skirt. According to Buchwald and Feingold (2013, p. 332), Blaeu's globe had dominated the market for decades until John Senex introduced globes around 1707 based in part on Newton's work. Other globes, however, would need to be examined before drawing conclusions about the globe Newton was using in the 1680s. For example, another Dutch globe maker Petrus Plancius also using Tycho's coordinates depicted what seems to be a more distinct belt or girdle consistent with Newton's description. Other possibilities include those made by the London globe maker Joseph Moxon, who learned the craft in Holland in the 1650s, and London rivals such as Robert Morden, William Berry, and Philip Lea (Wallis 1978, pp. 4, 12).[16] As the work progressed, Newton also began to rely on Bayer's atlas.

Whichever sources Newton used for identifying stars and plotting the approximate course of a comet, it seems reasonable to suppose for purposes of reduction Newton would have wanted the precise coordinates from Tycho's tables rather than protractions from a globe or map.[17] In which the case the question arises why Newton did not revise his descriptions to agree with those of Tycho as he seems to have done in 1664? Newton's accounts almost certainly were written up from more basic observation notes after consultation with Tycho's tables. In the one instance that Newton provided

[15] Stella exiquam in latere sinisrtro persei juxto cingulum ubi desinebat.

[16] Reproductions of individual images from Mercator's globe are available at lib.harvard.edu. Gores for Blaeu's globe are at the Bodleian Library, Oxford and for Plancius' globe at BNF, Paris. I have seen only small scale reproductions of one half of Blaeu's gores in Whitfield (1995, p. 84) and the full set of Plancius' gores in Lachièze-Rey and Luminet (2001, p. 96). I have not seen images from Joseph Moxon's celestial globes or those of his London rivals, but they undoubtedly displayed some variations in the depictions. A case in point is the celestial map by J. Moxon (Cosgrave 2001, p. 155) which included several images significantly different from those depicted by Mercator, Blaeu or Plancius and which also shows various points of agreement with Newton's descriptions of Aries and Cassiopeia but disagreements with Newton's descriptions in Perseus and Pisces. Cosgrave (2001) attributes the Moxon map to Joseph dated variously as 1654 (pp. 154–155) and 1674 (p. 196) but the dedication to John, the Archbishop of Canterbury on Cosgrove's reproduction of the map indicates otherwise. John Tuttle was Archbishop from May 1691 to November 1694. Joseph Moxon died in February 1691. The map was printed by his son, James Moxon. In any case, the connection between the iconography of the globe and the map needs to be established.

[17] An important exception to the use of Tycho's coordinates occurred with Newton's observation of December 29. Newton referred to a star in Andromeda, *annulum in termino catenae* (the ring at the end of the chain). Based on Newton's determination of the arc-distance from Andromeda's head, the star was not Tycho's star in Andromeda with a similar name, *Quae in extremo catenae annulo* (the outermost ring of the chain), but rather Bayer omicron Andromeda, *In Cathenae annulo, nonnullus tres in dextra manu antecedens* (the star in the ring of the chain preceding the three stars in the right hand). The latter star was catalogued by Ptolemy (1998, p. 380) without mention of a chain. According to Whitfield (1995, pp. 68–69) images of Andromeda including the chain became commonplace following the Vienna Manuscript c. 1440, the earliest surviving star map of the northern hemisphere. Mercator used Ptolemy's coordinates adjusted to 1551. Blaeu and Plancius, who usually adopted Tycho's coordinates, used Ptolemy's coordinates adjusted to 1601.

unambiguous coordinates for a star he used Tycho's values adjusted for 68′ increase in longitude (CUL MS Add. 4004, f. 99r).

Newton followed the comet by naked eye until the end of January using a high quality monocle lens to make it distinct (CUL MS Add. 4004, f. 101r; Westfall 1980, p. 392). The earliest observations in the one extant set of calculations (CUL MS Add. 965.11, f. 153) were based on equated (mean solar) time recorded on the hour, half hour, or quarter hour.[18] Experienced observers such as Flamsteed and Hevelius made repeated observations using apparent (true solar) time at whatever hour and minute conditions dictated. The only instrument Newton mentioned for the observations in December and January was a high powered three foot "perspective" to determine the arc distances between the head and nearby reference stars by comparing the separation to the full field of vision of the instrument (Newton 1959–1977, 2, p. 366).[19] This technique would serve as the basis for Newton's observations of the diameter of the comet's head on January 8 and its position on January 8, 11, 23, and 24. Newton's more general technique found alignments of the comet with one or two pairs of stars, sometimes including a phantom point midway between them, with the comet's distance from one of the alignment points estimated to be equal to or a simple fraction of the distance between two nearby stars. Little more than summaries of the alignments and arc distance determinations have been found. For example, CUL MS 3965.14, f. 614v lists, "Dec 30, 9 pm the comet was in the line drawn through the southern-most of the two stars in the breast of Pegasus [λ Peg] toward the middle star in the belly of Cassiopeia [η Cas]. It was distant from that southern star 3/4 of the distance that the southern one was from the northern one [μ Peg], and was located on the south side of both stars."[20] By April 1681, the only one of these observations Newton regarded as tolerably accurate was for January 11, and only after juggling the parameters (Newton 1959–1977, 2, p. pp. 365–366).

Newton switched to a seven foot telescope on January 30. Extant notes do not include major reference stars that would have been needed to determine the comet's coordinates (CUL MS Add. 3965.14, f. 598; MS Add. 4004, f. 100r; see also below). Even less complete notes are found for Newton's observation of February 5 (CUL MS Add. 3965.14, f. 615r). In short order, he devised a micrometer for this larger instrument to make precise measurements as the comet faded further from view. What may have been a trial run with the micrometer was made on February 10 (CUL MS Add. 3965.14, f. 615r). Detailed observations still using nonstandard star descriptions were made with this device from February 26 to March 9 but (as discussed below) evidently not reduced to standard coordinates until 1684. Newton also used a sector to obtain certain angle measurements (CUL MS Add. 3965.14, f. 554v). In this case,

[18] Newton's student notes indicate the same practice, following Snell (1619) who also recorded observations on simple fractions of an hour (McGuire and Martin Tamny 1983, pp. 412–419). A point that needs further research is how and who regulated clocks at Cambridge during these years.

[19] It is interesting to note that one of the scholars also used a small pocket perspective when viewing the November comet (Newton 1957–1977, 2, p. 344).

[20] Dec 30 Hor 9 Cometa erat in linea ducta per australiorem duarum in pectore Pegasi ad medium in ventre Cassiopeiæ & distabat ab illa australiori 3/4 partibus distantiæ australioris illus a borealiori, jacens ab illa australiores utrusque

he made observations according to apparent (true solar) time, still at quarter hour readings.[21]

5 Newton's first calculations

By early January, Newton had several fully reduced observations of his own, Flamsteed's original report for December 12 and a second letter from Flamsteed dated January 3, 1680/1681 providing positions for December 21, 24, 26, and 30 with moderately detailed descriptions of the tail. Newton quickly put these data to use. The actual worksheets are missing or not yet identified. Only summaries remain in CUL MS Add. 3965.14, f. 598 and MS. 3965.11, f. 153r. I prefer to think of these and related documents as having been retrieved from Newton's 'waste basket' because of mistakes or work completed elsewhere. It is easy to imagine Newton shifting from sheet to sheet, whatever was at hand, as he recorded observations, drew sketches, made calculations, assembled notes, and generally progressed with his work. Much of this material, especially the most polished effort, is lost. As such, extant documents indicate the direction of Newton's work but do not necessarily reflect his settled thoughts.

Results listed in CUL MS Add. 3965.14, f. 598 were based on Flamsteed's observations from December 12 to December 26 (as originally reported) and Newton's observations of December 30, January 4 and 11.[22] The times of Flamsteed's observations were rounded to the nearest quarter hour in agreement with Newton's practice. This procedure neglected roughly 1′ or 2′ changes in Flamsteed's positions. Changes in the manuscript indicate Newton discovered mistakes which he partially corrected before moving on to other work. The calculations might have been done during a spell of bad weather from January 13 to January 22.[23] No obvious application has been found. As a student, Newton had copied similar information from Snell (1619) without bothering to include Snell's subsequent treatment of the ascending node. Now Newton's work came to depend heavily on the apparent position of the ascending node. When and how it became an active factor in Newton's analysis is not clear to me.

Shortly after completing this treatment of apparent motion from December 12 to January 11, Newton turned to the more difficult task of determining the comet's actual trajectory under the assumption of uniform rectilinear motion. Again the original work sheets are missing. Only a summary of the work is extant (CUL MS Add. 3965.11, f. 153r). For example, the apparent distance of the comet from the sun on December 21, 4:30 pm was 31°59′ while the corresponding true distance was 52,912

[21] The sector would have permitted measurements of star altitudes from which the apparent time and equation of time could be calculated, if that was his method.

[22] Newton did not list the coordinates and provided only the distance between each pair of dates. For example Newton indicated the distance between the comet on January 4, 8 pm and January 11, 10:30 pm was 14°55′. My reductions using Newton's parameters and Tycho's co-ordinates plus 68′ place the comet on January 4 at about ♈ 6°51′, 26°36′ N and on January 11 at ♈ 22°52′, 23°12′ ½ N . The distance between these positions calculated by the cosine rule is 14°54′.

[23] The Paris observations had a gap between January 13 and 23. The gap at Greenwich was between January 11 and 25. Newton obtained a partial view on January 13. The final round of correspondence with Burnet on the sacred theory of the earth was completed late in this period (Newton 1959–1977, 2, pp. 321–335).

(a.u. = 100,000). Newton was thinking of work eventually covering both sets of appearances because the sheet starts with mean solar positions at noon based on tables in Wing (1669) for 10 day intervals from November 10, 1680 to March 30, 1681 plus December 12, the date of Flamsteed's first observation.

Newton specified the comet's positions for only three dates with corresponding solar positions based on Wing. Two of them were Flamsteed's observations for December 21 and 30 as posted on January 3 (Newton 1959–1977, 2, p. 354). He did not round off Flamsteed's times as before but assumed the observations were made according to equated time. He remained unaware that Flamsteed used true solar or apparent time until mid-February. Flamsteed's observation for the 30th was used rather than Newton's observation as in the previous document. The third place was Newton's observation of January 11 calculated for 10 pm rather than 10:30 as in that previous document (Newton had made several measurements over the course of 2 h). The latitude determined for the 11th was based on a 10 arc minute distance from sigma Pisces rather than the 9 arc minute parameter cited in Newton's letter of April 16, 1681. The stated logarithms of the sun-earth distances were seriously defective, for example, placing the earth closer to the sun on January 11 than on December 21. Several other listings have calculation or tabulation errors up to several minutes.[24]

Worked in reverse, Newton's parameters describe to graphical accuracy a straight line path reduced to the plane of the ecliptic at a uniform speed of about 0.0471 a.u./day or some three times the earth's mean speed (see Fig. 1).[25] A slight scatter in the reconstructed latitudes leads to some uncertainty in the exact time and place when the comet crossed the ecliptic, but it would have occurred around November 23 about 0.9 a.u. from the sun, as if emerging from the depths of space at a geocentric longitude of about ♐14°.

A fourth position would have been required to calculate such a path but Newton provided no parameters in the summary. December 12, already included in the preliminary list of solar positions, would have been the logical choice but Flamsteed's original coordinates do not fit Newton's solution. After a further look associated with the initial arc distance calculations, Newton may have set it aside as unreliable because at the first opportunity he queried Flamsteed as to whether allowance had been made for refraction in its reduction.[26] If Newton used another observed position there would have been no reason not to list it. Did he use extrapolated parameters for the node as a fourth point? The information listed by Newton establishes only the heliocentric longitude of the node which could have been obtained after the fact. By long standing practice the place of the node was readily determined using any two observations

[24] For example, in what could be a tabulation error, the comet's elongation from the sun on January 11, 10 pm is given as 80.17°(80°10′) whereas the sun's longitude is given as ♒ 2.7°(2°42′) and the comet's longitude as ♈ 22.817°22°49′ for a difference of 80.117°(80°7′).

[25] Newton commonly made calculations using ratios but sometimes used decimal equivalents.

[26] Flamsteed had corrected for refraction but had made a mistake in the position of the planet Venus used for one of the reference points. The corrected value differed by almost 2° longitude and 1° latitude (Newton 1959–1977, 2, p. 353). Most importantly the revised position fitted its expected spot along Newton's rectilinear path.

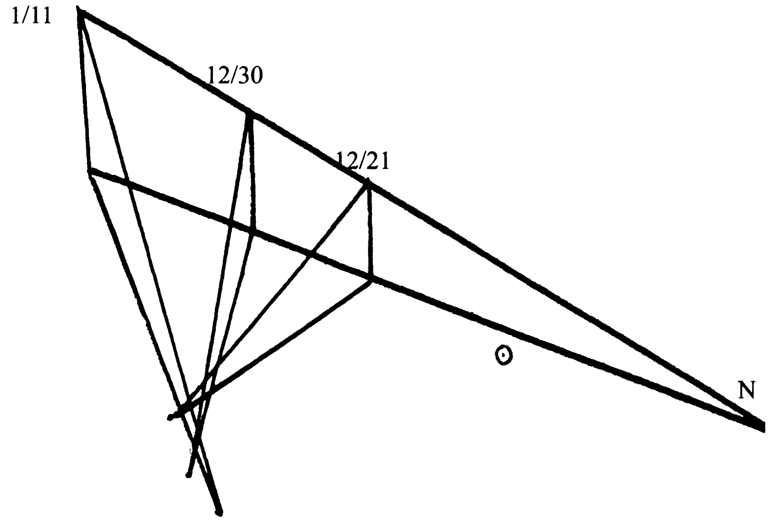

Fig. 1 Comet of 1680 reconstruction of Newton's original path (January 1680/81)

assuming the path followed a great circle arc.[27] The methods of Problem 16 or some other extrapolation technique would be needed to determine a date.[28] I have been unable to calculate geocentric parameters for the node that agree with those revealed in the reconstruction, although to be sure there are uncertainties in the exact placement of the node in that reconstruction. There are too many uncertainties to determine the approach Newton used or why he did not list parameters for the required fourth position. The lack of details for a position on January 23 or 24 may indicate the calculations were made before those observations or only Newton's distrust in their accuracy. The solution was certainly accomplished before the middle of February when Flamsteed's first set of revised data arrived.[29]

Newton shifted to a seven foot telescope on January 30. Notes for the observation (CUL MS Add. 3965.14, f. 598), added below the calculations of apparent motion discussed above, included a map, distances between the comet and stars within a few arc minutes, and a brief account of the tail. The reference points were telescopic or small uncharted stars along the comet's path between the constellations Triangulus and Perseus designated a, b, c, etc., for which no coordinates were available. Newton noted the position of the tail in relation to major stars such as Capella and Algol but listed no alignments of the head with these stars from which the comet's coordinates

[27] Newton may not have ventured beyond the analysis in the Snell/Rothmann (1619) and not yet realized how greatly the apparent position of the node can vary with different selections of data points, as in the case of 1680 (Forbes 1975, p. 28; Ruffner 2010, pp. 436–437).

[28] In 1685 Newton offered a graphical method to resolve Problem 16 more accurately in System of the World, Lemmas 3–5 (Newton 1728, pp. 147–150; Newton 1960, pp. 623–624).

[29] Wren also found from Flamsteed's observations of December and January that the comet was moving in accord with his hypothesis of uniform straight motion (Birch 1756–1757, 4, p. 67). At this point Wren believed there were two comets. Wren's reaction to Flamsteed's subsequent hypothesis of a single comet is not known.

could be calculated. Newton may have at least marked the approximate position on a globe. The entry concluded with an indication the comet moved about 5° from day 24 to day 30. This part of the document represents work in progress around January 30 or 31, 1680/1681. Around the end of January and early February, as will be shown, Newton was also trying to reconstruct details of the local November observations and had begun a systematic survey of historic comets, evidently beginning with Riccioli (1651).

Newton's third entry on CUL MS 3965.14, f. 598 (right panel) provided observations made by an anonymous person in Hamburg during December and early January. Since the entry is in English it would seem Newton copied it from a letter sent by a traveler to him or a colleague. Newton made no mention of these observations in his letter of February 28, 1680/1681. A revamped version in Latin was partially inserted in the margin and partially in chronological order with similar data in Newton's Waste Book (CUL MS Add. 4004, ff. 99r/v). It would seem Newton obtained the Hamburg information in late March or early April 1681 just as he was in the midst of making the compilation.

6 Newton's initial gathering of data

CUL MS Add. 3965.14, 613v-614r/v-613r entitled "*Observationes & Scolia*" has four distinct sections (Ruffner 2000). The first entry under the title at the top of folio 613v was a report of observations for December 15, 16, and 19, 1680 (subsequently identified in the Waste Book as from Scotland).[30] A list of memorable comets and associated phenomena followed. Newton's observations from December 29 to January 24 and a note for December 15 were added later because they snake around these initial entries. The entry for January 4 included an estimate of the maximum distance of the comet from the sun based on the apparent length of the tail. The calculations were made on the verso and the entry for January 11 was written to avoid obliterating them.[31] Untitled statements I have termed *Propositiones de cometis* were added still later from the end opposite the last observation on folio 613r. Attention has focused on these propositions because they contradict many points made by Newton in the Flamsteed correspondence and include a mixture of "new" and "old" ideas about the gravitational nature of comets from a later stage of development. Conjectured dates for the propositions have fluctuated between 1681 and 1684 or 1685 (Whiteside 1970, p. 14; Westfall 1971, pp. 459–460; Dobbs 1988, pp. 77–78; Bertoloni Meli 1993, p. 171; Ruffner 2000, p. 271).

The memorable listings and observations prove on close analysis to be of comparable significance to the propositions and can be dated with reasonable certainty to late January or early February 1680/1681. For example, the information from Scotland was cited in Newton's letter of February 28. Also the description of the tail for

[30] The original report included a mangled statement that mixed equatorial and ecliptic coordinates which Newton converted to preferred ecliptic style for the Latin version in the Waste Book, f. 99r. Its entry in the margin near the appropriate date may indicate Newton temporarily misplaced the sheet or only belatedly decided to include the information after correcting the error.

[31] The method is described in Newton (Newton 1728, pp. 127–128; Newton 1960, pp. 612–614).

December 15 was replaced in the subsequent Waste Book compilation by Flamsteed's description received in mid-February. Further, the observation for January 11 included the 10′ parameter used in the rectilinear resolution rather than the 9′ parameter cited in April. Such dates in 1680/1681 by themselves settle nothing about the date for the propositions because an individual document can span decades of use.

The memorable comets comprised a dozen appearing between 431 B.C. and 1618 A.D. selected based on reports of an extraordinary tail, great brilliance, or long duration. The list was consistent with what could have been gleaned from Riccioli (1651, pt. 2, v. 1, bk. 8, pp. 3–18, *passim*). A longer list would have been expected from the treatises of Hevelius (1668) and Lubieniecki (1666–1668) which presented more comprehensive and more detailed compilations. The great comet described by Aristotle was dated 372 B.C. perhaps in keeping with Riccioli who dated it 372 or 373 B.C.[32]

The memorable comets were followed by lists of phenomena noted in some thirty one comets since 1530. This material (Newton's scholia) was for the most part compatible with what could have been gleaned from Riccioli and would have been used by Newton to demonstrate location in the vicinity of the sun. Comet tails stand more or less opposite the sun and bend backward from the direction of motion.[33] The comets of 1577, 1585, and 1590 had no sensible parallax, the point being when last reported these comets were well beyond the orbit of the moon but close enough to the earth to be visible to the naked eye. The comets of 1582, 1596, and 1607 exhibited parallax from annual motion, another indication of positions amongst the planets. The comets of 1577, 1582, 1585, and 1569 did not follow a great circle exactly near the end of visibility when their apparent motions were very slow, arguably the result of the earth's annual motion and positions not far the sun.[34] Newton sorted the information into categories indicating the part of the heavens traversed: opposite the sun, in the solar region, in the polar region, in more than one region, or in unspecified regions with the

[32] An enhanced version of historical material selected from*Cometographia* is in CUL MS Add. 3965.11, ff. 172r-173v drafted in 1685 as a worksheet for *De Motu Corporum Liber Secundus*, CUL MS Add. 3990. Newton selected more than fifty major comets and accepted Hevelius' date of 371 BC for 'Aristotle's' comet. See Ruffner 2010, pp. 429–432.

[33] Tycho, Kepler, Gassendi, Hevelius, and others noted deviations from strict anti-solarity. Published indications that the deviation was backwards from the line of motion however have not been found. Newton's generalizations, here and elsewhere, may have been based on just a few examples in his own observations. Flamsteed's analogy of smoke trailing from a moving ship was also consistent with Newton's view.

[34] Riccioli did not provide exact statements of great circle departures in these four examples. Tycho had touted the exquisite apparent great circle path of the comet of 1577 and the data provided by Riccioli (p.11) was clearly based on such a track. The only counterclaim noted by Riccioli (p. 52) was due to Claramontius in *Antitycho* (1621). Whether Newton accepted Claramontius or used some other source is not clear. Riccioli's information for the comet of 1582 (p. 13) extracted from Kepler (1619, p. 126) indicated it was retrograde and became stationary at the end. Newton might have understood this evidence as indicating departure from a great circle track. Tycho's data for 1585 provided by Riccioli (p. 14) revealed a slight decline from an initial great circle track as was also demonstrated by Rothmann (Snell 1619, p. 85). Riccioli (p. 10) credited Kepler (1619, pp. 114, 129) with information the comet of 1569 deflected from its line of motion and became stationary at the end but Kepler provided different information about the comet of 1659 on those pages. The information on deflection in fact pertained to the comet of 1596 which Riccioli properly cited as from Kepler (1619, p. 120). Newton evidently accepted Riccioli's erroneous information about the comet 1569 without noting the discrepancy when selecting the comet of 1596 as an example indicating parallax due to the earth's annual motion. The error was first noted by Pingré (1783–1784, 1, p. 510).

largest number appearing in the solar region. Again, the lists were compatible with Riccioli's accounts except for comets in 1664, 1665, and 1680 seen by Newton. Four comets observed in 1618 were listed as 18 with four superscripts. The inclusion of only a single 80 would seem to support a date for the document after 1681 except the 80 seems to have been added later.[35] The lack of an entry for 1682 supports an earlier date for this part of the manuscript.

Perhaps the most revealing item for dating the memorable list around the beginning of February 1680/1681 is an entry "1531 *bis*" signifying that twice comets crossed the solar region. A comet appeared in the evening and again shortly after in the morning. Hevelius (1668, p. 909) accepted Peter Apian's view they were the same comet. Newton adopted the view reported by Riccioli (1651, pt. 2, p. 9) that two comets appeared. This bifurcation indicates Newton was not yet using Hevelius as he would before the end of February.[36] Apart from the outline of propositions the rest of the *Observationes & Scolia* document represents work in January or early February 1680/1681. Such a compilation indicates Newton was contemplating an extended treatment of comets apart from any stimulus from Flamsteed. It would not have been necessary to convince Flamsteed of any of these ideas apart from the notion that two comets in 1531 prefigured the current situation. The obvious target, if only for private satisfaction, was Descartes who placed comets beyond the sphere of Saturn or the scholastic tradition of the Catholic Church that considered them terrestrial phenomena.

7 Newton's 'crazy quilt' document

Over time beginning in February 1680/1681, Newton collected a crazy quilt of information about comets (CUL MS Add. 3965.14, ff. 615r/v-616r/v), some of which never appeared again, and some of which did not appear until the third edition of the *Principia* (1726). Fragments of Newton's observations on February 5 (CUL MS Add. 3965.14, f. 615r) include a sketched map of the comet within a few arc minutes of telescopic stars identified by strange designations such as 7c, 9a, 9d, and 10e. The next entries are fragments of Newton's observations for February 10 which do not include arc measurements but rather micrometer values that may represent a test phase for the new device. These fragments are followed by a map and significant material concerning observations between February 25 and March 9 (f. 615r/v).[37] The details and accompanying map on the document were augmented at least once and added piecemeal in different editions of the *Principia* (1686, pp. 491–493; 1718, pp. 456–457; 1999, pp. 905–908).

[35] A small 80 was squeezed in above the capital letter that begins the category in the next line, possibly at a later time using a different quill. There was ample room for double entries if Newton wanted them. In 1680/1681 Newton may not have made any entry for 1680 because he intended to list only the prior comets, but we cannot be certain. See facsimile (Ruffner 2000, p. 261).

[36] Having completely changed his mind, Newton drafted an enhanced version (CUL MS Add. 3965.11, f. 172r) in 1685 that lists 1531 as a single comet along with the notation 1680 = 1681. See facsimile (Ruffner 2010, p. 431).

[37] The gap in Newton's observations from February 11 to 24 was largely due to bad weather or the obscuring effect of the full moon. Observations at Paris Observatory had a similar gap between February 12 and 27.

Newton used an arcane process to convert the micrometer readings to arc measures from which position coordinates could be calculated. Basically he corrected the readings by subtracting some constant factor (CUL MS Add. 3965.14A2, f. 554r). Entries on the crazy quilt document indicate Newton had some difficulty in deciding the proper value. Correction factors for numerous pairs of identifiable stars ranged from 27 2/12 parts to 27 3½ /12 parts. This work evidently dated to about March 1681. Another star plot and table on the flip side used a correction factor of 27 3/12 parts for an indeterminate group of stars (f. 616v).[38] Information cited in April 1681 was drafted to avoid obliterating this information. None of these correction factors have been found in other documents.

The corrected readings can be converted to arc distances once the distance between the two reference stars in the south (left according to Tycho, right according to Bayer) foot of Perseus is known. Incomplete entries in the Waste Book (CUL MS Add. 4004, f. 97r) used a correction factor of 25 8/12 parts as in all editions of the *Principia* but included an erroneous distance of 1°46′6°.[39] Newton discovered inconsistency in the cross distances and left the summary incomplete. If the error was discovered in 1680/1681, Newton sought no help and did not press the issue. Indeed, in April 1681 Newton claimed he did not yet have their accurate coordinates.[40] At the same time, if not very forcefully, Newton suggested that Flamsteed might consider fresh determinations of the coordinates for another star. It is likely the aborted Waste Book entries represented work in December 1684 after Newton settled on a proper correction factor for the micrometer readings and just before seeking help from Flamsteed (Newton 1959–1977, 2, pp. 403–408).[41]

8 Collegial exchanges

Flamsteed had not seen the comet in November, but based on the report from his assistant at Greenwich and others Flamsteed predicted it would be seen again in the evening after passing the sun and eagerly awaited the return. When a tail appeared in the evening of December 10, Flamsteed began observations extending to February 8 which he passed seriatim to Newton and others. Giovanni Domenico Cassini began observations at the Paris observatory extending from December 19 to January 13, with cruder observations extending to March 8. Cassini also received November observations from observatories in the south of France and Italy where conditions had been better. Halley, who temporarily assisted Cassini, forwarded this material to

38 The maximum differences resulting from these correction factors would have amounted to about 5′ or 6′.

39 This data is the first entry of the section in the Waste Book devoted to comets. As explained in the appendix, the entry was probably one of Newton's last additions after earlier notes on comets ran out of space and reverted to a blank folio at the beginning of the section.

40 The stars were omicron and zeta Perseus. Coordinates were readily available in Tycho's tables used by Newton for other work at the time. Fresh measurements from Flamsteed in 1684 established the distance at the beginning of 1681 as 2°6′46″. Tycho's coordinates would have yielded 2°10°28″. Newton's result is a mystery unless it was one of his many computational errors.

41 A list of corrected values using different terminology is found in CUL MS Add. 3965.14, f. 552. The data for March 7 is deleted and also omitted from the first edition of the *Principia* but restored later.

Flamsteed who also received data from other correspondents on the continent, notably in Germany. In addition, Flamsteed received via another party several November observations from Thomas Hill, an amateur astronomer at Canterbury, the first one of which gave endless trouble for Newton and Cassini who tried to use it. Many other channels of information flowed through the Royal Society where comets and particularly the comets of 1680 and 1681 were avidly discussed. Even the possibility of very short lived sublunary comets was discussed (Birch 1756–1757, 4, p. 66).

Flamsteed's lost third letter of February 12, 1680/1681 to Newton by way of James Crompton included the first complete set of Greenwich observations from December 12 to February 5, observations for December 19 to January 13 from the Paris observatory, a garbled set of November observations by Jean Charles Gallet said to have been made at Rome, as well as a crude preview of Flamsteed's single comet solution.[42] In accord with his usual practice, Newton analyzed the arc distances between the comet's positions as soon as data were received. An examination of the Greenwich and Paris observations during December, January, and February as received in February proved them both sufficiently accurate, a view modified by further work in March or April. Gallet's observations from November, however, posed serious problems. About this time, Ellis showed Newton a letter by Cassini supporting a two comet solution (Newton 1959–1977, 2, p. 342).[43] Obviously, Ellis, the colleague who probably roused Newton's interest and assisted in the observation of December 15, had his own contacts. Could Ellis have been the conduit for the Scottish and Hamburg observations?

The precise details of Flamsteed's theory in the third letter have been lost because Newton copied only the useful data. Judging from Newton's response of February 28, 1680/1681, they were substantially the same as those sent to Halley on February 17, 1680/1681 (Newton 1959–1977, 2, pp. 336–340). In his first response (Newton 1959–1977, 2, pp. 340–347), Newton utterly rejected the purported solution of a single comet by which it was magnetically attracted by the sun during ingress, made a U turn before reaching the sun, and was repulsed during egress. Newton could allow an attractive power in the sun whereby the planets were kept in their orbits but argued it could not be magnetic as commonly understood because the vast heat of the sun would destroy it. As for comets, like the little needle of a compass, Newton argued, they would always be directed to the sun. Moreover, *if* a comet were so attracted it would never cross in front of the sun against the vortex flow but would be continuously attracted and made to fetch a compass about the sun. Allowing hypotheses to be shaped in the best manner for a confrere to use, however, was not the same as Newton accepting their validity.[44]

[42] Flamsteed or a copyist carelessly mixed old and new style dates. The same mistake was sent to Richard Towneley on February 7, 1680/1681 (Flamsteed 1995–2002, 1, p. 756.) The data sent to John Caswell on February 4 had correct dates, *ibid.*, 1, p. 753 In the *Principia* the correct place is Avignon as indicated by Cassini (1681).

[43] Cassini's book (1681) evidently did not arrive in England until May 1681 although there were two impressions under different titles. Hooke gave a review at the Royal Society meeting of June 8 (Birch 1756–1757, 4, p. 90).

[44] Later, in an unsent draft of April, Newton pointed out that Flamsteed's explanation required the speed of the comet to increase continuously. By that time, Flamsteed had recognized the problem which he solved by assuming the comet had attained its highest possible speed during ingress which weakened with increased distance from the sun (Forbes 1975, p. 115).

Newton agreed only that the path of a comet moving across the streamlines of the solar vortex would be somewhat deflected by the pressure of the flow, independent of any action by the sun. Later, Newton did in fact introduce some slight curvature in the path but failed to allow for a reduction of speed that also would have been entailed. Such retardation would have been most evident in the tail resulting in the backward deflection noted in Newton's "*Observationes & Scolia.*" While Newton granted approval to various aspects of Flamsteed's theory of the tail, presumably as detailed in a letter intended for Halley (Newton 1959–1977, 2, p. 339), he broke off the discussion as too tedious (too obvious?) without addressing Flamsteed's analogy between the deflection of a comet's tail and smoke that trails behind the chimney of a moving ship due to air resistance (Newton 1959–1977, 2, pp. 345–346). Since Newton allowed the vortex to deflect the path of a comet moving transversely across the pattern of flow, he would have agreed that the particles of the tail should have met resistance as a result of their cross current motion. There also should have been some slight retardation of the main body of the head of the comet, in contradiction to the principle of uniform motion.

9 The November comet

Newton and others at Cambridge who had not seen the comet in November had for some time been interested in those appearances. In early December, a Fellow of Trinity interviewed a scholar about what he had seen. About the end of January, Newton pressed the scholar for further details. The scholar initially recalled that around 4 a.m. on November 16, later corrected to November 19, the comet was about two degrees north of Spica which would have placed the comet very near the ecliptic in about ♎ 21° for an arc distance from the sun on the 16th of about 45°. The scholar also remembered a long comet tail extending to another first magnitude star, Deneb (the lion's tail), placing the end of the tail at about ♍ 17°, 12° N, about 36° from the head. Framed by such prominent stars these results could hardly be doubted. In a related interview, Humphrey Babbington recalled that around November 22 or 23 the tail ran just above the east end of Kings College Chapel rising a little higher at the west end. The head was hidden by buildings or located some distance below the horizon. Amplifying this report, most likely through the use of a globe or other analog device, Newton determined that at about 5 a.m. on either date alpha Corvus (its beak) was situated near the top of the Chapel's east end while the constellation Crater (the cup), more particularly alpha Crater, was situated a little higher and slightly beyond the Chapel's west end. Further, Newton estimated that the arc distance of the head from the beak was between 20° and 30° or a little more. Taking alpha Corvus at about ♎ 8°, 22° S and the sun November 23, 5 a.m. at about ♐ 12°, the arc distance would be about 66°. The simplest model, assuming an exact alignment of the sun, comet and end of the tail, would place the comet's head slightly below the horizon about 20° from the beak. A distance of 30° (or better 33°) would have yielded a value with the observed length on November 16 and placed the head well below the horizon at roughly ♏8°, 11° S, about half way between the beak and the sun (Newton 1959–1977, 2, pp. 343–344).

At some point in this process of reconstructing the circumstances of November, Newton received Flamsteed's botched set of Gallet's November observations.

The longitudes were clearly wrong, were the latitudes any more reliable? Gallet's observations indicated the November comet when first observed on November 17 was at the ecliptic, dipped to about 4° south latitude on November 21 and began to return northward before being lost on November 27 as it advanced toward the sun. Flamsteed took this behavior as evidence the comet was being attracted by the sun. Flamsteed also considered the continued northward tract in December evidence the comet was then being repulsed by the sun and accordingly evoked an explanation requiring the head to be polarized.

Newton diverted his research interests long enough to provide Flamsteed with detailed reasons why the attraction (if any) could not be magnetic and why Gallet's observations did not support a one comet solution. Trusting the gist of local accounts, Newton argued the latitude of the comet was much further south than in any of Gallet's observations and that within a few days later the comet probably continued so far south as not to be seen any longer (Newton 1959–1977, 2, p. 344). Newton obviously believed the November comet continued on a trajectory slightly inclined from north to south uninfluenced by the sun. This comet was quite distinct from the comet which according to Newton's analysis emerged from the depths of space in December and followed a south to north path under no solar influence. Newton cited the greater brilliance of the head in November described by the scholar informant as additional evidence for two different comets.

In the meantime, Flamsteed received (via Halley) Cassini's copy of Marco Antonio Cellio's observations at Rome (Newton 1959–1977, 2, p. 349). These observations showed the comet advancing toward the sun with near constant latitude 1° S. About the same time, Flamsteed received Thomas Hill's observation at Canterbury predating all the others that showed the comet at about 2° N on the morning of November 11 (Newton 1959–1977, p. 349). Newton's objections were received soon afterwards giving Flamsteed pause to think more deeply. Responding with his fourth letter of March 7, also sent by way of James Crompton, Flamsteed argued the similarity of apparent motion in longitude before and after passing their perigees (or points of swiftest apparent motion) proved their identity and Flamsteed reiterated the contention that the sun's magnetism attracted the comet during its ingress and repulsed it during the return. Flamsteed also faulted Newton for the mistake in Gallet's data and sent a correct copy. Although Flamsteed praised Gallet's general skills, he downgraded these observations except for the last one which agreed well enough with Cellio. Given fresh support from Hill and Cellio, Flamsteed asked how the comet could have started off north of the ecliptic dipped southward and then maintain nearly constant latitude without supposing attraction by the sun (Newton 1959–1977, 2, pp. 348–350). Flamsteed also entered a plea for help in deciding between a turn around the sun and a turn short of the sun (see Sect. 16).

In further diversions from his work, Newton (1959–1977, 2, pp. 358–359) acknowledged the two comets were less irreconcilable (largely because the trajectory of the November comet was not as erratic as indicated by the bungled set of Gallet's observations) but numerous flaws in Flamsteed's argument from the similarity of apparent motion precluded definitive agreement. Flamsteed failed to allow for changes in latitude and the accidents of actual distance. As for Flamsteed's claim the nearly constant latitude in Cellio's observations proved such attraction, Newton countered there was too much scatter in the various sets to be conclusive. Ignoring Flamsteed's discounting of Gallet's observations, Newton included them with Hill's observation and the superior observations of Cellio and argued none could be trusted to accuracy greater than half a degree or even one degree. Given this uncertainty Newton asked why the comet could not have retained a course continuously decreasing from about 2° N to something greater than 1° S before being lost in dawn's early light. This last latitude was considerably smaller than previously imagined but Newton was not about to concede solar attraction of the November comet any more than in the case of the comet that appeared in December.

A dubious claim in Flamsteed's theory (not found in his letter to Halley) added more grist for Newton's two comet solution. Flamsteed argued comets usually verge from south to north. Newton found from a search of comet histories, now probably including Hevelius (1666) and more recent sources, comparable numbers of comet moving southward as northward (Newton 1959–1977, 2, p. 345). Thus a southward moving comet in November was in good company. An interesting side note concerns Newton's listing of the otherwise unknown comet of 1666 seen by Robert Knox while in captivity in Ceylon. According to Knox (1681, p. 60), "In the year 1666 in the month of February there appeared in this Countrey another comet or stream in the West, the head under the Horizon, much resembling that which was seen in England in the year 1680 in December." The account was not yet published but reached Newton by the philosophical grapevine.[45]

An important new item in Flamsteed's March 7 letter was a terse account of Halley's observation of a comet's tail in the morning of December 8 while en route to Paris—it was sticking straight up from the horizon with the head still hidden. The circumstances indicated the comet was nearing conjunction with the sun and had already crossed the ecliptic. Newton, as well as Halley, identified this appearance with the comet seen in the evening later in December moving on a northward track. It could not have been the last appearance of the November comet which, despite Flamsteed's entreaties, Newton believed to have continued beyond the sun on a nearly straight southward track.

10 Newton presses his research agenda

Newton's initial response of February 28 went beyond a critique of Flamsteed's theory with a request for clarification or amplification of a number of points in Flamsteed's observations. When the head was first observed December 12, was it by naked eye or only through glasses and with what magnitude? Perhaps recognizing the poor fit of the observation in his rectilinear solution, Newton asked whether refraction was allowed for in computing its place. More generally, Flamsteed had described the appearance of the head as having scattered points of bright light which Flamsteed attributed to the reflection of solar rays from the solid parts of a broken globe that poked above a poorly reflecting fluid surface. Newton agreed that "ye atmosphere about ye head

[45] Knox returned to England in September 1680. Wren recommended the publication of Knox's memoir. Hooke's preface was dated August 1, 1681. When the December comet appeared Knox must have told friends of resemblances between it and what he had seen in 1666 and a report somehow reached Newton. Comets were a hot philosophical topic.

shines ... by the sun's light" but added the caveat "though not altogether by it," as if there were an internal source of light (Newton 1959–1977, 2, p. 346). The basis for the caveat evidently was Newton's observation for January 4 that while there was no bright 'star' in the center, the head was brighter in the center and gradually faded toward the circumference. Of course, the fading could have been due to a decrease in the density of reflecting matter. Hevelius (1668, Fig. F, p. 882, *passim*), now clearly on Newton's reading list, included reports by Hevelius and Cysatus of "great & perpetual variations" in the head. Deferring to Flamsteed's greater authority, Newton sought addition details that would clarify the role of the sun in illuminating the head (Newton 1959–1977, 2, p. 346).

Newton also asked for details of the placement of the tail in relation to nearby stars on dates when he had not minded it, namely December 10, 11, 12, 15, and 21. Newton did not ask about details on December 29 or later because he had his own observations. In a continued spirit of cooperation, Newton offered preliminary information on his observations of February 25 and 27 in the hope that Flamsteed would resume telescopic observations that ceased on February 5. As always, Newton wanted data rather than the ideas of others. To help Flamsteed find the comet again, Newton included a rough map locating recent positions in relation to key stars. Newton also offered Flamsteed the opportunity to request his recent observations and those he would make after one or two more clear days.[46] Chiefly, however, Newton wanted descriptions of the tail in December in order to develop a thought that had come to mind that very day (Newton 1959–1977, 2, p. 346). Newton had already determined how to use the apparent length of a tail to fix the maximum distance of a comet. This new thought must have been the germ of Newton's ill-fated law of tails.

11 Toward a harmonious law of tails

Flamsteed's fourth letter of March 7 sent by way of Crompton included most of the detailed descriptions of the tail requested by Newton along with a plea by Flamsteed to help resolve a dilemma between a turn short of the sun and a path around the sun. Because he had been absent from Cambridge from March 15 to 26 Newton did not receive the letter until his return (Newton 1959–1977, 2, pp. 363, 367 n. 1, 2). Newton quickly put Flamsteed's descriptions of the tail to work. Enhanced versions were included in what I call the "crazy quilt" document under the title, *"Ex Astrolabio Flamstedij"* (CUL MS Add. 3965. 14, f. 616v)[47] Newton evidently had taken details from Flamsteed's letter, plotted them on a celestial globe or star chart and determined more precise coordinates of the termini and intervening positions by following great circle arcs or other smooth curves. For example, Flamsteed indicated, "[December] 12. after it became darke it passed over ye middle of the Sagitta, but extended not far beyond it" (Newton 1959–1977, 2, p. 352). According to Newton's subsequent elaboration (CUL MS Add. 3965.14, f. 616v), "The tail ended with a sharply defined terminus at

[46] It is doubtful Newton understood at this point the effort required to get these observations into usable form.

[47] This material is inserted from the opposite end of the map and table of unknown stars cited in note 38. What Newton meant by Flamsteed's astrolabe is unclear to me.

the two small stars in the middle of Sagitta. The axis of the tail was equidistant from the two small stars in the foot of Antinous, from the left elbow of Antinous from the bend of the left wing of Corvus [slip for Aquila]. And that distance was one third of the distance between the elbow of Antinous and the star at the end of the end of the Serpent's tail."[48] What seems to be the earliest compilation of these results was in the next group of entries (CUL MS Add. 3965.14, f. 616v). The terminus for December 12 was listed as ♒ 6 lat 43. Only a few other intended dates were filled in, but Newton continued to work on the problem. He also began to compile the information received from Flamsteed and other sources in his Waste Book, adding further details based on his own analysis.

Newton used the enhanced data to develop what he viewed as a harmonious law of tails. Newton's basic premise was that the apparent ends of the tail fall very nearly along a great circle, perhaps by analogy with the apparent track of the head. He imagined that if the complex procedures outlined in CUL MS Add. 4004, f. 101r were implemented, the locus of the terminal positions "must be in either a geometric or arithmetic progression or some other regular function no matter what."[49] In practice, Newton evidently found the best great circle fit for terminal positions plotted on a celestial globe, noting (CUL MS Add. 4004, f. 101r), "if a great circle which cut the ecliptic in ♐ 20° at an angle of 54° is drawn on a globe it passes by the star in the northern wing of Sagittae [α Sag], then by the 4th magnitude star in the eastern arm of Cassiopeia [θ Cas], then by the star in the back of Perseus [ι Per] or by a point about 1/4 degree further south."[50] These great circle positions outline very nearly the observed end of the tail on December 11, December 26, and January 4. A few lines later, the position of the node was listed as ♐ 20° 1/3 at an angle of 54°. According to the Waste Book, the ascending node of node of great circle envelope of the tail termini cut the ecliptic in ♐ 20° 1/3 at an angle of 54°. This position was changed in the "crazy quilt" document to ♐ 19° 1/2 at an angle of 52° or 52° 1/2 and again in

48 Cauda desinebat accurate ad exiguas duas stellas in medio Sagittae. A stellis duabus in pede Antinoi, a çubito sinistrae Antonoi ab ancone alae sinistrae axis caudae equidistabat et distantia illa erat tertia pars distantiae cubiti illius Antonoi a stella in termino caudae Serpentis. This description presents many problems of interpretation that may depend on the particular pictorial guide used by Newton. The terminus at the stars in the middle stars of Sagitta may be a misstatement for two small stars above the middle of Sagittae since Flamsteed indicated that the tail extended a little beyond the middle. Moreover, Newton's coordinates are roughly consistent with certain largely un-cataloged "unformed" stars above the middle of Sagitta, particularly as depicted on Bayer's map, or as he might have identified himself. After further revisions, Newton indicated the end of the tail although not well defined extended above the middle of Sagittae to at least ♒ 4° with north latitude of about 42 1/2 or 43 (CUL MS Add. 4004, f. 99r). Few constellations showed more differences of artistic depiction than the intertwined combination of Aquila and Antinous. The brightest star in the foot of Antinous (λ Aql) was surrounded by several smaller stars which were portrayed variously in the foot or just outside. Tycho catalogued one star in the foot and one just outside. Bayer presents a better option with two small stars just above λ Aql in the foot. The overlapping nature of the two constellations in Bayer's depiction, however, would lead to both the knee of Antinous and the bend in the wing of Aquila being identified as δ Aql. Newton's only unambiguous designation is the star in the end of the serpent's tail (θ Ser).

49 ... debent esse in progression seu gemet[ri]ca seu arithmetrica aut alia quavis regulari.

50 Si in globo ducatur circulus maximus qui secet ecliplipiticam [sic] in ♐ 20 gr in angulo 54 gr transiens per stellam α in ala septentionali Sagittae, dein per stellam θ quartae magnitudinis in orientali brachio Cassiopeiae, denique per stellam ι in tergo Persei aut punctum 1/4 gradus australis.

Table 1 Relation of tail termini to a great circle arc

	Observations			Calculations of latitude		
	Longitude	Elongation	Latitude	@ 52°	@ 52° 1/2	@ 53°
Terminal envelope cut ecliptic in ♐ 19° 1/2						
Dec 10	♑ 19° 1/2	30°	34° 1/2	32°37′	33°5′	33°34′
Dec 11	♑ 26°43′	37°13′	38°34′	37°45′	38°15′	38°45′
Dec 12	♒ 4°	44°30′	42° 1/2 or 43°	41°45′	42°25′	42°56′
Dec 18	♓ 24°	94°30′	52°20′	51°55′	52°25′	52°55′
Dec 21	♈ 23°53′	123°23′	47° 1/2	46°54′	47°25′	47°56′
Dec 24	♉ 4°	134° 1/2	43°2/3	42°24′	42°54′	43°26′
	or ♉ 5°	135° 1/2	43°2/3	41°54′	42°25′	42°56′
Jan 3	♉ 22°27′	153°	30°50′	30°10′	30°37′	31°4′
Jan 4	♉ 23°40′	154°10′	30°36′ 1/2	29°9′	29°36′	30°2′

the draft of April 12 to ♐ 19° 1/2 at an inclination of 53°. The actual letter of April 16 finally put the inclination at 52° after initially writing 53°.

The expected latitudes of the tail termini follow simply from a basic relation in a spherical right triangle: tangent of the latitude = sine of elongation from the node x tangent of the inclination of the great circle at the node. Table 1 shows results for calculations at these various inclinations.

The results most nearly fit a great circle if it cut the ecliptic in ♐ 19° 1/2 at an angle of about 53° or a little less. The final choice of an inclination of 52° may have been based on recognition that the actual envelope traced was not an exact great circle. Despite these variations, the Waste Book data for tail termini with only a couple of minor changes were listed in the *Principia* through all of its editions (1687, p. 497; 1713, pp. 455–456; 1999, pp. 917–918). A curious defect was the longitude for December 21 listed as ♓ 26° in the preliminary compilation, ♓ 23°53′ in the Waste Book, and ♓ 24° in the first and second edition of the *Principia.* It was finally corrected to ♈ 24° in the third edition (Newton 1972, p. 735). Newton's failure to include any information about a prospective great circle envelope in the *Principia* and the inattention to the faulty zodiac sign indicate a loss of interest in a harmonic law of tails.

12 Evaluating the observations

Flamsteed's fourth letter of March 7 included a revised set of his Greenwich observations. The two most important changes from the set sent in February were new coordinates for the observation for December 12 that Newton had questioned and differences for January 10 that may have resulted from a copying error. In a characteristic move, Newton calculated the distances between observations finding several discrepancies and ended by transcribing Flamsteed's set of observations sent February 12 in the Waste Book in preference to the March revisions.[51] Newton's response in

[51] CUL Add MS 4004, f. 98v. Newton copied a few remaining details from Flamsteed's letter of February 12 onto a blank sheet of Flamsteed's March 7 letter. The full letter vanished as having nothing else of interest to him.

April (Newton 1959–1977, 2, p. 365) indicated Flamsteed's January 10 longitude of ♈ 20°49′ 1/2 in the "former" copy agreed better with the other observations than the longitude of ♈ 20°42′ in the copy last sent in March. It is not clear what basis Newton had for the determination. The technique of Problem 16, whether interpolated from positions on January 5, 9, and 13, or extrapolated from positions on December 30, January 5 and 9, actually confirm a position on January 10 of about ♈ 20°41′ or 20°42′ consistent with the March revisions. It is noteworthy that Newton subsequently altered the Waste Book entry from ♈ 20°49′ 1/2 to 20°41′ 1/2 with a heavy numeral 1 nearly obliterating the 9.

Taking Greenwich data adjusted to the same time as the standard, Newton also determined the corrections needed in the Parisian set received in February. Newton's results were added to the corresponding table in the Waste Book and cited in his draft of April 12 and letter of April 16. The greatest differences were January 8 (7′ or 8′) and January 13 (12′). Newton's calculations are lost or unidentified, but the method of Problem 16 yields results that agree quite well with Newton's results. He offered his observation of January 11 as a possible aid in resolving the differences. The same interpolation technique yields an hourly motion on January 11 of 4.4′ in substantial agreement with Newton's silently adopted 9′ parameter that represented 2 h of motion. It is not clear how 9 p.m. arose over against 9:20 for the time the comet was supposedly observed to have the same altitude as sigma Pisces. Even though he juggled the results Newton claimed his measurements were accurate within a minute or two. All that would be needed for the 11th were accurate coordinates of sigma Pisces when the comet was quite close to the star. The failure to offer details of his own observations of January 6, 8, 9, and 10 indicate Newton believed they contained errors greater than several arc-minutes. Newton specifically mentioned the limitations of his early instruments. There was no hint of happier times when Newton used Tycho's observations to obtain reductions used in several applications. Manifestly, Newton had become disillusioned with his (pre micrometer) observations and available star catalogs.

Newton envisioned better coordinates for sigma Pisces would be available in a month or two. Was it an expectation based on reduction of newly available raw data in Hevelius (1679)? If not, who was in the position to make a better observation? Newton questioned the necessity for Flamsteed to make the determination just to resolve the slight difference with the Parisian observations. No matter, if my sequencing is right, nothing more was heard of sigma Pisces or Newton's observations of December and January. As noted, Newton claimed he had not reduced the micrometer observations because he did not have the coordinates for the reference stars but he sought no help from Flamsteed. Indeed any such items or requests that might have served to prolong the correspondence were excluded from Newton's final letter.

13 Newton's revised trajectory

Twice in unsent drafts Newton alluded to a direct method to determine a comet's line of motion whatever it might be, adding in the second draft that the line could be determined almost as exactly as a planet's orbit, provided very accurate observations were

Table 2 Newton's heliocentric parameters for the revised trajectory

Date	Longitude	Latitude	Distance from sun
Dec 3 (4th at latest)	♐ 20°	0°	Scarce 1/2 a.u.
Dec 12 or 13 (perihelion)			About 1/5 a.u.
About Jan 2	♊ 9°	19° or 20° N	1 a.u.
Mar 9, 8:30 pm	♊ 13°	12° N	About 4 a.u.

available (Newton 1959–1977, 2, pp. 362, 366). Newton had devised the micrometer for his telescope around February 10 for that very purpose. At that time Newton was relying on the rectilinear method and had complete observations only up to January 24. He had limited information of the November comet gleaned from observers at Cambridge but had not yet received any November observations from the continent, at least from Flamsteed, or any later data that called for a revision of the initial rectilinear path. Newton's crude observations at the end of January and early February might have indicated some reorientation of the original trajectory would be required but Newton would have had no reason to suspect that radical changes would be needed. Thus, like Hooke (and Huygens), the method Newton had in mind in early February must have started with a rectilinear approximation to be fitted to further observations.[52]

Newton's original rectilinear solution was based on observations up to January 11. Given the considerable amount of new data received by March 1680/1681 he set out to reevaluate the solution. Newton claimed that he had determined a new trajectory without any calculation, that is to say, without application of techniques in Problem 52, but only by a general survey of the Greenwich and Paris observations (Newton 1959–1977, 2, pp. 362, 366–367). The heliocentric parameters for Newton's revised trajectory are shown in Table 2.

Newton obviously also surveyed his own observations. A plot on a globe or map (adjusted for precession) would have yielded the approximate coordinates. Worked in reverse the heliocentric position on March 9 taken as 4 a.u. from the sun in the plane of the ecliptic indicates Newton was using an observation, rounded to the nearest half degree, of about ♊ 0°30′, 11°30′N with an elongation from the sun of about 60°30′.[53] Perhaps, Newton found the original rectilinear path projected to March 9 would have yielded an elongation of only about 56°. One way or another Newton realized he had to modify that original solution. Newton could have chosen to change the direction or allow variation in speed. He decided to keep the speed constant and chart a path in a different direction.

The Paris observations in hand covered the period from December 19 to January 13 and served only as a crosscheck of other data (see above). The Greenwich observations extended to February 5 but for purposes of remapping the trajectory the only significant

[52] Halley indicated in a letter to Flamsteed from Paris dated January 22, 1680/1681 that the nearest hypothesis for the second comet was that it moved nearly in a line that crossed the ecliptic a great distance beyond the sun at an angle of about 13° and passed over the sun in the direction of 25° ♊ Halley found the line would have been straight only if the speed was allowed to change (Newton 1959–1977, 2, p. 339).

[53] In 1685 the coordinates for March 9 proved to be ♊ 0°43′2″, 11°44′ 3/5 N.

change was Flamsteed's revised position for December 12 which fell at its expected position on the original rectilinear solution. The revision was important since it added support to Newton's belief that the comet was beyond the sun on that date because the tail of the comet was narrow, becoming wider on later dates.[54] He had taken this narrowness as evidence the tail was pointing away from us, which given their anti-solar nature placed the comet beyond the sun. Newton took the subsequent broadening of the tail as evidence the comet was closer than the sun with the tail pointing toward us. By considering Newton's parameters for perihelion it appears that he took Flamsteed's revised position, or one very close to it, to establish heliocentric coordinates on December 12 about ♓ 4°, 0.183 a.u. from the sun and 1.049 a.u. from the earth.

The details of Newton's procedure are lost. The following analysis replicates Newton's numbers in Table 2 but is conjectural. The parameters can vary by almost one degree without changing the results viewed as round numbers. The heliocentric position of the comet on December 12 as confirmed by Flamsteed's revised observation established one position. The comet's place on March 9 based on Newton's observation established the elongation from the sun. Maintaining the original rectilinear speed yields a distance in the plane of the ecliptic of about 4.1 a.u. between December 12 and March 9 (87.156 days at 0.0471 a.u./day). Triangulation establishes the heliocentric coordinates on March 9 at ♊ 13°¼, 12° ½ N, 4.2 a.u. from the sun. The trajectory crosses the sphere of the earth's mean path on January 2 around 2 a.m. at about ♊ 9°, 19° ½N. The result, however, departs sufficiently from the original rectilinear solution so that the path no longer satisfies the requirement of uniform speed for the actual observations in December and January. Somewhat better results for these intermediate positions might have been attained if Newton had drawn a slightly curved path that crossed the earth's orbit at a heliocentric longitude of about ♓ 5°, although that position would have introduced other problems. Newton was prepared to introduce slight curvature to account for earlier observations.

The revised trajectory between December 12 and March 9 projected back to the ecliptic placed the comet in the evening sky on November 24 with an ascending node about 0.89 a.u. from the sun at a geocentric longitude of about ♐ 19°, while according to Halley's observation the comet was in the morning sky as late as December 8. Using what he called the analogy of its motion with the following observations, that is to say maintaining the same speed, Newton satisfied this requirement by extending the path along a line slightly concave to the sun (Newton 1959–1977, 2, pp. 362, 367). This revised solution, made according to Newton without calculation, placed the heliocentric position of the ascending node in ♐ 20° on December 3, or December 4 at the latest, at a distance slightly less than half as far beyond the sun as the sun is from the earth.[55]

[54] Newton's notes from various observers indicated: December 10, a small tail (Newton 1959–1977, 2, p. 315); December 11, the tail was broader than the moon (*ibid.*, 2, 352); December 15, the tail was broader than the moon (*ibid.*, 2, p. 315); Dec 16, 2° wide (CUL MS 4004, f. 99r); December 17, the tail was broader than 2° (*ibid.*); December 21, 2° wide (*ibid.*); December 23, the tail was wider than before (*ibid.*); December 28, the tail was wider than before (*ibid.*, f. 99v); December 30 the tail was 1? 36′ wide near the head widening to 4° at the end (*ibid.*); on January 4 the tail was 1° 15′ wide near the head widening to 3° ½ at the end (ibid., f. 100r). Newton's Waste Book entry for December 12 does not include width.

[55] Newton (1959–1977, 2, pp. 362, 367) originally wrote 3/5 and changed it to 1/5, an obvious slip for ½.

Newton did not explain how he reached this conclusion without calculation but several simple considerations would have sufficed. A separate entry related to his work on the law of tails, discussed above, indicated the apparent path of the trajectory cut the ecliptic in ♐ 21° (CUL MS Add. 4004, f. 101v). Given his use of a celestial globe Newton might have positioned Flamsteed's observations (adjusted for precession) for December 12 and 21 (those closest to the ecliptic), rotated the globe to a fixed great circle ring and noted the position at the ecliptic.[56] According to Halley's observation in the morning of December 8 the comet was not yet in conjunction with the sun but from the position of the tail it was already north of the ecliptic. Given ♐ 21° as Newton's estimate of the geocentric longitude of the node, the comet would already have been in conjunction with the sun December 2 at about 2 a.m., contrary to Halley's observation. Any time between December 2 and December 8 would have satisfied the general requirement under conditions of increasing curvature concave to the sun. If the time the comet crossed the ecliptic at an apparent longitude of ♐ 21° was arbitrarily set about one day later on December 3, 0:01 a.m. the least possible curvature would have been introduced with the comet at a heliocentric longitude of about ♐ 19° 2/3, 0.47 a.u. from the sun in agreement with Newton's stated values taken as round numbers. This approach would explain Newton's willingness to concede a date of December 4, but no later. After that date the curvature would have become totally unacceptable to Newton (see Fig. 2). The comet's position on December 3 marks the place where the comet would have crossed the ecliptic assuming its apparent position was about ♐ 21°. One day earlier in the region marked X would have placed the comet in conjunction with the sun and later dates would have increased the curvature substantially. The difficulty is the projected place of the comet on December 8 in the morning falls on a position east of the sun where it would have been visible only in the evening. Not only does Newton's solution beyond perihelion fail to account for observations in late December and January, but the portion before perihelion has insufficient curvature to account for Halley's observation.

Newton must not have inspected the proposed results very carefully, certainly not with the detail to which he subjected Flamsteed's scheme. Newton's approach was at best a highly simplified approach to the method he had in mind for accurately defining a path of motion. Obviously pressed for time and eager to cut off correspondence with Flamsteed and get on with other work, Newton used various expedients using only the first and last available observed positions and a nodal position selected to minimize the curvature. While he had a very good fix on the comet's last positions, he lacked their precise reduction and only sketchy information about the comet in early December. Recalculation using the techniques of Problem 52 would have been wasted effort which would have been subject to further modification as better information became available. As it was, Newton's solution for the curved segment before December 12 raises serious questions concerning his assertions about the true length of the tail.

[56] Calculations using available observations for December 12 and 21 available to Newton would have placed the node in ♐ 21°43′. Flamsteed's calculations using slightly different data placed the node in ♐ 21°46′2/3. Flamsteed found the values varied with different pairs of observations (Forbes 1975, pp. 28, 109).

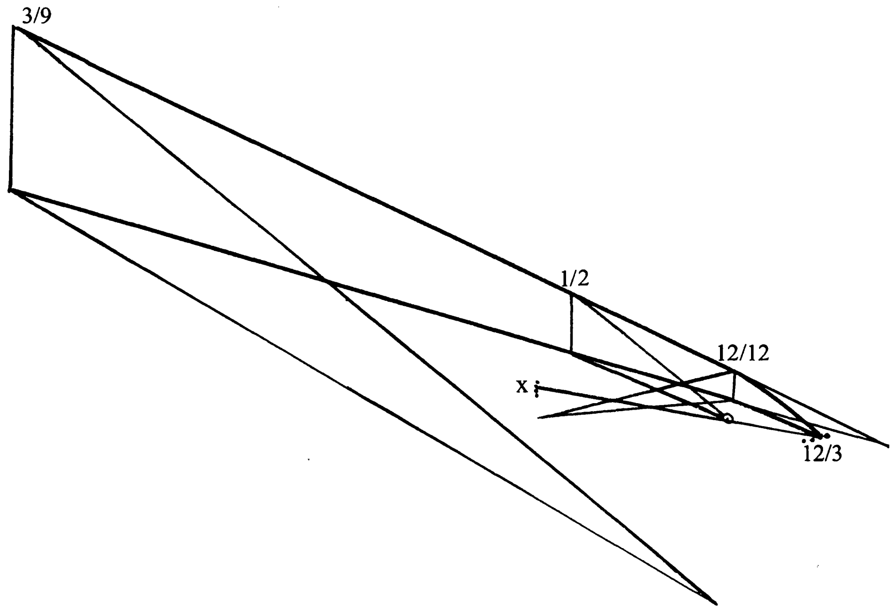

Fig. 2 Comet of 1680 reconstruction of Newton's revised path (April 1681)

14 A closer look at the length of the tail

Flamsteed had been reluctant to allow the comet to pass around the sun not only because it seemed to require excessive speed but perhaps more importantly that passage coupled with the apparent length of the tail pointing away from the sun entailed an enormous actual length, particularly on December 8 and 10. Newton's response was true in a narrow sense but more broadly disingenuous. In the undated draft, although wishing he knew the apparent length when Halley observed it on 8 December, Newton found that from 10 December onward the tail reached well beyond the sphere of Mars (Newton 1959–1977, 2, p. 361). In the actual letter, Newton asserted the tail extended beyond the sphere of Mars during the entire appearance of the comet and more particularly that the apparent length of the tail during December was not enormous but in agreement with the law it observed all that month (Newton 1959–1977, 2, p. 363).

The true length would have been estimated by triangulation of the geocentric and heliocentric coordinates of the revised trajectory under the assumption the sun, head and tail of the comet lay in a straight line. Here the problem arises. The speed and heliocentric position of the comet on December 12 used to map the path in accordance with Newton's description put the positions on December 10 and 11 in unfavorable positions leading, as Flamsteed's feared, to unlikely tail lengths (see Table 3). The second column represents the distance from the sun to the end of the tail, the third column allows for the approximate distance of the head of the comet from the sun. Any substantial increase in curvature of the path has the length of the tail on December 10 approaching infinity. The termini certainly extend beyond the sphere of Mars but contradicted Newton's view the tail was formed by the force of solar light. The true

Table 3 Approximate tail lengths

Date	End of tail	True Length
Dec. 10	5.4 au	5.2 au
Dec. 11	2.8	2.6
Dec. 12	2.0	1.8
Dec. 18	1.5	1.1
Dec. 21[a]	1.8	1.3
Dec. 24	2.3	1.6
Jan. 3	3.2	2.1

[a] This result uses the incorrect longitude sign. See above. If the correct sign as indicated in the third edition were used the resulting distance would be 1.9 a.u. and the length 1.4 a.u.

length should not decrease as the comet approached the sun. Perhaps another analyst can find a better fit of the pieces, I have not.

A different approach by Newton would have largely vindicated Flamsteed's conclusions. The raw observations of the tail were sketchy and subject to vagaries of sky condition and obscuration by the Milky Way. Newton assumed that accurate determinations of the apparent tail lengths follow some regular harmonic pattern represented by a great circle. The next step might logically have assumed the true length also follows some regular pattern of growth and accordingly Newton could have found heliocentric positions that yielded more reasonable lengths for December 12 and before. Apart from the requirement for the path to be nearly rectilinear, the main reason to place December 12 beyond the sun was the apparent narrowness of the tail which suggested but did not prove the tail was pointing away from the earth. Positions for December 10, 11, and 12 close to what Flamsteed proposed would have yielded tails about 0.8 a.u. long. Newton's casual and seemingly disingenuous handling of the data calling for a revised path and the associated treatment of the true tail lengths was in sharp contrast with the detailed effort Newton devoted to evaluating consistency between observations.

15 Hooke's *Cometa* and the comet of 1664

The Waste Book has a section at folios 103r-104v entitled *Ex Hookii Cometa edito ann 1678* (From Hooke's *Cometa* published in 1678) but which in fact abruptly change to include notes from Hevelius (1665, 1666). Newton copied details of Hooke's two observations of the comet of 1677 (pp. 1–5) noting its position, the orientation and extent of the tail, and apparent sizes of the nucleus and surrounding coma. Newton skipped Hooke's discussion of theoretical issues such as the duration of comets, their motion, and the growth of their tails and the issue of observation quality. Newton would have agreed with Hooke on the need for accurate observations, on the use of Wren's method of defining a rectilinear path to determine parallax, and found in Hooke a precedent for allowing comets to follow a concave path about the sun with greater or lesser deflection depending on the speed and variable outside influences, but Newton made no notes or comments. Instead Newton picked up Hooke's discussion (pp. 21–22) of the discrepancies found between the observations of top astronomers such Hevelius, Gottiginies, and Petit. A major debate, which Newton followed at the time through reports in the *Philosophical Transactions* (ULC Add 3958(B)1, ff. 9r-12r), had occurred over the final observations of the comet of 1664 which was decided against Hevelius. At some point Newton added a marginal note on those details from the account in Hevelius (1665, p. 2 and Fig. C). The main text continued with Newton's terse summary of Hooke's argument (pp. 45–46) that the head must provide some light of its own because the central part of the tail closest to the head was brighter than the outer parts more accessible to solar rays. The issue was precisely what concerned Newton in his letter of 28 February. Since Newton's *Cometa* notes followed sections that could not have been written before late March, the question arises whether Newton was highlighting Hooke's idea as confirmation or, having already read *Cometa,* was just now making or copying more systematic notes. Whatever the case, Newton's notes abruptly shifted to a meticulous copy of Hooke's Figure 4 depicting the comet's apparent trajectory according to various observers. An adjacent table of Tycho's star coordinates updated to 1664 is from Hevelius (1666, p. 128). The text continued with detailed comparisons between Hooke's observations of the comet of 1664 and those of Hevelius, Petit, and other observers listed in Hevelius (1666, pp. 133, 153, 155). A variant version of these details is on the "crazy quilt" document (3965.14, f. 616r).

Newton continued with a table of 1664 observations Hevelius (1665, Table B, pp. 97–104), annotated by Newton as to relative quality, with a subset selected for determining a rectilinear trajectory. Newton's "*Cometa* notes" concluded with further details extracted from Hevelius (1665, pp. 2, 8–11, 16).

The select observations were put to work in two unfinished error-ridden documents (CUL MS d. 3965.11, ff. 154r-155v) using the methods of Problem 52 and Wing's solar theory.[57] Newton's first calculations were made on the verso of a letter draft to a Mr. Todd with some latter calculations made on the margins and more or less empty spaces of the draft.[58] It is most plausible the calculations date from March or April 1680/1681, although a date in early winter 1684/5 may be possible shortly before Newton adopted Flamsteed's solar theory (CUL MS Add. 3965.5, f. 21; Ruffner 2012, p. 248, n. 19).

16 On fetching a curve

Newton argued in his letter of February 28 *if* only one comet had appeared it would have been continually attracted to the sun and made to fetch a curve around it. Flamsteed's

57 Newton indexed the initial page of Wing's solar tables which were based on London time on the flyleaf of his copy of Wing (1669) preserved in the Trinity College Library NQ. 18.36. He converted the times of observation to London time using an erroneous time difference between Gedani (Gedansk) and London of 36′. Wing (1669, tables, p. 69) listed the difference between Dantzig (using the alternative name) and London as 1 h 14′ or 1 h 16′. *Philosophical Transactions* #129 (1676, p. 724) lists the difference between Dantzig and Greenwich as 1 h 14′ 45″, also found in an annotation on a flyleaf of Newton's copy of Mercator (1676), Trinity NQ. 10.152. Perhaps Newton confused Gedani with another place but I have not found one with a time difference of 36′.

58 Newton 1959–1977, 2, p. 373. See Newton (1967–1981), 6, p. 329. The Todd letter concerns money owed to Newton's half sister Mary Pilkington from their mother's estate and possibly dates from about the middle of 1680 with the blank side retrieved later for the calculations.

response of March 7 reiterated his attraction/repulsion model. Flamsteed admitted that a passage around the sun would facilitate the inclination and bending of the comet's lines of ingress and egress but required too great an increase in speed. Flamsteed enclosed a carefully delineated path reduced to the ecliptic drawn to scale. Alternative turns both short of the sun and beyond were included. Yet Flamsteed indicated he still had problems (Newton 1959–1977, pp. 351–352). Since tails point away from the sun, the apparent lengths observed on December 8 and 10 required such great actual lengths as to be unlikely. Flamsteed asked Newton for assistance, indicating he not yet resolved the difficulties. Before Newton could respond, however, in a letter to Townely dated 22 March 1681 Flamsteed adopted a turn around the sun as if there had never been any doubt (Flamsteed 1995–2002, 1, pp. 780–784). Had Newton's initial arguments finally convinced Flamsteed?

Newton did not know Flamsteed changed his mind and tried to formulate suggestions that would correct Flamsteed's errors and resolve the dilemma. Newton's labored response to Flamsteed required an undated draft, a second draft of April 12, and the letter dated April 16, 1681. Newton's responses to the issue of the true length of the tail have already been discussed. Additionally, the undated draft offered a careful critique of the flaws in Flamsteed's reasoning when analyzing the apparent motions of November and December. Newton augmented his objections to Flamsteed's notion that the body of the comet was magnetized and that its line of polarization remained fixed in space. Newton also expanded the difficulties raised earlier for the notion that the sun's magnet first attracts and then repulses the body whether it is swept around it or not. Then in a famous passage Newton suggested Flamsteed could avoid all these difficulties by supposing the sun's alleged magnetism continually attracted the comet and "made to fetch a compass about the sun" where the *vis centrifuga* at perihelion would overpower the attraction and force the comet to begin to recede from the sun (Newton 1959–1977, 2, p. 361). The proposed mechanism was quite different from that developed by Newton in 1684 and was based on an idea of Giovanni-Alfonso Borelli which looking backward had figured in Newton's recent dispute with Hooke (Whiteside 1970, p. 13). For his part, however, Newton went on in the next two concluding paragraphs to offer parameters about the extent of the tail and the comet's path based on the assumption of uniform motion in a line that was very nearly straight, not one that had fetched a path about the sun.

Various commentators ignoring the final paragraphs suggest the statement on fetching a curve reflected Newton's actual thoughts at the moment. They variously imagine Newton was reconsidering magnetism as the motive force, entertaining the possibility the two comets were one and the same moving in accord with planetary laws, taking a step toward his mature cosmological theory, simply toying with a parabola, or envisioning an actual orbit (Kollerstrom 1999, p. 340; Wilson 1969, p. 155; Schechner 1997, p. 136; Hughes 1988, p. 58; Christianson 1984, p. 277; Nauenberg 1994, p. 252). It is more reasonable to assume that Newton was still outlining changes Flamsteed needed to make before publication without prejudice to Newton's views that underlie the rest of the document. This proposed "fetching" was followed in the next two paragraphs (Newton 1959–1977, 2, pp. 361–362) with claims about the extent of the tail and the description of a slightly curved path in which the ascending node was very nearly one half of the earth's orbit beyond the sun where it could not have by any stretch of the imagination joined the November comet. Newton (Newton (1959–1977), 2, pp. 364–365) continued most importantly with arguments against the comet having "fetched a curve" about the sun.

The subsequent draft of April 12 included another famous argument omitted from the letter of April 16. Flamsteed had been hesitant to allow the November comet to move past the sun. Newton argued it is against the nature of the thing for the comet to turn short of him. In a curious but revealing twist Newton then noted that his telescope was fitted with a micrometer because he thought he had a method to determine the comet's line of motion, whatever it might be, almost as accurately as the orbits of planets, but needed very exact observations. The idea had germinated in early February after having resolved the initial rectilinear path based on Wren's hypothesis. In space free of all forces such a path would result. In Newton's vortex filled world such a hypothesis if not strictly true would place appropriate limits on the motion of a comet which could be corrected after the fact by accurate observations comparable to those available for planets. In which case Newton was arguing not only was it against the nature of a comet to turn short of the sun but it was also against their nature to make any sharp turn at all and instead that they continue nearly in a straight line unless very strong forces were invoked. It is highly unlikely, as has been conjectured, that this juxtaposition of paths in ingress and egress with an untried method indicate Newton was still assuming the appearances were one and the same and the comet had indeed fetched a path about the sun (Wilson 1969, p. 154; Wilson 1989, p. 149; Hughes 1988, p. 58). Newton more likely believed that the November passage would prove to be slightly curved as it continued southward past the sun into the depths of space just as with his preliminary work on the "posterior" comet had shown it emerging northward from those depths. On the issue of determining the line *whatever it might be*, rather than assuming Newton was thinking of any possible bent line such as a parabola, it could easily be the case he was thinking of a uniform line of motion with any possible speed and direction consistent with the title of Problem 52. The need to add slight curvature to such a solution did not change Newton's expectation. There was no need for Newton to suddenly imagine attraction by the sun because flow across the solar vortex could readily account for, if not exactly predict, any slight deflection. Such a view was consistent with ideas expressed by both Hooke and Hevelius.

In the undated draft, Newton was prepared to offer a plausible mechanism under which a comet could "fetch a curve around the sun" to aid a confrere in offering a contrary view. Such suggestions however would have invited further correspondence and were dropped in the April 12 draft and the final letter where Newton developed empirical reasons why the comet did not make a sharp turn either short of or beyond the comet. Newton's argument hung on the credibility of the time and date for the ascending node discussed above but for which he offered no evidence. If that were doubted, Newton's clinching argument asserted a single comet hypothesis is paradoxical because it would have gone in a bent curve such as had never before been observed (Newton 1959–1977, 2, p. 364). Newton argued comets seen to move across the entire sky according to historic reports could not have done so unless their actual line of motion was nearly straight. Newton's prime counterexample was the comet of 1664. Thus, despite an earlier willingness to let Flamsteed publish contrary views, Newton now insisted such a turn was contrary to the phenomena.

17 An unresolved ending

Newton had a well formulated research program at the end of February and reason to believe he had developed convincing arguments against Flamsteed's approach. Many of Newton's requests for information were answered in Flamsteed's last letter of March 7 which also showed an unconvinced Flamsteed seeking further assistance. The new data enabled Newton to work on a law of tails and a revised path for the posterior comet of 1680. Working at a feverish pace, Newton also devoted considerable effort to help Flamsteed develop a more plausible theory before deciding Flamsteed was simply wrong. Since Newton did not receive the letter until nearly the end of March Newton devoted little more than 2 weeks for all this additional work including quite possibly the study of Hooke's *Cometa* and an associated attempt to determine a rectilinear path for the comet of 1664. Newton's failure was in placing too much trust in simple procedures to produce results that provided no more than rough estimates and varied according to the data selected. Newton can be credited for cogent criticism of particular details in Flamsteed's theory, if only he had done the same thing for his own work. Newton can also be credited for the realization that closer attention was needed to observations in early December, particular that by Halley of which only sketchy details were available in 1681.

Newton had been content to allow a valued colleague to argue hypothetically that the sun attracted a comet given a plausible mechanism which Newton drafted. But in the end Newton was convinced such a mechanism was not applicable to a comet. Newton's evidence to the contrary was based on the time and place the comet crossed the ecliptic in December, the length of the tail in early December, and the testimony of the historical record. The first point could have been readily refuted by Flamsteed if Newton had revealed the method, at least as I have analyzed it. Newton probably was never made aware of Flamsteed's analysis showing significant variation in the position of the node with different pairs of observation especially near the ascending node, and Newton seems to have adhered to the principle that the comet followed an apparent great circle path very nearly except near the end of visibility. Even if the node position were accepted, the date the comet crossed the ecliptic depended on allowing no more than the minimum amount of curvature. Curvature closer to Flamsteed's proposal would have established a date more consistent with Halley's observation. Moreover, Newton's analysis of the tail length in early December if fully exposed would have vindicated Flamsteed's analysis. Newton's claim about comets seen to traverse the full arc of the sky ignored the accidents of perspective when comets pass close to the earth. As for the particular claim for the comet of 1664, a rectilinear solution placing the comet outside the earth's orbit adjusted to account for observations before or after positions chosen for analysis would have introduced curvature little less than that found in planets traversing the region over the same time interval.

Newton's reluctance to proceed further in April 1681 could have been part of a newly acquired distaste for existing catalogs or simply a desire to get back to work that had been interrupted and not to get further involved with disputations as recently with Hooke. By early May 1681, Newton had results from revived work in chemistry (CUL MS Add. 3975, ff. 121–122). Although he left off his study of comets with problematic responses to Flamsteed, Newton certainly had accomplished enough with simple propositions to confute Descartes, if that had been an objective–an objective Newton would take up again in *System of the World*. For his part, Flamsteed gave no heed to Newton's critique beyond the sly adoption of a turn around the sun as if there had never been any doubt. Otherwise, views little different from those properly criticized by Newton occupied a central place in Flamsteed's lecture on 11 May 1681 at Gresham College. Moreover, as shown in his letter to Newton of 5 January 1684/1685, Flamsteed did not budge from a belief the sun was the most powerful magnet in the solar system (Newton 1959–1977, 2, p. 409).

It is evident from the discussion that Newton hewed to the principles espoused by Streete (1651) as noted above. The making and collection of observations from around the world is seen to be an equally important component of Newton's research. It is ironic that he allowed his predilection for straightness to ignore much of the observational material when he posited his final comments in April 1681 about the path of the comet and the length of the tail. It may have been the inevitable consequence of Newton's disillusionment about the accuracy of Tycho's star coordinates.

Newton had started with high hopes about his ability to determine the comet's positions amongst the stars using a single set of alignments with two or more stars.[59] He was sufficiently satisfied with these results reduced using Tycho's star catalog to combine them with observations made by Flamsteed in order to determine a rectilinear path amongst the planets. A desire to obtain more accurate positions led to the development of what he later would admit was a crude micrometer. Still needed would be a system to convert the micrometer readings to arc distances and to obtain star positions more accurate than those determined by Tycho. At the same time Newton had come to appreciate the quality of Flamsteed's observations, using them to gauge the accuracy of Cassini's observations, and urged Flamsteed to resume observations that would have overlapped Newton's micrometer aided work. Rather oddly, perhaps because he found the raw micrometer data more difficult to work with than he had imagined, Newton did not press Flamsteed for better star coordinates as he would do in 1684 and 1685. Flamsteed's observations and those based on Newton's micrometer, with at least two later revisions using more accurate star positions from Flamsteed, dominated all of Newton's subsequent work.

Newton's predilections led him to reject one of Flamsteed's key arguments by which observations made of the November comet showed it moving southward across the ecliptic and then due to solar attraction it was seen to move northward. Granting no such phenomenon, Newton rejected those observations due to scatter in favor of a crude reconstruction of casual local observations that placed the November comet on a continuous southward path uninfluenced by the sun. He did not throw them away or cross them out of the Waste Book. The November observations and those beginning in December had lives of their own, apart from any particular principles of motion. The parabolic approximation devised by Newton (1686, p. 494) and revised by Halley (Newton 1713, p. 458) were based on Flamsteed's observations. Newton's later micrometer based observations served to confirm the solution for that part of the path. Lacking better data, Newton combined various sets of November observations

[59] Contrast this approach with observations at elaborate fully staffed observatories headed by expert astronomers such as Tycho, Cassini, Flamsteed, Hevelius, and others who made repeated measurements.

received in time from around the world to determine some sort of "average" positions in order to confirm the fit of the solution for that part of the trajectory (Newton 1686, p. 496; 1713, p. 463).[60] The proposed ellipse in the third edition (Newton 1999, p. 912) excluded these "average" positions in favor of a single set of observations, discovered long after the fact, made in Saxony earlier in November by Gottfried Kirch.

No Baconian, Newton posited several important generalizations for which he found only a few scattered examples. Based on reports of 24 prominent comets observed since 1530, Newton found some four examples supporting a principle that comets follow great circle paths except near the end of visibility. The principle in turn justified extrapolations of positions close to the sun in an unobserved part of the trajectory. Newton's generalization that the tail bends back towards the parts it left behind was supported by observations of the comet of 1680 and perhaps two earlier examples. The phenomenon was readily explicable at first by the drag of the vortex, and by an entirely different mechanism later (Newton 1686, pp. 501–502; 1999, pp. 921–922). In short, Newton's selection and characterization of phenomena often depended on observations guided by theoretical concepts.

18 Aftermath

For Newton, two critical factors were missing in April 1681, the length of the tail as observed by Halley on December 8 and a reduction of his observations in February and March. The former was needed to provide a more definitive guide about the phenomena of early December, the latter to define the path toward the end of visibility. Newton must have taken the opportunity to question Halley during one of their meetings in 1684 to obtain the account of that observation found in the Waste Book along with a report of Richer's pendulum experiments recently made available. Almost as quickly Newton was back at work on the micrometer data seeking Flamsteed's help (Newton 1959–1977, 2, pp. 403–407).

Newton operated from a strong sense of universality in 1680/1681 as shown by his opposition to Flamsteed's ideas that contradicted conditions experienced at first hand on earth. That does not mean Newton had a belief in universal gravitation any more than magnetism operated universally between all types of matter. A change of attitude occurred around the end of 1683 or the beginning of 1684 that boil down in the mass of documents related to *Theologiae gentiles origines philosopicae* (*Philosophical origins of gentile theology*) to one important summary in Yahuda Newton MS 17.2, ff. 18r-19r (Schaffer 1993; Iliffe 1995). Ancient knowledge of the true system of the world became degraded through the use of mystical expressions, allegories, and false teaching. Ancient beliefs that the stars are worlds composed of the same matter as the earth and comets are a type of planet pointed to a concept of universal gravitation. At the same time Newton might have come to doubt the

[60] The process would have involved adjusting observations for time differences and hourly rates of change and comparing them among themselves. This comparison probably included averages where several values were involved with some sort of smoothing of the results.

empirical evidence that led him to drop the proposed mechanism once intended for Flamsteed's benefit. Had Newton already decided on gravity's universality and went searching for justification in antiquity or did he come to realize it as he pondered the ancient texts for other purposes? Either way, Halley's fateful encounter with him in fall 1684 met a Newton prepared to tackle one of the great unsolved mysteries. Planetary motion had already been solved, if he could only find the paper. The remaining mystery was the motion of comets. That would help explain why comets were introduced in the *de Motu* tracts with the intent of settling ongoing disputes about their paths and why immediate queries with Flamsteed in December 1684 and January 1684/5 showed equal intention to determining paths of the comets of 1664 & 1680 according to the principles planetary motion as to get to the bottom of the influence of planets upon each other (Newton 1959–1977, 2, p. 413). By 1684, comets had become fully gravitational objects and were central for developing the new cosmology. Hence Newton's triumphal two page foldout diagram when the issue was finally resolved.

The propositions added to the *Observationes & Scolia* document form an outline for a comprehensive treatment document on comets and begin to articulate the transition (Ruffner 2000). The exact placement in the sequence between *Philosophical origins*, the various *De motu* tracts, and *Liber Secundus* is open to further discussion. But they could plausibly be dated as late as the middle of 1685 as a draft of ideas that would be further developed in *Liber Secundus* as part of a popular *System of the World.* Propositions placing comets above the moon and invoking the heliocentric system of Aristarchus as restored by Copernicus flowed out of the "Origins" documents of 1683/1684 straight through to the "System" of 1685. The gravitational principles in the propositions were in general accord with those in *De motu* tracts with allowances for a more popular treatment and were far more sophisticated than the suppressed mechanism Newton had intended for Flamsteed in 1681. Placing the sun nearly in the focus suggests Newton may have been thinking of perturbations as first articulated in *de Motu* (*in fluidis*), CUL MS Add. 3965.5a, ff. 40–54, around December 1684. Perturbations may also have been in mind as he posited a proposition, for popular consumption, that the path of a comet orbit would be oval if it were periodic over against a non-periodic curve that would be nearly a hyperbola. A proposition dealing with the rate of growth of comet tails represented a dynamic approach entirely different from their behavior expressed in 1681. That behavior required that celestial material have no sensible resistance as first articulated in *de Motu* (*in fluidis*). The allusion to a vortex in the propositions was no bar to this period because an unused draft fragment of the "System" initially denied the possibility that comets travel along the edge of the vortex while a redraft later in the document indicated they do not travel inside the sphere of circumsolar force (CUL MS 3965.11, ff. 175r, 176v).

19 Conclusion

Newton was working on a detailed account of comets in Winter 1680/1681 based on rectilinear principles before becoming engulfed in correspondence with Flamsteed. The key note was a list of memorable comets and associated phenomena selected as evidence that comets were close to the sun when visible to the naked eye in contrast to Descartes and others who placed them beyond the orbit of Saturn. Newton adhered steadfastly to uniform rectilinear motion for comets subject only to slight deflection as they cut transversely across the solar vortex. His attempt to develop a law of tails turned out to be a blind alley. Newton adduced cogent comments based on uniform action between earth and the heavens but the attempt to aid Flamsteed was flawed by the uncritical use of simple calculation techniques and dubious historical evidence. Armed with an entirely different view of the physics, Newton entered on a more fruitful track in 1684 that proved to be far more difficult than he dared to imagine. Once again he fashioned the comet sections of *System of the World* with evidence against Descartes and others who ignored the plain evidence before their eyes and placed comets either far beyond the planets or caged them below the moon. The technical content of *System of the World* and its underlying manuscripts as a prelude to Newton's ultimate resolution in the *Principia* await a far more detailed and quantified study than they have received in the wider universe of Newtonian scholarship.

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Appendix: Dating the Waste Book entries

Newton's Waste Book has data entries for the comets of 1680 and 1682 (CUL Add 4004, ff. 97r-105r).[61] Incomplete entries for the micrometer readings based on an erroneous distance between two key reference stars are found at folio 97r. This entry probably dates from fall 1684 and was at issue in December 1684 when Newton sought corrections from Flamsteed. The first entries most likely were made around the end of March 1680/1681 starting at folio 98v, leaving several blank pages after the previous subject section. The November observations by Hill, Cellio, and Gallet (with correct dates) extracted from Flamsteed's letter of March 7 (received March 26) were listed first. Also listed was the scholar's observation as corrected in March. These data were followed by coordinates for the "subsequent" comet made at Paris and the "same" comet made at Greenwich beginning in December. On close examination the Greenwich data proves to be the version sent in February 1680/1681, with the addition of solar positions received in March. The longitude for January 10 was originally a now barely discernible ♈ 20°49′ ½ which according to Newton's April letter he preferred over ♈ 20°42′ listed in the revised set sent in March. Later Newton altered this position to ♈ 20°41′ ½, more nearly in accord with the March version. The discrepancies between the Paris and Greenwich observations noted in the Waste Book tabulation were cited in Newton's April letter.

The facing folio 99r continuing to folio 101r provided corresponding information about the tail for the "prior" and "posterior" comets from all four of Flamsteed's letters, Flamsteed's "astrolabe," Newton's observations, with a marginal entry for

61 I ignore the unused subject headings left by the previous owner of the notebook.

observations from Scotland. This marginal entry continued with a partial listing of observations from Hamburg with the rest of the Hamburg information incorporated in chronological sequence of the main text. Descriptions provided by Cassini (1681) and Ponthio (1681) from Gallet and other Jesuit observers in France and Spain were not included. Those sources began to be used in 1685. Another indication of an original date of March 1680/1681 was the use of Tycho's coordinates for a certain star with 68′ added for precession. Newton had become generally dissatisfied by the time of his April correspondence by all available star catalogues and in 1685 was pressing Flamsteed for more accurate coordinates. The compilation of tail data culminated in folios 101r/v with a discussion about a law of tails. This material was essentially work in progress in late March 1680/1681 with variations found in other manuscripts and Newton's drafts and letter of April 1681. Nothing was heard of the law of tails later.

The law of tails was followed in folio 101v with no obvious break or variation in handwriting by "what Halley told me" about his observation of December 8, 1680, information Newton wished for in April 1681. A close examination of the entry for December 8, 1680 in folio 99r reveals certain details from what Halley told him have been squeezed in with information that had come from Flamsteed in February 1680/1681. Again with no obvious break or variation in handwriting the text in folio 101v ended with an unrelated and incomplete note (in English) about Jean Richer's pendulum experiments at Cayenne (5° N latitude) in French Guiana. It is reasonable to suppose that the information attributed to Halley and Richer was an insertion following a meeting in fall 1684 when both comets and gravitation were in the forefront of Newton's thoughts.

Rounding out the data for the posterior comet, folio 102r provided information about the appearance of the head gleaned from Flamsteed's letters, arguably entered in March 1680/1681. Folio 102v is blank.

Newton's entries on Folios 103r-104v consisted of notes from Hooke's *Cometa* and related material from Hevelius that led directly to Newton's attempted rectilinear calculations for the comet of 1664 in CUL MS Add. 3965.11, ff. 154r-155v. While the use of Wing's solar tables in the calculations suggest a date in 1680/1681, Newton might have used them for a while in 1684/5 before shifting to Flamsteed's solar theory.[62] Overall, the selections and omissions from *Cometa* and sequel mirrored concerns of Newton in 1680/1681. A different version of some of this material on the comet of 1664 was added to the crazy quilt document (CUL MS Add. 3965.14, f. 616r).

Newton followed on folio 105r with his observations of 1682 that were probably entered contemporaneously. A brief mathematical entry at folio 107r may have been in place by 1684 to block continuation of comet data. This arrangement would explain why the incomplete listing of micrometer corrections that likely date from fall 1684 reverted to blank space on folio 97r left at the beginning of the section.

Cassini (1681) was not available in England until about May 1681. As work revived in early 1685, Flamsteed urged Newton to consult it. Newton probably did not get around to it until late summer 1685 at which time he noted discrepancies with other

[62] Flamsteed's tables were used to calculate solar places corresponding to Ponthio's observations. These calculations are found on a sheet with early drafts of definitions for the original book I of the *Principia*. (CUL MS Add. 3965.5, f. 22v.

reports and requested clarification from Flamsteed (Newton 1959–1977, 2, pp. 419–421). Newton squeezed additional observations from the book in the earlier table at folio 98v of Paris observations received via Flamsteed.[63] Continuing, folio 98r which had been blank, Newton included coordinates for the November comet from Ponthio, Pierre Ango, Geminiano Montenaro, and ended with observations from Hevelius (1685).[64]

The comet notes in the Waste Book almost certainly date in late March or early April with entries in 1682, 1684, and 1685 as noted.

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MS Add. 3965.14, f. 554r (1684–85); f. 554v (February 1680/1?); f. 598 (Jan–Mar 1680/1); ff. 613r–614v (Mar 1680/1, 1685?); ff. 615r–616v (Feb-Mar 1680/1 with additions possibly as late as the 1720s.).

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[63] The original set of data from Paris in the Waste Book at folio 98v was received by letter and differs slightly from listings in Cassini (1681, p. 86, either imprint). Limited space allowed Newton to add only three additional observations from Cassini's much longer table.

[64] The first known reference to Ponthio's observations was deleted from the manuscript of the original book two of the *Principia* CUL MS Add. 3990, f. 41r. The details attributed to Ango and Montenaro may have derived from Cassini (1681).

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