

**THE ACHIEVEMENT OF NEWTON'S  
"THEORY OF THE MOON'S MOTION"  
OF 1702**

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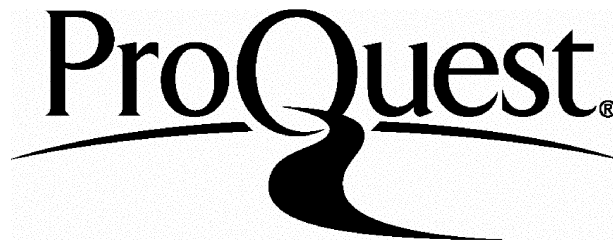
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## ABSTRACT

This study utilises computer programs to reconstruct the challenges which faced astronomers at the time of the founding of Britain's Royal Observatory. It focusses on the lunar theory articulated by Isaac Newton in 1702, showing how it was the precursor of what became embodied in the 1713 *Principia* as its lunar theory. Conceived as a kinematic mechanism, it has here been translated into trigonometric terms, and thence into machine-readable form. A computer replica of Newton's theory has thereby been composed and tested, and its accuracy for the first time assessed, resolving age-old controversies.

The first British lunar theory, formed by Jeremiah Horrocks in the 1630s, was published by Flamsteed, and later modified and developed by Newton. As such it spread across Europe in the first half of the eighteenth century. It was later replaced by lunar theories derived from the Newtonian theory of gravity, which came to be called the 'Newtonian' theory, causing the theory actually promoted by Newton to be overlooked.

Newton's theory had seven steps of equation as its distinctive feature, little appreciated by historians. No evidence remains that gravity theory, applied in a quantitative sense, assisted its composition.

Computer replicas of the lunar theories used by Flamsteed and Halley have been constructed and tested, and Halley's use of the Saros cycle to correct errors in the method is re-evaluated. A survey of astronomy textbooks containing tables over the period 1650-1750 has been the context for assessing to what extent the 1702 Newtonian procedure was an improvement upon existing theories.

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Of the several computer experts of Guildford who helped in setting up the computer replica of Newton's lunar theory here used, who are mentioned in chapter Eight, I especially thank Jonathan Loretto.



## FOREWORD

AT THE DAWN OF THE NEW CENTURY, there appeared the first two textbooks of Newtonian astronomy: Gregory's *Astronomiae Physicae* of 1702, and Whiston's *Praelectiones Astronomicae* of 1707 (which later appeared in English as *Astronomical Lectures*). They were meant to challenge the Cartesian philosophy then being taught in the schools of England, and both contained the full text of Isaac Newton's *Theory of the Moon's Motion* (hereinafter referred to as 'TMM'). Though occupying a mere five pages of Gregory's book, it formed an essential part of that challenge, for it purported to show that the new Newtonian philosophy had a practical and not merely theoretical significance.

At least one of these books definitely claimed that TMM had achieved what was then regarded as well-nigh impossible: showing how to predict the Moon's position in the sky well enough to be of service for finding longitude. French astronomical treatises in the opening decades of the eighteenth century struck a rather sceptical note over this claim, while British ephemerides-composers tended to regard TMM as a kind of Holy Grail: something which would render possible the production of what was most desired, a reliable lunar ephemeris, if only it could be rightly interpreted.

Few were the sailors who made grey hairs, as the saying went, in those days. As their ships sailed back, laden with chocolate from Africa or silks from India, they were as we still say today, quite 'at sea' once land disappeared. Huge prizes were offered for any means to find longitude. And yet, Britain's two most distinguished astronomers of the time - Captain Edmond Halley and the Reverend John Flamsteed - had more or less diametrically opposed opinions as to the real value of TMM. The former claimed that it profoundly improved lunar prediction, while the latter averred that it gave no real improvement upon existing tables.

Historians of science have been reluctant to comment upon the matter. Bernard Cohen was not exaggerating when in 1975 he stated:

'...this work [TMM] has hardly ever been discussed (or even referred to) in the literature concerning Newton or the history of astronomy (Cohen, p.1)

As the literature there alluded to is of no small volume, such an omission would tend to suggest that this brief work was hardly significant. On the other hand, TMM was frequently reprinted through the first half of the eighteenth century, suggesting that it was exerting some kind of influence. To what extent this was practical, or mythical, is the subject of our inquiry. Just about everything except the authorship of TMM remains unsettled. Was TMM ever in fact used? If so, would its prescriptions have defined the much-sought lunar position, to anything resembling the claim made by its publisher?

The onward-rolling tercentenary process has not yet reached the date of TMM's publication, which gives us some time to re-evaluate the traditional myths surrounding the subject. It has now (January 1992) passed by the anniversary of the commencement at Greenwich in 1691 of the most accurate series of positional astronomy readings ever made, and approaches Flamsteed's marriage, Newton's nervous breakdown, and then the historic commencement of the collaboration between these two on the great endeavour, not without strife, a linking together of theory and practice.

## Abbreviations used in Text

Correspondence - *The Correspondence of Isaac Newton*

Volume IV 1694-1709, C.U.P. 1967, Ed. J.F.Scott.

DOS - 'Doctrine of the Sphere' by John Flamsteed, published (anonymously) as *De Sphaera* in 1681, in Jonas More's *New Systeme of Mathematics*.

DOS-PC - Computer-simulated model of the DOS procedure (Chapter 10).

GHA - *The General History of Astronomy*, Volume 2A, Ed. Hoskins, 1989. The reference will normally be to the two chapters by Curtis Wilson.

PNM - *Principia Naturalis Philosophica Mathematica* by Isaac Newton. References are normally to the Second Edition of 1713. The Third Edition of 1727 is available in Motte translation, Cajori Edition.

P.T. or Phil. Trans. - *Philosophical Transactions of the Royal Society*.

TMM - 'Theory of the Moon's Motion' by Isaac Newton, published in Latin by David Gregory in his *Astronomiae Physicae* of 1702. An English translation appeared in 1702, possibly by Halley, which is the text here referred to as TMM, reproduced by Cohen in 1975.

TMM-PC - Computer-simulated model of the TMM procedure (Chapter 8).

## Ch . 1                    INTRODUCTION

### I    The Problem

Writing in 1975, Bernard Cohen posed the challenge:

'It would be most useful to have a careful analysis of Newton's attempts to produce a satisfactory lunar theory (in the 1690's), and the stages whereby he either partially or totally abandoned the program of deriving such a theory by mathematical methods applied to gravitational celestial mechanics' (Cohen, p.80).

Cohen offered no comment upon either the accuracy of the theory - whether it was an improvement upon those available - or, to what extent if any it was based upon a theory of gravitation. As Craig Waff commented in his review of Cohen's book:

'While I can sympathise with Cohen's reluctance to become involved in what would certainly be an extremely complex study, his failure to make even the slightest effort in this direction made it impossible for him to answer in any satisfying way a question which he constantly raises...' - i.e., that mentioned above (Waff, p.66).

Craig Waff commented upon the historical irony, that the brief 1702 essay, *Theory of the Moon's Motion* (hereinafter referred to as TMM), was 'probably the most obscure of Newton's publications', and yet it 'appeared in print during the early eighteenth century more times than anything else which left the hand of Newton.' Waff then made a claim which regrettably he has never substantiated:

'Newton's "rules" had been wholly or partially used by nearly a dozen astronomers or other interested individuals in order to construct lunar tables.'

Were that so, then an assessment of the Newtonian rules would be simple: one would merely take the ephemerides published by these persons, measure their 'error envelopes' in the manner that Owen Gingerich has so well pioneered, and thereby assess their accuracy. Let us merely remark that no-one has ever attempted to do this.

What strikes the modern reader about the text of TMM, apart from its obscurity, is the complete absence of any reference to a theory of gravitation. The *Principia* of 1687 dealt with motion under central forces as a two-body problem, and referring only relatively briefly to irregularities in lunar motion resulting from its motion as a three-body problem (Propn. 66 of Book 1, Propn. 32 of Book III). William Whiston gave the following fine eulogy to TMM, published in 1710 when he was occupying the Lucasian mathematics chair at Cambridge, as Newton's successor:

'The Moon, I say, which is a secondary planet, that hath in it such a complication of Motion, such intricacies and perplex'd Anomalies, that unto this very Day we are scarce able to bring it under Numbers, altho' it be so harrass'd (as it were) with Astronomical Researches. This hath been a knot well worthy of, and which requir'd the acutest Wit to untie. Nor wanted it such a one at length when the famous Sir Isaac Newton set himself to it; who hath this to glory in, That in the Compass of a few pages, he hath brought more light into this dark and intricate Business, than all the Volumes of the past ages had done.'

So finally, the Moon had met its match. Or had it? When Whiston came to explain how the Moon's position should be calculated, he said that, no doubt Mr Newton's theory was very excellent, however as no-one had yet reduced it to a form in which tables could be derived from it, he would give the rules as described by 'the famous Mr Flamsteed'. (Whiston, 1710, p.96) The abyss between theory and practice had not in fact been bridged.

No historian of science has acknowledged the validity of that judgement of Whiston, though he was in a fine position to assess the situation: that, in the year 1707, the procedure advocated by the Astronomer Royal was to be preferred to the Newtonian lunar rules, because the latter had not yet been unpacked, as it were. To what extent was Flamsteed concerned to develop a lunar theory of his own? This view was somewhat indicated by the astronomer Francis Baily, who rescued Flamsteed's reputation from mere oblivion with his *Account* of 1835 (p.703). On the oft-told version of events, Flamsteed was allocated no other role than delaying or perhaps refusing to supply Newton with lunar data in the 1690s, thereby impeding the formation of the

Newtonian lunar theory! That the Astronomer Royal had in some measure fulfilled the mandate of the Monarch who appointed him, by achieving an improvement in the lunar rules, is seldom considered.

We may wonder whether the six different sets of ephemerides which according to Craig Waff applied the Newtonian lunar rules, may to some extent have used those of Flamsteed or even of Halley: the achievements of Sir Isaac have after all shown some propensity to attract towards themselves those of others. Later on we will comment on the apparent disappearance of Flamsteed's version of the theory, and the possibility of it having migrated across the channel. The brief 1702 TMM reaches into the future in two different ways: as a series of no less than seventeen reprints appearing in the first half of the eighteenth century, and then secondly as the greatly expanded lunar section in Book Three of the second edition of the *Principia* of 1712.

We may trace three stages in the development of Newton's lunar theory. In 1694/5 the extensive correspondence with Flamsteed recorded a keen collaboration, when the mathematician clearly believed he could encompass the irregularities of the Moon's motion by applying his theory of gravitation. Whiteside has well described how this noble enterprise was shipwrecked in the spring of 1695 upon the sheer intractability of the problem. Indeed, Whiteside has even suggested that Newton's decision to move to London and abandon his lecturing post at Cambridge may have been a consequence of his recognised failure with the lunar theory (*Mathematical Papers* 1976, VII, p.xxv). Secondly, there was the TMM, published using Flamsteed's data but without the latter's knowledge or consent and despite two signed promises not to do such. As if in reaction against the failure of the first stage of the endeavour, no comment was made about a theory of gravitation. Thirdly, a decade later, there appear the mature Newtonian comments upon the three-body problem, which greatly impressed the cognoscenti. A review in the *Acta Eruditorum* (believed to be by Leibniz) commented on this section of the 1713 *Principia*:

'Indeed, the computation made of the lunar motions from their own causes, by using the theory of gravity, the phenomena being in accord,

proves the divine force of intellect and the outstanding sagacity of the discoveror.' (Cohen, p.41)

And Laplace said,

'Je n'hésite point à les regarder comme une des parties les plus profondes de cette admirable ouvrage.' (Cohen, p.41)

A modern evaluation of the achievement ought perhaps to start from the result which emerged somewhat unexpectedly from Owen Gingerich's computer in the Harvard University astrophysics department: namely that little by way of increase in accuracy of ephemerides appeared as a result of the Newtonian revolution\*. It was, let us say, a theoretical affair. During the period which we are reviewing, Paris became the main centre of ephemerides-production.

## II Perceived accuracy of the 'Theory'

We now review the spectrum of judgements which history has handed down as regards the accuracy of TMM.

Within two minutes: This claim was brazenly made by David Gregory in publishing the essay in his *Astronomicae Physicae et Geometricae Elementa* of 1702, and no doubt stimulated its sales. A two-minute accuracy in lunar prediction would be sufficient to attain a one degree accuracy in the estimation of longitude.

Two to three minutes: this was Newton's own view as expressed in a 1705 edition, given in some corrections to the text which he inserted: two minutes in syzygies, three in quadratures. When Gregory republished TMM in the English translation of his book in 1715, he echoed this view. Thus, reprinting the essay 13 years later, Gregory hardly found cause to alter his original judgement of its accuracy. William Whiston made much the same claim in his published astronomy lectures of 1707. The astronomy professors of Oxford and Cambridge thus concurred in this formidable affirmation of TMM's accuracy.

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\*'Perhaps the most surprising result of our analysis is how little immediate and direct impact Newton's work had on the computation of astronomical positions' (Gingerich & Welther, 1983, p.xi).

Two to five minutes: three decades later, Edmond Halley as Astronomer Royal affirmed that after himself preparing tables and ascertaining his calculation procedures, it was evident to him that:

'... Sir Isaac had spared no Part of that Sagacity and Industry so peculiar to himself, in settling the epochs, and other Elements of the Lunar Astronomy: the result many times, for whole months together, rarely differing two Minutes of Motion from the Observations themselves;' (Phil. Trans. 1732 p.191)

Halley went on to say that, on occasions where the theory did err up to five minutes, this was probably the fault of the observer i.e. Flamsteed, who had both supplied inaccurate data and failed to supply any in the third and fourth quarters of the lunar cycle. Halley was in a fine, indeed optimal, position to comment, though there is no reason to take seriously these slurs upon his predecessor. The latter's lunar positions achieved an accuracy of around half an arcminute (Kollerstrom and Yallop, in preparation) and covered the entire lunar cycle.

Five minutes: this seems to have been Newton's estimate when appointed in 1714 to the Board of Longitude. Lunar methods he judged to be too inaccurate to determine 'a Longitude within Two or Three Degrees.'

Eight to nine minutes: this was the recent verdict of Curtis Wilson, editor of volume 2A *The General History of Astronomy*, p.267. He was merely echoing Flamsteed's verdict. The latter found, in the beginning of the year 1703, that TMM generated errors which were 'frequently' of 5 or 6 minutes one side, and by the same amount negatively at the opposite point of the orbit, and that sometimes the errors rose to 8' or 9' in longitude, at positions near to quadrature (ie, the half-Moon position). These things, he explained to his correspondent Mr Caswell, he determined using old data between the years 1675 and 1689, ie prior to the setting up of his great mural arc. Plainly he would not use data gathered since that date, as it had all been sent to Newton so that he could construct his theory. The astronomer was especially shocked by the errors in lunar latitude contained in TMM, which he said 'were frequently 2,3, or 4 minutes, which is intolerable.'



Next, Flamsteed examined lunar eclipse data, where one might expect smaller errors, on the grounds that astronomy had traditionally concerned itself only with the syzygy positions in the lunar orbit, requiring these for the prediction of eclipses. Again, he discerned errors of 5-6' in their positions. (Baily, pp.213,4)

We may have more to say later concerning the protean flexibility of the estimates here represented. To place them in perspective let us cite some findings of Gingerich: that *La Connoissance (sic) Des Temps*, the ephemeris then produced yearly by Cassini from the Paris Observatory, did over the years 1695-1701 frequently display errors in its lunar positions of 20-30 minutes of arc; (Gingerich & Welther, 1983, fig.14) and secondly, that the much-desired predictive accuracy to two minutes of arc was not attained by any ephemeris prior to the British Nautical Almanack commencing in 1766. (Gingerich & Welther, 1983, p.xxi)

There were several notable disasters at sea which stimulated astronomers to work with greater zeal on their nigh-impossible quest, of using Luna's erratic path across the night sky to ascertain longitude.

1691: seven British warships wrecked near Plymouth, mistaking the Deadman for Berry head due to a misconception over longitude.

1694: Admiral Wheeler's fleet, ignorant of its position, sailed head-on into Gibraltar and disaster.

1707: Sir Cloudsley Shovell's squadron of the Royal Navy ran onto rocks off the Scilly Isles, with loss of four ships and nearly two thousand lives, when they were believed to be in a safe position.

The last of these was due more to inadequacy in the maps used than longitude determination (Howse, Autumn 1993, p.47), however it did much to arouse public opinion on the matter, and led to the passing of the Longitude Act: in 1714, huge rewards were offered by Parliament for anyone who could devise a method of locating the longitude on a ship, to within one degree or less. 'Finding the longitude' entered the vernacular as meaning an impossible task which one despaired of ever achieving. A life

and death issue, it revolved around the most obscure equations. The rewards offered began at £10,000 for predicting longitude at sea within one degree, and went up to £20,000 for half a degree. They began to be claimed in the mid-eighteenth century.

### **III The Two-Clock Method**

Local time varies around the globe by one hour per fifteen degrees of longitude. Therefore, if one had two clocks, one on local time and the other on universal time, the longitude would be given from the time difference between them. At sea, a clock can readily be set to local time by using the times of sunrise and sunset, with noon falling midway between them. If the Moon's 27.3 day cycle against the stars could be determined, then it would enable one to read universal time: it would be like a clock, whose hand revolved once in twenty-seven days. That was the beckoning dream, the impossible hope, the mirage on the horizon....

The great aim was to predict longitude within two minutes, for this would bring it within a useful - though not a safe - range. Without that one would be, as the saying went, 'at sea.' It became the most pressing scientific problem of the period, and was the reason for establishing the Observatory at Greenwich. To find longitude within a degree meant predicting the Moon's position within  $1/27.3$  degrees = 2.1 minutes.

We can see this merely by considering that the Earth revolves against the stars 27 times faster than does the Moon. So, a two minute error in fixing the Moon's position in zodiac longitude would logically imply a 54 minute error in one's position on Earth, in longitude, neglecting other sources of error. The ratio of the period of Earth's rotation to that of the Moon's revolution around the zodiac (27.3 days) gives the error multiplication factor inherent in the method.

How accurate was the method in practice? An example here comes from an entry in the diary of Edmond Halley, when he landed his frigate off the coast of Brazil in the year 1699. He was returning from his courageous antarctic voyage, and wanted to find out his longitude. He and the crew of

his 'Pink' (a type of Dutch sailing vessel) the *Paramore* found themselves near the town of Paraiba. We may assume that the inhabitants of Paraiba were ignorant of their longitude relative to London.

Halley first of all set up his telescope to view Jupiter, because his tables predicted an occultation of one of the Jovian satellites, on the night of February 25th. In this he found himself frustrated, because clouds obscured his view. (This Jupiter-satellite method would have given him a universal time estimate, from which longitude could be estimated as explained above) The Jupiter-moon method having failed, it so happened that the Moon was passing by a first-magnitude zodiac star Antares, and so an 'appulse' could be observed. An 'appulse' meant the time of nearest approach of two heavenly bodies. Halley noted both the time of this event to the nearest second, and the lunar altitude when it occurred, and from these wrote:

'I conclude the longitude of this Coast full  $36^{\circ}$  to the Westward of London.'

Halley was within almost one degree of the correct longitude, which is quite impressive (The longitude of Paraiba is  $34^{\circ} 52'$  West)\*. The ephemeris he used was probably the French *La Connoissance des Temps*.

In the seventeenth century, not the least source of error in using the method was the absence of any sound notion of mean time. The observations were made using apparent time and then converted to 'equall time' (i.e., local mean time). Only then could the comparison with universal time, from the lunar sidereal orbit, be accomplished. Tables for this conversion were wildly inaccurate: for example, the 'Table of the Aequation of Civill Dayes' given in Wing's *Harmonicon Coeleste* of 1651 had an average error of five minutes, and this error in time would give a two or three minute error in lunar longitude. I ascertained this using a RGO program for the Equation of Time, and also checked the equations of time given by Streete (1664) in several of his worked examples, and a column of such figures given in the

.....  
\* *The Three Voyages of Edmond Halley in the Paramore*, ed. N.Thrower, 1981, p.103. See also, 'The Edmond Halley Bull's Eye Enigma' N.Kollerstrom, *Jnl.Brit. Astron. Assoc.*, 1990, p.7

French annual ephemeris *La Connoissance des Temps* of 1686: which showed errors usually around 4-5 minutes. The first reliable Equation of Time was published by Flamsteed in 1673, in a postscript to the 'Opera' of Horrox, (pp. 441-464), after which his later more accurate table was published by Whiston in 1707. Historians of astronomy generally give little credit to Flamsteed for establishing Greenwich mean time, by discerning that the Earth's uniform sidereal rotation throughout the year should be its basis (though Bailly (1779 p.269) did credit him with having 'restored' the equation of time). An improvement of several arcminutes in lunar longitude determination came about from Flamsteed's discovery of the Equation of Time. Thus TMM could simply presuppose that mean time was being used.

When Newton sat on the Board of Longitude, set up in 1714, he there expressed the view that the lunar method only worked 'within two or three degrees' (Westfall,835). The preceding discussion showed that this was equivalent to lunar longitude accuracy of 4-7 minutes, which sounds fairly reasonable. This suggests that Halley having obtained longitude within almost one degree, two decades earlier, was something of a fluke. Or, perhaps after all, the people of Paraiba did know their longitude.

An account of the lunar method of finding longitude given by Howse (Address to Royal Society on Chronometry, June 25th 1993) elucidated the practical problems in the method, and also the success which the method eventually enjoyed, from the latter half of the eighteenth century: 'The heyday of lunars was probably from about 1780 to 1840' (Howse, November 1993, p.7). Chronometers became available from the mid-eighteenth century onwards, but remained prohibitively expensive for most vessels, so that the Greenwich 'Nautical almanac' published annually from 1767 offered the preferred method of finding longitude at sea\*.

.....  
\* According to Gingerich and Welther, 'By 1800 the accuracy of the best almanacs was comparable to our tables, that is, better than a minute of arc (Gingerich and Welther 1983). Their graphs of the error patterns of the Nautical Almanac over 1779-1787 shows errors generally between one and two minutes of arc, compatible with statements by Howse and Sadler that lunar tables of the 1760s enabled sailors consistently to find their

Further difficulties arose in the lunar method from the corrections due to atmospheric refraction and parallax, which had to be applied to the observations before further computations could be made; a matter with which TMM was concerned.

Merchant vessels using the lunar method came to adopt the Greenwich longitude meridian as their reference, as the lunar noon positions were for that longitude. Thus the endeavours of Newton and Flamsteed did in the end bear fruit, a century or so later, with the locus of their endeavours becoming accepted globally as the zero meridian of longitude. 1753 was the first occasion on which the lunar method was used with success at sea, by Nicholas-Louis de Lacaille in an Atlantic crossing (Howse op. cit. p.4).

A check of the first page of the 'Nautical Almanac' for January 1767 for the first twenty lunar meridian transits showed a mean error of  $16'' \pm 17''$ , one-third of that shown on the Gingerich-Welther error-graph (1983, p.xxi). The 'Nautical Almanac' gave positions in apparent time, so conversion to mean time was first necessary. The Yallop et. al. program for Equation of Time (1989) was used, to convert from apparent to mean time, plus an I.L.E. program for lunar longitude (both kindly supplied by Bernard Yallop of the R.G.O.).

For example, on January 7th 1767 the Almanac gave the noon lunar longitude as  $18^{\circ} 59' 25''$  of Aries. As the Equation of Time was then 6 minutes and 50 seconds, GMT was then 11am 53 minutes and 10 seconds (Mean time = apparent time - Equation of Time) for which the computer gives a position of  $18^{\circ} 59' 16''$ , a net error of  $9''$ . These values are more compatible with what was believed at the time about the tables, and help us to appreciate the extent to which the lunar-longitude method did in the end succeed. This case-study underscores the vital importance of having computer programs more accurate than the historical positions to be evaluated.

.....  
longitude 'within  $1^{\circ}$ ' (Howse op. cit. p.4; also Sadler 1976 p.117), for which two minutes accuracy in tables of lunar longitude were adequate.

## **IV Three Approaches to the Problem of Prediction**

In the latter half of the seventeenth century, three distinct approaches were taken for predicting the Moon's position. They intertwined, but were to some extent logically distinct.

### **(1) Empirical: Using the Saros**

Edmond Halley in Highbury commenced taking lunar longitude readings with a view to tracing a whole Saros cycle of 18 years, 11 1/3 days, (ie, 223 lunar months). It happens that all the principal irregularities in the Moon's motion repeat through this period in a precise and cyclic manner. Halley quite sensibly believed that a continuous sequence of observations over such a period was the best approach. He was later to be able to follow a complete Saros at Greenwich, though his successors did not deem his observations of much value.

### **(2) Use of a Model: the Method of Horrocks**

As a north-countryman, Flamsteed was proud of having made public and improved the technique invented by the young Jeremiah Horrocks in the 1630s. Horrocks invented a kinematic model, wheels within wheels, like some English Heath-Robinson version of the epicycles so recently banished by Kepler: but it worked. His great discovery was the rocking motion in both the apse line of the Moon (once every six months) and in its eccentricity. (N.B., all models of this Horrox-effect use a circle, not an ellipse, for lunar orbit, where eccentricity retains its old meaning of Earth's distance from centre of that circle) The version published in 1673 by Flamsteed was regarded as the Horroxian method improved by Flamsteed.

### **(3) Mathematical: The Theory of Gravity**

'For I find this theory so very intricate, and the theory of gravity so necessary to it, that I am satisfied it will never be perfected but by somebody who understands the theory of gravity as well, or better than I do.'

Newton wrote these words to Flamsteed on February 16, 1694. They were to be vindicated by the mighty labours of Clairaut, Lagrange and Laplace in the next century, using the Leibnizian calculus. But in that period, Newton was

faced by abject failure: he later wrote wrathfully to Flamsteed on hearing that the latter proposed to make public the fact that he had supplied 150 lunar positions for the Cambridge mathematician:

'I was concerned to be publicly brought upon the stage about what, perhaps, will never be fitted for the public, and thereby the world put into an expectation of what, perhaps, they are never like to have'  
(January 6, 1699).

That was his last known comment upon his endeavour with the lunar theory prior to TMM's composition, which is curious.

## V The Epoch of TMM

TMM was composed at the dawn of a new century, in February 1700, by the Master of the Mint. The stress of the great recoinage had passed away, and perhaps some new hope dawned that he could indeed resolve the problem. TMM opens with some basic parameters in celestial longitude. Its epoch spans twenty years from noon on December 31 1680 to noon on December 31 1700, over which it surveys the Moon's motion. Let us compare the mean values there given with the actual positions at the time. This will give an idea of what was involved.

### Noon 31.12.1680 (O.S.)

Mean sun	20° 34' 46" Cap,	true position 21° 1',	difference 26'
Mean moon	1° 35' 45" Libra,	true position 8° 3',	difference 6° 27'
Node	24° 14' 35" Virgo,	true position 24° 17',	difference 2'

### Noon 31.12.1700 (O.S.)

Mean sun	20° 43' 50" Cap.,	true position 21° 10',	difference 26'
Mean moon	15° 19' 50" Aqu.,	true position 16° 59',	difference 1° 39'
Node	27° 24' 20" Leo,	true position 27° 27',	difference 3'

The 'mean' sun and moon are mathematical abstractions. They move at a uniform rate, so will normally differ from the true positions. The goal of a lunar theory was to bridge that gap, which evidently could be as much as six degrees, and to do so within a few minutes. This is what TMM was supposed to accomplish. (We can either cite longitude by zodiac sign, as

above, or by quoting the number of signs starting from zero Aries: for the mean sun, for example, instead of  $20^{\circ}$  Capricorn, we could give  $9s\ 20^{\circ}$ . Both conventions were then used.)

## VI Apse Line Motion

The strange motion of the lunar apse was at the heart of the problem. A quote from Newton's *System of the World* will outline the problem:

'By the same theory of gravity, the Moon's apogee goes forwards at the greatest rate when it is either in conjunction with or in opposition to the sun, but in its quadratures with the sun it goes backwards; and the eccentricity comes, in the former case, to its greatest quantity; in the latter, to its least...' This gave a 'semiannual equation of the apogee', of amplitude  $12^{\circ}\ 18'$  'as nearly as I could determine from the phenomena' (PNPM, p.475).

Horrocks' great discovery, concerning the secondary motion of the lunar apse, made in 1638, was soon confirmed by telescope observations. The new eyepiece micrometers could measure the changing size of the Moon. Before that, no-one could well discern the motion of the apse line. There was an inequality called evection which was related (it was first given that name by Ishmael Boulliau in 1645, as the largest of the lunar inequalities); but there is no need for us to pursue it here. As Curtis Wilson has shown, Horrocks reached his new model by theoretical means, by re-analysing the lunar theory of Kepler (Wilson 1987); only later on in the 1640s did the North-country astronomers Gascoigne and Crabtree provide confirmatory evidence from the Moon's varying apparent diameter (Chapman 1982, pp.19-21). A letter of Flamsteed's printed in the Royal Society's *Phil. Trans.* of 1675 concerned the Horrocksian system, describing how it was only after 'many curious and careful measures of the *Moons diameters*' that he came to realise that no other theory could account for the phenomena (*Phil. Trans.* 1675, p.368-370).

On top of this rocking motion - nearly  $30^{\circ}$  to and fro in the case of perigee, twice a year - a notional line joining apogee and perigee revolves against the stars once in nine years. That line is a mathematical



abstraction, from which the true apogee and perigee positions can deviate quite widely.

We may view this perplexing issue retrospectively, by quoting the opinion of the French theoretical astronomer Clairaut given in 1748, in a letter to the Astronomer Royal Bradley:

'Au reste la theorie de la lune qui résulte de ma solution est fort différente de celle de M. Newton: je ne trouve point come lui les variations d'excentricité et les inégalités dans le mouvement de l'apogee.' (Gaythorpe, 1956, p.136)

That was the Horrocksian theory which Clairaut was rejecting. The lunar apogee does indeed have the 'inegalities' which Horrocks ascribed to it, more or less, but does its eccentricity vary as Horrocks described? Modern theory lacks anything resembling the  $\pm 21\%$  variation in the eccentricity function, that TMM utilised, a matter further discussed in the next chapter.

If one turns to a nineteenth-century account of these things, say Stevenson's *Newton's lunar theory exhibited Analytically*, (1834) then what there majestically unfolds as 'Newton's Lunar Theory' has no trace of that double motion of the apse line: it has merely a single rotation in nine years. The whole thing much resembles Clairaut's lunar theory, and that of Horrocks is nowhere to be seen. Clairaut's view, to quote further from his letter to Bradley, was: 'les différentes espèces de termes qui sont dans mon equation pourront bien faire le même effet que les variations dans l'excentricité et dans le mouvement de l'apogee.' Stevenson's 1834 version thus appears as a mythologised version of the 'Newtonian theory'. In a preface the author assures us he has merely translated the theory 'from the hieroglyphics of geometry' into the workaday language of algebra.

Herein lies the nub of the problem. The hieroglyphics of the geometric-kinematic forms in which seventeenth century lunar theory expressed itself may seem as remote from modern comprehension as an arcane alchemic sygil to a modern chemist. It will require quite an effort on our part to enter into the meaning of these old diagrams, from a period before trigonometric functions were used to describe the time-dependent variables of astronomy.

Whiteside's 1976 tercentenary essay marks the beginning of a realistic assessment of TMM. Entitled 'From high Hope to Disenchantment', it has been accepted by more recent scholars, pre-eminently Curtis Wilson, whose fine achievement in the *General History of Astronomy* (vol. 2A) includes an evaluation of TMM. It concludes that Newton's adoption of Horrocks theory was a historical mistake, which prevented his making further progress. That seems a rather pessimistic view. Also it may not adequately assess the extent to which Horrocks' theory was true. The young Horrocks has after all been viewed as initiating the tradition of British astronomy (Chapman 1982).

## **VII Enlightenment Reception of TMM**

Craig Waff and Curtis Wilson both affirm that Newton's lunar theory was applied to the construction of lunar ephemerides in the first half of the eighteenth century. If so, this is a matter of vital importance, for it would demonstrate beyond doubt that TMM had been decoded into practical rules. It seems a reasonable claim, for indeed why else would TMM keep on being reprinted over this period were in not used in practice? Curtis Wilson affirmed that the rules of TMM 'were incorporated in the tables of Charles Leadbetter's *Uranoscopia* (1735)'. (*General History of Astronomy*, p.269) Baily had said the same in 1835, affirming that Leadbetter had given 'a more perfect adoption of Gregory's *Newtonian rules* [Baily's term for TMM] reduced to a tabular form' (p.709) Baily added, however, that in 1742 Leadbetter brought out a new set of lunar tables, 'without any allusion to Newton's labours.'

Turning to the work published by Leadbetter in 1835, chapter nine of *Uranoscopia* is entitled, 'to calculate the true Place of the Moon more exactly than was ever yet done', however this contained no allusion to TMM. Half of the book consists of tables, but in the chapter introducing these tables, the last of the book, we again find no Newtonian allusions. The frontespiece of the book merely states that the book will give the 'Flamsteedian method of Computing times of Eclipses'.

There was an allusion to TMM, but it was ironical in tone. Discussing the work of a rival, Leadbetter advised his readers,

'Another tells us, that his Calculations are from Sir Isaac Newton's Theory of the Moon; and therefore nobody must question the truth of them. Indeed, if it were so, not any one living would dare to question them. But I deny the assertion; and can prove, that his calculation is not from Sir Isaac Newton's theory.'

It appears that kudos was available to any almanack claiming to be based upon TMM, and some rivalry is here evident. When Leadbetter compared some published predictions for an eclipse, he claimed to have made his own prediction 'from new Tables, founded upon Sir Isaac Newton's Theory of the Moon'. Naturally, this gave the most accurate eclipse time. Does that amount to a claim that the tables of his book had been derived from TMM? If so, one can only say that the claim has been made in a highly equivocal fashion. No such claim was made either in the two relevant chapters, or on the frontespiece\*.

The French astronomer M.Bailly struck a sceptical note over TMM not found amongst English historians: he declared that 'mais il [Newton] avoit souvent parlé à la manière des prophètes, qui disent ce qu'on ne peut voir'. (*Histoire de l'Astronomie Moderne*, III, p.150, quoted Bailly p.694). This is a matter which we may hope to resolve.

.....  
\* Elsewhere, Leadbetter says of a rival: 'Tycho Wing, in Coley's Almanack, which he says is from Sir Isaac Newton's Theory of the Moon; but this is a mistake, because it is so vastly wide of the truth, that it will not bear the test.' Leadbetter appears to be claiming to have fathomed TMM, without committing himself to saying that his own tables were based upon it.

## Ch.2 LUNAR INEQUALITIES AND APSE MOTION

### I Stages of Development

We here review some themes leading to what was nearly the last stage in Newton's lunar endeavour, in the second edition of the *Principia* of 1713. This developed matters which had been tersely stated in TMM of 1702. As was emphasised by Whiteside in his 1975 tercentenary address over the founding of the Royal Greenwich Observatory, the first edition of the *Principia* dealt most successfully with lunar motion as uniform and regular, as a one-body problem, the motion of a body around an immovable force-centre. In the Second Edition the position of the baricentre (Earth/Moon centre of gravity) was estimated, enabling two-body computations to be performed.

That accomplishment of 1687 did not assist the construction of lunar ephemerides. The first step in this direction came following a visit by Newton to Flamsteed at Greenwich in November of 1694, when he was shown a table comparing observed and theoretically-derived lunar longitudes over a series of meridian-transits. The theoretically-derived positions were from Flamsteed's Horrocksian method as published in DOS (1681), and these were compared with lunar centre positions for the same times, obtained from his lunar limb observations. A column had been drawn up showing the differences, ie errors in computed longitude, which averaged around eight minutes of arc. On the whole, Flamsteed's determinations were within half a minute of error, though cited to arcseconds.

Newton borrowed this tabulated data, and in the following months requested altogether just over two hundred lunar positions from Flamsteed: which he was sent - contrary to centuries of calumny about the latter refusing to part with his data - in the months following.

No mathematician ever had so many lunar positions of such accuracy. In the early months of 1695, Newton's letters to Flamsteed display a keen enthusiasm for the subject, and belief that his theory of gravity should be able to encompass the problem. After all, the rest of the Universe was obeying it.

In this section we will not enter into the political dimension of the problem, our prime concern here being with the mathematics. Suffice to say that Flamsteed was not permitted to claim any credit for his enormous labours in producing the lunar data, and that the 1702 treatise appeared without his knowledge or consent. This phase of Newton's lunar endeavour terminated rather abruptly in 1695, shortly prior to his moving to London and becoming Warden of the Mint. Optimism gave way to bitterness, and what had been a friendly and respectful correspondence since 1672 (when Flamsteed wrote to Newton over the latter's new colour theory) was replaced thenceforth by distrust, at least on the astronomer's part.

TMM, written by the Master of the Mint, surveyed the periods and inequalities of lunar motion, and described a kinematic model, basically that of Horrocks. TMM thus represents a diametric antithesis to the *Principia's* endeavour of 1687. The latter was a work of theory, of zero practical utility as far as lunar prediction was concerned. The former contained no theory as is nowadays understood (despite its title, conferred it is supposed by David Gregory), and gives no hint that its author had developed an inverse square law of gravity. It is as if the hope expressed in early 1695 had been extinguished, in that no theory was present, and its author had regressed to a kinematic approach, with the old epicycles and deferents still there. The frequent reprinting of TMM through the first half of the eighteenth century indicates that it was highly esteemed as (presumably) of practical utility. In it, Newton had begun to grapple with what was widely perceived as the most pressing scientific problem of the day, the finding of longitude from the wandering path of the Moon, for navigators at sea.

The tension between these two contrasting statements was resolved in 1713, when the new PNEM reviewed the lunar inequalities, and claimed to be accounting for them by the theory of gravity. To what extent it did so will be discussed later. It was an extended attempt to deal dynamically with a three-body problem, viz. the interaction of Sun, Earth and Moon. The lines of Halley's ode,

*At last we learn wherefore the silver moon  
Once seemed to travel with unequal steps,  
As if she scorned to suit her pace to numbers-  
Till now made clear to no astronomer;*

were composed for the 1687 edition, but did not properly apply to it, rather they expressed what Halley as an astronomer hoped he was going to find in it. However, they could be applied to the 1713 edition.

## **II Lunar Theory in the Second Edition OF PNPM**

As given in the *Principia* 1713, the lunar 'equations' - ie, angular distances between a mean and true moon - are based upon angles formed between the the Sun's position and both the apse line and nodal axis.

The Newtonian lunar 'theory' has three main components, which can be regarded as additive: (1) his 'equation of the centre', a variant of Seth Ward's 'empty focus' method of approximating to Kepler's second law; (2) the Horrocksian oscillation of the apse line, with its concurrent oscillation in the eccentricity of the lunar orbit; (3) six extra lunar 'equations' added to these, which were entirely original. His method of computing the Horrocksian oscillation used an approach of Edmond Halley, whereby the lunar ellipse had its center on an epicycle which revolved twice yearly around a point near the Earth. A second small epicycle revolved around its perimeter.

In addition to these major components, there was also what astronomers call 'the reduction', namely the transform necessary to move from the plane of the lunar orbit to the ecliptic. However, this was straightforward and uncontroversial, so need not be discussed here.

Let us start by viewing (2) and (3) as oscillations defined by sine functions of different periods. The computer can reconstruct the actual motions of the Moon over historical time, checking up on any element of the theory as required. Or, we may hope to discern them in Brown's lunar

theory, whose gargantuan equations served as the very definition of time up until the mid-1980s, when they were formally replaced by atomic time. Our historical treatment requires only the first few terms of the modern lunar equations.

### III The 'Horroxian year' and the Apse Line

'And those inequalities...generate the principle which I call the semiannual equation of the apogee; and this semiannual equation in its greatest quantity comes to about 12° 18', as nearly as I could determine from the phenomena.' (PNPM, p.475)

Over a period of one year and forty-five days the apse line (which is the line joining apogee and perigee positions) aligns twice with the Sun-Earth axis. Let us call this period the Horroxian year, as there is no current astronomical term for it (The latinised form of Horrocks' name will here be used, solely for such astronomical terms as pertain to his theory). Over half that period, Jeremiah Horrocks in 1638 affirmed, the apse line had an oscillation of 12° amplitude. It swung dramatically back and forward twice, in addition to its yearly mean motion of 40°. Newton referred to this as 'the semiannual equation' (Scholium of Prop. 35, PNPM, p.475), by which he meant that its period was half a year. More precisely, its period is 206 days\*. It goes through two cycles per Horroxian year. By plotting the longitudes of apogee and perigee positions each month, we may inspect this claim (Figure 2.1).

In TMM of 1702, Newton gives the 'greatest Equation of the Apogee 12°.15'.4" (p.19). In PNPM he gives it as 12° 18'. There is a drawback here, that no such oscillation is to be found in the heavens. An observer of the Moon's apogee, which is its position in the sky each month when it appears smallest, would perceive an oscillation in its ecliptic longitude of around two or three degrees only, twice each year, not twelve degrees.

.....

\* The duration of the 'Horroxian year' comes from the equation,

$$\begin{array}{rcccl} 1/365.24 & - & 1/3232.6 & = & 1/411.7 \text{ days} \\ \text{year} & & \text{apse rotation (9yrs)} & & \end{array}$$

The perigee position in contrast oscillates more vigorously, moving back and forth with an almost twenty degree amplitude. If we take an average of these two oscillations, then the figure of twelve degrees appears. Thus the concept of an apse line is a rather gross approximation, since perigee diverges greatly from such an axis. The apogee and perigee positions have distinct motions. It may be useful to make the contrast with the nodal axis: the two nodes appear as diametrically opposite in the sky, and have a more uniform motion, so it makes sense to visualise a nodal axis between them. To quote from a modern astronomy textbook:

'The oscillations [of the apsides] do not take place simultaneously, but alternately, so that the apsides are not always directly opposite one another in the zodiac, but are continually falling behind and overtaking these positions. The retrograde motion of the perigee (roughly  $40^\circ$ ) is very much larger than that of the apogee (roughly  $2^\circ$  to  $3^\circ$ ), meaning that the former moves much more quickly than the latter against the fixed star background...the perigee can regress by more than  $30^\circ$  in a single month, whereas the apogee moves for up to four months within a field of only about  $3'$ . (J. Schultz, *Movement and Rhythm of the Stars*, 1986, p.91)

The graph (see over) illustrates these motions, measured in ecliptic longitude, of the apogee and perigee positions in modern times (it was made using the *times* for these events as given in Meeus' *Astronomical Tables* (1983), and computing positions therefrom.) The graph shows a mean motion of the two positions of  $40^\circ$  per annum, which is approximately thirteen anomalistic months as plotted on the X-axis (The positions should really alternate on the graph, with apogee appearing first followed by perigee two weeks later, however the graph program cannot manage this, so they appear simultaneous).

A model approximates to reality. In this case, the Horroxian model took the apogee and perigee motions as having a mirror-symmetry which they lack in reality. As the model was not primarily concerned to account for lunar distance - reflected in its apparent size in the sky - but to predict longitude, let us hope that this was not too much of a disadvantage.



We have seen how the unwary reader of PNPM could here be misled on two counts: the inequality was not of the apogee as stated, but of the apse line; and it was only approximately half-yearly ('semiannual'). Also, because this function is discontinuous - there is only one perigee position per month, it has no existence in between these times - a degree of accuracy quoted to seconds may not be very meaningful.

## APSE LINE MOTION

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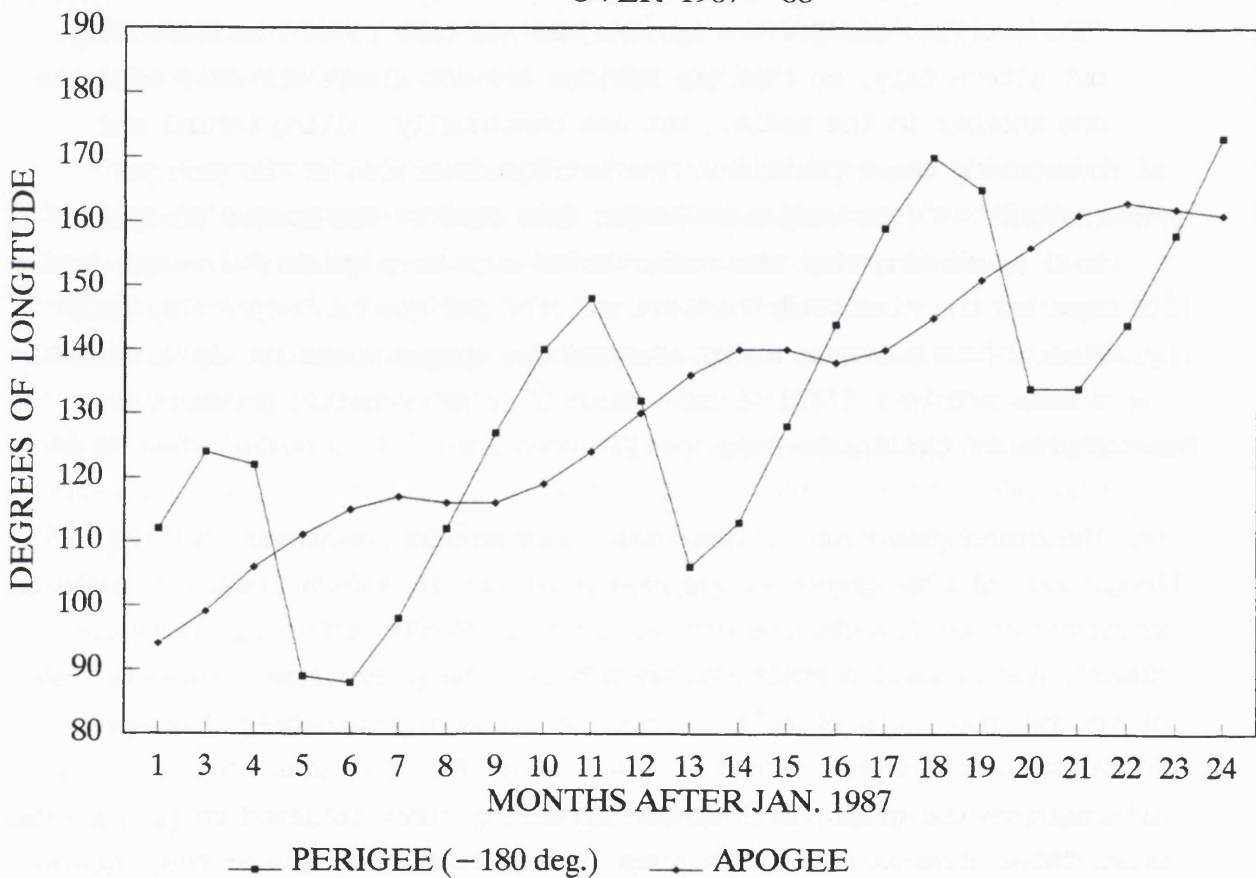


Figure 2.1: Apogee and Perigee Motions on a  $180^\circ$  scale of ecliptic longitude, illustrating their coincidence on an 'apse line', and the greater motion of perigee as compared to apogee. Programs do not give apogee and perigee positions, here reconstructed from times supplied by Meeus (1983).

The graph shows how the secondary motion of perigee is much larger than apogee.  $180^\circ$  has been subtracted from the perigee positions to align them

with apogee. Their mean motion can be seen as some  $80^\circ$  over two years. This is the 'mean apse' motion, of  $3^\circ$  per month or one revolution in nine years.

We can now accept Newton's account as given in PNP, if we just substitute the word 'perigee' instead of 'apogee':

'...the moon's apogee goes forwards at the greatest rate when it is either in conjunction with or in opposition to the sun, but in its quadratures with the sun it goes backwards' (p.475)

In figure (1), the maximal forward motion of perigee corresponds to the alignment of the Sun with the mean apse line (ie, the Sun in conjunction or opposition to apogee), whereas its retrograde motion becomes maximal at the quadratures. The converse applies for apogee.

Historians of astronomy tend only to discuss the mean apogee motion, of  $3^\circ$  per lunar month, and the historical problem of accounting for this motion by a gravity theory. They seldom acknowledge that the apse line really does have this rather interesting secondary motion, discussing the Horrocksian model as if it were merely a reformulation of the antique concept of 'evection'. Rather, this motion was a fine British discovery by the young North-country clergyman Horrocks, and it formed the core of what Newton recognised as the best lunar model available in the seventeenth century. Corollary 7 to PNP's Proposition 46 of Book I (Motte translation p.178) claims to deduce this oscillating motion from the theory of gravity, and one would like to have an expert opinion upon the cogency of its argument.

#### **IV An Altering Eccentricity**

Since Hipparchus, eccentricity had meant the Earth's distance from the centre of a circle, to which the lunar orbit approximated, as a fraction of the radius of that circle. In all the diagrams in the *Principia*, in those of Horrox and Flamsteed, in Whiston and Gregory in the eighteenth century, the lunar orbit appears as a circle. Did eccentricity still mean Earth's displacement from such a centre? Historians assume that these writers were dealing with an ellipse of varying eccentricity, as we nowadays define the term.

The modern value of lunar eccentricity is 0.05490, however it does not alter in modern theories. Gaythorpe (1925) explained how the modern evection terms were mathematically equivalent to Horrocks's varying eccentricity. TMM's value ranged between 66782 and 43319 parts per million, which is a mean of  $\pm 0.055050$ , varying by 21.31%. The earliest recognisably modern definition of eccentricity that I have come across appears in a glossary of astronomical terms by Leadbetter, wherein it was defined as follows:

'Eccentricity is the distance between the center of the ellipse and the focus.'  
(1742, Vol.II)

The classical notion of eccentricity signified the Earth's displacement from the centre of a circle. If A and P are then the apogee and perigee distances, expressed as  $R+x$  and  $R-x$  where R is the radius; then the eccentricity will be  $x/R$  or  $(A-P)/2R$ . This is equivalent to the modern ellipse-based definition if the circle in question has its diameter equal to the long axis of the ellipse. Measuring A and P in Earth semidiameters, R is 60.2. The computer was set to generate successive lunar distances at mean apogee and perigee positions and thereby obtained this function at monthly intervals. It is shown in Figure 2.2, with TMM's mean value inserted for comparison. It varies by somewhat less than 20%, but may serve to indicate how British astronomers of the Restoration viewed it as fluctuating. Figure 11.1 diagrams this fluctuation.

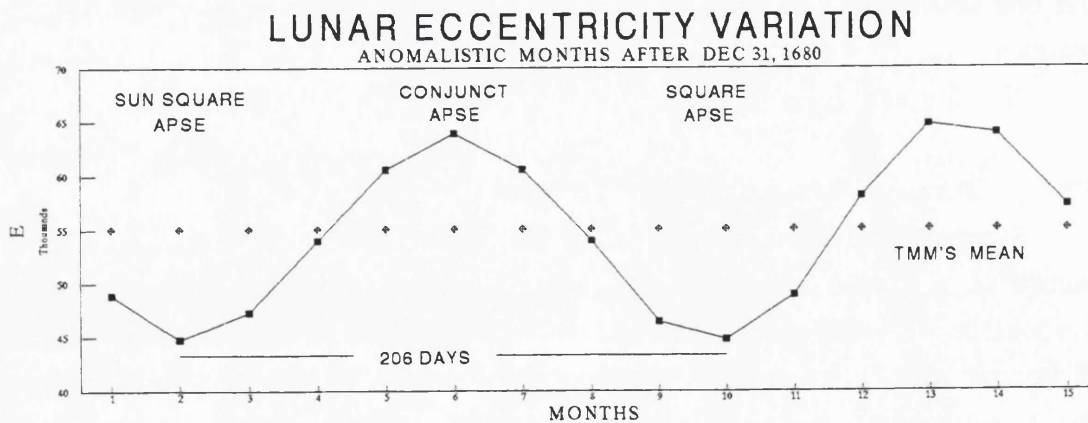


Figure 2.2: discrete monthly values of a simulated eccentricity value,  $(A-P)/2R$ , where A and P are mean apogee and perigee values in Earth semidiameters and R is 60.2, for the months of 1680 and 1681. Its mean value was 0.0543, fluctuating between 0.0639 and 0.0448.

The long axis of the ellipse (A+P) was found to vary by only 1% over this period, indicating that the elliptical *shape* of the orbit hardly altered. It is evident that William Whewell's interpretation of the Horrocksian model went somewhat astray:

'That the Inequality of the Eccentricity of the Lunar Orbit, which is greatest when the Line of the *Apsides* falls in the Conjunction or Opposition, and is then one and a half of what it is in the Quadratures; which consequently renders the Ellipsis perpetually mutable, sometimes coming nearer to a Circle, sometimes a great deal more remote from it, so as not to be reduc'd to any certain Species, and which is scarcely to be accurately defined...

(*Astronomical Lectures*, 1728, p.130)

While we have been able to discern something resembling his 50% alteration in eccentricity, this does not imply a corresponding alteration in the orbit shape, which would indeed be bewildering.

I could not generate values for the modern definition of 'e' in a like manner, owing to the absence of an iterative procedure for locating the minor axis of the ellipse: the 90° angle between Moon and mean apse is not halfway in time between apogee and perigee positions.

While the fluctuation in eccentricity every 206 days has similar periodicity to that of the apse line, of two cycles per Horrocksian year, the two are 90° out of phase: eccentricity reaches its maximum as the Sun aligns with the mean apse, whereas the perigee position is moving most quickly then, its rate of change in ecliptic longitude being maximal. Chapter Seven will observe how the model of Jeremiah Horrocks accounted for these two interlinked motions.

A comparable function over the Horroxian year was assigned to the Moon's varying speed, phased to the apse line's conjunction with the Sun. It was again a second harmonic (ie, a 2θ function), zero when the Sun was conjunct the apse line or at quadrature to it, and maximal at the octant positions. Newton also calls this equation 'semiannual', giving it the magnitude 3'45" (TMM, p.15; PNPM, p.474).

Likewise for the nodal axis, when the Sun is conjunct or in quadrature to it, which he calls the 'second semestrial equation,' again maximal at octants, of amplitude 47". This will be slightly shorter than the Horroxian year (TMM, p.17; PNPM, p.475). I have not looked at these.

The Anomalistic year is virtually the same as the Tropical year, as Earth's aphelion hardly moves. It appears in the annual equation (See next chapter). PNPM claimed that the apogee and nodes moved faster at perihelion than at aphelion (BkIII, Prop.35, Scholium) than at the aphelion. In other words, these two axes revolved faster in January, slower in July. Three motions were assigned to the apse line: the first being its rotation every nine years, the second that of Horrocks, viz its oscillation every 205 days with twelve degrees amplitude, and now a third of annual period and amplitude 19' 43" (PNPM, p.474).

### Ch. 3 SOME PERSPECTIVES ON TMM

#### I From Minutes to Seconds

TMM cited its longitudes to seconds of arc. Did astronomers of that time really enjoy such accuracy? Curtis Wilson has ascertained that the ephemeris of Thomas Streete was the most exact of any available in the seventeenth-century (GHA, p.180). Flamsteed wrote in 1669 that 'I esteem Mr Streete's numbers the exactest of any extant' (GHA, p.179). To give some idea of his data accuracy, let us consider a total solar eclipse which Streete cited in his *Astronomia Carolina* of 1661. This was visible in London, on May 22nd 1639. Streete gave the 'Apparent Time' of its end as 6 hours, 10 minutes, 27 seconds, from which he derived what he called its 'Equal time' of six hours, zero minutes, 27 seconds. That is to say, Streete's 'equation of time' was ten minutes, by subtracting which he converted to mean solar time. As the computer shows, his longitudes for that moment were well within half a minute, if not justifying their citation to the nearest second:

<u>Streete's positions</u>	<u>actual</u>	<u>differences</u>
Sun: 10° 49' 28" Gemini	10° 49' 53" Gemini	- 25"
Moon: 11° 58' 26" Gemini	11° 58' 0" Gemini	+ 26"

For readings taken in 1639, they are quite impressive.

Moving on to the 1690s, research conducted by the present writer in collaboration with Bernard Yallop at the RGO (unpublished), has indicated that the stellar observations tabulated in Flamsteed's *Historia* tended to be within five seconds of arc or so accuracy. For example, on March 8th 1695, the star Aldebaran was cited as having a zenith distance of 15° 51' 24", in the *Historia Coelestis'* Volume II. Subtracting that from 51° 28' 10", which was Flamsteed's value for the latitude of Greenwich, then subtracting out the appropriate value for refraction corresponding to that altitude as given by modern tables, gave 35° 36' 18" declination. The computer determines the correct declination for Aldebaran at that time as having been 35° 36' 15", a difference of three seconds. Likewise for the star Spica on April 12th, 1698, the error was 5 seconds of arc. These

vertical star readings are relatively simple to compare: there is no parallax correction as is needed for the Moon, nor any equation of time as is needed for right ascension, where accuracy in timing to minutes or even seconds is vital.

Allan Chapman has conservatively estimated the accuracy of the original Mural Arc at Greenwich at 12" (Chapman 1982 p.6), but as this historic instrument was lost on Flamsteed's death (Possibly a consequence of Halley filing a lawsuit against Flamsteed's widow, claiming the equipment as his own, on the grounds that he was the new Astronomer Royal. He lost the case, she lost the instruments), this estimate was an inference, merely based on comparable instruments of the time.

Lunar readings on that Mural Arc could not aspire to quite such exactitude as the stellar positions. Taking a vertical reading as the lunar limb touched the central filament of the telescope's eyepiece was a less accurate affair. By the time the data had been tabulated and had certain astronomical adjustments applied, the errors would be greater. We found that, for a batch of 16 positions sent to Newton in Flamsteed's letter of February 7th, 1695 (reproduced in *Correspondence*, IV, p.84) the mean error in longitude was  $0.4 \pm 1.2$  minutes of arc. That was after Flamsteed had applied various corrections and converted from equatorial co-ordinates (declination, right ascension) to ecliptic (longitude, latitude). The issue of how lunar data was reduced into a form suitable for theoretical use will be reviewed later. Here we are merely concerned to make some preliminary comments about data accuracy.

## II Conditions of Composition

The conditions under which TMM was composed have a couple of rather strange, indeed startling, features. The original manuscript (kept at Cambridge University Library, Add. 3966), was composed on February 27, 1700. Its date of composition comes from David Gregory, a reliable source because of the reverence with which he recorded matters Newtonian. His copy is in the library of the Royal Society, with the composition date marked. We have seen how the epochs of TMM are December 31st 1680 and 1700, the two

limits over which various celestial positions were given. The positions for the latter date were therefore predictions. They had not then been reached. It appears that, when the reference date of December 31st was reached, the positions there given were not checked or amended in any way prior to publication in 1702. Perhaps they did not need any amendment.

In the very month of TMM's composition, Newton was confirmed as the Master of the Mint. On the third of February, a royal edict proclaimed:

'Know yee that wee for divers good causes...do give and grant unto Our trusty and Well beloved Subject Isaac Newton Esqr. the office of Master and Worker of all our Moneys both Gold and Silver within our Mint in our Tower of London and elsewhere in our Kingdom of England...And know yee that wee for the considerations aforesaid have given and granted, and by these presents do give and grant unto the said Isaac Newton all edifices, buildings, Gardens, and other fees, allowances, profitts, privileges, franchises and immunities belonging to the aforesaid Office...'

It cannot but strike us as rather extraordinary, that within weeks of acquiring such a responsible position, one of the most demanding jobs in the country, Newton should find time to ponder the niceties of lunar motion, and compose a brief but obscure opus on the subject. Not long after, Newton would have to ready himself to stand for the Trial of the Pyx, whereby the quality of the gold of the nation's currency was tested and to which the Master of the Mint was personally answerable for deficiencies. Not less than two thousand pounds was expected to be submitted by the Master of the Mint in advance as a security for the operation. His full attention was expected over the problems of bimetallism, whereby the differing values of gold and silver defined the relative weights of the currencies cast in them. In these years he still retained his position - and income - as a Fellow and professor of Trinity College, Cambridge.

The twenty-year period specified by the epochs of TMM was a multiple of four, whereby the leap years would fit in and not disrupt the flow of



computation, and was the smallest such multiple to embrace a Saros and nodal cycle, each of eighteen years. But, in addition, these two decades had a very personal significance for Newton. Without wishing to over-generalise, they framed the main period of Newton's creative life in relation to astronomy. In 1680 there arrived the great comet which the Trinity lecturer sat up observing, followed in 1682 by what was later recognised as Halley's comet. His composition of TMM in 1700 appears as the grand finale of that output. There is no real evidence of his further studies of the matter after this date (Baily, p.706). His vast ruminations on the cosmic process were framed by these two decades.

TMM's date of composition being controversial, there are three further occasions when evidence relevant to this is treated: Chapter Four, Section III comments on TMM mean motions found in a separate document; Chapter Seven, Section III comments upon apse equation values and Chapter Nine, Section VIII, evaluates an alleged early draft of TMM.

### III Halley's Hope

TMM was published by David Gregory, formerly professor of Mathematics at Edinburgh University, who in 1702 became Savilian Professor of Astronomy at Oxford University. The title of Gregory's textbook, in which TMM was included, was (in English translation): *The Elements of Physical and Geometrical Astronomy*. The claim to have established a 'physical' astronomy echoes that made earlier by Kepler, at the front of his *Astronomia Nova*. Introducing TMM, Gregory dismissed previous endeavours in this area for their lack of a physical basis:

'But as they made their Tables not from known Physical Causes and their Periods, but only by attending to Observations, it is no wonder if they did not rightly distinguish the Inequalities from one another...' (Gregory 1715, p.132)

This alludes to the large question of the extent to which TMM was based upon 'physical causes', when nothing in its text indicated such. We will not now return to this issue. But, what did Gregory mean by claiming that his astronomy was also 'geometrical'?

In a sense, 'geometrical' merely signified, 'perfect,' alluding to the exact nature of geometrical proofs, as free from approximations. It echoed ancient Platonic notions about astronomy with which his readers would have been familiar. Later in the century it would become evident that, for matters involving time-dependent variables, geometry was not so suitable. Fluxional and differential methods were then just beginning to be adopted by mathematicians, and half a century later would become the new format for expressing these things.

A comment by Edmond Halley, made while discussing the *Principia's* lunar section in its first edition, is worth quoting in this context:

'And tho' by reason of the great Complication of the Problem, he has not been able to make it purely Geometrical, tis to be hoped, that in some further Essay, he may surmount the difficulty\*.'

If it strikes us as curious today, it is because we view progress in this area as having taken place through the discarding of geometrical methods, and their replacement by algebraic functions. Our ability to believe that a historical figure was applying a theory of gravitation to deduce or obtain results, is likely to depend upon their having progressed in some degree beyond a merely kinematic or geometrical mode of reasoning. However, TMM in 1702 developed a geometrical mode of reasoning, just as Halley had hoped.

Gregory extolled the accuracy of TMM highly in an introductory paragraph, though it was a thing he had no means of assessing:

'By this theory, what by all Astronomers was thought most difficult and almost impossible to be done, the Excellent Mr Newton hath now effected, viz. to determine the Moon's Place even in her quadratures, and all other Parts of her Orbit, besides the Syzygies, so accurately by calculation, that the Difference between that and her true Place in the Heavens shall scarce be two Minutes, and is usually so small, that it may well enough be reckoned only as a Defect in Observation.'

.....

\* 'A True Theory of the Tides', *Phil. Trans* of 1695, (19, pp.445-457). Allegedly this was destined for James II as part of Halley's presentation of the PNEM to the King.

Gregory has here made the bold claim that the theory developed so far that its predictions were only limited by the accuracy of the data on which it was based. It was what would nowadays be called a sales blurb. Gregory was a theoretical astronomer. Flamsteed referred scathingly to him as a 'closet astronomer' because he did no practical work (Baily, p.204). We will soon see how Flamsteed's opinion, at least over this specific issue, was quite justified.

On the other hand, Gregory's judgement was largely endorsed, years later, by no less a person than the Astronomer Royal. Edmond Halley, when after 1719 he assumed that post, did have the opportunity to check TMM against accurate data. His opinion, which we have already in part quoted, shows the strongly politicised nature of the discussion, which seems to have continued ever since. Halley found that 'for whole months together' TMM was:

'...rarely differing two minutes of Motion from the Observations themselves; nor is it unlikely but good part of that Difference may have ben the Fault of the Observer. And where the Errors were greater, it was in those parts of the lunar orb where Mr Flamsteed had very rarely given himself the Trouble of observing: viz, in the 3rd and 4th quarter of the Moon's Age, where sometimes these differences would amount to at least 5 minutes.' (*Phil. Trans*, 37, p.191)

My investigations have not confirmed either that errors of such magnitude were present in the lunar observations of Flamsteed, or that the data came mainly for the waxing half of the lunar orbit tending to omit the last two quarters. A later section will assess the question of data accuracy and reliability. It becomes a rather central issue, if both Whiston and Halley are claiming that the performance of TMM was limited primarily by the data on which it was based.

These days, the pendulum has swung in the opposite direction. Curtis Wilson boldly described Newton's great lunar endeavour as a 'failure' (1987, p.76), and the reference cited for that claim was the Whiteside tercentenary essay. There is room for doubt as to whether Whiteside adopted quite so extreme a position. It is worth quoting the conclusion of

D.T.Whiteside's tercentenary address, for this study has formed the starting-point of modern discussions of the topic:

'It is, unfortunately, one of the most tenacious myths of Newtonian hagiography that this demi-god of our scientific past made his dynamical explanation of the moon's motion in all its irregularity the supreme proof of his monolithic principle of the universal inverse-square law of gravitation which governs all celestial and terrestrial movement, and this in a surpassingly rigorous geometrical manner which he made inimitably his own. "Who", to quote Whewell's eulogistic phrase of a century and a half ago, "has presented in his beautiful geometry, or deduced from his simple principles, any of the [lunar] inequalities which he left untouched?" The truth, as I have tried to sketch it here, is rather that his loosely approximate and but shadowily justified way of deriving those inequalities which he did deduce was a retrogressive step back to an earlier kinematic tradition which he had once hoped to transcend, and to a limited Horrocksian model which was not even his own invention' (1976, p.324).

More recently, Wilson concluded a fine study of the matter by saying, that the Newtonian lunar endeavour had come unstuck because:

'Newton's effective adoption of Horrocks's lunar theory, by interfering with ongoing insight into perturbations not actually embraced by that theory, proved ultimately an insurmountable obstacle to him' (GHA, p.267).

That is a novel interpretation of the failure, if indeed we should regard it as such. A great British discovery, which formed the backbone of the finest lunar theory available (that of Flamsteed, the Astronomer Royal), is blamed for having prevented a mathematician from having been more successful, by virtue of his adopting it. Perturbation theory is something one thinks of as developing in the middle of the eighteenth century, and in France. We are merely pointing out that a problem seems to exist, in deciding whether or not an enterprise was a success or a failure, and if the latter, on what that should be blamed.

In a sense such verdicts must be conjectural. Until we know how TMM functions, as an integral whole, it must remain so. Until then, we can only

quote the radically opposed views of Halley and Flamsteed (for example), and perhaps side with one or other. Here we shall aspire to reach beyond such an armchair approach, and resolve centuries-old controversies in a practical manner. TMM is like a machine, a watch, which once wound up and set in motion will generate positions for the luminaries. It will do this, provided only that we can follow its instructions. We here aim to set its antique wheels in motion, to see how they move one against the other, thereby to gain insight into what has long been an obscure and neglected area in the history of science.

#### IV Moving the Goalposts

After the cognoscenti had been nodding their heads over these matters for three years, and rumours put about that such profound accuracy had now been achieved that Flamsteed need not bother any more in gathering lunar observations, for the job was done (Baily, p.176); Newton then submitted some 'Corrections' applying to TMM, one of which shifted the position of its 'mean moon' by ten minutes. He thereby displaced the values which TMM would generate by five times more than its supposed maximum error, as affirmed by Gregory. The concept of a 'mean moon' and of this adjustment will be elucidated further in a later chapter, but suffice to say that it is the fundamental starting-point for a lunar theory. It is hardly adequate to characterise such an alteration as an 'correction.' (Cohen, p.87. The Corrections appeared in *Miscellanea Curiosa* of 1705, published by the Royal Society.)

The year before these 'corrections' appeared, Flamsteed described how the Royal Society's President paid him a visit at Greenwich. He was shown some early lunar positions of the 1670s and 80s, and their disagreements of up to ten minutes with TMM:

'I showed him also my new lunar numbers, fitted to his corrections; and how much they erred: at which he seemed surprised, and said "It could not be." But, when he found that the errors of the tables were in observations made in 1675, 1676, and 1677, he laid hold on the time, and confessed he had not looked so far back: whereas, if his

deductions from the laws of gravitation were just, they would agree equally in all times.' (Baily, p.217)

This was in a letter to Abraham Sharp. Sharp was a mathematics teacher with whom Halley corresponded over a formula he developed for the convergence of  $\pi$ , who built the Mural Arc in 1690 that was much admired, and who stuck the stars onto Flamsteed's maps of the constellations. He is thus a fairly significant witness to the course of events. Flamsteed may have been by this time (1704) rather embittered by certain aspects of Newton's behaviour, but that is not a reason for dismissing or marginalising his opinions, as happens in some histories of these events. If perchance we are able to make TMM function, we should be able to test his view, that it works better for the 1690s than for the 1670s, as it was designed to fit data over the former period.

We can only speculate. Perhaps the errors shown on this visit impressed the President, and led him to decide that a slip-up had occurred. A subsequent chapter will ascertain which of these mean positions was the more correct.

## **V A Modern Approach**

We aspire to follow the path of a new generation of historians of astronomy, pioneered in America by Owen Gingerich and Curtis Wilson. They have used computers to probe into the past to ascertain how accurate were the endeavours of any astronomer in history. Thereby they have given a greater emphasis on the practical side of astronomy in a historical context, which was much needed. Centuries-old discussions are now resolvable, and a precise new basis can be given to the history of astronomy. Little has been done in Britain along these lines, to-date.

Owen Gingerich (at the Harvard-Smithsonian Center for Astrophysics) studied 'error patterns' in ephemerides, by comparing their predictions against actual positions over the years. This showed the extent to which the theories of astronomers were succeeding in practice. If we knew of a lunar almanac which had used TMM, we could assess its accuracy simply by following this approach. Curtis Wilson has probed into the specific



## Ch. 4 A COMMENTARY ON TMM

Our approach here is complementary to that of Bernard Cohen's excellent 1975 treatise. Cohen there discussed the circumstances of TMM's production, its various editions in Latin and English, the comments made upon TMM by astronomers, and why science historians had largely ignored the subject. We on the other hand are concerned with the argument of this document. This has scarcely (we believe) hitherto been attempted. This section will deal only with its first seven paragraphs. To each paragraph of TMM we assign a roman numeral. The reader may wish to refer back to page 6, where some preliminary comments were made, and also to values of some Newtonian constants in the Appendix.

I 'The Royal Observatory at Greenwich is to the West of the Meridian of Paris  $2^{\circ} 19'$ . Of Uraniborgh  $12^{\circ} 51' 30''$ . And of Gedanum  $18^{\circ} 48''$ .

The Paris meridian is  $2^{\circ} 20'$  East. In those days there was no general agreement on the 'Greenwich Meridian', so the distances between observatories in longitude was vital for comparing observations. For once-glorious Uraniborg, then fallen into rack and ruin yet still important for astronomers as the site where 'The prince of astronomers' as Flamsteed called him, Tycho Brahe, had once worked, the true longitude is  $12^{\circ} 27'.0$  East. TMM's value for its distance in longitude erred by 24 minutes! This would have introduced an error of  $1\frac{1}{2}$  minutes of time into any data that was being transcribed, from Uraniborg time to Greenwich time.

'Gedanum' referred to the observatory of Danzig (now Gdansk), where Hevelius worked. This would have grown into the most illustrious observatory in Europe, had it not tragically burnt to the ground in 1679. Flamsteed compared many of his observations with those of Hevelius, and was startled to find them agree to within a fraction of a minute in many cases, even though Hevelius used only his own eyesight unassisted by the new telescope plus micrometer-gauge. With such close agreement, the correct time-correction would have been a vital matter. Its correct longitude is  $18^{\circ} 24'.6$ , so TMM's position was again in excess, by  $23\frac{1}{2}$  minutes of arc.

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II        'The mean Motions of the Sun and Moon, accounted from the Vernal Equinox at the Meridian of Greenwich, I make to be as followeth. The last Day of December 1680 at Noon (Old Stile) the mean motion of the Sun was 9 <sup>sign</sup> 20° 34' 46". Of the Sun's Apogaeum was 3 <sup>sign</sup> 7° 23' 30"'. .

'Motions' signified 'positions' at specified epoch times, measured in ecliptic longitude, and cited for noon as the time of day for which an ephemeris had to define positions. The zodiac begins from 0° Aries at the Vernal Point, so '9 sign' meant the nine zodiac signs on from that position on the ecliptic, viz the sign of Capricorn (A modern astronomer would cite such a longitude as  $9 \times 30 + 20^\circ$  or  $290^\circ$ ). In the next section we consider how accurate was the Newtonian value for the Sun's mean position. Gingerich found that solar errors in ephemerides of this period were not more than several minutes of arc (1983 p.xix). Solar positions were straightforward to calculate, depending merely upon the eccentricity value used for the Earth's orbit.

The 'Sun's apogaeum' referred to the Earth's aphelion, its position of furthest distance at midsummer from the Sun. This was a remnant in terminology from the old geocentric terminology, whereby the Sun circled the Earth. The more or less fixed position of the aphelion is here specified to an accuracy of four minutes.

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III        'The mean Motion of the Moon at that time was 6 <sup>sign</sup> 1° 35' 45". And of her Apogee 8 <sup>sign</sup> 4° 28' 5". Of the Ascending Node of the Moon's Orbit 5 <sup>sign</sup> 24° 14' 35"'. .

'Motion' refers to position in ecliptic longitude. The node position has an accuracy of two minutes (p.6). As regards the accuracy which Newton hoped his mean moon position to have, we may quote from a letter of his to Flamsteed of January 15, 1695 - the period of early optimism:

'In trying to compute the mean motion of the moon from the *tempus apparens* in some of your observations, I find that the mean motion, gathered by my computations, differs sometimes from that in your

synopses 5" or 6", or above. Which makes me suspect that, in determining the *tempus apparens*, your servant followed some tables which are not sufficiently exact...'

Years later, some 'corrections' were specified for TMM, (Cohen, p.87) one of which was a ten minute displacement of the mean moon, from 1° 35' 45", to 1° 45' 45". The new value was used in Whiston's reprinting of TMM in 1707. It does better relate to the final lunar position given for 1700, in terms of the mean tropical lunar period which links them together.

Five seconds or ten minutes? Years later Flamsteed concluded that 1'30" should be added onto the Newtonian value for mean moon (August 31, 1714, Baily p.698). Chapter Five will ascertain to what extent he was correct on this matter.

Lunar apogee here refers to a notional mean apse line, ie one having uniform motion in longitude, and not the actual position of apogee (pp.11,12). This apse line position is cited as being 244° 28' 5". Its true position was about three minutes more than this, which was quite accurate for the period. The mean apse line was the foundation for lunar theories.

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IV 'And on the last Day of December 1700 at Noon, the mean Motion of the Sun was 9 sign 20° 43' 50". Of the Sun's Apogee 3 sign 7° 44' 30". The mean Motion of the Moon was 10 sign 15° 19' 50". Of the Moon's Apogee 11 sign 8° 18' 20". And of her ascending Node 4 sign 27° 24' 20". For in 20 Julian Years or 7305 Days, the Sun's Motion is 20 revolut. 0 sign 0° 9' 4". And the Motion of the Sun's apogee 21' 0".'

These positions are intended to be mechanically linked to the previous ones of 20 Julian years earlier, through their mean periods. The Sun's position in longitude has moved on by 9 minutes, as the Julian year of 365.25 days is not quite the same as the Tropical year and so it generates that much

.....  
\* These dates are January 11th and 12th New Style. To derive mean motions, the tropical year of 365.242 days and the tropical month of 27.321 days were used.

displacement in 20 years. Britain was at this time refusing to abandon the old Julian calendar, as most of Europe had a century earlier, for religious reasons.

Two months after TMM's composition, in April of 1700, we find the Master of the Mint again musing upon 'Elementa motuum Solis et Lunae ab Aequinoctio verno'. He gave some more mean positions to the nearest second, for January 1 1701, Old Style (Corr. IV, p.328). The computer gives us (after converting Old Style to New) the following comparison:

Mean Sun	21° 42' 38" Capricorn	true position	22° 12' Capricorn
Mean Moon	28° 30' 12" Aquarius	true position	28° 53' Aquarius.

This data clearly supports the date given by Gregory of early 1700 for TMM's composition. Normally, one would not require confirmation of so reliable a source; however, it was as we have seen a rather extraordinary period in life to choose an attempt to fathom this highly inscrutable issue, and we may be grateful for supporting evidence over its date of composition.

A mean sun moves 59' 8" per day, every day, thereby going round the 360° of the ecliptic in 365.24 days. For a mean sun, one adds on this amount to move from the sun's position at noon on December 31, 1700, to its position the next day January 1st, 1701. Let us see whether this has been done\*.

Solar motion, from noon December 31 1700- January 1st 1701

as given by Newton:	58' 48"
by mean sun:	59' 8"
actual motion:	1° 2'

Lunar motion, from noon December 31 1700 - January 1st 1701

as given by Newton:	13° 10' 22"
by a mean Moon:	13° 10' 34"
actual motion:	12° 54'

This confirms unequivocally that mean solar and lunar motions were being used. The discrepancies then become merely 20 and 14 seconds respectively, far less than the divergences from actual positions of 3 and 16 minutes.

The figure given for the motion of the Earth's apse line is of interest, as the motion or 'quiescence' as PNP put it of the planetary apses was controversial. Vincent Wing in his *Urania Practica* (1649) gave it a motion of 1'01" per annum, somewhat less than TMM's value of 21' in 20 years. Streete in his *Astronomia Carolina* said the motion of planetary apses was immobile with respect to the stars, ie that their motion in the tropical reference was identical with the precession value. Flamsteed in the Preface to his *Historia Coelestis* suggested 1' 3" as its annual motion (p.147) which is identical to the TMM value. Sidereally, the apse moves 11".8 per annum, and adding the Vernal Point's motion of 50".2 gives its tropical motion of 1'2" per annum.

William Whiston, in his astronomical lectures published in 1710, expressed surprise at TMM's putting the Earth's apse in motion. The notion that the planetary apses moved had been 'exploded' out of astronomy, he remarked (Cohen, p.149), and so why were they here brought back again?

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V 'The Motion of the Moon in the same Time is 247 Rev. 4 sign 13° 34' 5". And the Motion of the Lunar Apogee is 2 Revol 3 sign 3° 50' 15". And the Motion of her Node 1 revol. 0 sign 26° 50' 16".

The length of the tropical lunar month is here indicated. A figure of 247 revolutions was accidentally given, in this paragraph and the next, which was corrected in 1705 (Cohen p.87) to read 267. A most exact value then emerges. Dividing 20 Julian years by the number of revolutions here specified gives a mean period of the tropical lunar month within a fraction of a second\*. It must have been the most accurately known physical constant

.....  
\* Dividing 7305 days by 267.3710 gives 27<sup>d</sup>, 7<sup>h</sup>, 43<sup>m</sup>, 4<sup>s</sup>.9.  
The correct value is 27<sup>d</sup>, 7<sup>h</sup>, 43<sup>m</sup>, 4<sup>s</sup>.7.

at that period. Though PNPB only cited the sidereal lunar month within the nearest minute, TMM used its mean duration to a second.

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VI 'All which Motions are accounted from the Vernal Equinox: Wherefore if from them there be subtracted the Recession of Motion of the Equinoctial Point *in antecedentia* during that space, which is 16' 0", there will remain the Motions in reference to the Fixt Stars in 20 Julian Years; viz. the Sun's 19 revol. 11 sign 29° 52' 24". Of his apogee 4' 20". And the Moon's 247 revol 4 sign 13° 17' 25". Of her Apogee 2 revol 3 sign. 3° 33' 35". And of the Node of the Moon 1 revol 0 sign 27° 6' 55".'

A conversion from tropical to sidereal space here occurs. The reference framework becomes that of the fixed stars, no longer a moving zodiac system anchored to the Vernal Point. This move has theological implications, because sidereal space was the sensorium of the Deity for Newton, and the ascent into that reference framework, where the centre of mass of the solar system was immovable, away from the merely human perspective on things, was for him a religious exercise, or so he declared at the start of the PNPB (Cajori Edn. 1960, Vol.1, p.12). For now we merely note that the 30° signs here referred to are Sidereal, that is pertaining to that zodiac system invented by the Chaldeans and defined by fixed stars. TMM's instructions pertain to two different zodiac systems.

Against this immobile sidereal space, the monthly orbit of the Moon, used with such remarkable succes in PNPB to show that gravity reached as far as the lunar sphere, is here ascertained to a few parts in ten million.

Astronomers required the ability to make such a conversion, from tropical to sidereal longitude, as the positional data could well be given with respect to a fixed star. There was however no generally accepted sidereal reference framework. An inaccurate value for precession is here given, of 16' 0" in 20 Julian years, corrected in 1705 to 16' 40", which is one degree in 72 years.

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VII According to this Computation the Tropical Year is 365 days 5 hours 48' 57". And the sydereall Year is 365 days, 6 hours, 9' 14".

This concludes the dualistic system introduced in the previous paragraph, whereby two different reference frameworks are introduced, enabling the reader to switch over to the sidereal, and back to Tropical. The values are more accurate than those cited in *Streete's Astronomia Carolina* of 1661. The period of the sidereal year is given correct to five seconds, and that of the tropical year to ten.

## Ch. 5

### MEAN MOTIONS OF THE SUN AND MOON

To construct a lunar ephemeris, five different mean motions were required: of the Sun, Moon, apogee, aphelion and lunar node. As the aphelion was almost stationary, or only moved a degree or so per century, there were to all intents and purposes four positions which had to be located on the zodiac for any given time, as one's starting-point. Their accuracy could easily limit the accuracy that an ephemeris achieved: if a mean moon was out by several minutes, its predicted positions would err by that amount on average. How accurate were the mean motions of TMM, and were they better or worse than others of the period? This chapter will attempt to resolve these matters, with the aid of modern equations and a 16-megahertz microchip.

A comparison of mean motions gives a good criterion for ascertaining to what extent the tables used by ephemeris-makers were 'Newtonian' or not. Numerous tables in the first half of the eighteenth century claimed to be so, and William Whewell averred in 1837 that TMM was:

'...for a long period the basis of new Tables of the Moon, which were published by various persons; as by De L'Isle in 1715 or 1716, Grammatici at Ingoldstadt in 1726, Wright in 1732, Angelo Capelli at Venice in 1733, Dunthorne at Cambridge in 1739' (*History of the Inductive Sciences*, II, p.209).

The issue was discussed by Baily (1835, pp.701-705), and more recently by Craig Waff (Cohen, 1975) and Curtis Wilson (GHA, pp.267-8). Newton gave four successive versions of his mean motions, thrice modifying that given in TMM of 1702, so it is vital to determine which of these a 'Newtonian' ephemeris adopted.

Our concern is not with the ephemerides as such, which were daily tables of positions of the heavenly bodies, but with the mean motion tables employed to construct them. The French *Connaissance des Temps* which ran from 1678 onwards was a fine example of the former, probably used by Halley in his South-Atlantic voyages.

## I A Modern Definition

Mean motion refers to a concept of uniform angular velocity, without periodic terms. As such it is an average path of motion through time, measured in ecliptic longitude. It is nowadays computed in Julian time, that is by the number of Julian days from a given epoch. It is a function of  $T$ , which is time measured in Julian centuries from 1900. Its computation requires three terms: a constant representing the starting point, plus a  $T$  and a  $T^2$  term (Higher terms vary by less than an arcsecond).

Initially, one had supposed that long-period periodic terms should be incorporated, of amplitude around ten arcseconds. However, experts consulted were of the view that a historical conception of mean motion should not take account of any periodic terms. They are not used in the mean motions of the modern theories of Meeus and Chapront-Touzé, but were in the older theories of Brown and Newcomb. It would be an option to include them here, and would displace the error-values estimated in this chapter by the above amount.

It is remarkable that the computations here performed would hardly have been valid if attempted any earlier than their time of composition, namely 1992. The new copy of Dr Meeus' book, *Astronomical Algorithms*, (Willman-Bell, 1991) incorporates the improved parameters of Michel Chapront-Touze and Jean Chapront (1988), resulting from high-precision dynamical studies of earth-rotation, and has thereby improved the secular variation terms for the five variables that concern us.

In consequence, disagreements now exist between modern equations for mean motion. A cross-channel divergence of opinion continues to exist, where it seems likely that the French tables are to be preferred: the new issue of the *Explanatory Supplement for the Astronomical Ephemeris* (1992) is by the same US publisher as the Meeus book, Willman-Bell, but has slightly diverging mean motions. Its tables have not been revised since the earlier edition. The divergence is somewhat larger than the divergence of the historical tables from the Meeus values, especially for the Sun (See Appendix II). The previous chapter used the U.K. equations for assessing TMM's accuracy, and for historic interest it is left unchanged. However,



thanks to the work of the Chapront-Touze's, we are for the first time able to go back into past history, with terms probably accurate enough to assess the accuracy of astronomical mean tables for centuries gone by.

In the case of the Sun's motion, an accuracy of an arc second or two is required, while for the Moon the errors are usually measured in minutes. Only for the latter is the conversion from UT (Universal time) into TDT 'terrestrial dynamical time' (prior to 1984 this was ephemeris time, ET) relevant, due to its larger daily motion. ET was the uniform measure of time, derived 'from the uniform motions of the planets', while UT is 'defined by the rotational motion of the Earth' (Meeus), having replaced GMT in this context in the 1930s. The latter is subject to variations which are 'unexpected and unpredictable.' Here the equation was until recently expressed as:

$$\Delta T = ET - UT \dagger$$

which for historical studies we express as:

$$ET = GMT + \Delta T$$

Tables (in the *Explanatory Supplement to the Astronomical Ephemeris* 1992, pp.K8-K9) give this variable  $\Delta T$  from 1620 onwards, which may be a mere five or ten seconds of time, but for the early seventeenth century was over a minute. This normally makes a difference of a few arcseconds in lunar position.  $\Delta T$  is added on to the time function before the computation. The mean motion formulae give TDT (formerly Ephemeris Time), which for historical investigation must be translated into UT. The difference may be due to astronomical factors such as tidal friction from the Moon's pull, which affect the Earth's rotation rate.

The modern convention is to measure Julian time from noon on December 31, 2000, and not 1900 as was earlier done. Thus, for an epoch value in 1700 New Style we would substitute  $T = -3.0$  into the time-equations. Normally, however, the computations involve multiplying two ten-figure numbers together, for example Newton's epoch value of noon, December 31,

.....  
† For the development of TDT, Terrestrial Dynamical Time, out of Ephemeris Time, giving  $\Delta T = TDT - UT$ , see *The Astronomical Almanac* 1993 pp.B4-B7;.

1680 Old Style has a value of

-3.189650924 Julian centuries\*.

If only nine figures are used for this function, it may lead to erroneous seconds of arc positions†. The constant term is related to the tropical period in zodiac longitude, also specified to ten figures.

The 'Lotus 1-2-3' computer programme facilitates such comparisons, as it can reliably perform these ten-figure computations, giving the answer within a fraction of an arcsecond. The large computations are done using its 'modulus' function to give a longitude between zero and 360°, for example 'mod(730,360)' gives 10°, as the remainder after division.

By way of indicating the improvement that has come about, Appendix II shows the divergences in mean lunar motion estimates from several sources for integer Julian centuries. Meeus' 1986 textbook on positional astronomy used older formulae from the E.Brown's lunar theory and was considerably less accurate than his new algorithms (1991).

## II Newtonian Values

Ephemerides usually cited mean motions over twenty-year intervals. When an ephemeris cited a mean position for a date, say for 1701, it referred to the noon on the last day on the previous year, as remains the practice to this day. To quote John Flamsteed,

'The Radices of the mean Motions are fitted to the Meridian of London, and the Noon preceding the first of January.'

(DOS,1680, p.33)

.....  
\* The conversion equation is,

$$T = (JD - 2451545)/36525 \text{ J.cent.};$$

2451545 being the Julian date of AD 2000 epoch and 36525 the days in a Julian century.

.....  
† Assistance in transforming the epoch dates into Julian centuries was received from Mr Yallop at the Nautical Almanac Office. Further advice came from Neville Goodman, the British Astronomical Association's expert on lunar tables, concerning the need to maintain ten-figure accuracy.

For example, Whiston's *Lectures on Astronomy* (1715, 1728) contained tables with epoch values which for the year 1681 gave mean sun, moon, apse and node positions for December 31, 1680. These were identical with those specified in TMM, his being the first textbook to use them. TMM cites its positions as for noon, without specifying whether apparent or mean time is intended, and we shall assume the latter as it is normal practice for mean motions (the difference amounts to half a minute of arc). Comparing these epoch values from DOS and TMM (as corrected in 1705) with the Meeus-Chapront-Touze values:

The 'Meridian of London' we may take as five arcminutes due East of Greenwich. In the seventeenth-century, London rather than Greenwich would have been the prime meridian for British tables. The correction is small, one-third of a minute in time, equivalent to about ten arcseconds in lunar longitude.

1681 Mean Epoch Positions:

	Lunar	Apogee	Node
DOS (1681)	181° 42' 58"	244° 11' 51"	174° 14' 33"
TMM (1705)†	45' 45"	28' 05"	14' 35"
actual (for Greenwich)	<u>45' 46"</u>	<u>30' 53"</u>	<u>17' 6"</u>
TMM errors:	-01"	-02' 48"	-02' 31"

	Solar	Aphelion*
DOS (1681)	290° 34' 48"	186° 51' 40"
TMM (1705)	34' 46"	23' 30"
actual (for Greenwich)	<u>34' 51"</u>	<u>27' 24"</u>
TMM errors:	-05"	-03' 54"

A substantial improvement is generally evident over the two decades from DOS to TMM. Newton's 1680 mean happened to be within an arcsecond of .....

\* GHA gives the DOS aphelion value as 96° 50' 0" for 1679, citing its then correct value as 97° 25' 25" (p.192). As the aphelion moves one minute per year, our value is a minute in excess of this GHA value.

the correct value, which was possibly fortuitous. We may add that William Whiston took the first version of the mean motions, as given in TMM of 1702, and did not use the modified means that appeared in the PNPM's Second Edition of 1713. Naturally, as a FRS he was aware of the corrections that emerged with a 1705 edition of TMM. Others were not so fortunate: certain lunar-ephemeris constructors failed as we shall see to note this edition of *Miscellanea Curiosa*, thereby acquiring a ten minute error in mean lunar motion.

The 1701 epoch mean motions, for noon on December 31, 1700, were also modified in the *Principia's* second and third edition (Scholium, Propn. 35, Book III). Their values compared with TMM are as follows:

<u>1701 means:</u>	<u>Lunar</u>	<u>Solar</u>	<u>Apogee</u>	<u>Node</u>	<u>Perihelion</u>
TMM (1702)	15°19'50"	20°43'50"	8°18'20"	27°24'20"	7°44'30"
PNPM (1713)	15°20'00"	20°43'50"	8°18'20"	27°24'20"	7°44'30"
PNPM (1726)	<u>15 21'00"</u>	<u>20 43'40"</u>	<u>8 20'00"</u>	<u>27 24'20"</u>	<u>7 44'30"</u>
'true' means	15°20'23"	20°44'04"	8°19'49"	27°27'19"	7°48'04"

Each of them lags behind the modern value, at least prior to 1726. The node and perihelion remain unaltered, being out by two and four minutes respectively, and the apogee value is improved in the Third Edition, whereas the Sun's error has more than doubled. The Newtonian lunar mean motions contained errors in the region of half an arcminute:

Error Values for Newtonian Mean Epoch Positions

Epoch:	Lunar		Solar	Apogee	
	<u>1680</u>	<u>1700</u>	<u>1700</u>	<u>1700</u>	
TMM	1702	-10'	-33"	-14"	-1'29"
TMM correction	1705	-1"			
PNPM 2nd Edn.	1713		-23"	-14"	-1'29"
PNPM 3rd Edn.	1726		+33"	-24"	+11"

The Third Edition of the *Principia* concluded this section remarking, 'the mean motion of the moon and of its apogee are not yet obtained with sufficient accuracy.'

From comparing these 1700 epoch means, we can observe that the 1705 value was adopted by Whiston in his *Praelectiones* of 1707, Dunthorne's *Practical Astronomy* of 1739 and Wright's *New and Correct Tables* of 1732, while the the improved 1713 value was adopted by Leadbetter's *Complete System of Astronomy* of 1742 and Halley's *Astronomical Tables* of 1752. No-one used Newton's final 1726 value, as would have been preferable for them.

To construct an ephemeris one needed an estimate of the lunar revolutions performed (in zodiac longitude) in twenty Julian years, supposedly accurate to seconds, for one's tables of mean motion. This interval was crucial for the construction of ephemeris tables, as the error in it was cumulative. Values from some major sources are as follows:

Mean lunar motion per 20 years:

				<u>Error</u>
Wing	(1669):	267 rev., 133°	plus: 33' 44"	-57"
DOS	(1681):		33' 46"	-55"
TMM & Halley	(1749):	" "	34' 5"	-36"
PNPM	(1726):		35 15"	+35"
Cassini	(1740):	" "	33' 58"	-42"

Deviations from the then-correct value are given to the right, by comparing with the Meeus-Chapront-Touze equations, which only altered by an arcsecond or so over this period. TMM's value was accurate to one part in 10<sup>7</sup>, but this error was enough that the seconds column in tables of mean motion were not meaningful; and, in the the 1730s and '40s, it gave those using the TMM values a two minutes error. Two minutes of arc was the accuracy required to claim the longitude prize, enabling longitude to be determined within one degree, so this was a significant error.

Those are the vital decades because, to quote the Victorian astronomer Baily,

'...it appears that a period of more than 30 years elapsed before Gregory's *Newtonian rules* [Baily's somewhat perjorative term for TMM] were thrown into the form of tables for public use;' (p.702)

It was generally only in the 1730s and 1740s that lunar tables came to be based upon TMM: it took three decades for TMM to be put into practice. This may not fully accord with the statement by William Whewell quoted at the beginning of the chapter, but I have not as yet seen the earlier tables to which Whewell referred. Merely preparing tables of mean motion was an easier matter, and William Whiston was the first to prepare these in accord with TMM.

Was there indeed a school of lunar-position astronomers, in the early decades of the eighteenth century, who based their work upon TMM, as Dr Craig Waff has affirmed? If so, a simple criterion will detect them. Those who may be called 'the Newtonians' took their twenty-year epoch values for mean lunar motion as identical with that of TMM. This simple criterion yields the following rather impressive list of published tables:\*

- Whiston 1707
- DeLisle 1716 Paris (unpublished)
- Grammatici 1726 Ingolstadt
- Wright 1732
- Capello 1737 Venice
- Dunthorne 1739
- Brent 1741
- Leadbetter 1742
- Le Monnier 1746 Paris
- Halley 1749

These astronomers concurred to within a single second of arc per twenty years, in the above-defined parameter. The values they took for mean positions varied somewhat, but in their twenty-year intervals they were identical. As mentioned, Halley and Leadbetter added ten arcseconds to their mean position tables, through adopting PNP's 1713 values. This by no

.....  
\* Peter Horrebow's *Nova Theoria Lunae* published in 1718 in Uppsala described itself as Newtonian. Citing its mean motions as from Copenhagen, it gave only one set for the epoch 1700 'which agree with Newton's,' and contained no tables. Nicholas De Lisle's tables in the archives of the Paris Observatory, referred to at the start of this chapter by Sir William Whewell, also satisfy the above criterion.

means establishes that such astronomers were using TMM's method, but it is a start. This list does indeed hint at a rather wide impact made by TMM, as Dr Waff has claimed.

### III A Century of Mean Motions

The mean lunar motion specified by TMM was somewhat more accurate than any hitherto published. The trouble was, that it was mainly adopted several decades later, as we have seen, by which time it had accumulated an error of two minutes and so was no better than others. Cassini's tables were superior at the time when TMM began to be used. Figures 1-5 illustrate the situation.

To prepare these Figures, three sets of 20-year epoch values were selected from each table, centred around their time of publication. The difference between these values and the Meeus/Chapront-Touze value of the mean position were been plotted, with corrections added for local time where necessary. The errors were thus (historical values - modern values).

Each diagram contains the data from just six tables, but more are included in Table 5.1. The French tables were in New Style, and so were eleven days ahead of the British in their calendar, after February of 1700. For the French tables of Le Monnier and Cassini, their epoch values for the year 1700 were for 31 Dec 1699, not 31 Dec 1700 as for the English ephemerides. By obtaining the Julian date (see Appendix III) I found that there were 6929 days between the Newtonian epoch date of 31 Dec 1680, (ie, 11 Jan 1681 NS) and this French epoch date. Timezone adjustments were made for Paris (Cassini and Le Monnier), Venice (Capello), Bologna (Ricciolo), Belgium (Van Lansberge, Zelandiae, ie Middleburg).

The tables were located from various sources: Gingerich and Welther (1983), Curtis Wilson (GHA), and a collection sent by Dr Craig Waff, (Jet Propulsion Laboratory, Pasadena). Dr Waff had intended to review these ephemerides as regards the extent to which they had incorporated the TMM principles (Cohen 1975, pp.77, 79). He did not get round to this, his final doctorate treating instead problems with the mean apse line motion (Waff,1975, John Hopkins) and not with its secondary motion of oscillation.

**ERRORS IN MEAN MOTION, 1650-1750**

		<u>Moon</u>	<u>Sun</u>	<u>Apogee</u>	<u>Node</u>	<u>Aphelion</u>
Morinus	(1650)	+0'14"	-6"	+27'	+6'	-21'
Wing	(1651)	+3'12"	-8"	-6'	-3'	-22'
Shakerley	(1653)	+1'22"	-21"	-5'	-	-34'
J.Newton	(1657)	+1'22"	-42"	-9'	-	-33'
Street	(1661)	-	-	16'	+8'	-
Riccioli	(1665)	+10"	-2'8"	-	-15'	+1°34'
Flamsteed	(1681)	-1'59"	-3"	-19'	-3'	-36'
La Hire	(1687)	+2'57"	-40"	-45'	-3'	-1'
Greenwood	(1689)	-	-	-16'	-8'	+53'

.....

Whiston/TMM(1710)		-33"	-14"	-2'	-3'	-8'
Grammatici	(1726)	-1'12"	-23"	-0'.1	-4'	-3'
Halley	(1749)	-58"	-38"	+2'	-4'	-10'
Capello	(1738)	+10"	-41"	+1'	-4'	-3'
Cassini	(1740)	-1'57"	-20"	+5'	-1'	-11'
Leadbetter	(1742)	-2' 0"	-32"	-3'	-5'	-4'
Le Monnier	(1746)	-33"	-22"	+3'	-4'	-3'

Table 5.1: publication dates are cited, and mean motion errors computed (by subtracting the Meus/Chapront Touzé values) for the twenty-year epoch nearest to that date. Errors are cited in arcminutes for the node, apse and aphelion positions, and to arcseconds for the luminaries. For the British texts prior to 1700, London mean time was used. Missing values indicate that they were not as such given, probably because the tables gave anomaly values (M-A, S-E).

An extensive collection of tables from this period is contained in the Royal Astronomical Society's library in Piccadilly, but is not indexed according to subject.



An extensive collection of tables from this period is contained in the Royal Astronomical Society's library in Piccadilly, but is not indexed according to subject.

Some tables did not give values for mean solar and lunar motions, but rather gave mean anomaly values (see next chapter) plus the apogee, so that anomaly had to be added onto the mean apse position to obtain the mean position. This generates sizeable positional errors, as the apse and aphelion mean motions were an order of magnitude less well determined than those of the luminaries. Our table has omitted such.

The table cites a roughly chronological order of publication, modified somewhat by the range of usable mean epoch values. Error values for the 20-year epoch were usually nearest the date of publication, eg Morinus' 'Tabulae Rudolfinae' were published in 1650, and I have here taken his 1660 epoch, though I have only been able to locate these as republished in the second edition of Streete's opus of 1705. The mean tables of Shakerley and John Newton only went up to 1660, so had to be centred on 1640. It is evident that only tables published in the twenty years following TMM (that I could find) were those by Whiston.

The Table has its first two columns for the luminaries in arcseconds and the other three in arcminutes. It relates ancient and modern definitions of mean motion. Averaging these errors irrespective of sign, and comparing these means for the eighteenth and seventeenth centuries, shows the general drift of improvement:

	<u>Sun</u>	<u>Moon</u>	<u>Node</u>	<u>Apogee</u>	<u>Aphelion</u>
mean error (arcminutes):	0.5	1.3	5	16	22
% improvement C.18/C.17:	20%	35%	45%	85%	85%

Gaps in the Table indicate that I was not able to locate (or interpret) the relevant data. Van Lansberge's tables did not seem to contain the twenty-year epoch values. A more thorough search might locate more, perhaps necessary before definite conclusions can be reached. The 'Newtonians' Wright, Dunthorne and Brent have not been included, having identical tables

to those of Whiston. Halley compiled his tables, one gathers, around 1720, though they were only published posthumously, so his mean motions have been cited for 1720. That is why his errors appear different from Leadbetter's, though their tables were identical.

#### **IV The Missing Flamsteed tables**

Shortly after TMM appeared in 1702, the Astronomer Royal Flamsteed expressed his disapproval, and set about constructing tables of his own, claiming that these would give better positions (Baily, p.211). The terms of his employment drawn up by Charles II mandated him to this task. His new tables were '40 quarto pages and upwards' he told Abraham Sharp. His letters to Sharp described them, explaining why they occupied 'so many pages', adding that Sharp should feel free to tell the world that they had been drawn up, for 'I desire to have them published as soon as may be' (Baily, p.212). Nothing further was heard of these documents ... until, decades later, a Frenchman Lemonnier claimed to have them.

No trace of Flamsteed's decades of work on lunar theory appeared in the three bulky volumes of his *Historia Coelestis Britannica* which emerged posthumously in 1725. However, Pierre Le Monnier's *Institutions Astronomiques* of 1746 included the claim that his tables were both new and based upon those of the English astronomer Flamsteed. A letter by Flamsteed's co-worker Hodgson confirms this, discussed by Baily somewhat inconclusively (Baily, p.704). More recently, Curtis Wilson (GHA, p.201) averred that Halley had given Flamsteed's tables prepared in 1702 to Le Monnier. We can only wonder how Halley came to possess these vital documents, not published in his 'pirate' edition of Flamsteed's *Historia* of 1712. They would be most significant for evaluating Flamsteed's achievement as the Astronomer Royal. The Table shows that Le Monnier's mean positions were of a high standard.

For the Flamsteed tables to have migrated across the Channel in this manner, from Greenwich to Paris, three steps of transformation would have been required: a nine-minute difference for longitude, an eleven-day calendar change to New Style, and finally a year's difference for the mean motions owing to a difference in a convention in presentation, in the way

French tables cited their epoch years. We would have to assume Le Monnier performed these adjustments, though the Paris Observatory archives have no record of these manuscripts.

## V Mean Motion Graphs

Table 5.1 cited figures from a variety of epoch dates, from 1620 to 1760. Initially they were all compared over the 1700 epoch, but this was unfair on those published at some distance from it, because tables are normally more accurate around their time of publication. The present scheme normally scored errors at the epoch date nearest publication plus one on either side thereof. Yet, this hardly permits inferences as to who derived what from whom, a major aim of collating these mean motions.

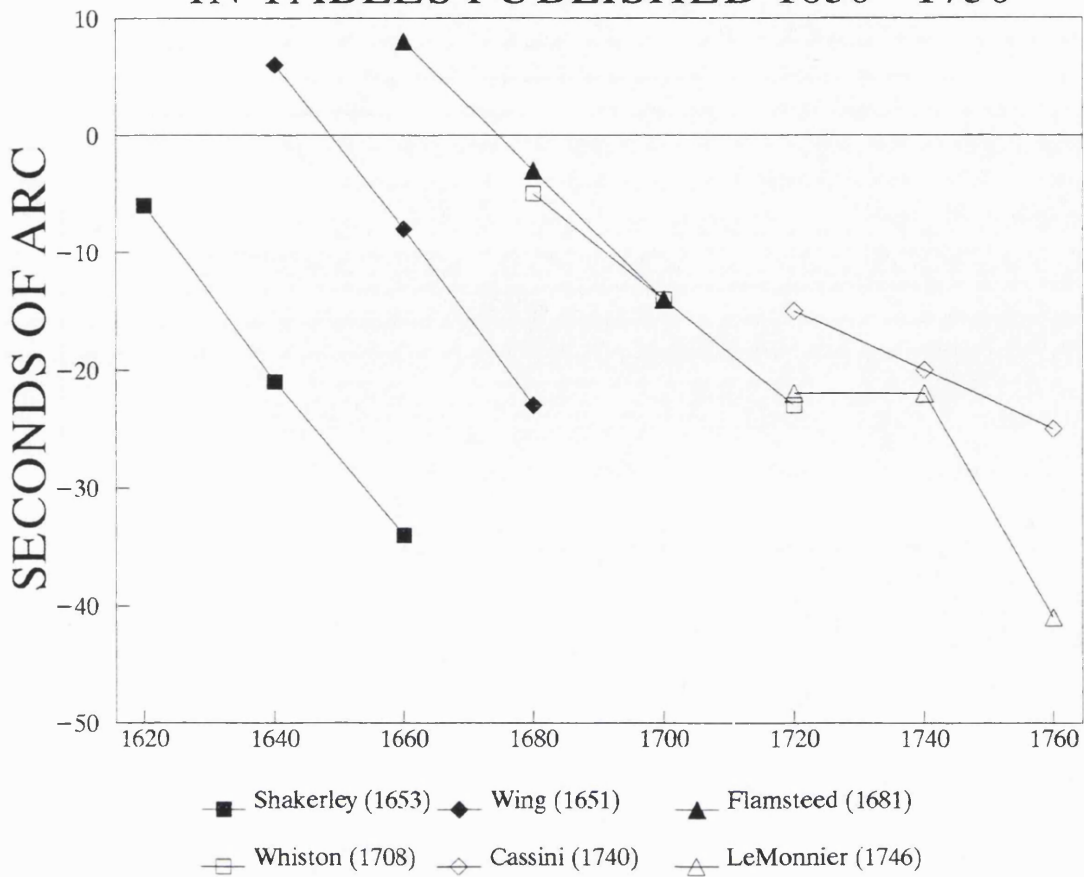
A graphical approach facilitates insight into who copied from whom, necessary to evaluate the extent of TMM's influence in this field. In the following graphs TMM is represented by William Whiston's opus of 1707, since his tables were the first to embody TMM's mean motions. Each line spans a forty-year period, over three twenty-year epochs, the middle one being that whose error was given in the Table. Source-data is given in an Appendix. The plotting of three points in this manner also serves as a check upon my arithmetic procedure.

The solar mean motions show a common downward slope, 18 arcseconds per forty years in the case of Whiston. As Chapter Four noted (Section VIII), Newton's period for the tropical year was in excess by ten seconds, in which time the Sun would move ahead by approximately  $10/24$  arcseconds, or seventeen arcseconds in forty years. The downward slope thus represents that tropical year error. LeMonnier's means look somewhat as if he were using the TMM values, or trying to, while Cassini's can be seen as an improvement.

Figures 5.1 & 5.2: These graphs show 'errors' in tables of mean motions from six different astronomers, for mean Sun and Moon positions. For three sets of twenty-year epoch values, the values derived from the Meeus/Chapront-Touze equations (1993) were subtracted from the published epoch values.

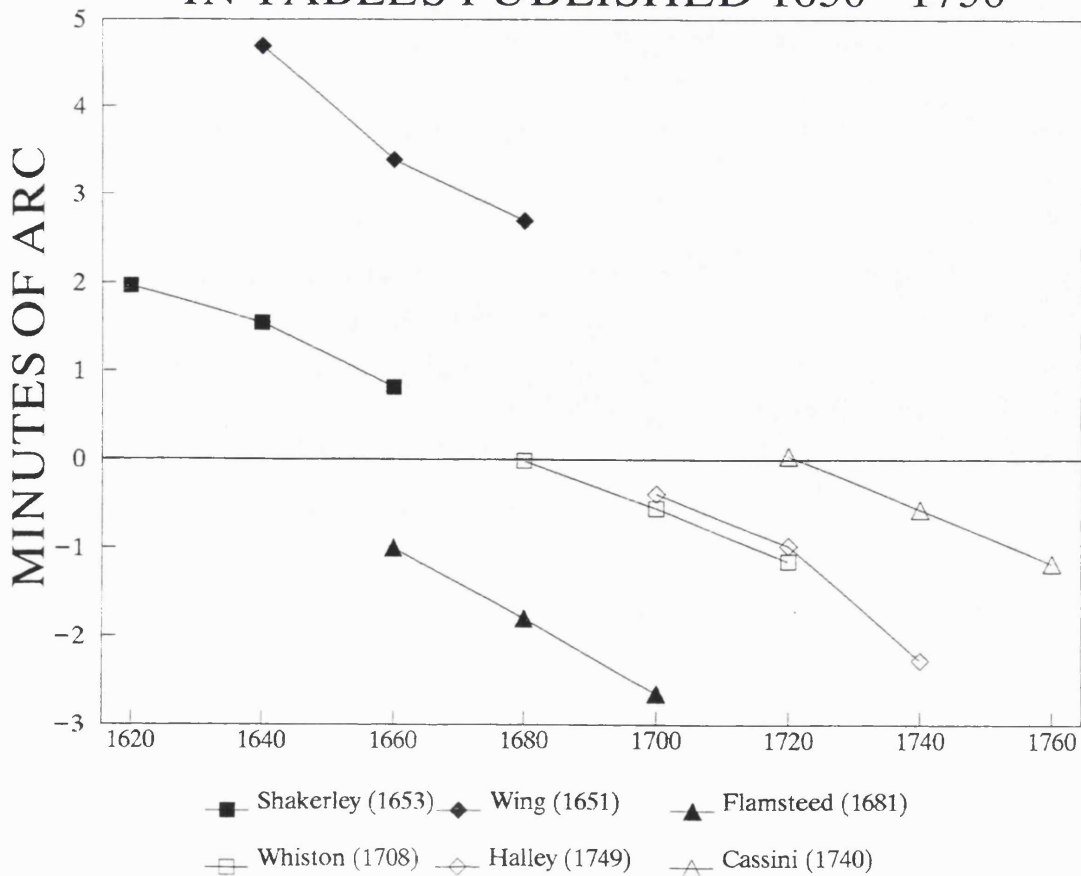
# MEAN SUN ERRORS

IN TABLES PUBLISHED 1650–1750



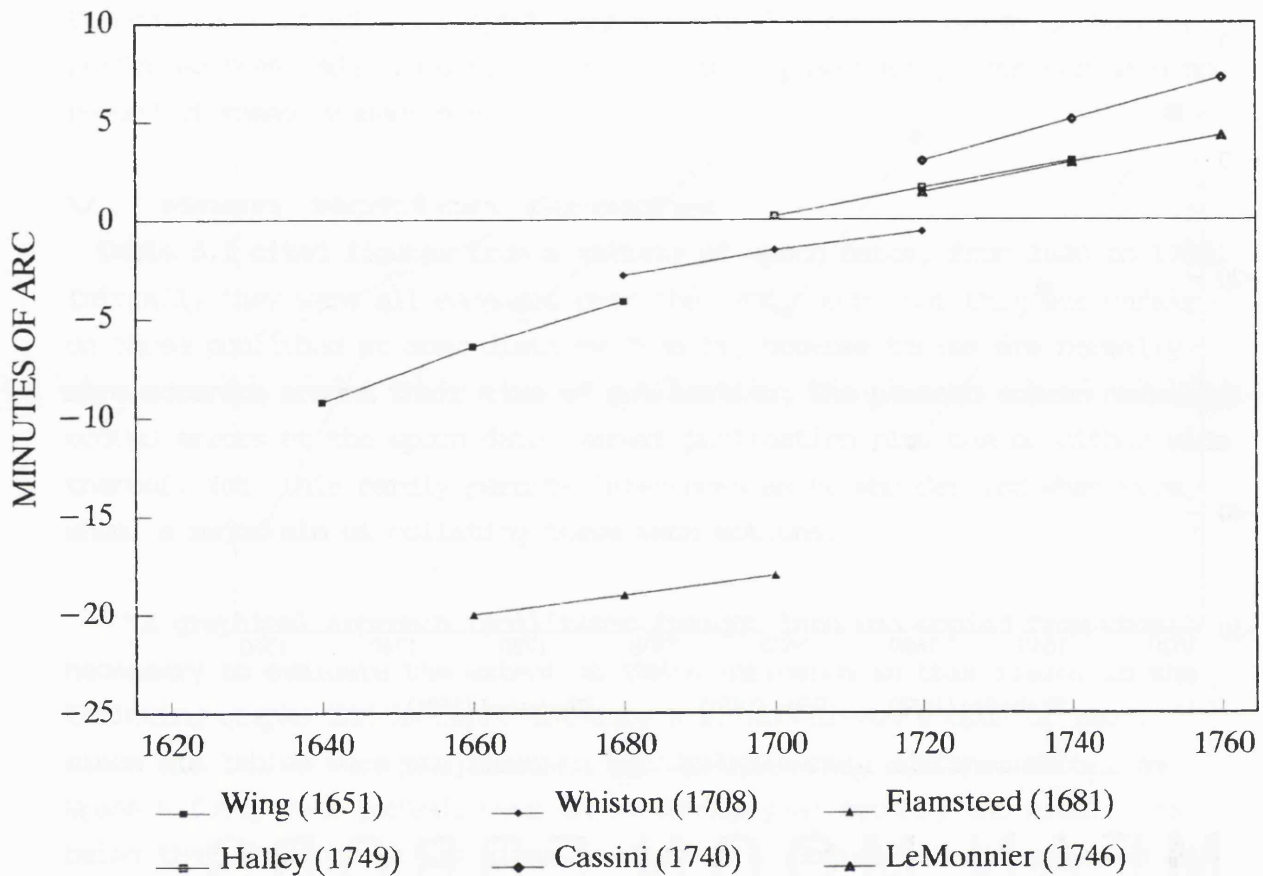
# MEAN MOON ERRORS

IN TABLES PUBLISHED 1650–1750



# MEAN APSE ERRORS

IN TABLES PUBLISHED 1650-1750



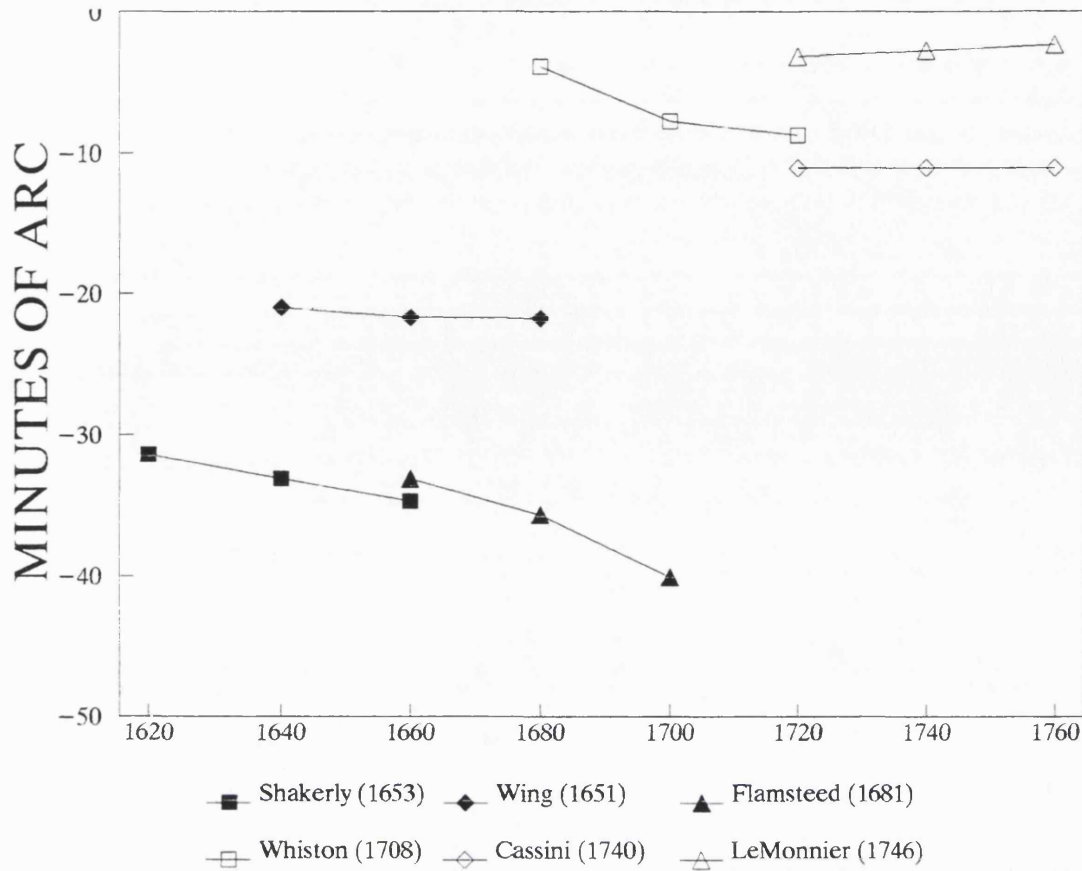
Figures 5.3, 4 and 5: as before, for apse, lunar node and aphelion mean motions.

Chapter Four saw how TMM cited the tropical lunar month correct to 0.2 of a second, and commented: 'It must have been the most accurately known physical constant at that period.' Here, we see the cumulative effect of that 0.2 of a second, whereby over four decades it generated an error of about one arcminute. (As can be seen, the graphs are not necessarily straight lines, though historical mean motions were linear, which is due to non-linear terms in the modern equations.)

The twenty-minute apse error in Flamsteed's 1681 publication is remarkable. In the year 1673 he caused Jeremiah Horrocks's theory to be published, whose equation of apse motion was its most remarkable feature. Flamsteed wrote, in 1675, of how he discovered the truth of this theory:

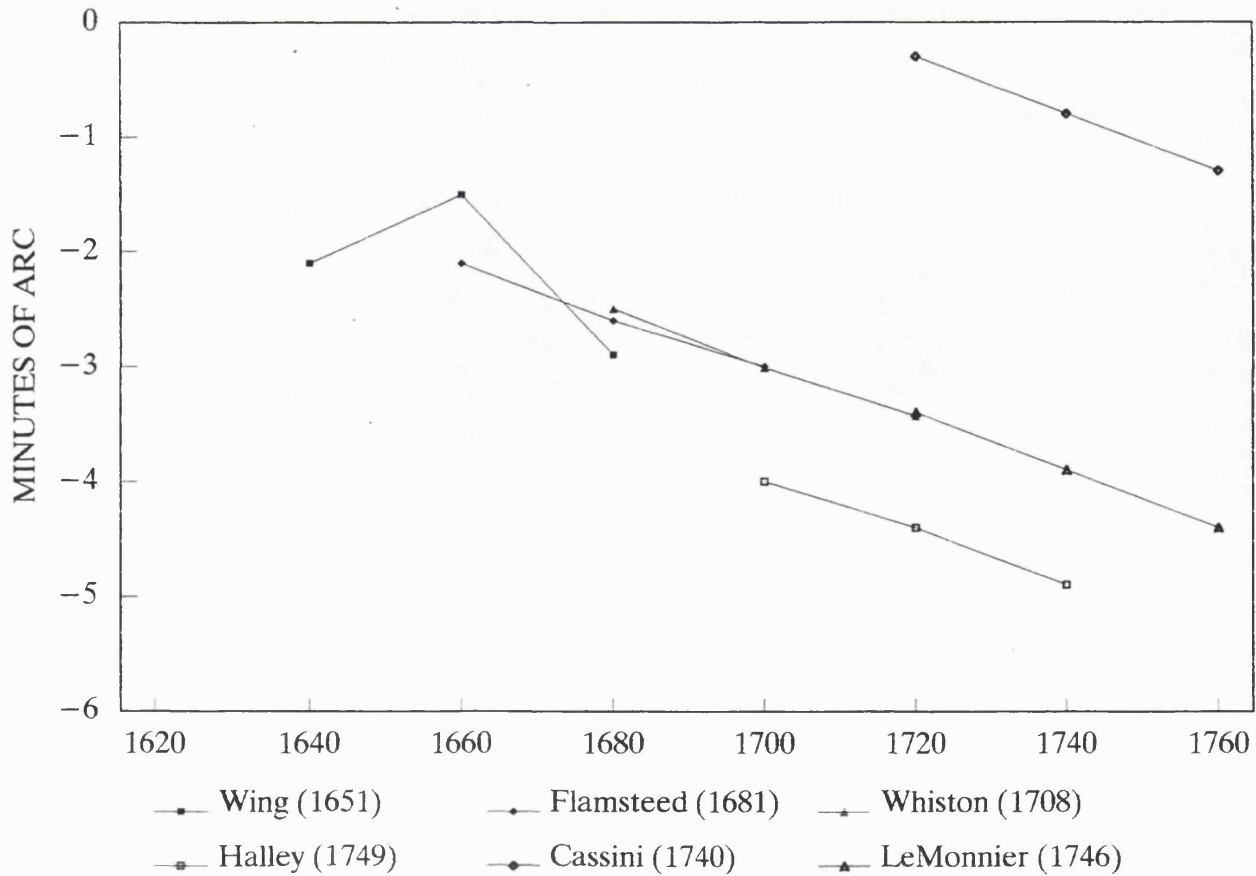
# MEAN APHELION ERRORS

IN TABLES PUBLISHED 1650-1750



# MEAN NODE ERRORS

IN TABLES PUBLISHED 1650-1750



'when I had found by many curious and careful measurements of the Moon's diameter, that the heavens would never admit those Hypotheses..'

(*Phil. Trans*, 10, pp.368-372)

referring to the pre-Horrocksian theories. Locating apse position with a micrometer screw-gauge can have been no easy matter. However, that same year Flamsteed published a criticism of Thomas Streete's textbook, for the way it claimed to be based upon Horrocks, in which he said:

'Mean time, when he hath done what he can (with his apse equation), it will not shew the true place to half a Degree.'

(op. cit., p.220)

Apart from the apse equation, his own mean position was hardly better.

### Conclusion

Whiston's mean values, representing those of TMM, are as good as any in the Table. Over the century a large improvement in the mean apogee and aphelion values appears, plus a smaller one for the lunar nodes. The graph shows Jacques Cassini's lunar means as more accurate than the Newtonian ones, reminding one of Owen Gingerich's account of how Paris in this period became the world centre for ephemeris construction (Gingerich and Welther, 1983, p.xi), though Cassini was a generation later than Newton. The graphs emphasise a major feature of the Table, whereby historic mean values mainly lag behind what are nowadays regarded as their correct values. All five of TMM's means, for both of its epochs, fall behind the modern values; except that, as the nodes are moving in the opposite direction, their historic values could be regarded as in advance!

The mean motions here examined are one method of comparing the 'flow of expertise' in constructing ephemeris tables. Another and equally important would compare constants used in the various equations. For example, Le Monnier observed concerning the Equation of Apogee:

'La plus grande Equation du lieu de l'Apogee avoit été établie autrefois par Flamsteed de  $11^{\circ} 47'22''$ . Mais M. Newton l'a augmenté & s'est assuré qu'elle devoit plus conformé aux Observations lorsqu'on la suppose de  $12^{\circ} 18'$ .'

(1746, p.191)

This shows a remarkable degree of interest in the English method of computing the secondary apse motion, four decades after the theory was developed. The French had not been using the Horrocksian method, and

Cassini used a quite different approach to developing the 'equation of the centre' (GHA, p.201). Thus Le Monnier becomes an important source for evaluating eighteenth-century reception of TMM. Later we return to this theme.

**Textbooks consulted, 1650-1750**

J.B.Morinus	1650	Tabulae Rudolphinae
Vincent Wing	1651	Harmonicon Coeleste
Jeremy Shakerly	1653	Tabulae Britannicae
John Newton	1657	Astronomia Britannica
Thomas Streete	1661	Astronomia Carolina
J.Baptista Riccioli	1665	Astronomia Reformata
John Flamsteed	1681	De Sphaera
Phillipo de la Hire	1687	Tabulae Astronomiae
Nicholas Greenwood	1689	Astronomia Anglicana
Isaac Newton	1702	TMM
"	1713	PNPM
Nicolas Delisle	1716	Tables du soleil & de la Lune
Nicas. Grammaticus	1726	Tabulae Lunares (Ingolstadii);
Robert Wright	1732	New & correct Tables of the lunar motions
Angelo Capello	1738	Astrosophiae Numericae
Jacques Cassini	1740	Tables Astronomiques
Charles Leadbetter	1742	Complete Astronomy
Richard Dunthorne	1739	Practical Astronomy of the Moon
Charles Brent	1741	The compendious Astronomer
Pierre Le Monnier	1746	Institutions Astronomiques
Edmond Halley	1749	Tabulae Astronomicae

**Tables not located:**

- a) J.Hecker, *Motuum Coelestium Ephemerides* 1662;
- b) Kirchius, *Ephemeridum Motuum Coelestum* 1681 (Lipsia);
- c) Bealieu Desforges, *Ephemerides des mouvements celeste* 1703;

Other tables had mean motions I could not fathom: in Phillipe Van Lansberge's *Opera Omnia* of 1663, Maria Cunita's *Urania Propitia* of 1650, and Comte de Pagan's *Les Tables Astronomiques* of 1658.



### Local Time adjustments

<u>Town</u>	<u>Old name</u>	<u>Long. East</u>	<u>Time</u>	<u>Astronomers</u>
Paris		02°20'	09.3 min	La Hire, Cassini, Le Monnier
Bologna, Bononia		11°20'	45.3 "	Riccioli
Venice		12°20'	49.3 "	Capello
Ingolstadt		11°26'	45.7 "	Grammaticus
Copenhagen		11°07'	44.5 "	Horrebow
Zelandiae		03°36'	14.4 "	Van Lansberge
Bicini, Silenorum		18°10'	72.7 "	Cunitia
Ven, Uraniborg		12°45'	51.0 "	Morinus (Denmark)

### Mean Motion Computation

Table 5.2 shows 'true' epoch mean motions in ecliptic longitudes, both for Gregorian time (English) and Julian (Continental). Values along the top row are labile and the rest are fixed (The top row here shown is for the epoch date of 1620). The G.M.T. value is fed in at the top left-hand corner, under 'J.DATE', which generates these labile values. In order to generate such, the time is required in Julian centuries, which the program derives from the Julian date in the next column using the formula  $(D-2451545)/36525$ , to ten decimal places of which five are shown. The Meeus-Chapront mean motion formulae are not here shown, being elsewhere on the spreadsheet, the values they generate being fed into the top row.

The Julian date values were derived from the epoch dates to the left. Two values are cited for each twenty-year epoch, French (N.S.) and English (O.S.). For eg Venetian tables, whose longitude is 12°20' East of Greenwich, one subtracts 49.3 minutes from the given values, and the result converted into a decimal of a day. For the Sun an arcseconds column was computed as historical positions warranted that accuracy, while, for that of the Moon, arcminutes to one decimal place were adequate. For the Moon the conversion from Universal Time to Ephemeris Time was required, whereas this difference was negligible for the other, slower-moving functions.

Table 5.2

MEAN MOTIONS FOR EPOCH DATES NOON DEC 31

G.M.T. Julian time plus Paris time Gregorian

	J. DATE	J. CENTURY	APHEL		SUN		
			deg	mins	deg	mins	sec
AD 2000:	2313128	-3.78965					
20 yrs:	7305		96	25.65	290	7.21	13
1620	2313128	-3.78965	96	25.66	290	7.21	13
1640	2320433	-3.58965	96	46.25	290	16.42	25
1660	2327738	-3.38965	97	6.84	290	25.63	38
French:	2327728	-3.38990	97	6.82	281	33	0
1680	2335043	-3.18965	97	27.44	290	34.85	51
Fr	2335033	-3.18992	97	27.41	280	43.07	4
1700	2342348	-2.98965	97	48.04	290	44.06	4
Fr	2342338	-2.98992	97	48.01	280	52.29	17
1720	2349653	-2.78965	98	8.64	290	53.28	17
Fr	2349277	-2.79995	98	7.58	280	16.69	41
1740	2356958	-2.58965	98	29.25	291	2.5	30
Fr	2356582	-2.59995	98	28.19	280	25.91	55
1760	2364263	-2.38965	98	49.85	291	11.72	43
Fr	2363887	-2.39995	98	48.79	280	35.13	8

	MOON			NODE		PERIGEE	
	deg	mins	mins,E.T.	deg	mins	deg	mins
1620	141	1.57		254	46.52	143	3.8
	141	1.57	2.69	254	46.52	143	3.8
1640	274	36.26	36.82	227	56.7	236	52.88
1660	48	10.95	11.28	201	6.89	330	41.91
Fr	289	30.56	30.89	201	35.5	329	41.71
1680	181	45.63	45.77	174	17.1	64	30.89
Fr	49	54.65	54.79	174	48.89	63	24.01
1700	315	20.31	20.39	147	27.31	158	19.82
Fr	183	29.33	16.48	147	59.1	157	12.94
1720	88	54.97	55.07	120	37.53	252	8.7
Fr	174	30.36	30.46	140	32.19	210	15.36
1740	222	29.64	30.47	93	47.76	345	57.53
Fr	308	5.02	5.13	113	42.43	304	4.19
1760	356	4.29	4.43	66	58	79	46.31
Fr	81	39.68	39.82	86	52.67	37	52.98

French cor: 376.0065 days (1 yr + 11 days + 9.3min)  
up to 1700: 10.0065 days (10 days + 9.3min)

## Ch. 6 FINDING THE ANOMALY

This chapter surveys how astronomers in Restoration England dealt with the motion of a body obeying Kepler's second law. It deals with the two fundamental concepts, of 'equation', as in for example the 'Equation of the Centre', and 'anomaly'. The astronomers would proceed from what they called mean anomaly to the coequated (or, 'true') anomaly, via the 'Kepler equation'. TMM instructs the reader to use tables at these crucial stages and that is all that we need to do. We are not obliged to go through the difficult stages then required to construct the tables, though it is appropriate that we should have some idea of their principles of composition.

### I The solar anomaly

Anomaly meant an angle, formed between two mean positions. As such it was a computational tool that could not be observed or measured directly. The mean anomaly was the angle between the apse line and a mean position in the orbit, at any given time. Whether it was measured from apogee or perigee was a matter of convention. To quote Curtis Wilson:

'...the aphelion was taken as the zero of anomaly in planetary tables, and the apogee as the zero of anomaly in solar and lunar theory.'

(GHA, p.275)

That became the agreed convention in the eighteenth-century. If however we go back to Flamsteed's DOS of 1680, that was not the case. For finding the Sun's anomaly, DOS instructed:

'Subtract the Longitude of the Perihelion from the Mean Motion, the Residue is the Mean Anomaly.' (p.34)

It made sense to start from perihelion at least for the Sun, if that was when the year began, in January. The tables gave their 'Equation of the Centre' as a function of anomaly, over the range 0-180°. We have to be clear as regards which convention is in use over the anomaly, as these tables were not symmetrical: the 'equation' would tend to be maximal at around 91° for the Sun and 94° for the Moon. The figure shows this, where  $M$  is the mean anomaly (here measured from apogee) and  $\theta$  is the Equation of Centre. Table 6.1 is a reproduction of a page from DOS, pointing out the maximum value reached at 94° anomaly.

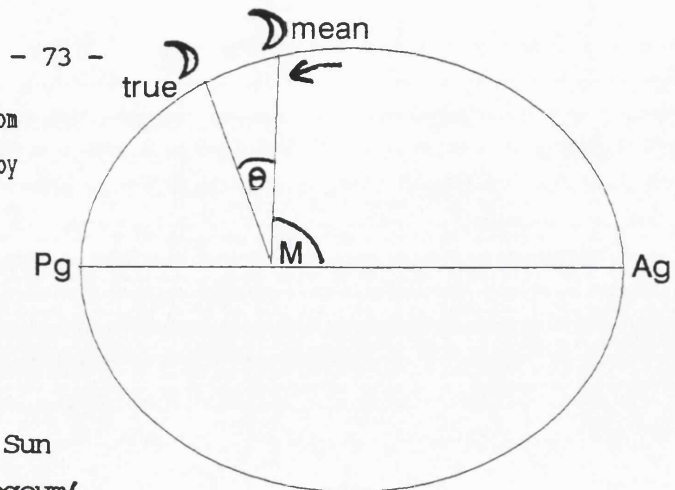


Figure 6.1: mean anomaly  $M$  as measured from the apse line, and the 'equation'  $\theta$  whereby mean and true positions diverge.

The Earth's orbit around the Sun was an ellipse with 'the Sun's Apogeeum' as TMM called it at one end. The new terms introduced by Kepler, of aphelion and perihelion,

had not yet caught on. The former was the point at which the Sun was furthest away, and so appeared to move most slowly, while the latter was the point of closest approach. They are positioned in ecliptic longitude, for which reason we use the tropical year period in defining the mean sun, of 365.24 days. The motion of the Earth was treated as the motion of the Sun around the Earth.

Worked example: We seek the solar anomaly on the epoch date of December 31 1680. TMM referred to the position of aphelion as 3 sign,  $7^\circ$ , designating a longitude of  $97^\circ$  measured from  $0^\circ$  Aries. This becomes January 10th, 1681 in New Style (that is, the Gregorian calendar), as there was a ten-day difference between the two systems in the seventeenth century. January 10th (N.S.) was 13 days after perihelion which then fell in the morning of December 29th. At the present time perihelion falls on January 3rd, at  $12^\circ$  Capricorn. In the 1680s it fell on December 29th, in  $7\frac{1}{2}^\circ$  Capricorn, so it has moved four and a half degrees in three centuries.

The mean Sun moves uniformly at  $59' 8''$  per day. The angular difference between TMM's mean Sun and perihelion position, for the date given of December 31, 1680, comes to  $13^\circ 11'$  (subtracting the epoch values given in Ch.4, Section II). Finding the mean anomaly was the first step in an arduous series of computations which the astronomer had to perform.

## II Kepler Motion and the Equation of Centre

An 'equation' then signified, to quote Curtis Wilson:

'the angle to be added or subtracted from a mean motion in order to 'correct' it, that is, in order to obtain a theoretical position in agreement with the position observed.' (GHA,p.277)

TMM gave the Sun's 'Equation of Centre', or 'equation of orbit' as it was called a greatest magnitude of  $1^{\circ} 56' 20''$ . The terminology derived from the old scheme of things, where the Sun had a circular orbit and Earth was not quite at the centre of that orbit, and the magnitude of its displacement from that point generated its 'Equation of Centre'. This equation was zero at the apses, growing to a maximum near the quadratures. It was subtracted while moving from aphelion to perihelion, and added during the other half of the year, since Earth's orbit is fastest at perihelion and slowest at aphelion.

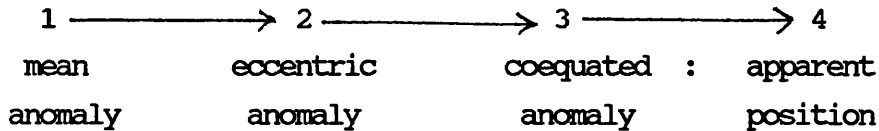
Estimates of the maximal value of this solar equation had been shrinking ever since Tycho Brahe estimated it as two degrees. It is of interest to look at values cited by Curtis Wilson (GHA, pp.168-191) as used by astronomers, comparing these with actual values for the period (the latter being derived from modern estimates of historic eccentricities):

<u>Astronomer</u>		<u>Eqn. centre</u>	<u>true value</u>	<u>Error</u>
Brahe	1580s	$2^{\circ} 3' 15''$	$1^{\circ} 56' 10''$	7'
Horrocks	1638	$1^{\circ} 59' 18''$	$1^{\circ} 55' 54''$	3' 22"
Cassini I	1660	$1^{\circ} 56' 53''$	$1^{\circ} 55' 50''$	1' 37"
Flamsteed	1675	$1^{\circ} 54' 13''$	$1^{\circ} 55' 49''$	-1' 36'
"	1679	$1^{\circ} 55' 0''$	$1^{\circ} 55' 48''$	-48"
"	1692	$1^{\circ} 56' 20''$	$1^{\circ} 55' 45''$	+35"

The last value was used in TMM, and then in all the tables of 'the Newtonians': Whiston (1715), Dunthorne (1739), Brent (1741), Leadbetter (1742), Le Monnier (1746) and Halley (1749). Indeed, I have only come across one compiler of astronomical tables in the first half of the eighteenth century in France or England who did not use that figure and

that was Jacques Cassini (1740), sometimes called Cassini II. He used the more accurate value of 1° 55' 50".

The computations as then performed involved three stages:



There was no simple means of moving from the mean anomaly to the 'coequated' anomaly, so-called because an 'equation' had been applied. It was done by using 'Kepler's equation' to find what was called the eccentric anomaly\*. This was the angular position, as viewed from the centre of the ellipse, of a body moving in a circle circumscribing that ellipse. Kepler's equation does not have an algebraic solution, so methods of approximate solution had to be developed and used. Through some means of solving 'Kepler's equation' one obtained the eccentric anomaly from the mean, and thence derived stage 3†. Then the 'coequated' anomaly had to be sought, which could then be compared to the actual position (called, 'apparent position') in the heavens. The goal of a theory was to minimise the difference between 3 and 4. What Newton had to say about moving between stages 1 and 2 appeared in the *Principia* and not in TMM, and need not concern us.

What the layman would call the true or actual position in the heavens, was and still is referred to by astronomers as the 'apparent' position. This is because the 'coequated' position used to be referred to as the 'true' position. A lucid account of these terms has been given in GHA by Curtis Wilson (GHA Appendix, Gi-Gvi).

.....

\* The Kepler equation is:  $M = E + e \sin E$ ,  
 where M is mean anomaly, e is eccentricity and E is the eccentric anomaly.

.....

† Gaythorpe, 1925,p.864; Newton used a geometrical equivalent of what is called 'Newton's method of approximation' to solve the Kepler equation in PNPM 3rd Edn p.112-116, see Whiteside *Math. Papers*, IV, 1971, p.665.

What is nowadays called the 'equation of the centre' is not the angle as we have understood it above, but a formula which generates that angle. From values of eccentricity (e) and mean anomaly (M), it gives that angle (in radians) by the following series:

$$\theta = (2e - e^3/4)\sin M - 5/4e^2\sin 2M + 13/12 e^3\sin 3M + \dots,$$

This is the modern expression for the difference between true and mean anomaly, measured in radians. For the small eccentricity value of the Earth's orbit, two terms of the series are generally adequate, namely:

$$\theta = 2e\sin M - 1.25e^2\sin 2M$$

These first two terms give the Equation of Centre within about half a minute for the Moon. To avoid confusion, we shall refer to the old meaning in upper case, as Equation of Centre. Surprisingly, this modern series expansion concurred within *one or two seconds of arc* with the tables of Flamsteed, indicating that by 1681 at least one astronomer had effectively solved the problem of computing elliptic, Kepler motion.

TMM described an elliptic orbit in terms of two different parameters, namely eccentricity and maximal Equation of Centre, and the the above equation relates these together. In the case of the solar orbit, TMM specifies eccentricity as 16 11/12 parts in 1000, and the maximum Equation of Centre as 1° 56' 20". The anomaly value which generates the maximal Equation of Centre is M=91°, ie solar tables will give the greatest 'equation' at 91°. For the lunar tables the maximal value arises at or near to 94°. Inserting 91° into the above equation for M links TMM's two solar eccentricity parameters together within one second of arc.

Worked example, continued: For the solar anomaly found of 13° 11', at the 1680 epoch date, we consult tables for the Equation of Centre:

	<u>DOS(1681)</u>	<u>Dunthorne (1739)</u>	<u>Cassini (1740)</u>
13°	25' 21"	25' 38"	25' 33"
14°	27' 16"	27' 34"	27' 29"
Max.Eqn.(91°)	1°55' 0"	1°56' 19"	1°55' 50"

Dunthorne used Newton's eccentricity value. Interpolating for an angle of  $13^{\circ} 11'$ , Dunthorne's tables give  $25' 59''$ . The modern formula gives the identical value (I have written a computer program for the function, which makes comparison easier). The DOS tables would give a slightly smaller value, as they were composed before Flamsteed had reached his final and more exact value for the earth-orbit's eccentricity.

This equation brings us to within half a minute of the Sun's actual position. Thus, the Sun's centre (or rather, the Earth's) was then departing from Kepler-motion by that amount.

### III The Lunar Equation of Centre

The lunar 'equation of centre' was a more complicated affair, varying not only with its mean position in orbit, but also with a half-yearly cycle. The Horrocksian theory used the altering eccentricity of the lunar orbit to modulate the amplitude of the 'equation of centre'. Here we merely introduce the subject, prior to a full account in the next chapter.

A mean moon has uniform angular velocity in ecliptic longitude, revolving once per tropical month. For the lunar anomaly one subtracted the mean apogee position therefrom. We have seen how the lunar apogee was a great deal more stable than perigee, which possibly accounted for such a tradition. TMM does not comment on these matters, but merely says that tables prepared in the usual way are to be consulted.

In defining mean anomaly as an angle measured from the mean apogee position, we are treating the motion of the apse as a continuous function, whereas in fact the apogee and perigee positions only exist at discrete positions once a month. The concept is a mathematical abstraction, defined as an angle between two points in uniform motion, in the same direction around the ecliptic\* (see next page). These intersect once per 27.554 days, the period of the anomalistic or apogee-perigee cycle.



The tables prepared by Flamsteed in 1679 for his *De Sphaera* were the first British tables to be computed from fully Keplerian principles, as the science historians Thoren and Gingerich pointed out in 1974. Gingerich and Welter found that the errors in the lunar Equation of Centre tables were up to 3" for minimum eccentricity and 10" for maximum eccentricity, concluding "On the face of it...in at least one important case Kepler's second law was being used in England before the publication of Newton's .....

\* This definition differs from that given in GHA. We take the mean anomaly between two angular positions at the same instant of time, while GHA advocates taking the angle between a mean moon and its last point of intersection with the apse line:

'an angle proportional to the time that has elapsed since the planet was last at the upper apse of its orbit, and such that  $360^\circ$  corresponds to a complete period.' (GHA,p.278)

In the case of the Sun and planets, the apses are virtually immobile making the two definitions equivalent. Were we to adopt this definition, we would first have to locate the previous point of intersection of apse and mean moon, by the method indicated above, which is  $1^\circ 56'$  of Sagittarius. This would give a mean anomaly of  $60^\circ 20'$ , which differs by nearly three degrees from that which we have taken.

This GHA definition of mean anomaly would introduce a discontinuity each month, as the zero-point jumped  $3^\circ$  from one apse-intersection point to that of the following month. It would cause TMM's lunar position to jump suddenly by three degrees each month at apogee. It is simpler and more logical to measure an angle between two positions existing at the same moment of time, rather than having a definition based on a conversion between time and space measures. Also, we are soon to give the apse line a to-and-fro motion of twelve degrees twice yearly; this model may only be workable if we measure from where the apse is at any given moment. My reading of the paragraph in TMM beginning 'Having from these Principles made a Table...' (1702, p.20) indicates that the 'mean Anomaly of the Moon corresponding to any given Time' means angular measure as here used.

*Principia*". My calculations showed a rather higher level of accuracy, that DOS's lunar Equation of Centre tables concurred within one or two seconds of arc with the modern formula. Flamsteed's computation methods used, in a very precise manner, the first and second laws of Kepler.† This is a remarkable historical fact, as Newton himself had not then used Kepler's second law in any astronomical context (Cohen 1980, p.250). It also implies that his concept of eccentricity was numerically identical with our modern definition. The calculations showing this level of accuracy are summarised at the end of the chapter. Table 6.1 shows the middle of the three tables for the DOS Equation of Centre, for 60° - 120° anomaly.

#### IV Finding the Prostaphaeresis

The amplitude of the Equation of Centre varied in accordance with what TMM called the 'Annual Argument.' This was the term employed by Horrocks, who took it from Kepler (Curtis Wilson, 1987 p.81). Chapter Two described how this has a 6½ month period, between conjunctions of syzygy and apse: as syzygy is the line joining Full and New Moon, the angle is formed between the Sun and the mean apse line, in zodiac longitude, as they line up twice per 'Horroxian year' of 411 days. Astronomers have no definite term for this function, because it does not feature in modern theory, and so I have proposed calling it the 'Horrox angle' (Φ). One may prefer not to use the TMM term 'Annual Argument' of the apogee, as the period is not really annual. It appears in Figure (3) as 46°.

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\* V.Thoren, *Kepler's Second Law in England* BJHS, 1974,7, pp.243-256;  
O.Gingerich and B.Welther, 'Note on Flamsteed's Lunar Tables', BJHS, 1974,7,p.258.

.....  
† Flamsteed's assistant Mr Hodgson recalled: 'Mr Flamsteed, under whom I had the happiness of my education, was pleased to set me upon computing his lunar tables, under his direction; when I computed the tables of central equations of the moon after the Keplerian method, which had never been done before.' (Introduction to Hodgson's *Theory of Jupiter's Satellites*,1750, quoted in Baily p.704)

Table 6.1  
Lunar Equation of  
Centre Tables in  
DOS, showing two  
columns spanning  
60°-90° and 90°-  
120°. The values  
peak around 94°  
of anomaly, at  
almost 6° 20'.

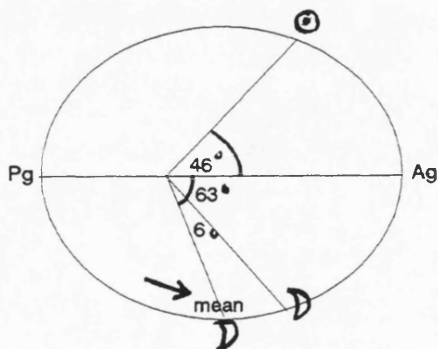
*A TABLE of the Equations of the Moons Center.*

Subtract.

Mean Anomaly	Sign 2.						Sign 3.						Mean Anomaly
	Least Exc		Middle.		Greatest.		Least Exc		Middle.		Greatest.		
	43619	55327	66854	43619	55327	66854	43619	55327	66854	43619	55327	66854	
0	4 12 40	5 17 27	6 21 18	4 59 30	6 18 59	7 38 11	4 59 30	6 18 59	7 38 11	4 59 30	6 18 59	7 38 11	17 30
1	4 15 18	5 20 54	6 25 32	4 59 48	6 19 23	7 38 52	4 59 48	6 19 23	7 38 52	4 59 48	6 19 23	7 38 52	29
2	4 17 56	5 24 17	6 29 39	4 59 56	6 19 40	7 39 20	4 59 56	6 19 40	7 39 20	4 59 56	6 19 40	7 39 20	28
3	4 20 28	5 27 35	6 33 40	4 59 59	6 19 50	7 39 40	4 59 59	6 19 50	7 39 40	4 59 59	6 19 50	7 39 40	27
4	4 23 00	5 30 47	6 37 36	4 59 58	6 19 54	7 39 51	4 59 58	6 19 54	7 39 51	4 59 58	6 19 54	7 39 51	26
5	4 25 24	5 33 53	6 41 25	4 59 49	6 19 51	7 39 53	4 59 49	6 19 51	7 39 53	4 59 49	6 19 51	7 39 53	25
6	4 27 44	5 36 54	6 45 08	4 59 36	6 19 40	7 39 47	4 59 36	6 19 40	7 39 47	4 59 36	6 19 40	7 39 47	24
7	4 30 00	5 39 49	6 48 44	4 59 20	6 19 23	7 39 33	4 59 20	6 19 23	7 39 33	4 59 20	6 19 23	7 39 33	23
8	4 32 12	5 42 39	6 52 14	4 58 53	6 18 57	7 39 9	4 58 53	6 18 57	7 39 9	4 58 53	6 18 57	7 39 9	22
9	4 34 19	5 45 24	6 55 36	4 58 24	6 18 25	7 38 37	4 58 24	6 18 25	7 38 37	4 58 24	6 18 25	7 38 37	21
10	4 36 21	5 48 02	6 58 52	4 57 48	6 17 46	7 38 58	4 57 48	6 17 46	7 38 58	4 57 48	6 17 46	7 38 58	20
11	4 38 18	5 50 35	7 02 01	4 57 06	6 17 00	7 38 9	4 57 06	6 17 00	7 38 9	4 57 06	6 17 00	7 38 9	19
12	4 40 12	5 53 02	7 05 03	4 56 19	6 16 08	7 38 12	4 56 19	6 16 08	7 38 12	4 56 19	6 16 08	7 38 12	18
13	4 41 58	5 55 22	7 07 57	4 55 27	6 15 08	7 38 6	4 55 27	6 15 08	7 38 6	4 55 27	6 15 08	7 38 6	17
14	4 43 41	5 57 36	7 10 45	4 54 30	6 14 00	7 38 52	4 54 30	6 14 00	7 38 52	4 54 30	6 14 00	7 38 52	16
15	4 45 19	5 59 44	7 13 25	4 53 27	6 12 46	7 38 29	4 53 27	6 12 46	7 38 29	4 53 27	6 12 46	7 38 29	15
16	4 46 53	6 01 46	7 15 58	4 52 19	6 11 25	7 38 57	4 52 19	6 11 25	7 38 57	4 52 19	6 11 25	7 38 57	14
17	4 48 22	6 03 42	7 18 24	4 51 03	6 09 56	7 38 17	4 51 03	6 09 56	7 38 17	4 51 03	6 09 56	7 38 17	13
18	4 49 44	6 05 31	7 20 42	4 49 45	6 08 20	7 38 28	4 49 45	6 08 20	7 38 28	4 49 45	6 08 20	7 38 28	12
19	4 51 02	6 07 15	7 22 53	4 48 21	6 06 37	7 38 30	4 48 21	6 06 37	7 38 30	4 48 21	6 06 37	7 38 30	11
20	4 52 15	6 08 52	7 24 56	4 46 51	6 04 48	7 38 23	4 46 51	6 04 48	7 38 23	4 46 51	6 04 48	7 38 23	10
21	4 53 22	6 10 23	7 26 52	4 45 16	6 02 51	7 38 8	4 45 16	6 02 51	7 38 8	4 45 16	6 02 51	7 38 8	9
22	4 54 23	6 11 46	7 28 39	4 43 34	6 00 48	7 38 44	4 43 34	6 00 48	7 38 44	4 43 34	6 00 48	7 38 44	8
23	4 55 20	6 13 03	7 30 20	4 41 44	5 58 37	7 38 12	4 41 44	5 58 37	7 38 12	4 41 44	5 58 37	7 38 12	7
24	4 56 12	6 14 14	7 31 52	4 39 56	5 56 19	7 38 30	4 39 56	5 56 19	7 38 30	4 39 56	5 56 19	7 38 30	6
25	4 56 59	6 15 19	7 33 16	4 37 58	5 53 54	7 38 40	4 37 58	5 53 54	7 38 40	4 37 58	5 53 54	7 38 40	5
26	4 57 38	6 16 17	7 34 32	4 35 56	5 51 23	7 38 42	4 35 56	5 51 23	7 38 42	4 35 56	5 51 23	7 38 42	4
27	4 58 14	6 17 08	7 35 40	4 33 47	5 48 43	7 38 35	4 33 47	5 48 43	7 38 35	4 33 47	5 48 43	7 38 35	3
28	4 58 45	6 17 52	7 36 41	4 31 33	5 45 57	7 38 19	4 31 33	5 45 57	7 38 19	4 31 33	5 45 57	7 38 19	2
29	4 59 10	6 18 29	7 37 34	4 29 13	5 43 04	7 38 55	4 29 13	5 43 04	7 38 55	4 29 13	5 43 04	7 38 55	1
30	4 59 30	6 18 59	7 38 17	4 26 49	5 40 05	7 38 23	4 26 49	5 40 05	7 38 23	4 26 49	5 40 05	7 38 23	0

Add.

Figure 6.2  
Mean positions on  
epoch date of  
December 31st, 1680.



Over this period the lunar Equation of Centre varied between  $7^{\circ} 39' 30''$  and  $4^{\circ} 57' 56''$  in its maximal value over a monthly orbit, according to TMM. Newton composed a table to assist finding this for Flamsteed (*Correspondence*, IV p.107, here reproduced as Table 7.1). This 'Equation of the Moon's centre,' was sometimes called the *Prostaphaeresis*, a function still harder to find than it was to pronounce.

The first step in determining this 'equation' is to find the Horrox angle,  $\phi$ . Then, as a first approximation we may assume that the oscillation of the 'Equation of the orbit' is a cosine function, maximal at zero degrees when the Sun is conjunct the lunar apse and minimal when the two axes are at right angles, ie a cosine of  $2\phi$  with the function oscillating twice per Horroxian year. Then, at any given moment, its magnitude will be

$$6^{\circ} 18' 43'' + 1^{\circ} 20' 47'' \cos 2\phi$$

giving a maximum value of  $7^{\circ} 39' 30''$ , when  $\phi$  is zero,

and a minimum of  $4^{\circ} 57' 56''$  when  $\phi=90^{\circ}$ ,

as TMM requires. This merely indicates how the functions are linked. TMM does not use such trigonometric functions, but develops a kinematic/geometrical approach, which is a little more complex than the above.

The greatest 'equation of the centre' is half as much again as its least value, whereby the second or 'Kepler motion' moon comes to differ by a greater amount from the mean moon. Astronomers of the period would have visualised this effect in terms of an unchanging circular orbit, where what altered was its relation to the Earth as epicenter, whose position deviated by varying amounts from the center of a circular orbit.

Worked Example, contd: On the epoch date of December 31st, the mean moon was at  $1^{\circ} 46'$  of Libra and the mean apogee at  $4\frac{1}{2}^{\circ}$  Sagittarius, giving a mean anomaly of  $-63^{\circ}$ . It was  $6^{\circ}$  ahead of its mean position, having moved ahead, travelling faster while near perigee (See Figure (3)).

The 'Horrox angle' was  $46^{\circ}$  giving  $6^{\circ} 15'$  for the maximal Equation of

Center, and an eccentricity of 0.05460\*. Inserting this into the formula, together with the mean anomaly of  $M = -63^\circ$  gives  $5^\circ 44'$  as the equation of centre. The difference between the mean and coequated positions that we are looking for was  $6^\circ 17'$  (Ch.1,V), so our computation is half a degree short.

In this chapter we have performed some rudimentary computations, obtaining the Sun's position within half a minute and the Moon's within half a degree. This may give us some respect for the difficulties involved, and a notion of how to apply an 'equation.'

Note on Accuracy of Flamsteed's Lunar Equation of Centre Tables

The Table below gives, for some selected mean anomaly angles, their DOS Equation of Centre, then subtracts therefrom the correct or Keplerian value as derived from the first three terms of the modern equation of centre (Ch.6,II) to give their errors. The  $75^\circ$  and  $90^\circ$  anomaly values are shown in the DOS tables of Figure 2, and Flamsteed's middle eccentricity value of 0.055237 was used in the formula. We regrettably lack details of how Flamsteed (with his assistant Mr Hodgson) accomplished these remarkably accurate computations.

<u>Anomaly</u>	<u>30°</u>	<u>45°</u>	<u>60°</u>	<u>75°</u>	<u>90°</u>
DOS	2°59'4"	4°15'47"	5°17'27"	5°59'44"	6°18'59"
Errors:	-1.5"	+0.5"	+2"	+2"	-1.5"

.....

\* When the Horrox angle  $\phi = 46.11$ , 0.05505 is the mean eccentricity and 0.01173 is half the difference between maximum and minimum eccentricity, then:  

$$e = 0.05505 + 0.01173 \cos 2\phi = 0.0546$$
This is discussed further in Ch.7, Section II.

## Ch. 7 THE HORROX-WHEEL IN MOTION

*'Horrox had left no description of the theory itself, but Crabtree was helped in his reconstruction by rough diagrams drawn on loose papers...'*

Forbes, 1975, p.63.

TMM embodies a developed version of Jeremiah Horrocks' lunar theory, what one might call Newton's interpretation of Halley's variation of Flamsteed's version of Crabtree's account of Horrocks's lunar theory. Had Horrocks lived beyond the brief span of twenty-two years, he might have described his theory more fully; and yet, even in the incomplete state in which he left it, it was in Flamsteed's view the greatest of his achievements.

Curtis Wilson has described how the theory began to dawn on Horrocks in January 1637 (JHA, 1987, p.86), and had been formed by December 1638, when he prepared 'the new calculus of the Moon' and sent it to William Crabtree. Figure 7.1 depicts Horrocks's kinematic model, and Figure 7.2 is another diagram, showing the process through a thirteen-month cycle\*. This same diagram was sent from Crabtree to William Gascoigne in June 1642. These were the three north-countrymen who initiated the tradition of British astronomy, whose work became known via Flamsteed, as he moved from Derby to London in the early 1670s.

Flamsteed became the chief exponent of the Horrocksian theory, such that astronomers knew it largely as Flamsteed's development thereof, as presented in his epilogue to *Jeremiae Horocci, ...Opera Posthuma* published by John Wallis in 1673. A succinct version thereof was given in a letter of Flamsteed's to Newton (*Correspondence*, Vol. IV, p.27).

.....  
\* Figure 7.1 occurs in Horrocks's *Philosophical Exercises* notebook, now in the R.G.O. library (1.68B, section 19), Cambridge, and is reproduced in GHA p.199. Figure 7.2 originally appeared in the letter from Horrocks to Crabtree dated 20 December 1638, published in a Latin translation in the *Opera Posthuma* 1673 pp. 467-8.

Figure 7.1:

Diagram in Horrocks' notebook for computing semi-annual variations in lunar eccentricity and apse (his 'Philosophical Exercises')

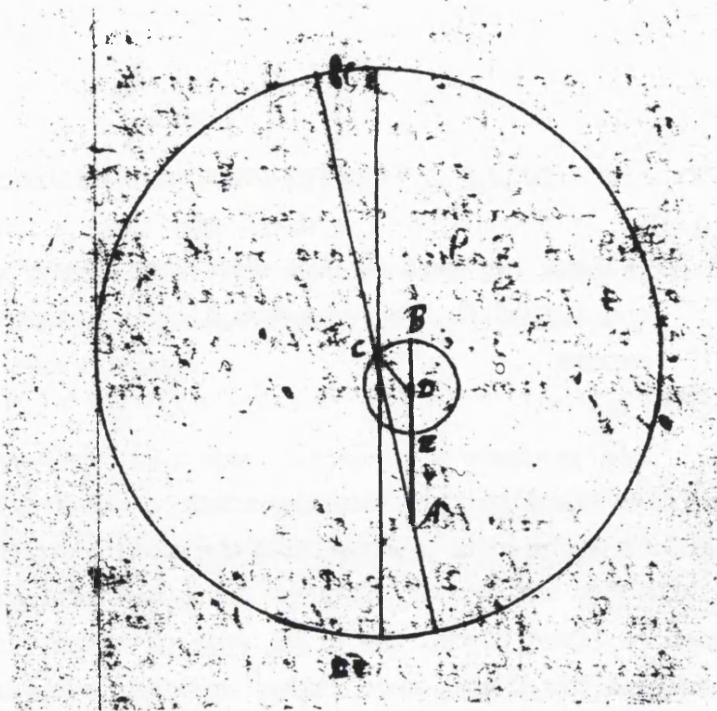
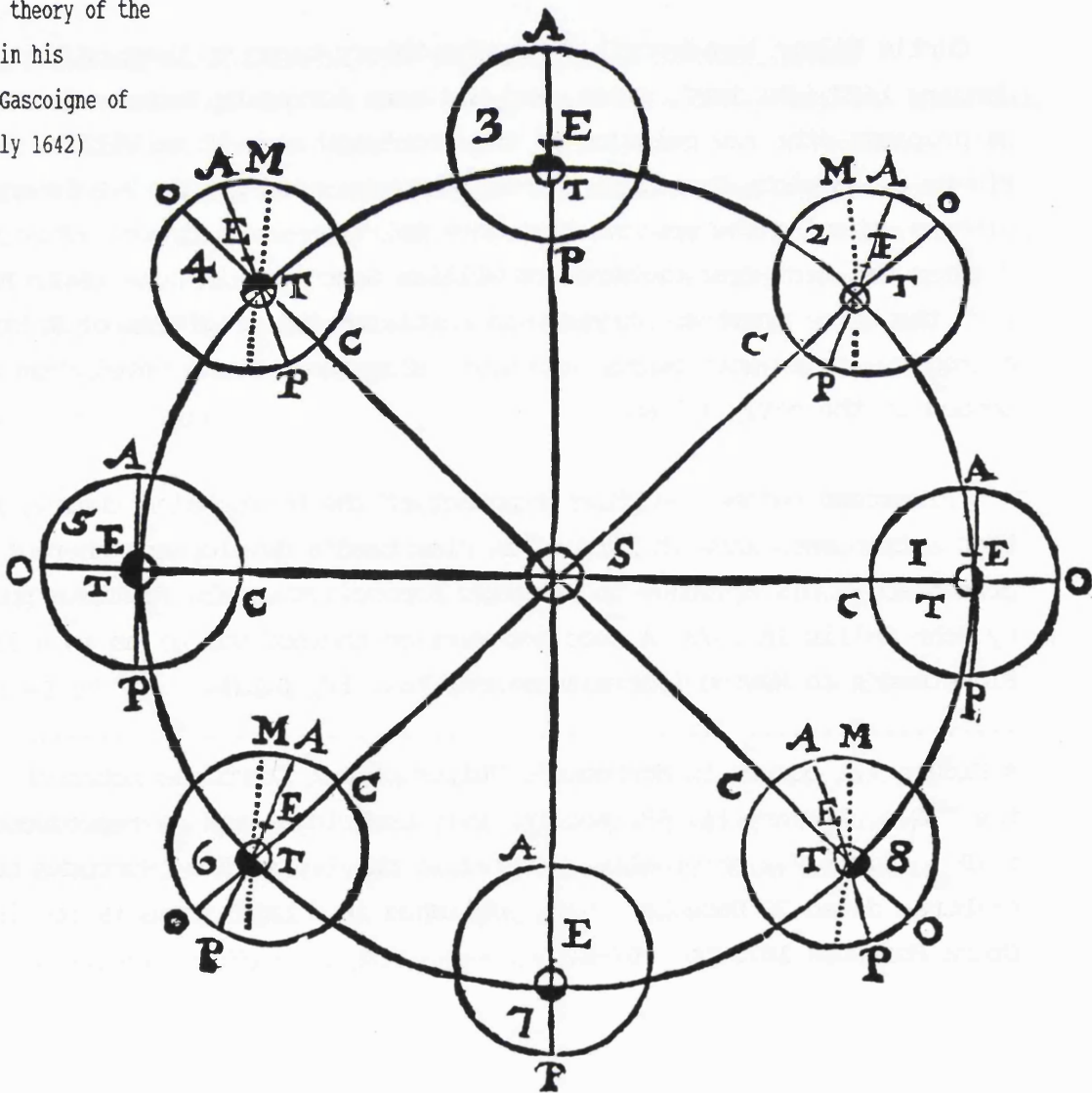


Figure 7.2:

Diagram drawn by William Crabtree to illustrate Horrocks' theory of the evection (in his letter to Gascoigne of June or July 1642)



Whiston referred to it as 'Mr Horrox's Lunar Hypothesis, as cultivated and explained by Mr Flamsteed' (1726, p.104). Before Flamsteed published this, Thomas Streete used what he called a Horrocksian scheme in his *Astronomia Carolina* of 1661, as he had gleaned it from Horrocks's notes, but I cannot claim to understand it.

### I A variable eccentricity

In dealing with the Horrocks model, one is to a large extent dealing with Hipparchus' concept of eccentricity, with its image of circular motion about an epicentre, where Earth's displacement from that epicentre is the eccentricity. Mathematically this is equivalent to the distance between a focus and the centre of an ellipse, if the circumscribing circle is of unit radius; one could suppose that this is what is really meant\*, however no seventeenth-century text states such a thing, nor does TMM contain any reference to an ellipse.

It was a model in which the apse line and eccentricity co-varied, by a similar amount and 180° out of phase, by a crank-wheel mechanism rotating once per 6½ months. To quote William Whiston, Newton's successor at the Lucasian chair at Cambridge, from a lecture of his given in 1703:

'...it is to be noted that the Eccentricity of the Lunar Orbit is mutable; and that the same, in the Conjunction and Opposition of the Apogee, is One and a Half of the Eccentricity, which is in the quadratures. So that TE the Distance between the Focus and the Center of the Ellipsis, in the position of her Orbit, marked [3] is One and a Half of the same Distance in the Position marked [5]'

(1726, p.104)

The figure to which Whiston was referring is Figure 7.2, Crabtree's illustration of the Horrocks model. In a section, 'To Determine the Earth's Eccentricity', Whiston explains how it is 'to be reckoned from Focus to Center.' All diagrams of the Horrocks theory without exception displayed .....

\* 'Kepler neglects the elliptical shape of the orbit in computing the evection... The error could not be very large, since the moon's orbit is constricted by only about  $e^2 = .002$  of its radius. ' (Stephenson,B., 1987, p.182, 186).



circular orbits - in Horrocks, in Crabtree, in Newton, in Flamsteed, and in Whiston - with displaced centres.

The Crabtree diagram displays the twin features of apse line oscillation and eccentricity variation, out of phase with each other. The primary motion of the apse line has been subtracted out, so that the diagram only depicts the secondary oscillation. It does not depict motion in sidereal space, because after one revolution of its eight stages, taking 411 days, the apse line will have revolved  $46^\circ$ , whereas it is represented by a vertical line in each phase.

Every  $6\frac{1}{2}$  months the Sun meets the apse line, depicted by steps 3 and 7 of the figure, with Sun at perigee and apogee respectively. These are supposed to depict maximum eccentricity, while steps 1 and 5 in contrast show minimum eccentricity. The octants of this diagram, when the Horrox-angle (our name for the Sun-apse angle) is  $45^\circ$  or  $135^\circ$ , correspond to the greatest size of the apse line's secondary motion. This amounts to some twelve degrees. *Here the dotted line MT depicts the mean apse.*

The model is straightforward to follow, provided we use the old, Hipparchus definition of eccentricity. A previous chapter, 'Finding the Anomaly' discussed how the first two laws of Kepler were encoded into the 'Equation of the Centre', which had indeed been used by Flamsteed in constructing his tables. Things would become rather complex, if one tried to picture Keplerian ellipses of varying eccentricity.

Whiteside in his tercentenary essay of 1976 affirmed that the young 22-year old Horrocks had constructed the following obscure edifice:

'The theory of the moon's motion thereby subsumed [ie, the Horrocks theory], by which (on conflating the Ptolemaic equation of excentre - essentially our modern elliptical inequality - and the evection from this) the lunar orbit is taken to be basically a Keplerian ellipse of periodically varying eccentricity with a corresponding fluctuation to and fro in the mean secular advance of its line of apsides, and further adjusted in fine by - in longitude - Brahe's twin inequalities of variation and the annual equation...' (p.318)

TMM contains no word about elliptical orbits and we follow its example. Figure 7.1 show the Horrocks diagram, as it appears in his unpublished *Philosophical Exercises* (This notebook is stored together with Flamsteed documents in the R.G.O. archives at Cambridge University Library, RGO 1.68B; Wilson, GHA p.197). Curtis Wilson's researches showed that Horrocks derived the diagram from van Lansberge's *Theoricae motuum coelestium*, but that he altered the theory involved, so that from the 'very inaccurate' Lansberge model, he constructed what remained for almost a century the finest available. I remained in the dark as to how the Horrocks model functioned, until I started to follow carefully the instructions given in TMM, its diagram being given in Figure 7.3. The two diagrams are basically identical, though separated in time by six decades.

## II TMM's Diagram

In the TMM diagram, there is an immobile Earth positioned at T, around which the Sun S revolves yearly, and an immobile mean apse line TB, around which we could picture the stars revolving every nine years. That is the required frame of reference in space and time. TF is the apse line varying by its second equation (its first equation, the annual, not being here represented). In Horrocks' version, Figure 7.1, a centre C to the lunar orbit is defined, but TMM refrained from specifying such. We are not told, for example, that C or F in Figure 3 represents the centre of a circle or ellipse of the lunar orbit.

According to Newton and Halley, the eccentricity was represented by the line TF in Figure 7.3, whereas according to Flamsteed it was represented by the projection of that line onto TB. These have the same maximum and minimum values, TB and TA respectively, but different mean values. The experts agree that the former is the correct view (from the modern equations for the evection inequality), but disagree over which was employed by Horrocks.

The previous chapter discussed (p.41) how the 'Equation of Orbit' could be considered as varying according to the sinusoidal function

$$6^{\circ}18'43'' + 1^{\circ}20'47''\cos 2\phi$$

In figure 7.3, STA is the Horrox-angle  $\phi$ , and FCB is defined as being of double its magnitude, ie  $2\phi$  - although TMM's diagram does not show it very well. The above formula thus gives us Flamsteed's version of the varying 'Equation of Orbit', if CF is equated to  $1^{\circ}20'47''$  (half the difference between minimum and maximum eccentricities), and  $6^{\circ}18'43''$  the mean value is equal to TC.

For the Newtonian version, we require the length of TF in terms of  $\phi$ . TC represents the mean eccentricity namely 0.055050 and CF is half the difference between maximum and minimum eccentricity, namely 0.011731. Applying the cosine formula in triangle FTC gives:

$$\begin{aligned} TF^2 &= TC^2 + FC^2 - 2FC \cdot TC \cos FCT \\ TF &= TC / (1 + FC^2/TC^2 + 2FC/TC \cdot \cos FCT) \\ &= 0.05505 / (1 + 0.2131^2 + 2 \times 0.2131 \cos 2\phi) \end{aligned}$$

or

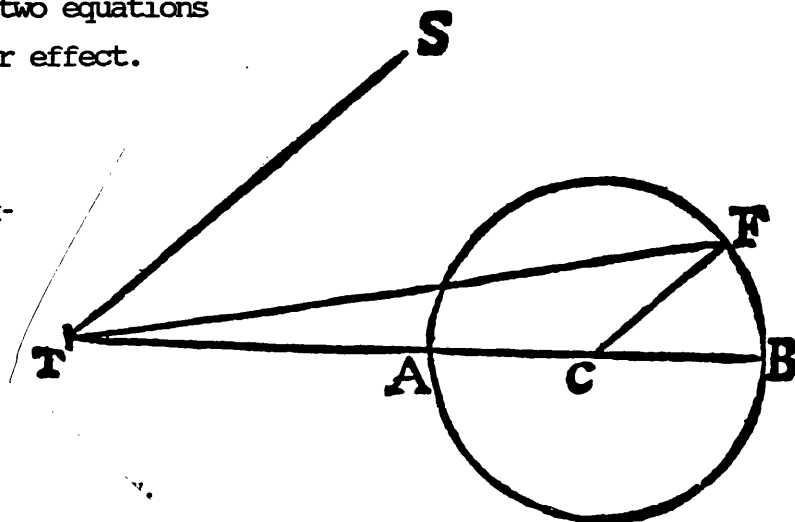
$$TF = 0.05505 / (1.0454 + 0.4262 \cos 2\phi) \quad (1)$$

That is our equation representing the Newtonian instructions in TMM. The important ratio CF/TC of 0.2131 represents the eccentricity fluctuation (Ch. 2 Section IV) of 21.3%. Using the same terms, Flamsteed's version as given above was simply:

$$\begin{aligned} \text{Eccentricity} &= TD = TC + CD && \text{(see Figure 4a)} \\ &= 0.05505 + 0.01173 \cos 2\phi && (2) \end{aligned}$$

We shall see later how the two equations differ considerably in their effect.

Figure 7.3: TMM's diagram of the Horrox-wheel, where angle PTB is the apse equation  $\delta$ , STB the Sun-mean apse angle  $\phi$  and FCB is  $2\phi$ , though not well drawn to scale (ST should be parallel to AF). TC is mean eccentricity, while TA and TB are its minimum and maximum values respectively.



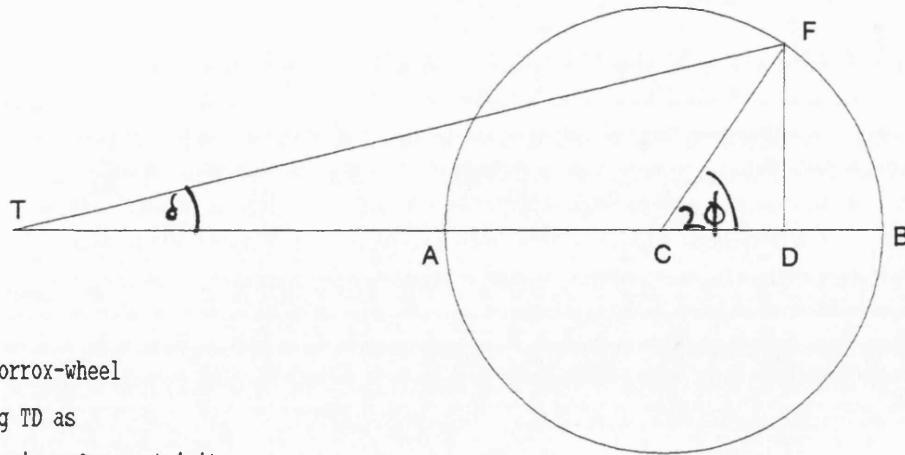


Figure 7.4a: Horrox-wheel diagram showing TD as Flamsteed's version of eccentricity and TF as Newton's. TB is the mean apse line and F is the centre of the lunar orbit.

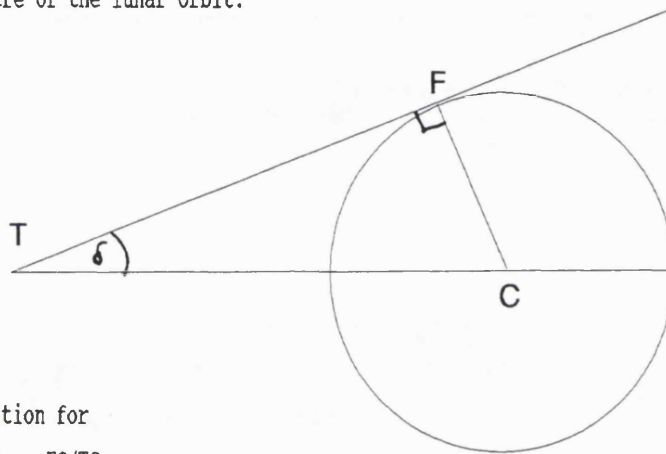


Figure 7.4b: the condition for maximum  $\delta$ , where  $\sin \delta_{\max} = FC/TC$ . TF is a tangent to the circle.

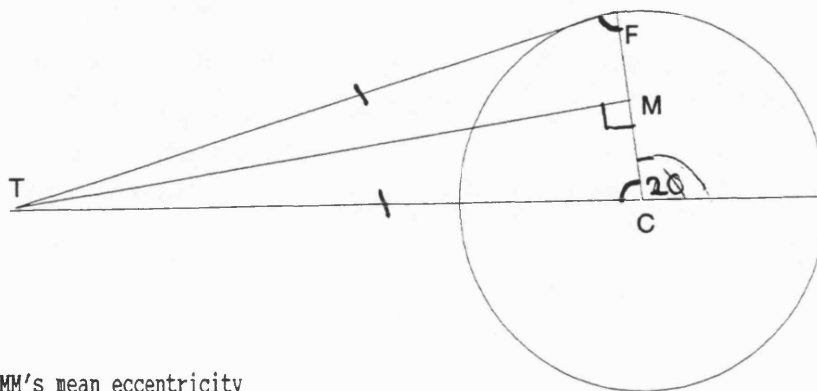


Figure 7.4c: TMM's mean eccentricity position:  $TF=TC$ , where TF is not tangential to the circle, then  $\cos \phi = CM/CT = FC/2TC$ .

To obtain 'Ye Equation of Ye Apogee' as Flamsteed called it, or the 'second Equation of the Moon's apogee' as TMM called it, we require the angle FTC. Following the example of Curtis Wilson in GHA we take this as  $\delta$ , then applying the sine formula to the same triangle gives

$$\begin{aligned} \sin\delta &= (0.011731 \sin 2\phi)/e, \\ &= \sin 2\phi / 85.25e \end{aligned} \tag{3}$$

In the next chapter, these procedures for obtaining the length TF and the angle  $\delta$  will be referred to as the functions f and g respectively.

### III Offbeat Octants

These Newtonian functions are asymmetric about the octants of Figure 7.2. In Figure 4b, the angle  $\delta$  will be maximal when TF is a tangent to the circle so TFC=90°. Then,

$$\sin\delta_{\max} = FC/TC = 0.2131 \tag{4}$$

so  $\delta_{\max} = 12^\circ 18' 15''$ .

This maximal value arises when the Horrox angle is given by  $\cos(180-2\phi) = 0.2131$ , or  $\phi = 51^\circ$ . It seems doubtful whether this asymmetry has any astronomical significance, but tables which follow the TMM instructions all have the apse equation peaking at 51° of the 'Annual Argument.' Table 7.1 shows the table which Newton sent to Flamsteed in April of 1695, with maximum value at 51°.

The three columns of this Table are each of 30°, so that the middle column at 21° is equivalent to 51° of the 'annual Argument', ie, the Sun-mean apse angle. The apse equation which there appears of 12°10'25" is the peak value given in the Table. It is larger than that given in DOS (11°47') and smaller than TMM's value of 12°15', indicating that TMM was not composed in this period. Adjacent to this column in the Table are the eccentricity values, and the mean value (55050 parts in 10<sup>6</sup>) appears as just after 48° of the annual argument.

The eccentricity values in Figure 7.2 reach their mean values at the octants, and indeed they did do in Flamsteed's version, given by equation

(2). In Newton's version however, when  $TF=TC$  (in Figure 4c) the eccentricity  $TF$  has its mean value: then  $\cos FCT = (180-2\Phi) = CM/CT =$

Table 7.1: Table sent by Newton to Flamsteed on 23 April 1695, with three columns 0-30°, 30-60° and 60-90°, for the Horrox angle ('Annual Argument'); showing mean eccentricity of 55050 generated by such an angle of 48-49°, while maximal apogee equation (12°10'25") falls at 51°.

The Equations of the Moons Apoge & the Excentricities of her Orbit in such parts as the radius is 1000000.

Add the Equations of the Apoge							
Annual Argument	Sign 0	Excentr.	Sign 1	Excentr	Sign 2	Excentri	Annual Argument
	° ' "	parts	° ' "	parts	° ' "	parts	
0	0 0 0	66850	9 22.50	61855	11.32.17	50406	30
1	0.20.54	66845	9.36.57	61537	11.22.59	50022	29
2	0.41.46	66827	9.50.31	61211	11.12.37	49645	28
3	1. 2.38	66798	10. 3.40	60878	11. 1.10	49274	27
4	1.23.27	66757	10.16.14	60438	10.48.39	48908	26
5	1.44.12	66705	10.28.17	60192	10.35. 2	48551	25
6	2. 4.54	66638	10.39.47	59838	10.20.21	48201	24
7	2.25.31	66562	10.50.41	59479	10. 4.36	47859	23
8	2.46. 0	66475	11. 0.58	59113	9.47.47	47527	22
9	3. 6.24	66375	11.10.40	58742	9.29.55	47204	21
10	3 26.41	66265	11.19.42	58366	9.10.59	46891	20
11	3.46.50	66146	11.28. 5	57986	8.50.58	45266	19
12	4. 6.48	66012	11 35.46	57600	8.29.57	45040	18
13	4.26.37	65870	11.42.44	57211	8. 7.57	44829	17
14	4.46.15	65716	11.48.58	56819	7.44.58	44633	16
15	5. 5.41	65549	11.54.27	56422	7.21. 1	44452	15
16	5.24.55	65373	11.59.11	56023	6.56. 8	44287	14
17	5.43.53	65185	12. 3. 6	55622	6.30.23	44138	13
18	6. 2.38	64988	12. 6.12	55218	6. 3.49	44824	12
19	6.21. 9	64779	12. 8.28	54814	5.36.28	44628	11
20	6.39.22	64562	12. 9.53	54408	5. 8.22	44447	10
21	6.57.20	64343	12.10.25	54001	4.39.34	44283	9
22	7.14.55	64094	12.10. 1	53595	4.10. 8	44134	8
23	7.32.14	63847	12. 8.43	53190	3.40.10	44003	7
24	7.49.11	63590	12. 6.28	52784	3. 9.41	43888	6
25	8. 5.47	63323	12. 3.16	52381	2.38.45	43789	5
26	8.22. 0	63046	11.59. 6	51980	2. 7.27	43709	4
27	8.37.51	62761	11.53.58	51581	1.35.51	43647	3
28	8.53.17	62467	11.47.45	51185	1. 4. 2	43602	2
29	9. 8.17	62165	11.42.14	50794	0.32. 3	43575	1
30	9.22.56	61855	11.32.17	50406	0. 0. 0	43566	0
	Sign 5		Sign 4		Sign 3		
	Sign 11		Sign 10		Sign 9		
Subtract the Equations of the Apoge							

calculo proprio  
correcti sequentes  
46891 numeri

46588 JF  
46298 Maij 4 h  
46019 1695  
45753  
45500

45260  
45034  
44823½  
44628  
44447

mean value  
55050

0.2131/2, giving  $\phi=48^\circ$ . The 'Newtonian' tables for eccentricity therefore reach their mean value at  $48^\circ$ . Again, I am not clear as to whether this has an astronomical meaning. It would give one pair of octants (moving from conjunction to square) a different form of motion from the other two (from square to conjunction).

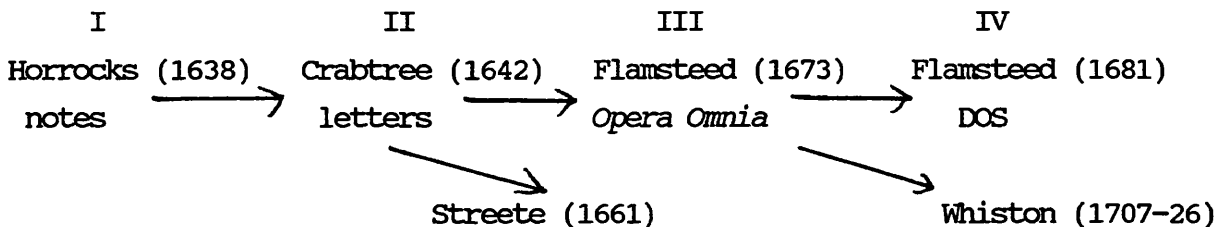
Thus, the 'Horrocksian' table of lunar eccentricity which Flamsteed published in 1673 (*Lunares Numeri Ad Novam Lunae Theoriam*, p.480) resembles the equivalent table in DOS in having a mean value at  $45^\circ$  of the Horrox angle, and so being symmetrical about the octants. This difference offers us a simple and distinctive fingerprint whereby we should be able to recognise who in the eighteenth-century was using Newton's version of the Horrocksian mechanism.

#### IV Halley's Contribution

*'Halley afterwards made a slight alteration; but hardly, I think, enough to justify Newton's assertion.'*

(William Whewell, *History of the Inductive Sciences*, Vol.1, p.466)

An adjustment to the Horrocksian model was recommended by Halley to Newton, which the latter regarded as quite valuable. Confusion has arisen over this matter, for the resolution of which we need to review the development of what was regarded as Horrocks's lunar theory. It took place in four stages:



The first coherent account of Britain's first lunar theory emerged from Salford, now a suburb of Manchester, in June of 1642, as penned by William Crabtree to Gascoigne from notes left by Horrocks. They had both been his colleagues. Crabtree cast the new theory into seven steps, the third of which is here of interest to us. In the 1673 publication of Horrocks's

Works, Flamsteed 'polished' Horrocks's method, to use his expression, chiefly by inserting his own Equation of Time in place of the imaginary Keplerian 'equation of physical parts' in which Earth's rotation rate altered through the year. In 1681, Flamsteed adjusted some of the mean motions as compared with his earlier 1673 statement, but otherwise left his method unchanged. Whiston merely repeated the DOS procedure, presenting it to his students in the early decades of the new century as the best lunar theory available.

Crabtree's letter was reprinted in the collection assembled by Flamsteed in 1673 as the posthumous works of Horrocks. His third step specified the manner in which eccentricity varied in the new theory:

'3. Duplicetur Argumentum annum, & duplicati Co-sinui addatur 3,065206 (Logar. numeri 1162, semi-differentiae inter mediam & extremam Excentricitatem) prodibit Logarithmus numeri addendi Excentricitatem mediae 5524, si duplum Arg. annui fit in 4° vel 1° quadrantibus, alias subtrahendi, & habetur Lunae excentricitatis vera.'

(Horrox, 1673, p.469\*)

This text describes the addition of mean eccentricity (the line TC in Figure 4a), here given the value of 5524, and what in the previous chapter we described as  $FC \cos 2\phi$ . FC as the radius of the Horrox-wheel here has magnitude 1162, being the amplitude of the sine function - described by Crabtree as half the difference between maximum and minimum eccentricity - and the cosine is of twice the 'annual argument' as it was called. Terms

.....  
\* Horrocks's own words on the subject are to be found in his notebook, *Philosophical Exercises* (RGO 1.68 B, Second Part, section 19), entitled, 'A New Theory of the Moon': '...to the sine of the remainder adde 306446 (the logarithme of 1160, or halfe the difference of the greatest and least eccentricity of the Moon) so have you the logarithm of a number to be added to 5493 (the middle eccentricity)...so have you the moons eccentricity' What Horrocks meant by 'the remainder' pertained to double the 'Sun's distance from ye moon's apogaeum or perigaum'. These notes have never been published.



are converted to logarithms and instructions given as to whether the  $FC\cos 2\phi$  term is added or subtracted by quadrant - done automatically by our cosine function. Horrocks's method projected the rotating radius vector FC onto the mean apse line TC to define the altering eccentricity.

Flamsteed adopted this account, whereby eccentricity was represented by the projection of the line TF (see Figure 3) onto TB. In the early 1690s, Halley came to disagree with this, averring that the eccentricity should rather be represented by the line TF itself. We lack any statement by Halley on this matter, which is regrettable. Flamsteed reported it to Newton, evidently puzzled, and the latter's reply a week later was,

'By your observations I find it to be a very good correction. I reckoned it a secret which he [Halley] had entrusted with me; and therefore never spake of it till now.'

(Correspondence, IV, p.34, letter of 24 October, 1694)

In the *Principia* of 1713, Newton gave this curious account thereof:

'Our countryman Horrox, was the first who advanced the theory of the moon's moving in an ellipse, about the earth placed at one focus. Dr Halley improved the notion, by putting the centre of the ellipse in an epicycle whose centre is uniformly revolved about the earth; and from the motion of the epicycle the mentioned inequalities in the progress and regress of the apogee, and in the quantity of eccentricity, do arise.' (PNPM, p.475)

The first sentence describes the achievement of Johann Kepler, attributing it to Jeremiah Horrocks, and the second describes the achievement of Jeremiah Horrocks, attributing it to Halley. We may add that the uniform revolution alluded to has a nine-year period: if Earth forms one focus of an ellipse, then the 'centre of the ellipse' will revolve round it as the apse line moves once round the zodiac.

This is all that PNPM has to say about Halley's contribution, though it bears little relation to the letters of October, 1694. Flamsteed presented the Horrocksian method to Newton in his letter of 11 October, and then added, referring to a diagram: 'But he [Halley] affirms that not Cx but CI is the excentricity in this & all other cases'. His letter then goes on to

describe how Thomas Streete (in his *Astronomia Carolina*) made a slight adjustment to the Horrocksian model, and for what purpose this was done.

It was fairly well known that Kepler had applied his laws to the lunar orbit but had not thereby made much progress (Wilson, 1987,p.80). An article by S.B.Gaythorpe, F.R.A.S. in 1957 on *Jeremiah Horrocks and his 'New Theory of the Moon* commented upon the *Principia's* text, that it  
'...does not indeed seem a particularly striking claim to fame, but the sentence implies more than it immediately conveys. Before Horrox no one had attempted to take an ellipse as the basis, so to speak, of the Moon's path, on account of the number, size, and rapid variation of the periodic inequalities involved, and the difficulty of combining them with other than circular motion.' (p.134)

As well as endorsing the *Principia's* version of Horrocks originality, in a manner that is questionable, Gaythorpe concluded that Horrocks had omitted a certain factor ( $\sec \delta$ ) in the eccentricity formula, and thereby '...he lost the honour which Newton gave instead to Halley...' (p.137)  $\sec \delta$ , or  $\cos \delta$ , is the factor by which the two recipes disagree (see Figure 4a).

A different viewpoint appeared in Whiteside's essay on the subject (1975, p.325, note 10) affirming that Halley had not made any innovation, but had merely adopted Horrocks's method:

'...it was Flamsteed's understanding (founded on a passage in Horrocks where he himself uses this simplification for ease of calculation) that the eccentricity of the lunar ellipse is not - as Horrocks himself indubitably took it to be in his basic precepts - TF [in Figure 3a] but the projection of this on to the mean apsis-line.'

Whiteside gave as his authority Gaythorpe (1956, p.137). As we have seen, however, this was not Gaythorpe's view.

Curtis Wilson in the GHA endorsed the Whiteside view:

'On one point of interpretation Flamsteed went astray, thereby deeply changing the structure of Horrocks's theory: in place of the varying eccentricities intended by Horrocks, he used their projections onto

the mean line of apsides, so diminishing their values by the factor  $\cos \delta$ . The mistake was later perceived and corrected by Halley.'

(GHA, p.199)

What Halley imparted to Newton as a secret, and to Flamsteed as his own idea, is viewed as merely a reversion to the earlier model. (See also *Correspondence*, vol. IV, p.32, note 6, discussing the above-mentioned letter of 11 October).

This Whiteside - GHA viewpoint detracts from Halley's originality and Flamsteed's competence. We cannot readily appreciate what people then meant by 'Horrocksian' if we adopt it. For example, in 1710 a Mr Cressner published in the *Philosophical Transactions* a comparison of two different longitude computations, one which he called Newtonian and the other Horroxian (P.T., 27, pp.16-19). The latter turned out to be William Whiston's *Praelectiones astronomicae* (1707) version of Flamsteed's 1681 DOS procedure.

We rather adopt the more traditional Forbes-Gaythorpe view, whereby Flamsteed's eccentricity procedure was simply that of Horrocks; whereby Halley's proposal was indeed an innovation; implying that Whiteside erred in believing Flamsteed failed to comprehend the nature of the theory which he brought from the North Country and ushered into the light of day. A later chapter will look at the question of how much difference was made by this adjustment, whether it was a 'very good correction' (Newton) or but a 'slight alteration' (Whewell).

Forbes affirmed that a geometrical construction for the altering eccentricity was supplied 'for the first time' by Flamsteed in his epilogue to the Horrox *Opera Posthuma* of 1673 (Forbes, 1975, pp.63-67). One could query such a claim on the grounds that the above Figure 7.1, showing his eccentricity equation and deferent-wheel, are from the Horrocks's 'Philosophical Excercises' adjacent to the passage above-quoted. The area has been little investigated by science historians.

### V Linkage of e and δ

How well do the apse and eccentricity equations tie up together? According to TMM, the maximal value of the Equation of the Apogee is a simple function of the varying eccentricity, as in the above equation (4). However, TMM's figures are not quite consistent with this relationship:

	<u>Eccentricity</u>	<u>Eqn of Apogee</u>	<u>Theor. Eqn.</u>
Horrocks	0.05524 ± 0.01162	11°47'22"†	12°8'35"
Flamsteed		11°47'22"	
Newton (1695)		12°10'25"	
(1702)	0.055050 ± 0.011731	12°15' 4"	12°18'15"
" (1713)	" "	12°18'.	"

What has here been called the theoretical greatest apse equation was derived from equation 4 in Section III, inserting the given eccentricity value. Not prior to the *Principia's* second edition were the values interlinked in accord with TMM's geometrical model. It thus appears that the form of the Horrocks model, above described, was originated by Newton. One cannot say which of these equations of apogee has the 'correct' value, as the concept is not used in modern lunar theory.

As was remarked earlier, Dunthorne in his *Practical Astronomy of the Moon* had faithfully reproduced the instructions of TMM in drawing up tables, etc, but gave an Equation of the Apogee as 12°18'15", and it now becomes clear that Dunthorne has simply calculated its value from the model, as it should be. In fact, this difference of three arcminutes is quite immaterial.

.....  
† In his letter of June 21 1642 to Gascoigne, William Crabtree cited the value above-quoted, in turn cited by Gaythorpe, (1956, p.137) as Horrocks' value. However, Horrocks' *Philosophical Exercises* notebook states 'The greatest *aequatio apogai* is 12°30' (RGO 1.68B,17). In 1675, Flamsteed in a letter to the *Philosophical Transactions* averred that: 'I find by Mr Horrockses papers, that he used at first 12° precise, but upon farther experience diminished it to 11°48.' (*Phil. Trans.* 1675, Vol.110 pp.369-70.)

Godfray's *Lunar Theory* of 1871 discussed the interlinkage between these two functions, and his treatment was recommended by Whiteside (1976, p328, note 47). Turning to the page recommended by Whiteside (Godfray p.70), we find equations given for the two above-defined functions in error by fifty percent:

$$\delta = 15m/8 \sin 2(\alpha' - \ominus)$$

and

$$E = e(1 + 15m/8 \cos 2(\alpha' - \ominus))$$

where  $\delta$  is the second equation of apogee,  $E$  is the varying eccentricity,  $e$  is mean eccentricity,  $m$  is the ratio of lunar tropical month/solar tropical year, and  $(\alpha' - \ominus)$  is the Horrox angle between apse and Sun. This gives maximal values for  $\delta$  of  $8^\circ$ , and an eccentricity fluctuation of a mere 12%, whereas it has to vary by 21% according to TMM. This may serve to remind us how difficult a matter is our subject, and how easy it is to err therein.

A more reliable maximal value for  $\delta$  was derived by Gaythorpe (1925, p.859, 1957, p.136) as  $\arcsin(\epsilon/2e)$ , where  $\epsilon$  is the coefficient of the evection term ( $1.274$ , see below). This is equal to  $11^\circ 39'$ , using the TMM value of eccentricity. The 'correct' value of this vitally important constant thereby appears as closer to Horrocks's final value, than to the considerably higher value which Newton, following Halley's advice, gave to it. Gaythorpe derived this value by showing how the modern evection and equation of centre terms were equivalent to a single equation of centre term using an oscillating apse line,  $\delta$  being its maximal oscillation.

## VI Not the Evection

'By thus coupling the libratory motion of the apse line AP with a variable eccentricity, Horrox (and subsequently Flamsteed) united the two principal lunar inequalities: namely, the equation of the centre and the evection.'

(Forbes, 1975, Ch.4, p.65.)

The first three of the modern equations of the lunar orbit are,

6.288 sin $M'$	or in our symbols: sin (M-A)
+1.274 sin (2D-M')	sin [(2(M-S) - (M-A)]
+0.658 sin 2D	sin 2(M-S)

where the first term is the elliptic inequality, the second is the evection and the third is the variation.  $M'$  represents the Moon's mean anomaly, ie distance from its mean apse in longitude,  $D$  its mean elongation ie distance from the mean Sun, and  $M$ ,  $S$  and  $A$  the mean positions of Moon, Sun and apogee. The Horrocks model conflates the first two of the above equations. How its performance compares with these, is something we may hope to apprehend in due course.

The evection has the characteristic that one cannot picture it, as varying with the sine of twice the elongation minus the anomaly. In this it contrasts with the kinematic model we are considering, which is wholly visual. The evection was named by Ishmael Boullieau in 1645, however its meaning varied rather (GHA, p.195). As the 'second inequality', it was discovered by Ptolemy, who fixed its maximum value at  $1^{\circ} 19'$ , described by Dreyer as 'very near the true value' (1953 *History of Astronomical Observations*, p.195). Its modern meaning, as having a period of  $31\frac{1}{2}$  days, developed in the later eighteenth-century.

## Ch. 8 THE SEVEN MOONS OF TMM

'...and the Moon's Place will be equated a seventh time, and this is her Place in her proper Orbit.'

TMM

Newton originated the concept of seven steps of equation as his distinctive approach to lunar theory, in his TMM of 1702. As he discerned just seven colours in a rainbow in 1675, and as his *Optics* of 1704 found seven steps of colouration in his 'Newton's Rings'\*, so in like manner he found seven steps appropriate for his lunar endeavours. We have seen how William Crabtree's formulation of the Horrocksian theory in 1642 described seven steps, which may also have influenced him.

This sevenfold structure became a distinctive hallmark of the various 'Newtonian' ephemerides that utilised TMM. To quote Dr Waff, "...nearly all *new* lunar tables constructed during the first half of the eighteenth century utilised in some fashion his [Newton's] tabular theory."

(Cohen,1975, p.79) That is a strong and bold claim by Dr Waff, but regrettably it has never been substantiated. It will here be investigated in due course. By 'tabular theory' Dr Waff was presumably referring to Newton's seven steps of computation. Leadbetter's *Uranoscopia* of 1735 contained the seven steps, Le Monnier's *Institutions* of 1746 in Paris contained them, as (mainly) did Halley's *Astronomical Tables* of 1752. Thus, its shadow stretched over half a century, greatly ignored by science historians.

The first summary of TMM's seven steps in trigonometric form was given by Francis Baily, President of the newly-formed Royal Astronomical Society .....

\* P.Gouk, 'The Harmonic Roots of Newtonian Science', in *Let Newton be!* Ed Fauvel et. al., 1988. For the sevenfold pattern of 'Newton's rings' see D.Castillejo, *The Expanding Force in Newton's Cosmos*, Madrid 1986, p.97. Castillejo also noted that *Optics* was composed in seven sections.

(1835, p.742), as follows:

Table 8.1: Baily's account of the 'Newtonian Rules':

I)	11' 49"	the annual equation
II)	3' 45" sin 2(D-A)	
III)	47" sin 2(O-Ω)	
IV)	Equation of centre, including evection	
V)	35' 15" sin 2D	the Variation
VI)	2' 10" sin (2D+a-A)	
VII)	2' 20" sin D	

Baily gave no details beyond this bare outline. He pointed out that four of the equations were entirely new, namely numbers two, three, six and seven. The magnitudes of the sine functions in Baily's summary were mostly mean values, and as we shall see they are made to vary, in relation to several different cycles. (His symbols are different from those used here: he took D as the Sun-moon angle, Ω as node, O as Sun, and a, A as solar and lunar anomalies).

The instructions of TMM have here been translated into a sequence of machine-readable functions. I accomplished this in the winter of 1991/2, and then with the aid of a computer expert, Mr Jonathan Loretto, it was written onto a 'Lotus 1-2-3' program. As input this program takes the time in days after noon GMT on December 31, 1680 Old Style, and as output it gives lunar longitude. Its latitude function is described later (Ch.9, V). Figure 8.1 is a diagram of TMM's sequence of operations. I was startled to discover that the program based on TMM did rather accurately accord with the heavens, at least around the time of its composition.

The seven 'steps of equation' are here presented as a sequence of additive functions, and are given without explanation. The reason for this, is that it seemed preferable to start with the complete sequence, showing its structure, and then in the next chapter to justify each step. The program starts with a given time, which defines five different mean motions (Chapter Five), and these mean motions become modified by successive 'equations'. A sequence of interactions takes place, ending with a seventh-



time equated moon. Later in the chapter some equivalent algebraic terms are given, for each step.

The sequence here presented contains no twentieth-century astronomical constants, using only those given in TMM; and, with only one exception, it contains no modern equation: it does include the 'equation of centre' formula as discussed in Chapter Six, since TMM merely states that tables for the equation of centre were to be compiled, implying that a standard procedure was to be followed, and merely gives maximum and minimum values for it. TMM's instructions on how to accomplish the 'reduction', ie conversion to the plane of the ecliptic, are also rather brief, this being a quite standard operation. Thus, with only one exception, what is here presented is merely:

"a translation from the hieroglyphics of geometry into what is now the vernacular language of science [ie, algebra],"

- as was claimed by Stevenson's 1834 opus, *Newton's Lunar Theory Exhibited Analytically* (1934, Preface). However, as was indicated earlier (Ch.1, VII), what Mr Stevenson presented was not in fact the Newtonian procedure, but an idealised version thereof, resembling the mid-eighteenth century French theories and quite lacking the Horrocksian mechanism (Cohen, 1975, p.79). Such a translation of TMM into 'the vernacular language of science' is here accomplished for the very first time.

Checks that were used to test the program have been included as shown below, together with a complete worked example in the form of the case-study by Richard Dunthorne, a Cambridge student who prepared tables which adhered closely to TMM. He published this in 1739 as *Practical Astronomy of the Moon: or, New Tables of the Moon's Motions*, the purpose of which was to see how well TMM actually worked. Dunthorne put his maximum equation of apogee at  $12^{\circ} 18' 15''$ , as given in the third edition of PNP, whereas TMM had given it as  $12^{\circ} 15' 4''$ , that being the sole difference.

While the equations below were all bar one derived from the instructions of TMM, I was at times uncertain about the signs, especially for the nodal equations. A worked example given in Dunthorne was here useful for checking that the addition and subtraction of the trigonometric

functions was proceeding correctly, for the varying angles. Dunthorne's 1739 opus appears to me as the one work which has embodied 100% the TMM rules in its lists of tables and instructions on how to use them. A convention has here been adopted that the faster orb was always subtracted from the slower, for example the solar 'anomaly' is represented by (H-S); bearing in mind that  $\sin(A-B) = -\sin(B-A)$  and  $\cos(A-B) = \cos(B-A)$ . The discussion of the four new Newtonian equations given in GHA (p.267) was also of assistance in rightly applying their signs.

The treatment of the 'equation of the centre' using the Horrocksian model is far larger than any other of TMM's 'equations', and is positioned in the centre of the seven steps, so that there are three antecedent stages and three following. The fifth stage comprises the well-known inequality discovered by Tycho Brahe called 'Variation'.

## **I TMM in Machine-Readable Form**

The five variables, measured in degrees of zodiac longitude from zero Aries, are: Moon M, Sun S, apogee A, aphelion H and node N. These have motion in degrees/day, and values from zero to 360°. They depend on time t, measured in days from noon G.M.T. Dec. 31, 1680 Old Style. The five variables have these starting positions at time zero and speeds of motion:

$$M = 181.763 + 13.17639535 \times t$$

$$S = 290.580 + 0.98564697 \times t$$

$$A = 244.468 + 0.1114083 \times t$$

$$H = 97.392 + 0.0000479 \times t$$

$$N = 174.243 - 0.0529551 \times t$$

These are the mean motions. These linear functions can be checked by putting t equal to 7305, the number of days in twenty Julian years. This will give the following positions for TMM's second epoch date (Ch.4, IV).

### Test One:

for t=7305,

$$M = 315.331$$

$$S = 290.731$$

$$A = 338.306$$

$$H = 97.742$$

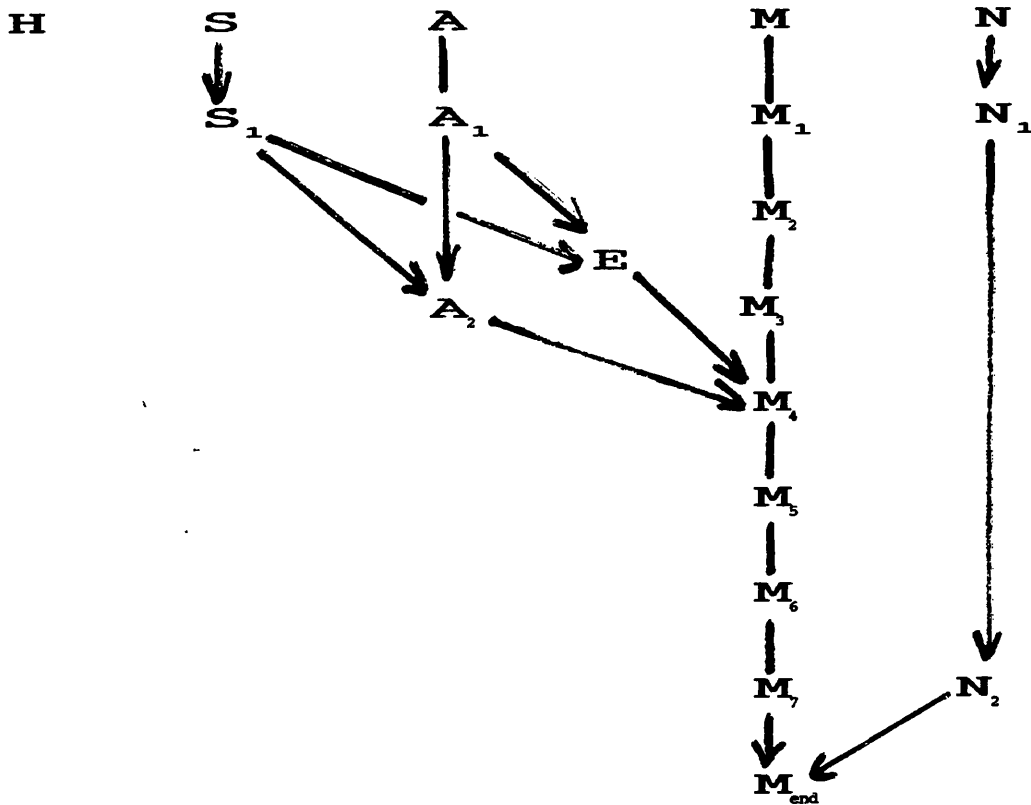
$$N = 147.406$$

These five longitude values are those specified by TMM for noon on December 31, 1700, confirming that the mean motions tie up with those specified. A 'modulo' function is employed to retain the value of each function within 0-360°.

The following flow-chart outlines the sequence of interaction of these five variables through the seven steps, with angles measured in degrees.

Figure 8.1

The Steps of Equation in TMM



THE FOUR FUNCTIONS f,g,h and j:

TMM-PC utilises four functions, whose operation may be outlined as follows:

$$\begin{aligned}
 f: & \quad A_1 + S_1 \rightarrow E \\
 g: & \quad \quad \quad A_1 \rightarrow A_2 \\
 h: & \quad E + A_2 + M_3 \rightarrow M_4 \\
 j: & \quad \quad \quad N_1 \rightarrow N_2
 \end{aligned}$$

Function 'h' applies the equation of centre, which gives radian measure (Ch.6,II) and so has a  $180/\pi$  conversion factor to bring it into degrees.

The next chapter will explain the derivation of these functions while here we merely describe them. They are as follows:

Eccentricity from Horrox angle (A-S):

$$f(A-S) \rightarrow 0.05505 \times \sqrt{\{1.0454 + 0.4262 \cos 2(A-S)\}}$$

Second Apse Equation from the Horrox angle:

$$g(A-S) \rightarrow \arcsin \left\{ \frac{\sin 2(A-S)}{85.24 \times f(A-S)} \right\}$$

Equation of Centre from lunar anomaly and eccentricity:

$$h(E, A-M) \rightarrow [2E \times \sin(A-M) - 1.25 \times E^2 \times \sin 2(A-M)] \times 180/\pi$$

The Node Equation:

$$j(N-S) \rightarrow \arctan \left\{ \frac{\sin 2(N-S)}{38.33 + \cos 2(N-S)} \right\}$$

The following Test Two will check whether the functions are working.

For	f, put A-S = 48°	→	0.05507,
	g, put A-S = 48°		12.23
	h, put A-M = 30° and E= 0.05		2.747
	j, put N-S =120°		-1.311

### The Seven Steps

The steps of equation are inserted in accord with the above flow-diagram. Thus, the apogee first-equated ( $A_1$ ) feeds into functions f and g, then adding g to  $A_1$  gives the apogee second-equated, which in turn feeds into function h, the equation of centre, to give  $M_1$ . The node on the other hand only receives its second equation after the seventh step.

STEP ONE - the annual equation

$$S_1 = S + 1.939 \times \sin(H-S) - 0.0205 \times \sin 2(H-S)$$

$$M_1 = M - 0.197 \times \sin(H-S)$$

$$A_1 = A + 0.333 \times \sin(H-S)$$

$$N_1 = N - 0.158 \times \sin(H-S)$$

This step begins from the 'mean motions', linearly time-dependent functions with modulo 360°, as given earlier.

STEP TWO  $M_2 = M_1 + [6.25 - 0.31 \times \cos(H-S_1)] \times \sin 2(A_1-S_1) \div 100$

STEP THREE  $M_3 = M_2 + 0.0131 \times \sin 2(N_1-S_1)$

STEP FOUR Put  $E = f(S_1, A_1)$   
 and  $A_2 = A_1 - g(S_1, A_1)$   
 then  $M_4 = M_3 + h(E, M_3, A_2),$

STEP FIVE - the Variation

$$M_5 = M_4 + [0.5923 - 0.0312 \times \cos(H-S_1)] \times \sin 2(M_4-S_1)$$

STEP SIX  $M_6 = M_5 + 0.0361 \sin(S_1-M_5+H-A_2)$

STEP SEVEN  $M_7 = M_6 + [0.0389 + 0.015 \times \cos(H-A_2)] \sin (S_1-M_6)$

REDUCTION Put  $N_2 = N_1 - j(S_1, N_1)$   
 then  $M(\text{end}) = M_7 + 0.1160 \times \sin 2(N_2-M_6)[1+0.0586\cos 2(N_2-S_1)]$

The following Test Three checks the entire sequence of equations, utilising Dunthorne's worked example (1739, pp. 50-59; Table 8.2), which took the instant of 3.40 pm on January 2nd 1737. Conversion to a TMM t-value as defined gives 20456.1528 days. This position has been used as a standard test for setting up the program. The TMM Lotus spreadsheet for this instant is given in Appendix V, which is comparable to Table 8.2. The following positions are generated by TMM-PC for this instant:

M=	80.119		M <sub>1</sub> =	80.069
S=	293.124		M <sub>2</sub> =	80.112
A=	3.453		M <sub>3</sub> =	80.124
H=	98.372	E = 0.04670	M <sub>4</sub> =	74.815
N=	170.985		M <sub>5</sub> =	74.207
			M <sub>6</sub> =	74.186
S <sub>1</sub> =	293.628		M <sub>7</sub> =	74.163
A <sub>1</sub> =	3.538		M <sub>end</sub> =	74.142 = 74°8'31"
A <sub>2</sub> =	354.212			
N <sub>1</sub> =	170.945			
N <sub>2</sub> =	169.572			

For this date and time, the t-value fed into the TMM-PC program was 20456.1528 days. One can be a day out in computing these t-values owing to leap-years, and so the solar values are first checked to see if they concur. A Lotus 1-2-3 spreadsheet has the form of a flow diagram, where from the t-value inserted at the top, all the other values are defined. It shows merely the figures generated at each stage, but not the functions that produced them. A page is reproduced in Appendix V.

The mean motions concur with those of Dunthorne within an arcsecond, confirming what was said in Chapter Five that 'the Newtonians' in this period based their mean motion tables firmly upon TMM. The programme has TMM's value for lunar tropical motion defined to ten figures as

$$13^{\circ}.17639535/\text{day}.$$

That level of accuracy is vital if results are to be quoted to arcseconds. It differs from the then 'true' value, that is to say as interpolated into historical time using Meeus's modern values (Ch.5,II), of

$$13^{\circ}.1763967/\text{day}.$$

The difference only appears in the sixth place of decimals, but without this accuracy our mean values would never concur so well with those of amateur astronomer Richard Dunthorne. For the slower, solar mean motion, eight figures appear as sufficient, for arcsecond accuracy. For comparison, the equations for locating correct lunar mean motions in historical time (Appendix II) require an eleven-figure term.

The final results differ in ecliptic longitude by eighteen arcseconds, which is tolerable. Richard Dunthorne was an eminent astronomer in his own right: it was he who first established Edmond Halley's conjecture of the secular acceleration of the Moon. Halley had proposed in 1696 that such an effect was causing eclipses in antiquity to be displaced by an hour or so from their expected times, but he never showed any computations on the matter. Dunthorne did this, and Brewster's *Memoirs* of Isaac Newton referred to Dunthorne in this context.

We can inspect the 'equations' for each of the seven stages, comparing Dunthorne's with TMM-PC (see over). The largest discrepancy is ten arcseconds, in the sixth equation. A 'correct' answer is given from a

modern program, showing how final values err by nearly seven minutes of arc, which is rather shocking considering that it is thrice the maximum error claimed by Gregory for TMM in 1702. Later on, we may hope to discover how often TMM would generate an error of such magnitude.

	<u>Dunthorne</u>	<u>TMM-PC</u>		<u>Dunthorne</u>	<u>TMM-PC</u>
Eqn (1)	-3' 2"	-3' 0"	1st node eqn:	-2' 27"	-2' 24"
(2)	+2' 33"	+2' 32"	2nd node eqn:	-1° 22' 23"	-1° 22' 23"
(3)	+43"	+42"			
(4)	-5° 18' 29"	-5° 18' 30"	1st apse eqn:	5' 9"	5' 5"
(5)	-36' 28"	-36' 28"	2nd apse eqn:	9° 19' 27"	9° 19' 34"
(6)	-1' 27"	-1' 17"			
(7)	-1' 28"	-1' 20"	eccy. (x10 <sup>6</sup> ):	46703	46705
Reduction	-1' 17"	-1' 16"			
Final ans.	74° 8' 13"	74° 8' 27"			
Correct value:	74° 15' 3"				

The above 'correct' value was obtained using a copy of the I.L.E. program kindly supplied by Dr. Bernard Yallop at the Royal Greenwich Observatory, said to be accurate to within a second or two of arc in historical time, which contains sixteen hundred terms.

Omitting the mean motions, we can cast the central chain of equations into a more algebraic format, as follows. The constants utilised have been listed at the end of the chapter. Concerning the signs of the functions, suppose for example one were doubtful about that present in "1-3Ecos(H-S)", a term which appears in the second and fifth stages and represents an annual fluctuation about a mean value. TMM states that this 'equation' has to be maximal at perihelion (ie midwinter) and minimum at aphelion. (The '3E' term derives from Newton's claim that the function varies as the cube of distance from the Sun, later linked with a theory of gravity, however this need not here concern us). To check the correctness of the expression, one inserts a date when the Sun is near aphelion giving S and H similar values, when the expression (in the Lotus program layout) should reach its minimum value, while conversely it should rise to a maximum value when 180° or thereabouts separates S and H. This was found to be the case.

## II The Seven Steps as trigonometric functions

STEP ONE: the solar equation of centre

$$S_1 = S + 180/\pi [2E \sin(H-S) - 1.25 \times E^2 \sin 2(H-S)]$$

E is solar eccentricity

180/π converts radians to degrees

$$M_1 = M - 11'49'' \sin(H-S)$$

M is mean lunar longitude

STEP TWO

$$M_2 = M_1 + 3'45'' [1 - 3E \cos(H-S)] \sin 2\phi$$

STEP THREE

$$M_3 = M_2 + 47'' \sin 2(N-S)$$

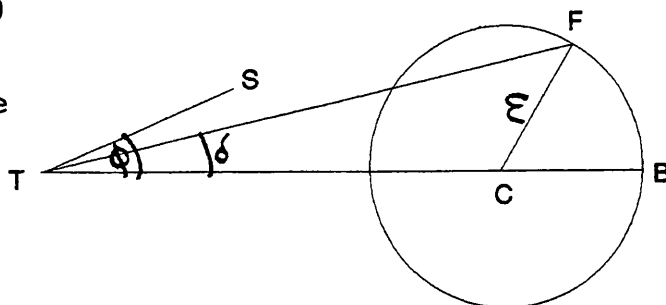
STEP FOUR: the lunar equation of centre

In the figure, TC is unity, and radius CF has length ε, where

ε = half the difference between maximum and minimum eccentricities

divided by the mean value (=0.2131).

Then TF represents the varying eccentricity e, FTC is δ the equation of apogee, and FCB is 2φ, twice the Sun-apse angle.



$$e = 0.05505 / (1 + \epsilon^2 + 2\epsilon \cos 2\phi) \quad \text{cosine formula on } \triangle TFC$$

$$\sin \delta = \frac{\sin 2\phi \times 0.01173}{e} \quad \text{sine formula on } \triangle TFC$$

0.011731' is half the difference between maximum and minimum eccentricity, 0.055050 is the mean.

$$A_2 = A_1 - \delta$$

$$\text{and } M_4 = M_3 + 180/\pi [2e \sin(A-M) - 5/4 e^2 \sin 2(A-M)]$$

STEP FIVE: the Variation

$$M_5 = M_4 + 35'32'' [1 - 3E \cos(H-S)] \sin 2(M-S)$$

STEP SIX

$$M_6 = M_5 + 2'10'' \sin(S-M+H-A)$$

STEP SEVEN

$$M_7 = M_6 + [2'20'' + 54'' \cos(H-A)] \sin(S-M).$$



The sign of the seventh equation is confusing, since one version of TMM (Cohen, 1975, p.113) specified that it be additive for the waxing Moon and subtractive for the waning, while another version a few pages later (Ibid, pp.138-9) specifies the converse. The latter version has been used by GHA, and is indeed the correct way round in accord with modern equations. Both these versions were published in 1702, but no-one, not even Flamsteed or Baily, has remarked upon this divergence. The sign of the sixth equation was later reversed, as Flamsteed pointed out (Cohen, p.59), but not the seventh. Its sign as above is negative for the waxing Moon.

### III A Comparison with Flamsteed

Table 8.2 shows Dunthorne's mode of summarising his computation. By contrast, the customary format for these matters, prior to TMM, is shown by a computation example as given in DOS (p.38), Table 8.3. More than half a century separates these two case-studies, indeed the arrival of the *Principia* separates them. How do they compare?

DOS presented ten steps for finding lunar longitude. It began with the equation of time, converting solar into mean time - a stage strangely omitted by TMM. The proud claim was made that:

'For he [the author, Flamsteed] will not dissemble it, that tho he esteems these [principles] far better than any yet published; he is sensible that the solar may be some little faulty, but scarce more than a Minute; the lunar he finds often to Err 5 or 6 Minutes, and sometimes (tho rarely, and at most) 10 or 11 minutes; which yet he can the easier bear, whilst he sees the Numbers of other more famous and celebrated Men to err 15 or 16 minutes, at the same time when his agree nearly with the Heavens' (p.34).

As ill-luck would have it, this declared maximum possible error turned up in the sole example to illustrate Flamsteed's theory! Its true place of the Moon was eleven minutes in advance of what it should have been: at 6.35 pm GMT, on December 22 1680 Old Style, its longitude was  $4^{\circ} 59'$  of Gemini, compared with the computed value of  $5^{\circ} 10'$  found in DOS. DOS's mean Moon positions were two minutes *behind* their proper values (Chapter 5), so it

would appear as if his ten steps had introduced no less than thirteen minutes of error.

The accuracy of this example accords with the pessimistic view that Flamsteed expressed in the *Philosophical Transactions* of 1683, after he had been the Astronomer Royal for seven years:

'the best tables of the Moon's Motions do err 12 minutes or more, in her Apparent Place' (PT, Vol.13, p.405).

In his view the moons of Jupiter offered the best means of finding longitude, as using the lunar method 'the calculations will be so perplexed and tedious.' This view expressed by Britain's Astronomer Royal was quoted in John Harris' *Lexicon Technicum* of 1704, so may well have expressed a general view. The research of Owen Gingerich (Ch.1, p.4) entirely confirmed this assessment, finding indeed that larger errors were common in ephemerides of the period.

Let us compare the accuracy of these two worked examples half a century apart, taking the three variables of solar, lunar and node positions.

<u>Source</u>	<u>Moon posn</u>	<u>Sun posn</u>	<u>node posn</u>
1) DOS	5° 09' 52" Gemini	12° 09' 35" Capricorn	23° 44' 30" Virgo
True posns:	<u>4° 59' 18"</u>	<u>12° 08' 00"</u>	<u>24° 01' 16"</u>
Errors:	+10' 32"	+01' 35"	-16' 45"
2)Dunthorne	14° 8' 13" Gemini	23° 37' 22" Capricorn	19° 35' 13" Virgo
True posns:	<u>14° 15' 00"</u>	<u>23° 37' 27"</u>	<u>19° 49' 00"</u>
Errors:	-6' 47"	-05"	-14'

- 1) 22 December, 1680 at 6.35pm GMT, London.
- 2) 2 January, 1737, 3hr 40' p.m., 'Time equated.'

Errors are measured here and throughout as {historic-true} values (We ask the computer for true node not mean node positions). Only for solar longitude is the position to seconds of arc really relevant.

Example continued.

Table 8.2: Lunar longitude computation by Dunthorne (1739) using a procedure identical to that of TMM. For 3.40 pm on Jan. 2nd, 1737, he found 74°8'13", the correct value being 74°14''29".

Equ. Time	Longitude $\Delta$				Apogee $\Delta$				Asc. Node $\Delta$				
	s	o	'	"	s	o	'	"	s	o	'	"	
1721	2	28	53	55	2	12	8	35	4	0	34	5	
16	10	22	51	16	9	21	4	12	10	9	28	12	
January	0	0	0	0	0	0	0	0	0	0	0	0	
d.2	0	26	21	10	0	13	22		0	6	21		
h.3		1	38	49		0	50			0	24		
'40			21	58			11				5		
mean Mot	2	20	7	8	0	3	27	10	10	9	35	2	
1 Equation		-	3	2		+	5	9		5	20	59	3
$\Delta$ 1 Equa.	2	20	4	6	0	3	32	19	5	20	56	36	1 Eq.
2 Equation		+	2	33		9	23	37	22	9	23	37	22 Suns Pl.
$\Delta$ 2 Equa.	2	20	6	39	9	20	5	3	4	2	40	46	An. Arg.
3 Equation			+	43		-	9	19	27		1	22	222 Eq. 2
$\Delta$ 3 Equa.	2	20	7	22	11	24	12	52	5	19	34	13	2 Eq.
4 Equation		-	5	18	29	2	25	54	30	mean Anom. 203'			
$\Delta$ 4 Equa.	2	14	48	53	Excentricity				8	24	35	17	Arg. Lat
5 Equation			-	36	28	46703				5	4	40	Inc. Lim.
$\Delta$ 5 Equa.	2	14	12	25	4	21	11	31	$\Delta$ a. $\Delta$				
6 Equation			-	1	27	9	12	23	2	doubled			
$\Delta$ 6 Equa.	2	14	10	58	3	8	22	18	Apogee $\odot$				
7 Equation			-	1	28	8	15	50	34	Ap. $\Delta$ a Ap. $\odot$			
$\Delta$ in Orbit Reduction	2	14	9	30	1	7	2	5	Arg. 6 Equations				
$\Delta$ in Eclip	2	14	8	13	←								
Lat. South			5	3	18								

Table 8.3: a similar computation by Flamsteed (1681) for 6.35pm, Dec. 22nd, 1680. He obtained 5°9'52" of Gemini, the correct answer being 4°59'.

	Mean Motion.				Apog.				Node Retrog				
	s	o	'	"	s	o	'	"	s	o	'	"	
1661	01	18	10	14	5	00	21	51	6	21	04	47	Radical place of the Node.
December 19	11	21	00	07	1	23	03	29	0	07	27	20	
22B		23	58	22	1	09	46	22		18	54	19	
h. 6		3	17	39		1	40				48		
' 34			18	40			10				5		
" 57			31		8	03	13	32	0	26	22	32	Motion of the $\Omega$ from the Radix.
Mean Motion	02	05	45	33	9	12	09	35	5	24	42	15	$\Omega$ true place.
Physical parts sub.			1	03	1	08	56	03	Annual Argument.				
Mean Motion Cor.	02	06	44	30		10	50	32	Equat. of the Apogee add.				
Apogee	08	14	04	04	8	14	04	04	True place of the Apogee.				
Mean Anomaly Equation subtract.	05	22	40	26	Ex. 57678				Eq. under the				
			54	22	M. Ex. 55237				Greatest Excen. 01 03 52 } Diff. 12' 1"				
$\Delta$ Equation place	02	05	50	08	Diff. 2441				Mean Excenter 00 51 51				
Suns	09	12	09	35					Part propo. add 00 02 31				
$\Delta$ from the Sun	04	23	40	33	$\Omega$ Mean pla.				Absolute Equat. 00 54 22				
Variation subtract			36	16	Suns				R.c.s. 5 01 36 9,99 8327				
$\Delta$ in her Orb	02	05	13	52	$\Delta$ from the $\Omega$				t. 71 29 22 10,475214				
Node	05	23	44	30	Equat. $\Omega$ sub.				t. 71 25 22 10,473541				
Argument of Lat. Reduction Subtra.	08	11	29	22	$\Omega$ true place				Red. sub. 4 00				
			4	00	In. of the Orb				R.s. 5 01 36 8,942600				
Place in the Eclip Latitude South	II	05	09	52	←				s. 71 29 22 9,976930				
			04	45	Hence the M. true place is II 5 09 52				s. 4 45 58 8,919530				
Moons Horiz. Paral			61	31	Latitude South 4 45 58								
Horizontal Semid.			16	4									

Dunthorne's 'to the Reader' does not extol TMM's accuracy, but rather admits 'that the Newtonian Numbers are a little deficient...' The above figures suggest a mild improvement over half a century. The DOS solar error of one and a half arcminutes is surprisingly large, considering that as we saw in Chapter Five his mean motion was within arcseconds at this period. Flamsteed's solar numbers were improved several times after DOS's composition.

A summary of the constant terms given for the equations of TMM appears in Table 8.4.

#### **IV A Test of Accuracy**

The accuracy of TMM was investigated using the above computer program (hereinafter referred to as TMM-PC), by comparing its results against a modern ephemeris program accurate to seconds of arc. Noon values of longitude on successive days of December 31st (Old Style) were taken, sampling at two year intervals over a period of six decades, 1680 - 1740. TMM-PC measures time from December 31st 1680, which means that the initial reading was at time zero, then the next was for 730.5 days, and so forth. Both solar and lunar longitude values were read off from the program, the former being necessary to check that the number of days inserted was correct, since an extra day from a leap year shows up as a degree displacement in longitude. The program to obtain the longitudes was checked against standard values obtained from the R.G.O. The results obtained are given in Table 8.5 (at end of Chapter) for six decades, depicted graphically in Figure 8.2.

There is a slight drift in the baseline of TMM through the decades, as cumulative error of its mean motion. The overall error-values in arcminutes were  $-1.6 \pm 3.8$  for the moon and  $0.2 \pm 0.3$  for the Sun. A long-term pattern appears as present in the data, of period fifty years or so, which is a consequence of the sampling interval used being a multiple of a major TMM period, viz the year. Chapter eleven will treat this issue more thoroughly.

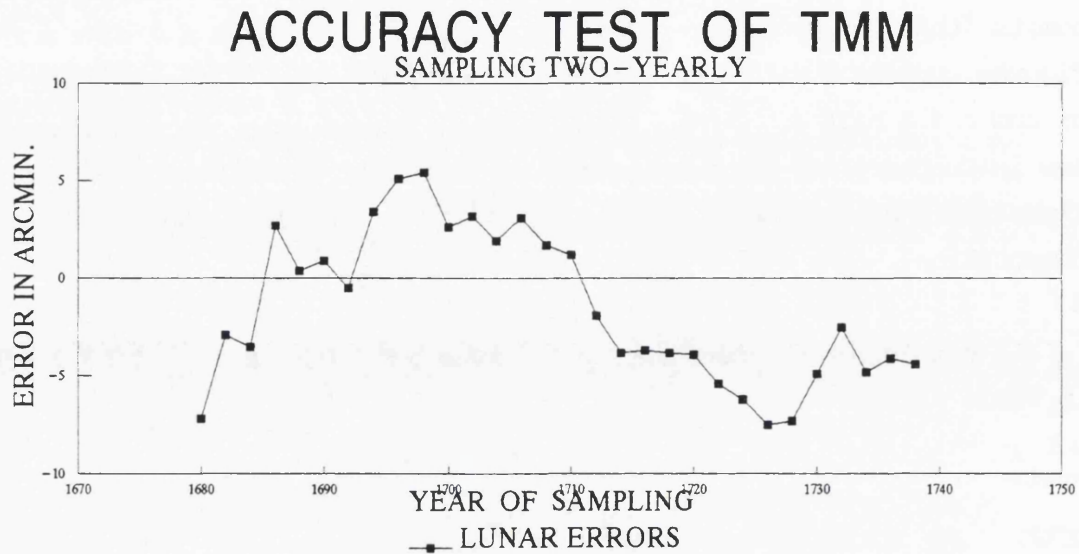


Figure 8.2: Sampling from Dec.31st noon GMT, Old Style, two-yearly (every 730.5 days) showing errors in arcminutes over six decades.

### V Halley's Judgement

Halley's mature and final opinion on the subject was given in 1731, when he was Britain's foremost astronomer and both Newton and Flamsteed were mere memories for him. Then, after consulting both his own lunar tables (as Astronomer Royal) and those of his predecessor, his view of TMM three decades after its composition was that:

'...the Faults of the Computus formed therefrom rarely exceed a quarter Part of what is found in the best Lunar Tables before that time extant.

...By this it was evident that Sir Isaac had spared no Part of that Sagacity and Industry peculiar to himself, in settling the Epoches, and other Elements of the Lunar Astronomy, the Result many times, for whole Months together, rarely differing two Minutes of Motion from the Observations themselves..'

(Halley, 'A Proposal of a Method for Finding Longitude at Sea within a Degree, or Twenty Leagues', *Phil. Trans.* 1731/2, Vol.37, p.191)

This comment was made a propos of the 1713 version given in PNPM, which Halley viewed as an improvement upon TMM. These remarks of his echo what he had written years earlier in 1710, in a Foreword to *Streete's Astronomia Carolina* that he re-issued.

Considering the above-discussed verdict of Britain's first Astronomer Royal, that even the best lunar ephemerides were liable to err by twelve minutes 'or more', it seems likely that TMM was capable of delivering a slight enhancement of predictive power. Plainly, however, it achieved nothing remotely resembling that which Halley has here claimed for it. It may be, however, that an improvement was accomplished in the *Principia's* second edition, which could somewhat justify Halley's remarks. The views of astronomers using TMM will be addressed in due course. Dunthorne was not as we saw over-impressed by its accuracy.

Of marginal relevance here is the note in Edmond Halley's diary for when he landed on the coast of Brazil in 1692 (Ch.1, III), and determined his longitude from an 'appulse' of Aldebaran (ie, time of nearest approach to a fixed star). This turned out to have an error of only  $1^{\circ}8'$ , which would imply a lunar position accurate to two or three minutes. It seems likely, either that this was a lucky chance, or that the inhabitants of Paraiba near to where Halley landed did indeed have some cognisance of their longitude.

Later, we will study the various Newtonian ephemerides which modelled themselves upon TMM, and try to determine whether or not they achieved a superior predictive accuracy to others such as that by Jacques Cassini in Paris, who did not use it.

**Table 8.4: The Constants of TMM**

The annual equation (I)

Moon	11' 49"
apogee	20'
node	-9' 30"

Equation (II)

maximum (in winter)	3' 56"
minimum (in summer)	3' 34"

Equation (III) 47"

The equations of the center

Sun	1° 56' 20"
Moon	7° 39' 30"

Equation of apogee (IV) 12° 15' 4"

Eccentricity

maximum (apse conjunct Sun)	0.066782
minimum	0.043319

Variation (V)

maximum (in winter)	37' 25"
minimum (in summer)	33' 40"

Equation (VI) 2' 10"

Equation (VII) 2' 20"

Angle to ecliptic

maximum (nodes conjunct Sun)	5° 17' 20"
minimum (quadrature)	4° 59' 35"

**Table 8.5: Solar and Lunar Longitude Accuracies from TMM**

Data for Figure 8.2 was obtained by sampling at intervals of two Julian years, ie 730.5 Julian days, and subtracting the ILE values from those of TMM. Solar TMM errors are shown for comparison.

	I.L.E.	TMM	arcminutes	of error	
	a	b	b - a	solar	
1680	188.043	187.924	-7.2	0.65	
82	81.359	81.311	-2.9	0.58	
84	357.762	357.704	-3.5	0.37	
86	251.588	251.633	2.7	0.42	
88	167.651	167.658	0.4	0.68	
90	62.325	62.341	0.9	0.35	
92	337.129	337.12	-0.5	-0.03	
94	233.537	233.593	3.4	0.08	
96	147.035	147.119	5.1	0.13	
98	44.748	44.837	5.4	0.02	
1700	316.977	317.021	2.6	0.42	
2	216.95	217.003	3.2	0.78	
4	126.913	126.944	1.9	0.65	
6	29.056	29.107	3.1	-0.33	
8	297.311	297.34	1.7	-0.02	
10	201.522	201.542	1.2	-0.12	
12	107.423	107.391	-1.9	0.05	
14	14.165	14.102	-3.8	0.03	
16	277.675	277.613	-3.7	0.12	
18	186.392	186.329	-3.8	0.33	
1720	87.903	87.838	-3.9	0.28	
22	358.499	358.409	-5.4	0.23	
24	257.715	257.612	-6.2	0.43	
26	170.328	170.203	-7.5	0.3	
28	67.681	67.56	-7.3	-0.02	
30	341.298	341.216	-4.9	-0.13	
32	237.361	237.319	-2.5	-0.25	
34	152.425	152.346	-4.8	-0.13	
36	46.896	46.827	-4.1	0.22	
38	322.811	322.738	-4.4	0.42	
			.....		
			-1.6	0.22	means
			3.8	0.29	S.D.s



## Ch. 9 COMMENTARY ON TMM, CONTINUED

This chapter continues the Commentary from Chapter 4, and indicates how the equations just described were obtained from TMM. This involves summarising the content of previous chapters, while avoiding repetition wherever possible.

### I An English Pamphlet

The quotations from TMM, here as in previous chapters, come from the first English edition, published in 1702. This edition had no editor or translator specified, but a Preface attached to it contained remarks indicating that it appeared shortly after Gregory's *Astronomiae Physicae*, containing a latin version of TMM, which appeared in that same year. Bernard Cohen endorsed the idea that Halley was the author of its Preface (1975, p.32), originally proposed by Augustus De Morgan in the nineteenth-century. While there is no definite evidence on the matter, by way of correspondence, it is plausible from considerations of Halley's style, and familiar manner towards the persons concerned, Newton and Gregory. Its brief Preface praised Gregory's book and added that, since many would not be able to afford it, the pamphlet would be convenient. It is not necessarily a translation from Gregory's latin, since, as Cohen argued, the original version of TMM was probably in English. The manuscript is identical in content with that published by Gregory, except for a divergence in the seventh equation discussed below, unnoticed by Cohen.

### II The Annual Equations

TMM conferred annual inequalities upon four of its ecliptic variables, the node, perigee, Sun and Moon, the first two being innovative:

'These mean Motions of the Luminaries are affected with various inequalities: Of which,

1. There are the Annual Equations of the aforesaid mean Motions of the Sun and Moon, and of the Apogee and Node of the Moon.

'The annual Equation of the mean Motion of the Sun depends on the Eccentricity of the Earth's Orbit round the Sun, which is 16 11/12 of such parts, as that the Earth's mean Distance from the Sun shall be 1000: Whence 'tis called the *Equation of the Centre*; and is when greatest 1:56'.20".

'The greatest Annual Equation of the Moon's mean Motion is 11'.49". of her Apogee 20'. and of her Node 9'.30"'. .

The Equation of Center derived from Flamsteed's value of 1692. There is an exact equivalence between it and TMM's eccentricity values, though the method by which this computation was then performed is unclear. Chapter Six looked at how the modern equation of center links the two together, viz

$$\Theta = (2e - e^3/4)\sin M - 5/4e^2\sin 2M + 13/12e^3\sin 3M \dots$$

In the case of the Earth's orbit, the function reaches a maximum value at an anomaly M of 91°. Inserting it in the equation together with an eccentricity value of 16 11/12 parts in 1000 (ie 0.016917) gave, using a Lotus 1-2-3 program for computing this Equation of Center, an agreement within one second of arc! This Equation differs from what was then the true value by 45 arcseconds, while the value given for eccentricity differs from the modern value by 0.5%, taking the latter as 0.01683\*.

TMM comments further about the interlinking of these annual equations, of interest as showing how tricky such things were before trigonometrical formulae became available:

'And these four Annual Equations are always mutually proportional one to another: Wherefore when any of them is at the greatest, the other three will be greatest; and when any one lessens, the other three will also be diminished in the same *Ratio*.

'The Annual Equation of the Sun's Centre being given, the three other corresponding Annual Equations will be also given; and therefore a Table of *That* will serve for all. For if the Annual Equation of the Sun's Centre be taken from thence, for any time, and be called P, and

.....  
\* Using the secular-variation term for the eccentricity of Earth's orbit, according to the Explanatory Supplement of the Astronomical Ephemeris, p.98.

let  $P/10 = Q$ ,  $Q+Q/60 = R$ ,  $P/6 = D$ ,  $D+D/30 = E$ , and  $D-D/60 = 2F$ ; then shall the Annual Equation of the Moon's mean Motion for that time be  $R$ , that of the apogee of the moon will be  $E$ , and that of the Node  $F$ .

'Only observe here, that if the Equation of the Sun's Centre be required to be added; then the Equation of the Moon's mean Motion must be *subtracted*, that of her apogee must be *added*, and that of the node *subtracted*. And on the contrary, if the Equation of the Sun's Centre were to be *subducted*, the Moon's Equation must be *added*, the Equation of her Apogee *subducted*, and that of her node *added*.'

These four functions vary as the sine of solar anomaly, so are maximal near the equinoxes and zero near the solstices. The four equations we extracted from these instructions were:

$$S_1 = S + 1.939 \sin(H-S) - 0.0205 \sin^2(H-S)$$

$$M_1 = M - 0.197 \sin(H-S)$$

$$A_1 = A + 0.333 \sin(H-S)$$

$$N_1 = N - 0.158 \sin(H-S)$$

The solar annual equation is the only one large enough to merit a second term of the equation of centre in the TMM program. It is evident that these constant terms are linked through the ratios specified by TMM, eg:

$R=61/60.P/10$  (since  $Q=P/10$  and  $R=61Q/60$ ) where  $P$  is the maximal solar equation of  $1.939$ , giving  $R = 0.197$ . These ratios nowadays appear as superfluous, as the amplitudes of the four annual equations have already been given.

Such are the positions 'first equated' in TMM's terminology, meaning fluctuations of yearly periods around the mean motions. The Sun has only this one equation, whereas the node and apogee receive second equations at later stages.

### III Two New Equations

The first of TMM's new equations now appears:

'There is also an *Equation of the Moon's mean Motion* depending on the Situation of her Apogee in respect of the Sun; which is *greatest* when the Moon's apogee is in an Octant with the Sun, and is nothing at

all when it is in the Quadratures or Syzygies. This Equation, when greatest, and the Sun *in Perigeo* is 3'.56". But if the Sun be in Apogeo, it will never be above 3'.34". At other distances of the Sun from the Earth, this equation, when greatest, is reciprocally as the Cube of such Distance. But when the Moon's Apogee is any where but in the *Octants*, this Equation grows less, and is mostly at the same distance between the Earth and the Sun, as the Sine of the double Distance of the Moon's Apogee from the next Quadrature or Syzygy, to the Radius.

'This is to be *added* to the Moon's Motion, while her Apogee passes from a Quadrature with the Sun to a Syzygy; but is to be *subtracted* from it, while the Apogee moves from the Syzygy to the Quadrature.'

The function is given as varying with the Horroxian year, which we have designated as the (A-S) function, marking solar conjunctions with the apse, of period 411 days. The function evidently varies as  $\sin^2(A-S)$ , peaking at the octants, ie at the 45° angles, since it passes through two maxima and minima per revolution. The phrase, 'nothing at all when it is the Quadratures or Syzygies' implies a sine function crossing its baseline four times per cycle.

While the annual equations had constant coefficients, here the amplitude itself varies during the course of the year, being maximal at perihelion i.e. midwinter. Multiplying by

$$1 - 0.0489\cos(H-S)$$

will accomplish this. As a cosine function it has maxima and minima at the solstices, giving the required range of  $\pm 11''$  about a mean value of 3'45" as specified. We express this mean amplitude as 0.0625°. The overall expression is thus:

$$M_2 = M_1 + 0.0625[1 - 0.0489\cos(H-S_1)] \times \sin^2(A_1 - S_1)$$

The terms as first equated are fed into this expression, to generate TMM's second 'Equation' of the Moon. This second equation was called by Halley *aequatio prima semestris*. Baily's comment was, 'We have nothing equal to it in amount (depending on the same argumant) in the tables of Mayer, Burgh, or Burckhart' (p.742). On the other hand, Curtis Wilson

expressed the view that, of the four new equations, it was the only correct one (GHA, p.267). The next chapter will resolve this matter.

The phrase 'reciprocally as the cube of distance' contains an echo of gravity theory, the sole trace in TMM, which was to be much developed in the second edition of PNP. The Earth's distance from the Sun varies by ± 1.70% in the course of a year, so an inverse-cube relation would give thrice this, which is ± 5.10%. The second and sixth equations have their amplitudes modified, supposedly varying inversely as the cube of the Earth's distance from the Sun. Wilson in GHA (p.267) gave the second equation as:

$$- 3'45''[1-3E\cos(S-H)]\sin 2(S_1-A_1)$$

where (S-H) is the Earth's 'true anomaly'. Our TMM program uses the term  $\cos(H-S)$ , which gives identical values.

Inserting the earth's eccentricity into Professor Wilson's term gives an amplitude modification of 5.1% as his '3e' term, for both the second and sixth equations. Wilson has derived his term from the instruction, 'inversely as the cube of the difference\*', while we have simply taken the amplitude variations specified, the results being similar. For equation 2, the given amplitude fluctuation is ± 4.9%, while for equation 6 it is ± 5.3%, which is tolerably close to the inverse-cube relationship. Our TMM program has used these latter values. These fluctuations are small changes in a three arcminutes function, so the differences are immaterial.

The third equation introduces the nodes:

'There is moreover another *Equation of the Moon's Motion*, which depends on the Aspect of the Nodes of the Moon's Orbit with the sun: and this is *greatest* when her Nodes are in *Octants* to the Sun, and vanishes quite, when they come to their *Quadratures* or *Syzygies*. This Equation is proportional to the sine of the double Distance of the Node

.....  
\* Wilson in GHA (p.267) has also assigned an inverse-cube amplitude modulation to the third equation, in which we do not follow him: it is merely the second and sixth equations which have this adjustment.

from the next Syzygy or Quadrature; and at greatest is but 47". This must be added to the Moon's mean Motion, while the Nodes are passing from their Syzygies with the Sun to their quadratures with him; but *subtracted* while they pass from the Quadratures to the Sygies.'

This is again a  $\sin 2\theta$  function passing through two cycles per solar revolution against the nodes. Its amplitude is fixed, and we readily ascertain its formula to be

$$M_3 = M_2 + 0.0131 \sin 2(N_1 - S_1)$$

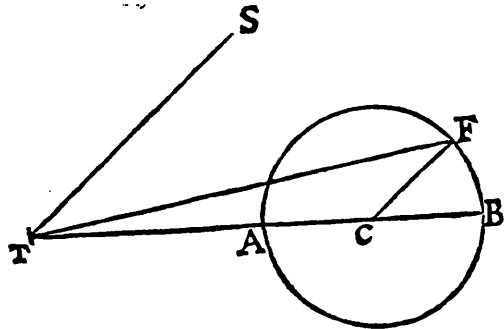
To ascertain the signs of these functions, we recall that  $\sin(A-B) = -\sin(B-A)$ , whereas  $\cos(A-B) = \cos(B-A)$ . Interpretations of whether a sign should be added or subtracted have been checked against the worked example of Richard Dunthorne. The instruction that a function is additive 'while the Nodes are passing from their Syzygies with the Sun to their Quadratures with him', and subtractive for the converse, is interpreted as  $-\sin 2(S-N)$ , or  $\sin 2(N-S)$ , as used in the function.

#### IV The Horrox-Wheel Mechanism

The fourth equation is by far the largest of the seven steps. The deferent-wheel invented by the young Horrocks in 1638 is here made to generate both the eccentricity fluctuation and the apse-line motion, as it revolves once per  $6\frac{1}{2}$  months. We have just seen how TMM's value for solar eccentricity agreed exactly with that used in the modern equation of centre within an arcsecond or so. That implies a definition identical with the modern one of  $b^2 = a^2(1-e^2)$ , where  $a$  and  $b$  are the major and minor axes of an ellipse. The eccentricity 'e' thereby defined is the square root of a ratio function, but it can more relevantly be viewed as the distance between focus and centre divided by 'a,' the radius of a circumscribing circle (Ch.2, IV).

'From the Sun's true Place take the equated mean Motion of the Lunar Apogee, as was above shewed, the Remainder will be the Annual Argument of the said Apogee. From whence the *Eccentricity of the Moon*, and the *second Equation* of her Apogee may be compar'd after the manner

following (which takes place also in the Computations of any other intermediate Equations.)'



Referring to the diagram, the first sentence defines  $(S_1 - A_1)$ , represented by the angle STA. What is here called the Annual Argument must not be confused with the Annual Equation, discussed earlier. The explanation given in Chapter 7 avoided the term Annual Argument, as liable to confuse, instead calling it the Horrox

angle. After all, the cycle is only quasi-annual. I have not grasped the meaning of the final phrase in brackets.

We now come to TMM's operating instructions, using the familiar Horrox-wheel diagram. We should note that angle FCB is supposed to be twice the size of STA.

'Let T represent the Earth, TS a Right Line joining the Earth and Sun, TACB a Right Line drawn from the Earth to the middle or mean place of the Moon's Apogee, equated, as above: Let the Angle STA be the Annual Argument of the aforesaid Apogee, TA the least Eccentricity of the Moon's Orbit, TB the greatest. Bisect AB in C; and on the Centre C with the Distance AC describe a Circle AFB, and make the angle BCF = to the double of the Annual Argument. Draw the Right Line TF, that shall be the Eccentricity of the Moon's Orbit; and the angle BTF is the second Equation of the Moon's apogee required.'

The revolution of F around the circle twice a Horrox-year defines two functions, which are thereby mathematically linked: the second equation of the apse line, FTA, with a maximum of twelve degrees, and the eccentricity of the lunar orbit as the length FT. The dimensions of the Horrox-wheel are then specified as follows:

'In order to whose Determination let the mean Distance of the Earth from the Moon, or the Semidiameter of the Moon's Orbit, be 1000000; then shall its *greatest Eccentricity* TB be 66782 such Parts; and the *least* TA, 43319. So that the greatest Equation of the Orbit, viz. when the Apogee is in the Syzygies, will be  $7^{\circ}.39'.30''$ . or perhaps  $7^{\circ}.40'$ . (for I suspect there will be some Alteration according to the position of the apogee in Cancer or Capricorn.) But when it is in Quadrature to the sun, the greatest Equation aforesaid will be  $4^{\circ}.57'.56''$ . and the greatest Equation of the Apogee  $12^{\circ}.15'.4''$ .

This is innovative, being the first time that these two functions had been so defined, as derived from the same geometry. The modern equation of centre enables us to check what TMM calls the Equation of Orbit, which is the amount whereby the Moon's position diverged from the mean motion, maximal at seven and a half degrees.

The magnitude of the apse equation here specified is considerably larger than that specified by DOS, about four percent more in fact. In Newton's letter of April 23rd 1695 a table for what was called the 'Annual Argument' gave an eccentricity function virtually unchanged from Flamsteed's, while the apse equation's maximum value has increased from  $11^{\circ}47'$  to  $12^{\circ}10'$ . It is here increased further, and will reach Newton's maximum value of  $12^{\circ}18'$  in the PNPM of 1713. Gaythorpe showed how this Newtonian value was more than half a degree larger than was warranted by its modern equivalent, the evection term (Ch.7,V).

Our three functions f,g and h accomplish these steps. The first of these obtains the eccentricity, as the length FT, given the angle FCB as  $2(S-A)$  and the lengths FC and TC as 1173 and 55050 as parts per million. From applying the cosine rule to triangle FTC:

$$f(A-S) = 0.05505 \times \sqrt{\{1.0454 + 0.4262\cos 2(A-S)\}}$$

As a cosine function it will make eccentricity peak at zero and  $180^{\circ}$  Horrox angles, the solar conjunctions to the apse. Then, applying the sine rule to the triangle FTC, angle FTC which is the second equation of apogee is found by the function  $g(A-S)$ , whose maximum value is  $12^{\circ}15'$ .



The suggestion here appears, that the apse position in relation to the aphelion line has some effect. In the winter of 1694, Newton urged Flamsteed to take lunar observations because of the great importance of 'apogee in ye summer signs', during 'ye sun's opposition in midwinter' (letter of November 17th). From the TMM program we discern that, when the Sun reached zero Capricorn in that midwinter, the mean apogee stood within a degree or so of zero Cancer, ie they were in close opposition. Newton's next letter reiterated the urgency of the matter:

'For the position of the apogee in the Sun's opposition in midwinter is a case of great moment and will not return for many years. The observation in the full and both the quadratures are of greatest moment... (Letter of December 4th, 1694, Baily, p.143)'

Flamsteed was well able to locate apogee, using the micrometer screw gauge on his telescope eyepiece to measure lunar diameter, a method he did much to pioneer\*, so would have appreciated this event. But, as for what equation Newton was then searching for, we remain in the dark. Evidently, by the time TMM was composed, he had reached no conclusion as to the relevance of the nine-year apse cycle (except for a very minor role in modulating the sixth equation, see below).

'Having from these Principles made a Table of the Equation of the Moon's Apogee, and of the Eccentricities of her Orbit to each degree of the Annual Argument, from whence the Eccentricity TF, and the Angle BTF (viz. the second and principal Equation of the Apogee) may easily be had for any Time required; let the Equation thus found be added, to the first Equated Place of the Moon's Apogee, if the annual Argument be less than  $90^\circ$ , or greater than  $180^\circ$ , and less than  $270^\circ$ ; otherwise it must be subducted from it: and the sum or Difference shall be the Place of the Lunar Apogee secondarily equated; which being taken from the Moon's Place equated a third time, shall leave the mean Anomaly of the Moon corresponding to any given Time. Moreover, from this mean Anomaly

.....

\* For a practical account of the development of this new technology in the North of England, chiefly by Yorkshireman William Gascoigne, while Flamsteed was living in Derby, see Chapman's *Three North Country Astronomers* (1982 p.21), and his *Dividing the Circle* (1990).

of the Moon, and the before-found Eccentricity of her Orbit, may be found (by means of a Table of Equations of the Moon's Centre made to every degree of the mean anomaly, and some Eccentricitys, viz 45000. 50000. 55000, 60000 and 65000) the *Prostaphaeresis* or Equation of the Moon's Centre, as in the common way: and this being taken from the former Semicircle of the middle anomaly, and added in the latter to the Moon's Place thus thrice equated, will produce the Place of the moon a fourth time equated.'

The lunar Equation of Centre was required for preparing tables, and here the instructions are to prepare them with anomaly angle against eccentricity values, using five columns of differing eccentricities as compared with DOS's three columns. Some astronomers of the first half of the eighteenth-century did follow this advice, eg Le Monnier in Paris. Interpolating between, say, anomaly values at one degree intervals and several eccentricity values was not in itself easy. In Chapter Eight, we saw how the main error in Dunthorne's worked example came from this fourth step of equation, creeping in during his interpolations over the Equation of Centre table.

Our function  $h$  is the modern formula for equation of centre, inserting the eccentricity and equated apse position as derived from the Horrox-wheel. Our method uses the modern equation rather than a function defined by TMM. As explained, this was felt to be justifiable because Flamsteed's method of deriving the Equation of Centre from elliptic orbits agreed within arcseconds of the modern formula (Ch.6, III). Our computer-model could be criticised for not properly modelling errors likely to arise at this step, from interpolating an Equation of Centre table, eg in Streete's *Astronomia Carolina*, as reprinted by Halley in 1710.

## V Amplitude of the Variation

The Variation was one of the three known lunar inequalities. Its cause was the Moon's swifter motion in the syzygies than in its quadratures, whereby it reached its maximum equation in the octants. In Proposition 66 of Book One of PNM Newton undertook to give a derivation of it, as

resulting from the Sun's pull on the lunar orbit. Tycho Brahe announced its discovery in 1598, giving it an amplitude of  $40'.5$ , which was quite close to its true value of  $39'.5$ . Horrocks in his first exposition in the 1630s had settled on a smaller value of  $36'45''$ , which he later reduced to  $36'27''$  (*Opera*, 1673, p.483). Despite being a keen disciple of Brahe's colleague Kepler, Horrocks adopted a much smaller value for his Variation term. In *TMM* Newton made it  $35'32''$ . His letter to Flamsteed of November 1st, 1694, discussed this matter.

This divergence mystified commentators for a while, with the nineteenth-century astronomer Gaythorpe declaring that the British astronomers had been simply mistaken (1956, p.40). More recently it was discovered by Jorgensen (1974, p.317) that the Horrocksian mechanism itself incorporated a sizeable fraction of the Variation, in fact "some  $5'.25$  of the variation" (GHA, p.265), making the correct amplitude of Variation in the Horrocksian theory a mere  $34'15''$ . The term used by Newton and Halley was thus more or less correct.

The GHA averred that Flamsteed took a value for the Variation of  $36'45''$  'obtained on the basis of observation' (p.264). Flamsteed's value for the maximum Variation was given in DOS's table for Variation, and this goes up to  $38'$ , the same value as was adopted by William Whiston in his 1707 opus.

*TMM* makes the term vary with the seasons:

'The greatest Variation of the Moon (*viz*, that which happens when the Moon is in an Octant with the Sun) is, nearly, reciprocally as the cube of the Distance of the Sun from the Earth. Let that be taken as  $37'.25''$ . when the Sun is *in Perigeo*, and  $33'.40''$ . when he is *in Apogeo*: and let the Differences of this Variation in the Octants be made reciprocally as the Cube of the Distances of the Sun from the Earth; and so let a Table be made of the aforesaid Variation of the Moon in her *Octants* (or its Logarithms) to every *Tenth*, *Sixth*, or *Fifth* Degree of the mean Anomaly: And for the Variation out of the Octants, make, as Radius to the Sine of the double Distance of the Moon from the next Syzygy or Quadrature : : [sic] so let the aforefound Variation

in the Octant be to the Variation congruous to any other Aspect; and this *added* to the Moon's place before found in the first and third Quadrant (accounting from the Sun) or *subducted* from it in the second and fourth, will give the Moon's Place equated a fifth time.'

TMM here specifies a sine  $2(M-S)$  function, with two cycles per lunar month, zero at the four quarters. It has an amplitude of  $35\frac{1}{2}'$ , or  $0.591$  (The TMM computer program requires conversion of arcminutes into decimals of a degree). This is then made to vary with the seasons, being maximal at perihelion (what TMM calls the Sun *in Perigeo*) and minimum at aphelion. It is therefore a cosine function.

As once before, an inverse cube relation to the solar distance is affirmed (which can be viewed as implying an inverse-square gravity principle, discussed in the Second Edition of PNPM), implying a ten percent fluctuation in the course of the year. Thereby our fifth equation becomes:

$$M_5 = M_4 + [0.591 - 0.03\cos(H-S_1)] \times \sin 2(M_4 - S_1)$$

The small value of Variation gives a convenient means of checking whether astronomers were using a Horrocks-based system.

Next we come to the sixth equation, which marred TMM in its initial version, as it was given the wrong way round. In the next chapter we see how Newton's additional equations for TMM's seven steps were all valid, except that this one operated in reverse, adding where it should be subtracted, as he later realised. Having a correct equation the wrong way round is much worse than having an irrelevant or mistaken equation: it continually creates an error of twice its amplitude. Here are the directions for the sixth and seventh:

'Again, as Radius to the Sine of the Sum of the Distances of the Moon from the Sun, and of her Apogee from the Sun's Apogee (or the sine of the Excess of that sum above  $360^\circ$ .) : : so is  $2'.10''$ . to a sixth Equation of the Moon's Place, which must be *subtracted*, if the aforesaid Sum or Excess be less than a Semicircle, but *added*, if it be greater. Let it be made also, as Radius to the Sine of the Moon's Distance from the Sun : : so  $2'.20''$  to a seventh Equation: which, when the Moon's light is encreasing, *add*, but when decreasing, *subtract*; and

the Moon's place will be equated a seventh time, and this is her place in her proper Orbit.'

The expression '::' meant 'in proportion to', used for comparing or equating (in the modern sense) ratios. Effectively, we are instructed to sum (S-M) and (H-A) in a sine function having an amplitude of 2'10", which gives:

$$M_6 = M_5 + 0.0361\sin(S-M+H-A)$$

William Whiston's comment upon the sixth, made in his Lucasian lectures to students of Cambridge University in 1703, is often quoted:

'How it should come to pass that this sixth Equation of the Moon should arise from Causes which are so unlike join'd together amongst themselves, as are the motion of the Moon from the Sun, and the Motion of the Apogee of the Moon from the Apogee of the Sun; I must acknowledge myself altogether ignorant; nor is there Opportunity for enquiring in these Matters merely Astronomical. In the mean while, I suspect that this Equation was rather deduced from Mr *Flamsteed's* observations, than from Sir *Isaac Newton's own Argumentation.*'

(Cohen,1975, p.361)

We will shortly observe how the modern equations of lunar longitude, at this amplitude range of 2-3', contain much stranger-looking combinations of terms than the above, as puzzled Mr Whiston.

Finding the Moon's place 'in her proper Orbit' reminds the reader that all the above computations have not been in the plane of the ecliptic, but rather in an orbit tilted at five degrees thereto. TMM does not give instructions over the 'reduction', for converting to ecliptic longitude, this being a standard procedure.

The seventh equation varies with lunar phase, additive in the waxing period and subtractive in the waning, ie as  $\sin(S-M)$ . Its amplitude is modulated by a nine-year period:

'Note here, the Equation thus produced by the mean Quantity 2'.20". is not always of the same Magnitude, but is increased and diminished according to the Position of the Lunar Apogee. For if the Moon's Apogee be in conjunction with the sun's, the aforesaid Equation is about 54". greater: but when the Apogees are in opposition, 'tis about as much

less; and it librates between its greatest Quantity 3'.14". and its least 1'.26". And this is when the Lunar Apogee is in Conjunction or Opposition with the Sun's: But in the Quadratures the aforesaid Equation is to be lessened about 50". or one minute, when the Apogees of the Sun and Moon are in Conjunction; but if they are in Opposition, for want of a sufficient number of Observations, I cannot determine whether it is to be lessen'd or increas'd. And even as to the Argument or Decrement of the Equation 2'.20". above mentioned, I dare determine nothing certain, for the same reason, viz the want of Observation accurately made.'

This is a cosine function because it reaches a maximum when the apse line conjuncts perihelion, so we represent it as  $\cos(H-A_2)$ . The apse-position twice-equated is used:

$$M_7 = M_6 + 0.0389[1 + 0.3857\cos(H-A_2)] \times \sin(S_1 - M_6)$$

At waxing Moon, lunar longitude must be larger than solar longitude, so the function  $\sin(S-M)$  must be negative, contrary to the above-quoted instructions. Professor Wilson however in GHA quoted the seventh equation in the above form. Corresponding with him over this dilemma, he pointed out that the latin text of Gregory's opus, published in 1702, has the converse instruction, namely 'Hanc aufer quando Lunae Lumen augetur, & (e contra) adde cum illud minuitur' (Cohen, 1975, p.127). Likewise an English translation of Gregory's text, published later in 1715, also reproduced in the Cohen opus, gives that version, which we should presumably take as authentic. In addition, this is the correct sense in the modern equation. It remains hard to imagine Edmond Halley, if indeed he was the producer of the TMM version we have been using, and possibly its translator, introducing such an error. The matter remains conjectural.

There is the hint of a second modulation to the seventh, in the reference to Conjunction and Opposition, which we have ignored. A further modulation of both sixth and seventh equations follows, which can also be ignored as well below the limit of detectability, adjusting by a mere few percent the anomalistic cycle:

'If the sixth and seventh Equations are augmented or diminished in a reciprocal *Ratio* of the Distance of the Moon from the Earth, i.e., in a

direct *Ratio* of the Moon's Horizontal Parallax; they will become more accurate: And this may readily be done, if Tables are first made to each Minute of the said Parallax, and to every sixth or fifth Degree of the Argument of the sixth Equation for the *Sixth*, as of the Distance of the Moon from the Sun, for the Seventh Equation.'

## VI A Second Horrox-wheel

A second Horrox-wheel now appears, for an 'Annual Argument' of the nodes. This varies with the  $6\frac{1}{2}$  month period, and it is all rather similar. A second diagram appears, identical with that here reproduced on p.110.<sup>109</sup>

'Let T as before represent the Earth, TS a right line conjoining the Earth and Sun: Let also the Line TACB be drawn to the Place of the Ascending Node of the moon, as above equated; and let STA be the Annual Argument of the Node. Take TA from a Scale, and let it be to AB :: as 56 to 3, or as  $18\frac{2}{3}$  to 1, Then bisect BA in C, and on C as a Centre, with the distance CA, describe a Circle as AFB, and make the Angle BCF equal to double the Annual Argument of the Node before found: So shall the Angle BTF be the second Equation of the Ascending Node: which must be *added* when the Node is passing from a Quadrature to a Syzygy with the Sun, and *subducted* when the Node moves from a Syzygy towards a Quadrature. By which means the true Place of the node of the Lunar Orbit will be gained : whence from Tables made after the common way, the *Moon's latitude*, and the *reduction of her orbit to the Ecliptick*, may be computed, supposing the Inclination of the Moon's Orbit to the Ecliptic to be  $4^{\circ}59'.35''$ . when the Nodes are in quadrature with the Sun; and  $5^{\circ}17'.20''$ . when they are in Syzygys.'

The ratio of TA to AB is  $18\frac{2}{3}$  to 1, echoing the period of the rotation of the nodes of 18.6 years. Our function j finds the angle FTA, the second nodal Equation, by dropping a perpendicular from F onto TB and taking the tangent of FTC.

The angle STA is (N-S), so the angle FCB being double its magnitude is  $2(N-S)$ . The ratio of TC:CB is, by the ratio given above, 38.3:1. If the perpendicular is FD, then the tangent of FTC is  $FD/(CD+TC)$ , whence the function j is obtained. The maximum value of this angle is  $\arcsin 1/38.3$  or

1°29'44" (See fig 7.4b). This second nodal equation appears after the seven steps of equation, being used solely for finding the reduction and celestial latitude.

Flamsteed's tables had a similar but larger node equation, of 1°39'46" at 45° anomaly; which equation came from Kepler (*Tabulae Rudolphinae* 1627 p.87) as Gaythorpe pointed out (1956 p.142), and these node tables were used by Whiston. This Keplarian value is more accurate, ie nearer the modern value\*, than that of TMM. On the other hand, the amplitude of TMM's newly-invented first nodal equation was within a remarkable 2% of the modern value. Summarising, we may compare the maximal values of the two node equations as follows:

	<u>Function</u>	<u>Modern value</u>	<u>TMM</u>	<u>Flamsteed/Kepler</u>
First node equation:	sin (S-H)	9'43"	9'30"	-
Second node equation:	sin2(S-N)	1°36'11"	1°29'44"	1°39'46"

The amplitude of TMM's second node equation is unspecified in the text, and had to be found by those constructing TMM-based tables. Persons composing such independently would be unlikely to arrive at the same magnitude to an arcsecond. This provides a 'fingerprint' technique for ascertaining who was working independently and who copied, that a later chapter will pursue.

The reduction as the final step for our TMM program was modelled on the reduction tables of DOS, but using the parameters given by TMM. The correction is zero at the two nodes, and also at their quadratures, i.e. it is a sin2θ function. The angle of the lunar orbit to the ecliptic varies, TMM tells us, as the Sun's angle to the nodal axis, being maximal at conjunction and minimal at quadrature. Consulting a standard table of reductions, for which we select Flamsteed's in DOS as was reprinted without alteration by LeMonnier in Paris in 1746, we observe that the reduction's .....

\* These two amplitudes were kindly found by Bernard Yallop, with the aid of Brower and Clemence's *Methods of Celestial Mechanics* (1961, p.312).



maximal value varies between 6'33" and 7'22", depending on the orbit's angle of inclination. TMM instructs us to follow the tables prepared 'after the common way'. DOS's maximal reduction varies by 12%, while the angle of inclination of the orbit to the ecliptic is given by TMM as varying by merely 6%, so the reduction is changing by twice as much as does the orbit angle each year. We require a modulating term with an amplitude of six percent, and the reduction term is therefore

$$M_{e_{nd}} = M_7 + 0.116 \times \sin 2(N_2 - M_6) [1 + 0.059 \cos 2(N_2 - S_1)],$$

the cosine term giving maximal values for solar conjunctions with the nodal axis and smallest at quadratures.

## VII Latitude

Celestial latitude varies as  $\sin(N-M)$ , going through one cycle per month. Nodal longitude is measured from the North Node, so that its latitude function starts off with increasing values. Latitude is maximal at the quadrature position midway between the two nodes, and this maximum value is  $\pm 5^\circ 17' 20''$  when the nodes are conjunct the Sun and  $\pm 4^\circ 59' 35''$  when in quadrature; thus there is a modulating function varying as  $2\theta$  function, as for the reduction but with half the amplitude. The mean value is  $5^\circ 8' 31''$  or  $5.142^\circ$ . Latitude becomes positive as the Moon passes the North node, when  $(M-N)$  has a positive value. Putting the slower-moving position first, a  $\sin(N-M)$  function will require a minus sign in front. Thus, TMM's instructions give us a celestial latitude formula of:

$$\text{Latitude} = -5.142 \times \sin(N_2 - M_6) [1 + 0.0288 \cos 2(N_2 - S_1)]^\circ$$

Later on we will ascertain how well this latitude function performs. Flamsteed found it to be TMM's weakest point when commenting on it in 1703: 'The errors in latitude are frequently 2, 3, or 4 minutes, which is intolerable' (to Caswell, 23 March 1703, Baily p.213). Newton's mean value for the inclination of the lunar orbit is 16 arcseconds less than the modern value.

TMM concludes with some remarks about parallax and refraction which do not concern us. Overall, as far as monthly cycles are concerned, TMM appears as largely based on the tropical and anomalistic cycles, with the phase and nodal cycles only playing minor, ancillary roles.

## VII An Early Draft of TMM?

A manuscript of Newton's published in the *Correspondence* (pp.3-5, Volume IV) is entitled *A Theory of the Moon*. The commentary there stated that while there was 'no clue' as to the date of its composition, it was probably written 'some time prior to' TMM, adding that its text was published 'almost verbatim' in PNPM of 1713.

Westfall referred to this manuscript in his view that:

'...a paper called 'A Theory of the Moon' listed rules for computing seven corrections without discussing their theoretical foundation ...

Several years later, Newton allowed David Gregory to take a copy of it and to publish it...' (1980, p.547)

adding that the version published in *Correspondence* was 'probably from 1695.' In Chapter Three a composition date of TMM was suggested as 1700, i.e., shortly before Gregory saw it. Westfall's view, in contrast, is that Newton had virtually composed it some years earlier, and merely reproduced it in 1700. Of relevance here is an irate letter from Newton to Flamsteed of January 6th, 1699, when the latter had planned to mention, in a forthcoming opus of John Wallis, his contribution to Newton's endeavours over lunar theory. People were wondering what Flamsteed had been doing all these years as the 'King's Observator', as he had published little, so he wanted to state his contribution towards the advance of theoretical astronomy, as having supplied the observations. After all, several years earlier he had heard stories, put about by Halley, that Newton had so far perfected the lunar theory that further observations by him were hardly necessary (Baily, p.162). Newton forbade this act, on the grounds that:

'...with respect to the theory of the moon, I was concerned to be publicly brought upon the stage about what, perhaps, will never be fitted for the public, and thereby the world put into an expectation of what, perhaps, they are never likely to have.'

That must surely be read as an admission of failure, as a statement to his colleague that his endeavours had not been such as to warrant any proclamation to the learned world. Are we to believe that TMM had then been substantially composed, implying that the above-quoted words to Flamsteed were mere deception? Much depends here on whether TMM is viewed as having

been a success, or a failure. The leading British theoretical astronomers, Halley, Whiston and Gregory, were as we have seen in no doubt upon this issue, once they saw it. That is why it has here been affirmed, that TMM was composed *after* the above-quoted remark and not before.

Westfall has claimed that TMM was composed in the 1690s, and therefore by implication viewing it as a failure, since, as he rightly observed of this period, 'Newton himself regarded the effort as a failure' (Ibid p.547). Science historians, as was pointed out in Chapter One in discussing Bernard Cohen's view, have never viewed TMM as a working mechanism, that defined five positions in ecliptic longitude and one in latitude for a given time.

A letter from Newton to Halley of March 14, 1695 requested that the latter would deny prevalent reports that he was 'about the longitude at sea.' As this goal was the stated purpose of TMM when it was published, we must assume that no such composition had been formulated at the end of his main period of endeavour over lunar theory in the 1690s; to do so would imply a level of duplicity that we should not readily contemplate.

The view here taken, is that Newton did indeed regard his endeavours of the 1690s as a failure, but that he was then attempting to accomplish a derivation of the lunar inequalities from a gravity theory. Only after that had ended in failure, was TMM composed, effectively lacking reference to a gravity theory and merely improving Horrox's kinematic model.

Against the Westfall thesis, let us note that the brief 'earlier' manuscript has no seven rules or stages as does TMM, has no Horroxian mechanism, is far from being a complete procedure for locating longitude positions, and is rather a fragmentary discussion of gravity theory as was attempted for the second edition of PNEP. I query the whole notion that it is an earlier draft. It refers merely to the first three equations of TMM, and none of the subsequent ones. Later on we address the manner in which gravity theory was related to the instructions of TMM, a matter of the utmost importance to subsequent astronomers, where a discussion of this manuscript's gravity arguments will be appropriate.

Comparing the two new annual equations introduced by TMM, viz. 20' for the apse line and 9'30" for the nodes, with those given in the unpublished 'Theory', the latter are seen to be more exact. Its figures are, 20'9" for apse and 9'34" for the nodes. An additional order of magnitude accuracy has appeared. There is no more distinctive difference between the first and second editions of the *Principia* than the increase in numerical accuracy. Often, the accuracy of the Second Edition went beyond what was warranted by the data, as if endeavouring to convey credibility by an increase in the number of decimal places (as Westfall described in his 'Newton and the Fudge Factor' of 1970). This strongly indicates I suggest the arrow of time, demonstrating that the undated manuscript was composed long after TMM, and not earlier as Westfall has assumed.

The manuscript has an interesting remark about the 'annual equation':

'Moreover in deducing celestial motions from the laws of gravity we also discovered that the annual equation of the Moon's mean motion which Kepler and Horrocks coupled with the equation of time, but Flamsteed published separately, arises from the varying expansion of the Moon's orbit by the force of the sun, in accordance with Corollary 6 to Proposition 66 in Book 1.'

Its value of 11' 49" derives from Flamsteed's discovery that Earth's rotation rate was constant, which he established from daily transits of Sirius. This was the major theoretical issue on which Flamsteed disagreed with Kepler, who had accounted for the annual equation by supposing that Earth's rotation rate varied over the course of the year. Subsequent astronomers credited Flamsteed with having discovered the Equation of Time, linking mean and apparent solar time, as the seasonal variation in day-length. In the *Principia's* second Edition this passage appears with the reference to Flamsteed deleted, for reasons into which we need not enter. The value of 11'49" is in excess by about 37".

The last paragraph of this manuscript speculates about a nine-year cycle varying with the apse rotation (which would be,  $\sin[A-H]$ ). TMM has no period of this length - though Newton speculated upon one for the fourth equation as we saw, while the apse position passed through Cancer-Capricorn. It has no terms longer than a Horroxian year of 411 days. That

is a quite surprising feature about it from a modern viewpoint. Newton only grappled with the subject for just over half a year, from September 1694 until June 1695, and his most reliable positional data was after 1690, when Flamsteed's mural arc was working. In contrast with this emphasis upon short-term cycles, Halley was convinced that the 18-year Saros cycle was of vital relevance. After their discussions on this topic, Newton may have considered incorporating a longer cycle into his theory:

'I have learned furthermore from the same theory of gravity that the Sun acts upon the Moon more strongly in the individual years when the Moon's apogee and the Sun's perigee are in conjunction than when they are in opposition. From this there arise two periodic equations, one for the Moon's mean motion, the other for the motion of her apogee. These equations are nil when the Moon's apogee is either in conjunction with the Sun's perigee, or in opposition to it, and in other positions of the apogee they have a given proportion to each other. The sum of these equations, when they are at their maximum, is about 19 or 20 minutes...'

This is a further basis for believing that it was composed years after TMM, perhaps a decade or so later.

While having to disagree with both Westfall and the *Correspondence* commentator, our conclusion happily accords with Whiteside's view: he characterised this published manuscript as an 'initial version' of the opening paragraphs of the revised scholium of Proposition 35 of Book Three of the 1713 Second Edition of PNPM (Whiteside 1975, p.327, Note 46).

## Ch. 10

### TESTING THE SEVENFOLD CHAIN

Having formulated a model in accord with Newton's instructions, we now test its validity. There are three ways we will do this, the first being a comparison with historic computations by astronomers who adopted the TMM procedures. Such a comparison may help to establish confidence in the validity of the TMM-PC model; and further, to ascertain the extent to which historical authors used TMM, a question not yet well resolved by historians.

The other two approaches to be developed in this chapter are analytical, and they test the individual steps of TMM. This is first done theoretically, by comparing modern equations of lunar longitude with those of TMM. Thereby we may evaluate statements made by Baily, Whiteside and Wilson on the subject. Complementing this is a practical approach, whereby any step of TMM can be tested, by altering TMM-PC in some respect, and noting whether, on average, it thereby becomes more or less accurate.

The latter method should be able to give a definite answer as to the validity of any component of TMM, as may not be readily discerned from theoretical considerations. After all, none of the modern equations have their amplitudes modulated, in the way of TMM, by long-period functions. If, for example, we should be curious as to how much of an improvement was accomplished by Halley's modification of the Horroxian model for eccentricity, as compared with Flamsteed's model, then such a testing on TMM-PC should resolve the matter.

#### I Five Historic Case-Studies

Astronomy textbooks of the period always carried examples of longitude computation. TMM-PC will model these worked examples, provided their method was Newtonian. If possible we should avoid worked examples involving eclipses, since exact longitude would then be known, as solar longitude could be determined with great accuracy. These would provide a tempting opportunity for the astronomer to claim a greater accuracy. By the time

that the worked examples appear in the 1730s and 1740s a systematic error had accrued of nearly two arcminutes in the mean motion.

If, as with Halley and Leadbetter, the method involved logarithms, the steps may not be easy to follow. The present approach overcomes this difficulty, by viewing merely the beginning and end of the operational sequence. For the selection of our case-studies, we are guided by the most recent claim as regards who adopted TMM's procedures, made by Professor Wilson in GHA:

'Newton's rules for calculating the place of the Moon were incorporated into the tables of Charles Leadbeater's *Uranoscopia* (1735); in the tables that Flamsteed constructed about 1702 and which, having been given by Halley to P.C.Lemonnier, were published by the latter in his *Institutions astronomiques* of 1746; and in Halley's *Tabulae astronomicae* (1749).'

(p.267)

William Whewell gave, in the nineteenth century, a more extensive list of such persons, which a later chapter will consider. Our historical comparison will use the works above-cited by Wilson, plus the Dunthorne example treated earlier.

Table 10.1 gives the 'mean moon' position for the local mean time in the left hand column, with final ecliptic longitude below that, as given in the worked examples cited. To the right of these historic computations are those of TMM-PC for these times, showing a 'correct' value for that moment in time. Subtraction gives the difference between the two methods, in the 'a-b' column.

The table has utilised three different computer programmes for going back into past time. A high-precision lunar ephemeris gives the longitudes shown in italics. Subtraction of these values gives the error-values, both of the historic textbooks and of our TMM-PC program, cited in arcminutes. All longitudes have been converted to degrees, thus  $6s\ 27^{\circ}\ 59'\ 18''$  is given as  $207.988$ . In the first example, Leadbetter can be seen to agree with the TMM-PC mean value within ten arcseconds, an acceptable error for him to make in reading his tables.

Table 10.1: TMM COMPUTATIONS, HISTORIC VS COMPUTER, OF LUNAR LONGITUDE

	<u>Historic</u> degrees a	<u>TMM-PC</u> degrees b	<u>Difference</u> arcseconds a-b
<u>Leadbetter (1735)</u>			
May 7th 1731, 10hrs mean	207.988	207.991	-10"
answer:	202.337	202.337	+ 0"
actual posn.	202.407		
errors:	-4'.2	-4'.2	
<u>Leadbetter (1735)</u>			
Sept 16 1734 noon mean:	183.112	183.111	+ 3"
answer:	188.426	188.424	+ 6"
actual	188.459		
error (answer-true posn.):	-2'.0	-2'.1	
<u>Dunthorne (1739)</u>			
Jan 2nd 1737, 3h 40m mean:	80.119	80.119	0
answer:	74.137	74.136	+4" - 6th
actual position	74.251		
errors:	-6'.8	-6'.9	
<u>LeMonnier (1746)</u>			
Aug 4 1739, 5h 55m 25s mean:	134.869	134.862	+25"
answer:	132.642	132.639	+ 9"
actual	132.622		
errors:	+1'.2	+1'.0	
<u>Halley (1749)</u>			
Dec 5th 1725, 9h 8m 5s mean:	51.428	51.431	-9"
answer:	45.709	45.701	+25"
actual	45.713		
errors:	-0'.2	-0'.7	



We have seen how Dunthorne is the case-study which exactly mirrors TMM-PC, and his answer differs from TMM-PC by a mere  $0.001 = 4''$ . The others have generally made slight adjustments, chiefly to the sixth and seventh equations, either omitting them or reversing its sign. A later chapter will examine the methods used by the eighteenth-century astronomers who claimed to be using TMM.

All the others -Dunthorne, Leadbetter, Halley and LeMonnier - reversed the sign of the 6th equation as in the 1713 version. As it has up to two and a half minutes amplitude, this is of considerable significance. The TMM-PC program is modified accordingly.

Of the above worked examples, only Halley's had the node position conjunct the Moon and described an eclipse. It was the only worked example in his posthumously published opus, so we had no choice. The accuracy of his worked example could be ascribed either to some improvement of the method, or to his selection of the eclipse. We refrain from more definite comments until Chapter 14.

The agreement in the right-hand column is generally within arcseconds, which conclusively endorses the GHA's claim, that the above persons were using TMM, albeit modified somewhat in the last two equations. It may tend to support Baily's view that: 'It was not until the year 1735, when Leadbetter published his *Uranoscopia*, that we find a more perfect adoption of Gregory's *Newtonian rules* reduced to a tabular form' (p.702).

The modern longitude program given in italics runs on Julian time, and so is suitable for all five dates except for LeMonnier in Paris. For LeMonnier's date and time, the procedure was: subtract eleven days, add twelve hours to the given time, then subtract nine minutes and twenty seconds to convert from Paris local time to G.M.T. This gave July 24th 1739 O.S., at 17 hours 46 minutes 5 seconds G.M.T., which was inserted into the program\*.

We saw in chapter Five that Le Monnier's mean moon was more accurate than TMM's over this period. Using Le Monnier's tables, for the epoch date

We saw in chapter Five that Le Monnier's mean moon was more accurate than TMM's over this period. Using Le Monnier's tables, for the epoch date 1720, differences were compared from true values as in Chapter five, showing that it was then out by a mere 7 arcseconds. The same was done for the TMM program, whence we find that Lemonnier's mean moon was displaced 1'12" or  $0^{\circ}.02$  from that of TMM, so that amount was added to the first step of TMM's procedure solely for the LeMonnier example. Summarising, we modelled Le Monnier's operation sequences by using TMM-PC with the sixth equation reversed, and with just over one arcminute added to its mean motion.

After that adjustment to LeMonnier's mean moon, his method still diverged from TMM-PC by forty arcseconds, which divergence arose in his fourth step, the Equation of Centre. A later chapter will consider characteristics of the different astronomers, here we merely compare the program with their worked examples in a general manner.

In the case of the two worked examples by Charles Leadbetter, their first three equations echo TMM, then what seems to be TMM's sixth equation came next as the fourth, followed by the Equation of Centre. He used Halley's mean motions (Appendix III) but differed in keeping the seventh equation. These two worked examples of Leadbetter's 'Uranoscopia' of 1735 had values for the 'Sun's true place' agreeing with TMM within one or two arcseconds.

The above Table does not show the error in mean positions. As was earlier explained, this amounted to nearly two arcminutes for this period, and the error values can be seen to cluster around this value.

.....  
\* The time-values fed into the TMM program (days after noon, December 31st 1680, GMT, Old Style) were, respectively: 20456.153 (Dunthorne), 18389.417 (Leadbetter 1), 19617.000 (Leadbetter 2), 21389.240 (LeMonnier) and 16410.381 (Halley).

## II The Erronous Sixth

In the year 1713, Flamsteed wrote to a friend:

'I told you that the heavens rejected that equation of Sir I. Newton, which Gregory and Whiston called his sixth. I had then compared but 72 of my observations with the tables: now, I have examined above 100 more. I find them all firm in the same, and in the seventh too. And whereas Sir I. Newton has in his new book (pages 424 and 425) thrown off his sixth, and introduced one of near the same bigness but always of a contrary denomination, and a bigger in the room of the seventh, if I reject them both, the numbers will agree something better with the heavens than if I retain them: so that I have determined to lay these *crotchets* of Sir I. Newton's wholly aside.'

This view of Flamsteed's appeared in response to the new edition of PNEM (Baily, p.698). Earlier he had commented in general terms about discarding some of TMM's ancillary equations, but this would appear to be his first definite statement upon the matter. He has plainly noticed the reversal of sign for the sixth and enlargement of amplitude of both sixth and seventh, but was not impressed. His unfortunate conclusion was, that both the sixth and seventh equations were best omitted, and that 'the heavens rejected' the sixth even with its sign reversed.

Flamsteed was probably the first to discern the erroneous nature of the sixth equation, but otherwise his view is mistaken. For, as we shall now show, all four of the Newtonian ancillary equations turn up in the modern formulae. It is ironic that the person who supplied the data from which the theory was wrought, should end up sceptical about what had been attained. Before making such a comparison, our versions of the formulae are compared with those of others.

## III Newton's New Equations

Four new equations appeared as the second, third, sixth and seventh stages of TMM. The present work is the fourth to propose an algebraic format for them. Versions given previously by Baily (1835), Whiteside

(1975) and Wilson (1989) are here compared with ours, though omitting the amplitude-modulating terms. Symbols used in the present text are employed for the comparisons.

The second Equation:

Baily	$3'45'' \sin^2(A-S)$	given as $\sin^2[(M-S) - (M-A)]$
Whiteside	$3'45'' \sin^2(S-A)$	
Wilson (G.H.A.)	$3'45'' \sin^2(A-S)$	given as $-\sin^2(S-A)$
TMM-PC	$3'45'' \sin^2(A-S)$	

Whiteside's term is reversed in sign as compared with the other three.

Third equation:

Baily	$47'' \sin^2(S-N)$	
Whiteside	$47'' \sin^2(S-N)$	
Wilson	$47'' \sin^2(N-S)$	given as $-\sin^2(S-N)$
TMM-PC	$47'' \sin^2(N-S)$	

Both Baily and Whiteside have the functions in reverse mode, ie  $180^\circ$  out of phase as compared with Wilson. I ascertained the plus and minus signs largely empirically, by writing the equation into the computer then observing whether the plus/minus values changed in accord with TMM's instructions for varying time-values.

Sixth equation:

Baily	$2'10'' \sin(M-S+A-H)$	given as $\sin[2(M-S)+(S-H)-(M-A)]$
Whiteside	$2'10'' \sin(M-S+A-H)$	
Wilson	$2'10'' \sin(S-M+H-A)$	given as $-\sin(M-S+A-H)$
TMM-PC	$2'10'' \sin(S-M+H-A)$	

The first two have the signs reversed as compared with the others.

Seventh Equation:

Baily	$2'20'' \sin(M-S)$	
Whiteside	$2'20'' \sin(M-S)$	
Wilson	$2'20'' \sin(S-M)$	given as $-\sin(M-S)$
TMM-PC	$2'20'' \sin(S-M)$	

To avoid confusion over signs, we quote GHA, that 'M-S is the angular distance of the Moon from the Sun' (p.267): envisaging the luminaries as revolving anticlockwise around Earth, angles measured anticlockwise are positive. In a sense, Baily and Whiteside were unconcerned with the signs of these terms, which only have meaning within an operating system.

#### IV Comparison with modern terms

The new equations of TMM appeared, as Baily complained, without justification:

'Newton has not explained, in the document under review, how he deduced these new equations, nor whether any of them are derived from his own theory of gravitation, or from Horrox' theory of the libratory motion of the lunar apogee...'  
(Baily, p.694)

While this is true, it will here be argued, in contrast with the views of others on this matter, that the new equations showed the profound intuition of their author. Not only did Newton originate the concept of ancillary equations in this context, an unheard-of thing prior to about 1695/6, but all four of them were substantially valid, and even had near to their optimal amplitudes. TMM was marred by having its sixth equation the wrong way round, however this was corrected in 1713, well prior to the period when astronomers commenced using it. We have already seen how several astronomers who took up the Newtonian theory accomplished this vital reversal of sign in the sixth equation.

The modern equations for lunar longitude are normally cast in terms of solar anomaly (M), lunar anomaly (M'), lunar elongation (D) (angular distance from the Sun) and mean lunar distance from ascending node (F). These are used because they turn up most often in the hundreds of terms comprising the theory. We may add an asterisk to the modern solar anomaly term, as M\*, to avoid confusion with the TMM symbol. To compare these with the TMM program, we must recall that the modern definitions of anomaly are with respect to perigee and perihelion, and so are 180° out of phase with the old. We may then transform them using the symbols M (Moon), S (Sun), N (node), A (Apogee), and H (Aphelion); thus,  $D=M-S$ ,  $F=M-N$ ,  $M^*=S-H+180^\circ$  and  $M'=M-A+180^\circ$ . The modern term

$$+0.041\sin(M'-M^*)$$

becomes  $2'28'' \sin [(M-A) - (S-H)] = 2'28'' \sin(M-S+H-A)$ , which we can recognise as the sixth equation. The first fourteen modern equations are presented in order of diminishing amplitude.

<u>MODERN</u>	<u>EQUIVALENT</u>	<u>NEWTONIAN</u>	
1) +6.2888sinM'	6°17'24" sin(M-A)	ellipse function	
2) +1.274sin(2D-M')	1°16'26" sin(M+A-2S-180)	evection	
3) +0.658sin2D	39'29" sin2(M-S)	35'32" sin2(M-S)	5th eqn.
4) +0.213sin2M'	12'49" sin2(M-A)	Horrocks theory	
5) -0.185sinM*	-11' 8" sin(S-H)	11'49" sin(H-S)	1st eqn.
6) -0.114sin2F	-6'51" sin2(M-N)	6'57" sin2(N-M)	reduction
7) +0.058sin(2D-2M')	3'32" sin2(A-S)	3'45" sin2(A-S)	2nd eqn.
8) +0.057sin(2D-M*-M')	3'26" sin(M-3S+A+H)	-	
9) +0.053sin(2D+M')	-3'12" sin(3M-A-2S)	-	
10) +0.046sin(2D-M*)	-2'44" sin(2M-3S+H)	-	
11) +0.041sin(M'-M*)	2'28" sin(M-S+H-A)	-2'25" sin(S-M+A-H)	6th,1713
12) -0.034sinD	-2' 5" sin(M-S)	2'20" sin(S-M)	7th eqn.
13) -0.030sin(M*+M')	-1'49" sin(M-A+S-H)		
14) +0.015sin(2D-2F)	55" sin2(N-S)	47" sin2(N-S)	3rd eqn.

Table 10.2: The first fourteen modern terms for lunar longitude are given on the left, then restated in the adjacent column using TMM-PC symbols. The TMM equations are given in the third column, where the first equation refers to the annual equation and the fifth to the Variation.

The first four terms indicate the fundamentally different basis of modern lunar theory from that of Newton. With the annual equation, the fifth in the modern sequence, the first of TMM's steps appears. Three or four of the modern terms do not correspond with anything in the old theory. If omitted, their amplitudes are such that they would generate errors well beyond the 6 arcminutes or so maximum of TMM. Only to a limited extent can we compare these sets of terms, as their mode of use differs: the modern procedure uses the *same* mean values at every stage, whereas the Newtonian procedure changes these at each step by 'equating' them.

Baily acknowledged the validity of three of the four new equations, though appearing very doubtful about their amplitude. Of the second, he

wrote rather sceptically that: 'This equation, depending on twice the annual argument, or  $2(D-A)$  according to Delambre's system of notation, does not amount to so much as  $1'$  in the tables of Mayer, Burgh or Burckhardt'(p.737). That is curious, since in the modern scheme it amounts to  $3\frac{1}{2}$  arcminutes, ie it should have been larger.

On the small third equation, Baily commented that it was 'somewhat greater in the tables of Mayer, Burgh, and Burckhardt.' One hopes it was not much greater, since in the modern scheme its amplitude is a mere  $55''$ . Of the sixth, Baily observed that its coefficient and sign as given in the 1713 *Principia* were adopted by Halley.

Whiteside in his 1976 essay did not commit himself to affirming that any of the new, Newtonian equations were valid, but merely concluded: 'Pity those - notably Halley - who in the early decades of the eighteenth century tried to found solidly accurate tables of the moon's motion upon such a flimsy, rickety basis.' That was far from being Halley's view. We are not able to support the GHA view that, of the four new equations, only the second 'has the correct form and, very nearly, the correct coefficient' (p.267).

## V 'Apogee in ye Summer Signs'

In the winter of 1694/5, as was mentioned in the previous chapter, the lunar and solar apses drew into alignment as the Sun crossed over them both at midwinter. In November of that year Newton sent an urgent request for lunar data when 'apogee is in ye summer signs' (Corr. IV p.47). Newton sought in vain for any (A-H) perturbation term linked to this nine-year cycle, absent from his TMM. On July 27th 1695 he wrote to Flamsteed,

'I had rather you would send me those [observations] from Aug. 24th, 1685, to July 5th, 1686, when the aphelium was in the same position as in the year 1677.'

At the Full Moon of December 21 1694, mean apogee was at  $5^\circ$  Cancer (a 'summer sign'), merely  $5^\circ$  away from the syzygy axis. It would appear from the above equations that there are no simple terms involving the (A-H) function that could have been discovered.

Would it have been possible for Newton to discern any further lunar equations? One answer is, that no further equations were discernible at that time, as they were too complex; that he found all there was to find, then left Cambridge for London. His sustained work on the lunar theory occupied half a year, from September '94 to June '95. In September, Halley discovered the cyclic return of the comet that bears his name, which may have tended to move Newton's attention away from the subject. In the autumn of 1695 TMM's author accepted a job as Master of the Mint: on November 26, 1695, Wallis wrote to Halley, 'We are told here [Oxford] that he [Newton] is made Master of the Mint, which if so, I do congratulate him' (More, 1934, p.435).

Terms such as ' $\sin(3M-A-S)$ ' are not intelligible as the seventeenth and first half of the eighteenth-century understood the notion: merely, they come out of the mathematics after complex differential equations have been applied, and as such belong to an entirely different epoch.

## VI Testing the new equations

To investigate TMM's four new equations, a sampling period of 160 days was chosen. This was selected as avoiding multiples and fractions of TMM's main cycles, namely 365, 205, 29.5 or 27.5 days. Forty such positions following the epoch date of 1680 were generated on TMM-PC, together with equivalent longitudes generated on a modern program, then the two values were subtracted, and the mean and standard deviations of the differences obtained. Lotus readily performs these operations for columns of figures, obviating human error. The means of these samples ought to be close to the error value of mean lunar motion used by TMM, or not significantly different from it, if our sample is indeed random with respect to the rhythms of the mechanism we are investigating.

The second, third and seventh equations were omitted, one at a time. This was done using TMM-2, ie with Newton's 1713 value of the sixth equation. As this version is more accurate, it will plainly be more sensitive to other factors. The third equation is of very small amplitude, so as can be seen its sign had to be reversed for appreciable effect.



Lastly, the Flamsteed-Horrocks method of varying the eccentricity was used in place of the Halley-Newton method. Here are the results, citing the percentage increases of standard error.

Table 10.3: Accuracies of TMM-2 Modifications

Percent error increases in right-hand column compare standard deviations cited against that of TMM-PC-2. Eg, on removal of the second equation in (b), the S.D. of errors appeared as  $\pm 3.08$  arcminutes, this being 64% more than 1.88, its optimal value.

a) TMM-PC-2:	$-0.38 \pm 1.88'$	
b) no second:	$-0.43 \pm 3.08'$	64% more
c) no third:	$-0.40 \pm 2.03'$	8% more
d) reversal of third:	$-0.42 \pm 2.30'$	22% more
e) no seventh:	$-0.40 \pm 2.75'$	46% more
.....		
f) TMM-PC	$-0.50 \pm 3.77'$	200% more
g) DOS eccentricity:	$-0.53 \pm 4.34'$	230% more.

The results for b-e confirm what was inferred from theoretical considerations above, that all four of the 'new' Newtonian equations contribute to predictive accuracy. Both their removal and their sign reversal impaired TMM's function - contrary to Flamsteed's opinion.

Line 'f' gives our second estimate of the error in the original TMM-1, the first being at the end of Chapter Eight, using a two-year sampling period, when a comparable result was obtained.

Flamsteed's Horrocksian method for finding eccentricity left it reduced by a factor  $\cos\delta$  as Gaythorpe pointed out (Ch. 7,IV), where  $\delta$  is the second equation of apogee. To estimate its accuracy, the function 'f' in TMM-PC was adjusted to give the simpler Horrocksian movement. Sir William Whewell took a derogatory view of Halley's adjustment in this regard, as being a mere 'slight alteration' (Ch.7,IV), and similarly Whiteside expressed the view that 'Newton himself (not that it matters a great deal from a computational viewpoint) was in fact to adopt Halley's variant on this...

(Whiteside 1975 p.327 note 35).’ In contrast, our quantitative approach has revealed that Halley’s contribution was the greatest single improvement whereby the Newtonian approach differed from the Horrocksian. Two-thirds of the error was removed by that one adjustment (line ‘g’ above), thereby confirming Newton’s own view that it was a ‘very good’ improvement (Corr. IV p.34).

A statistical ‘confidence limit’ is normally taken as double the standard deviation, being the range containing 95% of the data. On this basis, TMM-2 had a confidence limit of 3.6 arcminutes.

An attempt was next made to optimise the program, by giving the four new equations the amplitudes of their corresponding above-quoted modern functions. Those of the second and third were reduced slightly, while for the sixth and seventh they were increased. This increased the standard deviation by one percent. Next, the lunar eccentricity value was decreased, from the 0.055050 value of TMM, to 0.05490 as the modern value for this constant (equivalent to using the modern equation’s amplitude, as in the above-cited Table, of  $6^{\circ} 17' 24''$ , in place of TMM’s  $6^{\circ} 18' 3''$ ). Again, a slight increase in standard deviation for the forty data-points was observed. I could not find any case where adjustment of the TMM parameters improved accuracy.

## VII Comparison with DOS

Reconstructing the DOS procedure will help us to appreciate the relation between Flamsteed and Newton, as well as what was meant by ‘Horrocksian.’ Such a model ought to generate the same errors as Newton and Gregory were shown three centuries ago on their visit to Greenwich.

The idea of tackling lunar theory seems to have come to Newton in September of 1694 when with David Gregory he paid a visit to Flamsteed at Greenwich, and was shown a table of lunar latitudes and longitudes as observed, together with their discrepancies from what Flamsteed calculated ought to be their positions (both Gregory and Flamsteed have left notes of

this event). The challenge was for Newton to construct something better than the DOS method.

Flamsteed's *De Sphaera* gave the furthest development of the Horrocksian method of finding lunar longitude. It is the proper point of comparison for assessing TMM, being its immediate ancestor. William Whiston, in his astronomy lectures to the Cambridge University mathematics students, in the year 1703, advised them:

'Take, therefore, Mr Horrox's Lunar Hypothesis, as cultivated and explained by Mr Flamsteed.' (*Astronomical Lectures*, 1716, p.104)

It had three stages: the annual equation, equation of centre, and Variation. They are similarly described in the procedure given by Flamsteed in Horrox's *Opera Omnia* of 1673 (Horrox, 1673, p.494), except for minor alterations in constants and mean motions.

We shall call the program simulating the DOS procedure DOS-PC. Flamsteed's *De Sphaera* dealt with many other issues, but 'DOS-PC' designates solely its method of finding lunar longitude. The TMM program was deconstructed to reach this more primitive procedure, as follows: remove equations 2,3,6 and 7; remove annual equations from node and apse; diminish solar eccentricity to the Horroxian value; remove the modulating factor from equation 5 (the Variation) and increase its amplitude to 38 arcminutes; insert DOS epoch values in place of TMM's\* and DOS parameters for lunar eccentricity; measuring the latter along mean apse by the function:

$$E = 0.05524 + 0.01162\cos 2\phi$$

in place of the Newtonian

$$E = 0.05505/(1.0454 + 0.4262\cos 2\phi) \quad (\text{Ch.7,II});$$

add in a proportionality factor to make the equation of apogee, as produced by the Horrox-wheel, slightly smaller, of maximal amplitude 11° 47' 22" in

.....

\* Ch. 5, II, also Appendix III. The daily mean lunar motion was also adjusted, the difference between DOS and TMM being one arcsecond a year.

place of TMM's  $12^{\circ} 15' 4''^*$ ; and simplify the equation of the node, so that it becomes a simple sine function of  $2(S_1-N)$ , and no longer has a Horrox-wheel mechanism as TMM gave it.

Flamsteed's DOS gave ten steps to the method; The first comprised his 'Equation of Days' of which he was regarded as the pioneer (See, eg, Thomas Streete in his introduction to the second edition of his *Astronomia Carolina* of 1705); whereby the uniform rotation of the Earth in sidereal space became the basis for the definition of time, mean time as opposed to clock time, later standardised as Greenwich Mean Time. This was discovered by Flamsteed using an immobile telescope on his balcony, timing Sirius's transit each day.

His steps two and three obtained the mean motions from tables. Step four subtracted out the annual equation for the two luminaries. Step five used what was called the 'Annual Argument' and which we have called the Horrox-angle, to 'equate' the apogee and eccentricity. The sixth used the mean anomaly (ie,  $M_1 - A_1$ ) to give the true equation of orbit; which we may represent by

$$M_2 = M_1 + h(M_1 - A_1)$$

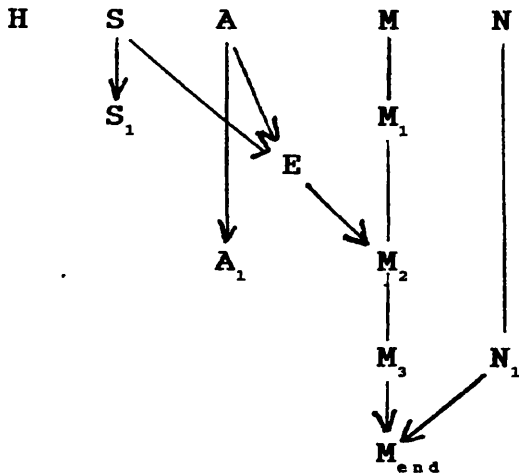
where h is the equation of centre function. Then the seventh stage adds the Variation. As our TMM-PC used the DOS reduction procedure, no adjustments are here required. The node had to be once equated before it could be used for finding the reduction and latitude, using tables based on the  $(S_1-N)$  angle. The overall latitude angle, ie the tilt of the Moon's orbit, likewise varied with that angle. The node's maximal equation was  $1^{\circ} 40'$ ,

.....

\* From Figure 7.4a, DOS's equation of apogee  $\delta$  is given by  $\tan \delta = FD/DT = 0.011286 \sin^2 \phi / E$ , where E is the varying eccentricity. Figure 7.4b depicts the maximal value of  $\delta$  which DOS gave as  $11.789$ , where  $CF/CT = \sin \delta = 0.2043$ ; taking the mean eccentricity line TC as equal to 0.055237, the radius FC is 0.011286. Effectively, DOS has two deferent wheels concentric upon C, the centre of Earth's orbit, the one for the apse equation being 2.9% smaller than that for the eccentricity function, as was also the case for Horrocks' theory (see 'linkage of e and  $\delta$ ' section in Chapter 7). Not prior to TMM did their magnitudes coincide.

such amplitudes being ascertained as the maximal values given in the DOS tables. Thereby a Lotus 1-2-3 program was constructed, here described firstly by a flow diagram of its steps of equation, and secondly by the trigonometric functions involved:

The Steps of Equation in DOS-PC



MEAN MOTIONS

$$M = 181.7328 + 13.1763946t \quad S = 290.580 + 0.9856469t$$

$$A = 244.1975 + 0.1114083t \quad H = 96.861 + 0.0000479t$$

$$N = 174.2430 - 0.0529550t,$$

where t is the time in Julian days from noon of December 31st, 1680.

ANNUAL EQUATION

$$M_1 = M - 0.197 \sin(H-S)$$

$$S_1 = S + 1.9368 \sin(H-S) - 0.0202 \sin^2(H-S)$$

EQUATION OF CENTRE

$$E = 0.055237 + 0.01162 \cos^2(A-S_1) \quad (\text{eccentricity})$$

$$A_1 = A - \arctan \frac{\sin^2(A-S_1)}{4.8943 + \cos^2(A-S_1)}$$

$$M_2 = M_1 + h\{E, (A_1 - M_1)\}$$

where h is the equation of centre function (Ch.6,II and Ch.8,I).

VARIATION

$$M_3 = M_2 + 0.633 \sin^2(M_2 - S_1)$$

REDUCTION

$$N_1 = N - 1.663 \sin^2(N - S_1)$$

$$M_{end} = M_3 + 0.116 \sin^2(N_1 - M_3) [1 + 0.059 \cos^2(N_1 - S_1)]$$

There are only two worked examples whereby we can check the working of this program. There was the worked example given in DOS, discussed in Chapter Eight; later in 1694 Flamsteed sent a table of computed positions to Newton, which we analyse in the next chapter. We may surmise that he had not by then altered his procedure, but his letter to Newton did not explicitly affirm such. Regrettably, William Whiston in 1703 used the same example as given in DOS; another was given by Cressner, discussed below. That is all, and it is not very much.

The DOS example had as we saw (Ch. 8, III, Table 8.3) an error of eleven arcminutes. London is five arcminutes due East of Greenwich, and we subtract one-third of a minute from its local mean time to obtain GMT. The Table below compares the magnitudes of its three equations: the annual equation, the Equation of Centre and the Variation, for the moment given in this example of 6.35p.m. London mean time, 22 December 1680 ( $t = -8.72596$  for the Lotus program).

	<u>DOS-PC</u>	<u>DOS</u>	
Eccentricity:	57681	57678	parts per million
Apse equation:	10°50'32"	10°50'32"	
1) Annual eq.:	-1'03"	1'03"	
2) Eq. of centre:	-54'27"	-54'22"	
3) Variation:	-36'15"	-36'16"	
Reduction:	<u>-3'59"</u>	<u>-4'00"</u>	
Long. in ecliptic:	65°9'51"	65°9'52"	

Five arcseconds is here the largest discrepancy in the steps of equation. Next, the accuracy of DOS-PC was tested using the method described earlier. Forty positions at 160-day intervals were generated, giving a mean error of:

$$-2.4 \pm 6.5 \text{ arcminutes,}$$

or a confidence limit of thirteen arcminutes. This well accords with Newton's remark made in a letter to Flamsteed of July 20, 1695, that 'The Horrocksian theory..never errs above 10 or 12 minutes' (to Flamsteed, Baily p.158); although, on January 15th, 1681, DOS-PC erred by 15 arcminutes. It also echoes the conclusion that Flamsteed expressed in the *Philosophical Transactions* of 1683, that 'even the best' lunar tables erred by at least

12 minutes. (*Phil. Trans.* 154, Vol.13, p.405); suggesting he had then made little progress with the problem.

Flamsteed gave a worked example in his collation of the posthumous works of Horrox (Horrox, 1673, p.494) which had, I found, an error of 13 arcminutes, which is similar to that in the above DOS example.

## VIII The First Computation

The first ever TMM-based calculation on record was published in the *Philosophical Transactions* of 1710, by 'the Reverend Mr H. Cressner, M.A., Fellow of the Royal Society.' He also gave a computation based on DOS, as published by William Whiston. The occasion was a lunar eclipse observed at Streatham in South London. Thus, at the dawn of the Age of Enlightenment, there existed two rival British lunar theories. Mr Cressner made the claim that he was the first to do this:

'There being therefore no Examples of any Calculation (that I know of) according to that Theory, nor of the Theory's Agreement with Observations yet made Public; I thought it proper to offer this one to this learned Society's perusal...I have added the Calculation from the famous Mr *Flamsteed's* Tables, according to *Horrox's* Theory, as I find them published in the Ingenious Mr *Whiston's* Astronomical Lectures, with the *Radix's* of the Mean Motions, corrected according to their first Author's later Observations, which are the same as Sir *Isaac Newton's* Theory.

'By comparing these two Calculations we may observe, that tho' most of the additional Equations in Sir *Isaac Newton's* Theory be very small in this situation of the Moon, yet they all conspire so as to make its Place considerable more agreeable to Observation, than those of *Horrox's* System.'

There is the curious assertion that TMM's mean motions represented Flamsteed's later views on the matter. The passage could be taken to imply that Flamsteed had not developed his lunar theory beyond what he published in 1681, even three decades later, except for slight modification of his mean motions.

We are told that both computations start from the same mean motions. The TMM-PC and DOS-PC programs were here used, the latter with TMM's mean motions, for the time given. The two calculations as shown by Cressner purport to ascertain the beginning and end of the eclipse. To my knowledge, TMM cannot be used for such, but only for the moment of exactitude. We take what Cressner called 'The Mean Time of the True Opposition' for the Julian date of 2nd February 1710 10 hrs (ie, 10pm), 54min, 48 sec at Streatham ( $t = 10625.4547$  for TMM-PC).

The steps of equation agreed tolerably well, and show that Cressner adopted the correct sign for the sixth equation. His finally-equated value ( $M_7$ ) differed from TMM-PC by 48 arcseconds, while for the DOS program it differed by a mere 25 arcseconds. The latter ought to be smaller, since tables then existed for the DOS procedure, while none then did for TMM's procedure. We cite the final values for  $M_7$ :

Longitudes for Feb. 2nd 1710, 10hrs 54 mins 48 sec GMT:

TMM-PC	145° 1' 12"
TMM (Cressner)	145° 00' 24"
DOS-PC	144° 55' 51"
DOS (Cressner)	144° 55' 23"
Actual	145° 2' 9"
Eclipse midpoint	144° 45' (at 10.45 p.m.)

The errors for TMM-PC and DOS-PC respectively amount to  $\frac{1}{2}'$  and  $5'$ . Cressner concluded, with regard to the *ending* of the eclipse:

'The Error therefore of Sir Isaac Newton's Theory is by this Observation but half a Minute, or none; of Horrox's System, Nine Minutes and a half,'

For the time specified, a few minutes after eclipse exactitude, for which Cressner presented his computation, Flamsteed's method erred by five arcminutes.



## IX Some Conclusions

We can now resolve certain issues that have remained conjectural for almost three centuries.

- 1) Newton's 1702 lunar theory had an error of  $-0.5 \pm 3.8$  arcminutes in the 1680s, taking mean motion values as corrected in 1705. Its mean error gradually increased with time.
- 2) This was almost twice as accurate as the lunar theory published by Britain's expert on the Horrox theory, Flamsteed, two decades earlier.
- 3) Halley's adjustment of the Horrocksian eccentricity function was the most important single improvement in Newton's construction of TMM, decreasing its mean error by nearly sixty percent.
- 4) TMM's four new equations were all sound, except that the sixth had its sign the wrong way round, and their coefficients were close to optimal, in giving agreement of the method of computation with observation.
- 5) Newton went as far as anyone then could have done in discerning such ancillary equations as were capable of improving the Horrocks theory.
- 6) With the sixth equation adjusted as specified by the 1713 *Principia*, TMM's standard deviation was no more than a mere 1.9 arcminutes (or, a confidence limit of  $\pm 3.8$  arcminutes).
- 7) The next chapter will show that TMM's accuracy increased at syzygy positions, traditionally the most important.

Historically, Flamsteed's assessment of TMM's longitude accuracy in the years prior to 1713 was sound while Halley's was, over that period, mistaken. It was not remotely within the bounds claimed by Gregory or Halley. Furthermore, Newton himself misjudged the matter, as evidenced by the several public statements of his on the subject discussed in Chapter One; we may also note an ascerbic recollection by Flamsteed, of a meeting at Greenwich on April 12th, 1704. At first he and Newton disagreed on optical matters (relations being less than cordial), after which:

'I showed him also my new lunar numbers, fitted to his corrections; and how much they erred: at which he seemed surprised, and said "It could not be." But, when he found that the errors of the tables were in observations made in 1675, 1676, and 1677, he laid hold on the time, and confessed he had not looked so far back: whereas, if his deductions



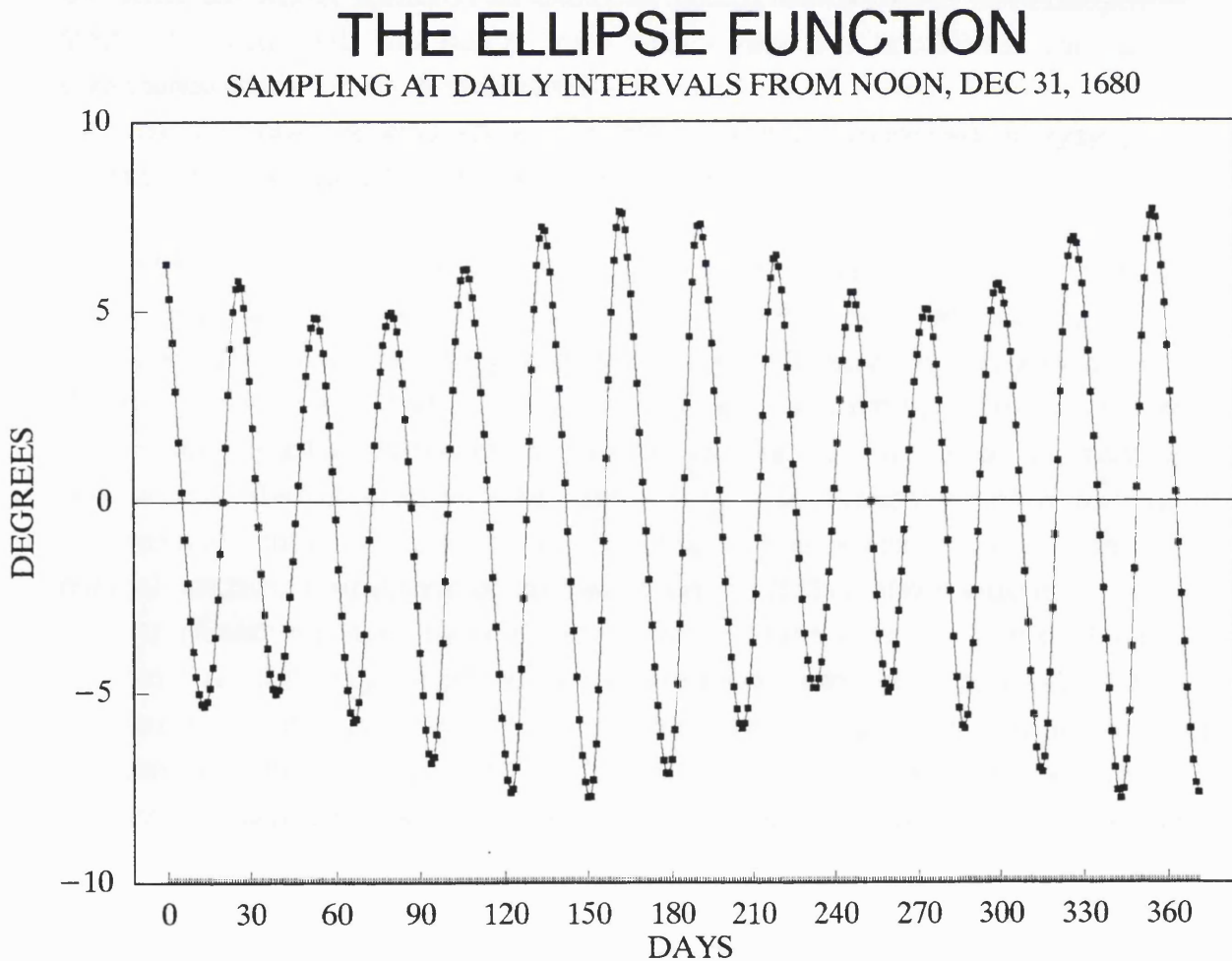
## Ch . 11 ERROR-PATTERNS

### I The Error-Envelope

To construct graphs using TMM, one needs to be able to run the program repetitively for given time-increments. The first step involves creating a table of sequential, TMM-PC-derived lunar longitudes. A 'macro' was written to accomplish this, which forms a loop in the program moving the time-value on by a fixed increment after printing out the corresponding longitude value.

Using this procedure, the program was first set to subtract mean lunar motion from the finally-equated position, at daily intervals. This gave the ellipse function, the lunar equation of center, whereby 'true anomaly' can

Figure 11.1: Mean lunar longitude subtracted from TMM's 'true' values, for 365 days.



move nearly seven degrees away from the mean position. It oscillates to the annalistic month period. Figure One shows this, sampling over a year. Its envelope has an amplitude varying with the 'Horroxian year' cycle, twice per thirteen months.

To construct the error-envelope of TMM, a lunar longitude program accurate to seconds of arc in historical time was used, able to generate columns of positions at any specified time-interval. Those columns had to be imported into the Lotus program. The program was set for the identical times as employed in the TMM-iteration procedure. Its columns of longitude data were then placed adjacent to those generated by TMM within the Lotus 1-2-3 spreadsheet, whereupon the error values {TMM - modern} could be found by subtraction.

Figure 11.2a: Comparison of daily error-values of original 1702 version of TMM (thin line) with that obtained after reversing the sixth equation (dotted line).

## ERROR PATTERN OF TMM

daily noon values GMT

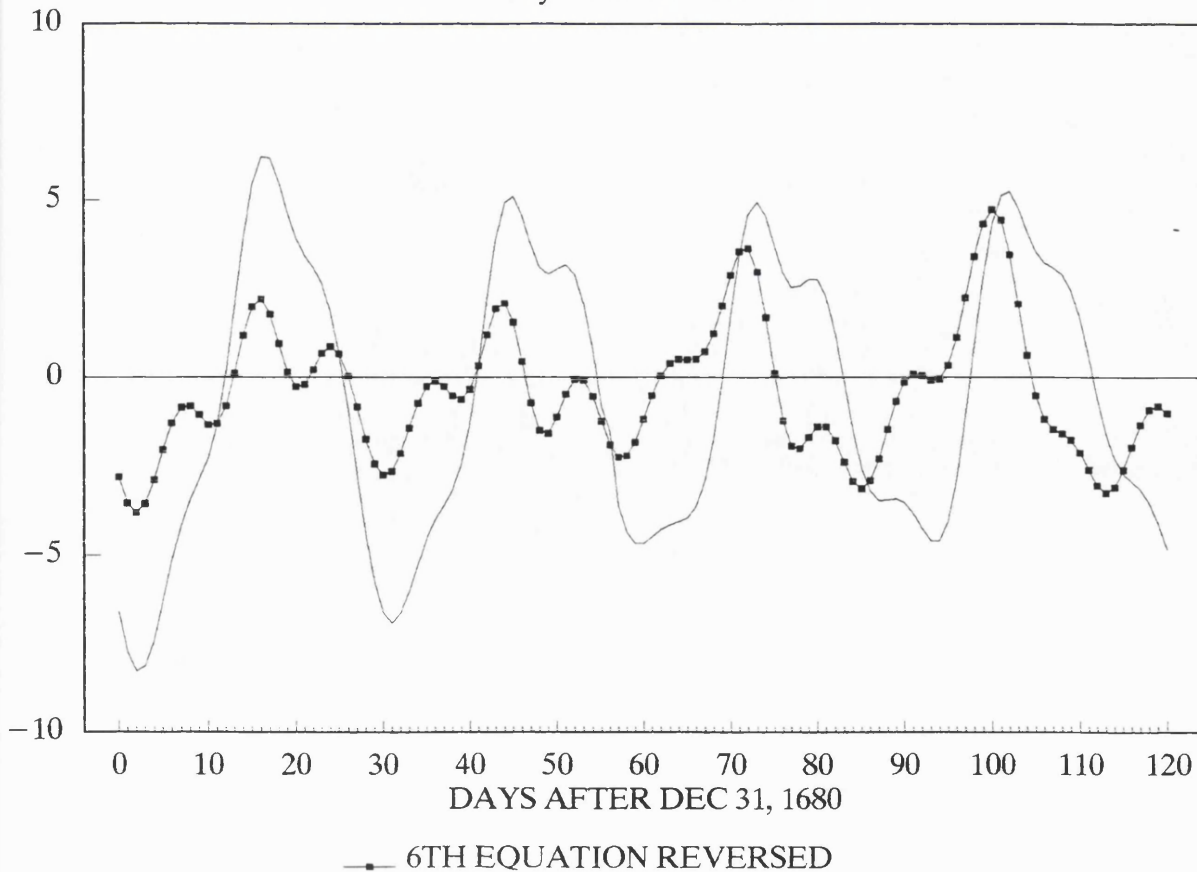


Figure 11.2a shows the graph of the two versions of TMM that were discussed in the previous chapter: the original of 1702, and that same program adjusted by reversing the sixth equation and slightly increasing its amplitude, as specified in 1713. A lunar-monthly rhythm is apparent, here peaking at the first lunar quarter, though this is not a permanent feature. A 50% decrease in mean error has appeared from reversing the sixth equation. Figure 11.2b shows the pattern continued somewhat longer, over eight lunar months.

## ERROR PATTERN OF TMM

daily noon values GMT

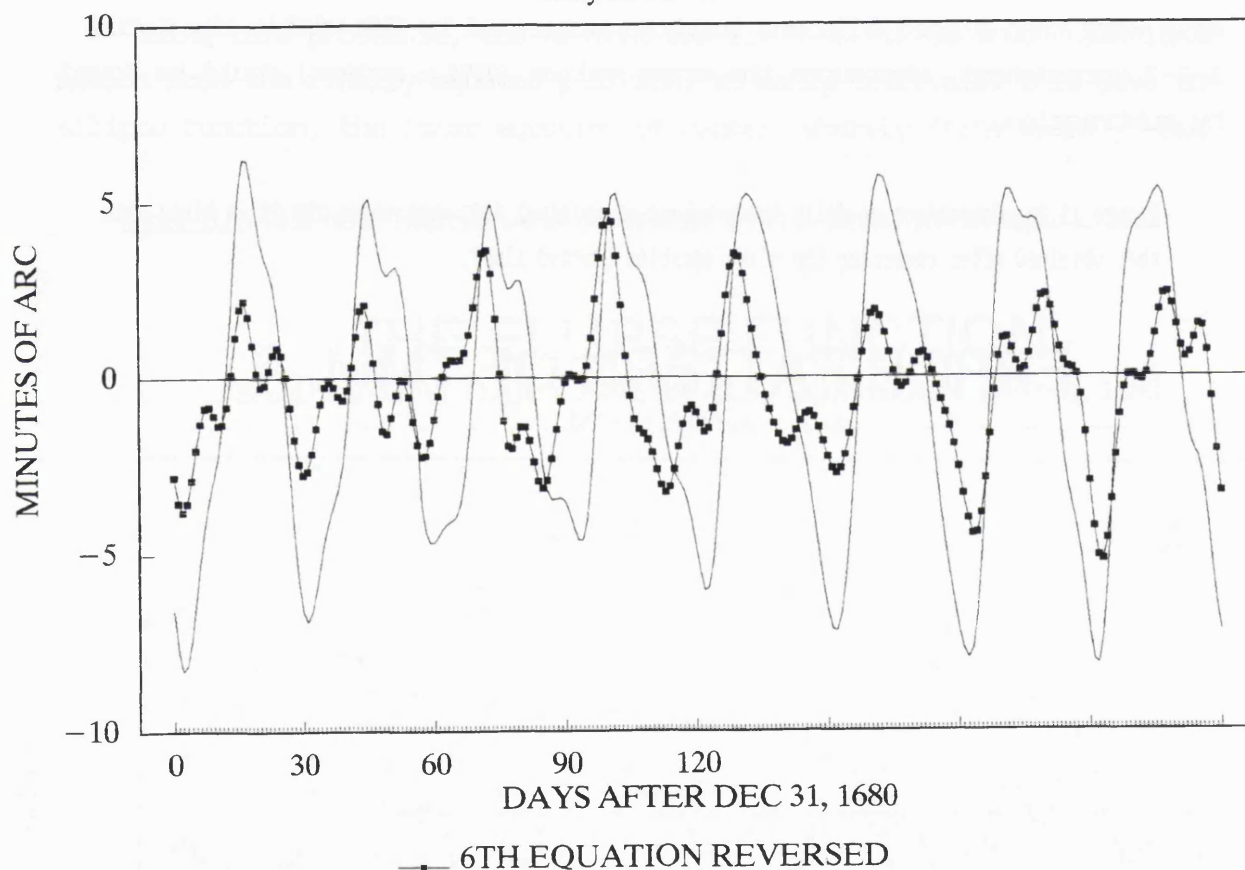


Figure 11.2b: As before, but sampling over 240 days.

## II Error-Periods

What error-pattern is generated by sampling periodically at apogee positions? TMM has several terms of the form  $(M-A)$  and  $(S-A)$ , so one should expect rhythms to be discernible at these periodicities. The patterns generated are shown in Figures 3a and 3b.

We start by locating an apogee time as the zero position. Mean positions are used, as they are required to stay in position over a period of time. Steps of 27.66 days were added, proceeding through a complete apse revolution of nine years. A large-amplitude rhythm of about six arcminute amplitude appears, going through seven cycles per apse revolution, of period 460 days. The astronomical motion generating such a cycle, TMM's strongest periodicity, is obscure.

### TMM'S APOGEE ERROR OVER A 9-YEAR APSE CYCLE

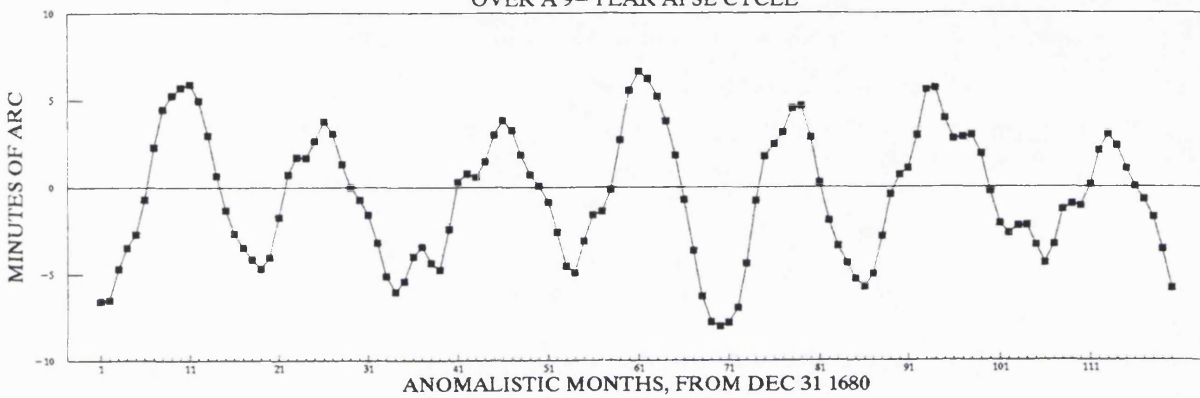
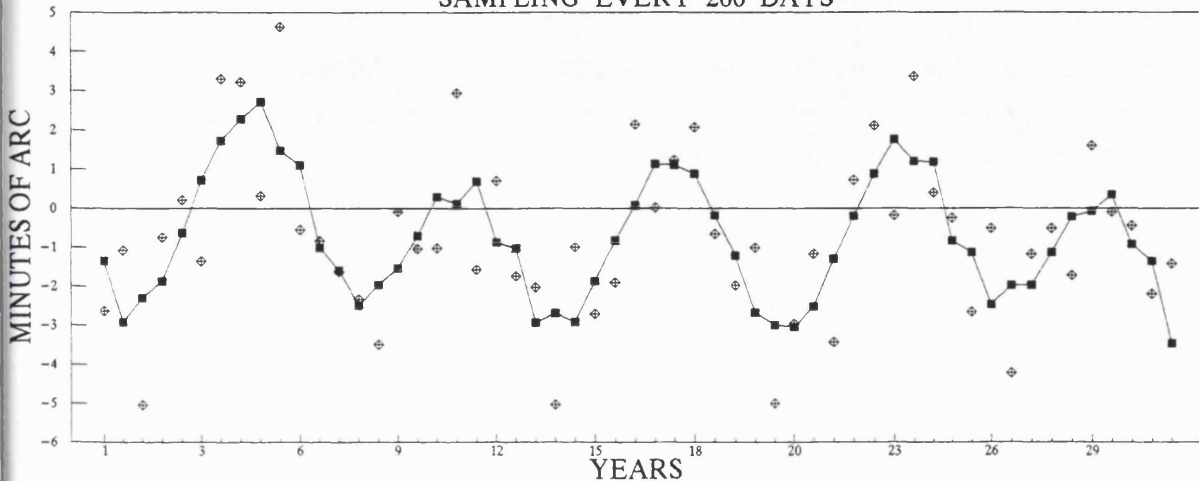


Figure 11.3a: Monthly TMM errors sampled at each mean lunar apogee, over a nine-year apse cycle.

Sampling instead at conjunctions of the mean Sun and mean apse, that is every  $6\frac{1}{2}$  months or 206 days - another period strongly encoded into the TMM program - then the rhythm shown in Figure 3b appears. The graph shown presents a six-yearly rhythm. A three-point moving average has been put

Figure 11.3b: TMM errors on successive Sun/apse conjunctions every 6.5 months over a thirty-year period. A three-point moving average has been added.

### TMM'S SUN-APSE ERROR SAMPLING EVERY 206 DAYS



through the data to smooth it. The effect is weaker than the previous monthly-iteration cycle, shown by its smaller amplitude of merely three arcminutes.

If instead sampling is done at each Full Moon, a fairly random pattern emerges. The syzygy errors are smaller than usual, as shown in Figure 3c, being mostly within two or three minutes of arc: Halley's claim made about the accuracy of TMM in his afterword to the third edition of Streete's *Astronomia Carolina* here appears as valid. And yet, the Full Moons do have an error-rhythm, albeit quite a weak one. Sampling was here done on alternate Full Moons, over a nine-year period, and a five-point moving average put through the data. This time (with some relief) we are able to identify the error-rhythm, as that of the nine-year apse cycle.

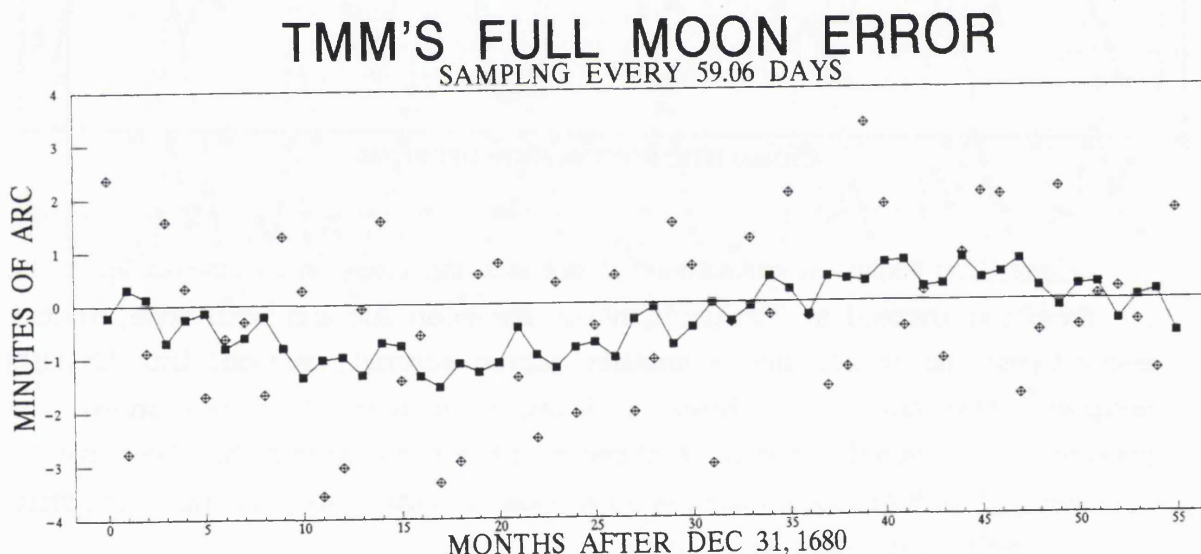


Figure 11.3c: TMM errors on alternate Full Moons over nine years, plus five-point moving average.

TMM is primarily linked to the anomalistic cycle, via its functions involving (M-A), and contains little by way of synodic terms involving (M-S), reflected in the differing amplitudes of these error-rhythms. The first has an amplitude of up to seven arcminutes, which is two or three times the claimed maximal error of the system. Furthermore, it is a coherent rhythm, in contrast with the other two which required smoothing with moving averages to discern them.

### III The 'Hidden Terms' of Longitude

In the previous chapter, it was observed that the modern equations for lunar longitude had four terms in the 2-3 arcminutes range, not included in TMM, ie modern longitude terms within TMM's amplitude range, not evidently incorporated into it. Were these related to its error-pattern? To ascertain this, the following sum was plotted over a three-month period:

$$3.4\sin(2D-M^*-M') + 3.2\sin(2D+M') + 2.7\sin(2D-M^*) - 1.8\sin(M^*+M')$$

(For these symbols, as used in the RGO formulae,  $M^*$  and  $M'$  represent anomaly values.) The sum of these functions gave nothing resembling TMM's error pattern, either in the shape of their envelope or in amplitude. They generate a function having a standard deviation of 4.0', far larger than TMM's error. The terms all have different periods, and so align now and then, giving an amplitude of up to ten arcminutes, while the TMM-2 function has only half such a maximal error. A puzzle thereby arises.

The TMM equations contain amplitude-modulating functions. Those for equations two and five vary through the course of a year, while that of the seventh varies through one apse cycle. As the modern equations do not have such, would the comparison be improved by their removal? To find out, TMM-PC2 had its modulating functions removed. This meant that, in the case of the second equation for example, in place of

$$6.25 - 0.31\cos(H-S_1)$$

merely 6.25 remained. Their removal did not increase the resemblance: the programme's standard deviation remained at  $\pm 1.8$  arc minutes over a period of daily sampling.

Did that result mean that TMM's modulating functions served no useful purpose, that their author had merely imagined their efficacy? After all, the modern equations have nothing like them. Forty values were generated at 160-day intervals as in the previous chapter, with these modulating terms removed, and compared with correct longitudes. The mean error thereby generated was  $0.4 \pm 2.1$  arcminutes. This is a larger value than TMM-PC2 gives otherwise. From this we conclude, that the three modulating functions within TMM's equations did serve a useful purpose.



If we compare the modern and traditional longitude equations, the former apply the mean motions at every stage, without any 'steps of equation' concept whereby each stage took 'equated' values from the previous level. They use *four* variables in their terms: using our symbols, these are given by  $(M-A+180^\circ)$  and  $(S-H+180^\circ)$  as the anomalies,  $(M-S)$  as elongation and  $(M-N)$  as the Moon's distance from its node. The terms add onto the mean lunar longitude  $L$ .

To ascertain how many of the modern terms give a comparable accuracy to TMM, the first dozen or so of them (see p.134) was written onto the Lotus spreadsheet. We here recall that their anomaly terms differ by  $180^\circ$  from those used in TMM, see p.63. These modern terms are quite standard, and used in lunar longitude computer programs. TMM's mean motion equations were used, with the four variables as above defined constructed from them. The error-estimation procedure was used as in the previous chapter, with a 160-day period. Thirty such times were taken, from the epoch date of December 31, 1680, showing a diminishing error as successive terms were added:

The first twelve terms only gave a mean error of	-0.44	±2.1	arcminutes
" thirteen	"	-0.48	±1.60 "
" fourteen	"	<u>-0.46</u>	<u>±1.56</u> "
TMM-PC2	"	-0.38	±1.88 " (p.137)

It is evident that TMM had an accuracy almost equivalent to the first thirteen of the modern equations.

I am not able to account for this phenomenon, beyond observing that TMM's construction was quite different from the modern set of functions and so there may be a limit to which we can compare them. In the previous chapter's list, the modern terms without any evident equivalence are numbers 8,9,10 and 13. It is accepted that the Horrox function incorporates equations 1 and 2 (elliptic function and evection), and presumably also 4. I believe that TMM cannot be improved by adding on these equations as extras, though admittedly, equations 8, 9, 10 and 13 have only been so checked altogether and not individually. Around the summer of 1695, when Flamsteed was puzzled that his letters were no longer being answered,

Newton had discerned all of the ancillary equations that could have then been found, to the level of accuracy at which he was working.

#### IV Latitude

A major criticism of TMM by Flamsteed was that it lacked accuracy in latitude:

'The errors in latitude are frequently 2,3 or 4 minutes, which is intolerable. They result not only from my own observations, but from those of others at the same time.'

(Letter to Caswell, March 1703, Baily p.213)

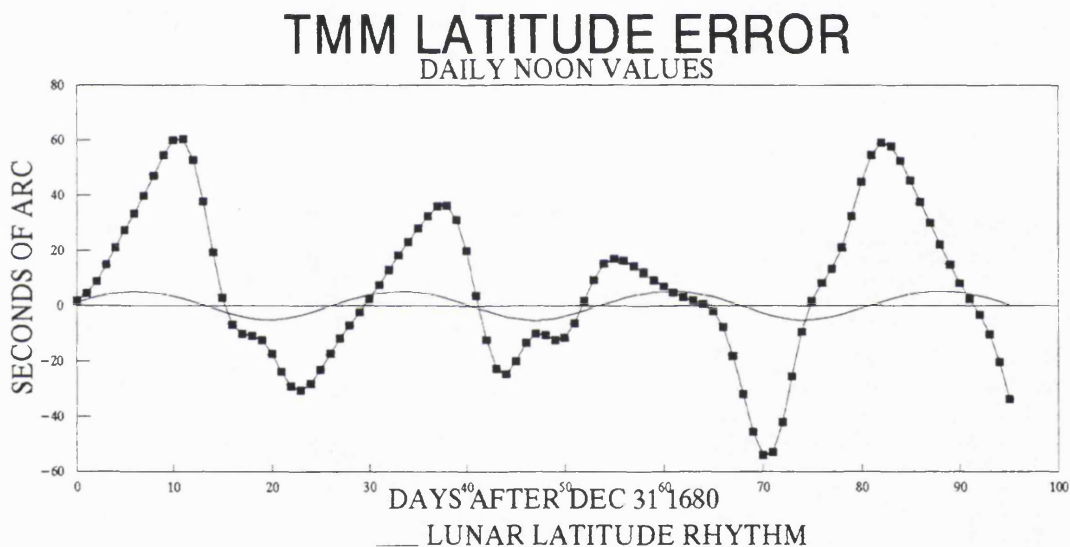


Figure 11.4: Daily errors in TMM latitude formula, in arcseconds, with lunar latitude shown for comparison.

In Chapter Nine a latitude formula was given, derived from the TMM instructions. The graph (Figure 11.4) depicts the error of this function, for the opening months of 1681. The error here remains largely within an arcminute. To check this, forty latitude values were derived from TMM at 160-day intervals, and subtracted from latitude values generated by the RGO program for those times. These forty values gave a mean of:

$$\text{latitude error} = -2 \pm 36 \text{ arcseconds,}$$

indicating a confidence limit of within an arcminute. This appears as the

second major issue where Flamsteed's judgement over TMM has turned out to be erroneous: he was as we have seen also mistaken in his dismissal of the new equations.

### V Flamsteed's View of DOS Errors

We may compare Flamsteed's comments upon supposed TMM latitude errors with those of his own latitude predictions, as he sent them to Newton in his letter of February 7th, 1695 (Table 11.1). The last column gives latitude errors of his own DOS procedure, and their mean error amounts to  $1'.4 \pm 1'.0$ . It is odd that he should have regarded errors in someone else's theory of two to three arcminutes as 'intolerable' when his own were

Annus Men die	Tempus Appar d h '	D A Rect o ' "	D dist a P o ' "	Longitud s o ' "	Latitudo o ' "	diff: a Tab Flam		
						Longit ' "	Latit ' "	
1692 Maij 16 17 19	8.59.11	199.39.20	103.22.50	♈ 23.10.58	4.41.40 A	+0.56	-2.50	
	9.52.05	213.56.50	108.55.50	♎ 8.01.45	4.59.22 A	+2.13	-2.16	
	11.46.15	244.36.00	116.04.40	♆ 7.15.47	4.34.50 A	+2.07	-1.24	
Junij 13 15 16	7.41.09	208.56.00	107.17.40	♎ 2.58.28	5.04.58 A	+2.06	-2.20	
	9.30.42	238.23.30	115.15.15	♆ 1.40.05	4.49.25 A	+8.18	-2.18	
	10.28.37	254.00.00	116.56.20	♆ 15.44.06	4.14.45 A	+7.50	-2.04	
1694 Dec 28 30 31	17.30.36	192.18.40	100.34.55	♈ 15.26.18	4.52.15 A	-4.31	-2.24	:: ob diei lucem
	19.12.57	220.08.30	109.14.30	♎ 14.31.38	3.25.25 A	-3.40	-1.19	
	20.10.47	235.42.40	112.05.05	♎ 28.29.53	2.16.48 A	-6.02	-2.09	
1695 Jan. 9 11 12 13 14 18	3.44.49	358.43.00	84.53.50	♈ 0.51.43	5.11.25 B	+2.11	+1.46	
	5.18.09	24.12.40	75.13.30	♈ 27.49.40	4.21.13 B	+7.13	+1.01	
	6.05.04	37.02.40	71.31.00	♄ 10.40.02	3.37.03 B	+8.02	+1.21	
	6.52.55	50.04.50	68.47.20	♄ 23.12.33	2.41.23 B	+8.31	-0.17	
	7.41.26	63.18.00	67.04.30	♀ 5.32.35	1.41.03 B	+8.13	+0.08	
	10.54.32	115.51.00	71.17.00	♄ 24.25.25	2.35.53 B	+7.17	+0.42	
omissa inter- ponatur 1695 Jan. 8	2.57.07	345.42.00	90.31.50	♄ 16.38.05	5.09.34 B	-0.03	+0.44	
Altitudo Poli Grenovici 51°.29'. <sup>(10)</sup>						differentiæ ostendunt quantum lunæ longitudines et latitudines observatæ supputatas e meis tabulis excedunt vel ab iis defi- ciunt		J.F:

Table 11.1: 16 of Flamsteed's lunar positions, with dates in Old Style and 'Apparent time' measured from noon, i.e. from solar meridian transit; lunar right ascension to the nearest sixth of an arcminute, North polar distance (90°-dec.) converted to longitude (measured 0-30°, together with zodiac sign) and latitude marked A or B to distinguish north or south of the ecliptic; plus errors from using his DOS tables; as sent to Newton (February 1695, Corr. IV p. 85). A transcription error is present: for Dec 30, 1694 longitude should read 13°41' instead of 14°31'.

of the much the same magnitude.

On September 1st 1694, Newton and David Gregory visited the Greenwich Observatory. Gregory's diary recalled how they were shown 'about fifty positions of the Moon reduced to a synopsis... Flamsteed is about to show him another hundred,' while Flamsteed recalled that the data included 'the places of ye Moon derived to ye same times & the differences or errors in 3 large sheets of paper in order to correct the Theorys of her motions' ('Memorandum by David Gregory', *Corr.* IV pp.7,8). Table 11.1 was presumably a part of that set of data. Sadly, it is all that survives of the lunar meridian readings which Flamsteed sent to Newton during their collaboration from September 1694 to June 1696.

The Table gives clock-time measurements ('Tempus Apparent') of meridian lunar transits, ie transit times for when the lunar *limb* first reached the meridian position. An Equation of Time was first applied to give mean time, then using the notion of the sidereal day, the column of Right Ascension was obtained. 'Distance from Vertex' ie ( $90^\circ$ -Declination) as measured was converted into 'Distance from Pole' using a value for the latitude of Greenwich taken as  $51^\circ 29'$ .

The data was converted from topocentric into geocentric form by applying parallax (and refraction) corrections to the vertical 'Distance from Pole' reading. Then, using a value for the obliquity of the ecliptic (taken as  $29^\circ 30'$ ), he derived longitude and latitude. The table gives longitudes for lunar centre, requiring a further correction based on lunar distance.

Thus, the data had to be considerably processed before it was usable to check a lunar theory. Newton once complained to Flamsteed, 'I want not your computations but your observations only (June 29, 1685, *Corr.* IV p.133).' There is no raw data in this Table. Flamsteed's observed data for a meridian transit would consist of: inaccurate clock times, up to half an hour out, a solar noon transit for estimating clock error, vertical mural arc angle, plus instrument correction(s) for that vertical reading. Flamsteed preferred not to reset his Tompion pendulum clocks each day,

giving his actual clock times in the *Historia Coelestis* Volume II, sometimes with the corrected clock times adjacent.

The longitude error column in the Table has a mean value of

$$3.1 \pm 4.8 \text{ arcminutes,}$$

notably less than the value ascertained in the previous chapter for the DOS model, of  $\pm 6.5$  arcminutes. This leaves open the possibility that positions generating larger errors on his theory had been removed. This column indicated how well his version of Horrocks's theory could work. The error column displays a large systematic error, as inherent in his mean motion. (The Table shows a contrast between the three early-morning observations, December 28-31, 1694, and the rest, but that is a mere coincidence, as the program has no diurnal component). These are the very error-values which Newton and David Gregory gazed upon in their September 1694 visit to the Greenwich Observatory.

Chapter Five showed that the DOS mean lunar motion was almost three arcminutes less in value than the 'true' mean. This we expressed using a negative sign. Flamsteed has here adopted a different sign convention, whereby the 'difference from Flamsteed's Tables' columns represent (actual longitude - theoretical longitude). For example, for June 15th, the longitude was given as  $1^\circ 40'$  in the sign Sagittarius, while our DOS-PC model gives  $1^\circ 32'$  of Sagittarius (ie,  $241^\circ$  measured from zero Aries). This deficiency of eight minutes is expressed as +8 arcminutes in Flamsteed's table. Thereby the systematic error of plus three arcminutes in his longitude error column accords with the error in mean motion which in Chapter Five we expressed as almost  $-3'$  for DOS in the 1690s.

From the clock times as given, GMT was reconstructed using a modern program for the equation of time (Hughes, Yallop & Hohenkerk, 1989). From those times, the actual longitudes were computed, shown in Table 11.2. Four things are here compared: Flamsteed's longitudes derived from observation  $F(\text{obs})$ , his computed longitudes  $F(\text{DOS})$ , the modern ILE estimate, and our reconstruction of DOS,  $PC(\text{DOS})$ , described in Chapter Ten.

The first columns give the dates plus estimated GMT values, in hours, minutes and seconds. These are followed by two longitude columns, firstly as generated using the ILE program, and secondly a reconstruction of Flamsteed's DOS longitudes (minutes and seconds only) for those times. The latter was obtained from Table 11.1, subtracting his longitude error estimates from his observed longitudes. These two columns are derived from theories: one using three equations and the other, sixteen hundred.

Table 11.2: Analysis of the Flamsteed Longitude Data (Feb 1695). Columns show: dates, 'GMT' in hours, minutes and seconds reconstructed from Flamsteed's LAT column in Table 11.1, longitudes from ILE program and by Flamsteed from his tables, and difference columns showing: accuracy of observational data, historic estimate of theoretical errors, and reconstructed errors in the theory.

	<u>Date</u>	<u>G.M.T.</u>	<u>ILE Long.</u>	<u>F(DOS)</u>	<u><math>\Delta F(\text{obs})</math></u>	<u><math>\Delta F(\text{DOS})</math></u>	<u><math>\Delta(\text{DOS-PC})</math></u>
	1) May 16, 1692	20/55/45	203° 11' 10"	10' 02"	0'.2	0'.9	1'.9
	2) May 17, 1692	21/48/46	218° 1' 19"	59' 32"	-0'.4	2'.2	2'.3
	3) May 19, 1692	23/43/12	247° 15' 42"	13' 40"	-0'.1	2'.1	1'.2
	4) Jun 13, 1692	19/42/54	212° 58' 23"	56' 22"	-0'.1	2'.1	6'.6
	5) Jun 15, 1692	21/32/53	241° 40' 39"	31' 47"	0'.6	8'.3	8'.3
	6) Jun 16, 1692	22/31/01	255° 44' 49"	36' 16"	0'.7	7'.8	8'.0
	7) Dec 29, 1694	5/38/07	195° 28' 03"	30' 49"	1'.7	-4'.5	-1'.3
ℓ	8) Dec 31, 1694	7/21/19	223° 42' 12"	45' 18"	0'.6	-3'.7	-3'.2
	9) Jan 1, 1695	8/19/34	238° 31' 35"	35' 55"	1'.7	-6'.0	-3'.9
	10) Jan 8, 1695	15/08/27	346° 36' 9"	38' 02"	-1'.9	-0'.1	-1'.3
	11) Jan 9, 1695	15/56/28	0° 50' 21"	49' 32"	-1'.4	2'.2	0'.7
ℓ	12) Jan 11, 1695	17/30/22	27° 48' 15"	42' 27"	-1'.4	7'.2	5'.3
	13) Jan 12, 1695	18/17/33	40° 39' 25"	32' 60"	-0'.6	8'.0	7'.1
	14) Jan 13, 1695	19/05/40	53° 12' 28"	4' 02"	-0'.1	8'.5	8'.4
	15) Jan 14, 1695	19/54/25	65° 32' 50"	24' 22"	0'.3	8'.2	8'.9
	16) Jan 18, 1695	23/08/29	114° 23' 29"	18' 08"	-1'.9	7'.3	5'.1
					-0'.1±1.1	3'.1±4.8	3'.4±4.2

$\Delta F(\text{obs}) = \text{ILE} - F(\text{obs})$  Our reconstruction of data accuracy

$\Delta F(\text{DOS}) = F(\text{obs}) - F(\text{DOS})$  Historic errors seen by Newton & Gregory

$\Delta F(\text{DOS-PC}) = \text{ILE} - \text{PC}(\text{DOS})$  Modern reconstruction of theoretical errors.

The  $\Delta F(\text{obs.})$  column has the difference between ILE longitudes and those in the above Flamsteed table. This gives the only estimate that exists of the accuracy of the raw data from which Newton derived his theory, a standard deviation of one arcminute. We may consider to what extent this was good enough, as TMM's third equation had an amplitude of less than an arcminute. Curiously, there was virtually no systematic error in the longitudes derived from observation (8 arcseconds). The above Table is regrettably our sole record of Flamsteed performing such computations, putting his data into a form needed by a theoretician, and as such is of especial interest.

The next column  $\Delta F(\text{theor.})$  merely repeats that given in the previous table, having as was found an unduly low error values of  $3' \pm 4'.8$ . We may say that the longitude data was some five times more accurate than predictions from the best theory available, clearly leaving scope for improvement.

Next, a PC(DOS) longitude value was obtained for each of these dates. Noon times were used for simplicity, as the error-pattern of such a lunar theory does not vary greatly with time of day. The last column shows the errors it generated, as {PC(DOS) - ILE} values, these being slightly smaller than those ascertained by Flamsteed from his computations.

#### Acknowledgements

Several persons assisted in developing the computer programmes here used, the present writer lacking any expertise in computing. Mr Jonathan Loretto set up the Lotus 1-2-3 program, and wrote the seven kilobytes of TMM-PC, after the Newtonian text had been translated into functional flow-diagrams suitable for the Lotus commands. The 'macro' used for plotting the graphs giving sequential values of TMM at specified time-intervals, was written by Vernon White. A modern program giving longitude positions correct to arcseconds was supplied by Bernard Yallop of the Royal Greenwich Observatory. The means of importing the astronomy program data into the Lotus 1-2-3 program, such that the two columns were created for identical time-series and so were comparable, was accomplished by Guy Atkinson.

## Ch. 12 TMM IN THE *PRINCIPIA*

The seven steps of TMM became embodied in the Second Edition of Newton's *Principia*, in the Scholium to Proposition 35 of Book III. This Scholium appears midway through the lunar arguments of Book III, following a summary of lunar inequalities in Proposition 22, and three sections deriving the Variation from gravity theory (Propositions 26, 28 and 29), and before the treatment of the Moon's influence upon the tides. The aim of its text was subtly altered, such that the prediction of longitude was no longer its primary goal.

The Scholium began with the affirmation:

'By these computations of the lunar motions I was desirous of showing that by the theory of gravity the motions of the moon could be calculated from their physical causes.'

The word 'theory' has here a different meaning from that used by Gregory in his title of 1702, *Theory of the Moon's Motion*. Whereas the text of 1702 had been prefaced by Gregory's claim that 'Physical Causes' had been reached at last, here that claim was made by its Author.

However, the Scholium apparently retained TMM's function of finding the longitude. The 1713 text served two different but hopefully concurrent purposes. After describing the seven steps, it averred:

'Sic habebitur locus verus Lunae in Orbe, & per reductionem loci hujus ad Eclipticam habebitur Longitudo Lunae.'

('Thus you have the true place of the Moon in her orbit, and by reduction to its place in the ecliptic will be found its longitude.') There was no mention of latitude, and indeed the paragraph making this affirmation vanished from the Third Edition.

The 1713 Scholium omitted what had previously been all-important, namely the numbered steps of equation. It lacked instructions for the sequence in which the various 'equations' were to be performed on the five zodiacal variables; although it did present the seven steps of equation in a sequence, almost identical with that of 1702. The Variation, treated earlier, was briefly recapitulated in the Scholium. The second node



equation of TMM was omitted. Was it the case that the 1713 text could only be 'worked', ie made to give longitude values, by presupposing the TMM operation sequence?

If TMM resembled a watch, then what appeared in 1713 was more an account of its gears, with a new gear added, rather than their assembly. Conceptually, the Variation is independent of eccentricity, being a deformation suffered by a circular orbit from the Sun's pull, and as such was presented in the *Principia* as a successful application of the three-body problem to 'explain' the inequality discovered by Tycho Brahe. It was therefore treated prior to the other TMM stages. A summary of where TMM's seven steps reappeared in the final Third Edition of 1726 may help:

	<u>1726</u>	<u>1702 Equivalents</u>
Book III, Propn. 29		Eqn.5 (the Variation)
Propn. 35, Scholium	Para 1:	Eqn.1 (lunar annual eqn.)
	Para 2:	Eqn.1 (other annual eqns.)
	Para 3:	Eqn.2
	Para 4:	Eqn.3
	Para 5:	Eqn.4 (Eqn. of Centre)
	Para 6:	Eqn.4 (Eqn. Centre epicycle)
	Para 7:	Eqn.6

There remain seven stages! All but the last of the above seven paragraphs in the Scholium began with a phrase like 'By the same theory of gravity...' or 'because of the Sun's force...' The kinematics of TMM was transformed into a new dynamics, with the *cause* of the equations given, in terms of forces. We can to some extent retrace the steps of the new approach.

## I Cotes' Contribution

The Scholium into which TMM metamorphosed in PNEM in 1713 comprised ten paragraphs. It had been changed considerably as a result of comments from Roger Cotes, astronomy professor at Trinity College, Cambridge, who assisted Newton in preparing his Second Edition. The correspondence of

Newton and Cotes was published by John Edleston in 1850, Edleston contributing some evaluations of Newton's modifications to the theory which remain of interest.

A 'New Scholium to Prop. XXXV' (reprinted in *Corr.* V pp. 291-5) was sent to Cotes, probably in the first week in July 1712 in the view of Edleston (1850, p.109). It comprised twelve paragraphs, of which the first seven opened with the repetitive phrases we have noted, 'by the theory of gravity', etc. These presented the first four steps of equation of TMM, and added *two* extra epicycles to the fourth: one was a yearly-period epicycle, to be discussed below, while the other was a nine-yearly one (in its seventh paragraph), varying the rate of motion of the apse line in relation to the Earth's aphelion. It omitted any discussion of the last three TMM equations.

There is an undated manuscript entitled *Theoria Luna*, published in the *Correspondence* (IV pp.1-5) probably belonging to this same period (see Chapter 9, section VIII). It discussed only the first three steps of equation of TMM, and also a nine-year inequality to the apse motion. Its logic is comparable to that of the 'New Scholium,' partly because both showed Newton contemplating a long-period epicycle.

Newton and Cotes discussed the yearly epicycle which was being added onto the Horrox-wheel. A letter of 20th July 1712 found Cotes apprehensive as to when the new emendations to the lunar theory would arrive. Finally, a 'revised draft' arrived, undated, inserted into the *Correspondence's* mid-August 1712 period (V, pp.328-9). This draft curtailed the fifth paragraph, re-cast the sixth, and added a new seventh, containing the sixth equation. The seventh paragraph now began, 'Computatio motus hujus difficilis est...', ('Computation of this motion is difficult..') instead of referring to the theory of gravity.

Sometime after that, an eighth paragraph must have been sent, starting 'Si computatio accuratur desideratur...'. It alluded to the Variation, which maximised in the octants, and then proposed an adjusted seventh equation, which it called the 'Variationem Secundum', which maximised at

the quadrants. We may conjecture that Newton found it harder to justify his sixth and seventh equations by reference to his gravity theory, which is why they were missing from the original 'New Scholium' draft. Thereby the Variation became the *sixth* equation, preceded by what had been TMM's sixth equation, now the fifth. We summarise this as follows:

<u>The 1713 sequence</u>	<u>The <i>Principia</i> names</u>	<u>the TMM steps</u>
3rd para	Aequatio semestris	2nd $\sin^2(A-S)$
4th para	Aequatio semestris secunda	3rd $\sin^2(N-S)$
5th & 6th paras	Aequatio centri	4th
7th para	Aequatio centri secunda	6th $\sin(S-M+H-A)$
8th para	Variatione Prima	5th $\sin^2(S-M)$
8th para	Variationem Secundam	7th $\sin(S-M)$

## II An Improvement on TMM?

From Edmond Halley onwards, commentators have inclined to the view that the theme of TMM was more fully developed in PNPM of 1713. In 1732 Halley as the Astronomer Royal wrote:

'.. the great Sir Isaac Newton had formed his curious Theory of the Moon, a first Sketch of which was inserted by Dr David Gregory in his *Astronomia Physicae & Geometria Elementa*, published at Oxford 1702; and again, in the second Edition of Sir Isaac's *Principia*, which came out in 1713, we have the same revised and amended by himself...'

(P.T., 37, p.190-1)

In 1977 Craig Waff wrote:

'... a revised and much expanded version of the 'Theory of the Moon' was published as the new Scholium to Proposition XXXV... I might further point out (from my own study in progress of the lunar tables based on Newton's 'Theory of the Moon') that many table-makers in the early eighteenth century considered the *Principia* version to be more up-to-date (as indeed it was) than the version which Cohen reprints, and consequently used it as a basic foundation for some of the lunar tables which they constructed.' (p.71)

Waff was criticising Bernard Cohen for supposedly not having appreciated that the Scholium of 1713 was a 'revised and much expanded' version of the 1702 opus.

On the other hand, William Whewell was a science historian who appreciated the practical significance of TMM, and he affirmed that 'These calculations were for a long period the basis of new Tables of the moon,' referring to TMM (1857, I, p.162). His review of these matters did not suggest that the 1713 *Principia* was an improvement, or that it was ever utilised as such by astronomers.

Table 12.1 shows the constants of TMM as modified in 1713. The lunar Equation of Centre maximal values were omitted from the *Principia's* Scholium. These would have had to be generated using the Kepler equation from the eccentricities, no simple task. Thereby the *Principia* text provided less of a practical guide to finding longitude than did TMM.

We may note that TMM introduced a tropical reference into the Second Edition of PNPM, whereas the First of 1687 was primarily sidereal. PNPM's quest for 'physical causes' was within sidereal space, this being the inertial reference framework - the immobile sensorium of the Deity, in Newton's language. TMM in contrast functioned within the tropical framework ie the zodiac, as being what astronomers used. Thus, the yearly motion of the nodes is given as  $19^{\circ}20'31''$  sidereally in the Scholium to proposition 33 (Motte, p.467) while also a tropical-year period is cited for comparison.

### III An Equation of Eccentricity

When Flamsteed originally explained the Horrox model to Newton in a letter of October 11th 1694, he added: 'To make the aequations bigger in winter yn Summer it will be requisite to make the diameter of this libratory Circle bigger in Winter yn Summer' (Corr. IV p.27). There was a hint that this modulation was Halley's idea, as being mooted between the three of them.

Table 12.1: The Constants of TMM, from 1702 to 1725

These constants represent the maximum values of 'equations', ie the peak values found in tables. When these values vary, eg seasonally, maximum and minimum values are given.

	<u>TMM</u>	<u>PNPM 1713</u>	<u>PNPM 1725</u>
Annual Eqns.: Moon	11'49"	11'52"	11'51'
Sun	1°56'20"	1°56'26"	same
Apogee	20'	19'52"	19'43"
Node	-9'30"	-9'27"	-9'24"
Eqn. 2	3'56"/3'34"	same	same
Eqn. 3	47"	49"/45"	same
Lunar Eqn. Centre	7°39'30"/4°57'56"	-	
Eqn. of Apogee	12°15'04"	12°18'	same
Ecc <sup>y</sup> in 10 <sup>6</sup>	66782/43319	66777/43323	same
Horrox-wheel size in 10 <sup>6</sup>	55050 ± 11732	55050 ± 11727	same
2nd Epicycle	-	± 352	same
Eqn. 5 Variation	37'25"/33'40"	37'11"/33'14"	same
Eqn. 6	2'10"	-2'25"	same
Eqn. 7	2'20"	1 - 2'	omitted
<u>Mean Motions</u>			
Aphelion in 100 yrs	21'40"	18'36"	
Moon 1700 epoch	315°19'50"	315°20'00"	15°21'00"
Apogee 1700 epoch	338°18'20"	338°20'00"	same
Sun 1700 epoch	290°43'50"	290°43'40"	same

In his letter of November 1st, 1694 (Corr. IV, p.42), Newton agreed:

'The excentricity & equation of ye Moons Orbit is sensibly greater in winter then (sic) in summer & seems to be sometimes as great as Mr Halley makes it, but ye law of its increase I am not yet master of, nor can be till I have seen ye course of the Moon as well when her apogee is in ye summer signes as in ye winter ones'

implying that several years of continuous data would be required to ascertain Halley's equation. That inequality was omitted from TMM, however it appeared in PNPM of 1713, as its chief innovation (For a discussion of Halley' theory here, which must remain conjectural as he published nothing on the matter, see *Correspondence V*, pp.296-8, note 3).

A new epicycle was added to the TMM mechanism, of yearly period, which, placed on the Horrox-wheel, generated a twice-equated eccentricity and apse motion. Cotes drew a helpful diagram, here shown (Figure 12.1), together with the *Principia's* diagram for comparison. The centre of the lunar orbit is now positioned at F instead of D as formerly.

This yearly expansion and contraction should not be confused with a supposed overall expansion and contraction of the lunar orbit through the seasons, whereby Newton was perceived as successfully having linked the 'annual equation' to a 'physical cause' ie gravity. Rather, it is a perturbation of the orbit that increases in the winter season, at *Principia* figure (p.424) Cotes' version (Corr.V p.285)

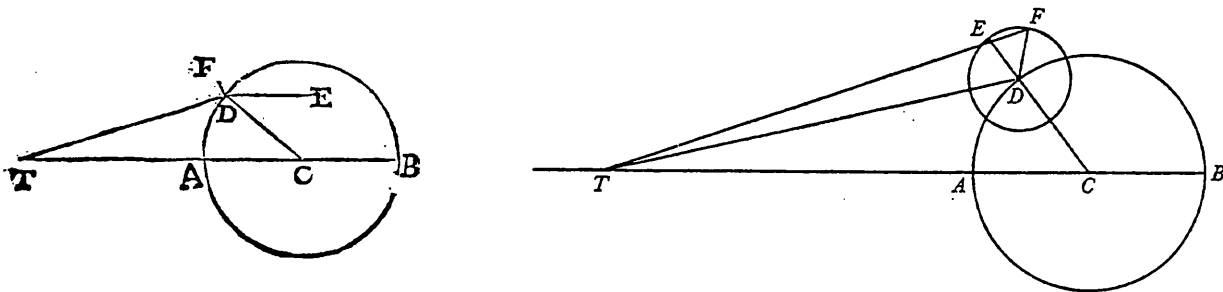


Figure 12.1: The New Epicycle of 1713. The first diagram is from the *Principia*, where 'F' represents the twice-equated position of the lunar orbit centre; the other is Professor Cotes' version, showing more clearly TF as the equated eccentricity value.

perihelion, with greater eccentricity and oscillation of the apse, from the Horrox-wheel's dilation.

In the diagram, TC represent the mean eccentricity. TD the eccentricity once equated and TE that twice equated. The apse equation now becomes FTC, instead of DTC. The wheels have magnitudes specified by: TC=5505, CD=1172.7 and DF=35.2. These dimensions signify that the lunar eccentricity fluctuates by ±21% semiannually, while this fluctuation itself varies seasonally by ±3%. Two paragraphs, the sixth and seventh, of PNPM explain this new epicycle, its period being given as follows:

'...and set off the angle EDF equal to the excess of the aforesaid annual argument above the distance of the moon's apogee from the sun's perigee forwards; or, which comes to the same thing, take the angle CDF equal to the complement of the sun's true anomaly to 360°.'

These two sets of instruction are equivalent. The first we may phrase as:

$$\begin{aligned} EDF &= (S-A) - (A-H+180) \\ &= 180+S-H-2A \end{aligned}$$

(S-A) is the 'annual argument', viz. the 'Horrox angle', while (A-H+180) gives the 'distance' in zodiac longitude 'of the moon's apogee from the sun's perigee.' 180° is added, as the TMM symbols A and H are measured from apogee and aphelion respectively. The second is given by:

$$CDF = [360-(S_1-H)] \quad - (1)$$

where  $S_1$  is the 'first-equated' solar longitude. However, we know the angle EDC, since  $DCB = 2(S-A)$ ,

so

$$EDC = DCT = 180-2(S-A),$$

and

$$\begin{aligned} EDF &= [360-(S_1-H)] - [180-2(S-A)] \\ &= 2(S-A) - (S_1-H) + 180 \\ &= 180+S-H-2A, \end{aligned} \quad - (2)$$

as above.

The *Principia's* two accounts are identical only if we ignore the difference between  $S_1$  and S. PNPM was not primarily concerned with the steps of equation. Its phrase 'true anomaly' was written as  $(S_1-H)$  in

equation 2 above. In TMM the difference between true and mean anomaly was crucial, here it is not.

This new epicycle responded to the varying distance of the Earth from the Sun, given in the sixth paragraph; no commentators have remarked upon its function. It amends the equation of centre, so should use a once-equated solar longitude.

Cotes was puzzled by this epicycle, and wrote (17 August, 1712, *Corr. V* p.325):

'It is evident that in the Earth's Aphelium DF will coincide with DG, & in ye Earth's Perihelium DF will coincide with DH, so revolving about the centre D, that the angle GDF may always be equal to the Suns mean Anomaly. Hence the angle EDF... will be equal to the excess of ye doubled Annual Argument above the Suns mean anomaly as I observ'd in my last. This is the only way according to which I can apprehend the motion of the point F in ye Secondary Epicycle.'

Cotes' phrase 'the excess of ye doubled annual argument above the Suns mean anomaly' is equivalent to equation (2) above - provided we overlook the difference between the sines of mean and true anomaly. At perihelion in midwinter, the Horrox-wheel is required to dilate and be larger than in summer, when F is furthest from C and reaches the point H as Cotes observed: in equation (1), CDF is then  $180^\circ$ .

In the original text of twelve paragraphs another epicycle was added of period nine years, to give eccentricity equated a third time and an apse equated a fourth time. This was subsequently omitted, so we have something to thank Cotes for.

#### **IV Constructing the New Epicycle**

The editor of the *Correspondence* commented as follows upon the new epicycle:

'Though Newton's description does not make this clear, the point F is now the empty focus (later centre) of the Moon's ellipse; it rotates semiannually about the point D.' (*Corr.V* p.298 note 3)



There are two errors here: the point F cannot be a focus of the ellipse, because the Earth's centre at T is such, and eccentricity was the distance between Earth's centre and the orbit centre; nor was the rotation period semiannual, but was seasonal, conferring a yearly expansion and contraction

upon the Horrox-wheel. To avoid such confusion, a further diagram is here presented, in which  $A_1$  represents apogee and  $S_1$  the sun's position, both once-equated, and the three lunar orbit centres, zero, once and twice equated, are shown as  $C$ ,  $C_1$  and  $C_2$ .

Figure 12.2: Orbit Centres for TMM-1713

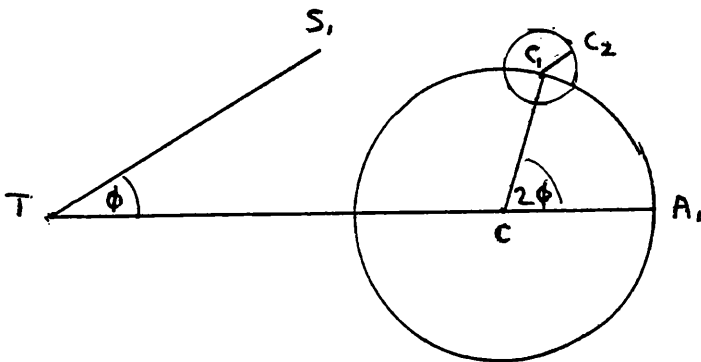
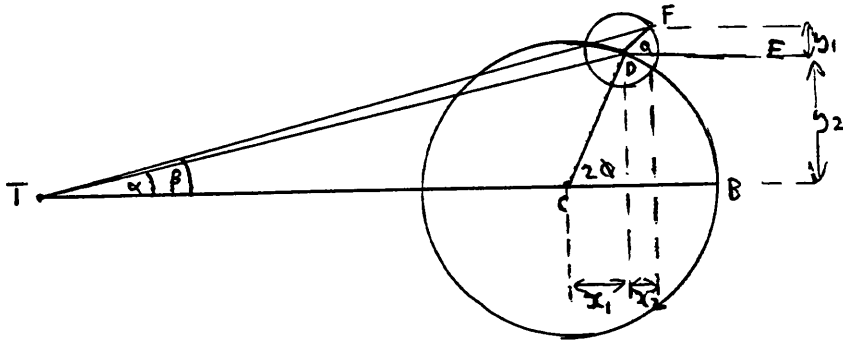


Figure 12.3: Reconstructing the 1713 epicycle, in which

$$\alpha = \text{CTD}, \beta = \text{CTF}, 2\phi = \text{DCB} \text{ and } a = \text{EDF}$$



The seventh paragraph of the Scholium described how to prepare tables that would give the angle FTD, to add onto the second apse equation. We shall not proceed in this manner, but will instead determine the horizontal (ie parallel to TC) and vertical co-ordinates of F with respect to T, (Figure 12.3). Putting TC equal to unity, CD becomes 0.2130 and DF 0.00639.

If  $y = y_1 + y_2$  and  $x = 1 + x_1 + x_2$ ,  
then  $y = CD\sin\phi + DF\sin\alpha$   
where  $a = EDF = 180 + S + H - 2A$  (equation (2) above) and  $\phi$  is the Horrox angle;  
and  $x = 1 + CD\cos\phi + DF\cos\alpha$ .

The eccentricity twice equated is given by

$$TF = 0.05505\sqrt{(x^2 + y^2)}$$

while two steps of equation for the apse line are conjointly given by

$$\beta = \arctan y/x$$

For the TMM program, we write this as

$$A_3 = A_1 - \beta,$$

replacing the  $\delta$  function, the angle CID, by  $\beta$  which is CTF; likewise the new term for TF simply replaces that for TD. The point D in Figure 1 was represented by F in TMM's diagram, so the point F has acquired a different position, though it retains the same meaning, viz the centre of the lunar orbit.

The eccentricity function has now its own steps of equation, as follows:

$$\begin{aligned} E_0 &= 0.055050 && = TC \\ E_1 &= E_0(1 + 0.2131\sin 2\phi) && = TD \\ E_2 &= E_0\sqrt{(x_2 + y_2)} && = TF \end{aligned}$$

## V The Sixth Equation

The seventh paragraph of the Scholium contains the sixth equation, its sign now reversed: 'addendam si summa illa fit minor semicirculo, subducendam si major' ('add if their sum comes to a minor semicircle (ie,  $<180^\circ$ ), subtract if it is more') the converse of the 1702 instructions. The great error of TMM was at last corrected.

On October 31st, 1713, three months after the publication of PNEP's Second Edition, Flamsteed complained to Sharp:

'...(Newton's) sixth equation is not allowed by the heavens. He has lately published his *Principia* anew, wherein he makes this equation ablative where it was formerly to be added, and to be added where it

was subductive; and has altered his seventh, so as in part to destroy it' (Baily p.304).

Flamsteed here erred, as the sixth equation is indeed 'allowed by the heavens' once its sign is reversed, as we saw earlier. He returned to this theme in another letter to Sharp of March 20th, 1714, averring that 'if I reject them both (ie, the sixth and seventh equations), the numbers will agree something better with the heavens than if I retain them' (Baily p.309).

Baily endorsed Flamsteed's general view as to how modified sixth and seventh equations appeared in PNP of 1713 (1835, p.697), as did Whiteside (1975, pp.323-4) and Cohen (1975, p.61-2). In 1989 however GHA stated categorically that: 'no mention is made of them (ie, the sixth and seventh equations) in any edition of the *Principia*' (p.267). We cannot endorse the GHA view. The sixth equation is clearly specified as having its argument (ie, angle) formed:

'by adding the distance of the moon from the sun to the distance of the moon's apogee from the apogee of the sun,' (Motte p.477)

which is the same (S-M+H-A) function about which Whiston had complained in 1703. Its amplitude has increased slightly, by 12% to 2'25", yet it remains unequivocally the same function.

Admittedly it was not referred to as the sixth equation, but as the 'Aequationum centri Secundam', and described cryptically as : 'the angle which the line DF contains with the line drawn from the point F to the moon...' Newton remarked, in a letter to Cotes of 12 August 1712 (*Correspondence*, V, p.320) that:

'The Equation described in this Paragraph I had first from observations of Lunar Eclipses, & afterwards found that it answered the Theory of gravity in the manner here described. Its quantity when greatest came to about 2'10" by eclipses. By ye theory tis 2'25".

The suggestion here (whether or not Cotes believed it) is that the 1702 amplitude was derived empirically, whereas the new amplitude was computed from theory. Lunar eclipses would have given accurate times at which the (L-S) component was 180°, presumably enabling the apse terms to be investigated.

Cohen published two statements upon this matter, before and after Dr Waff's comments, the latter appearing in a book review of Cohen's 1975 publication of TMM. In this 1975 opus, Cohen averred, concerning the modification of the sixth equation:

'But after 1713, when Newton had published the above-mentioned correction in ed. 2 of the *Principia*, there was no longer any excuse for continuing to reprint Newton's essay without alteration, as was done in both English editions of Gregory's textbook (and the second Latin edition), and the two English editions of Whiston's *Astronomical Lectures*, even though all declare in their second editions that the text has been 'corrected'. Nor was the correction introduced into the later reprintings of the *Miscellanea Curiosa* or of Harris's *Lexicon Technicum*; and it is not even mentioned as an annotation in Horsley's version in his edition of Newton's *Opera*.' (p.62)

Thus, astronomers were castigated by a historian for not having introduced a 'correction'. We concur with the importance of reversing the sign of the sixth equation, without which the function of TMM is greatly impaired, and shall in the next chapter survey these eighteenth-century publications in this context.

In 1980, Cohen merely made the cautious statement that:

'These results [ie, TMM of 1702] were then corrected and revised and in large measure introduced into the second edition of the *Principia* (1713)...' (p.276)

with which one can hardly disagree.

## VI The Seventh Equation

The eighth paragraph of the 1713 Scholium contains a restatement of the seventh equation, as the 'Variationem Secundam.' The Variation is a  $\sin^2(L-S)$  function, while the seventh equation has the form  $\sin(L-S)$ .

The 1713 text is:

'Ut radius ad sinum versum distantiae Apogaei Lunae a Perigaeo Solis in consequentia, ita angulus quidam P ad quartum proportionalem. Et ut radius ad sinum distantiae Lunae a Sole, ita summa hujus quarti proportionalis & anguli cujusdam alterius Q ad Variationem Secundam, subducendam si Lunae lumen augetur, addendam si diminuitur' (p.425).

where P and Q were assigned magnitudes of 2' and 1' respectively\*.

While PNP's language describing the seventh equation is obscure, Edleston interpreted its meaning as:

$$-[2'(1-\cos(A-H+180))+1']\sin(M-S)$$

Three sets of brackets within brackets may strain credulity, however Whiteside in 1975 (p.325) gave a comparable formula for the seventh equation. Its coefficient (ie, of sin(M-S)) he found to be:

$$1'+2'(1-\cos(A-H))$$

There is a 180° shift in the cosine function between the two, equivalent to a reversal of sign. Our TMM function was, approximately:

$$M_7 = M_6 + [2' + 1'\cos(H-A)]\sin(S-M).$$

We adjust the two modern interpretations for comparison:

Edleston:  $[1' + 2'(1 + \cos(H-A))]\sin(S-M)$

Whiteside:  $-[1' + 2'(1 - \cos(H-A))]\sin(S-M)$

TMM 1702  $[2' + 1'\cos(H-A)]\sin(S-M)$

From Edleston's formula, sin(M-S) has been changed to -sin(S-M), and -cos(A-H+180) to +cos(H-A).

Edleston's version of the function had the same signs as TMM's 1702 expression. We adopt his version of the TMM seventh equation in the *Principia*, this being the sole instance where we accept an equation on the authority of another. We thereby differ from Cohen's statement concerning the 1713 version that:

'this equation (the seventh) is, to all intents and purposes, no longer a part of Newton's system' (1975, p.62).

However, the language is obscure, and as we have already noted that some astronomers dropped the seventh equation, let us not wholly dismiss Cohen's .....

\* 'As the radius is to the versed sine of the distance in consequentia of the apogee of the Moon from the perigee of the Sun, so is the angle P to a fourth proportional. And as the radius is to the sine of the distance of the Moon from the Sun, so is the sum of this fourth proportional and of a certain angle Q to the second variation, to be subtracted if the light of the Moon is waxing, and to be added if it is waning.'

view. The paragraph containing the seventh equation disappeared from the Third Edition.

The seven steps finally reappeared in 1713, with amplitudes slightly adjusted, with the sixth and seventh reversed in sequence, with latitude omitted and omitting the second nodal equation. The last two steps were truncated from the Third Edition without explanation, in a paragraph which contained the statement, surely rather vital, that lunar longitude and latitude were discernible by these equations. Three components of TMM were thus omitted in the 1713 Scholium: the magnitude of the lunar equation of centre, the second nodal equation, which had its own epicycle, as would have affected the 'reduction,' and any latitude procedure. An extra epicycle was added to the Horrox-wheel, supposedly required by gravity theory.

While absent from the Scholium, a qualitative reference to the node equation was present in the earlier Proposition 22:

'But the nodes, on the contrary (by Cor. XI, Prop.LXVI, Book I), are quiescent in their syzygies, and go fastest back in their quadratures.'

That represents the TMM node equation, with syzygy meeting the nodal axis twice-yearly. Thus the syntax of TMM suffered a dismemberment, serving to support the theory of gravitation.

## VII The Truncated 1726 Version

The Third Edition of PNPM contained no explicit affirmation that the Scholium to Proposition 35 of Book III was of practical value. The paragraph containing such, the eighth as we saw in the 1713 edition, was omitted, along with TMM's fifth and seventh equations. We refrain from conjecture as to why that concluding paragraph was deleted. One would not expect astronomers who used TMM to have taken their directions from the Third Edition.

The Third Edition, the only edition to have been translated into English, concluded its account of the TMM equations with the cryptic words:

'And from the moon's place in its orbit thus corrected, its longitude may be found in the syzygies of the luminaries.'

This was followed by some considerations of refraction and mean motion. The literal meaning of the sentence is that longitude may be found at fortnightly intervals. The opacity of its meaning may have encouraged a tendency amongst posterity not to see a working mechanism, viz. TMM, buried under its gravity theory. It can however be understood by reference to the earlier 1713 edition, as follows.

The above statement concerning syzygies meant, in the Second Edition, that the theory thus far (ie up to the sixth equation) was accurate in those positions only, whereas, once the last two 'variation' equations were added, as was done in the following paragraph, it would become accurate over the whole month. Its omission thus damaged the meaning as originally intended.

### VIII No Baricentre Correction

If linkage with gravity theory was the goal, an equation could have been derived from the Earth's monthly path around the Earth-Moon centre of gravity. Such a displacement would affect the Sun's longitude by something resembling the solar parallax each month. Newton had written to Flamsteed in November of 1694 explaining how motion around a common Earth-Moon centre of gravity would lead to a monthly solar equation, maximum in the quarters:

'The quantity of this angle I do not yet know certainly. Tis not so great as I thought when I was in London. If you assume it to be 16" or 20" & find that by such an assumption ye greatest errors of ye suns place are diminished you may retain yt quantity, till it shall be determined more exactly.' (Corr. IV p.43)

Flamsteed missed the point of the argument, replying that:

'The parallactic equation of ye Sun is so small it will scarce be sensible by observation a single vibration of ye pendulum is equall to it...'  
(November 3rd, 1694, Corr. IV p.46)

- confounding diurnal motion with that around the zodiac: the 20" proposed by Newton, as motion in the Sun's longitude, takes eight minutes in time. It was far from Newton's view that such a magnitude could be ignored, but for whatever reason no such solar equation appeared in any version of TMM.

However, TMM in its concluding remarks did specify the value of the 'Sun's Horizontal Parallax' as 10", but made no use of it. Later in the century, D'Alembert argued that this baricentre equation would affect the Sun's position by 11"-13" (1754, p.xvii).

The equation is smaller, some 8" (Corr. IV p.44, note 11). Newton had initially overestimated the lunar relative mass by two hundred percent in PNPM of 1687 (Kollerstrom, 1991). In 1713 a baricentre computation appeared in PNPM with the lunar mass error reduced to an excess of merely 100% (Wilson 1980 p.60, Kollerstrom, 1985). Inclusion of such a term would have introduced an error as large as the equation, so its omission was just as well. D'Alembert greatly improved upon this mass ratio value.

## IX Adding the Epicycle

A century after Kepler had banished epicycles from the heavens with a new, physical astronomy, the second edition of the *Principia* employed two. We reconstruct them on the Lotus 1-2-3 spreadsheet, translating their revolutions into simple trigonometric terms. Regrettably, no improvement in accuracy thereby results. We use the interpretation given by Roger Cotes, as a check that our construction is sound.

What has here been called TMM-2 had but one modification, namely the sign-reversal of its sixth equation. Here we create two further steps of the 1713 version: the adding of an epicycle, designated as TMM-2E; and the insertion of the various adjusted constants given in Table 1, plus Edleston's version of the seventh equation\*. This final step was found to increase the accuracy of the end result by 1-2%, however this is a negligible amount. Astronomers of the eighteenth century would have noticed no improvement from so doing.

.....

\* Four Lotus spreadsheets were thereby used. The values they gave for lunar longitude at  $t=0$ , ie the TMM epoch of noon GMT on December 31st 1680 Old Style, are: TMM 187°.933, TMM-2 187°.993, TMM-2E 187°.979, TMM-1713 187°.979

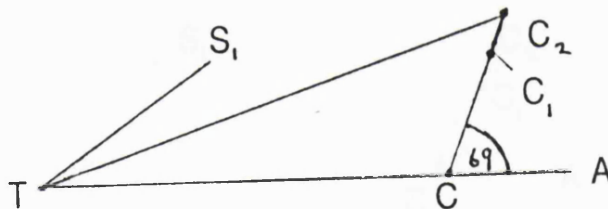


The working of the two combined epicycles in 'TMM-2E' was checked using the positions of the 1679 perihelion (midwinter) for which  $t = -13.3$  days and the next aphelion at  $t = 169.2$  days. These positions, for  $H-S = 180^\circ$  and  $0^\circ$  respectively, give simple triangles (see below, Figure 12.3) whereby the equations of apse and eccentricity can be found: to four figures (ie, as parts in  $10^5$ ), the lengths of  $CC_2$  are 1208 and 1138 respectively, ie  $1173 \pm 35$  units (of eccentricity, with respect to its mean value  $CT$  of 5505).

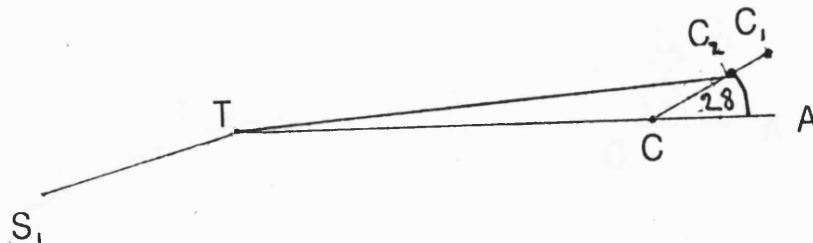
Cotes, in his comments to Newton upon the new epicycle, wrote more than once: 'As I apprehend it, the words *additur* and *subducitur* should change places' (Corr.p.285, Cotes to Newton, 3 May 1712). If even the Author experienced confusion on this matter, we should not expect to avoid this ourselves. We start with the relevant celestial longitudes, measuring angles anticlockwise as for the TMM diagrams.

Figure 12.4: Aphelion/Perihelion Positions for the epicycle, showing greatest and least radius of the 1713 Horrox-wheel, with the varying values for eccentricity and apse equation, at the perihelion (1679) and aphelion (1680) positions.

Perihelion:  
(S-A) =  $34^\circ$



Aphelion:  
(S-A) =  $194^\circ$



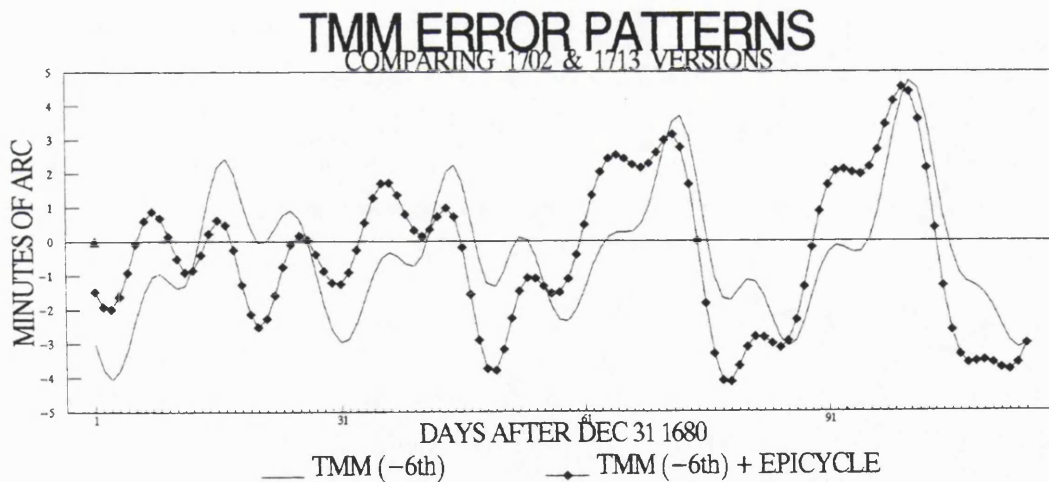
From simple trigonometry, at these dates of 1679 and 1680:

	<u>Horrox-angle STA</u>	<u>Eccy. <math>TC_2</math>, (parts in <math>10^5</math>)</u>	<u>Apse eqn., <math>ATC_2</math></u>
Perihelion:	$34^\circ$	6045	$10.7^\circ$
Aphelion:	$194^\circ$	6531	$4.7^\circ$

The functions  $f$  and  $g$  in the Lotus program were readjusted, using the above construction, to give the  $A_2$  and  $E_2$  values. The TMM-2E program took A-S as the Horrox-angle by convention, whereas we have here taken it as S-A, making its apse equations negative. Apart from this sign convention, it agreed with the above values. Thus, our function is working as Cotes specified it should.

The graph shown in Figure 12.5 compares error patterns of TMM-2 and TMM-2E, sampling daily over a four month period. It indicates that the latter was less precise. (As before, the program subtracts lunar longitude as given by an accurate modern program in arcminutes from that given by our reconstruction of a historical model). Both versions manifest a synodic error-pattern: adjusting the duration to give four repetitions of the pattern as shown, spanned 118 days of daily sampling, and dividing this period by four gives 29.5 days.

Figure 12.5: Diminished accuracy of the 1713 version (dotted line), compared with that of TMM-2, over daily sampling January-April 1681.



Error-values were sampled every 160 days, taking this period for the reason given earlier, that it was not near to multiples of the main periods of TMM. This gave, for groups of forty:

TMM-2	$-0'.4 \pm 1.9$
TMM-2E	$-0'.3 \pm 2.2$

The new epicycle was given a reverse direction such that it still aligned with  $CC_1$  at aphelion/perihelion, which gave:

$$TMM-2(-E) -0'.5 \pm 2.1$$

We thereby conclude, that the notion of a seasonal expansion and contraction of the Horroxx-wheel, Edmond Halley's second contribution to TMM, was erroneous. What we have called TMM-2 was the best version for astronomers, who had nothing to gain from the greater complexities propounded in the *Principia*.

The notion discussed by the three astronomers back in 1694 was an expansion and contraction of the Horroxx-wheel. One could merely put the radius of that wheel equal to

$$1173 - 35 \cos(S-H) \quad \text{parts in } 10^5$$

to give the required maximum value in winter, and the minimum in summer when  $S-H=0^\circ$ . Testing as before every 160 days, still gave no improvement to TMM-2.

At his house in Jermyn Street, behind St James' church in London, the seventy-year old Master of the Mint re-cast his earlier 'theory', so that it would more resemble the result of forces interacting between three bodies. Far from being a 'much expanded version of the "Theory of the Moon"' as Craig Waff claimed (1977, p.71), what appeared in 1713 was a rather abbreviated version. After toying with a nine-year periodicity based upon apse rotation, as two documents probably belonging to this period indicate, he finally decided against it, probably because his reliable data did not extend over a long enough period.

He did however introduce an epicycle, deriving from discussions of the 1690s. Having introduced four valid new equations, and linking up two of the zodiac variables to annual equations, he conferred an annual equation upon the Horroxx-wheel itself, in a manner that simply did not work. Thus, what we have earlier called TMM-2 was the optimal format for Newton's lunar theory. We now ascertain to what extent eighteenth-century astronomers, and Edmond Halley in particular, applied these modifications.

Ch . 13

HALLEY AND THE SAROS SYNCHRONY

Once he became established as the new Astronomer Royal, Edmond Halley commenced a systematic study of the error-patterns generated by TMM. Table 13.1 shows his manner of making the comparison, a page from his 18-year Saros Cycle of observations plus error-estimates, published posthumously. Greenwich mean time is specified on the left, together with lunar longitude, plus longitude as predicted by TMM for that time, and then the difference between these two in arcminutes\*. His procedure was thus rather comparable to that employed here.

It is normally averred that Halley's data from his two decades spent as Astronomer Royal was unpublished and inaccurate. It will here be argued that this data was (a) published and (b) rather accurate. Even more surprisingly, we shall conclude that Halley may have been justified in claiming that his method was accurate enough to win the longitude prize, being the most accurate method for determining longitude proposed anywhere in Europe in the first half of the eighteenth century, though largely ignored by posterity. The fate of his proposals lies outside the scope of our inquiry.

The approach here developed will indicate the benefits of a quantitative

.....  
\* The first line of data here tabulated is for June 21st 1732. On the left is GMT time for a lunar limb transit; adding the time that a lunar semidiameter takes to cross the meridian, 1.08 minutes, gives the time for lunar centre transit. The true lunar longitude then was 218°3'50" (ie, 8° Scorpio, as Halley wrote it), so Halley's observation was within 14." Our TMM-2 program gives for this time 218°2'52", thus having an error of one arcminute. This is somewhat more accurate than Halley supposed, however it is in the other direction. What Halley called 'Argument. Annum' is the Horrox angle, ie (S<sub>1</sub>-A<sub>1</sub>), which the program gives as 282°, within two minutes of Halley's value. The Sun-Moon distance, ie (M<sub>7</sub>-S<sub>1</sub>), is 118°4'.6, which is one minute more than Halley's value.

Table 13.1: a page of Halley's error estimates for longitude of lunar centre, for June-September 1732, with GMT given for lunar limb transits, from his *Tabulae Astronomicae*.

LUNÆ MERIDIANÆ LONGITUDINES GRENOVICI OBSERVATÆ CUM COMPUTO NOSTRO COLLATÆ.																				
Anno JULIANO MDCCXXXII. Currente.																				
Transitûs Limbi Lunæ T. æq.			Argument. Annuum.			Distantia ☾ à ☉			Longitudo Centri Lunæ Observata.			Longitudo Centri Lunæ Comput.			Error Comp					
M.	D.	H.	'	"	s.	o.	'	s.	o.	'	o.	'	"	o.	'	"	'	"		
Junii.	21	7	32	44	9	11	58	3	27	28	♃	8	3	36	♃	8	5	8	+1	32
	22	8	21	22	9	12	50	4	9	26	♃	20	50	35	♃	20	52	6	+1	31
	23	9	13	15	9	13	43	4	21	32	♃	3	58	20	♃	3	59	18	+0	58
	24	10	7	57	9	14	36	5	4	0	♃	17	29	31	♃	17	29	41	+0	10
	25	11	4	22	9	15	29	5	16	47	♃	1	24	13	♃	1	24	10	-0	3
Junii.	29	14	46	18	9	19	0	7	10	49	♃	0	11	19	♃	0	9	33	-1	46
Julii.	2	17	19	38	9	21	38	8	22	51	♃	14	57	41	♃	14	54	1	-3	40
	6	20	58	32	9	25	9	10	16	58	♃	12	18	24	♃	12	13	55	-4	29
	14	2	37	31	10	1	18	1	10	37	♃	13	50	26	♃	13	50	31	+0	5
::	15	3	18	32	10	2	10	1	21	53	♃	26	7	28	♃	26	8	20	+0	52
::	17	4	41	54	10	3	55	2	14	32	♃	20	32	35	♃	20	35	3	+2	28
	20	7	1	59	10	6	33	3	19	47	♃	28	4	48	♃	28	6	53	+2	5
	23	9	46	0	10	9	12	4	27	44	♃	8	49	1	♃	8	47	36	-1	25
	24	10	42	43	10	10	5	5	11	9	♃	23	20	20	♃	23	19	23	-0	57
	25	11	38	39	10	10	58	5	24	52	♃	8	17	45	♃	8	14	44	-3	1
	28	14	21	35	10	13	38	7	7	18	♃	24	27	48	♃	24	26	26	-1	22
	30	16	7	46	10	15	24	8	5	44	♃	24	52	16	♃	24	50	1	-2	15
Julii.	31	17	2	20	10	16	17	8	19	42	♃	9	37	46	♃	9	34	50	-2	56
Aug.	1	17	58	4	10	17	10	9	3	24	♃	24	2	38	♃	23	58	55	-3	43
	2	18	54	18	10	18	4	9	16	45	♃	8	6	56	♃	8	2	53	-4	3
	3	19	50	5	10	18	57	9	29	44	♃	21	52	10	♃	21	48	14	-3	56
	5	21	35	57	10	20	43	10	24	34	♃	18	31	36	♃	18	30	4	-1	32
	18	6	37	1	11	1	20	3	12	27	♃	18	41	24	♃	18	43	2	+1	38
	19	7	31	27	11	2	14	3	25	9	♃	2	10	33	♃	2	10	22	-0	11
	20	8	26	46	11	3	8	4	8	15	♃	16	9	7	♃	16	7	23	-1	44
	22	10	17	11	11	4	55	5	5	37	♃	15	41	46	♃	15	37	49	-3	57
	25	13	2	6	11	7	36	6	18	41	♃	2	43	17	♃	2	39	59	-3	18
	26	13	57	7	11	8	30	7	3	11	♃	18	25	45	♃	18	22	57	-2	48
	27	14	53	17	11	9	24	7	17	32	♃	3	50	55	♃	3	48	37	-2	18
	28	15	50	34	11	10	18	8	1	40	♃	18	52	41	♃	18	49	55	-2	46
Aug.	29	16	48	15	11	11	12	8	15	27	♃	3	27	0	♃	3	23	37	-3	23
Sept.	1	19	33	4	11	13	53	9	24	22	♃	14	32	17	♃	14	29	25	-2	52
	2	20	22	35	11	14	47	10	6	30	♃	27	31	54	♃	27	30	13	-1	41
	4	21	53	21	11	16	34	10	29	45	♃	22	50	7	♃	22	51	23	+1	16

study of historic positional astronomy computations, permitting conclusions not drawn from mere opinions handed down. It first estimates the accuracy of Halley's method in general terms, without evaluating the last column of his tables which shows his estimate of TMM's error over the years. Halley's method of applying TMM will need to be examined in more detail before attempting that. We here review his application of that cycle to which he gave the name: Saros.

What Halley began, upon becoming Astronomer Royal, comprised the first TMM-based computations of a systematic nature. Once he had finalised his procedure, and the tables which it utilised, he was as we shall see not at liberty to adjust it for the following eighteen years. The Second Edition of the *Principia* made some adjustments to the TMM protocol, beyond the mere reversal of the sixth equation, some of which Halley adopted.

The Saros cycle as was known to antiquity was given its name unintentionally by Halley, during his historical researches. He named it in 1691, by mistake (Armitage, *Edmond Halley* 1966, p.126; *Phil. Trans.* 1691, Vol.16, *Emendationes ac Notae* p.537; Gingerich, *The Saros Cycle in Babylonia*, JHA,1992, 23, p.229). When in 1682 the 26-year old Halley turned his telescope towards the Moon from his Highbury residence, it was his ambition to follow a complete 18-year Saros cycle, but turbulent events took him to London instead, involving the funding of the *Principia* from his wedding-dowry, which was probably just as well for posterity; however, once established as the Astronomer Royal in 1720 at the age of 64 he recommenced this scheme, that he had first aspired to 38 years earlier.

Halley set forth his notion of tackling the longitude problem in that same article of 1691 (P.T., Vol 16, p.536) in which he referred to the Saros. Years later, in 1716, he brought out a third, posthumous edition of Thomas Streete's *Astronomia Carolina*. After printing three years' of his sextant observations at the end of the book, he explained his view concerning the Saros cycle (without using that word), whereby it enables error-pattern in TMM-based tables to be accurately predicted. His reputation for the accurate prediction of eclipses derived, he explained,

from this cycle. He was now applying his insight into the Saros in a more general manner:

'...so that whatever Error you found in a former Period, the same is again repeated in a second, under the like Circumstances of the same Distance of the *Moon* from the *Sun* and *Apogaeon*...*Being thus assured from the Certainty of these Revolutions*, that all the intermediate Errors of our *Tables* were not uncertain Wanderings, but regular faults of the *Theories*; I next thought how I might best be inform'd of the Quantity and Places of these Defects... Nor was there any other way, but from the Heavens themselves, to derive this Correction; by a sedulous and continued series of Observations, to be collated with the *Calculus*, and the Errors noted in an *Abacus*: from whence, at all times under the like situation of the *Sun* and *Moon*, I might take out the Correction to be allow'd.'

## **I The President's Proposal**

That was the method. We next hear about it within the pages of the Journal Book of the Royal Society, in May, 1720. Halley had become the new Astronomer Royal, having taken residence in the Observatory two months earlier, and was explaining to the Royal Society the new terms of his employment, for the improvement of the art of finding longitude. Having sailed a ship across the South Atlantic as well as holding the Savilian Geometry chair at Oxford, he was indeed competent to hold an opinion on the matter. His advice as recorded made no allusion to the Saros concept! His concern was merely for the accurate positioning of zodiacal stars, whereby lunar 'appulses' thereto could be used to find longitude.

It was not Halley's view that lunar right ascension and declination could be accurately measured on board a tossing ship: his proposal was that a telescope of up to five feet in length could be used to give accurate measurement of such stellar transits, on a ship. His predecessor had left many gaps in the band of zodiac stars that were necessary for such, he complained, and he proposed to fill these in. Newton's lunar theory (ie, TMM) should be used together with such tables of stellar positions for finding the longitude. He added the fairly evident comment that lunar

quarters were optimal for observations because Full Moons were too bright for the timing of stellar appulses. That was all he said.

Sir Isaac Newton, occupying the presidential chair, was moved to comment - and proposed Halley's Saros method! This is a rather curious role reversal. Had we not got Halley's 1716 proposal of his method, it would appear from this altercation as if the whole idea came from the President:

'Upon mention made in the above Discussion that it was proposed to use the President's Theory of the Moon's motion for putting the method for the finding of longitude into practice, the President was pleased to observe that he founded his Theory chiefly upon observations of the Moon's place in the conjunctions and oppositions to the Sun, but it would be necessary for the further correction of the Theory, to collect first of all the errors of it in the quadrants, and afterwards what errors there are in the Octants, for which end he proposed it an useful work to frame an Ephemeris of the Moon's motion from the Theory for eighteen years in which period the errors return & this would be a ready means to Examine how much the Theory may Err from the Observations, made at any other time.'

(Journal Book of the Royal Society, XII, 1720-26, pp.11-12)

Halley did this. He commenced the vast labour of creating an almost daily ephemeris of lunar positions. After following half of a Saros cycle or one revolution of the lunar apse over nine years, he reported on his conclusions. In the year 1732 when aged 76 he submitted to the Royal Society's journal 'A Proposal of a Method for finding Longitude at Sea within a degree, or twenty leagues' (P.T. 1731/2, 37, pp.185-195). He concluded that his study of the Saros pattern enabled him to improve upon TMM, because its errors recurred over the Saros period. He did not suggest that this approach had derived from the Society's President in 1720.

The second Astronomer Royal completed a whole Saros cycle of observations in the year 1739. His posthumously published *Tabulae* contained a section 'Precepts for using the Tables' which gave instructions for using his complete Saros of error-computations, by which means, he explained, errors may 'in great measure' be corrected. One merely had to find a



comparable position in his 18 years, 11 days of observations (or 223 synodic months) as published before or after the date required. Preferably the exact day should be used, however one could manage with estimating the day before or after.

Halley then went on to claim that alternatively, 111 lunations could be used, as the period of one apse revolution, roughly half the Saros period, though he admitted this was not so exact. I believe that this method does not in fact work, because no such synchrony then occurs as for the Saros, which may well have undermined the credibility of his high-precision Saros proposal.

No-one could investigate Halley's proposal during his lifetime, since he never published his data! Applying his method depended on having almost daily readings such as he was amassing, but no-one else had them. He turned out to have a rather similar attitude towards the publishing of his data as his predecessor, though for a different motive. When a stern rebuke was delivered for the neglect of his public duty by Newton from the Presidential chair, at a meeting of March 2nd, 1727, warning Halley that it might be 'of ill consequence to continue in the neglect of it', ie the presenting of his observations (Baily, 1835, p.188), Halley explained by way of reply:

'he had hitherto kept his observations in his own custody, that he might have time to finish the theory he designs to build upon them, before others might take the advantage of reaping the benefit of his labours.'

Having an eye on the longitude prize, he explained, he wished to keep his data until he had perfected the method. His persistence in this attitude for the rest of his life surely goes far towards accounting for the ignoring of his 1731 proposal by posterity as seems to have happened. His observations were not published until 1749, by which time TMM had ceased to exercise a formative effect upon astronomers.

The challenge of finding longitude at sea within a degree implied a two-minute prediction of lunar longitude (Ch.1, III). Halley had by 1731

taken fifteen hundred lunar observations over a nine-year period, which is one every two days:

'And that these might be duly applied to rectify the Defects of our *Computations*, I have myself compared with the aforementioned Tables, made according to Sir *Isaac's* Principles, not only my own Observations, but also above eight hundred of Mr *Flamsteed's*....

'Comparing likewise many of the most accurate of Mr *Flamsteed*, made eighteen or thirty-six Years before (that is one or two *Periods* before mine) with those of mine which tallied with them, I had the satisfaction to find that what I had proposed in 1710 was fully verified; and that the Errors of the *Calculus* in 1690 and 1708, for example, differed insensibly from what I found in the like Situation of the Sun and apogee, in the Year 1726. The great Agreement of the *Theory* with the *Heavens* compensating for the Differences that might otherwise arise from the Incommensurability and Excentricity of the Motions of the Sun, moon and Apogee.'

Halley nowhere here names the Saros, as neither indeed did Sir Isaac Newton in his 1720 comments, only referring to it as the 'Period,' and TMM is referred to familiarly as 'the Theory.'

In 1735 Charles Leadbetter published the two volumes of his *Compleat System of Astronomy*, then in 1742 his *Uranoscopia*. Halley is mentioned respectfully as the Astronomer Royal, and the virtues of TMM are extolled, and the question as to whether anyone has as yet rightly applied it for the preparing of tables is aired, without any mention of Halley's method, and its glossary of terms gave under 'Saros' merely a method of predicting eclipses.

## II The Accuracy of Halley's Method

We shall now evaluate the degree of validity of Halley's claim, using the TMM program. The Saros is a period of 223 lunations, or 18 years and ten or eleven days, depending on how many leap-years are involved, plus an extra one-third of a day. It expresses three fundamental synchronies, by

what can only be described as a remarkable coincidence, causing the patterns of lunar motion to recur over this cycle:

synodic	223 x 29.5306 = 6585.32 days
nodal	242 x 27.2122 = 6585.35 days
anomalistic	239 x 27.5545 = 6585.52 days.

The sidereal cycle also coincides moderately well, within ten degrees or so (though that is here without relevance), the annual cycle does also as it is a mere ten days into the new year, and the apse cycle of just under nine years also coincides fairly well. Halley regarded the latter as quite important, though it has only a very minor function in TMM.

A new Moon fell on December 31, 1689, Old Style. We may conveniently start at twelve noon on that date. One Saros cycle takes us to January 11, 1708, 20 hours, and another to January 22nd, 1726, 4a.m.\* Precision in the time of day is not here required, since TMM has no diurnal component to it, however it is required in the tie-up between the Julian days on which TMM-PC runs, and the Julian date used for the longitude program. The dates were checked against the program in the usual manner, using solar longitude to ascertain them correctly.

We thereby model Halley's own investigation, since the above-quoted text cited the years 1690, 1708 and 1726. 1690 was the date when Flamsteed, with the help of Abraham Sharpe, erected the Greenwich mural arc, recently described by Allan Chapman as being for its time, 'the finest and most exact astronomical instrument constructed to-date' (Chapman, 1990, p.57), presumably why Halley chose to start from this date.

Three sets each containing a hundred TMM error-values were generated, sampling at two-day intervals, giving just over six lunar months, separated by 18-year intervals. Modern values of longitude were subtracted from TMM-PC2 longitudes at each of those 100 times, generating three columns of .....

\* The t-values for TMM-PC come out to 3287.000, 9872.333 and 16457.666 for these three dates of Saros-period intervals.

errors in arcminutes. These had average values of:

$$-0.6 \pm 1.7, -1.3 \pm 1.6, -2.0 \pm 1.6 \text{ arcminutes,}$$

These standard deviations are comparable to those given for TMM-2 in the previous chapter, while the mean values follow the increasing error in TMM's mean motion over the decades (Figure 13.1).

The graph shows these three plotted, greatly supporting Halley's approach. It shows how, over a half-year period, the errors recur exactly according to their position in the Saros cycle. The synchrony of the Saros does indeed provide a key to predicting perturbations, but was it good enough for the longitude prize? To answer that, we next subtract the three error columns one from the other. This was after all Halley's method. This gives three sets of error-differences, which came to:

Saros1 - Saros2	Saros1 - Saros3	Saros2 - Saros3
$0'.7 \pm 0.32$	$1'.4 \pm 0.64$	$0'.8 \pm 0.32.$

### TMM AND THE SAROS

errors in TMM-2 over 18year,11-day intervals

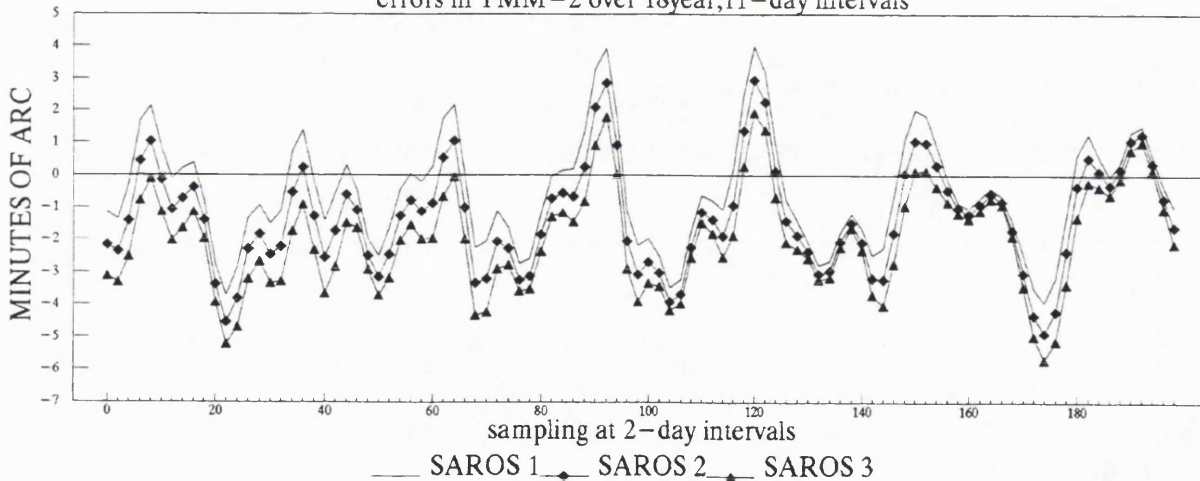


Figure 13.1: Halley's Saros Synchrony depicting three sets of TMM error-patterns, over half-year periods, in identical phases of successive Saros cycles, for the years 1690, 1708 and 1726.

The first of these figures shows a drift of 0.7 arcminutes in mean motion per eighteen years. Apart from this, our use of Halley's method, using one

error to estimate another one Saros later, has given a standard deviation of less than one arcminute.

This was by far the most accurate technique of lunar prediction proposed anywhere in Europe in the first half of the eighteenth century. It was a subtle new approach, depending upon periodic return of error-patterns. Whether it was sufficient to claim the longitude prize would depend upon the errors in two sets of observations: one in the present time, and another one Saros earlier. Regrettably, Halley undermined his own case by belittling his predecessor. 'A good part' of the merely two minutes of arc error which Halley viewed as TMM's error may have been he felt 'the Fault of the Observer.' This occurs in the same 1731 report from which we have just quoted. If Flamsteed's observations were so bad, how could readers trust his argument over Saros, which entirely depended on his predecessor's observations? Unpublished studies by Yallop and the present writer, indicate that Flamsteed's lunar-limb transit observations were within twenty arcseconds or so.

Part of the error in Halley's method comes from the drift of mean motions, losing about 41" per twenty years, or 0.00009 arcminutes per day\*. Subtracting this amount out from Halley's error patterns gives the 'corrected' graph, showing the marvellous synchrony of the Saros, within a fraction of an arcminute in its deviation from the TMM mechanism (Figure 13.2)! Subtracting these drift-corrected error columns from each other gave

$$0 \pm 0'.46, 0 \pm 0'.39 \text{ and } 0 \pm 0'.86$$

or 29,23 and 51 arcseconds as standard deviations of their differences.

How accurate were his observations? Halley's lunar meridian transit observations, published in 1749, began in January 1722 and ended in December 1739. He cited G.M.T. values on the left, together with right ascension values for limb transits, over the first five years of

.....

\* The mean motion error was found to be 41 arcseconds per twenty Julian years, equivalent to a 0.000082 arcminutes per day error (Chapter 5); however, adding that amount still left in a small systematic error over the three Saros cycles we are here investigating.

## TMM AND THE SAROS

using corrected mean motion

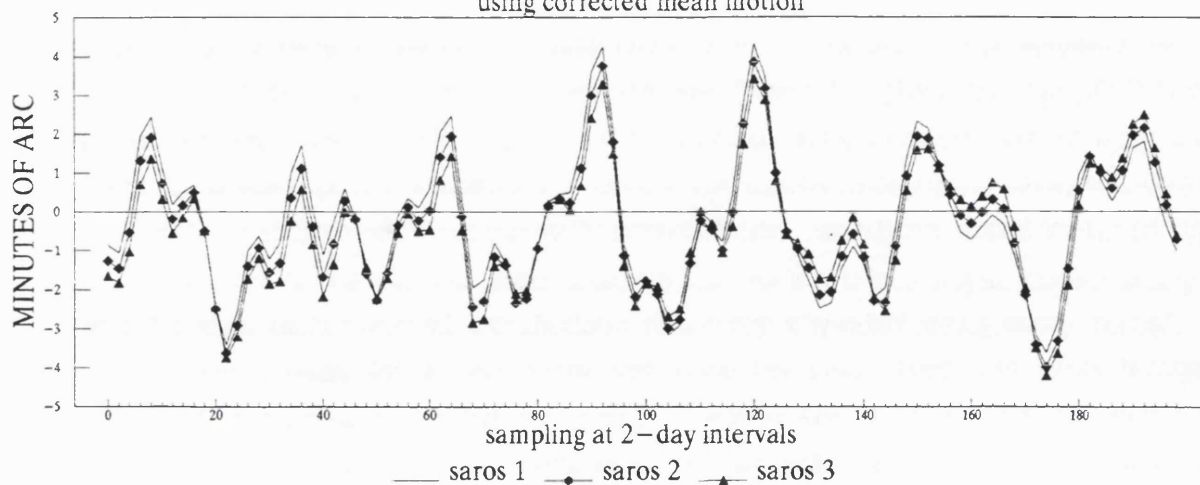


Figure 13.2: Halley's Saros Synchrony, as before but after subtracting out the error in mean motion over the three successive Saros cycles.

observation. Halley then computed right ascension values from TMM's latitude and longitude values for the given times, then took the difference, ie the error-value in right ascension.

From December 1725 he changed to a method more convenient for evaluation, giving positions of lunar centre in longitude instead of R.A. His predecessor Flamsteed's observations were all recorded merely as clock or apparent time and not as observed G.M.T., and were for limb transits as observed. Halley omitted declination values, which are not easy to take simultaneously with R.A. for a lunar transit. A copy of his notebook exists at the Royal Astronomical Society's library. His data post-1725 is in the form most convenient for comparison with TMM, which was the aim of the exercise.

The longitude accuracy of twenty of Halley's computed longitude positions for the year 1732 I found to be  $14'' \pm 20''$ . Considering that conversions from apparent to mean time and from limb to lunar centre had been applied, these are plainly the most exact observations recorded up till then within Britain. One is perplexed by the customary comments about inaccuracy and carelessness that historians bestow upon this series of over two thousand lunar transits, with times accurately given in G.M.T. for the first time ever.

As regards the accuracy of his published RA values, Bernard Yallop of the R.G.O. kindly analysed the first 35 which Halley published for January 1722, and found their errors to average  $-10'' \pm 33''$ . They were commonly cited to the nearest arcminute, which may account for their being less accurate than his longitude readings, which began five years later.

Let us summarise Halley's proposed method. It involved two sets of TMM computations, and could be used on a day which had a reliable lunar longitude observation of one Saros earlier (or later). The deviation of the old measurement from the TMM-computed longitude at that earlier time, conveniently tabulated by Halley, was added on to the TMM-computed position for the new position. The method had three sources of error: that in mean motion drift over eighteen years, that in the observations, and that between successive Saros periods in relation to TMM. Inherently, the method is accurate to about half of an arcminute, in terms of the third of these errors. This is probably more accurate than Halley himself suspected. For persons taking the view that Halley's data was more reliable than his predecessor's, 1740 would have been the first year on which his method could be tried, since Halley's observations started in 1722.

In principle, Halley's method could be used with any lunar theory. If, for example, one removed the four auxiliary equations from TMM, then one would merely obtain a larger error-pattern repeating through the Saros cycle, to be subtracted. It was Halley's opinion, however, that TMM was the best theory to use for applying his Saros-error correction procedure.

### **III The Misunderstanding of Halley's Method**

We have argued that science historians have hardly ever recognised the existence of TMM as a working mechanism. The problem becomes acute when we seek evaluations of what Halley was doing as Astronomer Royal, as the above-mentioned project then formed his principal occupation. We now quote Francis Baily, who was President of the Royal Astronomical Society in the year 1835, the year in which his *Account of Flamsteed* was published, which did so much to rescue the latter's reputation. The sarcasm of tone is unmistakable:

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

*Observations of Dr Halley*, 1835, p.189.)

I suggest that Baily had not apprehended the method that Halley was then proposing. Halley was not claiming that any lunar tables had attained such exactitude, but rather that a method of predicting the errors of those tables could reach such, based on the 18-year Saros cycle of which Baily made no mention.

To suggest that the Astronomer Royal was merely taking right ascension readings, while the 'major part' of his declination measurements remained useless because his telescope was not adjusted, implies some degree of incompetence. We merely note that, to compute longitudes as Halley published after 1726 requires both RA and declination readings.



To quote from a popular account, 'Perhaps because of his age, or because of the equipment used, Halley did not take the great care needed in making the proper adjustments to his equipment.' (B.Heckart, *Edmond Halley* 1984 p.78). In the Armitage biography, Baily's comments are alluded to:

'Baily concluded that no useful purpose could be served by publishing Halley's observations... Thus the great bulk of Halley's Greenwich observations remain unpublished.' (Armitage, 1960, p.205)

What Francis Baily said in 1834, in his Presidential Address to the RAS, was that 'The astronomical observations, which he [Halley] made in that situation, have never yet been published.' (Baily, 1835, p.169) In this he erred: rather, none were unpublished. How did such an idea develop, concerning the over two thousand meridian transit observations of Halley published in 1749, an unprecedented number of unprecedented accuracy?

To substantiate our claim, which may strain credulity, we specify the following. If the clock times as recorded in Halley's notebook, of which a copy exists at the R.A.S. library, are adjusted by applying the Equation of Time (see Howse, *Greenwich Time* 1980 p.38), they will equal the mean times as given in Halley's *Tabulae Astronomicae* of 1749. The 'Distance a vertice' readings in his notebook (from about 1725 onwards) are two or three degrees from the correct declination readings, implying an instrument correction, possibly specified somewhere in his notes (Zenith distance =  $90^\circ$  - declination). How Baily could have made so awesome an error of judgement, and why successive science historians should have followed him, is not our concern.

The source from which one would expect an authoritative account is Eric Forbes in the Greenwich tercentenary volume (1975). Forbes struck a note of scepticism over Halley's method, proposed in 1731:

'This proposal is a repetition of that published in the appendix to the second and third editions of Streete's *Astronomia Carolina*. He [Halley] claims optimistically that the differences between the predictions of the revised lunar theory published in the second edition of Newton's *Principia* (1713) and Flamsteed's lunar observations seldom exceeded  $\pm 2'$ ...'

(The proposal appeared only in the third edition of Streete's opus) The phrase, 'the revised lunar theory' as published in PNP of 1713 customarily refers to the inferring of lunar motions from gravity theory. That is the sense in which science historians understand it. The previous chapter evaluated to what extent PNP gave certain modifications to TMM, by way of adjusting its parameters, and to what extent it repeated the chain of equations.

Forbes' account gave no hint that eighteen years of observations had been published as the basis for Halley's accurate method of finding longitude:

'Seven years after Halley's death, his *Tabulae Astronomicae* was published in London by John Bevis. These tables, with precepts in both English and Latin, had been submitted to the press by their author as early as 1717 and printed off two years later -before he became Astronomer Royal. In fact, it had been as a result of this appointment that Halley decided to defer their publication so that the lunar tables could be compared with the results of his intended corrections.'

(1975, p.89)

Forbes' account implied that Halley's tables (as required for computing Halley's version of TMM) were printed in 1719, then held back for three decades to allow for their improvement using his new data acquired as Astronomer Royal. The rather important issues here raised will be treated in due course, when we come to the transmission of TMM-based lunar theories to France; beginning with Halley handing over certain documents to Delisle on a visit the latter made to London in 1724. Had it been Halley's aim to correct his tables by his long series of observations, then he would at once have noticed the drift in mean lunar motion, from his right-hand column of errors. On the page of positions reproduced above, the mean error in his TMM values is -1.4 arcminutes. He did not do so however, as we saw earlier in Chapter Five. Contrary to Forbes' claim, once Halley commenced his immense task of error-comparisons, he would not have wished

to readjust his tables any further, since that would have necessitated re-doing all the computations\*.

Thus the culmination of the Newtonian TMM endeavour, to which Britain's most eminent astronomer Edmond Halley dedicated his two decades as Astronomer Royal to the perfecting, so that he could claim to have resolved the most pressing scientific challenge of the age, the finding of longitude - passed into oblivion, remaining to this day unnoticed by historians.

#### IV Syzygy Accuracy

Newton's above-quoted belief given in 1720 implied that his theory was most accurate at syzygies and less so at the quarters. The data sent to him by Flamsteed contained no emphasis on the syzygies, but possibly he utilised more the syzygy data. It was traditional for a lunar theory to concentrate on this portion of the month when eclipses occurred. To check this on TMM-PC, fifty successive mean syzygy positions were selected, and fifty square positions, following the epoch date of 1680, and their

.....  
\* The French historians D'Alembert and Delambre both described Halley's Saros method, citing reasons as to why it could not work. D'Alembert argued that each of the 'arguments on which the inequalities depend' would have to return to the same value at the end of the period, which is plainly not the case, for example the mean anomaly is more than three degrees away while the solar anomaly is more than ten degrees. Halley's method compared residual errors from a theory separated by 223 lunations, as D'Alembert realised: 'l'erreur des Tables qu'on en tire doit se trouver la meme dans une seconde période' (1754, Vol.3, p.xv), which does not require the above assumption.

D'Alembert had a second criticism, that as the errors recorded by Halley were not generally within 24 hours of the previous Saros, the error may not be 'rigorously found.' However, D'Alembert should have been able to see from Halley's tabulations that the TMM error only changed gradually over days. Delambre found similar shortcomings in Halley's method, adding that others including LeMonnier had tried his method without success, (1827, p.282).

longitude errors were:

$$\text{syzygy} \quad -0'.21 \pm 1'.52$$

$$\text{quarters} \quad -0'.23 \pm 1'.70$$

That only spans a two-year period, but suggests that its accuracy may indeed have improved around the syzygy positions, as appeared in the error-pattern of Figure 11.3.

## V Halley's Version of TMM

We have previously assumed that Halley used TMM-2, as this concurred fairly well with the worked example given in Halley's *Tabulae Astronomicae* (Ch.10, I). Having reviewed the modifications added in 1713, we now recreate more exactly Halley's procedure.

Slight divergences from TMM-2 should not affect conclusions reached earlier, since the error-replication characteristic of the Saros synchrony will apply whatever lunar theory is utilised. We now describe what Halley did each day, over eighteen years. Not even the early accounts by Baily (1835 p.722) or Whewell (1837 II p.210) recognised this, nor have his biographers appreciated the extent to which the astronomer used Newton's procedure on a more or less daily basis during his tenure of the Greenwich Observatory.

His version of TMM is described in his *Tabulae Astronomicae*, under the section 'To find the Moon's Place for any given Time', and is given as a twelve-step procedure. The mean motions are formed from those of TMM as indicated in Ch 5: adding 1'40" to the mean apse, subtracting 1' from the node and adding 10" to the mean moon, for the 1680 epoch. The first three equations are unaltered, but then TMM's sixth equation was inserted before the equation of centre:

'For the argument of the fourth Equation add the Place of the Sun's Apogee to the Annual Argument, and subtract their sum from the Place of the Moon thrice equated. But this being Sir *Isaac Newton's* sixth Equation,...

The sine function involved thus has the 'argument'  $M_3 - [(S_1 - A_1) + H]$  or  $-(S_1 - M_3 + H - A_1)$ , the negative of TMM's sixth equation term (Ch.8, I).

The next section states:

'In the second column of *Tabula Aequationis Apogai & Excentricatum Orbis Lunae* is the second Equation of the Moon's Apogee; in the fourth, the Excentricity of her orbit; and in the seventh, the Logarithm for finding the Equation of Center, all answering to the Annual Argument.'

The modification to TMM given in 1713 is here alluded to, whereby the Horrox-wheel expands and contracts seasonally 'answering to the annual argument'; it being evident as we saw from the correspondence of 1694 that this idea came from Halley.

Newton added an extra epicycle to accomplish this, but Halley's procedure can be modelled by a sine function that will expand and contract the radius of the Horrox-wheel by the 3% specified in PNP, making it largest in winter (perihelion) and smallest in summer (aphelion). His tables have small additional tables to give the small increments according to the (S-H) argument. The previous chapter found no difference in accuracy between these different approaches, both being less accurate than the original design without them.

The Variation was his last equation, followed by the Reduction using a twice-equated node. The six steps of Halley's worked example, compared with our version, are as follows:

	<u>Halley's results</u>	<u>TMM-PC-'Halley'</u>	<u>TMM</u>
M <sub>0</sub>	1 21° 25' 40"	1 21° 25' 33"	
Eqn 1 (annual)	+2' 38"	+2' 36"	
Eqn 2	+1' 0"	+1' 2"	
Eqn 3	-0' 41"	-0' 40"	
Eqn 4 (- TMM's 6th)	-1' 41"	-1' 35"	
Eqn 5 (Eqn Centre)	-5° 3' 56"	-5° 4' 5"	-5° 2' 54"
Eqn 6 (Variation)	-36' 15"	-36' 17"	
Reduction	-4' 11"	-4' 12"	
M <sub>end</sub>	1 15° 42' 34"	1 15° 42' 21"	
Eccentricity	0.06643	0.06676	0.06643
Apse Eqn	-2° 41' 0"	-2° 45' 10"	2° 41' 2"
for December 5th 1725 O.S., 9hrs 8m 5., when t=16410.3806			

The steps of equation tie up tolerably well. However, the eccentricity and apse equation values indicate that Halley has used the simple Horrox-wheel of TMM. The month was December, when it would be maximally-expanded if varying seasonally.

Earlier, Halley gave a worked example at a New Moon, treated rather more briefly, with no eccentricity or apse equations specified, however we may compare its fifth equation, the 'Equation of Centre':

Halley's value	TMM-2 value	TMM-1713
4°58'28"	4°58'4"	5°0'26"

for July 2nd, 1684 O.S., 2h 41m.,  $t = 1279.1119$  days.

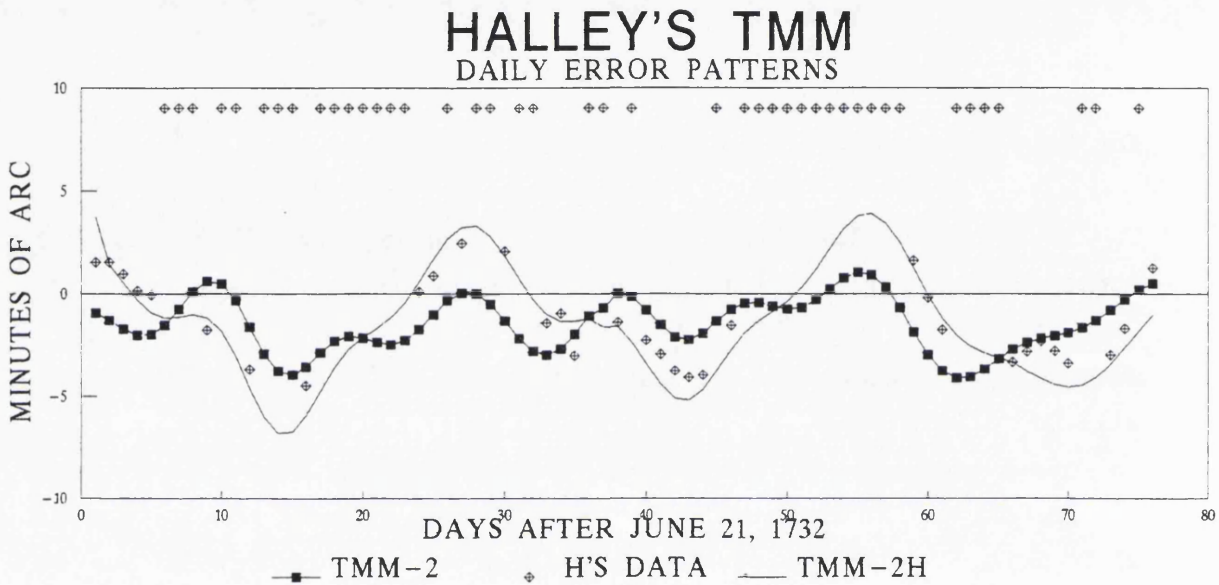
It is again evident that the simple TMM procedure has been used. These worked examples indicate that Halley made only two modifications to the TMM-2 procedure, as well as modifying its mean motions: he removed the seventh equation and made the sixth TMM equation his fourth. This version will be called, TMM-H.

In Chapter 12, a page of Halley's longitude data was given, from June 21 to Sept 4th, 1732. We now recreate the error-patterns of that period to compare with those recorded by Halley. As it was half a century after TMM's composition, we start by checking TMM-PC-H's mean motions (Appendix III) for 1700, 1720 and 1740: putting  $t$  equal to multiples of 7305 days, as the integer value for twenty Julian years, gives these three sets of mean motion within an arcsecond or so.

The error-values which Halley tabulated with such heroic patience, given in the last column of Table 13.1, were the result of a subtraction, between two computed values: (a) longitude of lunar centre, computed from meridian-transit lunar limb observations at 'time equated', ie GMT times as given, using his value of lunar parallax; subtracted from (b) the predicted value of that same parameter, as computed from his tables, using his version of TMM.

He did not do this every day, but rather performed it intermittently - 2100 observations over one Saros is just under one per three days. His method could still work using such data because, as we have seen, the TMM error-pattern has no diurnal component, it varies only gradually over days.

Figure 13.3: Accuracy of Halley's version of TMM. His own error-estimates, in arcminutes of ecliptic longitude (separate squares), are compared with the computer replicas 'TMM-2' and 'TMM-H' (Squares along top of graph represent no-observation days).



Halley's tables were arguably the first to be composed in a manner that was derived from TMM, and so the extent to which their values concur with our replica thereof is of interest. Figure 13.3 depicts a typical page of Halley's error-values, showing how they were slightly larger than would have been generated by TMM-2 (dotted line), the optimal form for Newton's lunar theory, whereas they are generally within an arcminute of the TMM-H error-curve (thin line).

If Halley's errors deviate by up to an arcminute from our theoretically-constructed error curve, we could conjecture that that was in part due to his observation errors and in part from the interpolating of his tables.

In addition, a 1713 version of TMM with the variable-radius Horrox-wheel (Ch.12, IX) was tested for errors over the same range of data as has been plotted in Figure 4a. Standard deviations of the respective error-patterns were as follows.

Halley's page of 34 error-estimates:	$\pm 2'.60$
TMM-2	$\pm 1'.30$
TMM-H	$\pm 2'.69$
TMM-1713	$\pm 3'.44$

This result establishes what had earlier been surmised, that Halley was not using a variable Horrox-wheel, with its extra equations of eccentricity and apse. Rather, he was proceeding with a six-step method using TMM's sixth equation as his fourth. In 1710 a Mr Cressner became the first person to apply TMM-2 (Ch.10, Section 9), the optimal format for Newton's lunar theory, reporting this in the Philosophical Transactions. Halley did not follow this example, but rather developed his own less accurate version.

Each of Halley's meridian readings (Table 13.1) were taken at a different time of day, whereas the error-curves plotted in Figure 13.3 were for the same time of day (arbitrarily set at 0.4 of a day after noon, or 9.36pm.) TMM's error-pattern only changes gradually from day to day.

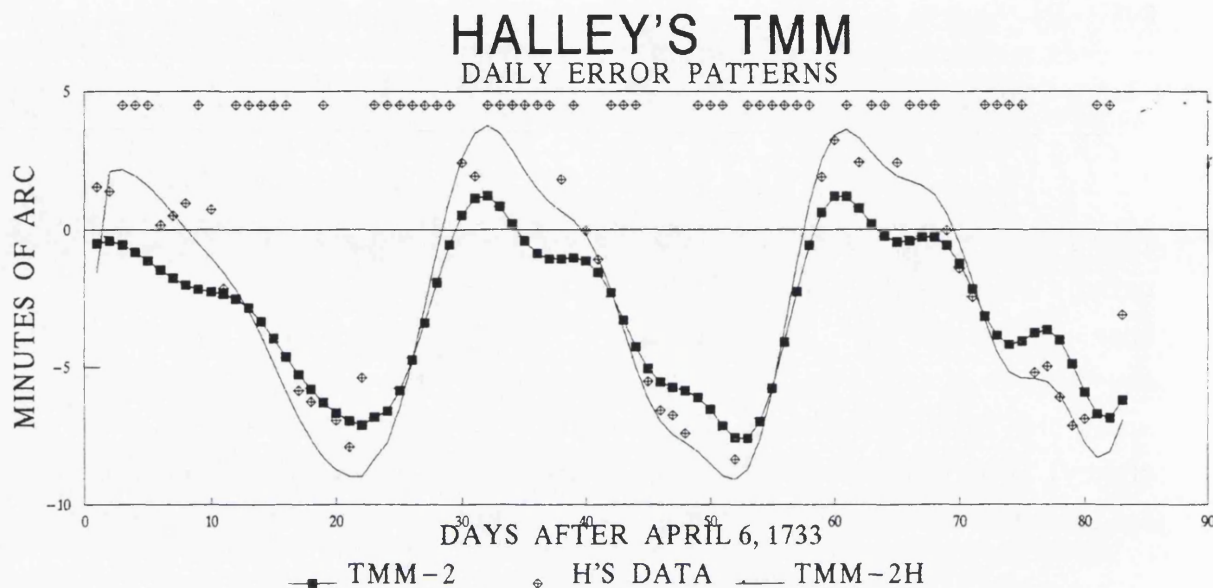
## VI A Silent Crisis

For most of the Saros period recorded by Halley, errors remain around the two to three arcminutes shown in Figure 13.3. In the spring of 1733 however, Halley's use of Newton's method started to give him errors which were twice that which he had averred before the Royal Society as TMM's maximum. Figure 13.4 plots exactly the same three parameters as before, less than a year after the previous page of data. Some combination has maximised conditions so that the 77-year old Halley was regularly, and faithfully, recording eight arcminutes of error.

In part this was due to the mean value having accrued two arcminutes of error, so that most of the error-values were negative - a fact analysed years later by D'Alembert, who inferred an error of  $-2'10''$  in Halley's value of mean lunar motion, on the basis of these error-values (D'Alembert



Figure 13.4: A Silent Crisis. The same variables plotted as in Figure 13.3, for April-June 1733, showing maximal errors arising at 30-day intervals (Squares along top of graph represent no-observation days).



1754, Preface p.xv). Halley's computations were made every two or three days on average, and the days omitted appear as points along the top of the graph. The graph plots the subtractions performed by Halley, expressed in minutes of arc: his computed lunar centre longitudes derived from observations, and those derived from his version of TMM. It is evident that his daily error-estimates concentrated upon the maximal error-periods and the descending parts of the graph, then ceased once the curve began its upward movement. His eighteen years of data tabulation shows no other period of such large errors. One could wish for some comment, but the historical record remains silent.

From these error-values may be derived a further estimate of Halley's accuracy, by subtracting from them the value of TMM-H. For the page of Halley's data represented in Figure 13.4, this gave a value of  $0'.2 \pm 0'.8$ , which may be compared with the above-found mean error in Halley's lunar longitudes as  $0'.2 \pm 0'.3$ . A small error of about half an arcminute has entered, presumably due to Halley interpolating values from his own tables.

Figure 13.4 shows maximal errors arising around April 26th and on two further occasions after 30-day intervals. They have a 30-day periodicity, coinciding temporarily with the 27.5 day recurrence of perigee. Let us perform what we may hope that Halley did, by comparing the maximal errors in their 30-day periods with those of the Saros before and after. Comparing the noon values then generated by TMM-PC-H, and also at noons nearest to one Saros before and after to these dates, in the usual manner, we obtain:

Maximal Error Values (in arcminutes) at Saros intervals, Compared:

	1715		1733	$\Delta$		1751	$\Delta$
16th April:	-5.8	27th April:	-7.1	1.3	8th May:	-8.2	1.1
+ 30 days:	-6.5	+ 30 days	-7.4	0.9	+ 30 days:	-8.5	1.1
+ 60 days:	<u>-5.9</u>	+ 60 days:	<u>-6.9</u>	<u>1.0</u>	+ 60 days:	<u>-7.5</u>	<u>0.6</u>
Mean:	-6.1		-7.1	1.1		-7.9	0.9

TMM accumulates a mean motion error of half an arcminute per Saros cycle. What we have called  $\Delta$  in the above Table, is the error that would result from using Halley's method, through comparison with the previous Saros\*. This reinforces the conclusion of Chapter Twelve, that Halley's method was in principle accurate enough for the longitude prize. His method gives an error of about one arcminute, or half that if the drift in mean motion is removed, even for the maximal error combination in the spring of 1733. (Halley's method could moreover have survived the abandoning of TMM: whatever more exact theory was used, its error-pattern would still recur through the synchrony of the Saros.)

Baily claimed that Halley's tables were based on Newtonian rules 'as corrected in the Second Edition of the Principia' (p.705). Halley occasionally used the adjusted values there given, for example his tables of eccentricity have their maximal values range from 66777 to 43323, as there specified, and not 66782 to 43319 as in TMM; on the other hand, his annual equations used the 1702 values. His method took little of significance from the 1713 version. His tables came to be widely used in France, and it is to France that we turn for further developments.

.....  
 \* Here the systematic error in the mean motion values appears as somewhat larger than it really was. Adding sinusoidal terms sometimes used for mean motions (Ch.5,I), the drift was -41" per 20 years (cf -36", Ch. 5,II).

## Ch. 14 CONSTRUCTION OF TABLES

Newton's proposal to add on half a dozen extra 'equations' was indeed innovative, not least in doubling the number of tables required. Three decades elapsed after TMM's publication before tables based upon it were published, in Britain. The historical record remains scant over the rather mysterious decades after TMM's composition, when the first tables were composed but not published. The key figures appear as Flamsteed, Nicholas Delisle in Paris, and Halley.

In 1712, in his preface to his 'Pirate' version of Flamsteed's *Historia Coelestis*, Halley wrote:

'...the fluctuations of this roving planet [the Moon] doubtless returning into orbit after the cycle of 223 synodic months. Thus the position of the Moon, discovered from the most perfect tables shortly to be published...'  
(Chapman, 1982, p.193)

What were these 'most perfect tables? Regrettably, the present study will not succeed in removing this vital question from the realm of conjecture. The previous chapter showed how the first systematic use of TMM-based tables was by Halley in his capacity as Astronomer Royal; however, all trace of the tables he used, and even the copies which others made thereof, appear to have vanished, all that remains being contained in his posthumously published work of 1749.

### I Flamsteed

A notebook of Flamsteed's in the archives of Cambridge University Library (RGO 1/50H) contains thirty-seven quarto pages of solar and lunar tables, preliminary drafts for which exist in another notebook (1/50 G). Each of TMM's seven steps of equation was there tabulated, with peak values as specified, reaching their maxima at the non-symmetrical positions required. The table for the apse equation, for example, reached its maximum of  $12^{\circ}15'00''$  at  $51^{\circ}$  of the 'annual argument' (Ch.7,III).

This is the sole surviving set of lunar tables composed in the eighteenth-century by Britain's first Astronomer Royal, dated February and

August, 1702. Some computations are given in the notebook, followed by an eighteen-step sequence of instructions for using the tables. On the following page is written 'July 1714, Burstow', Burstow being Flamsteed's parish church in Surrey. In 1702, some months after TMM's publication, Flamsteed wrote to Abraham Sharp that he had prepared some 'new tables' which were '40 quarto pages and upwards' (Dec. 14th, 1702, Baily p.210).

A letter to Sharp of January 1703 (Ch.5, IV) gave hints about the tables. His procedure:

'makes every sign [ie, a 30° interval] of mean anomaly take up a whole page...It cost me and Mr Hudson [Hodgson?] above 3 month's pains to calculate these tables...I have formed the tables for finding the variations with the small inequalities in the same manner... The tables of the second equations of the apogee and node...'

A 'second equation' of the node can only have come from TMM. The allusion to 'variations with the small inequalities' must likewise refer to the new equations. His letter to Sharp affirms that his forthcoming *Historia Coelestis Britannica* will contain his newly-constructed tables, so that his pains 'will be of great use.' This turned out not to be the case.

The letter continued with the strange claim that the tables were based upon his own procedure: '

'... to calculate the moon's place in my correct theory (I call it mine, because it consists of my solar tables and lunar numbers corrected by myself; and shall own nothing of Mr Newton's labours till he fairly owns what he has from the Observatory;) and I believe that none but myself would have been at the pains to make so many tables as I have for this purpose.'

TMM's 'solar equation' derived from Flamsteed (Ch.6, II), and he evidently believed that he had supplied other of its constants. (letter to Sharp, March 30th 1704, Baily p. 216).

Francis Baily inspected the RGO notebook, after which he wrote a letter to the then Astronomer Royal, Professor Airy. It was written in 1836, the year after the publication of Baily's *Account of the Revd. John*

*Flamsteed*, and has never been published. It contains a notable shift in his viewpoint and is worth quoting in full:

'My dear Sir,

I herewith return the Ms. of Flamsteed, Vol.50 I, which contains one of the sets of lunar tables that he composed for his own use. They are founded on the Newtonian Rules given by Dr Gregory in his *Astronomia Elementa*, pages 323-336; and are a curious and interesting historical document, inasmuch as they are the first that were computed according to Newton's theory, and afford incontrovertible internal evidence that they are the same as those which, according to Mr Hodgson's account (in his 'Theory of Jupiter's Satellites') were surreptitiously conveyed into the hands of M.LeMonnier, and published by him in his *Institutions Astronomiques*.

'D'Alembert, in his *Recherches sur differens points importants du Systeme du monde*, speaks highly of LeMonnier's lunar tables; & (notwithstanding the then existing tables of Euler, Clairaut & even Mayer) proposes them as the touchstone by which the lunar motions were to be rectified. If any merit however is due, it ought to have been given to Flamsteed. I have a paper on the anvil, in which I shall endeavour to set the public right in this respect.

'The tables in pages 51 & 52 were formed 12 years after the preceding ones, & are computed agreeably to the corrected values for the 6th & 7th equation given by Newton in his 2nd edition of the *Principia*. The second of these two tables was afterwards wholly abandoned by Newton in the third edition of the *Principia*. But nothing of these alterations is mentioned by, nor do they appear to have been known to, LeMonnier: who has rigidly followed the first set of tables, which were probably those that had been conveyed to him by some person or persons unknown to us.

Between us yours truly,

Francis Baily'

(the letter is folded into the Flamsteed notebook)

In Baily's *Account*, the tables composed in 1702/3 by Flamsteed were hardly viewed as TMM-based (pp.703-4), and it was therefore not made clear that LeMonnier's procedure was Newtonian. We shall see shortly how the tables of

LeMonnier formed the case *par excellence* of TMM's adoption by an astronomer. In consequence, Baily underestimated the extent of TMM's influence.

Table 14.1 shows how several sources have differently evaluated the astronomers who published textbooks utilising TMM. The only major source which Baily perceived was Halley, as Leadbetter abandoned TMM in his second publication while Wright confused the procedure somewhat, which explains the rather dismissive tone adopted in the *Account*.

Table 14.1: Listings of 'Newtonian' astronomers in the early eighteenth century

		<u>N.Delisle</u>	<u>F.Baily</u>	<u>W.Whewell</u>	<u>C.Waff.</u>	<u>G.H.A.</u>
		1750	1835	1837	1977	1989
N.Delisle	1717	*		*	*	
P.Horrebow	1718	*			*	
N.Grammatici	1726	*		*	*	
R.Wright	1732	*	*	*	*	*
A.Capello	1733	*		*	*	
C.Leadbetter	1735	*	*		*	*
R.Dunthorne	1739			*	*	
C.Brent	1740				*	
P.LeMonnier	1746				*	*
E.Halley	1720/1749	*	*		*	*

Baily never published the paper that he had 'on the arvil'. How, after all, could he have reconciled his wish to credit Flamsteed for LeMonnier's highly accurate tables, with Flamsteed's dismissive letters averring that TMM generated errors of up to 8-10 arcminutes? (to Sharp, 31 Oct 1713, 20 March & 31 Aug 1714; Baily, pp.309-11). The Flamsteed tables did however exert a great influence upon eighteenth-century Newtonian astronomers.

William Whiston, who was on good terms with Flamsteed, affirmed categorically in his Lucasian lectures of 1703 that no tables based upon TMM existed (Ch.1,I), published this view in 1707, and did not alter it in any successive edition. Abraham Sharp's letters of reply to Flamsteed in 1703, and later in 1716 when the construction of lunar tables was again

raised, contained no confirmation that he had seen the new tables (Baily pp.323, 328).

## II Delisle

The first claim to have composed 'Newtonian' tables was made by a Frenchman, Joseph-Nicholas Delisle\*. The claim was made in the context of the 1713 *Principia*, without reference to the 1702 publication. To quote Schaffer, Delisle was

'committed to the construction of astronomical tables in which the celestial mechanics of Newton would be compared directly with the best existing observations.' (Thrower, 1990, p.265).

Letters of Delisle's from 1717 (unpublished) remarked that Newtonian-based lunar tables could not be found, and that they would be hard to compose because a fresh start was required:

'J'en ai calculé pour le soleil & pour la lune uniquement sur les déterminations que M.Newton a tirées de la theorie de la pesanteur. Mais j'ai trouvé dans la construction de ces tables beaucoup plus de difficulté que l'on n'en a ordinairement dans la construction de pareilles tables, lorsque les fondements en sont établis; & cela parce que M.Newton reconnaissant beaucoup plus d'irrégularités dans les mouvements de la lune que les autres Astronomes n'en admettant & attribuent ces irrégularités à des causes physiques, le calcul s'en est trouvé fort long & fort embarrassé.'

(to Teinturier, 7 Feb 1717, Paris Observatory Archives)

Delisle had a prolific correspondence with European astronomers and became

.....

\* Historical reviews pertinent to TMM's reception in France are:

D'Alembert, *Recherches sur different points importants du Systeme du Monde*, Vol. 1, 1754;

M.Bailly, *Histoire de l'Astronomie Moderne* 1779 Volume II; and

M.Delambre, *Histoire de l'Astronomie au 18th Siecle* 1827;

J.Francois de Lalande, *Astronomie*, 1764, II, (p.171).

A recent essay by Dr Schaffer *Halley, Delisle and the Making of the Comet* (1990, Ed. Thrower, pp.254-298) has much of relevance.

a F.R.S. in 1724. He succeeded Phillipe de la Hire as lecturer at the College Royale in 1718. Of this period Greenberg has written: 'Delisle had already tried and failed to interest the Parisians in Newton's lunar theory as a means for resurrecting mathematical astronomy in Paris' (1984, p.152).

Delisle's tables, in manuscript at the Archives of the Paris Observatory, are entitled merely:

'Tables du Soleil & de le Lune suivant la théorie de Mr Newton dans la 2nd Edition de ses Principes'.

They have no author's name, being classified under 'De la Hire'. They contain no worked examples, making it hard to ascertain how well they worked. His *Lettres sur les Tables Astronomiques de M. Halley* were published in 1749 and 1750, the second of which exists in the library of the Paris Observatory, containing his claim to be the first to prepare such tables (Baily, p.705). It also gave the list of others as cited in the above Table.

### III Halley

The anonymous Preface to Edmund Halley's posthumous 'Tables' published in 1749 averred that they had been 'sent to the Press in the year 1717, and printed off in 1719,' a view echoed by Baily, that they were 'constructed in 1717 and printed in 1719' (1835, p.705). Contemporary accounts make these dates a little early, with the lunar tables only completed after Halley became Astronomer Royal in the spring of 1720.

In 1718, an account of lunar tables owned by Edmond Halley reached Delisle. Writing from Amsterdam after a visit to London, the Königsberg astronomy professor G.H.Rast informed Delisle that:

'...exist a [Halley] quidem perpetua motuum lunarium ephemeris (quae lunae ac solem situs post 18 annos et horas paucissimas, eosdem recurrere praecipue ostendere volvite)...(1 July)

Delisle replied by way of confirmation that Louville

'm'avoit communiqué à son retour d'Angleterre ce que vous appelez l'Ephemeride perpetuelle des mouvements de la lune...'



and added that Halley was claiming an accuracy of half an hour in past time (equivalent to 12 arcminutes, ie no improvement on earlier tables). Decades earlier, Halley had exhibited before the Royal Society such 'perpetual tables' for working his Saros method of eclipse prediction (2nd November 1692, McPike p.230), so there was nothing new in what was then reported to Delisle.

Halley was proposing to add on various smaller tables, Delisle added in the same letter, 'le tout suivant la theorie de M.Newton'. These were expected to limit the errors to no more than 4' (Delisle to Rast 16 July 1718 (I 103). Thus by mid-1718 a beginning had been made. Flamsteed reported on this enterprise in the last letter of his life, written to Sharp in November of 1719:

'Dr Halley has showed his new tables at the Temple Coffee-house: but I am told, by one that dwells in London, they are not yet finished.'

(Baily p.332) The first account of the new tables as having been completed comes in May of 1720, with Halley as the new Astronomer Royal. Crosthwaite wrote to Sharp that he was shown:

'Dr Halley's lunar tables (not yet published); but I cannot find they will give the moon's place so near the observed as Mr Flamsteed's.'

(Baily p.335.)

In 1720, Pierre des Maizeaux wrote to Conti that he was expecting to receive a copy of Halley's tables:

'Nous aurons bientôt les tables astronomiques de Mr Halley, corrigées & augmentées' (Corr. VII p.100, 11 Sept.).

Halley commenced his sequence of meridian-transit observation in January of 1722 (Ch.12,II). By the spring of 1722, Delisle was advised that the Tables were almost ready for printing:

'le livre de M.Halley sur ce sujet étoit presque achevez d'imprimer'

(N.Struyk to Delisle 4 April 1722 II34).

Delisle came to visit Halley in the summer of 1724 after which, to quote Schaffer: 'Delisle's endorsement of Halley's tables was little short of ecstatic' (1990, Ed Thrower, p.269). Contrasting his 'English' approach with that of his predecessor Phillipe de la Hire, Delisle explained:

'J'avais aussi eu soin de construire mes tables sur une théorie régulière & uniforme, tant géométrique que physique, qui étoit celle des Anglais... Prêt à publier ces tables j'ai enterpriés le voyage d'Angleterre, uniquement pour savoir ce qui en étoit des tables de M.Halley que l'on m'avait dit qu'il étoit sur le point de publier & qui devoient surpasser tout ce qui avait été fait jusqu'à present; & j'ai trouvé effectivement les tables de M.Halley déjà imprimées depuis quelques années main non pas encore publiées.'

(Delisle to P.Nicasius Grammatici, Ingolstadt Oct 1724 II 128)

It thus appears that, by 1724, Halley's tables were 'effectively' printed though not published. There was no hint that Halley had decided not to publish them, indeed Delisle's non-publication of his own tables would hardly make sense in such a context. No original manuscripts of Halley's tables remain (*Corr.* VII, Note 14 on p.101). DeLisle's 'Lettres sur les Tables astronomiques de M.Halley' of 1749 reviewed this situation.

#### IV LeMonnier

LeMonnier's *Institutions Astronomiques* of 1745 fully embodied the procedures of TMM, being in fact the publication giving Newton's sevenfold lunar theory in its most accurate form. Two modifications from the *Principia* were introduced, namely the reversal in sign of the sixth equation, first accomplished in 1710 by Mr Cressner (Ch.10,IX), and the adjustment of mean apse and Moon motions as in the 1726 Third Edition. Lemonnier used what we have called TMM-2, the optimal form of Newton's lunar theory.

Each table in LeMonnier's *Institutions* reached its maximum at the appropriate TMM value, with the equations in their proper sequence. Whereas Halley's tables alternated in using sometimes the 1702 constants, at other times those of 1713, LeMonnier consistently used the former. Thus his tables of the Variation reached their maximum at 37'25", as TMM specified, while his text discusses the fact that in 1713 Newton gave between 37'11" and 33'14" as the range of this maximal value, between winter and summer. For the seventh equation, Lemonnier used 2'10" as TMM had specified, while discussing the addition of 15" to that equation in 1713. The extra epicycle

was discussed as an option, and the mode of constructing the additional table outlined. Their author was aware of the modified values to the equations given in 1713, contrary to what Baily stated in the above-quoted letter.

Concerning the apse equation, LeMonnier discussed how Newton's value of  $12^{\circ}18'$  was greater than Flamsteed's value of  $11^{\circ}47'$ , and he used the former. His mean apse position (Ch.5, Fig 5) was one of the most accurate. LeMonnier's mean motions were more accurate than those of Halley, due to his adopting the Third Edition values for mean apse and Moon.

LeMonnier gave two worked examples, which differed by a mere two hours in time, and were immediately prior to a solar eclipse, of 1739. To convert TMM-2 to Lemonnier's procedure, 1.4 and 1.7 arcminutes are added on respectively to the mean Moon and apse positions (See Ch.5, Section V). In these two worked examples (subtracting nine minutes, twenty seconds of time from his 'temps moyen' to give GMT), his seven steps of equation appear as within arcseconds of the TMM program, except only for the Equation of Centre, which was forty arcseconds too small; this was the case for both his worked examples.

LeMonnier's title page merely averred that 'new tables' had been constructed, while its preface affirmed:

'On s'est donc appliqué uniquement à achever les Tables de la Lune de M.Flamsteed. (p.xxiv)

A later introduction to the Tables referred to the British astronomer seven times, opening as follows:

'Les Tables de la Lune que l'on donne ici sont dues principalement aux grandes découvertes que M.Newton a faites dans la Théorie de cette Planète: on avoit regardé jusqu'ici comme les meilleures celles que Flamsteed publia pour la seconde fois il y a plus de 60 ans dans le cours de Mathématique du Chevalier J. Moore; mais ces Tables étant encore fort imparfaites, l'Auteur s'appliqua depuis à les perfectionner, en y substituant la plus grande partie de celles que l'on trouve ici. Quoique ces dernières Tables de Flamsteed n'ayent pas été publiées, on ne scauroit assurer cependant si c'est uniquement parce

qu'elles n'étaient pas achevées. Dans l'état où elles se trouvaient lorsqu'elles nous ont été communiquées, on jugea d'abord qu'il n'y manquait que la Table qui sert à calculer les Latitudes...'

Lemonnier here appears as knowing more about Flamsteed's post-DOS labours than ever did the British, in the manner of one announcing a scoop who is not at liberty to disclose his source. The Flamsteed tables, it was claimed, contained all he had required except the procedure for finding latitude...

The historian Lalande endorsed this claim:

'Ces 'Institutions Astronomiques' sont un des meilleurs ouvrages qu'on ait faits en Français sur l'astronomie élémentaire. On y trouve des tables de la lune de Flamsteed... (pp.428-9)'

and Delambre wrote likewise:

'il fut le confident et le continuateur de Halley et de Bradley; par ses observations, il tient à l'école de Piccard; par ses livres, il est de l'école de Greenwich;'

adding, 'les tables de la lune sont une oeuvre posthume de Flamsteed.' (1827, p.179,182). This remark appears to be the sole basis for the GHA's claim, that Halley gave the Flamsteed tables to Lemonnier (GHA p.268).

D'Alembert described how, in mid-eighteenth-century France, the most widely used lunar tables were those of Halley and Lemonnier (1754, p.iv). He noted differences between the Halley and Lemonnier tables (III, pp.5,7, 10,13,33-35), eg that Halley had omitted the seventh equation while Lemonnier kept it.

Flamsteed's assistant James Hodgson inherited the Flamsteed archives through marriage to the astronomer's niece. He claimed, in the Foreword to a 1749 publication (concerning the positions of Jupiter's satellites, on which he had worked while at the Observatory), that:

'But, as to the lunar tables, the publication of them was delayed for very good reasons; and now to my very great surprise I find them printed in M. Le Monnier's *Institutions Astronomiques*: but how he came by them is to me at present a mystery... But now, after upwards of 20 years, when it was well known that I had the original by me, and did at

a convenient time intend to send them into the world according to Mr Flamsteed's own directions, it was base...' etc (Baily, p.704)

Yet more mysterious is the question as to why, if something resembling the set of tables published by LeMonnier were in Hodgson's hands when the *Historia Coelestis Britannica* went to print, no trace appeared therein. Hodgson made no subsequent effort to publish them.

Baily's remark that LeMonnier's tables were 'evidently copied' from the unpublished Flamsteed manuscripts is partially true, in that he had access to them. However, it is equally evident that he wanted his tables to differ sufficiently from Flamsteed's that he could not be accused of plagiarism. His six pages of Equation of Centre are substantially those of Flamsteed, and gave columns for five different eccentricity values as did Flamsteed, however the last two values have been altered, and so all his Equation of Centre values are different in these columns. The Flamsteed tables have pages of logarithms of the Earth-Moon distance, whose values were between 5 and 6.5 for the most part, as are often found in tables of this period, but these are absent from LeMonnier's opus. LeMonnier's second node equation is different, to which we now turn.

## V The Node Equation

The eighteenth century saw a diminution in the amplitude of this equation under the influence of TMM. We saw earlier how TMM defined a triangle that generated the second node equation, but did not as such specify its peak value (Ch.9, VI), and how this spawned a range of values for this function.

Table 14.2 shows how Kepler's 1627 value appears as generally more accurate than subsequent ones; also, that Dunthorne has copied from the Flamsteed tables, while LeMonnier constructed his own node table; and that, with the exception of Cassini (1740) whose opus appeared to have no node equation table, the De la Hire textbook and Leadbetter's later 1742 opus, all the eighteenth-century textbooks here consulted were seeking this node equation in accord with the TMM instructions.

Table 14.2: Amplitudes for the lunar node equation, ie the sin2(S-N) term

	<u>TMM - BASED</u>	<u>OTHERS</u>
1627	Kepler	1° 39' 46"
1653	Shakerley	1° 46' 00"
1661	Streete	1° 45' 00"
1681	Flamsteed	1° 39' 46" (+ Whiston 1707)
1703	Flamsteed MS	1° 29' 41"
1716	Delisle	1° 30' 00"
1726	Grammatici	1° 29' 58"
1732	Wright	1° 29' 45"
1735	De la Hire	1° 34' 00"
1736	Leadbetter	1° 29' 45"
1738	Capello	1° 30' 00"
1739	Dunthorne	1° 29' 41"
1742	Leadbetter	1° 45' 00"
1746	LeMonnier	1° 29' 34"
1719/49	Halley	1° 29' 45"

correct value: 1° 36' 11"

## VI Wright Claims a Longitude Prize

In 1728, *An Humble Address to the Rt Honorable Lords... relating to the Longitude* was published in London by 'R.W', claiming to contain 'Sir Isaac Newton's theory freed from some errors of the Press'. It contained no Tables, but had six worked examples showing TMM's seven-step procedure. Both its third and sixth equations had their sign reversed. The average error of these six worked examples was 8'.\*

.....

\* Selecting two worked examples from the 1728 publication:

	<u>1698 June 16th 14h 47 min</u>	<u>1714 Sept 6th, 6h 35m 34sec</u>	<u>IMT</u>
Wright's answer:	274° 17' 35"	316° 53' 10"	
TMM2-PC	273° 46' 0"	316° 50' 46"	
Correct	273° 47' 59"	316° 53' 30"	

Wright published a more extensive account in 1732, entitled *New and Correct Tables of the Lunar Motions according to Newtonian Theory* in which thirty worked examples were given, all at lunar eclipse times, a position which set equations three, five and seven to zero. The accuracy of these worked examples was ostensibly within an arcminute or two.

Baily observed that Wright was the first Briton to publish TMM-based tables, but then added:

'The whole however is an abortive production; for, only the second of Newton's equations is distinctly introduced; while the third & fourth seem to be wholly omitted, and the seventh united to the Variation. Moreover the maximum of eccentricity is quite at variance with Newton's assumptions.' (Baily p.701)

Baily was evidently misled by perusing Wright's pages of worked examples for the Moon at syzygy, where several of the new equations are omitted, having a zero value at the syzygy positions. Wright's lunar eccentricity was indeed strangely small, reaching its maximum at 0.0619 as compared with TMM's of 0.0668.

Wright badly confused the apse equation: 'And above all it is to be remembered, that in order to find the eccentricity, from the Sun's true place was subtracted, not the first aequated but the second or true place of the Moon's apogee...' (1732, p.81) Conversely, TMM's text indicates that the 'Annual Argument' ( $A_1 - S_1$ ) is represented in the TMM diagram by angle STB and not STF as it would be on Wright's interpretation (Ch.9, IV).

## VII Leadbetter

In the view of Baily, the 'more perfect adoption' of TMM into a tabular form was accomplished by Charles Leadbetter in his *Uranoscopia* of 1735 (Baily, p.702). The computation there presented has seven steps and is TMM-based (Leadbetter 1735 p.84).

What Leadbetter published in 1735 resembled Halley's then-unpublished version of TMM in several respects. It emulated this in employing:

- 1) the sixth equation as its fourth. It did not incorporate Newton's modulations of the equations by seasonal etc terms, in which respect it was a more rudimentary version.
- 2) the 1702 constants for the annual equations and for most of the other equations, except the sixth equation, which became the fourth, for which he took the 1713 value of  $2'25''$ .
- 3)  $35'10''$  for the variation without modulation.
- 4) the 1713 version of the maximal apse equation  $12^{\circ}18'$ .
- 5) the second node equation at  $1^{\circ}29'45''$ .

This suggests that Leadbetter obtained his tables primarily from Halley. On the other hand he maintained the seventh equation, which Halley omitted, and there was a distinctive feature in his presentation: his lunar Equation of Centre used the 'upper focus angle' of the ellipse, an approximate method associated with the Cambridge mathematician Seth Ward, and had a rather small maximal value of  $4^{\circ}57'40''$ .

On the question of whether Leadbetter used Halley's tables without acknowledgement, the testimony of Nicholas Delisle is relevant. His *Lettres sur les tables astronomiques de M. Halley* of 1749 recalled that, in the year 1724, Halley gave him a copy of his tables after he had promised not to show them to any astronomer, and to reserve them for his private use (Baily p.705).

In 1742 a more comprehensive two-volume textbook was published by Leadbetter, as the Second Edition of *The Compleat Astronomer*, the first edition having been in 1728 (I have not found any copy). Vol. 1 claimed that its tables were 'grounded upon Sir Isaac Newton's radices, and the Observations of Mr Flamsteed'; despite which, as Baily noted, all trace of the Newtonian theory has vanished, replaced by an older three-stage procedure.

In 1742 Leadbetter retained his 'upper focus' equation of centre method, calling it the 'evection', and what he called the 'reflection' now has the maximal value of  $37'33''$  (See Ch.9, V for the Variation having a higher value in non-Horrocksonian theories) His 'reflection' varies as  $2(L-S)$  and is clearly the same function. Delambre's *Histoire* had a section on



Leadbetter, though admitting 'Cet astronome est peu connu'. Concerning the tables in Leadbetter's lunar theory of 1742, given without specifying his source, Delambre averred that one was from Streete and another from Shakerley, 'souvent reproduite'. I could not discern these resemblances.

Each of the 1735 and 1742 Leadbetter textbooks had two worked examples, using radically different methods: the first using largely Halley's version of TMM-2 and the second his own version, of uncertain origin. Their accuracies for lunar longitude in arcminutes were as follows:

<u>Leadbetter's worked examples compared:</u>						<u>Error</u>
1735	p.88	1731	May 7	10h	6s 22° 20' 15"	-4'.2
1735	p.90	1735	Sep 16	noon	6s 08° 25' 32"	-2'.0
1742	p.384	1741	Aug 28	16h2m	5s 06° 34' 59"	+26'
1742	p.383	1740	Apr 7	noon	9s 18° 24' 04"	+16'

We can only wonder, as to how the author of the main British astronomy textbooks during Halley's tenure as Astronomer Royal came to lose faith in the procedure of Halley, and revert to a far less accurate method. Like Brent and in the same decade, he tried and then threw off the new equations. This may remind us of how novel was the notion of adding new equations.

## IX Other Tables

*The Compendious Astronomer* by Charles Brent of 1741 was similar to Wright's in giving both the third and sixth equations the wrong sign, explaining: 'that Author [ie, Newton] having, by an Oversight, made the third equation additive, where it should be ablative' (p.161). He also followed Halley and Wright in taking the sixth equation as his fourth. He not surprisingly concluded that Newton's new equations were hardly necessary, and gave several final examples omitting them. His equation of centre was also rather inaccurate, giving one or two arcminute errors. Brent's textbook was the worst of the 'Newtonians'\*

In Venice, tables were published by Angelo Capello in 1737, which followed the example of Halley and Leadbetter of omitting the seventh equation and adjusting the remaining sequence. Capello compared their

accuracy with the ephemerides of Manfredius and Ghislerius, and with tables of Nicasius and De la Hire, for lunar latitude and longitude, finding his own the most accurate.

Richard Dunthorne's 1739 opus reconstructed TMM-1, ignoring earlier works with its claim to be the first to construct such tables. Some years later he sent a letter on the subject to the Royal Society, *A Letter Concerning the Moon's Motion*, which offered two improvements to the TMM-2 procedure: from the data in Flamsteed's *Historia Coelestis*, Dunthorne ascertained more accurately the solar equation of centre at  $1^{\circ}55'40''$ , and that the mean moon required one arcminute to be added on, which was 'very nearly' that advocated in the Third Edition of the *Principia* (*Phil. Trans.* 1747, xliv pp.412-420; Delambre, 1827 p.598). Using these amended values, Dunthorne ascertained that, for 100 eclipse times, the method predicted the lunar longitudes within 2-3 arcminutes (Ch.11, III).

The treatise *Nova Theoria Lunae* (Uppsala archives) by the Danish astronomer Peter Horrebov of 1718 purported to be Newtonian. Quoting from an English translation of its preface:

'...The maximum equation of Apocentre Newton has established to be  $12^{\circ}18'$ , from which the table has been computed according to the Flamstedian method. The excentricities of the Moon have been taken over from the Horrox-Flamsteedian tables...'

(Source: Craig Waff, who at the University of Aahrenius in Denmark received an English translation from Niels Jorgensen). The six pages of tables prepared by Flamsteed in 1702/3 for the Equation of Centre involved solutions of the Kepler equation accurate to arcseconds, where both anomaly and eccentricity values varied. His achieving of such precision evidently had significance for other astronomers. Horrebov's theory was published in .....

\* Comparing Brent's two worked examples with the TMM-2 program gave:

	<u>1729 Jan 2nd noon</u>	<u>1738 Dec 12th 5.27 pm. GMT</u>
Brent obtained:	3s $2^{\circ}20' 15''$	2s $5^{\circ}24' 16''$
TMM-2-PC gives:	3s $2^{\circ}25' 59''$	2s $5^{\circ}27' 23''$
Correct value:	3s $2^{\circ}28' 20''$	2s $5^{\circ}28' 50''$

- averaging seven arcminutes of error (see Table 14.3 over).

Table 14.3: Accuracy of lunar longitude computations in textbook worked examples, 1650-1750. Author and publication date plus date and local mean time for the computation are given, then 'LONG.' as the estimated longitude in degrees, minutes and seconds is followed by the 'error' column, the divergence of these values from the 'correct' modern value, with TMM-based values displaced to the right.

AUTHOR	Publican.	DATE	L.M.T	LONG. E	ERROR (MIN)
Lansberg	1632	1601 11 29	12.15	081 27 45	-17
Lansberg	1632	1602 09 26	16.59	087 24 32	-13
Wing	1651	1587 08 17	18 33	086 22 47	-8
Shakerley	1653	1651 05 13	23 10	119 24 52	12
J.Newton	1657	1587 08 17	18.19	090 57 39	14
Pagan	1658	1638 09 04	11.47	127 37 17	31
Streete	1661	1586 09 22	14.24	067 24 24	-10
Streete	1661	1594 12 19	15.03	133 49 36	2
Wing	1669	1587 01 15	14 23	145 34 35	1
Flam/Horrox	1673	1672 02 23	11.35	055 37 13	13
Flamsteed	1681	1680 12 22	06.35	065 09 52	11
Greenwood	1689	1594 12 19	15.03	133 48 08	-0.4
Greenwood	1689	1586 09 22	14.24	067 26 24	-11
P.de la Hire	1727	1704 05 15	18 37	187 40 03	-6
Wright	1728	1692 03 18	20.55	139 41 26	+4
Wright	1728	1714 09 10	22.11	332 52 05	-10
Wright	1732	1690 06 11	12.13	273 24 15	+0
Wright	1732	1698 06 16	14.49	316 53 10	-2
Leadbetter	1735	1731 05 07	10.00	202 20 15	-4
Leadbetter	1735	1734 09 16	12.00	188 25 32	-2
M.de la hire	1735	1704 05 15	18 45	187 43 38	-3
Capello	1737	1719 10 30	10 42	065 52 53	+0
Capello	1737	1719 11 26	16 50	065 49 05	-5
Dunthorne	1739	1737 01 02	03.40	074 08 13	-7
Brent	1741	1738 12 12	05.27	065 24 16	-5
Brent	1741	1729 02 02	12.00	092 20 15	-8
Cassini	1740	1709 11 23	12.00	145 40 01	-6
Cassini	1740	1710 02 28	00.31	339 34 31	-2
Leadbetter	1742	1740 04 07	12.00	288 24 04	16
Leadbetter	1742	1741 08 28	16.02	156 34 59	-26
Le Monnier	1746	1739 08 04	03.41	131 31 44	-1
Le Monnier	1746	1739 08 04	05.55	132 38 31	+1
Halley	1749	1684 07 02	02.41	110 52 29	-2
Halley	1749	1681 08 18	15.19	336 13 32	+1

*Bibliotheca Novissima* in Magdebourg (according to Delisle's 'letter' of 1750), however a substantial section on Horrebov in Delambre's *Histoire* (1827) made no mention of it, not did it appear in Horrebov's posthumous three-volume *Operum* (1741) so it can hardly have been widely known.

Table 14.3 compares the lunar longitude accuracies from worked examples given in textbooks 1650-1750, against a modern program, citing up to two per textbook. The date and time given as local mean time have been cited, together with the longitude as given.\* Dividing these computations into the three groups of pre-1700, post 1700 non-Newtonian, and TMM-based, their mean errors appear as:

- 1) pre-1700                                     $1'.9 \pm 13'$      for 13 cases
- 2) post-1700 non-Newtonian             $-4'.5 \pm 12'$      for 6 cases
- 3) TMM-based                                    $-2'.7 \pm 4'$       for 15 cases

More of the non-Newtonian textbooks of Europe would improve the second group, which remains rather small. Nonetheless, this table provides evidence for a striking improvement in accuracy, even if not to the extent to which, as we have seen, the TMM procedure was capable of delivering. It supports Gautier's view that the Newtonian tables of this period

'...surpassèrent toutes les précédents en exactitude' (1817, p.13).

.....

\* Several adjustments are required to convert historical LMTs into the Ephemeris Time required for this test: a calendar change between New Style and Old for most European sources; twelve hours added on, as their LMTs started from noon; for European sources, longitude-based conversion from their LMT to GMT; and a  $\Delta T$  correction prior to around 1680 (Ch.5,I), after which date it remains around merely ten seconds and so can be ignored. The  $\Delta T$  adjustment as given in the *Explanatory Supplement* begins for the year 1620, and alters rapidly in the first half of the seventeenth century, from 124 seconds for 1620 (R.Stephenson and L.Morrisson, 1984). These  $\Delta T$  values have been re-evaluated by Dr R. Stevenson in a paper submitted to the *Phil. Trans.*, giving values of only about half this magnitude. He kindly provided  $\Delta T$  values from 1580 (90 seconds) to 1650 (50 seconds), enabling the early worked examples of Table 3 to be computed, though uncertainties in the value of  $\Delta T$  remain large over this period.

## CONCLUSION

Concerning the lunar observations supplied by Flamsteed to Newton in 1694, ~~S~~r William Whewell wrote:

'And during this interval [ie, after publication of the *Principia* in 1687], the result of the struggle depended upon the accordance of the theory with the best observations, which the Greenwich ones undoubtedly were. Upon these observations, then, depended a greater stake in the fortune of science than was ever before at hazard...' (1836, p.5).

A year later, he returned to the theme, this time in the context of Flamsteed's supposed reluctance to part with his observations, his comment upon what was achieved being:

'The reformation of the tables [by Newton] turned out more difficult than had been foreseen, and did not lead to any very great improvement till a later period.' (1837, p.180)

Whewell acknowledged TMM as the outcome of this endeavour (p.209), ie he appreciated that it worked to generate longitude positions from a given time. This apprehension hardly reappears until a century and a half later, in the 1989 account by Curtis Wilson (GHA pp. 266-268).

We have seen how TMM was a self-contained mechanism or set of procedures, devoid of theory. It cannot be defined *primarily* in terms of 'the reformation of tables', after all it was first used in 1710 by the Rev. Cressner to generate lunar positions, before any such tables existed. Likewise, the present work has expressed its instructions as a flow diagram written onto a computer spreadsheet.

One would be unlikely to find a history of astronomy or Newton biography which mentioned TMM's sevenfold structure - even though, as Dr Waff has observed, its tabular format became widely emulated in astronomy textbooks of the first half of the eighteenth century. Its seven-stage sequence was undoubtedly its distinctive feature. Starting with mean lunar motion, one 'equated' it through seven stages to reach an estimate of lunar centre in geocentric celestial longitude.

This Conclusion will survey briefly the evolution of lunar theory, as well as TMM's paradoxical role in Newton's theorising about gravitation. There was never much to indicate that the practical problem of finding longitude was as such of interest to him, his primary motive remaining theoretical. Our study began with Gingerich's finding of little improvement in ephemerides accuracy through the Newtonian era (Ch.1,I), which accords with the above-quoted view of Whewell, while the last chapter concluded that TMM-based worked examples in textbooks were indeed substantially more accurate than their rivals. We comment finally on the arrival of 'Newtonianism' into Britain in the form of Tobias Mayer's tables, which ended the career of the Horrocksian model.

This thesis has given definite answers to issues that have remained conjectural over centuries, by means of computer-aided reconstruction of the past. Home computers have of late become powerful enough to contain the very accurate equations required for reconstructing the observations of past astronomers, giving a new basis for evaluating their achievement. It is a fairly recent thing that this can be done reliably, eg the mean motion equations that Meeus published in 1991 were significantly more accurate than those in his earlier publications, to an extent that may have been critical for our investigation.

My ability to decode TMM's instructions came about from a comparison of its diagram with Crabtree's diagram of the Horrocksian mechanism of 1642 (Figs. 7.1 and 7.2). For about two years I wondered, what was the framework of space and time within which they revolved? I realised that these diagrams pertained to motion within a space defined by the immobility of the mean lunar apse. We could say that the Crabtree diagram thereby linked the numbers thirteen and eight: its eight stages unfolded over a period of thirteen months, depicting quarters and octants of solar apparent motion with regard to the lunar apse, during which period the epicycle in TMM's diagram revolved twice. This generated apse and eccentricity equations out of phase, such that when one was at its maximum/minimum the other reached its mean position, and vice versa. Once this had been grasped, then the way was open to writing all the TMM 'equations' as simple trigonometric

functions. Finally, the exact sequence of operations, here called the steps of equation, had to be interpreted from TMM.

Our computer model turned out to be considerably more accurate than the textbook worked examples of the period utilising TMM-based tables (Ch.14, IX). This was due in part to the computer model lacking interpolation errors from use of tables, primarily from the Equation of Centre: the average standard deviation of TMM's errors was nearly two arcminutes in longitude, using its optimal format, whereas a collection of worked examples from textbooks using TMM gave errors whose standard deviation was four arcminutes.

With that qualification, Table 15.1 traces the evolution of lunar theories from Ptolemy to Mayer, comparing their accuracy (standard deviation in arcminutes) against the number of 'equations' in each theory. The initial five error-estimate values were derived from computer reconstruction of the models, while sources for the last three are described below.

<u>Astronomer</u>	<u>Date</u>	<u>No. of 'Equations'</u>	<u>Accuracy (arcmin.)</u>
Ptolemy	145	2	±40
Flamsteed	1681	3	±6.5
Newton	1702	7	±3.8
Halley	c.1722	6	±2.2
Lemonnier (TMM-2)	1745	7	±1.9
Euler	1753		±1.7
Mayer	1754	13	±0.5
Mayer	1763		±0.3

Table 15.1: Lunar Theories from Ptolemy to Mayer, AD 145 - 1763, showing number of equations versus accuracy.

Gingerich reconstructed Ptolemy's lunar theory (1993, pp.60-62), showing not surprisingly that its main source of error was the then-undiscovered Variation, fluctuating by ±40 arcminutes. Two lunar equations were known in the time of Ptolemy, as remained the case in the time of Copernicus, these being the evection and equation of centre. Two further terms were

discovered by Tycho Brahe and Kepler, namely the annual equation and Variation (GHA p.194, Stephenson 1987 pp.176-7), as well as the node equation. The first of these did not receive its correct physical interpretation until Flamsteed (Delambre p.98). Horrocks combined the evection and equation of centre into a single mechanism, giving his theory just three stages. Flamsteed came down from the North of England with it, proclaiming it as the best lunar theory in existence.

The first edition of PNPM contained gravitational arguments over the known inequalities, viz. the annual equation and Variation, as well as the Horrocksian rocking apse mechanism, but it had no practical value for astronomers. Its celebrated 'Moon-test' used uniform circular motion around an immobile force-centre to demonstrate the inverse-square law of gravity. This utilised the sidereal period of the Moon (27.3217 days), its monthly rotation against the stars.

In the autumn of 1694, Newton perused Flamsteed's list of errors in lunar latitude and longitude as derived from the latter's *De Sphaera* of 1681, which had been ascertained by comparison with the astronomer's own observations of right ascension and declination. The tabulated errors of up to eight arcminutes were almost an order of magnitude greater than those inherent in the observations, wherein Newton discerned the possibility of a new endeavour.

In the months following this visit he made at least one definite improvement, by adopting Halley's modification of the Horrocksian procedure. In January 1696, Flamsteed was informed (by Halley, presumably) that Newton had discerned six new equations (*Corr.* IV p.192). Our inquiry did not find that these were then articulated into a synthesis: Whiteside has well characterised this period as a transition 'From high hope to disenchantment'. An unbridgeable gulf seemed to loom between the dynamics of an emerging gravity theory and the kinematic series of circular motions required by a lunar theory that would work. The latter was based on the tropical lunar month (27.3216 days) as was used in practice by astronomers.



Apparently abandoning the task, Newton departed from Cambridge to become Warden of the Mint. Later in the 1690s, expression of pessimism were reported, with Flamsteed prohibited from publicly announcing that he had devoted a year to preparing and rectifying his lunar observations for use by the mathematician (Ch1,IV). Instead, David Gregory was informed that Newton could not complete his lunar theory on account of the unwillingness of the astronomer to part with his data ( Memoranda by David Gregory, July 1698, Corr. IV p.277). Some years later and in Germany, Leibniz heard that 'Flamsteed withheld his observations of the moon from Newton. On that account they say he has as yet been unable to complete his work on the lunar motion' (to Roemer, 1706, Westfall p.546), a story which endures to this day.

TMM appeared as a distinctively new synthesis, with six new equations added, four to the Moon plus one each to the node and apse, with a neo-Horrocksonian formulation forming the centrepiece of its seven steps of 'equation'. Its sole and rather conjectural allusion to gravity theory lay in two of its equations varying inversely as the cube of the Earth-Sun distance. In 1975 Cohen asserted of TMM that 'the rules contained therein had been derived in a new manner from a physical theory' (1975 p.56), echoing Gregory's claim in his preface to TMM of 1702 and many others since. A year later, Whiteside argued to the contrary, pointing out that TMM had introduced a Ptolemaic-type equant, as had been banished from the heavens by Kepler a century earlier and resuscitated by Horrocks in the 1630s. It contained this because the mathematics of circular motion was the only means then available for describing the required inequalities.

A more recent assessment coming from Dr Craig Waff may be cited. From preliminary manuscripts for TMM remaining in the 'Portsmouth collection' at Cambridge, Dr Waff discerned:

'a total absence of any evidence of theoretical deduction of these new equations. Most of the folios in this collection deal with the construction of lunar tables, the calculation of eclipses and related subjects; but there are no references to or uses of the theory of gravitation found among them' (1977, p.70).

We can almost concur with such a view, except that, in these manuscripts,

the two new annual equations to node and apse are presented as so deduced\*. In general, however, Dr Waff's view concurs with the conclusion here reached, that Newton did not evidently deduce his new equations from such principles. And yet, we found that these equations were all valid and not far from their optimum amplitudes.

The separate components of TMM entered into the *Principia* of 1713, but in a manner that required familiarity with the original version to discern their coherence, at least for a modern reader. Not all were in sequence, and the context was now dynamical, no longer kinematic, with each component analysed in terms of gravity theory. This did not prevent a second epicycle from being added to the Equation of Center, a fact noted ironically by French historians†. It was chiefly in this form, as given in the Second

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\* In 1976, Whiteside observed that these manuscripts (ULC, Add.3966, section 15) were assembled 'mostly in wholesale confusion.' This largely remains the case, and is the reason why they have not been more referred to in the present treatise. Some evidently pertain to the 1694/5 period, while the latest are pages of Halley's tables transcribed, starting in June 1722. Of these which can be dated, I suggest that those prior to 1700 hardly indicate use of the four new lunar equations. *The Mathematical Papers of Isaac Newton* edited by Whiteside, Volumes VII and VIII, surprisingly contain nothing on this subject, except for a few brief allusions in an Introduction (VII, pp.xxiv-xxviii).

I found that TMM-oriented computations in these papers tended to be written in English, the language in which it was first composed (Ch.9,I), whereas those concerning the *Principia* were all in Latin.

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† 'Ce qu'il y a de plus remarquable dans ce traité ... adopté/ aujourd'hui de presque tous les Astronomes, et sur-tout par Newton, c'est que M. Machin y fait revivre les Epicycles, pour expliquer tous les mouvements et toutes les irrégularites lunaires' ('De l'Orbite de la Lune dans le système Newtonien', *Histoire de l'Académie Royale des Sciences*, Paris 1746, p.128). John Machin composed an exposition on lunar theory for the 1729 English translation of PNPM.

Edition of the *Principia*, that Horrocks's model spread across Europe, from Venice to Uppsala, a century after its birth, and flourished briefly.

The Second Edition's uneasy alliance between theory and practice in the lunar equations fractured in the Third Edition of 1725, with the deletion of a final paragraph in the Scholium to Proposition 37. As well as a derivation of the seventh equation (amongst the last to be added prior to 1713), the paragraph contained the affirmation that lunar longitude was obtainable from these several inequalities as the aim of the exercise. In the Third Edition, the only one to appear in English, this Scholium merely discussed how certain equations of motion were derivable from an inverse-square law.

## I French Comment

Pierre LeMonnier treated PNP's explanation of the Moon's annual equation as his prime case-study of gravity theory, and held forth for three pages on the matter:

'...l'orbite de la Lune se dilate, pour ainsi dire, plus ou moins par l'action du Soleil, selon que la Terre & la Lune se trouvent à une plus grande ou à une plus petite distance de cet Astre.' (1746, p.142)

D'Alembert rather doubted whether this derivation of the the annual equation was really sound:

'...il en est quelques-unes que M.Newton dit avoir calculées par la Théorie de la gravitation, mais sans nous apprendre le chemin qu'il a pris pour y parvenir. Telles sont celles de 11' 49' qui dépend de l'équation du centre du soleil...' (1754,I, p.xiv)

He was sure however that Newton had derived the Variation from gravity theory 'avec beaucoup de clarté et précision' (1754, Vol.1, p.xiii).

D'Alembert's also commented on the accuracy of Newton's lunar theory. Astronomers had assumed TMM's error was within two minutes of arc, he recalled, only later discovering that it could rise to five:

'...ce n'a été qu'après plusieurs années qu'on s'est aperçû que l'erreur montoit quelquefois à 5 minutes...' (1754, Preface, viii).

This was an underestimate as TMM had a twice-yearly periodic error of six or seven minutes of arc (Ch.11, Fig 3a), plus a mean motion error of one or two arcminutes over its period of use.

The root of the problem was a lack of credibility in accounting for the Horroxian rocking apse line and oscillating eccentricity by a gravity theory. Concerning the claim that the Horrocksian mechanism had been derived from gravity theory (Ch. 12, V), the French historian Bailly remarked:

'il [Newton] l'a laissé subsister comme une vraisemblance que peut faire attendre la vérité et tenir sa place' (1779, p.509).

He dismissed the Horrocksian theory tersely: 'ce n'est point une cause physique'.

## II The Silent Decades

Several decades elapsed prior to astronomers adopting the TMM procedure. Those in early eighteenth-century Europe who declined to do so retained a three- or four- stage method, the former based on that of Streete or Flamsteed and the latter exemplified by the French family traditions of the De la Hires and Cassinis. Notable in Britain was Leadbetter, who first advocated the Newtonian procedure with modifications due to Halley, then in 1742 renounced it in favour of a less accurate Flamsteedian procedure.

Edmond Halley was the first to use TMM systematically, completing his tables in the early 1620s, shortly after he became the new Astronomer Royal. Halley made one or two adjustments, simplifying TMM and adjusting the ordering of its equations, on the basis of having considered the matter for two decades, which as we saw rendered his procedure *less* accurate than the (corrected) 1702 version.

Around 1724, both Nicholas DeLisle in Paris and Halley at Greenwich had TMM-based tables ready for publication but declined to do so, for reasons not entirely evident. The view as given by eg Forbes, that Halley withheld publication of his tables on account of his wish to improve them

from his two decades of observations (Ch.13, III), was rejected, as his final tables published posthumously were unaltered from those he started off with in 1722.

A copy remains of Delisle's tables in the Cassini Observatory Archives in Paris, lacking a statement of authorship. No copy remains of the 'Flamsteed' tables copied by LeMonnier for his *Institutions Astronomiques* of 1749, nor have copies been located of the tables Halley used in his two decades as Astronomer Royal, even though they were eagerly copied by European astronomers. The non-publication of Nicholas DeLisle's extensive correspondence is here regrettable. Despite such gaps in the historical record, Dr Waff's thesis has been largely confirmed, that:

'...nearly all *new* lunar tables constructed during the first half of the eighteenth century utilised in some fashion his tabular theory [ie, TMM]' (Cohen 1975 p.79).

We have not improved upon his list of ten European astronomers who prepared such tables (Ch.14, I).

### III The Saros

Halley appears as something like the discoveror of the 18-year Saros cycle\*. His skill in eclipse prediction was based upon it, as DeLisle observed in his 1750 letter on the subject, which expertise was transferred to the problem of longitude. A section entitled 'Saros' in Leadbetter's textbook of 1742 is presumably its first mention in an astronomy textbook where its duration was clearly specified. Leadbetter there inserted an apology to the effect that, earlier, he had

'...called [it] Mr Whiston's Period; but Dr Halley assured me, that that gentleman had it from himself & desired me to let the world know so much' (*A Compleat System of Astronomy*, Vol. 1.),

indicating its novelty to astronomers of the period. Later, Nicholas Delisle, who was in wide correspondence with other astronomers, affirmed that:

'M.Halley avoit établi cette période de 223 lunations [the Saros] ou révolutions synodiques de la Lune, que s'achevoient, suivant lui, en 18 ans, 10 ou 11 jours, 7 heures, 43 minutes, 45 seconds' (letter of 1750).

adding that Halley had in 1714 'chosen' the Saros that began in 1700 to compute solar and lunar eclipses. Shortly after, D'Alembert referred to 'Le période de M.Halley' as comprising 223 lunations (1754, Vol.3, p.xv). While he was thus credited with discovering this period, posterity was less enthusiastic about his application of it.

Astronomers on both sides of the Channel rejected Halley's use of the Saros in an error-correction procedure. The French doubted that errors in a theory would recur in such a manner: Delambre damned the method as 'useless', and worse, 'ce n'était pas selon le science' (D'Alembert 1754, p.xv; Delambre 1827, p.282). In Britain it was widely misunderstood (Ch. 13, III). Our basis for disagreement with such experts lay in the computer's ability to reconstruct the synchrony which Halley sought, which confirmed that his method would in fact have worked, just as he claimed.

Halley's evident failure of communication on this matter suggests more originality on his part than is normally assumed. In Halley's time it was fashionable to attribute notions to the ancients by way of conferring respectability, and his attribution of this period to antiquity seems to have been unduly successful\*. One of the first astronomers to be overtaken by the pace of progress, his painstaking observations and his Saros technique were obsolete when published posthumously in 1749, replaced by *Newtonianism* arriving from the Continent.

.....  
\* Neugebauer argued against the view that the Chaldeans used the Saros cycle for eclipse prediction, or that they assigned any definite meaning to the terms 'Saros', describing these as 'generally accepted historical myth.' However, he accepted that they used a 'crude 18-year cycle' for predicting lunar eclipses. He noted that 'There exists no cycle for solar eclipses visible at a given place' (1957, pp.141, 142). Clearly, however, the cycle was given in Ptolemy's *Almagest* as known to the Chaldeans and as interlinking the synodic, anomalistic and draconic months (Neugebauer, *History of Ancient Mathematical Astronomy*, Vol. 1, 1975, p.310). For a more recent discussion see North, 1994, pp.35-47.

#### IV End of the Horrocksian Era

What came to be called the 'Newtonian' approach rejected Newton's actual procedure. The features which came to be ascribed to him in popular accounts, such as use of second-order time-differentials, and for that matter trigonometric functions of temporal variables, entered Britain in the shape of Tobias Mayer's theory of lunar motion. This was a theory in the modern sense, as having been derived from principles of gravitation. It had no oscillating apse line or varying eccentricity, the core of Horrocks's theory.

Mayer was posthumously awarded the longitude prize in 1760 for having accomplished what in 1683 Flamsteed viewed as unattainable. His work was grounded upon that of Leonhardt Euler, whose *Theoriae Motuum Lunae* was published in St Petersburg in 1753. The latter's theory had maximal errors of  $\pm 5'$  (Forbes, 1980, p.142), and as such was hardly any improvement upon that of Newton half a century earlier. One year later, Mayer's *Novae Tabulae Solis et Lunae* were published in the *Gottingen Commentarii* (Forbes, p.142), with Britain's Astronomer Royal Bradley expressing the view that they did not err by more than one and a half arcminutes (letter to the Lords Commissioners of the Admiralty of April 14, 1760, Mayer, 1770, p.cxi).

Britons read about Mayer's theory in the August 1754 edition of the *Gentleman's Magazine*. It was there explained how the remarkable accuracy had been obtained by use of eclipse positions separated by Saros intervals, or multiples thereof, which facilitated the checking of his mean motions. To quote Forbes, Mayer used 'the method proposed by Edmond Halley for the calculation of lunar and solar eclipses' (1980, p.136). Where Halley used the Saros to add an irregular error-correction to TMM, Mayer used it for discerning the fine adjustment to his equations (Forbes, 1980, p.136).

Mayer followed Euler in halting the oscillation of the apse and lunar eccentricity: Euler had 'first substituted a constant equation of the centre, along with the evection, instead of a variable eccentricity.' For a century, centred on the Newtonian endeavour, the lunar apse oscillated, since which it has retained but a single forward motion. The oscillating

ellipse in the Horroxian theory did however suggest to Euler his method for analysing the variation of orbital elements (GHA, p.201).

Lastly, the author of this article (J.B., identified as John Bevis) explained that 'the motion of the Moon's longitude is to be corrected by 13 equations.' It had thirteen stages requiring tables, although, using 'equation' in its modern sense, Mayer's theory contained a far larger number, a sequence of 122 such being listed in his *Theoriae Lunae* published posthumously in 1767 (pp.23-28).

Mayer died in 1762, leaving some improvements to his theory, tables utilising which were prepared in 1763. The *Nautical Almanac* of 1767 utilised Mayer's final version of his theory, and had a standard error of  $\pm 17''$  in its lunar noon longitudes (Ch.1,III). If we conjecture that what historical persons meant by maximum error of a theory, was thrice the standard deviation of its error, then Mayer's first theory had a standard error of  $\pm 30''$ , indicating the remarkable improvement he achieved between first and final formulations of his theory.

Eight decades after Newton and Gregory visited Flamsteed in the Autumn of 1694 at Greenwich, there inspecting tables of discrepancies between theoretical and observed values of lunar longitude, stimulating the mathematician to begin that endeavour which we have here examined, the great problem was resolved. The three-body problem was solved in a manner far beyond the scope of the present treatise. Ultimately it was a story of success, of successful endeavour: in the course of which, the stars received their numbers from the Observatory's first occupant, time became measured from the setting of his clock, and longitude divisions of the globe were marked from his workplace.



## APPENDICES

### I Some Astronomical Constants, compared with TMM

Some values such as solar eccentricity have a secular component, and the *Explanatory Supplement* of the Astronomical Ephemeris has been used to locate these, with advice from Bernard Yallop of the Nautical Almanack Office of the RGO.

	<u>NEWTONIAN</u>	<u>MODERN (for 1690)</u>	<u>DIFFERENCE</u>
LUNAR			
Eccentricity, mean	0.055050	0.5490	+0.2%
max	0.066782		
min	0.043319		
Inclination, mean	5°08'27"	5°08'43"	-16"
max	5°17'20"	5°20'06"	
min	4°59'35"	4°57'22"	
SOLAR			
Eccentricity	0.016917	0.016834	+0.5%
Apse motion	1°45'/century	1°43'/century	
Tropical year	365d 5h 48m 57s	365d 5h 48m 47s	+10s
Sidereal year	365d 6h 9m 14s	365d 6h 9m 9s	+5sec.

### II The Equations of Mean Motion

Table 16.1 shows mean epoch positions computed for several centuries, using formulae of: Meeus 1992 (Belgium), Chapront-Touze (France), the US-UK 1992 *Explanatory Supplement*, and occasionally the 1961 UK *Explanatory Supplement* (of the Astronomical Ephemeris). This Table shows a Lotus spreadsheet, which does not give formulae as such, but merely the numbers generated at each step by the formulae. The Chapront-Touze computation is in arcseconds, while those of the Astronomical Ephemeris and Meeus are in degrees. Mainly, Meeus has just converted Chapront-Touze's equations from arcseconds to degrees.

For lunar longitudes, the right-hand column (c-b) shows the Meeus and Chapront-Touze equations giving comparable positions, for integer Julian century values AD 1600 to 2000. A fraction of arcsecond difference results from a speed of light correction applied by Meeus. The adjacent column, (a-b) shows differences between the US-UK values and the Continental ones,

Table 16.1: Mean solar/lunar motions for century epoch dates, comparing results from modern formulae.

MEAN MOON COMPARISON		JULIAN TIME		NOON, DEC. 31, N.S.			
J.DATE	J.CENTURY	DATE	a	b	c	a-b	c-b
		ASTR. EPH.	ASTR. EPH.	MEEUS	CHAPR-T	differences in arcsecs	
2451545	0	2000	218.3164	218.3165	218.3166	-0.36	0.36
2415020	-1	1900	270.4335	270.4338	270.4338	-1.08	0
2378495	-2	1800	322.5474	322.5485	322.5487	-3.96	0.72
2341970	-3	1700	14.65816	14.6605	14.6607	-8.424	0.72
2305445	-4	1600	66.76568	66.7699	66.7701	-15.192	0.72
2335043	-3.18965	OS 1680	181.7579	181.7605	181.7607	-9.36	0.72
ASTR. EPH. for J.cent:							
-3							
218.3164	+1337r.T	+307.881.T	-0.0016.T.T				
218.3164	0	156.3562	0.0144	=	14.65816	degrees	
MEEUS for J.cent:							
-3							
218.316	+ 481267T	- 0.0013T*T					
218.3165	-203.644	0.011943		=	14.66049		
CHAPRONT-TOUZE:							
-3							
218.3166	-733118	-42.984		=	14.66068		
		-203.656					
.....							
SUN for J. cent: -3							
EXP.SUPP.	280.465	-357.151	0.0027	=	278.1622		
MEEUS	280.4665	-2.30949	0.002729	=	278.1597		
RGO 1961	279.6967	-1.53785	0.00121	=	278.16		
PERIGEE for J. cent: -3							
MEEUS:	83.35324	-327.041	-0.09292	=	116.2192		
CHAP-T:	83.3532	-327.041	-0.09292	=	116.2192		
.....							
NODE for J. cent: -3							
EXP.SUPP:	125.0445	-42.4087	0.018495	=	167.4717		
MEEUS	125.0446	-42.4086	0.018684	=	167.4718		
CHAP-T:	125.0446	-42.4086	0.018686	=	167.4718		
APHELION for J.cent: -3							
MEEUS:	102.9373	-5.15856	0.004	=	97.78279		
R.G.O. '61:	101.2208	-3.43835	0.001811	=	97.78426		

amounting to as much as 15 arcseconds for 1600. The latter are more up-to-date, and are more convenient, since they give the five variables as we require them, whereas the others give anomaly values. The rest of the Table compares different computations for mean node, perigee, sun and aphelion, for a time-value here set at -3 Julian centuries, ie the epoch Dec. 31 noon GMT 1700.

The formulae from Meeus' 'Astronomical Algorithms' (1992) supercede the formulae given in his 'Astronomical Formulae for Calculators' of 1986, "which were based on the older lunar theory by Brown" (Meeus). For solar longitude Chapront-Touze's table (1988, p.346) differed from Meeus', as Chapront's table gave solar position referred to a fixed equinox of 2000, whereas tropical longitude requires the equinox of date. See also M.Chapront-Touze and J.Chapront, 1992, p.12 for mean equations in degrees.

The Meeus 1992 equations for mean motion are as follows. Let JD be (classical) Julian Day, then the time T measured in Julian centuries from J2000 is given by:

$$T = \frac{JD - 2451545.0}{36525}$$

The the following Meeus expressions are all in degrees of tropical longitude, ie referred to the mean equinox of date.

Moon's mean longitude:

$$L' = 218.316459 + 481267.881342T - 0.0013268T^2$$

Mean longitude of Sun (geometric, ie without aberration):

$$L = 280.46645 + 36000.76983T + 0.0003032T^2$$

Longitude of mean ascending node of Moon's orbit:

$$\Omega = 125.044555 - 1934.136184T + 0.002076T^2$$

Longitude of perihelion of Earth (=aphelion of 'Sun'):

$$\pi = 102.93735 + 1.719526T + 0.00045962T^2$$

Longitude of perigee of lunar orbit:

$$\pi' = 83.353243 + 4069.013711T - 0.0103238T^2$$

Two examples of Explanatory Supplement Equations:

$$\Omega = 125^\circ 2' 40''.3 - (5^\circ + 134^\circ 8' 10''.5)T + 7''.4T^2$$

$$L' - \Omega = 93^\circ 16' 18''.9 + (1342^\circ + 82^\circ 1' 3''.1)T - 13''.25T^2$$

$$(5^\circ = 5 \times 360^\circ)$$

### III Mean Motions from Textbooks 1650 - 1750

Ecliptic longitudes were cited as signs (ie, 30° intervals), degrees, minutes and seconds, for mean positions, normally for noon on Dec. 31. Three sets of epoch values for the five variables are given, see Ch.5. There were different conventions for their use, some tables citing them for 1680 eg and others, 1681. The series of Julian dates shown in the spreadsheet (end of Ch.5) differ by 7305 days, as the period between 20-year intervals; however, sometimes I had to add one day to the 1620-1660 epochs to fit the tables. Thus, Shakerley had a solar longitude of 291° for 1660 epoch, while the Lotus table (Ch.5, V) gives 290° for that period. For continental values, calendar change and time adjustment also had to be allowed for: in matching the modern equations to historic values, a discrepancy of a year appears in apogee and node values, that of a day in solar position and that of hours in lunar longitude.

	<u>MOON</u>	<u>SUN</u>	<u>NODE</u>	<u>APOGEE</u>	<u>APHELION</u>
Wing (1651)					
1601	0.7. 33.29	9.19.58.34	9.11.35. 4	7.19. 0.30	3. 5.43.28
+40	8.27. 7.28	17.57	1.23.40.27	6. 7.43. 8	0. 0.41. 5
+60	1.10.41.12	26.56	2.20.30.40	9.11.34.42	1. 1.38
+80	5.24.14.56	35.54	3.17.20.54	0.15.26.16	1.22.10
Shakerley (1653)				anomaly	
1620	5. 4.15.14	9.21.6.15	-	6.11.16.17	3.5.54.14
1640	9.17.48.57	15.13		7.20.58.19	6.13.11
1660	2. 1.22.41	24.12		9. 0.40.20	6.32. 8
Newton,J (1657) (in decimals)				anomaly	
1620	154.2539	291.0982		191.271	95.9068
1640	287.816	291.2477	-	230.972	96.2227
1660	61.378	291.3972		270.672	96.5385
Streete (1661)					
1641	-	-	7.17.50. 0	1.26.35.0	-
1661			6.20.59.30	5. 0.25.0	
1681			5.24. 9. 0	8. 4.15.0	

	<u>MOON</u>	<u>SUN</u>	<u>NODE</u>	<u>APOGEE</u>	<u>APHELION</u>
<b>Riccioli (1665)</b>					
1600	7.25.19.20	9.19.55.58	9.11.22.50	0. 7.27. 9	3.7.39. 8
+40	8.27. 7.32	18.24	1.23.40.27	2.19.24. 2	0.41.23
+60	1.10.41.19	27.36	2.20.30.40	3.29. 6. 4	1. 2. 5
+80	5.24.15. 5	36.48	3.17.20.54	5.18.48. 5	1.22.46
<b>Flamsteed (1681)</b>					
1661	1.18.10.14	9.20.25.46	6.21. 4.47	5. 0.21.51	3.6.35. 0
1681	6. 1.43.58	34.48	5.24.14.33	8. 4.11.51	6.51.40
1701	10.15.17.44	43.50	4.27.24.20	11. 8. 1.51	7. 8.20
<b>La Hire (1687)</b>					
1600	0.20.38.25	9.11.5.45	9.11.38.14	7.18.52.56	3.6.25. 0
+60	1.10.42. 1	27.30	2.20.30.40	9.11.32.34	1. 1.30
+80	5.24.16. 1	36.40	3.17.20.54	0.15.23.25	1.22. 0
1700	6. 3.32. 1	9.10.52.27	4.28. 2. 4	11. 6.53.40	3.8. 7.30
<b>Greenwood (1689) anomaly</b>					
1661		8.17.44. 0	6.20.59.30	5. 0.25. 0	
1681	-	9.27.27.45	5.24. 9. 0	8. 4.15. 0	-
1701		11. 7.11.30	4.27.18.30	11. 8. 5. 0	
<b>Whiston (1710)</b>					
1681	6. 1.45.45	9.20.34.46	5.24.14.35	8. 4.28. 5	3.7.23.30
1701	10.15.19.50	43.50	4.27.24.20	11. 8.18.20	40.10
1721	2.28.53.55	52.54	4. 0.34. 6	2.12. 8. 6	56.50
<b>Grammatici (1726)</b>					
1700	5.19.58.10	9. 9.51.25	0.27.59.23	11. 7. 4.35	3.7.44.28
+20	10. 3.32.15	9.10. 0.29	4. 1. 9. 8	2.10.54.50	3.8. 5.28
+40	2.17. 6.20	9.10. 9.33	3. 4.18.53	5.14.45. 5	3.8.26.28

Wright (1732)

1721	2.28.53.55	9.20.52.54	4. 0.34. 6	2.12. 8.35	3.8. 5.30
1741	7.12.28.00	21. 1.58	3. 3.43.51	5.15.58.50	26.30
1761	11.26. 2. 5	11. 2	2. 6.53.35	8.19.49. 5	47.30

Capello (1737)

1720	10.3.32.20	9.10. 0.14	4. 1. 9. 8	2.10.54.50	3.8. 5.28
1740	2.17.6.25	9.17	3. 4.18.53	5.14.45. 5	25.28
1760	7.0.39.45	18.21	2. 7.28.38	8.18.37. 3	47.29

Dunthorne (1739)

1721	2.28.53.55	9.20.52.54	4. 0.34. 5	2.12. 8.35	3.8. 5.30
1741	7.12.28. 0	21. 1.58	3. 3.43.50	5.15.58.50	26.30
1761	11.26. 2. 5	11. 2	2. 6.53.35	8.19.49. 5	47. 3

Cassini (1740)

1720	5.24.29.14	9.10.16.26	4.20.31.53	1. 0.18.26	3.7.56.30
1740	10. 8. 3.13	25.35	3.23.41.39	4. 4. 9.17	8.17. 5
1760	2.21.37.12.	34.43	2.26.51.25	7. 8. 0. 9	37.40

Le Monnier (1746)

1720	5.24.30.29	9.10.16.19	4.20.28.46	1. 0.16.51	3.8. 4.25
1740	10. 8. 4.34	25.23	3.23.38.31	4. 4. 7. 5	25.25
1760	2.21.38.39	34.27	2.26.48.16	7. 7.57.20	46.25

Leadbetter (1742)

				anomaly	solar anomaly
1721	2.28.54. 5	9.20.52.54	4. 0.33. 5	0.16.43.50	6.12.47.24
1741	7.12.28.10	9.21. 1.58	3. 3.42.50	1.26.27.40	35.28
1761	11.26. 2.15	11. 2	2. 6.52.35	3. 6.11.20	23.32

Halley (1719/49)

1701	10.15.20. 0	9.20.43.33	4.27.23.20	11. 8.20. 0	-
1721	2.28.54. 5	15.39	4. 0.33. 5	2.12.10.15	
1741	7.12.28.10	28.45	3. 3.42.50	5.16. 0.30	

## I V GLOSSARY OF TERMS USED IN TEXT

**ANNUAL ARGUMENT** TMM's term for the angle in zodiacal longitude between the aphelion and mean sun.

**ANNUAL EQUATION** Correction to be applied, using Kepler's second law or some approximation thereto, to uniform circular motion in the course of a year, eg of the Sun.

**ANOMALISTIC MONTH** 27.5 days, the interval between lunar conjunctions with its mean apse.

**APOGEE** Furthest distance of Moon from Earth each month; this term was also used to denote aphelion, Earth's furthest distance from the Sun, in TMM.

**ARGUMENT** Angle from which an 'equation' was computed, eg the 'annual argument' referred to the Sun-apse angle.

**APSIDES** The two points in the orbit of a planetary body at which it is at the greatest or least distance from the body about which it revolves. The **APSE LINE** joins these two points. For an elliptical orbit (not assumed in TMM) it is the major axis.

**DECLINATION** Angle measured from the celestial equator and at right angles to it, taken conjointly with Right Ascension; this was replacing the older system of measuring position by celestial latitude and longitude.

**ECCENTRICITY** Conceiving an orbit as circular about a displaced centre, and taking the radius as  $10^6$ , then eccentricity was the distance of that orbit centre from the position of the Earth about which that orbit was described; or, about the Sun instead of Earth.

**EPHEMERIS** Table showing predicted positions of a heavenly body for every day, or some multiple of days, during a given period.

EPOCH for mean motion values: usually over twenty-year periods, mean motion 'radix' positions were specified usually for noon of December 31st.

EQUATION The angle that is required to be added to a mean motion in order to 'correct' it.

EQUINOCTIAL POINT Zero Aries in tropical longitude.

EQUATION OF THE CENTRE is the difference between true and mean anomalies. The principal adjustment used in TMM, whereby an approximation to the Kepler-equation, for a given eccentricity and apse position, was added to mean motion.

EQUATION OF TIME What Flamsteed called, 'The Equation of the Naturall Days', whereby the noon 'tempus apparens' deviated from uniform time, based on the Earth's uniform rotation.

'HORROX ANGLE' What TMM called the 'Annual Argument' varied with the angle between the mean sun and mean apogee, here referred to as the Horrox angle. Its period is thirteen months or 411 days.

'HORROX-WHEEL' The name here given to a deferent-wheel attached by Horrox to the mean apse line, whose revolution period was 6.75 months, which generated both the second apse equation and the varying eccentricity.

JULIAN YEAR, by which time in TMM was measured, was 365.25 days, as compared with 365.2425 days of the Gregorian calendar which Europe was then using.

MEAN ANOMALY Angle between planet or luminary and its mean apse.

MEAN SUN This moves with uniform angular motion along the ecliptic, and coincides with the true or co-equated sun twice yearly, at the apsides.

NODES Points where the lunar orbit intersects the plane of the ecliptic.



**OBLIQUITY OF ECLIPTIC** Inclination of the ecliptic to the celestial equator.

**OCTANTS** Usually the  $45^\circ$  and  $135^\circ$  angles between the Sun and Moon, formed four times per month; or, when Newton wrote 'when the moons apoge is in the Octants' it referred to the Sun-apse angle having such a magnitude.

**PERIGEE** Nearest approach of Moon each month (or of Earth to Sun, in TMM).

**PROSTHAPHAERESIS** Term(s) to be added to an anomaly value to 'co-equate' it whereby the 'true' orbit position was attained.

**QUADRATURE**  $90^\circ$  angles between (usually) Sun and Moon, formed fortnightly.

**REDUCTION** The transform of positions from the lunar orbit plane onto that of the ecliptic.

**RIGHT ASCENSION** This term is not used in TMM, but was the form in which data was supplied to Newton by Flamsteed, as degrees measured on the celestial equator.

**SAROS** A period of 223 lunations, or 11 years, 10 or 11  $1/3$  days (depending on leap years), when several monthly cycles closely coincided.

**SIDEREAL** Lunar period of 27.32 days as orbit period against stars, referred to but not used in TMM.

**SYNODIC** Lunar period 29.53 days, as mean period between Full Moons.

**SYZYGY** The conjunction and opposition of Moon and Sun, or the line joining these two positions in space.

**TROPICAL** Year is time for Sun to return to the Vernal Equinox.

**VARIATION** A lunar inequality of period half a lunar month, maximal in octants.

## V TMM on a Lotus Spreadsheet

The program has been set for the moment of Dunthorne's worked example, see Chapter 8, I and table 8.2, with time value at the top left, below which are the 'mean motions.'

T - VALUE		SEVEN MOONS		STEPS OF EQUATION			
				Deg	Min	Sec	
t =	20456.15	M1 =	80.06947		-3	0	Annual Eq
M =	80.11963	M2 =	80.1117		2	32	2nd Eqn
S =	293.124	M3 =	80.12357		0	42	3rd Eqn
A =	3.453208	M4 =	74.81506	-5	-18	-30	Eqn Centr
H =	98.37185	M5 =	74.20712		-36	-28	5th (Variar
N =	170.9854	M6 =	74.18568		-1	-17	6th Eqn
S1 =	293.6279	M7 =	74.1633		-1	-20	7th Eqn
A1 =	3.538003	M end =	74.1421		-1	-16	Reducn
A2 =	354.2118		ANSWER:	74	8	31	
N1 =	170.9451						
N2 =	169.5718						

THE FUNCTIONS		FIVE MEAN MOTIONS				PI
f =	0.046705	M:	181.763	13.1764	269720.1	3.141593
g =	9.326157	S:	290.579	0.985647	20453.12	
h =	-5.30851	A:	244.468	0.111408	2523.453	360
j =	1.37332	H:	97.392	0.000048	98.37185	
		N:	174.243	0.052955	-909.015	-189.015

FOUR ANNUAL EQUATIONS								
S1 =	293.6279	0.493744	0.254639	-3.39907	-194.752	-0.0101	-0.49249	-6.79813
M1 =	80.06947	-0.05016	0.254639	0.051224				
A1 =	3.538003	0.084795	0.254639					
N1 =	170.9451	0.040233	0.254639					

THREE FUNCTIONS							
f =	0.046705	0.848405	0.71979	-0.32562	-0.76402	-10.126	-580.18
g =	9.326157	0.162772	0.162054	13.81419	0.645187	-10.126	-580.18
j =	1.37332	0.023969	0.023974	37.91617	-0.41683	-4.28244	-245.365

SECOND AND THIRD EQUATIONS							
M2 =	80.1117	0.006452	0.645187	-10.126	-580.18	6.544831	-0.96476
M3 =	80.12357	0.011871	0.908985	114.6346			

EQUATION OF CENTRE							
A2 =	-5.78815		-0.02222	-10.186	-0.14222	3.2843	188.1766
h =	-5.30851	57.29578		0.093384			
			-5.33689	0.093384	-0.99746	4.783743	274.0883
			3rd term:	0.00616	0.000108	0.977204	102.2648

M4 =	74.81506						
M5 =	74.20712	-0.60794	-0.97677	-7.63801	-437.626	0.622401	-0.0301
M6 =	74.18568	-0.02144	-0.59369	5.64755	323.5808		-0.96476
M7 =	74.1633	-0.02238	219.4422	-0.6353	1.817935	-0.00367	0.035221

THE REDUCTION							
N2 =	169.5718			-0.02185	-0.37279	-4.33037	-248.112
		-0.02121	-0.02121				
M end =	74.1421			-0.02168	-0.18691	3.329605	190.7723

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