# 3. BIOGRAPHICAL EMBEDDING

Although this is not the place or time for a comprehensive biography of Tobias Mayer, a short account of his—likewise short—life is desired here. The narrative is biased towards the facets of Mayer's life (1723–1762) that are relevant to the current investigation of the development of his lunar tables. Therefore almost everything related to the practical pursuit of astronomy is left out, including what pertains to instruments, observing, or the reduction of observations.[1] On the other hand, several new results concerning the development of the lunar tables are included in this chapter. Many are worked out in more detail in the ensuing chapters, to which I will refer where appropriate. This chapter, devoid of most of the technical detail, may also serve as a backdrop against which the more technical topics in this thesis will fall into perspective.

## 3.1 YOUTH AND EARLY CAREER

Tobias was born in Marbach in 1723, and raised in Esslingen, both close to Stuttgart in the German state of Württemberg. His parents died while he was still a boy, but fortunately for him his talents were recognized, and he was enabled to attend Latin school, meanwhile developing his early interests in drawing, fortification, and cartography. Through these interests he came into contact with geometry. Mathematics was not a subject taught in his school, but Mayer found company with a local shoemaker with a similar interest. The shoemaker had money to buy mathematical texts but no time to read them, therefore they agreed that Mayer read the books and taught their contents to the shoemaker mending shoes. After mastering Christian von Wolff's *Anfangs-Gründe aller mathematischen Wissenschaften* and similar works, Mayer wrote his first treatise, published in 1741, on a new and general method for the inscription of polygons in circles, a subject clearly displaying his interest in the theory of fortifications.

Mayer's attempted career with the military did not materialize. Instead, he found employment in Augsburg with the publisher Johann Andreas Pfeffel in 1744. The next year saw the publication of Mayer's impressive *Mathematischer Atlas*, in which the then 22-year old author provided an overview of the mathematical sciences in 60 plates, divided over the subjects of arithmetic, geometry, trigonometry, geography, chronology, gnomonics, fortification, artillery, architecture, optics, and

1 For a more balanced biography see [Forbes, 1980], from which most biographical information in this chapter is taken.

mechanics.[2] Not long after, Mayer moved to Nuremberg, a town that could look back on a long astronomical heritage since the days of Regiomontanus and Bernhard Walther in the late 15th century. Here, with his *Mathematischer Atlas* providing credentials, Mayer entered the prestigious geographic firm of Johann Baptist Homann's heirs, which held a leading position in German cartography as well as high ambitions for the future.

A very fruitful period in his life began with Mayer joining in to draw several maps for the *Gesellschafts Atlas*, which the Homann house published in 1747. Soon Johann Michael Franz (one of the firm's directors), Tobias Mayer, and his colleague Georg Moritz Lowitz, were pursuing the plans of their own Cosmographical Society, aimed at the (re-)structuring of German cartography, no doubt also with an eye on the increasingly difficult cash position of the main firm.

By way of illustrative depiction of the accuracy—or rather the lack thereof—with which the latitudes and longitudes of German places were known, Mayer composed *Mappa Critica*,[3] a map showing three Germanies dislocated according to three geographers: de l'Isle (probably Guillaume, 1665–1726), Homann, and Mayer himself. Whereas the latitudes of the charted cities were mostly consistent, the longitudes of some places (such as Dresden or Prague) differed by as much as a degree, thus showing to what extent longitude was problematic even on land. Mayer drew on a wide variety of sources and was thus confronted with the conflicting data provided by the various reports that he employed. The handling of such data sets will be seen to be a recurring theme in his work.

Other projects for the Cosmographical Society included the production of lunar globes, and the dissemination of research results of its members in *Kosmographische Nachrichten und Sammlungen*, which was intended as a series with volumes to appear annually. Mayer contributed to these goals with a comprehensive programme of mapping the visible lunar surface, for which a careful quantification of lunar libration was necessary: here is another example of handling large amounts of data, which we will further explore in chapter 9. For diverse reasons the lunar globes were never completed.[4]

The intended series of the *Nachrichten* was rather short-lived, because only one volume was produced.[5] Characteristically, in his numerous contributions to the volume Mayer was keen to show his careful handling of astronomical observations and his ability to procure everything possible from them. He was now making astronomical observations both from the premises of the Homann firm and from the observatory of the former astronomer Eimmart—although the latter place was in such a state of neglect that he had to bring a hammer to operate the sector. In Mayer's

2 See [Mayer, 1745]

3 [Mayer, 1750c].

4 A number of sectors for the globe were engraved in copper during Mayer's lifetime. These sectors have been preserved and the missing sectors are being engraved at the time of writing, based on Mayer's drawings of a lunar map. It is expected that the globes will be produced near the end of 2007. This is a project initiated by the *Tobias Mayer Museum Verein* in Marbach.

5 [Mayer et al., 1750].

eyes, the chief instrument to bring improvement in geography, was astronomy:

> It is only too well known in geography that many, indeed most, of the numerous known and noteworthy places need to have their latitude and longitude more accurately determined. To achieve this is one of the most important objects of astronomy [. . . ].[6]

As Forbes showed, almost all of Mayer's further professional endeavours were guided by this assessment.

## 3.2 FIRST LUNAR TABLES

It is evident from various sources that Mayer used Leonhard Euler's lunar tables of 1746, and the almanacs in the *Berliner Kalendar* which were based on those tables, in the period when he was working at the Homann cartographic office in Nuremberg.[7] Mayer had his own manuscript copy of Euler's lunar tables, aliased `duin` by me.[8] In section 9.6 I discuss an attempt of Mayer's to adjust the coefficients in the `duin` tables to observations. Apparently he was not satisfied with Euler's `duin` tables, and he was eager to construct tables of his own.

In the beginning of 1751, when he was still in Nuremberg, Mayer proudly announced to the astronomer Joseph Nicholas De l'Isle (1688–1768), with whom he was in regular correspondence, that he had made new lunar tables. He averred that these (alias `kreek`) were accurate to about $2'$. He added that these tables were easy to use.[9] I have been unable to locate the `kreek` tables among Mayer's manuscripts,

6 Quoted from [Forbes, 1980, p. 42]. The original is in *Untersuchungen über die geographische Länge und Breite der Stadt Nürnberg* (researches into the geographical latitude and longitude of the town of Nuremberg), Cod. $\mu^{\sharp}_{11}$, reprinted in [Forbes, 1972, I, pp. 33 ff.].

7 E.g., [Mayer et al., 1750], correspondence of Franz to Euler, [Juškevič, 1975, pp. 119, 120]. See Forbes [Forbes, 1980, pp. 55, 56]. The Euler tables are [Euler, 1746].

8 See the aliases list in appendix A. Mayer's copy of Euler's tables is in manuscript Cod. $\mu^{\sharp}_{14}$.

9 'I am about to finish a very long and painstaking algebraical calculation that I have undertaken on the Lunar Theory in the Newtonian system. In it, I have deduced Lunar Tables that are much more exact than any others, because they give the true place of the Moon to about 2 minutes precision, as I have recognized after having compared them with more than 20 observations made in different aspects. They are not less convenient for the calculation, because one does not need to fetch first of all the true place of the sun, nor the true or eccentric anomalies, as in those of other Astronomers [such as Euler or Lemonnier]. It suffices to take the mean motions in order to find in the Tables the equAtions, which are at the same time all additive, so that the reduction of the mean place of the Moon to the true place happens at the same time & through a single addition.' (*«Je viens de finir un calcul algebrique fort long et penible, lequel j'avois entrepris sur la Theorie de la Lune dans le systeme Newtonien. J'en ai deduit des Tables Lunaires beaucoup plus exactes qu'aucunes autres puisque elles donnent le vrai lieu de la Lune à 2 minutes pres, ce que j'ai reconnu en les ayant comparées avec plus de 20 observations faites dans differents aspects. Elles ne sont pas moins commodes pour le calcul. Car on n'y a besoin de chercher d'abord ni le vrai lieu de soleil ni les anomalies vraies ou excentriques, commes dans celles des autres Astronomes. Il suffit de prendre les moyens mouvemens pour trouver dans les Tables les équations, qui en mêmes tems sont toutes additives, de sorte que la reduction du lieu moyen de la Lune au lieu vrai se fait à la fois & par une seule addition.»*) So far, he has made mention of Newtonian theory only, not of fitting his tables to observations, but

nor even an indication of their provenance. However, I found several calculations of the moon's position in Mayer's manuscripts Cod. $\mu_6^\sharp$ and the first folios of Cod. $\mu_{41}^\sharp$ that match his description of the tables. It is almost certain that Mayer employed the `kreek` tables for these calculations.

## 3.3 TO GÖTTINGEN

The ambitious Cosmographical Society of Nuremberg attracted the attention of the young and expanding Georg-August Academy in Göttingen. The academy attempted to transplant the Society to Göttingen, and Mayer was offered a professorship in practical mathematics together with the responsibility (to be shared with Johann Andreas Segner) for a new astronomical observatory. Plans for the observatory had developed after George II, King of Great Britain and Elector of Hanover, had visited Göttingen in 1748.

Mayer accepted the offer and arrived in Göttingen around March 1751, four years later followed by his colleagues of the Cosmographical Society, Moritz and Franz, who were also persuaded to come from Nuremberg to Göttingen. Although financial problems shattered the Cosmographical Society after its relocation into the Hanoverian state, Mayer's research took off on a new footing. The emphasis in his work had already been drifting away from cartography towards astronomy, and in his new academical surroundings an increasing demand was placed upon his research and teaching abilities.

In July of 1751, soon after taking up his professorship in Göttingen, Mayer started a correspondence with Euler who was then in Berlin. Euler had written several memoirs concerning orbital motion, including one on the great inequAlity of Jupiter and Saturn; he had also prepared the `duin` lunar tables already mentioned, from which ephemerides for the *Berliner Kalender* were computed. Thirty-one of the letters they exchanged have been preserved.[10] They provide a most valuable insight in many aspects of Mayer's scientific work and in the relation between Euler and Mayer. The topics that were discussed included refraction of light in the atmosphere, parallax of the moon, theories of gravitation and ether, lunar theory, and many more.

In his first letter, Mayer expressed his admiration for Euler's diverse treatises on celestial mechanics, especially the treatise on the great inequAlity of Jupiter and Saturn,[11] from which Mayer had learnt some essential mathematical techniques. Euler's treatise had won a contest of the Paris Academy, which was held annually

after having promised to send the tables he asked if De l'Isle would be willing to communicate some of his best lunar observations in order to be able to further improve my Tables before publishing them (*«pour être en état de pouvoir perfectionner encore plus mes Tables avant de les publier»*). Mayer to De l'Isle, Nuremberg 14 Jan. 1751. Published in [Forbes, 1983]. See also [Forbes, 1972, I, p. 15]; however, most of Forbes' assertions in his introductory chapter must be rejected.

10 The letters were published in [Forbes, 1971a]; see fn. 4 in chapter 1.

11 See p. 9.

in consequence of the will of Rouillé de Meslay. Considering the influence of Euler's treatise on Mayer, we see that the will of Rouillé de Meslay has indeed had a significant impact on the determination of longitude at sea. Mayer applied the techniques that he had learnt through Euler's publications in the derivation of his own lunar theory from the law of gravitation, to be discussed in chapter 5. However, he admitted that he was not completely successful, and I will show (chapter 6) that his theory remained incomplete and in a problematic state until 1755.

In that same first letter to Euler, Mayer referred to a method of predicting lunar positions that had been put forward by Edmond Halley. This method exploited the fact that the mutual positions of sun, earth, and moon in their orbits are nearly repeated after a Saros, so that the error pattern of an arbitrary set of lunar tables is repeated. Mayer, in his letter to Euler, quoted his own formula with which he could predict a future lunar position, given a position one or more Saros periods earlier: in other words, his formula expressed the deviation of the moon's position from full periodicity.[12] This seems to be the only reference of Mayer to the predictive use of the Saros periodicity. I have not been able to find computations among Mayer's manuscripts that are connected to either the derivation or the use of the above-mentioned formula. In later years, Mayer employed the Saros periodicity (but not the formula) to select pairs of observations of near-identical lunar positions, with which he could fit the mean motion coefficients in his lunar theories. In other cases, such a pair could indicate whether his tables or a faulty observation were to blame for an unusually large discrepancy.

## 3.4 THE FIRST GÖTTINGEN TABLES

Almost a year later, in January 1752,[13] Mayer reported to Euler a rather different set of lunar coefficients, to which I assigned the name `zand`. He made no mention any more of the former arrangements of the `kreek` tables, nor of Halley's Saros method; yet all the arguments of the set were still combinations of the mean motions. Mayer said that some of the coefficients had been adjusted to observations, but it is unknown to us how. I found traces of this stage of his work on lunar theory in several of his manuscripts, some of which I will present in the following chapters.

By that time Mayer had also finished an investigation of lunar parallax. In a memoir on that subject, he warned that parallax could not be accurately determined without paying attention to the oblate shape of the earth, and he explained two independent ways to arrive at lunar parallax: one way exploited a relation between the orbital period of the moon and the slowing of a pendulum at the equator (caused by it being further removed from the centre of the earth), the other depended on the rapidly changing effect of parallax on the observed position of the moon when the moon approaches the meridian.[14]

12 [Forbes, 1971a, pp. 34, 35]

13 [Forbes, 1971a, p. 48].

14 The essence of the second procedure is this: let the moon be observed at two known times, say

Forbes has asserted that: 'Mayer's recognition of the need to take account of the Earth's spheroidal shape now involved him in the calculation of a large number of new inequalities, of which he retained sixteen...'.[15] Indeed Mayer wrote to Euler that he had considered the influence of the earth's oblateness; I located Mayer's computations to that effect in Cod. $\mu^{\sharp}_{30}$, together with some of Mayer's work on atmospheric refraction. But the sixteen terms that Forbes alluded to, are simply too sizable to be the result of the earth's oblateness. In fact, they are part of the `zand` version of lunar tables. Mayer wrote their coefficients in a letter to Euler in a passage following his statements on the determination of the lunar parallax, hence, I presume, Forbes' misinterpretation. Mayer reporting later to Euler that: 'I have completely and utterly ignored many small equations, which are similar to those which depend on the shape of the Earth, since they scarcely amount to $12''$ or $15''$,'[16] suggests no more than that he was aware of the small size of possible oblateness equAtions in lunar longitude.[17]

## 3.5 A NEW COURSE: MULTIPLE STEPS

In January 1753, Mayer reported to Euler again a completely new version of tables, derived, as he says, from theory and adjusted to observations.[18] Four successive steps are needed to compute the position of the moon with these tables. In each step, some arguments with which the tables are entered are modified in a specific way: the arguments are no longer equal to the mean motion arguments, as they were before.

This 'shift in strategy'[19] which seems to fall out of the blue, leads us now to distinguish the *multistep* form of these tables, formulae, and computations where

an hour apart. Let $\alpha_1 - \alpha_2 = \Delta\alpha$ be the observed difference of the moon's right ascensions at those times, and let $\alpha_1' - \alpha_2' = \Delta\alpha'$ be the difference of right ascensions at these times according to lunar positions computed from tables. The parallax in right ascension is $P\cos\varphi\frac{\sin\beta}{\cos\delta}$, where $P$ is the unknown equatorial lunar parallax, $\varphi$ is the latitude of the observer (corrected for oblateness), $\beta$ is the apparent azimuth of the moon, and $\delta$ its declination. The difference of true and observed displacements equals the difference of parallax in right ascension, or

$$\Delta\alpha' - \Delta\alpha = P\cos\varphi\left(\frac{\sin\beta_1}{\cos\delta_1} - \frac{\sin\beta_2}{\cos\delta_2}\right),$$

in which $P$ is the only unknown. Thus Mayer circumvented the inaccuracy of lunar tables: over short time-spans they represented lunar velocities much better than positions. Mayer still used Euler's lunar tables from the *Berliner Kalender* when he composed this memoir [Mayer, 1753a].

15 [Forbes, 1980, p. 136]; see [Forbes, 1971a, pp. 10, 47–49] for the letter to Euler referred to in the text.

16 [Forbes, 1971a, p. 63].'

17 Mayer's tables after 1754 included an empirically detected equAtion, with argument the angular distance of the moon from the ascending node of its orbit, of only $4''$. Laplace later proved that this equAtion is caused by the oblateness of the earth [Laplace, 1802, pp. 658–659].

18 [Forbes, 1971a, pp. 61–63].

19 quoted from [Forbes and Wilson, 1995, p. 64].

arguments are modified in several steps, from the former *single-step* ones that use only mean motion arguments throughout. A more thorough explanation of these two forms is contained in section 4.1 and, more specifically, section 4.2.

Mayer had checked his new tables with more than 200 observations, and found only ten or twelve that deviated about $1\frac{1}{2}'$. Mayer mildly adjusted these tables, and he published them (henceforth designated as `kil`) a few months later in the proceedings of the Göttingen Scientific Society.[20] Among Mayer's manuscripts I have found new information showing that Newton's lunar theory of 1702,[21] as represented by Lemonnier's lunar tables,[22] had a profound impact on `kil`, in particular on Mayer's shift of strategy. This impact is the subject of chapter 6.

In this period Mayer created a new tool to help improve the accuracy of the tables. This tool has not been recognized and described in earlier research. It is of high importance for our understanding of Mayer's work on lunar tables and of the early development of statistical ideas. We will study it fully below in section 8.6. The tool embodied a means to keep track of the effectiveness of various amendments that Mayer tried to the coefficient values. The sheets of paper on which he systematically recorded these effects show a remarkable similarity, both in form and function, with the modern objects that we commonly refer to as spreadsheets. In absence of any indication of what Mayer called them, I will not hesitate to apply the anachronistic but apt term *spreadsheets* to these papers, or *spreadsheet tool* when the emphasis is on their functionality.

## 3.6 CONTENDING FOR THE LONGITUDE PRIZE

Because of the high accuracy of the published `kil` tables, the determination of longitude at sea by lunar distances came within reach. Prior to this development, references to longitude at sea are strikingly absent from Mayer's correspondence and notes, yet without the slightest doubt he must have been fully aware of the problem and of the rewards offered for a solution. Being rather modest about his own accomplishments, Mayer was encouraged by Euler, and urged by Michaelis, to submit the `kil` tables to the Board of Longitude in Great Britain, in order to compete for the Longitude Prize stipulated by the Act 12 Queen Anne. The philologist Johann David Michaelis (1717–1791) was a key figure in the events to be described below. He was secretary of the Göttingen Scientific Society and editor of its proceedings in which the `kil` tables were published. Besides, he was secretary for Hanoverian affairs in Göttingen. Because his cousin William Philip Best was one of the private secretaries of King George II, a diplomatic channel to London existed through Michaelis. Through this channel, communications on Mayer as a candidate for the Longitude Prize were started in September 1754.

20 [Mayer, 1753b].

21 See [Newton, 1975]. Newton's 'lunar theory' of 1702 held a prescription, or algorithm, for the computation of lunar positions; it may be a surprise to the modern reader that there was no explanation by way of a theory of gravity in it.

22 [Lemonnier and Keill, 1746].

A few months earlier, the Göttingen *Commentarii* containing Mayer's `kil` tables had already reached the other side of the North Sea, where they were received with guarded enthusiasm. A preliminary check of the tables against observations showed one or two exceptionally large differences, which were soon traced to errors in the reduction of the observations. An extract of Mayer's own preface to the tables was published in the *Gentleman's Magazine*.[23] As Forbes pointed out, Mayer himself was somewhat reluctant to approach the Board of Longitude, because he realized that the Board would not consider whether his sole *tables* were within the accuracy requirement, but that the Board rather required a complete *method* that could stand up to the accuracy requirements in an actual trial at sea.[24] So Mayer needed first of all to embed his tables in such a method, and secondly, he had to improve their accuracy, because when in use at sea, both the astronomical observations and the ensuing calculations would further dilute the overall accuracy of the method.

Thus, Mayer continued to refine his tables by observations and the spreadsheet tool. Previously he had employed timed lunar meridian passages and lunar eclipses. But since his published `kil` tables were already of about the same accuracy as those observations, improvement was no straightforward task. To proceed, Mayer selected observations of higher quality, namely those of occultations. At an occultation, the moon visually intercepts the light from a star or planet. The moon covers and uncovers the other body almost instantaneously, therefore occultations can provide accurately timed positions of the moon relative to the occulted body.

Mayer was well aware of the inaccuracy of stellar positions in the star catalogues then in use, and therefore he started out with occultations of only one star, Aldebaran. Any catalogue error in Aldebaran's position would then only be reflected in the lunar epoch position. To fix the epoch position, and consequently to correct the coordinates of Aldebaran, he used solar eclipse observations. Then, gradually, he admitted occultations of other stars into his data set.[25]

In the same period, Mayer was negotiating his resignation from Göttingen to accept an offer of a position in Berlin, where he would be working together with Euler, who had been most instrumental in offering him a position there. As it happened, though, Mayer's resignation was refused by nobody less than King George II, who was fully aware of the chances Mayer stood in regard of the Longitude Prize. Mayer's leave to Berlin would not only break the attempts to concentrate the Cosmographical Society in Göttingen, it could also mean a serious loss of prestige. In return, Mayer was in a position to demand the resignation of Segner, his superior with whom he had to share the observatory, and with whom relations were

23 The *Gentleman's Magazine* for August, 1754, contains a translation into English of a substantial part of Mayer's Latin preface, signed 'B.J.'. Forbes included most of the English article in his biography of Mayer and identified the author as John Bevis [Forbes, 1980, pp. 143–146].

24 [Forbes, 1980, p. 153].

25 Mayer reported this procedure to Euler; see [Forbes, 1971a, pp. 80–84], and Forbes' comment on p. 15, *ibid.* Concurrently he had derived the moon's latitude to a higher accuracy than any before.

in a particularly bad shape. The observatory, meanwhile, was in its last stages of completion and had still to be equipped with suitable instruments. Its most important instrument was to be a 6 foot mural quadrant made by the London instrument maker John Bird, who had recently constructed a similar quadrant of 8 feet for James Bradley at the Royal Greenwich Observatory. Bird's quadrant was installed in Göttingen in the winter of 1755/56.

Meanwhile, Mayer had prepared a memoir on how to find longitude at sea with his lunar tables and a new instrument (now known as repeating circle) of his own invention. The memoir, together with a description of the instrument, was transferred to William Philip Best in November of 1754.[26] Best sought the advice of James Bradley (the Astronomer Royal, successor of Halley), Lord Anson (First Lord of the Admiralty), and George Parker, 2nd Earl of Macclesfield (president of the Royal Society), all of them *ex officio* members of the Board of Longitude. Lord Anson must have held the most emphatic ties to the longitude problem of this threesome, since he had lost more than 70 of his men searching for potable water and the island of Juan Fernandes in the wrong longitude. Mayer received a reaction of the Board members (principally Bradley) via Best and Michaelis:

> But just as Prof. Mayer adduced that he has further improved the lunar tables in the second volume of the *Commentarii* and figured them out nearer; so they regard it necessary that such calculations, and at the same time the principles upon which they took place, are also shown, ere and before the request will be applied to the Admiralty.[27]

The improved tables that Bradley had desired reached Best before Christmas.[28] These tables, `rak` for short, have never been published, although apparently plans in that direction existed.[29] Nevil Maskelyne retrieved the underlying coefficients of the tables and published them in the Nautical Almanac for 1774; the manuscript tables themselves are apparently not anymore in the Royal Greenwich Observatory archives now kept in Cambridge and must be considered as lost.

Forbes has made mention of the improvements, but he was unaware of Mayer's techniques. Bradley extensively compared Mayer's tables to almost 1200 lunar observations until 1760, finding no differences in excess of $1\frac{1}{4}'$. The paucity of

26 These two were prepended to the edition of Mayer's last manuscript tables [Mayer, 1770], as *Methodus longitudinum promota* with several additions. Mayer's memoir [Mayer, 1754] is a much more general exposition of the usefulness of his tables for the longitude problem, intended for a broader audience; in it, Mayer announced a second part in which he intended to explain their application to that end. No doubt the *Methodus* is that part. Both these treatises had been intended for publication in the annual Göttingen *Commentarii*, and the withdrawal of the second part in view of the quest for the Longitude Prize led to a quarrel with the printer and disruption of the series.

27 *„Gleichwie aber der Hr. Prof. Mayer in seinem Manuscripte angeführt, daß er die in dem zweitenn Bande der in der Commentarien befindliche Mondstabellen annoch verbessert und näher ausgerechnet habe; also halten sie nöthig, daß auch sothane Ausrechnungen, und zugleich die Principien, wornach selbige geschehen, angezeigt werden, ehe und bevor das Gesuch bey der Admiralität angebracht wird"* [Michaelis, 1796] Best to Michaelis, November 19, 1754.

28 [Michaelis, 1796] Best to Michaelis, December 24, 1754.

29 See [Forbes, 1971a, pp. 96, 114]; the London printer Nourse had asked permission to print Mayer's new tables and apparently Mayer agreed.

Bradley's observations of the return of Halley's comet in 1758/9 is probably due to his elaborate checking of Mayer's tables.[30]

Contrary to Mayer's prompt response to Bradley's request for the improved tables, and repeated requests notwithstanding, it took Mayer almost a year to prepare an account of the '*Principien*' on which his tables were founded. Bradley's request embarrassed Mayer, because his tables were founded on a mix of theory and observations; his use of observations to adjust the coefficients lacked a theoretical foundation. To make matters worse, his tables embodied a multistep procedure that he had adapted for practical reasons from a kinematic contraption in Newton's 1702 lunar theory, for which Mayer had no theoretical justification (this is the subject of chapter 6). The difficulty to reconcile the theory with the multistepped computational structure of the tables may well have been the cause of his delay. As I will show in chapter 7, the multistepped structure in itself adds little or nothing to accuracy, but Mayer was probably not aware of that. In consideration of the lack of justification of the multistepped procedure, it may well be doubted whether Mayer had more than an incomplete sketch of a theory when Bradley's request reached him. Mayer's account of the theory and the circumstances of its composition will be further discussed in chapter 5.

## 3.7 FURTHER RESEARCH

After finally completing the theoretical tract, and sending it off to London in the fall of 1755, Mayer continued his efforts to improve the tables using his spreadsheet tool, although not as many table versions were produced as before. He also applied the same method set forth in his lunar theory to the computation of the orbits of Mars and Jupiter, and asserted that his theory of Jupiter was accurate to about $1'$.[31]

The quadrant that Bird had fabricated was installed at the new observatory in the winter of 1756. As soon as that was done, Mayer set out to examine the instrument and to register any remaining errors in it and its mounting. His next project was to use the quadrant to measure the positions of almost 1000 stars in the zodiac, and to prepare a catalogue of them. The necessity of such an undertaking must have appeared to him when he was using occultations of stars by the moon to improve his lunar tables, because then he had found significant inconsistencies in the tabulated

30 Arthur Alexander suggested this in [Rigaud, 1832], introduction to the 1972 reprint, p. ix. Bradley's comparisons were archived as RGO 3/33, and they inspired Bradley to improve Mayer's `rak` tables.

31 Curiously, Mayer wrote about his Jupiter success to Euler in February 1755, before the delay of his lunar theory was evident and long before its completion. It is not to be expected however that his Jupiter theory accounted for the great inequAlity of Jupiter and Saturn other than through an empirical equAtion (perhaps a secular one). The research into the Mars orbit may be dated a little later, for he read a memoir on that subject to the Academy in April 1756. Wilson mentions Jupiter tables in the *Connoissance des Temps* for 1763 that were based on Mayer's formulae, but I have not verified his claim [Wilson, 1985, p. 66].


Figure 3.1: A portrait of Tobias Mayer.

positions of the stars according to Flamsteed, Rømer, and others. He completed the project, but the catalogue was only published posthumously.[32]

It was by that time well enough known that the stars are not as fixed with regard to their mutual positions as had before been supposed; Halley had announced their proper motion in 1717. By a comparison of his own star positions with the well-documented data of Ole Rømer of half a century earlier, Mayer hoped to be able to detect a pattern in the proper motions from which to conclude the direction of motion of the entire solar system, just like a man walking through a forest perceives the trees as moving, in his own metaphor. He could not decisively discern such a pattern, but the idea was later picked up and developed further by William Herschel, among others.

Of Mayer's further research, his investigation of the variation of the thermometer must be mentioned, which we will return to in section 9.3. Other investigations

32 The catalogue was included in [Mayer, 1775], as was a treatise on the investigation of the quadrant. The catalogue attracted attention until at least the end of the 18th century, witness Auwers' re-reduction of Mayer's data [Auwers, 1894].

included a theory of colour mixing, of earthquakes (of interest after a quake shook Germany on 18 February 1756, when Mayer was still checking the mounting of his new mural quadrant), and of magnetism.[33] Mayer regularly read reports of his research to the Academy; abstracts were published in the *Göttingische Anzeigen von gelehrten Sachen*.

Meanwhile, Europe was engaged in one of its bitterest fights, the Seven Years' War (1756–1763); since the summer of 1757 Göttingen was on and off occupied by French troops. Conditions of living and working in the city deteriorated. The astronomical observatory was built on top of a tower which happened to be now also used as a powder-magazine. A similar tower blew up killing seventy people, whereafter Mayer reduced his habitual nightly visits—with a lantern in his hand—to his working place, in all likelihood convinced that it was his task to observe the stars, and not to join them. But performing his other duties was not easy either, with officers lodging in his house and lecture halls full of army supplies.

His death came in February 1762, just after his 39th birthday, not by sudden explosion but after a prolonged illness. He left a wife and four children, who lived under a high mortgage in a half-demolished house in a nearly bankrupt city. Unfortunately for them, the Board of Longitude had still not reached a final conclusion regarding either Mayer's claim for the Longitude, or John Harrison's almost simultaneous claim based on his timekeeper.

## 3.8 THE BOARD'S DECISION AND BEYOND

One of Mayer's last wishes was that the latest version of his lunar tables (alias `rede`) be sent to the Board of Longitude, together with the results of sea trials of his method by his pupil Carsten Niebuhr that had reached him just before his death.[34] These papers were forwarded more than a year later.

Meanwhile, Captain Campbell had been ordered to try Mayer's method of longitude determination at sea. Campbell disapproved of Mayer's repeating circle as being to cumbersome to operate, consequently the marine sextant was developed as a Hadley octant with an extended arc and engineered to a higher degree of precision. Although Campbell left the necessary lengthy calculations for finding the longitude to Bradley on shore, the tests showed that longitude could in principle be found.

A copy of the `kil` tables was also in the possession of Maskelyne, when he went on a sea trip to Saint Helena in 1761, principally to observe the upcoming

33 The treatises on temperature and colour mixing were also included in [Mayer, 1775]; Mayer's theory of the magnet was first published in [Forbes, 1972, III].

34 Carsten Niebuhr was a talented student of Mayer's. He went as a surveyor with a disastrous Danish expedition, planned by Michaelis, to Arabia Felix, and he was the only one to survive. The party travelled from Denmark to the Middle East by ship and Niebuhr performed his lunar distance trials underway. He transmitted the results of the sea trials by letter to Mayer. For further reading on this fascinating expedition, see [Hansen, 1964]. Niebuhr's reminiscences of his study period, recounted in various contributions to Von Zach's *Monatliche Korrespondenz*, form a valuable source of information on Mayer. The Niebuhr results are included in [Mayer, 1770, p. cxxvi].

transit of Venus from that island. While underway, Maskelyne used the lunar tables successfully to experiment with the lunar distance method of longitude determination, and he taught the method to the ships' officers on the way home. Upon his return he prepared and published the *British Mariner's Guide*, a manual of the lunar distance method for seamen, with the `kil` lunar tables appended. Gael Morris had adjusted the epochs of the tables to the meridian of the Greenwich Observatory, whereas Mayer's were based on the meridian of the Observatory of Paris.[35]

Likewise, the Abbé Nicholas Louis de Lacaille had experimented with lunar distances in 1753 on the way home from his outpost at the Cape of Good Hope, though with less accurate lunar tables. Perhaps he used the tables of Lemonnier, which we will discuss in chapter 6. Lacaille expounded a method to compute longitude from an observed lunar distance in the *Connoissance des Temps* for the year 1761, and Mayer's `kil` tables found their way into that volume, too.[36]

With the war drawing to a close, the Admiralty saw fit to submit Mayer's and Harrison's methods of longitude finding to a final test at sea. The necessary arrangements were made and the tests performed on a voyage to Barbados in the West-Indies and back, with—as might be expected with so much at stake—considerable quarrels which need not be mentioned here.[37]

Eventually, in an illustrious meeting held on February 9, 1765, the Board of Longitude advised to the British Parliament that both John Harrison (now over 70) and Tobias Mayer (already deceased, in the same year as Bradley, Lord Anson, and Lacaille) should be rewarded for their outstanding contributions to the longitude problem. The former was to receive £5000 or half the maximum reward, because although his chronometer had performed within the limits stipulated for the maximum reward, it was not yet clear how to produce enough copies of it for a whole fleet. Mayer's widow received £3000: her late husband's method was accurate enough to achieve the lower limit of 60 nautical miles set by the Longitude Act, but the necessary calculations were rather prolix, and the instrument that Mayer had proposed was considered less suitable than the newly developed sextant.

An unexpected reward of £300 went to Leonhard Euler for his alleged contribution to Mayer's lunar theory. Perhaps the British Parliament reached this decision after protests of Clairaut that not Mayer, but Euler and himself had conceived that theory.[38] If so, their argument can now be seen to be false, because I argue that Mayer's tables depended less on that theory than was apparent in those days.

At the same meeting of the Board in February 1765, Nevil Maskelyne, who was appointed to the office of Astronomer Royal only the day before,[39] was charged with

35 [Maskelyne, 1763], [Maskelyne, 1761]; see also [Howse, 1989, Ch. 3–5]. Howse's remark (p. 42) that Maskelyne published the `kil` tables are easily disproved by Maskelyne's own comments in [Maskelyne, 1763, p. 123].

36 [Lacaille, 1759].

37 See the standard literature on this topic, e.g., [Andrewes, 1996], [Forbes, 1975], [Howse, 1980].

38 E.g., [Forbes, 1975, p. 124], [Clairaut, 1752b, 2nd ed., p. 102], quoted on page 53. Clairaut also made a weak attempt to fix the Board's attention on his own lunar theory as a worthy alternative to Mayer's, in a letter directed to John Bevis (*Gentleman's Magazine*, May 1765, p. 208).

39 Maskelyne succeeded Nathaniel Bliss, who had succeeded Bradley after his death in 1762.

a project of his own invention: namely, the production of tables and computational tools to assist the mariner in finding his longitude by the lunar distance method. Maskelyne planned to produce two volumes of tables. One volume would contain an almanac with ephemeris data to appear annually, the other the permanent tables for refraction, parallax in altitude, tables of logarithms of trigonometric functions, and various tables necessary for the reduction of lunar distances.

The novelty in the almanac would be the inclusion of precomputed lunar distance tables showing the angular distances of the moon from the sun and selected stars for every three hours of local time on the meridian of the Royal Greenwich Observatory. Lacaille had proposed such tabulations, and example tables covering a fortnight had been appended to his exposition of the method of lunar distances in the *Connoissance des Temps*;[40] they would take away most of the computational burden of the navigator wishing to find his longitude by lunar distance. Maskelyne's project materialized in *The Nautical Almanac and Astronomical Ephemeris* and *Tables Requisite to be Used with the Astronomical and Nautical Ephemeris*; both first appeared in 1767. The Nautical Almanac is still in production today, albeit under a revised name and with contents adapted to more modern needs.

During the first decades of its existence, the lunar tables in the Almanac were computed from `rede`, Mayer's last manuscript tables, with (from 1777 on) several improvements and additions by Charles Mason (formerly an assistant of Bradley). Maskelyne prepared the `rede` tables for the press, and they were published with revised epochs, three years after he published Mayer's account of the lunar theory. Other authors adapted these tables for their own publications, e.g., Pilgram [Hell and Pilgram, 1772], Lalande in the second edition of [Lalande, 1764] (the first edition contained Mayer's `kil` tables; Lalande also applied them for the computation of the data in the *Connoissance des Temps*, the French almanac), and Pibo Steenstra [Steenstra, 1771]. Mayer's manuscripts of these tables are now kept in the Royal Greenwich Observatory Archives.[41]

Mason improved Mayer's tables a second time, fitting them to almost 1200 lunar observations of Bradley from the 1750's, and he added tables for eight equAtions that Mayer had included in his theory, but not in his tables. Mason's new tables were still in the multistepped format; they were used, with slight alterations and additions by Lalande, for the almanacs from 1789 to 1808.[42] Mason's tables in turn were improved by Bürg, who added still more equAtions from Mayer's theory and used more than 3200 lunar observations of Nevil Maskelyne to fit the coefficients. He still retained the multistepped format.[43] Mayer's *solar* tables, which were essen-

40 Cf. fn. 36.

41 RGO 4/125, see appendix of consulted manuscripts.

42 [Mason, 1787]. I have not looked into Mason's methods to improve the tables; anyone intending to do so should consult RGO 4/193, 2nd part, containing a draft of Maskelyne's instructions to Mason.

43 The tables, with an introduction by Delambre, are contained in [Delambre, 1806]. Regarding the tables used for the Nautical Almanac, the *Expanatory Supplement* is vague about the period 1809–1812 and maintains that the almanacs since 1813 were based on Bürg's tables of Laplace's theory [Seidelmann, 1992].

tially Lacaille's of 1758 with slight adjustments, were used for the almanacs from 1767 to 1804.[44]

The lunar distance method was, for most navigators, the only way of longitude determination at sea until chronometers were produced in sufficient quantities and their prices dropped, in the second quart of the 19th century. During most of the 19th century 'lunars' and chronometers co-existed, the former, when handled by a skilled observer, capable to provide a check on the rate of a chronometer during long voyages.

Although his tables were gradually replaced, two important aspects of Mayer's work persisted. Their multistepped format survived into the early years of the nineteenth century before it was abandoned. The principle of amassing data to fit parameters, on the other hand, is now firmly established in scientific practice.

44 [Forbes and Wilson, 1995, p. 61], [Seidelmann, 1992]; Wilson argues that Mayer's adjustments to Lacaille's coefficients were no improvement [Wilson, 1980, p. 188].