1. INTRODUCTION

1.1 SUBJECT

Three great mathematicians dominate the history of lunar theory in the middle of the 18th century: Leonhard Euler, Alexis Clairaut, and Jean le Rond d'Alembert. Each of them made a lasting contribution to the theory of celestial mechanics and their results had a broader impact than on lunar theory alone. To name but a few examples, Euler codified the trigonometric functions and pioneered the method of variation of orbital constants; Clairaut solved the arduous problem of the motion of the lunar apogee, thereby dealing a decisive blow to the sceptics of Newton's law of gravitation; and d'Alembert worked out an accurate theory of precession and nutation.

But during the second half of the 18th century, the most accurate tables of lunar motion were those of Tobias Mayer; not because he was better at solving the differential equations of motion, but because he was eager to handle large and conflicting data sets in what could nowadays be called a statistical way. He was a true pioneer in the 'combination of observations', which comprises the handling of observational data in order to infer from them certain quantitative aspects of the physical reality. His tables of the moon's motion had an important application in the determination of geographical longitude at sea, by the so-called method of lunar distances.

What is known of Mayer's work on lunar motion? Alfred Gautier, the 19th century astronomer, included a brief analysis of the contents of Mayer's *Theoria Lunae* in his essay on the history of the three-body problem.[1] Delambre, although full of admiration for Mayer, dedicated only a few lines to it.[2] The 20th century research by Eric Forbes, culminating in a biography of Mayer, has brought about something of a Mayer revival.[3] Forbes has constructed a unifying picture of the man Mayer and his work, after thorough investigation of correspondence, unpublished treatises and memoirs, and published sources as well. He produced an annotated edition of the correspondence between Tobias Mayer and Leonhard Euler[4] which provides intriguing glimpses of developing lunar theories, scientific life in the 18th century, and the relation between Euler and Mayer. Although Forbes' research is extremely valuable for anybody studying Mayer, he went astray in several aspects

1 [Gautier, 1817, p. 66–73].

2 Delambre discusses Mayer's lunar theory in [Delambre, 1827, pp. 442–446], reviewed on page 53 below.

3 [Forbes, 1980].

4 31 letters have been preserved, and they have been translated into English, annotated, and printed thanks to the efforts of Kopelevich and Forbes [Forbes, 1971a].

of Mayer's work on lunar motion: he left most of Mayer's manuscripts on lunar theory and tables uninvestigated and consequently his view on this important part of Mayer's work has to be corrected at some points.

On the other hand, a technique developed by Mayer to adjust a specific mathematical model to available observations is now reasonably well known among historians of statistics as one of the first successful attempts of non-trivial model fitting.[5] But the model is one of lunar libration, not of its orbital motion, although it has been suggested that Mayer applied the same technique to improve his lunar tables. Whether this is true has never been investigated thus far.

Most other commentaries on Mayer's investigations of lunar motion are either based on the sources just mentioned or they are repetitions of frail anecdotes. Thus it becomes apparent that the past 250 years have seen hardly any research into the making of Mayer's lunar tables. Until now we did not know how one of the most important lunar theories of the 18th century came into being. Harrison's work on timekeepers—the core object of the 'rival' method to determine longitude at sea—has attracted more historical research, I suppose partially because the principle of longitudelongitude!geographic, at sea determination using clocks is easier to explain than the lunar distance method, and partially because ticking clocks make more attractive museum exhibitions than almanac pages filled with numbers.

The goal of the present study is to investigate the evolution of Tobias Mayer's lunar theory and tables. A major point concerns the causes of the accuracy of these tables: on several occasions, Mayer had announced that their accuracy was a result of the adjustment of the theory to observations. However, he did not report his procedures in full, neither in publications nor in extant letters, and it remained unknown how he managed to fit a model involving tens of parameters to a corpus of over a hundred observations.

Before one undertakes to fit a model to observations, it is necessary to have a model in the first place. Which was the model that Mayer fitted? Was it the theory contained in his *Theoria Lunae juxta Systema Newtonianum*, which was published posthumously in 1767 by the Commissioners of Longitude in London? Most of the researchers in the history of celestial mechanics have skipped Mayer's lunar theory on the assumption that it is a straightforward derivative of Euler's. However, their assumption is no longer tenable after Euler's and Mayer's theories are compared, as we will do in this thesis, although the former certainly had a strong influence on the latter.

The relation between Mayer's theory and tables is complicated and oblique. It was not, principally, his theory as displayed in *Theoria Lunae* that he adjusted to fit better to the observations. This has not been remarked before. An aim of our study will be to provide a new picture of the relations between theory and tables, as well as between the theory of Mayer and the theories of others.

5 See e.g., [Stigler, 1986, pp. 17–24]; further discussion of the technique will be found in chapter 9.

Most of Mayer's manuscripts have been preserved after his death, including a number of treatises and memoirs most of which have been edited and published by now. The bulk of the manuscripts, however, consist of observations, calculations, (draft) tables, and similar material unsuitable for publication. These papers, as dense with numbers as words are sparse, have hardly been studied, yet the manuscripts contain valuable information about Mayer's process of fitting. The lack of relevant information in published sources, and the abundance of unpublished manuscripts, clearly put the spotlight on the latter. The results of this investigation are brought about mostly through detective work in Mayer's unpublished manuscripts. Though the work is not limited to statistics, it is in the spirit of what Stigler must have had in mind when he wrote:

> In studying the history of statistics, sometimes a question of conceptual understanding can be illuminated by a seemingly trivial computation. When writing in general terms, a scientist may be subject to a variety of interpretations, or seem to endorse a variety of methods of procedure. Yet a close numerical reworking of a statistical application by that same scientist may be revealing. A surprising twist may appear, or an unexpected limitation in scope of analysis may become apparent. Investigations into the exact numerical procedures a scientist followed can be frustratingly difficult, since the details are frequently absent, or errors of calculation or the typographer can confuse, leaving the historian to attempt speculative reconstruction. But when the exact results can be derived by a plausible procedure, by convincingly reconstructing in minute numerical detail what the scientist actually did, there can be an exhilarating sense of discovery and clearer understanding. For example, by reenacting Mayer's study of the geography of the moon, or Laplace's numerical investigation of the lunar tides of the atmosphere, or Quetelet's calculation of the propensities for crime, we gain insight into their conceptual understanding of statistics that could scarcely have been achieved otherwise.[6]

What emerges out of my study, is a drastically changed view of the origins of Tobias Mayer's lunar tables and lunar theory, of their interrelationship, and of their relations to the lunar theories of Euler and—surprisingly—Newton. I also present new insights into Mayer's dealing with random observational errors and related statistical concepts.

1.2 ORGANIZATION

The focus in this thesis is on lunar longitude; its latitude and parallax are almost completely neglected. The perturbations of the latter two coordinates[7] are smaller, and less interesting phenomena are encountered in studying them, notwithstanding that they formed part of Mayer's work.

I have also kept out of this thesis several other aspects of Mayer's work, some of which concern lunar motion. To these aspects belong some very interesting activities of Mayer, such as an investigation of variation of the eccentricity of the earth's orbit, his investigations of refraction and lunar parallax, research on the secular acceleration of the moon, and his critical analysis of some observations claimed by

6 [Stigler, 1999, p. 80]; see [Stigler, 1986] for the examples that he mentions.

7 (Horizontal) parallax can be considered as a coordinate in place of distance, cf. section 2.3.1.

Ptolemy.[8] Inclusion of these aspects could serve to show with how much expertise and exertion Mayer went about the perfecting of his lunar tables, but it would distract from the main questions of their provenance and refinement. I have excluded virtually everything that has to do with instruments or observational practise for the same reason.

With the scope of attention thus limited, still plenty of interest remains. It is divided over the chapters in the following way.

Some, mostly standard, background to the why and how of lunar theory is provided in chapter 2. A short biography of Tobias Mayer is sketched in chapter 3. This biography is based on earlier work of Eric Forbes, but many new details of the development of his lunar research have been added.

Chapter 4 explains how to work with Mayer's lunar tables. Similar explanations are included with the published tables, but those are presumably not readily available to most readers. The chapter is included because some of the particulars of working with the tables play an important role in the sequel. The paramount characteristic of the tables is their multistepped nature, which I will explain there, but which is defined in section 3.5. It will be of interest most of all in chapters 6 and 7.

Mayer's lunar theory, written in 1755, is the subject of chapter 5. I examine under what circumstances it was written, and I point out that it was delayed by difficulties which Mayer found hard to overcome. After a review of his theory, which has been wanting until now, I compare Mayer's theory to those of Euler (1753) and Clairaut (1752). Although either Euler's lunar theory or his similar treatise on the so-called great inequality of Jupiter and Saturn were sources of inspiration, Mayer's theory is certainly not a slight deviation from Euler's, as has been supposed before. Clairaut's theory, on the other hand, most likely had a hitherto unrecognized impact on it.

Mayer's theory leaves two things unexplained. The first of these is the multistepped format of his tables. Chapter 6 links, for the very first time, this peculiarity of Mayer's tables directly to Newton's lunar theory of 1702 (not to be confused with his *Principia*). I show that during 1752 Mayer developed the multistepped format from a reworking of the latter. This is a truly remarkable result because it implies that the role of mathematical analysis in Mayer's successful lunar tables is subtle and in need of reconsideration.

When working out his own lunar theory three years later, Mayer had to justify this multistepped format, and he had to show that it agreed well with theory. This may have been the most formidable obstacle responsible for the delay alluded to above. In chapter 7 I reconstruct Mayer's way out. There I also disprove the benefit that Mayer claimed for the multistepped format.

8 All of these subjects show up in the Euler-Mayer correspondence [Forbes, 1971a]. The pointers to the manuscript sources are (see section 1.3 for explanation of the references): eccentricity Cod. $\mu_{41}^{\sharp}$ pp. 51-52; refraction Cod. μ_{12} fol. 225-9, 236-8, Cod. $\mu_{30}^{\sharp}$; parallax Cod. $\mu_{6}^{\sharp}$, Cod. $\mu_{41}^{\sharp}$ fol. 14-17, 219-224, Cod. $\mu_{30}^{\sharp}$; secular acceleration Cod. $\mu_{41}^{\sharp}$ fol. 1, 2, 117, 155-178 (partly), 368; Ptolemy Cod. $\mu_{41}^{\sharp}$ fol. 145-6.

Also unexplained in Mayer's theory is how he went about to adjust it to observations. He had always been clear that he did so, but most of his procedures have never been disclosed. Yet, the task was a daunting one, because there were more than twenty parameters, and he used well over a hundred observations to adjust them. Not only were the computational tools for such a task nonexistent: even a method or conceptual framework to approach it were wanting.

Mayer's manuscripts reveal that he developed a remarkable technique that is conceptually very close to our understanding of a *spreadsheet*. The quality of his tables depended mostly on his successful application of this spreadsheet technique to the process of fitting of the coefficients. Several aspects of working with these spreadsheets are studied in chapter 8.

The topic of adjusting theory to observations brings up the subject of model fitting, and, related to that, of statistics. Astronomers were a precocious breed where this subject is concerned. Various aspects of Mayer's position in this field, which was but barely emerging in his time, are brought together in chapter 9. Among these is a technique that several historians of science have proposed as Mayer's technique to adjust the tables to observations, namely, the same one that he had exhibited in connection with his research into the libration of the moon. This technique is now known as the *method of averages*. I show that Mayer indeed tried to apply the very same method to the adjustment of tables to observations, and that there are plausible reasons why he failed and abandoned the technique. It is even unlikely that Mayer saw that particular technique as a *method*.

The final chapter brings a revised view of the development of Mayer's lunar tables and theory, based on the results of the previous chapters. The most important new insights are highlighted, and there is also a summary of the open questions.

1.3 CONVENTIONS

This section covers the conventions adhered to in this thesis. The technical terms that occur here, are explained in section 2.3. Unless otherwise stated, I will follow Mayer and adhere to ecliptic coordinates, using Mayer's notation for the four basic arguments:

- ω, longitude of moon minus longitude of sun;
- p, lunar (mean) anomaly (except in chapter 5);
- ς, solar mean anomaly;
- δ, longitude of the moon from the ascending node of the lunar orbit.

Depending on circumstances, these may relate to either mean or true motions. Occasionally, we will need minor modifications of this scheme to accommodate the differences between mean and true motions, as will be made clear in the text. As a fifth argument one might have expected lunar (mean) longitude, but we will have no need to assign a symbol to it. Since lunar true longitude is the holy grail that the tables are supposed to yield, it will never appear as an argument in those tables. We will also employ the following astronomical symbols:

☽, the moon;
⊙, the sun;
⊕, the earth;
☊, the ascending node of the lunar orbit; and
♈, the vernal equinox.

The word *equation* has acquired a meaning in astronomy quite different from, and incompatible with, its usual mathematical significance. Apparently, the words for 'correction' and 'equation' are similar in Arabic, and medieval translators of Arabic astronomical texts have accidentally used the latter where the former was intended by the original writer. To differentiate between the clashing meanings, I will write *equAtion* for the astronomical interpretation as a periodic correction of the mean coordinates. The declashion will be dropped in quotations. A similar distinction exists between mathematical and astronomical *inequalities*. In its astronomical sense, an inequAlity denotes a deviation from uniform motion. InequAlity and equAtion are very closely related concepts, even to the extent that they seem to be not always properly distinguished, and the terms are sometimes mixed at random. The former is associated with deviation from uniform motion, the latter with a mathematical device to model it.

In the past it was customary to measure anomaly of the planets and the moon from the aphelion or apogee. Such was still the case in the 18th century. Cometary orbits, however, once it had been recognized that they were closed ellipses governed by the same law of gravity, obstinately refused to yield to the convention. Due to their elongated orbits, the tiny comets were invisible along the outer part of their orbits, therefore their anomaly was naturally measured from perihelion. Laplace unified the planetary and cometary conventions in the beginning of the 19th century by taking the planetary and lunar anomalies with respect to the perihelion, respectively the perigee. I see no reason to enforce the modern, i.e., Laplace's, convention onto the older astronomers, and I decided to follow their own convention. The difference in anomaly is 180°; therefore, in formulae expressing the equAtion of centre half of the signs in the expressions of the coefficients change.

This thesis is concerned with Mayer's lunar tables, their conception and development. Quite some detailed technical information will show up that is itself best presented in tables. To avoid obtuse and confusing language I have decided that the word 'table' is reserved to refer to the historical specimens, while the ones appearing here in the text are referred to as 'displays'.

'Model fitting' refers to the process of adjusting the parameters in a mathematical model to observations. This is a modern notion which is referred to freely in this thesis. In no way do I intend to imply that Tobias Mayer thought of his activities in terms of 'model fitting' with its modern connotations. Rather, by the term 'model fitting' I will usually refer to the process of adjusting something that we now would call a mathematical model to observations. The mathematical model of lunar motion can take various guises: equAtions, sets of coefficients associated with those equAtions, or tables, i.e, equAtions in tabulated form. The process of fitting may always be assumed to apply to the parameters of the equAtions. Thus, when it is said that Mayer fitted (or adjusted) his tables to observations, it is implied that he adjusted the coefficients of the equAtions.

Mayer's work on lunar tables progressed quickly from about 1750 to 1755, and in this period several versions of his tables followed each other in quick succession. For ease of reference I have assigned aliases to the many different versions of the tables, theories, and the like. I harvested the aliases from the rich vocabulary associated with the often fuzzy boundary of land and water in the Netherlands. Incidentally, the mariner who finds himself in waters where these names are applicable, is advised to guard not his longitude but his soundings. The aliases are set in `this font`. A complete list of all versions that I located in Mayer's manuscripts and printed matter is contained in display A.1 of appendix A.

Mayer's manuscripts in Göttingen are referred to with symbols such as Cod. μ_1. Appendix C contains a list of these manuscripts with references to their classification in the Göttingen University Library. The same appendix lists the consulted manuscripts of the Royal Greenwich Observatory (RGO) archives now kept in Cambridge. References to the manuscripts normally follow the recto-verso convention: fol. 8r is the recto side of folio 8, and fol. 8v is its verso side.