10. CONCLUDING OBSERVATIONS

10.1 GENERAL CONCLUSION

The published treatise of Mayer's lunar theory leaves a number of features of that theory unexplained. The most important of the unexplained features are a justification of the multistep procedure, and an explanation of the process of adjusting its coefficients to observations. Mayer considered each of these features at least as important as the theory itself.

Through an investigation of the manuscripts that Mayer left behind, important new insights into this matter have here been obtained. The manuscripts show that Mayer had adapted the multistep procedure from the structure of Newton's *NTM*. Consequently, the role of the calculus in the coming into existence of Mayer's lunar tables is less preponderant than has hitherto been thought. Regarding the adjustment to observations, the manuscripts show that Mayer had developed an intriguing tool to help him, a tool that has a certain similarity to the modern electronic spreadsheet. To paraphrase Stigler's quotation on page 3: working with these manuscripts has brought exhilarating senses of discovery and clearer understanding, especially when the network of interrelations between the various archive items was uncovered.

10.1.1 Remarks on data analysis

The 18th century witnesses a slow change in the role of observations, from a collection of observed phenomena from which one picks examples at will, to a corpus that in its totality forms part of the protocol of scientific research.[1] Mayer plays a pioneering role in this development. He had a disposition for large and conflicting data sets, and he had such a strong confidence in his technique to fit an astronomical model to observations, that he did not hesitate to export the idea to other disciplines, such as a study of temperature distribution over the earth. His endeavours to adjust the coefficients of his lunar tables went much further than Euler's, Clairaut's, and d'Alembert's.[2]

It is remarkable that Mayer successfully fitted over 20 coefficients to over 100 observations, and at the same time it is characteristic of him. It is remarkable because nothing like it had been accomplished before, and because no theory of errors

1 [Pannekoek, 1951, p. 279].

2 Euler used 13 lunar eclipses to adjust the moon's epoch position, eccentricity, and one equation [Euler, 1753, Ch. XVII]. Clairaut mentioned the possibility of adjusting but hardly changed anything [Clairaut, 1752b, p. 83ff]. D'Alembert's proposal for adjusting coefficients has been discussed on page 136.

or statistics was available off-the-shelf. On the other hand, it is characteristic of him, because the theme of fitting models to data is a regular one in Mayer's work. Both aspects taken together make Mayer an interesting figure in the early history of statistics. The making of his lunar tables provides a rewarding case study, not merely because the tables had important implications for practical astronomy and navigation, but most of all because of the complexity of the task of model fitting that he completed successfully.

The quality of his fitted tables matches the quality of his data. This means that Mayer could only have achieved a significantly better result if he could take more and/or better data into account. However, the spreadsheet technique seems to have been more effective in reducing the outliers, than the errors in the centre of the distribution.

Yet, his spreadsheet tool formed part of a technique, not of a method. Its application was confined to the individual of Tobias Mayer performing one specific task, and Mayer did not carry out the generalization to a method that would allow someone else to fit for example a model of spatial temperature to data.

The so-called method of averages, in the form in which Mayer used it, was not general enough to attack the fitting of the lunar motion coefficients. Mayer made an attempt to apply it in that context, but he soon gave up. It cannot truly be regarded as a method before Laplace generalized it. In the generalized form, it became known as 'Mayer's method'.

Apparently, in the second half of the 18th century an attitude towards data seems to have prevailed in which a *lack of accuracy* (such as the dispersion in the input data at Mayer's disposal) was accepted, by many even recognized as partly inevitable, while at the same time there was a concern for a *loss of precision* that was undue in view of the limited accuracy of the data. Further research could show whether this was ubiquitous and what kind of pre-statistical thinking was behind it.

10.1.2 Remarks on the tables and their accuracy

The difficulty of the lunar theory lay not only in the complexity of the mathematics of the three-body problem, but also in the application of observationally obtained data. It had to be taken into account that the observations contain measurement errors, some of which could be modelled approximately (such as atmospheric refraction), while others were inherently unknown (such as random observational errors); astronomers were among the first to realize this and to develop methods to deal with them. Moreover, it is no easy matter to determine for instance the position of the lunar apogee from observations: not only because the radius vector changes its length slowly within narrow limits, but also because all the other small inequAlities are superimposed on the elliptic motion. 'A major obstacle... lay in the multitude of small inequAlities, which they had no way of beginning to discern,' wrote Curtis Wilson[3]. It takes a variety of skills to produce good lunar tables: an able observer,

3 [Wilson, 1989a, p. 196].

skilful reduction of observations, an understanding of the analytical theory, and a feeling for the handling of data and models. These skills were apparently united in Mayer's person.

Regarding the latter of these skills, he was careful to select observations under favourable circumstances, and he designed procedures and instruments with an eye on the control of errors. He was ready to discard outliers, although not always: the spreadsheet technique weighted the outliers perhaps too heavily. Mayer recognized the fact that random errors tend to cancel one another. Sometimes (e.g., the investigations of the latitude of Nuremberg) he averaged over results obtained from not equally trustworthy data sets without weighing them.

We have seen that Mayer was able to improve his tables using his spreadsheet technique almost to the utmost attainable quality: the residual errors show to us a standard deviation only little larger than that of his data set, and also very near the standard deviation that would result after a least-squares fit. This is remarkable because he seems not to have possessed a method. Yet he wasted much effort in trying to adhere to a level of exactness that was not warranted by the quality of data, as e.g., working the sheets often to $\frac{1}{2}''$.

Mayer's tables fulfilled his proclaimed expectations (errors smaller than $30''$) during his own era, but when they are used for modern day predictions, their accuracy is diluted through the drift in the mean motions, in particular in the mean anomaly (cf. p. 49). Thus, when a minor adjustment of the mean motions were made, the performance of the tables now would equal that in the 18th century.

10.1.3 Remarks on the theory

It must have been difficult for Mayer to write *Theoria Lunae*: his tables relied more on the adjustment to observations than on a theory, and he did not yet have a coherent theory from which they could be derived. Moreover, the multistepped format was not theoretically justified. Yet, a lunar theory was required to back up his claim to the Longitude Prize.

It took him longer than expected to complete *Theoria Lunae*. The reasons for the delay might have been the difficulty to link the single-stepped theoretical solution to the multistepped format of the tables, but further research would be needed to corroborate this.

In the introduction of *Theoria Lunae*, he prudently warned that agreement to observations was the real test of the tables, and that his theory aimed merely at showing that from the theoretical side, no objections against the tables could be raised.

Theoria Lunae has certain characteristics in common with the lunar theories of [Clairaut, 1752b] and [Euler, 1753]: they all treat the main problem of the moon. Mayer's theory differs by the choice of independent variable, and also by the treatment of the latitude, for which his colleagues had used the inclination and the node position. Mayer repeatedly stressed that he did not compute the lunar latitude through the inclination and the position of the node, as others did.

While Euler was engaged in experiments with variation of constants techniques, Mayer's theory has nothing of this kind. On the contrary, his decision to treat the latitude equation in parallel with the longitude equation can be regarded as a move in the opposite direction. The independent variable that Mayer employs, has a tinge of variation of constants, yet there are no signs that Mayer thought of it in that context. We are led to the conclusion that Mayer did not employ the fundamental variation of constants technique that proved to be so successful later in the century.

Another remarkable feature of Mayer's theory is that he manages to control the multitude of terms in the trigonometric series without getting bogged down in a notational nightmare. The influence of Clairaut's lunar theory on Mayer is not clear and may be elucidated through further research.

Mayer's theory may be criticized because it is not completely self-supporting: the determination of constants depends heavily on observationally obtained results, much more so than, e.g., d'Alembert thought appropriate: what the theory is supposed to provide is to some extent put into it. Mayer does not explain this in his text. It is not unlikely that the agreement between his theoretical solution of the differential equations and his lunar tables is a result of precisely this dependence.

10.1.4 Remarks on the multistepped format

Mayer developed the multistepped format of his tables during 1752. The format grew out of a reformulation, with the help of trigonometric functions, of Newton's *NTM* lunar theory, as embodied in Lemonnier's tables. Euler's development of a theory of trigonometric functions had been most instrumental, and the resulting tables were considerably easier to work with than Lemonnier's. But when the multistepped format is considered in the light of trigonometric series solutions of differential equations, then it turns up quite unexpected, because such solutions tend to be single-stepped. In a sense, Mayer's format takes a transitional position between the older models of lunar motion with kinematic elements, and the newer, dynamical, theories that were emerging at the time.

Mayer's change of strategy seems to have been made for pragmatic rather than theoretical reasons. He made this move when his theory provided a disappointing accuracy, while looking at the work of other colleagues did not provide adequate inspiration. His first version of the new design provided an accuracy in the predicted positions of the moon that was four times as good as his earlier theory, and slightly more accurate than Lemonnier's tables. The reason for the surprising increase in accuracy after adoption of the new scheme should be the subject of further research.

The preface to his first printed tables [Mayer, 1753b] offered very little in support of an alleged dynamical theory of the moon's motion. Mayer carefully avoided to assert that he indeed had such a theory, whereas the kinematics of Newton's *NTM* clearly shows through. Mayer's credits to Euler must be understood not in the context of a dynamical lunar theory, but rather in connection with the changing perception of trigonometry which the latter had brought about.

The theory that Mayer later worked out in *Theoria Lunae* yielded a single-stepped solution in the first place, which he transformed into the multistep one that was also given in that treatise. He used an approximative procedure to transform sines-of-sines into sums of sines; this procedure, which can also be found in [d'Alembert, 1756], was apparently not widely known, perhaps because the particular type of problem that it solves does not occur very often.

Mayer's stated motivation for his multistepped format was that it reduced the number of significant terms. In chapter 7, this was proved to be false at least for the later multistepped format with evection separated from the equAtion of centre. It has not yet been investigated whether his argument also fails for the original multistepped format.

Two important aspects of Mayer's work persisted: the multistepped format of the tables survived into the early years of the nineteenth century before it was abandoned, and the principle of amassing data to fit parameters is now firmly established in scientific practice.

The success of Mayer's 1753 (and later) lunar tables depends on a hybrid mix of a dynamical theory based on differential calculus, a reworking of Newton's prescriptions of *NTM*, and model fitting. The influence of the differential calculus on this success is much less significant, and the impact of Newton's 1702 theory much more significant, than has hitherto been assumed.

10.2 FURTHER RESEARCH

A number of items have turned up that deserve further research. Here they will be briefly summarised.

It would be illuminating to compare the standard deviations of Mayer's lunar tables (chapter 6) to those of the various theories of Euler, Clairaut, and d'Alembert. Computing power is nowadays easily available to almost every researcher. This makes it feasible to investigate and compare the accuracy of various historical lunar theories. Examples of such investigations can be found in the current work and (among others) in [Kollerstrom, 2000] and [Thoren, 1974]. But since all these investigations are geared towards their own specific goals, their results are hard to compare. This makes it nearly impossible to tell how good or bad any specific lunar theory performed. A comprehensive study, consistently carried out, of all the past lunar theories should be undertaken in order to provide an authoritative reference.

We have signalled that a discrepancy occurred between the stated accuracy of results and their inherent precision. This discrepancy is found for instance in Mayer's spreadsheet contents and also in the accuracy with which mariners were expected to work their lunar distances (see p. 168). It would be interesting to know how common this phenomenon was and how and when it was renounced. This would further our understanding of scientific attitudes in the era of emerging statistical awareness.

Mayer repeatedly averred that his multistepped format made many terms of his lunar theory negligible. His claim was not substantiated by our investigations in section 7.3. Therefore it is tempting to conclude that the accuracy of his tables was brought about not so much by the multistepped format, but rather by some other factor, such as a particularly happy choice of coefficient values. But before such a conclusion may be drawn, we have to consider if his claim also fails for his original multistepped format, where evection and equAtion of centre appeared conjointly. After all, Mayer's success came with his original implementation of the multistepped format, while we debunked his claim by the final format. Was his claim perhaps true for the original form of the multistepped procedure? If so, for what reason does it fail with the later format? An investigation of these questions is troubled by the circumstance that there appears to be no theory of Mayer's that was transformed into a multistepped format, as in *Theoria Lunae*.

The most important change to the multistepped format was the relocation of evection, which has considerable implications for the coefficients of many equAtions. Mayer calculated the effect on these coefficients on foll. 52–53 of Cod. μ_7. These calculations have still to be studied in order to establish the technique that he used.

In *Theoria Lunae*, Mayer compared the theoretically derived coefficients of the equAtions to the coefficients of his lunar tables. The differences between these two sets were generally smaller than the differences between any of these and any of the lunar theories of Euler, Clairaut, and d'Alembert, as Mayer remarked in the preface of his theory. As we now know, Mayer made use of his table coefficients for the computation of the theoretical coefficients; therefore the question arises whether his theory matches his tables precisely for that reason.

The many lunar theories that have been developed over time, are interconnected in many, sometimes obscure, ways. Clairaut's lunar theory had possibly two direct influences on Mayer: the method of computation of the coefficients, as outlined in section 5.4.9, and the inversion of series. Both were treated to some extent in Clairaut's theory. It is conceivable that Mayer's choice of independent variable was also inspired by Clairaut's lunar theory. There may have been other influences as well.

Turning now to the spreadsheets, we note a puzzling anachronism that is begging for an explanation. The sheets in Cod. $\mu^{\sharp}_{41}$, which are based on the `kil` tables of 1753, look as if they are more primitive and rudimentary than the others, which would indicate that they are the earliest spreadsheets. But some sheets in Cod. $\mu^{\sharp}_{28}$, although they look more evolved, are apparently related to older `put` and `zwin` table versions. In case that the former are indeed the oldest spreadsheets, it must still be explained why Mayer returned to the older table versions in order to fit them with his new technique; but if they are not the oldest, than their primitive appearance must be explained.

So far as the spreadsheets have been investigated, they seem to be used to fit the longitude equAtions only, including the epochs and mean motions. I found no spreadsheets that address latitude or parallax. Analysis of the remaining sheets that have not yet been investigated, might reveal whether Mayer ever fitted the latitude and parallax coefficients with this technique.

In 1751, Mayer had obtained a formula to use the Saros as a tool in predicting lunar positions, as was proposed by Edmond Halley.[4] It is currently not known how Mayer had obtained that formula, nor why he abandoned it. Although the use of the Saros in this way seems to have had little impact on his work, a further investigation of this question might illuminate his approach of the lunar motion problem at that time.

10.3 THE DEVELOPMENT OF MAYER'S LUNAR TABLES: A SUMMARY

Tobias Mayer's lunar tables of 1753 are of complex origin. They incorporated elements of Newton's *Theory of the Moon's Motion* with certainty, elements of a dynamical theory in all likelihood, and they had their coefficients adjusted with the help of observations. They originated certainly not merely from Euler's lunar theory, fitted to observations: neither was the content of Euler's theory known to Mayer at the time of publication of the tables, nor would it provide an inducement for the multistepped format.

Mayer's tables facilitated positions of the moon with a standard deviation of about 45″, so that determination of longitude within the limits set by the Longitude Act became a realistic possibility. Urged by Euler, Michaelis, and other colleagues, Mayer devised a method to find longitude at sea, consisting of improved tables, a description of a repeating circle, and prescriptions of the computation that had to be performed. Late in 1754, these were bundled and sent as a package to the Board of Longitude, whereupon Bradley requested an explication of the principles upon which the tables were based.

Mayer worked on the requested theory confidently during January and February of 1755, but unexpected complications delayed its completion until November. It is probable that difficulties to bring the theory into accord with the format of the tables were responsible for the delay.

Because the Seven Years' War intervened, the Board did not reach a decision before Mayer's death. On 9 February 1765, the Board proposed to British Parliament that rewards should be granted to the clockmaker Harrison and to Mayer's widow. Parliament decided that Harrison should receive £5000, the widow £3000, and that additionally Euler should receive £300 for his contributions to the theory of the moon's motion. With hindsight, the reward to Euler seems to be not wholly appropriate, because Mayer's success depended less on a sound theory than on an accurate fit of the parameters. Mayer was certainly an admirer of the great man and

4 Mayer wrote about this to Euler on 4 July 1751 [Forbes, 1971a, p. 34].

he learnt many techniques from Euler's writings. A significant source of the confusion concerning *Theoria Lunae* was Mayer himself, who never skipped an occasion to express his debts to Euler.

At the same time, the new Astronomer Royal, Nevil Maskelyne, was charged with the production of an annual Nautical Almanac, containing precomputed lunar distances based on the 'last manuscript tables' which Mayer's widow had sent in 1763. The first Nautical Almanac was produced for the year 1767. Mayer's 'last manuscript tables,' eventually improved and augmented by Mason and Bürg, influenced the Nautical Almanac until the beginning of the 19th century. The tables were published in various formats in publications such as [Mayer, 1770], [Lalande, 1764], and [Hell and Pilgram, 1772].

The multistepped format of the 1753 tables stemmed from the format of Newton's *Theory of the Moon's Motion*, which provided the most successful lunar tables extant during the second quarter of the 18th century, enjoying widespread distribution predominantly through Lemonnier's *Institutions Astronomiques*. Mayer's adaptation produced tables that were more straightforward to use than Lemonnier's. Besides, after modelling his tables in the multistep way, the tables reached a fourfold increase in accuracy over his former tables, without an increase in the number of equAtions. The reasons for this success lay largely in the lucky choice of coefficients, although it may be that its rendering of the Horrocksian mechanism of the variable eccentricity and oscillating apsidal line has meant a significant contribution, too. Mayer's lunar theory in the dynamical sense was still in an unsatisfactory state at the time.

The improved tables of 1754 with which Mayer entered the quest for the Longitude Prize, as well as the last manuscript tables on which the first almanacs were based, incorporated a streamlined multistep format that was easier to work with than the format of the 1753 tables. This change definitely broke the link with the Horrocksian mechanism. At that time, Mayer could well have done away with the multistepped format, which complicated the relation between the theory and the tables. However, Mayer mistakenly averred that it reduced the size of some equAtions to below the limit where they could be neglected.

Because the coefficients had been further adjusted to observations, the standard deviation of predicted lunar positions had come down to about $30''$. To adjust the coefficients, Mayer used over a hundred observations of occultations and eclipses of the seventeenth and early half of the eighteenth century; after 1755 he included perhaps a significant fraction of his own lunar observations. It was an immense operation to 'fit' more than twenty coefficients to over a hundred observations without a statistical method, being guided mainly by numerical and/or astronomical insight. To keep track of the effects of changes to the coefficients, Mayer devised a tool with a functionality similar to modern electronic spreadsheets. The organization of data into these spreadsheets helped him quickly to investigate the effect of amendments to coefficients. His knowledge of the subject matter, combined with his numerical abilities, guided him in the choice of amendments to make. It was not so much

Mayer's observational skill, but rather his careful handling of observations and his numerical abilities that were responsible for the success. His perseverance to use a multitude of observational data, apart from the skilful handling of them, was responsible for the accuracy of his lunar tables. This was a characteristic feature of his work in general. Mayer was a pioneer in data analysis.

The reputedly close resemblance between the lunar theories of Euler (1753) and Mayer (1755) goes as far as the formulation of the differential equations and ends there. Mayer developed an approximative solution to the differential equations in his own way. He used techniques that were modern for the time, and his theory fits in well among the best lunar theories of his time, such as Euler's and Clairaut's. Of Euler's theory we can be sure that it had an impact on Mayer's *Theoria Lunae*; Clairaut's theory may well have had an influence too. It is remarkable that Mayer chose an independent variable that had no physical interpretation.

Mayer's lunar theory does not contain terms of a higher order than may be found in Euler's and Clairaut's theories, nor does it include physical causes absent in those theories, such as perturbations due to the earth's shape. Yet the solution it offers is considerably closer to the true motion of the moon than that of the other theories. It is not evident what causes this success of the theory. The determination of the coefficients in the solution leaned heavily on available empirical knowledge, in the form of coefficients of lunar tables which had already been fitted to observations. This circumstance is likely to have had a positive influence on the apparent accuracy of the solution.

Moreover, Mayer maintained that his tables should be judged by comparison to observations rather than by verification of the theory. Indeed, theory alone was at that time still unable to accomplish the measure of accuracy that Mayer's tables provided.