

The Reception of Kepler's Astronomy in England: 1596-1650.

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ABSTRACT

This thesis attempts to gather all the evidence bearing on the English reception of Kepler's astronomy from the time when Kepler might first have been read up to around 1650, when foreign secondary influences changed the content of English keplerian astronomy. Kepler first attained fame in England in the early 1600s, and from that time, he was in direct communication with English mathematicians and other scholars. As a result, there was an audience awaiting his Astronomia Nova (1609), which contained his first two laws of planetary motion. Some of its readers adopted the first law, which states that the planetary orbits are elliptical. Over the following decade, there was little apparent promotion of keplerian astronomy in England, but in the universities, libraries were beginning to stock Kepler's books, and dons were recommending his writings to their students of astronomy. Thereafter, the assimilation of the first law into English astronomical thought was very rapid, owing to the work of mathematicians in the universities and in London. Some consideration was given also to Kepler's new celestial physics. Until his death in 1630, Englishmen continued to communicate directly with Kepler, for whom they felt fellowship in matters of religion. By 1640, English astronomy was very much keplerian. Kepler's second law of planetary motion, however, was never mentioned directly in this period, and his third, harmonic law was not adopted until, in the late 1630s, Jeremiah Horroxx adopted it and verified it by independent observation. Horroxx was also the first to attempt, to any great degree, to advance the physical foundation of keplerian astronomy. Kepler's astrology did not prove popular, with the exception of some of his forecasts, which were reprinted at the beginning of the turmoil of the 1640s. In 1645, there began a French influence on English astronomy when English astronomers took over, with modifications, the device of Ismaël Boulliaud for circumventing the second law. The second and longer part of the thesis is a collection of six very detailed case studies in which an attempt is made to explicate each man's approach to the new astronomy.

ABSTRACT

This thesis is intended to give an exhaustive account of Kepler's astronomy among the English in the chosen period. There is so little evidence pertaining to this subject that it was possible to bring nearly all of it to bear on the questions asked. The study was restricted to England because, retrospectively, England seemed to be the most important country in the development of astronomical thought in the seventeenth century as a whole, owing to the prominence of Newton. A further limit, of convenience but not entirely arbitrary, was set up at 1650. Kepler's laws of planetary motion were intended to be the chief objects of study, though it was soon found that Kepler's magnetic physics had a history in England, too, and therefore the 'celestial magnetics' also became an object of study.

One obstacle to writing a balanced history was that the evidence survived very irregularly so that the work of a few men was very well documented; some of these were men whose work was known to be important, but in other cases, men whose contribution to the history of astronomy was found to be very limited were much better represented than the former scholars. If the account was to maintain a sense of proportion, then either much interesting material would have had to be cut out, or all these men would have to be the subject of separate case studies. The latter course was chosen for six mathematicians whose studies of Kepler had not yet received a full treatment. Whereas the first part of the thesis is a survey of the ways in which Kepler's ideas were propagated

through the English scientific community and records various reactions to a keplerian astronomy and the ostensible reasons for these opinions, the second part, the case studies, seeks to elucidate each man's response to keplerian astronomy not only by what he wrote on the subject, but by means of his style of scientific thinking, his relationship with Kepler, and his opinions on various aspects of cosmology that are deemed to be relevant to keplerian astronomy. One part is chronological history with interspersed analyses of individual points of view, the other part intellectual biography. The two parts cannot be integrated (except to the extent that the first utilizes some of the facts uncovered in the second), but they are complementary.

The resulting thesis not only gives a more detailed and sharper picture of the place of Kepler's astronomy in England than any hitherto, it also changes our understanding of the process of its reception in innumerable small ways and in one major alteration: in the importance now attached to keplerian physics, and a concomitant change of time for what is to be reckoned as the integration of the first law of planetary motion into English astronomical thought. There was little to be added to what was already known of the years preceding the publication of Kepler's Astronomia Nova, containing his first two laws of planetary motion and the first account of his celestial physics, but there is a slight novelty in the attribution of Kepler's growing fame to his appointment as Imperial Mathematician and the appearance of his book on optics in 1604. As the history progresses, much new material is brought to bear, especially in the case studies, on our understanding of the ways in which the contemporary readers of the Astronomia Nova responded to it. Ultimately these initial responses, favourable and unfavourable, were to prove unimportant

in themselves for the history because, as far as can be found, the views of the early readers were seldom publicised or passed on in print. From the following decade, no new references to Kepler could be found, and the quiescence recorded by previous historians remains a fact, but we can be sure that the new astronomy had not been forgotten.

The readers should notice that at no point is the reception of Kepler's astronomy in England described as a gradual process. In this writer's opinion, the acceptance by the English, even with their selectivity in adopting parts of the new astronomy, was quite rapid after the first decade of dormancy. This view is occasioned by much new research. For example, the discovery, in the hitherto under-employed papers of John Bainbridge, the first savilian professor of astronomy at Oxford, of a lecture on keplerian astronomy and other studied remarks on Kepler by him is a prominent feature in the pattern produced by other freshly considered manuscripts that show that at Oxford, in particular, but also at Cambridge and among the professors at Gresham College, in London, there was increasing interest in Kepler to the point where, at an early stage, some dons were recommending his books to students and were familiar with a wide range of his works. This learned and critical audience did not uniformly accept Kepler's astronomical theories; Henry Briggs, for one, objected to elliptical orbits, and the opinions of such a man were not to be lightly dismissed. Nevertheless, the idea that orbits are elliptical prevailed, and that Bainbridge delivered, probably in the 1630s, his lecture or lectures in which the rudiments of Kepler's astronomy were set forth, confirms this view of the state of English astronomical thought.

Bainbridge's papers provide a clue toward the discovery of the

fate of Kepler's other ideas. Until now, it has been thought that Jeremiah Horrox, working around the end of the 1630s and the beginning of the 1640s, was the first astronomer to take a serious interest in Kepler's physics and to develop them further. Now, however, we know that Bainbridge, although not in any way as original a thinker or as perceptive a critic as Horrox, tried to improve keplerian physics, and when his notes are combined with other contemporary comments on the subject, there is a suggestion that the new physics, although probably not generally accepted, were a point of interest common to English astronomers in general. It would be fitting if, indeed, Kepler's physics were being debated concurrently with his astronomical laws, for in Kepler's conception, they were linked.

It may seem unremarkable that most discussion of Kepler and study of his works took place within the universities and in London, but the documentation given here is a contribution to a scholarly effort of only the past two or three decades to show that scientific activity did indeed take place there, and the argument is novel in not adducing all evidence of scientific work as significant, but in placing values on the individual contemporary contributions to the discussion. Many of those were not of a high standard, but provided a favourable atmosphere for the spread of Kepler's ideas among the more sophisticated.

Institutional science is not, however, to be emphasized at the expense of an understanding of scientific activity elsewhere or of the individual scientists. Unfortunately, there was not enough space to treat of the work of the rural astronomer, Jeremiah Horrox, but in the selection of some case studies, a diversity of approaches to Kepler is described. Six men are considered. Thomas Harriot, a subtle mathematician and keen observer, collaborated

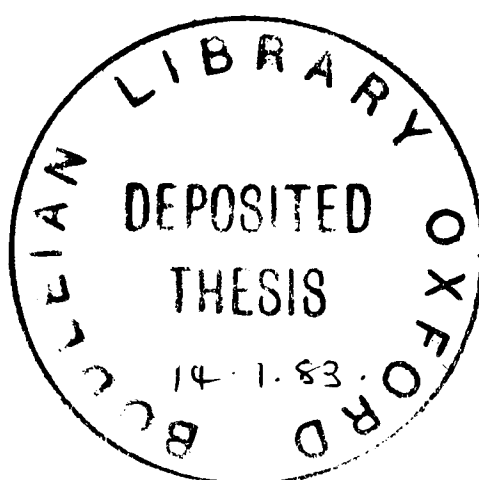
with his friend, Sir William Lower, in working through the Astronomia Nova page by page, correcting arithmetical errors and geometrical blunders, and they adopted the elliptical orbits. Sir Christopher Heydon, a country gentleman and mathematical dilettante, read the book naively but eagerly, even though he was not a copernican, to keep up-to-date with new ideas. Henry Briggs, a careful and skilled mathematician, shrewdly weighed Kepler's arguments and found many of them wanting. He demurred from making Kepler's radical change from the principles of classical astronomy without a surer foundation to replace them. Thomas Lydiat read the Astronomia Nova, or just parts of it, to be able to refute Kepler's arguments for copernicanism, and Henry Gellibrand, a trained astronomer, read the book page by page, like Harriot, but left no comment upon it. But such a summary cannot do justice to the case studies, for much of their significance lies in their details.

By approaching its subject from two directions, so that it is seen first in its breadth, and then in its depth, and by not eschewing precision or analysis in either approach, this thesis, it is hoped, will contribute to an understanding of the progress of theoretical astronomy and of the nature of astronomical thought in renaissance England.

THE RECEPTION OF KEPLER'S ASTRONOMY IN ENGLAND: 1596-1650

Adam Jared Apt

[MT 1982]



To my parents, Charles M. Apt and Frances L. Apt, and the Right Honourable the Lord Bullock, who, in different ways, have enabled me to write this thesis.

PREFACE

In the preparation of any thesis, an author may be expected to incur many debts. Two of mine are especially great, because my subject, Kepler's astronomy in England, has twice before been discussed at some length. That I have written on it again is not a slight against the paper by Father John Russell (on the European response to Kepler), and the thesis by Professor William Applebaum (on Kepler's reception in England to 1687), both learned and clearly written. On the contrary, I am very much aware that I should not have been able to write this thesis without the preparatory work of each, which is still not superseded, the one in its breadth, the other in the span of time it considers. Of course, had I been entirely anticipated, I would not have written this thesis. I found, though, that there was yet a great mass of evidence to be uncovered and to be applied to the same question. This is, I believe, to change our picture of this episode in the history of science both in detail and in its general perspective.

Others have rendered more immediate assistance to me. Dr. John Roche, my supervisor, and Dr. D.T. Whiteside, who have commented extensively and critically on the whole or most of the thesis, caught numerous errors, from the very small to the very large, and suggested many improvements to my discussions. Professor Owen Gingerich supplied several corrections to the chapter on Thomas Harriot (which, I fear, still shows in point of style that it was my first piece of historical writing), and Dr. M.

Feingold, both in conversation and through the medium of his admirable thesis, equipped me with many references, pointed out errors, and, for my chapter III, provided a model of historiography. I have nevertheless arrogantly continued to differ with all of these men on some matters of interpretation, for which I alone am to blame.

Numerous librarians have aided me. I thank, in particular, the ever-helpful staff of Duke Humfrey's Library in the Bodleian.

I feel I must defend my decision to supply English translations of the Latin passages I quote in my text. It may be objected, as Liddell and Scott feared some might object to their use of English translation, that this constitutes 'an unworthy condescension to the indolence of the age'. It is a condescension in part, yes, but it is not unworthy. Many of the passages are couched in very difficult Latin, and English versions must be supplied if the thesis is to be accessible to all who might wish to consult it. But the greater reason is that, as John Selden said, 'Tis good to have Translations, because they serve as a Comment, so far as the Judgement of the Man goes.'

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ABBREVIATIONS

B.L.	British Library
DNB	<u>The Dictionary of National Biography</u>
T.C.D.	Trinity College Library, Dublin
Venn, <u>Alum.</u> <u>Cantab.</u>	John Venn, and J.A. Venn, <u>Alumni Cantabrigiensis</u> , part i: From the Earliest Times to 1751 (Cambridge: at the University Press, 1922), three vols.
Wood, <u>Athen.</u> <u>Oxon.</u>	Anthony à Wood, <u>Athenae Oxoniensis</u> , third ed. with additions by Philip Bliss (London, 1813), four vols.

INTRODUCTION

'Galileo was a great genius, and so was Newton; but it would take two or three Galileos and Newtons to make one Kepler.'
S.T. Coleridge¹

This is not a study of Johannes Kepler, and we will not seek to put a value on his genius. To understand his contribution to the history of science, it is necessary to discover how other men evaluated him and his work, and to find the extent to which they adopted his ideas. This, in small measure, is attempted in this thesis.

The decision to study the response to Kepler's work in England alone was made not merely because the English materials were closest to hand and because the examination of a national, rather than the European response was more easily practicable, but also very much because the English response was especially important. It is generally accepted by historians of science that the major significance of Kepler's astronomical work lies in Newton's derivation, in Book III, 'Of the System of the World', of his Principia (1687), of Kepler's three laws of planetary motion from his own physical theory in order to show that he had produced a

¹ Coleridge, 'Table Talk', 8 Oct. 1830, quoted in Coleridge on the Seventeenth Century, ed. R.F. Brinkley (New York: Greenwood Press, 1968), p. 399.

universal theory of gravitation and motion.¹ But how did the late seventeenth-century Englishman come to know and to adopt a certain ideas of the early seventeenth-century German? What was happening in the intervening years? And did Kepler in fact contribute more to posterity than just his three laws? By answering, in part, the second question, this thesis, it is hoped, contributes towards the answers to the other two.

The decision to study science in a national context, rather than in Europe as a whole, may be queried. A full reply is not necessary here, because it has been supplied by others,² and it will suffice to point to institutional science, such as the savilian professorships, and the far more frequent correspondence and conversation among English scientists than between them and their continental colleagues, however important the latter exchanges may have been; both these factors gave rise to a distinct continuity in English science, and are well-illustrated in at least the second half of the period discussed in this thesis. During this period we can, perhaps, witness the emergence of a national scientific style, in the works of Gilbert, Harriot, Bacon, and many others. In our chosen field this is not easy to illustrate in any detail, and I have not felt warranted to make any explicit comments on it. This thesis, and others like it, may, however, make a contribution towards defining any such English scientific character.

It was necessary, for reasons of economy, to place a further

1 I.B. Cohen, 'Kepler's Century: Prelude to Newton's', Vistas in Astronomy, xviii (1975), 15.

2 Maurice Crosland, 'History of Science in a National Context', The British Journal for the History of Science, x (1977), pp. 95-113.

limit on the scope of this thesis. Therefore, it is confined to the period during which Englishmen acquired their knowledge of Kepler's work primarily from his own books, correspondence, and emissaries, that is, up to approximately 1650, after which time, although Kepler's books were still read, secondary sources, in particular, the Astronomia Philolaica (1645) of Ismaël Boulliaud, were more influential.¹ The exact year, 1650, is a choice of convenience and will not be treated as an absolute limit.

Kepler wrote on a wide variety of topics, from snowflakes and optics, to astronomy and astrology, and he is regarded as having made lasting contributions to several fields. As has just been said, we will be concerned throughout with the work for which he is most famous, that which he did in astronomy. Since much of his work grew out of his unifying world-view; because a book about optics could give a preview of his astronomical theories; and because the presence of any of Kepler's writings in a particular library or citation may tell us something about his fame, reputation, and about the relative estimation of the value of each of his works, Englishmen's readings of all of Kepler's books, and all of Kepler's correspondence with Englishmen on all topics will be looked at, but, especially in Part II, emphasis will be placed on familiarity with his main astronomical works, the Astronomia Nova, the Harmonice Mundi (together containing the three laws of planetary motion), and the Epitome Astronomiae Copernicanae (containing all three laws), and on analysis of reactions to the ideas expounded therein.

In order to understand the English response to Kepler, one must have some knowledge of what Kepler did, so there will now

¹ See below, pp. 84-85, 90-91.

follow the briefest possible account of Kepler, his life and works. As one might expect of a scientist whose works fill, in their modern edition, twenty-two folio volumes (excluding many manuscripts), the ideas and issues expounded are very complex and often technically intricate. Fortunately, the English response to Kepler in our period and the surviving evidence for it are such that a long exposition of Kepler's work and of its technicalities is unnecessary. The emphases in this account are unusual, but this is necessary to make sense of the rest of this thesis.

Johannes Kepler (1571-1630) was a mathematics teacher in Graz when he produced his first important book, the short Mysterium Cosmographicum (1596). In it, he set forth what he believed to be the scheme whereby God had constructed the universe. His neoplatonic conception was that the spacing of the planets in the copernican scheme -- and Kepler believed firmly in the heliocentric form of the universe -- was determined by the placement of one of each of the five regular solids between each pair of imaginary planetary spheres, the cube between Saturn and Jupiter, and so to the octahedron between Venus and Mercury. Kepler summarized his world-view in a letter of 1595 to his old university teacher of astronomy:

'Now, if the Universe has been created in conformity with the measure of numbers, then it has been done in conformity with the measure of quantities... The spherical shape befits the celestial vault. In effect, the Universe has a double nature: mobile and immobile. The one is in the image of the divine essence considered by itself, the other is in the image of God in so far as He creates, and therefore has that much less significance. A curve has a natural relationship with God; the straight line has relationship with that which has been created. The sphere has a threefold quality: surface, central point, intervening space. The same is true of the motionless Universe: the fixed stars, the Sun, and the aura or intermediate aether; and it is true of the Trinity: the

Father, Son, and Holy Ghost... There are six bodies in motion around the Sun. Therefore, the Sun which keeps its place, motionless, in the midst of the planets, and which is nevertheless the source of all motion, provides the image of God the Father, the Creator, for creation is to God, as motion is to the Sun. As the Father creates through the Son, so the Sun gives motion in the midst of the fixed stars... But the Sun diffuses and bestows motive power across the intermedium, in which the planets are placed, in the same way that God the Father, considered as Creator, acts through the Holy Ghost, or in virtue of the Holy Ghost. Consequently, it necessarily follows that motion is proportional to the distances.'¹

Because it seems obvious to a modern reader that Kepler was a mystic, it must be emphasized that such a judgement is too simple. As modern scholarship has come to appreciate,² there is unity to Kepler's thought, his assumptions and premises being those of a neoplatonist whose symbolism of the Trinity derives from Nicholas of Cusa (1401-1464), and in his belief that the human intellect can perceive the structure of the universe, he is entirely unlike most mystics, such as his antagonist, Robert Fludd (1574-1637), who argued that one could attain true knowledge of the universe and of God only through an understanding of occult qualities, and that 'quantitative shadows' (umbrae quantitativae)³ obscured this understanding. Kepler was supremely rational, but his assumptions are foreign to us, and his arguments are sometimes weak or demonstrably wrong because of carelessness and error. His English contemporaries noted these peculiarities.

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- 1 Translation from Alexandre Koyré, The Astronomical Revolution, trans. R.E.W. Maddison (London: Methuen & Co., 1973), pp. 153-4.
 - 2 E.J. Aiton, 'Johannes Kepler in the Light of Recent Research', History of Science, xiv (1976), 77-78.
 - 3 W. Pauli, 'The Influence of Archetypal Ideas on the Scientific Theories of Kepler', in C.G. Jung and W. Pauli, The Interpretation of Nature and the Psyche (London: Routledge & Kegan Paul, 1955), p. 196.

Kepler vigorously promoted his new book, and he sent a copy of it to Tycho Brahe (1546-1601), who, though critical of Kepler's 'a priori' arguments,¹ was nevertheless so impressed by the book that he invited Kepler to be his assistant. This invitation was opportune for Kepler, who, expelled from Graz because he was a Lutheran and would not accede to the demand of the encroaching Counter-Reformation that he convert, joined Tycho's establishment near Prague late in 1600. Kepler's Protestantism and the misfortunes to which it led him, especially later in life, were to a large degree the cause of his personal sympathy with the English, and theirs with him. Tycho had, at the time he first met Kepler, assigned him the task of working out an orbital theory for Mars, and entrusted him with his observations of this planet. Within a few days of Tycho's death in the autumn of 1601, Kepler was appointed his successor as Imperial Mathematician to Rudolph II, and thus exalted (but sometimes denied the promised subsidies), he pursued his work on Mars.

Despite Tycho's wish that the orbit be reckoned according to the tychonic model of the universe, in which the planets revolved about the Sun, which in turn revolved about the earth, Kepler held fast to his copernican belief. His aim was to take up the challenge of Petrus Ramus (1515-1572) to produce an astronomy without hypotheses, that is, without arbitrary constructs, an astronomy that followed from irrefutable physical arguments.² His theory was substantially complete by 1605, but the book that

1 Max Caspar, Kepler, trans. C. Doris Hellman (New York: Abelard-Schuman, 1959), pp. 70-71.

2 E.J. Aiton, 'Johannes Kepler and the Astronomy without Hypotheses', Japanese Studies in the History of Science, xiv (1975), 49-71.

proclaimed his results, the Astronomia Nova, was not published until 1609. This large and difficult book not only presents his solution, but describes the tortuous route by which Kepler arrived at it, and does so incompletely. In order to understand the English reaction to it, the book's demands on its readers must be borne in mind. Other scholars have presented adequate accounts of the Astronomia Nova's contents,¹ and no summary will be attempted here. All the same, a few points must be mentioned if only to explain some terms used in the following chapters.

Kepler's first result for the orbit, after laborious calculations, was what he named his 'hypothesis vicaria'. In this, the planet moves on a circle, the plane of which passes through the Sun and inclines to the plane of the Sun's orbit, as Kepler found, at a constant angle. Kepler's referring of the orbit to the position of the true Sun, and not to another, non-physical point, such as Copernicus had used, is sometimes referred to as his 'zeroth' law, for it is the starting point of the new, physical explanation of celestial motion. The centre of the circular orbit, however, was not at the Sun, but at a short remove (relative to the radius). Furthermore, the motion of Mars on its circle was uniform, not with respect to the centre, but with respect to a punctum aequans, a point colinear with the Sun and the centre, but 180 degrees from the Sun with respect to the centre. This model is very similar to that of Ptolemy, with the Sun where Ptolemy had placed the earth. Another of Kepler's innovations was his laborious calculating of the individual distances of the Sun and

¹ Koyré, op. cit., pp. 172-279, is the best one in English, but it is a non-technical summary, supplemented with remarks drawn from Kepler's correspondence, and not a full, critical analysis.

the punctum aequans from the centre, instead of following Ptolemy's bisection of the total distance by the centre ('bisection of the eccentricity'). The result was a disappointment: it accounted for position in longitude, but when the calculations were repeated with a different set of observed latitudes, the orbital parameters that were found were different, and, indeed, agreed with the ptolemaic bisection of the eccentricity. Even then, Kepler found that although the positions of Mars given for the apsides (aphelion and perihelion, the greatest and least distances from the Sun), were correct, intermediate positions were wrong by as much as eight minutes of arc. He had much faith in the accuracy of Tycho's observations, and refusing to ascribe the discrepancy to observational error, he scrapped the theory. He continued to use it to calculate heliocentric longitudes as a standard for his later theories, for which purpose it was very good, but the flaw already discovered showed that it was incapable of producing correct planetary distances.

Kepler worked his way through a number of what he believed to be oval orbits, discarding each one as he found it inadequate, and all the while he was guided, in part, by physical considerations. His physical theory was an object of great pride, but in its novelty and lack of firm grounding, it was accepted by hardly anyone else.¹ This proposed that the Sun emitted the species motrix of the planets, rigid magnetic filaments that swept the planets around with the Sun's rotation -- another keplerian

1 This point will be qualified in Chapters III and IV, but for the European response, see J.L. Russell, 'Kepler's Laws of Planetary Motion: 1609-1666', The British Journal for the History of Science, ii (1964), 16-18.

postulate, and, as it happens, a correct one -- and whose force varied inversely with distance. This accounted for the circular motions of the planets, but, in fact, it was obvious that the planets did not describe circular orbits, or rather, from Kepler's point of view, they deviated from circular orbits. To produce this deviation, he first postulated animal intelligences in the planets, which produced the necessary corrections, but later, as he approached the final form of his theory, he declared, without discarding the intelligences entirely, that the planets were magnets, whose magnetic axes, inclined from the perpendicular to the ecliptic, pointed always in the same direction, and as the planets revolved about the Sun, varied between attraction to and repulsion from the Sun. The idea that all the planets were magnets was, for Kepler, an obvious consequence of the demonstration by the Englishman, William Gilbert (1544-1603), that the earth was a magnet, and Kepler professed his indebtedness to Gilbert's De Magnete (1600).

This slight indebtedness of Kepler's should not mislead one into underestimating the novelty, otherwise complete, of Kepler's physical theory. This together with faults in its reasoning and its appearance as a flight of imagination, may explain the relatively poor reception accorded the 'magnetic philosophy' by other astronomers, even though the aristotelian physical theory of the heavens, incorporating solid crystalline spheres, had been more or less overthrown, at least among astronomers, in the previous generation, and nothing had been established in its stead.¹

¹ William Donahue, 'The Solid Planetary Spheres in Post-Copernican Natural Philosophy', in The Copernican Achievement, ed. Robert A. Westman (Berkeley: University of California Press, 1975), pp. 244-275.

Because Kepler believed planetary velocity to vary inversely with distance, his calculations necessarily entailed working with large numbers of distances. He found that a convenient short-cut in his work was to represent the sums of distances by the areas, or sectors, of the orbit in which they fell. This representation, as he later realized, produced an exact law, that the planet swept out equal areas (defined by radii drawn from the Sun to the orbit) of the orbit in equal times. This is now known as his 'second law', for its exact and concise statement was made only after the declaration of the first law, although it was discovered earlier.

The first law was Kepler's final result for the path of the orbit. It was, that planets move in ellipses. Thus, the circular astronomy, which had been received from antiquity, was overthrown. Ellipses had been introduced into astronomical orbits before Kepler's time, however. Auxiliary ellipses were a consequence of the combination of circular motions in part of Copernicus's lunar theory, as he, himself, recognized.¹ At the turn of the century, the French mathematician, François Viète (1540-1603), incorporated auxiliary ellipses not compounded of circles into his geocentric planetary theory. There exists no indication either that these suggested to Kepler his planetary elliptical orbit, which was conceptually far different, or that these ellipses prepared the way in England for the reception of Kepler's first law. As they are qualitatively different from the elliptical orbit, it is hard even to imagine how familiarity with these

¹ Noel Swerdlow, 'The Derivation and First Draft of Copernicus's Planetary Theory: A Translation of the Commentariolus with Commentary', Proceedings of the American Philosophical Society, cxvii (1973), p. 458, n. 2.

earlier astronomical ellipses could have so prepared the English.¹

In 1609, then, Kepler's first two laws of planetary motion were proclaimed to the world. They presented a difficulty for Kepler's readers quite apart from the opacity and confusions of the Astronomia Nova, which were considerable:² there was no obvious way of applying the laws, especially the second, even if one was persuaded by Kepler that they held. There was an inherent mathematical difficulty in applying the area law in particular, and this found clear statement in what was to become known as 'Kepler's Problem':³ how to draw a line from a given point on the diameter of a semicircle to meet the circumference at another point in such a way that the area is divided in a given ratio. Eventually, various approximate methods were put forward for calculating the planetary positions, and an exact solution of the problem was not found until Wren proposed one in 1658.⁴

1 Noel Swerdlow, 'The Planetary Theory of François Viète: 1. The Fundamental Planetary Models', Journal for the History of Astronomy, vi (1975), 185-208. The connection of the English with Viète through Nathaniel Torporley, once said to have been his amanuensis, is now discounted. A search has failed to turn up any reference in England, in our period, to the Ad Harmonicon Coeleste, the work in which Viète used ellipses, and which is still extant only in manuscript; for more on this, and the first English discovery of the MS. (circa 1642), see Correspondance du P. Marin Mersenne, ed. C. de Waard, xi (Paris: Éditions du Centre National de la Recherche Scientifique, 1970), 307-9, and B.L. Add. MS. 4417, ff. 26^r-37^r.

2 For an example, see O. Neugebauer, 'Notes on Kepler', Vistas in Astronomy, xviii (1975), 781-5.

3 Johannes Kepler, Astronomia Nova (Frankfurt, 1609), p. 300. Kepler thought the problem could not be solved.

4 A.R. Hall, 'Wren's Problem', Notes and Records of the Royal Society, xx (1965), 140-4. For a different solution, by James Gregory (1638-1675), see J.E. Hofmann, Leibniz in Paris: 1672-1676 (Cambridge: at the University Press, 1974), pp. 217-220.

While Kepler had been pursuing his astronomical studies, he had been prosecuting his optical and astrological ones, as well. The former resulted in his Astronomiae Pars Optica (1604), which contained an announcement of the forthcoming Astronomia Nova, and the latter most notably in his De Stella Nova Serpentarii (1606). It is wrong to suppose, as some writers once suggested, that Kepler practised astrology only for financial gain. Astrology was dearer to him than astronomy, and while he believed that there was much nonsense in the subject, he sought to eliminate what he considered to be its foolish and irrational elements and to set the subject on a firm, rational foundation.¹ He was, therefore, vehement in his polemics in defence of astrology, and he tried to propagate his innovations in the field. At least in England, he was not very successful in this.

Shortly after the publication of the Astronomia Nova, Galileo issued his Sidereus Nuncius (1610), announcing to the world his telescopic discoveries, and having sent a copy to Kepler, Galileo received an enthusiastic reply in the form of a printed book, Kepler's Dissertatio cum Nuncio Sidereo (1610), which was followed by a volume that was more or less a companion, the Narratio de Observatis Quatuor Jouis Satellitibus (1611). These non-mathematical books were more accessible to the educated general reader than most of Kepler's works, and the former in particular proved popular.

In 1619, there appeared the Harmonice Mundi, containing a peculiar mix of essays on solid geometry and on harmonics, and including also Kepler's third law, which states that the square of

¹ E.J. Aiton, 'Johannes Kepler in the Light of Recent Research' pp. 78-80.

a planet's period of revolution is proportional to the cube of its mean distance from the Sun. All three laws were set forth clearly in his Epitome Astronomiae Copernicanae (1618-1621, second edition, 1635), which was actually an epitome of keplerian astronomy.

By this time, Kepler was suffering from unhappy vagaries of fortune consequent upon the deposition and death of Emperor Rudolph II in 1612. He eventually found a mean patron in Wallenstein, and all the while, up to his death in 1630, he continued to work and to write. The most important astronomical work of this later period, or the one that at least seems to have been most highly valued by his contemporaries, was the long awaited Rudolphine Tables (1627), which enabled the calculation of precise planetary, lunar, and solar positions. They were not, however, tables of the positions themselves; such tables are called 'ephemerides', and among Kepler's minor publications were number of these.

The corpus of Kepler's writings on astronomy is, therefore, enormous. We will examine the English response to the whole of it, but will concentrate our attention on the three laws of planetary motion, as Kepler's pre-eminent and most enduring achievement. An admonition to the reader is in order, for this involves a slight anachronism. This lies not in the choice of the laws for emphasis, for the historian may look where he will, but in singling them out as 'laws', for their identification as such was not made until later generations.. It is not yet possible to cite a source for naming them as laws, but it is probably of the eighteenth century. Even without calling them 'laws', contemporaries were aware of the first, and eventually, the third, as ideas of prime importance. The second law having been, in a way, born before its time, because it was not useful as long as it could not be the basis

of calculation, is rather more difficult to distinguish in contemporary writings. Nevertheless, the great importance of the problem to which it is applied, the determination of a planet's speed in its orbit, justifies its treatment as a distinct concept and focus of our attention.

The somewhat peculiar structure of this thesis is partly a matter of convenience, but is owing mostly to the irregular pattern of the surviving evidence. Part I is a synthetic view of the entire topic of the reception of Kepler's astronomy in England from 1596 to about 1650 (a convenient termination, as stated earlier, though marked by no one event or publication), in which is surveyed all the surviving evidence, with the exception of that treated in Part II, which in the first part is merely summarized. It happened that several English mathematicians (a label to be understood in the rather broad, contemporary sense, of men skilled principally in the quadrivium), left behind them or were fortunate enough to have had preserved for them and us extensive records of their thought and, in particular, of their considered responses to Kepler's work. While all these men are of the greatest importance for our story, to have discussed them fully in one continuous history would have exaggerated their significance beyond all justification. Therefore, Part II, the longer of the two main parts, treats each of these men in turn, and may be considered a set of case studies supporting Part I. Two men who deserve to be included in Part II are omitted from it altogether: Robert Fludd and Jeremiah Horrox. From both men, a large amount of evidence survives, but Fludd, who was dull and did not, in any case, write directly on astronomy, has already received

proper treatment from Wolfgang Pauli,¹ and Horrox was so brilliant, so intimately tied to Kepler in his thought, and was, in his short life, so prolific, that he cannot be rendered justice in less than another entire dissertation; partial accounts of his work already exist.²

Throughout this thesis, and especially in Part I, emphasis is placed on continuity of thought from one man to another. However interesting one man's views on Kepler may be, they can be of value to later generations only if, of course, he has passed them on, through conversation, correspondence, publications, or lectures. Such evidence often survives (conversation sometimes being recorded), but that the ideas were actually transmitted, and how the reader or listener reacted to them, can seldom be known, and therefore, much of the following discussions depend on arguments of varying plausibility. The problem of transmission among Englishmen is the same as that of the transmission of Kepler's ideas to individual Englishmen, but for the latter problem, the evidence is usually sufficient for an entirely credible answer to be given, and this done in the case studies in Part II. In this part, attempts will be made to place an individual mathematician's response to Kepler's ideas in the context of his preceding scientific work and of the personal characteristics of his approach to scientific thinking, such as, for example, whether he had a love of precise numbers, and whether he was given to flights of speculation. There will not be any broad generalizations or even

1 Pauli, op. cit., pp. 190-240.

2 Wilbur Applebaum, 'Kepler in England: The Reception of Keplerian Astronomy in England, 1599-1687', (State University of New York at Buffalo Ph.D. thesis, 1969), pp. 65-89, gives an account with references to others. Professor Applebaum is writing a book on Horrox.

hints at generalizations about an English national character of science. There is sufficient diversity among the few men we shall consider to render such abstractions extremely difficult.

All known English references to Kepler's astronomy, and most other English references to Kepler, from before 1650 may be found by carefully sifting the full thesis with its notes. There are a few exceptions: Fludd's and Horrox's references to Kepler are cited selectively, as are those of several men of the 1640s, such as Jeremy Shakerley. This comprehensiveness is owing less to the paucity of the evidence (though that is one reason for it), than to a desire to assist future researchers. It is hoped that despite this peculiar feature, the text does not read like a catalogue of information, and that all evidence receives its due, not being honoured simply for existing.

It should be noted that Part III, 'Appendices and Texts', contains several documents that have hitherto been obscure, if not unknown, but that deserve some attention. Here, as in the first two parts, texts quoted from manuscript are given in exact transcription, except that a) the 'æ' and 'œ' digraphs are written as separate letters; b) the capitals 'I' and 'J', which are written indifferently, are here distinguished as in print; c) lineation is not preserved. Editorial interpolations or substitutions are in square brackets, [], except for ... , indicating an omission. In the first two parts, all but the briefest quotations from the Latin are presented in the text in translation, with the originals quoted in the footnotes. The knowledgeable reader will be able to judge whether this writer's understanding of a Latin passage agrees with his own interpretation of it. No elegance is attempted, but some exactitude is claimed

for the translations. Book titles are cited by their familiar names, or, if not well-known, by brief titles, with the full or nearly full titles in the notes. A full title is cited in the text if it supplies a necessary summary of the book's contents.

PART I

THE RECEPTION OF KEPLER'S ASTRONOMY IN ENGLAND:

1596-1650

I. KEPLER'S RISING FAME: 1596-1610

Johannes Kepler was scarcely known to the English when he was just a mathematics teacher in a seminary in Graz. His appointment as Imperial Mathematician, in 1601, made him famous. His first major work, the Mysterium Cosmographicum, though eventually to be found in many English libraries, usually in its second edition (1621), was never a popular book in England. Only Thomas Harriot (1560-1621) is known to have read the book within the first fourteen years of its publication, and he did not record his opinion of it.¹ One Englishman, Edmund Bruce, who lived in Italy, did befriend Kepler while he was still relatively obscure. Bruce was familiar with Galileo and with other Italian savants.² Because Bruce collected scientists, it was probably he who, upon reading the Mysterium Cosmographicum, initiated his correspondence with Kepler, although the first surviving letter of the series was from Kepler to him, in July, 1599. The exchange continued until late 1603.³ Bruce's letters consist for the most part of the gossip of intellectuals, and at one point he suggested the unlikely

1 See below, p. 113.

2 Bruce is entirely unknown except for these letters. He himself said he was English, for he signed himself 'Edmundus Brutius Anglus'.

3 The entire correspondence is to be found in Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1949), xiv, pp. 7-16 (Kepler to Bruce, 18 July 1599), 256 (Bruce to Kepler, 15 Aug. 1602), 441 (Bruce to Kepler, 21 Aug. 1603), 444-5 (Kepler to Bruce, 4 Sept. 1603), 450-1 (Bruce to Kepler, 5 Nov. 1603).

story that Galileo had adopted some of Kepler's ideas. Kepler's replies to Bruce were far more substantial, as were his replies to almost all of his correspondents throughout his life, and were full of considerations of celestial harmonies and descriptions of his current research, including that into Mars' orbit.¹ Kepler did not depreciate Bruce's thoughts, for, after re-reading one of the latter's letters, in 1610, Kepler noted that it presaged his celestial physics; this seems an over-estimate of the value of the brief letter.² Bruce is entirely unknown except for what can be distilled from these letters, and one matter that is consequently indeterminable is whether he had any contact with other Englishmen. At least from the lack of evidence from other sources, it appears that he did not and that he played no rôle in introducing Kepler to England.

Kepler's appointment as Imperial Mathematician was probably what prompted Englishmen to acquire and to read his next major work, the Astronomiae Pars Optica.³ This book, which contained an incidental reference to his work on Mars, by way of advertisement, led Sir Christopher Heydon (15??-1623), an avid astrologer and busy dabbler in astronomy, to write, early in 1605, a letter full of praise to Kepler, to ask for useful observations, for opinions on astrology, and for any comments on the nova in Ophiuchus. Kepler replied with his usual generosity, telling

1 Ibid., especially the letter of 4 Sept. 1603, p. 445.

2 Ibid., p. 451.

3 I have not made a census of ownership of this book, but its many readers included Harriot, Heydon, Briggs, and Bainbridge. The Bodleian Library owns a copy that seems once to have belonged to John Pontois (Savile T. 14.), who acquired many books from John Dee's library, and hence, this copy may have belonged to Dee. I am indebted to Mr. Julian Roberts for this information.

Heydon what the current state of his work was, both on the nova, on which he was shortly to publish a book in Latin, and on Mars' orbit, the theoretical physical basis of which he outlined. He added, too, his thoughts on celestial harmonies, and thus he summarized, for an Englishman, some of his Mysterium Cosmographicum.¹ Heydon was sympathetic with Kepler's work and outlined the latter's ideas on celestial harmonies in a book on astrology,² but nonetheless he rejected them, except for some astrological trifles. Heydon was well-connected with the mathematical community. If he showed Kepler's letter to anyone else, this person was Henry Briggs, who, as he much later made clear in a letter to Kepler, had a much lower regard for the latter's speculations.

Kepler informed Heydon that if he wanted Tycho Brahe's observations, he ought to inveigle Tycho's son-in-law, Tengnagel, then visiting England, into publishing them. We do not know if Heydon ever met Tengnagel, but the very distinguished mathematician, Thomas Harriot (among whose many devoted colleagues was not, as far as is known, Heydon), certainly did, and he greatly impressed Tengnagel and his associate, Johannes Eriksen, who reported to Kepler on the work of this learned man. Kepler then became the suppliant for scientific knowledge, and wrote to Harriot late in 1606, thereby initiating a correspondence that lasted up to, but not beyond, the date of publication of the Astronomia Nova (1609). The discussion, especially from Kepler's side, was wide-ranging, detailed, and profound, but it scarcely touched on astronomy despite, as is clear from the first letter, Eriksen's

1 See below, pp. 165-166.

2 See below, pp. 166-168.

having given Harriot an oral account of Kepler's work on Mars, including the 'celestial magnetics', and despite Kepler's specifically asking Harriot for his opinion of these ideas. From all that we know of the character of Harriot's own scientific work, and from his very oblique answer to Kepler's request for criticism, we may deduce that Kepler's physical theory was distasteful to him. Still, at least this one Englishman knew what Kepler was doing, and was not only expecting but eagerly looking forward to the publication of the Astronomia Nova.¹

England, or Heydon, at any rate, had been forewarned by Kepler of the publication of his earlier book, the De Stella Nova Serpentarii, and this, accessible to a broader public than geometers alone, became a great success in England, especially the astrological underside of English scientific culture,² and, while broadcasting the idea of the mutability of the heavens, added greatly to Kepler's fame. Much of the content of the book is astrological, and for this material, Kepler sought the attention of a singular British scholar: King James. The presentation copy, containing a dedication in which Kepler compares himself to Diogenes recommending his philosophy to the philosopher-king of Plato,³ was accompanied by a letter⁴ that was not merely respectful -- Kepler wrote that he had been told that not since the time of Alphonso of Aragon, sponsor of the Alphonsine Tables, had Europe had such a learned king -- nor just commending his book, but

1 See below, pp. 112-113.

2 See below, pp. 62-63.

3 This copy is now in the British Library. The meaning and aptness of this conceit are not obvious.

4 Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1954), xvi, pp. 103-4 (Kepler to James I, 1607).

also suggesting that if the king read the book carefully, he would adopt a rather more favourable outlook on astrology than he was reported to hold. (He instructed the king to read certain chapters, and told him which he might pass over if he just gathered their meaning from their titles.) Kepler argued that while much nonsense was attributed to astrology, the science itself should not be disposed of simply on account of the malpractices of its practitioners. This was an oft-repeated theme of Kepler's, and to illustrate it, he drew James' attention to the emblem on the title-page, showing a careful hen studiously extracting good seeds from a dung-heap. Kepler's letter was part of a determined effort of his at proselytizing for his reformed, harmonic astrology in England. The year before, in his first letter to Harriot, he had recommended the forthcoming De Stella Nova to him, too,¹ for its presentation of the astrological 'harmonic doctrine', because he had heard that Harriot, also, rejected the claims of astrology. He even suggested to Harriot that someone ought to repeat his ideas faithfully to James, in order to sway his opinion. Despite the efforts of Kepler and the broad dissemination of this book, there is no evidence that he won any converts to astrology, to say nothing of his own harmonic astrology, among the learned mathematicians of England, although professional astrologers in England returned to the book in the early 1640s.

John Donne (1573-1631) was another reader of the De Stella Nova, and as such, he can possibly represent the ordinary educated Englishman who encountered the book, although in the case of Donne, there is no evidence that this book prompted him to read Kepler's

1 Johannes Kepler, Gesammelte Werke, ed. Max Caspar (1951), xv, p. 350 (Kepler to Harriot, 2 Oct. 1606).

later productions.¹ Donne's copy, which survives, is unannotated.² Its main influence on him appears to have lain in its convincing him that the starry heavens were mutable. In his Biathanatos (1644?), he parenthetically attacks 'Aristotles followers' who declare the heavens to be 'inalterable', 'though by many experiences of new Stars, the reason which moved Aristotle seems now to be utterly defeated,' and a note refers the reader to chapter 23 of the De Stella Nova.³ Similar, less explicit references occur in his poetry.⁴ Donne seems otherwise to have been unaffected by Kepler's work and ideas, and probably was not even a copernican.⁵ He did, however, help to spread Kepler's

- 1 Not even the very thorough C.M. Coffin, John Donne and the New Philosophy (New York: Columbia University Press, 1937), can find even a hint of a reference to any other of Kepler's books. The best discussion of Donne and Kepler is by Wilbur Applebaum, 'Donne's Meeting with Kepler: A Previously Unknown Episode', The Philological Quarterly, 1 (1971), pp. 132-4.
- 2 Geoffrey Keynes, 'More Books from the Library of John Donne', The Book Collector, xxvi (1977), pp. 30-32; facing p. 35 is a photograph of the title-page with Donne's signature. I have personally examined the book, now in the John Rylands Library (L. 523. 8K.), and have found no annotations other than a multitude of marginal pencilled ticks, said by Keynes to be characteristic of Donne.
- 3 John Donne, Biathanatos, ed. J. William Hebel (New York: The Facsimile Text Society, 1930), p. 146 (Part 2. Dist. 6. Sect. 8.). The date of composition is unclear.
- 4 'And in those constellations there arise/ New starres, and old do vanish from our eyes', Donne, 'The First Anniversarie: An Anatomy of the World', ll. 259-60 (1611), quoted from John Donne, The Epithalamions Anniversaries and Epicedes, ed. W. Milgate (Oxford: at the Clarendon Press, 1978), p. 29.
- 5 John Donne, Paradoxes and Problems, ed. Helen Peters (Oxford: at the Clarendon Press, 1980), p. 105; the editor points out that a reference to the proximity of Venus to the earth (Problem X, p. 33), makes sense only if construed in a ptolemaic scheme. The 'Problems' were composed between 1603 and 1609 or 1610 (page v.) John Donne, Ignatius His Conclave (London, 1611), was anti-copernican.

fame with a subtly distorted quotation from the De Stella Nova in his anonymously published Ignatius His Conclave (1611): '...Keppler, who (as himselfe testifies of himselfe) ever since Tycho Braches death, hath received it into his care, that no new thing should be done in heaven without his knowledge.'¹ Kepler was offended by this remark, and said as much in his Somnium (1634; in a note written in 1622),² and never knew that he had met the author personally in 1619.

Two more Britons are known to have had some familiarity with Kepler's work by the time of the publication of the Astronomia Nova, and these two were Scots, Thomas Seget (1569 or 1570-1627) and John Wedderburn (fl. 1610), students of Galileo's living in Italy. This is the moment to remark that apart from these men, there is not any indication that Scots knew anything of Kepler before the second half of the seventeenth century, and therefore Scotland did not lie on any route by which Kepler's ideas travelled

1 John Donne, Ignatius His Conclave, ed. T.S. Healy (Oxford: at the Clarendon Press, 1969), pp. 7, 102; there is a parallel paraphrase in Donne's 'Problems', op. cit., Problem VIII. There was a contemporaneous Latin edition of Ignatius His Conclave, which was the one Kepler read.

2 Johannes Kepler, Somnium, seu Opus Posthumum de Astronomia Lunari (Frankfurt, 1634), p. 32: '... author Satyrae procacis, cui nomen Conclave Ignatianum, exemplar nactus erat hujus opusculi; pungit enim me nominatum etiam in ipso principio.' For more on Donne's alteration of the quotation, referred to in the previous note, see Kepler's Somnium: The Dream, or Posthumous Work on Lunar Astronomy, trans. with commentary by Edward Rosen (Madison: The University of Wisconsin Press, 1967), Appendix E, pp. 212-3.

into England.¹ Seget became, for a short time at least, rather close to Kepler. He brought Kepler, in April 1610,² a copy of Galileo's Sidereus Nuncius (1610), and he remained with Kepler for some while during which they made telescopic observations together, as reported in Kepler's Narratio de Observatis Quatuor Jouis Satellitibus (1611).³ He also contributed a series of verses in commendation of Galileo to the Narratio.⁴ He seems to have been of no importance in establishing Kepler's reputation or spreading his ideas in England, but the one Englishman whom he did know was Sir Henry Wotton, who himself showed, some ten years later, the high esteem in which he held Kepler.⁵

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- 1 King James and his eldest son, Prince Henry, may be the only exceptions to this sweeping statement. For some indication of the primitive state of astronomy in the Scottish universities in our period, see J.L. Russell, 'Cosmological Teaching in the Seventeenth-Century Scottish Universities', Journal for the History of Astronomy, v (1974), pp. 122-132, 145-154. That, as Father Russell shows, the examination questions were backward and crude, does not prove that more advanced scientific texts were not being read there, and I have not performed the systematic research that might rule this out. Nevertheless, there is no other indication that Englishmen owed any familiarity with Kepler to discourse with Scots. Likewise, there is no reason to think that Kepler's ideas entered England via Ireland, though they did travel the opposite way, to Trinity College Dublin, thanks to James Ussher.
 - 2 For some of the few details we have concerning Seget, see Edward Rosen, 'Thomas Seget of Seton', Scottish Historical Review, xxviii (1949), 91-95. In his Dissertatio cum Nuncio Sidereo, Kepler speaks highly of Seget, whom he calls a good friend ('noster congerro'); Kepler, Gesammelte Werke, ed. Max Caspar and Franz Hammer (Munich: C.H. Beck, 1941), iv, p. 306. Date of Seget's arrival: *ibid.*, p. 285.
 - 3 *Ibid.*, pp. 320-2.
 - 4 *Ibid.*, pp. 323-4.
 - 5 Wotton succeeded in 1605 in gaining the acquittal of Seget, who was imprisoned in Venice on a charge of libel against a noble; The Life and Letters of Sir Henry Wotton, ed. Logan Pearsall Smith (Oxford: at the Clarendon Press, 1907), i. 68.

Wotton was also the only Englishman who is known to have been familiar with Seget's colleague, John Wedderburn, who is a much more obscure figure than Seget. Wedderburn is known almost only by his one book, his Confutatio (1610)¹ of a book written by Martinus Horky against Galileo's Sidereus Nuncius, and Wedderburn dedicated his book to Wotton. Wedderburn's book, in defence of Galileo, is reported to be laced with supportive quotations from Kepler.² Dedication of a book to an Englishman did not, as we shall see in the case of Kepler's Harmonice Mundi, ensure wide readership for it in England, and the paucity of copies of Wedderburn's book suggests that it was uninfluential.

Nonetheless, it cannot be neglected completely, for it contributes in a small way to our understanding of the extent of Kepler's reputation in England and the degree to which Englishmen, in general, and mathematicians, in particular, were acquainted with his ideas by the end of the first decade of the century. His first book was virtually unknown, but his next two major works were familiar to, even if not always read by, Englishmen, the first of these, the Astronomiae Pars Optica, probably because of his appointment as Imperial Mathematician, and the second, the De

1 Johannes Wedderborn, Quatuor Problematum quae Martinus Horky contra Nuntium Sidereum de Quatuor Planetis Novis Disputanda proposuit. Confutatio. (Padua, 1610). There exists also a small book of Theses Philosophiae (Edinburgh, 1629), for disputation at St. Leonard's College, St. Andrew's University, by a John Wedderburn. The astronomical theses are purely ptolemaic or aristotelian, but this is fitted to the style of such Scottish disputations; see J.L. Russell, 'Cosmological Teaching'. However, Wedderburn is not an uncommon name, and this may not be the same John Wedderburn.

2 I have been unable to find a complete copy of the book either in Oxford or in the British Library. The Bodleian's copy is, in fact, only the first four pages. The Kepler references are mentioned by Coffin, op. cit., p. 121.

Stella Nova, in part because of the fame of the preceding volume, in part because of its deliberate promotion by Kepler. He had personal, first-hand acquaintance with a couple of well-connected English mathematicians and astronomers, and indirect contact with others. His cosmology seems to have been either ignored, or known but rejected. Englishmen hoped for practical advantages from Kepler's work, despite his disclaimers of the importance of these. By the time the Astronomia Nova was published, a small English readership was secured and growing; his virtually contemporaneous Dissertatio and Narratio attracted even more readers.¹

1 We shall see below many references to the Dissertatio. The copy in the British Library (531/1-2. e. 5) has an inscription to William Camden. But this inscribed leaf may actually belong to the next little book with which it is bound.

II. ENGLISHMEN DISCOVER THE NEW ASTRONOMY: 1609-1620

Kepler's personal fame and the publicity he had given his forthcoming book ensured that there were several interested English readers of the Astronomia Nova almost as soon as it was published. John Bill, the bookseller, probably was the London distributor.¹ From late 1609 and throughout 1610, Thomas Harriot (1560-1621), William Lower (1570?-1615), Christopher Heydon (15??-1623), and Henry Briggs (1560-1630) worked through the book. Thomas Lydiat (1572-1646), returning to London in the summer of 1611 after two years of virtual isolation in Dublin, had the importance of the new book impressed upon him by Briggs, and as a learned advisor to Prince Henry, he sent a copy to the prince's tutor shortly thereafter.²

These five men are the only ones in England whom we know to have read the Astronomia Nova when it first appeared. There may, of course, have been other readers, but not many more, for the astronomical community was small, and there were even fewer who might have appreciated the content, if the reactions of these five are representative. All of these men respected Kepler and were

1 Bill was one of the largest sellers of scientific books in London. He was Harriot's major supplier and sold him Kepler's Epitome, among other volumes; John W. Shirley, ed., Thomas Harriot: Renaissance Scientist (Oxford: at the Clarendon Press, 1974), plate 3.

2 Full documentation for this and other unreferenced statements concerning, and quotations from Harriot, Heydon, Briggs, Bainbridge, and Lydiat will be found in Part II of this thesis.

impressed by his rejection of the circular astronomy -- this seems, not surprisingly, to have been the most striking aspect of the work -- but only Harriot and Lower accepted the elliptical astronomy, and they do not seem to have built upon the theory or to have propagated it.

In England, the immediate response was generally affected by two factors: whether the reader was a copernican, and the degree of technical difficulty he found in following the arguments of the book in detail. This latter in itself comprises two factors: the level of mathematical competence of the reader, and certain inherent difficulties in reading the book. (There were two additional preconditions for specific English reactions, which will be discussed below.) Of the five men just mentioned, only Harriot, Lower, and Briggs seem already to have been copernicans. They were also mathematically competent, which is probably more than coincidental, but not necessarily the cause of their copernicanism. They read the book with the greatest attention to detail and surmounted the obstacles that Kepler had clumsily left on the road to understanding his work. Harriot and Lower, and probably Briggs, too, repeated many of Kepler's calculations and, despite finding errors, accepted the need for a new astronomy, though Briggs distrusted the particular new astronomy of Kepler. Heydon, not a copernican and not mathematically competent, struggled through the book nonetheless. It is, perhaps, surprising that he read it at all. He was a country squire who fancied himself as conversant with all manner of new ideas and was a friend and patron of intellectuals, but he was all the same fundamentally conservative and incapable of ingenuity. More an astrologer than an astronomer, he read the book because the author was famous and the book was

advertised as full of innovations. Thomas Lydiat may never have read the entire book. He was so sure of himself in his anti-copernican views that he read only Kepler's introduction solely in order to be able to refute Kepler's arguments against the fundamentalist position on the structure of the universe; Lydiat was a fundamentalist. Later, he browsed through the rest of the volume, and coming upon the penultimate chapter, which was on the length of the year, he, being a learned chronologer, drafted a short tract of animadversions against this passage.

The two other conditions helping to determine the early English response to Kepler were the extent of each reader's adoption of neoplatonic thought and his opinion of Gilbert's magnetic philosophy. There ought to be, but there is not, a study of neoplatonism in England between John Colet and Henry More. Lacking such a study, we cannot generalize about the potential receptivity of Englishmen to Kepler's belief in divine archetypes and to his belief in God as a geometer. All we can say is that none of the five early readers of the Astronomia Nova reveals the least sympathy with this outlook of Kepler's, and no later Englishman (except John Wilkins, who was not a mathematician) paid any attention to this aspect of Kepler's work, despite its being, from Kepler's point of view, fundamental. The approach of these Englishmen to Kepler's ideas was through their empirically determinable consequences, as revealed in planetary positions. Thus, Harriot and Lower tested Kepler's calculations against Tycho's observations, as presented by Kepler, and Briggs and Heydon hoped that the new astronomy would enable better planetary tables to be produced. Briggs, impatient for the Rudolphine Tables, himself calculated some based on Kepler's theory. Astrology was

integral to Kepler's world-view, for he believed that the soul of man acted in sympathy with the celestial harmonies; in contrast, Harriot and probably Lower were at most ambivalent in their views on astrology, and Briggs was outspokenly hostile to the subject. Even Heydon, who, as an active astrologer, might have been sympathetic to Kepler's ideas, rejected Kepler's theory while accepting Kepler's three newly proposed celestial aspects because, so he thought, they worked; in other words, he accepted only that part of Kepler's astrology that he had found empirically to be 'true'. We see, then, that there is not merely a lack of evidence that these men shared the keplerian world-view, but there exists some evidence to show that they did not.

There remain explicit statements that prove that these men did not accept Kepler's 'magnetic philosophy' either. Lower wrote that he 'could not phansie those magnetical natures,' and Harriot probably agreed; Heydon wrote that 'Kepler is utterly destitute of any helpe from Geometrye as well as of y^e reason either of nature or Arte: why y^e planet...should choose [the ellipse]'; Briggs complained that Kepler relied too much on conjecture, and that his 'physical hypothesis' diverged unnecessarily from the well-established ideas handed down from antiquity; and Lydiat, who had developed his own physical theory, based on the Bible, could not have been less concerned with Kepler's magnetic natures, and he considered the attribution of vital or animal spirits to the celestial bodies to be among the errors of the ancients. Not one of these men, apart from Lydiat, presented an alternative physical theory, and by rejecting the putative foundation of Kepler's work, they unintentionally denigrated his aetiological astronomy, which appears so clearly to us as the precursor of later physics.

Thus, when Kepler's Astronomia Nova arrived in England, which it did immediately, two distinguished mathematicians, Harriot and Lower, accepted the proposed concept of elliptical orbits, and perhaps the Area Law (though there is no direct evidence for this), but only because these orbits seemed to them, as to Kepler, best to fit observation, and not because they found Kepler's metaphysical and physical arguments for them compelling. Their lack of commitment to the circular astronomy is therefore remarkable. Another important mathematician, Briggs, recognized that, in the face of Kepler's results, astronomical theory would have to be revised, but not only did he not accept Kepler's arguments for his theory, he also rejected elliptical orbits as a description of reality because they departed from classical astronomical principles, in which he trusted. His objection, on mathematical grounds, is not to be too lightly dismissed, because Mars' ellipse, having a very small eccentricity, is so close to a circle as to permit some hope of fitting the orbit with a combination of circular motions instead, or at least as to leave room for doubt as to the actual shape of the orbit. The belief in the ellipse required a theoretical leap that Briggs was unwilling to make, and was not a fact necessarily obtruded by observations. Other men, notably Heydon and Lydiat, read the book, or parts of it, but their minds were already set in such a way that each knew he could not accept anything contained therein, although Heydon hoped that the theory might be useful, even if it was not a description of reality. Heydon, too, singled out the Area Law as important, but he did not think through its consequences for planetary theory.

All of these men, except Lower, lived through the following decade, yet, as far as we can tell, none of them ever returned to

the theories enunciated in the Astronomia Nova. It would not be an exaggeration to say that Kepler was famous in England; others besides these five knew of him and may, at their instigation, have read the Astronomia Nova. Harriot's circle, for example, included several men who shared his interests. We may infer from Henry Briggs's statement in 1611 that the book was 'praecipuum et celeberrimum' that it was itself famous. Nevertheless, we can find no one at all in England who returned to the book in the course of the next ten years. It would be reckless to attribute this apparent dismissal to any one characteristic of English thought of the time. Did English astronomers in general disapprove of the magnetic philosophy, as did a few we have seen? This is possible, but it seems surprising when we realize that the magnetic philosophy was largely English in origin, and that Kepler freely acknowledged his debt to William Gilbert. Perhaps the most that can be said by way of explanation is that there was a constant interest in the useful and observational parts of Kepler's writings, and that the Astronomia Nova was put to one side as the English astronomers awaited publication of the related, but more practical Rudolphine Tables; we know that this is how Kepler's work was treated by Briggs.

In the next chapter, we shall see how Kepler was gradually received into the universities, beginning in the decade 1610-1620. We shall now examine several other hints that Kepler was far from forgotten in this period. The first reference below is useful as a reminder that the magnetic philosophy did not repel all English philosophers.

Marke Ridley, the most prominent English writer on magnetism after Gilbert's death, followed Gilbert in believing the earth to

be a magnet and to have diurnal motion, and he supported his arguments in favour of the latter proposition thus, in 1613:

'Although these arguments will hardly perswade vs to beleue the earths motion; yet because that it is lately obserued vnto our sences by helpe of the trunckspectakle, both by Galileus and Kepler, famous Mathematicians, that the great body of the globe of Iupiter, being twelue times greater then the Earth, doth turne about in lesse time then a day vpon his axis and poles, who also haue obserued foure Moones, attendant on Iupiter, which moue round about him, the slowest in 14 dayes, the next in seuen dayes, and the rest in shorter time... Galileus and Kepler haue seene the Planet Venus to moue about the Sunne in ten moneths, and to haue her light from the Sunne in this time encreased and diminished vpon her body, as we see the Moone here neerer vnto vs to haue; therefore it being certaine by obseruation, that the globe of Iupiter and the Sunne do turne about their axis and poles, whose materials we know not, we need not doubt that the Earth should haue a circular motion for her great good.'¹

Ridley's unsophisticated reading of Kepler's Dissertatio cum Nuncio Sidereo serves, therefore, only to buttress his arguments and to support his own prejudices.² Still, this passage is useful for us not merely as an indication that this particular book had arrived in England, but also as a suggestion that the English scientific community had not lost interest in Kepler. Samuel Purchas was another one of the very few who mentioned Kepler in print during these years. In his compendious Purchas his

1 Marke Ridley, A Short Treatise of Magneticall Bodies and Motions (London, 1613), pp. 14-15.

2 Galileo did not discover that Jupiter rotated. Kepler, in his Dissertatio, reported that his friend Wacker von Wackenfels speculated that, if Kepler's magnetic celestial physics were correct, then Jupiter must rotate to account for the revolutions of its moons. Ridley had, therefore, probably not read Galileo's Sidereus Nuncius. See Kepler's Conversation with Galileo's Sidereal Messenger, trans. and annotated by Edward Rosen (New York: Johnson Reprint Corporation, 1965), p. 42. I owe this point to Terri Gail Fried, 'Galileo in England' (Harvard University senior thesis, 1979).

Pilgrimage, he cited Kepler among his authorities when he tried to explicate the story of the creation of the world: he acknowledged the work of Kepler as one who claimed that fire existed in the 'Aetherial Throne' and showed that 'new Starres and superlunarie Comets' demonstrated the corruptibility of the superlunary regions.¹

When the year 1618 brought comets, including a very prominent one, to the skies of European astronomers, among the many books written on the phenomena was one by Kepler, published the following year, and as a matter of course, English astronomers turned to it as an authoritative account. Among them was Heydon, who once again was intent upon extracting usable observations from Kepler's work, which he discussed with Briggs, and once again he differed with Kepler over theory, for Kepler proposed that comets moved in straight lines, whereas Heydon believed their paths, like those of all celestial bodies, to be circular.² Another Englishman, John Bainbridge (1582-1643), was induced to pore over Kepler's book, if he had not already done so, at the instigation of Heydon. Bainbridge had also, in 1619, published his own book on the comet, from which it is possible to learn that he had become familiar with Kepler's previously published remarks on comets and even accepted some aspects of his (and Tycho's) early theory, which Kepler altered before 1619.³ That Bainbridge, by the end of the second

1 Samuel Purchas, Purchas His Pilgrimage. or Relations of the World and the Religions..., second edition (London, 1614), p. 8. The passage is not in the first edition of 1613; the second, with its reference to the De Stella Nova Serpentarii, contains for the first time many other references to recent scientific literature. The fourth edition (1626) refers to the De Cometis (1619), as well (p. 7).

2 See below, p. 168.

3 See below, p. 190.

decade of the seventeenth century, was reading Kepler's books for more than just data is significant, as we shall see in the next chapter. For now, let us merely note that some English astronomers tended to use Kepler's books only for the information, and not the theories, to be derived from them, but let us remember, before generalizing about the English response, that we are considering just a handful of men.

There are hints, in fact, that many more Englishmen were aware, in at least a vague way, of Kepler's achievements, for at the end of the decade we are considering, he received two very distinguished English visitors. The first of these was John Donne, who arrived in Linz on 23 October 1619 in the retinue of Lord Doncaster.¹ Kepler described the meeting in a letter in such a way that it is obvious that he did not know that Donne was the author of Ignatius His Conclave. He enlisted Donne's aid in delivering presentation copies of a book, probably the Harmonice Mundi, in England, and entrusted him with a letter. Doubtless, Donne's visit was in part one of homage, and the two men may well have discussed astronomy and cosmology, but of their talk, unfortunately, there survives only Kepler's terse letter.

In late August 1620, Sir Henry Wotton, on a futile diplomatic mission following the outbreak of war, spent a night at Linz, where he called apparently for the single purpose of visiting Kepler. Several months later, he described his visit in a letter to Francis Bacon, who was then still securely lord chancellor. The letter is well known, but worth quoting all the same:

1 Wilbur Applebaum, 'Donne's Meeting with Kepler: A Previously Unknown Episode', Philological Quarterly. 1 (1971), pp. 132-4. Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1954), xvi. p. 215 (Kepler to anonymous woman).

And indeed I owe your Lordship even by promise (which you are pleased to remember, thereby doubly binding me) some trouble this way; I mean, by the commerce of philosophical experiments, which surely, of all other, is the most ingenuous traffic. Therefore, for a beginning, let me tell your Lordship a pretty thing which I saw coming down the Danuby, though more remarkable for the application than for the theory. I lay a night at Lintz, the metropolis of the higher Austria, but then in very low estate, having been newly taken by the Duke of Bavaria, who, blandiente fortuna, was gone on to the late effects. There I found Keplar, a man famous in the sciences, as your Lordship knows, to whom I purpose to convey from hence one of your books, that he may see we have some of our own that can honour our King, as well he hath done with his Harmonica. In this man's study I was much taken with the draft of a landscape on a piece of paper, methought masterly done: whereof inquiring the author, he bewrayed with a smile it was himself; adding, he had done it non tanquam pictor, sed tanquam mathematicus. This set me on fire. At last he told me how. He hath a little black tent (of what stuff is not much importing) which he can suddenly set up where he will in a field, and it is convertible (like a windmill) to all quarters at pleasure, capable of not much more than one man, as I conceive, and perhaps at no great ease; exactly close and dark, save at one hole, about an inch and a half in the diameter, to which he applies a long perspective trunk, with a convex glass fitted to the said hole, and the concave taken out at the other end, which extendeth to about the middle of this erected tent, through which the visible radiations of all the objects without are intromitted, falling upon a paper, which is accommodated to receive them; and so he traceth them with his pen in their natural appearance, turning his little tent round by degrees, till he hath designed the whole aspect of the field. This I have described to your Lordship, because I think there might be good use made of it for chorography: for otherwise, to make landscapes by it were illiberal, though surely no painter can do them so precisely.¹

Here is one of only three explicit references by an Englishman to the Harmonice Mundi before the late 1630s. The book of Bacon's that Wotton intended to present to Kepler was probably the Novum Organum (published October 1620).² Wotton may or may not have eventually presented it, but in any event, he was the only point of

1 The Life and letters of Sir Henry Wotton, ed. Logan Pearsall Smith (Oxford: at the University Press, 1907), ii. 205-6. Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1959), xviii. 42.

2 The preceding part of the letter suggests this book, although it is unnamed.

contact between these two antithetical minds, and, not surprisingly, neither Kepler nor Bacon betrays any acquaintance with the other's ideas, or even knowledge of his person, in his writings.¹

In the course of Kepler's friendship with the English, and theirs with him, there is one very striking anomaly: Kepler did battle with Robert Fludd (1574-1637), an anti-galenical, paracelsan, quasi-rosicrucian London physician. The weapons employed in the battle amount to five substantial publications, but because Fludd's attitude toward Kepler was not only unrepresentative of English thought, but also, as far as we can tell, unique, only an outline of the debate will be presented here.² Still, it is worth noting that, whatever was the intellectual milieu that informed the English response to Kepler, it was not homogeneous, and that no serious astronomer shared, or could have shared, Fludd's views.

Kepler began the fight with a few pages of an appendix to Book V of the Harmonice Mundi, prompted by his reading Fludd's Utriusque Cosmi Maioris scilicet et Minoris Metaphysica, Physica Atque Technica Historia In Duo Volumina secundum Cosmi differentiam

1 Bacon was not even a copernican. He condemned the efforts of the pythagoreans who 'endeavored phantastically to finde [the size of the universe] by Musically proportions'; Graham Rees, 'An Unpublished Manuscript by Francis Bacon: Sylva Sylvarum Drafts and Other Working Notes', Annals of Science, xxxviii (1981), pp. 385, 401.

2 A much fuller account is given by Wolfgang Pauli, 'The Influence of Archetypal Ideas on the Scientific Theories of Kepler', in C.G. Jung and W. Pauli, The Interpretation of Nature and the Psyche (London: Routledge & Kegan Paul, 1955), pp. 190-240. Pauli uses the term 'archetype' in both Kepler's and Jung's entirely different senses, and though he himself never confuses the two meanings, his readers should be warned that he does not make explicit the distinction between the two.

divisa... Tomus Primus De Macrocosmi Historia (Oppenheim, 1617).¹ Fludd's counterattack was the Veritatis Proscenium: in quo Aulaeum Erroris Tragicum Dimovetur, Siparium ignorantiae scenicum complicatur, ipsaque veritas a suo ministro in publicum producitur. Seu Demonstratio Quaedam Analytica (Frankfurt, 1621). Kepler answered with Pro suo Opere Harmonices Mundi Apologia (Frankfurt, 1622),² and Fludd had the last word with Mononchordium mundi symphoniacum seu replicatio ad Apologiam viri clarissimi et in mathesi peritissimi J. Kepleri, adversus demonstrationem suam analyticam nuperrime editam, in qua Robertus validioribus Joannis objectionibus, Harmoniae suae legi repugnantibus, comiter respondere aggreditur (Frankfurt, 1622).³

It is a paradox that the one contemporary Englishman in whose writings neoplatonic ideas found strong expression should have come into conflict with Kepler, but in fact it was the attempts of both to see the universe as a harmonic whole (to state the view most crudely) that led them to regard each other at all; the two were otherwise entirely different, Fludd being a mystic of the deepest (or, rather, most shallow) sort, and Kepler being, at least in contrast, quite rational.

The issues discussed were complex, and the lengthy argument befitted them. They are of scant interest to us. Some flavour of

1 Johannes Kepler, Gesammelte Werke (Harmonice Mundi), ed. Max Caspar (Munich: C.H. Beck, 1940), vi, pp. 373-377.

2 Ibid., pp. 381-457.

3 For comments on Fludd in Kepler's correspondence, see Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1955), xvii. p. 384 (von Schallenberg to Kepler, 14 Sept. 1619), Gesammelte Werke, xviii, pp. 75 (Kepler to Bernegger, 11/21 Aug. 1621), 93-94 (Kepler to Seussius, 15 July 1622), 96-97 (Müller to Kepler, 3/13 Aug. 1622).

the argument may be had from the following extracts, which point up the principal issues. Fludd:

'What [Kepler] has expressed in many words and long discussion I have compressed into a few words and explained by means of hieroglyphic and exceedingly significant figures, not, to be sure, for the reason that I delight in pictures (as he says elsewhere) but because I (as one of whom he seems to hint further below that he associates with alchemists and Hermetic philosophers) had resolved to bring together much in little and, in the fashion of the alchemists, to collect the extracted essence, to reject the sedimentary substance, and to pour what is good into its proper vessel; so that, the mystery of science having been revealed, that which is hidden may become manifest; and that the inner nature of the thing, after the outer vestments have been stripped off may be enclosed, as a precious gem set in a gold ring, in a figure best suited to its nature -- a figure, that is, in which its essence can be beheld by eye and mind as in a mirror and without many-worded circumlocution... For it is for the vulgar mathematicians to concern themselves with quantitative shadows; the alchemists and Hermetic philosophers, however, comprehend the true core of the natural bodies... [Kepler] excogitates the exterior movements of the created thing whereas I contemplate the internal and essential impulses that issue from nature herself; he has hold of the tail, I grasp the head; I perceive the cause, he its effects. And even though his outermost movements may be (as he says) real, nevertheless he is stuck too fast in the filth and clay of the impossibility of his doctrine...'¹

Kepler:

'If you know of another mathematics (besides that vulgar one from which all those hitherto celebrated as mathematicians have received their name), that is, a mathematics that is both natural and formal, I must confess that I have never tasted of it, unless we take refuge in the most general origin of the word and give up the quantities. Of that, you must know, I do not speak here. You, Robert, may keep for yourself its glory and that of the proofs to be found in it -- and how accurate and how certain those are, that, I think you will judge for yourself without me. I reflect on the visible movements determinable by the senses themselves, you may consider the

¹ Quoted from Pauli, op. cit., pp. 195-7, trans. by Priscilla Silz. For Latin text, see Appendix A, below.

inner impulses and endeavour to distinguish them according to grades. I hold the tail but I hold it in my hand; you may grasp the head mentally, though only, I fear, in your dreams. I am content with the effects, that is, the movements of the planets. If you shall have found in the very causes harmonies as limpid as are mine in the movements, then it will be proper for me to congratulate you on your gift of invention and myself on my gift of observation -- that is, as soon as I shall be able to observe anything.'¹

Even in his own field, medicine, Fludd was nearly an outcast: several times, early in his career, he clashed with the College of Physicians because of his anti-galenical (and pro-paracelsan) beliefs.² His dispute with Kepler was, for other Englishmen, irrelevant. It had no effect on astronomical science as practised either in the universities or elsewhere, because no one could both oppose quantification and yet be interested in knowing exact planetary positions.

Had Kepler accepted a suggestion of Sir Henry Wotton's, he might have exerted greater influence on English thought, for Wotton, either on his own inspiration or at the instigation of friends, invited Kepler to leave a continent at war and to come and settle in England. No more than a few days after the visit, Kepler wrote to a friend of Wotton's kind invitation, and explained that his rejection of it was owing to love of his own country and a desire not to appear ungrateful to the emperor.³ (While Kepler was absent from Linz to defend his mother from a charge of witchcraft, there were, in fact, local rumours that he had accepted

1 Quoted from Pauli, op. cit., pp. 199-200, trans. by Priscilla Silz. For Latin text, see Appendix A, below.

2 Article 'Fludd or Flud, Robert, M.D. (1574-1637)' in the DNB.

3 Kepler, Gesammelte Werke, xviii, p. 41 (Kepler to Bernegger, 29 Aug. 1620).

the offer.¹⁾ Nevertheless, Kepler was still pondering the offer several months later, only to turn it down again, for, as he wrote to the same friend:

'You see that the conflagration of civil war burns in Germany... Ought I therefore to cross the sea, whither Wotton calls me? I, a German? Lover of the continent? Dreading the confines of an island? Forseeing its dangers? Dragging along a dear little wife and a flock of children?'²

Despite these protestations, England's affection for Kepler was reciprocated, for in 1619, Kepler followed his own wishes and those of several of his associates in dedicating his Harmonice Mundi to King James, both because, as he had been told, James was attracted to matters astronomical and might, unlike the emperor, reward him, and because the king's son-in-law, who was said to share the interest, was the Winter King of Bohemia, in whom Kepler and his co-religionists placed such great hopes.³

Even without the man himself, England had many of Kepler's books, and the most prominent English astronomers were well-acquainted with these by 1620. They did not accept many of the ideas contained therein, but, perhaps as emphases changed within

1 Max Caspar, Kepler, trans, C. Doris Hellman (New York: Abelard-Schuman, 1959), pp. 256-7.

2 'Flagare vides incendium belli civilis in Germania... An igitur mare transibo, quo me vocat Wottonus? Ego Germanus? Continentis amans? insulae angustias horrens? periculorum ejus praesagus? uxorculum trahens et gregem liberorum?' Kepler, Gesammelte Werke, xviii, p. 63 (Kepler to Bernegger, 5/15 Feb. 1621).

3 Tengenagel, while in England, had arranged for James's approval of the dedication, but it is not clear whether this was during his trip of 1605 or later; Kepler, Gesammelte Werke, xvii, p. 374 (Quietanus to Kepler, 13 Aug. 1619). Caspar, Kepler, pp. 251-2.

the English pursuit of astronomical knowledge, astronomers made increasing use of the practical parts of Kepler's books over the next decade and a half and even began the integration of the first law into their vision of the structure of the universe.

III. KEPLER IN THE SCHOOLS: 1610-1635

The professional astronomical and mathematical community, as far as this can be defined, is associated, in our period, with the universities and with Gresham College, and it is therefore important that we find Kepler's books not only being used by university men and a Gresham man, Henry Briggs, but also being recommended to students. It would not be an exaggeration to say that by the time John Bainbridge, as savilian professor of astronomy, began to lecture on Kepler's astronomy, probably in the 1630s, the integration of keplerian astronomy into English astronomical thought had been accomplished, though not completely.

Some of Kepler's books were familiar to university men from quite early on and were used in the instruction of those studying for the M.A., they being the ones permitted to study astronomy. Edmund Lee, a Cambridge student, recorded in his commonplace book, perhaps between 1607 and 1611, a series of questions to be disputed, with their possible answers, including the question, 'Cur nigra color facilius inflammentur' (sic), for the answer to which he referred to the Astronomiae Pars Optica; he used the same book for arguments concerning the relative strength of the Sun's heat compared with that of the Moon.¹ Occasionally, though, he did

¹ Cambridge University Library Add. MS. 102, ff. 114^v, 245^r; the card-catalogue in the MS. room of the C.U.L. attributes this notebook to Edmund Lee, circa 1607, but no Edmund Lee of that time is listed in Venn, Alum. Cantab., and the reference on f. 209^r to Kepler, Strena seu de Nive Sexangula (Frankfurt, 1611), proves that that note, at least, must have been written no earlier than 1611.

not refer to Kepler where one might have expected him to have done so if he were familiar with more of Kepler's work, as in a discussion of the earth's magnetism.¹ Another Cambridge student, Richard Fletcher, who died shortly after incepting M.A. at St. John's College, Cambridge, in 1611, owned a copy of Kepler's Astronomiae Pars Optica.² And in Oxford, the famous divine, Richard Crakanthorpe (1567-1624), author of a well-known text-book on aristotelian metaphysics, drew on a wide range of contemporary references, including Galileo's Sidereus Nuncius and Kepler's Dissertatio cum Nuncio Sidereo, in a commentary on Aristotle's De Caelo.³ Some students owned, also, Kepler's Narratio de Observatis a se Quatuor Iovis Satellitibus, one copy of which passed from one student to another before arriving in the Bodleian,⁴ and even quite late, in 1650, a student at Trinity College, Cambridge, John Holland, owned a copy of the Epitome Astronomiae Copernicanae.⁵

College and university libraries gradually acquired many of Kepler's books, often as gifts from old students. The Bodleian Library's first catalogue (1605)⁶ listed both the Mysterium Cosmographicum and the Astronomiae Pars Optica, Kepler's most

1 C.U.L. Add. MS. 102, f. 242^r.

2 M. Feingold, 'Science, Universities and Society in England, 1560-1640', (Oxford Univ. D.Phil thesis, 1980), p. 143.

3 Ibid., p. 82.

4 The copy Bodleian Savile G. 18. has a title-page inscribed, 'Jo: Ward Jan [77] 1619' and 'Don: Heylett. Colleg: Eman: [Cambridge] May. 3. 1632'.

5 This copy is in Jesus College, Oxford.

6 Thomas James, Catalogus Librorum Bibliothecae Publicae quam Vir Ornatissimus Thomas Bodleius... Instituit (Oxford, 1605), pp. 341, 611.

important books then published. The next catalogue (1620)¹ added to these his Astronomia Nova, the Dissertatio cum Nuncio Sidereo, the De Nive Sexangula, the Dioptrice, and shortly after this catalogue was published, the Bodleian acquired the De Cometis. The 1635 Appendix to this last catalogue² added the Harmonice Mundi, the Ephemerides Novae Motuum Coelestium (1617-1620), the Ephemeridum ab Anno 1621 ad 1636 (1630), the second edition of the Mysterium Cosmographicum (1621), the Epitome Astronomiae Copernicanae (first edition),³ the Chilias Logarithmorum (1624), the Supplementum Chiliadis Logarithmorum (1625), the Tychonis Brahei Hyperaspistes (1625), and the Rudolphine Tables. In short, the Bodleian acquired all of Kepler's major works as they were published, and by 1635 it had many of his minor works as well. It is surprising, however, that the library did not have the De Stella Nova Serpentarii.

College libraries either bought or were given copies of Kepler's works, if the following examples are representative of colleges in general. In Oxford, St. John's College was given a copy of the Astronomia Nova in 1620 by a student, John Edwards,⁴ and at Trinity College, Cambridge, the library made use of money donated by Sir Michael Stanhope, brother of a fellow, to buy, in 1625, both the Mysterium Cosmographicum and the

1 Thomas James, Catalogus Universalis Librorum in Bibliotheca Bodleiana (Oxford, 1620), pp. 278-9.

2 John Rous(?), Appendix ad Catalogum Librorum in Bibliotheca Bodleiana, Qui Prodiit Anno Domini 1620 (Oxford, 1635), pp. 105-6.

3 The second edition (1635), contemporary with the catalogue, was acquired later. The full list of books appears to include those that were kept separate in the same building as part of the savilian professors' library.

4 Personal examination.

Astronomia Nova. The relative esteem in which these books were held may be gauged by Trinity's delay in buying Copernicus's De Revolutionibus until 1637 and by their not buying the Harmonice Mundi at all during the seventeenth century.¹ Many other colleges owned copies of Kepler's books, which they acquired at times now indeterminable.¹

That some dons other than the full-time astronomers and mathematicians were reading Kepler may be evidenced by the case of Robert Burton (1577-1640), of Christ Church, Oxford, who is distinguished by nothing so much as his wide reading and eclectic quotation. He was well-aware of arguments over the earth's motion; he believed Oslander's preface to Copernicus's De Revolutionibus to be by the author (which fact proves that he had not read the Mysterium Cosmographicum, wherein Kepler revealed the preface's true author)² and himself took an agnostic stance on the theory, noting that 'Digges, Gilbert, Keplerus and others defend this Hypothesis of his in sober sadness.'³ The book of Kepler's that impressed him most was the Dissertatio cum Nuncio Sidereo, to

1 Philip Gaskell, Trinity College Library: The First 150 Years (Cambridge: at the University Press, 1980), pp. 90, 250.

2 See Appendix B, below.

3 There are references to the 'earth's triple motion', assigned to it by Copernicus, in Burton's Philosophaster, ed. W.E. Buckley (Oxford, 1862), p. 35; the editor writes (p. xi), that the play was begun, if not entirely written, in 1606, but was first performed in 1617. Burton refers to 'Copernicus's' preface, in which is attributed to Copernicus the position that his system was not a description of reality, in The Anatomy of Melancholy, sixth. ed. (Oxford, 1651), p. 253, though earlier, he seems to have thought that Copernicus did believe in the reality of the system, as in the first edition (Oxford, 1621), p. 43, and this passage is retained in the sixth edition, p. 46, despite its contradiction two hundred pages after.

4 Burton, Anatomy, first ed., p. 43.

which he referred again and again in his Anatomy of Melancholy;¹ Kepler's suggestion, in that book, that the Moon and other worlds were inhabited, intrigued him. For 'this reason, it is not surprising that, when Kepler's Somnium, elaborating on this theme, was published in 1634, Burton read it and included a reference to it in the next edition of the Anatomy of Melancholy (1638).² He even re-read the Dissertatio, for in a late edition, but not in the first, he cites Kepler's objection to the idea of an infinity of worlds.³ Although he was interested in astrology so deeply as to make a practice of casting horoscopes,⁴ he seems not to have taken note of Kepler's innovations in this field. Nonetheless, he was aware of Kepler's attribution of an animating spirit to the earth;⁵ it is likely that he knew of this from the Epitome Astronomiae Copernicanae (rather than from the Astronomia Nova), which he cites when describing planetary motions, in a passage so vague as to suggest that he did not understand Kepler's planetary theory at all:

'... the Planets, which enterfeire and cut one anothers orbs, now higher, and then lower, as ♂ amongst the rest, which sometimes, as Kepler confirms by his own, and Ticho's accurate observations, comes nearer the earth than the ☉, and is again eftsoons aloft in Iupiters orbe...'⁶

1 Some of the references are implicit, others, explicit, as in the first edition, p. 327.

2 That is, the fifth edition. The same reference occurs in the sixth edition, p. 255.

3 Sixth ed., p. 255; cf. the fourth ed. (1632), where the passage does not occur.

4 See the marginalia in Bodleian 4^o. R9. ART.

5 Burton, Anatomy, sixth ed., pp. 252-3.

6 This passage first appears in the second ed. (1624), p. 215.

Because Burton's library, so far as we know, contained none of Kepler's books,¹ he must have read them in the Bodleian or, possibly, in the Christ Church library, though there is no record of their having been there, either.

At Corpus Christi College, Oxford, the antiquary, Brian Twyne (1579?-1644), who had long had an interest in mathematics, and in geometry in particular, was supplementing his studies of Euclid's Book XI by reading in Book I of the Harmonice Mundi, and this drew him on to read the account of the third law.² Twyne is the only Englishman other than Robert Fludd who can be shown to have read the Harmonice Mundi before the time of Horrox. Twyne's library included other books by Kepler: the Dissertatio cum Nuncio Sidereo, the Dioptrice, and the Narratio.³

Nathanael Carpenter (1589-1628?), of Exeter College, Oxford, who rebelled against what he perceived to be a stultifying devotion to Aristotle in the schools,⁴ read a couple of Kepler's books or, at least, discussed them with his colleagues. He helped

1 See the bibliography of Burton's books in the Christ Church library, Bodl. MS. Top. Oxon. c. 152; the list does, however, include Copernicus, whose book Burton acquired in 1607.

2 Corpus Christi College (Oxford) MS. 254 (on deposit in the Bodleian), ff. 106^r-107^v; I owe this reference to Mr. Mark Haeffner. Twyne refers to Book I of the Harmonice Mundi (f. 106^r), and has copied out verbatim somewhat more than the first page of chapter 3 of Book V. This leads to the presumption that he read the third law, which is contained in this chapter.

3 R.F. Ovenell, 'Brian Twyne's Library', Oxford Bibliographical Society Publications, new series, iv (Oxford: at the University Press, 1952), p. 28. There are more brief references to Kepler at Corpus Christi Coll. MS. 254, ff. 88^v, 89^r, quoted by Mark Curtis, Oxford and Cambridge in Transition: 1558-1642 (Oxford: at the Clarendon Press, 1959), p. 121, n. 16.

4 Richard Foster Jones, Ancients and Moderns, second ed. (Berkeley: University of California Press, 1965), pp. 65-71.

broadcast some news of what Kepler had done, though he did not disseminate Kepler's ideas in their full depth, in his two textbooks, Philosophia Libera (second edition, 1622), and Geographie Delineated (1625).¹ The latter contains a reference to the earth's 'circular reuolution about her owne Poles; which Kepler and Galileus haue obserued aswell in the Sunne, as Iupiter,' thereby revealing Carpenter's slight familiarity with the Dissertatio cum Nuncio Sidereo.² A somewhat different version of this passage occurred earlier in the second edition of the Philosophia Libera.³ In this book, Carpenter, who inclined toward a version of the tyconic theory of the planets in which the earth possessed diurnal rotation, presented a fair but far-too-brief exposition of Kepler's theory of elliptical planetary orbits in a heliocentric system:

'For no greater indication of a Nature is to be found in bodies than Motion; it is therefore improbable that the most splendid bodies are deprived of that common priviledge of Nature, which is given to all others. Accordingly, in the Keplerian System, enough is allowed to suffice for saving the celestial Phenomena; this is so notwithstanding that I take exception to his principles of Nature. 1. Because the aforesaid author acquits his Hypotheses not with whole circles, but with Elliptical Lines, which he calls oval. But in the matter of these great works of Nature, I do not attribute to God any intention except the best: whence it

1 The first edition of Philosophia Libera (Frankfurt, 1621), had considerably less astronomical matter than the second edition (Oxford, 1622); see Jones, Ancients and Moderns, p. 288. I refer to the second edition of Geographie Delineated (Oxford, 1635), which, despite being 'corrected', is unchanged from the first edition (Oxford, 1625).

2 Geographie Delineated, p. 85. Like Ridley, Carpenter made the mistake of attributing to Galileo the observation of Jupiter's rotation. See above, p. 35, n. 2.

3 Philosophia Libera, second ed., pp. 274-5.

seems that the circuits of the stars are to be measured more by whole and perfect circles than by imperfect circles, which detract as much from the integrity of Nature as they are deficient in roundness. 2. All motions reckoned by such lines cannot be mixed and composite, if indeed all motions are either absolutely straight, or absolutely circular, as the sounder Philosophers [say]; but composite and mixed cannot be fit for the heavens, because they are considered the most simple of all bodies. Last comes the Tychonic scheme, which is allowed to approach the truth as closely as possible...'¹

Probably from this book and from Burton's, as well as from conversation with men like Carpenter and Burton, a vague knowledge of Kepler's ideas filtered down to the authors of semi-popular books. The much-travelled George Sandys (1578-1644), in the introduction to his translation of Ovid's Metamorphoses,² attributes to Kepler the theory that comets are 'of a certaine thick matter, encompassing almost alwaies the body of the Sun.'³ John Swan, in his conservative and elementary textbook, Speculum Mundi (1635),⁴ in which he displays his preference for the tychonic system, renders and refutes Kepler's physics thus:

'Kepler...who in good earnest affirmeth and beleeveth that the earth is a great living creature, which with the

1 Ibid., p. 386; see Appendix C, below, for Latin text.

2 George Sandys, Ovid's Metamorphoses Englished (Oxford, 1632); this is the second edition. The first (London, 1626), lacks the introduction.

3 Ibid., p. 12.

4 John Swan, Speculum Mundi, or a Glasse Representing the Face of the World; Shewing Both that It did Begin, and Must Also End: The Manner How, and Time When, being Largely Examined (Cambridge, 1635). 'The arguments [Swan gives against the earth's motion] are the traditional ones of the Aristotelians and the theologians. They reveal the author's lack of any real scientific training and prove him to be merely the recorder of the conservative opinion of his day.' F.R. Johnson, Astronomical Thought in Renaissance England (Baltimore: The Johns Hopkins Press, 1937), p. 277.

mightie bellows of her lungs first draweth in the waters into her hollow bowels, then by breathing respire them out again. A prettie fiction this; and well worthy the pen of some fabling poet, rather then to be spoken in good sober sadnesse, and affirmed as a truth.'¹

The phrase 'sober sadnesse' is a clue to Swan's source: Burton. This suspicion is confirmed by his lifting almost verbatim Burton's description of the planets' cutting of each other's orbs, quoted above, with the identical reference to Kepler's 'Epit. Astron. lib.4.'² Thus was learned opinion of Kepler diffused from the universities through the larger half-educated public.

From 1621, when Henry Briggs and John Bainbridge came up to Oxford as the savilian professors of geometry and astronomy, the universities were prepared to receive some of Kepler's ideas on astronomy, and the intellectual atmosphere was to continue to be favourable to his less hypothetical contributions.³ The 1620s were also, partly in consequence, a period when the intellectual commerce between Britain and Kepler became more equitable: Kepler discovered the usefulness of Nathaniel Torporley's Dicliides Coelometricas (London, 1602),⁴ and, much more important, he had, several years earlier, recognized the worth of the work of the

1 Swan, op. cit., p. 210.

2 Ibid., p. 316.

3 For unreferenced statements concerning Briggs and Bainbridge, full documentation can be found below in Part II, chapters VIII and IX. For a broader view of the receptivity of the universities to new scientific ideas, see M. Feingold, thesis, op. cit. Of course, Kepler did not consider his work to be 'hypothetical'; see above, p. 6.

4 Johannes Kepler, Gesammelte Werke, xviii, p. 192 (Crüger to Kepler, 15/25 July 1624).

Scot, John Napier.¹ He traded notes and books on logarithms and trigonometry with Edmund Gunter (1581-1626),² of Gresham College, and this led Henry Briggs to write to him as well, and to present him with a number of cogent criticisms of his work. John Bainbridge, too, corresponded with Kepler.

Gresham College, at least in the persons of two of its astronomy professors, Gunter and Henry Gellibrand (1597-1636), Gunter's successor, was familiar with some of Kepler's work, though these two probably had little to do with the propagation of Kepler's ideas, because Gresham seems not to have fulfilled its function as an educational institution.³ Gunter's reactions to Kepler's astronomy are unrecorded, and Briggs, himself an earlier Gresham professor of geometry, was highly critical but not dismissive of it. Gellibrand carefully read the Astronomia Nova from cover to cover, but his appreciation of it and his ultimate response to it, favourable or unfavourable, are unknown. It must not be forgotten that these men were members of a larger scientific community that doubtless discussed the ideas, and there is ample evidence that Gunter and Briggs did so.⁴

This was most important in Oxford, where scholars were indeed being taught astronomy, and therefore, John Bainbridge's researches

1 Kepler's work on logarithms deserves a separate study; a brief account may be found in Yu. A. Belyi, 'Johannes Kepler and the Development of Mathematics', Vistas in Astronomy, xviii (1975), 654-7. An early notice of his appreciation of Napier may be found in Kepler, Gesammelte Werke, xvii, pp. 297-8 (Kepler to Mästlin, 3 Dec. 1618).

2 See below, p. 185.

3 Ian Adamson, 'The Administration of Gresham College and its Fluctuating Fortunes as a Scientific Institution in the Seventeenth Century', History of Education, ix (1980), 13-25.

4 See below, p. 185.

into and lectures upon Kepler were not anomalous, but may be seen as marking the complete acceptance of parts of keplerian astronomy, chiefly, of elliptical orbits, into English astronomical thought. Bainbridge, largely self-taught in astronomy, had made the acquaintance of Briggs early in his career and had been invited by Sir Henry Savile (1549-1622; whose own stance on astronomy was that of a non-realist,¹ and who, even if he read Kepler, does not appear to have directed others to his books), to be the first savilian professor of astronomy; from that time, in 1621, Bainbridge had immersed himself in the study of the works of the great astronomers. Kepler figured prominently among them. Some time after 1627, Bainbridge worked up a long lecture on keplerian astronomy, probably from the Epitome Astronomiae Copernicanae, though he cited no source, and the didactic composition is entirely original. In this lecture, Bainbridge not only described elliptical astronomy and worked out, following Kepler, various problems of planetary position, but he had also, first of all, to introduce his audience to various elementary mathematical properties of ellipses, which, apparently, they were not expected to have been taught in separate lectures. Bainbridge's importance can be emphasised further, for he was the first English astronomer who can be shown to have taken a serious interest in Kepler's

1 Carpenter, Geographie Delineated, p. 143: 'Here I cannot but remember a merry answer of that great Atlas of Arts, Sir Henry Savile... Being once invited vnto his Table, and hauing entred into some familiar discourses concerning Astronomically suppositions: I asked him what he thought of the Hypothesis of Copernicus... His answer was; he cared not which were true, so the Apparences were salued, and the accompt exact: sith each way either the old of Ptolemy, or the new of Copernicus, would indifferently serue an Astronomer: Is it not all one (saith he) sitting at Dinner, whether my Table be brought to mee, or I goe to my Table, so I eat my meat?'

physics; he attempted to modify at least one aspect of the dynamics, though in just what way, we cannot say.¹

Bainbridge was succeeded in his position at Oxford in 1643 by John Greaves (1602-1652), previously Gresham professor of geometry.² Greaves's most notable achievements accomplished during his Gresham tenure were his travels to the Near East to examine and to collect oriental manuscripts and to make observations from classical positions in order to check the work of Hipparchus and Ptolemy. Greaves's opinions on theoretical astronomy are unknown, despite his having left several volumes of astronomical notes,³ but the words of Kepler were cited by one of his sponsors, William Juxon (1582-1663), Bishop of London, in justification of his travels: 'This worke I find by the best astronomers, especially by Ticho Brache and Kepler, hath been much desired as tending to the advancement of that science [astronomy].'⁴ This hardly suggests that Greaves, too, advanced Kepler's ideas in England, but it does demonstrate that Greaves was a reader of Kepler (if we accept that Juxon spoke for him), and that his appointments at Gresham College and at Oxford were not, despite Greaves's interests having been tangential to the development and spread of planetary theory, setbacks in the absorption of keplerian astronomy by English astronomers.

1 See below, pp. 195-197.

2 Article 'Greaves, John (1602-1652)' in the DNB.

3 Bodl. MS. Savile 41.

4 Nicholas Tyacke, 'Science and Religion at Oxford before the Civil War', in Puritans and Revolutionaries, ed. D. Pennington and K. Thomas (Oxford: at the Clarendon Press, 1978), p. 84.

A concern for correct planetary positions and a hope that Kepler's work would aid in their determination were, we have constantly seen, of paramount importance for English astronomers. It is not, therefore, surprising that the Rudolphine Tables, of 1627, quickly found niches in English libraries. In Oxford, Henry Briggs was rather disappointed by them, but the reasons for his disappointment can no longer be known. His is the only critical English comment to be found.¹ John Bainbridge, in contrast, made full use of the Tables. The Tables were highly technical and of value only to those prepared to undertake lengthy calculations. Nevertheless, an astonishing number of persons at least referred to them. It may be a measure of their importance in England that even an obscure cleric and astrologer, William Bredon, a thoroughgoing ptolemaist, said to have been Christopher Heydon's chaplain,² was by 1630 using them at his home in Thornton, Bucks., to cast horoscopes for his friends, and that he took them to be the standard against which other astronomers' tables were to be measured. He found Kepler 'very obscure' but 'very exquisite', and

1 See below, p. 186.

2 William Lilly, The Last of the Astologers: Mr. William Lilly's History of his Life and Times... ed. K.M. Briggs (London: The Folklore Society and D.S. Brewer, 1974), p. 29: 'In this Year [1633] also William Bredon, Parson or Vicar of Thornton in Buckinghamshire, was living, a profound Divine, but absolutely the most polite Person for Nativities in that Age, strictly adhering to Ptolomy [his Tetrabiblos], which he well understood; he had a Hand in composing Sir Christopher Heydon's Defence of Judicial Astology, being that time his Chaplain [there is no independent confirmation of these statements]; he was so given over to Tobacco and Drink, that when he had no Tobbacco, he would cut the Bell-ropes and smoke them.'

therefore determined to buy his Ephemerides, too.¹

The Rudolphine Tables later received a boost in popularity by being used as the basis of the Latin Ephemerides (Gouda, 1632), of Adrian Vlacq, a Dutchman.² These were directed toward a broad audience, probably consisting mainly of astrologers. Vlacq was in London some time after their publication, and he made the acquaintance of Gellibrand and through him, of Bainbridge; he had already become well known to Henry Briggs, now lately deceased.³ The two professors of astronomy were very likely responsible for Vlacq's producing an English edition of the Ephemerides in 1635,⁴ and this implies that they knew that there was a large English audience for the book that was unable to read Latin. The transformation of this little volume from the Latin to the English edition is a peculiar demonstration of the English view of Kepler. In the preface to the 1632 edition, Vlacq writes in effusive praise of Lansberg's tables, which he takes as the standard from which Kepler's tables 'very often err by a whole

1 Bodl. MS. Ashmole 240, f. 99^r (Bredon to Richard Napier, 21 Dec. 1630). In this letter, he described the Rudolphine Tables (1627), as 'lately this year by Keppler published,' which is curious. Perhaps this means that only in 1630 had they arrived generally in England, though Briggs had them by 1629. See also MS. Ashmole 240, f. 98^r (Bredon to Richard Napier (?), 16 May 1631; '...neither was a little Tobacco sufficient recompence that I should be so farre ingaged vnto you... to laboure so extraordinarilie.').

2 Adrian Vlacq, Ephemerides Motuum Coelestium... 1633. 1634. 1635. & 1636. (Gouda, 1632).

3 T. C. D. MS. 382, f. 119^r (Gellibrand to Bainbridge, 6 Dec. 1634); John Ward, The Lives of the Professors of Gresham College (London, 1740), p. 125 (life of Henry Briggs).

4 A[drian] V[lacq], Ephemerides of the Celestiall Motions... (London, 1635). It is worth noting that the copy in the Cambridge U. L. was that of Christopher Towneley.

degree or more in the motion of the Moon,'¹ and he explains that he uses Kepler for the planetary positions only because Lansberg's corresponding tables have not yet been issued. In the English edition, Vlacq's preface does not appear, and toward the end of the introduction, there has been inserted an English translation from Kepler's Ephemerides of his comments on the eclipses of 1633 to 1636.² These changes may be taken as a suggestion that Kepler was more highly regarded in England than on the continent. To the widespread use of the Rudolphine Tables is surely attributable an increasing English familiarity with elliptical orbits and other components of keplerian astronomy.

By the end of his life, Kepler was not merely respected, but venerated in England, and the distress that all Englishmen felt at the routing of the Protestant forces in the course of the first ten years of the continental war became, among scholars, a particular dismay at the sorry fate of Kepler. Their concern for him had led Sir Henry Wotton to extend his invitation of 1620; ten years later (and only two years after Henry Briggs had spread a false alarm of his demise³), Kepler was dead. Not long after, John Bainbridge

1 '...saepissime in motibus Lunae ad integrum Gradum & amplius a Lansbergianis differant'; Vlacq, Ephemerides (1632), p. 3.

2 pp. 13-16.

3 'Mr. Briggs tells me that Kepler is living, and confesses his mistake in the advertisement of his death, by being deceived in the similitude of his name with one D. Kapper, who died in that manner as he related.' The Whole Works of the Most Reverend James Ussher..., ed. C.R. Elrington (Dublin, 1847), xv. p. 431 (Bourgchier to Ussher, 26 Mar. 1629).

received from an Englishman in Vienna an account of his death and of the fate of astronomical learning in the empire:

'Keplerus dyed at Ratisbone in the time of the last diet, my Lord was at his funerall he ended his life in our faith as he was bred. I heare of none that lookes after his titular dignitie of Mathematicus Imperatorius; for there is but litle encouragment giue by this imperour to any but religious men & cloister learninge, the Duke of Friedlant [Wallenstein] whose maintained Keppler is much infatuated wth y^e opiniō of Judiciall astrologie & keeps now about him 2. or 3. in that trade, tis commonly reported that Keppler foretold his master y^e forenamed Duke that after his exauctoration at Ratisbone y^e last diet 1630 he should remaine soe but one year, & that 1632 he should be Imperator exercitus as he now is, y^e practise of Astrologie is too much followed in these parts...' ¹

¹ T.C.D. MS. 382, f. 103^v (Samuel Johnson to Bainbridge, 28 Feb./9 Mar. 1632). This Samuel Johnson is obscure. Perhaps he is the one admitted to Emmanuel College on 15 June 1620, who died 19 July 1658; Venn, Alum. Cantab.

IV. KEPLERIAN ASTRONOMY DEFENDED AND PROMOTED: 1635-1650

Kepler's astronomy was fully integrated into the expanse of English learning by the early 1640s, for its traces then appear in even the lowest stratum of didactic writing: the almanac. As early as 1624, John Rudston, an almanac-maker rather superior to most of his trade, who once wrote to Thomas Harriot in order to make certain that some of his facts were correct,¹ listed for his readers 'new discoeries in the celestiall Regions,' among which were:

'[K]epler Mathematician to the Emperour of Germany, obserued spots in the Sunne, which he probably argued to bee fuliginous vapours, issuing out of the body of the Sunne, such watry vapours as we haue issuing out of the earth,' and 'Kepler thinks [comets] to be bred in the ayre, as fishes in the Sea.'²

No more such references to Kepler are known in almanacs before 1641, in which year Arthur Sofford, an experienced almanac-maker, cites Kepler's authority for some astronomical data: his own calculation of the angular distance of the vernal equinox from the

1 B.L. Add. MS. 6789, f. 424^r (Rudston to Harriot, 9/19 June 1615).

2 John Rudston, A New Almanack and Prognostication (London, 1624), sigs. B4^r, B4^v. I owe this and several of the following references to Bernard Capp, Astrology and the Popular Press: English Almanacs 1500-1800 (London: Faber and Faber, 1979). He writes (p. 347), that almanacs were generally published in the autumn preceding the year for which they were printed.

first star in Aries, the obliquity of the ecliptic, and a table of 'The Auges of the Planets according to the Rudolphine Tables.'¹ Nathaniel Nye, a copernican, in the 'Astronomicall Notes' of his almanac of two years later also included a figure, derived from the Rudolphine Tables, for the precession of the vernal equinox from the first star in Aries, and he compared this value with those derived from other tables.² Vincent Wing was another almanac-maker to draw upon Kepler's work.³

Almanacs were also the principal purveyors of astrological doctrine and 'information', and it may be more than a coincidence that in the 1640s and 1650s, when all affairs in England were in flux, and millenarianism flourished, almanacs began to mention Kepler, and his astrological writings first found favour. In one millenarian tract, the Nuncius Propheticus (London, 1642), by 'T.B.',⁴ Kepler was one of four principal authorities cited, (among whom was also Tycho), and his 'De Trigono Igneo', the principal astrological section of the De Stella Nova Serpentarii, is quoted frequently. There are also citations of the Dioptrice (1611; a work surprisingly little read by English scientists), the Astronomiae Pars Optica, the Dissertatio, the preface to the

1 Arthur Sofford, A New Almanack and Prognostication (London, 1641), sig. B5^v. A similar passage, without Kepler, appears in his almanac for 1619.

2 Nathaniel Nye, A New Almanacke and Prognostication (London, 1643), sig. B8^r. The corresponding passage in his almanac for 1642 does not mention Kepler.

3 See below, p. 89.

4 T.B., Nuncius Propheticus: sive Syllabus Selectiss. Vaticiniorum Theologico-Mathematicorum... (London, 1642). Capp, op. cit., p.421, n. 226, reports the identification of the author as Thomas Boreman.

Astronomia Nova, and the preface to the Rudolphine Tables.¹ The references themselves are unimpressive and often slight, but it is striking that T.B., in his exaltation of Kepler as an astrologer, should have sifted through so many of his books for fact and fancy. Johannes Alsted, another of T.B.'s authorities, had himself, in his Diatribes de Mille Annis Apocalypticis (Frankfurt, 1628), referred to the De Stella Nova Serpentarii, as more English millenarians were to discover when Alsted's book appeared in William Burton's translation as The Beloved City or, the Saints Reign on Earth a Thousand Years (1643). To this translation, Burton appended a further generous extract from the 'De Trigono Igneo'.² The latter work was quoted and translated again by that distinguished astrologer and shrewd self-publicist, William Lilly (1602-1681) in his A Prophecy of the White King: and Dreadfull Dead-man Explained. To which is added the Prophecies of Sibylla Tiburtina and Prediction of Iohn Kepler: All of Especiall Concernment for these Times (London, 1644).³ Other astrologers followed this lead, and in the following decades they made a serious study of Kepler's purely astrological work.⁴

From the time of Kepler's first importation into England, Englishmen had sundered his astrology from his astronomy, and

1 Pp. 7, 14, 20, 45, 46, 47, 66, 69.

2 Published in London. In the original book, the Kepler reference is on p. 24. William Burton, the translator was not Robert Burton's brother; see article 'Burton, William (1609-1657)', in the DNB.

3 The quotation from Kepler (p.27), is much abridged.

4 See, for example, B.L. Sloane MS. 2279, ff. 57^r-59^v (anonymous; circa 1659); B.L. Sloane MS. 533, ff. 85^v-97^r (anonymous; appears to be from the later seventeenth century); Bodl. MS. Ashmole 348, ff. 36^r-48^r (by John Booker (1603-1667)).

though the astrology underwent a late flowering, this was no concern of the astronomers. Among them, the greatest interpreter of Kepler now arose. Jeremiah Horrox (1618?-1641) was also Kepler's most learned critic. His work was so tightly connected with Kepler's that a large number of his writings were published posthumously under the collective title, 'Astronomia Kepleriana Defensa et Promota'.¹ He was the first Englishman who advanced keplerian astronomy to any great degree, and he transmuted keplerian physics. The amount of work that he accomplished in his short life is astounding and his research was of a high standard. He has long been known, though probably not as well as he ought, to historians of science, and it would be fatuous to summarize his achievements here.² It must be borne in mind that he was sui generis, and therefore it is virtually impossible to see him as a product of the English keplerian tradition, however much he was a part of it. Nevertheless, we shall, in the next few pages, consider to what extent his work was related to that of his contemporaries and try to see how it can be compared with theirs.

He was born in Toxteth in 1618 and matriculated, as a sizar, into Emmanuel College, Cambridge, in May 1632.³ He left without taking his degree and returned to Toxteth from where, in 1636, he began his correspondence with William Crabtree (1610-1644?) of Broughton, Lancashire. By the summer of 1639, he had moved to the nearby village of Hoole, where he died in January, 1641, only one

1 Jeremiah Horrox, Opera Posthuma, ed. John Wallis (London, 1673), pp. 1-239.

2 See above, p. 15, n. 2.

3 S.B. Gaythorpe, 'Jeremiah Horrocks: Date of Birth, Parentage and Family Associations', Transactions of the Historic Society of Lancashire and Cheshire, cvi (1954), pp. 23-33.

day before an intended journey to meet Crabtree for the first time. His work, with Crabtree's collaboration, was carried on more or less in isolation, though Crabtree had a somewhat broader range of acquaintance with the outside world of mathematics than did Horrox, for he corresponded frequently with Samuel Foster (d. 1652), who was from 1636 the Gresham professor of astronomy, in London, and with William Gascoigne (1612?-1645).¹ Both these men, in the course of their work, discovered the superiority of Kepler's values for certain parameters over the numbers supplied by his rivals, and Foster's conclusions, at least, were transmitted to Horrox.² Through his association with Crabtree, Horrox can be located at the fringe of the astronomical circle around Christopher Towneley.³

The precocious Horrox must have begun to study mathematics and astronomy before our records of him commence, for it was with a well-trained critical eye that he anatomized Philip Lansberg's Tables, which he bought in 1635.⁴ Increasingly sceptical of Lansberg's gross claims of high worth, he was advised by Crabtree in 1637 to consult, instead, Kepler's Rudolphine Tables, which he

1 On the correct date of Gascoigne's death, see Charles Webster, 'Richard Towneley (1629-1707), The Towneley Group and Seventeenth-Century Science', Transactions of the Historic Society of Lancashire and Cheshire, cxviii (1966), pp. 61-62.

2 For Foster, see below, pp. 74-75; for Gascoigne, see the references to Kepler in S.P. Rigaud, ed., Correspondence of Scientific Men of the Seventeenth Century... in the Collection of the Right Honourable the Earl of Macclesfield (Oxford: at the University Press, 1841), i, p. 33 (letter of 2 Dec. 1640 to William Oughtred) and p. 44 (letter of Feb. 1641 to the same). Gascoigne had a wide acquaintance; he knew, also, Sir Charles Cavendish (B.L. Add. MS. 4278, f. 180^r, Cavendish to Pell, 16/26 Aug. 1644).

3 Webster, op. cit., pp. 51-76.

4 This is now in the library of Trinity College, Cambridge (shelf-mark T. 5. 116).

soon valued so much that he wrote, 'Daily experience indeed convinces me that what Lansberg says (whether with less modesty or truth I know not) of his own tables may be affirmed with propriety of Kepler's, namely, that they are superior to all others.'¹ He quickly mastered, in addition, Kepler's Astronomiae Pars Optica, Astronomia Nova, Epitome Astronomiae Copernicanae, and, notably (because he was the first Englishman to do so), Harmonice Mundi. Although he did not adopt Kepler's ideas entire -- one prominent difference lay in Horrox's refusal to credit much of the 'more than puerile vanities' of astrology² -- he was nonetheless in concord with Kepler's approach to scientific enquiry, for not the least of Horrox's distinctions from contemporary astronomers was his harmonious integration of a deep concern for precise observations, which does appear to have been common to all the Englishmen with whom we are familiar, with a broad, well-considered cosmology, similar to that of Kepler, which embraced a celestial physics. In a summary of his own physics, Horrox wrote:

'...the spirit of God (like the rays of the Sun) doth draw our hearts, desirous to rest in themselves, and force them unwilling to follow Christ, (as the planets follow the Sun's circumvolution, which begets a circular circumference) which following is the onely cause of our coming neer to god (as the sun's circumference brings the planets towards itselfe). All which agrees excellently with that mysticall adumbration of the thrise sacred trinity in (those poor types of God as one³ calls them) round circles; where the father (the

1 Jeremiah Horrox, The Transit of Venus across the Sun, trans. with a memoir by Arundell Blount Whatton (London, 1859?), p. 111. Crabtree's own copy of the Astronomia Nova, with the purchase date of 1637, is now in the Salisbury Cathedral Library. I owe this information to Professor Owen Gingerich and to Ms. Suzanne Edward, of the library. I have been unable to examine the book.

2 Horrox, Transit of Venus, p. 113.

3 'Donns poems' (Horrox's note).

center) doth beget the Son (the circumference) by efflux of the spirits (the rays). Keplers astronomy differs from mine, as his religion: He gives the planets a divers nature (good + bad) that they may eyther come to the Sun of fly away at their pleasure, or at least (as his second thoughts are) so dispose themselves (in spite of all the suns magneticall power) that the sun is bound to attract or expell them, according to that position, which themselves defend against all the suns labouring to incline the fibres. I, on the contrary, make the planet naturally to be averse from the sun, and desirous to rest in its owne place, caused by a materiall dulnes naturally opposite to motion, and averse from the sun, without eyther power or will to move to the sun of itselife. But then the sun by its rays attracts, and by its circumferentiall revolution carrys about the unwilling planet, conquering that naturall selfe rest that is in it, yet not so far but that the planet doth much abate and weaken this force of the sun... for my part I must ever thinke that God created all other things as well as man, in his own image, and that the nature of all things is one, as God is one, and therefore an harmonically agreeing of the cause of all things, if demonstrated, were the quintessence of most truly naturall philosophy.'¹

Horrox, who also wrote, 'He knows but little of astronomy who is ignorant that the figure of the orbit is elliptical; that its centre is the body of the Sun, and not a fictitious point near it; that the motion of the planet is really unequal; that the whole apparent inequality does not proceed from its eccentricity alone; and finally, that the inclination of all the orbits to the ecliptic is not influenced by the annual motion, but is fixed and constant,'² could not agree with all of Kepler's arguments concerning magnetic fibres originating within the earth as the cause of the earth's departure from a circular path about the Sun,³ and so, partly by analogy with a plumb-line being swung by a hand, representing a central force, he developed a different

1 Jeremiah Horrox, 'Philosophical Exercises', transcribed by S.P. Rigaud, Bodl. MS. Rigaud 14, ff. 31^r-35^r.

2 Horrox, Transit of Venus, p. 181.

3 Horrox, 'Philosophical Exercises', ff. 7^r-8^r.

theory of the earth's revolution. Thus did he, like Kepler, create an aetiological astronomy.¹

Likewise did he follow Kepler in using precise observations (his own) to prove that the universe was structured harmonically and according to plan. The ultimate purpose of his famous observation of the transit of Venus across the face of the Sun, on 24 November 1639, which he was the only man to predict, and, as the event turned out, to observe exactly, was not only to prove the Rudolphine Tables superior to their rivals and to correct them, which he did, but to check, by means of a correction to the value of the solar parallax, the accuracy both of Kepler's law relating a measure of the planet to the size of its orbit -- this law had caught Bainbridge's attention, too² -- concerning which Horrox came to the conclusion that planetary diameter was proportional to distance from the Sun, and, more important, of the law that 'the proportion that obtains between the periods of the motions of the planets and the semi-diameters of the orbits [the third law] is most exact, as Kepler, who discovered it, very justly remarks, and as I have accurately proved by repeated observation. Indeed there is not an error even of a single second.'³ Horrox never quoted

1 Ibid., ff. 20^r-23^r.

2 See below, p. 198.

3 Horrox, Transit of Venus, pp. 203-204; see also Curtis Wilson, 'Horrocks, Harmonies, and the Exactitude of Kepler's Third Law', in Science and History (Studia Copernicana, xvi), (Wrocław: Ossolineum, 1978), pp. 248-255.

the second law, however.¹

Horrox's other great accomplishment was his creation of a complex lunar theory, based on an elliptical orbit, which he developed in the space of a few months in 1638. Of this, a scholar has recently written:

'That [as Newton wrote], "our Horrocks was the first to determine that the Moon is revolved in an ellipse about the Earth, situated in the lower focus", does not indeed seem a particularly striking title to fame, but the sentence implies more than it immediately conveys. Before Horrocks 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.'²

Horrox's astronomical work differed from that of his English contemporaries both in the use of precise observations to extend and to confirm rigorously keplerian doctrines and in his extensive re-thinking of keplerian physics. He was the first Englishman to adopt the third law. All the same, he was one with them in his belief in at least the first law, in expressing an interest in Kepler's physics, and in his studied examination of Kepler's planetary orbital parameters, the hard data to be gleaned from his books.

In 1638, at about the same time that Horrox was studying

1 I have been unable to find such a reference among his papers, and Wilbur Applebaum, 'Kepler in England: The Reception of Keplerian Astronomy in England, 1599-1687' (State University of New York at Buffalo Ph.D. thesis, 1969), p. 71, reports having been unable to find such a reference, also.

2 S.B. Gaythorpe, 'Jeremiah Horrocks and his "New Theory of the Moon"', The Journal of the British Astronomical Association, lxvii (1956-57), p. 134.

Lansberg, Kepler, and the natural universe, John Wilkins (1614-1672), aged twenty-four, published the first of his two books in which Kepler was given more publicity in English print than he had ever yet received. Paradoxically, these books are of hardly any importance in the story of the integration of Kepler's astronomy into English thought. The reason for this is that both of these books, The Discovery of a World in the Moone (1638) and A Discourse Concerning a New World & Another Planet (1640),¹ are polemical works and popularizations of some of the latest astronomical theories, and not textbooks or research reports. They are part of an episode in the long struggle of the heliocentric astronomy against the astronomy justified by literal interpretation of scripture, and are replies to the fundamentalist Alexander Ross (1591-1654).²

Wilkins's books are of the utmost importance in the history of this struggle. But however important the struggle itself was, it appears, in England, to have taken place among natural philosophers and theologians, and not among astronomers. In any case, it is impossible to distinguish, in England, between a learned astronomer whose personal religion prevented his acceptance of copernicanism, and the divine who, as fundamentalist, felt impelled to meddle in mathematics and astronomy despite his ignorance, in order to condemn copernicanism, for the resulting characters were essentially the same: a man whose views were so backward, in comparison to what they might have been at the time, that he was

1 Both published in London. The Discourse incorporated the Discovery as the first of its two books.

2 For a much more full account of this dispute and its background, see Grant McColley, 'The Ross-Wilkins Controversy', Annals of Science, iii (1938), pp. 153-189.

irrelevant to the development of science. These two types may be less blurred together on the continent where, for example, Clavius, a formidable astronomical scholar, was an anticopernican, but in England, there were no men of similar astronomical learning and competence who rejected the heliocentric theory as inconsistent with Holy Writ. Therefore, although the conflict between science and religion and their gradual reconciliation are part of the intellectual environment within which Kepler's astronomy was considered by learned men, and may have impinged on attitudes within the university and on the patterns of employment of the universities' graduates, it has little immediate bearing on our present concern, the reactions of astronomers to Kepler's writings.

Wilkins's two books may, however, be useful to us as an illustration of the degree of acceptance of Kepler by the universities or, at least, by Oxford. Wilkins was an M.A. of only four years' standing, and a tutor, when the Discovery was published, and it seems very probable that most, if not all, of the astronomical knowledge that the book displays was gained by Wilkins as he pursued his M.A. course. We may see in it the influence of Bainbridge, who had gone so far in accepting keplerian theories. The Discovery, whose contents are mostly selenological, contains references to the Astronomiae Pars Optica, the Astronomia Nova,¹ the Dissertatio cum Nuncio Sidereo, and very frequently to both the Epitome Astronomiae Copernicanae and the Somnium (1634), Kepler's posthumously published fiction about a trip to the Moon, in which many clever and fanciful speculations were elaborated, and which

¹ p. 32 of the Discourse. The reference is to the introduction only of the Astronomia Nova.

appeared at about the time that Wilkins took his M.A. The Discourse, which was concerned almost entirely with buttressing copernicanism, cited all these books and the Mysterium Cosmographicum as well. Wilkins confessed himself especially obliged to 'Keplar: unto whom I doe acknowledge my selfe indebted for sundry particulars in this discourse.'¹

Even if Wilkins did not become acquainted with Kepler while at Oxford, though it is most probable that he did, it is very unlikely that this well-connected scholar was utterly self-taught; he would have been informally instructed by his Oxford friends. Therefore, his exposition of certain components of Kepler's astronomy in his Discourse, published three years after he left Oxford and eight years before he returned, may serve to reinforce our view that Bainbridge and, perhaps, others at Oxford had begun to find Kepler's physics acceptable and worthy to be taught to students. Wilkins accepted nearly the whole of Kepler's cosmology, for he wrote:

'There are...Arguments...much insisted on by eminent Astronomers, taken from that harmonically proportion which there may be betwixt the several distance and bignes of the Orbs, if we suppose the sun to be in the centre.

'For according to this (say they) wee may conceive an excellent Harmonie both in the number and the distance of the Planets: (and if God made all other things numero & mensurâ, much more those greater Works, the Heavens) for then the five Mathematicall bodies, so much spoken of by Euclid, will beare in them a proportion answerable to the severall distances of the Planets from one another.'²

Wilkins proceeded, then, to expound Kepler's doctrine of the five solids determining the spacing and number of the planets, and

1 p. 233.

2 pp. 139-140.

also his theory that planetary sizes were related to the sizes of the orbits. He delivers a summary of Kepler's celestial physics thus:

'If it be yet enquired, what cause there is of [the earth's] annuall motion: I answer, 'Tis easily conceivable, how the same Principle may serve for both these [rotation and revolution], since they tend the same way from West to East.

'However, that opinion of Kepler is not very improbable, That all the Primary Planets are moved round by the Sunne, which once in twenty five, or twenty six dayes, do's observe a revolution about it's owne Axis, and so carry along the Planets that encompassse it; which Planets are therefore slower or swifter, according to their distances from him. If you aske by what means the Sunne can produce such a motion?

'He answers: By sending forth a kind of Magneticke Vertue in strait Lines, from each part of it's Body; of which there is alwaies a constant succession: so that as soone as one beame of this vigor has passed a Planet, there is another presently takes hold of it, like the teeth of a Wheele.'¹

Despite his apparent wholesale acceptance of keplerian astronomy, Wilkins does not once cite any of the laws of planetary motion. This is the reverse of what has been suggested of the English response to Kepler by earlier historians; that his astronomy was acceptable while his physics were not. Wilkins's

1 pp. 214-5. It is amusing to read Ross's intemperate attack on Kepler's physics, which he knew only from Wilkins's account: 'Keplar's opinion that the Planets are moved round by the Sunne, and that this is done by sending forth a magneticke vertue, and that the Sunbeames are like the teeth of a wheele, taking hold of the Planets, are senselesse crotchets, fitter for a wheeler or miller, then a Philosopher: This magneticke vertue is a salve for all sores, a pin to every hole, for still when you are reduced to a non-plus, magneticke vertue is your onely subterfuge, like Aeneas his target, Vnum omnia contra tela Latinorum: If you had told us that the North starre had a magneticke vertue, because the needle touched with the magnes looketh towards it, some silly people perhaps would have beleaved you, and yet the magneticke vertue is in the needle, not in the star; But that in the Sunne there should be a magneticke vertue, it hath no show of probability.' Alexander Ross, The New Planet No Planet: or, The Earth No Wandring Star; Except in the Wandring Heads of Galileans (London, 1646), pp. 112-113.

silence over the laws is probably the result both of their not being directly relevant to his purpose, which was to argue that copernicanism was observationally and theoretically well-grounded, and of his lack of mathematical sophistication. He may also have intended to spare his readers such technical complications. All in all, however, Wilkins's books are yet another indication that Kepler's physics were far from unacceptable to the astronomers of the universities.

The English fondness for Kepler's works on account of their usefulness continued unabated. Samuel Foster (d. 1652), Henry Gellibrand's successor as professor of astronomy at Gresham College, in whose rooms there many scientists were wont to meet,¹ took a more practical interest in Kepler than Gellibrand may have taken. In his posthumously published Miscellanies,² (a collection of his writings to which it is not possible to assign dates of composition), one work is an adaptation of Tycho's star catalogue, as published in the Rudolphine Tables,³ and in some works, we can see that, like Horrox and others, he compared predictions according to Kepler's and Lansberg's tables with his own observations. In one of these comparisons, he found that Lansberg gave the better result, but he drew no general conclusion from this one instance,⁴ for as William Gascoigne wrote to Horrox in 1638:

1 E.G.R. Taylor, The Mathematical Practitioners of Tudor and Stuart England (Cambridge: at the University Press, 1954), p. 206.

2 Miscellanies: or Mathematical Lucubrations of Mr. Samuel Foster... (London, 1659).

3 The first item in the book, pp. 1-28.

4 Ibid., fourth item, pp. 11, 12.

'I recently received a letter from Mr. Foster (that man whom we esteem most worthy). He satisfied some of my doubts. He says that Lansberg is magniloquent but of suspect trustworthiness; and Gellibrand finally rejected him, after he caught his Tables erring more in an approach of the Moon to the Pleiades than did either the Prutenic or the Alphonsine [Tables]; indeed, the Rudolphine [Tables], which Foster prefers before the others, [erred] least of all.'¹

Foster fully accepted at least the first law, though there is some indication that, in view of its difficulties, he was unable to apply it for himself, for in describing a 'planetary instrument', an aid to calculation, he wrote:

'The way that I goe is (in general) agreeable to Copernicus his frame of the World; and in particular, to that which Kepler useth in his Rudolphin Tables. Onely this difference there is: Kepler makes the Orbits of the Planets to be Ellipses, which is the better way; and I here doe make them perfect Circles, which is the easier way. And though it be defective yet it makes no great difference in these small Instruments.'²

In the late 1630s and the 1640s, some mathematicians and mathematical practitioners outside the universities were studying Kepler and making use of his work with rather more sophistication than their predecessors of the previous two decades. It is reported that

1 Jeremiah Horrox, Opera Posthuma, ed. John Wallis (London, 1673), p. 306: 'Accepi nuper à D. Fostero literas. (Vir dignus ille est quem aestimemus). Dubiis ille meis aliquot satisfecit. Lansbergium ait magniloquum esse, sed suspectae fidei; & Gellibrandus tandem rejecit, postquam in appulsu Lunae ad Pleiadas magis errâsse Tabulas ejus deprehendit, quam vel Prutenicas, vel Alphonsinas; Rudolphinas vero omnium minimi, quas & Fosterus prae caeteris aestimat.' (Gascoigne to Horrox, 10 Apr. 1638).

2 Foster, Miscellanies, third item in book, p. 25; this same work is the one at B.L. MS. Sloane 3722, ff. 94^r-110^v -- is this in autograph or is it a copy by a friend? Foster was familiar with the Epitome Astronomiae Copernicanae, too; see the eighth item in the Miscellanies, 'Epitome Aristarchi Samii De Magnitudinibus', p. 1.

'William Milbourn Master of Arts, Curate of Brancespeth near Durham, aged about forty years [fl. 1620-43], was very knowing in Arithmetick, particularly in Algebra...and in Geometry. But his greatest Labours were in Astronomy, and in his Observation of the Stars,...he discovered the weakness of Lansbergius his Astronomy, and verified Kepler's Tables, which he turned into Decimals, and made Tables after Kepler's subsidiary way (pag. 97. Tab. Rudolph.)'¹

One Thomas Brush, 'Gardenor & practitioner of mathematickes' (fl. 1643-1656), in an odd little volume that somewhat resembles an almanac, employed the Rudolphine Tables in calculating the Sun's altitude above the horizon 'at all y^e houres of y^e day. at y^e Sunes entrance into each signe. wth y^e Sunes declination from y^e Equator at y^e same time,' and he very clearly depicted the earth's motion about the Sun as an ellipse, though he showed the ellipse to be compounded of an epicycle and a deferent. This last feature may derive from the work of Boulliaud, of whom more will be said below.² The mathematician John Pell (1611-1685), in the Netherlands from 1643 but in regular communication with other Englishmen, apparently owned at least the Epitome Astronomiae

1 Edward Sherburne, The Sphere of Marcus Manilius... (London, 1675), appendix, 'A Catalogue of the Most Emin[^]ent Astronomers, Ancient & Modern', p. 91. The passage concludes, '...which were sent to his Brother Mr. Milbourn a Stationer in London, to be Printed; but never passed the Press, being yet preserved in MS. in the hands of Sir Jonas Moore Knight. All his Observations and other Papers, &c. were most unhappily lost, by the coming in of the Scots, in the year 1639.' The meaning of 'turned into Decimals' is unclear. Does it mean that Milbourn used log₁₀, unlike Kepler? Or, and this fits the words better, did he express angular measure in decimal notation? For more on Milbourn's life, see Taylor, op. cit., p. 207.

2 B.L. MS. Sloane 3881, ff. 127^r, 59^r; see Taylor, op. cit., p. 232.

Copernicanae, of which he made some use, and he probably had other books by Kepler, too.¹

Some learned men without mathematical training were making use of Kepler's astronomical books in this period. John Selden (1584-1654), lawyer and historian, owned an extensive library full of scientific and mathematical works, several of which he may have been unable to understand (such as Harriot's Artis Analyticae Praxis), and including Kepler's Astronomia Nova, Nova Stereometria Doliorum Vinariorum (1615), Ephemerides Novae Motuum Coelestium (1617), Harmonice Mundi, Rudolphine Tables,² Mysterium Cosmographicum,³ Astronomiae Pars Optica,⁴ Eptiome Astronomiae Copernicanae,⁵ and De Stella Nova Serpentarii. To this last, he made a passing reference as early as 1615 in his Analecton Anglo Britannicon (1615),⁶ again demonstrating that this was one of Kepler's more successful books in England, so far as it had a wide readership. Unlike, say, Donne, another non-mathematician who had also read this book, Selden made extensive use of Kepler, but

1 B.L. Add. MSS. 4430-4431, f. 115^r contains Pell's abstract of a section of the Epitome Astonomiae Copernicanae on lunar inequalities; see also f. 395^r. Ibid., f. 120^r, headed, 'Libri Mathematici', contains almost all such great works. It is probably a catalogue of Pell's library, and it includes the Epitome, the Rudolphine Tables, '& reliqua omnia'.

2 Bodl. MS. Add. C. 40, p. 20.

3 John Selden, De Iure Naturali & Gentium, Iuxta Disciplinam Ebraeorum (London, 1640), p. 429.

4 Ibid., p. 202.

5 Ibid., p. 430.

6 John Selden, Analecton Anglo Britannicon (Frankfurt, 1615), preface, sig. ¶4^r. There is another reference to the same book, its section 'De Anno Natali', in Selden's De Synedriis & Praefecturis Iuridicis Veterum Ebraeorum (London, 1650), p. 355.

often, however, just to reinforce his long lists of references, which sometimes included such names as Boulliaud and Lansberg, too.¹ In his De Iure Naturali et Gentium (1640), Selden refers to the Epitome Astronomiae Copernicanae in the course of a consideration of the first visibility of the new Moon, and for the most part, he cites Kepler only for his discussions of chronology, without ever mentioning his cosmology or planetary theory. Still, it is worth noting that he refers to both the Epitome and the introduction (the most commonly read part), of the Astronomia Nova in support of copernicanism,² in his De Synedriis Veterum Ebraeorum (1650), and that, in the De Iure, he gives a favourable nod towards the music of the spheres, for which he directs the reader's attention to Kepler's Mysterium Cosmographicum and Harmonice Mundi, Book V, chapter 3, in which Kepler presents a digest of all his astronomical doctrines, including, quite clearly, the first and the third laws of planetary motion.³

Selden's was definitely not an idiosyncratic or isolated use of Kepler's books, for his friend, the famous Cambridge platonist Ralph Cudworth (1617-1688), wrote to Selden in 1643, when he was just a young tutor at Cambridge, about the date of the creation of the world: 'I take it y^e Beginning would fall about Sum̄er where Kepler in his Rudolphines places the first Epochæ of y^e Planets,'⁴ which is a roundabout way of saying that Kepler taught

1 Selden, De Synedriis, pp. 531-2.

2 Ibid., p. 532.

3 Selden, De Iure, p. 429. In the Harmonice Mundi, Kepler does not expound the second law very clearly.

4 Bodl. MS. Selden Supra 109, f. 258^r. I owe to Dr. M. Feingold this reference and the suggestion to search Selden's works for the preceding references.

that the creation took place in the summer.

Selden's and Cudworth's are slight, unsophisticated uses of Kepler's books, but all the same, the very fact that they had access to them and cited them for the authority of the author is an indication of familiarity with and acceptance of at least some of Kepler's work by the English academic community at large, to which they belonged.

Thomas Hobbes (1588-1679), resident in France during most of the period under review in this chapter, may be thought of as another inexpert reader of Kepler's, though he, with his pretensions to mathematical learning, would not have accepted this epithet. It is difficult to date his study of Kepler's books, and his most interesting references to Kepler were published well after this time. His first mention of Kepler, however, is in his critique of Thomas White's De Mundo, and this work, not published until recently, is thought to have been composed in late 1642 or early 1643.¹ He refers, without comment or elaboration, to Kepler's physical explanation of the Moon's irregular motion in Book IV of the Epitome Astronomiae Copernicanae,² and, with apparent approval, he cites Kepler as one authority who believed in the Sun's rotation.³ He was unquestionably a copernican. The former reference suggests that he had acquired sufficient knowledge of Kepler by the early 1640s to have made the criticisms of Kepler's physics that he published in 1655 in his Elementorum

1 Thomas Hobbes, Critique du De Mundo de Thomas White, ed. Jean Jacquot and Harold Whitmore Jones (Paris: J. Vrin, 1973); idem, Thomas White's De Mundo Examined, trans. by Harold Whitmore Jones (Bradford: Bradford University Press, 1976); for date, see *ibid.*, p. 3.

2 pp. 279-280.

3 p. 289.

Philosophiae Sectio Prima de Corpore (translated in 1656; this translation is the one quoted below).¹ Here, he sometimes cites Kepler for small details, such that, for example, he says, 'I suppose with Kepler, That as the distance between the Sunne and the Earth, is to the distance between the Moon and the Earth; so the distance between the Moon and the Earth, is to the Semidiameter of the Earth,'² and nearly the whole of his unoriginal account of astronomy, and of Copernicus in particular, seems, because of its judicious citing of Kepler, to derive from the Epitome, even where citations are absent, though this is a guess and cannot be proven. Hobbes more or less accepted Kepler's first law. He tautologically argued that the 'Excentricity of the Earth is the cause why the way of its annual motion is not a perfect circle, but either an Elliptical, or almost Elliptical line.'³ Hobbes's slight uncertainty may be attributable to his mathematically-inclined friends' pointing out to him Briggs's likely objection, that Kepler's ellipse, being of such small eccentricity, is barely distinguishable from a circle or slight oval.

We see the imprint of Kepler's 'zeroth' law and his physics when Hobbes considers the cause of planetary motion:

'... in the whole compasse of the heavens there appears no other Body, from which the cause of the Phenomenon can in probability be derived. Besides, I could not imagine, that so many and such various motions of the Planets should have no dependence at all upon one another. But by supposing motive power in the Sunne, we suppose motion also; for power to move, without motion, is no power at all. I have therefore supposed that there is in the Sunne for the governing of the primary

1 Thomas Hobbes, Elementorum Philosophiae Sectio Prima de Corpore (London, 1655); idem, Elements of Philosophy, The First Section, Concerning Body (London, 1656).

2 Hobbes, Elements, p. 318.

3 Ibid., p. 323.

Planets, and in the Earth for the governing of the Moon, such motion, as being received by the primary Planets and by the Moon, makes them necessarily appear to us in such manner as we see them; Whereas, that circular motion (which is commonly attributed to them) about a fixed Axis (which is called Conversion) being a motion of their parts only, and not of their whole Bodies, is insufficient to salve their Appearances. For seeing whatsoever is so moved, hath no endeavour at all towards those parts which are without the circle, they have no power to propagate any endeavour to such Bodies as are placed without it. And as for them that suppose this may be done by Magnetical Virtue [Hobbes certainly intends Kepler, but is he referring to English and European keplerians as well?], or by incorporeall and immateriall Species, they suppose no naturall cause; nay no cause at all. For there is no such thing as an Incorporeal Movent; and Magnetical Virtue is a thing altogether unknown; and whensoever it shall be known, it will be found to be a motion of Body. It remains, therefore, that if the primary Planets be carried about by the Sunne, and the Moon by the Earth, they have the simple circular motions of the Sunne and the Earth for the cause of their circulations.'¹

Hobbes, the mechanist, could not accept the idea of an immaterial cause of motion, such as 'Magnetical Virtue' was claimed to be, but his suggestion that, even if it were found to exist, it would be nought but another way of describing motions inherent in bodies, begs many questions, and his last statement, that the cause of planetary motion is the circular motion of the Sun, is an even more feeble explanation, for no connection between the bodies is even hypothesized. He continues with an argument of a standard equally low, but, being intended as counterfactual, sufficient to rebut any charge of atheism:

'Otherwise, if [the Planets and the Moon] be not carried about by the Sunne and the Earth, but that every Planet hath been moved as it is now moved ever since it was made, there will be of their motions no cause naturall. For either these motions were concreated with their Bodies, and their cause is supernatural; or they are coeternal with them, and so they have no cause at all. For whatsoever is Eternall was never generated.'²

1 Ibid., p. 320.

2 Ibid.

Hobbes makes more explicit his mechanistic objections to Kepler's physics a little later in the same book:

'I am therefore of Keplers opinion, in this, that he attributes the Excentricity of the Earth to the difference of the parts thereof, and supposes one part to be affected, and another disaffected to the Sunne. And I dissent from him in this, that he thinks it to be by Magnetick virtue, and that this Magnetick virtue, or attraction and thrusting back of the Earth is wrought by immateriate Species; which cannot be; because nothing can give motion, but a Body moved and contiguous. For if those Bodies be not moved which are contiguous to a Body unmoved, how this Body should begin to be moved is not imaginable; as has been...often inculcated in other places, to the end that Philosophers might at last abstain from the use of such unconceivable connexions of words. I dissent also from him in this, that he says the similitude of Bodies is the cause of their mutual attraction, For if it were so, I see no reason why one Egg should not be attracted by another.'¹

In short, as Hobbes much later wrote, 'Kepler ascribes [the eccentricity of the earth's annual motion] to a Magnetique vertue, viz. that one part of the Earths Superficies has a greater kindness for the Sun than the other part... I am not satisfied with that. It is Magical rather than Natural, and unworthy of Kepler.'²

Hobbes's objections to Kepler's theory are, by his principles, sound and rational, though the physics he substitutes are ill-conceived. If we have devoted a disproportionate amount of time to his less-than-brilliant discussions, it was certainly not because of their intrinsic worth, but rather, because they are among the longest remaining commentaries on Kepler from this period, and because they may well foreshadow later English opinions of Kepler (even though these are beyond the scope of this thesis).

1 Ibid., p. 323.

2 Hobbes, Decameron Physiologicum: or, Ten Dialogues of Natural Philosophy (London, 1678), pp. 38-9.

It is to be doubted whether Hobbes's criticisms were the same as those of his English predecessors who passed over and rejected keplerian physics, for the mechanistic philosophy on which they were based was probably inherited by Hobbes more from his French connections than from the English tradition.

In the 1640s, the French may have begun in other, slighter ways to influence the keplerian tradition in England, and to make keplerian astronomy more acceptable by altering its form. Noël Durret had issued, in Paris in 1639, his Latin-French Première Partie du Supplement des Tables Richelienes de N. Durret Cosmographe Ordinaire du Roy, & du Tres-Eminent Cardinal Duc de Richelieu. Avec une Briefue Theorie de Planetes selon Kepler. John Flamsteed (1646-1719), as a young man, found this useful and translated it, and it is just possible that Englishmen were reading it and making use of it at the time of Flamsteed's birth.¹ Pierre Gassendi (1592-1655) recounted the law of ellipses and Kepler's physics in his De Motu Impresso a Motore Translato (Paris, 1642), but though Gassendi was cited by contemporary English writers, he and Kepler were never mentioned in the same breath. Certainly, the French had a high regard for Kepler; Marin Mersenne (1588-1648) wrote to Theodore Haack (1605-1690) in London, in 1639, that among the great modern writers on optics was Kepler, and among

1 Francis Baily, An Account of the Reverend John Flamsteed... (London, 1835), p. 21; Flamsteed thought that Durret's preface was full of 'various faults', which could not be attributed either to the printer or to 'the ingenious Kepler', but found much information that was new to him and that made it worth the effort of translating. Wilbur Applebaum, 'Kepler in England', p. 121, n. 15, is probably correct in saying that B.L. MS. Sloane 533, ff. 33^r-81^r is Flamsteed's translation; the handwriting seems similar to a known sample of Flamsteed's. In 1641, Durret published a huge keplerian book, but this went entirely unnoticed in England.

the great modern compilers of astronomical tables were Kepler 'et nostre Durret'.¹

By far the greatest French influence on English keplerianism was exerted by the Astronomia Philolaica (Paris, 1645), of Ismaël Boulliaud (or Bullialdus; 1605-1694). Boulliaud's first work, his copernican Philolai, sive Dissertationis de Vero Mundi, Libri IV (Amsterdam, 1639), attracted little attention, but the massive Astronomia Philolaica, in which he accepted Kepler's first law but replaced the second by a version of a common European model², was quickly noticed by the English and attained some popularity. His elliptical orbit was compounded of a deferent circle and an epicycle with a contrariwise motion twice the deferent's speed of rotation, and the planet's variation in speed was owing, not to a physical law, but to the purely geometrical device of letting the empty focus be related to an equant point, with respect to which the planet moved uniformly. The bullialdan device, although actually disobeying the second law, was more practical for the calculation of what were regarded as tolerably good approximations to the true planetary positions.³ The first English notice of

1 Correspondance du P. Marin Mersenne, ed. C. de Waard (Paris: Editions du Centre National de la Recherche Scientifique, 1963), viii, pp. 581-2 (Mersenne to Haack, 1 Nov. 1639).

2 The simplest version of this model, in which the empty focus is considered to be a ptolemaic equant point, was first propounded by Albert Curtius and alluded to favourably by Kepler in the Rudolphine Tables; Kepler, Gesammelte Werke (Rudolphine Tables), ed. Franz Hammer (Munich: C.H. Beck, 1969), x. p. 172.

3 Kepler had, of course, rejected the use of the equant in the course of his work, as described in the Astronomia Nova. D.T. Whiteside, 'Newton's Early Thoughts on Planetary Motion: A Fresh Look', The British Journal for the History of Science, ii (1964), p. 121, notes that some equant-empty-focus theories gave rise to a maximum theoretical error of less than 1' of arc, an error that was unlikely to have been detected observationally at that time.

the book was not favourable: Sir Charles Cavendish (1591-1654), a distinguished mathematician temporarily resident in Paris, wrote to John Pell that, 'I haue not yet seen Bullialdus astronomie, but m:^r Hobbes thinkes he hath not much aduanced oure knowledge.'¹ But Boulliaud, who seems to have known John Bainbridge and was later to correspond constantly with Englishmen,² was eventually to be found in many libraries³ and to be frequently cited.

Vincent Wing (1619-1668), of Rutland, and Jeremy Shakerley (1626-1653?), who never continued long in one place, were Boulliaud's first English lieutenants. Both of these remarkable men were entirely self-taught as mathematicians and astronomers and progressed from almanac-making to the ability to comment critically on fine points of astronomical detail, methods of problem solving, and choices of parameters, but not on theory. Let them conclude our study, for they mark the beginning of the next vigorous phase of English keplerianism.

Shakerley, born in Halifax, became at twelve years, by the arrangement of his schoolmaster, a colonist in Ireland,

'continuing there till the Rebellion begun there, & then returning to my native place. Euer since which time I haue liued obscurely. About two years agoe [about 1646], I gaue my mind to the Mathematicks, & haue sithence neglected some imployments which carried the face of advantage, induced thereto by my desire to continue these studies... I now liue wth M^r Towneley from whom Meat & drink is all I can

1 B.L. Add. MS. 4278, f. 196^r (Cavendish to Pell, 27 June/7 July 1645).

2 See below, pp. 198-199, and The Correspondence of Henry Oldenburg, ed. A. Rupert Hall and Marie Boas Hall (Madison: University of Wisconsin Press, 1965-1972, and London: Mansell, 1975-1976), i-xi, passim.

3 I have not conducted a systematic search, but I have seen many copies in Oxford and Cambridge libraries. See also Appendix B, below.

expect. Clothes I want, my friends & kindred are unserviceable... I desire not much; food, raiment & convenience to prosecute my studies are the modest limits of my ambition.

'I receiued with much gladnesse your [William Lilly's] ouerture of making Planetarie tables in English, the best now extent for that purpose is Bullialdus, printed Parisij 1644. I had the use of it for some time about a yeare since but am now deprived of it, If I had that I should need few others.'¹

Despite his loss of Boulliaud's book, the Puritan Shakerley (who sought Major General Lambert's patronage²) had, in the library of the learned and remarkably tolerant Roman Catholic Towneley family,³ access to many of Kepler's books and, almost as important for him, Horrox's papers. Shakerley was in matters of theory so completely a keplerian, of the original or of the bullialdan variety, that nearly his entire published corpus as well as many of his manuscripts show the influence of Kepler, and only a mere sketch of his work can be presented here. In his almanac for 1651, in presenting a predicted transit of Mercury across the face of the Sun, he wrote,

'We come now to the Calculation of this Mercurial Eclipse; which if our narrow room would have permitted, we had presented according to the tables of Ptolemie, Alphonsus, Rheinholdus, Longomontanus, Argol and Lansberg, but finding them so erroneous...I have been the rather drawn to neglect those celebrated Hypotheses, vitious in their very forme, and incapable of Emendation, and to proceed to those who have

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- 1 Bodl. MS. Ashmole 423, f. 117^r (Shakerley to Lilly, 10/20 Feb. 1648/49).
 - 2 Ibid.: 'I entend to make choise of Major Generall Lambert for my Patron his generous uertues induce me thereto.'
 - 3 The Towneley family, part of the background against which we see Shakerley in perspective, are discussed by Charles Webster, 'Richard Towneley (1629-1707), The Towneley Group and Seventeenth-Century Science', Transactions of the Historic Society of Lancashire and Cheshire, cxviii (1966), pp. 51-76.

found out that the Planets moved not in a Circular, but an Ecliptick [sic] line, the undoubted path of their motions; and first for Kepler, according to whose Rudolphine tables this Conjunction happens...'1

But the definitive result, as Shakerley presents it, is calculated according to Boulliaud.

Shakerley had a continuing, but variable belief in astrology, but despite this, there is no mention in his papers of Kepler as an astrologer.² It is not easy to reckon him either a bullialdan or a keplerian astronomer. In his Tabulae Britannicae (London, 1653), he wrote,

'we, doubtfull upon what foundation to build, have corrected some things according to our own opinion, and the comparing of Observations. Some things we have set down according to the opinion of Bullialdus, but in most things we have credited Kepler.'³

At no point in his papers, as we have them now, is there any discussion of celestial physics or of the explicit law concerning planetary speed, keplerian or bullialdan.

He seems to have avoided physical speculation, rather like Boulliaud, for, in a consideration of the Moon's 'variation' (a particular variation in orbital speed), he somewhat snidely wrote, '...Keplers variation may be justly thought too bigge, notwithstanding he seems to deduce it from Physicall and

1 Jeremy Shakerley, Anni Aerae Salutis Christianae. 1651 Synopsis Compendiaria, or, A Brief Description of the Yeer of Humane Redemption MDCLI (London, 1651), sig. C1^r.

2 There are, among Shakerley's papers, several astrological discussions, as in Historical Manuscripts Commission, Report on Manuscripts in Various Collections, viii: The Manuscripts of the Hon. Frederick Lindley Wood (London: H.M.S.O., 1913), pp. 61-64.

3 p. 26.

Archetypicall demonstrations, which he so much affected....'¹

This last quotation occurs in a treatise entitled, The Anatomy of Vrania Practica. or, A short Mathematicall Discourse; Laying open the Errors and Impertinencies Delivered in a Treatise Lately Published by Mr. Vincent Wing, and Mr. William Leybourne, under the Title of Vrania Practica (London, 1649), and Wing did not permit this book to go unanswered.²

This flyting, although performed with sharp tongues, was essentially amicable, for the contestants were agreed on principles.³ Vincent Wing,

'although it was his luckless Fortune and unhappiness, to want an Academical Education, yet such was his Natural Inclination and Propensity to an acquaintance with Letters, that by his own Industry, and study, he had in time, conquered a competent Portion of Learning, Viz. A perfect acquaintance of the Latine Tongue, and a moderate understanding in the Greek, &c. By which happy advantages, the greatest and most critical Authors and Masters of Astrologie, and the Mathematiques, in their own Language, were no strangers unto Him.'⁴

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- 1 Shakerley, The Anatomy of Urania Practica (London, 1649), p. 13.
 - 2 Vincent Wing, Ens Fictum Shakerlaei: or the Annihilation of Mr. Jeremie Shakerley, his In-Artificiall Anatomy of Urania Practica. Wherein His Falacies or Ignorance, are Demonstratively Detected His Malice in Its Groundless Colours Display'd, and the Authors of the Said Urania Practica Justly Vindicated from His Unjust Aspersions (London, 1649).
 - 3 Bodl. MS. Ashmole 423, f. 174^r (Wing to Lilly, 28 July/7 Aug. 1650): Wing hopes that Shakerley did not take his reply 'unkindly'; '...I wish him well, & should bee glad to heare from him now and then.'
 - 4 J(ohn) G(adbury), A Brief Relation of the Life and Death of the Late Famous Mathematician and Astrologer, Mr. Vincent Wing (London, 1670), p. 3.

He began producing almanacs, his first (unpublished) one of 1641¹ showing no especial ability or theoretical allegiance, but by 1649, he had compiled an almanac based on the Rudolphine Tables² and was blunting Shakerley's blade with a shield of the Epitome Astronomiae Copernicanae, the Rudolphine Tables, and Boulliaud.³ He became, in fact, the first thoroughly bullialdan English astronomer. In his large, comprehensive Harmonicon Coeleste: or the Coelestiall Harmony of the Visible World; Conteigning, an Absolute and Entire Piece of Astronomie (London, 1651), although he is full of the praise of Kepler, he wrote,

'That most admirable Mathematician John Kepler, by the help of Tycho's Observations, did make the most absolute and best restauration of Astronomy, of any that ever did precede him, having not onely framed new Tables of the AEquations of the Planets, but also endeavoured by the assistance of ancient select Observations, by comparing them with Tycho's, to rectifie and establish their middle Motions, yet, nevertheless, since his time hath Bullialdus (by the help of other Observations) attempted to reform what his learned Predecessor had done, he comparing and limiting their middle Motions chiefly...'⁴

Wing even introduced a slight cosmetic alteration into Boulliaud's scheme for constructing the planets' orbits.⁵

1 Bodl. MS. Ashmole 190, ff. 73^r-84^r.

2 Wing, Speculum Vranicum, Anni Aerae Christianae. 1649 (London, 1649), sig. A3^r.

3 Idem, Ens Fictum Shakerlaei, pp. 20-21.

4 p. 158. Wing may have been influenced by Durret, also: Applebaum, 'Kepler in England', p. 152, places in parallel two nearly identical passages from the introduction to Durret's Tables and from Wing's Harmonicon Coeleste.

5 Wing, Harmonicon Coeleste, p. 44. In fact, Wing removed the subtle alteration that Boulliaud had introduced into the 'empty-focus' device, and, thinking his idea original, reverted to the older, cruder form of the same.

These two men, not of the universities, mark the end of the purely native tradition of the study of Kepler and introduce a foreign strand into the history of Kepler in England, and Boulliaud was soon being read in Oxford, too, for there, Seth Ward (1617-1689), who had become savilian professor of astronomy in 1649, and was, like his predecessor, Bainbridge, in communication with Boulliaud,¹ refined Boulliaud's system in his In Ismaelis Bullialdus Astronomicae Philolaicae Fundamenta, Inquisitio Brevis (Oxford, 1653). The influence of Boulliaud in England must not be exaggerated, though. He had rejected keplerian physics and introduced some geometrical devices (not refinements) that made Kepler's discoveries more manageable by the calculators. It was for this reason that Englishmen valued his book. England already had its own, strong, keplerian tradition, nurtured in Oxford.

From the middle of the century, the English were to become so dominant in astronomy that Robert Plot (1640-1696), writing in Oxford in 1676, could describe the history of the elliptical astronomy in a passage that distorts history and depreciates Kepler to the same extent that Kepler was hyperbolically over-rated by Coleridge in the quotation with which this thesis was introduced:

1 This fact has not been recorded by recent historians and must be taken into account by anyone who continues this history. John Aubrey, Aubrey's Brief Lives; 1669-1696, ed. Andrew Clark (Oxford: at the Clarendon Press, 1898), ii. p. 290, records, apparently from first-hand knowledge, 'In the bishop's [Ward's] study are several letters between Bullialdus and him, and between Hevelius and him.' I have been unable to find these. In the Bibliothèque Nationale, MS. Fran. 13050, f. 263^r, is a letter from Ward to Boulliaud, 19/29 Mar. 1659/60, carried by Charles Willoughby, the only letter from the correspondence that I have been able to find.

'In [1653, Seth Ward] first Geometrically demonstrated, the Copernico-Elliptical Hypothesis to be the most Genuine, Simple and Uniform, the most easie and intelligible, answering all Phenomena without Complication of Motions, by Excentrics, Epicycles, or Epicyc-Epicycles. That the Excentricities of the Planets and their Apoge's according to the Ptolomaic Hypothesis, and the Aphelions according to the Copernican, might all be solved by a simple Elliptical Line, was first indeed noted by Kepler, but how their proper and primary Inequalities, or Anomaliae Coaequatae, should thence be demonstrated Geometrically, he profest he knew not, and utterly despaired it would ever be done: which stirred up the Learned Ismael Bullialdus to attempt the Removal of this Disgrace to Astronomy, which accordingly he thought he had done, finding the Method of the Aphelions, and demonstrating (at least as he thought) the first Inequalities Geometrically, and making Tables...

'But how far he came short of what he pretended, was so plainly and modestly made appear by the Reverend Bishop [Ward, in his book], that the Ingenious Bullialdus himself, sent him a Letter of Thanks, and Recognition of his Errors.'¹

Thus was Kepler's achievement and England's former high regard for it to be forgotten, their continuance making them an inconspicuous commonplace. But the true history of Kepler in England from 1650 is a subject both vast and far more complex than the earlier history we have been recounting.

¹ Robert Plot, The Natural History of Oxford-shire, (Oxford, 1677), pp. 225-6.

V. CONCLUSIONS

'In alle Welt ergeht das Wort,
jedem Ohre klingend, keiner Zunge fremd:
Die Himmel erzählen die Ehre Gottes,
Und seiner Hände Werk, zeigt an das
Firmament.'

G. van Swieten

Just as the currents of European culture carried the cosmology of Milton to be adapted to Haydn's Creation, so had they flowed in the opposite direction almost two centuries earlier, and more rapidly, to carry the cosmology of another Central European, Kepler, to England, and likewise was his work adapted to a different vision of the universe.

At the end of our period, Kepler's cosmology, as a whole, was not accepted by Englishmen and was ignored, although John Wilkins had described it favourably. Neoplatonic ideas may not have flourished sufficiently in England to provide a trunk onto which Kepler's cosmology could be grafted.

All serious astronomers accepted the 'zeroth' law and the first law, of planetary orbital ellipses, as parts of the true description of the real motions of the planets. The second law, of areas, which governed the planets' real speed, was recognized but hardly ever mentioned directly. The third law went almost entirely unnoticed, except in the works of Horrox, which were as yet unpublished and little known. Perhaps it smacked too much of neoplatonism.

The process by which the zeroth and first laws were accepted was that an initial period of ten years, during which the Astronomia Nova was mulled over, was followed by a decade and a half in which the Epitome Astronomiae Copernicanae and the Rudolphine Tables fulfilled the needs of the English for authoritative textbooks and accurate astronomical tables, and concurrently popularized and promoted the immediate, astronomical (as distinct from remote, cosmological) principles on which they were founded. At the same time, a new professor of astronomy in Oxford, who had been won over to Kepler's new astronomy, actively promoted it and even, after a while, lectured upon it. Oxford and the astronomers in London, already well-disposed toward Kepler because of some of his earlier books and increasingly aware of the new astronomy, came to accept it as fact, even if they could not understand it fully or apply it in practice, apart from the indirect route through Kepler's tables. The history of its discovery, as narrated by Kepler in the Astronomia Nova, seems to have been superfluous. For the English, the proof of the ideas lay in the fact that they worked, as the tables and their own observations showed, and that they made sense.

That they made sense, I have argued (though not, I admit, conclusively), was related to the partial or tentative adoption of Kepler's physics; there is at least a hint of this amidst the evidence. Kepler's aetiological astronomy therefore replaced the old astronomy, which had lost its physical basis with the dissolution of the celestial spheres. Schematically, then, it may have appeared to the English astronomers that the first law was reasonable in part because the zeroth law was reasonable, and the physics were another consequence of the zeroth law, itself a

physical principle. The second law, which did not alter the visualization of geometrical astronomy as the first law did, and which was more or less incapable of application, was not so much an accepted part of astronomy as much as an obstacle that the English knew they had to overcome in the reckoning of planetary orbital speeds. Some dissented from the new astronomy, in a few cases, perhaps, on very sound grounds, but by 1640, Kepler's zeroth and first laws and, among those who cared about such matters, some form of the magnetic physics, were accepted in England.

The sequel to our history may have changed course with the arrival from France of a substitute for the second law and a burst of interest in the third law,¹ but there was a link with the old tradition in Oxford, in the person of Seth Ward, and the native English keplerian tradition must surely have led on, however indirectly, to Newton.

1 For some accounts of the continuation of this history, see Applebaum, 'Kepler in England'; Curtis Wilson, 'Horrocks, Harmonies, and the Exactitude of Kepler's Third Law'; and D.T. Whiteside, 'Newton's Early Thoughts'.

PART II

ENGLISH MATHEMATICIANS AND KEPLER'S ASTRONOMY:

CASE STUDIES

INTRODUCTION

Thomas Harriot was the most distinguished English mathematician to read the Astronomia Nova upon its publication, but he was not the only one to do so, nor was he the only one to correspond with Kepler. Some of the other mathematicians who did so have left scattered traces of their understanding of, and sometimes of their reactions to, Kepler's astronomy. One fortuitous event, the comets of 1618, led a couple of these men to consider aspects of Kepler's astronomical work that were unconnected with his planetary theory. All of these mathematicians have been mentioned briefly in the survey of the 'Kepler tradition' in Part I, Chapter II, and now I will consider them more carefully, beginning with Harriot and Lower, continuing with a group of acquaintances, Heydon, Briggs, and Bainbridge, proceeding to a contemporary of theirs, Lydiat, and concluding with a man who came upon Kepler somewhat later, Gellibrand. This part will thus end with the death of Gellibrand, in 1636, at about the time that Jeremiah Horrox was first encountering Kepler's books.

I will try to use biographical facts to set each man's keplerian studies in the context of his scientific work as a whole, but because some lives have left more traces than others, this will affect the fulness of my descriptions. My procedure is, in outline, to try to find out how each man approached the study of astronomy; to determine his views on Copernicus; and, following the modern interpretation of Kepler in considering astrology to be

integral with his cosmology, to determine each man's views on this subject, wherever possible, as a touchstone for his likely reaction to Kepler's world-view; to report on his professional relationship with Kepler; and finally, to use this knowledge to explicate his reading of the Astronomia Nova and Kepler's other astronomical books. Harriot's and Heydon's lives are the best documented.

VI. THOMAS HARRIOT

Thomas Harriot was one of the first Englishmen to read Kepler's Astronomia Nova. He did this with particular care, and an extensive record of his reading has come down to us. Harriot's accomplishments as a scientist justify a minute examination of his relationship with Kepler and his understanding of the new astronomy.

We know of Harriot's first twenty years no more than that he was born in Oxford in 1560,¹ attended St. Mary's Hall, Oxford, from 1577, and took his degree of B.A. in February 1579/80.² It has been suggested that it was here that he made his acquaintance with Sir Walter Raleigh (1552?-1618), who had studied at Oriel College, which was next-door to St. Mary's Hall. This can be only a guess, and one should remember that Raleigh had left Oriel by 1572 at the latest. In any event, Harriot soon entered the circle and the employ of Raleigh, whom he assisted in a number of ways. The

1 Anthony à Wood, Athenae Oxoniensis, third edition with additions, by Philip Bliss (London, 1813), ii, col. 299: 'Thomas Hariot, or Harriot, tumbled out of his mother's womb into the lap off the Oxonian muses an. 1560, but in what parish, I cannot yet tell.' Andrew Clark, Register of the University of Oxford, vol. ii, pt. ii (Oxford: at the Clarendon Press, for the Oxford Historical Society, 1887), p. 79, confirms the birthplace, but John Aubrey, Aubrey's Brief Lives: 1669-1696, ed. Andrew Clark (Oxford: at the Clarendon Press, 1898), i, p.285, reports the statement of Elias Ashmole (1617-1692) that he was born in Lancashire.

2 John W. Shirley, 'Sir Walter Raleigh and Thomas Harriot', in Thomas Harriot: Renaissance Scientist, edited by John W. Shirley (Oxford: at the Clarendon Press, 1974), p. 17.

best documented of these are Harriot's voyage to Virginia in 1585/86 and subsequent publication of his book, A Briefe and True Report of the New Found Land of Virginia (1588),¹ which demonstrates his ability in natural history, and his work, in about 1589, as one of Raleigh's colonists in Molana Abbey in Ireland.² Back in London, Raleigh provided him with housing and space for experiments, in Durham house. It was probably during his time as a protégé of Raleigh's that Harriot did most of his work on navigation.³

We know very little of Harriot's knowledge and studies of theoretical astronomy before 1610. (That is to say, the astronomy of actual planetary motion, as distinct from that of apparent motion, the basis of navigation, of which Harriot was a past-master.) This is owing in part to the state of his surviving papers, as will soon be shown. What is known of all the aspects of Harriot's astronomy has been classified and listed in great detail

1 For history of publication, see *ibid.*, pp.18-20. Professor Shirley refers to more thorough discussions of the book. In general, Shirley, Thomas Harriot, should be referred to for its excellent, but by now somewhat dated bibliography at pp. 166-174.

2 Shirley, 'Sir Walter Raleigh', pp. 20-22. Much work on this period of Harriot's life was done by the late Mr. W. A. Wallace, of Louisburgh, County Mayo, Eire.

3 For this aspect of Harriot's work, see Jon V. Pepper, 'Harriot's Earlier Work on Mathematical Navigation: Theory and Practice', in Shirley, Thomas Harriot, pp. 54-90. Also, John J. Roche, 'Thomas Harriot's Astronomy', (Oxford Univ. D.Phil. thesis, 1977). In this brief account of Harriot's life, I shall allow such studies as these to direct the reader to the relevant manuscripts, which are many and difficult to sort out.

by Dr. John Roche.¹ I will attempt here a summary, before I discuss the particulars that could have affected his study of Kepler's book.

It is important to bear in mind that early in Harriot's career, his mathematical talent was established and that his interests were directed towards practical matters. Mathematicians and natural philosophers in England, even those in the universities, had no completely organized, systematic means of pursuing their researches before about 1660, and, acting individually or in 'circles', their styles of research tended to be idiosyncratic. We see, in the 1590s, Harriot's style of science developed as he was to pursue it for the rest of his life. Harriot's mathematical talent² would probably have revealed itself under almost any circumstances, but his association with a man of action, and his application of his talents towards practical problems for his patron, helped to determine the character of his science, which was a combination of the expirical and quantifiable with the mathematical. Virtually any aspect of his researches exemplifies this. For example, his work on the nature of refraction shows great experimental care (with repeated observations), such that very accurate data were obtained, from which he derived, in July 1601, the Sine Law of refraction, now

1 Roche, op. cit., pp. 27-71, and 'Bibliography: Astronomical Material in Harriot's Papers', *ibid.*, pp. 354-356. The list of scattered references to astronomy, p. 355, is admitted by the author to be not fully accurate, and experience shows that it should be used with caution.

2 Jon V. Pepper, 'The Study of Thomas Harriot's Manuscripts: II. Harriot's Unpublished Papers', *History of Science*, vi (1967), 17-40, is a useful though slightly dated introduction to this subject. Pepper's later papers on particular topics should be referred to.

known as Snel's Law, after Willebrord Snel (1580-1626), who afterwards discovered the law independently, around 1621.¹

There is another distinct peculiarity of Harriot's style of research. This is his relative lack of concern for the philosophical and metaphysical bases of natural philosophy. As a student at Oxford, he would, in his quadrivium years, have studied aristotelian logic, ptolemaic astronomy, and euclidean geometry, as well as something of the foundations of natural philosophy.² Nevertheless, no question of epistemology ever intrudes into his writings. He was confident in his work, not bothered by either the scepticism or its antithesis that were characteristic of the age. Some scholars have made Harriot appear to be a nineteenth-century positivist, but, of course, this anachronism is far from the truth, for Harriot did indulge in a sort of metaphysical speculation; he was one of England's first atomists,³ in a period when atomism was disreputable, because tinged with atheism, and when the empirical evidence for atoms was at best very scanty. Atomism suggested atheism because, so it was thought, when an atomist said that atoms were hard and indestructible, he was suggesting,

1 For Harriot's work on refraction, see Johannes A. Lohne, 'Thomas Harriot als Mathematiker', Centaurus, xi (1965), 34-39, also Lohne, 'Thomas Harriott (1560-1621), The Tycho Brahe of Optics', Centaurus, vi (1959), 113-121, and especially D.T. Whiteside, 'Thomas Harriot's Breakthrough in Refractive Optics: A Study in Total Internal Reflection', (Cambridge: unpublished, 1980). The Sine Law was first published by Descartes in his La Dioptrique (Leiden, 1637). See Dirk Struik, 'Willebrord Snell', Dictionary of Scientific Biography, xii, 501.

2. Mark Curtis, Oxford and Cambridge in Transition: 1558-1642 (Oxford: at the Clarendon Press, 1959), p. 91, and Bodleian MSS. Savile 29-32.

3 Robert H. Kargon, Atomism in England from Harriot to Newton (Oxford: at the Clarendon Press, 1966), pp. 5-42.

following Lucretius (d. 55 B.C.),¹ that they could never have been created, and when creation was denied, God was denied. In this connection, we may note that Harriot, as well as Raleigh, was accused of atheism in 1592 and 1593,² and the charge was repeated by John Aubrey (1626-1697).³ There is no evidence, however, that Harriot ever really was guilty of this offence.⁴

In March, 1590, he met the ninth earl of Northumberland (1564-1632), another patron of scholars, probably at a supper given by the earl, to which Harriot accompanied Raleigh.⁵ As Raleigh's career declined, Harriot passed into the employ of Northumberland, and when the latter was arrested in 1605 on suspicion of a connection with the Gunpowder Plot conspirators, Harriot, too, was arrested and his house and papers searched.⁶ It was discovered that he had cast the horoscope of King James, a treasonable offence.⁷ Here we catch another glimpse of Harriot's attitude

1 Lucretius, De Rerum Natura, ed. Cyril Bailey (Oxford: at the Clarendon Press, 1947), i, 202-203: Book I, ll. 543-545: 'docui nil posse creari de nilo neque quod genitum est ad nil revocari, esse immortalis primordia corpore debent.'

2 Jean Jacquot, 'Thomas Harriot's Reputation for Impiety', Notes and Records of the Royal Society, ix (1952), 164-187, is a thorough discussion. See also Shirley, 'Sir Walter Raleigh', pp. 23-25.

3 John Aubrey, Aubrey's Brief lives, ed. Oliver Lawson Dick (Harmondsworth: Penguin Books Ltd., 1972), pp. 281-282.

4 Jacquot, op. cit.

5 Gordon R. Batho, 'Thomas Harriot and the Earl of Northumberland's Household', (Durham: unpublished, 1979), p. 2.

6 Shirley, 'Sir Walter Raleigh', pp. 27-29. Hatfield House, Salisbury Papers, C.P. 113/43, C.P. 134/86, C.P. 114/40, C.P. 114/41.

7 Public Record Office, State Papers 14; 216 Pt. II: 'The Examination of Nathaniel Tophrey [sic]', 27 November 1605.

toward 'science'. Although, as many scholars are at pains to point out, astrology at this time was not the silly diversion it is today, neither was it a full-fledged, respectable science. There was a long and distinguished history of opposition to it, and though some of the old arguments against it might embarrass its present-day opponents, some opposition was just the result of traditional scepticism. As has just been said, however, there is no evidence that Harriot was a sceptic. All the same, the amount of attention he was willing to pay to astrology seems to have been very small. There is little evidence, such as horoscopes, in his surviving papers to indicate astrological work, and Kepler, writing to him in 1606, after having received a detailed account of Harriot's researches and beliefs, said, 'I grant you that there is bad falsification from astrology. I sincerely ask you whether you think it fitting that its logic has to account for such things?'¹ This seems to show that Harriot at least had doubts of its worth. (Kepler thought that King James shared these doubts,² and did not understand that James was trying to discourage rumours about his reign and to prevent the revealing of possible state secrets. James probably considered his horoscope a

1 Johannes Kepler, Gesammelte Werke, ed. Max Caspar, (Munich: C.H. Beck, 1951), xv, p. 349, ll. 73-74: 'Audio tibi malum ex Astrologia conflatum. Obsecro an tu putas dignam esse, cuius causa talis sint ferenda?' (Prague, 2 October 1606.) I am preparing an edition of Harriot's correspondence. All quotations from this correspondence in this chapter are based on my text and all translations are my own. Nevertheless, for the reader's convenience, I have referred to Caspar's edition of Kepler's correspondence. Because there is not yet an authoritative edition of the rest of Harriot's correspondence, although much of it has been printed, I will give only the manuscript references for this.

2 Ibid., p. 350, ll. 80-82. For laws against casting the monarch's horoscope, see K.V. Thomas, Religion and the Decline of Magic (London: Weidenfeld and Nicholson, 1971), p. 344.

threat because he did believe in astrology, and in any event, the casting of the king's horoscope had long been recognized in law as a treasonable offence.)

Harriot petitioned for release from prison¹ in letters in which we first hear him complaining of the ill health that was to oppress him until his death.² His request was soon granted, and for the rest of his life, he lived quietly on a pension from Northumberland,³ in rooms provided by Northumberland at Sion House, near London. There he continued his researches and aided explorers of the New World,⁴ and from there he often travelled to the Tower to visit and to assist in their studies Raleigh and Northumberland. He also, on occasion, visited Northumberland's estates.⁵ His life was made uncomfortable not only by illness, but also by his fear of the authorities, who would naturally have suspected such a talented associate of two prisoners in the Tower. In 1608, he wrote to Kepler, 'Affairs continue among us such that it is still not permissible for me to philosophize freely. We still stick fast in the mud. I hope that God, best and greatest,

1 Hatfield House, Salisbury Papers, C.P. 114/40 (no date, 1605), C.P. 114/41 (16 December 1605).

2 On Harriot's illness and death, see R.C.H. Tanner, 'Henry Stevens and the Associates of Thomas Harriot', in Shirley, Thomas Harriot, pp. 93-94, which gives further references.

3 The Household Papers of Henry Percy Ninth Earl of Northumberland (1564-1632), ed. Gordon R. Batho (Camden Soc., 3rd Ser., xciii, 1962), p. 154.

4 This, however, to only a small extent. See David Beers Quinn, 'Thomas Harriot and the New World', in Shirley, Thomas Harriot, pp. 49-50.

5 Batho, Household Papers, p. 154.

is shortly going to bring an end to these matters.'¹ He was present at Raleigh's execution in 1618, though probably much enfeebled,² and died in 1621,³ famous and respected by the learned men of his day. I will quote just two of the compliments passed by his distinguished contemporaries: William Gilbert's 'that most accomplished scholar,'⁴ and George Chapman's encomium, 'my worthy and most learned friend, M. Harriots...whose judgement and knowledge in all kinds I know to be incomparable and bottomlesse -- yea, to be admired as much as his most blameless life and the right sacred expence of his time is to be honoured and revered.'⁵

Kepler learned of Harriot around 1605 from Johannes Eriksen,

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- 1 Harriot, in Kepler, Gesamelte Werke, xvi, p. 172, ll. 32-34. 'Ita se res habent apud nos vt non liceat mihi adhuc liberé philosophare. haeremus adhuc in luto. spero deum optimum maximum his breui daturum finem. Inde meliora expectanda.' (Syon, near London, 13 July 1608.)
 - 2 Harriot's notes on Raleigh's speech from the scaffold are at B.L. Add. MS. 6789, f. 533^r. Raleigh was executed on 23 October 1618, and on 13 June 1619, Harriot was writing to Northumberland that because of his illness, he had to put aside his work. See B.L. Harleian MS. 6002, ff. 21^r-21^v.
 - 3 2 July 1621. Harriot's will was first published by Henry Stevens, Thomas Harriot and his Associates (London: privately printed, 1900, and reprinted New York: Burt Franklin, 1972), pp. 193-203, and published again, somewhat less precisely, by R.C.H. Tanner, 'Thomas Harriot as Mathematician: A Legacy of Hearsay', Pt. I, Physis, ix (1967), pp. 244-247.
 - 4 William Gilbert, De Magnete (London, 1600), translated by P. Fleury Mottelay as On the Loadstone, reprinted in Great Books of the Western World, xxviii (Chicago: Encyclopedia Britannica, Inc., 1952), p. 7.
 - 5 George Chapman, Chapman's Homer, edited by Allardyce Nicoll (Princeton: Princeton University Press, second edition, 1967), i: 'The Iliad', pp. 15-16, from 'The Preface to the Reader'. Chapman's complete Iliads, with the full preface, was first published in London in 1611. A poem to Harriot was included at the end of Chapman's Achilles' Shield (London, 1598).

who, like Kepler, had been a student of Tycho Brahe's.¹ Eriksen had been in England with Tycho's son-in-law² -- the exact reason for their journey is not known -- and brought back a detailed account of Harriot's work and opinions. Kepler at this time was, as Harriot had been, attempting to investigate the nature of refraction and of the rainbow,³ and when Eriksen returned to England in 1606, he carried a long, friendly letter from Kepler, detailing his work and begging Harriot's opinions and data bearing on these problems and on several other matters. The correspondence of the two men, through five letters, lasted until at least 1610.⁴ There are signs in Kepler's last letter that more information was to be exchanged, but if there was, this may not

- 1 J.L.E. Dreyer, Tycho Brahe; A Picture of Scientific Life and Work in the Sixteenth Century (London: Adam & Charles Black, 1890), p. 301.
- 2 Ibid., p. 367. Dreyer says that the son-in-law, Tengnagel, 'was employed on various foreign embassies -- among others, to England, whither he was accompanied by Eriksen, who also gave up astronomy,' but he adds in a footnote that 'Eriksen observed the solar eclipse of October 1605 in London, and brought letters backward and forward...He had...for some time assisted Kepler.' See Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1955), xvii, pp. 161-2 (Kepler to Bianchi, 13 April 1616). Harriot notes at Petworth House, Leconfield MS. Hist. MSS. Comm. 241/II, f. 3, that a piece of information (concerning Tycho) was given to him by Eriksen and Tengnagel when they were in London.
- 3 Max Caspar, Kepler, translated by C. Doris Hellman (New York: Abelard-Schuman, 1959), pp. 144-145. The work leading up to Kepler's Astronomia Pars Optica (Frankfurt, 1604) was, obviously, done somewhat earlier, but the correspondence itself evidences Kepler's continued active interest in refraction.
- 4 National Library, Vienna, Cod. 10703, Bl. 378-380, Bl. 381-382, Bl. 383-385, printed, in Kepler, Gesammelte Werke, xv, pp. 348-352 (2 October 1606, Kepler to Harriot), pp. 365-368 (2 December 1606, Harriot to Kepler), xvi, pp. 31-32 (2 August 1607, Kepler to Harriot), pp. 172-173 (13 July 1608, Harriot to Kepler), pp. 250-251 (1 September 1609, Kepler to Harriot). Kepler, unlike Harriot, seems to have kept rough drafts of all his letters.

have been in full-length letters, and at any rate, such have not survived.

The correspondence touches only briefly on matters of astronomy, but a look at it now will reveal the different characters of Harriot and Kepler, both as men and, more important, as scientists. (We must be content with this general comparison of scientific character, rather than a more interesting comparison of scientific method, because Harriot's notes are not extensive enough to allow us to examine his methods with any certainty, although Kepler's writings enable us to study his.) Harriot's prose is matter-of-fact, straightforward, expository writing, not very lively, and revealing a 'no-nonsense' approach to scientific investigation. Kepler's letters froth over with ideas; he rambles and digresses: 'I almost forgot about the rainbow,'¹ he says, coming back to the main topic of ^{the} letter after an exposition of the nature of the equal-arm balance. In some ways, in his great enthusiasm, his explanations of phenomena are rather ad hoc, as he himself seems to realize, as when he says, 'In Chapter One [of the Astronomiae Pars Optica] I used theological principles more than optical, which act proves that I did not thoroughly know the nature of light.'²

Two long extracts from this correspondence, quoted below, on a particular topic, the rationale of refraction, show most clearly the differences in style of the two men. Kepler argues that light has the qualities of 'surface' and 'density', and explains the

1 Kepler, Gesammelte Werke, xv, p. 350, l. 115. 'De Iride penè eram oblitus.'

2 Ibid., p. 348, ll. 18-19. 'Capite primo principijs usus sum Theologicis magis quam Opticis: quae res arguit, me naturam lucis penitus ignorare.'

partial reflection of a ray of light at the surface of a transparent medium by the interaction and mutual repulsion of surfaces, whereas, to the extent that light has density, by which he means something like 'corporeality', the ray is able to be divided, and the part not reflected is transmitted by the medium. This explanation is rather makeshift and incomplete. Harriot, eschewing this sort of obscurity, accounts for partial reflection and transmission by a resort to first principles, and uses the atomic theory of matter, thereby foreshadowing the reliance on mechanical argument of later seventeenth-century natural philosophers. He shows that some of the ray hits particles and is reflected, and that the rest of the ray passes through the 'incorporeal parts' of the medium until it strikes another corporeal part; thus the refracted part does not pass straight through, but is actually reflected internally very many times. Reversing chronological order, I quote first Kepler's response to Harriot, where the latter's argument has forced him to express himself most clearly:

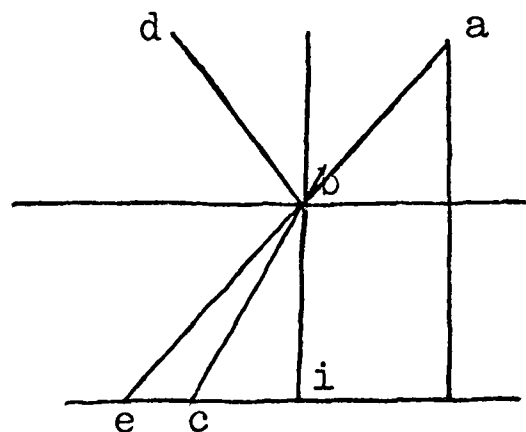
'You devise an objection: why is the ray not refracted in its whole transit?...I both rejected an examination of the end or of the good [teleological explanation] in optical laws, and presented a reason why a surface refracts, but a body does not refract. For I said that light is of the nature of things that partake of surface, and so suffers from surfaces, not from corporeality, since it does not partake of that as such...

'You certainly drag the matter to the twofold way of refutation, and partly by arguing, partly by allegories, in the fashion of chemists, you seem to dismiss me by playing at atoms and vacua of nature. But what seems absurd to you, that the same point at the same instant both transmits and reflects a ray -- that, to me, is not absurd...For although [in my Optics] I attributed surface to light, nevertheless, at the same time I also attributed density, on account of which it is able to be pulled asunder into two thinner lights. And what of the absurd if, while it is the nature of the absolutely

opaque to reflect absolutely, and of the absolutely transparent (that is, of that without surface) to transmit absolutely, if, as I say, something is intermediate between the opaque and the transparent, as are all translucent bodies bounded by surfaces? It partly transmits light, in so far as it is transparent, and partly reflects, in so far as it partakes of the opaque, and it is bounded by a surface.'¹

Kepler is responding to Harriot's argument, which follows. Harriot had, obviously, read the Astronomiae Pars Optica by this time.

'Let there be some luminous ray ab, in a rare medium. Let it strike the surface of some dense transparent [medium] obliquely at point b. Now, what does the ray do? Does it enter or not? It enters, you say, because the medium is posited as transparent and pervious to rays. But [the ray] does not proceed directly to point e, but is refracted and inclined at



point b toward the perpendicular bi, and goes through to point c. So it is, I concede, that experience teaches. But why? You say that it is impeded at the surface in entering and therefore is refracted. Is there not the same impediment after the entry? [Are] not the surfaces of the material of one nature with the whole body? [Are there] not the same surfaces in the whole body in every position, even if they are not perceived? Is it not therefore perpetually refracted in transit, just as in the beginning? You reply: Because the ray finds no surface during [its] action within the body or, if you wish, because it finds, as it were, infinite surfaces, namely in every position, the ray does not know by reason of which it turns, and therefore, after the first fracture by action at the surface, it proceeds without any other bending to point c. I say that it is more to the advantage of the ray that it proceed directly by way of line be in the beginning, than that it decline brokenly when it can find no cause in the beginning, but that the same [cause] opposes [it] in transit, namely, the impediment of resistance. And when the resistance is the same everywhere, the deflection is perpetually the same. But what is this, and by what means? Wait and attend for a bit. Let us return to the start, where we began.

¹ Kepler, Gesammelte Werke, xvi, p. 32, ll. 45-62. See Appendix D(2) for text.

'When the ray ab struck the surface of the dense transparent [medium] at point b, it also entered refracted. I ask whether at the same instant it was reflected from the same point or not. Because it is reflected in the direction of d, it is not to be doubted that the second angle [is] equal to [the angle] of incidence. Experience, besides, teaches [this, and] the eyes see [it], but nevertheless, there remains doubt about the identity of the time and of the point. Indeed, [this] response was not given without an absurdity. For the same point was pervious and resistant to the ray at the same instant, once; or in different instants, each of the two changed [its] nature.

'Therefore, what is replied: When one part of a contradiction is false, it necessarily follows that the other part is true. It remains to be said that the same point is not, to the ray, pervious and resistant; nor do I reckon that the same ray is reflected from the surface of the dense, transparent body and at the same time received within the body. Therefore, a dense, transparent body that seems by sense to be continuous through all parts is not, in reality, continuous; but it has corporeal parts that resist rays and incorporeal parts pervious to rays. So, as refraction is nothing other than internal reflection, and the part of the ray received within yet seems to the sense to be a straight line, it is, nevertheless, in reality, composed of many [such].'¹

Harriot then urges Kepler, half-jocularly, to contract himself into an atom in order to enter through the narrow portals of the abode of nature.

Neither man convinced the other. As an aside, it should be mentioned that although Harriot sent Kepler a sizeable table of results from refraction experiments,² he did not tell him the Sine Law, despite his having discovered it five years earlier.³ He merely stated that the results were everything Kepler needed to know, and that he left the conclusions to Kepler's 'genius for

1 Harriot, in Kepler, Gesammelte Werke, xv, pp. 367-368, ll. 58-100. See Appendix D(1) for text. The diagram is Harriot's.

2 Ibid., pp. 365-366, ll. 11-29.

3 See above, p. 100.

speculation.'¹ Why did Harriot not tell Kepler the Sine Law? There is hardly enough evidence to permit even a guess. Perhaps, having been under suspicion for treason, he was reluctant to reveal a discovery whose 'effectes shall so shew themselues shortly, to the good liking & allowance of the state & common weale'² to a foreigner, even if he, himself, knew that it was not of commercial or military value. We do not know the reason, not did Kepler's 'genius for speculation' ever lead him to the Sine Law.

Although the correspondence is primarily about optics, astronomy is touched upon. I will quote these passages now and return to one or two later in this chapter. Kepler, after defending his revised, harmonic astrology, says in his first letter, 'I suppose Johannes [Eriksen] told you what is the manner of motion of the planets. It would be tedious to handle that business here. I ask if you have any information about this, and let you speak your opinion. Certainly [you can say] something about my celestial magnetics.'³ One should remember that, although the Astronomia Nova was not published until 1609, Kepler had by 1605 discovered his First Law of planetary motion, and this, together with his earlier discovery of the Second Law, would have

1 Harriot, in Kepler, Gesammelte Werke, xv, p. 366, l. 41. 'Quae linquo tuo ingenio ad speculandum.' I disagree with John Roche, op. cit., p. 31, that this is Harriot's subtle way of making light of Kepler's manner of theorizing, and instead I accept it only as a statement, with a compliment clearly implied.

2 Harriot to the Privy Council, 16 December 1605, Hatfield House, Salisbury MS. C.P. 114/41, ll. 10-12. Harriot, in this petition for release from prison, is telling what advantages will accrue to the state if he is released and permitted to pursue his studies.

3 Kepler, Gesammelte Werke, xv, p. 350, ll. 83-85. 'Qua via motus planetarum demonstrum opinor dixisse tibi Joannem. Longum est, hic illud negocium tangere. Rogo siquid de hoc inaudisti, et hic tuam sententiam dicas: nimirum de meis Magnetibus caelestibus.'

permitted Eriksen to give Harriot an account of the principle features of the Astronomia Nova three years before its publication. The chronology of his thinking about the dynamic force causing planetary motion is not clear, but he seems to have begun considering celestial magnetism by 1603.¹ Kepler was seeking a critical review before publication. Harriot, in his reply, defers matters other than optics 'to another time.'² In his response to Kepler's second letter, however, he writes, 'We await your astronomical commentary [the Astronomia Nova] daily and eagerly. I beg that you write back something about it in your next [letter].'³ A little later, in the same letter, he writes, 'Master [William] Gilbert (now dead), who wrote about the magnet, left with his brother a book, whose title is, Concerning the Globe and Our Sublunary World, A New Philosophy, Against the Peripatetics, in 5 books. I hear that this year it becomes of public right. I make mention of this because, as I conclude from your writings, the philosophy of the former is very much congenial to you. I have seen a copy and read a certain chapter where I saw that he defended the vacuum with us against the peripatetics.'⁴

1 Alexandre Koyré, The Astronomical Revolution, translated by R.E.W. Maddison (London: Methuen and Co., 1973), p. 413, note 3.

2 Harriot, in Kepler, Gesammelte Werke, xv, p. 368, l. 15. 'Caetera differenda in aliud tempus.'

3 Harriot, in Kepler, Gesammelte Werke, xvi, p. 172, ll. 35-36. 'Tuas Astonomicas comēntationes indies et auidē expectamus. quaeso vt de illis aliquid in proximis rescribas.'

4 Ibid., p. 173, ll. 43-48. 'D. Gilbertus (iam mortuus) qui scripsit de magnete, reliquit cum suo fratre librū, cuius titulus: De globo et mundo nostro sublunari, philosophia noua, contra peripateticos. lib. 5. Audio q̄ isto anno fiet publici iuris. Huius mentionem feci, quoniam vt ex tui scriptis conijcio, illius philosophia tibi maximē placet. Exemplar vidi et quaedā capitā legi. vbi video q̄ vacuū defendit nobiscū contrā [sic] peripateticos.'

When Harriot said that the philosophy would be congenial, he meant the natural philosophy, Gilbert's magnetics, of Kepler's interest in which, we have just seen, Eriksen had earlier informed Harriot. But the reference to Kepler's writings must be to his Mysterium Cosmographicum¹, Astronomiae Pars Optica, or his De Stella Nova Serpentarii (which we know that Harriot read at some point), because, although they do not contain any magnetic philosophy, they were Kepler's only substantial works published by 1608, the date of Harriot's second letter. Harriot, then, sensed, probably from this as well as from Eriksen's report, that Kepler was attracted to highly speculative reasoning, which was evidently not to his own taste. As far as is known, Kepler never saw this later book by Gilbert, which was not published until 1651,² at which time it had little relevance for astronomical research. Incidentally, we see here how much the travel of news depended on chance: Harriot informed Kepler of Gilbert's death five years after the event, yet he heard of the Astronomia Nova three years before its publication. In his last letter, Kepler told Harriot the full title of his new book, his Of the New Aetiological Astronomy, or Of the Celestial Physics, and said that it had been published in Frankfurt, but that he did not yet have a copy. He said simply that he grieved at the fate of Gilbert, and that he hoped the latter's last book would be issued under one cover with the

1 There is a reference to the Mysterium Cosmographicum at Petworth House, Leconfield MS. Hist MSS. Comm. 241/III.2, f. 12.

2 De mundo nostro sublunari philosophia nova, (Amsterdam, 1651). A facsimile of the first edition, with a companion volume containing a study by Sister Suzanne Kelly, was published by Hertzberger, Amsterdam, in 1965.

Magnetics. (Kepler's printer was actually in Heidelberg,¹ so the reference to Frankfurt is probably to the book-fair.)

Much of Harriot's astronomical opinions, unfortunately, must be left to conjecture. We cannot even tell when Harriot became a copernican. Though Copernicus's magnum opus, the De Revolutionibus Orbium Coelestium, had been published in 1543,² no one before Harriot's time had been raised as a copernican; rather, astronomers were weaned on Ptolemy and later, if ever, switched to the heliocentric theory. There had been copernicans in England, the most notable being Thomas Digges, who published his Perfitt Description of the Coelestiall Orbes in 1576.³ Harriot, if he attended astronomical lectures at Oxford as a student, might have heard the young Henry Savile (1549-1622) expounding Ptolemy and Copernicus.⁴ This would have had to have been before 1578, when

1 Caspar, Kepler, p. 141.

2 First edition, Nuremberg, 1543. The best account of the reception of Copernicanism in England is still Francis R. Johnson, Astronomical Thought in Renaissance England (Baltimore: The Johns Hopkins Press, 1937, reprinted New York: Octagon Books, Inc., 1968), which should be supplemented with John L. Russell, 'The Copernican System in Great Britain', in Colloquia Copernicana I (Studia Copernicana v), ed. Jerzy Dobrzycki (Wrocław: Ossolineum, 1972), also published as The Reception of Copernicus' Heliocentric Theory (Dordrecht: Reidel, 1972).

3 Published as a supplement to a second edition of his father's, Leonard Digges's, A Prognostication euerlastinge (London, 1576). See the discussion in Johnson, op. cit., pp. 161-210.

4 Savile's lecture notes are at Bodl. MSS. Savile, 29-32.

Savile departed Oxford for a spell.¹ There is clear evidence of Harriot's friendship with John Dee, who had studied Copernicus, although he was not himself a copernican.² In particular, there is Dee's note in a copy of El Viaie que Hizo Antonio de Espeio in el Anno de Ochenta y Tres, a work on navigation, that it was given to him by Harriot.³

Harriot could have been familiar with the works of Giordano Bruno (1548-1600), the Nolan philosopher, who was in England from 1583 to 1585. Bruno was an atomist, like Harriot, was also a copernican, and defended the concept of an infinite universe.⁴ Harriot certainly knew of him, at least indirectly. There is a

1 Article 'Savile, Sir Henry (1549-1622)' in the DNB. There is a mention of a Mr. Savill' on a scrap of paper of Harriot's (B.L. Add. MS. 6787, f. 200^r), and from two of the accompanying names, 'Wernerus' and Mr Warner' ('Wernerus is Johnnes Werner (1468-1522), German mathematician and astronomer; 'Mr Warner' is Walter Warner (1560?-1643), well known as an associate of Harriot's. For information on him, see, first, R.C.H. Tanner, 'Thomas Harriot as Mathematician: A Legacy of Hearsay', pt. ii. Physis, ix (1967), 265-266), we may presume that this Mr. Savill was also a mathematician or natural philosopher, probably Sir Henry. But we could have assumed, even without this, that Harriot at least knew of Savile. There is a possibility, also, suggested by Dr. Roche, that this reference is to Sir Thomas Savile, Sir Henry's younger brother (this is possible but not likely since Thomas Savile died in 1593, and I suspect that the note is later; for this Savile's death, see Clark, Register, ii, pt. i (1887), p. 246), who at the same time was travelling on the continent and communicated with Tycho Brahe (see the letter from Tycho to Sir Thomas Savile, B.L. MS. Harleian 6995, Art. 40, printed in A Collection of Letters Illustrative of the Progress of Science in England, ed. J.O. Halliwell-Phillipps (London, 1841), pp. 32-33.

2 Dee was a strong supporter of Copernicus, but there is no evidence that he ever believed in the physical reality of his system. See Peter J. French, John Dee: The World of an Elizabethan Magus (London: Routledge & Kegan Paul, 1972), pp. 97-98.

3 Shirley, Thomas Harriot, p. 167.

4 Bruno expounded this doctrine in several works, including Dell' infinito Vniuerso et Mondi (London, 1584).

reference to Bruno in Harriot's later correspondence: Sir William Lower in 1610 wrote to Harriot that,

'[We] were a Considering of Keplers reasons [in the De Stella Nova Serpentarii] by wch he indeauors to ouerthrow Nolanus & Gilberts opinions concerninge the immensitie of the spheare of the starres and that opinion particularlie of Nolanus by wch he affirmed that the eye beinge placed in anie parte of the vniuers the apparence would be still all one as vnto vs here. when I was a sayinge that although Kepler had sayd something to moste that mighte be vrged for that opinion of Nolanus, yet of one principall thinge hee had not thought; for although it may be true that to the eye placed in anie starre of [Cancer] the starres in Capricorne [opposite in the sky] will vanish, yet he hath not therfore so soundlie Concluded (as he thinkes) that therfore towards that parte of the world ther wilbe a voidnesse or thin scattering of little starres wheras els round about ther will appeare huge starres close thruste togeather: for sayd I (having heard you say often so much [emphasis added]) what if in that huge space betweene the starres and Saturne ther remaine euer fixed infinite numbers wch may supplie the apparence to the eye that shalbe placed in [Cancer] wch by reason of ther lesser magnitudes doe flie our sighte what if about [Saturn, Jupiter, Mars] &&. ther moue other planets also wch appeare not. iust as I was a saying this comes your letter. wch when I had redd, loe, qd I, what I spoke probablie experience hath made good.'¹

Harriot had evidently just informed Lower of Galileo's discoveries of the medicean stars, that is, the moons of Jupiter, and of the vast number of faint stars that compose the Milky Way.

We see from this passage that Harriot had had some sympathy for Bruno's infinite universe, but that he may not have believed in it; Sir William's reference to the great distance between Saturn and the stars does not make this clear. If he believed in this vast distance, it is evidence for copernicanism, for, in order that there be no noticeable stellar parallax as the earth orbits the Sun -- and none had been observed -- the stars must be far more

¹ B.L. Add. MS. 6789, f. 425^v, ll. 3-25. Letter dated 11 June 1610.

distant from the earth than Ptolemy had supposed, as Copernicus had understood.

Had Harriot ever adopted the tychonic planetary system? Despite his great respect for Tycho's instrumentation and the information derived therefrom, it seems he never believed in Tycho's theory. John Roche has discussed briefly Harriot's connections with Tycho.¹ The two men seem never to have corresponded, but Harriot was familiar with three of Tycho's students, Eriksen, Kepler, and Tegnagel, Tycho's son-in-law.² Some of Harriot's papers reveal an interest in Tycho's figures for star and planet sizes and the motion of the solar apogee, among other topics,³ but this tells us little more about Harriot's view of the world system.

There is one interesting scrap⁴ on which Harriot has given the relative sizes and distances from the earth of the Sun and Moon. The method of determining them is the standard one, invented by Aristarchus of Samos (fl. 280 B.C.).⁵ The values at which he arrives for the Sun are, however, quite large by the standards of Harriot's time, and much larger than those of Copernicus.⁶ The Sun's diameter is given as twenty times that of the earth; the

1 Roche, op. cit., pp. 49-53.

2 See above, p. 105-106.

3 Roche, op. cit., pp. 52-53, gives the manuscript references.

4 B.L. Add. MS. 6787, f. 371^r.

5 The most thorough account of the method, with an edition of Aristarchus' text, is in Thomas L. Heath, Aristarchus of Samos: The Ancient Copernicus (Oxford: at the Clarendon Press, 1913).

6 Copernicus, op. cit., Book IV, chaps. 17-20. Nicholas Copernicus, Complete Works, Vol. II: On the Revolutions, translated, with notes, by Edward Rosen (Warsaw: Polish Scientific Publishers, 1978), pp. 204-208.

Moon's is a quarter that of the earth; the Sun is 4587 earth-radii from the earth; the Moon is 57 earth-radii away; or the Sun is 80 times as distant as the Moon.¹ If the modern value for the earth's radius is adopted, the earth-Moon distance is close to the modern value, but the earth-Sun distance is still far too small.² These may be the sizes of Harriot's universe, whose shape Kepler was to change.

We have no way of knowing if Harriot believed these figures to be real, or if they were just the results of an exercise. This is a crucial point; the uncertainty is owing to the state of Harriot's writings, both as he left them, and as they have been received by us. Harriot published only one book during his lifetime, his A Briefe and True Report, and another, the Artis Analyticae Praxis, was issued by his executors, eleven years after his death,³ yet throughout his life, his colleagues, including Kepler,⁴ urged him to publish. We do not have Harriot's replies to these requests. Perhaps the reason for his refusal to put more into print was his fear of authority owing to his one-time imprisonment and that of

1 The following table uses rounded figures for an approximate comparison:

	<u>Harriot</u>	<u>Copernicus</u>
Sun's Diameter :	20	5.5
Moon's Diameter:	0.25	0.29
Sun's Distance :	4587	1179
Moon's Distance:	57	65.5 max., 55.13 min.

where the earth's radius is the unit.

2 Cf. Roche, op. cit., p. 59, for an account of another of Harriot's distance calculations. These exercises are possibly the continuation of Harriot's work on the exact moment of the lunar quadrature (Petworth House, Leconfield MS. Hist. MSS. Comm. 241/IX, ff. 1-9). I hope to examine this in a later paper.

3 Thomas Harriot, Artis analyticae praxis, algebraicas nova methodo resolvendas (London, 1631).

4 Kepler, Gesammelte Werke, xvi, p. 32, ll. 63-64. 'De coloribus et de Iride differs me cupiditate videndj libellos tuos, quos obsecro edas primo quoq; tempore.'

his patrons; as he said, 'I cannot philosophize freely.'¹ There is, however, no other sign of this fear or of a threat against him, and when we examine his papers, there seems to be nothing that could possibly give offence to the state. All the same, it is possible that he thought that any obscure work could prove prejudicial against him in the eyes of an ignorant and overly sensitive authority. It could be, on the one hand, that Harriot was too much a perfectionist to permit anything that was not absolutely right to appear in print, for there are often several drafts of a work among his papers. His notes are frequently, though not always, sketchy pages of calculation with little explanation -- his posthumously published book shares in this description -- and are thus puzzles for the scholar, even though the pages are gathered, roughly, by topic. On the other hand, it seems sometimes that after working out a problem, he lost interest in it and passed on to another, occasionally not bothering even to write down the last few steps of the solution. Perhaps he was too intellectually restless, and too lacking in pride, to apply his patience and to see his work preserved for posterity, although in his will, he asked his executors, friends, to do the work he had never done, to publish whatever was worthwhile among his notes.² We cannot know his reasons for not making his discoveries better known, and because so little is written out in full, in words, we have great difficulty in dating them (although Dr. R.C.H. Tanner has achieved

1 See above, pp. 103-104.

2 Harriot's will, ed. Stevens, op. cit., pp. 200-201: '...to the end that after hee [Nathaniel Torporley] vnderstand them [the papers] hee may make vse in penninge such doctrine that belonges vnto them for publique vses as it shall be thought Convenient by my Executors and him selfe.'

some success with handwriting analysis¹), and in distinguishing what he actually believed from what he merely postulated for the purpose of an exercise, or from what he copied from the books he studied. This must be borne in mind when we consider the next facet of Harriot's investigations.

By February of 1609/10, Harriot had acquired a copy of the Astronomia Nova, had worked his way through much of it, and was corresponding with his friend, Sir William Lower, about it. Now that Sir William is entering our story, he deserves an introduction.

Lower came from an old Cornish family and, though his date of birth is not known, there is a record of his matriculation into Exeter College, Oxford, at the age of sixteen on 10/20 June 1586.² (We presume that this William Lower is our man.) He was, then, ten years Harriot's junior. He left Oxford in 1593 without taking a degree (not an unusual procedure at that time) and made a small career for himself in public life, being elected an M.P., at different times, for two Cornish constituencies, and being knighted in 1603 at the accession of James I.³ Probably through his wife, he gained a farm of about three thousand acres, named 'Trefenti', near Carmarthen. His wife was the step-daughter of the ninth earl

1 R.C.H. Tanner, 'The Ordered Regiment of the Minus Sign: Off-beat Mathematics in Harriot's Manuscripts', Annals of Science, xxxvii (1980), pp. 153-158.

2 S.P. Rigaud, Supplement to Dr. Bradley's Miscellaneous Works (Oxford: at the Clarendon Press, 1833), pp. 68-70. Professor Rigaud collected most of what is known about Lower. Clark, Register, ii, pt. ii, p. 151.

3 Wm. A. Shaw, The Knights of England (London: Sherratt and Hughes, 1906), ii, p. 107 (11 May, 1603), and p. 134 (4 July, 1604). There were two William Lowers knighted within fourteen months. It is impossible to tell which of the two is Harriot's friend. Rigaud, Supplement, p. 69.

of Northumberland,¹ and we may guess that it was through Northumberland that Lower first met Harriot. There is reason to believe that they knew each other by 1598,² and they certainly knew each other by 1607, at which time Lower sent Harriot a letter, which survives,³ on the comet of that year. Sir William died in 1615, and though the friendship surely lasted until then, his letters to Harriot, with the exception of the one just mentioned, are from 1610 and 1611.⁴ There are no surviving letters from Harriot to Lower.

Sir William was a charming man, as his letters witness. In contrast to Harriot's dryness, he reveals a personal liveliness, as when, on learning from Harriot of Galileo's astronomical discoveries, he says:

'me thinkes my diligent Galileus hath done more in his threefold discouerie then Magellane in openinge the streights to the South sea or the dutchmen that weare eaten by beares in noua Zembla. I am sure with more ease & saftie to him selfe & more pleasure to me.'⁵

His intellectual interests were probably the consequence of this personal liveliness, rather than the result of innate brilliance,

1 Batho, Household Papers, p. 147.

2 Tanner, 'The Ordered Regiment', p. 158. Dr. Tanner here places a paper written in Sir William's hand, and in Harriot's collection, among a group of pages written before 1598.

3 Letter, no date, 1607. Petworth House, Leconfield MS. Hist. MSS. Comm. 241/VII, ff. 1^r-3^r. This letter is curious, because it seems to contain two drafts with the same information.

4 See Appendix F, pp. 235-236. Also, there are undated letters, B.L. Add. MS. 6789, ff. 444^r-445^v, 435^r-436^v, and 437^r-437^v.

5 Letter of 11/21 June 1610. B.L. Add. MS. 6789, f. 425^r, ll. 4-9.

for while the interests shown by his letters are wide-ranging, there is very little original thought. Also, he often had much leisure to devote to affairs of the mind.¹ He knew Latin, though he did not know it very well -- Harriot made several minor corrections to his writing in that language -- and he relied chiefly on English.² He practised algebra with Harriot,³ engaged in astronomical observations and speculations,⁴ and may very well have been the only country gentleman, apart from Sir Christopher Heydon, to rack his brains in reading Kepler's Astronomia Nova from cover to cover. As we shall see, he makes fairly clear that he mirrors Harriot's ideas concerning this book, although he has a few ideas of his own.

I have already noted his discussion of a point in Kepler's De Nova Stella Serpentarii,⁵ which shows a familiarity with Kepler.

On 6/16 February 1609/10, Sir William wrote to Harriot:

'Kepler I read diligentlie. but therin I find what it is to be so far from you. For as himselfe, he hath almost put me out of my wits. his Aequants, his sections of excentricities, librations in the diameters of Epicycles, revolutions in Ellipses, have so throughlie seased upon my imagination as I doe not onlie ever dream of them, but oftentimes awake lose

1 'indeed I haue here much otīū and therefore I may cast awaye some of it in vaine pursuites.' Letter of 19 July 1611. B.L. Add. MS. 6789, f. 433^r, ll. 19-21.

2 See the alterations in the undated Latin letter, B.L. Add. MS. 6789, f. 444^r.

3 Tanner, 'The Ordered Regiment', passim, and Sir William's correspondence, passim.

4 The best evidence for Sir William's observational work is the letter of 1607; see above, p. 121, n. 3. The speculations occur throughout his letters.

5 Above, p. 116.

myselfe, and power of thinkinge with to much wantinge to it.'¹

Lower had plunged into the book, and he read it quickly at first; as he says a little later, 'I have read Kepler twice over cursoridlie. I read him now with Calculation.'²

Even if his grounding in astronomy had been extensive (and he certainly must not have been unlearned in the subject), he would have found these devices difficult, for Kepler's equants were not the same as those of classical antiquity; his sections of eccentricity, that is, Kepler's separate calculations of the positions of the equant point and of the Sun with respect to the centre of the planetary orbit, were much harder to comprehend than Ptolemy's bisection of the eccentricity; and the 'librations in the diameters of Epicycles' were invoked by Kepler in a chapter inessential for his arguments.³ Harriot had apprised Lower of the novelty of Kepler's elliptical orbits: 'His theorie...me thinkes (although I cannot yet overmaster manie of his particulars) he establisheth soundlie and as you say overthrowes the circular Astronomie.'⁴ The forthright use of the word 'overthrowes' leaves little doubt that Harriot and Lower believed that Kepler's work did just that, and that they considered the new astronomy to provide a true description of reality.

Of course, both men had long had a foretaste of what Kepler proposed to say in his book, for Eriksen and Kepler, himself, had

1 Letter of 6 February 1609/10. See below, Appendix F. The original manuscript of the first half of this letter has been lost since it was first published by Baron von Zach in 1803. See below, p. 125, n. 2. The two halves had already been separated by that year. I accept the date that F.R. Johnson assigns to this letter; F.R. Johnson, *op. cit.*, p. 227, n. 37.

2 Letter of 6 Feb. 1609/10.

3 Kepler, Astronomia Nova, Chapter XXXIX, p. 188.

4 Von Zach, *op. cit.*

told Harriot of the research into Mars' orbit. Perhaps it was a distant, confused memory of this that prompted Lower, later in the same letter, to write, 'I remember long since you told me as much, that the motions of the planets were not perfect circles,' a 'great invention';¹ Sir William had the impression -- one sees this better from the context of the quotation -- that Harriot had himself invented non-circular orbits. But because there are few signs of Harriot's conducting any research into planetary theory, this is most improbable.

Sir William also wrote, 'I cannot phansie those magnetical natures.'² One would expect this of a follower of Harriot's. This, too, was a matter of which Eriksen must surely have told Harriot, for, in his first letter, Kepler asked Harriot for his opinion of the 'celestial magnetics.'³ Harriot never replied. As for Kepler's doctrine of the determination by the regular solids of the spacing of the planets, Harriot was, at most, indifferent, for although he read the Mysterium Cosmographicum,⁴ he never commented on this, either.

Resuming a passage I quoted a moment ago, Sir William says, 'I read him now with Calculation. Sometimes I find a difference of minutes sometimes false prints, and sometimes an other [utter?] confusion in his accounts.'⁵ Kepler had made some errors of calculation, and the book is full of misprints, only the most

1 Ibid.

2 Ibid.

3 Kepler, Gesammelte Werke, xv, p. 350, ll. 83-85. 'Qua via motus planetarum demonstrum opinor dixisse tibi Joannem...Rogo siquid de hoc inaudisti, et hic tuam sententiam dicas: nimirum de meis Magnetibus caelestibus.'

4 See the reference at Petworth House, Leconfield MS. Hist. MS9. Comm. 241/III.2, f. 12.

5 Ibid.

serious of which are corrected in the printed list of errata. 'To give you a tast of some of thes difficulties that you may judge of my capacitie, I will send you only this one.'¹ At this point, most unfortunately, there is a gap in our received text.²

There follows a long passage that is worth quoting in full:

'For his theorie I am much in love with these particulars; 1^o his permutation of the medial to the apparent motions, for it is more rational that all dimensions as of Eccentricities, apogaeis, etc..., should depend rather of the habitude to the sun, then to the imaginarie circle of the orbis annuus.

2^o His elliptical iter planetarum, for me thinks it shewes a Way to the solving of the unknown walkes of comets. For as his Ellipsis in the Earth's motion is more a circle & in mars is more longe & in some of the other planets may be longer againe so in thos Commets that appeare fixed the ellipsis may be neere a right line./

3. his phansie of ecliptica media or his via regia of the sun. vnto wch the walke of al the other planets is obliq, more or lesse; euen the ecliptica uera vnder wch the earth walkes his yeares iournie; by wch he solues handsomelie the mutation of the starres latitudes. &&. indeed I am much delighted with his booke but he is so tough in manie places as I cannot bite him. I pray write me some instructions in your next, how I may deale with him to ouermaster him for I am readie to take paines. te modo iura dantem indigeo, dictatorem exposco./ But in his booke I am much out of loue with thes particulars. 1. first his manie and intollerable atehnies. whence deriue thos manie & vncertaine assayes of calculation. 2. his finding fault with Vieta for mending the like things in Ptol: Cop: &&. but se the iustice Vieta speakes sleightlie of Copernicus a greater than Atlas. Kepler speakes as sleightlie of Vieta a greater then Appollonius whom kepler euerie wher admires. for whosoeuer can doe the things that Kepler cannot doe, shalbe to him great Appollonius.'³

1 Ibid.

2 It is assumed that von Zach carried away most of the papers that interested him. I have searched, by correspondence, through his own papers in East Germany -- and I should like to thank the librarians in the Sächsische Landesbibliothek, Dresden, the Universitätsbibliothek, Karl-Marx-Universität, Leipzig, the Deutsche Staatsbibliothek, Berlin, and particularly, Dr. Helmut Claus of the Forschungsbibliothek Gotha, for their assistance -- but I have not found the missing Harriot material. When von Zach printed the first half of the letter of 6 February 1609/10 (which, for some reason, he thought was from Northumberland), he omitted Sir William's 'difficulty'. The two halves were first linked by Professor Rigaud, Supplement. See below, p. 235.

3 Von Zach, op. cit., and B.L. Add. MS. 6789, f. 427r, ll. 1-25.

Sir William's first commendation is a well-judged appreciation of the zeroth law. His second point, that the elliptical paths are an important new explanation of the nature of planetary motion, is, of course, important, but it is additionally interesting to read of his suggestion of a way in which they can be applied. It is difficult to know exactly what he intends, but it does appear that he is proposing that, first, the ellipse is a general solution for all cometary orbits, only the eccentricity being characteristic of specific comets, and that, second, comets behave like planets in possessing orbits that pass around the Sun and that share the basic mathematical properties of planetary orbits. It is a pity that we cannot see Harriot drawing out of Lower some of the consequences of this idea. Harriot did not write of his opinions concerning the true, as distinct from the apparent, paths of comets.¹ It is far too simplistic to believe that Lower was anticipating the cometary theory of more than three quarters of a century later, but it is striking to see him arguing not only with, but from, Kepler's theory; advances on Kepler's reasoning were to be rare for quite a few decades.

Sir William's first criticism requires the explanation that the 'atechnies' are Kepler's iterative procedures for solving for the parameters of an orbit, as when he determined the section of the eccentricity of the hypothesis vicaria by a double iteration. 'Ἀτεχνία' was the word Kepler himself used, following Viète. Kepler's own comment on it is, that if it is difficult to grasp the method of the atechny, it is much more difficult to conduct his

¹ John Roche, op. cit., pp. 212-213.

investigation without it.¹ Lower's second criticism needs no gloss, except that it provides indirectly an indication that Lower had read the statement of Kepler's Problem, as it is now called, where Kepler makes his comment that whoever can solve it shall be to him great Apollonius.²

Lower was, in fact, at the same time that he was reading Kepler, also reading Viète's second appendix to his Apollonius Gallus,³ which provided the most eminent criticism of the mathematical methods of Ptolemy and Copernicus. Had Kepler's astronomy been accepted as a whole, all at once, Viète's work would have been rendered nugatory. In reality, however, the transition from the old to the new astronomy was not abrupt, and Lower at almost the same time pondered whether Viète's work would help in

1 Kepler, Astronomia Nova, p. 95: 'Si difficilis captu est methodus, multo difficilior investigatu res est sine methodo.'

2 Kepler's words are a variation on the classical tag, 'erit mihi magnus Apollo.'

3 François Viète (1540-1603), Apollonius Gallus (Paris, 1600), reprinted in Francisci Vietae, Opera Mathematica (Leiden, 1646), p. 343. There are many pages of Harriot's work on the book throughout the manuscripts in the British Library, though very little on the second appendix, 'Appendicula II. De Problematis Quorum Factionem Geometricam Non Tradunt Astronomi, Itaque Infeliciter Resolvunt.' For Lower's work, see below, p. 128 n. 1, and the letter of 3 April 1611, B.L. Add. MS. 6789, f. 431^r, ll. 11-14. 'I fell since into Vieta's last probleme of his second apendicle Apol: Gal: and compared his way with yours that you last gaue me: but to confesse a truth I can haue my will of nether.' Little has been written on this appendix recently, but see O. Neugebauer, 'On the Planetary Theory of Copernicus', Vistas in Astronomy, x (1968), pp. 89-103. For more on Harriot and Viète, see Lohne, 'A Survey of Harriot's Scientific Writings', Archive for the History of Exact Sciences, xx (1979), pp. 288-290, and D.T. Whiteside, 'In Search of Thomas Harriot', History of Science, xiii (1975), pp. 68-69, n. 11.

the derivation of Saturn's copernican orbit.¹

More than a year later, Sir William had received Harriot's reply to his letter and was writing back, probably not for the first time.² This letter was his last on Kepler, except for a brief mention a few months afterward. By this time, he was assisting Harriot in his researches into the new astronomy. Difficulties may have appeared to Sir William independently, but for the most part, Harriot seems to have encouraged his studies in order to make use of them himself. So, to understand Lower's work in relation to his mentor's, we must turn again to Harriot and consider, at last, what he did when he finally came to possess a copy of the Astronomia Nova.

We may assume that Harriot set to work on a study of the book as soon as he had received it, if we may judge from the dates of Sir William's letters. He seems to have pursued two courses, one very extensive, and probably antedating, rather than contemporaneous with the other. The first course was a detailed reading of the book, during which he checked all of Kepler's work.

We might wish to know exactly how he read the book, but we lack any guide. It is evident from Sir William's letters quoted above that Harriot had read at least a large part of the book by February 1609/10, but along with Sir William, he was working through it a month or more later. Perhaps he, too, read it 'twice

1 Letter of 6 February 1609/10, B.L. Add. MS. 6789, f. 427^r, ll. 29-33. 'for your declaration of Vieta's appendicle it is so full and plaine as you haue abundantlie satisfyed my desire. for which I yeild you the thanks I ought. onlie in a word tell me whether by it he can solue Copernicus, 5 Cap: of his, 5. booke./'

2 Letter of 4 March 1611. B.L. Add. MS. 6789, ff. 429^r-430^v. Any intervening letters there may have been are now lost.

over cursoridlie...now with Calculation.'¹ We might like, also, to see Harriot's copy of the book, with any notes he might have made in it, but this wish is unlikely to be fulfilled. His books were left, in his will, to Nathaniel Torporley, and passed from him to Sion College, where they were probably destroyed by fire.² Nevertheless, from the number of pages of his separate notes and their disorganized form, we may assume that Harriot put most of his comments here, rather than in the margins of his copy of the Astronomia Nova. There are three distinct sets of notes on the book in the present state of the Harriot collection, but the notes were probably together at one time. What is by far the largest part of the collection is in Petworth House.³ These are in some disorder, as witnessed, for example, by their pages of summary being in the middle.⁴ They are sometimes numbered in groups, which have occasional leaves out of order. The first nine folios concern a special topic, and should and will be considered separately. The last eleven folios, with the exception of one,⁵ are by Lower, the last eight being numbered consecutively. (That

1 Above, p. 124.

2 About a third of the library was lost in the Great Fire of 1666. See E.H. Pearce, Sion College and Library (Cambridge: Cambridge University Press, 1913), p. 248.

3 Leconfield 241, Hist. MSS. Comm. 241/III.2, ff. 1-63. I have been working from a microfilm of these papers. I am certain that the blank folios have not been photographed, and I suspect that some pages may be out of order. It would be a false claim of precision if I were to attempt to quote exact folio numbers, recto and verso. I have, therefore, simply numbered the pages in the order in which they appear on the microfilm (kindly lent by Dr. John Roche), and the reader should consider any folio reference in this chapter to the Leconfield MSS. to be only an inexact page reference.

4 Ibid., ff. 23-25.

5 Ibid., f. 54.

the pages are by Sir William was first observed by Professor Rigaud.¹⁾ At least this last cluster is certainly one of the 'two supplements' referred to by Lower in his letter of 4 March 1611.² Although some of Harriot's clusters of notes can be re-ordered, or rather, their order can be reconstructed, there is little value in doing so, as their meaning is clear enough.

The second collection of Harriot's notes on the book, much smaller, is in the British Library.³ These pages are headed, 'De Anomalijs', and are less interesting than the former sheets, but will be treated in due course. The third set, also in the British Library, and headed, 'Ellipsis σ ',⁴ is in some disarray and is very obscure, consisting mostly of algebraic calculations. The analysis of these presents special difficulties and they will not be considered here. It is not evident how the three sets of notes became separated, if ever they were together. The history of Harriot's papers has been described elsewhere.⁵ The only point we need note here is that this separation may indicate that Harriot came back to the Astronomia Nova some time after his first reading or readings. Certainly, when we consider how soon after its

1 Bodl. Rigaud MS. 50, f. 62^r. This and the preceding folio contain the only account of Harriot's 'De Stella Martis' papers written before this chapter's.

2 B.L. Add. MS. 6789, f. 429^v, ll. 3-7. 'neuerthesse because you require it I haue sente them [the difficulties] in two supplements. the one vnto my letters that you haue alreadie. the other vnto thes. wch vnlesse you had remembred me of, I should haue omitted againe.'

3 B.L. Add. MS. 6787, ff. 175^r-188^r. See Appendix F.

4 B.L. Add. MS. 6787, ff. 408^r-424^r. See Appendix F.

5 Rigaud, Supplement, passim, and Tanner, 'Thomas Harriot as Mathematician, Part 2', pp. 261-264, contain much information, though many other writings on Harriot add to this story.

publication he was preparing the first set of notes, as the correspondence with Sir William bears witness, we cannot believe that the 'De Anomalijs' and 'Ellipsis δ ' antedate these.

Harriot's first concern in reading Kepler was to make sure that his astronomy was correct. We have seen what Sir William thought of Kepler's physics, but Harriot's surviving papers do not mention the 'celestial magnetics' at all. We may be justified, therefore, in thinking that Harriot held them in low regard, especially when we remember what he thought of Kepler's physical optics. This, however, did not impede his acceptance of Kepler's mathematical astronomy. Kepler's mathematical mistakes did, but Harriot corrected these.

Most of the surviving papers of the Leconfield collection are re-calculations of the parameters of the earth's and Mar's orbits, based on the data Kepler himself presented and used. Kepler presented his results in Chapter LIV,¹ but much of the book was devoted to these ends. Harriot's results appear in the midst of his three pages of disorganized comments on and extracts from the book.² These notes, each of which begins with a page reference, are in a quite random order, so that one is left with the strong suspicion that Harriot had copied them from other papers. A passage copied from the book reveals, by its being singled out, Harriot's appreciation of Kepler's achievement in astronomy. It is Kepler's most famous sentence. Harriot quotes:

1 Kepler, Astronomia Nova, pp. 264-265.

2 Petworth House, Leconfield MS. Hist. MSS. Comm. 241/III.2, ff. 23-25, reproduced here as Appendix E.

'Eight minutes led the way to the complete reformation of Astronomy.'¹

The enormous burden placed on the mere eight minutes was a challenge to others than Kepler. It was a challenge, too, to his readers, who had to be convinced that Kepler's work was correct.

It is clear from the number of Harriot's conclusions, and from the dearth of work-sheets pertaining to them, that we have but a tiny fraction of Harriot's work on the book. We do, though, have what appears to be a complete set of his notes on Chapter XXVII, and on a problem on page 333, and these will have to stand as examples of the way he approached the book.² (It would be senseless to conclude that we do have the full set of notes; we clearly do not have the work-sheets for the bulk of the conclusions, and the surviving sheets appear to give page 148 of Chapter XXVII attention out of all proportion to its importance.)

Chapter XXVII is entitled, 'From Four Different Observations of the Star of Mars, away from the Acronychal Place, but in the same position on the Eccentric, to Determine the Eccentricity of the Earth's Orbit, with its Aphelion, and a Comparison of [this] Position of the Orbits, together with the Eccentric Position of

1 Ibid., f. 23. Quoted from Kepler, Astronomia Nova, Chapter XIX, p. 114. 'octo minuta viam praeiverunt ad totam Astronomiam reformandam, suntque materia magnae parti hujus operis facta.'

2 Kepler, Astronomia Nova, Chapter XXVII, pp. 148-151. Harriot, Petworth House, Leconfield MS. Hist. MSS. Comm. 241/III.2, ff. 32-47 (on Chapter XXVII), and ff. 48-52, 54 (on page 333). See Appendix F, p. 233, for other references to p. 333. Harriot numbered the pages of his notes on page 148 as follows:

f. 32: 1.; f. 33: 1; f. 34: 1.; f. 35: 2.; f. 37: 3.;
f. 38: 5.; f. 39: 5.2⁰; f. 40: 6.; f. 41: 7.; f. 43: 4.;
f. 45: 8.

Mars, under the Zodiac.'¹ The purpose of this exercise is to confirm Kepler's earlier calculation and conclusion demonstrating the bisection of the eccentricity of the earth's orbit. Harriot compares three of Kepler's four² observations of Mars, taken from Tycho, with positions derived from the ephemerides of Maginus.³ One differs by four and a half degrees. Another of these positions (for 12 February 1589, 5:13 A.M.), he corrects from $9^{\circ} 46' \frac{2}{3} \mathcal{M}_{\alpha}$ to $9^{\circ} 47' 16'' \mathcal{M}_{\alpha}$, which is very close to the figure of $9^{\circ} 47' \frac{1}{6} \mathcal{M}_{\alpha}$ which Kepler's own manuscripts in Pulkovo show.⁴ He then writes, 'Igitur satis recte ponamus $\delta. 18^{\circ} 16'. 30'' \mathcal{M}_{\alpha}$ '⁵ (which is $4^{\circ} \frac{1}{2}$ different from Tycho's figure). Next, Harriot provides a check on the positions of the Sun, presented by Kepler in the same table.⁶ (It is not clear with whose ephemerides Harriot is checking these, but presumably, they are Maginus's.)

- 1 'Ex aliis quatuor observationibus stellae MARTIS extra situm acronychium in eodem tamen eccentrici loco, demonstrare, eccentricitatem orbis TERRAE, cum ejus aphelio, & proportionem orbium ejus loci, una cum loco Martis eccentrico, sub zodiaco.'
- 2 Kepler, Astronomia Nova, p. 148. Observations for 10 May 1585 $6^h 11^m$, Mars at $26^{\circ} 54' \frac{1}{2} \mathcal{Q}$; 28 March 1587 $5^h 42^m$, Mars at $18^{\circ} 12' \mathcal{M}_{\alpha}$; 12 February 1589 $5^h 13^m$, Mars at $8^{\circ} 46' \frac{2}{3} \mathcal{M}_{\alpha}$; and 31 December 1590 $4^h 44^m$, Mars at $9^{\circ} 46' \frac{2}{3} \mathcal{M}_{\alpha}$. Harriot, Leconfield MS. Hist. MSS. Comm. 241/III.2, ff. 33, 34, checks the first, second, and fourth of these.
- 3 Harriot appears here to use linear interpolation. Giovanni Antonio Magini (1555-1617), Italian astronomer, mathematician, and astrologer, rejected copernicanism, but his Ephemerides coelestium motuum (Venice, 1582), Continuatio Ephemeridum coelestium motuum (Venice, 1607), and Ephemeridum coelestium motuum, ab anno Domini 1608 usque ad annum 1630 (Frankfurt, 1608) became standard reference works for astronomers of any persuasion.
- 4 Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1937), iii, pp. 216, 466.
- 5 Leconfield MS. Hist. MSS. Comm. 241/III.2 f. 33.
- 6 Leconfield MS. Hist. MSS. Comm. 241/III.2, ff. 35-37.

Finally, he neatly sets out his results and Kepler's, shows the amounts of Kepler's errors, and re-calculates some of the angles used in Kepler's determination of the bisection of the earth's eccentricity.¹ Then he stops. There is no indication that he followed through with this work. Was this before or after Sir William Lower had sent him one of his difficulties? We cannot know, since the 'difficulty' has been abstracted from the letter, but on 4 March 1611, Sir William wrote to Harriot, and sent him two mathematical supplements, 'one vnto my letters that you haue alreadie.'² This supplement is most likely the first page of the notes in Sir William's hand at the end of the Leconfield collection of notes on Kepler,³ and it deals with the same page, 148, that was attracting Harriot. This manuscript page is not a complete elaboration of a problem, presumably because the first part, the statement of the problem, the initial workings out, and a drawing are in the missing fragment of the last letter. We know that there was a drawing, for Sir William writes,

'I did not mistake that, $\alpha\beta$. in the diagramme I sent you was double the eccentricitie. and therfore before the recepte of your letter in the second worke I had placed, β . att the center and, ω . att the centroide not onlie that, $\alpha\beta$. mighte still remaine with kepler the single eccentricitie, but to make it also corresponde with your viceroyall probleme. beare with this mutation./'⁴

1 Ibid., f. 45.

2 See above, p. 130, n. 2.

3 Leconfield MS. Hist. MSS. Comm. 241/III.2, ff. 53, 54. The heading contains the words 'suplementū ad litteras nostras ultimas.'

4 Letter of 4 March 1611. B.L. Add. MS. 6789, f. 429^v, ll. 24-31.

If this passage is indeed related to the supplement, then the drawing must have been similar, though not identical, to that on page 149. The 'centroide', whose label Lower changed from ' β ' to ' ω ', was a 'focus', Kepler's word, which Harriot and Lower did not adopt.

At the end of his letter of 4 March, Lower added in a postscript that,

'by the helpe of your dogs, I will revew thos workes of anie that exhibite the distance in the ellipsis equal or neerest to the same distance found before (for the workes vppon the former positions of, δ . in the eccentricke, wch were onlie to find out this, I thinke you care not for) and when I haue perfected thē, I will send thē vnto you with all the numbers giuen and found, whether the quesita consent or no, since you so require it./'¹

(The promised numbers do not survive among Harriot's papers.) This passage, in conjunction with the reference to ω as a centroid in the passage quoted before, make it very likely that, under Harriot's tuition, but at his own initiative, Sir William was repeating some of Kepler's earlier calculations, with the hindsight gained from the invention of the elliptical orbit, and that in this instance, Sir William was determining the eccentricity of the ellipse, albeit with difficulty. This is not obvious from the meagre surviving calculations alone. Still, we may observe a few things about his work-sheets.²

For one thing, the nature of Sir William's difficulty is clear. Different calculations, done in parallel, seem to lead to

1 Ibid., f. 430^r, ll. 22-29.

2 Ibid., ff. 53, 55-63.

slightly different values for some of the angles he is determining.¹ More interesting, however, is his heading on the supplement: 'posito, $\odot$.tis in eccentrico. An: 1585. May. 10. fuit in $5^{\circ} 21' 26''$. $\approx$./² This is not the figure used by Kepler, which is $5^{\circ} 22' 2''$ $\approx$,³ so we must assume that Sir William had been reworking this passage of Kepler's from scratch. He seems to have derived the position from the eccentric hypothesis -- in the postscript just quoted, it is fairly clear that this is what he did -- before proceeding to use the position in the elliptical hypothesis. Because the use of different observations causes the equant of the hypothesis vicaria to vary somewhat about its mean position, thus changing the direction of Mars' major axis, and because Kepler continued to use this hypothesis as the basis on which he calculated longitudes for other models, it is possible, using different data for any of his models, to arrive at planetary positions, for a given date, that vary by as much as two or three degrees.⁴ This is probably what happened in the case of this particular calculation by Lower.

The last observation to be made on these pages is that, when calculating distances, Sir William is giving the standard, the distance from Mars to the Sun, a value of 10,000,000 parts, rather than the 100,000 parts that Kepler used, and so is using greater precision, perhaps not altogether justified, in his calculations. (Since Mars is assumed to be at the same point in its orbit at the various times used in this exercise, there is no difficulty caused

1 This is shown very clearly on *ibid.*, f. 53.

2 *Ibid.*

3 Kepler, Astronomia Nova, p. 148.

4 D.T. Whiteside, private communication, Dec. 1980.

by the otherwise variable distance between it and a given focus (the Sun), of its elliptical orbit.)

In further elucidation of Sir William's postscript, I should add that the 'dogs' are trigonometrical formulae or algorithms and formulae of interpolation invented by Harriot, and the 'viceroyall probleme', in the earlier passage, is another of these. The latter appears again,¹ but there is not enough information here to determine what it is.

Sir William's second set of supplementary notes,² though extensive, tells us little that we have not learned from the preceding set. It is a re-working of Kepler's Chapter XXVIII,³ in which he again proves the bisection of the eccentricity of the earth's orbit. Whether Sir William was again checking the value of the eccentricity of an elliptical orbit is not clear, but this could be determined by reconstructing his drawing, by using the angles he has given. The demonstration would, however, be of only slight interest, adding little to our knowledge. (The last page is a discussion of one of Harriot's 'dogs', which, as he writes at the beginning, 'hunts slowlee; or I not vnderstand him yet.'⁴ Sir William here demonstrates what his understanding of the dog's use was. What is, perhaps, a draft of Harriot's reply is to be found

1 Lower, Leconfield MS. Hist. MSS. Comm. 241/III.2, f. 57, where Sir William is working on Chapter XXVIII (p. 152).

2 Ibid., ff. 56-63.

3 Kepler, *Astronomia Nova*, pp. 151-157. The chapter is entitled, 'Assumptis non tantum locis SOLIS sub zodiaco, sed etiam distantis SOLIS a TERRA, per eccentricitatem 1800 extructis; per aliquammultas observationes MARTIS in eodem loco eccentrici versantis videre, an unanimi consensu eadem distantia MARTIS a SOLE, idemque locus ejus eccentricus ubique eliciatur. quo argumento comprobatur erit, eccentricitatem SOLIS 1800 justam esse & recte assumptam.'

4 Leconfield MS. Hist. MSS. Comm. 241/III.2, f. 63.

earlier in the Leconfield papers.¹ Alternatively, this last bit of work could be an initial draft of the invention of the algorithm.)

The Kepler work is mentioned only one more time in Sir William's correspondence. On 19 July 1611, he writes:

'Since you incourage me so much I will proceede in thos calculations of, ϕ . and as I finish anie I will send them vnto you. indeed to find the issue of diuers and in the later so impossible to be reconciled had vtterlie discouraged me, but that now by your letter I perceauē ther may bee good vse made euen of ther discordance; therfore of this I will say no more till I send you more.'²

We have seen that Sir William had already been having trouble, producing conflicting results, but in this gap of several months, he had probably moved on to other aspects of Kepler's book. There survive no more letters or notes of Sir William's, but since he lived until 1615, we may assume that he went on working with Harriot until then, and that he was continuing to work on Kepler after this last letter. The remains of the working relationship of Sir William and Harriot are almost entirely the former's, except for their possession by the latter. They seem to show Harriot assisting Sir William, which, when one considers the qualities of the two men, would look like condescension to an unusual degree, but for suggestions in Sir William's letters that Harriot was interested in the results of his calculations. Indeed, this probably was not mere flattery, for Harriot considered that his slower companion could assist his own reading of Kepler. As a glance at Appendix E shows, there are no major corrections of

¹ Ibid., f. 5.

² B.L. Add. MS. 6789, f. 433^r, ll. 1-8.

Chapters XXVII and XXVIII among Harriot's list; nevertheless, he may well have used Sir William's results toward other ends, corrections of other values. In fact, Harriot's own corrections of page 148 are not listed here, yet we may conjecture at the end toward which he wished to use them if we consider f. 19. This is headed, 'Observations convenient for finding the apogee and eccentricity of the Sun and the distances of Mars from the Sun',¹ and is followed by a list of dates and times of observation of Mars compiled from throughout the Astronomia Nova, including page 148. On f. 20 is a preliminary working diagram. We lack the rest of the work, as we do so much else, but we may suppose that neither Harriot's work on page 148 nor Sir William's different work on the same page and on the next chapter was for nought.

Harriot's other collection of worksheets on Kepler among the Leconfield MSS. is concerned with page 333, one of the last pages of the book.² Here, in Chapter LXIX, Kepler reconsiders three ptolemaic observations and checks them against his theory. On page 333 in particular, he calculates the length of the sidereal year, the time the Sun occupies in completing a circuit from a point fixed with respect to the stars, this being an essential element in any prediction or retrodiction of a planetary position. Harriot's work is a re-calculation of the length of the sidereal year and a comparison of this with different values, such as the ones derived

1 'Observationes adcomodā ad inveniendū apogaeū et eccentricitatem Solis et distantias ☿ à ☉.'

2 Ff. 48-52, 53. See also Appendix F, p. 233.

from the Prutenic Tables¹ and from Tycho, and the value he, himself, had been using. He also re-calculates the length of the tropical year.² This is all, to a large extent, simple arithmetic.

Harriot's results from these calculations are worth noting, as are the values of the parameters of Mars' orbit that he lists on his summary page. These are the more important planetary data. As we can see, Harriot's results are not a great improvement on Kepler's. Kepler's errors must have been fairly small, and both men were working within the limitations of Tycho's data.

1 Prutenicae tabulae coelestium motuum (Tübingen, 1551, 1562), ed. M. Mästlin, (Tübingen, 1571), the work of Erasmus Reinhold (1511-1553), were calculated by means of Copernicus's theory, and being rather accurate and therefore widely adopted, they helped to popularize the theory.

2 Leconfield MS. Hist. MSS. Comm. 241/III.2, f. 51. 'Annus Tropicus. 365^d. 5^h. 48'. 59". 21'''.' The tropical year is the time between two successive passages of the Sun through the vernal equinox.

Harriot (source) Kepler (source) Modern¹

Eccentricity	0.092652	f.24	0.093008	p.267	0.093037
Perihelion ²	329° 40'	f.24	329° 40'	p.262	328° 41' 41".25
Semi-Major Axis ³	152,349.5	f.24	152,342	p.267	152,369
Sidereal Year	365 ^d ₆ ^h ₉ ^m ₄₁ ^s ₁₃ ^{'''}	f.51	--4	--	365 ^d ₆ ^h ₉ ^m ₉ ^s ₂₈ ^{'''} .2

Table I

The two other remaining sets of Harriot's notes on the Astronomia Nova display a character very much different from that of those papers we have been examining.⁵ They are closely related to one another and to some of Harriot's other work on conic sections,⁶ and as they are more or less purely mathematical, being only tangentially related to keplerian astronomy, which was,

1 Source: Explanatory Supplement to the Astronomical Ephemeris (London: H.M.S.O., 1974), p. 111. Values are retrodicted to 1600.

2 I have taken Harriot's and Kepler's value for the position of the aphelion and have added 180°. Harriot seems to have adopted Kepler's value after due consideration. The large error is probably a fortuitous result owing to a combination of Kepler's careful selection, for another purpose, of the four observations on which he based the hypothesis vicaria, and to the incapacity of the double iteration performed in its development of correcting for any moderate error in the direction of the line of apsides. (D.T. Whiteside, private communication, Dec. 1980.)

3 Where the astronomical unit = 100,000 parts.

4 Kepler does not give a value for this. Harriot derived his value from Kepler's data on page 333 of the Astronomia Nova.

5 See above, p. 130, n. 3,4.

6 One set of notes on conic sections is at Petworth House, Leconfield MS. Hist. MSS. Comm. 241/I.4. Other relevant ones are referred to in Appendix F, p. 232.

however, Harriot's inspiration for these studies,¹ a mere précis of their contents should suffice here.

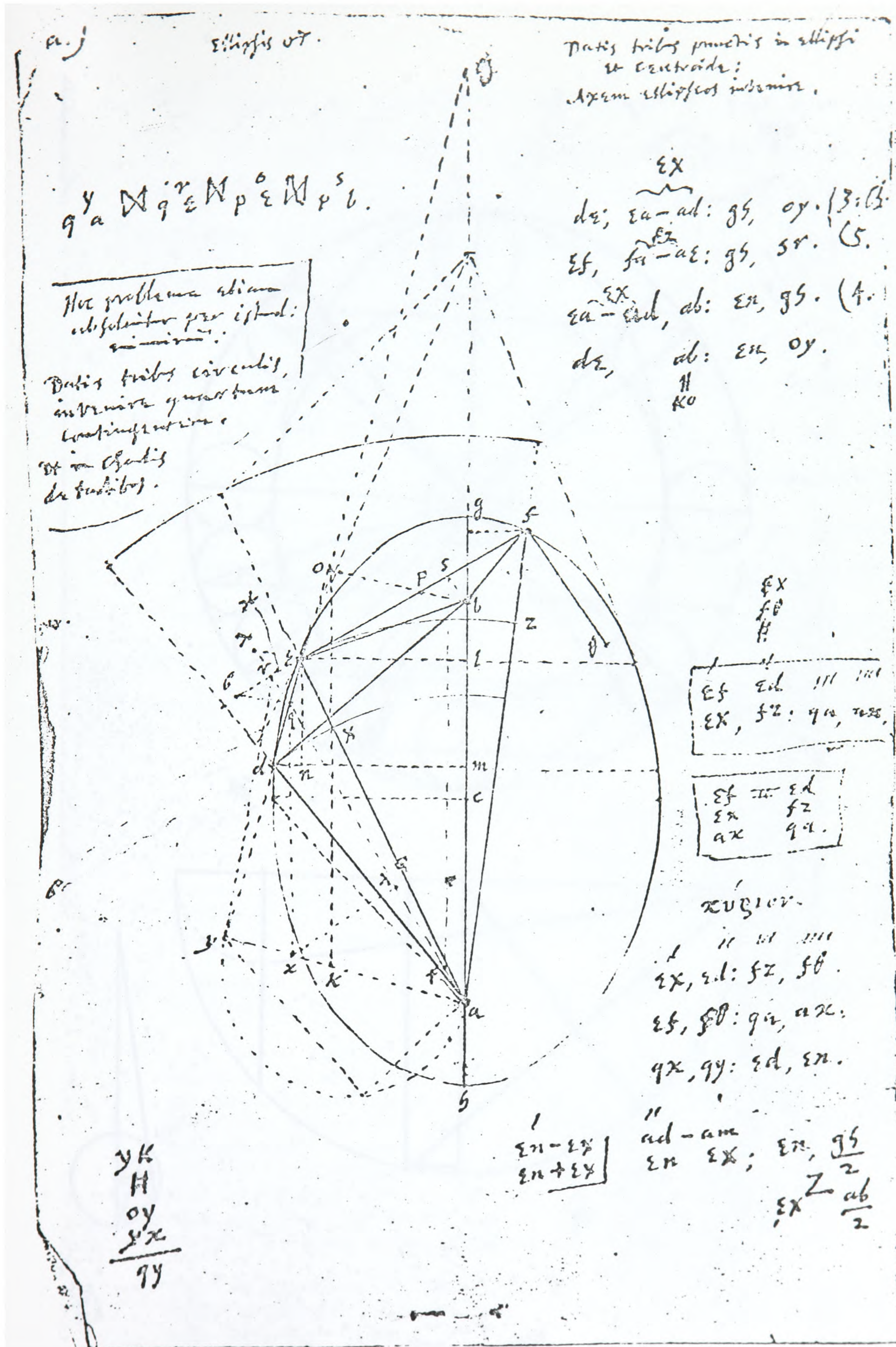
Principally, the problem that Harriot was solving, as seen in Plate I, was: 'given three points on an ellipse and a focus, to find the axis of the ellipse', which, Harriot pointed out, could be solved by means of the related apollonian problem: 'given three circles, to find a fourth touching them'. Harriot constructed at least a couple of solutions to this problem,² which clearly fascinated him, for diagrams bearing on related problems of 'de tactibus [circulorum]' are scattered throughout those of his papers that are in the British Library. It is just possible that he had in mind an astronomical application of the solution, that is, if he took the Sun as one focus and knew three positions of a planet with respect to the Sun, he could determine the length and direction of the orbit's major axis (and its eccentricity)³ but for lack of any further evidence to support this conjecture, we must assume that Kepler's description of the ellipse had fired in Harriot an interest in one case of a general problem in pure mathematics.

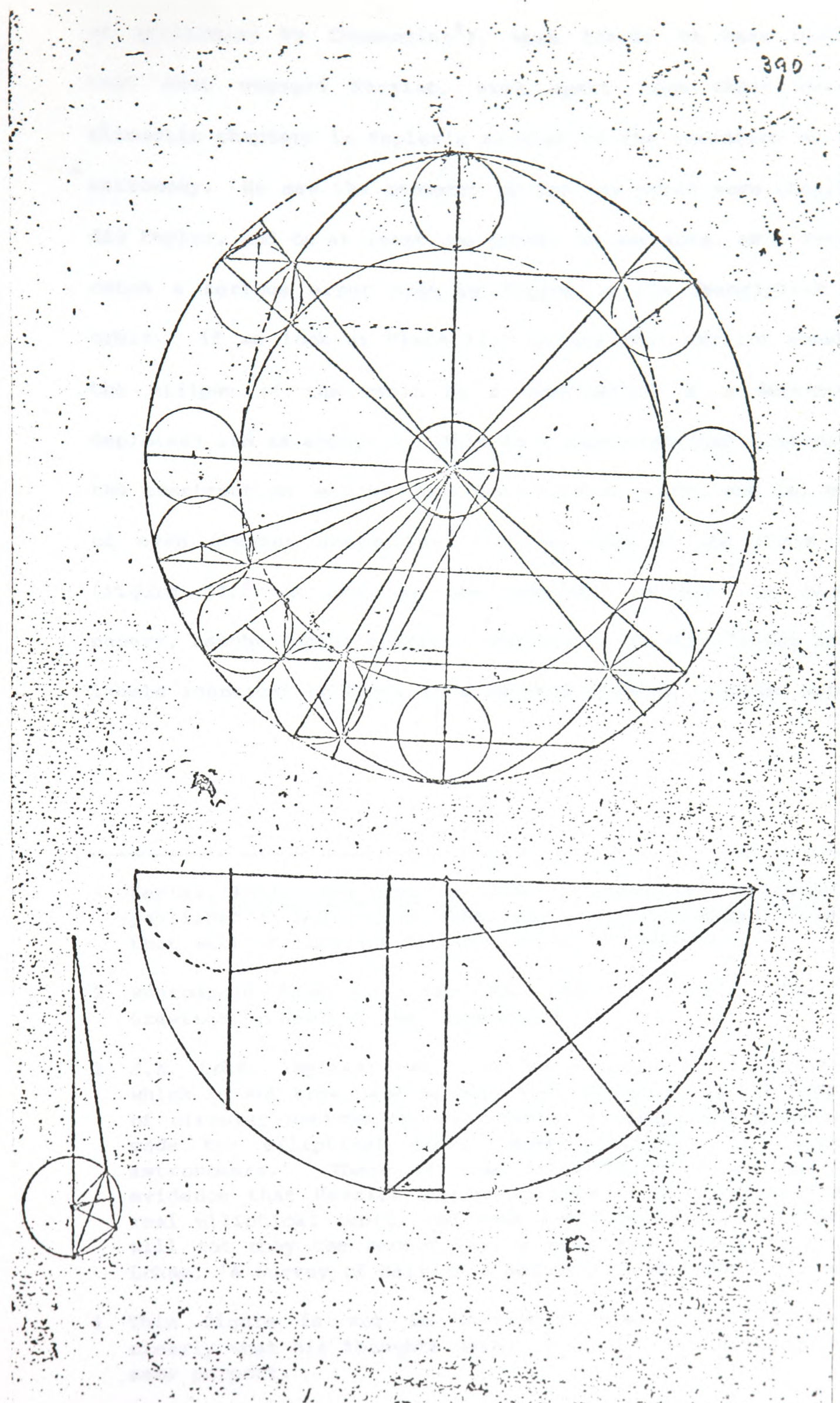
Indeed, Kepler's chapters LVIII, LIX, and LX, the three that drew most upon the results of apollonian geometry (from the edition

1 As is evidenced by the very fact that they are headed, 'De Anomalijs [Planetarum]' and 'Ellipsis 3'. That the mathematics were inspired by the astronomy is further witnessed by the fact that lines of construction in the drawing of Plate I are absent from the similar, but neater, drawing at B.L. Add. MS. 6787, f. 511^r, which is devoid of astronomical references.

2 B.L. Add. MS. 6787, f. 176^r. The construction is, basically, that of a circle of centre b, touching the three given circles of centres d, e, and f, each of which passes through a single point, a. Then, a and b are the foci of an ellipse upon which lie points d, e, and f, and the radius of the fourth circle is the length of the major axis of the ellipse.

3 The fact that the distances of the planet from the Sun would have to be accurately known for all three positions would probably be sufficient to preclude the astronomical application.





B.L. Add. MS. 6787, f.390^r

of Apollonius by Commandino¹), were likely to have been those that most engaged Harriot, even apart from their being the climactic chapters in Kepler's account of the invention of the new astronomy. He saw the geometry of the new orbit more clearly than did Kepler, and on at least one point, he was able, as a result, to catch a serious error made by Kepler in his description of the orbit. If we look at Plate II,² we see that Harriot constructed the ellipse of the orbit by a combination of a deferent (not depicted) and an epicycle. This is a more practical alternative to the construction we have just seen Harriot using, and is, perhaps, of even greater antiquity.³ If we look at the plate closely (Figure I),⁴ we can see how Harriot, in his 'De Anomalijs' papers, caught Kepler's error. The points in this figure are given labels identical to those in a related drawing, without epicycles,

1 Kepler, *Astronomia Nova*, p. 289. Commandino's translation was published in Bologna in 1566, and contained lemmata from Pappus that were of interest to both Kepler and Harriot.

2 Reproduced from B.L. Add. MS. 6787, f. 390^r. For similar drawings by Harriot, see Appendix F, p. 232.

3 J.A. Lohne implies that this construction was Harriot's own, which is not true, and he says that it could, by its combination of circular motions characteristic of classical astronomy, have made the elliptical orbit 'more acceptable to contemporary astronomers.' There is some truth in this, but there is no evidence that Harriot suggested this as a substitute for the real elliptical orbit. Without modification, this construction will not obey the second law or even approximate to it. J.A. Lohne, 'A Survey of Harriot's Scientific Writings', p. 269.

4 This figure is not in Harriot's papers, but it represents clearly what his thoughts were. Harriot's own drawings have the same purport.

clear account of it was not published until 1621, when it is unlikely that Harriot would have seen it, he being very ill before he died in that same year. Another point of interest in the Astronomia Nova that was yet of no interest to Harriot was the so-called Kepler's Problem. One would have thought that the mathematician in him would have singled out this difficulty. He did not. And last, there is Kepler's third law. There is no reference to Kepler in Harriot's writings that we might date after the period of the surviving correspondence with Sir William Lower. The third law, published in 1619, would, then, have had to inspire Harriot afresh to work on astronomy. By this time, however, Harriot's illness was far advanced, and on 13/23 June 1619, he wrote to the earl of Northumberland about his work on collisions:

'but my infirmitie is yet so troublesome that I am forced as well that, as other traits [tractates] to let alone till time of better abilitie.'¹

It is likely that, when the Harmonice Mundi arrived in England, Harriot, who must surely have known of it, was too ill to devote himself to prolonged study of it.

Harriot's work on Kepler, with the aid of Sir William Lower, was carried on in isolation. Harriot had a circle, of such men as Walter Warner, Nathaniel Torporley, and Sir Thomas Aylesbury, but there is no evidence of their having read Kepler or having passed Harriot's studies on for others to consider. (Torporley, however, at least saw the notes on Kepler when he included them in his index

¹ B.L. Harleian MS. 6002, ff. 21^r-21^v. This is a copy in the hand of Sir Charles Cavendish (1591-1654).

of Harriot's papers.¹⁾ Had Harriot's work been known, it would doubtless, through Harriot's own reputation and through the thoroughness of his corrections, have added to Kepler's respectability in England. Nevertheless, it would not have advanced astronomy so much as the study of astronomy. Harriot did little that was creative with Kepler's discoveries. What he did was therefore very much the result of his own style as a scientist. He was precise, and liked 'hard facts', such as planetary positions, and so he corrected Kepler's numbers. He also liked theories that led to hard facts, and so he adopted elliptical orbits, presumably because Kepler had satisfied him that this was the best astronomical theory. He saw that it overthrew the circular astronomy, but Harriot was not very dogmatic, and once he had observed this, he had little more to say. Kepler's magnetic physics had no attraction for him, and he may be forgiven his failure to see the significance of its role in aetiological astronomy (if he did so fail), because, in fact, Kepler's physics were, despite Kepler's intention, rather ad hoc, and his astronomy still basically kinematic. On the contrary, the astronomy inspired mathematical investigations. Harriot used his knowledge of Kepler neither to advance the study of astronomy in England nor to advance his private studies. Nevertheless, he was the first Englishman to appreciate, to understand, and to accept Kepler's most finished achievement of the first years of the 1600s, the law of elliptical planetary motion.

1 B.L. Add. MS. 6789, ff. 448^r-450^r.

VII. CHRISTOPHER HEYDON

Sir Christopher Heydon was not by any reckoning a profound thinker, but his fascination with the intricacies of astronomy and physics was genuine. He is the only man to be considered here who was not a professional mathematician, and on the contrary, he paid professionals to do the dirty work of calculation for him; he was a patron of mathematicians. He was, despite his being a pedant, a representative thinker of his time and is unique among his English contemporaries in having left us in his correspondence¹ a fairly extensive record of his research and thought. Harriot's correspondence, by contrast, is very patchy and consists mostly of letters not from, but to him. As Heydon's biography has never been written, it will be necessary to write of him at slightly greater length than his scientific work merits.

Sir Christopher Heydon was born into a gentle family, whose

¹ Heydon's scientific and pseudo-scientific correspondence is to be found at: Bodl. MS. Ashmole 242, ff. 2^r-70^v (scattered among his other papers), ff. 162^r-170^v; Trinity College Library, Dublin MS. 382, ff. 59^r-71^v, MS. 387, ff. 52^r-52^v, MS. 386/6 ff. 157^r-160^v (Heydon's observations with Edward Wright); Gonville and Caius College, Cambridge MS. 73, ff. 348^r-388^v (twenty-one letters); William Camden, *Gulielmi Camdeni, et Illustrium Virorum ad G. Camdenum Epistolae*, ed. Thomas Smith (London, 1691), pp. 128-131, 165-167 (On page 129 (letter of 6/16 July 1610), Heydon briefly describes for Camden his observations of the heavens through a telescope, so it seems that he was one of the first users of the new instrument in England); Johannes Kepler, *Gesammelte Werke*, xv, ed. Max Caspar (Munich: C.H. Beck, 1951), pp. 148-150, 231-239. There are several other Heydon letters on various matters scattered about other manuscript sources. I hope to publish much of the scientific correspondence in a forthcoming study of Heydon.

ancestors in the fifteenth century had created the family fortune and established their principal home not far from the village of Heydon, in the Norfolk village of Baconsthorpe, a hamlet that ^{had} been given its name by the Bacon family and had already become associated with intellectual endeavour in the person of the philosopher, John of Baconsthorpe (d. 1346).¹ The Heydon family wealth grew, so that it encompassed several estates, then levelled off, it seems, and finally started to dissipate under Sir Christopher's father, William.² It was still declining when Sir Christopher suffered his 'unhappiness', which I will mention again.³ The family at last disappeared from public view after the civil wars, with the death of Sir Christopher's collateral

1 William Camden, Britannia: or, a Chorographical Description of the Flourishing Kingdoms of England, Scotland, and Ireland, and the Islands Adjacent; from the Earliest Antiquity, translated and enlarged by Richard Gough (London, 1789), ii. 112. Francis Blomefield, An Essay towards a Topographical History of the County of Norfolk, vol. vi., by Charles Parkin (London, 1807), pp. 508-510. The early Heydons were prominent rivals of the Pastons, and are often mentioned unfavourably in the letters of that family. H.S. Bennett, The Pastons and their England (Cambridge: at the University Press, 1970), pp. 5, 9, 167, 185.

2 S.E. Rigold, Baconsthorpe Castle (Department of the Environment Official Handbook), (London: Her Majesty's Stationery Office, 1976), pp. 4-5. The family's decline seems to have been linked to some extent with the decline of the woolen industry; *ibid.*, and Joan Thirsk, ed. The Agrarian History of England and Wales, Vol. iv (Cambridge: at the University Press, 1967), pp. 638-640. Furthermore, William Heydon, in return for a quick profit, began to fell more trees on his lands than was conducive to long-term gain, and he tried to have Baconsthorpe Castle razed. This led his own son, Christopher, to sue him; Blomefield, *op. cit.*, p. 508; Acts of the Privy Council of England, New Series, vol. xxii: 1591-1592 (London: Her Majesty's Stationery Office, 1901), pp. 160-161; *ibid.*, vol. xxiv: 1592-1593 (London: H.M.S.O., 1901), pp. 11-12, 371-372.

3 See below, p. 154. 'vnhapines'; Gonville and Caius MS. 73, f. 350^r.

descendent, a disreputable astrologer, John.¹

Of Sir Christopher's early life, we know rather little, not even the year of his birth. However, the activities of his grandfather and father hint at his intellectual inheritance. The family had long been linked with the University of Cambridge.² Sir Christopher Heydon, the grandfather of our Sir Christopher, left a scholarship to Gonville and Caius College and added to the endowment of its library, though neither he nor his namesake was ever enrolled there.³ The family was puritan, one of the few families among the Norfolk gentry so inclined, and the ecclesiastical authorities found it necessary to keep in close touch with them for this reason.⁴ Sir Christopher has left no trace in his writings of his own religious inclinations, and there seems rather little to connect Heydon's family's puritanism with

- 1 For John Heydon (1629-?), see article 'Heydon, John (fl. 1667)' in DNB, and C.H. Josten, ed., Elias Ashmole: (1617-1692) (Oxford: at the Clarendon Press, 1966), ii, p. 734, n. 3. Eventually, long after Sir Christopher's death, the family sold their main seat, Baconsthorpe Castle, now a picturesque ruin; Rigold, op. cit., p. 6.
- 2 John Venn and J.A. Venn, Alumni Cantabrigiensis, Part i: From the Earliest Times to 1751, vol. ii (Cambridge: at the University Press, 1922), p. 363, and John Venn, Biographical History of Gonville and Caius College, vol i: 1349-1713 (Cambridge: at the University Press, 1897), pp. 96, 164. Several members of the family were educated at Gonville and Caius College, which had strong East Anglian associations; *ibid.*, p. xiii.
- 3 *Ibid.*, vol. iii (Cambridge: at the University Press, 1901), p. 245, and Anonymous, 'List of Exequies' (privately printed sheet posted in the library of Gonville and Caius College, 1970).
- 4 Patrick Collinson, The Elizabethan Puritan Movement (London: Jonathan Cape, 1967), p. 203, and The Letter Book of John Parkhurst Bishop of Norwich: Compiled During the Years 1571-5, ed. R.A. Houlbrooke (Norfolk Record Society Publications, vol. xliii, 1974 and 1975), pp. 45-46, 49, 51; Cambridge University Library MS., 'Parkhurst Epist.' Ee. 2. 34., ff. 99^v-101^r, 122^v, 152^v.

his scientific interests. Indeed, one of his sons was a royalist officer in the civil wars.¹

We first hear of Sir Christopher himself when, in 1576, he matriculated at Cambridge, as a member of Peterhouse.² Here he befriended the young earl of Essex -- perhaps Heydon was the same age (aet. nine) -- then a student at Trinity College.³ It is difficult to determine whether Heydon received any scientific training at Cambridge. Many years later, he complained that he had 'no knowledge of physik' (physiology)⁴, and he was unskilled in mathematics, but this does not exclude the possibility of his scientific interests having been fostered by the university.⁵ Heydon naturally became an aristotelian, but not, at least in maturity, a dogmatic one. In 1607, he wrote to Richard Foster:

1 Article 'Heydon, John (d. 1653)' in the DNB.

2 Venn and Venn, Alum. Cantab., loc. cit. In the same year, Christopher Heydon was to be found near the rear of his grandfather's funeral procession; Bodl. MS. Ashmole 818, f. 26^r

3 Bodl. MS. Ashmole 1729, f. 200^r (Heydon to Cecil).

4 Gonville and Caius MS. 73, f. 356^r (Heydon to Fletcher). Heydon wrote also of his education: 'When I do confesse my selfe far frō the title of a scholar, and neyther sufficiently grounded when I was of y^e vniuersity, nor practised in y^e exercise of lerning since to atteyne to y^t pfection in any thing y^t nedeth not y^e assistance and helpe of an other y^t is iudicio []'; Gonville and Caius MS. 73, f. 358^r (Heydon to Fletcher).

5 Heydon was listed by Gabriel Harvey among mathematicians who 'begin to carrie credit', in a note said to have been written in 1580; Virginia F. Stern, Gabriel Harvey: His Life, Marginalia and Library (Oxford: at the University Press, 1979), p. 168. Heydon is there included among eminent company, a ranking that for any date seems grossly inappropriate, but especially so for the early date of 1580. Nonetheless, the internal evidence proves the note to have been written no later than the early 1590s, and this suggests that, quite early, Heydon had received mathematical training.

'I perceau [that] yo^u thinke some of my groundes, and positions strang from the receaued Philosophy in Schooles: But I hope yo^u conceau neuer the worse of me for that. ffor is Aristotle hymself, (who is the oracle of the Schooles) assumed liberty to abandon his Master Plato, where Plato swerued from the trueth); Why maye not I wth like freedome imitate hym, and saye, I loue Aristotle well, but trueth better? There is no profession, whereof Aristotle hath written, but is wronged notoriously by his erro^rs, as in Phisike...If therefore accordinge to my poore talent, I doe either Addere Inuentis w^{ch} is Facillimū, or while I forsake the co^mon, and directe cutte, bringing nothing of my owne, w^{ch} is not grounded either vpon Geometricall demonstration, or the Authority of the best learned, or of reason it self, and experience; I hope in this case, yo^u will not blame my zeale...'¹

The next twenty years of Heydon's life are of small concern to us, though they were of great moment to him, encompassing as they did two marriages, a dispute with his father, election as a member of parliament (in a closely-fought contest), his father's death, the legal defence of his pugnacious younger brother, and the raising of a large number of children.² It is clear, however, that his interest in astronomy had developed by the mid-1590s, for he has left records of solar observations made in 1594-1597 in London with his fellow East Anglian, Edward Wright, the expert on navigation.³ These two men also jointly made observations of Mars at about this time, as Wright testified in 1599, and they were

1 Bodl. MS. Ashmole 242, f. 15^r.

2 Article 'Heydon, Sir Christopher (d. 1623)' in DNB; J.G.N., '"Heydon with One Hand": An English Duel in the year 1600', Gentleman's Magazine, vol. xxxix (1853), pp. 481-488; Heydon had fourteen children; Bodl. MS. Ashmole 1729, f. 202^r (Heydon to Cecil).

3 For some of the evidence of Heydon's friendship with Wright, see T.C.D. MS. 386/6, ff. 157^r-160^v and MS. 387, ff. 52^r-52^v.

aware of the astronomical work of Tycho Brabe.¹ Perhaps by comparing their own observations with positions derived from common tables, Heydon gained a limited ability to appreciate the value of Kepler's work on Mars.²

In 1597, Heydon, as a navigator, perhaps, joined the expedition that concluded with the sack of Cadiz, and for his part in this enterprise, he was knighted, along with many others, by his old friend, Essex.³ In 1601, Essex very nearly swept Heydon away with himself. Heydon later claimed, in a petition to Cecil, that, encountering Essex in the street in London, he had been duped by the earl, for whom he still faithfully, and pathetically, professed great warmth of friendship, into joining the rebellion. Heydon was soon released from prison, but his fine of two thousand pounds must have brought him to the verge of ruin.⁴

1 Edward Wright, Certaine Errors in Navigation Arising either of the ordinarie erroneous making or using of the sea Chart, Compasse, Crosse staffe, and Tables of declination of the Sunne, and fixed Starres (London, 1599, corrected reprint, London, 1610), sig. ¶¶ 2, reference to Heydon, Tycho, the Landgrave of Hesse, and Wright himself as fellow observers of errors in the published declinations of the fixed stars; p. 349 (1610 edition only), 'But for the making of the table of the Suns Prosthaphaereses, because it may be gathered not only out of the obseruations of Mars, that both my selfe, together with that honourable and learned Knight Sir Christopher Heydon, haue taken for many yeeres...'

2 See below, p. 171.

3 Wm. A. Shaw, The Knights of England (London: Sherratt and Hughes, 1906), ii. 92.

4 See Heydon's petitions to Sir Robert Cecil; Bodl. MS. Ashmole 1729, ff. 200^r-206^r (Some of these letters are not autograph). He describes the state of his finances at f. 202^r. Heydon was free by 10/20 November 1601; see his letter of that date, from Baconsthorpe; Gonville and Caius MS. 73, f. 350^r (Heydon to Fletcher). It has been suggested that one measure of his poverty is that his eldest son, Sir William (on whom see Venn, Biographical History, i. 164), was forced to become a soldier.

From this time until his death in 1623, Heydon seems to have lived a largely contemplative and scholarly life. He had already accumulated a very large library.¹ His work as a justice of the peace often brought him into communication with Nathaniel Bacon of Stiffkey, half-brother of Francis Bacon,² but neither Heydon nor the lord chancellor mentions the other in his writings, nor is any Baconian influence to be detected in Heydon's work. He was what later ages would call a dilettante, but he was also a patron, as well as a friend, of distinguished astronomers. (He does not appear to have patronized Wright, however.) His first major collaboration, after working with Wright, was with a well-known astrologer, John Fletcher, of Caius College, whom he had chosen to be tutor to his son.³ Heydon has always been best remembered as a defender of astrology, and the subject was in large measure the cause of his writing to Kepler.

1 Acts of the Privy Council of England, New Series, vol. xxxi: 1600-1601 (London: His Majesty's Stationery Office, 1906), p. 194 (a letter to the high sheriff of Norfolk concerning Heydon's possessions sequestered after his arrest; 1 March 1600): 'Where wee are likewyse informed that there is a great library in one of his houses and that the bookes are purloyned, wee require you to have care that none of them be taken away but kept together and locked up in the study where they are.' Unfortunately, because Heydon died intestate, as did the son, Miles, who was made his administrator and died five years later, there is no inventory of his books and instruments; J.H. Morrison, Prerogative Court of Canterbury -- Letters of Administration 1620-1630 (Inclusive) -- Abstracts, Translated from the Original Latin (London: J.H. Morrison, 1935), p. 53 (nos. 3549 and 3552).

2 B.L. Add MS. 41140, ff. 75^r-76^v (1609), 100^r-101^v (1616), 140^r-141^v; B.L. Stowe MS. 150, ff. 224^r-225^v (1608).

3 Fletcher helped Heydon to write his A Defence of Iudiciall Astrologie. Most of the Gonville and Caius MSS. of Heydon are Heydon's letters to Fletcher concerning the preparation of the book. For a brief biography of Fletcher, see Venn, Biographical History, i. 95.

In 1601, John Chamber (1546-1601), fellow of Eton and canon of Windsor, published A Treatise Against Ivdicial Astrologie (London), and this prompted the lately-freed Heydon, with the paid assistance of Fletcher,¹ to respond with A Defence of Ivdiciall Astrologie (Cambridge, 1603), which work made Heydon famous throughout England. For several decades, astrologers resorted to his book when they were under attack. It is long and tedious.² Much more interesting is Heydon's second book, published posthumously, An

1 For evidence of payment, see Gonville and Caius MS. 73, f. 358^r.

2 Don Cameron Allen, The Star-Crossed Renaissance: The Quarrel about Astrology and Its Influence in England (Durham, North Carolina: Duke University Press, 1941), p. 129, has accurately described the conflict: 'Sir Christopher Heydon, who wrote the reply to Chamber's work, was a man of a completely opposite character. Chamber is light and concise; Heydon is ponderous and prolix. Chamber is witty and enamored of literary and topical allusions; Heydon is downright pedestrian and tramps through his discussions on feet of lead. We are convinced, however, that Heydon knows his subject much better than Chamber does, for he has read widely enough in the literature of the astrological controversy to detect like a conscientious pedant all of Chamber's unacknowledged borrowings.' Chamber, stung by Heydon's vehemence, wrote a charming counterblast, but he died before it could be published. The beautifully written and bound manuscript volume, on whose frontispiece is written, 'From Windesore Chappell. Februarij. 2. 1603. Your Maiesties most humble subiect and servant, Iohn Chamber,' is now to be found at Bodl. MS. Savile 42. In his conclusion, Chamber describes the various ways he might have replied to Heydon; some of his friends 'would haue had me to haue painted the purple fishe & send yt him for a token. for by this fishe they weare wont to signifie in old time such venemous railers as wth bitter words did pinshe thoroughe & paye home after the manner of that fishe whose tongue is so hard & sharpe that it will pierce thoroughe the hardest shelfishe;' f. 230^r. This book might well repay further study by historians of folk belief. Another reply to Heydon was George Carleton, ΑΣΤΡΟΛΟΓΟΜΑΝΙΑ. The Madnesse of Astrologers. Or An Examination of Sir Christopher Heydons Booke... (London, 1624), which was written nearly twenty years before it was published.

Astrological Discourse,¹ which was probably written in about 1607. Several strands of scientific tradition are brought together here; Heydon disagrees with Kepler over the doctrine of qualities and the neo-pythagorean harmonic system, as applied to astrological theory by Kepler, but he treats Kepler, unlike Chamber, as a worthy opponent, and he often uses Kepler's work as the basis for his arguments.

Heydon believed that the planets and the stars affected the sublunary world by means of 'beams', like beams of light. In Chapter VII ('The method set down which is observed in refelling Kepler'), Heydon, building up the case for astrology from the fact of astronomical correlations with seasonal changes,² writes:

'...because it concerneth the very Foundation of Astrologie, I must crave leave to answer, not doubting but if I can demonstrate that both Cold and all the rest of the first

1 An Astrological Discourse With Mathematical Demonstrations, Proving the Powerful and Harmonical Influence of the Planets and fixed Stars upon Elementary bodies, in an Astrological Judgement upon The great Conjunction of Saturn & Jupiter 1603. Written by that worthy learned Gentleman Sir Christopher Heydon, Knight, and now published by Nicholas Fiske, Iatromathematicus. London...1650 (Bodl. [MS.] Ashmole 297; the copy at Bodl. 8^o. I. 86. Linc. has a different title page, lacking Fiske's name). The book contains a foreword by William Lilly (1602-1681), who held a very high opinion of Heydon. In his copy of George Carleton's book (Bodl. Ashmole 551), which he annotated with 'scurrilous, trite, and empty notes in MS.' (Wood, Athen. Oxon., ii. col. 424), he writes, p. 82, 'S^r Ch. Heydon was an admirable [astrologer]: of whose workes I haue seen many.' The autograph MS. of the book is at Bodl. MS. Ashmole 242, ff. 2^r-12^v. The date of composition is roughly determined by a reference to it in a letter from Heydon to Richard Foster (I owe the identification of Foster to Dr. M. Feingold), 1/11 March 1607/08; Bodl. MS. Ashmole 242, f. 15^r. All my quotations, however, are drawn from the printed version.

2 This is a traditional argument, dating back at least as far as Ptolemy's Tetrabiblos; see Ptolemy, Tetrabiblos, ed. and translated by F. E. Robbins (bound with Manetho), (London: William Heinemann, Ltd., 1940), pp. 7-19.

qualities do proceed essentially from Heaven, and are only to be found by accident in the Elements...And the better to clear this point, I will first shew, that Heat is neither essential to the Light, nor so inseparably united unto it, but that they are and may be severed as diverse in Nature...Which thus premised, the Reader shall be the better prepared...to conceive how Intentions & Remissions of every season may grow by the particular passions of the Planets and the rest of the Stars, and not from naked Privation, as Kepler would have it.¹

That is, cold is a quality, and not merely the absence of heat. Heydon is defending a well-known doctrine. Kepler was not, of course, the only person to have held the contrary opinion -- Heydon cites Picus and Cardanus along with Kepler² -- but Kepler was regarded by Heydon as his most honourable adversary.

In fact, in this little book, Heydon often refers to Kepler's work as the source of his own opinions, which were usually progressive, as in the following passage:

'But could Aristotle be informed, that now this latter age (more diligent then the former) hath observed new bodies in Heaven, or were it possible that our mortal eyes might from the Heavens behold the Earth, as now from the Earth we behold them: I verily am persuaded, that both the Philosopher would change his opinion, and that we should from Heaven behold as little alteration in the Globe of the Earth, as now we observe in Heaven. He that listeth to read more of this matter, let him peruse Kepler himself, cap. 23 de Nova Stella, where he doth purposely treat of this Subject, and proveth by five particular Reasons, That the matter of Heaven is alterable.'³

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- 1 Heydon, Astrological Discourse, pp. 16-17.
 - 2 Ibid., p. 14. For Kepler's views, though not from the source from which Heydon drew them (see below, pp. 164-165) -- the actual source is not cited -- see his De Fundamentis Astrologiae (Prague, 1601), Theses 11 and 12, and 'Johnannes Kepler's On the More Certain Fundamentals of Astrology', ed. and translated by J. Bruce Brackenridge and Mary Ann Rossi, Proceedings of the American Philosophical Society, cxxiii (1979), 92.
 - 3 Heydon, Astrological Discourse, pp. 34-35. The passage from Kepler is to be found at Kepler, Gesammelte Werke (De Stella Nova Serpentarii), ed. Max Caspar (Munich: C. H. Beck, 1938), i, pp. 259-267.

Heydon's careful reading of and reliance upon Kepler is most evident in the appendix, on the conjunction of Saturn and Jupiter in 1603.¹ Here, he uses Kepler's observations to check his own and compares their accuracy with his own. He writes:

'...to speak ingenuously, not trusting my Observation more than I should, partly because the daylight would not suffer us to observe other fixed Stars with the Planets, and partly by reason of the slow motion of Saturn and Iupiter, I layd them aside till after perusing Kepler de Stella Nova, I was the more encouraged to make account of my observation.'²

He reduces Kepler's observation at Prague, of the time of the great conjunction, to the time of his own meridian at Baconsthorpe,³ and finds that, 'if the Meridians be compared, the difference of time between his Observation and mine is not 13m. which is not worth speaking of, and may happen as well by the Error

1 The appendix, pp. 98-105, entitled 'An Astrological Judgment upon The great Conjunction of Saturn and Jupiter 1603 being its first Entrance into the Firy Trigon', is an abridgement from Heydon's letter to Richard Foster, 2/12 April 1608; Bodl. MS. Ashmole 242,ff. 18^r-19^v.

2 Heydon, Astrological Discourse, p. 101.

3 Heydon writes, ' I found in pag. 48. [of the De Stella Nova; Kepler, Gesammelte Werke, i, pp. 200-201] that he, by such Observations as he could make, constituteth the time at Prage to be the 7 day, 8 hor. and 20 min., although afterwards he shew as little confidence in the hour as my self did before, I was confirmed by his Observation;' Heydon, Astrological Discourse, p. 101. However, Kepler merely reports that at noon of 7/17 December 1603, Saturn was at 8. 8'. 38". of Sagittarius, and Jupiter was at 8. 5'. 32". of Sagittarius, and he gives their latitudes (Kepler, Gesammelte Werke, i, p.201), according to calculation from Tycho's observations of planetary motions. He points out that these differ from his own observation of the conjunction (50 min. 7/17 December, the day having begun at noon), by no more than 23 hours. The figure Heydon attributes to Kepler is apparently, then, his own reduction, in some fashion, of some of Kepler's results, and he therefore attributes to Kepler a claim for precision that Kepler did not make but explicitly denied.

of Longitude, as of the Observation',¹ but he thinks his own observation more accurate than Kepler's anyway, since Kepler's 'Observation, being only by the Quadrate and Azimuth with many operations and corrections, as you may read, I hold to be not so certain as my own.'²

This is routine, practical work, of a sort in which nearly all observers engaged. Heydon checked his observations against Kepler's and others' again, in 1618, when in that one year, three comets appeared in the sky, and all serious astronomers measured their positions. Heydon discussed the last, by far most prominent comet (visible from 18/28 November to 16/26 December 1618),³ in letters to his friends and protégés, Henry Briggs and John Bainbridge.⁴ In these discussions, Kepler was just one of several authorities whose work was used or corrected. For example, Heydon requested Briggs to compute the size (volume or 'solidity')

1 Heydon, Astrological Discourse, pp. 101-102.

2 Ibid., p. 102. Unfortunately, I have been unable to find any reasonably detailed description of Heydon's instruments.

3 These dates are those given by John Bainbridge; see below, page 188, n. 2. See also John J. Roche, 'Thomas Harriot's Astronomy' (Oxford Univ. D.Phil thesis, 1977), pp. 229-236.

4 The correspondence with Briggs is at Bodl. MS. Ashmole 242, ff. 168^r (undated), 169^r-169^v (undated), 170^r (21 April 1619), 170^v (12 March 1619); for the state of this correspondence, see p. 172, n. 1. The correspondence with Bainbridge is at T.C.D. MS. 382, ff. 59^r-59^v (2 October 1619), 60^r-62^v (12 October 1619 -- Bainbridge's draft reply), 64^r-64^v (25 October 1619), 65^r-66^r (8 November 1619 -- Bainbridge's draft reply), 68^r-71^v (6 December 1619). As we have seen (page 156, n. 1), Heydon paid John Fletcher for his pains, and he seems similarly to have paid Briggs, for he writes (f. 170^r), 'And though I send you nothinge, now, assure your selfe as ther [is] any worth in me, I will take a time to consider you.' There is, however, no evidence of his having patronized Bainbridge to the extent of paying him for his efforts. Heydon's prognostications based on the comet are to be found at Bodl. MS. Ashmole 242, ff. 66^r-71^r. Kepler's principal writings about this comet are in his De Cometis libelli III (Augsburg, 1619/20).

of the comet, and in so doing to use specified data, including Kepler's figure for the length of the tail on a certain date.¹ In a follow-up letter, Heydon urged Briggs on with the task and added the requirement that in triangulating the distance of the comet (which he believed to be supra-lunar²), Briggs should use Kepler's value for the earth's distance from the Sun, 1678 terrestrial radii.³ Similarly, several months later, in a letter to Bainbridge, Heydon made use of Kepler's observations,⁴ but he also engaged Bainbridge in a discussion on how to calculate the path of the comet. Heydon seems sometimes to have difficulty with quite simple calculations, as when, in the passage just cited, he asked Briggs to work out the size of the comet, and on another occasion, when he asked Briggs to plot the comet's path according to Bainbridge's scheme of projection;⁵ yet at other times, he was willing to bluster into the calculations by himself, as here, when he followed Kepler, to Bainbridge's consternation.⁶ Heydon writes:

1 Bodl. MS. Ashmole 242, f. 170^v.

2 Not everyone believed this; even Galileo did not; see Stillman Drake and C.D. O'Malley, eds. and translators, The Controversy on the Comets of 1618 (Philadelphia: University of Pennsylvania Press, 1960), pp. xxiii, 149-336. Heydon, aristotelian as he was, made his one recorded joke at the expense of an aristotelian commentator on the comets, because he, Heydon, had adopted the modern position. 'There is one Piso [Physicvm Cometae Speculum... Avthore Carolo Pisone (Lorraine, 1619); see, in particular, chapter 3, p. 18] hath written so disgracefully in disgrace of y^e mathematicall opinion of y^e Comets and w^t new subtiltyes seekes to uphold y^e Periphereticall [= 'peripatetic' and 'heretical'] opinion as I could find in my hart to haue him by y^e ears he is so absurde and iniurious;' Bodl. MS. Ashmole 242, f. 168^r.

3 Bodl. MS. Ashmole 242, f. 170^r.

4 T.C.D: MS. 382, f. 64^v (6 December 1619). Bainbridge, after the publication of his book on the subject (see below, pp. 188-191), was regarded as the English expert on the comet, and the book presumably prompted Heydon to write to him.

5 Bodl. MS. Ashmole 242, f. 169^r.

6 See Appendix G for more extensive quotations from this document.

'But wheras y^{ou} also reprove my Methode in inquiring the Nodes, and therfor demand of me what Astronomer did euer fynd them as I doe in the Planets, as for example in δ or η .--by two observations. Suerly sir if yo^u please to turne to the 12 chap: of Kepler de motib⁹ Martis [the Astronomia Nova], yo^u shall fynd that Tycho did by two observations of δ , one in his greatest latitude, an other in his propinquitye to his Intersection of the Ecliptick fynd out the place of his Node. neyther can Kepler quarrell wth his successe: though he sayth Demonstracionis ἀκριβεία. comendat aliam. In comets not Tycho alone, but Rothman, Snellius, and Kep[ler] him self euen in this last Comet¹...doth inquire both Nods, angle, and Arks by this very method of 2 observations, as I doe...[Heydon discusses his calculations]...neyther as I suppose am I far wyde: yet thus yo^u se both Kepler and I by the same Method, w^{ch} yo^u reprehend, do fynd the Nods wthout any great difference by 4 seuerall observations, for his + myne be diuerse.'²

A combination of his appreciation of Kepler's work on optics, his desire for corroborative astronomical observations, and his fellow-feeling with another keen astrologer inspired Heydon to write to Kepler early in February of 1604/05.³ Heydon's letter is, like so much of his writing, too long-winded to justify extended quotation, and some paraphrasing should capture its full meaning.⁴ Heydon had intended to write from the time he had first read Kepler's Astronomiae Pars Optica (1604). Much of the first third of the letter is an encomium of Kepler, the successor of 'most Noble Tycho', who had easily overcome the problems of refractions, the light of the stars, the observations of eclipses,

1 Kepler, Gesammelte Werke, i. 220-222.

2 T.C.D. MS. 382, f. 68^r (6 December 1619).

3 Johannes Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1951), xv, pp. 148-150.

4 Yet he apologizes that 'non me sinit fusius at te scribere, vt vellem;' *ibid.*, p. 148, l. 2.

parallaxes, and other secret matters of 'sublime knowledge'¹; all of this proves that Heydon had read the book with enthusiasm, even if not with care. He also urges Kepler to publish his advertised 'Theoric of Mars', which planet, he points out, departs more than any other planet from predicted positions, and he asks for, in addition, the observational basis of Tycho's lunar theory and Tycho's unpublished positions of fixed stars.

Speaking of positions, he settles down to specific problems.² He asks Kepler, not only for himself but for the whole astronomical community, for his observations on the nova in Serpentarius, of October 1604. (Kepler's book on the subject was published a year after this letter, in 1606.) Heydon sends Kepler some of his own observations of the phenomenon. 'Today,' he writes, 'it is discerned to be just equal to Spica, in Virgo, so that (as far as we are able to come up with a conjecture), it gradually wanes, until at last it will fade altogether, unless perchance it seems better to agree with the truth (which, moreover, I see elsewhere is not quite displeasing to you), that this Phenomenon, after the fashion of that of 1572, by daily ascending

1 '...et reliquis hujus sublimis scientiae intimes arcanis adeò clarè philosophatus sis;' *ibid.*, p. 149, ll. 3-4. Heydon, astrologer though he was, does not appear to have been a mystic, and for him, all real phenomena had rationally comprehensible causes; furthermore, he was interested in quantification of information. This reference by him to arcane knowledge is therefore anomalous. It also bears no relation whatsoever to Keplers work, for Kepler, neo-pythagorean though he was, was not a mystic either, and Heydon's reference puzzled him. In his reply, he wrote, 'Quo nomine Clauem Astronomiae penitioris in Opticis iure promisse videor,' thus referring to his theory of the magnetic effluvium, by way of clarification; *ibid.*, p. 233, ll. 58-59.

2 These occupy the middle third of the letter; *ibid.*, p. 149, ll. 25-49.

higher into the sky, it will at last be withdrawn altogether from our sight.'¹

Finally, he seeks Kepler's help in defending judicial astrology from the attacks of several calumniators whom he had been trying to refute. He quotes, without naming the source, two objections that one of these opponents has put forward. Perhaps Heydon assumed that Kepler would not have heard of the man, who, it is clear, was Henry Briggs, with whom Heydon had been waging a friendly struggle, and whose objections Heydon himself had refuted more than a year earlier.² He writes that he has tried again and again to acquire a copy of Kepler's De Fundamentis Astrologiae

1 '...hodie Spicam virginis tantum adaequans cernitur, adeo vt ('quantum coniectura assequi possumus') paulatim indies decrescat, donec tandem omninò euanescat, nisi fortè magis veritati consentaneum videbitur, ('quod et tibi non admodum displicere alibi perspicio') Phaenomenon hoc, quemadmodum illud Anni 1572. altius quotidie ascendendo in ipsum coelum tandem ab oculis nostris se omnino recepturum fore;' *ibid.*, p. 149, ll. 44-49. Kepler nowhere suggested this as the cause of the varying brightness of novae. Heydon has applied to the nova Kepler's remarks on the variation in brightness of the planets with their changing distances; Kepler, Gesammelte Werke (Astronomiae Pars Optica), ed. Franz Hammer (Munich: C.H. Beck, 1939), ii, pp. 223-226. He was not foolish to do so; this is an original idea, but it begs many questions.

2 Kepler, Gesammelte Werke, xv, p. 150, ll. 60-62. Heydon respected Briggs, and, consequently, his arguments: 'I thank you agayne and agayne...for your probable and acute discourse about y^e aspects which (though knitte up concisely in :2: pages) is more to be ualued then...y^e whole Rapsodie scraped together by M^r Chambre...'; Bodl. MS. Ashmole 242, f. 164^r (letter of 5 November 1603). The argument of Briggs's that Heydon repeated to Kepler, and Heydon's own answer, are: 'you [Briggs] brooke not y^t any of these Aspects should be hurtfull, ffor as you take all y^e stars to be good and fauorable, so you think also y^t all other aspects should be likewise good and surely if wee speak in regard of y^e starrs them selues, to affirme them in theyr owne nature naught were to lay y^e blame therof upon god (w^c were blasphemye). but thus wee see other things no lesse necessary as fier and water nay euen as Serpents and poyson it self are good in theyr kinde...'; *ibid.*, f. 166^v. Heydon most likely presented Briggs's objection to Kepler because he was not satisfied with his own response.

Certioribus (1601),¹ which, as far as he was able to determine, had not yet arrived in England, and he desires, therefore, that Kepler send him word of anything he had written in that book that might be of use in confuting the opponents of astrology.

Kepler, glad to have found in England an eminent man who shared his interests, complied handsomely with Heydon's requests, in a letter four times as long as Heydon's. I will refrain from paraphrasing this at length, for it tells us more about Kepler than about the English response to him. For the most part, it responds point by point to Heydon's letter. He describes the state of work on the books Heydon enquired after and touches upon the difficulties he had had with Tegnagel over the commentary on the motions of Mars.² He says that Tycho's lunar theory is not his to publish, and he sees no hope of its publication in the near future but suggests that Heydon himself try to urge Tegnagel, who was visiting England, to produce the book. (I know of no evidence that Heydon ever met Tegnagel or his travelling companion, Johannes Eriksen.)³ He writes that he has produced a small book in German on the nova in Serpentarius, and is working on a larger book in Latin on the subject, but he sends Heydon some observations nevertheless. The last three quarters of Kepler's letter are on

1 It seems unlikely, therefore, that Kepler's astrological work was taken up by Englishmen, at least in the first decade of the century. For a translation of this work, see above, p. 158, n. 2.

2 Kepler, Gesammelte Werke, xv, p.232.

3 'Quid si vero tu ipse hic negocium promouere possis? Tegnaglius enim nuper peregrinatum abiit, praetextu visendae patriae; priuatim vero aiebat, se adire Angliam;' *ibid.*, p. 233, ll. 62-64. On Tegnagel's meeting with Harriot, see above, p. 105-106. The secrecy of Tegnagel's trip is puzzling. I know of no explanation for it. It is clear from what Kepler says here that Tegnagel was not the bearer of this letter to Heydon.

the subject nearest to the hearts of both men: astrology. 'Would that God free me from Astronomy,' Kepler writes, 'that I might return to the care of my work on the harmony of the world.'¹

Kepler then lovingly unfolds his theories on world harmonies.

Surely Heydon, who was only slightly influenced by neoplatonism, could not have expected this. His was a much plainer, purely ptolemaic astrology. It is a measure of his admiration of Kepler that he expounds the harmonic astrology² in his Astrological Discourse. This is the only such account in England in Kepler's lifetime. His own theory of astrological aspects, we recall, was based on beams being emitted by celestial bodies, analogous to light beams.³ Introducing Kepler's theory of aspects, Heydon writes that it 'is more noble, and to be admired then the other. For this favoereth nothing of Matter, but hath only consideration of Form, not so much respecting the streight beams of light which flow from every Star, as valuing and esteeming how their Beams meet at the Earth between us and their Light...'⁴ He follows Kepler in drawing the analogy between astrological aspects

1 'Deus me ex Astronomia expediat, vt ad curam operis mei de harmonia mundi conuertar;' *ibid.*, p. 233, ll. 91-92.

2 Heydon was influenced by neoplatonism, but only to the very slight extent that Ptolemy, Heydon's authority, was himself influenced by this school of thought. Ptolemy, in his Tetrabiblos, i. 13 (ed. Robbins, pp. 72-75), 'Of the Aspects of the Signs,' related aspects to harmonies, and Heydon elaborated on this account in his letter to Briggs, already cited; Bodl. MS. Ashmole 242, ff. 164^r-164^v, and in his Astrological Discourse, pp. 77-80.

3 See above, p. 157. He supposed, however, that these beams, unlike those of light, were immaterial. The 'flux and emission of light... is made by a right line; it is attenuated by the distance thereof from the Star...;' *ibid.*, p. 64. Thus we see that Heydon believed the intensity of light to vary inversely with distance.

4 Heydon, Astrological Discourse, p. 65.

and musical harmonies, and describes this in some detail. He goes so far as to agree that by this analogy, it is clear that there are more than the five aspects alone that the ancients recognized,¹ but so far only. He writes:

'Yet all this hitherto doth rather illustrate a Simili, and doth more and more express unto us, that Nature indeed hath adorned these proportions with singular priviledges above any other, then satisfie us with the true Reason, why in the infinite variety in Sounds and Lights these only should consent most sweetly in Musick, and be effectual in the operations of Nature. Neither hath any man herein endeavored with more probability to give satisfaction unto the learned then Kepler, who having wittily laboured to demonstrate, That God in the Creation of the World hath observed the same proportion in the Magnitude and distance of the heavenly Spheres which is found in the regular Solides, which (as Geometry teacheth) have their Original from the Ordinate Playns: in the end concludeth with good probability, That the heavenly Motions shall then consent sweetly, and co-operate strongly together, when the nature of these sublunary things, indued (as he supposeth) with a sensitive or knowing faculty, apprehendeth the Beams of the Stars to observe that respect in their concurrence at the Center of the Earth, which answereth unto the Ordinate Playns, from whence the Regularity of these proportions is derived, as the impressed Characters of that Symmetry which God is said to have used in the Creation of the World it self. So imagining, that as often as the nature of any thing meeteth with these proportions, it exerciseth it self as it were by this Idea, which retaineth still, and that in such sort, as what it doth but ordinarily and slackly at other times, it performeth now much more effectually, and as it were with extraordinary diligence; Not (saith he) that these proportions work any thing of their own virtue; for in Musick it is neither the Sounds, neither the proportion of the Concords, that work any thing of themselves, or beget any delightful humor in a man, but the Soul approaching to the Instruments of Sense, first there entertaineth the sounds inwardly, then valueth their proportions, and (finding the same good and Geometrical) lastly exhilarateth it self, as with an Object wherein it taketh delight. And surely were these proportions set down by Kepler exactly found in the

1 Ibid., pp. 94-95: 'And because hitherto we have spoken only of the old Configurations, known to the ancient Astronomers, I may not forget to signifie, that in these our days our late Artists (whereof Kepler is the chief) have added unto these former Aspects three others, viz. the Quintile, consisting of 72 degr. the Biquintile of 144 degr. and the Sesquiquadrate of 135 degr... Neither dare I for my part contradict these new additions...' Heydon found that they worked when no other astrological explanation would.

of the World, then I could the better give ear unto him, and believe this Mystery to rest in that which he hath said. But he himself having inscribed the several Spheres of the Planets within Regulare bodies, in the end findeth their distances from the Center of the World to differ very much, both in Jupiter, the Earth (which he supposeth to occupy the Suns place,) and Mercury, from that which Copernicus and other Astronomers do set down by observation.^[1] I love not (after the expectancy of Geometrical demonstration, though he come near some) to feed my self with Fancies in the rest: neither can I well conceive, in that poor understanding which I have in Philosophy, how Configurations, consisting only of intelligible Angles, should be objects to any sensitive power; or how the sensitive power, which he doth imagine in the Elements, and all elementary things, should apprehend without organical Instruments.'²

Therefore, 'leaving this Conceit of Keplers, without prejudice of his Conceit or Judgement, unto others of riper Judgement' than himself, Heydon reverts to the expositon of his quasi-optical theory.

Heydon was thus out of sympathy with Kepler's entire world-view. He also differed with him on particular theories, such as, for example, that of cometary motion. He told Bainbridge that

'in Keplers book...newly Cum to my hands...I can se no reson why he leaues a Circle to embrace y^e Paradox of Rectilineall motions. I say it is agaynst Philosophie because ther I fynd Rectilineall motione proper to this sublunary Spher of mortality and Circular motione only agreable to the aetheriall and heuenly Regions as is euident by Experienc in the motion of all other caelestial bodys.'³

This shows, too, that although he could differ with Aristotle (as

1 Heydon seems not to have known the Mysterium Cosmographicum at first hand. His account of Kepler's use of the regular solids to explain planetary spacing is derived from Kepler's letter to him.

2 Heydon, Astrological Discourse, pp. 82-85.

3 T.C.D. MS. 382, f. 69^r (letter to Bainbridge, 6 December 1619); see Appendix G.

on the nature of comets, which Aristotle thought to be sublunary), he adhered to the aristotelian account of celestial motion.

This leads us to consider one of the most important of the particular ideas promoted by Kepler: heliocentrism. It seems fairly evident that, at least until 1603, Heydon was not a copernican, for in a letter of that year to Briggs, he writes:

'...god hath endued y^e planetts wth uariety of motion w^c is pformed in diuers piods, so hath he likewise ordayned one vniforme motion to y^e Primum mobile, to the end it may conuey the diuersity w^c happneth to y^e planetts amongst them selves to the whole worlde + so apply theyr action to y^e matter w^c suffereth here beneathe for the producinge of y^t vicissitude w^c wee see in worldlye generation and corruption.'¹

Innumerable references to the earth as the 'centre' seem to be intended literally. All this is scarcely surprising, as Heydon's quasi-optical explanation of astrology is dependent upon geocentrism. Consequently, we may infer from the Astrological Discourse that Heydon still believed in an earth-centred universe when that was written, in about 1607. There he writes, '...this Planet doth (as I may say) by this Retrograde Motion assuredly purpose some particular effect,'² and this retrograde motion seems to be thought real, not just apparent. Indeed, in his 1605 letter to Kepler, Heydon says that he does not care for speculation on planetary motion unless such knowledge is applicable to astrology, and Kepler, in his reply, after referring to the planets' being carried around the Sun, adds for emphasis that both

1 Bodl. MS. Ashmole 242, f. 165^v.

2 Heydon, Astrological Discourse, p. 53.

Tycho and Copernicus agree on this, as if to win Heydon over with the voice of authority.¹ Heydon's implicit geocentrism is apparent even near the end of his life, as we saw in his 1618 objection to rectilinear motion of comets, as being inappropriate to the heavenly regions.²

Let us now take stock of Heydon's views before we investigate his reaction to the Astronomia Nova. He did not share Kepler's neoplatonic world view, though he had some understanding of it. He held to aristotelian physics, differing from them only when they were demonstrably wrong, as when novae exposed the mutability of the heavens.³ He was not a copernican, but held no firm views on the nature of planetary motion, except that he thought geocentrism rendered judicial astrology explicable. Finally, he was nevertheless an admirer of Kepler, and gladly used data gleaned from Kepler's writings. We can therefore anticipate what will be, for us, a rather dull response to the Astronomia Nova.

1 'Nam quid est quod Planetas circa Solem rapit? Consentunt enim Tycho et Copernicus in eo;' Kepler, Gesammelte Werke, xv, p. 232, ll. 51-53. Heydon had written a couple of years earlier the same remark that he sent to Kepler: '...For whether any of their opinions be true, or whether they be false, whether they be (as Tycho would have it) but one continued orbe, or many, or whether (as Copernicus saith) the Sun be the center of the world, and the earth be in the Sunnes place, betweene the sphere of Mars and Venus, the Astrologer careth not. For so that by any of these Hypotheses, he may come to the true place and motion of the Starres, this varietie of opinions, whether such things be indeede, and in what order they be, is no impeachment to the principles of Arte;' Heydon, A Defence of Ivdiciall Astrologie, In Answer to a Treatise lately published by M. Iohn Chamber (Cambridge, 1603), p. 371. He even shows a trace of animosity toward Copernicus: '...As for the distance of Mercurie from the Sunne, if he [Chamber] meane the respect which his orbe hath, to the Sunnes orbe, I know none since Ptolemie, that doeth varie it, except Copernicus, who altereth the whole order of nature, to ratifie his Hypothesis;' *ibid.*, p. 386.

2 See above, p. 168.

3 See above, pp. 158.

Heydon looked forward to the publication of the 'Theoric of Mars' with great eagerness, as much because of his knowledge that tables derived from existing models did not match observed positions of the planet,¹ as because of his omnivorous curiosity in all matters of astronomy. He had told Kepler of his desire for this book, as we have seen, in his letter of February 1604/05, fired off by his reading of the Astronomiae Pars Optica, where he had found a mention of it.² Kepler saw that Heydon did not appreciate the significance of, if, indeed, he even knew of, his accomplishment. In his reply, after telling Heydon of the obstacles in the way of the publication of the Astronomia Nova, Kepler writes:

'Concerning the [Astronomia Nova], however, something must needs be said as regards you. For, what you expect from [it], very correct positions of the star of Mars, I care for them hardly at all. Before anyone else, what I had a go at here, I also, by divine grace, succeeded at. There are two inequalities of the planets, as you know: one from the Sun, common to all [i.e., retrograde motion]; the other characteristic of each [i.e., the sidereal period]. I thus investigated the former, to the end that I hoped it would be satisfactory for all four remaining [planets, other than Mars]. I treated this for just a little while with very steadfast labours, so that finally it was accommodated to the laws of nature; therefore, insofar as relates to this, I am able to boast of an Astronomy without hypotheses.'³

1 See above, pp. 153-154, 163.

2 Kepler, Gesammelte Werke (Astronomiae Pars Optica), ed. Franz Hammer (Munich: C.H. Beck, 1939), ii, p. 282, l. 31.

3 Kepler, Gesammelte Werke, xv, p. 232, ll. 41-49: 'De ijs tamen aliquid est dicendum in tuam gratiam. Nam quae tu ex ijs expectas, correctissima loca stellae Martis, ea parum ego curo: prae alijs, quae hic ego attentavi, et diuina gratia consecutus sum. Duae sunt vt nosti planetarum inaequalitates, altera ex Sole, communis omnibus: altera cuique propria. Illam ego sic inuestigavi; vt sperem omnibus quatuor residuis satisfacturam. Hanc pertinacissimis laboribus tantisper tractavi vt denique sese naturae legibus accommodet, itaque, quod hanc attinet, de Astronomia sine hypothesibus constituta gloriari possim.' Here is an anticipation of Newton's 'Hypotheses non fingo.'

He adds the briefest possible sketch of his astronomical magnetic philosophy, for which he gives credit to William Gilbert, 'of your race'.

Heydon's opinion of the magnetic philosophy can only be surmised from a few words in three letters to Henry Briggs, which are the sole surviving evidence of his thoughts upon the Astronomia Nova. There are: one undated letter; one of 14/24 December 1609; and one of 1/11 January 1609/10¹ -- the undated letter appears to be the last of the series. These show Heydon to have been reading the book within a few months of its publication. (Perhaps his copy was in the same shipment as Harriot's.)² When Heydon acquired the book in early December, he was in London, and he called on Briggs for a tête à tête over it. After a week of not finding Briggs at home, Heydon returned to Baconsthorpe, perused the book, and dashed off a letter to Briggs, on the 14th:

'I Am hard at [Mars]: and doe find y^t for y^e proportion of y^e Eleipsis, Kepler is utterly destitute of any helpe frō Geometrye as well as of y^e reason either of nature or Arte:

1 Bodl. MS. Ashmole 242, f. 168^v (undated letter, and 14 December 1609), f. 170^r (1 January 1609). These are not in Heydon's hand (pace Josten, Elias Ashmole, iii, 1043-1044), except for the first half of the letter of 1 January. The rest are in the same hand as that of the letters to Briggs about the comet. These could be copies by an amanuensis of Heydon's for the latter's own use, and prepared much later. He did employ an amanuensis (see Gonville and Caius MS. 73, f. 366^r), but this is only just reconcilable with the fact that letters written ten years apart appear in identical hand on the same page (f. 170^r), and does not explain why the 1619 letters were dated 1629 (long after Heydon's death), and then corrected to 1619. Perhaps then the letters were by an amanuensis of Nathaniel Fiske or William Lilly (for the hand does not match that of either of these two men) who seem to have possessed Heydon's papers at some time, and f. 170^r was a copy constructed around an autograph fragment. There seems to be no completely satisfactory account of the provenance of these letters.

2 See above, p. 29.

why y^e Planet leauing a pfect circle, should choose this forme: agayne as far as yet I pceiue he maketh not this Eleipsis to supply y^e place of y^e Epicycle; but y^e Excentrick it selfe to be Elyptically but I must truely confesse I speak this by ghesse upō superficially ouerseing ye book not hauing yet orderlye proceeded: beyond y^e 6: chapter.'¹

He requests that Briggs, by return post, tell him what he has made of the book.

This is not a favourable first reaction. We must not take too seriously Heydon's complaint that Kepler is 'utterly destitute of any help from Geometry,' for Heydon was not himself a skilled geometer, and he admits that at this point, he has only skimmed over the book. His complaint that Kepler is destitute of any help from 'reason of nature or Arte' is rather more telling, since he had already, four years earlier, been told of the magnetic philosophy, and even in skimming through the new book, he would have acquired some grasp of Kepler's theory. It is a pity that we do not know exactly why he rejected it.

By the first of January, Heydon had already received at least one reply from Briggs, and having made rapid progress himself, he was writing back. He shows Briggs an exercise he had performed: having read up to Chapter LX, he had used the elliptical orbit to calculate the distance of Mars from the Sun at the time of one of Tycho's observations. (This observation, probably chosen at random by Heydon, is that of 6 November 1588, recorded on page 253 of the Astronomia Nova. Heydon arrives at a Mars-to-Sun distance for that date of 156,391, where the earth's semi-major axis is 100,000.) The points in his illustration are labelled in exactly the same way as those in Kepler's drawing in Chapter LX. Heydon tells Briggs

¹ Bodl. MS. Ashmole 242, f. 168^v.

that with the distances thus found, and the angles, he can work out the longitude of Mars on the given date -- a pointless exercise (except as a check on his calculations), as Briggs apparently replied, since the position was a datum.¹ Heydon directs Briggs to Chapter LX, most probably unnecessarily, and concludes, 'Nothing humbles me but how we should attaine the anomaliam mediam wthout the middle longitude.'² Heydon, by 'middle longitude', intends Kepler's 'anomaliam eccentrici', and he was humbled because Kepler himself said that there was no 'geometrical method' of calculating the anomaliam eccentrici from the anomaliam media, that is, of establishing exactly the true angle of anomaly with respect to time, though both were determined by the anomaliam eccentrici. It was, in fact, this very difficulty that Kepler restated abstractly as his famous 'Problem'.³ Heydon did, however, have a basic understanding of Kepler's second law, which he stated in an imprecise, and hence, unusable form: '...in Aphelio he [Mars] mooueth slower, in Perihelio swifter, y^e inaequality wherof is resumed [sic] by those Areas he [Kepler] speaketh of.'⁴

Heydon, we can now see, continued to hope that the 'Theoric of Mars' would allow the positions of that planet to be found more accurately, which indeed it did, eventually. He also continued to ignore, despite Kepler's instruction, the greater significance of the theory, its construction from physical principles. Heydon's reading of the Astronomia Nova was distinctly superficial.

1 Ibid., f. 168^v (footnote to letter).

2 Ibid., f. 170^r.

3 Kepler, Astronomia Nova, pp. 299-300.

4 Ibid., f. 168^v.

It may seem that I have spent a disproportionate amount of time on a very minor thinker. My justification is twofold: first, Heydon was one of very few of Kepler's English contemporaries who were familiar with his work, and his thoughts on Kepler's astronomy are better documented than those of any Englishman before Horrox; second, Heydon does not seem unrepresentative of English opinion, and he may, especially in the case of Briggs, have prompted better minds than his own to consider Kepler's books along similar trains of thought.

VIII. HENRY BRIGGS

Henry Briggs has left far fewer traces of his life than Sir Christopher Heydon, but his bibliography, to say nothing of his achievements, is far greater, and so, consequently, his fame. I do not intend to say anything new about his life, but though most (but not all) of his comments on Kepler have already appeared in print, my quotations and analysis will be more thorough, I believe, than any hitherto.

Briggs, born in Warley, just west of Halifax, on 23 February 1560, attended grammar school in Yorkshire, after which he was sent to St. John's College, Cambridge, around the year 1577. He took his B.A. in 1581, and M.A. in 1585, and was elected a fellow of his college in March 1588. In 1592, he was made an examiner and lecturer in mathematics, and shortly thereafter, reader of the physic lecture. When the Gresham trustees finally chose professors for the newly-opened Gresham College, in 1596, Briggs was elected the first professor of geometry.¹

1 There are several barely adequate accounts of Briggs's life, and I have relied upon these. The two better ones are among the older ones: chapter iii, 'Henricus Briggsius', in Thomas Smith, *Vitae Quorundam Eruditissimorum et Illustrium Virorum* (London, 1707), translated by J.T. Foxell in Alexander John Thompson, 'Tracts for Computers: No. XXII', *Logarithmetica Britannica: Being a Standard Table of Logarithms to Twenty Decimal Places*, Part II (Cambridge: at the University Press, 1952), pp. lxxvii-lxxviii; John Ward, *The Lives of the Professors of Gresham College* (London, 1740, reprinted New York: Johnson Reprint Corporation, 1967), pp. 120-129 -- this relies heavily on Smith. There is some information to be had from John Aubrey, *Aubrey's Brief Lives; 1669-1696*, ed. Andrew Clark (Oxford: at the Clarendon Press, 1898), i. 123-125, and from Wood, *Athen. Oxon.*, ii., cols. 491-493. There is another brief life, based mainly on these sources, in the DNB, article 'Briggs, Henry (1561-1630)'. D.M. Hallows, 'Henry Briggs, Mathematician', *Transactions of the Halifax Antiquarian Society* (1962), pp. 79-92, the only modern biography, points out errors in some earlier references to Briggs's birthday.

Briggs was principally a mathematician, and not an astronomer, nor, in any way, a physicist; that is, even more than Harriot, he avoided speculation concerning the physical world.¹ In this, he was the reverse of Kepler. Still, his mathematical work generally had practical applications. He is best remembered for his work on logarithms, and it was this subject that led, in the last ten years of their lives, to Kepler's acquaintance with Briggs, and their personal contact. The story has often been told of Briggs's fascination with the new invention, logarithms, and his friendship and collaboration with their inventor, John Napier of Merchiston (1550-1617); of Briggs's practical modification of Napier's creation, so that the logarithm of 1 is 0; and of the last years of his life, from late 1619, when he became the first savilian professor of geometry at Oxford, to his death in 1630, during which time he devoted most of his efforts to producing tables of logarithms, some of which were published in 1624 as Arithmetica Logarithmica, a copy of which he sent to Kepler in the following year.² Kepler's first book on logarithms was also published in 1624.³ In his Rudolphine Tables, in which he uses his own logarithms, Kepler briefly describes the 'forma diversa'⁴ of

1 I am not forgetting his practical work, such as that which attempted to associate magnetic variation with longitude for navigational use, or his work on shipbuilding. Such matters are not the same as scientific investigation of the physical world, or a concern with the foundations of natural philosophy.

2 Kepler, Gesammelte Werke, ed. Max Caspar (Munich: C.H. Beck, 1959), xviii, p. 220, ll. 9-11 (Briggs to Kepler, 20 Feb/2 Mar. 1625/26).

3 Johnannes Kepler, Chilias Logarithmorum (Marburg, 1624).

4 Kepler, Gesammelte Werke (Tabulae Rudolphinae), ed. Franz Hammer (Munich: C.H. Beck, 1969), x, pp. 48, 61.

Briggs's logarithms, but it was Napier, not Briggs, who was Kepler's immediate mathematical forbear in this subject.

But 'Briggs did not so entirely devote himself to geometrical problems as not to be drawn away sometimes to investigate the mysteries of Urania, as a matter of interest, though foreign to his own proper sphere.'¹ Perhaps Christopher Heydon lured him into astronomy. Briggs, as we shall see, approached astronomy as a geometer, and he was completely unsympathetic with Kepler in the matter of astrology. We have seen that Heydon went so far as to ask for Kepler's advice to help refute one of Briggs's objections to astrology.² William Lilly (1602-1681), the foremost astrologer of his century, wrote that 'Lord Marchiston [Napier], was a great Lover of Astrology, but Briggs the most Satyrical Man against it that hath been known; but the Reason hereof I conceive was, that Briggs was a severe Presbyterian, and wholly conversant with Persons of that Judgement.'³

It is more than likely that Briggs became, at some point, a believer in the heliocentric theory, although there is no entirely unambiguous statement of this position. Briggs was one of many scholars who contributed accounts of modern inventions to George

1 Thomas Smith, trans. Foxell, op. cit., p. lxix.

2 See above, pp. 164-165. Briggs's rejection of astrology seems quite thorough, but we must not be misled into attributing to him modern views. After all, the arguments he ranged against astrology were not those we might now use. Did he believe in celestial influences on the weather? We do not know how he might have answered this question.

3 William Lilly, The Last of the Astrologers: Mr. William Lilly's History of his Life and Times... reprinted from the second edition of 1715, ed. Katharine M. Briggs (London: The Folklore Society, 1974), p. 98. Lilly is seldom to be trusted, but the motive for Briggs's scepticism that Lilly attributes to him is plausible, as at least a partial explanation.

Hakewill's An Apologie of Declaration of the Power and Providence of God in the Government of the World: Consisting in an Examination and Censure of the Common Errour Touching Natures Perpetuall and Universall Decay. Briggs's contribution, on 'Mathematics unknown from the Ancients', which first appeared in the second edition (1630), and not in the first (1627), begins with the copernican theory, and includes Galileo's discoveries of the moons of Jupiter, Napier's logarithms, some of Harriot's achievements (in solid geometry), and other matters, but makes no mention of Kepler.¹ Briggs used the copernican theory earlier when, in 1615, he was working on eclipses '(perhaps in order to derive a series of predictions²), and based these calculations on the Prutenic Tables.³ It is far from unreasonable to suppose that by 1609,

1 Hakewill's book was published in Oxford. Briggs's statement is to be found in the second edition at pp. 263-264, and in the third edition (Oxford, 1635), at pp. 301-302. It is reprinted in Ward, op. cit., Appendix, pp. 38-39. Ward, p. 129, refers to an unpublished work by Briggs entitled 'Commentaries on the geometry of Peter Ramus'. If this is to be identified with B.L. MS. Harleian 6796, ff. 81^r-154^r, as Dr. M. Feingold suggests (M. Feingold, 'Science, Universities and Society in England, 1560-1640' (Oxford Univ. D.Phil thesis, 1980), p.66), and as seems very reasonable because ff. 147^r-154^r, which seem to be the preface to the preceding section, are dated Cambridge, 1588, at which time Briggs was a mathematical lecturer at St. John's College, Cambridge, then Briggs was not lecturing to his students, at that time, on copernicanism. The account of astronomy he presents there (f. 150^r), is entirely ptolemaic and conventional.

2 Smith, trans. Foxell, op. cit., p. lxix.

3 The Prutenic Tables of Erasmus Reinhold were based on the work of Copernicus. 'Concerning eclipses, which my cousin Midgeley putteth me in mind of from you...Mulerus in his Fris. Tabulis hath mightily discouraged me, for he hath weakened the Prutenics, my foundation, in three places of his book at least, yet hath not helped it, or showed the fault in particular, that others might remedy;' Briggs to James Ussher, 10/20 March 1615/16, in The Whole Works of the Most Rev. James Ussher, D.D. Lord Archbishop of Armagh and Primate of all Ireland, ed. Charles Richard Elrington, (Dublin, 1847), xv, pp. 89-90.

when he worked through the Astronomia Nova, Briggs was already a copernican, though of course, this is only a conjecture.

Briggs read Kepler's major astronomical works (with the possible exceptions of the Epitome Astronomiae Copernicanae and the Harmonice Mundi, of which no evidence of his reading survives), but we do not know when, or the order in which he read them. In 1625/26, he wrote to Kepler of the latter's first important work that, 'while your Mysterium Cosmographicum and some new opinions introduced into Astrology^[1] reveal you to allow too much to conjecture, I desire and recommend that your evidence be propped up with more, and more complete observations.'² Briggs does not say what sort of observations he has in mind, and it is difficult to imagine what sort Kepler could have adduced to satisfy him. Perhaps this was only a polite way of Briggs's saying he had rejected most of Kepler's neoplatonic conjectures.

Briggs read the Astronomiae Pars Optica as well, and admired it. Since his comments on it to Kepler³ entirely concerned its geometry (conic sections), they will not be considered here.

Briggs's astronomical studies as a whole, like Heydon's in part, seem to have been directed solely toward the production of more accurate predictions, and not at all toward the theoretical foundations, except for his adoption of copernicanism. While

1 I do not know Briggs's source for the new opinions introduced into astrology, because he is not specific. It could be Kepler's Mysterium Cosmographicum, his De Fundamentis astrologiae certioribus, or even his private letter to Heydon.

2 'Nam cum Mysterium tuum Cosmographicum et novae quaedam in Astrologiam introductae opiniones, te nimium coniecturis tribuere arguant, auctoritatem tuam plurimis et integerrimis observationibus suffulciendam cupio et suadeo;' Kepler, Gesammelte Werke, xviii, p. 225, ll. 187-190.

3 Ibid., pp. 225-228.

Heydon wanted to improve horoscopes with better predictions, Briggs apparently wanted the better results for their own sake, and perhaps also because they would be of use to sailors. Briggs's only documented practical interest was in navigation: his first two published works were navigational tables¹; he was a member of the Virginia Trading Company²; and he was a friend of Edward Wright, who was also a close friend of Heydon's, and in whose famous Certaine Errors of Navigation some of his practical tables were published. This concern with positions and predictions is evidenced, too, by some of his remarks to Kepler: that he had entered upon 'a new table of Lunar Prosthaphaireses, which reflects the Tychonic [lunar theory] as more clear and no less certain' (than what, he does not say), and that, like Heydon, he desired that 'we have the whole and undiminished fundamentals of the entire work [of the restored Astronomy], namely, the original document of the Tychonic observations, so often promised by you -- the collected Tychonic observations, I say, from that period of so many years, built up at such expense and over so many sleepless nights.'³

1 'A table to find the height of the pole, the magnetical declination being given', published in Thomas Blundeville, The Theoriques of the seuen Planets (London, 1602); and various tables in the second edition of Wright's Certaine Errors in Navigation (London, 1610).

2 Ward, op. cit., p. 125. For a reference to Briggs and shipbuilding, see Jon V. Pepper, 'Harriot's Manuscript on Shipbuilding and Rigging (c. 1610)', privately issued for the Third International Reunion of the History of Nautical Science and Hydrography (London, 1979), p. 1.

3 Kepler, Gesammelte Werke, xviii, p. 225: 'Aggressus sum etiam novam tabulam Prosthaphaeresium Lunarium, quae Tychonica reddit magis expedita et non minus certa,' and '...habeamus totius operis fundamenta integra et illibata; Protocollum nempe observationum Tychonicarum toties a te promissum, observationes inquam Tychonicas vniversas, ab illo tot annorum spatio, tantis sumptibus et vigilijs congestas.'

Briggs's response to the Astronomia Nova might very nearly be predicted from the foregoing account of his approach to astronomy: of copernicanism tempered with small toleration of physical speculation; of an overriding interest in the geometry of astronomical theory and in hard results, in terms of positions; and of no sympathy for Kepler's world-view. Briggs must have acquired his copy of the Astronomia Nova at about the same time as Heydon, in December 1609. His considered reaction is succinctly stated in a letter to James Ussher, dated August 1610, by which time he must have read through the book at least once.

'Kepler hath troubled all, and erected a new frame for the motions of all the seven upon a new foundation, making scarce any use of any former hypotheses; yet dare I not much blame him, save that he is tedious and obscure; and at length coming to the point, he hath left out the principal verb, I mean his tables of middle motion, and prosthaphaerese^wn; reserving all, as it seemeth, to his Tab. Rudolpheas, setting down only a lame pattern in Mars. But I think I shall scarce with patience expect his next books, unless he speed himself quickly.'¹

(The meaning of this last sentence is not altogether clear. Briggs seems to be saying that he is eager for more books from Kepler.)

Heydon's letter of 1/10 January 1609/10² is probably a reply to a letter from Briggs, no longer extant, about the Astronomia Nova, but it is impossible to deduce from this anything of what

1 Ussher, op. cit., p. 63. Briggs's friendship with Ussher can be dated to 1609 by a reference in the 1610 letter just cited to a promise of the year before (cf. Smith, trans. Foxell, op. cit., p. lxix), but it may have begun several years earlier, because Ussher's regular trips to England began in 1606 (see below, p. 195).

2 Bodl. MS. Ashmole 242, f. 170^r; see above, p. 172.

Briggs had written. The same, happily, need not be said of Heydon's last, undated letter on the subject,¹ but most unfortunately, it is extremely difficult to work out from Heydon's quite possibly garbled account just what Briggs had been doing.² It appears that he was attempting to preserve the old astronomical principles (chiefly, uniform motion with respect to the centre of the circle of motion), and had constructed an ellipse by a combination of circular motions. I think we may ignore Heydon's complaint that Briggs proposed an eccentricity of the ellipse that was greater than Kepler's. Heydon's account is definitely confused here, so he probably misunderstood Briggs, but Heydon's statement that Briggs made the epicycle move uniformly about the centre of the eccentric (thus producing the ellipse) is unambiguous:

'...whereas you make ♂ mooue aequallye upon y^e Center of yo^r Eccentrick, so doth not Kepler eyther in his true hypothesis or vicaria...'³

Heydon, siding with Kepler, points out that the anomaly is determined by the law of areas.

It seems that, in the end, Briggs, a copernican, paradoxically adopted a non-realist position, and settled for the hypothesis

1 Bodl. MS. Ashmole 242, f. 168^v. For a later exchange between Heydon and Briggs in which Kepler is cited as an authority, see above, pp. 160-161.

2 It is just possible that more sense can be wrung out of this passage than I have been able to extract from it. For the benefit of readers more ingenious than I, the text of this letter is printed in Appendix H.

3 Bodl. MS. Ashmole 242, f. 168^v. In this letter, Heydon appears to accept Copernicus and Kepler, but this is probably just a position adopted temporarily, as he immerses himself in keplerian astronomy.

vicaria, which gave correct longitudes, which he wanted, but incorrect values for distance.¹ Positions, and the means to predict them, were what he wanted from Kepler, as his letter to Ussher made clear, when he complained that Kepler had left out the crucial part of the book, 'I mean his tables of middle motion, and prosthaphaeresewn,'² and in his impatience for these, he did some of the necessary work himself. Heydon received a table of longitudes, based on Kepler, from him,³ and even Kepler received a summary table of solar and martian prosthaphaereses and distances that Briggs had constructed when he first encountered the Astronomia Nova. This latter table was taken from one in which Briggs had calculated the quantities for every degree of first anomaly, but he did not send the complete version to Kepler, because that would have been 'sending coals to Newcastle.'⁴ The reason for Briggs's sending Kepler these figures becomes clear when we observe that the table of solar prosthaphaereses contains columns with the values calculated according to Tycho, according to the hypothesis vicaria, and according to Kepler's first and second laws ('secundum Keplerum'). Below the table, after complaining that Kepler 'allows too much to conjecture,' Briggs writes, 'Since that Physical Hypothesis of yours does not accord at all with the opinions of ancient or recent Astronomers, and for the Sun, is detected by me to retreat scarcely up to 30" from the [Hypothesis] Vicaria, I desire that the latter, as the more familiar and more

1 For the hypothesis vicaria, see above, pp. 7-8.

2 Ussher, op. cit., p. 63 (August, 1610).

3 Bodl. MS. Ashmole 242, f. 168^v.

4 Kepler, Gesammelte Werke, xviii, pp. 224-225, ll. 174-175: 'Habeo has sex integras tabulas pro singulis gradibus. sed quorum Noctuas Athenas.'

Geometrical, be retained if it can conveniently be so.'¹ It is difficult to understand why Briggs included the prosthaphaereses according to Tycho's theory in the table; possibly he was subtly making the point that these, too, deviated relatively little from those according to the Astronomia Nova, but it is doubtful that he could have been so quiet about such a serious contention if he intended it. It is probably just irrelevant information, like the distances he included in the table. In any case, it is not completely certain that his cited 30" deviation is taken from the table of prosthaphaereses, though this seems very likely, because there is no other obvious reference for such a figure.

Briggs may have heard rather little of the Rudolphine Tables until, probably in 1625, he read the letter Kepler had sent Briggs's good friend and Gresham College colleague, Edmund Gunter.² In early 1622, Gunter had sent Kepler a copy of his first published book, Canon Triangulorum,³ from which, when it finally arrived a year and a half later, Kepler learned of Briggs's work on logarithms. Kepler, in his reply,⁴ which was chiefly about these, and which was the ostensible justification for the wide-ranging letter from Briggs that I have cited frequently

1 Ibid., p. 225, ll. 189-194: '...te ninium coniecturis tribuere arguant... Et cum tua illa Physica Hypothesis de Planetarum motibus minus consentiat Astronomorum antiquorum; vel recentiorum placitis, et in o deprehensa sit a me vix per 30" recedere a Vicaria, istam vt magis familiarem magisque Geometricam si commode fieri possit retineri cupio.'

2 Their stays of residence in the college overlapped by one year.

3 Edmund Gunter, Canon Triangulorum (London, 1620).

4 Kepler read the book immediately. See his letter of thanks to Gunter in Kepler, Gesammelte Werke, xviii, pp. 144-145 (4 December 1623), especially lines 1-3 for dates, and his letter to Landgrave Philipp of Hesse (December 1623), *ibid.*, p. 150, ll. 4-130.

already, wrote of his perplexity over the choice of logarithms to use in the Rudolphine Tables,¹ and added as a postscript,

'I cannot publish, being oppressed by the hardships of the times. I find a haven in the Rudolphine Tables. If the Emperor (who seems, indeed, to hold the wolf by the ears) has peace, they will shortly see the light.'²

When Briggs finally did acquire the Rudolphine Tables, he was disappointed.³ In early 1629, a friend of his wrote to Archbishop Ussher that Briggs had found that the Tables 'answer not the expectation which he had raised of them.'⁴ It is to be regretted that there is no record of just what Briggs had considered wanting from them.

Had Briggs spoken the modern idiom, he might have described Kepler as 'brilliant but unsound'. Like his friend Heydon, but for different reasons, what Briggs wanted most from Kepler was accurate tables of planetary motion.⁵ Heydon wanted to be able to produce better horoscopes, but Briggs probably wanted the tables in order to aid navigation. He was primarily a geometer, and when he and Kepler, as aging men, finally corresponded with each other,

1 Ibid., p. 145, ll. 47-48, 'valde haereo quid sit mihi sequendum in Rudolphinis.'

2 Ibid., ll. 58-60: 'Ephemerides omnino continuo; at temporum difficultate circumventus, edere non possum. In Rudolphinis video portum. Si Caesar pacem habeat ('qui equidem lupum auribus tenere videtur') brevj lucem videbunt.'

3 They were, apparently, rather late in arriving in England; see above, p. 58, n. 1.

4 Ussher, op. cit., p. 431, Sir Henry Bourghier to Ussher (26 March 1629).

5 He wanted, also, Kepler's long-promised but never-written magnum opus, the Hipparchus; ibid., p. 224, l. 149.

they wrote chiefly of mathematics. To the extent that Briggs bothered himself with physical theory, he found Kepler's to be too conjectural and, in fact, his whole world-view seems to have been different from Kepler's, if we may judge from his views on astrology. He preferred, despite being a copernican, to stick as closely as possible to the familiar, traditional methods of astronomy. Furthermore, he apparently found Kepler's elliptical astronomy to be clumsy, less 'geometrical' than that of circular motion, although he knew, from Kepler's values of planetary distances, that even Kepler's hypothesis vicaria, toward which he inclined, did not produce a true picture of celestial motions.

IX. JOHN BAINBRIDGE

John Bainbridge, the polyglot editor of Ptolemy, was the first English keplerian astronomer. Bainbridge, born in Ashby-de-la-Zouch in 1582, was educated first at the grammar-school there, and then at Emmanuel College, Cambridge, where his tutor was a distant relation, the yet-to-be-famous Joseph Hall. He proceeded B.A. in 1603, M.A. in 1607, and M.D. in 1614. He returned to Ashby, where, for a while, he practised medicine, but eventually he removed himself to London, where he was licensed by the College of Physicians in November 1618.¹

All this while, it seems, Bainbridge had been studying astronomy and some geometry, for following the great comet of 1618, there appeared, in 1619, his first book, dedicated to King James, An Astronomicall Description of the Late Comet.² This was a semi-popular work, Bainbridge leaving the technical details of his observations to his 'Latine Cosmography', which, however, was never

1 There is no modern life of Bainbridge. The article 'Bainbridge, John, M.D. (1582-1643)' in the DNB is adequate, and the article 'Bainbridge (John)' in Biographica Britannica: or, the Lives of the Most eminent Persons Who have flourished in Great Britain and Ireland... (London, 1747), i, cols. 419-421 is useful as well. I have relied for biographical information chiefly upon these, but see also Smith, Vitae, chapter iv, 'Joannes Bainbridgius', and Wood, Athen. Oxon., iii, cols. 67-69.

2 John Bainbridge, An Astronomicall Description of the late Comet from the 18. of Nouemb. 1618. to the 16. of December following (London, 1619). The book was written in December 1618; see p. 42.

published.¹ The existing work allows us to establish Bainbridge's opinions on various matters: he clearly accepted the modern view that comets were supra-lunar, and condemned 'Those Philosophers, who still walke in the way of the Gentiles, are afraide to induce generation, or any other mutation into the heauens, rather choosing to follow their blind guide [Aristotle]';² he was not a follower of Ptolemy, for he writes that 'planetary retrogressions and stations of Planets bee but φαῖνόμενα, appearances; the Planets still making progresse in their own circles, (as is well known to them who are well versed in the Labyrinths of Astronomy)';³ and he was a believer in astrology, for the book contains, as a long postscript, certain 'Morall Prognosticks of Applications of the late Comet or Blazing-Starre'.⁴ This last may have been included to attract a large readership for the book, for Bainbridge's attitude towards astrology was ambivalent, and later, at least, he was opposed to it, for he wrote 'Antiprognosticks, in which is briefly detected the vanity of Astrological predictions, grounded upon the idle conceits of celestial houses and triplicities, and the grand conjunctions of

1 Ibid., p. 3. The book has vanished, if it ever existed.

2 Ibid., p. 24.

3 Ibid., p. 6. Bainbridge could thus have been either a tychonian or a copernican. I prefer to believe he held the latter position, because there is no subsequent evidence, in his manuscripts, for the former, and it accords with his ready acceptance of keplerian astronomy.

4 Ibid., pp. 27-42.

Saturn and Jupiter.'¹ This is not a statement of total disbelief in all forms of astrology, but there is, otherwise, no astrological material surviving among his manuscripts, so it seems likely that he was, at most, indifferent to Kepler's developments in this field.

An Astronomicall Description reveals to us, further, that Bainbridge was already, in 1618, familiar with Kepler's work on comets. He cites approvingly 'the more accurate & late obseruations of Keplerus (Mathematician to two Emperors)' of the maximum possible parallax of the comet;² his statement that

'the Comets taile is nothing else but an irradiation of the Sunne through the pellucide head of the Comet. For though the Sunne-beames be not of themselues conspicuous in the pure aery or aetheriall regions, yet passing through the Comets more condensed substance and there by refraction recollecte and more neerely vnited they did not only illustrate the Comet it selfe, but also a long tract beyond him...'³

reflects an early view of Kepler's, propounded in his Astronomiae Pars Optica but later discarded;⁴ and finally, his opinion that

1 Antiprognosticon: in quo παντικυνη Astrologicae, caelestium Domorum it Triplicitatem commentis, magnisque Saturni et Jovis (cujusmodi anno 1623; et 1643. contingerunt, et vicesimo fere quoque deinceps anno, ratis Naturae legibus, recurrent) conjunctionibus innixae, vanitas breviter detegitur. (Bodl. MS. Smith 92, pp. 1-15, transcribed by Thomas Hearne in 1711); title trans. in Biographica Britannica, loc. cit. The work was never published, and it lacks a date.

2 Bainbridge, Astronomicall Description, p. 15.

3 Ibid., pp. 9-10.

4 Kepler, Gesammelte Werke, ii., pp. 230-232; Drake and O'Malley, op. cit., p. 346 (from the translation of the 'Appendix' to Kepler's Hyperaspistes (1625)). This was Tycho's theory, too, which was in turn a modification of that of Apianus; John Roche, op. cit., p. 208.

the comet's proper motion was rectilinear¹ was also a view held by Kepler. Christopher Heydon berated Bainbridge for holding this last opinion, and objected that it was 'agaynst philosophie.'²

Bainbridge's book had brought him to the notice of Heydon, who then corresponded with him regularly about the comet, at the same time that he was discussing it with Briggs.³ Briggs and Bainbridge had already become acquainted by late 1618, even before the latter's book was published, and Briggs, the first Savilian professor of geometry, may have helped to bring Bainbridge to the notice of Sir Henry Savile, who appointed Bainbridge to be the first savilian professor of astronomy, in 1619. Thus, the two men came up to Merton College, Oxford, in that year or early in the following one, and remained there until their deaths, Briggs's in 1630, Bainbridge's in 1643. Bainbridge appears to have become well-established as a teacher in his old profession as well, for, having been incorporated M.D. in 1620, he was appointed junior, and then senior reader of Linacre's lecture, in 1631 and 1635.⁴ In Oxford, though, Bainbridge had the opportunity for extensive study of astronomy, as he prepared his lectures, required by the savilian statutes,⁵ and his work on Kepler's books now began in earnest.

1 Bainbridge, Astronomicall Description, p. 3.

2 T.C.D. MS. 382, f. 69^r. See Appendix G.

3 T.C.D. MS. 382, f. 59^r-71^v. For a reference to Bainbridge in a letter to Briggs, see Bodl. MS. Ashmole 242, f. 169^r.

4 DNB, article 'Bainbridge, John, M.D., (1582-1643)'.

5 Oxford University Statutes, translated by G.R.M. Ward (London, 1845), i. 272-284 ('The Foundation of Two Lectureships in the Mathematical Sciences, by Henry Savile, Warden of Merton College, Published and Confirmed in the Venerable House of Convocation, in the Year of our Lord 1619, August the Eleventh'): p. 272 ('Chapter 2. Of the Functions and Office of each Professor').

His personal library contained Kepler's Dioptrice and Astronomiae Pars Optica. The professor of astronomy was expected to lecture on optics, and we have seen evidence for Bainbridge's having read the latter book earlier. Bainbridge's library also contained the Astronomia Nova, the Epitome Astronomiae Copernicanae, and the Rudolphine Tables,¹ among a host of other books. (Among his tables, for example, he had also the Alphonsine Tables and Lansberg's Tables.) At the same time, he corresponded with Kepler about his other work, such as his editing of Ptolemy's Syntaxis, and, presumably, about his thoughts on Kepler's books. Unfortunately, none of these letters survives, except for Briggs's transmission of Bainbridge's regards to Kepler, in 1625/26.²

1 T.C.D. MS. 385, ff. 139^r-143^v: index of Bainbridge's instruments and books; the Kepler material is recorded at f. 140^v. In addition, Bainbridge used a copy of either Kepler's De raris mirisq; Anni 1631. Phaenomenis...Admonitio ad Astronomos... (Leipzig, 1629), or his reissue of the same, Admonitio ad Astronomos...De raris mirisq; Anni 1631 Phaenomenis... (Frankfurt, 1630), more likely the latter, because he refers to it as Kepler's 'admonitio Astronomica'; see his unpublished tract, De Stella Veneris Diatriba, at the point where he describes the probable appearance of a transit across the Sun; Bodl. MS. Add. A. 380, ff. 204^r-209^v, a copy by Thomas Hearne (1711), f. 207^v in particular.

2 Kepler, Gesammelte Werke, xviii, p. 516: a reference to the earliest catalogue of Kepler's papers, which included correspondence with Bainbridge. Ibid., p. 129, ll. 328-334, Briggs to Kepler: 'Doctissimus vir Joh. Bainbridgius Med. Doctor et Collega meus Astronomiae professor, dixit se velle vna cum his meis ad te literas dare, et suis verbis tibi salutem dicere. is Ptolemej μαθηματικὴν σύνταξιν integram impressam, diligenter contulit cum libro MS. Dignus sane est qui iterum imprimatur, et si per molem tuorum scriptorum vacaret, gratissimam in eo edendo operam locares.'

The result of his studies was a long lecture¹ on keplerian astronomy, whereby Oxford students were introduced to the elliptical planetary orbits, apparently treated as factual, for Bainbridge nowhere in his lecture suggests that they are hypothetical. It is the historian's misfortune that we cannot date this lecture, which Bainbridge may have delivered at any time up to his death in 1643. It would be pleasant indeed if we could date it as being early, that is, from the mid-1620s, and assume that Bainbridge read it again and again for nearly two decades. We could thereby push far back the date of the incorporation of Kepler's theories into English astronomy. I believe that there are two reasons, admittedly rather weak, for so dating the lecture. First, I have already shown that Bainbridge was, before 1620, familiar with some of Kepler's work. Second, Bainbridge was, in January 1624/25, applying the theory of elliptical orbits to the moon's motion, and if his notes on this are lecture notes, too,² then at that date he was already speaking on one of the subjects of the other lecture. However, because he uses keplerian logarithms in the lecture, which he had probably acquired through the Rudolphine Tables, and not through Kepler's Chilias

- 1 T.C.D. MS. 385, ff. 101^r-109^v. The simple expository structure of these notes, and their progression from basic mathematics of ellipses, and definitions, to worked problems, reveal them to be lecture notes, and not notes made in the course of private study for personal use only. For the Latin text, see Appendix I. It is just possible that T.C.D. MS. 383, f. 160^r is part of, or a draft of part of, this same lecture.
- 2 T.C.D. MS. 386/4, ff. 14^v-16^v, dated 22 January/1 February 1624/25, Thursday. These notes are very difficult to read. Nicholas Tyacke, following the Trinity College Library catalogue, believes these to be lecture notes (Nicholas Tyacke, 'Science and Religion at Oxford before the Civil War', Puritans and Revolutionaries, ed. Donald Pennington and Keith Thomas (Oxford: at the Clarendon Press, 1978), p. 78.

Logarithmorum (1624), which he does not mention anywhere, the notes must be from after 1627.

The lecture is admirably lucid.¹ After a very elementary discussion of ellipses, he says, 'Let the orbit of a planet be an ellipse...' and proceeds to define the characteristics of this planetary orbit (and uses Kepler's word, 'focus', for the first time in England, as far as I know). He mentions that all the planetary orbits share the Sun as one focus, and that the Moon's orbit is an ellipse, with the earth at one focus. As the discussion continues, he introduces more mathematical properties of the ellipse, such as: if a circle ('eccentric') is circumscribed about the ellipse, then a perpendicular to the major axis, as it is translated along that axis, is cut in a constant ratio by the ellipse (a theorem proved by Apollonius). He describes the planet's variation in speed in qualitative terms, and by using Kepler's practical form of the second law, that is, by reference to the eccentric circle. The latter half of the lecture comprises various worked problems, such as, 'Given the anomaly of the

1 The lucidity is owing, in part, to the simplicity of Bainbridge's Latin. One wonders about the quality of his studies of Persian astronomy. Walter Pope tells the following story about Bainbridge's Latin: 'there goes a Story of him, which was in many Scholars Mouths when I was first admitted [to Oxford], That he put upon the School Gate an Affiche, or written Paper, as the Custom is, giving notice, at what time, and upon what Subject the Professor will read, which ended in these Words, Lecturus de Polis & Axis, under which was written by an unknown Hand, as follows,

Doctor Bambridge, came from Cambridge,
To read De Polis & Axis.

Let him go back again, like a Dunc as he came,
And learn a new Syntaxis.

But this by the by...' Walter Pope, The Life of Seth Ward Lord Bishop of Salisbury, ed. J.B. Bamborough (Oxford: published for the Luttrell Society by Basil Blackwell, 1961, original edition, 1697), pp. 10-11. This story is probably, at least in part, apocryphal, because it is highly unlikely that he could have lectured on poles and axes, which are not a pivotal subject.

eccentric, the anomaly of time will be given,' and the determination of the planet's distance from the Sun when its eccentric anomaly is known. (The calculations are carried out with the aid of keplerian logarithms, as has been said.)

This is a straightforward exposition, showing that Bainbridge had digested the contents of Kepler's Epitome and Astronomia Nova, but it is not incontrovertible evidence that he fully accepted the theory. The evidence that he nearly did so does exist, in the form of a statement that he did not fully accept the theory; he modified its physical basis in order to make it more palatable to himself, thereby revealing that the rest was acceptable as it stood.

This statement is not Bainbridge's own, but is in the hand of James Ussher (1581-1656), from 1607 to 1621, professor of divinity at Trinity College, Dublin, and from 1625, archbishop of Armagh, and still well-remembered for his work on biblical chronology. From 1606, Ussher paid triennial visits to Oxford, Cambridge, and London, and formed friendships with, and enlisted the aid of Thomas Lydiat, Briggs, and Bainbridge.¹ Bainbridge, upon his death, left his papers to Ussher. As Ussher's most recent biographer has written, his 'mathematical interest was ancillary to his chronological studies,' but Ussher was, through his friends, well-informed of all the most recent scientific developments.² He inserted among Bainbridge's papers several pages that describe planetary motions, and he attributed the content to

1 See article 'Ussher, James (1581-1656)' in the DNB.

2 R. Buick Knox, James Ussher: Archbishop of Armagh (Cardiff: University of Wales Press, 1967), p. 111.

Bainbridge.¹ As the ideas contained therein are recorded nowhere else in Bainbridge's papers, we must assume that these notes are based either on lost manuscripts or on Ussher's discussions with Bainbridge. The passage on Kepler is worth quoting in extenso.

'Kepler thinks that the Planets have no proper motion about the Sun, but that all of their motions arise from the Sun; slower about the aphelion or higher Apsis, swifter about the perihelion or nearer apsis: that is, that the Planet completes equal spaces in unequal times, according to the varying distance of the Planet from the Sun. Mercury, for example, completes its Period about the Sun (by the force of the Sun) in 87 days 23 hours 15 minutes 36 seconds, by the established relation to the fixed stars. About the middle distances, it completes one degree of space in one degree of time: about the aphelion, it completes one degree of space, equal to the previous one, in 1 degree 12 minutes 15 seconds of time: about the perihelion, it completes one equal degree of space in 0 degrees 47 minutes 25 seconds of time.

'But I think that the Planets have a proper motion about the Sun: which the Sun stimulates and accelerates more or less, according to the varying distance of the Planet from the Sun. Thus Mercury, by its proper motion about the Sun would complete its Period in 175 days 22 hours 31 minutes 12 seconds, but its motion being accelerated [decelerated] by the Sun, one Period becomes half of that time: 87 days 23 hours 15 minutes 36 seconds.

'The Planet by [its] proper motion completes equal degrees of space in equal times: but the motion being cut by the Sun, it completes equal degrees of space in unequal times. Thus Mercury, around the middle distances, completes one degree of space in two degrees of time; but the promotion by the Sun subtracts one degree of time. About the aphelion, it likewise completes one degree of space in two degrees of time; but the promotion by the Sun subtracts 0 degrees 47 minutes 25 seconds of time, and 1 degree 12 minutes 35 seconds remain, as according to Kepler. About the perihelion, Mercury completes one degree of space also in two degrees of time: but the advance from the Sun subtracts 1 degree 12 minutes 35

1 T.C.D. MS. 794, ff. 47^r-47^v. The librarian of manuscripts at Trinity College Library kindly identified these for me as being in Ussher's hand. The attribution to Bainbridge is made at the end of the passage. For the Latin text, see Appendix K.

seconds and there remain 0 degrees 47 minutes 25 seconds as in Kepler.'¹

This little exposition makes it clear that Bainbridge had accepted Kepler's astronomy, but was unhappy with the physical basis, which he tried to change. The way he did so is not properly expounded. The description of Kepler's theory, that 'the Planets have no proper motion about the Sun, but all of their motions arise from the Sun,' is so simple as to be a travesty of Kepler's ideas, and the corresponding description of Bainbridge's theory, that the planets do have proper motions, is probably, likewise, an extreme over-simplification. These brief accounts may, of course, be Ussher's abridgements of what Bainbridge actually said or wrote. We may speculate on the nature of Bainbridge's theory: whether it was an attempt to preserve the proper motions that, in the classical theory, had been provided by solid celestial spheres, or whether he was adumbrating the work of later men in suggesting that each planet produced its own force; but we have not enough evidence really to know anything about it.

This statement reveals, also, that Bainbridge had not firmly grasped the second law of motion, for there is no reference here to equal areas being covered in equal times, but rather to the (incorrect) law Kepler had originally intended the Area Law to approximate to, that planetary angular speed was proportional to

1 Mercury, having a very irregular orbit, is an odd choice to use as an example of general planetary theory. Perhaps Ussher carelessly selected this from a complete description by Bainbridge of the motions of each planet. The passage concludes with the note that Kepler's value for the obliquity of Mars' orbit, painstakingly worked out to be $1^{\circ} 50'$, is the value used by Ptolemy in his Planetary Hypotheses, which Bainbridge thinks Kepler did not read.

the planet's distance from the Sun. Bainbridge, despite his defective understanding of the second law, attempted to improve upon Kepler's method of calculating the eccentric anomaly of the planet in its ellipse from the coequated anomaly, and he expounds this, too, in his lecture.¹

Did Bainbridge know of the third law? He almost certainly did, but he does not quote it anywhere. He would have come across it in his reading of the Epitome, and in fact, he does refer, among some odd notes, to Kepler's less-well-known law, stated a few pages after the third law in the Epitome, that the ratios of the planetary sizes are determined in such a way that the volumes of two bodies are in the ratio of their distances.² If Bainbridge took an interest in these ratios, as he seems to have done, it is more likely than not that he took notice of the third law.

Bainbridge may have helped in one more way, a rather circuitous one, to promote Keplerian astronomy in England. When Ismaël Boulliaud's Astronomia Philolaica,³ containing a modified version of Kepler's second law, was published in 1645, there was a fresh surge of interest, in England, in Kepler's planetary theories. Tucked away near the end of that large work are a couple of eclipse observations by Bainbridge,⁴ which do not appear in his published writings. Boulliaud must have acquired these

1 Bainbridge's method; T.C.D. MS. 383, ff. 159^r-160^r. Discussed in lecture; T.C.D. MS. 385, f. 106^r.

2 T.C.D. MS. 383, f. 98^r. See Kepler, Gesammelte Werke (Epitome Astronomiae Copernicanae), ed. Max Caspar (Munich: C.H. Beck, 1953), pp. 281-282.

3 See above, pp. 84-85.

4 Ismaëli Bullialdi, Astronomia Philolaica (Paris, 1645), p. 467.

through private correspondence. Just possibly, the two men discussed Kepler.

The consequences of Bainbridge's influence are difficult to find. I know of no one who claimed to owe his familiarity with Kepler to Bainbridge's teaching. Nevertheless, I think we can say that, from the time of the work of this one man, Kepler's theories were incorporated into English astronomical thought.

X. THOMAS LYDIAT

Thomas Lydiat, a friend, or more precisely, an associate of Briggs's and Bainbridge's, as well as of Ussher's, was a chronologer and biblical fundamentalist, whose immense learning burgeoned into a full-blown pedantry that ensnared the minor errors of Kepler. He was born in 1572 in Alkerton, in North Oxfordshire, and passed from Winchester College to New College, Oxford, from which he proceeded B.A. in 1595 and M.A. in 1588/89, and where he obtained a fellowship, which he relinquished in 1603.¹ He devoted his time almost solely to the study of chronology, and this interest, shared with James Ussher, may have led the latter to invite Lydiat to join him at Trinity College, Dublin, which Lydiat did, from around 1609 to 1611. In 1609, he dedicated his Emendatio Temporum² to Prince Henry, and he was thereupon appointed 'chronographus et cosmographus'³ to the prince, until the latter's death in 1612. He then took up the family living at

1 The lives of Lydiat in the DNB and in Wood are better than those of any of the other men discussed in this chapter. I have relied on these articles for biographical details; article 'Lydiat, Thomas (1572-1646)' in the DNB, and Wood, Athen. Oxon., iii, cols. 185-189.

2 Thomas Lydiat, Emendatio Temporum Compendio Facta ab initio Mundi ad praesens vsque. Qua, Praeter Alia Plvrima, restituta sunt Natiuitas & Baptisma & Cruciatus Domini Christi ad annos fere quaternos post vulgi calculos: Confvtatis Opinionibus Scaligeranâ Baronianâque, pariter ac Iesuiticâ Ioannis Deckerijs & Laurentij Suslygae, atque Ioannis Kepleri Caesarei Mathematici (London, 1609).

3 See DNB, loc. cit., and Bodl. MS. Bodl. 313, f. 84r.

Alkerton, where he remained, through several vagaries of fortune, the effects of the worse ones being exacerbated by his alienation of his acquaintances,¹ until his death in 1646.

In his published books, all of which are written in a Latin even more difficult and obscure than his latinate English, his method of composition was to put forth his own views in the guise of a learned dissection and refutation of the works of other scholars without actually being abusive. Such a technique is difficult to epitomize, but a fair example of his prolix style, by way of analogy, may be his response to Henry Briggs's mistaken address of a letter:

'You endorsed it to Alkerton in Buckinghamshire. Indeed my direct way to Alkerton from London, whence I came upon Friday was seuennet, the 11th of July, which day your letter bears date, is to Ailesbury, and so all along through Buckinghamshire; but Alkerton, my native soil and dwelling place, is in the utmost skirt of Oxfordshire northward, as I have heretofore, although not demonstrated, yet declared without a diagramme in mine Astronomical Epistle, a copy whereof I remember I gave you.'²

Indeed, Kepler took the full measure of the man upon reading one of his first two books, the Tractatus De varijs Annorum formis (London, 1605):

1 Lydiat's relations with his colleagues are well and succinctly summarized in M. Feingold, 'Science, Universities and Society in England, 1560-1640' (Oxford Univ. D.Phil. thesis 1980), pp. 185-186.

2 James Orchard Halliwell-Phillipps, ed., A Collection of Letters Illustrative of the Progress of Science in England (London, 1841), pp. 47-49: Lydiat to Briggs, 23 July/2 August 1623.

'I picked up the little book of the English Lydiat on the forms of the years. I noticed some diligence in his reading, weighing, and castigating the work of Scaliger on Times; herein, therefore, some use of the former author is possible. Otherwise, the work fails in its theme as much as the title is huge. He indiscriminately wipes away an established tyranny and establishes a new one. That tyranny is the more honest, certainly, [which is] under Scaliger's repeated utterances of IT IS, IT WAS, when neither an authority nor a reason is cited, because it is manly. Lydiat intrudes upon us the most arrogant and wholly intolerable tyranny of his own doctrine, as of a very fastidious woman, to whom the contrary is always apparent.

Therefore, I desire proper modesty in a man, especially when so ignorant of Astronomy, and moreover with such a huge beam in his eye, when he plucks a mote from the eye of Scaliger.'¹

In 1609, Kepler, who had added chronology to his many disciplines with a tract on the date of the birth of Jesus, which was appended to his De Stella Nova Serpentarii (1606),² was

1 'Lidiati Angli libellum de Annorum formis inter primos emi. Diligentiam nonnullam in legendo et pensitando castigandoque Scaligeri opere de Temporibus animadverti: itaque aliquis hic illius Authoris usus esse poterit. Caeterum in themate ipso deficit opus à tituli amplitudine. Tyrannidem inditam passim abrogat, novam ipse stabilit. Honestior est sanè illa tyrannis sub creberrimis Scaligeri vocibus EST, FUIT, nullo nec authore nec ratione dicta, quia mascula est: at pro hac nobis Lydiatus subintrudit arrogantissimum et planè intolerabilem tyrannidem suae doxae, fastidiosissimae feminae, cui semper contrarium videtur.

'Itaque in tanta praesertim Astronomiae ignorantia, tanta praeterea trabis magnitudine in oculo proprio jure modestiam hominis desidero in eruenda festuca ex oculo Scaligeri,' Kepler to Joh. Reinhard Ziegler, 18 October 1606; Kepler, Gesammelte Werke, xv, p. 357, ll. 9-21. Because Lydiat's book is actually 362 pages long, though small in format, Kepler's 'libellum' is, possibly, an additional slight. Aubrey quotes an expression that is a curiously exact parallel to Kepler's comments on Scaliger and Lydiat: '...one may say of [Hobbes], as one sayes of Jos. Scaliger, that where he erres, he erres so ingeniosely, that one had rather erre with him then hitt the marke with Clavius;' John Aubrey, Aubrey's Brief Lives, ed. Oliver Lawson Dick (Harmondsworth: Penguin Books, 1962), p. 310.

2 Kepler, Gesammelte Werke, i, pp. 357-390, De Iesu Christi Servatoris Nostri Vero Anno Natalitio.

himself the object of an assault by Lydiat, in his Emendatio Temporum, in whose full title he found himself in the company of Scaliger as one of those whose opinions would therein be confuted. The details of Lydiat's attack, and an evaluation of the merits of each side, need not concern us here, for we are not principally interested in chronology.

Kepler had referred, in the passage just quoted, to Lydiat's 'ignorance of Astronomy', but Lydiat, who was, in fact, very widely read, was not so much ignorant as wilfully obtuse. The other of his first two books, the Praelectio Astronomica (1605),¹ set forth his cosmology and natural philosophy, which, though very much of a classical and theological mould, were yet unique to Lydiat. Unable to reconcile the received views of astronomy and physics with his steadfast belief in the literal truth of the Bible, he threw away all of ptolemaic astronomy and not a little of aristotelian natural philosophy, and modified the rest so that it made sense in the light of the word of God, as he understood it. W.H. Donahue, who first drew attention to this book, has paraphrased the content very well:

'His considerations are negative; thus, he rejects the scholastics' moving intelligences as superfluous, yet regards

1 Thomas Lydiat, Praelectio Astronomica De Natura Coeli & conditionibus elementorum: tum autem de causis praecipuorum motuum coeli & stellarum. Item Disquisitio Physiologica de origine fontium perennium frigidorum & calidorum: eâq; occasione de ortu & causis plerorumq; omnium subterraneorum, atq; etiam aestus & salsedinis maris, nec non diluuij vniuersalis. Quibus duabus commentatiunculis adumbratur constitutio vniuersi: ita vt receptae a multis hodiè Philosophorum Peripateticorum opiniones de quinta coeli essentiâ immutabili, & de elementorum proportionibus situq; refutentur; naturalis autem historia sacrarum literarum de aquâ supercoelesti atq; igne subterraneo iuxta genuinam antiquitus receptam earum sententiam confirmetur (London, 1605).

belief in innate moving forms (souls) as impious... The dilemma which he faced arose from a conflict in the very foundations of the Christian cosmology. In its most general form, this is the impossibility of asserting both the absolute transcendence (and hence, incomprehensibility) of God, and the comprehensibility of God's plan in creation. As it occurred to Lydiat, it was the question of how God, though omnipresent, appears to act mainly at the circumference of the world. His solution is freely to grant that God is directly responsible for all phenomena, but to maintain at the same time that He has implanted in man "ad illustrandum sapientissimi naturae opificis gloriam" the ability to see all phenomena as interconnected. In other words, although there is no real causal relationship between the diurnal rotation of the primum mobile and that of the stars and planets, it nevertheless appears to us that such a relationship exists... He regarded the diurnal motion as a result of the motion of the waters above the heavens. This arises from a slight tendency towards motion, which he believed to inhere in water wherever it is found.¹

Lydiat therefore not only believed in a geocentric universe but was explicitly anti-copernican, and later summed up his view of heliocentrism in the memorable phrase, 'coenum in coelo'.² Most interesting, at least as a curiosity, is his derivation, from his own physics, of a non-circular orbit for the Sun:

'Now since we have shown above that the natural place of any star suits it according to some certain rarity of the air; since the motion of the equinoctial circle [the equator] is greater than at a tropical one, because it is faster than the motion of the tropical circle, it follows of necessity that the air under the equinoctial circle, just as at a given

1 W.H. Donahue, 'A Hitherto Unreported Pre-Keplerian Oval Orbit', Journal for the History of Astronomy, iv (1973), 192-194; Lydiat's critique of Aristotle can be found in his Praelectio Astronomica, cap. 2, pp. 15-23.

2 Bodl. MS. Bodl. 313, f. 83^r.

distance from the centre of the world, persists in the same rarity that it would have under the tropics at a lesser distance, and therefore the Sun comes so much nearer the earth in the equinoxes than in the solstices. Hence, when it crosses from one solstice through the equator to the other, it describes a portion, not of a circular, but of an oval line.¹

It is easy and amusing to draw, as Donahue does, parallels through the lives and thoughts of Kepler and Lydiat, both devout men of theological training, whose beliefs led them to fashion new physics from which they derived non-circular orbits, but when one examines their exact beliefs and the quality of their thinking, one sees that the two men could not have been more dissimilar. In fact, Kepler's physics are not derived from, though they are related to, his theology, and his neoplatonic cosmology with its concomitant belief in God as a geometrical artificer bears no relation to Lydiat's biblical literalism. Nevertheless, Lydiat himself was later to see some of the similarity.

In the Summer of 1611, while passing through Richmond on his way to London, Lydiat happened upon his friend, Henry Briggs, a man he greatly respected, and they naturally fell into a long conversation, during which Lydiat asked Briggs if Kepler had, during the two years Lydiat was absent in Ireland, published any further support of the claims he had made concerning the date of the birth of Jesus, and which Lydiat felt he had refuted in his

1 'Nam cum supra ostenderimus stellae cuiusque naturalem locum ei competere ex certa quadam aeris raritate: quandoquidem motus circuli aequinoctialis maioris tropico, propterea sit velocior motu tropici; necesse est ut aer sub aequinoctiali sicut in eadem a centro mundi distantia maiorem obtinet ita in minori obtineat eandem raritatem quem habuerit sub tropicis, ideoque Sol tanto propius accedat ad terram in aequinoctiis quam in solstitiis. Quare dum transit ab altero solstitio per aequatorem ad alterum, nequaquam circularis, sed ovalis lineae portionem describit;' Lydiat, Praelectio Astronomica, p. 64, quoted in Donahue, op. cit., pp. 193-194, n. 8. The translation is my own.

Emendatio Temporum. Kepler had not, but Briggs told Lydiat of his extraordinary and already very famous new book, the Astronomia Nova. A few days later, Lydiat acquired a copy and sent it, with a covering letter giving his opinion of it for Prince Henry's edification, to Sir Adam Newton (d. 1630), secretary and tutor to the prince.¹

Lydiat's comments in this letter are, as we might expect, long-winded and disapproving, and it is worth remarking that it is clear that he had read no more than Kepler's introduction, wherein Kepler argues against those who would use the Bible as a textbook of natural science, reiterates his belief in copernicanism, and only most sketchily announces the discoveries he about to narrate. Lydiat gainsays the copernicanism, presents a contradictory analysis of the biblical passages Kepler had used in his arguments, and finishes his lesson for Prince Henry as follows:

'At last Kepler fruitlessly weaves a long analysis of Psalm 104, in such a way that he distorts that which is said there about the removal or rather moving of the earth during the present age. Indeed, God is said there to have laid the foundations of the earth, that it should not be removed forever...In fact, into difficulties of this sort Copernicus...and Kepler, too, flung themselves for the sake of one or another postulate of astronomy, of which, of course, the authors were Pythagoreans and Peripatetic Ethnic philosophers, desiring to add [to the world] an infinity of heaven and stars^[2] and that the motions of these be regular and circular. But they also seem to find that neither of these canopies is in good repair. Indeed, Kepler is known to concede that the earth was put in the position of a planetary body, to be moved by an uneven process, slower in the

1 Bodl. MS. Bodl. 313, ff. 83^r-84^r; for text, see Appendix L.

2 If 'cupientes' modifies 'Copernicus atque Keplerus,' rather than 'Pythagoraei et Peripatetici Ethnici Philosophi' (and even if not), Lydiat is completely misrepresenting these men, who, unlike some of their followers, believed in a finite universe.

aphelion, or greater distance from the Sun, and faster in the perihelion, or lesser distance, and that it does not just seem so, but is so in reality; and further, that the path of a planet in the sky is in no way the mill-wheel of a circle, so to speak, but deviates perfectly toward an elliptical oval. He adds, as if in the place of a crown for the Sun, that it rotates, remaining in the same place, as if on a potter's wheel, the work being seen to by a vital faculty. In this same introduction, he holds more [positions] that are hardly likely; but this letter [of mine] must have a limit. Anyway, when I set out, in my Praelectio Astronomiae (first-born of my genius), published seven years ago, to deal with the same problem that Kepler [dealt with] long ago in his Mysterium Cosmographicum and just now in the long introduction of the Astronomia Nova or Physics of the Heavens, namely, outlining the constitution or system of the universe, I made the basis of my work the natural history of the sacred literature together with authentic understanding of it received from antiquity; and further, those consentaneous accounts of the position and motion of celestial bodies [drawn] from the most ancient philosophers, according to Plutarch, most learned of Greeks, and Pliny, whom Kepler calls 'that priest of Nature', most celebrated of the Latins, and from others, as much as I was able to gather; that vital or animal faculties were granted to the Sun together with the other stars, which are certainly not organic bodies, as causes of their motion, I put among the errors of the Ethnic Philosophers, and that by the judgement of Aristotle, one of the greatest names from among them. But in fact, I recognized, already to some extent in its epilogue, that much perfection was wanting in that little work of mine...'¹

Even through this self-advertisement, we see that Lydiat felt that he and Kepler were pursuing the same quest.

Lydiat long continued to worry Kepler's comments on chronology as if they were captive prey. The spirit of his Emendatio Temporum lived on into his Recensio et Explicatio (1613)² and his Epistola Astronomica, de Anni Solaris mensura (1621), in which he plays off against Kepler's account of astronomical refraction in

1 Bodl. MS. Bodl. 313, ff. 83^v-84^r; for Latin text, see Appendix L.

2 Thomas Lydiat, Recensio it Explicatio Argumentorum Prodvctorum libello Emendationis Temporum compendio factae... (London, 1613).

his Astronomiae Pars Optica, the account of Ptolemy.¹ He kept up to date with Kepler's work, for in 1622/23, we find him ordering from his bookseller a copy of the Epitome Astronomiae Copernicanae.² He returned, probably about this time, to the Astronomia Nova, on which he wrote a twenty-nine page tract of 'Animadversions', never published and now lost, which he showed to Henry Briggs for his comments, which likewise have not survived.³ This little tract was on the penultimate chapter of Kepler's book, which, being a revaluation of lengths assigned to the year, was the only section of the book of interest to a chronologer, and which had attracted the attention, also, of Harriot, who included chronology among his many interests.⁴

Lydiat's reaction to the Astronomia Nova was undeniably that of an Englishman, but it otherwise had no points in common with the responses of his English contemporaries. His world-view, while probably not unique, formed the basis of a natural philosophy that

1 Thomas Lydiat, Ad Clarissimum virum, D. Henricum Savilium, Equitem Auratum, ipsum Matheseos omnigenae scientissimum, adeoque geminae in eadem Professione Lecturae, in florentissima Academia Oxoniensi Fundatorem perquam munificum: Thomae Lydyat Epistola Astronomica, de Anni Solaris mensurâ (London, 1621), pp. 28-29.

2 Bodl. MS. Bodl. 313, f. 67^r, Lydiat to Waterson, 23 February/5 March 1622/23.

3 Bodl. MS. Bodl. 313, ff. 74^r-74^v is a list, perhaps in Lydiat's hand, of works by Lydiat in manuscript, and included on f. 74^r is: '2. Tractat. Animadversiones in doctissimi uiri Jo. Kepleri penultimū caput Theoriae Martis et septimū librū Epitomes Astronomiae Copernicanae. pages 29.' This work is not included in the lists of Lydiat's unpublished writings in the articles in Wood and the DNB. '...mine Animaduersions vpon Longomontanus and vpon Kepler: both which I shewed you...' Lydiat to Briggs, dated 'Monday 23 Februarie', probably 1623/24: Bodl. MS. Bodl. 313, ff. 73^r-73^v.

4 See above, pp. 139-140.

was, as there appears to have been no contemporary of his who shared it. Therefore, despite his familiarity with other learned readers of Kepler, his comments on the book could have had little influence, which conjecture is supported by what little evidence we possess. At most, it was only his niggling criticisms of Kepler's work on chronology that may have found a sympathetic audience, but even if these did, they are unlikely, because of their narrow application, to have affected the overall English response to the new astronomy.

XI. HENRY GELLIBRAND

Henry Gellibrand, another friend of Briggs's and Bainbridge's, but described as a 'plodding' mathematician,¹ took a curiously ambivalent view of Kepler's work. He was born, the son of a physician,² in London, in 1597. He went up to Trinity College, Oxford, in 1615, and took his B.A. in 1619. It is said that 'he was good for little a great while,'³ until he happened upon one of Sir Henry Savile's geometry lectures, and, fascinated by it, he went on to more profound study of mathematics, before receiving his M.A. in 1623. For a while, he was a curate in Kent (and later in life became a noted Puritan), but the professorship of astronomy at Gresham College falling vacant upon the death of Edmund Gunter, Gellibrand succeeded to the position late in 1626, at the recommendation of many of the dons at his old college.⁴ He continued to hold this position until his death in February, 1635/36.

1 Most of my biographical information comes from the article 'Gellibrand, Henry (1597-1636)' in the DNB. The epithet is that of Gordon Goodwin, author of the DNB article.

2 Information on Gellibrand's family background may be had from John H. Raach, 'Five Early Seventeenth Century English Country Physicians,' Journal of the History of Medicine and Allied Sciences, xx (1965), 217-219.

3 John Aubrey, Aubrey's Brief Lives: 1669-1696, ed. Andrew Clark (Oxford: at the Clarendon Press, 1898), i. 261.

4 John Ward, The Lives of the Professors of Gresham College (London, 1740, reprinted New York: Johnson Reprint Corporation, 1967), p. 82.

He was not a thoroughgoing copernican, which was rather unusual for an Englishman of his age and of his training. Toward the end of his life, he wrote,

'It is not unknowne to the world, how the Greatest Masters of Astronomie, which this age hath afforded, for the more easy salving the apparent anomalar motions of the fixed and erratique caelestiall lights, and avoyding that supervacaneous furniture of the Ancients, do with all alacrity embrace that admirable Copernican Hypothesis of the diurnal, Annual, & Secular motions of the earth, in so much as conferrng with that Great Astronomer D.Phil. Lansberg in Zealand but who spent some time in England, and whose tables were rivals to the Rudolphine Tables about Astronomicall matters, did most seriously affirme unto me, he should never be dissuaded from that Truth. This which he was pleased to stile a truth, I should readily receive as an Hypothesis, and so be easily led on to the consideration of the imbecillity of Mans apprehension, as not able rightly to conceive of this admirable opifice of God or frame of the world, without falling foule on so great an absurdity. Yet sure I am, it is a probable inducement to shake a wavering understanding.'¹

Perhaps he held this view on account of his slightly ambiguous motto: 'Belief in a mathematical thing is destructive in the extreme. For I prefer to err with probable reasons, to being led by blind truth.'²

Although in his fastidious physics, Gellibrand was unlike his

1 Henry Gellibrand, A Discovrse Mathematical On The Variation Of The Magneticall Needle. Together with Its admirable Diminution lately discovered. (London, 1635, but dated 28 July/7 August 1634), p. 20. Francis R. Johnson, Astronomical Thought in Renaissance England (Baltimore: The Johns Hopkins Press, 1937, reprinted New York: Octagon Books, Inc., 1968), pp. 268-269, is wrong to state that Gellibrand was a copernican; this statement makes explicit his lack of belief in the reality of heliocentrism.

2 'Credulitas in mathematico res est summe exitiosa. Rationibus enim verisimilibus errare, quam caeca veritate duci, maluisssem;' John Ward, op. cit., p. 85.

professorial colleagues at Oxford,¹ he, like them, read Kepler's Astronomia Nova very carefully. His scrupulously annotated copy passed, upon his death, into the hands of his friend, the legal historian, John Selden.² In this book, there is hardly a page without a note. Typographical errors are corrected, calculations re-performed, and useful tables are tipped in. There are, not surprisingly, no comments on Kepler's physics. However, there are extensive notes, on separate sheets of paper, on page 299, which, as we have seen, interested Bainbridge, too, and which, because it is the practical part, is in a way the nub of the Astronomia Nova. They comprise a thorough demonstration of how to calculate Mars' distance from the Sun, the coequated anomaly being given. The worked example uses a coequated anomaly that seems to have been calculated from Gellibrand's own observations. A later reader copied Gellibrand's notes verbatim, and this copy is to be found with the original between the pages of the book.

It can hardly be doubted that Gellibrand intended these notes for a practical purpose, though the point of the exercise is nowhere stated, for, as his comments on the copernican system

1 For Gellibrand's friendship with Bainbridge, see T.C.D. MS. 382, f. 119^r, Gellibrand to Bainbridge, 6/16 December 1634; there are other letters from Gellibrand to Bainbridge in this collection. Gellibrand completed Briggs's Trigonometria Britannica (Gouda, 1633); John Ward, op. cit., p. 83.

2 Gellibrand's name is not in the book, shelf-mark A. 1. 2. MED. SELD., but the hand can be identified by comparison with that of the notes in Bodleian 4^o. S. 24. Art. Seld., Willibrodi Snellii, Cyclometricus, where Gellibrand's name, in the same hand, occurs on the title-page. Gellibrand's only published reference to Kepler, other than the one quoted on the previous page, occurs in The Strange and Dangerous Voyage of Captaine Thomas Iames...With An Appendix concerning Longitude, by Master Henry Gellibrand... (London, 1633), sig. R3^v.

reveal, he could not have believed keplerian astronomy to give a true account of the world, and he was not, therefore, working out the planetary distances as part of an investigation into the constitution of the universe. Nevertheless, it is possible that, just as he believed the copernican system to give the best workable model of the universe, he came to believe that the mathematical part of Kepler's work superseded it. We cannot know this for certain, however, and it is remarkable how little his extensive notes on the Astronomia Nova really tell us.

PART III

APPENDICES: TEXTS AND NOTES

APPENDIX A

Latin text of Robert Fludd's comments on Kepler, taken from his Veritatis Proscenium (Frankfurt, 1621), pp. 5, 12, 36, and Kepler's comments on Fludd, taken from his Apologia (Frankfurt, 1622), p. 43, as quoted in Wolfgang Pauli, 'The Influence of Archetypal Ideas on the Scientific Theories of Kepler', pp. 195-197, and 199-200.

Fludd:

'Quod igitur ille multis verbis et longa oratione expressit, hoc ego brevibus contraxi, figurisque hieroglyphis et valde significantibus explicavi; non sane ideo, quia picturis delector (ut ipse alibi dicit) sed quoniam multa paucis congregare et more Chymicorum (quippe quem cum Chymicis et Hermeticis versari infra innuere videtur) extractam essentiam colligere, faeculentam vero substantiam reiicere, et quod bonum est in suo proprio vasculo collocare decreveram, ut detecto sic scientiae arcano occultum manifestaretur, reique natura interna exutis vestibis, more gemmae pretiosae aureo annulo insertae, figurae naturae suae magis aptae includeretur, in qua eius virtus, tanquam in speculo, absque verborum plurimorum circuitione oculo et animo conspiceretur.'

'Nam mathematicorum vulgarium est circa umbras quantitativas versari; Chymici et Hermetici veram corporum naturalium medullam amplectuntur.'

'...ipse motus rei naturatae exteriores excogitat, ego actus internos et essentielles ab ipsa natura profluentes considero; ipse caudam tenet, ego caput amplector; ego causam principalem, ipse illius effectus animadvertit. Et tamen ipse, quamvis motus eius extremi sint reales (ut dicit), magis coeno et luto impossibilitatis suae doctrinae inhaerit...'

Kepler:

'Mathesin si tu aliam nosti (praeter vulgarem illam, a qua denominati fuerunt quotquot hactenus mathematici celebrantur), quae scilicet sit naturalis et formalis, eam ego fateor nunquam delibasse, nisi ad generalissimam vocis originem confugimus, dimissis quantitatibus. De illa igitur scito me hic non esse locutum; habeas tibi, Roberte, laudem et illius et demonstrationem in illa, quae quam sint accuratae, quam certae, tute tecum judicabis sine me arbitro. Motus ego cogito visibiles sensuque ipso determinabiles, tu actus internos considerato deque iis in gradus distinguendis laborato; caudam ego teneo sed manu, tu caput amplectaris mente, modo ne somnians; ego contentus sum effectis seu planetarum motibus, tu si in ipsis causis invenisti harmonias adeo liquidas, quam sunt meae in motibus, aequum erit, ut ego et tibi de inventionem et mihi de perceptione gratuler, ubi primum percipere potero.'

APPENDIX B

A Note on Kepler's Books in British Libraries

I have tried to find, in copies of Kepler's books, marginalia and notes that might supplement the more conventional manuscript sources I have examined. I have not had the time to undertake a full, systematic search, but I have examined all the copies of the De Stella Nova Serpentarii, the Astronomia Nova, the Harmonice Mundi, the Epitome Astronomiae Copernicanae (the first and second editions, both of which have very narrow margins, unsuitable for long notes), and the Rudolphine Tables that were to be found in the Bodleian Library, Trinity College Library, Dublin, the 'Inter-Collegiate Catalogue of Pre-1640 Foreign Books in Oxford outside the Bodleian', ed. Paul Morgan (Oxford: unpublished, 1979), the British Library, the Cambridge University Library, and the Houghton Library, Harvard University. I am grateful to all of the librarians, especially the college librarians, some of whom went to some trouble for me, who enabled this research to be done. Alas, I found rather little. The only extensive contemporary notes I found are noted in chapter XI, on Henry Gellibrand, and most of the names of owners and dates of purchase are recorded in Part I, throughout. For the most part, where there were marginalia or notes written in copies (and this was infrequent), these consisted of corrections of obvious typographical and numerical errors, often copied from the publisher's list of errata; other marks were: underlinings, notes of 'N.B.' in the margins, and pencil ticks next to those passages the readers considered to be important. Among these last marks, there seemed to be no great consistency, and the only significant observation to be made was that the introduction to the Astronomia Nova was often the only part of that book that was marked.

I list below some copies, not mentioned in my text, that are worth recording:

Astronomia Nova: Bodleian Savile Q. 8. contains notes from the early eighteenth century, as is determined by a reference to Newton's Arithmetica Universalis (1707).

A copy in Jesus College, Oxford, was owned by Lord Herbert of Cherbury and was acquired by the college in the 1930s.

Harmonice Mundi: The copy in the Houghton Library, Harvard, is inscribed by Thomas Hatcher, otherwise unknown, with the motto, 'ῥορνείν εἰς τὸ σωῤορνείν'. The writing appears to be of the seventeenth century. This copy was later owned by the Jesuit College in Paris.

One of the two copies in Trinity College Library, Dublin, (B.C. 31) belonged to William Molyneux (1656-1698).

Epitome Astr. Cop.: Bodl. [MS. Ashmole 104] is William Lilly's copy of the second edition. He has made many of the alterations recommended in the list of errata.

Boulliaud, Astronomia Philolaica: I examined only a small selection of the copies of this in the libraries I visited and found no notes in any copies except that in New College, Oxford, in which a few notes and many underlinings indicate that the book was read with particular care, and the copy in the Cambridge U. L., inscribed with the name of the donor, Richard Foxton, 'Collegii Emmanuelis olim Alumnus, in Artibus Magister', and the date of the gift, 1656.

APPENDIX C

Latin text of Nathanael Carpenter's comments on keplerian astronomy, taken from his Philosophia Libera, second ed. (Oxford, 1622), p. 386.

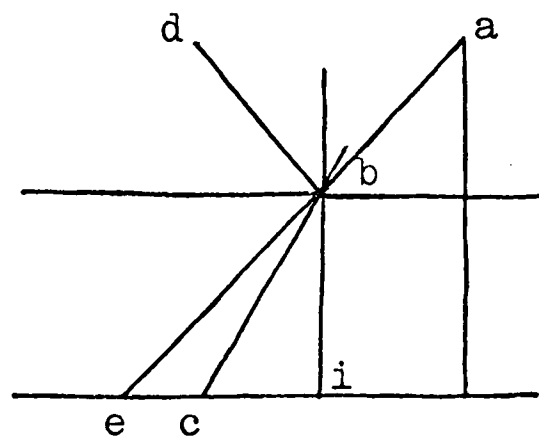
(Decad. III. Exercitat. X.) Nullum enim in corporibus maius Naturę indicium invenitur, quàm Motus: Non verisimile est itàque corpora haec splendidissima communi Naturae 'priuilegio spoliari, quo reliqua donantur omnia. Deinde in Systemate Kepleriano, licet ad Phaenomena coelestia saluanda satis sufficienti, non minus est quod excipiam ex ipsiûs Naturae principiis. 1. Quia Hypotheses suas absoluit praedictus author, non integris circulis, sed Lineis Ellipticis quas ovales vocāt. At de maximis hisce Naturae operibus cogitare non didici [sic] nisi optimum: Vnde probabilius videatur circuitus stellarum circulis integris & perfectis metiri, quàm Imperfectis, quae tantùm à Naturae recedunt integritate, quantùm à rotunditate deficiunt. 2. Motus omnes talibus mensurati lineis non possunt esse simplices sed misti & compositi: siquidem omnes motus simplices sunt aut absolurè [sic] Recti, aut absolutè circulares iuxta saniores Philosophos: At motus compositi & misti coelo non possunt competere, quod omnium corporum censetur esse simplicissimum. Vltimò accedit Schema Tichonianum...

APPENDIX D

Thomas Harriot and Johannes Kepler on the nature of refraction;
Wien, Nationalbibliothek, Cod. 10703, ff. 381^v-382^r, 383^v.

1. Harriot to Kepler, 2 December 1606.

Sit radius aliquis luminosus in medio raro ab. feriat superficiem alicuius densi diaphani obliquè in puncto b. Iam, quid faciet radius? Ingredietur necne? Ingredietur, inquis, quia medium ponitur diaphanū et radijs peruium. Sed non directè procedit



ad punctum e, sed refringitur et inclinatur in puncto b versus perpendiculū bi, et peruenit ad punctum c. Ita est, concedo, experientia docet; sed quare? Dices, impeditur in superficie ingrediendo atq; ideo refringitur. Nonnè idem impedimentū post ingressum? Nonnè superficies materialis eiusdem naturae cū toto corpore? Nonnè eadem superficies in toto corpore in omni positione, etsi non conspiciuntur? Nonnè igitur perpetuò refringitur in transitu sicut in principio? Respondes. Quia radius invenit nullam superficiem actu intra corpus, vel si vis, quia invenit quasi infinitas ~~quasi~~ superficies videlicet in omni positione, nescit radius quò se vertat, et ideò post primam fractionem in superficie actu, procedit sine alia flexione directè ad punctū c.

Dico quod magis esset pro commodo radij vt directè procederet per lineam be in principio quā fractè declinare, cum nullā causam invenire potest in principio, quin eadem obuiaret in transitu, scilicet impedimentum resistantiae. et cum eadem sit vbiq; resistentia, eadem esset deflexio perpetuò. sed quid hoc, et quo

modo? expecta et attende parumper. Revertamus ad initium vbe incipiebamus.

Quando radius ab feriebat superficiem densi diaphani in puncto b et ingrediebatur refractè: Quaero an eodem instanti ~~et~~ ab eodem puncto b reflectatur necne? Quod reflectitur ad partes d secundū angulū aequalem incidentiae non est dubitandum...experientia etiam docet, oculi vident: sed dubium restat de indentitate temporis et puncti. Iam non dabitur responsio sine absurdo. Nam idem punctum esset radio peruium et resistibile eodem instante et eadem pars radij esset in ~~duobus locis simul~~ duobus locis simul; vel in diuersis instantibus, vtrumq₃ mutaret naturam.

Quid igitur respondendū. cum vna pars contradictionis est falsa, necessariò sequitur alteram esse veram. Restat dicendum idem punctum non esse ~~peruiū~~ radio peruiū et resistibile; nec eundem radium numero reflecti à superficie densi diaphani, ac simul recipi~~at~~ intra corpus. Ergo corpus densū diaphanū quod sensu videtur esse continuum per omnes partes, revera continū non est. Sed habet partes corporeas quae radijs resistunt, et partes incorporeas radijs pervias. Ita vt refractio nihil aliud est quā interna reflexio, et pars radij intrò recepta etsi videtur sensui esse recta, est tamen revera composita ex multis. Hic sisto.

2. See p. 109, n. 1, above. Kepler to Harriot, 2 August 1607.

Aliam struis obiectionem cur radius non toto transitu refringatur, et fingis me respondere de quaesito commodo. ~~transit~~
Ego vero et repudiavj considerationem finis seu bonj, in legibus opticis, et causam dedi, cur superficies refringat, corpus non refringat. Lucem enim dixj ex genere esse superficie

participantium, itaq₃ a superficiebus patj, non a corpulentia, cum ea non participet ipsa. Lege primum caput meae Optices.

Tu verò rem ad contradictionis bivium, pertrahis et partim argumentando partim allegorijs, chymicorum in more, ludendo me ad Atomos et vacua Naturae ablegare videris. At quod tibi videtur absurdum idem punctum eodem instantj et transmittere radium et repercutere, id mihi absurdum non est. Defendj hoc aperta propositione capite I. optices. Lucj enim, itsj superficiem, unâ tamen et densitatem tribuj: cui₉ gratia in tenuiores duas luces distrahj possit. Et quid absurdj, si, cum sit merè opacj natura mere repercutere, merè perspicuj ('id est, superficie carentis') merè transmittere, si inquam quod est inter medium inter opacum et merè perspicuum, ut sunt corpora omnia pellucida superficiebus terminata, id partim transmittat lucem, quatenus perspicuum. partim repercutiat, quatenus de opaco participat, et superficie terminantur.

APPENDIX E

Harriot's brief notes on Kepler's Astronomia Nova, Petworth House,
Leconfield MSS. H.M.C. 241/III.2, ff. 23-25.

A typescript facsimile.

Overleaf.

De motu ♂. Kep.

pag. 313. 1585. Januar. 25. fuit Maxima latitudo ♂
circa 19. ♂. w̄w̄.

Maxima latitudo borea. 4. 31. $\frac{3}{4}$ quæ in coniunctione
solis, cū is distat 101280. apparet. 1. 8. 30."

Maxima lat. austrina 6. 52. 20." quæ in ♂. appar: 2. 4. 20."

312. locus limitis borei 16. 50. ♂.

Jntrod. §. 10. { ♂ 1608. mense Augusto. superat locū ex prutenicis
paulo minus 4. gradibus
1593. augusto et septembri. paulo minus 5. gr.
(1610. octob. 6. differt à calculo pruten. 3. gr.)
Jn dissertatione cū nuncio sidereo. pag 5. }

285. latitudo lunulæ siue differentia² semidiametrorū
ellipseos. 429. Dimidiū videlicet 858. pag. 225. 223.
432. correctius. pag. 267. 267. *.

304. Dist: ♂ a terra. Distantia: sinus sinus
♂. à. ☉. latitudinis Inclinationis
planorū.

orbita ♂. ellipsis.

Maior semidiameter. 100,000.

Minor semidiameter. 99 570.

Differentia. 430. fd. 432. $\frac{430}{2} = 215.$
pag. 267.)

è centro ad centroides
vbi centrū ☉. } 9265. pa: 299.

$$\frac{100000}{215} = \frac{465\frac{1}{10}}{1}$$

siue eccentricitas.

Aphelia. 109 265.

Perihelia. 90 735.

465. ^{inches} = 38 $\frac{75}{100}$ feet

$$\frac{100000}{9265} = 10 \frac{79}{100}.$$

114. octo solū minuta viam præiuerunt ad
totam Astronomiam reformandam. &c.

f. 24.De motu δ . Kep.

- pag. 332. Jta cor Ω et spicam η ptolomæus prodit intervallũ. $\overline{54}$. 00.¹
333. Medius motus $\odot$ annis 1460 ægyptijs Tycho.-- $\overline{53}$. 59.¹
~~deficit~~ minor quã ex prutenicis., 17'. 42". Tycho. $\overline{54}$. 2.¹
-
- 265 | correctâ aphelia. 166,465. | vt radius 152,350.
 ca. 54 | perihelia. 138,234. | semidiameter δ .
 267. | latitudo lunulæ. | Diameter. | pag. 267.
 660 fere | Suma. 304 699. | radius.
 sed vt | Dimid. 152 349 $\frac{1}{2}$.
 semid. δ | Drâ. 28 231.
 100,000. 432 | Dimid. 14 115 $\frac{1}{2}$. | eccentricitas. et: $9265\frac{2}{10}$
 vt radius. 100,000.
-
234. | eccentricitas inventa cap. 42. fuit 9264.
 | Dimidiũ eccentricitatis æquantis cap. 17. fuit .9282. cap. 41. pag. 209.
262. | Apheliũ δ . correct. 1600. completo. 29 . 0'. $\frac{2}{3}$. δ .
 | Motus medius eodẽ tempore. 10^s 7. 14'. 34".
 | observationes δ . 28.
151. Eccentricitas $\odot$. 1800. iustè vt radius 100,000.
209. correctio Aphelij. eccentricitatis. ^[9264.] Medij motus. δ . Cap. 42.
202. 1590. octob³¹. apheliũ. 28 . 53'. δ . cap. 16.
208. | Integra revolutio δ^{tis} ad eadem stellã fixã. 687^d-0^h 428'. | pa. 73.
 | Dimidia periodus. 343^d 11^h 46'. | hoc est. 1^a 321^d 23^h 32'. | pag. 127.
 vide 165 | Diurni motus δ sunt quã proximè in dupla proportione
 166 | distantiaru. Jn apogæo diurnus 26'. 13". in perig. 38'. 2". Med. 31'. 27".
55. Brahei tabula de 10. locis acronychijs δ .
303. Nodi δ . correcti. 1595. Completo. 16 . 46'. $\frac{1}{3}$. δ^{as} . m_{δ}^{des} .
305. Inclinator orbis martis. 1 . 50'. 30". correctâ. vide pag. 78. 79.
107. Motus. nodi ^{a limitis.} in anno. 0'. 40". aphelij. 1'. 4".
-
53. | 1585. Apog. δ . 23 . 45'. δ . secundũ Tychonem.
 54. | Maxima eccentricitas 20160. qualiũ semidiameter
 maioris epicycli 16380. vide. pag. 92. &. 112.
106. eccentricitas $\odot$. δ . & apogea secundũ ptolomæũ &c

f. 25.

De motu ♂. Kepl.

pag.

67. Hoc ipso tempore 1604, quo de parallaxibus cogito (solis
 magis an ♂ haud queo dicere, Nam postulat Hipparchus
 meus suis etiam eclipsibus Lunae à ♂ subsidu.) commo=
 dissima se obtulit occasio obseruandi, si sub alio climate fuisset
 Marsq₃ altius paulo incessisset. Mars namq₃ &c.

pa.	Nam si planeta rectis lineis angulos conformatibus
91.	incederet (vt si latera quinquanguli perambulare
	in quibus agitationibus olim fui.) pro ratione &c.

 Note

f. 24.

Line '208...' The '7' in '687^d' appears to have been
 written over an original '6'.

APPENDIX F

Harriot's Manuscripts, Notes, and Correspondence Pertaining to
Kepler.

Harriot's Manuscripts Pertaining to Kepler

Petworth House, Leconfield MS. 241/III.2, ff. 1-63. Headed 'Ellipsis $\oslash$.' Discussed above, pp. 131-140. F.1 is reproduced in Plate I, p. 143. Ff. 53, 55-63 are in Sir William Lower's hand.

B.L. Add. MS. 6787, ff. 175^r-188^r. Headed ' De Anomalijs'. See above, p. 145-146. These pages might repay further analysis.

B.L. Add. MS. 6787, ff. 408^r-424^r. Headed 'Ellipsis $\oslash$.' (F. 407^r, preceding, is headed ' de ellipsi'.) Ff. 413^r, 414^r, 416^r, 418^r, and 422^r lack this heading. F. 422^r is blank, as are the versos, with the exceptions of ff. 413^v, 414^v, 416^v, and 422^v. Most of these pages are numbered in an elaborate system.

B.L. Add. MS. 6787, f. 113^r. A note appears to say, 'Martis booke,' but could be 'Martin's booke.'

B.L. Add. MS. 6787, ff. 455^r, 458^r-463^r. These pages concern the determining of an ellipse from five points, and are in the midst of papers on conic sections.

B.L. Add. MS. 6787, f. 511^r. This is nearly identical to f. 1 of the Leconfield papers, but for the lack of any reference to Kepler or Mars.

B.L. Add. MS. 6787, f. 556^r. There is, here, a drawing of an ellipse, similar to that on f. 1 of the Leconfield papers, but here it appears in the midst of a series of pages on conic sections.

B.L. Add. MS. 6788, ff. 463^r-465^v. F. 463^v is headed, 'Kepl. ♂. pag. 333', and these pages appear to belong with those at the end of the Leconfield collection.

B.L. Add. MS. 6789, ff. 448^r-450^r. These are two sets of notes by Nathaniel Torporley on Harriot's papers, presumably drawn up after Harriot's death. (ff. 448^r-449^v, 450^r-450^v.) On f. 448^v are the following two entries:

'De motu ♂. 1.b. problema, 1.b. pblema insigne, 1.b. p,6.'

'Ellipsis ♂, 1, b. 2, other bundles out of h[?].'

At the end of f. 450^r is

'Ellipsis Martis. _____ pag. 8.'

B.L. Add. MS. 6789, ff. 464^v, 465^v. These appear to belong to the set of Harriot's notes at the end of the Leconfield collection, being headed, 'De motu ♂. Kepler. pa: 333.', and concerning the annual motion of the Martian aphelion.

B.L. Add. MS. 6789, f. 491^r. This is a list of books, beginning with 'Keplerus de mot stella ♂ fol.'

Mention of Other Books by Kepler

(List taken from R.C.H. Tanner, 'Works Mentioned or Implied in Harriot's MSS.', unpublished, revised edition.)

<u>Eclogae Chronicae...</u> Prag, 1606.	B.L. Add. MS. 6789, f. 522 ^v .
<u>Ephemerides novae ad 1618.</u> Linz.	Leconfield MS., Hist. MSS. Comm. 241/IV, ff. 10 ^r - 10 ^v .
<u>Epitome Astronomiae Copernicanae.</u>	Leconfield MS., Hist. MSS. Comm. 241/IV, ff. 10 ^r - 10 ^v .
<u>Narratio de observatis a se quatuor</u> <u>Jovis satellitibus.</u> 1611.	Leconfield MS., Hist. MSS. Comm. 241/IV.2, f. 13 ^r .
<u>De Stella nova.</u> 1606.	Leconfield MS., Hist. MSS. Comm. 241/II, f. 4 ^r .
<u>Stereometria nova.</u> 1615.	B.L. Add. MS. 6789, f. 427 ^r (?).

(See also Roche, op. cit., p. 54.)

This list does not include books referred to on the pages already catalogued above.

Drawings of Ellipses Generated by Circles

B.L. Add. MS. 6787, f. 390^r. This is the clearest example.
Reproduced above as Plate II.

B.L. Add. MS. 6787, ff. 391^r, 392^r (where the ellipse is not traced in), 402^v (where the epicycles are not drawn), and 453^r (where the drawing is rather complex, lacks epicycles, and may belong to a different exercise).

B.L. Add. MS. 6788, f. 311^r.

Harriot's Correspondence with Kepler

See above, p. 106, note 4. Discussed and quoted above, pa

Sir William Lower's Correspondence with Harriot about Kepler

B.L. Add. MS. 6789, ff. 425^r-426^v. Letter of 11 June 1610.

Franz von Zach, editor. Monatliche Correspondenz, Achter Band (Gotha, 1803), let. III, pp. 30-60. First half of letter of 6 February 1610/11, also published in

Baron François de Zach, editor. Correspondance Astronomique, Géographique, Hydrographique et Statistique, No. II (Geneva, 1822), Lettre V, pp. 111-138.

B.L. Add. MS. 6789, ff. 427^r-428^v. Second half of letter of 6 February 1610/11.

B.L. Add. MS. 6789, ff. 429^r-430^v. Letter of 4 March 1611.

B.L. Add. MS. 6789, ff. 433^r-434^v. Letter of 19 July 1611.

All these letters are discussed and quoted above, passim.

APPENDIX G

Transcript of extracts from Trinity College Dublin MS. 382, ff.
68^r-70^v, Christopher Heydon to John Bainbridge, 6 December
1619.

But wheras y^{ou} also reprove my Methode in inquiring the Nodes, and therfor demand of me what Astronomer did euer fynd them as I do in the Planets, as for example in δ or φ ._____by two obseruations. Suerly sir if yo^u please to turne to the 12 chap: of Kepler de motib⁹ Martis, yo^u shall fynd that Tycho did by two obseruations of δ , one in his greatest Latitude, an other in his propinquitie to his Intersection of the Ecliptick fynd out the place of his Node. neyther Can Kepler quarrell wth his succeſſe: though he sayth Demonstrationis ἀκριβεῖα. comendat aliam. In Comets not Tycho alone, but Rothman, Snellius, and Kep= $\square$ him self euen in this last Comet, as yo^u may se cap 2^o pag 71 & 72, doth inquire ~~the~~ both Nods, ~~and~~ angles, and Arks by this very method of 2 obseruations, as I doe: one $\frac{19}{29}$ Nouember, the other 3^o Januarij, and thervppon fyndeth the Node of this last Comet in 16: 18' of η : Neyther I suppose will yo^u deny those to be Astronomers: It wer better I Confess, (if it had ben possible) to haue obserued the Comet when he had no Latitud, or befor a little, and after his intersectione, to fynd out the place of his Crossing the Ecliptik: But he was then sub radijs, and I doe say, wthin one day after his Emersione yo^u obserued him as nere the Ecliptik as his motion wold giue yo^u leaue, wherfor taking yo^r firste: 18: of November, and my owne vppon the 14 of December, I fynd the Nod to fale out in the 16: 42': 51" of the sam Signe nerer yo^u, then Kepler: and the Ark of the Comets way Comprehended betuen both obseruations, w^{ch} he measured in 26 dayes, to be 67: 36': 56". the rest of that ark betwene yo^r firste, & the Ecliptik: 5^o: 18': 17": and the Angle 63: 11': 46" the base of intersectione 2^o: 23': 51": neyther as I supose am I far wyde: yet thus yo^u se both Kepler and I by the same Method, w^{ch} yo^u reprehend, do

fynd the Nods wthout any great difference by 4 seuerall obseruations, for his & myne be diuerse. but I Confesse it must be wth Caution, Chosing one obseruatione euer as nere the Ecliptik as may be, the other as ner lykwyse as one Can, to one of the limitts: for when we Can not do as we wold we must do as we may, otherwyse in taking any two meane obseruations I Confesse ther may be varyety, therfor I think that next the Ecliptike, must be euer one invariabl.

yet if I shold not be to tedious because yo^u fale frō y^e f. 69^r
 Circulare Course of the Comet to Rectileneall traiection, I wold be bould---in a word or two to tell yo^u that as it is agaynst philosophie, agaynst all former obseruatione and practise, and to my iudgement all one wth a Circle: So in Keplers book being newly Cum to my hands yet I can se no reson why he leaues a Circle to embrace y^e Paradox of Rectilineall motione. I say it is agaynst Philosophie because ther I fynd Rectilineall motione proper to this sublunery Spher of mortality and Circular motione only agreable to the aetheriall and heuenly Regions as is euident by ~~th~~ Experienc in the motion of all other caelestiall bodys. I say it is agaynst the obseruations of former Comets Because Tycho Mestline Rothman in fiue Comets fynd them all to shape their Courses by a great Circle, the like doth Kepler him self affirm of that Comet a^o 96 and ϕ 1607 If therfor 7 Comets successiuely one after an other did describe a great circle I think the Induction good to Conclud that all Comets do the lyke. Lastly I say it sems to me all one, for that we from the Erth can behold no right lyne in heauen w^{ch} doth not subtend a great Circle ther: So as to me it is all one whether the motione be Circular or rectilyneall I haue Keplers

book, but the bookbynder hath neyther inserted his Diagrams to w^{ch} his demonstrations do poynt nor the Table of this Comets motion only I fynd two resons for w^{ch} he will haue----this last to decline or swarue a little from a Circle, and those by yo^r patience I will mak bould to ventilate according to the liberty yo^u allowe me:

It also went 1^o: 30': 37" to the North of Penultima cauda f. 69^r Draconis w^{ch} is that I vndertook to demonstrat, and therfor granting what Kepler desyres and that the Comet did Cutt the lyne of those two fixeds, he may se this proues no deflexione from a great Circle. I Cum now to his second argument in the second Chapter: p: 70 wher though he Confesseth that the Course of this Comet pricked into a Glob doth seme quam proxime sub vnam circuli magnū ordinari; yet after he addeth Ab hoc vero circulo Cometam flexisse parum in Austrum, preter superiora etiam sic probatur euidentiū. Now S^r what successe he hath in his first proof I refer to yo^r lerned iudgm^t: It remayneth yt we examine his second mor euidēt proof. Wherfor thus he resoneth: Sectio Circuli cum Ecliptica, fuit in 16: m̄: Ergo Limes Boreus in 16 ∩: Thus far we agre: And it is also true w^{ch} he inferreth further: Hic ergo debuit esse Latitudo maxima But how doth he disproue that here the Comet had not his greatest Latitud? doth he produce any accurat ~~of~~ obseruations eyther of his owne or of any other mans at the moment or vppon y^e day that the Comet passed his Boreall Limit to the end we may know how much this Comet then fayled of his greatest Latitude? no such matter: But Kepler ses wth an other mans eys, to whom neuerthelesse he giues no Credit as we shall se by and by: and by other mens differing obseruations vppon other tymes infers

diuerse days after by his Coniecture, when the Comet shold passe his Boreall limit, and that then his latitud was lesse because it declyned from a gret Circle to the south and held not proportion ~~wth~~ of Latitud wth the lyk distances from his limit befor & after. ffor thus he sayth At Die 3. Januarij in 19ℓ: (potius in 21: vel 22ℓ: fuit Latitudo 63: 15: But stay here and befor we cum to his Conclusions let vs examine this passage a little better: what means Kepler out of Coniecture to correct Ingolstadiensis his obseru[at]ions who sayth the Longitud of the Comet this day (being their 3 of Januarij our 24 of december) was in...

Kepler him self professeth he could discerne neyther head not tayle f. 70^r
 uherfor dars affirme Nothinge: At Ingolstadiensis acutiori Visu pollens ea Vespera circa horam 8: a meidie Cometam Vidisse profitetur inter Lucidam in Quadrilatero Minoris Vrsae, & australem posteriorum Plaustri in linea fere vltimarum Caudae Draconis, Viae pauculis minutis remotam a Penultima. Ergo (sayth Kepler) in 11ℓ cum Lat: 62: This is his Palladium, and if this obseruatione be extenuated Certeynly his argument is ouerthrone. But at the first sight here is no obseruatione wth any Astronomicall instrument therfor my Ergo is better then Keplers that this can be no certeyne and accurat obseruatione and so no good argument to disproue the Circular motione of the Comet.

Neyther can I beleue that I err much though I do not beleue my f. 70^v
 self. but eyther think ther is some error in Tychos tables or else Ingolstadiensis was deceiued in his marks, or Kepler wer deceiued in his Computation.

APPENDIX H

A transcription of part of Bodl. MS. Ashmole 242, f. 168^v,
Christopher Heydon to Henry Briggs (no date).

Good Mr Briggs because I expect your selfe euery day I will not now be troublesome unto you: only I will now giue you thanks for your Table your new supputation & Diagram: & in my Judgment I thinke the cause why you erre twelue munutes in the longitude is y^t for y^t you haue mistaken y^e proportions sumthing: for I haue examined your calculation and it is right:

In your new Diagram though y^e inuention delighted me much yet I think twill hardly be brought to supply our desires for I haue reduced your lynes to y^e proportion yo^u directed me making them all to consist of such parts as y^e diameter of yo^r Epicycle is 100000: but it will not doe by many degrees: neither in truth can it (to speak my opinion) for first you make your Elypsis to uary more from a circle then Keplers hypothesis w^c he calls Genuine: ffor in his description therof he maketh y^e greatest ingresse of y^e planet at y^e sydes to be but 432: such pts as y^e whole Radius ϕ his orbe is 100000: or else 660: such pts as the same diameter is 152350: wheras you make y^e whole Diameter of y^e Epicycle being 100000: to be y^e measure of y^e planets ingresse in his midle longitudes: so here you see a great difference for according to Keplers positions y^e planet can but measure $\frac{1}{4}$ of his Epicycle in y^t place by you he shall be in Perihelio Epicycli: by reason of his double motion ther to y^e middle Anomalye is y^e Excentrick wherfore he leaueth half y^e Epicycle w^tin y^e Circumference of y^e Elypsis: you the whole diameter out of y^e same: Besides wheras you make ϕ mooue aequallye upon y^e Center of yo^r Eccentrick so doth not Kepler eyther in his true hypothesis or vicaria: but in y^e one he assigneth a fre center of an aequant to this purpose and in y^e other (wh y^e first) he supplyeth his inaequalitye by finding y^e Area of y^e whole anomalye computed

at y^e Sunn and added or subtracted to or fro y^e Arke by y^e middle motions, as occasion & y^e situation of the starre shall require: ffor in Aphelio he mooueth slower, in Perihelio swifter, y^e inaequality wherof is resomed by those Areas w^c he speaketh of: lastly altho y^e addition of your sine of y^e complement or subtraction of the same, may serue to supply y^e center of y^e aequant in the upper part of y^e orbi yet being uariable w^t y^e Anomalye it and diminisheth y^e excentricity so infinitly as there can be no truth in it but at your comming w^c I wish speedily wee shall confer more liberally of these matters:

In y^e mean time I can but assure you of your Welcome how mean soeuer your farre and so w^t all our hastiest commendations I Rest

I know I obserued out p^portion Your euer most assured freind

in my diagram for I know y^e Christ: Heydon:

semidiameter of y^e Anuall orbe is $\frac{2}{3}$ of y^e whole semidiameter of ♂ his Orbe I likewise know y^t y^e sun is y^e center of y^e Zodiak by kepler & therfor the place therof giuen & y^e locus excentricus of ♂ and his place in y^e Zodiak, I know all y^e angles are knowne at y^e planet & earth w^t out supputation but yet for triall I thought good by his distances to make experience:

APPENDIX I

John Bainbridge's lecture notes on keplerian astronomy; Trinity
College Dublin MS. 385, ff. 101^r-109^v.

The text of this lecture presents unusual difficulties. First, it is clearly not a single lecture: for example, the first three folios (ff. 101^v-102^r) appear to be concerned only with the pure mathematics of a conic section, the ellipse, though there are hints that this lesson is for application, while the remaining folios concern elliptical orbits, but begin with a partial recapitulation of the properties of ellipses. This second, much longer part, may represent more than one lecture. Second, some parts of the lecture are simply illegible. The largest lacuna in the following transcription is from the middle of f. 104^r to the bottom of f. 106^r, which is unfortunate, because the occasional patches that can be read hint at moderately interesting matter.

To present a true, typescript facsimile of these notes would have been self-defeating. All the contractions and corrections, if faithfully transcribed, would have rendered the typescript unreadable. Therefore, I have expanded most contractions, ignored lineation, and passed over anything, from letters to paragraphs, that has been crossed out; the majority of the items deleted are illegible, anyway. I have, however, indicated with empty square brackets the places where words or parts of words are illegible, when the surrounding writing is clear. Doubtless, I have made a number of errors of transcription, but I have represented, as best I can, the text as I see it.

The transcription follows, overleaf.

Elleipsis est figura plana, una contenta lineae, quae si tangit
circulum circumscriptum duobus ex diametro punctis, in reliquis
deficit, defectus vero in eadem ratione qua sinus recti crescunt, +
decrescunt.

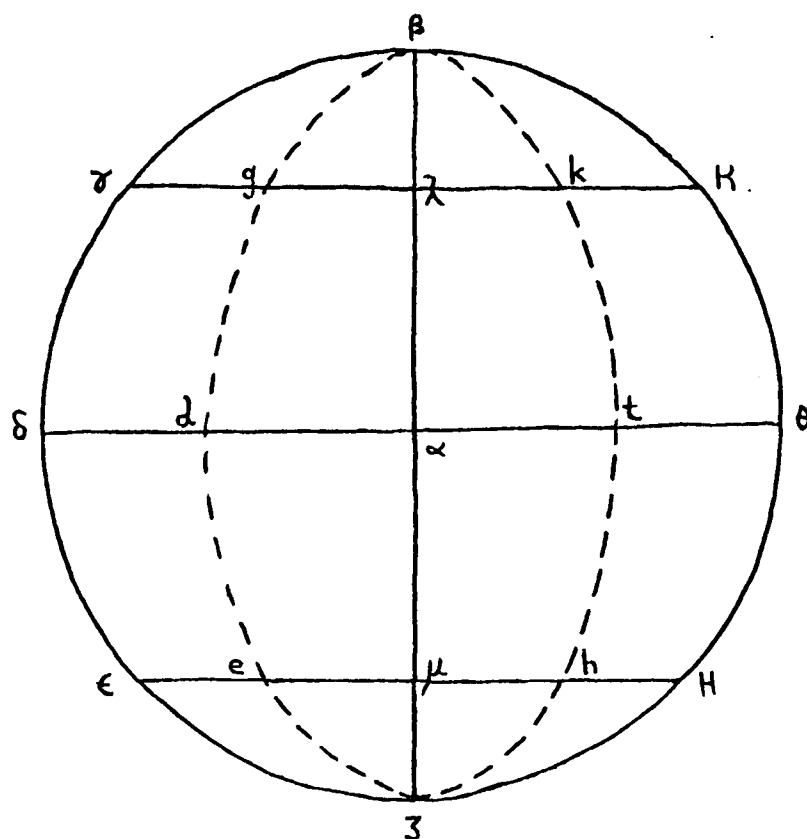
f. 101^r

figura $\beta\delta\zeta\theta$, una linea contenta, tangit circulum $\beta\delta\zeta\theta$. in β , ζ ex
diametro punctis in reliquis vero a circulo deficit. et defectus
 δd . $\pi\rho\omicron\varsigma$ defectum γg eandem habet rationem, quam habet $\delta\alpha$ $\pi\rho\omicron\varsigma$ $\gamma\lambda$.
et sic in caeteris. α est centrum eum circuli, eum Elleipsis $\beta\alpha\zeta$
est diameter circuli, et longius diameter Elleipsis; dicatur etiam
diameter apsidum. $d\alpha t$, est diameter Elleipsis brevior $d\alpha$, $g\lambda$, $e\mu$:
ab Apollonio, et alijs antiquis dicuntur ordinatim applicatae $\tau\eta$.
 $\beta\zeta$. dicantur verò sinus Elleiptici

Coroll: 1. pro varia quantitate $d\tau$ infinitae elleipsis possunt
describi intra eundem circulum, omnesque circulum iisdem punctis
tangentes, ita ut tandem circulus per infinit[] Elleipsos deficiat
in rectam $\beta\zeta$.

Coroll. 2. sinus elliptici eandem[] habent, quam, sinibus sinus
circ[] ut $\delta\alpha$ $\pi\rho\omicron\varsigma$ $\gamma\lambda$. sic $\delta\alpha$ [] Cum enim ex definiti[]

elleipsis ut $\delta\alpha$ $\pi\rho\omicron\varsigma$ $\gamma\lambda$. sic δd $\pi\rho\omicron\varsigma$ erit etiam (per 19 per 5,

f. 101^v

punctum lineae Elleipti cum quanto altera major fuerit $\alpha\beta$, tanto altera erit minor. sit punctum g . et ducantur ηg , θg per g ducatur sinus $\gamma g\lambda$. per Coroll 3. ut quadrant $\delta\alpha$, $\pi\rho\omicron\varsigma$ quadrat $[\gamma\lambda]$. sic differentia quadratur $\delta\alpha$, $\delta\alpha$ $\pi\rho\omicron\varsigma$ differentiam quadratur $\gamma\lambda$. $g\lambda$ sed diff quadr $\delta\alpha$, $\delta\alpha$. et quadr $\alpha\theta$. nam $d\theta$, $\delta\alpha$ sunt aequales. prò ut quad $\gamma\alpha$. (hoc est $\delta\alpha$) $\pi\rho\omicron\varsigma$ quad. $\gamma\lambda$. sic quad $\alpha\theta$ $\pi\rho\omicron\varsigma$ quad $\theta\kappa$ (per 4 et 22 p. 6 Elem) quare per 9 p. 5 El. quad $\theta\kappa$ et diff quadr $\gamma\lambda$, $g\lambda$. at proinde quadratur $\gamma\eta$, $g\eta$, atque etiam quadrat $\gamma\theta$. $g\theta$. sed etiam quare $\gamma\theta$ $\gamma\kappa$ $g\theta$. $\gamma\kappa$ sunt aequales. excessus verò $g\theta$ sive $\gamma\kappa$ supra $\gamma\alpha$ est $\alpha\kappa$ deinde $\eta\mu$. est aequalis $\theta\kappa$. quare quadr $\eta\mu$ est diff quadr $\gamma\lambda$ $g\lambda$ et quadr $\gamma\eta$. $g\eta$. est etiam diff quadratur $\gamma\eta$, $\gamma\mu$, quare $g\eta$. $\gamma\mu$ sunt aequales defectus vero $g\eta$, sive $\gamma\mu$ a $\gamma\alpha$ est $\mu\alpha$, aequalis $\alpha\kappa$: quare quantò $\gamma\kappa$ vel $g\theta$.) est major $\tau\eta$ $\gamma\alpha$ tantò $\gamma\mu$ (vel $g\eta$) est minor. utraeque verò simul sumptae aequant

[The remaining few lines, to the bottom of the page, are largely illegible.]

Sit planetae Orbita Elleiptica, in cujus longiori diametro sunt duo puncta (aequalibus a medio puncto intervallis) a quibus ad quodvis Orbitae punctum duae rectae lineae simul sumptae aequant diametrum longiorem. haec puncta dicantur foci, maxime alterum in quo residet potentia Planetam movens. Hic focus 5 planetarum, et Terrae orbitis est ipse Sol. $\odot$, in γ^{ae} orbitâ est Terra punctum orbitae in diametro longiori a foco remotissimum in illis dicatur aphelion; in hâc apogaeon; punctum foco proximum in illis perihelion, in hac perigaeon. puncta vero Orbitae in diametro breviori mediam a foco habent distantiam. puncto diametrorum medio (sive intersectionis $\pi\rho\omicron\varsigma$ $\sigma\rho\theta\alpha\iota$) ut centro describatur circulos, tangens Orbitam in aphelio, et perihelio. qui dicatur Excentricus. linea inter focum,

f. 103^r

et centrum, dicitur Eccentricitas. Planeta in Orbita suâ semper positi / locus refertur ad eccentricum per lineam ordinatim applicatam, sive ductam a planeta ad Eccentricum proximum, et diametro breviori parallelam locus hic planetae in Excentrico ab aphelio, vel apogaeo numeratus in consequentia, dicatur Anomalia Eccentrici.

Tempus verò qui planeta anomalia Eccentrici auficit dicatur Anomalia temporis, haec Anomalia reducenda est (more Vulgari/ ad gradus Et quales totum tempus periodicum habet 360 . Aequalibus Ecc: arcubus inaequalia respondent tempora. Uni quidem Ecc gradij per quem medium transit diameter brevior (producta/ unus debetur temporis gradus: hîc tempus habetur medium, quod universim ita dicatur quandò quislibet Eccentrici gradibus aequ[] Temporis gradus deberi supponui[] Arcus aphelij aut apogaej majori tempo[] peragantur, nam illic planeta a foco remotior est ideòque tardior; arcus verò perihelij aut perigaej brevior tempore hic enim planeta foco prior; ideoque ceteros.

quando distantia planetae est major mediâ, excessus dicatur f. 103^v recessus, quando minor, defectus dicatur accessus. quando Eccentrici gradibus aliquot, plures [?] debentur Temporis gradus, excessus dicatur Mora; quando pauciores, defectus dicatur velocitas.

Duobus aequalibus Eccentrici arcubus, morae aut velocitates eandem rationem habent, quam recessus, et accessus, ita ut planetae in suâ orbitâ locus utrobique referatur ad arcuum datorum punctum medium. Eodem modo temporis ad nempus eadem ratio, quae distantiarum.

Data Eccentrici Anomaliâ. dabitur accessus aut recessus: Si

Anomalia sit in 1^o quadrante sumatur [?] compl ad 90. si in quad:
excessus supra 90, si in 3^o complea [?] ad 270. Si in 4^o
excessus supra 270: et arcûs logarithmo adde log Eccentricitatis,
fit logarithmus recessûs in 1^o, et 4^o quadrantibus accessûs in
2^o, it 3^o.

si recessus addatur, accessus auferat a distantia media, sive radio
Eccentrici, fiet distantia planetae a foco, ad locum in sua orbita:

Ut distantia media ad recessum in 30' ab aphelio aut apogaeo. Sic $\bar{1}$
temporis, ad moram it toto primo gradu Eccentici ut quae recessus
logarithmo addatur logarithmus $\bar{1}$ temporis. et fiet logarithmus
moriae. $\bar{1}$ temporis non quaerendus est inter gradus sed sinus.
sumatur. $\underline{1000}$ cujus log. 230258.

Ut sinus unius gradus anomaliae Ecc: ad moram sic radius ad omnium
morarum per totum quadr: summam. quare a logarithmo morae auferatur
logar: $\bar{1}$ omaliae Ecc: reliquata log. morarum summa.

log Compl. $\bar{0}$. 30.' Anoml Ecc.

log $\bar{1}$ Temporis vel $\underline{1000}$

$\begin{array}{r} 38 \\ 2302585 \end{array}$ adde

2302623 sub

log $\bar{1}$ Anomaliae Ecc.

$\underline{4048278}$

log omnibus planetas communis

1745655*

Log. communis auferatur a log Eccentricitatis et relinquetur

log summae morarum omnium in trio quadrante Eccentrici.

f. 104^r

Datâ Anomaliâ Eccentrici dabitur anomalia Temporis.

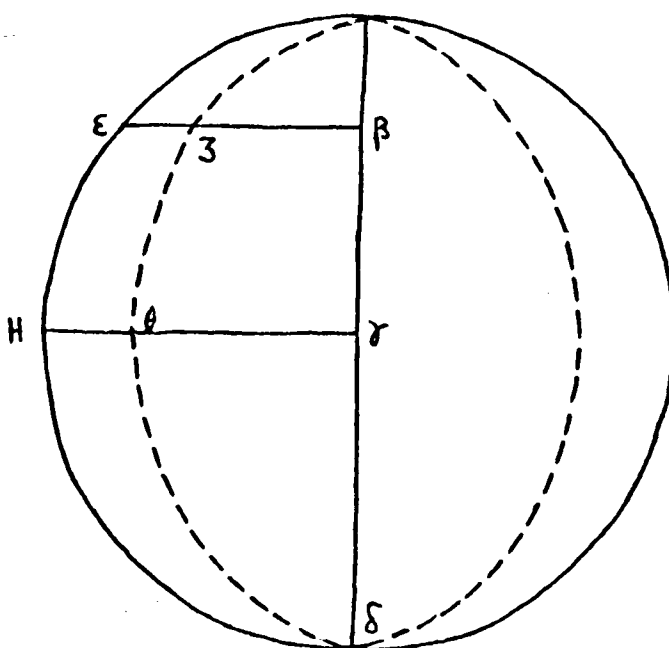
Ut radius ad sinum Anomaliae Eccentrici (vel complenti ad $\overline{180}$ in 2 quadrante) sic prostaphaeresis temporis maxima

[This page becomes increasingly illegible; f. 105^r is random calculations; f. 105^v is mostly crossed out; some notes comparing a method of Kepler's and 'our method', all but completely illegible, are at the bottom of f. 106^r.]

Si centro Elleipsis, et in eodem plano describatur circuli circumferentia tangens Elleipsin in utroque diametri longioris extremo, diameter illa, dicatur Apsidum.

f. 107^r

Iisdem positis, Elleipsis dividit sinus ad Diametrum Apsidum ductos in eandem rationem.



Sint $\epsilon\beta$, $\eta\gamma$. sinus ad $\alpha\delta$ Diametrum Apsidum ducti. dico eas in eandem rationem dividi ad Elleipsi $\alpha\zeta\theta$. Nam per posteriorem partem propositionis [].^{ae} libri 1 Conicarum Apollonij Pergaej. ut rectangulum $\alpha\beta\delta$. ad rectangulum $\alpha\gamma\delta$. sic quadratum $\epsilon\beta$ ad quadratum $\eta\gamma$. Et sic quadratum $\zeta\beta$ ad quadratum $\theta\gamma$. quare ut quadratum $\epsilon\beta$ ad

quadratum $\eta\gamma$. sic quadratum $\zeta\beta$ ad quadratum $\theta\gamma$. et permutando ut
 quadr: $\epsilon\beta$ ad quadr: $\zeta\beta$. sic quadr: $\eta\gamma$ ad quadr: $\theta\gamma$. quare per 22
 pr. lib. 6 Elem. ut $\epsilon\beta$ ad $\zeta\beta$. sic $\eta\gamma$. ad $\theta\gamma$.

Corollarium. Cum ut $\epsilon\beta$ ad $\zeta\beta$. sic $\eta\gamma$ ad $\theta\gamma$ et permutando ut $\epsilon\beta$ ad
 $\eta\gamma$. sic $\zeta\beta$ ad $\theta\gamma$ erit per 19 pr. lib. 5 Elem. sic $\epsilon\zeta$ ad $\eta\theta$. hic est
 sic differentia $\epsilon\beta$, $\zeta\beta$. ad differentiam $\eta\gamma$. $\theta\gamma$: et per 22 pr. 6
 Elem. ut quadr $\epsilon\beta$ ad quad $\eta\gamma$. sic differentia quadratarum $\epsilon\beta$. $\zeta\beta$.
 ad differentiam quadratarum $\eta\gamma$, $\theta\gamma$.

locus planetae a) in Elleipsi refertur ad Eccentricum sinu recto f. 109^r
 per planetam ad Diametrum^{b.)} Apsidum ducto.

Arcus inter Apsidem summam et locum Planetæ Eccentricum in
 consequentia dicatur Anomalia Eccentrici. Tempus quo planeta
 conficit Anomaliæ Eccentrici dicatur Anomalia Temporis

a.) in Orbitâ suâ Elleipticâ reucitur f. 108^v

b.) lineam apsidum dimisso.

haec Anomalia reducatur more Vulgari ad gradus,^{c.} Etc qualis f. 109^r
 totum tempus periodicum habet 360.

Data anomalia Eccentri invenire distantiae d.) In 1 quadrante.
 sumatur Anomaliae compl ad 90. In 2. quadrante excessus supr 270.
 ut radius ad Eccentricitatem, sic sinus arcus assumpti ad
 differentiam (distantiae mediae et quaesitae; quae in 1 et 4
 quadrantibus addenda, in 2 et 3 quadr: auferenda mediae distantiae
 et dabitur quaesita. Tempora quibus planeta duos quodlibet aequales
 Eccentici gradus conficit sunt in ratione distantiarum./ majori
 distantia majus consumitur tempus, minori minus.* distantiae verò a
 punctis ad medios gradus relatis sumantur.

c.) Scrupula primas secunda, etc. quales gradus

f. 108^v

d.) In semicirculo superiori sumatur Anomaliae defectus infra 90. vel excessus supra 270: in semicirculo inferiori excessus supra 90. et defectus infra 270.

e.) Sumantur illae, quae gradus bifariam secant.

Eodem modo. differentiae temporum et gradus unus sunt in ratione differentiarum distantiae mediae et daturum. diff unius gradus et temporis quaesiti. dicatur aequatio. et distantia media (radius) ad τ . vel 3600". sic differentia inter distantiam Mediam et datam ad aequationem quae in distantia majori addatur in minori auferatur τ gradui ut habeatur tempus. Aequationem [] in τ quadrant summam invenire.

f. 109^r

1 ut distantia media (radius) ad λ 3600". sic eccentricitas sive differentia inter distantiam $\int$ mediam et maximam. ad aequationem $\dot{\lambda} \tau$ gr per quem medium absis summa transit [?]

2. ut sinus 30' duplicatus at aequationem. sic radius ad aequationem summus.

$\lambda \tau$ gradum temporis, vel

f. 108^v

$\int$ datam et mediam

$\dot{\lambda}$ quae in distantia majori addatur, in minori auferatur uni gradui temporis.

[A sentence and a paragraph are crossed out, followed by:]

f. 109^v

$\dot{j}$ (pag. praeced.) Tempora quibus Planeta duos aequales Eccentrici

gradus conficit, sunt in ratione distantiarum.

Si Planeta fuerit in distantia mediâ, unus gradus Eccentrici in uno gradu temporis conficitur. Si distantiae fuerit major mediâ, uni gradui Eccentrici plus uno gradu temporis respondet: si distantia minor mediâ, minus uno gradu temporis consumitur.

APPENDIX K

James Ussher's account of John Bainbridge's theory of planetary motions (Trinity College Dublin MS. 794, ff. 47^r-47^v), extracted from his short tract, 'Planetæ' (ff. 46^v-47^v).

Keplerus putat Planetas nullum habere motum proprium circa Solem, sed omnem eorum motum fieri à Sole; tardiozem circa aphelion sive Apsidem Summam, velociorem circa perihelion sive Apsidem imam: hoc est, Planetam aequalia spatia inaequalibus temporibus conficere, pro variâ Planetæ à Sole distantia. Mercurius, exempli gratia, conficit suam Periodum circa Solem (Solis potentia) in dieb. 87. hor. 23. min. 15'. 36". ad fixas factâ relatione. Circa medias distantias, unum gradum spatij conficit, uno gradu temporis: circa aphelion, unum gradum spatij, priori aequalem, conficit gradu 1. 12'. 35". temporis; circa perihelion, unum aequalem gradum spatij conficit 0. 47'. 25". temporis.

Sed puto Planetas motum habere proprium circa Solem; quem Sol incitat et accelerat magis minusve, pro variâ distantia Planetæ à Sole. Ita Mercurius motu proprio circa Solem conficeret suam Periodum diebus 175. hor. 22. minut. 31'. 12". sed motu ejus à Sole accelerato, una Periodus fit temporis illius dimidio; dieb. 87. hor. 23. min. 15'. 36".

Planeta motu proprio conficeret aequales spatij gradus aequalibus temporibus: sed accidente motu à Sole, aequales spatij gradus conficit temporibus inaequalibus. Sic Mercurius, circa medias distantias, conficeret unum gradum spatij duobus gradibus temporis; sed promotio à Sole subtrahit unum gradum temporis. Circa aphelion, unum gradum spatij conficeret etiam duobus gradibus temporis: sed promotio à Sole subtrahit 0. 47'. 25". temporis, et relinquitur 1. 12'. 35". ut Keplero. circa perihelion Mercurius conficeret unum gradum spatij duobus etiam gradibus temporis: sed promotio à Sole subtrahit 1. 12'. 35". et relinquuntur 0. 47'. 25". ut Keplero.

f. 47^rf. 47^v

Idem Keplerus ut magnum et pluriosum suum εὔρημα nobis commendat Hypothesin constantis λοξώσεως Eccentrici Planetarum ad Eclipticam et Eccentrici Martis ad Eclipticam perpetuam et constantem inclinationem post multos exantlatos labores, ex asiduis Tychonis Brahei observationibus esse grad. $\overline{1}$. minut. 50'. demonstrâsse se asserit. quorum tamen utrumq₃ in Ptolemaei libro Hypotheseôn (quem ille nunquam vidit) invenitur quod sane merum est. (Bainbr.)

APPENDIX L

A transcription of Bodl. MS. Bodl. 313, ff. 83^r-84^r, Thomas Lydiat to Sir Adam Newton, 26 July/5 August 1611.

Honorato viro M^{ro} Neutono, Serenissimo Principi Henrico a f. 83^r
secretis, S. Benigna comitas tua Honorate vir in acceptis prioribus
literis meis, et promptus fauor ad praestandum quod ijs praecipue
petiueram, animum mihi addit ad interpaellandum te his alteris
tantulo interiecto temporis interuallo: quas ne illis posthabeas
oro. Altero die postquam Londinum redij Richmondo, laetus benigno
aspectu micantis inter omnes illius nostri Britannici orientis
sideris velut lunae inter ignes minores: conueniens doctissimum
Mathematicum integerrimumq₃ virum M^{ru} Briggium, amicum meum,
incidi in sermones de Joannis Kepleri Mathematici Imperatoris
Rudolphi scriptis in lucem editis intra hoc biennium secessus mei
in Hiberniam, sciscitabundus ecques suppetias emisisset ad
confirmandam opinionem Jesuiticam Deckerij et Suslygae et suam de
Natali Christi, quam libro meo de Emendatione Temporum redargueram.
Quum autem diceret se habere praecipuum et cebeberrimum operum eius
haud ita pridem editum, nempe Commentaria de motibus stellae
Martis, rogavi pro amicitia vt tantisper mihi percurrenda
accommodaret: quod ille, qua est facilitate erga profitentes studia
bonarum artium, haud grauata fecit. Ecce autem tibi volumen amplum,
adeoq amplius integro Ptolemaei opere Astronomico, quod Graeci
μεγάλην Σύνταξιν, ac Barbarograeci Almagestum appellant; praefixo
titulo, Astronomia Noua Αἰτιολόγητος, seu Physica Coelestis,
tradita commentarijs de motibus stellae Martis,---Jussu et
sumptibus Rudolphi II Romanorum Imperatoris &c. Plurium annorum
pertinaci studio elaborata Praegae, a Caesareae maiestatis
Mathematico Joanne Keplero. In Epistolâ vero operis nuncupatoriâ
ipsi Imperatori profitetur se exhibere idq₃ felici omine, Captium
nobilissimum, scilicet Martem suo marte captum. Quod si scire
cupias quibus vinculis: nimirum haud illis quibus Poetae decantant

irretitum olim a Dulcano, sed vinculis Calculi: calculi nimirum constructi hypothesebus Nicolae Copernici, statuentis Solem loco immobilem quasi in centro (id si imo) vniuersi; Terram autem longe supra illum pariter ac Veneris et Mercurij astra; nempe coenum in coelo, circa ipsum annuo motu eoq₃ duplici vtpote cum axium inclinatione mouendam, insuperq₃ circa suum centrum quotidianâ vertigine voluntandam: ac vicisim stellas inerrantes vna cum firmamento coeli immensâ altitudine prorsus immobiles. Neq₃ verò hoc contentus est ponere Keplerus solummodò gratiâ facilioris et expeditioris demonstrationis reuolutionuum orbium coelestium, quemadmodum quidam Copernici hypotheses excusare conati sunt, quasi nihil necesse esset existimare rerum naturam vere ac re ipsâ ita se habere: sed palam contendit eam esse plane naturalem conditionem ac ipsissimam constitutionem siue systema vniuersi. Quod cum videatur manifeste pugnare cum sacris Scripturis: in Introductione multis nititur probare Scripturas sacras subinde loqui secundum oculorum indicium potius quam rei veritatem. Sit (inquit) Christus Petro (videlicet Luc. Sc.) Duc in altum. quasi mare sit altius littoribus, sit enim apparet oculis. Siccinè vero, inquam ego? imo non magis quam Sol oculis apparet mouere. Atqui superficierum quidem comparatione altius significet excelsius vel sublimius: caeteroquin altum aequè significat profundum ac sublime: vocabulum a Lucâ adhibitum est βάθος quod tantummodo profunditatem, sicut ὕψος sublimitatem denotat. quemadmodum etiam Psaltes loquitur, Psalmo 107: Qui descendunt ad mare nauibus. Deinde autem idem Keplerus, Psalmo 19, vt genuinum grammaticum sensum effugiat, nomine Poëticae allusionis conatur obdurere meram allegoriam, qualem haud scio an quisquam Origeni starum vrgeret, quasi sub imagine Solis cursus Euangelij, adeoq₃ Christi in hunc mundum.

peregrinatio decantaretur, ita vt ipsius Solis interim nullus decantaretur cursus vel peregrinatio in coelis eo modo enarrantibus gloriam Dei. Vide tamen quomodo intereā imprudenti verum inter falsa exciderit. Non exire (inquit) Solem ex horizonte tanquam è tabernaculo (etsi sic oculis apparet) sciebat Psaltes: moneri verò Solem existimabat, propterea quia oculis ita apparet. Itane? an Psaltes, nempe Θεόπνευστος, nec ille idiota, sed ipse regius propheta Daud, vt liquet ex Psalmi inscriptione, moueri Solem (scilicet ab vnâ extremitate coelorum ad alteram) existimabat: Keplerus verò non existimat? quam autem rationem addit cum ait, propterea quia oculis ita apparet, idem paulò postea ipse ferè totidem verbis negat inquit, scilicet cum neq₃ Solis motus comprehendatur visu oculorum, sed ratiocinatione solúm &c. Frustra etiam effugium quaerit in historiâ Josuae, quasi id tantum obtinuisset Josua, vt Sol die integro ratineretur Sibi, in coeli medio respectu sensus oculorum suorum: ideoq₃ eam dictionem Sibi, quae nusquam occurrit in contextu historiae, capitalibus literis exprimendam curauit: quum simpliciter et absolute dicatur ibi Sol non solúm metaphoricâ emphasi ($\square \mid \top$) silentio quieuisse et inhibuisse cursum tanquam vocem, sed etiam propriâ locutione stetisse immotus (scilicet praeter solitum) in medio coelorum, et non festinasse (vtique quemadmodum solebat) eundo versus occasum, quasi per diem integrum. Historiam verò illam aegrotationis Ezechiae, quâ Isaias disertè narrat Solem redijsse decem gradibus, quibus gradibus (ostensis vmbra solarij Ahazi) descenderat, silentio praetermittit. Atq₃ tanquam foueam transibit ipsam historiam creationis, quâ Moses clarè affirmat, Deum die quarto simul creasse ac collocasse (ambe Luminaria) Solem pariter ac Lunam, inter et cum caeteris stellis tanquam corporibus cognati

f. 83^v

generis, in siue ad firmamentum coelorum, vt collucerent super terram tanquam corpus diuersi generis, quam die tertio detexerat a quis marinis, in concaua illius loca tanquam alueum confluentibus, subter idem firmamentum coelorum: vnde necessariò consequens est maris et terrae globum (primo die creatum) inferiorem esse vniuerso luminarium et astrorum genere, ac longiùs abesse a Firmamento coelorum vndique, quàm Solem aut Lunam aut caeterorum astrorum vllum: ideoq; eundem globum, neutiquam verò Solem, possidere imum id est medium vniuersi. Praeterea quod ait sapientissimus Ecclesiastes, generationem vnam abire, aliam aduenire, terram autem in aeternum stare: incassum molitur interpretandum tantummodò de essentiâ, non etiam de loco nam eiusmodi intelligendus est eius status, qualis opponendus sit abitioni et adventui, itemq; aliorum quorum deinceps fit mentio nempe ventorum ac fluminum motui: ac hic localis: ergo terrae status siue mansio vicisim etiam localis. qualis autem ventorum ac fluminum, talis quoque Solis ibidem memoratus motus manifestè significatur, nempe à loco in locum; quemadmodum etiam ex verbis ibidem adhibitis liquet: nempe Et oritur Sol, et vadit ad occasum Sol, et ad locum suum (𐤓𐤍𐤁𐤏) currendo anhelat (vehementissimae emphaseos metaphorâ) vbi oriebatur. Denique nequicquam texit Keplerus longam analysin Psalmi 104, vt declinet id quod ibi dicitur de terrâ neutiquam inclinâtâ vel inclinandâ in seculum. Dicitur enim ibi Deus fundasse terram super stabilitates siue stabilimenta eius, (𐤕𐤓𐤕𐤓 - 𐤅𐤁) ne inclinetur vel locomouetur in seculum seculi. siquidem vox Hebraïca 𐤕𐤓𐤕𐤓 adeo propriè significat locomoueri, vt ipsam Latinam dictionem (Motum) appareat inde oriundam est. In huiusmodi verò difficultates Copernicus (qui volumen suum de Reuolutionibus Orbium coelestium, Cardinalis Capuau in primis, cuius etiam Epistola

operi praefixa est, hortatu in lucem edidit, et dedicauit Paulo
 tertio eius nominis Pontifici Romano) atq₃ Keplerus se coniecerunt,
 gratiâ vnius aut alterius postulati Astronomici, quorum scilicet
 auctores fuerunt Pythagoraei et Peripatetici Ethnici Philosophi,
 cupientes coeli et astrorum aeternitatem astruere: nimirum ipsorum
 motus esse aequales, et circulares. Sed et horum neutrum sartum
 tectum tueri videntur. Siquidem cogitur Keplerus concedere Terram
 loco astri Planetae positam, moueri inaequali ratione, tardius in
 aphelio siue maiore distantîâ a Sole, velocius in perhelio siue
 minore distantîâ; idq₃ non ad visum tantum, sed etiam in reipsâ.
 Itemq₃ Planetae iter in coelo neutiquam esse Circuli pistrinum
 (scilicet) sed perfectè ellipticum declinans ad Ouale. Addit verò
 etiam tanquam coronidis loco, Soli quoque vt manens suo loco
 rotetur veluti in torno, vitali facultate opus videri. Plura autem
 habet in eâdem Introductione parum probabilia: sed haec Epistolae
 modo sufficient. At ego quum in praelectione Astronomicâ (primo
 ingeniosi mei partu) septimo abhinc anno in lucem editâ, idem ferè
 argumentum tractandum mihi proposuerim, quod Keplerus pridem in
 Mystero Cosmo[|]graphico, et modò in Introductione Astronomiae nouae f. 84^r
 seu Physicae Coelestis, nempe adumbrandam Constitutionem siue
 Systema Vniuersi: fundamentum operis mei posui naturalem historiam
 Sacrarum literarum iuxta genuinam antiquitûs receptam earum
 sententiam: atq₃ illi consentaneas rationes situs et motus corporum
 coelestium, ex antiquissimorum Philosophorum placitis apud
 Plutarchum Graecorum doctissimum, et Pliniam, quem Keplerus Mysten
 illum Naturae, Latinorum celeberrimum appellat, atq₃ alijs, quantum
 potui collegi: atq₃ concessas Soli pariter ac caeteris astrorum,
 vitales siue animales facultates, tanquam causas motus ipsorum,
 quippe corporum neutiquam organicorum, posui inter Ethnicorum

Philosophorum errores, idq₃ iudicio Aristotelis, vnus maximi nominis ex ipsis. Verùm illi meo opusculo ad perfectionem multum deesse, in epilogo ipsius iam tam agnoui. Affici autem animum meum desiderio eius perficiendi inter caetera quae hactenus parturiui, ne dicam prae caeteris, idq₃ in re primogeniti suam postremò natum multis nominibus omnium charissimum habeo): optimam autem et expeditissimam rationem perficiendi ipsius pariter ac aliorum, esse peregrinationis historiam, quâ habeantur observationes sub rectâ Sphaera siue Aequinoctiali circulo, et vltra Tropicum Capricorni, quae cum nostratibus citra Cancri tropicum factis conferantur, cùm aliàs tum in scriptis meis editis iamdudum pluribus locis significauì. Oportunè autem iam institutae sunt nauigationes nostratium Londinensium circa Caput Bonae spei secus orientalem et aequinoctialem oram Africae, vt spero continuanda, ac indies magis magisq₃ frequentandae, Christianae reipublicae bono, et honori Serenissimi Principis Henrici, et patriae nostrae emolumento. Libenter autem ipse, postquam ea praestiti quae prioribus literis illustrissimo Equiti, D. Thomae Chalonero et tibi promisi, si commoda detur occasio et conditio, ad easdem partes peregrinationem aliquot annorum susciperem, tùm Astronomiae adeoq₃ totius Cosmographiae, tum Historiae, praesertim Ecclesiasticae Atassinorum eidem orae Africae finitimorum, tum denique Orientalium linguarum comparandarum cum Hebraeâ (cuius etiam rationem ac methodum, vt quidem plerisque doctorum animorum meorum videtur, planè novam, vt verò ego me aliquando probaturum spero, non antiquam atq₃ planè genuinam iam diu animo concepi) penitioris cognitionis ergo: quod etiam ipsum quarto capite Defensionis meae, quarto abhinc anno editae, 37 pag. significauì. Haec occasione nuperrimè visae atq₃ intra proximum quatrìdum leuiter transcurssae Nouae

Astronomiae Caesarei Mathematici tibi significanda duxi; non solùm aequum sed etiam officium meum censeus, quatenus te mediante Serenissimi Principis gratiàm renouatam, et vti spero saepiùs renouandam atq; continuandam, cum eiusdem beneficiat̃ consecutus sum, praecipua studiorum ac votorum animi mei nota tibi facere. Quae te aequo et beneuolo animo accipere, ac tempestiuè apud Celsitudinem eius promouere etiam atq; etiam, debitâ obseruantiat̃ oro. Vicissim verò Deum optimum maximum obtestor vt te in Nestoreos annos conseruet fidelem ac felicem Consiliarium Nobilissimo Principi Henrico, ad summam Diuini numinis gloriam, Celsitudinis ipsius honorem, Patriaeq; nostrae ac vniuersae Ecclesiae Christi bonum. Londini, 26d. Jul. 1611.

Dignitatis tuae studiosus Thomas Lydyat; (ac liceat mihi quaeso oblatâ occasione etiam renouare illud votum meum tibi,) vtinam appellari dignus, Serenissimi Principis Henrici

Chronographus et Cosmographus.

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