JOURNAL FOR THE HISTORY OF
ASTRONOMY

VOLUME 30

EDITED BY M. A. HOSKIN

SCIENCE HISTORY PUBLICATIONS LTD
1999

Editor: M. A. Hoskin, Churchill College, Cambridge, England, CB3 0DS (phone: 01223-840284; fax: 01223-565532; e-mail: mah15@cus.cam.ac.uk)

Associate Editor and Reviews Editor: Owen Gingerich, Harvard-Smithsonian Center for Astrophysics, Cambridge, MA 02138, USA

Associate Editor: J. A. Bennett (Oxford)

Associate Editor of Archaeoastronomy *Supplement:* C. L. N. Ruggles, School of Archaeological Studies, University of Leicester, Leicester, England, LEl 7RH

Advisory Editors: David H. DeVorkin (National Air and Space Museum, Washington), Stephen J. Dick (U.S. Naval Observatory), Jerzy Dobrzycki (Polish Academy of Sciences), Douglas C. Heggie (Edinburgh), David A. King (Frankfurt), John Lankford (Kansas State), G. E. R. Lloyd (Cambridge), Raymond Mercier, J. D. North (Groningen), David Pingree (Brown), C. L. N. Ruggles (Leicester), F. Richard Stephenson (Durham), Noel M. Swerdlow (Chicago), Albert Van Helden (Rice)

Publisher: Science History Publications Ltd, 16 Rutherford Road, Cambridge, England, CB2 2HH (phone: 01223-565532; fax: 01223-565532; e-mail: shpltd@aol.com; web site: http://members.aol.com/shpltd)

Copyright: ©1999 by Science History Publications Ltd

COPYING: This journal is registered with the Copyright Clearance Center, 21 Congress Street, Salem, Mass., 01970, USA. Permission to photocopy for internal or personal use or the internal or personal use of specific clients is granted by Science History Publications Ltd for libraries and users registered with C.C.C. subject to payment to C.C.C. of the per-copy fee indicated in the code on the first page of the article. This consent does not extend to multiple copying for promotional or commercial purposes.

SUBSCRIPTIONS: *Journal for the History of Astronomy* will be published four times in 1999, in February, May, August and November, whilst its Supplement *Archaeoastronomy* will be published once, in August. A subscription to Volume 30, 1999 (including *Archaeoastronomy* Supplement no. 24) is £79.00 (US $158.00 in the Americas and Japan) for institutions, £36.00 (US $72.00 in the Americas and Japan) for individuals subscribing direct, including postage. Single issues and *Archaeoastronomy* each cost £19.00 (US $38.00) for institutions, £11.00 (US $22.00) for individuals, including postage. A complete set of vols 1–30 plus *Archaeoastronomy* 1–24 costs £825.00 (US $1650.00), or £700 (US $1400) without *Archaeoastronomy.*

ISSN: 0021-8286

Printers: University Printing Services, University Press, Cambridge

Journal for the History of Astronomy

Vol. 30 Part 1 February 1999 No. 98

CONTENTS

INSTRUCTIONS TO CONTRIBUTORS

The *Journal for the History of Astronomy* is devoted to the history of astronomy, astrophysics and cosmology, from the earliest civilizations to the present day. Its subject matter extends to such allied fields as the history of navigation, timekeeping and geography, and the use of historical records in the service of astronomy. In 1999 there will be five issues of 96 pages each, one of the issues being the *Archaeoastronomy* supplement.

Papers offered for publication should in the first instance be submitted in typed form, using *JHA* style. Further instructions concerning the presentation and documentation of papers may be obtained from the Editor. Original artwork should not be sent until the paper has been accepted for publication.

If the paper is accepted, the author will then be asked to provide it on PC disk in any of the standard Windows file formats or Wordperfect 5.1 for Dos. Macintosh disks are usually acceptable. Files can also by sent as e-mail attachments to shpltd@aol.com.

All communications on editorial matters should be sent to the Editor, and books for review to Professor Owen Gingerich, Harvard-Smithsonian Center for Astrophysics, Cambridge, MA 02138, USA.

JHA, xxx (1999)

REGIOMONTANUS'S CONCENTRIC-SPHERE MODELS FOR THE SUN AND MOON

N. M. SWERDLOW, The University of Chicago

It seems curious that the astronomer and mathematician who did the most to establish a serious understanding of Ptolemy's mathematical astronomy in Europe also had a serious interest in concentric-sphere models for the motions of the heavens. But such is undoubtedly true. The first known indication of Regiomontanus's interest appeared in a letter to Giovanni Bianchini published from the manuscript Nuremberg Cent V app. 56c by C. T. von Murr in 1786. The letter is undated, but must fall between letters from Bianchini to Regiomontanus dated 21 November 1463 and 5 February 1464 since in it Regiomontanus responds to problems posed by Bianchini in the letter of 21 November and in the letter of 5 February Bianchini responds to problems posed by Regiomontanus in the undated letter. The principal subject of the letter is a description of a set of tables, on which Regiomontanus seems to have been working but had not yet completed, applicable to spherical astronomy. These are in fact the *Tabulae primi mobilis*, eventually dedicated to Matthias Corvinus while Regiomontanus was in Hungary in 1467–71 and published in Vienna in 1514 in an edition by Georg Tannstetter. They are double-entry tables for solutions to right spherical triangles in the form $a = \sin^{-1}(\sin \alpha \sin c)$, and may be applied to all solutions based upon the law of sines by substituting the complements of a, α or c. α and c are tabulated at 1° intervals from 1° to 90°, a and differences of a for $\alpha + 1°$ and $c + 1°$ are given to seconds, which makes interpolation quite secure. The tables run to 90 pages of 90 entries per page or, not counting duplications, no fewer than 4095 solutions. Considering all the computation Regiomontanus did in these tables, his two great sine tables for $R = 6{,}000{,}000$ and $R = 10{,}000{,}000$, the *Tabulae directionum*, and the *Ephemerides* for 1475–1506, it is my suspicion that he was a lightening calculator, and a very accurate one too.[1]

After explaining the arrangement of the tables and giving the solutions to problems proposed by Bianchini in the preceding letter, Regiomontanus gives a list of 40 problems in spherical astronomy that may be solved using the tables. When the *Tabulae primi mobilis* were completed, the list had grown to 63 problems with detailed solutions and examples running to 38 double-column quarto pages in the printed edition.[2] But the last two problems in the original list had been dropped, and this is of some interest. The last four problems and Regiomontanus's concluding remarks to Bianchini are as follows:

> 37. To compute the equation of the eighth sphere according to the methods of Alfonso.

0021-8286/99/3001-0001/$2.50 © 1999 Science History Publications Ltd

> 38. To compute the equation of the eighth sphere according to the theory of Thābit.
> 39. To compute the equation of the Sun.
> 40. To compute the equation of the anomaly of the Moon.
>
> But where does this vexatious and audacious pen hasten? Will it perhaps direct all things sought of astronomers to this singular table? I say that a great part of such things as are sought will be found by means of this table if we shall first have established a complete concentric astronomy. Why that? It will be beautiful to save the irregularities of the motions of the planets through concentrics. We have already provided the method for the Sun and Moon, while concerning the rest some foundations have been laid through which, when completed, one will be permitted to compute the equations of all the planets by means of this table. But concerning this matter, no more for the present lest, in reading my letter, you experience a weariness greater than the delight I take in writing. If I seem to have said any of these things more boldly than justly, you will receive my reasons more clearly, I believe, in a following letter. I would, nevertheless, prefer to treat of these things by speech rather than by the pen, for it would be much more easy and convenient. As long as that is not possible, a letter will take on the function of speech which, whatever it relates, will be subject to correction by your judgement.[3]

Now it is entirely sensible to provide solutions to the equation of the eight sphere, the inequality of the motion of the fixed stars, in the Alfonsine Tables and in *De motu octauae sphaerae* attributed to Thābit because both are solutions of spherical triangles, but it seems odd to treat the equations of the Sun and Moon as spherical because the normal epicyclic and eccentric models are in the plane and use only plane triangles. The reason becomes clear when Regiomontanus says that his tables will be applicable to all things sought by astronomers once he has established a "complete concentric astronomy", which he thinks will be "beautiful" — *pulchrum*, perhaps just "excellent" — and is obviously his intention. He has, he says, worked out a method for the Sun and Moon, and at least laid some foundations by which, when completed, the equations of all the planets will be computed with his tables. Ultimately, he gave up on this, for solutions to the solar and lunar equations do not appear in the *Tabulae primi mobilis*, which concludes with the two forms of the equation of the eighth sphere just mentioned, and nothing more was heard of concentric spherical models for the planets. Nevertheless, just as he said, he had worked out concentric spherical models for the Sun and Moon, but that was a discovery that came many years after von Murr published the letter to Bianchini.

In 1953 Ernst Zinner published a brief description of a letter written in 1460 by Regiomontanus, then in Grosswardein (Oradea), to Bishop Johann Vitez, in which he describes just such models for the Sun and Moon.[4] The letter is contained in a manuscript we shall call F, Florence, Magliabechiana XI, 144, ff. 16r–17v, which also contains, among other works, the *Epitome almagesti Ptolemaei*, the *Disputationes* *contra Cremonensia in planetarum theoricas deliramenta*, and Peurbach's *Theoricae nouae planetarum*. What makes F of particular interest, aside from being the unique copy of this letter, is that it was copied in 1476 in Buda and presumably belonged to Martin Ilkusch (Ilkusz), from about 1466 astronomer to King Matthias Corvinus. Regiomontanus knew Ilkusch in Italy, for he is "Cracoviensis" who learns of the faults of the old *Theorica planetarum* from "Viennensis" in the *Disputationes*, which is set in Rome during the papal election between the death of Pius II on 8 August 1464 and the election of Paul II on 31 August. In F, and to my knowledge in F alone, the two characters of the dialogue are called "Martinus" and "Joannes", showing that Ilkusch knew that he was "Cracoviensis", meaning from the University of Cracow, and Regiomontanus "Viennensis", meaning from the University of Vienna. It is nice to see people identifying themselves first and foremost with their universities in the fifteenth century.[5] Regiomontanus and Ilkush also knew each other during 1467–71 when Regiomontanus was at Matthias's and Vitez's newly founded, and short lived, University of Pressburg (Bratislava), so any manuscript associated with Ilkusch has a particular authenticity, and indeed F is quite an excellent manuscript, with notably detailed figures for Peurbach's *Theoricae nouae* and a good text of the *Epitome*, although prior to the final version in Venice lat. 328.

I first came across F about twenty years ago when collecting films of manuscripts of the *Epitome*, and at the time made a transcription and translation of the text (after a fashion) along with some other works of Regiomontanus and Peurbach. But I did nothing with it because I was, to say the least, puzzled by whatever relation it might have to Regiomontanus's notes on al-Biṭrūjī, written in his own hand at ff. 45r–47v of Vin 5203, a large manuscript dating from his years in Vienna, containing works of other authors, in particular Peurbach, some in Regiomontanus's hand, some not, along with various notes of his own. And all I could learn from F. J. Carmody's edition and study of the notes was that Regiomontanus, far from being interested in concentric-sphere models, was highly critical of the entire principle.[6] These difficulties were removed only recently when Michael Shank published a new edition and the first translation of the text in which he argued, convincingly I believe, that Regiomontanus's notes are not his own, but a copy of a criticism of al-Biṭrūjī written in Vienna or Klosterneuberg, at some time after a solar eclipse of 1433 that the author says was annular and seems to have observed himself. And this criticism was in turn based upon an earlier criticism in a treatise on an instrument called a *planitorbium*, a kind of equatory, written in 1310 by one G. Marcho, a Franciscan from Aquitaine, who was then (or had been) a student in Paris and cites an annular eclipse of 1309.[7] It follows that the notes are something Regiomontanus copied and do not necessarily represent, in fact do not represent, his own thoughts on either al-Biṭrūjī or concentric-sphere models in general, and this makes it possible to consider the models described in the letter to Vitez independently of the notes on al-Biṭrūjī.

In fact, far from entirely rejecting al-Biṭrūjī, I believe that Biṭrūjī was the source of Regiomontanus's models, but in a peculiar and critical way. Even if he was not the author of the notes, Regiomontanus had every opportunity to study Biṭrūjī's work as he owned a manuscript containing the Latin version by Michael Scot (Nuremberg, Cent V 53, ff. 74r–111v, not in Regiomontanus's hand), which he annotated, at times critically.[8] In Biṭrūjī's model for the planets, an epicycle is placed on the surface of a sphere removed from the pole of the celestial equator by the obliquity of the ecliptic, and the planet is placed a quadrant from the epicycle, thus near the ecliptic. Biṭrūjī presumes that as a point moves about the epicycle in the anomalistic period, the planet, always a quadrant from the epicycle, will take on an inequality in its motion in longitude and also a motion in latitude, both with the anomalistic period. The model accounts for only the second or solar inequality of the planets, and has nothing corresponding to the first or zodiacal inequality, produced in Ptolemy's model by equant motion in an eccentric. Biṭrūjī's model, and its considerable defects, are described very well by Bernard Goldstein in his edition and translation of the Arabic text.[9] But Goldstein does something more, and this turns out to be of great interest. After showing the problems of Biṭrūjī's model, he describes a modification of the model that can work properly, at least for the second inequality in longitude, by letting the epicycle move at an arbitrary distance about the pole of the ecliptic rather than the equator, with the planet, still a quadrant from the epicycle, *oscillating* in the plane of the ecliptic about its mean position. The principle of this model turns out to be *identical*, except for the direction of motion on the epicycle, to Regiomontanus's models for the Sun and Moon. Now, we may safely exclude the possibility that Regiomontanus read Goldstein's account of Biṭrūjī's planetary model, but he was certainly clever enough to come up with the very same modification, except that he first applied it to the Sun and Moon by changing the direction of motion on the epicycle. His model is definitely not based upon Biṭrūjī's solar model, in which the Sun, in addition to moving with its own mean motion, is a quadrant from a point that moves at twice that speed on a circle eccentric to the pole of the equator. But it could in principle be based upon modifying Biṭrūjī's lunar model, the last to be described in Biṭrūjī's treatise, which is essentially the same as the planetary model except for the direction of motion on the epicycle, in order to make the Moon oscillate in the plane of its inclined circle, and likewise applied to make the Sun oscillate in the plane of the ecliptic. In addition, Regiomontanus devises a way of accounting for part of the second inequality of the Moon, not even considered by Biṭrūjī, that is also based upon the principle of converting motion about an epicycle to oscillation along a great circle.

This is what I believe to be the origin of Regiomontanus's models, a correction of al-Biṭrūjī's planetary, or perhaps lunar, model, and I can see no relation to other concentric-spherical models for inequalities, as the motion of the eighth sphere attributed to Thābit. And it goes without saying that it has nothing whatever to do with Eudoxus's models, as described by Aristotle or Simplicius, or an attempt to discover Eudoxus's models. Why Regiomontanus was interested in concentric-sphere models, I leave to heads wiser than my own. That he was serious about them is shown by his intention to write a work, in four treatises no less, to refute “the old theory of eccentrics and epicycles” and to propose and confirm by geometrical proofs a theory of concentric spheres accounting for all inequalities. One thing certain is that his motivation was physical and was not concerned with finding more accurate models for the Sun, Moon, and planets. If anything, his goal was to devise concentric-sphere models identical in effect to Ptolemy's, which he surely understood adequately to his task in 1460, and he evidently persevered in his search even after he became a complete master of Ptolemy's astronomy in writing the *Epitome*, which predates the letter to Bianchini. Further, he must have known from the beginning that the variation in the apparent size or brightness of the Moon and planets indicates a variation of distance incompatible with concentric spheres, as is patently obvious and also pointed out in detail in the notes he copied on al-Biṭrūjī, which devote particular attention to the variation of the lunar parallax and the apparent size of the Moon and shadow in solar and lunar eclipses, citing the annular eclipse of 1433 as evidence.[10] How he thought he could get around these problems, I also leave to heads wiser than my own. In the criticism of the contemporary state of astronomy that Regiomontanus wrote to Bianchini following Bianchini's letter of 5 February 1464, he singles out as a defect the effect of the variation of distance implicit in Ptolemy's models on the apparent sizes of Mars, Venus, and the Moon, which are not observed to take place, but it is a far cry from this exaggerated variation to concentric spheres in which distances do not vary at all.[11] Perhaps despite his best intentions, he gave up on concentric-sphere models because he ultimately found them inapplicable, to the planets because he could devise no model combining the first inequality, second inequality, and motion in latitude, and to the Moon because he could devise nothing corresponding to the correction of the direction of the apogee of the epicycle as a function of the mean elongation, which is not present in his model. Perhaps he just decided that the whole principle of concentric spheres was without value after all. In any case, this must have occurred between the letter to Bianchini at about the end of 1463 and the dedication of the *Tabulae primi mobilis* to Matthias Corvinus, presumably before Regiomontanus left Hungary in 1471, since, as noted, spherical solutions for the equations of the Sun and Moon are not present in the instructions.

What follows is an edition of the text from F with a translation in which I have added a few phrases in brackets for clarity. The text of F itself is excellent and requires no emendation aside from punctuation and division into paragraphs, which I have done as seems appropriate. The figures from the manuscript are shown as Plates 1 and 2, and I have redrawn them in Figures 1 and 2. Note that in Plate 1 of the solar model the letters *L* and *M* are reversed and in Plate 2 of the lunar model the letters *G* and *H* are reversed. The analysis following the text and translation is purely technical, and again I leave historical speculation to wiser heads.

Text and Translation

[De sole]

Sol habet duos orbes mundo concentricos, quorum superior polos suos habet sub polis zodiaci octauae sphaerae, et eclipticam sub ecliptica eiusdem. In huius orbis concauo circulus est paruus habens polum extra eclipticam praedictam. Sed orbi inferiori corpus solare infixum est.

Mouetur autem orbis superior regulariter super polis suis omni die naturali per 59 minuta et 8 secunda fere. Orbis uero inferior hoc pacto in conuexo eius est punctus ex directo centri corporis solaris inuaribiliter permanens, alius quoque punctus a priori per quartam circuli magni distans. Horum unus mouetur regulariter in circumferentia parui circuli praedicti, in eo tempore complendo reuolutionem unam quo et orbis superior. Alius autem terminus istius quartae, qui apud solem est, semper adhaeret eclipticae in orbe superiori signati.

Quod ut facilius appareat, pingenda est theorica in qua quidem puncta litteris alphabeti nunc appellabo. Post uero cum maior fauebit commoditas, singulis punctis nomina sua fingam.

Opus quoque nouum quatuor absoluam tractatibus, in quorum primo antiquam

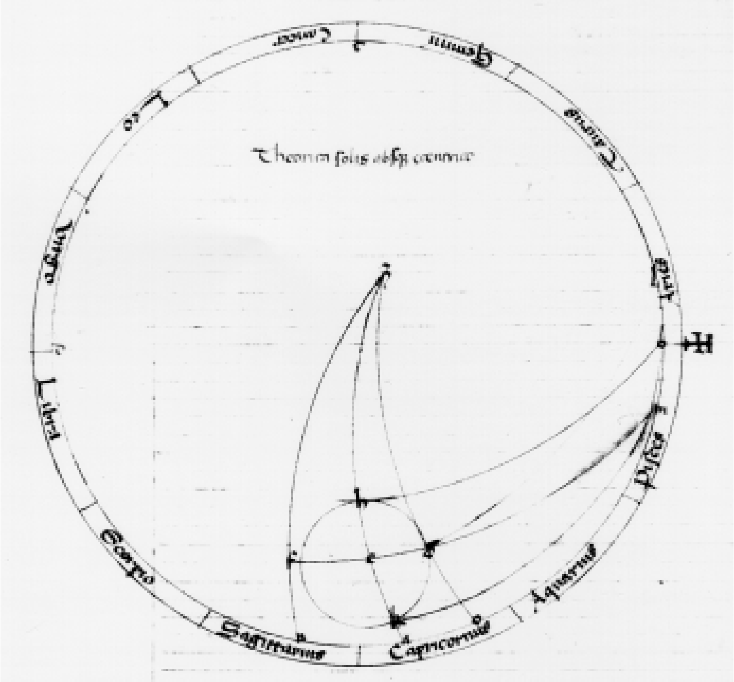


PLATE 1. Theorica solis absque eccentrico.

[On the Sun]

The Sun has two orbs concentric to the world, of which the higher has its poles under the poles of the zodiac of the eighth sphere, and [its] ecliptic under the ecliptic of the eighth sphere. In the concavity of this orb is a small circle with a pole outside the ecliptic just mentioned. But the solar body is fixed in the lower orb.

Now the higher orb moves uniformly about its poles by about 0;59,8° in each natural day. And the lower orb is so arranged that in the convexity of it there is a point invariably remaining directly at the centre of the solar body, and also another point distant from the first by a quadrant of a great circle. One of these points moves uniformly in the circumference of the small circle mentioned before, completing one revolution in the same time as the higher orb. However, the other end of the quadrant, which is at the Sun, always adheres to the ecliptic designated in the higher orb.

In order that this will more easily be clear, a representation should be drawn in which I shall now call the points by letters of the alphabet. And later when the opportunity is more favourable, I shall assign to the individual points their names.

I shall also complete a new work in four treatises. In the first of these I shall present the old theory of eccentrics and epicycles demolished by strong reasons and observations that will be made. In the second I shall clearly set out a theory of concentric orbs by which all inequalities of the motions can be saved. And in the third I shall confirm what is in the second by geometrical proofs. The fourth will contain the way by which

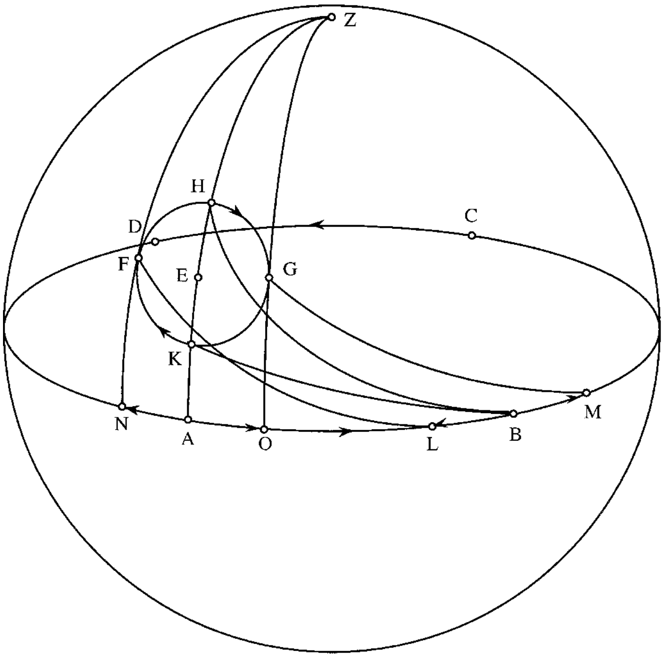


FIG. 1. Theory of the Sun without an eccentric.

speculationem de eccentricis et epicyclis rationibus firmis atque obseruationibus futuris destructam dabo. In secundo speculationem orbium concentricorum quibus omnes diuersitates motuum saluari poterint aperte ponam. In tertio uero testimoniis geometricis ea quae in secundo tractatu sunt confirmabo. Quartus quo pacto motus isti numerari et tabulae ad illas nouas radices fundari possint continebit.

Se quorsum euagabar? Iam rediens describo eclipticam superioris orbis, quae sit *ABCD*, cuius unus polus sit punctus *Z*, a quo descendat quarta circuli magni *ZA*. In hac quarta signetur punctus *E*, super quo describatur circulus paruus *KFHG*. Deinde in conuexo orbis inferioris estimetur quarta circuli magni, cuius unus terminus adhaereat circumferentiae dicti circuli parui, quem punctum nota *K* repraesentat. Reliquus uero terminus huius quartae, qui ex directo centri corporis solaris est, semper adhaereat eclipticae superioris.

Mouetur itaque omnis punctus eclipticae *ABCD* regulariter circa centrum mundi, et similiter mouetur paruus circulus *KFHG*. Quod si quarta *KB* in conuexo orbis inferioris semper, ut dictum est, in eisdem punctis adhaereret eclipticae, sol non haberet diuersitatem in motu suo, semper enim centrum solis ex directo puncti *B* regulariter moti reperiretur. Sed non est ita, immo terminus quartae qui adhaeret circumferentiae parui circuli mouetur in circumferentia parui circuli uersus punctum *F*, et trahit secum reliquum terminum quartae, qui apud solem est, adhaerente tamen eclipticae.

Intellige ergo arcum per polum eclipticae et terminum quartae usque ad eclipticam transeuntem similiter moueri ad motum termini. Tunc quanta est portio eclipticae quam claudunt duo arcus, quorum unus per polum eclipticae et polum parui circuli transit, et alius per polum eclipticae et dictum quartae terminum, tanta est etiam portio ea quae inter punctum *B* et terminum quartae adhaerentem eclipticae deprehenditur.

Cum itaque terminus quartae circuiens ad punctum *F* perueniet, punctum, inquam, in quo arcus *ZN* contingit paruum circulum, maxima est illa, de qua dixi, portio eclipticae, unde et reliquus quartae terminus adhaerens maxime a puncto *B* remouebitur. Postea uero portio haec pedetentim minuitur quousque nulla fiet, quando scilicet terminus quartae circuiens in puncto *H* parui circuli fuerit. Recedente autem termino quartae circuente a puncto *H*, augetur iterum portio eclipticae inter duos arcus comprehensa, donec terminus quartae circuiens ad aliud punctum contactus, quod est *G*, perueniet. Tunc iterum maxima est huiusmodi portio, quae tandem, propter motum termini circueuntis, continue decrescit donec reuolutio integra perficitur in paruo circulo.

Haec itaque est habitudo motuum solis. Nunc ad descriptiones terminorum uenio.

Linea medii motus solis est quae a centro mundi exiens per punctum *B* usque ad zodiacum protenditur.

Linea ueri motus a centro mundi per centrum corporis solaris usque ad zodiacum continuatur.

the motions can be computed and tables [with] new epoch positions established for them.

But where have I wandered? Now returning, I shall describe the ecliptic of the higher orb, which let be *ABCD*, one pole of which let be point *Z*, from which let descend a quadrant of a great circle *ZA*. In this quadrant let a point *E* be designated, about which let the small circle *KFHG* be described. Then in the convexity of the lower orb, let a quadrant of a great circle be estimated, one end of which let adhere to the circumference of the small circle just mentioned, which point let letter *K* represent. And let the other end of this quadrant, [point *B*], which is directly at the centre of the solar body, always adhere to the ecliptic of the higher orb.

Accordingly, every point of the ecliptic *ABCD* moves uniformly around the centre of the world, and likewise the small circle moves [uniformly around the centre of the world]. And if the quadrant *KB* in the convexity of the lower orb always, as was mentioned, adhered to the ecliptic in the same points, the Sun would have no inequality in its motion, for the centre of the Sun would always be found directly at the point *B* that is moved uniformly. But it is not so, rather, the end of the quadrant which adheres to the circumference of the small circle moves in the circumference of the small circle [from *K*] towards point F, and draws with it the other end of the quadrant, which is at the Sun, yet adhering to the ecliptic.

Understand, therefore, that an arc, [as *ZFN*, *ZHKA* or *ZGO*], passing through the pole of the ecliptic [*Z*] and the end of the quadrant [at the small circle, as *F*, *H*, *G* or *K*, extended] as far as the ecliptic, [as to *N*, *A* or *O*], likewise moves in accordance with the motion of the end [at the small circle]. Accordingly, as much as is the portion of the ecliptic which two arcs contain, one of which [*ZEA*] passes through the pole of the ecliptic and the pole of the small circle, and the other, [as *ZFN*, *ZHKA* or *ZGO*], through the pole of the ecliptic and the end of the quadrant [at the small circle extended to the ecliptic], so much is also the portion [of the ecliptic] which is found between point *B* and the end of the quadrant adhering to the ecliptic, [as *L* or *M*].

And thus when the revolving end of the quadrant arrives at point *F*, the point, I say, in which arc *ZN* is tangent to the small circle, there the portion of the ecliptic [*NA*] of which I have spoken is the greatest, and hence the other, adhering end of the quadrant draws the farthest away from *B*, [namely to *L*]. But afterwards this portion gradually decreases until it becomes zero, namely, when the revolving end of the quadrant is in point *H* of the small circle. When, however, the revolving end of the quadrant recedes from point *H*, once again the portion of the ecliptic contained between the two arcs increases until the revolving end of the quadrant arrives at the other point of tangency, which is *G*. Then once again the portion of this kind [*AO*] is greatest, which finally, on account of the motion of the revolving end, decreases continuously until an entire revolution in the small circle is completed.

And thus this is the disposition of the motions of the Sun. Now I come to the description of the technical terms.

The line of the mean motion of the Sun is the line which, proceeding from the centre of the world through point *B*, is extended as far as the zodiac.

Medius motus et uerus [sunt arcus eclipticae] principio arietis et lineis suis iam dictis intercipiuntur.

Aequatio solis est arcus eclipticae quam lineae ueri et medii motuum concludunt. Haec nulla est cum terminus quartae circuiens est in puncto *K* parui circuli. Cum autem est in puncto contactus, scilicet *F*, maxima contingit aequatio, quam signat in figura arcus *BL*. Item in puncto *H* nulla est aequatio, sed tandem maxima redit aequatio cum terminus quartae circuiens est in puncto *G*, quam quidem aequationem arcus *BM* significat. Quamdiu autem terminus quartae circuiens est in medietate parui circuli *KFH*, medius motus maior est uero, quare tunc aequatio, ut uerus habeatur motus, a medio subtrahitur. In alia uero medietate contra fit, quare tunc aequatio additur.

[De luna]

Luna quatuor requirit orbes mundo concentricos, quorum inferiores duo per omnia se habent sicut in sole, hoc dempto, quod terminus quartae circuiens non complet reuolutionem in eo tempore praecise, quo orbis secunda deferens paruum circulum, suam reuolutionem perficit. Item quod terminus quartae adhaerens

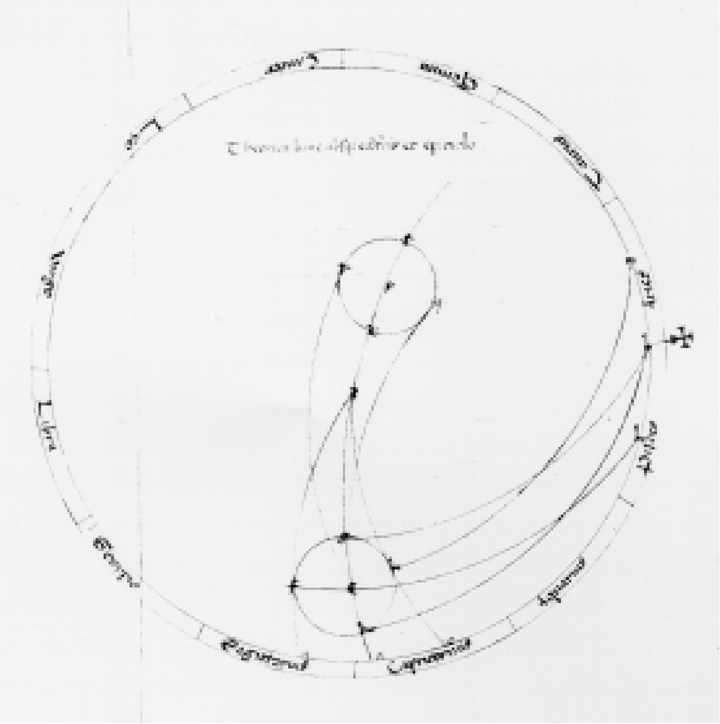


Plate 2. Theorica lunae absque eccentricis et epicyclo.

The line of the true motion is extended from the centre of the world through the centre of the solar body as far as the zodiac.

The mean and true motions [are the arcs of the ecliptic] cut off by the beginning of Aries and their lines just mentioned.

The equation of the Sun is the arc of the ecliptic which the lines of the true and mean motions contain. This is zero when the revolving end of the quadrant is in point *K* of the small circle. When, however, it is in the point of tangency, namely *F*, the greatest equation occurs, which arc *BL* designates in the figure. Likewise in point *H* the equation is zero, but finally the greatest equation returns when the revolving end of the quadrant is in point *G*, which equation arc *BM* designates. And as long as the revolving end of the quadrant is in the half *KFH* of the small circle, the mean motion is greater than the true motion, wherefore then the equation is subtracted from the mean motion in order to obtain the true motion. But in the other half [*HGK*] it is the contrary, wherefore then the equation is added.

[On the Moon]

The Moon requires four orbs concentric to the world, of which the lower two are arranged in all respects just as for the Sun, with this exception, that the revolving end of the quadrant does not complete a revolution in precisely the time in which the

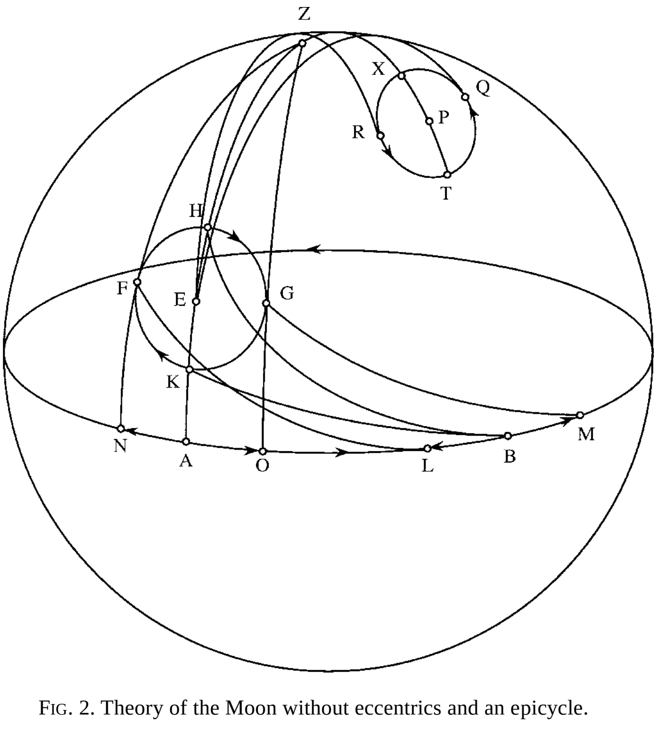


FIG. 2. Theory of the Moon without eccentrics and an epicycle.

non adhaeret eclipticae, sed circulo decliui qui in orbe quarto signabitur.

Circulus autem ille paruus circulus argumenti lunae uocabitur. Tertius orbis in sui concauitate paruum habet circulum, quem circulum centri medii nominabo, et signatur hic in figura notis *XQTR*, cuius polus *P* tantum distat a polo *Z* quantum punctus *E*, qui est polus circuli argumenti, a puncto *A*, qui in uia lunae decliui est, remouetur. Dicta tamen quatuor puncto in uno circulo magno sunt.

Intellige denique quartam circuli magni in conuexo secundi orbis, cuius unus terminus, qui est directe super punctum circuli argumenti, semper adhaereat circumferentiae *PZEA*, reliquus uero terminus semper adhaereat circumferentiae circuli centri medii.

Quartus et supremus orbis suos habet polos sub polis zodiaci, et in eo sunt duo puncta diametraliter opposita, quorum quodlibet a polo dicto sibi uicino distat per quinque gradus. Haec duo puncta sunt poli uiae decliuis ab ecliptica, sub qua quidem uia luna semper reperitur. Mouetur autem orbis iste contra successionem signorum, et transmutat sectiones draconis lunae.

Tertius uero orbis mouetur regulariter super polis uiae decliuis lunae secundum successionem signorum omni die naturali 13 gradibus fere, et defert secum circulum centri medii, qui in eo est, et circulum argumenti, qui in secundo orbe est. Verum unus terminus quartae quam in conuexo secundi orbis dixi imaginandam mouetur in circumferentia circuli centri medii, in mense lunari aequali duas faciendo reuolutiones, et trahit consequenter secum circulum argumenti. Hoc enim pacto argumentorum aequationes fiunt nunc maiores, nunc minores.

De motibus reliquorum orbium superius uisum est. Definitiones autem terminorum post, si Deus uolet, accipies. De reliquis planetis, nunc nihil.

Pauca haec scribere institui, ne hosce dies uentri modo indulgens inerti praeterirem otio. Quod si rem uelis ampliorem, faue praesul dignissime reducere me Vienna suscipiat. Iam enim Euclides, Ptolemaeus, caeteri quoque amici plurimi cum quibus ante hac consueui, nisi animus me fallit, de spe reditus decidere, quos apprime formido, ne si mora longiori detineat, perpetua me obliuione deserant. Da precor o et praesidium et dulce decus meum praeceptorem reuisere Georgium, qui res tuis placitura uotis absoluet, si prius discipulo mihi quae prior quaeque posterior ueniat ordo dabitur.

Haec magister Joannes de Kunigsperg Germanus in Varadino domino Johanni episcopo, anno domini 1460.

second orb carrying the small circle completes its revolution. Likewise, that the adhering end of the quadrant does not adhere to the ecliptic, but to an inclined circle which is designated in the fourth orb.

Now the small circle will be called the circle of the anomaly of the Moon. The third orb [also] has a small circle in its concavity, which I shall call the circle of the mean centre, and here it is designated in the figure by the letters *XQTR*. The pole *P* of the small circle is distant as far from pole *Z* as point *E*, which is the pole of the circle of the anomaly, is removed from point *A*, which is in the inclined path of the Moon. Yet the four points just mentioned [*PZEA*] are in the same great circle.

Finally, understand a quadrant of a great circle in the convexity of the second orb, of which one end, which is directly over the [centre] point of the circle of the anomaly, let always adhere to the arc *PZEA*, while the other end let always adhere to the circumference of the circle of the mean centre.

The fourth and highest orb has its poles under the poles of the zodiac, and in it are two diametrically opposite points, of which each one is separated from the pole of the zodiac near to it by 5°. These two points are the poles of the path inclined from the ecliptic, under which path the Moon is always found. Now this orb moves opposite to the order of the signs, and it shifts the intersections of the dragon of the Moon.

But the third orb moves uniformly about the poles of the inclined path of the Moon in the order of the signs by about 13° in each natural day, and it carries with it the circle of the mean centre, which is in it, and the circle of the anomaly, which is in the second orb. However, one end of the quadrant, which I have said is to be imagined in the convexity of the second orb, moves in the circumference of the circle of the mean centre, making two revolutions in a mean lunar month, and consequently draws with it the circle of the anomaly. For in this way the equations of the anomalies become now greater, now smaller.

It was evident earlier concerning the motions of the other orbs. If God wills, you will receive the definitions of the technical terms later. Concerning the other planets, nothing now.

I have undertaken to write these things lest I pass these days in unproductive idleness, merely giving in to the stomach. But if you wish something more substantial, be well disposed, worthy Bishop, that Vienna may undertake to recall me. For already Euclid, Ptolemy, and also many other friends to whom I was formerly accustomed, unless my mind fails me, have perished from hope of my return, which I fear especially, lest if my return be detained by a longer delay, they may abandon me to eternal oblivion. O grant, I pray, the aid and dear honour to again see my teacher Georg [Peurbach], who will complete these things in a way pleasing to your wishes, if first the order of what should come first and what after be given to me his pupil.

This letter [is from] Johannes Germanus of Königsberg in Grosswardein to Lord Bishop Johannes [Vitez], A.D. 1460.

Analysis

The principle of Regiomontanus's models is a conversion of circular to reciprocal motion, which is illustrated in Figures 3(a) and 3(b) by a rigid linkage mechanism in a plane. A rod of length $r = AB$ rotates uniformly through angle ϑ about A, and attached to it is a rod of length $l = BC > AB$, which is constrained such that C oscillates in a straight line over a distance s. The reciprocation may be either in-line with A as in 3(a) or offset by a distance $d = AA'$ as in 3(b). As illustrated in the figure, the motions are reversible, that is, if the source of motion is at A, rotational motion is converted to reciprocal, and if the source of motion is at C, reciprocal motion is converted into rotational. One may think of the mechanical trains attached to steam or internal combustion engines and electric motors. For example, a rotational motion at A may drive a pump or a press at C, or the piston of a heat engine at C may turn a wheel or crankshaft at A, as in a steam locomotive or automobile. In the theory of machines, the device is called a slider crank mechanism, probably the most common of all mechanical systems, in the terminology of which A is called the crankshaft, r the crank, B the crankpin, l the connecting rod, C the wrist pin, s the stroke, and d the offset.

The maximum displacement of C from its centre position takes place when the linkages r and l lie, either extended, $l + r$, or overlapping, $l - r$, in a straight line, necessarily passing through A. However, the alignment of the rotation and reciprocation

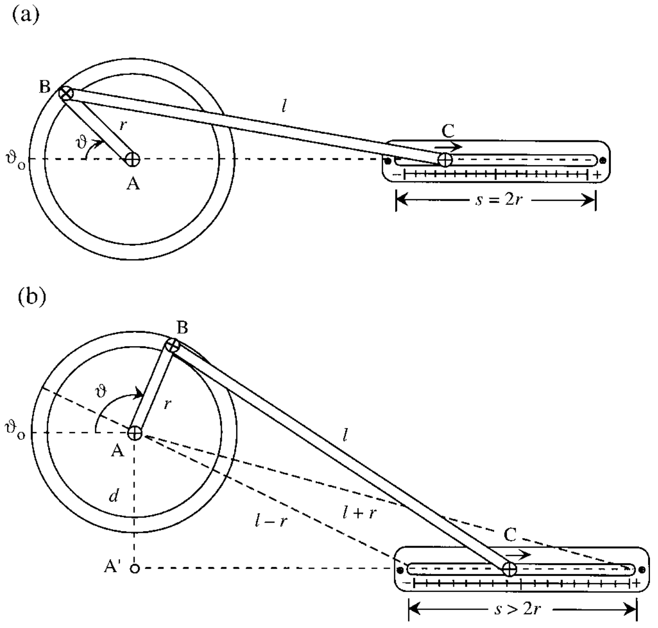


FIG 3. Conversion between rotation and reciprocation.

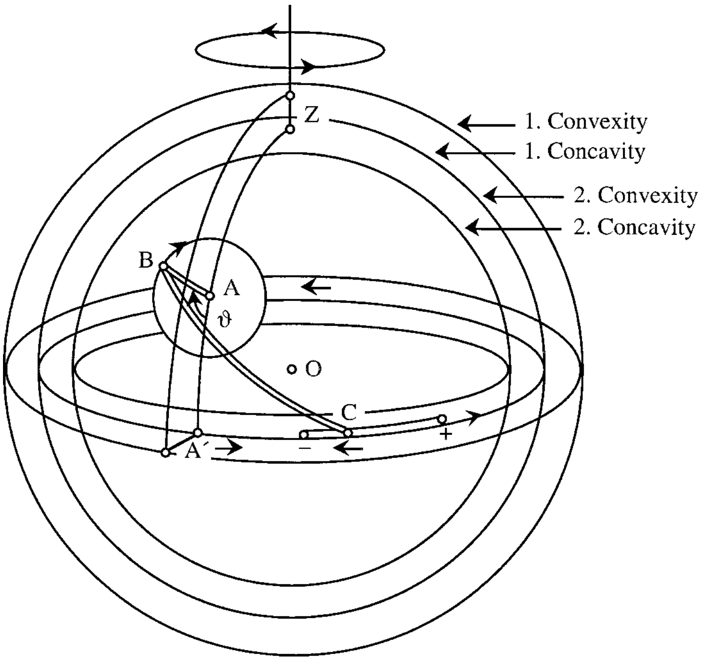


FIG. 4. Order of spheres and reciprocation.

is of importance and produces remarkable effects. If, as in 3(a), the constrained path of C is in-line with A, it is obvious that the range of the displacement of C is $s = 2r$ and its limits are reached at $\vartheta = (0°, 180°)$. However, if, as in 3(b), the line of the reciprocation of C is removed from A by an offset d, the range of the displacement of C is given by $s = ((l + r)^2 - d^2)^{\frac{1}{2}} - ((l - r)^2 - d^2)^{\frac{1}{2}}$. In the in-line mechanism of 3(a), $d = 0$ and the formula reduces to $s = (l + r) - (l - r) = 2r$. But in the offset mechanism of 3(b), in which $d < (l - r)$ and $r < l$, considering the triangle formed by $(l + r)$, $(l - r)$, and s, since the sum of any two sides of a triangle is greater than the third side, $(l - r) + s > (l + r)$, and since $(l - r) + 2r = (l + r)$, thus $s > 2r$ and the range of the displacement is greater than in the in-line reciprocation. Further, the displacement of C corresponding to any value of ϑ differs greatly for in-line and offset reciprocation, the offset reciprocation being highly irregular and having almost no relation to the in-line reciprocation for the same value of ϑ. Interestingly, all of these difficulties disappear when the mechanism is transferred to spheres under the special conditions chosen by Regiomontanus.

The conversion of this device to the motions of spheres as imagined by Regiomontanus is shown in Figure 4. The spheres are hollow bodies, shells, of some unspecified thickness, each having an outer, convex surface or convexity (*conuexum*, *conuexitas*) and an inner, concave surface or concavity (*concauum*), all surfaces centred about the observer at O, and are taken in pairs, with the convex surface of the

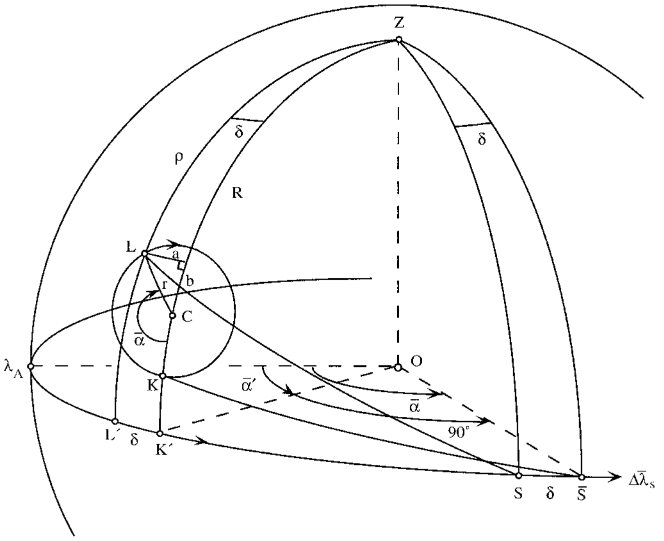


FIG. 5. Theory of the Sun.

inner Sphere 2 touching the concave surface of the outer Sphere 1. The spheres must also be rigid bodies, that is, solid spheres, for the motion of a pole is transferred to the equator of the sphere, and motions are also transferred through quadrants of great circles that behave like the rigid rods of Figure 3. The outer Sphere 1 is given a rotational motion about its pole *Z* that carries both the equatorial great circle of the sphere, located a quadrant from *Z* at *A′*, and a small circle with centre *A* located in its concavity. In the convexity of Sphere 2, which is also carried about *Z*, is a point *B* that moves uniformly through angle ϑ in the circle about *A*, and at the end of a quadrant from *B* is a point *C*, likewise in the convexity of Sphere 2, that oscillates over some distance in the equatorial circle of Sphere 1 as *B* moves about the small circle. Now this is very strange. One might imagine *A* to be a pole and *AB* the radius of a small circle, both in the convexity of Sphere 2, so that *B* is carried around the circle as Sphere 2 rotates about its pole at *A*. But it is clear from Regiomontanus's description that both *A* and the small circle are in the concavity of Sphere 1; hence point *B* of Sphere 2 must be displaced about the small circle rather than rotate around a pole, which is not what usually happens in concentric sphere models. And how, one might ask, does point *C* manage to confine its oscillations to the equatorial circle of Sphere 1, as there is no guide, as in Figure 3, to keep it there?

What has just been described is actually the solar model, which is shown in Figure 5. The ecliptic is a circle in the higher of two spheres, rotating with the sphere through the mean motion of the Sun in longitude $\Delta\bar{\lambda}_s$, and let $\bar{\alpha}$ be the mean anomaly of the Sun measured from λ_A, the longitude of the apogee in an eccentric or epicyclic model, to the mean sun $\bar{S}$. In this higher sphere, let a quadrant descend from the pole of the ecliptic Z to the ecliptic at K' and move about Z through $\lambda_A K' = \bar{\alpha}' = \bar{\alpha} - 90°$. On quadrant ZK' at a distance R from Z, let a point C be taken in the concavity of the higher sphere, which will also move through $\bar{\alpha}'$ about Z, and thus parallel to the ecliptic. C is the centre of a small circle of radius r, likewise in the concavity of the higher sphere, about which a point L, in the convexity of the lower sphere, moves through the mean anomaly $\bar{\alpha}$ such that $\bar{\alpha} = 0°$ and the direction of the motion of L is opposite to the motion of C about Z when L lies at K, at the greatest distance from Z on quadrant ZK'. Let a second quadrant descend from Z through L to meet the ecliptic at L', and as L moves about pole C, L' departs from K' by arc δ. Next, in the convexity of the lower sphere let a quadrant from K "adhere" to the ecliptic at the mean sun $\bar{S}$ which moves 90° ahead of K through the mean anomaly $\bar{\alpha} = \bar{\alpha}' + 90°$ measured from λ_A. Let a second quadrant from L "adhere" to the ecliptic at S, the true Sun, such that as L rotates about C, S oscillates on either side of $\bar{S}$ through δ, the equation of the anomaly, in the plane of the ecliptic. Hence, the distance of S from λ_A is the true anomaly (not shown) $\alpha = \bar{\alpha} \pm \delta$.

What Regiomontanus does not show is that $K'\bar{S}$ and $L'S$ in the ecliptic are equal, so that $S\bar{S} = L'K' = \delta$, which is not obvious but is true provided that $K\bar{S}$ and LS are both quadrants. These relations were demonstrated by Bernard Goldstein in his own ingenious *correction* of al-Biṭrūjī's planetary model, and I must confess that, having begun my analysis with reciprocation in a plane, I did not believe that Regiomontanus's model could work properly until I came upon Goldstein's simple and elegant demonstration. And, as noted before, I believe that Regiomontanus devised the principle of his model as a correction to al-Biṭrūjī, just as Goldstein did. Thus, since $Z\bar{S} = K\bar{S} = 90°$ and ZKK' lie in the same great circle, $K'\bar{S} = K\bar{S} = 90°$. And since $ZS = LS = 90°$ and ZLL' lie in the same great circle, $L'S = LS = 90°$. Hence, $K'\bar{S} = L'S = 90°$ and $S\bar{S} = L'K' = \delta$. Nevertheless, although the geometry is satisfactory, it is hard to see how the model can work mechanically. For although $\bar{S}$ may "adhere" to the ecliptic a quadrant from the fixed point K without difficulty, one may reasonably ask how S is made to "adhere" to and oscillate along the ecliptic a quadrant from the moving point L since there is surely no slot or guide for S of the sort shown in Figure 3. To expect S to know how to remain in the ecliptic while L moves about a circle is to expect a great deal.[12]

Regiomontanus promises a treatise in which he will explain how to compute equations from his concentric-sphere models. It is not difficult. If $\bar{\alpha}$ is measured from K and the distance $\rho = ZL$, from the law of cosines, $\rho = \cos^{-1}(\cos r \cos R - \sin r \sin R \cos \bar{\alpha})$, and then from the law of sines $\delta = \sin^{-1}((\sin r \sin \bar{\alpha})/\sin \rho)$. With only derivatives of the law of sines, for which the *Tabulae primi mobilis* may be used, drawing a perpendicular from L to ZC, so that $a = \sin^{-1}(\sin \bar{\alpha} \sin r)$ and $b = \cos^{-1}(\cos r/\cos a)$, it follows that $\rho = \cos^{-1}(\cos a \cos (R + b))$ and $\delta = \sin^{-1}(\sin a/\sin \rho)$.[13] The equation δ itself is determined by the distance R of C from Z and the ratio r/R. The distance R is arbitrary, but once selected, the ratio r/R must be chosen such that $\sin^{-1}(\sin r/\sin R) = \delta_{\max}$, for in this way angle CZL and arc $L'K'$ will be equal to the required δ, and $\delta_{\max}$ will occur when ZL' is tangent to the small circle and perpendicular to CL just below

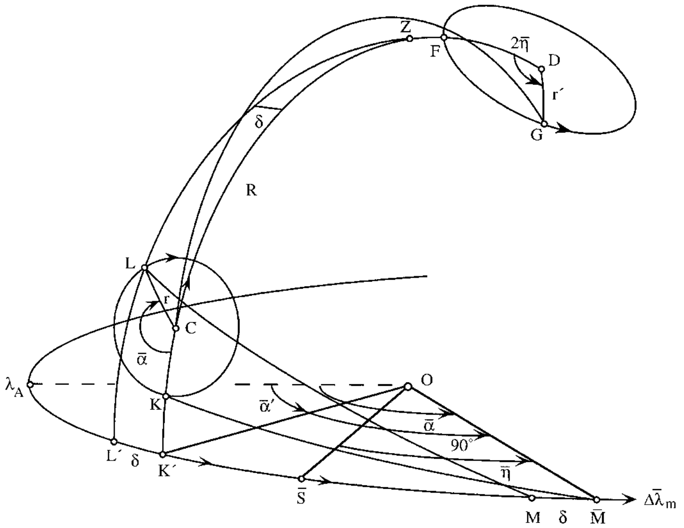


FIG. 6. Theory of the Moon.

$\bar{\alpha} = \pm(90° + \delta_{\text{max}})$ because of the spherical excess in triangle *CZL*. In a planar epicyclic or eccentric model the maximum equation occurs at $\bar{\alpha} = \pm(90° + \delta_{\text{max}})$ exactly, but because r/R is small, the difference will be small and, in general, δ will differ only slightly from a planar model for a corresponding $\bar{\alpha}$. After all, what Regiomontanus, like al-Biṭrūjī, has done is to place an epicyclic model on the surface of a sphere, and since neither Regiomontanus nor anyone else had any idea of how accurately a simple epicycle produces the solar equation, any small difference must be considered harmless. Perhaps he even thought that in tabulating δ the solar equation $c_3(\bar{\alpha})$ from the Alfonsine Tables could be used directly. The motivation of the model, as we have noted, is entirely physical and has nothing to do with accuracy. It follows from the condition for r/R that $r = \sin^{-1}(\sin \delta_{\text{max}} \sin R)$. If, as appears reasonable, Regiomontanus has in mind the solar equation of the Alfonsine Tables in which $\delta_{\text{max}} = 2;10°$, then if *C* were located at *K′* in the ecliptic so that $R = ZK' = 90°$, $r = \sin^{-1}(\sin 2;10° \sin 90°) = 2;10°$. But if, say, $R = ZC = 60°$, then $r = \sin^{-1}(\sin 2;10° \sin 60°) = 1;52,35°$, and it may be confirmed that $\delta_{\text{max}} = \sin^{-1}(\sin 1;52,35°/\sin 60°) = 2;10°$, as required.

So much for the Sun. We next consider the lunar model, shown in Figure 6, which is more complex than the solar model but still works on the same principle. There are four spheres, of which the two inner are for the equation of the first inequality at syzygy, the third is for the variation of the equation due to the second inequality, and the fourth and outermost, which is never illustrated, is for the motion of the nodes and the inclined circle of the Moon along the ecliptic. The Moon is said to move in the path of the inclined circle in the fourth sphere, although it is actually located in the convexity of the innermost sphere, and *Z*, the pole of the inclined circle, is 5° from the pole of the ecliptic. The first two spheres are analogous to the two spheres of the Sun, that is, the convexity of the innermost contains the quadrants $K\bar{M}$ and LM extending from the small circle to, respectively, the mean position $\bar{M}$ and true position M of the Moon, and the concavity of the next higher contains the small circle itself, called the "circle of the anomaly", with centre C which moves on quadrant ZK' through the mean motion in longitude $\Delta\bar{\lambda}_{\mathrm{m}}$. In the case of the Moon, the mean motions in longitude and anomaly differ notably, but if we define a moveable apogee λ_{A}, as in an eccentric or epicyclic model, we may consider the mean anomaly $\bar{\alpha}$ measured from λ_{A} to the mean moon $\bar{M}$. The small circle of the anomaly with centre C and the equation of the anomaly δ are the same as for the Sun so that $M\bar{M} = L'K' = \delta$ as required. The distance R is arbitrary and again the ratio r/R has the condition $\sin^{-1}(\sin r/\sin R) = \delta_{\mathrm{max}}$, so that $r = \sin^{-1}(\sin \delta_{\mathrm{max}} \sin R)$. Taking the maximum equation of the Alfonsine Tables, in which at syzygy $\delta_{\mathrm{max}} = 4;56°$, if $R = 90°$, meaning that C is in the plane of the Moon's inclined circle, $r = 4;56°$, and, as in the example used for the Sun, if $R = 60°$, $r = \sin^{-1}(\sin 4;56° \sin 60°) = 4;16,16°$, and we may confirm that $\delta_{\mathrm{max}} = \sin^{-1}(\sin 4;16,16°/\sin 60°) = 4;56°$.

The second inequality, which alters the equation of the anomaly as a function of the mean elongation $\bar{\eta}$ of the Moon from the mean sun, is produced by another small circle, called the "circle of the mean centre", located in the concavity of a third, higher sphere. Thus, D is the centre of this circle, located on arc $K'CZ$ extended beyond Z in the concavity of a third sphere such that D is a quadrant from C, from which it follows that $ZD = K'C$. A point G in the convexity of the lower sphere carrying C moves in the circle about D of radius $DG = r'$, completing a rotation through $2\bar{\eta}$ in one-half a mean synodic month — $2\bar{\eta}$ is called the "mean centre" in medieval lunar theory, hence the name of the circle — such that G coincides with F, at its least distance from Z, at syzygy when $2\bar{\eta} = 0°$, and is at its great distance from Z at quadrature when $2\bar{\eta} = 180°$. The direction of rotation is arbitrary. A quadrant extends from G to C, and as G rotates about the circle of the mean centre, the centre of the circle of the anomaly C, which "adheres" to the arc DZK', is drawn towards Z, reaching its least distance at quadrature. The principle is exactly that of the in-line reciprocation of Figure 3(a). The motion of C along arc ZK' reduces $ZC = R$ and thus increases the ratio r/R, which in turn increases the equation δ such that r/R and δ are maximum at quadrature and minimum at syzygy. This is just what happens in Ptolemy's lunar model, in which the distance R of the centre of the epicycle from the observer varies between syzygy and quadrature so that also r/R and δ are maximum at quadrature and minimum at syzygy.

The effect is shown in Figure 7, in which the centre of the circle of the anomaly, C_1 at syzygy, has been drawn along ZK' to C_2 by the motion of G in the small circle, increasing the equation of the anomaly at syzygy δ_1 by the amount δ_2. If we call the least distance $R' = R - 2r'$, it is now required that $\sin^{-1}(\sin r/\sin R') = (\delta_1 + \delta_2)_{\mathrm{max}}$. Since r is given by $\delta_{1\mathrm{max}}$ at syzygy, we find R' from $R' = \sin^{-1}(\sin r/\sin (\delta_1 + \delta_2)_{\mathrm{max}})$, and the radius r' of the circle of the mean centre then follows from $r' = \frac{1}{2}(R - R')$. In the Alfonsine Tables the maximum equation at quadrature, $(\delta_1 + \delta_2)_{\mathrm{max}} = 7;34°$. Thus, if R

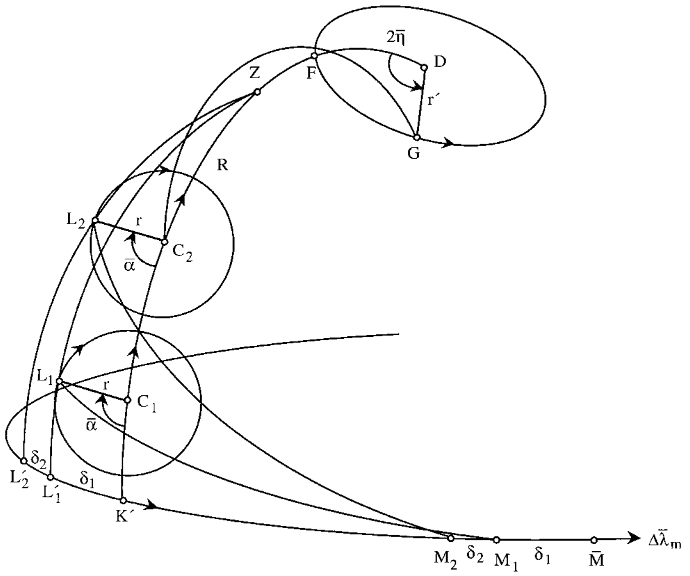


FIG. 7. Second inequality of the Moon

$= 90°$, so that C is in the Moon's inclined circle at syzygy and $r = 4;56°$, $R' = \sin^{-1}(\sin 4;56°/\sin 7;34°) = 40;46,25°$ and $r' = \frac{1}{2}(90° - 40;46,26°) = 24;36,47°$, which is very large indeed. If, as in our previous example $R = 60°$ and $r = 4;16,16°$, $R' = \sin^{-1}(\sin 4;16,16°/\sin 7;34°) = 34;26,34°$ and $r' = \frac{1}{2}(60° - 34;26,34°) = 12;46,43°$; and we confirm that $(\delta_1 + \delta_2)_{\max} = \sin^{-1}(\sin 4;16,16°/\sin 34;26,34°) = 7;34°$. Table 1 shows R' and r' for selected values of R, assuming, as before, that $\delta_{1\max} = 4;56°$ and $(\delta_1 + \delta_2)_{\max} = 7;34°$.

TABLE 1.

R	r	R'	r'
5°	0;25,46°	3;15,47°	0;52, 7°
10	0;51,20	6;30,40	1;44,40
15	1;16,31	9;43,50	2;38, 5
30	2;27,52	19; 3,33	5;28,13,30
45	3;29,10	27;30, 6	8;44,57
60	4;16,16	34;26,34	12;46,43
75	4;55,34	39; 3,29	17;58,15,30
90	4;56	40;46,26	24;36,47

In every case, the circle of the mean centre r' is larger than the circle of the anomaly r, in fact, from two to five times larger, just as in Ptolemy's lunar model the eccentricity of the second inequality is nearly twice the radius of the epicycle, for the principle of increasing the ratio r/R by reducing R is the same in both models. But of course here there is no problem of an exaggerated variation in the distance of the Moon from the Earth because the distance does not vary at all.

The position of the Moon is given at syzygy by the quadrant L_1M_1, which "adheres" to the inclined circle of the Moon at M_1, and with the correction for the second inequality by quadrant L_2M_2, which "adheres" to the inclined circle at M_2. With the mean position of the Moon 90° ahead of K' at $\bar{M}$, the Moon at syzygy is 90° ahead of L'_1 at M_1 with equation δ_1, and not at syzygy, with the additional correction δ_2 for the second inequality, 90° ahead of L'_2 at M_2, for which the equation is thus $\delta_1 + \delta_2$. For computing the equations, Regiomontanus may have in mind using three columns of the Alfonsine Tables, that is, the equation $c_6(\bar{\alpha})$ for syzygy, the addition $c_5(\bar{\alpha})$ for quadrature, and the coefficient of interpolation $c_4(2\bar{\eta})$ for intermediate elongations in the form $\delta_1 + \delta_2 = c_6 + c_4 \times c_5$. But since he mentions that he intends to write a treatise on how motions and tables may be computed for his new models, perhaps he has other ideas. The formulas for spherical solutions given earlier for the Sun could be applied to δ_1 and to $\delta_1 + \delta_2$, with columns then tabulated for δ_1 and $\delta_2 = (\delta_1 + \delta_2) - \delta_1$, with some kind of coefficient of interpolation a function of $2\bar{\eta}$. Note that there is no equivalent in Regiomontanus's model to the inclination of the apogee of the epicycle in Ptolemy's model, and thus no correction of the mean anomaly by the second inequality as a function of the mean elongation, that is, the correction $c_3(2\bar{\eta})$ in the Alfonsine Tables is simply ignored.

Finally, and this is not illustrated, the fourth and outermost sphere shifts the intersections of the 'dragon' of the Moon, that is, the nodes of the Moon's inclined circle, opposite to the order of the signs, in the direction of decreasing longitude. Thus, the poles of the fourth sphere coincide with the poles of the zodiac, and the poles of the inclined circle in the fourth sphere are removed 5° from the poles of the zodiac. As the sphere turns about the poles of the zodiac, the inclined circle in the fourth sphere with its nodes and limits of latitude will slowly regress, and this motion of the inclined circle is transmitted to the three lower spheres.

REFERENCES

1. The *Tabulae primi mobilis* and the sine tables have been carefully analysed by E. Glowatzki and H. Göttsche, *Die Tafeln des Regiomontanus: Ein Jahrhundertwerk.* Algorismus. Heft 2 (Munich, 1990). In the former, they find 1072 errors of ±1″, 48 of ±2″, 4 of ±3″, 2 of ±4″, 1 of +5″. The sine table for $R = 10{,}000{,}000$ contains 1820 errors of ±1, 1 of +2, and 12 of –2 in 5400 entries. Is there anyone who could do that today?
2. The best guide to the *Tabulae primi mobilis*, although they are nowhere mentioned, is the edition of Johann Werner's *De meteoroscopiis libri sex* by A. Björnbo and, following his death, J. Würschmidt in *Abhandlungen zur Geschichte der mathematischen Wissenschaften*, xxiv/2 (1913). The reason is that Werner, using his instrument, the *meteoroscope*, which he also calls a *saphea*, a universal astrolabe containing stereographic projection of circles of longitude and latitude, provides solutions for all of the problems in the *Tabulae primi mobilis*, and a good many more besides. It is a very interesting work, very well edited, and should be better known. A description and illustration of such an instrument may be found in John North, *Horoscopes and history* (Warburg Institute Surveys and Texts 13; London, 1986), 67–69. Obviously, the precision of using the *Tabulae primi mobilis* far exceeds that of any instrument that could be made.
3. Nuremberg Cent V app. 56c, f. 39v:

> 37. Aequationem octauae spherae secundum Alfonsi fundamenta numerare.

38. Aequationem octauae spherae secundum imaginationem Tebith computare.
39. Aequationem solis colligere.
40. Aequationem argumenti lunae dinumerare.

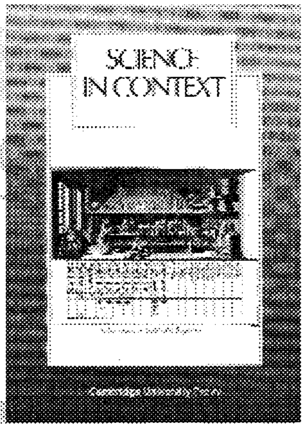
Sed quo ruit calamus ille molestus atque audax? Forsitan omnia astronomorum quaesita ad hanc unicam tabulam appellet? Dico ego bonam partem huiusmodi quaesitorum per hanc repertum iri tabulam, si prius concentricam astronomiam totam fundauerimus. Quid illud? Diuersitates motuum planetarum per concentricos saluare pulcrum erit. Iam soli et lunae uiam dedimus, de reliquis autem quaedam initialia iacta sunt, quibus completis aequationes omnium planetarum per hanc tabulam numerare licebet. De hac re nihil amplius impraesentiarum, ne legendi scripta mea maius patiamini fastidium quam ego scribendo uoluptatem habeam. Si quid harum rerum audentius aequo dixisse uideor, posteris litteris rationes meas luculentius (sic puto) accipietis. Mallem tamen uoce quam calamo hisce de rebus disserere, facilius enim multo et expeditius foret. Dum id fieri nequit, littere uoci officia sumant, [*margin:* quae, quicquid afferent, uestro iudicio limandum erit].

M. Curtze's text in *Abhandlungen zur Geschichte der mathematischen Wissenschaften*, xii (1902), 218, is, as usual, faulty. While mine may not be perfect, it is at least better and intelligible. For this passage, I have not checked the editions of von Murr (1786) and S. Magrini, *Atti e memorie della deputazione ferrarese di storia patria*, xxii/3 (1917), which are *always* preferable to Curtze's. Regiomontanus follows this with the, possibly deleted, remark that he intends to go to Milan on some business, and it appears that he did, for the following spring he reports in his oration on the mathematical sciences delivered in Padua that he saw Giovanni de Dondi's *astrarium* kept safely by the Duke of Milan in his Castle of Pavia.

4. E. Zinner, "Neue Regiomontan-Forschungen und ihre Ergebnisse", *Sudhoff's Archiv*, xxxvii (1953), 107–8. Zinner again mentions the letter in *Leben und Wirken des Joh. Müller von Königsberg genannt Regiomontanus*, 2nd edn (Osnabrück, 1968), 151, and the remarks in the letter to Bianchini, 101. There is also a reference by H. Grossing, "Regiomontanus und Italien", *Regiomontanus-Studien*, ed. by G. Hamann, *Sitzungsberichte der österreichische Akademie der Wissenschaften*, Phil.-hist. Kl., ccclxiv (1980), 223–4, p. 234, and discussions by A. Gerl, *Trignometrisch-astronomisches Rechnen kurz vor Copernicus* (Boethius 21; Stuttgart, 1989), 210–13, and "The most recent results of research on Regiomontanus", in E. Zinner, *Regiomontanus: His life and work*, transl. by E. Brown (Amsterdam, 1990), 335–8. I would politely suggest that not one of these accounts is based upon a study of the text or can withstand scrutiny.
5. In the heading on f. 18r, the work is even addressed to Ilkusch: "Johannes Germanus ad Martinum Ilkusch Cracoviensis in theoricas veteres a Gerardo aiunt Cremonensis editas."
6. F. J. Carmody, "Regiomontanus' Notes on al-Biṭrūjī's astronomy", *Isis*, xlii (1951), 121–30, which Carmody wrote in connection with his edition of Michael Scot's Latin translation, *Al-Biṭrūjī, De motibus celorum* (Berkeley, 1952), and "The planetary theory of Ibn Rushd", *Osiris*, x (1952), 556–86.
7. M. H. Shank, "The 'Notes on al-Biṭrūjī' attributed to Regiomontanus: Second thoughts", *Journal for the history of astronomy*, xxiii (1992), 15–30. The eclipses are those of 17 June 1433, which seems to have been total, not annular, and 31 January 1310, which was annular and was dated by Marcho to 1309 since the year in Paris began at Easter. Shank identifies G. Marcho with the Francisan Guy de la Marche. See also M. H. Shank, "Regiomontanus and homocentric astronomy", *Journal for the history of astronomy*, xxix (1998), 157–66. I wish to thank Michael Shank and Richard Kremer, who are engaged in a far more extensive study of Regiomontanus's interest in concentric-sphere models, for encouraging me to retrieve my transcription and translation and write this paper.
8. Zinner, *Regiomontanus* (ref. 4), 61; Shank, *op. cit.* (ref. 7), 17.
9. B. R. Goldstein, *Al-Biṭrūjī: On the principles of astronomy* (2 vols, New Haven, 1971), 7–12.
10. Shank, *op. cit.* (ref. 7), 25–26.
11. N. M. Swerdlow, "Regiomontanus on the critical problems of astronomy", *Nature, experiment,*

and the sciences, ed. by T. H. Levere and W. R. Shea (Dordrecht, 1990), 165–95, pp. 173–4. Let me correct a mistranslation in this paper that has bothered me for years. On p. 172, paragraph 3, lines 2–3, for “familiarly known” substitute “obstinate” (*inveterati*, i.e. “chronic”). Then on p. 183, paragraph 2, delete the first sentence. Regiomontanus’s point is that Jupiter and Saturn do not present the long-standing problems of Mars.

12. If one asks why Regiomontanus did not simply put the mean sun at K' and the true Sun at L', with ZCK' in advance of λ_A by $\bar{\alpha}$, the answer must be that ZK' and ZL' are not physical or structural quadrants, as $K\bar{S}$ and LS, but just a way of measuring the equation δ in the ecliptic; and such an arrangement would not be a correction or modification of Biṭrūjī’s model, in which the quadrant from the epicycle to the Sun, Moon or planet is essential.
13. This is the solution in *De triangulis omnimodis* IV, 28, for two sides and an included angle, adapted to an exterior angle; V, 2 is equivalent to the law of cosines, although using versed sines, but is not in a form suitable to this problem and one does not know whether Regiomontanus even knew it when he wrote this work.



JHA, xxx (1999)

A CAREER OF CONTROVERSY: THE ANOMALY OF T. J. J. SEE

THOMAS J. SHERRILL, Los Altos, California

Few historical figures of early twentieth-century science inspire a degree of rancour comparable to that evoked by the American astronomer Thomas Jefferson Jackson See (Figure 1). At a time when the revolutionary developments of Einstein's theory of relativity and quantum mechanics were overturning the old physics, when new discoveries on the nature of the atom were astonishing the world, and when technological breakthroughs were occurring almost weekly, public interest in science grew to unprecedented levels. It seems perhaps odd that during this productive period there should emerge as a major "spokesman" for science — consulted by newspapers and other media to interpret scientific discoveries or worldly events such as eclipses, earthquakes, or volcanic eruptions — a scientist whose own theories ran counter to the revolution.

Although he had a solid background in celestial mechanics and was a respected telescopic observer early in his career, when he turned to theoretical work T. J. J. See began diverging from his astronomical colleagues in striking ways. He developed his own hypothesis of solar system evolution, as well as theories that explained the many diverse phenomena in the universe. In public lectures, numerous books, and dozens of articles in popular science magazines he frequently managed to convince a significant segment of the public that his unorthodox astronomical views were to be preferred over more accepted contemporary theories. From astronomy he ventured into other scientific fields, notably geology and physics, generating controversy after controversy. He helped lead scientific attacks on relativity, sought classical explanations for atomic and electromagnetic forces, and challenged Hubble's concept of an expanding universe. With each controversy he alienated himself further from the scientific establishment.

Despite his limited support among astronomers, See had a devoted public following who hailed him as "the American Herschel" and "the greatest astronomer in the world". This following was looked upon with embarrassment in the scientific community, and it served as a constant reminder of the difficulty of explaining science to the world at large. Once See had achieved some credibility with the public, it was hard to counter his influence with reasoned arguments that might be too technical for mass consumption.

Although his work is little known to today's younger astronomers, it is perhaps worthwhile to review See's career, if only as a counterexample to the case for strict application of the scientific method. While attempts have been made over the years to 'rehabilitate' his reputation, or to suggest that some of his ideas were indeed visionary for his time, the fact remains that many of his methods were deplorable and eventually detrimental to science.

0021-8286/99/3001-0025/$2.50

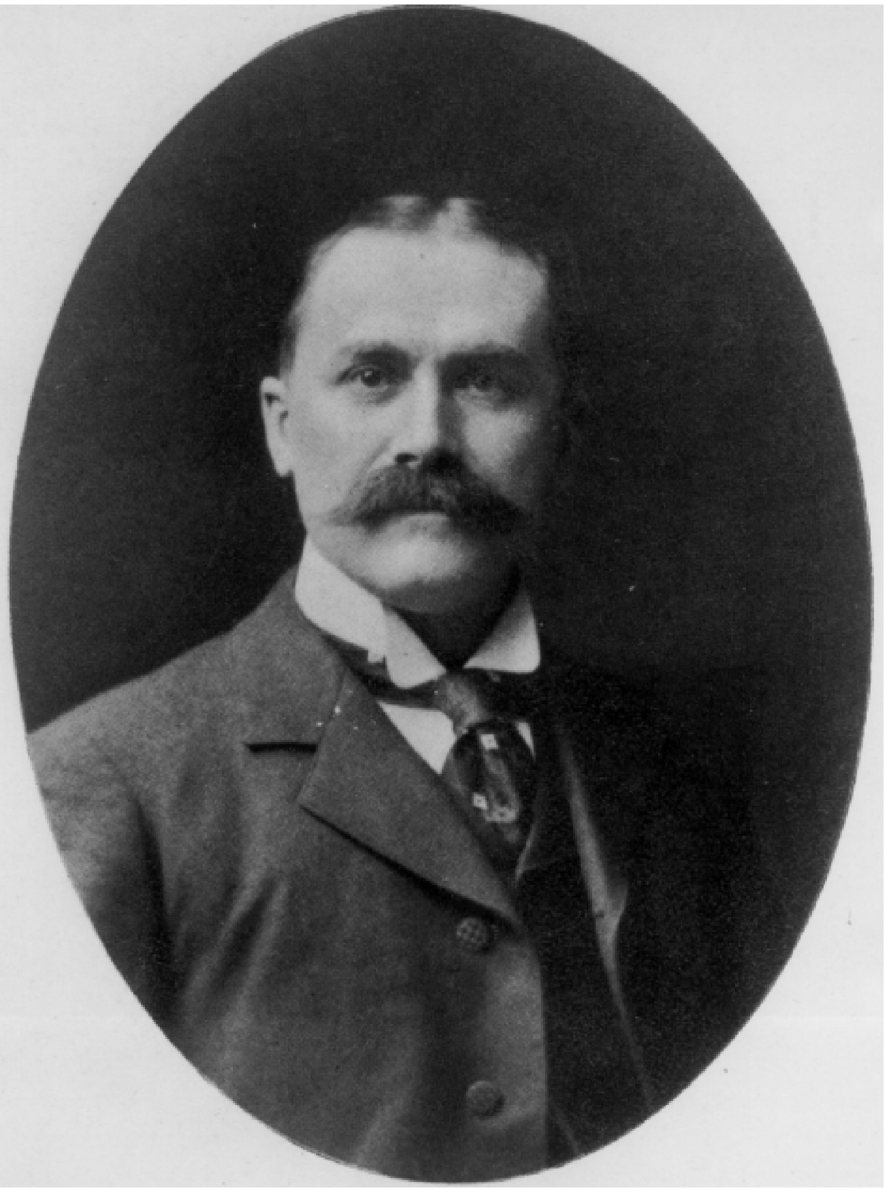


FIG. 1. T. J. J. See, from the frontispiece of W. L. Webb's *Brief biography* (ref. 1).

Background and Education

Born near Montgomery City, Missouri, on 19 February 1866, Thomas Jefferson Jackson was the sixth of nine children of Noah See, a well-to-do farmer. His early education was typical for a farmer's son in the post-Civil War era, consisting of no more than four winter months a year in a country schoolhouse. Despite his obvious brightness, it was only when he was seventeen years old that he was allowed to attend a proper high school in the county seat, where he so distinguished himself in science and mathematics that his father was persuaded to send him on to college.

At the State University of Missouri, in Columbia less than fifty miles from his home, See continued to excel in all the sciences, but it was astronomy that drew him most strongly. He was allowed to use the $7\frac{1}{2}$-inch equatorial telescope at the university's small observatory to study the planets, comets, and stars; he took a particular early interest in double stars. He graduated in 1889 as class valedictorian.

See was sent abroad for his graduate education, to the University of Berlin, which had one of the world's most respected faculties in the sciences, presided over by Hermann von Helmholtz. The astronomy professors allowed the hard-working student to observe and measure double stars with the Royal Observatory's 9-inch refractor. He learned the most modern techniques for calculating double star orbits, and was to write his dissertation on the subject of the origin of such multiple systems. He received his Doctor of Philosophy and Master of Arts degrees *magna cum laude* in December 1892.[1]

At the University of Chicago

Upon graduation, See was offered an instructorship with the astronomy department of the new University of Chicago by its president, William R. Harper, whom he had met in Berlin. At the time the University's astronomy department consisted solely of the astrophysicist George Ellery Hale and a celestial mechanics specialist, Kurt Laves. The department had considerable promise for the future, however, for Hale and Harper had recently persuaded Charles T. Yerkes, a Chicago streetcar tycoon, to purchase the 40-inch glass lenses then lying unground at the Massachusetts shop of Alvan Clark and Sons. They were intended for the largest refracting telescope in the world, to be the centrepiece of the planned Yerkes Observatory, a facility costing around $250,000.

In consultation with S. W. Burnham, the noted double star observer who was awaiting a faculty appointment, it was decided that virtually all published double star orbits — some of them decades old — required revision based on more recent observations. See was assigned the task of leading a few graduate students in collecting old and new observations, adding some of their own made at cooperating nearby observatories, and systematically recalculating the orbits of the forty best-observed binary systems. With such an ambitious project some shortcuts had to be taken, so new graphical methods worked out with Burnham were utilized to speed up the process of reducing the orbits.

See quickly realized that the project provided a fertile source of recognition, and he began submitting papers almost monthly to the *Astronomical journal* in America and the *Astronomische Nachrichten* in Germany, as the binary orbits were analysed one by one.[2] This was the start of a prolific career as an astronomical writer and popularizer, as he also began publishing articles in non-professional scientific monthlies such as *Popular astronomy*.[3]

In the meantime, the University of Chicago's efforts to get the Yerkes Observatory out of the planning stage had bogged down. After an observing session in Europe, Hale was preoccupied with establishing the *Astrophysical journal*, and Harper became increasingly worried about a projected estimate of $30,000 a year which might be required to run the Observatory once it was built. At a meeting with Harper, See offered to draw up a plan for running Yerkes on a much reduced budget, and this offer was accepted. Supposedly this budget reduction earned the young astronomer Hale's animosity; among other things, See felt that this animosity was behind the University's reneging on a promise to publish his investigation of the forty binary orbits in book form.[4] Thus See had to arrange to publish this work, entitled *Researches on the evolution of the stellar systems*: *Volume 1* (for he had further plans for the subject), at his own expense.[5]

In the spring of 1896, in consequence of See's growing reputation as an observer, Percival Lowell invited him to undertake a survey of the southern sky using the 24-inch Clark refractor at Flagstaff, Arizona, for the discovery and measurement of double stars. See accepted, and Harper offered him leave of absence to do this work, with the rank of assistant professor. See, however, insisted on the position of associate professor as his price to remain connected with Chicago. Since this was the rank of the much more prominent Hale, Harper could not grant the request, and See simply left.[6] Thus he lost the opportunity to work with the Yerkes 40-inch, which was completed and the Observatory opened in September 1897.

At the Lowell Observatory

The work for Lowell was another major undertaking for See, as many southern doubles had not been measured since their discovery by Sir John Herschel during his expedition to the Cape of Good Hope from 1834 to 1838. Because observations of Mars had top priority at Flagstaff, the binary survey with the 24-inch (Figure 2) had to be done when the planet was least accessible. During the winter of 1896–97 the telescope was transported to a site near Mexico City, where the low latitude enabled the survey to reach as far south as declination –65° (again vying for time with Lowell's Mars work). The results, which included the discovery of 600 new doubles and the re-measurement of 1400 previously recognized by Herschel, were catalogued in the March 1898 issue of the *Astronomical journal*.[7]

Although this work was generally well-received, there were some astronomers who began to accuse See privately of carelessness as an observer.[8] In addition, they felt that the young man's overconfidence frequently led him to mischaracterize

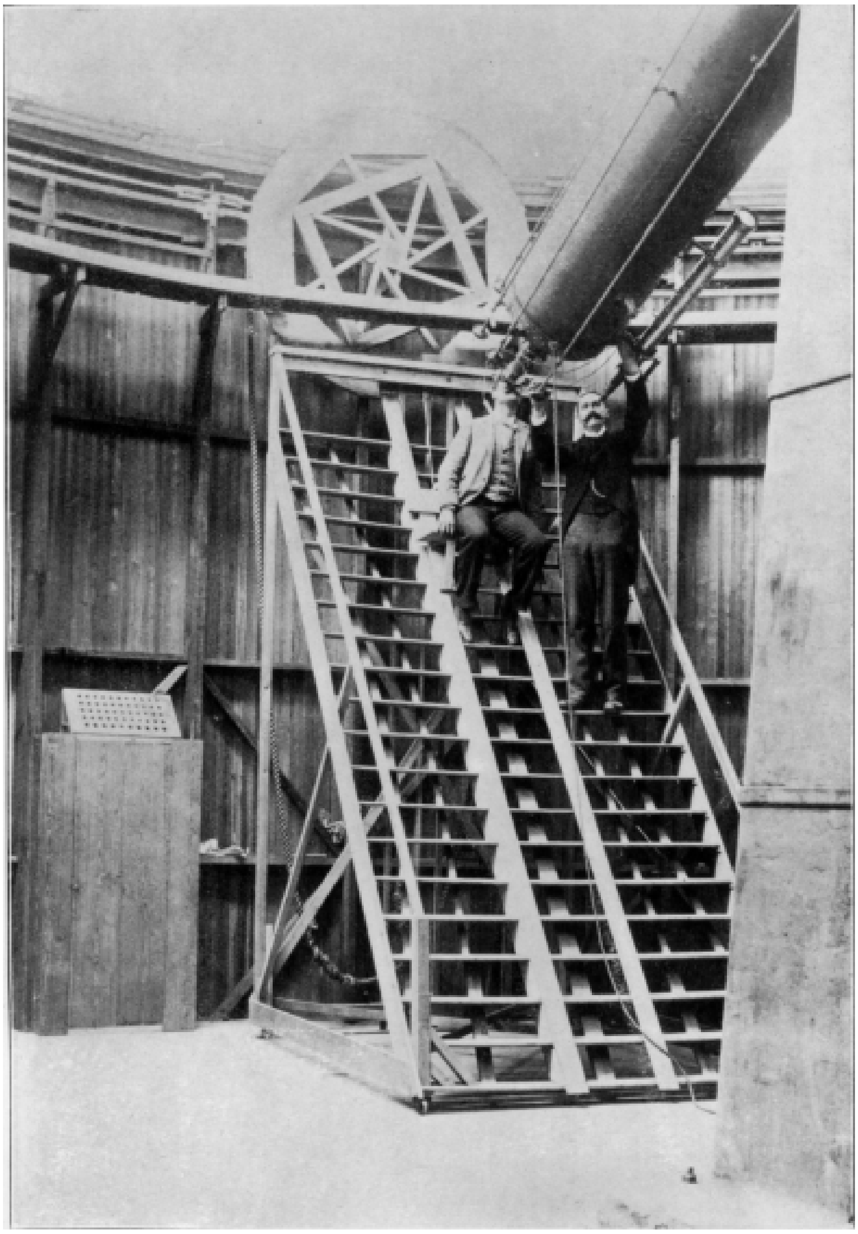


FIG. 2. T. J. J. See (right) and his assistant, W. G. Cogshall, sweeping for double stars with the Lowell Observatory's 24-inch Clark refractor in Mexico City during the winter of 1896–97. Their positions were normally reversed, with See at the main telescope's eyepiece and his assistant recording the micrometer measurements and sketching the finder's field. From Webb's *Brief biography* (ref. 1), plate following p. 66.

results, and sometimes to engage in rank speculation in his writings. For example, while reducing the forty binary orbits at Chicago he had written about one of them in an October 1895 letter to the *Astronomical journal*:

> Since August 20, when I first announced to you the existence of peculiar anomalies in the motion of the companion of [70 Ophiuchi], I have succeeded in showing conclusively that the system is perturbed by an unseen body.... I find that the dark body has a period of approximately forty years.... The sudden deviation of the companion from Schur's ephemeris proves the existence of the dark body here assumed.[9]

Elsewhere, writing of his Lowell double star survey, he claimed that

> Among these obscure objects about a half a dozen are truly wonderful, in that they seem to be dark, almost black in color, and apparently are shining by a dull reflecting light. It is unlikely that they will prove to be self-luminous.[10]

See stopped just short of declaring these dark bodies to be "... the first case of planets ... noticed among the fixed stars".

In October 1897 See published his first article for a non-scientific national magazine, *Atlantic monthly* — a nine-page dissertation highlighting his work on double stars.[10] Apparently he wanted to ensure that his accomplishments received due notice in the world at large, as well as in the scientific community.

It had been intended to extend the Lowell survey even farther south by temporarily relocating the 24-inch refractor in Peru, but Percival Lowell suffered an unexpected nervous breakdown in 1898, so that most of his observatory projects had to be suspended for over two years. In February 1899, on the recommendation of the Secretary of the Navy, President William McKinley appointed See to a professorship of mathematics in the Navy, on assignment to the U.S. Naval Observatory in Washington D.C. In fairly short order he was put in charge of the Observatory's biggest telescope, the 26-inch Clark refractor.[11] This instrument was as excellent as the Lowell 24-inch, although its achievements were limited by the unsteady skies of Washington, which could seldom match the seeing at Flagstaff.

Thus, in 1899 the 33-year-old See had one of the most promising futures of any young astronomer in the country.

The Dark Star

A comedown was in store. See had followed up his 1895 letter to the *Astronomical journal* on the orbit of 70 Ophiuchi with a paper analysing the apparent systematic departures of the 88-year-period binary from the orbit given earlier by Schur and from one recalculated by See which included more recent observations. He confirmed that the most likely explanation was an unseen satellite of the companion, whose period he revised to approximately 36 years. He confidently stated that the nature of the residuals between observed and computed positions "... would be a necessary consequence of the orbital motion of the visible companion about the common center of gravity, and may be said to establish completely the reality of that phenomenon".[12]

In the 15 May 1899 issue of the *Astronomical journal* Forest Ray Moulton, who had been a graduate student of See's at the University of Chicago (and had not yet been awarded his Ph.D.), published a paper which showed that the postulated dark satellite could not exist. On the basis of the solution of the restricted three-body problem, Moulton proved that the orbit of such a satellite would be highly unstable.[13] He pointed out that Eric Doolittle, another former student who had performed most of the calculations for See's paper, had in the meantime calculated a new orbit for 70 Ophiuchi which represented the observations reasonably well without assuming the existence of a third body.[14]

See could have considered this disclosure just a temporary setback, as double star work is replete with examples of misidentifications of single stars or false conjectures of additional components.[15] However, he attempted to evade the charge of coming to an erroneous conclusion. "Those who will examine my original papers in *A. J.* 358, 363", he wrote to the journal in response, "will see that I foresaw from the first the difficulty of securing stability, and that while I assigned the unseen body to the companion, ... I never entertained any very decided view as to which star the dark body attended".[16]

The *Astronomical journal* did not print the bulk of See's letter, so it is unknown what additional claims he made on his behalf. But from their appended note it is clear that the *Journal*'s editors felt that See was obscuring the issues:

> Dr. See's remarks were transmitted to Mr. Moulton to afford him the opportunity, if he desired, to reply; but he declines, on perfectly correct and dignified grounds, to do so; his essential and sufficient reason being that the statements are not in accordance with the facts....
>
> The present is as fitting an opportunity as any to observe that heretofore Dr. See has been permitted, in the presentation of his views in this journal, the widest latitude that even a forced interpretation of the rules of catholicity would allow; but that hereafter he must not be surprised if these rules, whether as to soundness, pertinency, discreetness or propriety, are construed within what may appear to him unduly restricted limits.[17]

The *Journal*'s note, with its implicit threat to censor future contributions by See, was interpreted as banishing him from the publication. Such an outright act was almost unprecedented for a scientific journal, and represented a severe reverse for someone in the process of making a name for himself as an astronomer. See continued to use the *Astronomische Nachrichten* as a vehicle for publishing detailed technical papers (mostly in English), and *Popular astronomy* and other, wider-circulation magazines for more popular articles on his work. If the articles he wrote were sometimes self-serving, they were also widely read by a public anxious for information on the latest developments in science.

FIG. 3. Captain See in his office at the small U.S. Naval Observatory at Mare Island, California. He is shown examining the plans of William Herschel's 40-ft reflector. Working up to eighteen hours a day, in this room the indefatigable See wrote thousands of pages expanding upon his scientific theories. From Webb's *Brief biography* (ref. 1), third plate following p. 82.

With the Navy

See was able to take full advantage of the 26-inch refractor at the Naval Observatory, making micrometrical measurements of asteroids, faint satellites, and planetary diameters. Although he did not publicly state so at the time, he later claimed that he observed faint belts on Neptune and glimpsed crater-like markings on Mercury during this period.[18] He also participated in the international effort to redetermine the solar parallax by observing the asteroid Eros at its close approach to the Earth in 1901.[19]

However, See did not take well to the highly organized program of work at the Naval Observatory, and the tension and overwork he experienced there contributed to a "breakdown" in 1902.[20] See himself described symptoms of stomach trouble and insomnia, due to "a severe internal catarrhal condition approaching a mild form of appendicitis".[21] He was to suffer similar bouts off and on for several years.

Following a six-month leave of absence to recuperate, See was transferred to the U.S. Naval Academy at Annapolis, where he was an instructor in mathematics for one semester. He did not fully recover from his illness there, and so was transferred once more in November 1903, this time to be placed in charge of the Naval Observatory at Mare Island, near Vallejo, California. The more healthful west coast climate agreed with him, and he was to remain there for the rest of his life.

The Observatory, however, was little more than a chronometer and time station attached to the huge naval shipyard, and had no telescope larger than a 5-inch refractor. It became clear to See that if he was to make further discoveries, it would not be as an observational astronomer. Instead, he was to turn to theoretical work (Figure 3), with a view toward making the work the core of vol. ii of his *Researches on the evolution of the stellar systems.*

The work proceeded only slowly. See's Navy superiors at Mare Island, apparently pleased to have an astronomer of some stature at their little observatory, allowed him much leeway in what he chose to investigate. Working up to eighteen hours a day, he was often sidetracked when some area captured his interest — particularly if he thought his work on the subject would likewise capture *public* interest.

Thus, after the great San Francisco earthquake of 1906, See embarked on a two-year study to attempt to shed light on the origin of such events. He concluded that the main cause of earthquakes was leakage of the ocean floor. Coming into contact with lava beneath the sea bed, the leaking water generated steam, which expelled the lava landward to produce the upheaving of the Earth's crust during a coastal earthquake. See also alleged that many repetitions of this process over time had been responsible for the formation of mountain ranges. He expounded these views most thoroughly in hundreds of pages in the *Proceedings* of the American Philosophical Society of Philadelphia.[22]

Professional geologists in the main were not impressed by See's incursion into their field, and except for a few European investigators tended to ignore his work.

In a letter to *Science*, See proclaimed that “I have proved that mountains are formed by the sea, ... and ... that the oceans are gradually drying up and the land increasing”.[23] He furthermore stated that his arguments had been so convincing that “... geologists have discreetly kept silent”. In response, a noted Yale geologist, Joseph Barrell, wrote to *Science* that ocean leakage was an old idea, and that See’s work was merely

> ... a dressing out of this old and, to say the least, doubtful hypothesis with many speculative additions, with much repetition of well-known facts and theories, and with specific applications in such frequent discord with modern teaching of the principles of physiography and known details of geologic structure and history, that no geologist has felt called upon to comment.[24]

But See was not deterred by attacks, and continued to explore other fields while working on plans for his *Researches*. He travelled and lectured widely, and spent most summers at his childhood home in Missouri. On one such visit, in June 1907, he married Frances Graves, a physician’s daughter, in Montgomery City. See was 41. The couple were to have one surviving son, Ernest, although another son died in infancy.

The Capture Theory of Cosmical Evolution

When See returned to astronomical subjects, he took up the popular topic of solar system evolution. He had for years doubted the long-held eighteenth-century Laplacian hypothesis that the gravitationally contracting solar nebula had deposited gaseous rings at the orbital radii of the planets, and that these rings subsequently had condensed to form the planets themselves. Forest Ray Moulton had shown in 1900 that this idea was not consistent with the Sun’s current rotation rate, on the basis of the laws of conservation of angular momentum.[25] By the close of the first decade of this century, most astronomers had come to reject Laplace’s hypothesis, but there was no consensus in favour of a theory to replace it.

In 1904 Moulton and T. C. Chamberlin, a geologist at the University of Chicago, developed the *planetesimal hypothesis*, which proposed that nebulous matter surrounding the early Sun condensed into small solid bodies called planetesimals, which eventually aggregated through collisions to form the planets.[26] Although they claimed it was not required by the theory, they favoured as the origin of the solar nebula tidal disruption of the Sun by an encounter with a passing star. Along with many astronomers of the day, Moulton and Chamberlin came to believe that the stunning time exposure photographs of spiral nebulae coming out of Lick Observatory and other places represented direct evidence of forming solar systems. (The Shapley–Curtis debate touching on whether the spirals were nearby nebulae or distant galaxies was still sixteen years in the future.) It was thought that the spiral’s nucleus was condensing into a star like our Sun, while the knots and clumps of gaseous matter arrayed along the spiral arms were the infant planets and satellites.

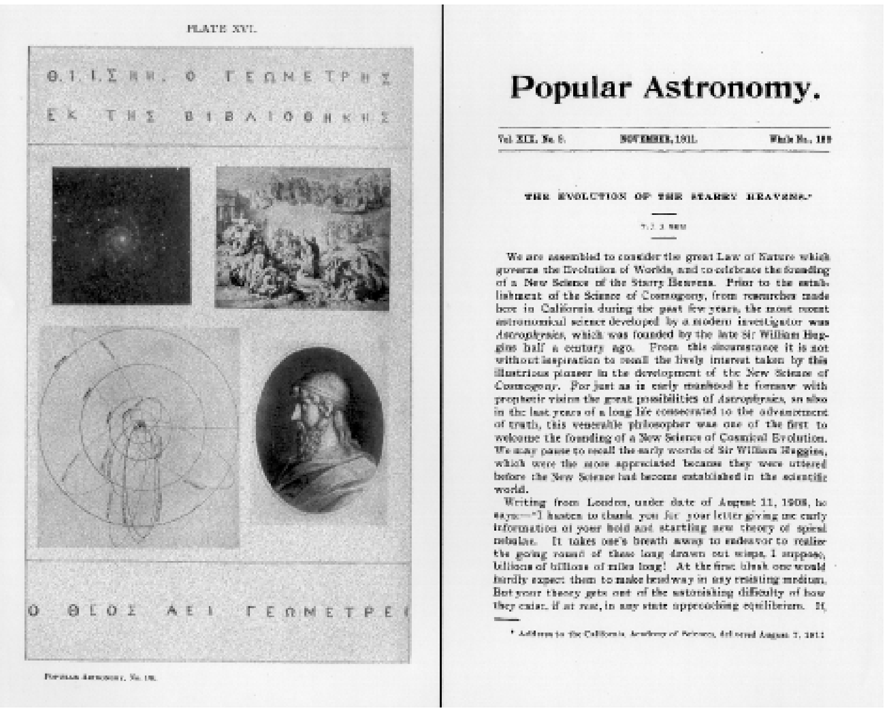
See at first strongly opposed this interpretation of the nebula photographs, writing in a 1906 *Popular astronomy* article with characteristic assertiveness: “The speculations on spiral nebulae have been decidedly overdone, and it is time to call a halt. There is not the slightest probability that our solar system was ever a part of a spiral nebula, and such a suggestion is simply misleading and mischievous.”[27]

But See changed his mind, apparently feeling that he needed the evidence of the suggestive photographs to make a case for his own developing theory. He was to call his picture of solar system formation the *capture theory* after its central tenet: that instead of being separated off from the Sun the planets had been captured gravitationally from where they had formed farther out in the solar nebula. Their initially eccentric orbits had been reduced in size and circularized by the resisting action of the nebular gas surrounding the Sun. In a similar way, the planetary satellites originally rotated about the Sun but were captured by the larger planets.[28]

See developed these ideas, some of which originated with earlier investigators, over a period of several years (Figure 4), and planned to showcase them in his follow-up volume of the *Researches*. He decided to give a preview of this portion of the work at a January 1909 meeting of the Astronomical Society of the Pacific at Chabot Observatory in Oakland, California. However, he developed full-blown appendicitis about three weeks prior to the meeting, and lay in the hospital for sixteen days before his doctors felt it was safe to operate.

Thus, T. J. J. See was not even present at the event he was to later characterize as one of his life’s greatest triumphs. His paper was read before the open meeting by Russell T. Crawford of the University of California at Berkeley, the Society’s secretary. The front page of one section of the Sunday *San Francisco Call* on the following day declared “Prof. See’s Paper Creates Sensation at Meeting of Astronomical Society”,[29] while an article in the *San Francisco Examiner* was headlined “Scientists in Furore over Nebulae”.[30] Over the next few days the opinions of astronomers who were present at the meeting, as well as those of others who were not, were solicited by the San Francisco newspapers. Charles Burckhalter of Chabot Observatory was quoted as saying, “Professor See’s theory seems to me more reasonable than any other that has yet been advanced.... I believe that See has not solved the whole mystery of the universe, but he is enthusiastic in his discovery, which is a great one.”[31] Crawford himself thought See’s theory was plausible, but was basically undecided “until I am able to give a closer examination to the facts that Professor See has compiled”.[32] John Brashear, the well-known astronomical instrument maker, issued a signed statement from Pittsburgh that he could not tell from newspaper accounts exactly what See was proposing, but that “there is little doubt Dr. See’s paper ... should be given much weight, because he is a man whose researches and mathematical studies entitle him to a hearing among his scientific colleagues”.[33]

Interviewed in his sick bed at Mare Island’s naval hospital, See asserted with little modesty, “I am fairly convinced that I have solved the problem and that no astronomer in the future will be able to disturb the chain of reasoning and



Popular Astronomy.

Vol. XIX, No. 9. NOVEMBER, 1911. Whole No. 189

THE EVOLUTION OF THE STARRY HEAVENS.*

T. J. J. SEE

We are assembled to consider the great Law of Nature which governs the Evolution of Worlds, and to celebrate the founding of a New Science of the Starry Heavens. Prior to the establishment of the Science of Cosmogony, from researches made here in California during the past few years, the most recent astronomical science developed by a modern investigator was *Astrophysics*, which was founded by the late Sir William Huggins half a century ago. From this circumstance it is not without inspiration to recall the lively interest taken by this illustrious pioneer in the development of the New Science of *Cosmogony*. For just as in early manhood he foresaw with prophetic vision the great possibilities of *Astrophysics*, so also in the last years of a long life consecrated to the advancement of truth, this venerable philosopher was one of the first to welcome the founding of a New Science of Cosmical Evolution. We may pause to recall the early words of Sir William Huggins, which were the more appreciated because they were uttered before the New Science had become established in the scientific world.

Writing from London, under date of August 11, 1908, he says:—"I hasten to thank you for your letter giving me early information of your bold and startling new theory of spiral nebulae. It takes one's breath away to endeavor to realize the going round of these long drawn out wisps, I suppose, billions of billions of miles long! At the first blush one would hardly expect them to make headway in any resisting medium. But your theory gets out of the astonishing difficulty of how they exist, if at rest, in any state approaching equilibrium. If,

---

* Address to the California Academy of Sciences, delivered August 7, 1911.

FIG. 4. One of the sixty or so articles in *Popular astronomy* by which See established his firm reputation with the public. The frontispiece is a copy of a bookplate presented to him by California admirers, and depicts (clockwise from upper left): M74; Kaulbach's painting of Homer and the Greeks; Plato; and the solar system. The Greek caption at the top reads: "T. J. J. See, the geometer outside the library", while that at the foot quotes the Platonic aphorism, "The Deity always geometrizes". From *Popular astronomy*, xix (1911), 528–9.

mathematics that I have worked out".[30] A five-page paper summarizing his theory appeared in the *Astronomische Nachrichten* on 24 February 1909, but it contained no such mathematical chain.[34] This must have been a disappointment to those in the astronomical community who seemed willing to listen, but it appears to have mattered little to See. Rather, he seemed to relish the attention he was getting, whether his peers embraced his hypotheses or not.

While his summary papers were at least serious and reasoned, See apparently could not resist a desire to be recognized as a prophet: in a postscript submitted to the *Astronomische Nachrichten* nine days after his summary paper, he predicted "... that there is certainly one, most likely two, and probably three unknown planets beyond Neptune".[35] On the basis of unpublished work he had done in 1904, he placed the nearest of the new bodies at a radius of 42.25 AU from the Sun, and the others at 56 and 72 AU. He recommended a photographic search of a specific region of the ecliptic for the first body, which he tentatively named "Oceanus". See never revealed how he arrived at his predictions, but it seems most likely that he applied some graphical technique to observed perturbations of Neptune. There is no evidence that anyone seriously sought his postulated new planets.

In addition to his local exposure, See sought a wider audience for his latest ideas. In a letter of 28 May 1909 to *Science* he attacked "... the inconsistent and purely destructive criticisms recently put forth at Chicago by Chamberlin and Moulton.... Most of the recent speculations on cosmogony are not worth the paper they are written on; and yet some of them have been published by the *Astrophysical journal* and the Carnegie Institution." He went on to promote his own competing theory:

> It is only fair to say that no constructive results of consistent character had been reached on this subject till my own investigation was completed last year, of which an account is given in Astronomische Nachrichten.... As I have worked on this subject uninterruptedly for twenty-five years, I am prepared to speak with some degree of authority.[23]

Forest Ray Moulton had been silent during the earlier debate over See's theory, but the *Science* letter represented direct and personal criticism. In a response published in the 23 July issue of *Science*, he accused See of "extravagant pretensions" regarding his (See's) background in cosmogony (as cosmology was then called), pointing out that See's "alleged twenty-five years of uninterrupted work" in the field had apparently resulted in only two published papers. He also castigated See's claim of consistency, contrasting his 1906 claim that there was "not the slightest probability that our solar system was ever part of a spiral nebula" with his latest paper's statement that "The solar system was formed from a spiral nebula". In fact, Moulton averred that

> In See's paper there are only two points of divergence from the ideas fully developed by Professor Chamberlin and myself. The first is that spiral nebulas have their origin in "the meeting of two or more streams of cosmical dust". The second is that satellites are captured bodies.[36]

Moulton strongly implied that See had deliberately adapted portions of the planetesimal hypothesis to his own purposes without giving credit to its authors.

Moulton's accusations were printed on the front page of the *San Francisco Call*, under the headline "Astronomers Warring — Moulton Exposes See" and the subheading "Chicago Professor Says Mare Island Observer Stole Discarded Theories".[37] See responded the following day that the Chicago professors were "exercised" because See's researches had been accepted by the scientific world and had completely upset several years' worth of their work.[38] He also maintained in a letter of reply to *Science* that "I have since developed a rigorous proof ... of just how the capture of satellites comes about".[39] The proof was not forthcoming, and Moulton's conflict with See continued to smoulder for nearly three more years.

Vol. ii of See's *Researches*, subtitled *The capture theory of cosmical evolution*, appeared late in 1910, running to some 735 oversize quarto pages. Beautifully printed (again at his own expense) and profusely illustrated, it presented a review of many topics in celestial mechanics and drew together much of See's work since vol. i, but it also contained much that was new. In See's world view, most of the phenomena of nature could be explained by suitable application of his capture theory and attendant hypotheses. Besides his treatment of solar system evolution, some of his key points were as follows:[40]

The Moon's craters were caused by the impact of other, smaller satellites of the solar system, rather than by volcanic action.

The Earth and other planets once had craters like the Moon's, but these have since been obliterated by water and atmospheric erosion.

Direct (west to east) planetary rotation resulted from the cumulative effect of collisions with smaller bodies, as did the tendency of a planet's equatorial plane to coincide with its orbital plane about the Sun.

Comets originate in the outer reaches of the remnant solar nebula, and periodic comets are those that have been gathered in by the gravitational action of Jupiter.

In addition to gravitational attraction, repulsive forces are at work in the universe, in the form of gas and dust continuously being expelled from stars.

The matter expelled from stars tends to aggregate in vacant regions toward the poles of the Milky Way, which explains why spiral nebulae are more numerous in these directions.

After stars form out of the spiral nebulae, they eventually drift back into the plane of the Milky Way.

Diffuse nebulosity results when the gas expelled from stars is too tenuous to condense into more compact form.

Collisions of smaller bodies are the cause of most of the light of nebulae.

All single stars have planetary systems revolving about them.

Double and multiple stars are formed when a large spiral nebula divides into two or more parts, which condense separately.

Star clusters evolve from incredibly vast nebulae, and from the gathering in of neighbouring stars under the clustering power of gravitation.

Variable stars occur when the orbits of attending dark companions cause them to eclipse the light of the primary star, or occasionally when a companion gives off light due to nebular resistance encountered when closest to the primary.

Novae result from the conflagration produced when a planetary body collides with its central star.

The Milky Way is much more extensive than previously thought; extinction of light by cosmic dust is so great that starlight from the farthest distances is cut off.

Life is a general phenomenon of the physical universe, and almost as universal as matter itself.

Some of See's ideas in the book were quite original and even prescient of more modern theories, but many more were speculations presented with little justification, and others were borrowed from his contemporaries. Still, he could fairly claim that he had unified many of these concepts for the first time.

The astronomical community's reaction to See's tome was reasonably polite, although some continued to be disappointed by the lack of sufficient evidence for many of the author's conjectures. His publisher issued a circular quoting about two dozen scientists' comments on the work, some of which must be taken with a grain of salt (since they were undoubtedly solicited in exchange for a free copy). A few were possibly left-handed compliments, such as that of the celestial mechanics specialist, E. W. Brown of Yale: "The beautiful printing and magnificent illustrations are a very unusual feature, and make the book a welcome addition to any library, quite apart from the contents."[41]

If some astronomers were equivocal in expressing their support, See himself apparently had no qualms about claiming it. To *Popular astronomy* and elsewhere he submitted an article asserting that E. W. Brown had verified his ideas of the capture of satellites,[42] and in other articles he stated that the French mathematician Henri Poincaré had adopted some material from See's book into a course of lectures in Paris.[43]

Forest Ray Moulton, however, was in no way equivocal in his reaction to See's book. In an article in the February 1912 *Popular astronomy* entitled "Capture theory and capture practice", Moulton demonstrated that an important section of the work had been "captured" from Moulton's 1902 book *Introduction to celestial mechanics* without credit.[44] The article reprinted three pages of See's book and the corresponding parts of Moulton's text face-to-face to show that except for changes of notation the equations involved were identical. The failure to give specific credit might have been attributed to oversight — the chapter was after all a review of the three-body problem and not claimed to be original, and See had referenced Moulton's book in the second chapter previous (his only such mention of it in the *Researches*) — but Moulton also asserted that See's discussion of the equations showed a serious lack of understanding of the fundamentals of celestial mechanics. Moulton claimed that equations were misused, illustrations misdrawn, and important concepts played havoc with. The critique made numerous sarcastic references to See's immodest claims on behalf of the capture theory, especially the claims that Brown and Poincaré supported it, emphasizing that "these astronomers have not announced in their publications that they have taken such a position". As in some of his earlier attacks, Moulton appeared galled by the fact that See was garnering wide attention for a theory which he had developed largely from the efforts of others, without having mathematically derived a significant number of his own results. Moulton expressed contempt for "those who make books with shears".

See never again wrote for *Popular astronomy* (except for an obituary piece on a close astronomer friend in 1920[45]), although he had previously written some sixty articles and letters over twenty years. It does not seem likely that the magazine's editors "banished" him as had the editors of the *Astronomical journal*, for he was a popular contributor and the Moulton affair was just one black mark. Rather, See was probably incensed and hurt that the editors would print such a scathing and personal attack upon him.

A Growing Dichotomy

This crushing blow to his self-esteem served only to widen the gap between T. J. J. See's position within the professional astronomy community and his stature as perceived by the public at large. On the one hand, except for a modest core of supporters, astronomers remained cool toward the *Researches*; the work was seldom cited in professional papers. The public, on the other hand, was largely unaware of Moulton's highly technical critique, and knew little of See's humiliation within the scientific world. They saw his theories discussed in the *New York Times* and national magazines, as well as presented as the subject of editorials. In an era of expanding scientific discovery, scientists who could expound impressively on their ideas were held up to public adulation, and See was a grand example. In his naval captain's uniform (he was commissioned an officer in 1913), the six-foot four-inch, athletically built professor must have been an imposing figure on the lecture platform. At the time the public lecture was both a popular form of entertainment and an educational experience, and a riveting speaker could transfix an audience with the help of a lantern slide projector and never-before-seen astronomical photographs. If the speaker was not quite accepted by the scientific establishment, his listeners could probably convince themselves that they were witnessing the revelation of a startling new view of the cosmos.

In 1913 a longtime admirer and amateur astronomer from Independence, Missouri, William Larkin Webb, published the 300-page *Brief biography and popular account of the unparalleled discoveries of T. J. J. See.* The introduction declares:

> Professor See is universally recognized as the most intrepid and indefatigable of the explorers of Nature; and since the death of Poincaré and Sir George Darwin, in 1912, occupies easily the front place among living natural philosophers.[46]

Chapter after chapter cites See's numerous accomplishments, leading up to "the triumph of the Capture Theory". Of the acceptance of the latter the book is blithely optimistic:

> Considering the extremely revolutionary character of See's discoveries, it must be held that they have had a very favorable reception from the scientific world.... [As] time has elapsed it is noticed that acceptance of the results is general, and that acquiescence in See's conclusions becomes more and more universal.[47]

Considering the sometimes fawning prose in the biography, most reviews of the book were not overly unkind, although *The nation*'s review noted that it abounded in "parlous surfeit of superlatives". Less than one-half of the book actually describes events in See's life (the remainder reprints several of his papers and lectures), and *The nation* took the opportunity to poke fun at the hyperbole in the scientist's portrayal:

> The infant See, we are told, first saw the light on the 393d anniversary of Copernicus's birth, ... [and] showed himself "every inch a natural philosopher" by speculating on the origins of sun, moon, and stars at the tender age of two, never so much as dreaming that he should grow into a "little boy with methodical methods", and one day become "the greatest astronomer in the world".[48]

With considerable insight, the reviewer presented a more balanced view of the scientific world's actual reception of See's theories:

> During the past twelve years he [See] has pursued researches in universe building to which has not as yet been accorded that full acceptance which their author and his biographer seem to believe has been the case.... So far his revolutionary theories seem only in small part acceptable to his scientific contemporaries.

New Theory of the Aether

While See's astronomical views were at least treated with a modicum of respect, his ensuing researches into more fundamental physics provoked reactions approaching scorn. In April 1914 he announced to the press that he had discovered the cause of gravitation, thereby answering a question that had puzzled scientists since the days of Isaac Newton. He claimed that gravity results from particles being expelled at the speed of light on electrical streams from the millions of stars in the universe. The attractive force between two bodies is really only the apparent result of each body screening the other from the bombardment of these particles from all directions; since each body experiences fewer collisions from the direction of the other, they are forced together.[49]

Like many of See's earlier ideas, this one was essentially derivative, from hypotheses dating back two centuries. Natural philosophers in the time of Newton had developed the basic concept, but the scheme was most clearly spelled out in the "theory of ultramundane corpuscles" of George Louis Le Sage, published in 1818.[50]

See's alleged discovery was a secondary result of a discovery of the nature of light, which he announced at the same time. This hypothesis stated that light consists of egg-shaped particles of matter rotating about their shorter axes and bearing an electrical charge on the sharper ends. See claimed to have proved his theory, and overthrown the theory that light consists of waves transmitted through the ether.

Mainstream astronomers were quick to pronounce judgement on See's new theories. The *San Francisco Chronicle* submitted a summary of them to the University of California at Berkeley, where a committee of scientists that included the Lick

Observatory director, W. W. Campbell, met and issued a statement that “The whole thing is an unsubstantiated theory and as such cannot be dealt with by scientists. Until he shows proofs there can be no discussion. The scientific world will ignore the theory as it now stands.”[51] See sent a 600-page account of his theory to the Royal Society of London in November 1914, but it was disregarded. In 1917 he privately published a revised version, which also received little serious attention.[52]

Between 1920 and 1926 the ever-indulgent *Astronomische Nachrichten* finally published See’s re-revised theory as the “New theory of the aether” (which he apparently decided did exist after all), a series of eight papers comprising some 300 pages.[53] The theory asserted that gravity, electricity, and magnetism were all due to ether waves propagating at the speed of light. The ether was an extremely rarefied but enormously elastic gas, consisting of tiny particles called etherons, each one-billionth the size of an electron and travelling at a velocity 57% faster than light. Now, instead of his 1914 “screening” explanation, the attractive gravitational force between two bodies was explained in terms of helical electrodynamic waves emitted by each, as many rotating in a right-handed sense as in a left-handed sense. When the waves from one body encountered oppositely-rotating waves from the other, their interpenetration acted to undo the stress in the ether, so that the ether contracted and drew the bodies together.

In See’s theory, virtually all of the phenomena of the universe — light, atomic forces, radioactivity, molecular forces, explosive forces, sunspots, lightning, auroras, magnetic storms, earth currents, etc., etc. — could be addressed in terms of waves in the ether. Thus, his was a “theory of everything” which could be called upon to explain physical happenings from the cosmic to the mundane.

Although See wrote hundreds of pages on these subjects, there was a serious lack of the substantive kind of proofs that the Berkeley committee had asked for. There were numerous historical anecdotes dating back to Laplace, Newton, or even the ancient Greeks; there were dozens of equations from Maxwell, Lord Kelvin, or Poisson, some of them irrelevant to the subject under discussion; there were trivial, hard-to-follow, or spurious arguments connecting the equations; but little was proved or even made to seem plausible. As an example of ‘non-sequitur’ reasoning, See deduced the velocity of etherons from a relationship he had worked out between the mean molecular velocity v and the velocity V of sound wave propagation in monatomic gases,

$$v = \pi/2V,$$

but he failed to justify why particles one-billionth the size of an electron should behave like a monatomic gas, or why electromagnetic waves should behave analogously to sound waves.[54]

In the last of the eight new papers See calculated the probability that his wave theory was a correct interpretation of nature to be as infinity to the 200th power to one ($\infty^{200} : 1$)![55] To his contemporaries the reasoning in the papers must have seemed to be only so much smoke and mirrors. As the Berkeley group had foretold, his new theory was not taken seriously, and went virtually undiscussed elsewhere in the scientific literature. (It may be speculated that the only reason the *Astronomische Nachrichten* continued to publish his papers is that its longtime editor, Hermann Kobold, was an old friend of See's.) Only in the popular press did his case for etherons get a hearing (Figure 5).[56]

Adversarial Physics

During this same period See began attacking Albert Einstein's theory of relativity, which he considered a "crazy vagary". Although many scientists of the day rejected relativity, See was especially vitriolic in his criticism. As most of the scientific world was applauding the confirmation of the theory's prediction for the bending of light rays passing near the Sun, based on solar eclipse observations in 1919 and 1922, See accused Einstein of having plagiarized the formula for this effect from the 1801 result of J. von Soldner, a German physicist, who had used Newton's corpuscular theory of light in his derivation.[57] See made much of the fact that Einstein had at first calculated a value for the magnitude of the deflection exactly one-half of his theory's final value, and accused Einstein of revising the theory in order to hide the error.[58]

Later on See claimed that Einstein had made more than eighty errors in his basic calculations on relativity theory. See entered into numerous debates on the theory with other scientists in articles and letters in the *New York Times* and elsewhere, and never missed an opportunity to attack Einstein or promote his own ether wave theory. The Cambridge astronomer Arthur S. Eddington branded See's criticism "all bosh and nothing to it",[59] but Einstein himself avoided the fracas. Einstein's only recorded comment regarding See came after his wife relayed a telegram to him in Holland containing the text of See's October 1924 claims to have demolished relativity: "Too bad about that long telegram from New York."[60]

See also rejected Edwin Hubble's idea of an expanding universe almost from the time it was first proposed in 1929. See contended that the observed increase in the red shift of extragalactic nebula spectral lines with distance was due not to motion away from the Milky Way galaxy but to physical changes of the light waves themselves. As interpreted by his theory, light waves lost energy in collisions with cosmic dust, and this loss produced a wavelength increase in proportion to the nebula's distance.[61]

See's many controversial ideas attracted substantial public attention and put him at odds with other scientists time and again. During this period he advanced a theory that sunspots were caused by meteors raining down upon the Sun's surface, directed by the gravitational influence of Jupiter and Saturn. He deduced that the eleven-year sunspot cycle period was a combination of the orbital periods of the two giant planets. He received considerable press coverage from his declarations that sunspots were responsible for climatic cycles of flood and drought on Earth.[62]

See also announced his interpretation of several intercontinental radio signal experiments which made news at the time. He claimed that radio waves travelled about 11% slower than light, bending around the Earth's surface during long-distance transmissions.[63] He also considered that radio waves travelled preferentially over the night

THE SAN FRANCISCO EXAMINER: SUNDAY, MARCH 11, 1928 K-5

HAS MYSTERY OF ETHER AT LAST BEEN SOLVED BY SCIENCE?

"I Have Calculated Its Density and Measured It sVelocity," Says Dr. See

Navy Astronomer Says That Ether Fills All Space and Calls It a "World Gas"

By GOBIND BEHARI LAL

Gravitation and Lightning Among Miracles Attributed to Etherons by Scientist

FIG. 5. A feature article on See's ether wave theory in the Sunday edition of the *San Francisco Examiner*, 11 March 1928, p. K-5. By this time See had largely stopped writing popular articles on his astronomical work, as there were numerous journalists only too willing to do this for him.

hemisphere rather than the day hemisphere because the ether is quieter at night.[64]

See retired from the Navy at age 64 in 1930, but he remained at Mare Island, now free to work on his theories full time. As he became increasingly strident in expressing eccentric views, scientific publications shunned his papers. Following the death of Hermann Kobold, even the *Astronomische Nachrichten* published no articles by him after 1938.

As the new physics gained acceptance by the scientific world and eventually by the public in the 1930s, See's views began to fall out of popular favour. Many scientists were especially relieved when controversy over relativity died down. Gradually, even the press stopped seeking out See's opinions on such topics.

See was to write eleven more volumes expanding on his ether wave theory. Issued under the collective title *Wave-theory!* between 1938 and 1952, these volumes were published as rotographic prints of typewritten copy, with equations written in and illustrations (Figure 6) drawn by hand.[65] In the preface to the first volume See compared his struggles to get the definitive version of his theory published to the Royal Society of London's hedging on the publication of Newton's *Principia*, which Halley eventually printed at his own expense.

Wave-theory! re-covered much of the ground gone over in the eight *Astronomische Nachrichten* papers, then went on to extend See's ideas to further physical phenomena and to scientific discoveries made since 1926. The arcane illustrations, most drawn by the author himself, bore more of a resemblance to those seen in metaphysical treatises of the period than to those in scientific works. Perhaps in an effort to appear modern, See "proved" in vol. v that the developing quantum theory was really just a branch of his wave theory, and "derived" the value of Planck's constant from his theory's basic principles. Never reluctant to go out on a limb, in vol. ix he calculated the age of the solar system as at least 10.44 trillion years, 3000 times the age estimated by other astronomers at the time.

Conclusion

But by the time of the last *Wave-theory!* volume See was 86 years old, and most of the scientific community had long ceased to take him seriously. For over thirty years few scientists bothered to criticize his work in a public forum, as if to do so would dignify it. See was debated mainly when he publicly attacked other scientists such as Einstein, and in their responses others would seldom mention See's own new theories.

An unhappy aspect of See's becoming virtually a pariah to the scientific world is the deterrent that this might present to later investigators with unorthodox or otherwise unpopular views. Fortunately, most scientists are willing to grant a degree of respect to those with opposing opinions, as long as these opinions have some rational basis. They are all too aware that today's unpopular theory can become tomorrow's fundamental truth. In addition, there is no permanent stigma attached to being wrong on some issue: Albert Einstein himself could not accept some of the basic principles of quantum mechanics, as evidenced by his famous quote, "God does not play

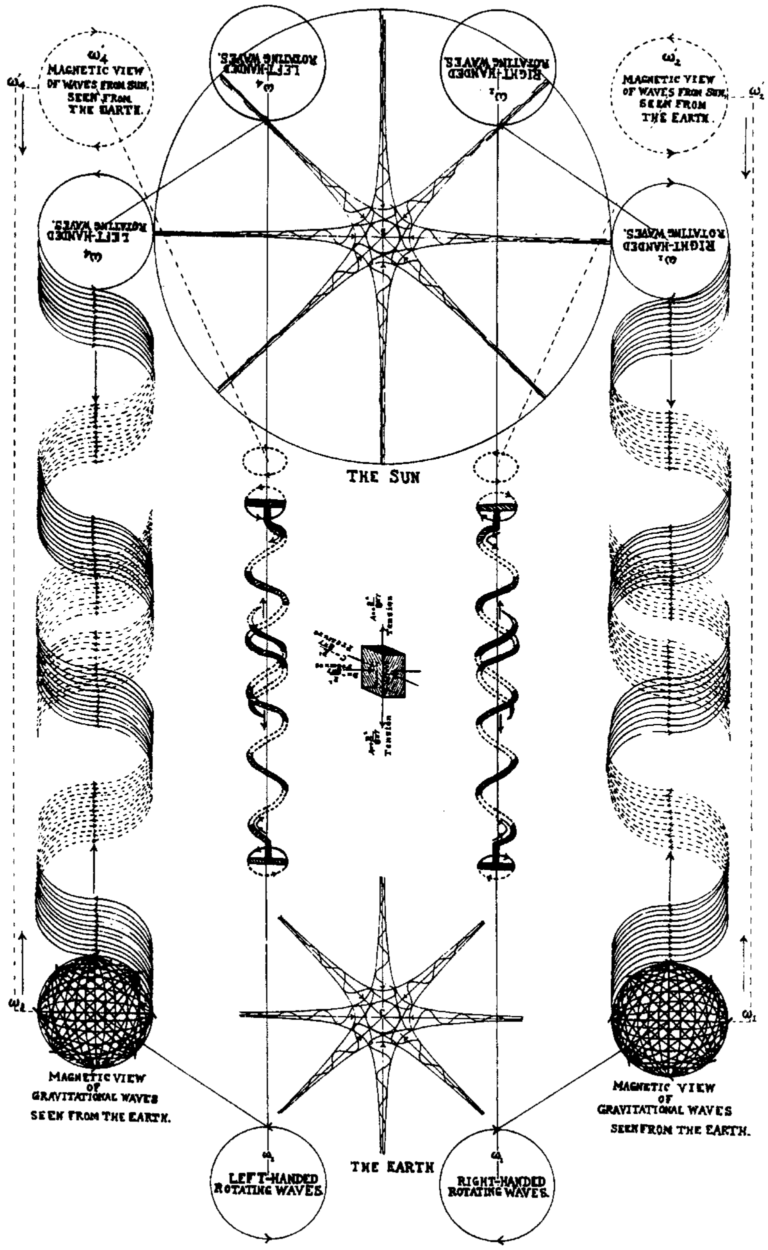


FIG. 6. Diagram illustrating See's theory of gravitation, from p. 27 of vol. i (1938) of his *Wave theory!* He saw the attraction between the Sun and Earth as arising from helical electrodynamic waves emitted by each. Interpenetration of waves rotating in opposite directions acts to undo the stress in the ether, so that the ether contracts and draws the bodies together.

dice". See became anathema because he insisted on taking his case to the public at large when he was unable to convince his peers. It was also considered questionable in an age of growing specialization to venture outside of one's field of training — as See did into geology, climatology, and numerous branches of physics.

Nevertheless, See's excesses should not be allowed completely to overshadow his accomplishments. His early double star work was in large part sound, and his observational skills were generally to be trusted. He was basically on the right track in promoting — although in many cases he did not originate — the ideas that the solar nebula was initially very cold, that stars constantly expel gaseous matter, that cosmic dust gives rise to extinction of distant starlight, that comets originate in the remnant outer regions of the solar nebula, and that the Moon's surface gives evidence of bombardment by small planetoids. In addition, capture phenomena, the central feature of See's ill-starred theory of solar system evolution, are today held by many astronomers to be responsible for a large proportion of planetary satellites (as well as for events such as the crash of Comet Shoemaker-Levy 9 into Jupiter in 1994). Finally, See deserves credit for pressing British scientific societies to publish Sir William Herschel's collected works; this resulted in the release of a beautiful two-volume edition in 1912.[66]

See died at Oak Knoll Naval Hospital in Oakland, California, on 4 July 1962, at the age of 96. Although he merited only a four-line obituary in *Science*,[67] the *New York Times* saw fit to print almost an entire column of biographical material.[68] It is sad that a promising astronomical career was sacrificed to See's need for public attention. Other than the controversy that he generated during his heyday, little is remembered today of his astronomical work. But it cannot be denied that he played a part in an exciting time for American science, when the man on the street was beginning to develop a real passion for the subject, and to assign hero status to some of its practitioners.

History has taken a divided view on Thomas Jefferson Jackson See. Much the majority opinion is expressed by the *Encyclopedia Americana*: "Although he had an unremarkable career and made no important contributions to science, See is remembered for his numerous controversial papers on astronomical subjects and his unfailing knack for espousing the discredited side of scientific theories."[69] Nonetheless, a revisionistic dissent is registered by the *Dictionary of scientific biography:* "See's numerous publications were considered unorthodox and were dismissed by scientists of his time. Many of his ideas, however, are in striking agreement with current theories."[70] While it is desirable that See's career be judged dispassionately, it is to be hoped that one day his more eccentric theories are not 'rediscovered' and made a part of the pseudo-science revolution!

REFERENCES

1. W. L. Webb, *Brief biography and popular account of the unparalleled discoveries of T. J. J. See* (Lynn, Mass., 1913).
2. See, for example, See's articles in *Astronomical journal*, xv (1895), 33, 54, 97, 101, 129, 155, 156, 161, 169, 171, 180, 188; xvi (1896), 17, 24, 41, 92, 125, 137; and in *Astronomische Nachrichten*, cxxxvii (1895), cols 159, 359; cxxxviii (1895), cols 25, 55, 149, 345, 369, 373; cxxxix (1896), cols 17, 63, 161, 163, 169, 341, 367; cxl (1896), cols 33, 35, 39; cxli (1896), col 7 (only first pages/columns here listed).
3. See, for example, See's articles in *Popular astronomy*, i (1893), 373; ii (1894), 103, 249, 289, 337, 385; iii (1895) 49, 178; iv (1896), 297, 533, 550 (only first pages here listed).
4. Webb, *op. cit.* (ref. 1), 59.
5. T. J. J. See, *Researches on the evolution of the stellar systems*: *Volume 1* (Lynn, Mass., 1896).
6. J. Ashbrook, "The sage of Mare Island", *Sky and telescope*, xxiv (1962), 193.
7. T. J. J. See, "Discoveries and measures of double and multiple stars in the Southern Hemisphere", *Astronomical journal*, xviii (1898), 181–7.
8. Ashbrook, *op. cit.* (ref. 6), states for example that W. H. van den Bos had pointed out that all of See's binary position angles for about a month had the same large systematic error, apparently due to a failure to check the micrometer's zero point each night.
9. T. J. J. See, "Perturbations in the motion of the double star F.70 Ophiuchi = Σ2272", *Astronomical journal*, xv (1895), 180.
10. T. J. J. See, "Recent discoveries respecting the origin of the universe", *Atlantic*, lxxx (1897), 484–92.
11. Ashbrook, *op. cit.* (ref. 6).
12. T. J. J. See, "Researches on the orbit of F.70 Ophiuchi, and on a periodic perturbation in the motion of the system arising from the action of an unseen body", *Astronomical journal*, xvi (1896), 17–23.
13. F. R. Moulton, "The limits of temporary stability of satellite motion, with an application to the question of the existence of an unseen body in the binary system F.70 Ophiuchi", *Astronomical journal*, xx (1899), 33–37.
14. E. Doolittle, "The orbit of F.70 Ophiuchi", *Astronomical journal*, xvii (1897), 121–2.
15. Subsequently, 6.5-year and 17-year perturbations were postulated by others for 70 Ophiuchi, but today these suggestions are discounted.
16. T. J. J. See, "Remarks on Mr. Moulton's paper", *Astronomical journal*, xx (1899), 56.
17. Editor's Note, *Astronomical journal*, xx (1899), 56.
18. Webb, *op. cit.* (ref. 1), 72, and the plate following p. 74.
19. Webb, *op. cit.* (ref. 1), 76.
20. Ashbrook, *op. cit.* (ref. 6).
21. Webb, *op. cit.* (ref. 1), 78.
22. T. J. J. See, articles in *Proceedings of the American Philosophical Society (Philadelphia)*, xlv (1906), 222–87; xlvi (1907), 191–299, 369–415; xlvii (1908), 157–275.
23. T. J. J. See, "Fair play and toleration in science", letter to editor of *Science*, xxix (1909), 858.
24. J. Barrell, "Fair play and toleration in criticism", letter to editor of *Science*, xxx (1909), 21.
25. F. R. Moulton, "An attempt to test the nebular hypothesis by an appeal to the laws of dynamics", *Astrophysical journal*, xi (1900), 103–30.
26. See, for example, T. C. Chamberlin and F. R. Moulton, "The development of the planetesimal hypothesis", *Science*, xxx (1909), 642–5.
27. T. J. J. See, "Significance of the spiral nebulae', *Popular astronomy*, xiv (1906), 614–16.
28. T. J. J. See, "On the cause of the remarkable circularity of the orbits of the planets and satellites and on the origin of the planetary system", *Popular astronomy*, xvii (1909), 263–72.

29. "Solar system was larger than now", *San Francisco Call*, 31 January 1909, p. 41.
30. "Scientists in furore over nebulae", *San Francisco Examiner*, 1 February 1909, p. 3.
31. "See's theory said to be reasonable", *San Francisco Call*, 1 February 1909, p. 12.
32. *Ibid.*
33. "Takes issue with See", *San Francisco Call*, 1 February 1909, p. 12.
34. T. J. J. See, "On the cause of the remarkable circularity of the orbits of the planets and satellites, and on the origin of the planetary system", *Astronomische Nachrichten*, clxxx (1909), cols 185–94.
35. T. J. J. See, Note to above paper, *Astronomische Nachrichten*, clxxx (1909), col. 194.
36. F. R. Moulton, "Remarks on recent contributions to cosmogony", letter to editor of *Science*, xxx (1909), 113–17.
37. "Astronomers warring — Moulton exposes See", *San Francisco Call*, 4 August 1909, p. 1.
38. "See says Moulton was only menial", *San Francisco Call*, 5 August 1909, p. 2.
39. T. J. J. See, "Geology and cosmogony", letter to editor of *Science*, xxx (1909), 479–80.
40. T. J. J. See, *Researches on the evolution of the stellar systems*: *Volume 2. The capture theory of cosmical evolution* (Lynn, Mass., 1910).
41. Webb, *op. cit.* (ref. 1), 246.
42. T. J. J. See, "Professor E. W. Brown's verification of the capture of satellites", *Popular astronomy*, xix (1911), 422–5.
43. T. J. J. See, "The evolution of the starry heavens", *Popular astronomy*, xix (1911), 529–47.
44. F. R. Moulton, "Capture theory and capture practice", *Popular astronomy*, xx (1912), 67–82.
45. T. J. J. See, "Historical notice of John Nelson Stockwell of Cleveland", *Popular astronomy*, xxviii (1920), 565–84.
46. Webb, *op. cit.* (ref. 1), p. viii.
47. *Ibid.*, 248.
48. "Professor See", review of *Brief biography and popular account of the unparalleled discoveries of T. J. J. See*, *The Nation*, xcviii (1914), 307–8.
49. "Dr. See solves gravity puzzle", *San Francisco Examiner*, 25 April 1914, p. 8.
50. *Traité physique mechanique par George Louis Le Sage*, ed. by P. Prévost (Paris, 1818).
51. "Want proofs before discussing discovery", *San Francisco Chronicle*, 25 April 1914, p. 13.
52. T. J. J. See, *Electrodynamic wave-theory of physical forces: Volume 1* (Lynn, Mass., 1917).
53. T. J. J. See, "New theory of the aether", *Astronomische Nachrichten*, ccxi (1920), cols 49–86, 137–90; ccxii (1920), cols 233–302, 385–454; ccxiv (1921), cols 281–359; ccxv (1922), cols 49–138; ccxvii (1922), cols 193–284; ccxxvi (1926), cols 401–97.
54. See, for example, the third of these papers (ref. 53), col. 233.
55. See the eighth of these papers, col. 493.
56. "Has mystery of ether at last been solved by science?", *San Francisco Examiner*, 11 March 1928, p. K-5.
57. T. J. J. See, "Capt. See scores Einstein", letter to editor of *New York Times*, 1 May 1923, p. 20.
58. "Prof. See declares Einstein in error", *New York Times*, 14 October 1924, p. 14.
59. "Denies error in relativity", *New York Times*, 16 October 1924, p. 12.
60. "Einstein ignores Capt. See", *New York Times*, 18 October 1924, p. 17.
61. T. J. J. See, *Wave-theory!*, v (Lynn, Mass., 1940).
62. "Meteoric rain on Sun blamed for heat wave", *San Francisco Chronicle*, 9 July 1921, p. 1.
63. "Finds the radio wave slower than light; around the globe at 165,000 miles a second", *New York Times*, 12 May 1924, p. 1.
64. T. J. J. See, "Radio waves by night", letter to editor of *New York Times*, 11 September 1924, p. 22.
65. T. J. J. See, *Wave-theory!*, i–xi (Lynn, Mass., 1938–52).
66. Webb, *op. cit.* (ref. 1), 268.

67. *Science*, cxxxvii (1962), 272.
68. “Capt. T. J. J. See, Astronomer, 96”, *New York Times*, 5 July 1962, p. 23.
69. S. D. Gossner, “See, Thomas Jefferson Jackson”, *Encyclopedia Americana* (New York, 1995), 515.
70. R. A. J. Schorn and L. D. G. Young, “See, Thomas Jefferson Jackson”, *Dictionary of scientific biography*, xii, 280–1.

JHA, xxx (1999)

THE BABYLONIAN FIRST VISIBILITY OF THE LUNAR CRESCENT: DATA AND CRITERION

LOUAY J. FATOOHI, F. RICHARD STEPHENSON, and
SHETHA S. AL-DARGAZELLI, University of Durham

1. *Introduction*

The problem of predicting the first visibility of the lunar crescent attracted attention throughout much of the historical period, from the many nations who used lunar calendars to regulate their activities. The oldest available records that reveal organized interest in this matter date back almost three thousand years — to the time of the Babylonians. Predicting the first visibility of the lunar crescent aroused great interest among medieval Muslim astronomers, largely because the dates of religious practices in Islam — such as the beginning and end of the fasting month of Ramadhan — are determined by a lunar calendar.

In modern times, scientific interest in understanding the visibility of the lunar crescent has been motivated mainly by two factors: (i) the need of historians correctly to interpret past records of nations that used the lunar calendar; and (ii) the need of present-day Muslims to ascertain each month when the lunar crescent may be visible for the first time after conjunction with the sun — and hence to know when to look for it, and also to know when it cannot be seen.

Predicting the earliest visibility of the lunar crescent after conjunction is a matter of considerable complexity. It is a problem where astronomical, atmospheric, optical and human factors are all at work. The fact that even modern astronomers cannot agree on the best criterion for determining the first visibility of the lunar crescent only attests to the complex nature of this matter.

Throughout history, attempts have been made to put forward criteria for predicting when the young crescent will first be seen in any given month. Each attempt has followed either an empirical or a theoretical approach. The empirical approach, which is more frequently employed, is based on analysing a collection of observational data and then formulating a criterion that best fits the observations. On the other hand, the theoretical method is embodied in attempts to resolve the problem through considering the various factors affecting crescent visibility and designing a descriptive mathematical model. While the Babylonian criterion was empirical, the Arab astronomers took mostly a theoretical approach. Recent studies on the subject have presented prediction models from both aspects: empirical and theoretical.

In this paper, we address the observational aspect of the Babylonian approach to the problem of first visibility of the lunar crescent and also consider the criterion that they have possibly used for predicting the first visibility of the crescent.

0021-8286/99/3001-0051/$2.50

2. *The Role of Observations in the Study of First Visibility of the Lunar Crescent*

Real observations of first visibility of the lunar crescent are crucial for the formulation of an empirical model, yet they are equally important for testing any theoretical solution. Whether empirical or theoretical, the reliability of any criterion can be established with confidence only by testing it against real observations of first visibility of the crescent. This critical role of observational data has urged researchers into the problem of predicting the first visibility of the crescent, to compile such data from the astronomical literature.

It was Fotheringham[1] who made the first such collection in 1910, when he compiled 76 observations of the new moon made by August Mommsen and Julius Schmidt at Athens in the second half of the previous century. Fotheringham used these observations to design his criterion for predicting the first visibility of the lunar crescent. The most comprehensive lists of observations made by experienced observers, including those compiled by Fotheringham, have recently been published by Schaefer[2] and by Doggett and Schaefer.[3] These authors compiled and carefully checked observations from a large number of publications as well as from moonwatches that they organized.[4]

The dates of the observational data compiled by Schaefer and Doggett range from 1859 to 1996, and are from various northern and southern latitudes. The total number of the observations they cite is 294, of which 23 are observations of last visibility of the old moon — rather than first visibility of the new moon. One very important aspect of the 271 evening observations is that they are not all positive sightings: 81 are negative observations, i.e., unsuccessful attempts to spot the new moon. Such negative observations are of exceptional importance in determining the limits of first visibility of the lunar crescent.

In this paper, we present the oldest observations of the lunar crescent that have so far come to light. We have extracted 209 positive observations from the Babylonian "Astronomical Diaries", with their dates ranging from –567 to –73 (568 to 74 B.C.). In the following sections we explain the Babylonian source of data, the conversion of the Babylonian dates into Julian dates, and how we determined the exact Julian date of first visibility of the lunar crescent. Finally, we discuss the possible visibility criterion that the Babylonians may have used.

3. *The Babylonian "Astronomical Diaries"*

The ancient Babylonians developed great interest in astronomical observations. This interest was motivated mainly by their concern with astrology, though calendrical needs contributed as well. In fact, there was never any distinction between the astronomers who made observations and the astrologers who interpreted the observations; both tasks were performed by the same people.[5]

From the eighth century B.C. onward, the Babylonians systematically and continuously recorded their astronomical observations on clay tablets. The Babylonian heritage of astronomical cuneiform texts is usually classified, after Sachs,[6] into four categories: (i) “Almanacs”, which are yearly lists of various predicted lunar and planetary phenomena, solstices and equinoxes, etc.; (ii) “Goal-Year Texts”, which were designed for the prediction of lunar and planetary phenomena based on certain fundamental periods and were prepared from the “Astronomical Diaries” (see below); (iii) “Normal-Star Almanacs”, texts on the positions of thirty-one stars, close to the ecliptic, which the Babylonians used for reference and which were denoted “*Normalsterne*” (“Normal stars”) by Epping[7] (a list of these stars, with longitude and latitude at the epoch 164 B.C., is given by Stephenson and Walker[8]); and (iv) the “Astronomical Diaries”, the only category of interest for the purpose of this study.

The Astronomical Diaries, or more briefly “diaries”, is the modern term used to refer to the tablets known in Akkadian as *nasaru ša ginê*, which means “regular watching”. These diaries represent records of daily astronomical observations made in the Neo-Babylonian period by *professionals* who, according to excavated late documents, were employed and paid specifically to make these observations. Their job also included recording their observations in the diaries and preparing astronomical tables and yearly almanacs. A diary usually covered about six months of observation. The entries for each month typically include information on the following: the length of the previous month; lunar and solar eclipses; lunar and planetary conjunctions with each other or with Normal stars; solstices and equinoxes; heliacal risings and settings of planets and Sirius; meteors; and comets. In the diaries, the Babylonians also systematically recorded the six time-intervals termed by A. Sachs “Lunar Sixes”. These may be described as follows. On the first day of the month the Babylonians recorded the time between sunset and moonset (*na*). Around the middle of the month they recorded four intervals related to the full moon: the time interval between moonset and sunrise when the moon set for the last time before sunrise (*ŠÙ*); the interval between sunrise and moonset when the moon set for the first time after sunrise (*na*); the interval between moonrise and sunset when the moon rose for the last time before sunset (*ME*); and the interval between sunset and moonrise when the moon rose for the first time after sunset (GE_6). Finally, near the end of the month the Babylonians recorded the time between moonrise and sunrise when the waning crescent moon was visible for the last time (*KUR*). In addition to the astronomical data, the diaries also contain some non-astronomical information: on the weather, the prices of six basic commodities, the height of the river Euphrates, and certain historical events.

It should be emphasized that although the major bulk of celestial phenomena referred to in the diaries are actual observations, some of the recorded events are not observations but rather predictions based on certain mathematical calculations. Sometimes this is clearly stated whereas on other occasions it is implicit, as in the case when the sky is mentioned as having been overcast.

Most of the available tablets containing the diaries are damaged to varying degrees

— often extensively. In some cases the date of the tablet is broken away. Such tablets can often be dated by using a unique combination of astronomical data that they record — for example, eclipses and lunar and planetary positions. This is how Sachs and Hunger determined many of the dates of the diaries, which they recently published in transliteration and translation in three volumes.[9] These volumes, which form the exclusive source for the Babylonian data of the current study, cover diaries from –651 (652 B.C.) to –60 (61 B.C.).

The following is an example of the diary reports, for the first seven days of the lunar month whose first day corresponds to B.C. 163 August 11 (parentheses denote editorial comment, square brackets indicate damaged text that has been restored by the editors, while the number at the beginning of each paragraph indicates the line number in the text):

> 1 Year 149 (Seleucid), king Antiochus. Month V, (the 1st of which was identical with) the 30th (of the preceding month), sunset to moonset: 10°, it was very low; measured (despite) mist.
>
> 2 Night of the 2nd, the moon was 1 cubit behind γ Virginis. Night of the 3rd, the moon was 1 cubit above α Virginis, the moon having passed 0.5 cubit
>
> 3 to the east. The 3rd, the north wind blew. Night of the 4th, the moon was 4 cubits in front of α Librae. The 4th, the north wind blew. Night of the 5th,
>
> 4 beginning of the night, the moon was 2.5 cubits below β Librae. The 5th, the east wind blew. Night of the 6th, beginning of the night, the moon was 20 fingers above β
>
> 5 Scorpii. The 6th, ZI IR (unidentified), the east wind blew. Night of the 7th, beginning of the night, the moon was 3 cubits in front of θ Ophiuchi,
>
> 6 the moon being 2.5 cubits high to the north, it stood 1 cubit 8 fingers in front of Mars to the west, the moon being 2 cubits high to [the north;]
>
> 7 last part of the night, Venus was 4 cubits below ε Leonis. The 7th, clouds were in the sky, ZI IR, the east wind blew.[10]

As seen in the above example, a typical diary starts with a mention of the Babylonian year and month. This is followed by a phrase stating that the first day of that month was either "identical with" or "followed"[11] the 30th of the preceding month, so indicating whether the previous month contained 29 or 30 days, the only lengths permitted by the Babylonian time-reckoning. After that there is a mention of the measured or predicted *na* interval, which is the time between sunset and moonset of the first day of the month — usually known as 'moonset lagtime' in modern terminology. This is one of the six quantities termed Lunar Sixes already mentioned.

During each month, the Babylonian observers recorded when the moon and planets passed near to each other or near to normal stars. In a diary, the relative position of a celestial body to another may be described by one of the terms "above" (*e*), "below" (*šap*), "in front of" (*ina IGI*), or "behind" (*ar*). The terms "behind" and "in front of" are roughly synonymous with "to the east of" and "to the west of", respectively, following the apparent rotation of the celestial sphere.

For the measurement of angles, such as the position of celestial bodies and magnitudes of eclipses,[12] the Babylonians used the units 'finger' (*SI*) and 'cubit' (*KÙŠ*), which contained twenty-four fingers in the Neo-Babylonian period.[13] It was previously suggested that the cubit was approximately equivalent to 2°.[14] However, a recent investigation of Babylonian measurements of close planetary conjunctions has shown that the cubit closely equalled 2.2°.[15] This last study has also shown that the Babylonians did not use horizon coordinates (altitude and azimuth), but there was little evidence to determine whether ecliptical or equatorial coordinates were used. However, because of the Babylonians' introduction of the concept of the zodiac around 400 B.C. it appears more reasonable to suppose that the Babylonian astronomers used an ecliptical system.

For the measurement of time intervals shorter than a day, such as the durations of the phases of an eclipse,[16] the Babylonians used the unit *uš*. According to Neugebauer, "The 'degree' (*uš*) is the fundamental unit for the measurement not only of arcs, especially for the longitude, but also for the measurement of time, corresponding to our modern use of right ascension. Therefore, 1 degree = 4 minutes of time".[17] Accordingly, Sachs and Hunger, who translate *uš* as "time degree", have converted all measurements in *uš* in the diaries, especially those of the Lunar Sixes, into time-degrees. We have confirmed, through the investigation of Babylonian records of lunar eclipse durations, that the modern equivalence of the *uš* is accurately 4 minutes and have shown that the definition of this unit showed no variations over the centuries covered by the Late Babylonian astronomical texts.[18]

4. *Determination of the Julian Date of First Visibility of the Lunar Crescent*

We have thoroughly scanned Sachs and Hunger's three volumes[19] and compiled a list of dates of Julian years and Babylonian months in which the moon was first sighted. This is not simply a list of each year and month cited in the extant diaries because, as already mentioned, the Babylonians did not depend solely on observation when determining the first day of the month, though this seems to have been the practice in ideal weather. The Babylonian astronomers did use mathematical methods for determining the first day of the month, at least when visibility of the lunar crescent was prevented by unfavourable weather conditions. Since our purpose was to collect dates of actual observations rather than predictions of first visibility of lunar crescents we have selected only the entries that contain explicit statements confirming that the moon was indeed sighted. Terms and phrases used by the Babylonians to indicate actual sighting of the moon include "visible", "seen", "first appearance", and "earthshine". Descriptions of the position of the moon or its brightness, such as "low", "could be seen", "was low to the sun", "faint" and "bright", are also indications of actual observations. Below are examples from different years of reported first sightings of the lunar crescent:

> Month V, (the 1st of which was identical with) the 30th (of the preceding month), first appearance of the moon; sunset to moonset: 12°; the moon was 2 cubits in front of Mercury.[20] [Julian date: B.C. 373 July 23]
>
> [Month V,] the 1st (of which followed the 30th of the preceding month), sunset to moonset: 15.5°; the moon was 1.66 cubits in front of α Virginis.[21] [Julian date: B.C. 334 August 12]
>
> Month IX, the 1st (of which followed the 30th of the preceding month), sunset to moonset: 15°, measured; the moon stood 1.5 cubits in front of Mercury to the west.[22] [Julian date: B.C. 274 December 4]
>
> Month IX, (the 1st of which was identical with) the 30th (of the preceding month), sunset to moonset: 17.5°; it was bright, earthshine, measured; it was low to the sun.[23] [Julian date: B.C. 204 December 10]
>
> [Month V, (the 1st of which was identical with) the 30th (of the preceding month), sunset to] moonset: [*nn*°]; it was faint, it was low to the sun; (the moon) [stood] 3 cubits in front of Mars, 5 cubits in front of Saturn to the west.[24] [Julian date: B.C. 171 August 9]

In order to confine ourselves to actual sightings of the lunar crescent, we have excluded all entries where the text contained explicit statements and terms implying invisibility of the moon, such as “I did not watch”, “I did not see the moon”, “overcast”, “mist”, and “clouds”. We have also ruled out all entries in which the moonset lagtime or interval between sunset and moonset (*na*) is said to have been predicted as this might well be due to the fact that the moon was not seen. As an essential measure of extra caution, we have discounted any entry that does not contain a specific statement that the moon was seen, even if it does not contain any explicit or implicit indication to the contrary. Accordingly, the final list of acceptable entries, though numbering as many as 209 in total, was unavoidably only a small part of the original material. The following are examples of the kinds of entries that have been discarded for one or more of the reasons mentioned above:

> [Month XI, (the 1st of which was identical with) the 30th (of the preceding month),] sunset to moonset: 14°; there were dense clouds, so that I did not see the moon.[25] [Julian date: B.C. 453 February 12]
>
> Month VIII, the 1st (of which followed the 30th of the preceding month), sunset to moonset: 18.5°. Night of the 1st, clouds crossed the sky.[26] [Julian date: B.C. 271 November 2]
>
> Month II, (the 1st of which was identical with) the 30th (of the preceding month, sunset to moonset): 13°; dense clouds, I did not watch. Night of the 1st, [clouds] crossed the sky.[27] [Julian date: B.C. 256 April 23]

> [Diaries from month VII to the end] of month XII, year 113, which is the year 177, King Arsaces. Month VII, the 1st (of which followed the 30th of the preceding month), sunset to moonset: 11.5°; mist [...].[28] [Julian date: B.C. 135 September 30]

Having collected all reliable dates of first sightings of the moon after conjunction, we made a preliminary conversion of all dates, which are given by Sachs and Hunger in terms of Julian year and Babylonian lunar month, to their full Julian equivalents. This could have been achieved using the specially prepared tables of Parker and Dubberstein[29] which cover the period 626 B.C. to A.D. 75. However, the use of these manual tables would not be very practical when a large number of data are involved. Therefore, we used only the intercalary scheme from these tables, i.e., the recorded positions of the additional months (which always followed the 6th or 12th month). We then integrated this scheme in a specially designed program that reads in the Babylonian date and converts it to its Julian equivalent, totally independently of the tables.

The program uses the lunar visibility criterion suggested by Schoch[30] to determine the expected dates of first visibility of the crescents. This is the criterion on which the tables of Parker and Dubberstein are based. The use of a specific lunar visibility criterion for this purpose is of no critical importance, because the converted dates, whether found manually by tables or by the program, could be considered only a first approximation anyway. The reason is that the date of actual observation of the crescent in any given month, which is the date that really matters for the purpose of this study, is not necessarily the same as that predicted by any theoretical calculation. For instance, a crescent that in theory should have been easily noticed could have set unseen because of unfavourable weather and its actual first visibility could have occurred the next evening. Therefore, in each instance the calculated date of first visibility must be checked against real observational data — usually in the form of a time or positional measurement from the month under consideration (see next section). In this way, one can be sure whether the theoretically calculated date is exact or in need of amendment. In practice, such amendments never exceeded a single day, but even such a seemingly small discrepancy is crucial for the purpose of crescent visibility studies.

In 136 of the 209 entries that we compiled, the measured moonset lagtime is given; since the lagtime changes from one day to another by an average of 54 minutes (some 13.5°), this quantity could be used to determine the exact date of first sighting of the lunar crescent. The following are two different explanatory examples:

> Month III, (the 1st of which was identical with) the 30th (of the preceding month), the moon became visible behind Cancer; it (i.e. the crescent) was thick; sunset to moonset: 20°.[31]

This observation is from year –567. According to the date conversion program, the

Julian date of this event is –567 June 20. From our further computations, the moonset lagtime on that day was 89 minutes, i.e. 22.25 time-degrees, which is close to that given in the Babylonian text; hence B.C. 568 June 20 is confirmed to be the exact Julian date of observation.

> Month VIII, the 1st (of which followed the 30th of the preceding month), sunset to moonset: 17°; it could be seen while the sun stood there.[32]

This entry belongs to year –283. The calculated Julian date of this event is B.C. 284 October 26. However, the computed moonset lagtime on that date is 31 minutes, i.e. 7.75° time-degrees, which indicates that the exact date of observation was in fact the next day, i.e. B.C. 284 October 27; on this latter date the lagtime was 69 minutes, i.e. 17.25° time-degrees — almost the same quantity as measured by the Babylonians.

We found 9 entries where the difference between the measured and the computed lagtime was more than 4°, i.e. more than 16 minutes of time. The difference could well be due to inaccurate measurement of the lagtime or scribal error in the original text, and does not necessarily indicate an error in the date. Measurement of the *na* interval would be a difficult task since the young crescent moon can be seen only for a short time about midway between sunset and moonset. However, as a measure of caution, we re-checked these entries using additional data from the text. For this purpose, we used observations of lunar horizontal separation (i.e. when the moon is “behind”, “east”, “in front of”, or “west”) from a star or planet recorded during the same lunar month. Because the moon traverses about 13° every day, the date of any lunar conjunction in the month can be exactly determined, and this date can be used as a reference for verifying the date of the first day of the month, i.e. the date of the observation. However, if the text did not mention the horizontal separation we used the vertical separation (i.e. when the moon is “above” or “below”) because the latter would be given only when the moon was horizontally close to the planet or star.

In the other 73 of the compiled 209 entries, the observed lagtime was missing, mostly because the text is broken away. In this case, we used other astronomical data from the same month to verify the date, exactly as in the case of the 9 entries mentioned above. Table 1 includes the exact Julian dates of the 209 Babylonian observations of the lunar crescent mentioned in the astronomical diaries.

For calculating the lunar coordinates, we designed a program that uses the semi-analytical lunar ephemeris ELP2000-85.[33] Although Chapront-Touzé and Chapront[34] suggest that ELP2000-85 is valid over a time span of several thousand years, using this theory for ancient times requires a significant modification. The ELP2000-85 solution assumes a value of $-23.895''/\mathrm{cy}^2$ for the tidal secular acceleration of the moon. However, recent results from lunar laser ranging (LLR) suggest a higher lunar acceleration of $-25.88 \pm 0.5''/\mathrm{cy}^2$.[35] In a recent communication to one of the present authors,[36] J. L. Williams of the LLR team claims that consistent results for lunar acceleration are being found in the range –25.8 to $-26.0''/\mathrm{cy}^2$. Although the difference between these recent results and that assumed by ELP2000-85 may seem

Table 1. The 209 Babylonian observations of first visibility of the lunar crescent.

No.	Year	Month	Day	No.	Year	Month	Day	No.	Year	Month	Day	No.	Year	Month	Day
1	−567	4	22	54	−284	11	6	107	−192	3	18	160	−143	8	11
2	−567	5	22	55	−283	10	27	108	−192	9	11	161	−143	9	9
3	−567	6	20	56	−281	11	4	109	−190	3	26	162	−143	10	9
4	−566	2	12	57	−277	3	28	110	−190	5	24	163	−142	11	26
5	−566	3	14	58	−277	4	26	111	−189	10	9	164	−141	5	23
6	−418	10	20	59	−277	5	26	112	−189	11	7	165	−141	10	17
7	−381	5	6	60	−273	12	4	113	−188	4	2	166	−140	4	12
8	−381	7	4	61	−266	10	19	114	−187	10	16	167	−140	7	9
9	−378	10	27	62	−266	11	18	115	−187	11	14	168	−140	12	3
10	−374	1	20	63	−264	9	26	116	−185	3	2	169	−139	2	1
11	−374	3	20	64	−255	3	25	117	−183	5	7	170	−137	12	31
12	−372	2	27	65	−255	9	17	118	−183	8	4	171	−136	9	22
13	−372	3	28	66	−251	10	3	119	−183	10	31	172	−134	10	30
14	−372	7	23	67	−250	2	28	120	−181	2	15	173	−133	2	25
15	−370	8	1	68	−249	8	13	121	−179	3	24	174	−133	8	20
16	−370	10	28	69	−246	1	15	122	−179	7	20	175	−133	9	19
17	−368	7	10	70	−246	4	14	123	−178	8	8	176	−133	10	19
18	−366	6	18	71	−246	5	14	124	−178	9	6	177	−132	3	15
19	−366	8	17	72	−246	10	8	125	−178	10	6	178	−132	10	7
20	−346	12	2	73	−245	5	3	126	−176	9	13	179	−131	10	26
21	−345	3	1	74	−245	7	1	127	−176	10	13	180	−129	7	9
22	−342	12	17	75	−237	7	3	128	−175	5	9	181	−124	12	7
23	−333	6	14	76	−237	8	1	129	−175	12	1	182	−123	2	4
24	−333	8	13	77	−234	9	26	130	−173	11	10	183	−123	6	2
25	−332	9	29	78	−234	11	24	131	−173	12	9	184	−119	4	19
26	−328	10	13	79	−233	2	20	132	−172	2	6	185	−119	6	17
27	−328	12	12	80	−233	3	21	133	−170	8	9	186	−118	5	8
28	−324	4	6	81	−232	10	3	134	−170	10	8	187	−117	10	22
29	−324	7	3	82	−231	2	28	135	−170	11	7	188	−111	3	22
30	−324	8	2	83	−225	1	23	136	−169	2	3	189	−111	6	19
31	−324	9	30	84	−218	10	28	137	−169	5	2	190	−111	8	18
32	−322	12	7	85	−217	2	23	138	−168	8	17	191	−107	4	7
33	−321	1	5	86	−210	7	4	139	−168	12	13	192	−105	4	16
34	−321	2	3	87	−209	5	24	140	−164	6	5	193	−105	5	15
35	−321	4	3	88	−207	4	2	141	−164	10	31	194	−105	6	13
36	−321	7	30	89	−207	5	1	142	−163	4	25	195	−105	9	9
37	−321	8	29	90	−203	12	10	143	−163	5	26	196	−105	10	9
38	−307	6	26	91	−201	12	18	144	−163	11	19	197	−104	8	29
39	−307	8	24	92	−200	3	16	145	−162	3	16	198	−96	5	5
40	−302	7	29	93	−198	6	21	146	−162	8	11	199	−95	5	24
41	−302	8	28	94	−197	3	14	147	−162	9	10	200	−87	7	23
42	−302	10	27	95	−197	10	7	148	−161	9	29	201	−87	9	20
43	−302	11	26	96	−197	11	5	149	−158	6	29	202	−86	3	17
44	−302	12	26	97	−196	2	2	150	−158	8	26	203	−86	11	7
45	−301	1	24	98	−195	11	12	151	−156	12	1	204	−83	7	9
46	−301	6	19	99	−194	1	11	152	−154	1	18	205	−77	6	4
47	−294	5	4	100	−194	6	7	153	−151	3	15	206	−77	8	2
48	−293	1	25	101	−194	10	3	154	−149	11	14	207	−77	9	1
49	−291	5	1	102	−193	4	28	155	−145	1	9	208	−77	10	30
50	−291	6	29	103	−193	5	28	156	−145	2	7	209	−73	7	19
51	−291	8	26	104	−193	10	22	157	−144	9	21				
52	−289	6	8	105	−192	1	19	158	−144	10	20				
53	−286	6	4	106	−192	2	17	159	−144	11	18				

small, it does nevertheless accumulate significant errors over a long period as in the case of the Babylonian data. We have remedied this situation by using a special formula given in the *Astronomical almanac* which accounts for the deficiency in the tidal secular acceleration of the moon by modifying the Julian day number of the event so that the computed lunar coordinates are for a lunar acceleration of $-26''/\mathrm{cy}^2$.[37]

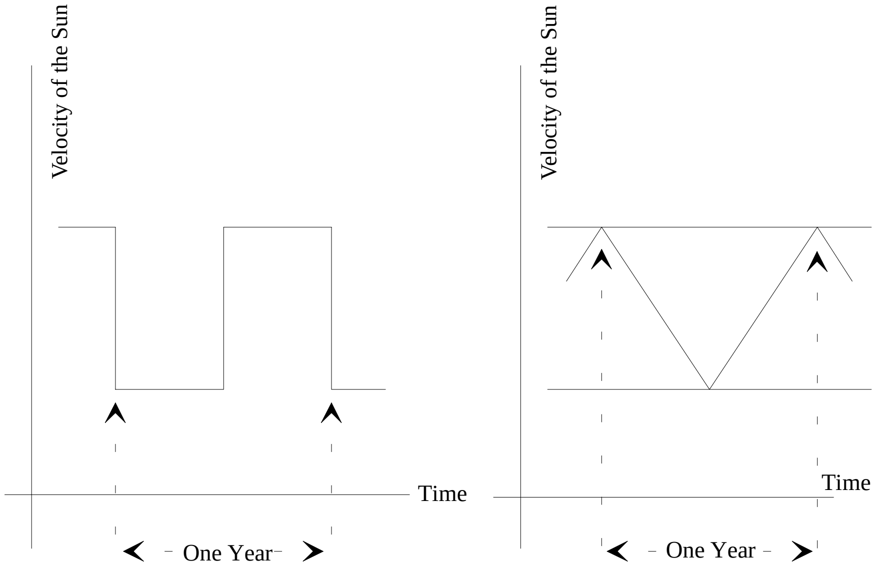
In order for the calculations to be valid for an ancient epoch such as the Babylonian, it is also necessary to make allowance for the cumulative effect of changes in the length of the day (ΔT) which results from variations in the earth's rate of rotation due to tides and other causes.[38] For example, ΔT is estimated to have been as much as about 16800 seconds (4.66 hours) in the year –500 which corresponds to changes of about 2.5° and 0.2° in the lunar longitude and latitude, respectively. We have incorporated into our calculations ΔT using the values recently derived by Stephenson and Morrison[39] from their analysis of historical records of astronomical events — mainly eclipses, including those from Babylon.

We computed the solar coordinates using the solution VSOP82 (stands for *Variations Séculaires des Orbites Planétaires*)[40] and the planetary positions using the analytical theory VSOP87.[41] We designed a special program for calculating the stellar coordinates.

5. *The Babylonian Criterion of First Visibility of the Lunar Crescent*

The accurate prediction of the evening of first visibility of the new crescent was of major significance for the Babylonians. Indeed, this matter was of such importance that it was the main goal of the Babylonian lunar theory in the Seleucid period (311 – 64 B.C.).[42] The Babylonians succeeded in formulating a truly mathematical lunar theory which they used for predicting various parameters of the lunar motion, as found recorded in the lunar ephemerides they prepared.

Modern investigators of the problem of first visibility of the new crescent, who are not themselves scholars of Babylonian astronomy, have systematically claimed that the Babylonian conditions of visibility were that the age of the new moon is more than 24 hours and that the arc of separation(s) should be equal to or greater than 12°, i.e. that the moon sets at least 48 minutes after sunset. This supposed Babylonian criterion is also often cited as being only $S \geq 12°$. It seems that Bruin[43] was the first modern researcher to attribute this criterion to the Babylonians and that all subsequent researchers who reiterated this claim were simply relying on his account.[44] However, it should be noted that Bruin cited no reference in support of his claim. Bruin seems to have suggested it because he noted that the simple rule of $S \geq 12°$ was used by Arab astronomers from the seventh century onward; he believed that it might have transmitted to them from the Hindus who would have learned it from the Babylonians. However, Bruin's claim with regard to the Babylonian condition of lunar visibility is, at best, inaccurate. The 12° equatorial difference is indeed the crescent visibility criterion adopted by the Indian *Suryasiddhanta* (*c*. 600) and the *Khandakhadyaka* (650), as pointed out by King.[45]



System A

System B

FIG. 1. The two representations of solar motion in the Babylonian lunar theory.

However, even though Babylonian astronomical knowledge had passed to the Indians (by way of the Greeks), this does not necessarily imply that this was the Babylonian criterion of crescent visibility.

Study of the Babylonian lunar ephemerides has revealed that they are based on two somewhat different versions of lunar theory, usually referred to as “System A” and “System B”. According to System A, the sun moves with constant velocities on two different arcs of the ecliptic, whereas System B assumes that the solar velocity changes with time in a linear zigzag function.[46] The difference between the two theories is usually represented by Figure 1.

It is interesting to note that although System B must have been an improvement of System A, both Systems were used simultaneously throughout the period 250–50 B.C. in preparing ephemerides. Neugebauer notes that such a practice, which is contrary to our modern scientific concepts where new theories replace old ones, is yet more prominent in the planetary theory.[47] The lunar ephemerides were used by the Babylonians to predict the first and last visibility of the moon. A comparative list of the main columns of computations of a complete ephemeris in the two systems is given in Table 2.[48]

Although the existence of procedure texts that give criteria for determining the first and last visibility of the moon is hard to doubt, so far, unfortunately, no such texts have come to light. Therefore, it is only through the analysis of individual

TABLE 2. The columns of astronomical calculations included by Babylonian astronomers in each ephemeris of System A and System B. As seen, some parameters are calculated in ephemerides of both Systems whereas others are restricted to one System or the other. Although the last four quantities are missing from the tables of System A, preserved procedural texts tell us they were calculated; they would be necessary for finding the lagtime.

<table>
<tr><th>System A</th><th>System B</th></tr>
<tr><td></td><td></td></tr>
<tr><td colspan="2">Dates</td></tr>
<tr><td>Relative velocity of the moon with respect to the sun(?)</td><td></td></tr>
<tr><td></td><td>Velocity of the sun</td></tr>
<tr><td colspan="2">Longitude of the moon</td></tr>
<tr><td colspan="2">Length of daylight</td></tr>
<tr><td></td><td>Half length of the night</td></tr>
<tr><td colspan="2">Latitude of the moon</td></tr>
<tr><td colspan="2">Magnitude of eclipses</td></tr>
<tr><td colspan="2">Velocity of the moon</td></tr>
<tr><td colspan="2">Length of the month in first approximation</td></tr>
<tr><td></td><td>Correction related to the next column</td></tr>
<tr><td colspan="2">Correction in the length of the month caused by the variability of solar velocity</td></tr>
<tr><td>Second correction to the length of the month</td><td></td></tr>
<tr><td colspan="2">Length of the month</td></tr>
<tr><td></td><td>Date of syzygy, midnight epoch</td></tr>
<tr><td>Date of syzygy, evening epoch</td><td>Date of syzygy, evening or morning epoch</td></tr>
<tr><td></td><td>Time difference between syzygy and sunset or sunrise</td></tr>
<tr><td></td><td>Elongation of first or last visibility</td></tr>
<tr><td></td><td>Influence of the obliquity of the ecliptic</td></tr>
<tr><td></td><td>Influence of the latitude</td></tr>
<tr><td colspan="2">Duration of first or last visibility (lagtime)</td></tr>
</table>

cases in the ephemerides that certain criteria can be concluded.

Contrary to what is commonly assumed, Neugebauer[49] found from the study of extant ephemerides that the moonset lagtime alone could not have been used as the visibility criterion by the Babylonians in any of the two Systems. He suggests that a criterion of the following form might have been used by the Babylonians for both Systems:

elongation (L) + moonset lagtime (in degrees) (S) > constant.

Neugebauer suggests that the rationale behind the inclusion of the elongation in the criterion of first visibility would be that the elongation measures, in addition to the angular distance between the sun and moon, the width of the visible crescent. Therefore, this criterion would imply that the chance of sighting the new crescent increases with the width of the crescent and with the time for which the crescent remains above the horizon before setting.

As for the value of the constant in the above criterion, Neugebauer found from his study of preserved texts that in the case of System A the constant could have been about 21°. In other words, the Babylonian visibility criterion for System A is:

$$L + S > 21^\circ.$$

In the case of System B, Neugebauer found two ephemerides that suggest a value of about 23° for the constant, whereas another suggests ≥ 20° and a fourth accepts a value as low as ≥ 17°. This represents a considerable range.

Interestingly, Neugebauer notes that the moonset lagtime might have been used alone for predicting the visibility of the new moon in some cases. He concludes this from the existence of isolated lists of lagtimes that seem to have been collected for several years in succession. The lowest values found in these texts are 11.33°, 11.66°, and 11.83°, and these are followed by a phrase of unknown meaning. The highest value of lagtime given is 25.16° without alternative, although an ephemeris preserved for the same year accepts instead 12°. One alternative solution of 20.5° for a full month (30 days) and 10.5° for a hollow month (29 days) is also given.[50]

We have found that the smallest value of L + S in the 209 observations is 22° (observation 89), which is very close to the limits of 21° and 23° suggested by Neugebauer for Systems A and B respectively. The highest value of L + S that we have found is 57.9° (observation 143). Therefore, while exceeding the 22° limit does not ensure visibility of the lunar crescent, this value may possibly have been used by the Babylonians as the lowest limit for the visibility of the crescent.

The latitude of Babylon is about 32.6° N. To test the reliability of the above criterion that the Babylonians might have used, we applied it to the observations of Table 1 as well as all entries of latitudes within the range ± (30° – 35°) from the modern compilations.[51] We assumed that the Babylonian criterion was $L + S \geq 22^\circ$, as this is the smallest value in the Babylonian data. We found that the quantity L + S is less than 22° for only 2 of the 231 positive observations of latitudes close to that of Babylon. But while this criterion misjudges only 0.9% of the *positive* observations, it has 7 of the 19 *negative* observations in the visibility zone, i.e. L + S greater than 22°. The latter result represents a very high percentage of error, 36.8%. The unreliability of this criterion becomes even more manifest when applied to the data from all latitudes. Five of the total of 399 Babylonian and modern positive observations, i.e. 1.3%, are wrongly placed according to the Babylonian criterion, but as many as 37 of the 81 negative observations, i.e. 45.7%, contradict the visibility condition. Certainly, this would be a very bad global criterion.

There have been modern attempts to formulate modern crescent visibility criteria that would predict the dates when the crescent could have been visible in Babylon. These attempts were originally triggered by interest in determining the beginnings of the Babylonian months, which would help in establishing the equivalent dates of Babylonian records. One such solution was first attempted by Karl Schoch, who designed tables for determining the evening of the first sighting of the lunar crescent that are applicable to all places whose latitudes differ little from that of Babylon. Schoch also presented his lunar visibility tables, following Fotheringham,[52] in the form of a curve of true lunar altitude (h) (parallax is not accounted for) versus the azimuthal difference between the sun and moon (ΔZ) at sunset, so that the new moon would be first visible on the first evening after conjunction in which the moon falls above the curve (see Table 3).[53] However, the criterion of Schoch suffers from the important flaw of being based on both observations and predictions of the lunar crescent.[54] Even Schoch's identification of what he considered to have been observations was not totally sound. For instance, Schoch states that "The most valuable observations for my purpose are the most ancient, belonging to a time when the Babylonians were unable to compute the appearance of the crescent, i.e. the time from Rim-Sin to Ammizaduga and from Nebuchadnezzar to Xerxes".[55] But the fact that the Babylonians were at some stage of their history unable to predict the first appearance of the crescent does not necessarily mean that they did not follow some simple rules in fixing their calendar, the most probable and simplest of such rules being that the month would be of either 29 or 30 days. If so basic a rule was followed, then the lengths of the Babylonian months determined according to this rule would have no implications whatsoever for the visibility of the moon. (It should be stressed that the skies of Babylon are often cloudy in winter, for example.) It was exactly to avoid using such pseudo-observational data that for the present project we collected only actual observations of the lunar crescent. Although Fotheringham[56] expresses his confidence in Schoch's criterion for computing the first visibility of the lunar crescent at Babylon, Schoch's use of predictions in addition to observations in setting his criterion has been criticized by O. Neugebauer.[57] It seem fair to conclude that Schoch's solution can be regarded as neither observational nor theoretical, and hence it is likely to lead to errors in predicting the dates of first sightings of the lunar crescent in Babylon.

Another criterion for determining the first visibility of the lunar crescent at Babylon was suggested by P. V. Neugebauer. This solution uses the same two parameters employed by Schoch, i.e. ΔZ and h, but the suggested curve lies a little below that of Schoch for smaller ΔZ and slightly above it for larger ΔZ.[58] However, the differences between both curves are too small to be of any significance in practical use. Neugebauer's curve also extends to 23° of ΔZ, in contrast to that of Schoch which covers only up to 19° of ΔZ (see Table 3 for both criteria). We did not come across any other modern criterion that is based on Babylonian data or is designed to predict the lunar visibility in Babylon in particular. Researchers into the Babylonian

Table 3. The criteria of K. Schoch and of P. V. Neugebauer. At any specified azimuthal difference from the sun, the crescent is expected to be visible when the moon is not lower than a critical true altitude at sunset.

True azimuthal difference (ΔZ)	Minimum true lunar altitude (h)	
	Schoch	Neugebauer
°	°	°
0	10.7	10.4
1	10.7	10.4
2	10.6	10.3
3	10.5	10.2
4	10.4	10.1
5	10.3	10.0
6	10.1	9.8
7	10.0	9.7
8	9.8	9.5
9	9.6	9.4
10	9.4	9.3
11	9.1	9.1
12	8.8	8.9
13	8.4	8.6
14	8.0	8.3
15	7.6	8.0
16	7.3	7.7
17	7.0	7.4
18	6.7	7.0
19	6.3	6.6
20	-	6.2
21	-	5.7
22	-	5.2
23	-	4.8

calendar have relied on one or the other of the above criteria (for example, Parker and Dubberstein[59] used Schoch's model while Huber[60] opted for that of Neugebauer).

We have examined both criteria of Schoch and Neugebauer using the 209 observations that we have collected from the Babylonian diaries. Because these are real observations, they can serve as a very reliable indicator of the accuracy of both criteria. We have plotted in Figure 2 the visibility curves of Schoch and Neugebauer as well as the 209 Babylonian observations. The graph shows that both models are reasonably good in predicting the observations. Of the 209 positive observations, only 8 fell below the visibility curves. In other words, according to the criteria of Schoch and Neugebauer about 3.8% of the sighted crescents would have been invisible. However, if the visibility curve is drawn downwards starting from about $h = 9.45^\circ$ for $\Delta Z = 0^\circ$, then all of the observations would be above the visibility curve, i.e. in the visibility zone.

It should be stressed, however, that the fact that this modified curve would have almost all positive observations in the visibility zone does not tell us anything about the suitability of this criterion for hypothetical negative observations from Babylon. In other words, it is obvious that while lowering the dividing line would include all

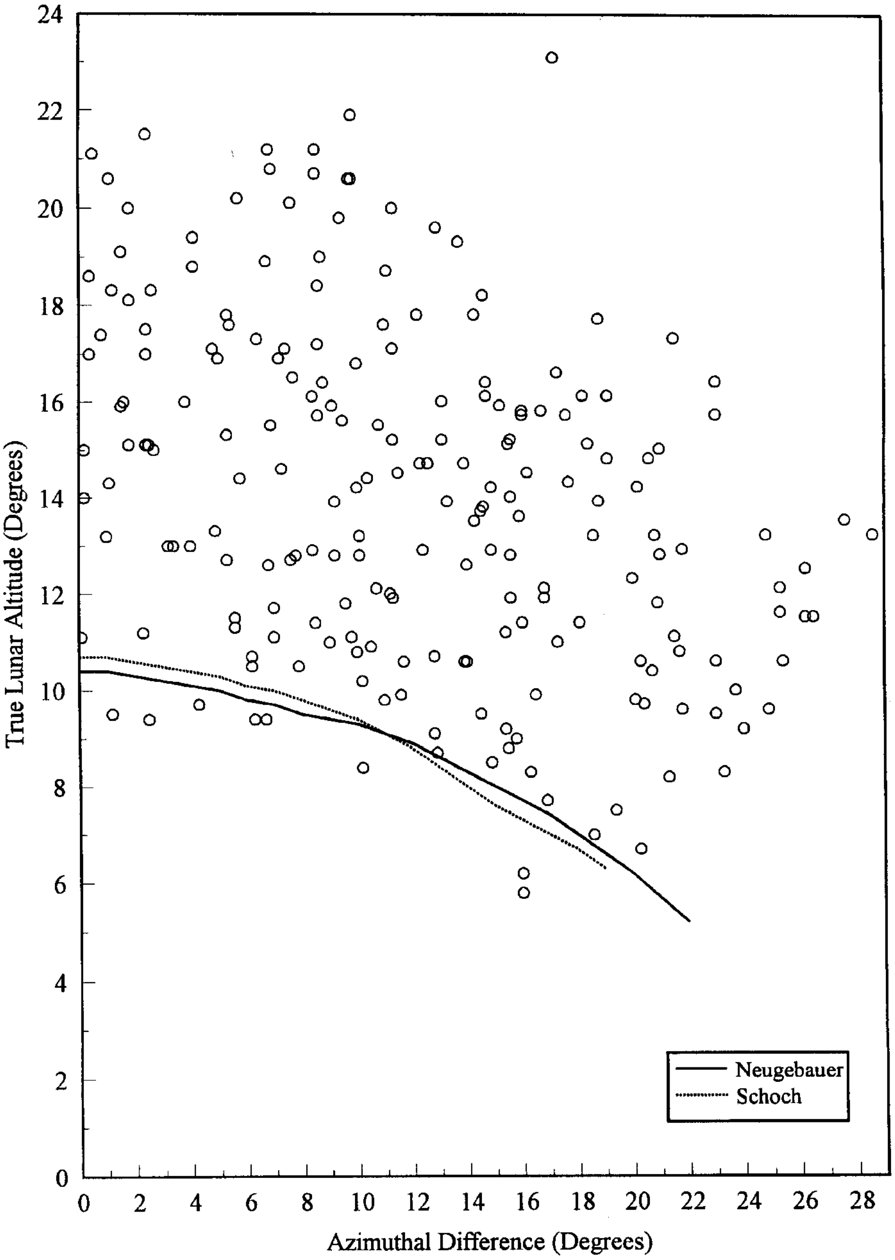


FIG. 2. The visibility criteria of Schoch and P. V. Neugebauer together with the Babylonian positive observations.

the positive observation in the visibility zone, i.e. above the curve, the curve will certainly become so low that it would have more negative observations in its visibility zone than would the original curves. While lowering the criterion curve would definitely give better results as far as positive observations are concerned, it would also increase the number of Babylonian months that actually began one day later than the solution predicts. This drawback in the criteria of Neugebauer and Schoch would have become manifest if the Babylonian data included actual negative observations in addition to the positive.

A realistic evaluation of the h – ΔZ criteria can be made with the help of the modern lists of observations which, unlike the Babylonian collection, include negative in addition to the positive observations. Neugebauer's curve misjudges 22 of the 81 negative observations, i.e. 27.2%, and misses 50 of the 399 positive, i.e. 12.5%. Obviously, this criterion cannot be considered satisfactory. Schoch's curve would not give a significantly different results.

While Schoch suggested that his model is applicable to all latitudes close to that of Babylon, Fotheringham claimed that his h – ΔZ criterion is independent of the geographical latitude of the observer. We have used the data in Table 1 as well as the modern lists to investigate whether or not the h – ΔZ criterion depends on the observer's latitude. We have, therefore, separated the observations into two categories according to the geographical latitude of the observers, including in the first category only the observations made from latitudes ± (30° – 40°). These consisted of 372 observations, 310 positive and 62 negative. The second group included all the other 108 observations, 89 of which are positive and the remaining 19 negative.

We have plotted in Figure 3 the mid-latitudes data and Neugebauer's form of the h – ΔZ criterion. The curve has 27 of the 310 (7.7%) positive observations (denoted by circles) in the invisibility zone and 13 of the 62 (21%) negative observations (denoted by crosses) in the visibility zone. Figure 4 is similar to Figure 3 but it includes the observational data from all the latitudes other than ± (30° – 40°). Here the curve of Neugebauer has as many as 9 of the 19 (47.4%) negative observations and 26 of the 89 (29.2%) positive in the wrong zone (though Figures 3 and 4 may show smaller numbers of points because of coinciding data points).

It seems from Figures 3 and 4 that Neugebauer's criterion gives much larger errors when applied to latitudes away from that of Babylon. This shows that, contrary to Fotheringham's assertion, this type of solution is latitude-dependent. This and the high percentage of error that all forms of this criterion give cannot be overcome by simply lowering or raising the curve or even changing its shape. Any such changes can improve the reliability of the criterion with respect to some of the data but only at the cost of worsening its assessment of the rest. For instance, lowering the curve would decrease the number of positive observations that are already in the invisibility area, but this would then raise more negative observations to the visibility zone. Similarly, any change to make the criterion more suitable to a certain range of latitudes would make it more unreliable for other latitudes. The observational

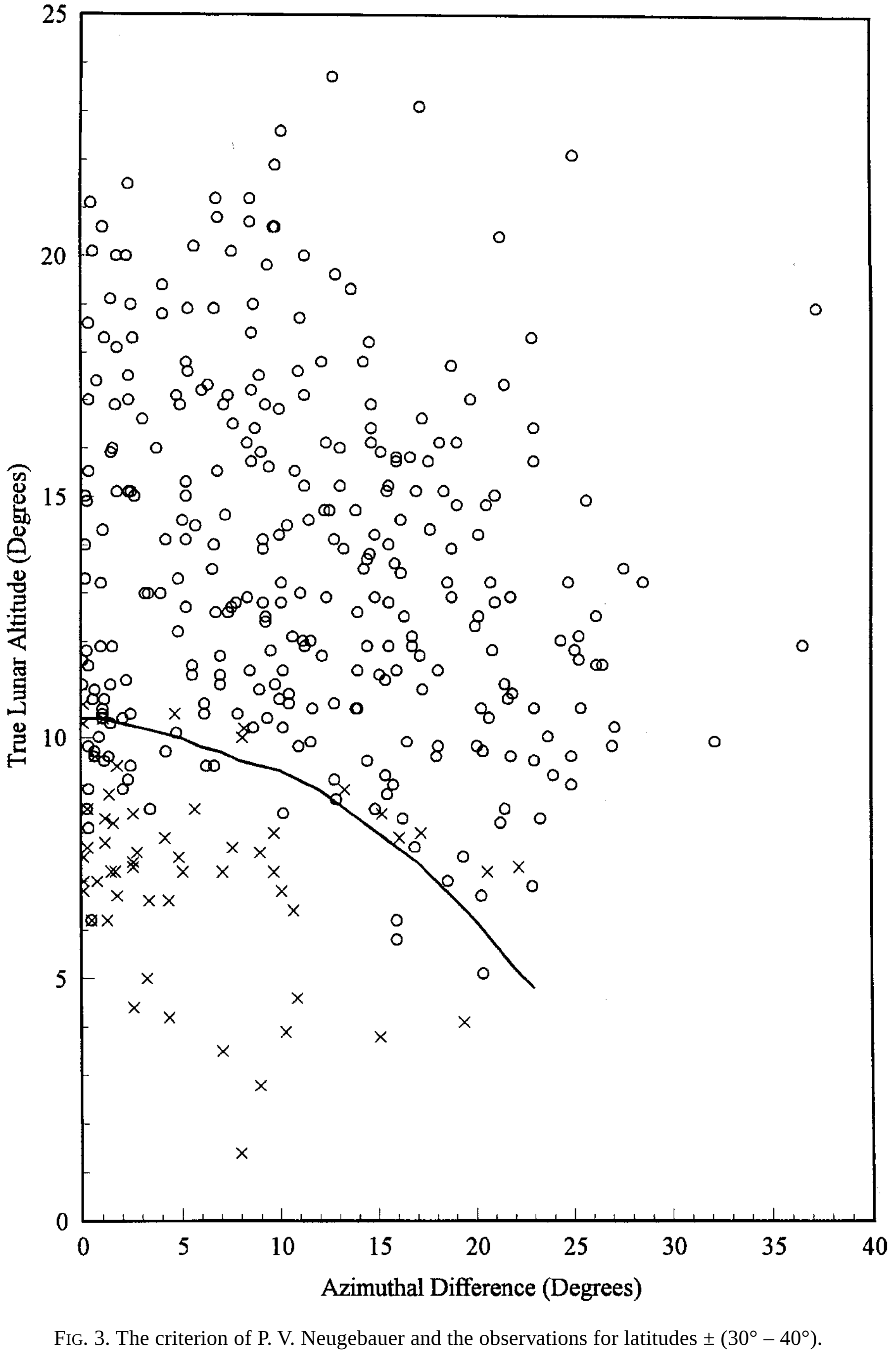


FIG. 3. The criterion of P. V. Neugebauer and the observations for latitudes ± (30° – 40°).

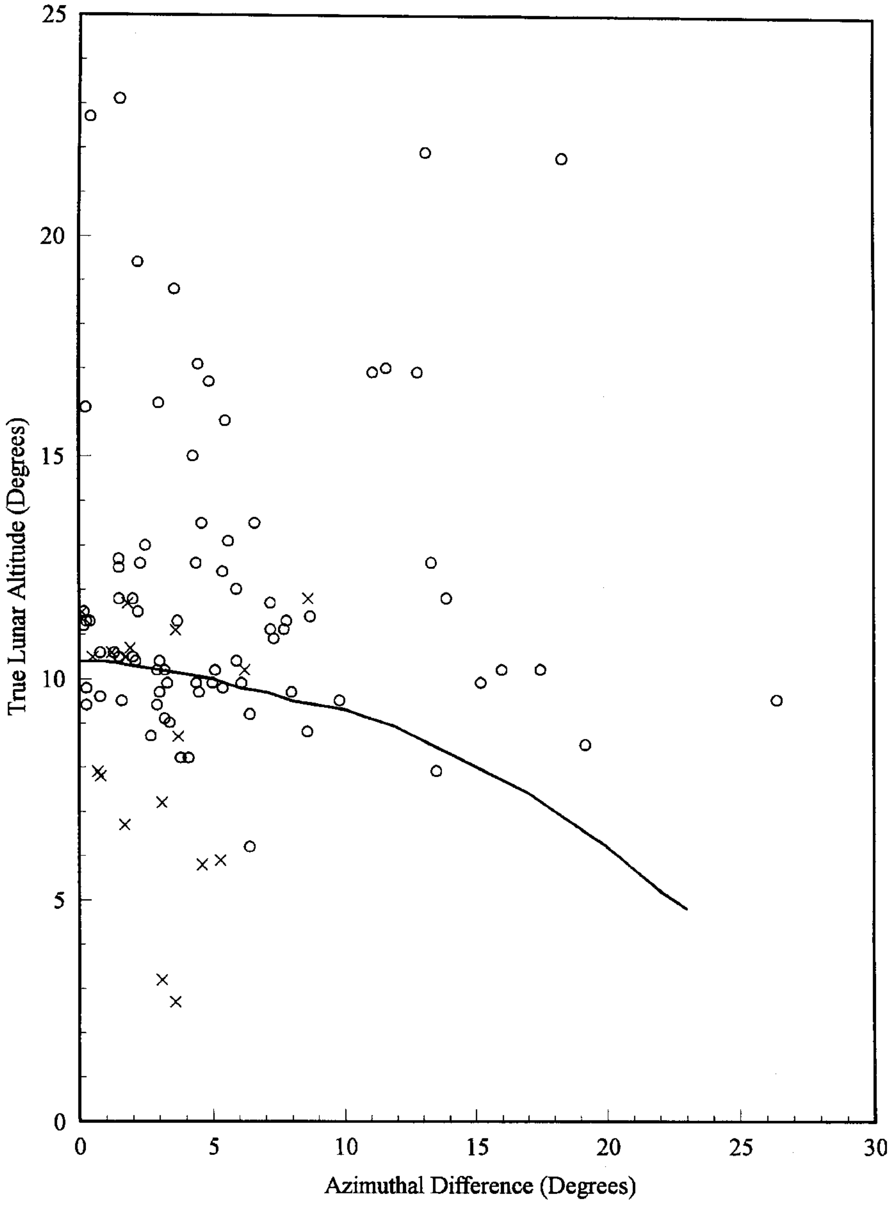


FIG. 4. The criterion of P. V. Neugebauer and the observations for latitudes other than ± (30° – 40°).

data show that at azimuthal difference of 0.5° a crescent that is as low as 6.2° has been seen. This is not an isolated observation. Another observer saw a crescent of 8.1° altitude and 0.4° azimuthal difference with the naked eye only. On the other hand, still for very small ΔZ, many crescents that are higher than 10° or even 11° have been missed. Therefore, it seems fair to say that the $h - \Delta Z$ criterion, in its present form, is itself inherently of limited utility for predicting the first visibility of the lunar crescent on the global level. Neither this criterion nor the L + S that *may* have been used by the Babylonians can be used confidently for predicting the first visibility of the lunar crescent.

REFERENCES

1. J. K. Fotheringham, "On the smallest visible phase of the moon", *Monthly notices of the Royal Astronomical Society*, lxx (1910), 27–531.
2. B. E. Schaefer, "Visibility of the lunar crescent", *Quarterly journal of the Royal Astronomical Society*, xxix (1988), 511–23; *idem*, "Lunar crescent visibility", *ibid.*, xxxvii (1996), 759–68.
3. L. E. Doggett and B. E. Schaefer, "Lunar crescent visibility", *Icarus*, cvii (1994), 388–403.
4. L. E. Doggett, P. K. Seidelmann and B. E. Schaefer, "Moonwatch — July 14, 1988", *Sky & telescope*, July 1988, 34–35.
5. J. Britton and C. B. F. Walker, "Astronomy and astrology in Mesopotamia", in C. B. F. Walker (ed.), *Astronomy before the telescope* (London, 1996), 42–67, p. 44.
6. A. J. Sachs, "A classification of the Babylonian astronomical tablets of the Seleucid period", *Journal of cuneiform studies*, ii (1948), 271–90.
7. J. Epping, *Astronomische aus Babylon* (Freiburg, 1889).
8. F. R. Stephenson and C. B. F. Walker, *Halley's Comet in history* (London, 1985).
9. A. J. Sachs and H. Hunger, *Astronomical diaries and related texts from Babylon*, i: *Diaries from 652 B.C. to 262 B.C.* (Vienna, 1988); ii: *Diaries from 261 B.C. to 165 B.C.* (Vienna, 1989); iii: *Diaries from 164 B.C. to 61 B.C.* (Vienna, 1996).
10. Sachs and Hunger, *op. cit.* (ref. 9, 1996), 25.
11. The phrases "identical with" and "followed", which describe the length of the month, have been introduced by Hunger in substitution of the very brief original cuneiform text.
12. F. R. Stephenson and L. J. Fatoohi, "The Babylonian unit of time", *Journal for the history of astronomy*, xxv (1994), 99–110.
13. Sachs and Hunger, *op. cit.* (ref. 9, 1988), 12.
14. F. X. Kugler, *Sternkunde und Sterndienst in Babel*, ii (Münster, 1909/1910), 547–50; O. Neugebauer, *Astronomical cuneiform texts: Babylonian ephemerides of the Seleucid period for the motion of the sun, the moon, and the planets*, i (Princeton, 1955), 39; Sachs and Hunger, *op. cit.* (ref. 9, 1988), 22.
15. L. J. Fatoohi and F. R. Stephenson, "Angular measurements in Babylonian astronomy", *Archiv für Orientforschung*, xliv/xlv (1997/98), 210–14.
16. F. R. Stephenson and L. J. Fatoohi, "Lunar eclipse times recorded in Babylonian history", *Journal for the history of astronomy*, xxiv (1993), 255–67.
17. Neugebauer, *op. cit*. (ref. 14), 39.
18. Stephenson and Fatoohi, *op. cit*. (ref. 12).
19. Sachs and Hunger, *opera cit.* (ref. 9).
20. Sachs and Hunger, *op. cit.* (ref. 9, 1988), 115.
21. *Ibid.*, 167.
22. *Ibid.*, 341.

23. Sachs and Hunger, *op. cit.* (ref. 9, 1989), 205.
24. *Ibid.*, 451.
25. Sachs and Hunger, *op. cit.* (ref. 9, 1988), 59.
26. *Ibid.*, 351.
27. Sachs and Hunger, *op. cit.* (ref. 9, 1989), 19.
28. Sachs and Hunger, *op. cit.* (ref. 9, 1996), 193.
29. R. A. Parker and W. H. Dubberstein, *Babylonian chronology 626 B.C. – A.D. 75* (Providence, 1956).
30. K. Schoch, “Astronomical and calendrical tables”, in S. Langdon and J. K. Fotheringham, *The Venus Tablets of Ammizaduga: A solution of Babylonian chronology by means of the Venus observations of the First Dynasty* (Oxford, 1928). For further details of the program, see Louay J. Fatoohi, “A computer program for the conversion of Babylonian into Julian dates”, *Journal for the history of astronomy*, xxix (1998), 378–9.
31. Sachs and Hunger, *op. cit.* (ref. 9, 1988), 49.
32. *Ibid.*, 305.
33. M. Chapront-Touzé and J. Chapront, “ELP2000-85: A semi-analytical lunar ephemeris adequate for historical times”, *Astronomy and astrophysics*, cxc (1988), 342–52.
34. *Ibid.*
35. J. O. Dickey *et al*., “Lunar laser ranging — a continuing legacy of the Apollo Program”, *Science*, cclxv (1994), 482–90.
36. F. R. Stephenson, *Historical eclipses and the rotation of the Earth* (Cambridge, 1997).
37. *Astronomical almanac*, 1992 (Washington, D.C.), K8.
38. L. V. Morrison and F. R. Stephenson, “Contemporary geophysics from Babylonian clay tablets”, *Contemporary physics*, xxxviii (1997), 13–23.
39. F. R. Stephenson and L. V. Morrison, “Long-term fluctuation in the Earth’s rotation: 700 B.C. to A.D. 1990", *Philosophical transactions of the Royal Society*, A, cccli (1995), 165–202.
40. P. Bretagnon, “Théorie du mouvement de l’ensemble des planètes: Solution VSOP82", *Astronomy and astrophysics*, cxiv (1982), 278–88; P. Bretagnon and J.-L. Simon, *Planetary programs and tables from –4000 to + 2800* (Richmond, Virginia, 1986).
41. P. Bretagnon and G. Francou, “Planetary theories in rectangular and spherical variables: VSOP87 solutions”, *Astronomy and astrophysics*, ccii (1988), 309–15.
42. Neugebauer, *op. cit.* (ref. 14), 41.
43. F. Bruin, “The first visibility of the lunar crescent”, *Vistas in astronomy*, xxi (1977), 331–58, p. 333.
44. See, for instance, M. Ilyas, “Lunar crescent visibility criterion and Islamic calendar”, *Quarterly journal of the Royal Astronomical Society*, xxxv (1994), 425–61; Schaefer, *op. cit.* (ref. 2, 1988).
45. D. King, “Some early Islamic tables for determining lunar crescent visibility”, *Annals of the New York Academy of Sciences*, d (1987), 185–225.
46. Neugebauer, *op. cit.* (ref. 14).
47. Neugebauer, *The exact sciences in Antiquity* (Providence, 1957), 115.
48. Table 2 is adapted from Neugebauer, *op. cit.* (ref. 14), 43.
49. *Ibid.*, 43.
50. *Ibid.*, 67 and 84; O. Neugebauer, *A history of ancient mathematical astronomy* (Berlin, 1975), 539–40.
51. Schaefer, *opera cit.* (ref. 2); Doggett and Schaefer, *op. cit.* (ref. 3).
52. Fotheringham, *op. cit.* (ref. 1).
53. Schoch, *op. cit.* (ref. 30), 95.
54. O. Neugebauer, “The Babylonian method for the computation of the last visibilities of Mercury”, *Proceedings of the American Philosophical Society*, xcv (1951), 110–16.

55. Schoch, *op. cit.* (ref. 30), 98.
56. J. K. Fotheringham, “The visibility of the lunar crescent”, in Langdon and Fotheringham, *The Venus Tablets of Ammizaduga* (ref. 30), 45–48, p. 48.
57. O. Neugebauer, *op. cit.* (ref. 54).
58. P. V. Neugebauer, *Astronomische Chronologie,* ii (Berlin and Leipzig, 1929), Table E 21.
59. Parker and Dubberstein, *op. cit.* (ref. 29).
60. P. J. Huber, “Astronomical dating of Babylonian I and UR III”, *Monographic journals of the Near East, Occasional papers*, i/4 (1982), 107–99.

JHA, xxx (1999)

BOOK REVIEWS

GALILEO'S *DIALOGO*

Galileo on the World Systems: A new abridged translation and guide. Translated and edited by Maurice A. Finocchiaro (University of California Press, Los Angeles, 1997). Pp. 448. $55 (hardcover), $19.95 (paperback).

Translations and abridgements of fundamental texts in the history of science are as important for contemporary students as they are difficult to accomplish. Maurice Finocchiaro has for some time been an important interpreter of Galileo, especially from the point of view of the philosophy of science. His *Galileo and the art of reasoning* (Reidel, 1980) is a comprehensive analysis of the arguments set forth in Galileo's *Dialogue on the two chief world systems.* More recently, in *The Galileo affair: A documentary history* (University of California Press, 1987), he edited and translated the principal documents concerning the theological and philosophical controversies associated with Galileo's defence of Copernican astronomy. His new translation and abridgement, as part of a special National Endowment for the Humanities program for "guided studies of historically significant scientific writings", is an excellent resource for both students and teachers. Since the original text is both long and difficult, Finocchiaro's book is especially welcome.

Finocchiaro's text contains about two-fifths of the original text along with substantial notes and commentary. He provides an extensive introduction in which he displays his excellent analysis of the intellectual context in which the book needs to be understood. The abridgement of the text is well done: Finocchiaro provides those portions of Galileo's text that go to the heart of the arguments concerning Aristotelian cosmology and the possibilities of the Earth's daily rotation and annual revolution around the Sun. Finocchiaro is especially alert to the rhetoric of scientific demonstration and the role of demonstrative claims in Galileo. Finocchiaro's own analysis is set forth in three appendices: on critical reasoning, methodological reflection, and varieties of rhetoric. An extensive glossary at the end contains key historical, philosophical, and scientific terms and concepts. The entire work reveals the clarity and thoroughness that is characteristic of Finocchiaro's writings.

Throughout the notes, commentary, and appendices we find Finocchiaro's well-established claim that Galileo, despite the rhetoric of demonstration, never succeeded in demonstrating that the Earth actually moves, even though such a goal was important to the Tuscan scientist. In this light, I think that Finocchiaro's decision to omit the brief opening discussion of the Fourth Day — the day in which the argument from the motion of the tides (as effect) to the double motion of the Earth (as cause) is set forth — is unfortunate. For here the type of argument that will follow is described: a search for 'true and primary causes' from which the effects (in this case the tides) necessarily follow. Here we can see evidence that for Galileo a true scientific demonstration seeks to discover a unique causal nexus.

The introduction, which includes a very good account of the geostatic world view and the Copernican controversy, helps to make this translation and abridgement an ideal text for courses in the history and philosophy of science. The extended commentary found in the footnotes adds significantly to the understanding of the text and also illuminates current scholarly discussion of Galileo's work. Throughout the translation, Finocchiaro provides succinct summaries of sections omitted in the abridgement as well as references to Favaro's Italian edition and to Stillman Drake's English translation.

Finocchiaro's new book is an exceptional achievement and those of us who teach undergraduates — not to mention those whom we teach — will benefit greatly from it.

Cornell College, Mount Vernon, Iowa WILLIAM E. CARROLL

ASPECTS OF THE EXACT SCIENCES IN ISLAM

From Baghdad to Barcelona: Studies in the Islamic Exact Sciences in Honour of Prof. Juan Vernet. Edited by Josep Casulleras and Julio Samsó (Annuari de Filologia (Universitat de Barcelona), xix (1996), B-2; Instituto "Millás Vallicrosa de Historia de la Ciencia Arabe", Barcelona, 1996). Pp. 830.

These two volumes contain the papers from a symposium of this title at the XIXth International Congress of History of Science held in Zaragoza in 1993. Eighteen well-credentialed specialists present papers that can provide a rich source of details for fellow historians. Severely scholarly in nature, these contributions represent the bricks from which the edifice of history of astronomy is built. Seldom do we glimpse here the larger outlines of the structure itself.

An exception is the opening paper, by George Saliba, who attempts to correct the commonly-received view that the transfer of Greek knowledge to the Islamic world basically took place through a golden age of translations in Baghdad in the first half of the nineteenth century, and that this was primarily a stepping-stone to their subsequent transfer to the Latin West. Using neglected material that had been available in English and German for nearly seventy years, Saliba paints a complex picture of transformation as well as transfer.

In the papers that follow (mostly in English, but a few in Spanish), we can learn particulars of astrolabes, of astronomical tables, of trepidation, and just a touch of mathematical astrology and magic squares. Among these one paper in particular caught my eye, John North's inquiry into "Just whose were the Alfonsine Tables?". Emmanuel Poulle has argued in the pages of this journal that they were essentially a Parisian invention from around 1320. He has compared the forms of the (no longer extant) Castilian Tables, as specified in the existing canons, with those tables now known as the Alfonsine, and has concluded that they are so different from any

Castilian predecessor that we must credit what survives to the Parisian school.

North, however, has probed deeply into the underlying parameters of the solar and precessional theories and has made a persuasive case that the Parisian tables, while original in form, are actually based on constants taken over from Alfonso's astronomers. Surely the last word has not yet been written on this fascinating controversy, but at the moment the pendulum seems to have swung back towards an essential input from the Alfonsine astronomers whose original tables still seem totally lost.

Harvard-Smithsonian Center for Astrophysics OWEN GINGERICH

THE ECLIPTIC IN ANCIENT EGYPT

Astronomische Konzepte und Jenseitsvorstellungen in den Pyramidentexten. Rolf Krauss (Harassowitz, Wiesbaden, 1997). Pp. xvi + 297. DM 118 (paperback).

Rolf Krauss, for two decades known as an authority on Egyptian chronology and philology, has here placed himself in the company of the four authors of the most voluminous scholarly books on ancient Egyptian astronomy issued this century, namely O. Neugebauer and R. A. Parker, C. Leitz, and M. Clagett. At the same time he ventured to extend the field of pertinent erudition back to the Egyptian Old Kingdom, a task requiring philological skill to a larger degree than was necessary for the later epochs covered by these other authors. Such a high philological standard is exemplified not only by Krauss himself but also by the four Egyptologists who acted as referees for this work, which was previously submitted as an *habilitation* thesis to Hamburg University.

Owing to this lengthy refereeing process the main features of its contents have been known to insiders for some years, a fact which, for example, enabled M. Römer to announce its central thesis in an article in the daily press in December 1994 ("Auf dem Himmelsgewässer" in the *Frankfurter Allgemeine Zeitung*, no. 290, p. N6), an article warmly recommended as a supplement to this review. This thesis states that the mythological Kha-canal that occurs dozens of times in the Pyramid Texts meant a "sinuous" or "oblique" band along the ecliptic, and its equally frequently mentioned ferrymen *Mẖntjw* are the Moon and certain planets. This claim puts the Egyptians' knowledge of the ecliptic back by two millennia.

I have become fairly convinced of it, though having some residual resistence mainly because of the occasionally occurring epithet "sinuous" of that ecliptical canal. A rejection of this central thesis would fatally invalidate about half of Krauss's numerous further astronomical interpretations, and any critic of it would be forced to deal with a considerable part of the author's 1197 predominantly philological footnotes.

Having often been one of Krauss's collaborators in minor questions, I have been able over many years to watch his steadily increasing familiarity with the sky and with astronomical notions. He has now achieved an astronomical reliability rarely encountered in scholars whose background is in the humanities. His 34 astronomical figures are correct and clear, with the exception only of no. 5c. They are computer-generated mainly in Chapter 12, where he argues for the identification of certain deities with Mercury and Venus, relying on their geometrical relations to the morning horizon as fairly clearly stated by the textual sources. This chapter is largely independent of the above-mentioned ecliptical hypothesis.

Even less dependent are Chapters 7 and 8, which deal with Orion and Sirius, whose identity with Egyptian deities has been largely incontestable since Plutarch, and is paralleled in textual astronomy of the Middle Kingdom. Finally, Chapter 15 gives a comprehensive summary of the preceding parts without their detailed argumentation, so that it may be helpful to read it first.

Römer's preannouncement had a side-effect scarcely intended by Krauss but no doubt helpful to the commercial success of the book. By uncritical readers the *ecliptic* thus put back by 2000 years has been readily confused with or conjecturally connected with the *zodiac*, though the textual sources clearly contradict any such idea. These numerous readers are recruited from the semi-academic wave of theosophy and esoterism, among whom Egyptology enjoys an increasing philosophical interest in general, but who in particular are eager to absorb any historical clue that might seem to prove their belief that their beloved *astrology* has roots far back in all ancient cultures. I see this trend as a valuable challenge to historians of astronomy, whom I recommend to look at Krauss's book also from this somewhat distorted standpoint.

Kantonsschule, Wetzikon, Switzerland KURT LOCHER

HARRISON'S RIGHTS AND WRONGS

The Quest for Longitude. Edited by William J. H. Andrewes (The Collection of Historical Scientific Instruments, Harvard University, Cambridge, Mass., 1996). Pp. 448. $75/£49.95.

This lavishly produced and beautifully illustrated volume presents the papers and the associated documents generated at a Harvard conference held in 1993 to mark the bicentenary of the birth of the Humberside clockmaker John Harrison. As its editor points out, appetite for longitude stories seems inexhaustible. It was this conference that also prompted Dava Sobell's bestseller, *Longitude*, a paean to Harrison's virtues and a chronicle of his sufferings. In Umberto Eco's recent bulky pastiche of Baroque travellers' tales, *The island of the day before*, visionary longitude schemes occupy pride of place. Several, such as the use of the weapon-salve at long range on a wounded dog, are discussed here by Owen Gingerich in a chapter on "nutty solutions to the longitude problem". The novelist Thomas Pynchon has also imagined a vast panorama of eighteenth-century navigation, surveying and astronomy, *Mason and Dixon*, which gives speaking parts to the marine chronometers tested by his protagonists in 1761 at the Cape and St Helena. Pynchon also satirically defends his eponymous heroes against the machinations of the then Astronomer Royal, Nevil Maskelyne.

Maskelyne and his colleagues have not emerged unscarred from these stories. In the volume under review, Harrison is privileged by eulogists and scholars. Chapters are devoted to the wooden clocks this ingenious carpenter made before 1730 where many of his key innovations were early introduced; to the designs of the series of clocks from H1 to H4 that he offered for the Longitude Prize set up by the 1714 Act; and, notably, to a passionate argument by Martin Burgess, who has rebuilt a version of Harrison's regulator using modern materials and methods, that the failure to recognize Harrison's achievements was due mainly to a "gross failure of human relationships". Several thus reproduce and justify Harrison's own much publicized sense of injury, of the prejudice of "priests and philosophers" against his masterly clocks. At least part of the appeal of this collection is the opportunity it offers to reconsider the roles of practical astronomy and chronometry, of navigational skill and instrument making, in the "quest for longitude".

This quest rather resembles a complex and branching network than a single moment of heroic insight. Readers of this journal will be familiar with the argument of J. A. Bennett (1993), but already present in the works of Rupert Gould (1923) and Humphrey Quill (1966), that John Harrison was a superb social operator who won enormous sums from the Board of Longitude, patronage from the monarch and reward from the Royal Society, and who, in the phrase of Gould cited here in a fine paper on late eighteenth-century French horology by Catherine Cardinal, "built a wonderful house on the sand". Harrison certainly showed the plausibility of the chronometric method, but it was the designs of Pierre Le Roy and the remarkable production techniques of John Arnold and Thomas Earnshaw that at last turned the method into a viable approach capable of providing reliable clocks for hosts of mariners.

An historian of astronomy will be impressed by the argument made here by Albert van Helden that observations of the eclipses of Jupiter's moons, especially as systematized by the efficient programme of Giovanni Cassini between 1668 and the 1690s, provided the key technology for land-based longitude measures. This prompted ambitious early seventeenth-century programmes from Gassendi and Peiresc, the foundation of the Paris Observatory in the 1660s, French then global geodesy, and the key debates on light speed started by Cassini and Ole Roemer in the 1670s.

Derek Howse complements this valuable reminder with his account of the lunar longitude method, favoured by Maskelyne and his coterie, the method of choice for navigators until the mid-nineteenth century. This choice relied on the co-ordination of Hadley's quadrant, Tobias Mayer's lunar tables (1755), and the star tables of John Flamsteed and of Nicolas-Louis de Lacaille (1757) in handbooks released from Paris and Greenwich. Though the best chronometric methods offered double the accuracy of the lunar method, the latter was still often required to calibrate marine clocks. As Eric Forbes once neatly put it, at the key meeting of the Board of Longitude in February 1765 where Maskelyne's plan for a Nautical Almanac was endorsed and the performance of H4 decisively assessed, the Board acutely judged that Harrison had made the chronometric method practicable but not generally useful — so he was awarded half the maximum prize; while the astronomers had made the lunar method generally useful but not yet practicable — so Mayer's heirs were awarded half the minimum prize, and, thanks to remarks by Alexis-Claude Clairaut published in May 1765, Leonhard Euler was compensated for his aid with Mayer's tabulations.

Polemics about Harrison's sufferings and the claims of the makers and theorists too often simply revisit the debates of that year. Add to all this the significance of the superb cartography that enabled early modern mariners to travel the world without a direct longitude method (a theme less thoroughly treated in this collection); the fascinating stories, given careful attention here by David Penney, of artisan skill in the metropolitan clockmakers' shops of Paris and London; and the eventual development of global positioning systems during this century. It is not hard to see why the story of the quest for longitude rightly maintains a central place in our understanding of science, technology and society.

Darwin College, Cambridge SIMON SCHAFFER

ESSAYS IN THE HISTORY OF HOROLOGY

Of Time and Measurement: Studies in the History of Horology and Fine Technology. A. J. Turner (Variorum, Aldershot, 1993). Pp. xii + 301. $103.95.

In his preface Anthony Turner acknowledges the likelihood that collections of republished articles will be "marginalised by polite non-reviews". The present review will confirm this opinion, but it is difficult to do more. This is a disparate collection, despite the relative unity of the subject: the pieces were written for different purposes and different audiences and, although there is a common character that derives from the individuality of the author, there is, of course, no sustained narrative and no directional argument. How could there be? Even the retention of the variety of layouts, typefaces and paginations signals a diversity of sources and intentions, while in terms of impact on the subject, the important articles have already achieved this. The reviewer is thrown back on the staple observation that it is convenient to have them ready to hand in an accessible format. If this is marginalization, the author can hardly blame the reviewer.

As for the facts, there are 22 articles originally published between 1972 and 1990. They range from notes of a page or two to major and influential articles, but most, by the author's characterization, are "investigations into details". They are grouped between sun-dials, mechanical horology and precision instrumentation.

The highlights are the accounts of William Oughtred's "horizontal instrument", of the historical background to the chronometry of Ferdinand Berthoud, and of the instrument making of Philippe Danfrie, together with one of the earliest but most influential among the articles included, the much-cited "Mathematical instruments and the education of gentlemen", originally published in 1973.

Readers of *JHA* may be most interested in, and possibly least familiar with, "The pre-history, origins and development of the reflecting telescope" from the obscure *Bollettino del Centro Internazionale di Storia della Spazio e del Tempo*. There are some very interesting points here that deserve elaboration. Only with Newton and James Gregory, for example, does Turner feel that we have a scheme for a practical telescope, rivalling the refractor, instead of intriguing consequences of the optical properties of mirrors coupled with experimentation on burning glasses. For him the latter characterization covers the period from Digges to Mersenne. There are also promising suggestions about the diffusion of technical information on the making of reflectors into the London trade and the rise of commercial manufacture in this area.

But the article also illustrates a problem with this form of republication, namely that there is little scope for the kind of improvements that would come with a new edition. This may be a particularly unfortunate example, since it appeared first in a non-English publication, but it is littered with mistakes — mostly misspellings and typographical errors, but also lack of clarity and signs of hurried production.

Despite these reservations, and at the risk of resorting to the commonplace observation, I have to admit that I will find it useful to have a good number of these articles conveniently to hand.

University of Oxford JIM BENNETT

LAUDING THE NRAO

The History of Radio Astronomy and the National Radio Astronomy Observatory: Evolution Toward Big Science. Benjamin K. Malphrus (Krieger Publishing Co., Melbourne, Florida, 1996). Pp. vi + 210. $49.50.

This book, an outgrowth of an Ed.D. thesis in education at West Virginia University and published near the Cape Canaveral spaceport, is a celebratory history of the U.S. National Radio Astronomy Observatory (NRAO) as seen through the development of its major radio telescopes over the period 1956 to the present. Basing himself primarily on internal and annual NRAO reports and the published astronomical literature, Malphrus unremittently gushes on with statements such as "The Green Bank interferometer system ... has carried out a wide variety of excellent research and made significant discoveries". Although not a house publication, it could well have been produced by the NRAO public relations folks. The book is of value primarily for its excellent illustrations, and as a starting outline for whoever might want to do a critical history of this important institution.

Malphrus's first chapter sets the stage for the establishment of NRAO in the late 1950s by looking at the origins and development of radio astronomy. The account, however, is full of errors and misconceptions, starting with the preface's first sentence: "The science of radio astronomy began in the 1930s with Karl Guthe Jansky's discovery of radio emission from the galactic nucleus." The term radio astronomy was not even coined until 1948 and it was still many more years before one had a recognizable specialty (and never a science). And Jansky was able to establish only that the radio emission was coming from the general direction of the galactic centre, not precisely from its nucleus. On p. 10 Grote Reber's remarkable 31-ft diameter dish antenna, built in his backyard in Illinois in 1937, is illustrated and captioned as "the first true radio telescope" without any discussion of the problematic meaning of 'true' or the astounding idea of combining the words 'radio' and 'telescope' together (which also did not happen until the late 1940s). Another example (p. 23) is the citation of Joe Pawsey and John Bolton as examples of men who represented a "diffusion of the great minds" from one research group to another. In both cases Malphrus starts them off in Cambridge (England), inferring that they were part of the leading postwar radio astronomy group there under Martin Ryle. But in fact Pawsey obtained his Ph.D. at Cambridge for ionospheric work and returned to his native Australia before the War, and Bolton was only a wartime physics undergraduate at Cambridge before joining the Royal Navy and eventually emigrating to Australia. This first chapter is the only one that pays any attention to the rest of the world besides NRAO; once Malphrus begins tracking NRAO's history, with rare exception is any broader context ever drawn from other institutions, research groups, or nations.

Chapter 2 describes the fascinating debates surrounding the creation of a national radio observatory, with key players such as Merle Tuve, Lloyd Berkner, Alan Waterman of the National Science Foundation, and the U.S. Navy (through the ill-fated Sugar Grove 600-ft dish project). But most of this chapter, the most satisfying to a historian's sensibilities, is simply a retelling of Alan Needell's excellent study published in *Osiris* in 1987. Nevertheless, one still encounters statements such as "by 1954 there was a pervasive feeling that the US lagged behind other countries in astronomy" — tell that to the rest of the world envying the Mt Wilson 100-inch and Palomar 200-inch telescopes! For some reason the author also feels compelled to insert a couple of paragraphs about how the NRAO staff have always been proud that "research in radio astronomy [as opposed to nuclear physics] has no malevolent applications", and yet he himself points out the Navy's use of NRAO telescopes for precise astrometric work — why does he think the Navy wanted precise navigational and missile-aiming capabilities?

In sum, this is a book of little value for historians of science. Despite the term "Big Science" in his subtitle, Malphrus never analyses the concept in the least, apparently thinking it unproblematic; nor does he ever cite any of the voluminous historical literature on scientific institutions and postwar science. Instead, one is left with ahistorical conclusions such as “[NRAO’s] cutting edge instrumentation has allowed frontier research from which major contributions logically followed”. Enough said.

University of Washington, Seattle WOODRUFF T. SULLIVAN, III

MEASURING MAGNITUDES

The Measurement of Starlight: Two Centuries of Astronomical Photometry. J. B. Hearnshaw (Cambridge University Press, Cambridge, 1996). Pp. xiv + 511. $95/ £65.

If only the photoelectric cell (or the CCD) had been invented two millennia ago! As John Hearnshaw’s fine book so clearly underscores, a prodigious amount of time and effort was spent first on defining a magnitude system that astronomers slavishly linked to the 6-magnitude system used by Ptolemy in his *Almagest*, and then searching for ways to measure magnitudes accurately using both the eye and the photograph. As late as 1950, Stebbins, Whitford and Johnson showed that a scale error of about 0.6 magnitudes still persisted in the photographically-determined magnitudes for faint stars in the standard Selected Areas. Mercifully, five years later, the International Astronomical Union put to rest the International System of magnitudes and the use of the North Polar Sequence.

The first five chapters, just half of *The measurement of starlight*, takes us chronologically through this difficult and frustrating quest, beginning with the first use of stellar magnitudes followed by the various ways of measuring them visually. This work resulted not only in several important star catalogues, the *Bonner Durchmusterung* being the best known, but also the discovery of several hundred variable stars. Chapter 4 describes the early days (1839–1922) of photographic photometry, and Chapter 5 picks up the story of the origins of “photoelectric” photometry, where Hearnshaw uses this word to include photoconductive and photovoltaic cells as well as the photoelectric effect. This chapter takes us from 1892 to 1945 and the advent of the photomultiplier.

The scene then switches to “Photometry at longer wavelengths” beginning with William Herschel’s discovery of “ultra-red” radiation from the Sun announced in the year 1800. This chapter continues to 1970 by which time the galactic nucleus had been discovered by Becklin and Neugebauer and circumstellar dust had been detected around several stars. Meanwhile, back in the darkroom, photographs were still being processed at a great rate, and Hearnshaw summarizes the on-going struggle to measure magnitudes accurately, finally aided by the photoelectric multiplier with which faint stars in the plate fields could be measured and used as standards.

Beginning in the twentieth century, a number of enormously important developments came about photographically despite the calibration problems: the explosion of the discovery of variable stars, especially in globular clusters and the Magellanic clouds, Russell's and Shapley's studies of binary stars, the outlining of the Galaxy, and Henrietta Leavitt's "remarkable discovery" of the period–luminosity law for Cepheid variables. The final two chapters describe, respectively, the post-Second World War successes of photometry made with photoelectric photomultipliers, and image tubes and television systems, bringing us up to 1970, the year of the first mention of the charge-coupled device (CCD) by Boyle and Smith at Bell Laboratories and the cut-off date of the book.

Hearnshaw has successfully avoided making this history a dreary compilation of abstracts, and has nicely linked together the steps that have led photometrists to 1970 by which time accurate magnitudes and colours had been established over some 50 magnitudes — a factor of 1020 in brightness. Furthermore, he has made the reading more lively by including excerpts from oral histories. Some of the remarks of Joel Stebbins, "one of the most notable of the early pioneers in stellar photoelectric photometry" — and also one of the most humorous — are real treasures.

In my opinion Hearnshaw has done a great service by weighting properly, I believe, the relative importance of European and American researches, and he notes that much solid research was stifled in Europe because many observatories devoted inordinate amounts of time to the making of the ill-fated *Carte du Ciel*. Hearnshaw also notes the on-going East Coast–West Coast rivalry in twentieth-century USA. As an undergraduate at Harvard in the late 1940s, I was able to experience both of these since the oft-mentioned Harlow Shapley, Bart Bok, and Cecilia Payne-Gaposchkin were all still active and visitors frequently came from overseas to speak of their work.

One notable feature of *The measurement of starlight* is the large number of references: I counted 1,825 of them. Also excellent are the name and subject indices, which have obviously been made with some care.

On the down side, I was annoyed that a number of the figures, especially graphs, had no labels on their axes; one often has to guess at what was being plotted. The only other substantive criticism I have concerned "the author's personal selection", which left out practically all mention of, for example, Hubble's demonstration of the extragalactic nature of the spiral nebulae, surely one of the larger milestones in twentieth-century astrophysics; in the entire book, Hubble receives only two brief references. Also, I would have at least mentioned Dorrit Hoffleit's numerous contributions and Arlo Landolt's important establishment of UBV photoelectric sequences along the celestial equator begun in the 1960s.

This carefully researched and edited book should be much enjoyed by modern-day photometrists and by astronomers interested in the development of one vital part of astronomy. I highly recommend it to all of them.

Instituto Isaac Newton, Santiago, Chile WILLIAM LILLER

THE *DSB* ON LAPLACE REPRINTED

Pierre-Simon Laplace, 1749–1827: A Life in Exact Science. Charles Coulston Gillispie with the assistance of Robert Fox and Ivor Grattan Guiness (Princeton University Press, Princeton, New Jersey, 1998). Pp. xii + 322. $49.50/£35.

The volume under review is a largely verbatim reprint of the lengthy article on Laplace in the first supplement to the *Dictionary of scientific biography.* Apart from a few new references and some light rewriting, the text dates from 1978. Gillispie and his co-authors came to compile the entry and to place it in the supplement because the contributor first engaged to write Laplace's life had not finished when the volume that was to contain it went to press. No doubt the assignment was burdensome within an article of standard size. Laplace's writings were many and varied, his bibliography twisted, and his life apart from his career poorly documented. As editor-in-chief of the *DSB*, Gillispie could allow himself the space necessary to summarize the writings and straighten out the bibliography. The life remains to be written.

The reprint has the merits over the original of a convenient format and a useful index. No advantage was taken of the opportunity to introduce illustrations or diagrams or to ease entry into Laplace's work for readers unprepared for the rigours of the *DSB*. Gillispie and the Princeton University Press deserve thanks for downsizing and indexing the original article but not for giving the impression that the result is a new work. The assertion on the jacket blurb, that the contributions of Gillispie *et al*. will not be "duplicated" in our time, is good puffery but bad history.

Worcester College, Oxford J. L. HEILBRON

A SIXTEENTH-CENTURY VIENNESE INTELLECTUAL

Humanismus zwischen Hof und Universität: Georg Tanstetter (Collimitius) und sein wissenschaftliches Umfeld im Wien des frühen 16. Jahrhunderts. Franz Graf-Stuhlhofer (Schriftenreihe des Universitätsarchivs, Band 8; WUV-Universitüts Verlag, 1996). 268 Schilling (paperback).

A Bavarian by birth and a master of arts from Ingolstadt, Georg Tanstetter Collimitius (1482–1535) went on to become the leading teacher of astronomy and a well-connected figure in early sixteenth-century Vienna. (The word "Collimitius", which is sometimes fashionably appended to his name, is the latinization of his hometown, Rain, which means "border".) His students included Petrus Apianus (the author of the spectacular *Astronomicum Caesareum* of 1540), and the Swiss humanist Joachim Vadian was one of his best friends. He was also Emperor Maximilian's deathbed physician (1518–19), an advisor and *Leibarzt* to King Ferdinand of Habsburg and his children, a computer of calendars, an editor, and an author in his own right. Not least, he contributed, as a biographer, to the history of astronomy in fifteenth-century Vienna.

In 1980, Franz Stuhlhofer wrote a dissertation on Tanstetter for the University of Vienna. He has now updated the fruits of his research in a more accessible form, published under his hyphenated married name. The result is likely to remain for some time the definitive reference on Tanstetter's life. Indeed, the systematic and exhaustive collection of information assembled here suggests that Tanstetter probably deserves an engaging narrative biography, in which his activities could be fleshed out in the full richness of their context.

As the title hints, Tanstetter is significant for several reasons. As an important academic, he held prominent teaching and administrative positions at the University of Vienna (including professor of astronomy and medicine, vice-chancellor, dean, and rector). But his activities also illustrate the interaction between university and court, medicine and astrology, and late-medieval and humanist trends, to say nothing of their interest for the history of publishing and historiography. Since he resists stereotyping according to traditionally polarized historiographical and disciplinary categories, his life offers an opportunity for an intriguing and salutary case-study.

The book surveys all extant materials known to the author, and these encompass an impressive range. Tanstetter is responsible for some twenty calendars, almanacs, and *judicia* (1504–1526, several of them directed to the Viennese city fathers); a proposal for calendar reform; a plague treatise for 1521; a map of Hungary; university lectures on astronomy/astrology (including notes on the astronomical sections of Pliny); editions of works in the mathematical sciences (from Proclus to Peurbach and Regiomontanus, through Witelo, the latitude of forms, and John of Murs); and — last and least — some forgettable poetry honouring a deceased youth with the unforgettable name of Arbogast Strub.

The centre of gravity of Tanstetter's activities was astrology, an endeavour that connected his astronomical work, his involvement with the imperial court, his publishing ventures, and his later medical career. Among contemporaries, he was renowned for his prediction of the Emperor Maximilian's death six years before it occurred.

For all of his multifarious activities, Tanstetter is remembered today primarily as an historian of astronomy and editor of astronomical works. His *Viri mathematici quos inclytum Viennense gymnasium ordine celebres habunt* (Vienna, 1514) is a biographically-organized history of Viennese astronomy in the fifteenth century, from Henry of Langenstein (d. 1397) to Tanstetter's own times. It reflects Tanstetter's pride in belonging to an institution with such a glorious tradition. Graf-Stuhlhofer conveniently provides the Latin text and a translation of this short, relatively rare work.

Graf-Stuhlhofer raises once again the much-vexed problem of "humanist science", largely following in the footsteps of Helmuth Grössing's *Humanistische Naturwissenschaft* (Baden-Baden, 1983). He assigns Tanstetter an intermediate position between the extreme “ideal types” of the humanist and the scholastic. Tanstetter’s scholarly work, for example, is not oriented to Antiquity. On the contrary: apart from Proclus’s *Sphere* and his own comments on Pliny, his publishing efforts encompassed what now pass for “medieval” works. Graf-Stuhlhofer nevertheless argues that Tanstetter saw himself as a humanist, categorizing him as a “humanist dove”, not an agressive “humanist hawk”. Just the same, he suggests that in terms of content one “should scarcely expect clearly recognizable differences between scholasticism and humanism” (p. 108) — apparently a distinction without a difference.

In outlining Tanstetter’s life, Graf-Stuhlhofer sheds light on poorly-understood aspects of early sixteenth-century university life in Vienna. In particular, he examines the fate of the famous “College of Poets” founded by Conrad Celtis, a Habsburg-funded college associated with the university and outfitted with two chairs of poetry and rhetoric and two in the mathematical disciplines. He hypothesizes that this college functioned as a kind of fourth higher faculty, effectively a graduate school for the Faculty of Arts. Although scholars have doubted that the college survived beyond Celtis’s death (1508), Graf-Stuhlhofer plausibly suggests that it did: indeed, it would explain the contemporary designation of Tanstetter as “ordinary professor of mathematics”, despite the lack of references to such teaching in the Acts of the Faculty of Arts itself.

While Graf-Stuhlhofer alludes to Tanstetter’s “reflection of contemporary politics and mentality” (p. 15), he does not develop these themes very much in the book. The most sensitive exception is perhaps his brief treatment of Tanstetter’s ambiguous relation to the religious upheavals of the day. While his friend and correspondent Vadian — now back in Switzerland — had given his allegiance to the emergent Reformation, Tanstetter the Habsburg advisor was not surprisingly reticent on the subject. The book is well-organized and easy to use. It is helpfully equipped with separate indices for proper names and all names appearing in Tanstetter’s books. The twenty-two illustrations include the known and presumed portraits of Tanstetter, and the title-pages of his books. In short, this is a useful reference work on a prominent intellectual, teacher, and courtly advisor in sixteenth-century Vienna.

University of Wisconsin–Madison MICHAEL H. SHANK

THE PERIHELION OF MERCURY

In Search of Planet Vulcan: The Ghost in Newton’s Clockwork Universe. Richard Baum and William Sheehan (Plenum Publishing Corporation, New York, 1997). Pp. x + 310. $28.95.

The subject of this interesting and clearly written volume is actually broader than its title suggests. Not until halfway through the volume do we reach the problem of

Vulcan. The first half, the best popular history of celestial mechanics I have seen, describes in vivid terms the problems in celestial mechanics from Newton to the discovery of Neptune in the mid-nineteenth century. In particular, it details how the challenges to Newtonian gravitational theory were overcome one-by-one, from Clairaut's triumphant explanation in 1749 of the motion of the lunar apsides, to his prediction (accurate within 33 days) of the 1759 perihelion passage of Halley's comet. It includes Laplace's solution in 1784 to the problem of the great inequality in the orbits of Jupiter and Saturn, and finally Le Verrier's prediction of the location of the Neptune.

All of this gives us a much greater appreciation for the intransigent problem treated in the second half of the book, the advance in the perihelion of Mercury, which is thus properly placed in the context of the history of celestial mechanics. Le Verrier is the hero of this story, but there is much prior and lesser known history as well. Predictions of the transits of Mercury (which occur considerably more frequently than transits of Venus) gradually improved with respect to observation, from one-day discrepancies in 1707, to hours in 1753, to 53 minutes in 1786, to 16 seconds in 1845 based on Le Verrier's prediction. In 1859 Le Verrier began his assault on Mercury, comparable to Kepler's famous "battle with Mars" 250 years earlier involving eight-minute-of-arc discrepancies. After taking into account the theory of the Sun and errors of observation, Le Verrier found that planetary perturbations accounted for 527 arcseconds per century in the advance of Mercury's perihelion, leaving 38 arcseconds per century unexplained (later shown by Simon Newcomb to be 43 arcseconds/century). This gave rise to his hypothesis that unknown masses must exist between Mercury and the Sun, a suggestion that set off a 20-year observing frenzy. Already by 1860 the name "Vulcan", the Roman god of fire, was given to the hypothetical planet, even though no one knew whether it might be a single object or many.

Le Verrier did not wait long for confirmation; in early 1859 the French country doctor Edmond Lescarbault claimed to have observed the transit of such an object across the Sun. This might not have had much historical effect, but after severe questioning by Le Verrier of the details of Lescarbault's observations, the Paris Observatory Director made the triumphant announcement of a new intramercurial planet to the Académie des Sciences. This was only the first of numerous observational claims, in a saga that Baum and Sheehan describe in considerable detail. In the United States James Craig Watson became the champion of Vulcan, and claimed actually to have observed it during the 1878 solar eclipse. He was bitterly opposed by C. H. F. Peters, among others, but was supported by an observation of the comet discoverer Lewis Swift. Further confirmation remained elusive, however, and even Le Verrier realized that twenty Vulcans (based on a mass deduced from Lescarbault's measurement of Vulcan's diameter) would be needed to explain the anomalous motion of Mercury. As more eclipses passed without confirming observations, Vulcan passed into history. As the authors point out, the whole Vulcan episode is another illustration of (in David Brewster's words) "those illusions of the eye or of the brain, which have sometimes disturbed the tranquillity of science”. Sheehan has documented plenty more in his book *Planets and perception.*

But the compelling story of this volume is that of celestial mechanics, for as we all know, the advance in the perihelion of Mercury in the end could not be explained by classical Newtonian physics. The answer came not from observational astronomers, but from a physicist, Albert Einstein, whose General Theory of Relativity finally explained the infamous 43 arcsecond anomaly. That story, beyond the scope of this book, has been well-told elsewhere. But Baum and Sheehan remind us that the search for bodies “felt” before they have been seen continued with the search for a trans-Neptunian planet, the trans-Plutonian Planet X, and continues in ever more subtle form — and apparently finally successfully — with the detection of gravitational wobbles induced by extrasolar planets. With the latter success, celestial mechanics now has before it the prospect of charting entirely new solar systems, a process that is already bringing fresh challenges for gravitational theory.

Although the authors state that this book is not intended as an academic history, it is nevertheless well documented, full of insight, and (unlike many weightier tomes) a joy to read.

U.S. Naval Observatory STEVEN J. DICK

EINSTEIN AND THE EINSTEIN TOWER

The Einstein Tower: An Intertexture of Dynamic Construction, Relativity Theory, and Astronomy. Klaus Hentschel, translated by Ann M. Hentschel (Cambridge University Press, Cambridge, 1998). Pp. xiv + 226. $45.

Whereas a whole publication industry has been erected around Einstein and his theory of relativity, much less historical attention has been devoted to the more secondary figures in the history of relativity and even less to the institutions that were involved in the history. Klaus Hentschel’s book, a translation of the German 1992 original, is related to the Einstein literature but has neither Einstein nor his theory as its focal subject. The main part of the book is devoted to an astronomer, Erwin Finlay Freundlich, and an institution, the Potsdam solar observatory known as the “Einstein Tower”. Contrary to what is often believed, Einstein was greatly interested in the possibilities of testing his general theory of relativity. On the basis of his 1911 theory he had found that a ray of light passing the limb of the Sun should be deflected an angle of $0''.85$, or half the value predicted by the fully developed theory of 1916. Influenced by Einstein, the young Berlin astronomer Freundlich decided to test the “Einstein effect” and soon became Einstein’s mouthpiece in the generally conservative German astronomical community.

Hentschel gives a fine biographical account of Freundlich, a passionate advocate of relativity but also a difficult person who was constantly engaged in controversies and whose career was anything but smooth. Through Freundlich's life story we get to know not only the person but also, and even more interestingly, his social and professional environments. Hentschel provides a fascinating insight in the Berlin astronomical community during the Weimar republic and the dramatic changes that followed in 1933 when the National Socialists came to power. The book is not an ordinary biography of a scientist. As indicated by its title, it is as much concerned with the observatory that Freundlich started to establish in 1919 with the explicit purpose of testing Einstein's theory. The Einstein Tower in the Berlin suburb Potsdam was modelled after the 150-ft tower telescope at the Mount Wilson Observatory. What made it famous and brought it on the front page of the news magazines was not its scientific results (which were disappointing) but its innovative design, the creation of the architect Erich Mendelsohn. This is the third of the book's main themes. Hentschel interweaves in the scientific, institutional, and political history of the 1920s an informative and interesting account of German architectural history with Mendelsohn in the focus.

The Einstein Tower is contextual history at its best. Contrary to many other works in the fashionable contextual genre, Hentschel does not merely deal with the contexts but also with what these contexts are about, that is, the scientific questions. And he does so expertly and in considerable detail. There are many detailed historical examinations of the early solar eclipse expeditions aiming at testing Einstein's light-bending prediction (e.g., by J. Crelinsten, D. Moyer, J. Earman and C. Glymour) and Hentschel provides additional information about the later expeditions in the 1920s. When Freundlich finally succeeded in measuring the light deflection, in Sumatra in 1929, he found a value considerably larger than the predicted $1''.75$. An undisturbed Einstein dismissed Freundlich's result as being due to an "erroneous calculation of the experimental results".

Hentschel argues that in order to understand an episode in the history of science we should pay attention not only to the main actors, say Einstein or Eddington, but also to the more secondary figures (such as Freundlich and his antagonist Hans Ludendorff) and the entire network of people, instruments and institutions in which they operated. This view, neither particularly novel nor particularly controversial, may or may not lead to good and interesting history. It all depends on the way it is practised. In the case of Freundlich and the Einstein Tower, as analysed by Hentschel, this kind of "common folk's history" works well. Contributing to the fine result is Hentschel's meticulous scholarship, his careful examination of often obscure archival sources, and the many illustrations. It is not the author's fault that Stanford University Press charges $45 for a book of less than 250 pages.

Aarhus University HELGE KRAGH

JHA, xxx (1999)

NOTICES OF BOOKS

Tycho Brahe: Instruments of the Renewed Astronomy. Alena Hadravova, Petr Hadrava and Jole R. Shackelford (Institutum Studiis Classicis Promovendis, Academia Scientiarum Rei Publicae Bohemorum, Prague, 1996). Pp. xvi + 175. (Paperback.)

Tycho Brahe: Prístroje Obnovene Astronomie. Alena Hadravova, Petr Hadrava and Jole R. Shackelford (Institutum Studiis Classicis Promovendis, Academia scientiarum Rei Publicae Bohemorum, Prague, 1996). Pp. xvi + 188. (Paperback.)

These are handy new editions of Tycho Brahe's *Astronomiae instauratae mechanica* (1598), one in Czech and one in English (revised somewhat from the 1946 translation by Raeder, Strömgren and Strömgren). The Czech version is a little longer because it includes the poems omitted in the English translations. The English version does however include the dedication to Rudolf II omitted in the earlier English version.

Dictionary of Minor Planet Names. Lutz D. Schmadel (Springer-Verlag, Heidelberg and New York, 1997). Pp. xii + 940. DM 168.

This huge revised compendium lists the discoverers and discovery dates of the first 7041 asteroids, and gives succinct information about the source of the names. A fascinating amount of miscellaneous detail is included, from obituary references of some of the honorands to a list of the most prolific asteroid finders.

Doomsday Asteroid: Can We Survive? Donald W. Cox and James H. Chestek (Prometheus Books, New York, 1996). Pp. 337. $26.95.

A popular book by two space enthusiasts, riding on the wave of interest generated by several close approaches and the dinosaur extinction conclusions.

Exploring the Unknown: Selected Documents in the History of the U.S. Civil Space Program, ii: *External Relationships*. Edited by John M. Logsdon with Dwayne A. Day and Roger D. Launius (National Aeronautics and Space Administration, Washington, D.C., 1996). Pp. xxxvi + 636. $40.

A thick book of documents, ranging from congressional testimony to memoranda for the President, recording both civilian–military rivalries and the NASA-university-industrial interplay. Included is a useful biographical appendix.

Exploring the Unknown: Selected Documents in the History of the U.S. Civil Space Program, iii: *Using Space*. Edited by John M. Logsdon with Roger D. Launius, David H. Onkst, and Stephen J. Garber (National Aeronautics and Space Administration, Washington, D.C., 1998). Pp viii + 608. $41.

A continuation of the document series, with essays on the history of satellite communications and observing the Earth from space.

Aiming at Targets: The Autobiography of Robert C. Seamans. Robert C. Seamans, Jr (National Aeronautics and Space Administration, Washington, D.C. 1996). Pp. x + 291. $25 (paperback).

Seamans was a Deputy Administrator of NASA during the Apollo period; this lively account is part of the ever-burgeoning NASA History Series.

Stages to Saturn: A Technological History of Apollo/Saturn Launch Vehicles. Roger E. Bilstein (National Aeronautics and Space Administration, Washington, D.C., 1996). Pp. viii + 511. $37 (paperback).

A reprint of a 1980 study of the development of the Saturn launch vehicle used in the Apollo program; this is another of the NASA History Series.

L'Astrolabe. Raymond d'Hollander (Les Astrolabes du Musée Paul Dupuy de Toulouse, Toulouse, 1993). Pp. xii + 151. 270 FrF (paperback).

A book filled with pictures, diagrams, and formulas, primarily about the common planispheric astrolabe; included is an astrolabe rete on a transparent disk.

Total Eclipses of the Sun. J. B. Zirker (Princeton University Press, Princeton, New Jersey, 1995). Pp. 228. $12.95/ £10.95 (paperback).

Zirker's book explains well why modern astronomers still chase eclipses, what they have found and what they are still looking for. The opening chapter gives a succinct historical background.

Greenwich Time and the Longitude. Derek Howse (Philip Wilson Publishers and National Maritime Museum, London, 1997). Pp. 199. £19.95.

This "official millennium edition" of the late Derek Howse's 1980 book is now in a larger format with many more illustrations. For example, instead of a small black-and-white detail of Flamsteed from the ceiling painting of Greenwich Hospital, there is now a full-page colour plate. The original book was reviewed in *JHA* in the February 1982 issue.

Preceptum Canonis Ptolomei. David Pingree (Corpus des Astronomes Byzantins, viii; Academia-Bruylant, Louvain-la-Neuve, 1997). Pp. 174. 850 Belgium Francs or 140 FF (paperback).

Composed around A.D. 534, this Latin treatise gives instructions for using Ptolemy's *Handy tables.* This critical edition, based on six surviving manuscripts, includes an English translation and 24 pages of commentary with copious passages in Greek. In the end Pingree concludes that the *Preceptum* was an inadequate guide to the *Handy tables.*

Galileo: Decisive Innovator. Michael Sharratt (Cambridge University Press, Cambridge, 1996). Pp. xiv + 247. £35/$54.95 (paperback).

The paperback edition of a book reviewed in the February 1996 issue of *JHA*.

Histoire générale des sciences (4 volumes: *La science antique et médiévale*; *La science moderne*; *La science contemporaine,* (i) *Le xix^{e} siècle*; and *La science contemporaine,* (ii) *Le xx^{e} siècle*). Edited by René Taton (Quadrige / Presses Universitaires de France, 1995). Pp. 3472. 498 FF (boxed set of four paperbacks).

A large number of authors contributed in 1957 to the original edition of this sweeping survey of the history of science from Antiquity to the twentieth century. In 1966 it was revised, bringing the history up to 1960. The four volumes have now appeared in a handy boxed reprint.

NOTES ON CONTRIBUTORS

SHETHA S. AL-DARGAZELLI obtained her Ph.D. in Nuclear Physics from Durham University in 1979. She is interested in Historical Astronomy in general and Islamic astronomy in particular. She is currently Research Fellow in the Electronic Engineering and Computer Science Department at Aston University.

JIM BENNETT, Keeper of the Museum of the History of Science, Oxford, is currently occupied with a major redevelopment at the Museum, which will probably remain closed until the year 2000.

WILLIAM E. CARROLL is professor of European intellectual history and the history of science at Cornell College (Iowa). He is the director of Cornell's interdisciplinary program in science and religion. His work includes studies of the reception of Aristotelian thought in the Latin Middle Ages as well as the controversy between Galileo and the Inquisition.

STEPHEN J. DICK is an historian of science at the U.S. Naval Observatory in Washington, D.C., President of IAU Commission 41 (History of Astronomy), and the author of *The biological universe* (Cambridge University Press, 1996). He is writing the history of the U.S. Naval Observatory.

LOUAY FATOOHI has recently been awarded his Ph.D. from Durham University for a thesis on "First visibility of the lunar crescent and other problems in historical astronomy". He is currently Visiting Fellow in the Physics Department at Durham University.

OWEN GINGERICH notes with alarm the increase in stolen copies of Copernicus's *De revolutionibus*; using data from his census he has assisted in identifying and recovering some copies from Eastern Europe, but others are still missing.

JOHN HEILBRON, formerly professor of history and vice-chancellor at the University of California, Berkeley, is a senior research fellow at Worcester College, Oxford. His book, *The sun in the church*, a history around and about meridian lines in Catholic churches, is in press with Harvard University Press.

HELGE KRAGH is professor in the History of Science Department of Aarhus University, where he works in the history of the modern physical sciences. His book on twentieth-century physics will be published next year by Princeton University Press.

WILLIAM LILLER has worked in Chile since 1981 as "a reborn amateur astronomer" after serving for twenty years as a professor of astronomy at Harvard University. His wide-ranging interests have included stellar photometery, Easter Island archaeoastronomy, and musical composition.

KURT LOCHER (Rebrain 39, 8624 Grüt, Switzerland) is a college teacher of physics with a special interest in pre-Ptolemaic astronomy.

SIMON SCHAFFER is Fellow of Darwin College and Reader in the History and Philosophy of Science, University of Cambridge. He is currently taking part in a collaborative project on the history of instrumentation and scientific travel in the eighteenth and nineteenth centuries.

MICHAEL H. SHANK is currently immersed in Regiomontanus research. He is completing a book on the text and contexts of the *Disputationes* and a detailed study of the printing of that work, and is beginning with Richard Kremer a major study of the "Defence of Theon against George of Trebizond".

THOMAS J. SHERRILL (226 Solana Drive, Los Altos, CA 94022, USA) is an astronomer whose background is in celestial mechanics. He worked on the Hubble Space Telescope project for Lockheed Martin Corp. for 18 years, and developed the HST Design Reference Mission, a simulation of 30 days of orbital observations used as a reference for spacecraft design. He continues to consult for Lockheed Martin on the development of the Space Infrared Telescope Facility (SIRTF).

RICHARD STEPHENSON continues his work on various aspects of Applied Historical Astronomy. He was on the organizing committee of the Third International Conference on Oriental Astronomy at Fukuoka, Japan, which took place in October last.

N. M. SWERDLOW is professor in the Department of Astronomy and Astrophysics at The University of Chicago. He is currently working on a book on Galileo's astronomy.

WOODY SULLIVAN is a Professor of Astronomy and Adjunct Professor of History at the University of Washington in Seattle. He has long been researching and writing a monograph on the early worldwide history of radio astronomy (prior to 1954). His passion is gnomonics.