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# *GALILEUS DECEPTUS, NON MINIME DECEPIT*: A RE-APPRAISAL OF A COUNTER-ARGUMENT IN *DIALOGO* TO THE EXTRUSION EFFECT OF A ROTATING EARTH

PAOLO PALMIERI, University of Pittsburgh

## 1. *Introduction*

In a justly proud reminder to himself of his achievements on centrifugal force, Christiaan Huygens noted: “Galileus deceptus.… Neutonus applicuit feliciter ad motus ellipticos Planetarum. [H]inc quanti sit haec vis centrifugae cognitio apparet.”[1] Though we may doubt whether Newton would have acknowledged his debt to Huygens, and wonder what Galileo might have replied, Huygens’s comment on Galileo’s deluding himself on centrifugal force seems, with few exceptions, to have found favour with twentieth-century historians and philosophers of science. It is unclear exactly to which passage of Galileo’s *Dialogue concerning the two chief world systems* Huygens referred, so perhaps we have to take his comment as applicable to the whole argumentative strategy propounded by Galileo in the relevant sections of the Second Day of the *Dialogue*.[2] As we shall see, Galileo basically wishes to prove that no matter how fast the Earth rotates daily on its polar axis, objects on its surface would never be extruded, i.e., they would never fly off toward the sky. That this should be the case was a rather common objection raised by anti-Copernicans at that time. Thus, in the late 1930s, Alexandre Koyré pointed out that “Galileo’s argument … is extremely subtle and seductive. Unfortunately it is incorrect; and what is worse, it is manifestly incorrect”.[3] Others followed Huygens and Koyré in their negative assessment of Galileo’s argument.[4]

About twenty years ago, however, David K. Hill went so far as to claim that Galileo crossed the line between honest argument and conscious deception, and that he knew full well that his counter-argument to the anti-Copernicans was seriously flawed.[5] Eventually, dissent in the debate was expressed by Stillman Drake in a rejoinder note to Hill.[6]

In my view, the merit of Drake’s short rejoinder consists in having exposed the bundle of sometimes confused assumptions about the behaviour of bodies on a rotating Earth, on which the contemporary chorus of negative opinion was based. Drake argued that bodies on a Earth rotating faster and faster eventually reach the condition of weightlessness, after which they continue to orbit the Earth, remaining at rest with respect to a terrestrial observer. In other words, weightless bodies behave like geostationary satellites situated in close proximity of the surface of the Earth. Thus Drake thought he could rescue Galileo’s argument, on purely physical grounds, and salvage Galileo’s moral reputation. However, Drake somehow missed the point of Hill’s criticism of Galileo. For, as we shall see, Hill did not analyse Galileo’s counter-

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argument on the basis of classical (i.e., Newtonian) physics, as had been done by other scholars in the twentieth century. On the contrary, he pointed out a flaw in Galileo's reasoning in the light of Galileo's own physics of projectile motions, an internal and destructive objection that, according to Hill, Galileo himself could not have failed to raise. Hence Hill's claim about Galileo's morally deplorable presentation of a fundamentally flawed argument, a conscious act of deception.[7] More recently, Maurice Finocchiaro analysed in great detail the logical structure of Galileo's argument.[8] Finocchiaro shifted the focus of the controversy, coming to the conclusion that "Galileo's reflections on the nature of physical mathematical reasoning, when proper contextualized…, do not conflict with the definition [of Galileo's mathematical reasoning] I extracted from his extrusion argument".[9] Finocchiaro's definition of Galileo's mathematical reasoning is as follows: "Physical-mathematical reasoning is reasoning about physical processes and phenomena such that various aspects of them are represented by mathematical entities, various mathematical conclusions are reached about these mathematical entities, and then these mathematical conclusions are applied to the physical situation."[10] In the light of his analysis, Finocchiaro was able to dissolve some of the tensions in Galileo's extrusion argument (although Finocchiaro stopped short of commenting on Hill's conclusions).

Finocchiaro's study suggested to me a possible new standpoint from which to tackle, once again, the issues raised by Galileo's extrusion argument, namely, contextualization. The relevant context in which I will place Galileo's argument is that of late sixteenth- and early seventeenth-century mathematical reasoning.

In this paper, I will re-examine Galileo's counter-argument to the extrusion effect in the context of his understanding of the "angle of contingence", a dimension of Galileo's reasoning that has so far been neglected in the debate. I will argue that it is precisely this dimension that further illuminates the counter-argument, thus resolving the apparently internal conflict in Galileo's physics, and that Hill's claim — when viewed from the standpoint of Galileo's understanding of the "angle of contingence" — becomes untenable. Galileo went wrong (by the lights of subsequent developments in mathematical physics), but he did not consciously deceive.

Section 2 will present a brief sketch of the history of interpretations of the extrusion effect. Section 3 will reconstruct Galileo's views on the angle of contingence and similar parabolic trajectories. It will focus on a letter by Galileo on the angle of contingence and on related preparatory material for *Two new sciences* — two key documents that have so far been virtually ignored. Both these sections will emphasize the need for placing Galileo's take on the anti-Copernican argument from extrusion in the context of a culture at the intersection of orality and writing. Section 4 will discuss in some detail Hill's fascinating claim and Galileo's counter-argument to the extrusion effect, on the basis of the results of the two preceding sections. I will finally draw some conclusions, and point to directions for future research in Section 5.

## 2. *An Historical Sketch of the Extrusion Effect*

To prepare the reader, here I give a brief sketch of the history of interpretations of the extrusion effect, only underlining certain aspects that seem more relevant for the limited scope of my paper. A broader discussion of the history of the extrusion effect and of its role in the emergence of centrifugal force can be found in a recent study by Harald Siebert.[11] In what follows, I will restrict my analysis mostly to textual aspects that I found problematic and especially significant. The extrusion effect of the diurnal rotation of the Earth is presented by Galileo in the *Dialogue* as follows.

> Now there remains the objection based upon the experience of seeing that the speed of a whirling has a property of extruding and discarding material adhering to the revolving frame. For that reason it has appeared to many, including Ptolemy, that if the Earth turned upon itself with great speed, rocks and animals would necessarily be thrown toward the stars, and buildings could not be attached to their foundations with cement so strong that they too would not suffer similar ruin.[12]

The question immediately arises of Galileo's attribution to Ptolemy of a similar argument. The implicit reference seems to be to *Almagest* Book 1, Chapter 7. Here is G. J. Toomer's translation of the relevant passage from the original Greek (on the basis of Heiberg's text).

> If the Earth had a single motion in common with other heavy objects, it is obvious that it would be carried down faster than all of them because of its much greater size: living things and individual heavy objects would be left behind, riding on the air, and the Earth itself would very soon have fallen completely out of the heavens. But such things are utterly ridiculous merely to think of.[13]

A more literal reading of the passage has been suggested to me by James G. Lennox, as follows:[14]

> But if there were some motion of the Earth that was one and the same and shared with the other heavy bodies, it is clear that it would overtake everything in descent on account of its much greater magnitude, and the animals and individual heavy bodies floating on the air would be left behind, and the Earth would very quickly fall from the very heaven itself. But even contemplating such things would appear the most laughable thing of all.

This text from the *Almagest* is highly problematic. It is not obvious, at least to my mind, what the meaning conveyed by the image of an Earth's falling from the heaven exactly is. The beginning of the passage highlights a common motion. The phrasing is consistent with both a rectilinear and a circular motion. Presumably, however, given the general context of the initial discussion in Chapter 7, a rectilinear motion is intended by Ptolemy. The subsequent portion of Chapter 7 focuses on circular motion explicitly and eventually goes on to dismiss the possibility of a diurnal rotation of the Earth around its polar axis. The challenge posed by the passage is reflected in the difficulties probably encountered by the translators of the versions circulating in the Renaissance. In the Latin edition from Greek by George of Trebizond (1395–1484) we read that the Earth "velocissime extra coelum quoque ipsum excideret".[15] Giovan Battista della Porta (1535–1615) published a partial edition from Greek of the *Almagest* limited to Book 1, in 1605, where he rendered the passage similarly, "ipsa et celerrime postremo cecidisset et ab ipso coelo".[16] The fact is that there is a potential ambiguity with the rendering of the verb ἐκπίπτω in this context. It basically means "to fall from", but it also means "to go forth, to issue forth". The Latin cognate, "excido", chosen by George of Trebizond, has two distinct semantic values, namely, "to fall from" and "to raze, to demolish". Whether these values are in fact to be found in the original ἐκπίπτω is highly debatable. Della Porta has avoided ambiguity choosing "cado". On the other hand, as we shall see in a moment, Copernicus seems to have interpreted "excido" precisely in the sense that the Earth would demolish the heavens.

Again, whether the second value of "excido", i.e., "to demolish", conveys a possible value of ἐκπίπτω, or whether it is too strong, is debatable. It also true, however, that, for those who took the heavens to be solid crystalline orbs, the Earth's falling from the heavens would have to cause some damage to the crystalline orbs enveloping the Earth.[17] It is clear that both George of Trebizond and Della Porta intended the passage in the sense of "falling". But they constructed their phrasings with different prepositions, "extra" and "ab", to reinforce the idea of motion *beyond* a place, and motion *from* a place (where "place" must not be construed as a technical term in cosmology, but simply as a placeholder for the prepositional phrase).

In the version of the *Almagest* from Arabic, however, published in 1515, and apparently in Galileo's personal library, the problematic passage is resolved somewhat more openly, in a bifurcating rendition with two verbs, "et terra velociter omnino caderet: et pertransiret celum solum".[18] Here the translator opted for a solution that emphasized both the "falling [*cado*]" (without specifying the place from which the Earth was supposed to fall, though) and the "going through [*pertranseo*]" the heavens. Moreover, Chapter 7 in the version from Arabic is headed "De eo quod indicat quod terra motum localem non habeat", whereas in the version from Greek by George of Trebizond Chapter 7 is headed "Quod terra nullo motu progressivo movetur".[19] The two texts signal slightly different interpretations, the translator from Arabic more broadly emphasizing the Earth's being deprived of local motion, while George of Trebizond spotlights the Earth's not moving by progressive motion. Finally, George of Trebizond's translation has a rather awkward "universandum deferetur". The gerundive "universandum" is problematic, in my view. I am at a loss as to how to translate it. Della Porta, who in Chapter 7 is otherwise in general agreement with George of Trebizond, gets rid of it. The 1515 edition of the *Almagest*, from Arabic, has simply "inferius iret". In sum, there is little doubt that the semantic options open to a Renaissance reader of *Almagest*'s Chapter 7 were multifarious, and many passages badly in need of interpretive work.

It is quite possible that Copernicus's reading of the *Almagest*'s difficult passage led Galileo to interpret the *Almagest*'s passage as referring to the diurnal rotation of the Earth. In Copernicus's reading the extrusion argument is attributed to Ptolemy explicitly (with the verb "excidere" used by Copernicus, I think, in the second sense, as I already anticipated). Here I follow Siebert's intimation that we should read the passage, according to grammar, taking the verb "excido" in the second sense already mentioned.[20]

Further evidence suggests, on the other hand, that very early on in his career Galileo consciously (and perhaps independently of Copernicus) attributed the extrusion argument to Ptolemy. In Galileo's rather traditional "Treatise on the sphere" — used as a basis for lectures at the university of Padua — we find a section entitled "That the Earth is immobile", in which Galileo seems to imply that he is closely following Chapter 7 of Ptolemy's *Almagest*. However, even though the text is presented as a quasi-paraphrase of Ptolemy's own rebuttal of the Earth's diurnal rotation, the series of arguments attributed to Ptolemy does not fully match *Almagest*'s Chapter 7, and surprisingly ends in crescendo with a clear statement of the extrusion effect.[21] Moreover, and to complicate matters further, Galileo's assertion that "essendo il moto circolare e veloce accommodato non all' unione, ma più tosto alla divisione e dissipazione" is strongly reminiscent of Copernicus's assertion that "[q]uae vero repentina vertigine concitantur, videntur ad collectionem prorsum inepta, magisque unita dispergi". To cap it all, in the text of the Latin version from Arabic immediately preceding the problematic passage an image is presented of moving bodies aggregating toward the centre, and remaining fixed and compressed there because of pressure coming from all parts uniformly seeking to reunite at the centre. This obviously runs counter to Copernicus's image, according to which things rotating fast around a centre are "ad collectionem prorsum inepta".[22]

To complete this historical sketch of the argument from extrusion, another relevant item of evidence needs to be considered, namely, Cristoph Clavius's presentation of the extrusion effect in his *Commentary on the Sphere*.[23]

The *Commentary on the Sphere* might indeed have reinforced the polemical appeal of the extrusion argument, its value as a target for convinced Copernicans, so to say, given the popularity of the commentary and reputation of its author.[24] Clavius rehearses the argument as follows. If the Earth rotated around the axis of the world in twenty-four hours, "all edifices would be destroyed, and in no way could they remain firm".[25] In effect the textual context in which Clavius's vision of collapsing buildings is delineated suggests an intriguing possibility. The section "That the Earth is immobile" of Galileo's *Treatise on the Sphere*, might have been modelled, at least in part, precisely on Clavius's presentation of the argument from extrusion. I believe that this conclusion is further supported by the list of arguments not matching *Almagest*'s Chapter 7 that are summarized by Galileo in that section of the *Treatise*, and which appear in Clavius's text. In particular, the argument of an arrow thrown upwards vertically, which would not fall back in the same place, and the image of a stone falling from the mast of a moving ship, are discussed by Clavius immediately following the catastrophic picture of collapsing buildings.[26] Galileo reversed the order of presentation, reserving the extrusion effect for his short finale, but kept to the substance of Clavius's argumentative strategy.

Thus, Ptolemy's, Copernicus's, and Clavius's texts coalesced in Galileo's memory, forming a converging framework of ideas. He reorganized, so to speak, the intricate network of verbal arguments and mental images, directly or indirectly related to *Almagest*'s Chapter 7, that he found in relevant contemporary works. Eventually he attributed the argument from extrusion to Ptolemy himself. We should not forget that Galileo's culture was still influenced by a style of intellectual approach to texts typical of oral cultures. Memorizing content rather than checking for the verbatim exactness of quotations was often a scholar's more urgent mode of interaction with books. As Walter Ong has masterfully taught us, oral cultures are aggregative rather than analytic. The aggregative character of orality-based thought, Ong suggests, tends to emphasize not so much integral units as clusters of units.[27] In the present case, we see not so much an integral argument, but rather a cluster of arguments, the mode of appropriation of which is the act of memorizing the cluster around a central theme.

Thus, we should not find it exceptional that Galileo aggregated a sparse network of ideas into a memorable framework for thinking about Earth's diurnal rotation and the extrusion effect.[28] Within Galileo's mind, Ptolemy simply became the attractive pole that oriented the aggregative effect of oral modes of cognition in contact with written material.

Finally, two further developments are worth noting, which tend to corroborate the conclusion that Galileo's style of reading was still part of an orality-dominated mode of assimilation of texts. The first is a gut-feeling response by Galileo himself in the form of a marginal postil to a book presenting the extrusion argument. The second is the appearance of an historical text sanctioning the legitimacy of reading the *Almagest*'s controversial text as intimating the extrusion effect.

In 1612 the philosopher Giulio Cesare La Galla (1576–1624), a friend of Galileo's, published a long dissertation refuting the plausibility of Galileo's recent astronomical discoveries.[29] Galileo wrote numerous postils in the margin of La Galla's book. La Galla discusses the argument from extrusion at length, referring to it as "that formidable argument by Ptolemy".[30] When, further on in the text, La Galla reiterates the point that if the Earth rotated diurnally then all edifices, trees, and everything else would be destroyed in less than a day, Galileo inscribed in the margin "[m]elius dixisset Ptolemaeus…".[31] When solicited by the textual cue of the extrusion effect, Galileo's memory naturally responded activating the framework of ideas converging on Ptolemy.

Four decades later, G. B. Riccioli (1598–1671), in his massive *Almagestum novum* (1651), gave a detailed résumé of the history of the argument from extrusion up to the mid-seventeenth century. Riccioli quoted many authors but anchored the progression of readings of *Almagest*'s Chapter 7 to Copernicus.[32] He juxtaposed a verbatim quotation of the latter's comments with a quotation of the difficult passage from *Almagest*'s Chapter 7 (actually in a Latin version slightly different from all of those I have mentioned, presumably his own, or one that I have not identified). Significantly, Riccioli claims that Copernicus attributed the extrusion argument to Ptolemy. At the same time Riccioli seems implicitly to accept that the *Almagest*'s problematic passage may, at least obscurely, hint at the extrusion effect, especially since he refrains from commenting on Copernicus's attribution.[33] In this way, I would argue, Riccioli sanctioned the legitimacy of reading Ptolemy's passage as the first sediment of an accretive deposit of interpretations thrusting upward to the extrusion effect. A set of sparse references, which had originally been nurtured in an amalgam of orality-shaped interactions with books, was historicized by Riccioli into an incipient, written textual tradition.

### 3. *Galileo on the Angle of Contingence and Similar Parabolas*

In a letter written in 1635 to the mathematician Giovanni Camillo Gloriosi (1572–1643), who had succeeded him in the chair of mathematics at Padua in 1613, Galileo expounded his views on the angle of contingence.[34] Apart from the technical content strictly relevant to our goal in this paper, which I shall discuss presently, the letter affords us a rare glimpse of ideas that Galileo never committed to writing in full, for reasons on which unfortunately we can only speculate.

Galileo begins the letter with a typical old-person's complaint about his failing memory due to his age. Then, he opens his arguments by saying that he will relate a discourse on the angle of contingence which ran into his imagination [*fantasia*] a long time before.[35] A little further on, he remembers that some time in the past he also excogitated many "discourses" on the same question, only one of which he will expand on in the letter. Both the reference to the "imagination" and the rather ambiguous use of the term "discourse" suggest that Galileo was reconstructing mental content from his memory rather than from written material in his notebooks (although, in fairness, it must be said that in 1635 he was on the brink of blindness).

The first discourse related by Galileo in the letter to Gloriosi is intended to prove that the angle of contingence is called "angle" only equivocally, it being in fact not a true angle. Galileo makes his first move from what he takes to be the accepted definition of *angle*, i.e., the inclination of two lines touching each other at a point that are not placed straight with respect to each other.[36] He then proposes the following argument (*cf*. Figure 1).

Let us consider a regular polygon inscribed in a circle. The inclinations of the sides are as many as the sides, if the number of sides is uneven, or half the number of sides if the latter is even (since in this case two opposite sides will have the same inclination). If we now imagine that a side of the polygon is applied to any straight line whatever, no angle will be formed between the side and the straight line since

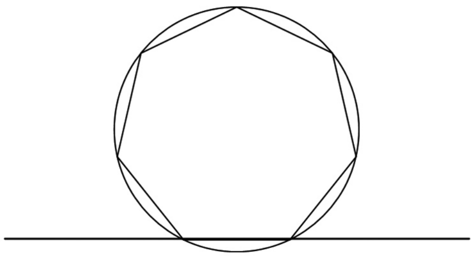

Fig. 1. This figure, I think, more accurately reflects Galileo's thinking than that printed in *Opere*, xvi, 331, which was based on the first edition of the letter given by Gloriosi. The absence of references in Galileo's text to the lettering of the diagram printed by Gloriosi might suggest that Galileo's original figure was different, or that there was no figure at all accompanying the reasoning. In the latter case my reconstruction would have only a didactic value.

they progress along the same direction. But the subsequent side will form an angle since it is inclined to the line and touches it. Given that the circle is conceived of as a polygon of infinite sides, then all directions will be found in its perimeter, that is infinite directions. There will thus be the direction of any line whatever, which can only be thought of as that of the side applied to it. Therefore the side of the circle applied to the straight line does not form an angle with the straight line, and this is the so-called point of contact. It is also inappropriate to say that although a point on the circumference does not contain an angle with the tangent at that point, the contiguous point will contain such an angle, exactly as in the polygon it is the subsequent side that forms the angle with the direction of the preceding side. The reason is that the point subsequent to the point of contact does not touch the straight line, which is touched only by one point of the circumference. Therefore since in the definition of angle both the inclination and the contact are required, the so-called angle of contingence is not a true angle and has no quantity.[37]

Galileo now goes on to propose another "discourse" in support of his view that the angle of contingence is no angle at all, which he remembers to have crafted long ago (Figure 2). Let us consider line *FG* turning on point *C*. The mixed angle *ACG* will become more and more acute until eventually it will transform into mixed angle *OCA*. This transformation cannot occur unless the angle annihilates, which, Galileo argues, can happen only when the turning line, *GF*, coincides with the horizontal line (*cf.* Figure 2). If we look at the history of the controversy on the angle of contingence we find a strikingly similar view, i.e., the angle of contingence is not a true angle and not a quantity, and a strikingly similar argument in Jacques Peletier (1517–82).[38] Galileo might have read the argument in Peletier's edition of Euclid, or in one of the publications by Peletier in which similar arguments are repeated.[39] However, I believe it is more likely that he would have seen the résumé of the discussion (with verbatim quotations) published by Peletier's opponent in the controversy, namely, Christoph Clavius.[40]

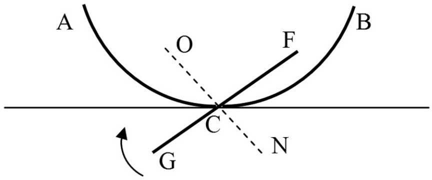


FIG. 2. A straight line forming a mixed angle passes from one side to another of a horizontal line so that the mixed angle must be annihilated. I have slightly simplified Galileo's original figure.

Peletier claims that the angle of contingence is not an angle because it forms no section with the circumference, while *angle* consists precisely in forming a section [*sectio*, or *decussatio*] not a contact [*contactus*] (Figure 3). A line, *ED*, turning on point *A,* forms angles more and more acute with the circumference because it sections it. But when the line coincides with the horizontal tangent a section will no longer occur. We might say that Peletier has a punctiform view of the angle of contingence, since for him "all angles consist in no more than one point".[41] Clavius held a conception radically different from Peletier's, according to which the angle of contingence is indeed a true angle and has quantity.[42] As we shall see in a moment, Galileo might have elaborated Peletier's punctiform view of angles, while rejecting Clavius's opinion.

In the salient part of the letter to Gloriosi, Galileo claims to refute the "discourse" [*discorso*], according to which not only is the angle of contingence a true quantity, but as such it is also infinitely divisible. Infinite divisibility, Galileo argues, is warranted

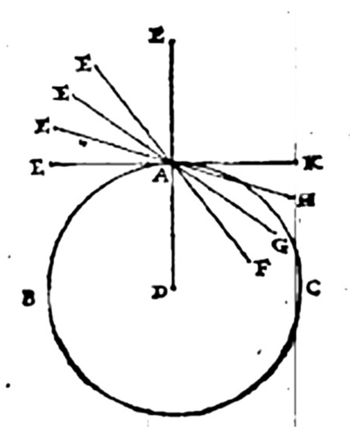


FIG. 3. The diagram accompanying Peletier's argument (*op. cit.* (ref. 39), 75). An identical diagram was published by Clavius (*op. cit.* (ref. 40), 117).

by the possibility of constructing greater and greater circles passing through the same point of contact between circumference and tangent (this example was one of Clavius's counter-arguments to Peletier). Galileo's reasoning strategy is paramount for our purposes because it involves a recourse to similar figures. We will see in the second part of this section that for Galileo parabolic trajectories are similar curves; and in the next section, that similar parabolic trajectories, in the broader context of the angle of contingence, are the hidden scaffolding of Galileo's counter-argument to the extrusion effect.

Not the *angle*, as Clavius had claimed, but the *space* between the circumference of the circle and the tangent line, Galileo argues, can actually be divided by greater and greater circumferences passing through the same point of contact between circumference and tangent.[43] This, he continues, can be shown starting with the simple example of rectilinear similar polygons (Figure 4).[44]

The perimeter of the greater hexagon divides the space between the smaller hexagon and the tangent, but angle *IBE* is not divided. In consequence, regardless of the number of sides of the similar polygons angle *IBE* will never be divided. The angle, Galileo points out, could be divided only by a dissimilar polygon, one with a greater number of sides. Hence, in Galileo's view, since *all circles are similar polygons of infinite sides*, when they are applied to the same tangent at *B*, the space between the tangent and the circumference is divided by the circumferences of the greater circles, but the angle of contingence, which is common to all, is not divided. Further, Galileo concludes, since the circles are polygons of infinite sides it cannot be said that a greater circle is a polygon of more sides and thus capable of dividing the angle, on the analogy of polygons of a finite number of sides. Interestingly, Galileo notes that, since when the number of sides of the polygons increases angle *IBE* becomes more and more acute, it looks as though the angle will be infinitely acute when the number of sides rises to infinity, in which case the angle will become "non-quantifiable, and not angle [*non quanto e non angolo*]".[45]

Something of Peletier's punctiform analysis is reflected in Galileo's line of

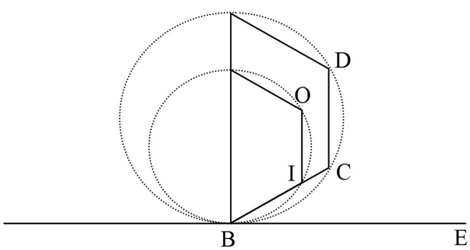


FIG. 4. Two rectilinear similar polygons (hexagons in this case, only half of which are diagrammed by Galileo) inscribed in two circles, passing through the same point of contact *B*. I have simplified the diagram by dotting the lines of the circumferences and limiting the lettering to what is needed for my discussion.

reasoning. At point *B* no true angle can be formed since, we might say, the inclinations of circumference and the tangent being the same the point alone cannot constitute a section, whereas, in Galileo's definition, it is the inclination of two lines touching each other at one point while not being placed straight with respect to each other that forms an angle.

To sum up the first part of this section, we have seen that Galileo begins his response to Gloriosi with an argument, or rather the recollection of a discourse, apparently based on a conception of the composition of lines in terms of points, since he refers to a point "subsequent" to the point of contact. He then moves on to another discourse, strongly reminiscent of one of Peletier's arguments aimed at proving that the angle of contingence is actually no angle at all. Finally, in my view, Galileo propounds his most original reflection on the angle of contingence based on similar figures, perhaps elaborating on Peletier's punctiform view of the nature of an angle.

Galileo starts by remembering one of his (presumably) first discourses about the angle of contingence. When moving to his second discourse he does not remember his past reading of Peletier's arguments, nor does he bother to clarify whether his ideas have been inspired by others. He shapes ideas on demand, so to say, solicited by Gloriosi's inquiry, through the medium of reconstructive recollection.

We have noted that for Galileo all circles are *similar* polygons of infinite sides. From a manuscript sheet, written in preparation for the calculation of the ballistic tables published in *Two new sciences*, we can gather that he held analogous views concerning parabolas (Figure 5).[46]

Parabolas can be found, Galileo says, *similar* to each other.[47] We now know that *all* parabolas are indeed similar curves.[48] Galileo is not explicit about this possible generalization, since obviously he did not have an analytic framework, that is, a Cartesian framework, for thinking about conic sections in all generality. There is, however, a tantalizing statement concerning *similar paraboloids* in Archimedes that may have been the source of Galileo's thinking about similar parabolas. Wilbur Knorr has actually claimed that Archimedes "asserts the theorem that all parabolas are similar in the Preface to *Conoids and Spheroids*".[49] I surveyed two Renaissance editions of Archimedes that Galileo would have seen, but I did not find an explicit assertion of that theorem. In the Archimedes edition annotated by Galileo, listed in his own personal library, and which we may thus assume was the one he used to consult, we find the statement that "omnia vero conoidalia rectangula [i.e., paraboloids] sunt similia". Almost the same phrasing is used in the Commandino edition.[50] I conclude, therefore, that all Galileo could have gathered from Archimedes is a pronouncement on similar paraboloids, although, admittedly, he might have extended this view to the parabolas generating paraboloids. But fortunately Galileo's views on similar parabolas emerge more clearly when we investigate in detail the text associated with the diagram presented in Figure 5, on f. 122v of *Manuscript 72*.

The text concerns the calculation of the parabolic trajectories of projectiles launched from point *D* (*cf.* lower left corner, in Figure 5, note that the parabolic trajectories are

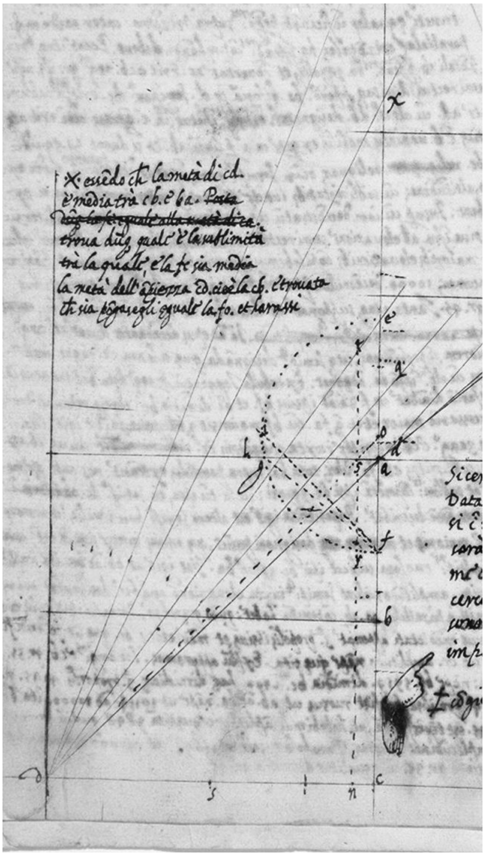

FIG. 5. The relevant portion of f. 122v, in *Manuscript 72*. The diagram printed in *Opere*, viii, 432, is incomplete.

not drawn in the diagram), at different inclinations but with the same initial energy (*impetus*, rather, in Galileo's terminology). Galileo starts from the parabolic trajectory determined by an elevation of 45°, and looks for the parabolic trajectories generated by the same impetus at point *D* with different shooting elevations, for example that with an elevation of 55°. We need not consider the technical details of the ingenious procedure excogitated by Galileo. It is basically an approximation technique based on the well-known *regula falsi*. Galileo first guesses the trajectory by assuming that its axis is the same (vertical) axis of the parabola generated by a 45° elevation, then he linearly scales the "false" trajectory to the true one by applying a simple proportionality rule. The warrant of the scaling operation is in fact the *similarity* between the two parabolas, which are (so to speak) "boxed" by, or inscribed in, *similar* triangles.

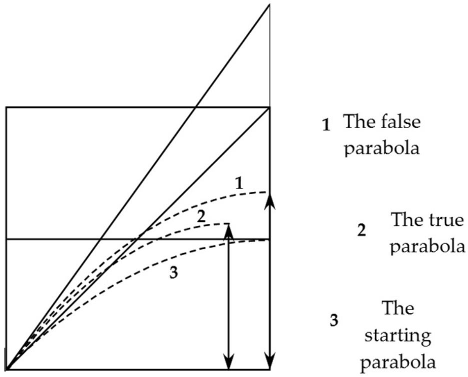


FIG. 6. A simplified version of the diagram on f. 122v, showing the similar parabolas used in the calculation but not represented by Galileo. Note that I have plotted here only semi-parabolas and their axes of symmetry.

Since Galileo did not draw the parabolas I have provided a reconstructed diagram in order to clarify the strategy of Galileo's procedure (*cf.* Figure 6).

The similarity attributed to parabolas that emerges from this procedure is reducible to Euclidean similarity between rectilinear figures, in our case simple right triangles. In Figure 7, I have drawn a diagram with the false and true semi-parabolas used in Galileo's procedure (remember that Galileo's idealized ballistic trajectories are symmetrical with respect to a vertical axis). I have "boxed" them by grey-shaded right triangles, in order to highlight their similarity. Thus, what Galileo has in mind when speaking of *similarity* between parabolas is the simple Euclidean idea that a proportionality transformation somehow connects the two similar figures. Galileo has of course no algorithm to compute a complete transformation in the case of curves such as parabolas. But all he needs in order to construct the ballistic tables are the characteristic dimensions of the parabolas, which he calls "amplitude" and "height". These are in fact characteristic dimensions of the triangle within which the semi-parabolas are inscribed. These characteristics can be proportionally transformed so as to obtain each true trajectory from the corresponding false one for any chosen elevation. The lack of a complete transformation procedure for parabolas also explains why Galileo did not draw the parabolas on his folio. He did not have a simple point-by-point drawing procedure from the linearly transformable characteristic dimensions.

To sum up, Galileo had recourse to *similar* parabolas in order to construct the ballistic tables presented in *Two new sciences*. The procedure is based on the extension of Euclidean similarity, valid for rectilinear figures, to parabolas. Similar semi-parabolas are inscribed in similar right triangles. This simply means that the semi-parabolas are tangent to the common hypotenuse of the triangles at the vertex in the lower-left corner (Figure 7), a constraint imposed by the ballistic condition of launch at the same

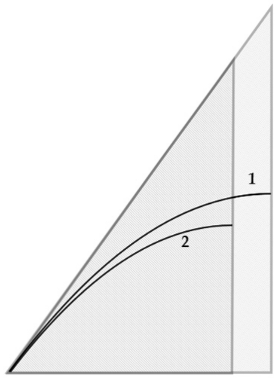


FIG. 7. The false (1) and true semi-parabolas (2) (for an elevation other than 45°), which, in Galileo's view, are similar in that they can be thought of as "boxed", or inscribed, by similar right triangles.

elevation, and their vertical axes coincide with the vertical side of the triangle.

In the next section, I shall combine Galileo's approaches to similar parabolas and to the angle of contingence, and argue that they are the two hidden structures scaffolding Galileo's counter-argument to the extrusion effect.

## 4. *Galileo's Counter-argument to the Extrusion Effect*

The counter-argument to the extrusion effect, presented by Galileo in the *Dialogue*, is embedded in a complex dialogical structure. The three famous interlocutors, Salviati, Sagredo, and Simplicio, a collective mouthpiece for the collage of Aristotelian positions that Galileo confronted in his career, engage in a lively discussion driven by Salviati's re-enactment of Socratic maieutics with Simplicio.[51] The questioning of Simplicio's mind, however, is coloured by pungent irony. Indeed irony, with its suspension of the literal level of meaning, is always a threatening presence in this long section of the *Dialogue*. In the process of questioning and eliciting answers, Galileo will raise objections to his own reasoning too. But since the *Dialogue*, written in Italian, was mostly aimed at neutralizing entrenched presuppositions against Copernican astronomy, which were common to a broad audience, Galileo did not cast the progression of questions and answers in a strictly technical language. He rather let ideas flow in a cyclical, wave-like movement of thinking.

The three interlocutors are agreed that any circular motion, like that of a sling, or a wheel, has a faculty of extruding objects placed on the circumference, and that the direction of the object's motion upon leaving the extruding device is the tangent to the circumference at the point of separation. Further, they agree that the motion after separation will be uniform and that if the circular motion is fast enough extrusion in slings and wheels will at some point occur. But all heavy bodies on the Earth's surface have a natural tendency downwards. The three interlocutors have no doubt on this either. Then Galileo issues his challenge to Simplicio. Galileo wishes to prove that no matter how small that downward tendency, and no matter how fast the diurnal rotation of the Earth, all heavy objects will remain firmly attached to the Earth's surface.[52]

Two objections are subsequently raised by Sagredo. I here summarize and paraphrase Galileo's text. Imagine a body a few instants along the tangent after leaving the rotating Earth. Immediately upon leaving the Earth's surface it will start descending toward the centre of the Earth in naturally accelerated fall. The first objection is as follows. The downward tendency, in terms of degrees of speed of fall, decreases *ad infinitum* as the body is thought of as approaching backwards the point of separation. Galileo has in fact already introduced in the *Dialogue* the law of falling bodies, and the idea of the uniform increase of a falling body's degree of speed with time. Second objection, how about the weight of the object? Going by Aristotelian physics the lighter the body the less fast it will fall. Thus, Sagredo concludes, by combining the two effects one has good reason to doubt that at least some objects will be able to escape the grip of the Earth and eventually be extruded. To this conclusion Galileo replies with the following counter-argument. It is this reply that has given rise to the controversy that led David Hill to his claim of dishonesty (*cf.* Figure 8).

Let us assume that at point *A* an object leaves the surface of the Earth along the tangent at *A*. Since the motion will be uniform evenly spaced points on line *AB* represent instants of equal intervals of time. The degrees of speed, and the vertical distances fallen through, are represented by segments *FG*, *HI*, *KL*.

I need to pause here. In his own explanation of this diagram, Galileo initially states that these segments represents "degrees of speed" acquired during *AF*, *AH*, *AK*. Only at some later point in the passage does Galileo equate these segments (*FG*, *HI*, *KL*) with distances fallen through as well. But this equation is questionable, not to say illegitimate, because (by Galileo's own law of fall) the distances fallen vary

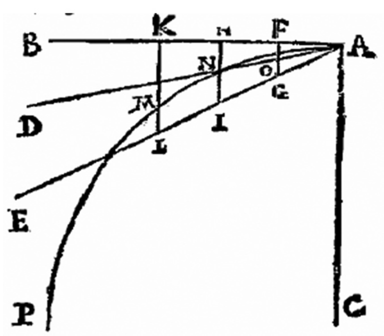


FIG. 8. The diagram supporting Galileo's counter-argument.

as the square of the times elapsed. So he may be fairly charged with committing some kind of equivocation.

Galileo can also incorporate in the argument the Aristotelian assumption that lighter bodies fall slower. He depicts this with the device of differently inclined lines, so the lighter the body the less acute the angle of inclination of lines *AE*, *AD*, i.e., the less fast the body will fall. There is a simplifying hypothesis implicitly made by Galileo. The directions of fall are vertical, not in the sense that they tend towards the centre of the Earth, but in the sense that they are parallel to the radius of the Earth at the point of separation. In Galileo's diagram the diminution *ad infinitum* of both weight and degree of speed are thus captured. Galileo continues as follows:

> The degrees of speed, infinitely diminished by the decrease of the weight of the moving body and by the approach to the first point of motion (the state of rest), are always determinate. They correspond proportionately to the parallels included between the two straight lines meeting in an angle such as the angle *BAE*, or *BAD*, or some other angle infinitely acute but still rectilinear. But the diminution of the spaces through which the moving body must go to return to the surface of the wheel is proportional to another sort of diminution included between lines which contain an angle infinitely narrower and more acute than any rectilinear angle whatever.… Now the parallels included between the straight lines, as they retreat toward the angle, always diminish in the same ratio…. But this is not thus with the line intercepted between the tangent and the circumference of the circle.[53]

Since the curvilinear angle is infinitely narrower and more acute than any rectilinear one, then the downward tendency will always be more than enough for the falling body to cover the distance between the tangent and the surface of the Earth. *But what about the actual trajectory of the projected object?* Galileo must have known that it is a parabolic arc. The problem of the actual trajectory is the hub around which the accusation of dishonesty raised by David Hill turns. Let's see how.

Hill has raised the following objection to Galileo's counter-argument to the extrusion effect. In Hill's words, it

> contains an interesting and fairly well-concealed fallacy which can be characterized either as a non-sequitur partly disguised by the vagueness of a key term or as a classical equivocation on that term. Galilei successfully argues that as we approach the point of contact, *A*, the distances which need to be covered to prevent projection necessarily vanish more quickly than the speeds of fall. But this does not imply that centripetal tendencies must overwhelm centrifugal tendencies. To prove this Galileo would have to show that the distances which *need* to be covered to prevent projection necessarily vanish more quickly than the distances a falling body would *actually cover* (as the point of contact is approached). This, however, cannot be established. These two distances vanish at the same rate, both being as the square of the speeds (and times).[54]

Further, Hill goes on to qualify Galileo's "mistaken inference as surprising and suspicious".[55] The reason for Hill's sceptical conclusion is that since, as is well known, by the time he completed the *Dialogue* Galileo had long reached his results about the parabolic trajectories of projectiles, it seems

> difficult to believe that Galileo simply never saw the relevance of the parabolic trajectory to the examination of the projection argument.… Can a projected object rise above, and remain above, the spinning Earth? Clearly, it could, *if* its speed of projection is large enough to produce a sufficiently flat parabolic arc … a parabola sharing a tangent might always *lie between* tangent and circle, in which case distances covered in fall are always less than those which must be covered to prevent projection.[56]

In Figure 9, I have visualized what Hill presumably has in mind when speaking of parabolic trajectories for projected bodies, by adding them to Galileo's original diagram. When the parabolic arcs are sufficiently flat, as Hill has suggested, they must leave the Earth's surface and thus extrusion will eventually ensue.

In the remaining part of this section, I will try to show that not only do we find in Galileo's *Dialogue* vestiges of the objection that Hill has raised (though under the guise of a language that Galileo wanted accessible to a vast audience), a point strangely missed by Hill, but that Galileo responded to that self-raised objection, in a way that needs to be unpacked and illuminated in the context of his approach to the angle of contingence and similar parabolas.

As I have already suggested, if we are to understand Galileo's counter-argument fully we need to follow the wave-like movement of his thinking carefully. The counter-argument in fact is not exhausted by the portion examined and criticized by Hill. It cannot be separated, in other words, from the self-objections subsequently raised by Galileo and the answers to those self-objections.

Sagredo, the layman not committed to any philosophical school, is dissatisfied

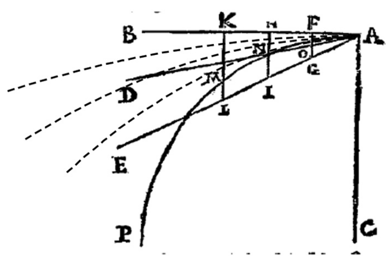


Fig. 9. Hill's objection to Galileo. The dotted lines represent parabolas tangent at *A* to the Earth (circle *AP*), i.e., the actual trajectories of projected bodies, as Galileo knew. Being open paths, when they are sufficiently flat, as Hill has suggested, they must leave the Earth's surface and thus extrusion can eventually ensue.

with Salviati's diagrammatic construction. If the speed of fall decreasing with weight (under the assumption of Aristotelian physics, for the sake of argument) followed the proportion of the line segments between tangent and circumference, or even a greater proportion, what would then happen?[57] Salviati is quick to mention that experience refutes the assumption of Aristotelian physics. The case is thus thrown out on empirical grounds. But this is besides Sagredo's point. The fact is that Salviati-Galileo is deeply intrigued by Sagredo's objection and wants to show that regardless of that proportion extrusion will never occur. We must take stock here. The language of this passage makes no sense in terms of Euclidean proportionality.[58] What does Sagredo really mean? It is in fact by making the angle of lines, such as *AD*, *AE*, with the tangent at *A* more and more acute that speed can be diminished *ad infinitum*. It is the inclination of those lines in the diagram that represents the decrease in speed of fall owing to the decrease in weight, whatever the relation between these two magnitudes might be. It makes no sense to talk of the proportion of that diminution as though "following" the proportion of the line segments between tangent and circumference approaching the point of contact! Thus, I take Sagredo's passage as intimating, though in a veiled allusion, the fact that Galileo actually imagines the parabolic path of the extruded object, exactly as Hill argues that he should have done. On the other hand, Sagredo's language makes perfect sense if we assume that he is in fact describing the trajectory of the extruded object. For, in this way it is perfectly meaningful to talk of the line segments between tangent and circumference following a certain *proportion*, or rather a certain *progression*, as they approach the point of contact. Here, then, proportionality has no Euclidean technical meaning.

Let us now turn to examining how Galileo goes about resolving this self-objection. Extrusion does not occur, even under the circumstance hinted at by Sagredo.

> What makes me believe this is that a diminution of weight made according to the ratio of the parallels between the tangent and the circumference has as its ultimate and highest term the absence of weight, *just as those parallels have for their ultimate term of reduction precisely that contact which is an indivisible point*. Now weight never does diminish to its last term, for then the moving body would be weightless; but the space of return for the projectile to the circumference does reduce to its ultimate smallness, which happens when the moving body rests upon the circumference at that very point of contact, so that no space whatever is required for its return.[59]

Here Galileo has introduced the point of contact as an *indivisible point*, the point at which no distance is required of the falling body to rejoin the surface of the Earth. The ground has been prepared for the small finale but the last movement must break through Simplicio's misconceptions about the contact between tangent and straight line. Simplicio is flabbergasted by Salviati's argument and raises the question that geometry, though it functions very well in the abstract, does not work in the real world. When it comes to matter, Simplicio claims, it makes no sense to say that *sphera tangit planum in puncto*. Thus, in Simplicio's view, the tangent at *A on the*

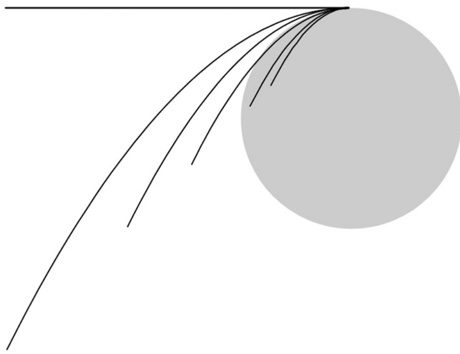

Fig. 10. A visualization of similar parabolic trajectories, according to Galileo, in the case of extrusion. The parabolic arcs are all similar to one another in that they are inscribed in similar rectangles. To avoid confusion in the diagram I have not represented the similar rectangles framing the parabolic arcs. (I have constructed these similar arcs with the help of the drawing software *Canvas 9*.)

*real Earth* not only touches one point, *A*, but grazes the surface for many miles. To which, Salviati replies as follows.

> But don't you see that if I grant you this, it will be so much the worse for your case? For if even assuming that the tangent lies removed except at one point, it has been proven that the projectile would not be separated, because of the extreme acuteness of the angle of contingence (*if it can indeed be called an angle*), how much less cause will it have for becoming separated if that angle is completely closed and the surface united with the tangent?[60]

Galileo's approach to the angle of contingence as no angle, no quantity, is hinted at here. It is of great importance to realize that in this final part of the argument *Galileo is exploring the limit behaviour of the falling object in proximity of the point of contact*. What happens to the falling object at the point of contact? This is the hovering question the answer to which can seal the counter-argument to the extrusion effect.

That the trajectory is parabolic in the vicinity of the point of contact Galileo has intimated already. But are the parabolic trajectories distinguishable in terms of the motion of the extruded object *in the vicinity of the point of contact*? Hill argues that this must be the case (*cf*. Figure 9). I suggest that in the framework of Galileo's mathematical physics they are not. Galileo has no calculus to fine-tune his analysis of the limit behaviour of the falling body. On Galileo's footsteps, Huygens will accomplish exactly this, a few decades later. It is only Galileo's views on the angle of contact (as being no quantity) and his views on similar parabolas that allow us to explore the limit behaviour of the falling object within the context of his mathematical physics.

Similar parabolic arcs in the case of extrusion can be represented — according to Galileo's preparatory analysis for the ballistic tables — by inscribing parabolic arcs with vertex at *A* in similar rectangles. Some arcs may intersect the surface of the Earth (thus extrusion would not follow, according to Hill's analysis), others escape from its surface (thus extrusion would follow, according to Hill's analysis). At the point of contact, however, the angle of contingence is the same for all arcs in the sense that it is no angle at all. All that can be said is that for Galileo the angle of contingence is not divided. Thus, as in the letter to Gloriosi, even in this case of extrusion, it is not the angle that can be divided. It is only the space between the circumference of the Earth and the tangent line at *A* that can actually be divided by greater and greater parabolic arcs passing through the same point of contact between the circumference of the Earth and the tangent. However, this fact that the space between the circumference of the Earth and the tangent line at *A* can actually be divided cannot serve our present purpose of exploring the limit behaviour of the falling body. Since all similar parabolic arcs may be reduced to similar polygons of infinite sides — as we may speculate in accord with Galileo's reasoning about circles being all similar polygons of infinite sides — then, when similar parabolic arcs are applied to the same tangent at *A*, we must conclude that the angle of contingence common to all parabolic arcs is not divided, even though the space between the tangent and the circumference is divided. In other words, in Galileo's physics there is available no measure whatsoever for the angles of contingence of different but similar parabolic arcs at the point of tangency.

Hence, in Galileo's physics, the limit behaviours of a falling body moving along different but similar parabolic arcs — in the vicinity of the point of contact — cannot be distinguished by discriminating among the angles of contingence at the point of contact. The angles of contingence at the point of contact are all the same. On this ground, the limit behaviour is therefore independent of the characteristics of the trajectory. It is at the point of contact, *A*, that the falling body *need fall no distance* to rejoin the Earth, regardless of the different, incipient parabolic trajectories.

What Galileo would have required to further his investigation of the limit behaviour of the extruded body, and thus come to terms with the error in his analysis, is some basic understanding of curvature of the trajectory, how to measure it, and a good helping of some form of infinitesimal calculus. It is such an understanding of curvature as a local property associated with curves (which we tend to take for granted today), that, I believe, has derailed Hill's fascinating analysis. Galileo, however, must be credited with the merit of realizing that the imagery behind the extrusion effect, the vision of buildings collapsing and animals and trees flying off toward the sky, was the fruit of deep-rooted misconceptions about centrifugal effects. Projection in rotating devices occurs *not along the radial direction* of the rotating device *but along the straight line tangent to the circumference at the point of separation*. Galileo succeeded in re-orienting discussion of the centrifugal effects of a rotating Earth, although he lacked the mathematical machinery to tame the problem.

To conclude, there is some irony in this story. Knowledge of the parabolic trajectory of projectiles, one of Galileo's lasting achievements in mathematical physics, was nowhere near enough for him to analyse the incipient behaviour of a falling body in the process of being extruded, or rather projected, by a fast rotating Earth. The analysis of the local behaviour of bodies at the point of separation from the extruding device requires a mathematical approach which goes into the infinitesimal. Galileo's understanding of the parabolic trajectory of projectiles rested on his classical approach to conic sections, as had been illustrated by Apollonius, not on the mathematics of curves analytically describable as *loci* in terms of local properties. The merit of fully understanding the properties of centrifugal effects was left for Christiaan Huygens, later in the seventeenth century, although, I am convinced, Huygens's possible dependence on at least some of Galileo's ideas deserves further scrutiny.

## *5. Conclusion*

Galileo's attribution of the extrusion argument to Ptolemy has an intriguing history that can be illuminated in terms of the effects of orality-shaped interactions with books. The form that cognition takes in a culture at the intersection of orality and writing, such as that of the late Renaissance, is a fertile terrain for exploring a Renaissance scholar's modes of appropriations of scientific ideas. Both Galileo and the intellectuals whom we have encountered in this story still participated in that form of cognition.

In the *Almagestum novum*, as we have noted, Riccioli sketched a brief history of the arguments and counter-arguments inspired by the extrusion effect. While marking the transformation of a loose bundle of ideas floating in the elusive space between orality and writing into a written tradition he also conceded defeat. In fact he headed the main section containing his discussion of the extrusion argument as follows. "Proponuntur argumenta quinque, *sed invalida* …", against the diurnal rotation of the Earth.[61] The argument from the extrusion effect, one of the five proposed in that section, was *invalid* in Ricclioli's view. It would be fascinating to speculate what might have led Riccioli to his conclusions. Unfortunately for us he only lists some counter-arguments but does not comment on their substance. He compiles a detached reportage of the *status quaestionis* but mutes his personal voice. Thus we are left with a tantalizing question, why is the extrusion argument against the diurnal rotation of the Earth invalid for Riccioli? Together with lesser works, Riccioli mentions Galileo's *Dialogue*, Kepler's *Epitome*, and Ismael Boulliau's *Philolai, sive dissertationis de vero systemate mundi*. However, both Kepler's and Boulliau's discussions of the extrusion argument are rather obscure, and boil down to no more than a few comments in passing.[62]

Thus I believe that the only credible source of Riccioli's conviction must have been Galileo, to whom, on the other hand, vast portions of the *Almagestum novum* are devoted. This is also indirectly suggested by one comment that Riccioli makes, referring the reader who wishes to know more about centrifugal effects to what Galileo relates on this subjects in the *Dialogue*.[63] A history of the reception of Galileo's counter-argument and of the eventual demise of the argument from extrusion would be highly rewarding for historians of seventeenth-century science. It remains for future research.[64]

Finally, if my reconstruction of the context of this Galilean counterargument is correct, we must reject Hill's conclusion that Galileo engaged in a morally deplorable act of conscious deception; that, deploying mathematical "trickery" to strengthen the rhetorical force of persuasion of his reasoning, Galileo published what he knew was a fundamentally flawed argument.[65] This conclusion is untenable, the fruit of historical anachronism.

I have argued that Galileo's thinking in the *Dialogue* cannot be disembodied from its dialogical framework. It is a wave-like flow of argumentation that incorporates self-objections and answers to them. In this maieutic process Galileo quite possibly adumbrated the type of objection from the parabolic trajectory of projectiles that, in Hill's view, should have proven to him the blatant inconsistency of his counter-argument. But Galileo's views on the angle of contingence and similar parabolic arcs cast a raking light on the counter-argument. They allow us to catch a glimpse of the thought-processes that prevented him from seeing that inconsistency. To think locally about the point of separation from the extruding device Galileo should have mastered the apparatus of calculus and curvature that Hill seems to have taken for granted in his analysis. Huygens was right, Hill went wrong. *Galileus deceptus, non minime decepit*.

*Acknowledgements*

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REFERENCES

1. C. Huygens, *Oeuvres complètes de Christiaan Huygens* (23 vols, The Hague, 1888–1950), xviii, 665. Actually Marin Mersenne had already expressed a negative opinion on Galileo's argument, *cf.* M. Mersenne, *Harmonie universelle* (Paris, 1636), 145, and the discussion in J. MacLachlan, "Mersenne's solution for Galileo's problem of the rotating Earth", *Historia mathematica*, iv (1977), 173–82.
2. *Cf.* Galileo Galilei, *Le opere di Galileo Galilei, edizione nazionale*, edited by Antonio Favaro (20 vols, Florence, 1890–1909; hereafter: Galileo, *Opere*), vii, 214–44, and the translation by Drake, in Galileo Galilei, *Dialogue concerning the two chief world systems*, transl. by Stillman Drake, 2nd edn (Berkeley and Los Angeles, 1967), 188–217.
3. *Cf.* A. Koyré, *Galileo studies*, transl. from the French by J. Mepham (Hassocks, Sussex, 1978), 195, and the original French, *Études galiléennes* (Paris, 1966), 268.
4. W. Shea, *Galileo's intellectual revolution* (New York, 1972), 140–1; M. Clavelin, *La philosophie naturelle de Galilée* (Paris, 1996; 1st edn, Paris, 1968), 244–53; MacLachlan, "Mersenne's solution for Galileo's problem of the rotating Earth" (ref. 1); S. Gaukroger, *Explanatory structures: A study of concepts of explanation in early physics and philosophy* (Atlantic Highlands, NJ, 1978), 189–98; A. Chalmers and R. Nicholas, "Galileo and the dissipative effect of a rotating Earth",

*Studies in history and philosophy of science*, xiv (1983), 315–40, p. 321; Y. Yoder, *Unrolling time: Christiaan Huygens and the mathematization of nature* (Cambridge, 1988), 35–41; D. Hill, "The projection argument in Galileo and Copernicus: Rhetorical strategy in the defence of the new system", *Annals of science*, xli (1984), 109–33; M. Finocchiaro (ed.), *Galileo on the world systems: A new abridged translation and guide* (Berkeley and Los Angeles, 1997), 179–95; and *idem*, "Physical-mathematical reasoning: Galileo on the extruding power of terrestrial rotation", *Synthese*, cxxxiv (2003), 217–44, p. 234. Negative conclusions, on the basis of a reconstruction of Galileo's argument according to Newtonian mechanics, were also reached by P. Palmieri, in "Re-examining Galileo's theory of tides", *Archive for history of exact sciences*, liii (1998), 223–375, pp. 281–94. It is important to realize that Galileo gives several other counterarguments to the extrusion effect, which have been analysed in detail especially by Maurice Finocchiaro and by A. Chalmers and R. Nicholas (references above): a physical counterargument comparing the extrusion along the tangent with fall along the secant; another physical counterargument contrasting how extrusion depends on linear speed and how it depends on the radius; and two other mathematical counterarguments: one claiming that on a rotating Earth extrusion is mathematically impossible because the ratio of an exsecant to the corresponding tangent segment tends toward zero, the other claiming that it is impossible because the ratio of one exsecant to another (at twice its distance from the point of tangency) tends to zero.

5. Hill, "The projection argument in Galileo and Copernicus" (ref. 4), 133, for example.
6. S. Drake, "Galileo and the projection argument", *Annals of science*, xliii (1986), 77–79.
7. "Though there is presumably a rhetorical dimension to all argument meant to persuade, there must always be a basic distinction between honest argument and conscious deception. I have argued that Galileo crossed the line in the case at hand", Hill, "The projection argument in Galileo and Copernicus" (ref. 4), 133.
8. Finocchiaro, "Physical-mathematical reasoning" (ref. 4).
9. Finocchiaro, "Physical-mathematical reasoning" (ref. 4), 235.
10. Finocchiaro, "Physical-mathematical reasoning" (ref. 4), 235.
11. *Cf.* Harald Siebert, *Die Große kosmologische Kontroverse: Rekonstruktionsversuche anhand des Itinerarium exstaticum von Athanasius Kircher SJ (1602–1680)* (Stuttgart, 2006); see especially the discussion on pp. 132–54.
12. Galileo, *Dialogue concerning the two chief world systems* (ref. 2), 188.
13. *Ptolemy's Almagest*, transl. and annotated by G. J. Toomer (Princeton, NJ, 1998; first edn, London, 1984), 44. *Cf.* the Greek text in Heiberg's edition: εἰ δέ γε καὶ αὐτῆς ἦν τις φορὰ κοινὴ καὶ μία καὶ ἡ αὐτὴ τοῖς ἄλλοις βάρεσιν, ἔφθανεν ἄν πάντα δηλονότι διά τήν τοσαύτην τοῦ μεγέθους ὑπερβολήν καταφερομένη, καὶ ὑπερλείπετο μέν τά ζῷα καί τά κατά μέρος τῶν βαρῶν ὀχούμενα ἐπὶ τοῦ ἀέρος, αὐτὴ δὲ τάχιστα τέλεον ἂν ἐκπεπτώκει καὶ αὐτοῦ τοῦ οὐρανοῦ. ἀλλὰ τὰ τοιαῦτα μέν καὶ μόνον ἀλλὰ τὰ τοιαῦτα μέν καὶ μόνον ἐπινοηθέντα πάντων ἄν φανείη γελοιότατα. *Cf.* Ptolemy, *Syntaxis mathematica*, ed. by J. L. Heiberg, Part 1, Books 1–6 (Leipzig, 1898), 23–24. We may notice that even the great Heiberg shied away from translating the *Almagest*, commenting at the end of his preface to the 1898 edition, "interpretationem meam sive Latinam sive linguae recentioris in tanta rerum difficultate addere ausus non sum; de re videant astronomi, si interpretationem desideraverint" (*ibid*., p. vi). It is ultimately for the astronomers to tackle the issues raised by Ptolemy's text!
14. I wish to thank James Lennox for translating the passage and sharing with me his insights into the semantic complexities of the original Greek.
15. Ptolemy, *Omnia quae extant opera* (Basel, 1541), 7. The whole passage is rendered as follows, "Quod si communis caeteris ponderibus singularisque motus ipsi quoque inesset, patet quia propeter tantum (sui magnitudine) excessum universandum deferetur, praeveniret caeterisque relictis in aerem animalibus, dico aliisque ponderibus, ipsa velocissime extra coelum quoque ipsum excideret. Verum haec ridiculosissima omnium intellectu videntur" (*ibid*.). The translation was first published in a 1528 edition. *Cf.* R. De Vivo's Introduction, in G. B. Della Porta, *Claudii Ptolemaei Magnae constructionis liber primus*, ed. by R. De Vivo (Naples, 2000; first edn,

Naples, 1605), pp. viii ff.

16. Della Porta, *Claudii Ptolemaei Magnae constructionis liber primus* (ref. 15), 84. The whole passage is rendered as follows, "Si vero et ipsius esset aliqua latio communis, et una et eadem aliis ponderibus, praeoccuparet utique omnia, videlicet ob tantum magnitudinis excessum deorsum lata, et relinquerentur quidem et animalia et vecta in aere secundum partem ponderum, ipsa et celerrime postremo cecidisset et ab ipso coelo, sed talia quidem et tantum excogitata maxime omnium ridicula viderentur" (*ibid.*).

17. On the solid nature of the celestial orbs in the period spanning the late Middle Ages to the Renaissance and the seventeenth century, see Edward Grant, "Celestial orbs in the Latin Middle Ages", *Isis*, lxxviii (1987), 153–73; B. R. Goldstein and P. Barker, "The role of Rothmann in the dissolution of the celestial spheres", *The British journal for the history of science*, xxviii (1995), 385–403; B. R. Goldstein and Giora Hon, "Kepler's move from orbs to orbits: Documenting a revolutionary scientific concept", *Perspectives on science*, xiii (2005), 74–111; and more generally M. A. Granada, *Sfere solide e cielo fluido: Momenti del dibattito cosmologico nella seconda metà del Cinquecento* (Milan, 2002).

18. Ptolemy, *Almagestum Cl. Ptolemei* (Venice, 1515), 4. The whole passage is rendered as follows: "Quo si terre et reliquorum corporum gravium que sunt preter eam esset motus unus communis: terra propter superfluitatem sue molis et gravitatis vinceret omnia gravia que sunt preter ipsam: et inferius iret. Et remanerent animalia et relique species gravium sita in aere. Et terra velociter omnino caderet: et pertransiret celum solum. Tamen imaginari hoc et eius simile est derisio et illusio imaginantis ipsum" (*ibid.*). I have preserved the Latin morphology of the 1515 printed Latin, only expanding the abbreviations. *Cf.* A. Favaro, "La libreria di Galileo Galilei", *Bullettino di bibliografia e di storia delle scienze matematiche e fisiche*, xix (1886), 219–90, for the catalogue of Galileo's library, in which the 1515 edition of Ptolemy's *Almagest* is listed.

19. Ptolemy, *Omnia quae extant opera* (ref. 15), 6; and Ptolemy, *Almagestum Cl. Ptolemei* (ref. 18), 4. Della Porta repeats George of Trebizond's title almost verbatim, "Quod terra neque motum progressivum aliquem facit". *Cf.* Della Porta, *Claudii Ptolemaei Magnae constructionis liber primus* (ref. 15), 83.

20. Copernicus's rendition of Ptolemy is as follows: "Si igitur, inquit Ptolemaeus Alexandrinus, terra volueretur, saltem revolutione cotidiana, oporteret accidere contraria supradictis. Etenim concitatissimum esse motum oporteret, ac celeritatem eius insuperabilem, quae in xxiiii. horis totum terrae transmitteret ambitum. Quae vero repentina vertigine concitantur, videntur ad collectionem prorsum inepta, magisque unita dispergi, nisi coharentia aliqua firmitate contineantur: et iam dudum, inquit, dissipata terra caelum ipsum (quod admodum ridiculum est) excidisset, et eo magis animantia atque alia quaequnque soluta onera haud quaquam inconcussa manerent." N. Copernicus, *De revolutionibus orbium coelestium, Libri VI* (Nurenberg, 1543), ff. 5r–v. I agree with Siebert's suggestion (*Die Große kosmologische Kontroverse* (ref. 11), 138) that here the meaning of *excido* should be that of "demolish" since the sentence is constructed with an accusative. Siebert renders the passage as follows: "Und schon lngst, sagt er, hätte die zersprengte Erde das Himmelsgwölbe selbst (was völlig lächerlich ist) zerstört …" (p. 134, and discussion in footnote 6). As for *excido* in the sense of "fall out", on the other hand, I found that when it is intended to mean "fall out" then it is generally constructed with a prepositional phrase and an ablative, not with a direct object expressed in the accusative form. Siebert comments: "Die ptolemischen Ausdruck 'extra coelum excidere' verkürzt er [i.e., Copernicus] in ein 'coelum excidere', wodurch 'excidere' nicht mehr 'herausfallen' (ex+cadere) bezeichnet, sondern sich in dieser transitiven Verwendung trotz gleicher Schreibung als ein anderes Verb entpuppt (ex-caedere), welches die Bedeutung hat von 'heraushauen, aufbrechen, zerstören'" (p. 138). Hill, too, in "The projection argument in Galileo and Copernicus" (ref. 4), 112–15, discusses at length Copernicus's reasons for interpreting Ptolemy's passage in such a way. A referee suggested a further, broader interpretative possibility in terms of "rhetorical" strategy. I report his suggestion almost verbatim. "Copernicus makes a very personal appropriation of Ptolemy's argument. With an intentional deformation of the *Almagest* text, he explains (in *De revolutionibus* I 8, at f. 5v), that if the supposed violent Earth's rotation were to disperse all things not firmly bound together, and

eventually bring the terrestrial globe itself to disintegrate and fall out the heavens (a consequence not imaginated by Ptolemy), then it is even more obvious (and this is a consequence Ptolemy should fear), that, due to the ever increasing speed of their violent circular motion, the heavens would become more and more immense, if not infinite, in being driven away from the centre. This being so, Copernicus's use of Ptolemy is to be understood not as the result of a faulty reading of the *Almagest* passage (either in Greek, or in one of its Latin translations), but the product of a rhetorical strategy." I thank the anonymous referee for the suggestion.

21. "Considerando Tolomeo questa opinione, per distruggerla argomenta in questa guisa.... E finalmente, essendo il moto circolare e veloce accommodato non all' unione, ma pi tosto alla divisione e dissipazione, quando la terra così precipitosamente andasse a torno, le pietre, gli animali, e l' altre cose, che nella superficie si ritrovano, verriano da tal vertigine dissipati, sparsi e verso il cielo tirati; così le città e gli altri edificii sariano messi in ruina." Galileo, *Opere* (ref. 2), ii, 223–4.
22. "... et aggregantur mota: et stant fixa in medio ex sustentatione et coangustatione vel fulcimento et impulsione eorum ad invicem ab omnibus partibus equaliter et similter." *Cf.* Ptolemy, *Almagestum Cl. Ptolemei* (ref. 18), 4.
23. See C. Clavius, *In sphaeram Ioannis de Sacro Bosco commentarius* (Rome, 1585).
24. Various editions of the *Commentary* appeared in 1570, 1581, 1585, 1593, 1596, 1601 and 1607, and also in 1611 (as the third volume of the *Opera mathematica*).
25. "Praeterea, si terra tanta celeritate circa axem mundi volueretur, ut videlicet circuito expleret spacio 24. horarum, sicut quidam fabulantur, omnia aedificia corruerent, et nulla ratione diu consistere possent." See Clavius, *In sphaeram Ioannis de Sacro Bosco commentarius* (ref. 23), 196. Clavius does not mention Ptolemy, however. *Cf.* James Lattis's comment, "Clavius's apocalyptic vision of collapsing buildings is not a recitation of anything found in Ptolemy…" (*Between Copernicus and Galileo: Christoph Clavius and the collapse of Ptolemaic cosmology* (Chicago, 1994), 121).
26. Clavius, *In sphaeram Ioannis de Sacro Bosco commentarius* (ref. 23), 196.
27. W. Ong, *Orality and literacy: The technologizing of the word* (London, 1988), 38.
28. An analogous situation can be described in a case which has puzzled Galileo scholars for a long time, i.e., the myth concerning the formation of planets that Galileo explicitly attributes to Plato, for example, in the Fourth Day of *Two new sciences*. Fabio Acerbi, in a paper summarizing the *status quaestionis* of this little Galileo mystery, has shown that in Galileo's text there is a marked correspondence of themes and syntactic structures with *Timaeus* 38c 7–8, 38e 3–6, but it is impossible to pin down precise textual references to Renaissance editions of *Timaeus*, in both Greek and Latin, potentially available to Galileo. F. Acerbi, "Le fonti del mito Platonico di Galileo", *Physis*, xxxvii (2000), 359–92. Though Acerbi does not take this possibility into consideration, I suggest that we are here, once again, in the presence of a typical effect of orality-shaped modes of appropriation of written material; hence the loss of precise correspondences between texts.
29. The text of La Galla's dissertation (in Latin) has been re-published partially, together with all Galileo's postils, in Galileo, *Opere* (ref. 2), iii, 311–99.
30. Galileo, *Opere* (ref. 2), iii, 345.
31. Galileo, *Opere* (ref. 2), iii, 346.
32. G. B. Riccioli, *Almagestum novum* (2 vols, Bologna, 1651), ii, 432–3.
33. Riccioli, *Almagestum novum* (ref. 32), ii, 433. However, the quotation from Ptolemy is introduced by Riccioli with the possibly adversative "Ptolemaei autem verba lib. 1 cap. 7 fuerant…", which might suggest that he saw a certain discrepancy between Copernicus's reading and Ptolemy's original passage. On the other hand, it has been suggested that Riccioli, traditionally portrayed as a staunch anti-Copernican, might in fact have harboured doubts about the geocentric model of the universe. *Cf.* A. Dinis, "Was Riccioli a secret Copernican?", in M. T. Borgato (ed.), *Giambattista Riccioli e il merito scientifico dei Gesuiti nell' età barocca* (Florence, 2002), 49–77, espec. pp. 59ff. Therefore one might read his self-effacement in the historical reconstruction of the extrusion effect as part of a conscious rhetoric of ambiguity.

34. Galileo, *Opere* (ref. 2), xvi, 330–4. The letter was written in 1635 and first published by Gloriosi himself, in G. C. Gloriosi, *Exercitationum mathematicarum decas tertia* (Naples, 1639), 146–51. This fascinating document has not been studied in detail so far. On Gloriosi, see Pier Daniele Napolitani, "Galileo e due matematici napoletani: Luca Valerio e Giovanni Camillo Glorioso", in F. Lomonaco and M. Torrini (eds), *Galileo e Napoli* (Naples, 1987), 159–95. *Cf.* also C. R. Palmerino, "Una nuova scienza della materia per la Scienza nova del moto: La discussione dei paradossi dell' infinito nella Prima Giornata dei Discorsi galileiani", in E. Festa and R. Gatto (eds), *Atomismo e continuo nel XVII secolo* (Naples, 2000), 275–319, pp. 284–6, for a few comments, in passing, on the angle of contingence, in relation to her claim that there is a certain similarity between Galileo's letter and his solution to the Rota Aristotelis paradox (published later on in *Two new sciences*). Finally, see Carl B. Boyer, "Galileo's place in the history of mathematics", in E. McMullin (ed), *Galileo: Man of science* (New York, 1967), 232–55. Boyer's article contains a brief discussion of Galileo on the angle of contingence, but Boyer sees in Galileo's reasoning only an anticipation of the concept of "the order of an infinitesimal", an opinion which, in my view, is anachronistic.
35. "… certo mio discorso che gran tempo fa mi passò per la fantasia", Galileo, *Opere* (ref. 2), xvi, 331.
36. "… angolo sia l' inclinazione di due linee che si toccano in un punto e non son poste tra di loro per diritto", Galileo, *Opere* (ref. 2), xvi, 331.
37. Galileo, *Opere* (ref. 2), xvi, 331–2.
38. The controversy was prompted by Euclid's Proposition 16, in Book III of the *Elements*. "The straight line drawn at right angles to the diameter of a circle from its end will fall outside the circle, and into the space between the straight line and the circumference another straight line cannot be interposed, further the angle of the semicircle is greater, and the remaining angle less, than any acute rectilinear angle." *Cf.* Euclid, *The thirteen books of the Elements*, transl. and ed. by Thomas Heath, 2nd edn (3 vols, New York, 1956), ii, 37. An important Renaissance episode in the controversy was the debate between Jacques Peletier and Christoph Clavius. *Cf.* L. Maierù, "In Christophorum Clavium de contactu linearum Apologia: Considerazioni attorno alla polemica fra Peletier e Clavio circa l' angolo di contatto (1579–1589)", *Archive for history of exact sciences*, xli (1990), 115–37. For a detailed study of that episode, and more generally, on the history of the controversy, see Heath's comments, *loc. cit.*, ii, 39–43.
39. J. Peletier, *In Euclidis Elementa geometrica demonstrationum libri sex* (Lyons, 1557), 73–78; *Commentarii tres* (Basel, 1563), 28–48; *In Christophorum Clavium De contactu linearum apologia* (Paris, 1579), 3r–9v; and *De contactu linearum, commentarius* (Paris, 1581).
40. Galileo was familiar with Clavius's edition of Euclid's *Elements*, in which the résumé was republished together with Clavius's response (P. Palmieri, "The obscurity of the equimultiples: Clavius' and Galileo's foundational studies of Euclid's theory of proportions", *Archive for history of exact sciences*, lv (2001), 555–97). In fact Clavius quotes Peletier's comments in the latter's edition of Euclid verbatim. *Cf.* C. Clavius, *Commentaria in Euclidis Elementa geometrica* (Hildesheim, 1999), facsimile edition of the first volume of *Christophori Clauii Bambergensis e Societate Iesu Opera mathematica V tomis distributa* (5 vols, Mainz, 1611–12), 117.
41. "Cum igitur omnis angulus in pluribus punctis non consistat, quam uno" (Clavius, *Commentaria in Euclidis Elementa geometrica* (ref. 40), 117). An interpretive caveat is necessary, however. For the passage continues as follows: "fit ut punctum *A* tam sit ineptum angulo constituendo, quam modo erat punctum sectionis *E*, linearum rectarum." It seems difficult to reconcile the punctiform view with the claim that point *A* is "inept" to form an angle. I owe this insight to Curtis Wilson.
42. Clavius, *Commentaria in Euclidis Elementa geometrica* (ref. 40), 119.
43. Clavius asserted that "... quemvis angulum contactus, etsi ab Euclide minor ostensus est omni acuto rectilineo, dividi posse in partes infinitas". Clavius, *Commentaria in Euclidis Elementa geometrica* (ref. 40), 119.
44. Galileo, *Opere* (ref. 2), xvi, 332ff.
45. Galileo, *Opere* (ref. 2), xvi, 334.

46. See Galileo Galilei, *Two new sciences: Including centres of gravity and force of percussion*, ed. by Stillman Drake (Madison, 1974), 249– 52, for the ballistic tables. Folio 122v, *Manuscript 72*, is preserved in the National Library at Florence. The sheet was published in Galileo, *Opere* (ref. 2), viii, 432. It is now also available on-line, retrievable at: http://echo.mpiwg-berlin.mpg.de/content/scientific_revolution/galileo.
47. Galileo, *Opere* (ref. 2), viii, 432.
48. *Cf.* a classic treatise, such as, for example, W. H. Besant, *Conic sections* (London, 1895), 154, for an example and a discussion. It can be easily seen, for instance, by writing the polar equation of a parabola.
49. W. Knorr, *The ancient tradition of geometric problems* (New York, 1993), 335.
50. Archimedes, *Archimedis Syracusani philosophi ac geometrae excellentissimi Opera* (Basel, 1544), 59. In the Commandino edition, we find "omnia conoidea rectangula sunt similia". See Archimedes, *Opera Archimedis Syracusani philosophi et mathematici ingeniosissimi* (Venice, 1558), 27v.
51. Galileo, *Dialogue concerning the two chief world systems* (ref. 2), 188ff.
52. *Ibid.*, 197.
53. *Ibid.*, 200–1.
54. Hill, "The projection argument in Galileo and Copernicus" (ref. 4), 121.
55. *Ibid.*, 122.
56. *Ibid.*, 123.
57. Galileo, *Dialogue concerning the two chief world systems* (ref. 2), 201–2.
58. Galileo's proportional reasoning is a form of reasoning based on the principled manipulation of ratios and proportions, according to the rules set forth in the fifth book of Euclid's *Elements*. As for Galileo's use of Euclidean proportionality, a considerable body of literature is now available, which allows us to understand most of its technical aspects better. *Cf.* C. Armijo, "Un nuevo rol para las definiciones", in J. Montesinos and C. Solís (eds), *Largo campo di filosofare: Eurosymposium Galileo 2001* (La Orotava, Tenerife, 2001), 85–99; S. Drake, "Velocity and Eudoxan proportion theory", *Physis*, xv (1973), 49–64 (reprinted in S. Drake, *Essays on Galileo and the history and philosophy of science* (3 vols, Toronto, 1999), ii, 265–80); *idem*, "Galileo's experimental confirmation of horizontal inertia: Unpublished manuscripts", *Isis*, lxiv (1973), 291–305 (reprinted in Drake, *Essays*, ii, 147–59); *idem*, "Mathematics and discovery in Galileo's physics", *Historia mathematica*, i (1974), 129–50 (reprinted in Drake, *Essays*, ii, 292–306); *idem*, "Euclid Book V from Eudoxus to Dedekind", *Cahiers d' histoire et de philosophie des sciences*, n.s., xxi (1987), 52–64 (reprinted in Drake, *Essays*, iii, 61–75 ); A. Frajese, *Galileo matematico* (Rome, 1964); E. Giusti, "Aspetti matematici della cinematica Galileiana", *Bollettino di storia delle scienze matematiche*, i (1981), 3–42; *idem*, "Ricerche Galileiane: Il trattato 'De motu equabili' come modello della teoria delle proporzioni", *Bollettino di storia delle scienze matematiche*, vi (1986), 89–108; *idem*, "Galilei e le leggi del moto", in Galileo Galilei, *Dicorsi e dimostrazioni matematiche intorno a due nuove scienze attinenti alla meccanica ed i movimenti locali*, ed. by Enrico Giusti (Turin, 1990), pp. ix–lx; *idem*, "La teoria galileiana delle proporzioni", in L. Conti (ed.), *La matematizzazione dell' universo: Momenti della cultura matematica tra '500 e '600* (Perugia, 1992), 207–22; *idem*, *Euclides reformatus: La teoria delle proporzioni nella scuola galileiana* (Turin, 1993); *idem*, "Il filosofo geometra: Matematica e filosofia naturale in Galileo", *Nuncius*, ix (1994), 485–98; *idem*, "Il ruolo dela matematica nella meccanica di Galileo", in A. Tenenti *et al.*, *Galileo Galilei e la cultura veneziana* (Venice, 1995), 321–38; F. Palladino, "La teoria delle proporzioni nel Seicento", *Nuncius*, vi (1991), 33–81; and P. Palmieri, "The obscurity of the equimultiples" (ref. 38). For a general treatment of the various aspects of the Euclidean theory of proportions I have relied on: I. Grattan-Guinness, "Numbers, magnitudes, ratios, and proportions in Euclid's elements: How did he handle them?", *Historia mathematica*, xxiii (1996), 355–75; C. Sasaki, "The acceptance of the theory of proportions in the sixteenth and seventeenth centuries", *Historia scientarum*, xxix (1985), 83–116; K. Saito, "Compounded ratio in Euclid and Apollonius", *Historia scientiarum*, xxxi (1986), 25–59; and *idem*, "Duplicate ratio in Book VI of Euclid's *Elements*", *Historia scientiarum*, l (1993), 115–35. P. L. Rose, *The Italian renaissance*

*of mathematics: Studies on humanists and mathematicians from Petrarch to Galileo* (Geneva, 1975) is an extensive, immensely erudite survey of Renaissance mathematics in Italy from a non-technical point of view. *Cf.* also E. D. Sylla, “Compounding ratios: Bradwardine, Oresme, and the first edition of Newton’s *Principia*”, in E. Mendelsohn (ed.), *Transformation and tradition in the sciences: Essays in honor of I. Bernard Cohen* (Cambridge, MA, 1984), 11–43.

59. Galileo, *Dialogue concerning the two chief world systems* (ref. 2), 202–3. Emphasis is mine.
60. *Ibid*., 203. I have slightly altered Drake’s translation. Emphasis is mine.
61. Riccioli, *Almagestum novum* (ref. 32), 429. Emphasis is mine.
62. *Cf.* J. Kepler, *Opera omnia*, ed. by C. Frish (8 vols, Frankfurt A. M., 1858–70), vi, 183–4; and I. Boulliau, *Philolai, sive dissertationis de vero systemate mundi* (Amsterdam, 1639), 20–21.
63. Once again, however, Riccioli pairs the Galileo reference with a “balancing” reference to the *Commentary* on Aristotle’s *Meteorologica* by Niccolò Cabeo (1586–1650), who squarely opposes Galileo on centrifugal force.
64. The history might reveal interesting insights not only about seventeenth-century astronomy but also about seventeenth-century natural philosophies; for example, William Gilbert thought the extrusion argument to be “frivolous” and of “no moment”. See W. Gilbert, *De mundo nostri sublunari philosophia nova* (Amsterdam, 1651), 161–2. Antonio Rocco (1586–1652), an Aristotelian natural philosopher who attacked Galileo’s *Dialogue* in 1633, thought the argument to be of no value and “di niun momento e falso”, qualifying it with almost the same words as Gilbert’s (Galileo, *Opere* (ref. 2), vii, 682).
65. Hill, “The projection argument in Galileo and Copernicus” (ref. 4), 133.

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# THE NUB OF THE LUNAR PROBLEM: FROM EULER TO G. W. HILL

CURTIS WILSON, St John's College, Annapolis

On 17 April 1766, Johann Albrecht Euler, son of Leonhard Euler, read to the Berlin Academy a paper entitled "Réflexions sur la Variation de la Lune".[1] (The term 'Variation' is the name Tycho Brahe gave to an inequality of the Moon he discovered in the 1590s: with the mean motion counted from New Moon or Full Moon, he found the Moon about $\frac{2}{3}$ of a degree behind its mean position a week before New Moon and before Full Moon, and about $\frac{2}{3}$ of a degree ahead of its mean position a week after New Moon and after Full Moon.) In the "Réflexions", Euler gave a derivation of the Variation from the law of gravitation. This is how he posed the problem:[2]

> To determine the motion of a moon making its revolutions around the Earth in the plane of the ecliptic, without eccentricity, while the Sun moves uniformly in a circle around the Earth.

This is a three-body problem. Its statement here abstracts from well-known features of our Moon's actual motion, chiefly the eccentricity of the Moon's orbit about the Earth, the inclination of its orbit to the ecliptic, and the eccentricity of the orbit of the Earth-Moon system about the Sun. The assumption of a circle for the orbit of the Sun (or Earth) implies that the Moon's mass is being taken as negligible. Of the simplified problem thus enunciated, Euler made bold to declare:

> However chimerical this question may appear, I dare assert that if one succeeded in finding a perfect solution of it, one would hardly find any further difficulty in determining the true movement of the actual Moon.

Before Euler, Isaac Newton had already given a geometrical derivation of Tycho's Variation from the inverse-square law.[3] Euler was no doubt aware of it. Newton showed that, if the Moon were to move pristinely in a circle about the Earth and the Sun's force was then introduced, it would flatten the circle in the direction of the Earth–Sun line (line of syzygies). The ratio of the minor axis to the major axis of the resulting oval would be approximately 69:70, and the Moon would move more rapidly through the syzygies than at the quadratures. Euler, by contrast, was asking in his paper of 1766 for a 'perfect' solution to the problem of the Variation. He undoubtedly meant an analytic solution consisting of a converging series of terms. In his paper he derived the first two terms of such a series. (His second term was mistaken, owing to an easily correctable arithmetical error.) His method would have permitted him to derive further terms, thereby improving the precision of his solution progressively.

For a century Euler's dare found no response from lunar theorists. Then in two papers of 1877 and 1878, George William Hill (1838–1914), a mathematician in the U.S. Nautical Almanac Office, accomplished all that Euler could have wished.[4] (We have no evidence that Hill ever read Euler's paper of 1766, but he was familiar with Euler's third lunar theory of 1772, which proceeds along the lines of the 1766 paper.) In his paper of 1878, Hill solved the very problem that Euler had proposed. He computed with high precision (to 15 decimal places) the numerical parameters defining the orbit that yields Tycho's Variation (he dubbed this orbit the 'Variation Curve'). His method was such as to permit increasing the precision to any degree that might be required. Moreover, he laid out a plan for developing the entire lunar theory on the basis of the Variation Curve.[5]

Hill's paper of 1877 assumes as known the Variation Curve developed in his paper of 1878. In this earlier paper Hill imagined an infinitesimal amount of eccentricity injected into the Variation Curve, and asked what motion of the perigee would ensue. His result, obtained by means of sophisticated summations of infinite series, was in astonishingly good agreement with the observed motion of the Moon's perigee.[6] No prior lunar theorist had come anywhere near as close. This was a stunning validation of his plan for taking the Variation Curve as the starting-point for developing the lunar theory.

Assigned to another engrossing task at the Nautical Almanac Office, Hill was deflected from completing his lunar theory. He bequeathed its further development to a younger man, Ernest W. Brown (1866–1938). Between 1891 and 1908, by untiring paper-and-pencil calculations, Brown carried Hill's theory to completion. This further development consisted in modifying Hill's solution to his differential equations in such a way as to take into account the features of the lunar motion that the first solution had abstracted from. These features were re-introduced as terms proportional to small parameters and their powers and products — chiefly the eccentricity of the Moon's orbit ($e \approx 1/18$), the sine of half the orbital inclination ($\gamma \approx 1/22$), the eccentricity of the Earth's orbit ($e' \approx 1/60$), and the parallax of the lunar orbit from the distance of the Sun ($a/a' \approx 1/390$). All these terms were sinusoidal; astronomers referred to them generically as "inequalities". In its final form the theory contained some 3000 sinusoidal terms, accurate to about a hundredth of an arc-second in all terms of the longitude and latitude, and to about a thousandth of an arc-second in parallax.

The Hill-Brown theory is the direct precursor of present-day lunar theory. In recent decades, lunar theory has been developed to yet higher degrees of precision, the aim always being to match the precision of the observations. At the present day observations can locate the Moon's position with respect to the Earth to within 2 or 3 cm in radius vector, and 5 or 6 cm in longitude. Further increases in observational precision can be expected. The theoretical predictions are being obtained by electronic computer through numerical integration of two differential equations that are recognizably Hill's, modified in minor respects.

Lunar theorists before Hill, from Alexis Clairaut (*Théorie de la Lune,* 1752) to Charles Delaunay (*Théorie du mouvement de la Lune,* 1860, 1867), with the notable exception of Leonhard Euler in his third lunar theory (*Theoria motuum Lunae,* *nova methodo pertractata,* 1772), typically took their start from a solution of the two-body problem, in which the Moon moves in an ellipse about the Earth in one focus. They then perturbed this solution so as to include the effects due to the Sun's gravitational action.

The ellipse differs from the Variation Curve. Both curves are symmetrical about two mutually perpendicular axes. Given the lengths of the two axes, there is one and only one ellipse that passes at right angles through the four endpoints of the axes, and one and only one Variation Curve passing at right angles through the same four points. The ellipse is expressed by a well-known algebraic formula, but the Variation Curve cannot be expressed by any finite formula. It must be calculated from the dynamics of the situation by successive approximations.

The choice of the ellipse as a starting-point for developing the lunar theory is understandable. What is sometimes called 'the elliptical inequality' — the inequality dependent on the eccentricity of the Moon's orbit about the Earth — is by far the largest of the inequalities in the Moon's motion. It leads to a departure of the Moon from its mean longitude by as much as $\pm\, 6°17'$. The next greatest inequality is the Variation. Its maximum value, which occurs in the octants of the syzygies, is $\pm\, 40'$, only 10.6% as large as the elliptical inequality.

Taking the ellipse as starting-point, however, was problematic, because it completely ignored the Sun's force on the Moon, which was over twice the Earth's force on the Moon. In circular orbits, Newton had shown that the gravitational force toward the central body is as the radius of the orbit and inversely as the square of the period.[7] Let the Moon's orbit about the Sun and its orbit about the Earth be treated as circles, which they approximately are. The ratio of the Sun's to the Earth's gravitational action on the Moon is approximately as the mean ratio of the Earth–Sun distance to the Moon–Earth distance, and inversely as the square of the ratio of the length of the year to the length of the month. Astronomers determined the distances in terms of horizontal parallaxes, the angles subtended at the Sun and Moon by the Earth's radius. The Moon's horizontal parallax was known to be $57\frac{1}{22}$ arc-minutes, implying that the Moon's mean distance was about 60 times the Earth's radius. The Sun's parallax had generally been over-estimated (and the Earth–Sun distance under-estimated) up to the 1760s, but Venus transits in the 1760s led to a value of about 9 arc-seconds;[8] this value was used in the "Réflexions" of 1766 and in Leonhard Euler's lunar theory of 1772. To three decimal places the present-day value is 8.794 arc-seconds. Given Euler's values for these parallaxes and the ratio of the sidereal month to the year (about 1/13.369), the Sun's gravitational action on the Moon is found to be 2.18 times the Earth's action on the Moon, so that the Earth's force is less than a third of the total force exerted on the Moon.[9] Yet a computation by successive approximations calls for capturing at each stage more than half of the quantity remaining to be found; otherwise the process is in danger of failing to converge. Since the Earth-focused ellipse leaves out of account more than $\frac{2}{3}$ of the total force acting, it was a questionable beginning for calculating the Moon's motion.

The problematic character of this starting-point showed itself in the earliest efforts to compute the motion of the Moon in algebraic form. Leonhard Euler, Alexis Clairaut, and Jean le Rond d'Alembert, the first mathematicians to undertake this computation, followed somewhat different calculative routes, but each started from a solution of the two-body problem which left the Sun's force initially out of the account. In 1747 all three reported finding only about half the observed motion of the Moon's apse. They undoubtedly regarded the apsidal motion as a sensitive indicator of the nature of the perturbing force. Thus if one assumes that the gravitational force varies as a power of the distance, then, as Isaac Newton showed in Proposition 45 of Book I of the *Principia,* the motion of the apse determines the power of the distance that the force is proportional to, and *vice versa.* Euler and Clairaut — influenced by prior (and different) metaphysical and epistemological assumptions — concluded that Newton's law was inexact.[10] D'Alembert believed Newton's law to be exact, but proposed that some force besides gravitation, perhaps a magnetic force, was also acting.

Then in 1749 Clairaut carried out a second-order approximation. In his first-order solution of the differential equations, he had been able to identify the larger terms with an expression for a *rotating* ellipse:

$$\frac{1}{r}=\frac{1-e\cos m\varphi}{k}.$$

Here $r$ is the radius vector from Earth's centre to Moon's centre, $e$ is the eccentricity of the ellipse, $k$ is called the parameter of the ellipse, $\varphi$ is the Moon's angular distance from a fixed line, and $m$ is a constant whose difference from unity gives the motion of the apsidal line or major axis of the ellipse. From his identification of expressions in his solution with the constants in the above equation, Clairaut was able to evaluate the constants $k$, $e$, and $m$. The initial solution of the differential equations for $1/r$ contained, besides the term expressing the rotating ellipse, three much smaller terms. To obtain a second approximation, Clairaut substituted the first-approximation value for $1/r$, including the three much smaller terms, back into the differential equations, and then re-determined the constants $k$, $e$, and $m$. The new value of $m$ turned out to include most of the missing apsidal motion. By an independent calculative route, Leonhard Euler confirmed that the inverse-square law was not in error.[11] The difficulty had been one of slow convergence. But, even when the second-order approximations were included, Clairaut's lunar theory still failed to locate the Moon precisely enough for determining the longitude at sea to within a degree. Slow convergence was apparently a *pervasive* difficulty in the development of lunar theory.

Leonhard Euler pondered this fact. He believed that a similar difficulty had emerged in computing the motions of the planets. Writing on the inequalities in the motions of Jupiter and Saturn in 1748,[12] he had introduced trigonometric series to express the perturbing forces — a major innovation. However, his derivations failed to account for puzzling variations in the motions of these two planets. He attributed this failure to slow convergence of his trigonometric series. This attribution was a mistake — as would become clear from P. S. Laplace's discovery in 1785 of the quite different source of the anomaly in the motions of Jupiter and Saturn.[13] Throughout the 1760s

Euler viewed slow convergence as the major analytical difficulty in celestial mechanics, and he sought radical measures to cope with it.

Thus, in 1762 Euler proposed calculating perturbations both of planets and the Moon by numerically integrating the differential equations, starting from observed positions and velocities. No series approximations would be employed, and no attempt would be made to obtain a theory valid for all time.[14] In a paper of 1763, he showed how accurate initial conditions could be obtained through an application of the calculus of finite differences to a series of observations.[15] These methods were later applied to comets, for instance by Laplace. At the present day Jet Propulsion Laboratory computes the Moon's positions by numerical integration, with initial conditions derived by finite differences in the manner Euler proposed.

In the "Réflexions sur la Variation de la Lune" of 1766, Euler undertook to treat the inequality known as the Variation separately from the other inequalities of the Moon.[16] As justification he stated that, in the absence of a solution of the general three-body problem, the surest way of perfecting the lunar theory was to simplify the question as much as possible. Lunar theorists since Newton had already tried abstracting from both the inclination of the Moon's orbit to the ecliptic and the eccentricity of the Earth's orbit. According to Euler, the other inequalities to be dealt with were those dependent on the angular elongation of the Moon from the Sun (the Variation), and on the eccentricity of the Moon's orbit (the so-called 'elliptic inequality'). Inequalities deriving from the Moon's inclination and the orbital eccentricity of the Earth could be dealt with later; they were small enough to be considered separately from each other and from the other inequalities. The "Réflexions" proposed carrying this simplification one step farther, by abstracting from the eccentricity of the lunar orbit.

Why did Euler choose the inequality of the Variation over the 'elliptic' inequality as the first problem to be coped with? He does not spell out his thinking here. Possibly he had come to see the Variation as more nearly the *gist* of the lunar problem. It was a simplified form of the three-body problem. With the lengths of the year and the month determined — they were among the best-known constants of astronomy — this problem of finding a periodic orbit was completely *determinate*; it had a perfectly definite solution, though reachable only by successive approximations. By contrast, starting from the lunar eccentricity meant leaving the effect of the Sun entirely out of account. Moreover, the eccentricity was variable, and measuring its mean value was difficult. Because the lunar problem was a three-body problem, it *had* to involve the Variation. But it did not have to involve eccentricity.

The solution curve for the Variation problem is depicted in Figure 1, with the following obvious distortions: the flattening of the orbit is exaggerated, and so is the size of the lunar orbit relative to the Earth–Sun distance (a solar parallax of 9 arc-seconds makes the mean radius of the lunar orbit only 1/390$^{\text{th}}$ of the Earth–Sun distance). In the figure, **S** is the Sun, **E** the Earth, and **abcd** the path of the Moon relative to the Earth. The flattening of the oval **abcd** can be understood as follows. When the Moon is at **a**, it is more accelerated toward the Sun than the Earth is, because it is closer to the Sun; when it is at **c**, it is less accelerated toward the Sun than the Earth is. In


FIG. 1

each of these cases, the difference in solar force has the effect of slightly reducing the Moon's acceleration toward the Earth. The Moon's path at **a** and **c** is therefore less incurvated toward the Earth. As a result, the diameter **ac** of the oval is a little shorter than the diameter **bd**. Newton found the ratio of these two diameters to be 69:70. Euler's numerical results in the "Réflexions" agree with this value.

The orbit of the Moon about the Earth represented in Figure 1 is the simplest case of a Moon that we could have — a Moon seen by Earthlings to be going round the Earth. But the Moon's path must be continuously incurvated toward *both* the Sun and the Earth. To adjust our imaginations to this fact we must go beyond a static diagram like Figure 1 and take account of the relative motions of the Moon with respect to both Sun and Earth. The Moon's motion from **a** to **b** (from New Moon to First Quarter) requires 7.4 days on average. At the Sun, the path thus executed subtends an angle of 8.8 arc-minutes, about a seventh of a degree. But during the same 7.4 days, the whole Earth-Moon system moves through about 7°.3 about the Sun, an arc fifty times greater. The Moon's motion relative to the Earth adds to or subtracts from the mean motion of the Earth-Moon system about the Sun up to a fiftieth, and it adds to or subtracts from the mean Moon–Sun distance about 1/390th. The paths of both Earth and Moon are always concave to the Sun, but the two bodies weave in and out as first one and then the other is closer to the Sun.

The Variation, more than any of the other inequalities in the Moon's motion, may have triggered in Euler a sharp focus on the mathematical complexity presented by the Moon's motion. As stated earlier, the Variation Curve is determinate given the lengths of the month and the year, but its precise shape is only progressively knowable, by the extraction of successive approximations. Newton approximated it with an ellipse,[17] but it is not an ellipse or any other oval with a finitely expressible formula. In this respect it resembles the lunar theory as a whole: the exact character of the motion is hidden in the dynamics. These realizations can have led Euler to his claim that, if the problem of the Variation were solved perfectly, no major difficulties would remain in developing the lunar theory.

In the "Réflexions" of 1766 Euler treated the Variation as a completely periodic motion. He let $\eta$ represent the varying angular elongation of the Moon from the Sun, and let $d\theta/dt$ represent the Sun's angular motion with respect to the Earth, assumed uniform. The rate of motion $d\eta/dt$ is slightly variable but nearly constant. Euler gave the mean value of $d\eta/d\theta$ as $n$ = 12.3708, and set

$$\frac{d\eta}{d\theta}=n+P+\frac{Q}{n}+\frac{R}{n^2}+\frac{S}{n^3}+\frac{T}{n^4}+....$$

This was a way of ordering the quantities contained in $d\eta/d\theta$ with respect to smallness. In a similar way, he signified the mean Moon–Earth distance by $b$, and the actual Moon–Earth distance by $bu$, where

$$u=1+\frac{p}{n}+\frac{q}{n^2}+\frac{r}{n^3}+\frac{s}{n^4}+\frac{t}{n^5}+....$$

Finally, he set another quantity in the differential equations, involving the ratio of the Sun's and Earth's gravitational forces, equal to a series in $1/n$, and labelled it $m$:

$$m=\alpha n^2+\beta n^1+\gamma n^0+\frac{\delta}{n}+\frac{\varepsilon}{n^2}+\frac{\zeta}{n^3}+\frac{\eta}{n^4}+....$$

He substituted all three expressions back into his differential equations. These equations represented the Sun's and Earth's forces producing accelerations in the Moon's motion. He then obtained expressions for $P$, $Q$,…, $p$, $q$,…, $\alpha$, $\beta$,…, etc., by setting the resulting coefficients of $1/n$, $1/n^2$, etc., equal to zero. To fourth-order terms in $1/n$, Euler found

$$\frac{d\eta}{d\theta}=n+M\cos 2\eta-N\cos 4\eta,$$

where $M=\frac{11}{4n}+\frac{13}{3n^2}+\frac{32}{9n^3}+\frac{89}{54n^4}$,

and $N=\frac{41}{\;}+\frac{3281}{\;}.$

$$N=\frac{41}{64n^3}+\frac{3281}{720n^4}.$$

Euler's $1/n$ is the same as Hill's **m**, the ratio of the Sun's mean motion to the Moon's mean synodic motion, or of the month to the year.[18] Euler also obtained $bu$, the Moon–Earth distance in the Variation Curve, as a function of $\eta$.

The method of the "Réflexions" appears to have been a stepping-stone to Euler's method in his third lunar theory, the *Theoria motuum lunae, nova methodo pertractata* of 1772. Here he treated all the inequalities of the Moon as proportional to successive powers and products of small parameters. It was this strategy that Hill seized upon when, disillusioned with earlier algebraic ways of developing the lunar theory because of slow convergence, he began looking for a new way of proceeding. Thus, in the introduction to his essay of 1878, "Researches in the lunar theory",[19] Hill proposed dividing the periodic developments of the lunar coordinates "into classes of terms in the manner of Euler in his last lunar theory", and treating first separately and then conjointly the following five classes of inequalities:

1. inequalities dependent on the ratio of the mean motions of the Sun and Moon,

2. inequalities proportional to the lunar eccentricity,
3. inequalities proportional to the sine of the lunar inclination,
4. inequalities proportional to the solar eccentricity, and
5. inequalities proportional to the solar parallax.[20]

Of the project thus set forth, Hill was able to complete his analysis only of the first class.

Like Euler, Hill used rotating rectangular coordinates. But whereas Euler made them rotate with the mean speed of the Moon, Hill made them rotate with the mean speed of the Sun.

Although Euler's "Réflexions" paper, if Hill had read it, would have suggested to him the desirability of obtaining an exact representation of the Variation Curve, Hill's decision to seek such a representation appears to have been triggered rather by the failure of his love-affair with Delaunay's method of treating the lunar theory.[21] Delaunay's method, which Hill began studying in the 1870s, at first aroused his enthusiastic allegiance. In an article published in 1876, he introduced Delaunay's differential equations to his American colleagues. His first sentence reads:

> The method of treating the lunar theory adopted by Delaunay is so elegant that it cannot fail to become in the future the classic method of treating all the problems of celestial mechanics.[22]

We can guess what aroused Hill's enthusiasm. Delaunay's method was systematic and transparent. It was sophisticated in making use of post-Lagrangian refinements in dynamics. For Hill, it must have stood in sharp contrast with Hansen's lunar theory, which had been adopted both in Great Britain and France, beginning in 1862, as the basis for the nautical almanacs.

Hansen's theory was ostensibly based on the law of gravitation alone. From the inception of the *Nautical almanac* in 1767, a large measure of empiricism had entered into the tables used in computing the lunar ephemerides for the *Nautical almanac.* Hansen's theory, claiming to be free of such empiricism, was in that respect epoch-making. But it was a *numerical* theory. Numbers were substituted for the arbitrary constants at an early stage. Therefore, if the theory failed to agree with observation (and discrepancies soon appeared), it was difficult or impossible to discover the source of the discrepancy. The derived numbers effectively concealed the route by which the tables had been derived. Hansen's theory had required twenty years to construct in the first place, and to correct it responsibly would have required beginning all over again. A *literal* or algebraic theory like Delaunay's was clearly preferable because each of its results was traceable to its theoretical roots.

But soon Hill discovered a serious difficulty with Delaunay's method as applied to the Moon. It was incurably afflicted with slow convergence. Starting from the two-body, elliptical solution for the Moon's motion, Delaunay had carried the successive approximations farther than ever before. But he found that approximations taken to the ninth order of small quantities were still insufficient to match the precision currently attainable in observations. Consider, for instance, the inequality called the *evection,* which depends on *e,* the eccentricity of the lunar orbit, and on $m = n'/n$, the ratio of the Sun's mean motion to the Moon's mean motion.[23] The coefficient of the *evection*, when derived analytically, consists of a series of terms. As the first term of the series Delaunay obtained (15/4)*me,* and as the 9th-order term,

$$\frac{413277465931033}{15288238080} m^8 e.$$

Here $m \approx 1/13.369$, and $e \approx 1/18.349$. The ninth-order contribution proves smaller than unity, but it is not much smaller than the 8th-order term; and the sum of the terms Delaunay calculated failed to determine enough significant figures in the coefficient to match the precision of modern observations. This difficulty recurred in the coefficients of other inequalities. The successive contributions of the higher-order terms were factored by top-heavy numerical fractions and by *m* raised to high powers. Wherever slow convergence made its appearance, so also did the parameter *m*.

Hill concluded that Delaunay's project, grand in conception and initially promising in precision, had failed. The convergence was too slow and too consuming of human time and labour to yield a result of the precision desired. (Whether the series were "convergent" in the strict mathematical sense was unknown, but the immediate issue here was whether a precise enough result could be extracted in a responsible way.) This failure provoked Hill to turn back to Euler and take up the construction of a new lunar theory on the basis of the Variation Curve.

As indicated earlier, Hill originally planned to compute all five of the classes of inequality that Euler had listed. He had to abandon this project when Simon Newcomb, superintendent of the U.S. Nautical Almanac Office from 1877 to 1897, asked him to develop the theories of Jupiter and Saturn, a task that absorbed Hill's time and effort throughout the decade preceding his retirement in 1892. The only part of his plan that he completed was that given by his two papers of 1877 and 1878, relating to the Variation.

In what follows we outline what Hill accomplished in those two papers. As origin for his coordinate system he chose the Earth's centre, with *x*-axis passing through the Mean Sun, and *y*-axis also in the ecliptic, at right angles to the *x*-axis. The Mean Sun is a point moving around the ecliptic with a constant angular speed $n'$, equal to the true Sun's mean speed. Hill posited a moon confined to the ecliptic and so without latitude, and having a mass too small to influence the motion of the Earth (the mass of the real Moon is less than 1/81 of the Earth's mass). To derive differential equations for this fictive moon's motion, he used an algorithm due to Lagrange (we omit a description of it, except to say that it starts from expressions for the moon's kinetic and potential energy). The result was

$$\frac{d^2x}{dt^2} - 2n'\frac{dy}{dt} + \left[\frac{\mu}{r^3} - 3n'^2\right]x = 0,$$

$$\frac{d^2y}{dt^2} + 2n'\frac{dx}{dt} + \frac{\mu}{r^3}y = 0.$$

Here $\mu$ is the Earth's mass, and $r = \sqrt{x^2 + y^2}$ is the distance from fictive moon to the Earth.

The coordinates $x$, $y$ specify the position of the fictive point-moon in the coordinate system just described. The equations do not tell us what path this moon will move in, unless we stipulate initial values for the position and velocity. Through any point in the $x$-$y$ plane, an infinite number of possible trajectories pass, each in accord with the restrictions imposed by the above equations. If we stipulate both position and velocity, the path that our fictive moon must follow is uniquely determined.

In solving these equations, Hill chose expressions for $x$ and $y$ such as to yield the particular solution that is the Variation Curve:

$$x = \sum_{-\infty}^{+\infty} \mathrm{a}_i \cos(2i+1)\nu(t-t_0),$$

$$y = \sum_{-\infty}^{+\infty} \mathrm{a}_i \sin(2i+1)\nu(t-t_0).$$

Here $i$ is an integer running from minus to plus infinity, $\nu$ is the Moon's mean synodic speed ($2\pi$ radians per 29.5305889 days, or 0.2127687 radians/day), and $t$ is the time measured from $t_0$ when the moon passes through conjunction with the Mean Sun. The product of $\nu$ and $(t - t_0)$ yields the angle about the Earth from New Moon to the Moon's position on the orbit **abcd**. The choice of the odd integers $(2i + 1)$ as multipliers of $\nu(t - t_0)$ guarantees that when $\nu(t - t_0)$ is $\pi/2$ or an odd integral multiple thereof, $x$ and $dy/dt$ will both be zero, and when $\nu(t - t_0)$ is zero or $\pi$ or an integral multiple of $\pi$, $y$ and $dx/dt$ will both be zero; thus the orbit where it crosses the $x$-axis and $y$-axis will do so perpendicularly — the conditions necessary and sufficient for a periodic orbit. To obtain the values of the coefficients $\mathrm{a}_i$, Hill substituted the expressions proposed above for $x$ and $y$ into the differential equations, and proceeded by successive approximations to determine the ratios of the successive $\mathrm{a}_i$ to $\mathrm{a}_0$. This way of solving differential equations is an example of what is called 'the method of undetermined coefficients'. The rate of convergence that Hill obtained is impressive:[24]

$$\begin{aligned}
\mathrm{a}_1/\mathrm{a}_0 &= +0.001515707479563, & \mathrm{a}_{-1}/\mathrm{a}_0 &= -0.008695746961540,\\
\mathrm{a}_2/\mathrm{a}_0 &= +0.000005878656578, & \mathrm{a}_{-2}/\mathrm{a}_0 &= +0.000000163790486,\\
\mathrm{a}_3/\mathrm{a}_0 &= +0.000000030031632, & \mathrm{a}_{-3}/\mathrm{a}_0 &= +0.000000002460393,\\
\mathrm{a}_4/\mathrm{a}_0 &= +0.000000000175268, & \mathrm{a}_{-4}/\mathrm{a}_0 &= +0.000000000012284,\\
\mathrm{a}_5/\mathrm{a}_0 &= +0.000000000001107, & \mathrm{a}_{-5}/\mathrm{a}_0 &= +0.000000000000064,\\
\mathrm{a}_6/\mathrm{a}_0 &= +0.000000000000007, & \mathrm{a}_{-6}/\mathrm{a}_0 &= +0.000000000000000.
\end{aligned}$$

The constant $\mathrm{a}_0$ depends on the mass $\mu$ of the Earth and mean motion $n$ of the Moon:

$$\mathrm{a}_0 = 0.999093141975298\left[\frac{\mu}{n^2}\right].$$

The constants thus computed, when substituted back into Hill's expressions for $x$ and $y$, determine the shape of the Variation Curve. The derivatives $dx/dt$ and $dy/dt$ determine the motion of the fictive moon upon that curve. With his solution for the

Variation in hand, Hill wanted to find out what the apsidal motion would be if a tiny bit of eccentricity were injected into the Variation Curve. This curve, though not a circle, was not eccentric: it was symmetric with both the $x$- and $y$-axes. If $x$ and $y$, initially determined so as to fit the Variation Curve, were allowed to receive increments, $\delta x$ and $\delta y$ respectively, small enough so that their squares could be neglected, but such as to cause the curve to deviate from its original symmetry, the altered curve would have a perigee and apogee (maximal departures from the Variation Curve toward or away from the Earth's centre), and these points would move because the introduction of $\delta x$, $\delta y$ violates the conditions for periodicity originally imposed. Could the rate at which the perigee moves be determined?

Hill addressed this question in his lunar paper of 1877, "On the part of the motion of the lunar perigee which is a function of the mean motions of the Sun and Moon.[25]. He first set out the differential equations used in deriving the Variation as follows:

$$\frac{d^2x}{dt^2}=\frac{\partial\Omega}{\partial x},\quad \frac{d^2y}{dt^2}=\frac{\partial\Omega}{\partial y}. \qquad (1)$$

Here the partial derivatives express the forces — the gravitational forces and also the apparent forces due to the choice of rotating coordinates. These equations admit of an integral, as first shown by C. G. J. Jacobi in 1836.[26] Jacobi's integrating factors were, for the first and second equations respectively,

$$F=\frac{dx}{dt}+n'y,\quad G=\frac{dy}{dt}-n'x,$$

where $n'$ is the mean angular speed of the Sun. The sum of the two products could be integrated, yielding

$$\frac{dx^2+dy^2}{dt^2}-n'\frac{xdy-ydx}{dt}=\Omega+C. \qquad (2)$$

Here $C$ is a constant of integration. Hill called (2) the Jacobian integral.

Let $x_0$ and $y_0$ designate the coordinates in the Variation Curve, and let Equations (1) be written, first with $x_0$, $y_0$ replacing $x$, $y$, then with $x_0+\delta x$, $y_0+\delta y$ replacing $x$, $y$. The difference between the two results yields the differential equations for $\delta x$, $\delta y$:

$$\frac{d^2\delta x}{dt^2}=H\delta x+J\delta y,\quad \frac{d^2\delta y}{dt^2}=K\delta y+J\delta x, \qquad (3)$$

$$\text{where } H=\left(\frac{\partial^2\Omega}{\partial x^2}\right)_0,\ K=\left(\frac{\partial^2\Omega}{\partial y^2}\right)_0,\ J=\left(\frac{\partial^2\Omega}{\partial x\partial y}\right)_0.$$

The subscript zero attached to the expressions for $H$, $K$, $J$ signifies that these partial derivatives are to be evaluated in the coordinates $x_0$, $y_0$ of the Variation Curve.

Hill's process leading to a solution for (3) was complicated, and we shall do no more than indicate the main steps.[27] He first showed that $\delta x = F$, $\delta y = G$ is a solution that defines $\delta x$, $\delta y$ in terms of the variables of the Variation Curve. This solution reveals nothing about *departures* from that curve. It permitted, however, a reduction of the order of the problem, always an advantage in solving differential equations. Hill effected this by introducing new variables $\rho$, $\sigma$ such that $\delta x = \mathrm{F}\rho$, $\delta y = G\sigma$. The result was a second-order differential equation containing a first-order term (proportional, say, to $d\rho/dt$) and also a term proportional to $\rho$. To eliminate the first-order term, Hill introduced a new variable w in accordance with the equations

$$\frac{d\rho}{dt} = -\frac{G^2}{F^2}\frac{d\sigma}{dt} = \sqrt{\frac{JG}{F}}\mathrm{w}.$$

He thus reduced the equation to the form

$$\frac{d^2\mathrm{w}}{dt^2} + \Theta\mathrm{w} = 0. \quad (5)$$

Here $\Theta$ turns out to be given by an infinite series $\theta_0 + \theta_1 \cos 2\tau + \theta_2 \cos 4\tau + \ldots$, where $\tau$ denotes the difference between the Moon's mean longitude and the Sun's mean longitude, and the $\theta_i$ denote constants definable in terms of $\mathrm{m} = n'/(n - n')$. If $\Theta$ were equal to the constant $\theta_0$, Hill tells us that the complete integral of (5) would be

$$\mathrm{w} = K\zeta^{\mathrm{c}} + K'\zeta^{-\mathrm{c}}.$$

Here c has been put for $\theta_0$, and $\zeta = \varepsilon^{\tau\sqrt{-1}} = \cos\tau + \sqrt{-1}\sin\tau$, where $\varepsilon$ stands for the base of natural logarithms, and the equivalence of the exponential to the trigonometric terms is a famous result of Euler's. According to Hill, if additional terms of $\Theta$ are included, the effect is to modify the value of c and add to w new terms of the form $A\zeta^{\pm\mathrm{c}+2i}$.

With the idea of using the method of undetermined coefficients to obtain a solution of (5), Hill proposed a particular integral, $\mathrm{w} = \sum_i b_i\zeta^{\mathrm{c}+2i}$. Here the $b_i$ are constants to be determined, and $i$ takes all integral values from $-\infty$ to $+\infty$. Substituting the particular integral into (5), he got the equation

$$[\mathrm{c}+2j]^2 b_j - \sum_i \left(\theta_{j-i} b_i\right) = 0. \quad (6)$$

This holds for all integral values of $j$, positive and negative, and thus yields an infinite number of homogeneous linear equations, each equation containing an infinite number of terms. The conditions (6) determine the ratios of all the $b_i$ to one of them, for instance $b_0$, which could be taken as the arbitrary constant. They also determine c, which is the ratio of the synodic to the anomalistic month (the time for the Moon to go from perigee back to perigee). To exhibit the properties of the equations more clearly, Hill wrote out a few of them *in extenso,* using the symbol $[i] = (\mathrm{c} + 2i)^2 - \theta_0$:

$$\begin{array}{llllll}
\ldots & & & & & \\
\ldots + [-2]b_{-2} - \theta_1 b_{-1} & -\theta_2 b_0 & -\theta_3 b_1 & -\theta_4 b_2 & -\ldots = 0, \\
\ldots - \theta_1 b_{-2} & +[-1]b_{-1} - \theta_1 b_0 & -\theta_2 b_1 & -\theta_3 b_2 & -\ldots = 0, \\
\ldots - \theta_2 b_{-2} & -\theta_1 b_{-1} & +[0]b_0 & -\theta_1 b_1 & -\theta_2 b_2 & -\ldots = 0, \\
\ldots - \theta_3 b_{-2} & -\theta_2 b_{-1} & -\theta_1 b_0 & +[1]b_1 & -\theta_1 b_2 & -\ldots = 0, \\
\ldots - \theta_4 b_{-2} & -\theta_3 b_{-1} & -\theta_2 b_0 & -\theta_1 b_1 & +[2]b_2 & -\ldots = 0, \\
\ldots & & & & &
\end{array}$$

If the Equations (6) had been finite in number, with each equation consisting of only a finite number of terms, the condition of their being solved simultaneously would have been that their *determinant* be equal to zero.[28] Hill assumed that this condition would still apply when the equations were infinite in number. Using summations he may have obtained from Euler,[29] Hill famously solved the infinite determinant resulting from the infinity of Equations (6) obtained when all the $b_i$ are eliminated save $b_0$. He thus found for c the value 1.071583277416012, which he believed to be exact to nearly the 15th decimal.

Now the ratio of the motion of the perigee to the sidereal mean motion of the Moon is given by

$$\frac{1}{n}\frac{d\mathrm{w}}{dt} = 1 - \frac{\mathrm{c}}{1+\mathrm{m}}.$$

Hill's values for c and m gave this ratio as 0.008572573004864, exceeding the value given by observation, viz. 0.008452, by 1.43% or about 1/69th. Hill had anticipated that his result would be too large. His derivation assumed a zero inclination of the Moon's orbit to the ecliptic; therefore, since the inclination reduces the effectiveness of the Sun's action, he had in effect introduced more solar action than is actually exerted.

Hill's purpose had been

> … to compute the value of this quantity [the Moon's apsidal motion], so far as it depends on the mean motions of the sun and moon, with a degree of accuracy that shall leave nothing further to be desired.

His result showed that if the Variation Curve were inoculated with just a tiny amount of eccentricity, deranging slightly the symmetry of the curve and the periodicity of the fictive moon's motion upon it, then the resulting curve would acquire an apse, or maximal departure from the Variation Curve, and this apse would move forward with 101.43% of the real Moon's apsidal motion. If the fictive moon's orbit were inclined to the ecliptic to the same degree as the real Moon's orbit, most or all of the extra 1.43% could be expected to disappear.

### *Conclusion and Epilogue*

In 1747 Euler, Clairaut, and d'Alembert obtained in their first-stage calculation only about one-half the apsidal motion of the real Moon. Their successors before Hill fared no better in their first-stage calculations, and even with multiple successive approximations did not come as close as Hill to the apsidal motion of the real Moon. Thus the eccentric ellipse — used as a first approximation by all our lunar theorists except Euler and Hill — did poorly as a starting-point for computing the apsidal motion, while Hill's Variation Curve did well. Indeed, remarkably well.

Could other curves do as well or better? Certainly we would expect that curves obtained by "correcting" the Variation Curve so as to achieve a still closer approximation to the orbit of the actual Moon would do as well or better. Such correcting was precisely what E. W. Brown did in calculating the remaining four of the five classes of inequality that Euler had listed, the departures from the Variation Curve that are proportional to the small parameters $e, \gamma, e'$ and $a/a'$ and to their products and powers. To verify the results of his calculations, Brown compared them with 'the variation' of the Variation Curve (that is, the differential of the algebraic expression of this curve).[30] The Moon's actual path departs from being a projection of the Variation Curve only 'infinitesimally'.

Thus Euler, in his claim about the importance of the Variation in solving the lunar problem, was right, but he can hardly have *known* that he was right. In expressing his claim as a dare, he did well rhetorically. But was the proof in the pudding? That is, did the Hill-Brown theory fit the observed motions of the Moon with near exactitude?

Not immediately. The Moon was accelerating at a somewhat greater rate than planetary perturbations implied, and it was also subject to fluctuations that could not be derived in any fashion from the law of gravitation. This topic would require another paper, but the reader will want to know how the issue was at last resolved. The extra acceleration could be explained as due to the slowing of the Earth's rotation caused by tidal friction, and the fluctuations could be due to random changes in the Earth's angular momentum, produced by changes in sea level and atmospheric pressure, electromagnetic coupling or de-coupling of the Earth's core and mantle, and other causes. Brown was convinced that the Hill-Brown theory as completed by himself gave an accurate account of the gravitational effects on the motion of the Moon, but for some years entertained the possibility that other forces (possibly magnetic) were acting on the Moon. By 1920 the writings of J. K. Fotheringham and Harold Jeffreys persuaded him that the extra acceleration and fluctuations were more plausibly attributable to changes in the Earth's rotation.[31] In 1926 he published an extended study of the evidence for this, along with speculations about the possible physical cause of the fluctuations.[32] The first firm confirmation of the *fact* that the Earth's rotation was varying came in 1939, with H. Spencer Jones's demonstration that the Moon's acceleration is mirrored by accelerations in the motions of the Sun and Mercury.[33] Full confirmation came after 1955 when atomic clocks were introduced, replacing the Earth as the astronomer's clock. From then till now, the Earth's variable rotation has remained a lively topic of ongoing geophysical research.[34]

*Acknowledgements*

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REFERENCES

1. J. A. Euler, "Réflexions sur la Variation de la Lune", *Histoire de l'Académie Royale des Sciences et Belles-Lettres*, xxii (1766), 334–53. I have capitalized the first letters of "Variation" and "Lune" to indicate that they are used here as proper nouns.
2. I translate from Euler's French. In view of both the technical expertise of the paper and its bold

declarative style, I am strongly inclined to regard the senior Euler, rather than his less distinguished son, as the author.

3. In Book I of the *Principia*, Prop. 66, Corollaries 2–5, and in Book III, Props. 26–29.
4. The two papers can be found in *The collected mathematical works of George William Hill* (4 vols, Washington, DC, 1905–7), i, 243–70 and 284–335. Their titles and places of original publication are given in the next two references.
5. G. W. Hill, "Researches in the lunar theory", *American journal of mathematics,* i (1878), 5–26, 129–47, 245–60.
6. G. W. Hill, "On the part of the motion of the lunar perigee which is a function of the mean motions of the Sun and Moon", separately published, Cambridge, MA, 1877; reprinted in *Acta mathematica,* viii (1886), 1–36.
7. Newton, *Principia,* Book I, Prop. 4, Corollary 2.
8. See Albert Van Helden, "Measuring solar parallax: The Venus transits of 1761 and 1769 and their nineteenth-century sequels", in René Taton and Curtis Wilson (eds), *Planetary astronomy from the Renaissance to the rise of astrophysics*, Part B: *The eighteenth and nineteenth centuries* (Cambridge, 1995), 153–68.
9. The result can be obtained from Newton's *Principia,* Prop. 4 of Book I.
10. On Euler's views, see Curtis Wilson, "Euler on action-at-a-distance and fundamental equations of continuum mechanics", in P. M. Harman and Alan E. Shapiro (eds), *The investigation of difficult things: Essays on Newton and the history of the exact sciences in honour of D. T. Whiteside* (Cambridge, 1992), 399–420. For Clairaut, see Craig B. Waff, "Clairaut and the motion of the lunar apse: The inverse-square law undergoes a test", *Planetary astronomy from the Renaissance to the rise of astrophysics,* Part B (ref. 8), 35–46.
11. See his second lunar theory: *Theoria motus lunae exhibens omnes eius inequalitates* (Berlin, 1753), in *Leonardi Euleri, Opera omnia*, ser. 2, xxiii, 64–336.
12. Leonhard Euler, *Recherches sur la question des inégalités du movement de Saturne et de Jupiter, sujet proposé pour le prix de l'année 1748, par l'Académie Royale des Sciences de Paris*, in *Leonardi Euleri, Opera omnia*, ser. 2, xxv, 45–117.
13. On 23 November 1785 Laplace announced to the Paris Academy that the anomalies in the mean motions of Jupiter and Saturn could be accounted for on the assumption of universal gravitation. See Curtis Wilson, "The Great Inequality of Jupiter and Saturn from Kepler to Laplace", *Archive for history of exact sciences*, xxxiii (1985), 15–290.
14. Leonhard Euler, [E398] = "Nouvelle méthode de determiner les dérangemens dans le movement des corps célestes, causés par leur action mutuelle", *Mémoires de l'Académie des Sciences de Berlin*, xix (1763), 1770, 141–79. To be published in *Leonardi Euleri, Opera omnia*, ser. 2, xxvi.
15. Leonhard Euler, [E401] = "Nouvelle manière de comparer les observations de la Lune avec la théorie", *Mémoires de l'Académie des Sciences de Berlin*, xix (1763), 1770, 221–34. *Opera Omnia*, ser. 2, xxiv, 90–100.
16. The term 'inequality' has been used at least since the seventeenth century to refer to sinusoidal terms that have to be added to the mean motion of a planet or satellite in order to obtain the true motion.
17. See Prop. 28 of Book III of the *Principia,* where Newton derives the ratio 70:69 of the two axes of the Variation Curve.
18. In the notation used by Hill and Brown, m = $n'/(n - n')$, where $n'$ is the mean motion of the Sun and $(n - n')$ the Moon's mean synodic motion. Here $n$ is the Moon's mean sidereal motion, not to be confused with Euler's $n$ = 1/m.
19. Hill, "Researches in the lunar theory", *Collected mathematical works of G. W. Hill* (ref. 4), i, 286.
20. *Ibid.*
21. On Hill's undergraduate studies at Rutgers under Theodore Strong, and their emphasis on Euler, see Edward R. Hogan, "Theodore Strong and ante-bellum American mathematics", *Historia mathematica*, viii (1981), 439–55.
22. G. W. Hill, "Demonstration of the differential equations employed by Delaunay in the lunar theory",

*The analyst,* iii (1876), 161–227.

23. Hill, "Researches in the lunar theory", *Collected mathematical works of G. W. Hill* (ref. 4), i, 285.
24. G. W. Hill, "Researches in the lunar theory", *Collected mathematical works of G. W. Hill* (ref. 4), i, 323–4.
25. G. W. Hill, "On the part of the motion of the lunar perigee which is a function of the mean motions of the Sun and Moon", *Collected mathematical works of G. W. Hill* (ref. 4), i, 243–70; separately published, Cambridge, MA, 1877, and reprinted in *Acta mathematica,* viii (1886), 1–36.
26. C. G. J. Jacobi, "Sur le mouvement d'un point et sur un cas particulier du problème des trois corps", *Comptes rendus de l'Académie de Paris*, iii (1836), 59–61; reprinted in C. G. J. Jacobi's *Gesammelte Werke*, iv, ed. by K. Weierstrass (Berlin, 1886), 35–38. According to Jacobi in his *Vorlesungen über Dynamik* (ed. by A. Clebsch) in *Gesammelte Werke, Supplementband* (Berlin, 1884), 10, Euler regarded the *vis viva* integral as valid only about a *fixed* centre of attraction; Jacobi got his Jacobian integral by applying it to a *moving* centre.
27. A presentation of the derivation less cryptic and more reader-friendly than Hill's is given by D. Brouwer and G. M. Clemence, *Methods of celestial mechanics* (New York and London, 1961), 336–66. In the 1930s, E. W. Brown showed that Hill's results could be got by a much simpler route; see Brouwer and Clemence, *op. cit.*, 370–3, "Brown's method of differential correction".
28. See, for instance, Henry B. Fine, *A college algebra* (Boston, 1901), chap. 31, "Determinants and elimination".
29. Another possible source is C. Briot and T. Bouquet, *Théorie des fonctions doublement périodiques* (Paris, 1859).
30. See "The President's Address", *Monthly notices of the Royal Astronomical Society*, lxvii (1907), 310. *Cf.* Henri Poincaré, *Les methods de la mécanique céleste* (repr. New York, 1957), i, chap. 4.
31. See J. K. Fotheringham, "The longitude of the Moon from 1627 to 1918", *Monthly notices of the Royal Astronomical Society*, lxxx (1920), 289–307, and Harold Jeffreys, "Chief cause of the lunar secular acceleration", *ibid*., 309–17.
32. E. W. Brown, "The evidence for changes in the rate of rotation of the Earth …", *Transactions of the Astronomical Observatory of Yale University*, no. 3 (1926), 205–35 + 3 plates.
33. H. Spencer Jones, "The rotation of the Earth, and the secular accelerations of the Sun, Moon and planets", *Monthly notices of the Royal Astronomical Society*, xcix (1939), 541–58.
34. See Kurt Lambeck, *The Earth's variable rotation: Geophysical causes and consequences* (Cambridge, 1980).

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# KEPLER AND BRUNO ON THE INFINITY OF THE UNIVERSE AND OF SOLAR SYSTEMS

MIGUEL A. GRANADA, University of Barcelona

## *Introduction*

When Kepler published his *De stella nova in pede Serpentarii* in 1606, four models of the universe existed. The first two are familiar:

(1) The traditional geocentric worldview of Aristotelian and Ptolemaic origin, with a finite sphere of fixed stars in motion enclosing the visible universe.[1] To this we could add the geoheliocentric conception of those authors who believed in a finite universe and conceived of the sphere of fixed stars as finite, e.g. Tycho Brahe and Helisaeus Roeslin.

(2) The new (or, according to Kepler, renewed) heliocentric worldview, as formulated by Copernicus, also with a finite sphere of fixed stars enclosing the universe, notwithstanding the fact that this sphere begins at an immense distance from the sphere of Saturn and is totally unmoved. Indeed, after considering the possibility of this sphere of fixed stars being "infinite outwards" (in chap. I, 8 of his *De revolutionibus*), Copernicus had passed the problem of the infinite or finite extension of this sphere to the natural philosophers, accepting in his work a finite universe or a finite sphere of fixed stars. This was also the position of the other two German Copernicans in the final years of the sixteenth century, Michael Maestlin and Johannes Kepler.

However, these two basic conceptions become four, if we take it that the sphere of fixed stars is actually infinite (or at least indefinitely open) outwards:

(3) The geocentric authors, who attributed the daily motion to the Earth, e.g. Raymarus Ursus with his individual conception of the geoheliocentric world-system, published in 1588 in his *Fundamentum astronomicum*. This could also be the case with William Gilbert's *De magnete* (London, 1600), if we accept that in this work Gilbert did not yet adhere to the annual motion of the Earth. In a limited and loose sense, it could also be the case with Francesco Patrizi, who in his *Nova de universis philosophia* (Ferrara, 1591) conceived of the sphere of fixed stars (though endowed with motion) as vastly extended outwards, although this extension was finite and limited from 'above' by the superior region of the Empyrean. Similar to Patrizi's conception was that of the Czech physician Johannes Jessenius in his *Zoroaster* (Wittenberg, 1591). Interestingly for us, Jessenius was personally known to Kepler, since he had mediated in the settling of differences between Kepler and Tycho concerning the terms of their professional collaboration in Prague during the final years of Tycho's life.[2]

(4) This solution was also (and more clearly) available for Copernicans, who needed only to take seriously the possibility touched on (but abandoned) by Copernicus of

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an indefinitely extended sphere of fixed stars. As is known, this possibility was all the more credible, since Copernicus had put this sphere at rest by attributing without hesitation all the apparent motions of the stars to the Earth. This step was taken by the Englishman Thomas Digges in his *A perfit description of the celestiall orbes* (London, 1576), although he identified this infinite stellar region with the divine Empyreum or theological heavens.[3] Most probably, Kepler did not know this work, which was written in English. Nevertheless, this conception was known to him through the formulation by William Gilbert in his *De magnete*, if we accept that Gilbert had already adopted the annual motion of the Earth.

Common to all these representations was, however, the principle that there was only one planetary system, namely our system, centred on the Sun or on the Earth. Thus, the Sun was very different from the fixed stars, which lacked planets and also had a different ontological status: much brighter and greater than the Sun for Digges,[4] or inversely for Kepler. In any case, all these cosmic representations had one point in common: a profound heterogeneity between the unique planetary region and the region (finite, indefinite or actually infinite) of fixed stars.

To these four visions of the cosmos we can add a fifth, proposed by the Italian philosopher Giordano Bruno (1548–1600) as an original expansion of the heliocentric planetary system to an infinite and homogeneous universe. After an initial presentation in the Italian dialogues published in London in 1584, Bruno offered a complete exposition of his cosmological and metaphysical views in several Latin works printed in Germany, the most important of them being the following: *Acrotismus camoeracensis* and *Articuli adversus mathematicos*, both printed in 1588, in Wittenberg and Prague respectively; and *De triplici minimo et mensura* and *De immenso et innumerabilibus*, both issued in Frankfurt in 1591. All these works presented an infinite and homogenous universe, where infinite planetary systems (called by Bruno “synodi ex mundis”) coexisted, each of them separated from the adjacent ones by a vast extension of space filled with pure air or ether. Every planetary system had a central sun or star around which a number, more or less great, of planets (called by Bruno “earths” or “waters”) and of comets (intended as a variety of planets) moved, each with its own intelligent soul, as a principle of motion. In this universe, conceived as the necessary production and expression of God’s infinite power and goodness, the centre was everywhere and the circumference or periphery nowhere. Thus, not only were our Earth and man totally deprived of any prerogative or special finality; our planetary system with the “central” Sun was entirely indifferent with respect to the infinite number of planetary systems or “synodi ex mundis”.[5] To sum up, in Bruno’s infinite and everlasting universe there was no place for a providence of God supernaturally directed to human redemption. Mankind was reinstated in nature and eschatology refuted. The only way to God was by means of infinite nature and its knowledge through philosophy or science.

## 1. *Kepler, Bruno, and Wackher von Wackenfels*

When Johannes Kepler (1571–1630) published in 1596 his *Mysterium cosmographicum*, he knew neither Bruno's cosmology nor his metaphysics and theology. Kepler's cosmological and religious intention, although Copernican too, was notwithstanding completely different: Kepler's universe, although created by infinite God, was rigorously finite, with the unique Sun in its absolute centre and the immobile stars in the spherical periphery. The six planets (Earth and Moon formed a unity, as is known) moved around the Sun at distances proportional to their respective periods and thus they formed the unique planetary system, whereas the stellar periphery was at an enormous distance from the largest planetary revolution (that of Saturn), as required by Copernicus in order to explain the failure to detect annual parallax in the stars. Moreover, Kepler highlighted "the splendid harmony of those things which are at rest, the Sun, the fixed stars [the two immobile points of reference as centre and periphery] and the intermediate space [through which the planets moved] with God the Father, and the Son, and the Holy Spirit". Thus, the universe, although finite, had a "resemblance" (*similitudo*) to the divine Trinity,[6] whereas for Bruno, whose theological conception was rigorously monistic or unitarian, the infinite and eternal universe was simply the Son or "unigenita natura".[7]

Nevertheless, there was some similarity between the projects of Bruno and Kepler. Both conceived of Copernicanism as a restoration of "ancient wisdom", mainly Pythagoreanism, whose authentic cosmological doctrines (e. g., the counter-earth as the Moon and the central fire as the Sun) had been misunderstood and misrepresented by Aristotle. Both intended to develop Copernicanism by recovering still undisclosed Pythagorean truths: thus, for Kepler, the application of the five regular solids to the number of the planets and their distances to the Sun; for Bruno, the conception of comets as permanent heavenly bodies or his speculations on the motion of the Sun in the centre of its system.[8] One decisive difference, however, in their respective approaches to this question was that Kepler intended to interpret ancient Pythagorean cosmology in the light of his own Copernican cosmology, whereas Bruno intended to read Copernican cosmology in the light of his own understanding of ancient Pythagoreanism.

If Kepler did not know of Bruno's cosmological ideas and their theological import before and immediately after the publication of the *Mysterium cosmographicum*, he soon learned of them in the first years of the seventeenth century. Kepler's source was not in fact William Gilbert's *De magnete* (printed in 1600), although he later associated in his *De stella nova* of 1606 Gilbert's views on the indefinite extension of the sphere of fixed stars — more akin to Raymarus Ursus's views as expressed in the *Fundamentum astronomicum* from 1588 — with those of Bruno. Rather, Kepler was indebted to his correspondent Edmund Bruce (dates unknown) and to his friend, the Imperial Counsellor Johann Matthäus Wackher von Wackenfels (1550–1619), who was also living in Prague.

In the letters he wrote to Kepler from Italy in 1602–3, Edmund Bruce did not mention Bruno. However, scholars agree that Bruce was adopting Bruno's fundamental tenets, when he presented to Kepler his own cosmology in the letter of 5 November 1603:

> I believe that there are infinite worlds, each of them, however, being finite, just as [that] whose middle point of the planets is the centre of the Sun. As with the Earth, the Sun does not rest, since it revolves most swiftly in its place around its own axis; the other planets, among whose number I reckon the Earth, follow this movement, each of them being slower proportionally to its increasing distance from the Sun. The stars are also moving like the Sun, but they are not carried around it by its force, because each of them is a Sun in a world that is not smaller than our own world of the planets. I do not think that the elementary world is exclusive to us and the only one existing, because air is also between those bodies which we call stars, and consequently fire, water and earth as well.... Planets receive their light from the Sun.[9]

This letter is interesting also for Bruce's closing request to Kepler to transmit it "to your Neighbour and my friend, whose answer I expect".[10] Bruce possibly has in mind Wackher von Wackenfels, whom he could have met in Italy some years before on the occasion of Wackher's travelling to the country in 1598. Thus, in the first years of the seventeenth century, when he began building his physical astronomy, later published in the *Astronomia nova* of 1609, Kepler could have been exposed to the joint Brunian pressure of Bruce and Wackher.

It was, however, the Imperial Counsellor Wackher von Wackenfels, also living in Prague and in close relation with Kepler, who proves to be the most interesting person for us in tracing Kepler's knowledge and reaction to the cosmology of Giordano Bruno. Wackher had travelled to Italy in 1598 in the company of Kaspar Schopp and knew of the particulars of Bruno's death in Rome through the famous letter sent by Schopp to Konrad Rittershausen on the very day (17 February 1600) of Bruno's execution. From Wackher, Kepler knew the details of the heroic death of Bruno, as he would recall to Johann Georg Brengger in 1608: "I learnt from Wackher that Bruno was burnt in Rome; he says that Bruno suffered his torment with steadfastness."[11] Significantly, Kepler added that Bruno, according to Wackher, "affirmed the vanity of all religions, and transformed God into the world, into circles, into points",[12] something that Kepler simply abhorred.

Kepler found in the Imperial Counsellor an enthusiastic follower of Bruno's cosmology and therefore a source for his own knowledge of Bruno's conception of an infinite universe. This occurred through face-to-face discussions, as Kepler said from 1606 onwards, and surely also through access to Bruno's works owned by Wackher. We know that Wackher owned two dialogues by Bruno in Italian (the *Spaccio* and *De l'infinito universo e mondi*), which Kepler probably was unable to read, but he also owned the three philosophical Latin poems published in Frankfurt in 1591.[13]

In the *Strena seu De nive sexangula* (published in 1611 and dedicated specifically to Wackher) we are informed that Kepler had access to Wackher's library: "For I saw recently in your house the volumes on unique and uncommon objects."[14] It is then a reasonable inference that Kepler could have consulted Wackher's copies of *De minimo* and of *De immenso et innumerabilibus.*

We can also assume that Kepler had no difficulty in tracing and consulting a copy of Bruno's *Articuli adversus mathematicos*, published in Prague in 1588 and dedicated to Emperor Rudolph II,[15] or of the *Acrotismus camoeracensis*, published in Wittenberg in the same year. Bruno had sent a copy of the latter work to Tycho Brahe with a flattering dedication, and it is not impossible that Kepler could have had access to it.[16] It is noteworthy that Wackher's copy of *De immenso* (now located in Olomouc National Library, Czech Republic; see Figure 1) includes marginal notes and underlinings in two most important chapters: the third chapter of the first book (entitled "Disposition of planetary systems in the universe. Difference between stars shining by themselves and by another. Why planets around other suns are not visible") and the first chapter of the third book ("It is the intention of the Aristotelians and similar philosophers,

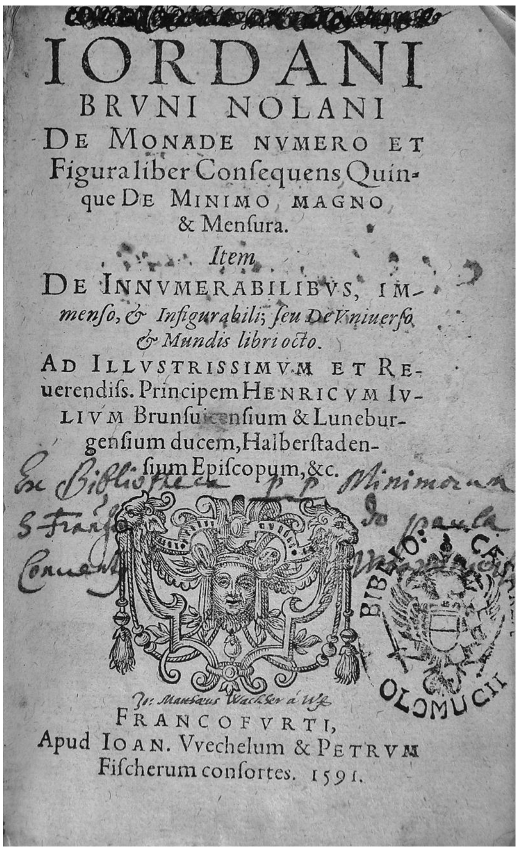


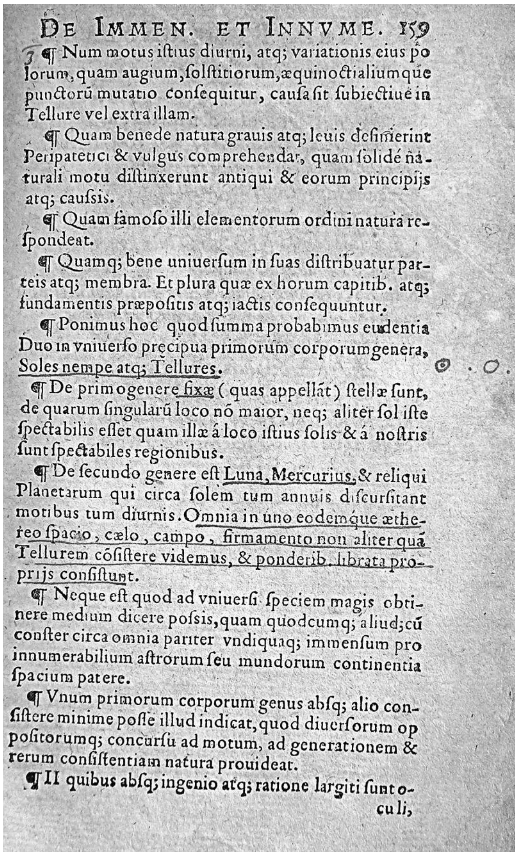

FIG. 1. Wackher's copy of Giordano Bruno's *De immenso* (1591).

as well as that of children, to start from principles; therefore I intend to teach them in the same order as Nature, the best of mothers, has educated us").

According to Rita Sturlese, these marginal notes and underlinings to *De immenso* are from Wackher.[17] I can neither affirm nor deny it; it is certain, however, that they testify to an accurate reading, first, of Bruno's presentation of the salient points of his cosmology in chap. I, 3, and second, of Bruno's derivation of the necessity and infinity of the universe from God's immutability and infinite power in chap. III, 1. Restricting ourselves for the moment to the first of these two chapters, there are three points successively enumerated (1, 2, 3) and selected by Bruno for further consideration: (1) whether the stars are made from the same substance and elements as the Earth ("vide num stellae sint eiusdem substantiae et ex iis conflentur elementis atque tellus"), (2) whether the Moon, Sun and all the stars are placed in the middle of the air or aether, as it is with the Earth ("num sicut Tellus in medio aere vel aethere consistit, ita et luna et sol et omnia astra"), and (3) whether the subject of the diurnal motion and of this other motion connected with the mutation of the poles and the solstitial and equinoctial points is the Earth itself ("num motus istius diurni atque variationis eius polorum, quam augium, solstitiorum, aequinoctialiumque punctorum mutatio consequitur, causa sit subjective in Tellure vel extra illam"; see Figure 2). More interesting for us, the reader underlines Bruno's distinction of the "two genres of first bodies in the universe, namely suns and earths"[18] (or planets) and draws in the margin the images of both (see Figure 2).

Immediately afterwards, the reader underlines the word *fixae*, which designates the first genre, with the additional implication that our Sun, when seen from their various places, would present the same magnitude and appearance as they present when viewed from our Sun;[19] he also underlines that the planets surrounding our Sun with their proper motions constitute the second genre of bodies in the universe; and he underlines Bruno's statement that all of them are placed "in one and the same space" ("omnia in uno eodemque aethereo spacio"). It should be noted that in these lines Bruno does not explicitly affirm that the other suns or fixed stars are also surrounded by planets, but this point had been previously established in the series of verses preceding the comment in prose, where we read: "Just as around this Sun, the Earth, the Moon, Mercury, Saturn, Venus and Mars, as well as Jupiter, wander, ... so the same occurs around any other such body, because it is necessary by law of nature that the flames [the stars] take nourishment from the waters [the planets]". In this line, shortly after the last passage underlined, Bruno's text affirmed that "one genre of first bodies cannot subsist without the other" ("unum primorum corporum genus absque alio consistere minime posse"). Also without annotation was Bruno's immediately preceding statement that no body can be said to attain the centre in the universe with more reason than any other body, because around any star infinite space extends in all directions, equally able to contain an infinity of heavenly bodies.

Thus, on this page Wackher (and possibly Kepler) could find the fundamentals of Bruno's infinite and homogenous universe with an infinite plurality of solar systems, given the absolute homogeneity between the Sun and stars and the absolute


DE IMMEN. ET INNVME. 159

3 ¶ Num motus istius diurni, atq; variationis eius polorum, quam augium, solstitiorum, æquinoctialiumque punctorũ mutatio consequitur, causa sit subiectiue in Tellure vel extra illam.

¶ Quam bene de natura grauis atq; leuis definierint Peripatetici & vulgus comprehendat, quam solidé naturali motu distinxerunt antiqui & eorum principijs atq; caussis.

¶ Quam famoso illi elementorum ordini natura respondeat.

¶ Quamq; bene uniuersum in suas distribuatur parteis atq; membra. Et plura quæ ex horum capitib. atq; fundamentis præpositis atq; iactis consequuntur.

¶ Ponimus hoc quod summa probabimus euidentia Duo in vniuerso precipua primorum corporum genera, Soles nempe atq; Tellures.

¶ De primo genere fixæ (quas appellãt) stellæ sunt, de quarum singularũ loco nõ maior, neq; aliter sol iste spectabilis esset quam illæ á loco istius solis & á nostris sunt spectabiles regionibus.

¶ De secundo genere est Luna, Mercurius, & reliqui Planetarum qui circa solem tum annuis discursitant motibus tum diurnis. Omnia in uno eodemque æthereo spacio, cælo, campo, firmamento non aliter quã Tellurem cõsistere videmus, & ponderib. librata proprijs consistunt.

¶ Neque est quod ad vniuersi speciem magis obtinere medium dicere possis, quam quodcumq; aliud; cũ constet circa omnia pariter vndiquaq; immensum pro innumerabilium astrorum seu mundorum continentia spacium patere.

¶ Vnum primorum corporum genus absq; alio consistere minime posse illud indicat, quod diuersorum oppositorumq; concursu ad motum, ad generationem & rerum consistentiam natura prouideat.

¶ II quibus absq; ingenio atq; ratione largiti sunt oculi,

FIG. 2. Bruno's *De immenso*, I, 3, in Wackher's copy.

indifference of space. There is no doubt also that this page contains all the points constantly adduced by Wackher against Kepler in his personal adoption of Bruno's cosmology.

## 2. *Kepler's Attack on Bruno in* De stella nova *(1606)*

Kepler reacts for the first time to Bruno's conceptions in 1606, in his *De stella nova*. He presents and rejects the infinite universe of Bruno in the course of four brief pages forming part of a discussion on the presumed pre-existence of the matter of the nova in the alleged immensely vast sphere of fixed stars.[20] However, Kepler's words suggest that he is repeating the arguments *pro* and *contra* advanced in previous face-to-face discussion between him and several followers of Bruno's ideas.[21] There is no doubt that Kepler means Wackher and his group, but the Latin word expressing the time of the discussion ('olim', 'some time ago') can refer both to a moment previous to the appearance of the star in October 1604 or, more probably, to the months elapsed thereafter. It is important also to note that Kepler indulges (with some distaste) in a discussion of Bruno's ideas on the infinite universe in a second place, and in the course of criticizing the explanation of the nova connected with an infinitely extended and heterogeneous sphere of fixed stars with the unique planetary system (World-systems 3 and 4 outlined above). It is of interest, then, to present Kepler's criticism of Bruno's homogeneous universe within his more general refutation of the pre-existence of the nova in an infinite sphere of fixed stars.

It is also true, however, that Kepler had begun discussing the opinion of the theologian and astronomer David Fabricius (1564–1617), with whom he was at the time in intense epistolary exchange concerning his new elaboration of the theory of Mars.[22] Fabricius accepted the widespread opinion amongst theologians that God had ceased to create after the creation of the world. Consequently, the nova existed necessarily from the beginning of the world. Only the light with which it had become visible was new. Fabricius adduced also, after the nova in Cassiopeia, the similar and more recent cases of the star later known as Mira Ceti, which he himself had discovered in 1596, and the new star in the constellation of Cygnus, which appeared in 1600. All of these objects testified to the existence of non-radiant stars, which were at precise moments lighted by God to signify good and evil to men.[23] Fabricius adduced other similar facts from heavenly bodies (moved or unmoved) with periodical lighting, such as the Moon and comets (interpreted by him as perpetual bodies in heaven).[24]

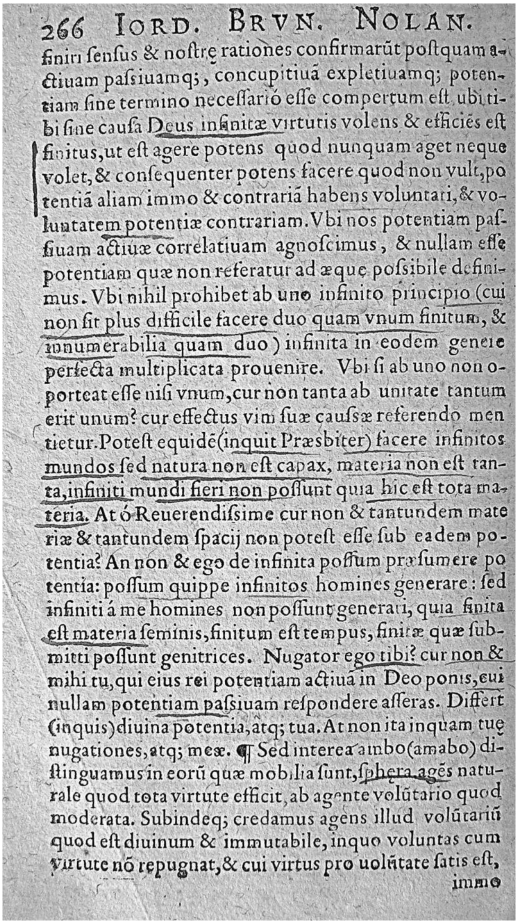
Kepler questioned the denial to God of new creations and objected that he could not see any difference between creating a body and illuminating it.[25] He rejected also the analogy with the Moon and comets, and concluded that the attribution of pre-existence and continuing existence outside illumination by God had no plausibility.[26] On the contrary, Kepler inclined to explain the nova as a complete novelty, both in its body and in its light.[27] And of the two possibilities — a miracle produced by God's absolute power outside the ordinary course of nature or a natural generation "by a certain force of heavenly nature", according to an analogy and homogeneity with the sublunary region which extended generation and corruption to the heavens[28] — Kepler inclined to the second. Thus, chap. XXII excluded the miraculous interpretation as implying the end of rational discussion,[29] and gave priority to a natural explanation, which seemed to Kepler all the more appropriate since this nova was the third or fourth in the last thirty years and since Pliny and Tycho reported that there existed other testimonies in the past to such phenomena. Hence, Kepler presented in the following chapters the first attempt of a resulting programme of natural explanation of the novas according to a complex natural philosophy, taking as his point of departure Tycho's conception of the Milky Way as the rough material for the nova of 1572, but expanding it to heavenly matter everywhere.[30]

Nevertheless, in the middle of this development Kepler introduced, as chap. XXI, a digression whose aim was to criticize also a variant of Fabricius's explanation: the nova, pre-existing both with its body and with its light in the deep vastness of the sphere of fixed stars, had descended, by God's command, to the point at which it was first noticed, and from there it returned progressively to its point of departure, diminishing continuously its magnitude until it disappeared from human view. Kepler did not mention any supporters of this interpretation, which attributed to the star in Serpentarius a circular motion, but he linked it with the similar interpretation of the nova in Cassiopeia as descending and mounting with rectilinear motion in the immense sphere of the fixed stars. Here again, no names are given, but we can suppose that Kepler referred to the examples of Cornelius Gemma, John Dee and Landgrave Wilhelm IV, discussed by Tycho in his *Progymnasmata*.[31] As with Fabricius, this variant too interpreted the phenomenon of the nova as an intervention of God's absolute power with the same result of preventing any rational discussion.[32] Important, however, for our present discussion is the fact that this interpretation of both novas presupposes that the sphere of fixed stars extends outwards in an infinite (or at least indefinite) altitude. Thus, we are confronted with World-systems 3 and 4 (described above), a single planetary system centred on the Earth or on the Sun, with an 'immense' sphere of fixed stars. Kepler had insisted in his exposition on the flaws and arbitrary points of this conception, but he finally concluded that to affirm the infinity of the fixed stars was to enter into an inextricable labyrinth.[33] Accordingly, in order to provide for a more accurate explanation of the nova, it was necessary to deprive them of that 'immensity', a point that had been taken from the ancient philosophical schools with the argument or excuse that, once Copernicus had rendered motionless the region of the stars, this region could be infinite.[34] Surprisingly, however, Kepler proceeded to refute the infinity of the sphere of fixed stars by taking into account Bruno's conception of the infinite universe, which was of a very different kind.

Kepler begins by indicating the two main points of Bruno's doctrine of an infinite universe: (1) its *religious* dimension, in so far as an infinite universe is the necessary production or expression of the infinite power of God, as Kepler could have found in *De immenso*, chap. III, 1, whose pages are heavily underlined in Wackher's copy (see Figure 3);[35] (2) the characteristic Brunian concept of the infinite universe, extremely different from that of Gilbert, Ursus or Digges, inasmuch as Bruno affirms not only the immensity or actual infinity of the stellar region as opposed to the finite central region of the unique planetary system, but also the infinity of an homogeneous universe in which there is no centre and no periphery (or the centre and periphery are everywhere) and every fixed star is a sun like our Sun and consequently is circumscribed by a number of planets. Kepler presents Bruno's conception in the following terms:

> Bruno made the world so infinite that [he posits] as many worlds as there are fixed stars. And he made this our region of the movable [planets] one of the innumerable worlds scarcely distinct from the others which surround it; so that to somebody on the Dog Star (as, for instance, one of the Cynocephals of Lucian) the world would appear from there just as the fixed stars appear to us from our world. [36]

Kepler expounds Bruno's conception acccurately, as he could find it in *De immenso*,



266 IORD. BRVN. NOLAN.
finiri ſenſus & noſtrę rationes confirmarũt poſtquam a-
ctiuam paſsiuamq;, concupitiuã expletiuamq; poten-
tiam ſine termino neceſſarió eſſe compertum eſt ubi ti-
bi ſine cauſa Deus infinitæ virtutis volens & efficiẽs eſt
finitus, ut eſt agere potens quod nunquam aget neque
volet, & conſequenter potens facere quod non vult, po
tentiã aliam immo & contrariã habens voluntati, & vo-
luntatem potentiæ contrariam. Vbi nos potentiam paſ-
ſiuam actiuæ correlatiuam agnoſcimus, & nullam eſſe
potentiam quæ non referatur ad æquę poſsibile defini-
mus. Vbi nihil prohibet ab uno infinito principio (cui
non ſit plus difficile facere duo quam vnum finitum, &
innumerabilia quam duo) infinita in eodem genere
perfecta multiplicata prouenire. Vbi ſi ab uno non o-
porteat eſſe niſi vnum, cur non tanta ab unitate tantum
erit unum? cur effectus vim ſuæ cauſsæ referendo men
tietur. Poteſt equidẽ (inquit Præsbiter) facere infinitos
mundos ſed natura non eſt capax, materia non eſt tan-
ta, infiniti mundi fieri non poſſunt quia hic eſt tota ma-
teria. At ó Reuerendiſsime cur non & tantundem mate
riæ & tantundem ſpacij non poteſt eſſe ſub eadem po-
tentia? An non & ego de infinita poſſum præſumere po
tentia: poſſum quippe infinitos homines generare: ſed
infiniti á me homines non poſſunt generari, quia finita
eſt materia ſeminis, finitum eſt tempus, finitæ quæ ſub-
mitti poſſunt genitrices. Nugator ego tibi? cur non &
mihi tu, qui eius rei potentiam actiuã in Deo ponis, cui
nullam potentiam paſsiuam reſpondere aſſeras. Differt
(inquis) diuina potentia, atq; tua. At non ita inquam tuę
nugationes, atq; meæ. ¶ Sed interea ambo (amabo) di-
ſtinguamus in eorũ quæ mobilia ſunt, ſphęra agẽs natu-
rale quod tota virtute efficit, ab agente volũtario quod
moderata. Subindeq; credamus agens illud volũtariũ
quod eſt diuinum & immutabile, inquo voluntas cum
virtute nõ repugnat, & cui virtus pro uolũtate ſatis eſt,
immo

FIG. 3. Bruno, *De immenso*, III, 1; Wackher's copy.

chap. I, 3 (again heavily underlined in Wackher's copy). Kepler could, however, have found this obsessive idea of Bruno in many other places of *De immenso*, e.g. in chap. IV, 3 ("On the ascent to the heavens and the true contemplation of the world, where the image of the Earth is seen from the orb of the Moon"). Here Bruno had given expression to several points echoed by Kepler: the imaginary ascent to the heavens, the analogy with Lucian of Samosata,[37] the Dog Star as a star of the first magnitude or one of the "suns nearer to us, whose earths also are necessarily less distant from the earths of this system",[38] "so that if we were in one of these stars of first magnitude, this sun of ours would appear equally as a star of the first magnitude".[39] The elimination of absolute places, whose "very cogitation carries with it I don't know what secret, hidden horror; indeed one finds oneself wandering in this immensity, to which are denied limits and centre and therefore also determinate places",[40] was also present in *De immenso*, chaps. I, 3[41] and III, 1.[42] Even the somewhat surprising reference by Kepler to Moses's "waters" (Genesis 1: 2 and 6–7) can be connected with the usual denomination by Bruno of the planets as "waters" (*undae*, *aquae*, *lymphae*).[43] Kepler's reference to Moses is intended as an argument of authority in favour of the *finite* number of "waters", something rather strange given its absence in the Mosaic account of creation and sounding as an expression of the finitist prejudice in Kepler. Again, Kepler's confessed "horror" before the infinite and the absence of fixed determinations of place in it is the opposite to Bruno's sense of freedom and liberation from the prison of the finite universe with which *De immenso* opens.[44] Kepler's language echoes that of Bruno when he says that he pretends to reduce the *madness* (*insania*) of those philosophers who

> misuse the authority of Copernicus as well as that of astronomy in general, which proves — particularly that of Copernicus — that the fixed stars are at an incredible altitude. Well, let us seek the remedy in Astronomy herself, so that by her arts and soothing blandishments this madness of the philosophers (a madness that was provoked by her indulgence, once the bolts were broken and confined spaces abandoned [*ruptis locis et repagulis*] and carried itself out into this immensity), might be led to come back within the bounds of the world and its prisons [*intra Mundi metas, atque carceres suos*]. Surely, it is not good to wander through that infinity [*vaganti per illud infinitum bene non est*].[45]

When Kepler limits himself to refuting Bruno's madness in astronomy, he reduces the controversy to a matter of observation, namely to an undue and incorrect extrapolation by Bruno and his followers from sound astronomical evidence. As we know, for Kepler observation shows that the stars are not distributed uniformly (as Bruno and his followers affirm, according to the logic of homogeneity), but it indicates the existence of a vast cavity in whose centre the Sun is enclosed by an outer wall of stars separated by shorter distances, so that the view of the universe from one of them would be very different from our perception from this single central hollow.[46]

On the contrary, Kepler does not confront Bruno's thesis of more than six planets in our Copernican system and of every star being a sun with planets around it.[47] The reason for this is, certainly, that there is no empirical evidence for the existence of such planets around the Sun or around the stars. There is, however, another reason, in this case metaphysical: Bruno's conception presupposes the essential identity between Sun and stars and goes against Kepler's principle of the singularity of the six planets around the unique Sun. In support of this supposition, Kepler need only summon the archetypical function of the five regular solids as determining necessarily the number of planets and their distances to the Sun, as well as the metaphysical and theological dimension of the *finite spherical* form of the universe with its determined places — centre, circumference and internal space. All this had been fully presented in the *Mysterium cosmographicum* in 1596, but re-emerged in dramatic form in

Kepler's discussion with Wackher after the arrival at Prague of the announcements of Galileo's telescopic observations.

### 3. *The Refutation of the Plurality of Solar Systems in the* Dissertatio *and the* Epitome

Kepler's *Conversation with Galileo's Sidereal Messenger* (*Dissertatio cum Nuncio sidereo*, Prague, 1610) was written in response to Galileo's *Sidereus nuncius* of the same year.[48] However, while he defended publicly the truth and validity of Galileo's discoveries, Kepler also gave public expression to his discussion with Wackher (and Bruno) on the plurality of solar systems. Moreover, the *Conversation* indicates in some places that Kepler had been debating with Wackher on Bruno's cosmological ideas in the years between 1606 and 1610. Thus, in his examination of the discoveries involving the Moon, Kepler relates that he was "deeply engaged [with Wackher] in these discussions last summer [1609]"[49] regarding the lunar relief, adding that he adopted Wackher's and Bruno's opinion that "the bright areas [in the Moon] were seas" and that on this occasion he had written the first version of his *Somnium*.[50] Thus, the first oral notices in Prague of the discovery by Galileo of four new planets, transmitted by Wackher to Kepler as meaning that "these new planets undoubtedly circulate around some of the fixed stars",[51] seemed to imply that "a small difference of opinion of long standing between us had unexpectedly been settled"[52] by the teaching of Bruno, for

> if four planets have hitherto been concealed up there, what stops us from believing that countless others will be hereafter discovered in the same region [of the fixed stars], now that this start has been made? Therefore, either this world is itself infinite, ... or ... there is an infinite number of other worlds (or earths, as Bruno puts it) similar to ours. [53]

The difference established by the telescope between the planets (strongly magnified in size) and the stars (perceived as tiny points of light, deprived by the telescope of the luminous halo, the product of ocular vision) seemed also to speak in favour of Bruno's ideas: "What other conclusion shall we draw from this difference, Galileo", Kepler said, "than that the fixed stars generate their light from within, whereas the planets, being opaque, are illuminated from without; that is, to use Bruno's terms, the former are suns, the latter, moons or earths?".[54]

All seemed, then, to accord with the intended meaning of Bruno, with Kepler forced to accept "chains and a prison amid Bruno's innumerabilities, I should rather say, exile to his infinite space".[55] Was Bruno's thesis of the plurality of solar systems and of the identity between the Sun and stars indeed confirmed? Interestingly for us, Kepler now describes Bruno's thesis as follows:

> They [Bruno and his followers] supposed it was the fixed stars that are thus accompanied [by planets]. Bruno even expounded the reason why this must be so. The fixed stars, forsooth, are of the nature of Sun and fire, but the planets

> of water. By an indefeasible law of nature these opposites combine. The Sun cannot be deprived of the planets; the fire, of its water; nor in turn the water, of the fire.[56]

It seems clear that Kepler is here referring to chap. I, 3, of *De immenso* underlined in the copy of Wackher, where Bruno had established the interdependency of suns (fires) and planets (waters) as a universal and necessary law of nature.[57]

Nevertheless, this threat vanished, since the planets discovered by Galileo do not orbit a star, but simply Jupiter. "I rejoice", Kepler says, "that I am to some extent restored to life by your work. ... by reporting that these four planets revolve, not around one of the fixed stars, but around the planet Jupiter, you have for the present freed me from the great fear that gripped me as soon as I had heard about your book from my opponent's triumphal shout".[58] Moreover, these new planets (or "satellites", as Kepler will name them a few months later)[59] around Jupiter can be integrated perfectly into Kepler's harmonic planetary system. In fact, they complete the singular Moon–Earth relation with a proportion (either arithmetical or geometrical, it is soon thereafter to be affirmed) that surely extends to the other planets as well.[60] In addition, the presence of satellites enables us to account for some discrepancies between calculated planetary distances and the regular solids.[61]

As a consequence, Kepler does not accept Bruno's conception of the stars as suns with planets revolving around them. But he expresses himself in moderate language, for he concedes that the position is that Bruno's conception has not been confirmed and still remains a mere hypothesis, in need of possible confirmation by more accurate observations in the future:

> In the first place, suppose that each and every fixed star is a sun. No moons have yet been seen revolving around them [the stars]. *Hence this will remain an open question until this phenomenon too is detected by someone equipped for marvellous refined observations. At any rate, this is what your success threatens us with, in the judgement of certain persons*.[62]

As Edward Rosen correctly noted, the sentences here in italics were absent in the letter of 19 April sent to Galileo.[63] Kepler inserted them at the instigation of "certain persons", most probably the Brunian Wackher,[64] who no doubt considered that the confirmation of Bruno's theory was only a matter of time. On the contrary, as we shall demonstrate, Kepler considered it otherwise: he was not really disposed to concede that "each and every star is a sun" — this being only a momentary rhetorical concession with no import, given that the consequence, namely planets revolving around every star, had not been confirmed[65] — and, most important, he was convinced that the infinity (or at least the plurality) of planetary systems was metaphysically implausible, if not absurd.

For one thing, the enormous increase in the number of fixed stars discovered by the telescope was interpreted by Kepler as a confirmation of his observational argument in *De stella nova* against the homogeneous and indifferent distribution of stars in Bruno's infinite universe:

> Let him [Bruno] not lead us on to his belief in infinite worlds, as numerous as the fixed stars and all similar to our own. Your third observation comes to our support: the countless host of fixed stars exceeds what was known in Antiquity. You do not hesitate to declare that there are visible over 10,000 stars. The more there are, and the more crowded they are, the stronger becomes my argument against the infinity of the universe, as set forth in my book on the "New Star", chap. XXI, page 104 [*KGW*, i, 204]. This argument proves that where we mortals dwell, in the company of the Sun and the planets, is the primary bosom of the universe; from none of the fixed stars can such a view of the universe be obtained as is possible from our Earth or even from the Sun. For the sake of brevity, I forbear to summarize the passage. Whoever reads it in its entirety will be inclined to assent.[66]

Kepler adds a further argument against the homogeneous infinity of Bruno: if the stars were like our Sun in size and brightness, then the sky would necessarily be as brilliant in the night as in the day: "If this is true, and if they are suns having the same nature as our Sun, why do not these suns collectively outdistance our Sun in brilliance? Why do they all together transmit so dim a light to the most accessible places? ... Will my opponent tell me that the stars are very far away from us? This does not help his cause at all. For the greater their distance, the more does every single one of them outstrip the Sun in diameter."[67] Thus, for Kepler, the only solution is to acknowledge the difference between the unique Sun and the rest of stars: "Hence it is quite clear that the body of our Sun is brighter beyond measure than all the fixed stars together and therefore this world of ours does not belong to an undifferentiated swarm of countless others."[68] From this enormous difference in size and brightness, added to the immense distance between the Sun and the very close stars, Kepler feels obliged to conclude that there is space only for one planetary system and that only this cosmic structure has a metaphysical foundation. He refers to this when he says, "I shall have more to say about this subject later on".[69]

The moment for this arrives towards the conclusion of the *Dissertatio*, when Kepler adduces a third reason "to which he [Wackher] seemed by his silence to assent".[70] This appears to imply that this third reason would also oblige Bruno to assent. This reason or 'consideration' obtains its force from the archetypical foundations of the universe, that is, from the geometrical principles "unique and eternal, and shining in the mind of God",[71] even constituting God's essence,[72] after which God created the universe. Now, archetypes for the number and distances of the planets (i.e., for the structure of the planetary system) are the five regular solids, as Kepler had already established in 1596 in his *Mysterium cosmographicum*.[73] As he puts it in the *Dissertatio*: "in geometry the most perfect class of figures, after the sphere, consists of the five Euclidean solids. They constitute the very pattern and model according to which this planetary world of ours was apportioned."[74] Now, if, as Bruno pretends, every star is the centre of a planetary system (or "synodus ex mundis" in Bruno's terms),[75] these new worlds or planetary systems would be either exactly as our own system of planets, that is, constructed after the pattern of the five regular solids, or different, constructed according to a different pattern. Kepler's response is as follows:

> Suppose then that there is an unlimited number of other worlds. They will be unlike ours or like it. You would not say, "like it". For what is the use of an unlimited number of worlds, if every single one of them contains all of perfection within itself? ... If they differ in their distances, then they must differ also in the arrangement, type, and perfection of their solids, from which the distances are derived. Indeed, if you establish universes similar to one another in all respects, you will also produce similar creatures, and as many Galileos, observing new stars in new worlds, as there are worlds. But of what use is this? Briefly, it is better to avoid the march to the infinite permitted by the philosophers.[76]

For Kepler, then, a repetition of the single perfect planetary system (still more, an endless repetition) makes no sense; it is absurd. For the perfect and complete realization of the geometrical archetype, one instance suffices, repetition adding nothing and perhaps even proving degrading in Kepler's eyes. Different and necessarily inferior planetary systems have no more of a chance of existing. Here Kepler limits himself to stating that these worlds would be necessarily "less noble",[77] but it is also evident that he excludes their existence.

Even though if, as Kepler says, Wackher accepted with silence Kepler's refutation, it is difficult to believe that all this could have moved Bruno from his opinions. Most probably he would have dismissed it as another geometer's speculation deprived of physical foundations, more or less the same criticism of the aprioristic position, arrived at by enthusiasm and obstinately maintained, that Kepler had applied in *De stella nova* to the defenders of infinity.[78] Nevertheless, towards the end of the seventeenth century, Christiaan Huygens (1625–97), himself more inclined to a Brunian conception, discarded Kepler's considerations as contrary to sound reasoning and empirical evidence, and based only upon mere aprioristic prejudices:

> But beneath this argument is hidden another reason why Kepler wished to be able to view the Sun as an object ranking above the other stars, as the only one that Nature had furnished with a system of planets, and as the one situated in the middle of the world. In fact he needed this to confirm his cosmographic mystery according to which he would have the distances of the planets from the Sun correspond proportionately to the diameters of the spheres inscribed within and circumscribed about the Euclidean polyhedra. This could be plausible only if in the world there existed just one single system of planets, and consequently the Sun formed a unique species. But the whole of this 'mystery', rightly considered, seems to be no more than a dream born of the philosophy of Pythagoras or of Plato. Indeed the proportions by no means completely conform to reality, as the author himself concedes; and to explain this discrepancy he invents further causes that are completely frivolous. It is again with flimsy arguments that he establishes

> the sphericity of the exterior surface of the world which is said to contain all the stars; and that he shows that the number of these is necessarily finite, arguing from the fact that this is the case for the size of each one of them. His most extravagant conclusion is that the distance from the Sun to the concave surface of the sphere of the fixed stars would be 600,000 Earth diameters.... For our part, we have no hesitation in accepting, along with the principal philosophers of our day, that the nature of the stars and that of the Sun are the same. This leads to a conception of the world altogether more splendid than that corresponding to the more-or-less traditional views we have just outlined. For what prevents us from thinking that each of the stars or suns has planets around it just like our own Sun?[79]

We come to the same conclusion if we consider the other aspect of the archetypes. As is known, the regular polyhedra are included under the straight line, which forms with the curved line the category of 'quantity'. Kepler, who in this follows Nicholas of Cusa, is convinced that God created the world according to 'quantity', the primordial archetype present in the very essence of God and realized in the first day of creation.[80] While the straight, as it has been said, is related to creatures, the curved line relates to God himself: "For in this one respect Nicholas of Cusa and others seem to me divine, that they attached so much importance to the relationship between a straight and a curved line and dared to liken a curve to God, a straight line to his creatures."[81] Since the perfection and goodness of God required that creation was as perfect as possible,[82] the world was created as an image of God's essence, and consequently as a finite sphere which in its three constituents (the Sun in the centre, the fixed stars in the circumference, and the 'aura aetherea' in the intermediary space) represents God's Trinitarian essence, "the image of God the Three in One in a spherical surface; that is, of the Father in the centre, the Son in the surface, and the Spirit in the regularity of the relationship between the point and the circumference".[83] Kepler remained faithful to this conception until the end of his life, as the enlarged repetition in the *Epitome astronomiae Copernicanae* clearly indicates.[84] In this last work, Kepler draws special attention to the facts that prove the analogy: just as the Father, in the Trinitarian process, is the origin and Christ as Son is the way to the Father, the three persons being distinct although partaking in the same essence, in the same manner the centre and periphery or surface are, in the world, distinct; and the (absolute and unique) centre (the Sun), through its flowing, is the origin and source of the surface and of the radius, the surface (the sphere of fixed stars), like the Son with respect to the Father, being the only way to see the centre.[85]

We cannot, therefore, agree with Judith Field, when she considers that the Trinitarian analogy "may, of course, have weighed with him in private, but he never, as far as I know, presented it as an argument for others".[86] It is difficult to concede that the analogy is not presented as an objectively valid argument in the *Epitome*, where, as we have seen, it appears at the beginning (second part of the first book), in the section entitled "De figura coeli" following the refutation of the Brunian infinite. There are grounds to believe that the analogy represents for Kepler a strong *a priori* argument (as the doctrine of the five regular solids) for the finite size of the world, as well as for the singularity of the Sun and for the existence of the sphere of fixed stars not accompanied by any planets. Although Kepler would have unquestionably abandoned his doctrine if observational evidence had obliged him — Uranus was discovered only in 1781[87] and extrasolar planets are a matter for our own day — it is also evident that Kepler, besides refuting the infinity of the universe with the traditional arguments of Aristotelian origin,[88] interprets observational evidence in the frame of this archetypal analogy with the Trinity. We see this in the *Epitome*, when, "on the position, order, and movement of the parts of the world; or, on the system of the world" (Book IV), he appealed to the Trinitarian archetype as his basis for the claim that the Sun, sphere of the fixed stars and intermediary region of the planets have the same quantity of matter:

> Since these three bodies are analogous to the centre, the surface of the sphere and the interval, three symbols of the three persons of the Trinity; it is believable that there is only as much matter in one as there is in either one of the two remaining: in such fashion that a third part of the matter of the whole universe should be packed together into the body of the Sun, although in comparison with the amplitude of the world the body of the Sun is very limited; that likewise a third part of the matter should be spread out thinly throughout the immense expanse of the world; ... and that finally, a third part of the matter should have been rolled out in the form of a spherical surface and thrown around the world on the outside as a wall.[89]

In this manner, the Sun, as the image of the Father, represents one-third of the universe and is equivalent to the totality of the sphere of fixed stars. It is difficult to express more cogently the singularity and pre-eminence of the Sun with respect to the fixed stars. Thus, the central position of the Sun is fully justified, together with the fact that around it alone is a set of planets disposed in number and distances relating to the five polyhedra. This reduces the stars to "mere points",[90] and Kepler can conclude triumphantly against the supporters of the assimilation of the Sun to the stars: "these observations do not prevent the Sun from having a body of greater bulk than the fixed stars. Moreover, the view of the Sun from such a great interval would be brighter than that of whatever fixed stars."[91]

Contrary to the analogy of the Trinity with the (finite) sphere, Kepler neither mentions nor employs another symbol frequently used by Cusa and the Hermetic and Neoplatonic tradition to indicate God: that according to which "God is an infinite sphere, whose centre is everywhere and the circumference nowhere".[92] Cusa had employed it in his *De docta ignorantia* (Book II, chap. 12) to indicate both God and the universe.[93] It seems that Kepler knew only indirectly (from hearsay) of this work of Cusa, and that he did not come to know the symbol of the infinite sphere.[94] This symbol, however, contradicts Kepler's rigorously finitist conception of geometry (for him, all that has a figure, e. g. a sphere, is necessarily limited),[95] and therefore it is highly improbable that he could have made positive use of the symbol.

On the contrary, Bruno had adopted the symbol of the infinite sphere as the best definition of both God, and the universe as God's necessary production, in which all His infinite power is actually realized. Already in his Italian dialogue *De l'infinito universo e mondi* (1584), Bruno had rhetorically asked: "Why do you desire that centre of divinity which can (if one may so express it) extend infinitely to an infinite sphere, why do you desire that it should remain grudgingly sterile rather than extend itself, like a father, fecund, ornate and beautiful?"[96] But one can find the symbol throughout all his Latin works. Thus, in *De immenso*: "This [the universe] is what Xenophanes defined as an infinite sphere, whose centre is everywhere, the circumference nowhere.… Thus, the infinite is nowhere as concerns the circumference, everywhere as concerns the centre."[97]

For Bruno, the "infinite sphere" is not only the sole adequate expression of God's infinite power, but it also manifests God's simplicity and unity, since Bruno rejects the Trinitarian dogma and reduces the persons of the Son and the Spirit to infinite nature itself and the soul of the world in it, respectively. In this manner, infinite (and homogeneous) nature, since it is also the necessary and total outcome of God's power and will, becomes itself divine. The fact that in the infinite the centre is everywhere allows us to recognize God in totality (*maximum*) and in each minimal point in the universe (*minimum*) as well.[98] Contrary to Kepler's careful distinction between God and His (finite) creation, in Bruno God becomes identified with the infinite universe. For our purpose and concerning the relation between the Sun and the stars, the fact that the centre is everywhere makes of every point the centre of force by which God radiates his creative energy towards a periphery. The Sun can be, therefore, in any point, and consequently there are infinite points in the universe from which infinite suns act as centres of planetary (and cometary)[99] motions. Significantly, Bruno had said in his first Italian dialogue (*La cena de le ceneri*, or *The Ash Wednesday Supper*): "the region of the Bear's tail no more deserves to be called the Eighth Sphere than does that of the Earth (on which we live)",[100] or our Sun. Consequently, if every star is a sun, there are according to Bruno as many planetary systems as stars or suns, and viewed from any star our Sun has the same appearance as that same star when viewed from the Earth.

## *Conclusion*

As we have seen with Huygens concerning the question of the planets, refusal to accept Kepler's finite sphere as an archetype for the entire universe implied in the seventeenth century the disposition to adopt a view of the universe more akin to that of Bruno. Thus, for Newton in a draft written after 1684 and entitled "Of the Sun and Fixt Starrs",

> The Universe consists of three sorts of great bodies, Fixed Stars, Planets, & Comets.... The fixt Stars are very great round bodies shining strongly with their own heat & scattered at very great distances from one another throughout the whole heavens. Those wch are nearest to us appear biggest & those wch are

> further of appear less & less till they vanish out of sight & cannot be seen without a Telescope.... Our Sun is one of ye fixt Stars & every star is a Sun in its proper region. For could we be removed as far from ye Sun as we are from ye fixt stars, the Sun by reason of its great distance would appear like one of ye fixt stars. And could we approach as neare to any of ye fixt Stars as we are to ye Sun, that Star by reason of its nearness would appear like our Sun.[101]

For its own part, the General Scholium to the second edition of Newton's *Principia* (1713) accepted the possibility of planets moving around the stars, subject to God's providence.[102] In Bruno, however, who had no notion of universal gravitation, the immanent providence of God had placed planetary systems at great distances from one another, these systems being stable by virtue of their internal equilibrium. On the contrary, for Newton this equilibrium was always unstable and necessitated regular interventions by God's providential rule and supervision.[103]

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REFERENCES

1. We omit the starless spheres (the ninth, tenth, eleventh, …) postulated since Ptolemy to account for the different motions of the stars (precession, trepidation, ...). In this spirit, the Jesuit astronomer Christoph Clavius had adopted a geocentric universe of eleven spheres. See J. L. Lattis, *Between Copernicus and Galileo: Christopher Clavius and the collapse of Ptolemaic cosmology* (Chicago and London, 1994). The complete works of the authors studied are quoted according to the following editions: Johannes Kepler, *Gesammelte Werke*, ed. by M. Caspar *et al.* (Munich, 1937– ), indicated by *KGW* followed by the volume number; and Giordano Bruno, *Opera latine conscripta*, ed. by F. Fiorentino *et al.* (Naples and Florence, 1879–91), indicated by *BOL* followed by the volume number. Other editions of single works are fully quoted in their first appearance. Italics in the quotations are always ours unless otherwise indicated.
2. *Cf.* Max Caspar, *Kepler*, transl. by C. Doris Hellman, 2nd edn (New York, 1993), 105 ff.
3. See A. Koyré, *From the closed world to the infinite universe* (Baltimore, 1957), 38 f.; M. A. Granada, "Thomas Digges, Giordano Bruno e il copernicanesimo in Inghilterra", in *Giordano Bruno 1583–1585: The English experience / L'esperienza inglese*, ed. by M. Ciliberto and N. Mann (Florence, 1997), 125–55, pp. 133–7.
4. Digges's famous diagram of the universe said about the stars that they were "farr excellinge our sonne both in qvantitye and qvalitye". See the reproduction in Koyré, *op. cit.* (ref. 3), 37.
5. For an exposition of the meaning of this term in Bruno, see M. A. Granada, "Synodus ex mundis", *Bruniana & Campanelliana*, xiii (2007), 149–56.

6. *Cf. Mysterium cosmographicum*, *KGW*, i, 9; English translation by A. M. Duncan, *The secret of the universe* (New York, 1981), 63.
7. This expression, which identifies the Word of God (Christ) with the universe itself, appears in the Italian dialogue *De la causa, principio et uno* (1584). See *De la causa, principio et uno,* in G. Bruno, *Oeuvres complètes*, iii (Paris, 1996), 207.
8. *Cf.* M. A. Granada, "Considerazioni sulla disposizione ed il movimento del sole e delle stelle in Giordano Bruno", *Physis*, xxxviii (2001), 257–82. For the cometary theory of Hippocrates of Chios and Aeschylus, two presocratic authors adduced by Bruno (see *De immenso*, *BOL*, i/2, 229 f.) against Aristotle's theory, see now D. Tessicini, *I dintorni dell'infinito. G. B. e l'astronomia del Cinquecento* (Pisa and Rome, 2007), 188 ff., and M. Wilson, "Hippocrates of Chios's theory of comets", *Journal for the history of astronomy*, xxxix (2008), 141–60.
9. *KGW*, xiii, 450 (letter no. 272): "ego opinor mundos esse infinitos; unusquisque tamen mundus est finitus sicut Planetarum in cuius medio est centrum Solis. Et quemadmodum tellus non quiescit sic neque Sol; Volvitur namque velocissime in suo loco circa axem suum; quem motum sequuntur reliqui Planetae: in quorum numero Tellurem existimo; sed est tardior unusquisque quo ab eo distat longior. Stellae etiam sic moventur ut Sol; sed non illius vi sicut Planetae circumaguntur; quoniam unaqueque earum Sol est, in non minori mondo hoc nostro Planitarum. Elimentalem mundum nobis proprium et particularem non puto: nam aer est et inter ipsa corpora: quae stellas vocamus; per consequens et ignis et aqua et terra.… Planetae vero a Sole eorum lumen assumunt." Bruce seems to combine here Bruno's cosmology (Bruce's 'mundus' being the 'synodus ex mundis' of Bruno, who occasionally called it 'mundus' as well) with Kepler's conception of the solar system. Thus, the notion of a solar force moving the planets around the Sun with periods proportioned to their respective distances probably derived from the reading of the *Mysterium cosmographicum* (*cf.* chap. XX, *KGW*, i, 68 ff.; English translation, Duncan, *op. cit.* (ref. 6), 197 ff.).
10. *KGW*, xiii, 450: "et mitte has literas ad tuum Vicinum et meum amicum a quo responsum expecto."
11. *Ibid.*, xvi, 142 (letter no. 488, 5 April 1608): "Brunum Romae crematum ex Wackherio didici, ait constanter supplicium tulisse."
12. *Ibid.*: "Religionum omnium vanitatem asseruit, Deum in Mundum in circulos in puncta convertit."
13. See R. Sturlese, *Bibliografia, censimento e storia delle antiche stampe di Giordano Bruno* (Florence, 1987), p. xxxi and *ad indicem*.
14. "Nuper enim apud te vidi volumina rerum singularium et rararum." We quote from J. Kepler, *The six-cornered snowflake*, transl. by C. Hardie (Oxford,1966), 4–5.
15. Of this work, only four copies have survived, none of them connected with Wackher or the Emperor. *Cf.* Sturlese, *op. cit.* (ref. 13), 99.
16. This copy is preserved in the National Library of Prague (Clementinum). On this, see Sturlese, *op. cit*. (ref. 13), 92 f., and *eadem*, "Su Bruno e Tycho Brahe", *Rinascimento*, 2nd ser., xxv (1985), 309–33. The verdict by Tycho on Bruno's cosmological ideas was strict and summary: "Nullanus nullus et nihil, Conveniunt rebus nomina saepe suis", he wrote at the end of the volume, making fun of Bruno's denomination 'Nolanus', after his birthplace Nola, in the vicinity of Naples.
17. See Sturlese, *op. cit*. (ref. 13), 125.
18. "Ponimus hoc quod summa probabimus evidentia. Duo in universo praecipua primorum corporum genera, Soles nempe atque Tellures", *BOL*, i/1, 212 (original edn, Frankfurt, 1591, 159; see reproduction in Figure 2).
19. *Ibid.*: "De primo genere *fixae* (quas appellant) stellae sunt, de quarum singularum loco non maior neque aliter sol iste spectabilis esset quam illae a loco istius solis et a nostris sunt spectabiles regionibus."
20. *De stella nova*, *KGW*, i, chap. XXI, 253–6. See chaps. XX ("If the matter and the body of the new star existed previously"), 248–51, and XXI ("If this star has been endowed with its motion in the profound ether, and if the sphere of fixed stars extends infinitely"), 251–7.
21. *Ibid.*, 254. 23–25: "Hoc ego thema cum olim quibusdam proposuissem; qui, ut me exercerent,

infinitatis causam, ex ante dictis authoribus susceptam, adversum me propugnabant acriter...."

22. On this see J. Voelkel, *The composition of Kepler's* Astronomia nova (Princeton and Oxford, 2001), 170–210. Fabricius had expounded his theory of the nova's origin and significance in his letter of 14 Jan. 1605 (old style; letter no. 319), *KGW*, xv, 117 f. Kepler answered critically in his letter of 11 Oct. 1605 (no. 430), *KGW*, xv, 257 f. In his letter, Kepler referred also to a tract on the nova in Dutch dialect published by Fabricius, which is not extant. Fabricius published three further treatises on the nova, two in German and one in Latin.
23. "… Deus illas illuminet certis temporibus, ad praesignificanda bona vel mala hominibus", letter by Fabricius to Kepler, 14 Jan. 1605, *KGW*, xv, 117. See also *De stella nova*, *KGW*, i, 248.
24. *De stella nova*, 249 f. Letter to Kepler, 14 Jan. 1605, *KGW*, xv, 118.
25. *De stella nova*, 249: "Itaque lucem producere, creare est."
26. *Ibid*., 251: "Satis opinor patere, causam nullam idoneam esse, cur quis existimet, novas istas stellas prius extitisse, quam viderentur; et postquam extinctae sunt, reservari superstites, ad novam illuminationem."
27. *Ibid*., 248: "non lumen tantum, sed et corpora ipsa in coelo existere repentina et nova."
28. *Ibid*., lines 21–26.
29. *Ibid.*, 257. 23–24: "Priusquam autem ad creationem [by God], hoc est ad finem omnis disputationis, veniamus, tentanda omnia existimo." On this point, Bruno agrees with Kepler. *Cf.* M. A. Granada, "Cálculos cronológicos, novedades cosmológicas y expectativas escatológicas en la Europa del siglo XVI", *Rinascimento*, 2nd ser., xxxvii (1997), 357–435, pp. 423 f.
30. *De stella nova*, 258 f. *Cf.* 259. 25–26: "Itaque potius in eo sum; ut credam, coelum undiquaque aptum ad materiam hisce sideribus praebendam."
31. *Ibid.*, 251 f. For Tycho, see *Astronomiae instauratae progymnasmata*, in Tycho Brahe, *Opera omnia*, ed. by J. L. E. Dreyer (Copenhagen, 1913–29), iii, 78 (for Gemma), 204 (for Dee, Wilhelm IV, Gemma).
32. *De stella nova*, 252. 35–38: "Verum et alias saepe, et nunc iterum abrumpo disputationem, quoties ad absolutam Dei potentiam provocant. Certum enim, nihil nos ad rem dicere posse, quod quicquam in ullam partem habeat momenti, si naturae terminos excesserimus." For Bruno's similar position, see ref. 29 above.
33. *Ibid.*, lines 38–39: "Hoc potius illis dicamus, illâ fixarum infinitate seipsos, ceu labyrinthis inexplicabilibus induere."
34. *Ibid*., 252. 40–253. 3.
35. *Ibid*., 253. 3–8: "Itaque defendit illam [infinitatem] infelix ille Jordanus Brunus: nec obscure asseruit, specie dubitantis, et Gulielmus Gilbertus, libro de Magnete, caetera praeclarissimo, religiosum tamen affectum eo demonstravit, quod existimaret non aliâ re rectius intelligi infinitam Dei potentiam, quam si infinitum mole conderet mundum." It has been assumed that Kepler here attributes to Gilbert the connection between an infinite universe and an infinite God's power; see e.g. Koyré, *op. cit*. (ref. 3), 60–61. Gilbert, however, does not affirm in *De magnete* the infinity of the universe as a corollary of God's infinite power. Moreover, he does not affirm even the infinity of the universe or of the stellar sphere, speaking instead with Copernicus of the immensity of this sphere "cuius finis ignoratur scirique nequit"; see M.-P. Lerner, *Le monde des sphères*, ii: *La fin du cosmos classique* (Paris, 1997; 2nd edn, 2008), 148 (in both editions). For this reason, we believe that Kepler's quoted passage in *De stella nova* can be understood as referring by "religiosum" to Bruno, the sentence speaking explicitly of Gilbert being a kind of parenthesis. It is true that in the *Epitome astronomiae copernicanae* (*KGW*, vii, 81) Kepler will attribute the connection to Gilbert, but in this case he probably reads in the other sense the ambiguous text of 1606. In *De immenso*, chap. III, 1, Bruno rejects the scholastic distinction between God's absolute and ordained power (*potentia absoluta et ordinata*) and affirms that God's production reflects necessarily his infinite power and essence; see 1st edn, p. 266: "Ubi nihil prohibit ab uno infinito principio (cui non sit plus difficile facere duo quam unum finitum, & innumerabilia quam duo) infinita in eodem genere provenire" (underlining in Wackher's copy; *cf.* Figure 3). For an analysis of Bruno's conception, see Miguel A. Granada, "'Blasphemia vero est facere

Deum alium a Deo': La polemica di Bruno con l' aristotelismo a proposito della potenza di Dio", in *Letture bruniane I.II del Lessico Intellettuale Europeo 1996–1997*, ed. by E. Canone (Pisa and Rome, 2002), 151–88.

36. *De stella nova*, 253. 8–14 (English translation by Koyré).
37. *De immenso*, chap. IV, 3, in *BOL*, i/2, 16.
38. *Ibid*., 20: "Propinquiores soles, quorum quoque terras a terris istius synodi minus distare necesse est, sunt astra fixa maiora, quae primae dicuntur et accipiuntur magnitudinis."
39. *Ibid*., 21: "Hinc patet quod si essemus in uno de astris illis primae magnitudinis, sol iste pariter primae magnitudinis astrum videretur." *Cf.* also chap. I, 3, 159 (in 1st edn, reproduced as Figure 2), quoted above, ref. 19.
40. *De stella nova*, 253. 15–17 (translation by Koyré).
41. Quoted above.
42. *De immenso*, chap. III, 1, in *BOL*, i/1, 318: "Omnia quippe argumenta quae sunt … ex differentiis sursum & deorsum, extimi & intimi, centri et circumferentiae … sunt passiones finiti, nec probant sed supponunt finitum, petunt finitum, accipiunt finitum" (underlining in Wackher's copy).
43. *Cf. De immenso*, chap I, 3, in *BOL*, i/1, 209 ("lege necesse est naturae, flammas [suns] fomentum sumere ab undis"; and 213 ("opaca planetarum corpora, in quibus elementum aquae dominatur"); chap. IV, 3, in *BOL*, i/2, 20 ("lymphae"). See also Granada, *op. cit*. (ref. 5).
44. *De immenso*, chap. I, 1, in *BOL*, i/1, 201 f.: "Intrepidus spacium immensum sic findere pennis / Exorior, neque fama facit me impingere in orbes, / Quos falso statuit verus de principio error, / Ut sub conficto reprimamur carcere vere, / Tanquam adamanteis cludatur moenibus totum.... / Aethereum campumque ex omni parte pererro, /Attonitis mirum et distans post terga relinquo" ["Fearless I rise, endowed with feathers, to traverse infinite space, and fame does not cause me to collide with the orbs established from a false principle by a true error, so that we were truly oppressed in a feigned prison and the whole was closed by diamantine walls.... I run everywhere through the ethereal field, leaving at my back in the distance the glance of the astonished"].
45. *De stella nova*, 253. 20–27.
46. *Ibid*., 253–6; Koyré, *op. cit*. (ref. 3), 61–72.
47. *Cf. De immenso*, chap. I, 3, in *BOL*, i/1, 209: " Ut solem hunc circa Tellus, Luna, aliger Hermes, / Saturnus, Venus et Mavors, et Juppiter errant, / Et numerus fasso major, nam caetera turba / Partim pro vicibus, partim non cernitur unquam, / Sic circum fit quemque alium" (English translation given above, Section 1).
48. Galileo's work was printed in March, Kepler's *Dissertatio* in May. On 19 April, Kepler presented to the Florentine ambassador in Prague a letter to Galileo, which was the written answer requested by Galileo. The *Dissertatio* is the printed version of this letter. For the slight divergencies between both versions see *KGW*, iv, 506 f.
49. *Kepler's Conversation with Galileo's Sidereal Messenger*, translation, introduction and notes by E. Rosen (New York and London, 1965), 25 f.
50. *Ibid*., 26. This indicates that Kepler's decision to write the *Somnium* (whose first idea goes back to a failed dissertation in the Tübingen years in defence of the movement of the Earth) occurred in the course of this intense and enduring discussion with Wackher on Bruno's cosmology and metaphysics. For the *Somnium*, see the edition by E. Rosen: *Kepler's* Somnium*: The Dream, or posthumous work on lunar astronomy* (Madison, Milwaukee and London, 1967). The composition of the *Strena* (later dedicated to Wackher as a New Year's gift for 1611) at the beginning of 1610 also indicates a serious discussion with Wackher on philosophical matters regarding the *minimum*, that is, Bruno's conception of minimal particles. In this case, the discussion revolved around Bruno's *De minimo* — present, as I said, in Wackher's library — probably in connexion with Bruno's *Articuli adversus mathematicos*. For an account of Kepler's debt to Bruno in his thoughts on snow particles, see Ch. Lüthy, "Bruno's *Area Democriti* and the origins of atomist imagery", *Bruniana & Campanelliana*, iv (1998), 59–92, especially pp. 79–83. Kepler's debt to Bruno has been recognized also by M.-L. Heuser-Kessler in her essay "Maximum und Minimum: Zu Brunos Grundlegung der Geometrie in den *Articuli adversus mathematicos* und

ihre weiterführende Anwendung in Keplers *Neujahrsgabe oder Von sechseckigen Schnee*”, in *Die Frankfurter Schriften Giordano Brunos und ihre Voraussetzungen*, ed. by K. Heipcke, W. Neuser and E. Wicke (Weinheim, 1991), 181–97. As rightly indicated by Lüthy, Kepler’s mocking words in *Strena* to himself (“My endeavour to give almost Nothing [*Nihil*] almost comes to nothing! From this almost Nothing I have almost formed the all-embracing Universe itself!”, Kepler, *op. cit*. (ref. 14), 39) seem to be addressed actually to Bruno, possibly a pun played on Bruno’s patronymic *Nolanus,* as Tycho had done previously. See ref. 16 above. Wackher’s copy of *De minimo* (preserved in Prague’s Clementinum) bears some traces of reading according to Sturlese; see *Bibliografia, censimento e storia delle antiche stampe di Giordano Bruno* (ref. 13), 113 f. (no. 102). Nevertheless, these traces do not correspond to the chapters pertaining to the problem discussed by Kepler. I am grateful to Petr Hadrava and Alena Hadravova for having verified this point for me.

51. *Kepler’s Conversation with Galileo’s Sidereal Messenger* (ref. 49), 11.
52. *Ibid*., 10.
53. *Ibid*., 11.
54. *Ibid*., 34.
55. *Ibid*., 36 f.
56. *Ibid*., 39. *Cf.* the Latin text: “At putabant fixas stellas esse quae sic circumirentur; causam etiam dixit Brunus cur *esset necesse*: Fixas quippe Solaris et igneae esse Naturae, Planetas aqueae; et *fieri lege Naturae inviolabili, ut diversa ista combinentur*, neque Sol Planetis, ignis aqua sua, neque vicissim haec illo carere possit” (*KGW*, iv, 305. 13–17; italics ours).
57. *Cf. De immenso*, chap. I, 3, in *BOL*, i/1, 209–13, especially 209 f.: “ Ut solem hunc circa Tellus, Luna, aliger Hermes, / Saturnus, Venus et Mavors, et Juppiter errant, / Et numerus fasso major, nam caetera turba / Partim pro vicibus, partim non cernitur unquam, / Sic circum fit quemque alium: *nam lege necesse est / Naturae, flammas fomentum sumere ab undis*.... Sic circum unumquemque Phoebum cytharoedum / Plures discurrunt Nymphae ... / Quas vegeto sensu, ac clara ratione videmus, / Quando unam ad normam venit abstans atque propinquum, / Nec variat numerus primorum principiorum” (italics ours). Later in the *Conversation*, Kepler speaks of the Sun as “truly an Apollo, the term frequently used by Bruno” (45). If in the passage just quoted Apollo is designated as “Phoebum Cytharoedum”, Bruno does speak frequently of Apollo in this sense. See e.g. *De immenso*, chap. IV, 3, in *BOL*, i/2, 20: “hinc circa unum medium plures nymphae seu Musae, inde unus intra plures nymphas Apollo.”
58. *Kepler’s Conversation with Galileo’s Sidereal Messenger* (ref. 49), 36 f.
59. See J. Kepler, *Narratio de observatis a se quatuor Iovis satellitibus erronibus*, *KGW*, iv, 317–25. *Cf.* also the recent edition with French translation by I. Pantin: J. Kepler, *Dissertatio cum Nuncio sidereo: Narratio de observatis Jovis satellitibus* (Paris, 1993).
60. *Kepler’s Conversation with Galileo’s Sidereal Messenger* (ref. 49), 14.
61. *Ibid*., 47.
62. *Ibid*., 39. *Cf.* the Latin text: “Primum esto ut fixa quaelibet Sol sit, *nullae illas [fixas] Lunae hucusque circumcursitare visae sunt: hoc igitur in incerto manebit, quoad aliquis subtilitati observandi mira instructus, et hoc detexerit: quod quidem hic successus tuus, iudicio quorundam nobis minatur*”, *KGW*, iv, 305. 18–22 (italics ours).
63. *Kepler’s Conversation with Galileo’s Sidereal Messenger* (ref. 49), 137, note 340 to this passage. For the variant in the letter sent to Galileo, *cf. KGW*, iv, 506.
64. A few lines below, the “recent gathering of certain philosophers”, where it was observed that the new moons of Jupiter are inhabited (*Conversation*, 39), is said in the letter to Galileo to have been held “at Wackher’s dinner table” (*ibid*.,138, note 345). *Cf. KGW*, iv, 506: “quod nuperrime in mensa nostri Vakherii iucunde motum.”
65. More precisely, Kepler could concede that every fixed star was like the Sun an igneous body, shining by itself with a proper light (see ref. 54 above), but not that it was indifferent to the Sun in size and brightness.
66. *Kepler’s Conversation with Galileo’s Sidereal Messenger* (ref. 49), 34 f.

67. *Ibid.*, 35. As has been repeatedly observed, Kepler here formulates the so-called 'Olbers's Paradox'. On this see E. Harrison, *Darkness at night: A riddle of the universe* (Cambridge, MA, 1987), 49 f. on Kepler.
68. *Kepler's Conversation with Galileo's Sidereal Messenger* (ref. 49), 35 f.
69. *Ibid.*, 36.
70. *Ibid.*, 43.
71. *Ibid.*
72. See *Mysterium cosmographicum*, *KGW*, i, 24. 1–6: "Cum igitur Idaeam mundi Conditor animo praeconceperit ... ut forma futuri operis et ipsa fiat optima: Patet quod ... nullius rei Idaeam pro constituendo mundo suscipere potuerit, quam suae ipsius essentiae" ["Since, then, the Creator conceived the Idea of the universe in his mind ... so that the Form of future creation may itself be the best: it is evident that ... the only thing of which he could adopt the Idea for establishing the universe is his own essence", English translation, *op. cit.* (ref. 6), 93]. *Cf.* the later formulation in the *Harmonice mundi*, *KGW*, vi, 223. 32–34: "Geometria ante rerum ortum Menti divinae coaeterna, Deus ipse (quid enim in Deo, quod non sit Ipse Deus) exempla Deo creandi mundi suppeditavit, et cum imagine Dei transivit in hominem" ["Geometry, which before the origin of things was coeternal with the divine mind and is God himself (for what could there be in God which would not be God himself?), supplied God with patterns for the creation of the world, and passed over to Man along with the image of God", *The Harmony of the World by Johannes Kepler*, transl. by E. J. Aiton, A. M. Duncan and J. V. Field (Philadelphia, 1997), 304].
73. *Mysterium cosmographicum*, *KGW*, i, 13 (Preface to the reader), 25 f. (chap. II) [English translation, 67 f., 97 f.]. See J. V. Field, *Kepler's geometrical cosmology* (London, 1988), chap. 3.
74. *Kepler's Conversation with Galileo's Sidereal Messenger* (ref. 49), 43 f.
75. In this formulation, 'mundus' means 'heavenly body'. This is the most usual meaning of 'mundus' in Bruno. Nevertheless, he often uses also the word in the sense of 'universe' and of 'planetary system'. See Granada, "Synodus ex mundis" (ref. 5), 149–56.
76. *Kepler's Conversation with Galileo's Sidereal Messenger* (ref. 49), 44.
77. *Ibid.*: "Suppose those infinite worlds are unlike ours. Then they will be supplied with something different from the five perfect solids. Hence they will be less noble than our world."
78. *De stella nova*, *KGW*, i, 252. 30–253. 3. For a contemptuous judgement in Bruno of contemporary astronomers, see his *Articuli adversus mathematicos*, dedicated to the Emperor and probably known to Kepler: "Sphaerae ergo mundanae corporum ordo, qualem fingunt et pingunt pauperes isti [mathematici, i.e. astronomers], nusquam est", *BOL*, i/3, 77.
79. C. Huygens, *Cosmotheoros*, in *Oeuvres complètes* (The Hague, 1888–1950), xxi, 810 f. (our translation). Huygens, who nevertheless describes Kepler as "cet Homme si génial, qui fut le grand instaurateur de l'Astronomie" (*ibid.*, 812), conceded to the nearest star to our Sun (Sirius) a distance of 27,664 a.u. (*ibid.*, 816). Huygens accepted for any pair of stars a distance separating them of at least this amount (*ibid.*). Thus, he arrived at a view similar to that of Bruno, except that he considered the infinite number of stars as unsure (*ibid.*). On Huygens, see S. J. Dick, *Plurality of worlds: The origins of the extraterrestrial life debate from Democritus to Kant* (Cambridge, 1982), 127–35, and now J. Seidengart, *Dieu, l'univers et la sphère infinie: Penser l'infinité cosmique à l'aube de la science classique* (Paris, 2006), 554–60. Huygens owned in his library Bruno's Italian dialogue *De l'infinito, universo e mondi* and also the Latin philosophical poems *De monade* and *De minimo*. On this, see Sturlese, *op. cit.* (ref. 13), 57, 122.
80. *KGW*, i, 12–13: "Quantitas enim initio cum corpore creata; coeli altero die" ["For quantity was created in the beginning along with matter, but the heavens on the second day", English translation, 67]. *Cf.* the note to this page in the second edition (1621), *KGW*, viii, 30: "Imo Ideae quantitatum sunt erantque Deo coaeternae, Deus ipse" ["Rather the Ideas of quantities are and were coeternal with God, and God himself", English translation, 73].
81. English translation, 93 ["Hac enim una re divinus mihi Cusanus, aliique videntur: quod Recti, Curvique ad invicem habitudinem tanti fecerunt, et Curvum Deo, Rectum creaturis ausi sint comparare", *KGW*, i, 23].

82. *Ibid*.: "By a most perfect Creator it was absolutely necessary that a most beautiful work should be produced" ["a Conditore perfectissimo necesse omnino fuit, ut pulcherrimum opus constitueretur", *KGW*, i, 23]. Kepler appeals for this to the authority of Plato in the famous passage in *Timaeus* 29d–30a through the translation by Cicero. If theology affirmed traditionally that God had created the world through the Word by appealing to the exemplary ideas in it, Kepler considered that the mathematical archetypes constituting God's very essence were the model. On this, see J.- L. Marion, *Sur la théologie blanche de Descartes: Analogie, création des vérités éternelles et fondement* (Paris, 1981), 178–203. According to Marion, there is in Kepler "une transcription de l'exemplarisme en termes de mathématiques, ou, plus exactement, d'une attribution aux mathématiques du statut, des propriétés et des fonctions jusqu'alors reconnues aux idées divines. Les idées divines régressent au rang des *ideae quantitatum*", *ibid*., 180. Moreover, "Dieu ne pratique pas tant les mathématiques qu'il ne consiste en elles.… Sans doute, Dieu tout entier ne s'épuise-t-il pas dans les vérités mathématiques, mais toutes les vérités mathématiques s'inscrivent en Dieu comme Dieu même…; [les vérités mathématiques] non seulement se soustraient elles-mêmes à la création, mais président à la création de toutes autres choses", *ibid.*, 182 f. For this reason, Marion considers it very probable that Descartes formulated his doctrine of the creation by God of the eternal mathematical truths as a reaction against the necessitarianism implicit in Kepler's conception.
83. *The secret of the universe*, 93 [*Mysterium cosmographicum*, *KGW*, i, 23: "Dei triuni imago in sphaerica superficie, Patris scilicet in centro, Filii in superficie, Spiritus in aequalitate σχεσεως inter punctum et ambitum"]. Kepler found this analogy in the *Complementum theologicum* of Nicholas of Cusa and in *De harmonia mundi* (1525) of the Venetian Platonist Francesco Giorgio Veneto (*c.* 1460–1540). See D. Mahnke, *Unendliche Sphäre und Allmittelpunkt* (Halle, 1937), 141 f. and 106 f.
84. See *Epitome astronomiae Copernicanae*, *KGW*, vii, 47: "*Mundi Archetypus Deus ipse est*, cujus nulla figura similar est ... quam sphaerica superficies. Nam uti Deus est ens Entium, antecedens omnia, ingenitum, simplicissimum, perfectissimum, immobile, sibi ipsi creaturisque omnibus sufficientissimum, creans et sustentans omnia, *unus essentia, in personis trinus*: sic sphaericum etiam easdem rudi quodam modo proprietates habet inter figuras caeteras."
85. *Ibid*., 51: "In Sphaerico tria sunt, Centrum, superficies, et aequalitas intervalli; quorum uno negato caetera corruunt, suntque distincta inter se, ut unum non sit alterum. Centrum est quasi Origo Sphaerici.... Centrum seipso est invisibile et impervestigabile; monstratur vero undique flexu aequabilissimo superficiei, mediante aequabilitate intervalli. Itaque superficies est character et imago centri, et quasi fulgor ab eo, et via ad id; et qui superficiem videt, is eo ipso videt et centrum, non aliter. Intervallum resultat ex comparatione Centri cum superficie, et sic procedit ab utroque." The implicit references to the relations between the persons in the Trinity are obvious: the *intervallum* ('aura etherea' where the six planets are disposed according to the distances determined by the five polyhedra) proceeds, as the Holy Spirit "proceedeth from the Father and the Son" according to the Creed, "from both the Centre and the surface"; for Christ as way to the Father, see John 14: 6–7: "I am the way, the truth, and the life: no man cometh unto the Father, but by me. If ye had known me, ye should have known my Father also." *Cf.* J. Hübner, *Die Theologie Johannes Keplers* (Tübingen, 1975), 191–2; F. Hallyn, *La structure poétique du monde: Copernic, Kepler* (Paris, 1987), 187 ff.
86. Field, *op. cit*. (ref. 73), 18.
87. *Ibid*., 53.
88. *Cf. De stella nova*, chap. XXI, *KGW*, i, 256 f.; *Epitome*, *KGW*, vii, 45 f. See also Seidengart, *Dieu, l'univers et la sphère infinie* (ref. 79), 356 f., 367–73, and A. Segonds, "Kepler et l'infini", in F. Monnoyeur (ed.), *Infini des philosophes, infini des astronomes* (Paris, 1995), 21–40, especially pp. 23, 33.
89. *Epitome of Copernican astronomy & Harmonies of the world*, transl. by C. G. Wallis (Amherst, NY, 1995), 45 [*KGW*, vii, 287 f.: "Cum haec tria corpora sint analoga centro, superficiei sphaericae, et intervallo, tribus Symbolis trium in SS. Trinitate personarum: credibile est tantundem esse materiae in uno, quantum in uno quolibet duorum reliquorum; sic ut tertia pars materiae totius

universi compacta sit in corpus Solis, quamvis id sit respectu amplitudinis mundi angustissimum: Tertia item pars materiae extenuata et explicata per immensum mundi spacium...: Tertia denique pars materiae expansa in orbem, et mundo exterius pro moenibus circumjecta"].

90. *Epitome of Copernican astronomy,* 46; *KGW*, vii, 289.
91. *Ibid*.
92. This formulation constitutes the second definition of God in the medieval treatise attributed to Hermes Trismegistus, *Liber XXIV philosophorum*: "Deus est sphaera infinita cuius centrum est ubique, circumferentia nusquam." *Cf. Liber viginti quattuor philosophorum*, ed. by F. Hudry (Corpus Christianorum, Continuatio Mediaevalis, 143 A; Brepols, 1997), 7–8.
93. *Cf.* the citation by Koyré, *op. cit*. (ref. 3), 17: "Thus, the fabric of the world (*machina mundi*) will *quasi* have its center everywhere and its circumference nowhere, because the circumference and the center are God, who is everywhere and nowhere."
94. See Mahnke, *op. cit*. (ref. 83), 130–3, 141–3, and more recently S. Meier-Oeser, *Die Präsenz des Vergessenen: Zur Rezeption der Philosophie des Nicolaus Cusanus vom 15. bis zum 18. Jahrhundert* (Münster, 1989), 286, 290f.
95. *Cf. De stella nova*, chap. XXI, 256f.: "omnis figura finibus quibusdam est circumscripta, hoc est finita vel finiens"; *Epitome*, *KGW*, vii, 45. 36–40.
96. G. Bruno, *On the infinite universe and worlds*, transl. by D. Waley Singer in *eadem*, *Giordano Bruno: His life and thought, with annotated translation of his work* On the infinite universe and worlds (New York, 1968), 260. *Cf. De l'infinito, universo e mondi*, in G. Bruno, *Oeuvres complètes*, iv (Paris, 2006), 85: "Perché volete che quel centro della divinità, che può infinitamente in una sfera (se cossì si potesse dire) infinita amplificarse, come invidioso, rimaner più tosto sterile che farsi comunicabile, padre fecondo, ornato e bello?" The impossible 'envy' in God towards His creation of the best (infinite, according to Bruno) world alludes to Plato's *Timaeus*, 29e. For Bruno's adoption of the 'principle of plenitude', see the classic study by A. O. Lovejoy, *The Great Chain of Being: A study of the history of an idea* (New York, 1960), 116–21. For Bruno's logic of omnipotence, see M. A. Granada, "Il rifiuto della distinzione fra *potentia absoluta* e *potentia ordinata* di Dio e l'affermazione dell'universo infinito in Giordano Bruno", *Rivista di storia della filosofia*, xlix (1994), 495–532.
97. *De immenso*, chap. II, 9, in *BOL*, i/1, 291: "Hoc [the universe] est quod sphaeram definivit Xenophanes infinitam, cuius centrum est ubique, circumferentia nusquam.… Sic infinitum nusquam est circumferentialiter, ubique est centraliter." *Cf. Articuli adversus mathematicos*, *BOL*, i/3, 72: "For us [Bruno says against contemporary mathematicians or astronomers] the universal sphere is one continuous universe, infinite and immovable, where there are infinite spheres or particular worlds [Nobis sphaera universalis est unum continuum universum infinitum immobile, seu in quo consistentia sunt numero infinitae sphaerae seu particulares mundi]". *Cf.* Mahnke, *op. cit.* (ref. 83), 49–59, and 53 note 1 for a list of references.
98. This is probably the reason for Kepler's statement, in a letter to Brengger, that Bruno "transformed God in the world, in circles, in points". See ref. 12 above, and the recent study by A. Del Prete: "'Une sphère infinie dont le centre est partout et la circonférence nulle part': L'omnicentrisme chez Giordano Bruno", in F. Tinguely (ed.), *La Renaissance décentrée* (Geneva, 2008), 33–47.
99. See M. A. Granada, "Aristotle, Copernicus, Bruno: Centrality, the principle of movement and the extension of the universe", *Studies in history and philosophy of science*, xxxv (2004), 91–114, and Tessicini, *op. cit*. (ref. 8).
100. G. Bruno, *The Ash Wednesday Supper*, transl. and ed. by E. A. Gosselin and L. S. Lerner (Hamden, CT, 1977), 203 [*La cena de le ceneri*, in *Oeuvres completes*, ii (Paris, 1994), 233: "Non è più degno d'esser chiamato ottava sfera dove à la coda de l'Orsa, che dove è la terra, nella quale siamo noi"].
101. I. Newton, MS. Add. 4005, fols. 21–22, in *Unpublished scientific papers of Isaac Newton: A selection from the Portsmouth Collection in the University Library Cambridge*, ed. by A. R. Hall and M. Boas Hall (Cambridge, 1962), 374 f.
102. "And if the fixed Stars are the centers of similar systems, they will all be constructed according

to a similar design and subject to the dominion of *One*.... And so that the systems of the fixed stars will not fall upon one another as a result of their gravity, he has placed them at immense distances from one another", Isaac Newton*, The Principia: Mathematical principles of natural philososophy,* transl. by I. Bernard Cohen and Anne Whitman (Berkeley, Los Angeles and London, 1999), 940.

103. See Michael Hoskin, "Newton, Providence and the universe of stars", *Journal for the history of astronomy*, viii (1977), 77–101, and now *idem*, "Gravity and light in the Newtonian universe of stars", *Journal for the history of astronomy*, xxxix (2008), 251–64 . Needless to say, God's providence is in Newton very different from Bruno's concept of it, since the Italian philosopher identifies it with the very same law of nature related to his pantheism. Bruno's concept, which included the principle of motion proper to matter, formed the basis for John Toland's attack against the Newtonian ideology promulgated from the pulpit in the Boyle lectures. For this, see M. C. Jacob, "John Toland and the Newtonian ideology", *Journal of the Warburg and Courtauld Institutes*, xxxii (1969), 307–31; and *eadem*, *The Newtonians and the English Revolution, 1689–1720* (Cornell, 1976), chap. 6.

# ORIENTATIONS OF CHANNEL ISLANDS MEGALITHIC TOMBS

DAVID LE CONTE, La Société Guernesiaise

## *Introduction*

The British Channel Islands (see Figure 1) lie in the Bay of Mont St Michel. The archipelago comprises two main islands: Jersey (12,028 hectares) and Guernsey (6,344 hectares), and several smaller ones, the largest of which are: Alderney (818 hectares), Sark (525 hectares) and Herm (132 hectares).[1] The latter three islands, together with Guernsey, form the Bailiwick of Guernsey.

The islands were part of the French mainland during the lowered sea levels of the last ice age, but the rising post-glacial sea resulted in Guernsey and Alderney becoming islands sometime about 9000 B.C., with Jersey following perhaps 3000 to 3500 years later.[2] Guernsey was in fact two islands until 200 years ago, the northern part, where most of its megalithic tombs are found, being separated from the southern and larger part by a narrow channel which could be crossed at low tides, and which was reclaimed in the early nineteenth century.

Archaeological investigations in the Channel Islands have yielded evidence of

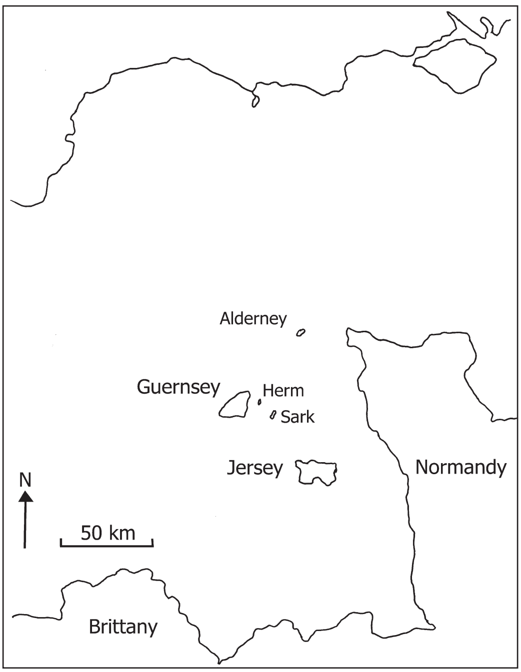


FIG. 1. The Channel Islands.

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Palaeolithic, Mesolithic and Neolithic habitation. Sebire points out that the location of the Channel Islands has produced two separate traditions of monuments: the passage graves of the Atlantic coastal distribution and the long mounds of the North European plain.[3] The passage graves generally date from 3500 B.C., and cists from 2500 B.C. The earliest date of one Neolithic tomb (Les Fouaillages on Guernsey) is 4500 B.C.[4] The oldest in Jersey, La Sergenté, dates from *c*. 4500–4000 B.C., and is the only corbelled passage grave in the Channel Islands.[5] Interestingly, the orientations of these two are quite different from those of the other tombs.

All are built of the local granite. Excavation of most of them was carried out in the nineteenth century, and a number have undoubtedly been disturbed or lost, some as a result of an active stone quarrying industry during that century. Archaeological excavation of earlier (Mesolithic) and later (Bronze Age, Iron Age and Roman) sites has continued to the present day.

New measurements have been made by the author of the orientations of the Channel Islands' megalithic tombs, with a view to determining whether there is any correlation with astronomical phenomena, such as sunrise. The tombs selected for this study included the known passage graves (Figures 2 and 3), whose alignments could, in general, be well defined, and those cists (Figure 4) that appeared to have an undisturbed alignment. This paper summarizes the study, which has been published

FIG. 2. La Pouquelaye de Faldouet, Jersey.

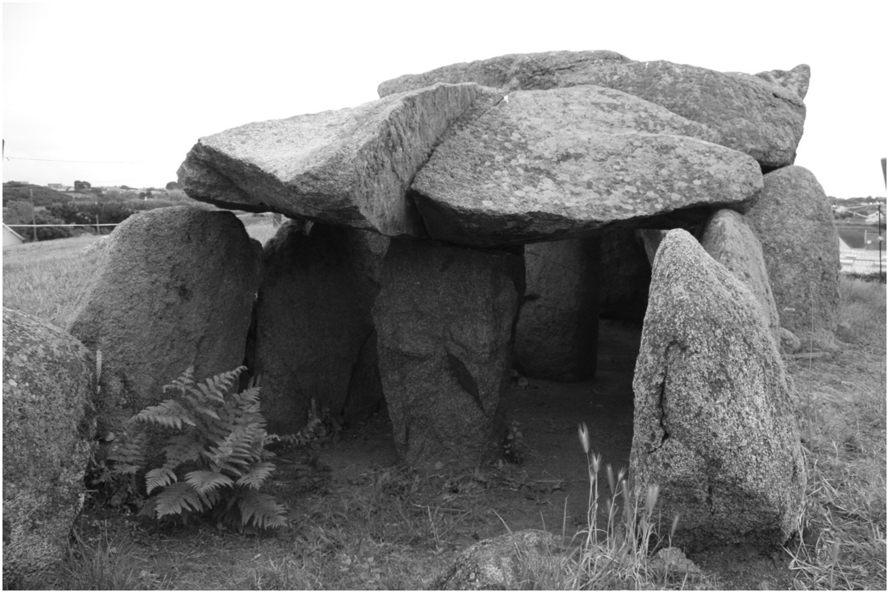

FIG. 3. Le Trepied, Guernsey.

FIG. 4. Sandy Hook cist-in-circle, Guernsey.

in greater detail elsewhere.[6] In this paper the names of the tombs generally follow those used by Kendrick[7] and Hawkes.[8]

*Methodology*

Measurement of the tomb orientations was made by use of the accurate digital maps (Digimaps) of the islands, and the Differential Global Positioning System, which can give positions with accuracies of the order of one centimetre. The method uses an inter-comparison of signals from up to eleven positional satellites, together with accurately surveyed ground base stations located on Jersey and Guernsey. It involves taking positional measurements at two locations along the axis of each tomb, several metres apart, plotting them on the Digimap, and, from their coordinates, calculating, trigonometrically, the azimuth of the line joining them.

This method was supplemented by compass bearings, sightings on distant landmarks which could be identified on the Digimap, direct measurements from aerial photographs, measurements from plans that appear in various publications,[9] and from a previously published graphic of tomb orientations.[10] Not all of these methods could be used on every tomb. In some cases intervening hills or trees prevented reception of the GPS base station or satellites, aerial photographs did not always show the tomb clearly, or the topography was such that no landmarks could be sighted.

An analysis was made to determine the respective error magnitudes for each tomb and method. Weightings were then used to calculate the best values. The azimuths of sunrise and moonrise at the relevant dates were established using standard commercial software.[11]

Sunrise (and sunset) is defined astronomically as occurring when the Sun's upper limb is on the horizon.[12] While sunset may be perceived as occurring at this time, it may be conjectured that sunrise is perceived as occurring at a later instant, such as when the centre of its disc is on the horizon. This can produce differences from the astronomically defined sunrise azimuths of up to half a degree. Therefore, the azimuths of sunrise for the solstices and equinoxes were computed corresponding to this later time.

Local topography will also have an effect on perceived rising times, and therefore azimuths and solar declinations of these events.[13] At the vernal equinox, for example, the angle of the ecliptic to the horizon means that, for each degree increase in horizon altitude, the time of the rising Sun is delayed by over 7 minutes, and shifted in azimuth by 1.4° south. Examination was made of the relative topographies along the axes of each of the tombs, to determine the relevant horizon altitudes and corresponding declinations.

The locations of the Guernsey and Jersey tombs are shown in Figures 5 and 6, respectively. Herm has a large number of tombs for its small size, although only one (Robert's Cross) survives in a measurable state. (Orientations of the remainder were estimated from early sketch plans.) New excavations of the Herm tombs are to be carried out in 2008 and 2009, and it is possible that further measurable orientations

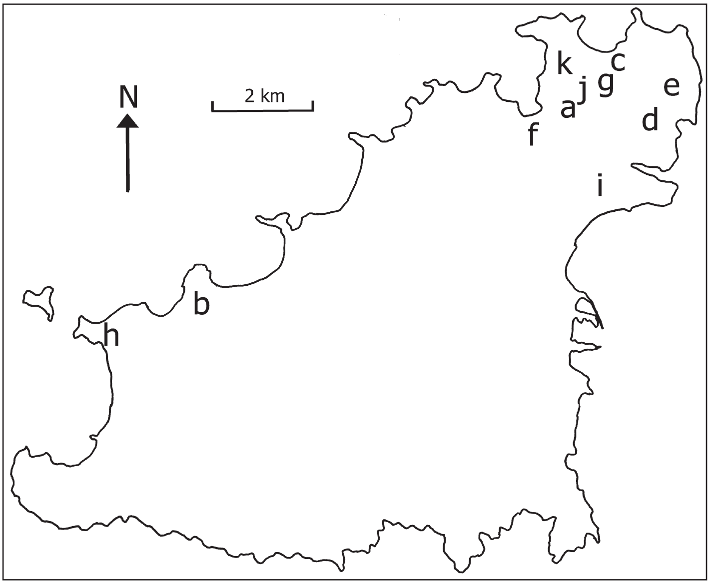


FIG. 5. Map of Guernsey showing tomb locations.

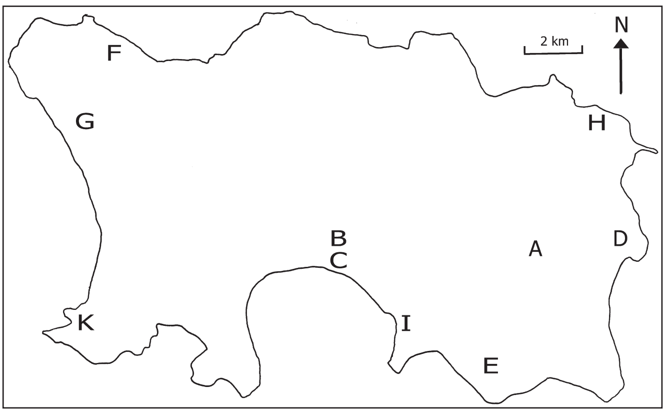


FIG. 6. Map of Jersey showing tomb locations.

may result. Excavations in the neighbouring island of Jethou in 2007 revealed nothing measurable.[14] There is only one tomb, a cist, in Alderney surviving in a measurable state, and that was measured by means of an on-site photograph and aerial photography, a church tower conveniently lying directly on the tomb alignment. The tombs in Sark are either too disturbed or have insufficiently clear orientation for any meaningful measurement.[15]

The Differential GPS measurements and sightings were carried out in Guernsey in May 2003 and July 2004, in Jersey in October 2007, and in Herm in January 2008.

*Results*

The results are shown in Table 1, and graphically in Figures 7 and 8. The latitude of Jersey is 49.2º N, that of Guernsey and Herm is 49.4º N, and that of Alderney is 49.6º N. The declination corresponding to the azimuth and horizon altitude of each tomb has been calculated using Ruggles's *GETDEC4* computer program.[16]

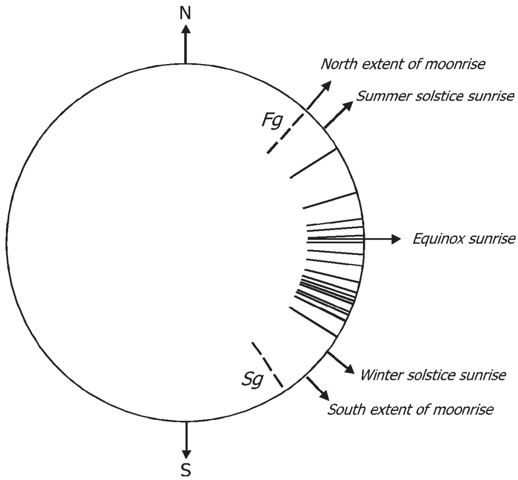

FIG. 7. Orientations of passage graves. Fg: Les Fouaillages, Sg: La Sergenté.

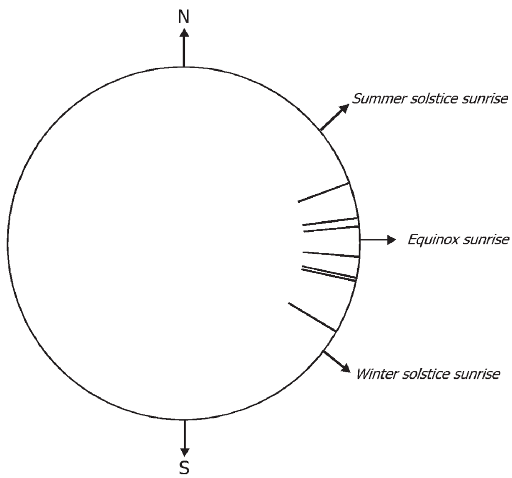

FIG. 8. Orientations of cists.

TABLE 1.

| Island | Map ref. | Site | Az. ° | +/– ° | Alt. ° | Dec. ° |
|---|---|---|---|---|---|---|
| *Long mound (c. 4500 B.C.)* | | | | | | |
| Guernsey | a | Les Fouaillages | 42 | 1 | 0 | 29 |
| *Corbelled tomb (4500–4000 B.C.)* | | | | | | |
| Jersey | K | La Sergenté | 151 | 3 | 0 | –35 |
| *Passage graves (c. 3500 B.C.)* | | | | | | |
| Guernsey | b | Le Trépied | 58 | 2 | 0 | 20 |
| Guernsey | d | La Rocque qui Sonne | typ.† | | 0 | |
| Herm | R | Robert's Cross (Number 12) | 89 | 5 | 7 | 6 |
| Guernsey | e | Le Déhus | 82 | 1 | 0 | 5 |
| Alderney | L | Essex Hill * | 85 | 10 | 0 | 3 |
| Guernsey | h | Le Creux ès Fées | 88 | 2 | 0 | 1 |
| Jersey | D | La Pouquelaye de Faldouet | 89 | 1 | 0 | 0 |
| Jersey | A | La Hougue Bie | 94 | 3 | 0 | –3 |
| Jersey | H | Le Couperon de Rozel | 98 | 2 | 0 | –6 |
| Alderney | N | Les Pourciaux south * | 98 | 10 | 0 | –6 |
| Herm | U | Number 13 * | 109 | 10 | 7 | –7 |
| Guernsey | i | Delancey | 103 | 1 | 0 | –9 |
| Herm | T | Number 6 * | 106 | 10 | 1 | –11 |
| Jersey | C | Ville-ès-Nouaux long-cist | 108 | 2 | 0 | –12 |
| Jersey | G | Les Montes Grantez | 108 | 1 | 0 | –12 |
| Alderney | O | Les Pourciaux north * | 110 | 10 | 0 | –13 |
| Jersey | I | Mont de la Ville * | 113 | 10 | 0 | –15 |
| Guernsey | k | La Varde | 113 | 4 | 0 | –15 |
| Jersey | E | Mont Ubé | 114 | 2 | 1 | –16 |
| Jersey | F | Dolmen des Géonnais | 116 | 1 | 0 | –17 |
| Alderney | P | SW of Fort Essex * | 122 | 10 | 0 | –21 |
| *Cists–in–circles (c. 2500 B.C.)* | | | | | | |
| Guernsey | c | Unnamed site near tower 7 | 74 | 5 | 0 | 10 |
| Guernsey | f | Sandy Hook | 85 | 4 | 0 | 3 |
| Guernsey | g | La Mare ès Mauves | 88 | 3 | 1 | 2 |
| Herm | Q | Number 15 * | 92 | 10 | 0 | –2 |
| Jersey | B | Ville-ès-Nouaux short-cist | 93 | 5 | 0 | –2 |
| Alderney | M | Fort Tourgis | 99 | 2 | 0 | –6 |
| Guernsey | j | La Platte Mare | 111 | 2 | 1 | –13 |
| | | *North extent of moonrise* | *42* | | | *28* |
| | | *Summer solstice sunrise* | *50* | | | *24* |
| | | *Equinox sunrise* | *89* | | | *0* |
| | | *Winter solstice sunrise* | *128* | | | *–24* |
| | | *South extent of moonrise* | *139* | | | *–30* |

* The orientations of sites marked with an asterisk are based on published historic sketch plans, as the sites themselves do not survive in a measurable state. Comparison with tombs that can be measured with orientations derived from published plans shows discrepancies of several degrees, and therefore relatively large errors have been assigned to those tombs for which only plan data are available.

† La Rocque qui Sonne is too disturbed for measurement, but was clearly orientated in the typical easterly direction.

FIG. 9. Les Fouaillages, Guernsey.

*Conclusions*

The Guernsey tombs' entrances, with one exception, point towards directions lying between sunrise at the summer and winter solstices. The exception is Les Fouaillages (Figure 9), whose date and structure are unique in the island, and whose orientation, perhaps coincidentally, matches the northern limit of moonrise. The Jersey, Alderney and Herm tombs generally point south of east, but north of the midwinter sunrise. The sole Jersey exception, La Sergenté (Figure 10), is unusual, being of an early date and atypical design. As might be expected, a few, such as Le Creux ès Faïes in Guernsey and La Hougue Bie in Jersey, are close enough due east that the equinoctial rising sun penetrates to the back of the tomb.

These results are consistent with the hypothesis, expressed by Hoskin and Ruggles,[17] that the tombs were orientated towards sunrise at the date their construction was started. Using Hoskin's terminology,[18] of the 30 tombs, 28 are Sun Rising (SR), one is Sun Climbing (SC), and one does not conform.

*Acknowledgements*

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FIG. 10. La Sergenté, Jersey

Limited of Guernsey for the loan of the differential GPS equipment, for assistance in its use, and for downloading the readings.

REFERENCES

1. Charles David, Guernsey Biological Records Centre (personal communication, 2008).
2. John Renouf, Jersey (personal communication, 2008).
3. Heather Sebire, *The archaeology and early history of the Channel Islands* (Stroud, Gloucestershire, 2005), 64.
4. *Ibid.*, 60.
5. *Ibid.*, 172.
6. David Le Conte, "The orientation of megalithic tombs in Guernsey: An astronomical connection?", in La Société Guernesiaise, *Report and transactions [2005]*, xxv/5 (Guernsey, 2006), 889–906. An addendum to that paper is being published in the 2007 issue of that journal. Data and photographs of the field work have been deposited in the archives of that society.
7. T. D. Kendrick, *The archaeology of the Channel Islands*, i: *The Bailiwick of Guernsey* (London, 1928).
8. Jacquetta Hawkes, *The archaeology of the Channel Islands*, ii: *The Bailiwick of Jersey* (Jersey, 1937).
9. Kendrick, *op. cit.*, and Hawkes, *op. cit.*
10. David E. Johnston, *The Channel Islands: An archaeological guide* (Chichester, Sussex, 1981), 25.
11. *SkyMap Pro 10* computer program (web site: http://www.skymap.com/); and *Starry Night Pro 3*

computer program (web site: http://www.starrynight.com/).

12. P. Kenneth Seidelmann, *Explanatory supplement to The Astronomical Almanac*, rev. edn (Sausalito, CA, 1992), 483.
13. Clive Ruggles, *Astronomy in Prehistoric Britain and Ireland* (New Haven, CT, 1999), 22.
14. Professor Chris Scarre, Durham University (personal communication, 2008).
15. Johnston, *op. cit.* (ref. 10), 120; and Ian Kinnes, "Megaliths in action: Some aspects of the Neolithic Period in the Channel Islands", *The archaeological journal*, cxlv (1988), 13–59, p. 25.
16. Web site: http://www.le.ac.uk/archaeology/rug/aa/progs/getdec.html.
17. Michael Hoskin, *Tombs, temples and their orientations: A new perspective on Mediterranean prehistory* (Bognor Regis, West Sussex, 2001), 216; and Clive Ruggles and Michael Hoskin, "Astronomy before history", in *The Cambridge concise history of astronomy*, ed. by Michael Hoskin (Cambridge, 1999), 1–17, pp. 2 *et seq*.
18. Hoskin, *Tombs, temples*, 20.

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# ORIENTATIONS OF DOLMENS OF WESTERN EUROPE: SUMMARY AND CONCLUSIONS

MICHAEL HOSKIN, St Edmund's College, Cambridge

Over the past two decades, a series of papers in this journal and its *Archaeoastronomy* supplement have reported on the orientations of the Neolithic communal tombs in the west of Europe.[1] Such tombs are by no means uniformly spread throughout this region. Communication was often by water, and so it is not surprising that in Iberia (for example) tombs deep in the interior are rare. But on the broader canvas of Europe, there are large areas where such tombs are plentiful and others where they are seldom found. For example, in Italy and Sicily, there are almost no Neolithic tombs; and they are rare along the northern French coast in Normandy and points east.

The result is that Iberia together with the region of France lying west of a line from Nice to the Channel Islands has formed a convenient geographical area for our study, an area relatively isolated from those other regions of Europe that have concentrations of Neolithic tombs. This study is now complete, and the published papers contain lists of orientations of communal tombs of all the various types to be found in each part of our area. It is therefore possible to attempt an overview and to reach conclusions.

At first encounter, the variety of forms that the tombs take is bewildering. Most are built on the surface, but a few are excavated out of the bedrock.[2] Of those on the surface, most are megalithic, built with a small number of large stones, but some are made of large numbers of small stones, most notably the false-cupola tombs known as *tholoi*.[3] Of the megalithic tombs, some are modest in size and could be built by a single family in a matter of days, others are monumental on a scale that defies belief. Most are passage graves, but the passages may be long or short.

However, all these communal tombs were designed to permit the introduction of additional bodies as the need arose, and although very occasionally access was from overhead,[4] nearly always the chamber has a well-defined entrance opposite the backstone, and therefore an orientation, the direction 'faced' by bodies imagined as looking out through the entrance.

With rare exceptions, the passage (if any) has the same orientation as the chamber; that is, the monument as a whole has an axis of symmetry and its orientation is unproblematic. On the French Causses, however, there are a few small 'coudé' tombs in which the passage is set at an angle to the chamber,[5] and the same is true of a handful of major tombs in the Carnac region of Brittany;[6] in these the orientation (if any) intended by the builders is unclear, although the dual directions involved are far from random. The great Breton dolmens 'à entrée latérale' — in effect, east-facing *allées couvertes* with the entrance located around the corner on the south side — appear at first sight to be similarly anomalous, but I have argued that this is not so.[7]

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With these few exceptions, the Neolithic dolmens of the area have well-defined and uncontroversial orientations that can be measured. The first question then to be asked of any group of dolmens is, Do the orientations fall into a pattern or are they random? The answer, for all regions, all periods, and all forms of structure, is invariably: Yes, they fall into a pattern. That is, the builders always felt constrained to construct the tomb so that its orientation conformed to custom.

The second question is, Was the pattern motivated by the sky? Investigators often assume that this is the case, but this is a methodological error: the orientations of mosques display a clear pattern and we know this has nothing to do with the sky. The clearest evidence of a pattern that was certainly motivated by the sky is to be found in the seven-stone 'antas' of the Alentejo region of Portugal.[8] These tombs are of unique construction (the side-stones are not orthostats but each leans on its predecessor) and so they form a well-defined group. Of the 177 measured (see Figure 1), every single one faced within the narrow range of the eastern skyline where the sun (and the moon) rose. That this could happen by chance is out of the question, and as the tombs are scattered over a vast area of Portugal (and even into Spain), the custom of orientation cannot be terrestrial in motivation and so must be celestial.

The range of moonrise extended a little further north and a little further south of the range of sunrise, and so these tombs that sometimes faced sunrise would also sometimes face moonrise. However, in the summer the would-be builders must have been occupied in growing food and only in the autumn could they turn to building work, after the harvest was in. Overwhelmingly, the anta orientations do in fact face sunrise in the autumn, which strongly suggests that the builders embarked on

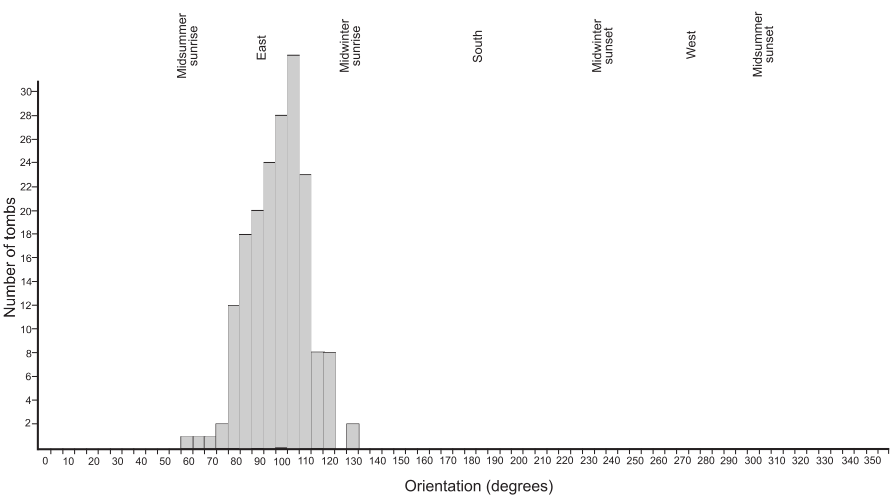


FIG. 1. Histogram of orientations of 177 antas of central Portugal and neighbouring Spain. The two with orientations 128° and 129° respectively are in a valley with steep sides and so they too faced within the range of sunrise.

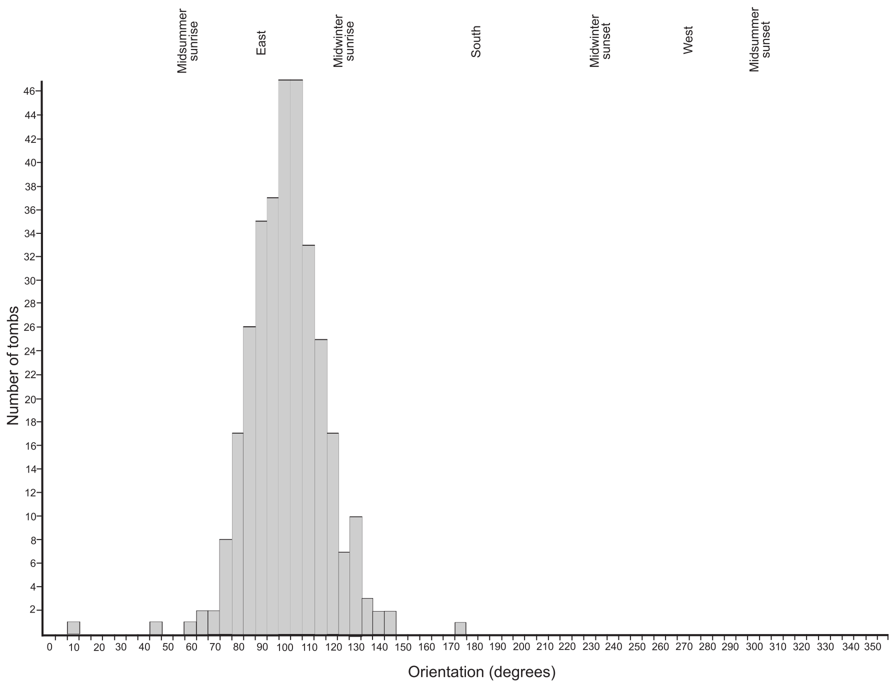


FIG. 2. Histogram of orientations of 334 tombs of west Iberia.

construction around that time of year and that they aligned the tombs to face the rising sun on the day that work started (as was not uncommon in the Middle Ages with Christian churches). The alternative hypothesis, that the tombs faced moonrise, makes it less easy to explain this preference for orientations a little south of east. We therefore conclude, first, that the custom of orientation was without doubt celestially motivated; and second, that this custom probably required the anta to face the rising sun on the day building started.

A tomb that is oriented within the range of sunrise I characterize as 'SR'. We find that not only the antas but the tombs of western Iberia as a whole are overwhelmingly SR: of the 334 tombs I have measured (Figure 2), no fewer than 324 (97.0%) faced within the range 60°–130°, that is, within the range of sunrise (or marginally further south).[9]

In southern Spain there are other groups of tombs that are SR, although with occasional anomalous orientations. The megalithic sepulchres of Montefrío provide one example,[10] and the tholos tombs of Los Millares another.[11] However, as we move further from the Atlantic seaboard where the earliest tombs are to be found, the SR custom appears to be relaxed, and we find increasing numbers of tombs that face south of midwinter sunrise; that is, in directions where the sun had risen and was climbing in the sky (these I term 'SC'). In total, I have measured 945 tombs in

Spain, Portugal and the region immediately across the Pyrenees in France, and of these no fewer than 911 (96.4%) faced the sun when rising or climbing (or around culmination: in the range 60°–190°).[12]

In the interior of southwest France, on the Causses, we find that the numerous tombs are predominantly 'simple dolmens', formed of just four stones: a backstone, a stone to each side, and a capstone. Even when such a modest tomb is in pristine condition its orientation is poorly defined, and many have in fact been disturbed over the centuries. Furthermore, our information on their orientations is owed mainly to (French) archaeologists, not all of whom had this datum as one of their primary concerns. This makes the SR/SC pattern of reported orientations all the more remarkable.[13] Every one of nearly 600 such tombs (outside the southerly *départements* of Ardèche and Gard, of which more later) faced within the range 0°–192°: westerly (and northerly) orientations are unknown, and over 92% of the tombs faced within the range 60°–166°.

Figure 3 shows the orientations of the 945 tombs of Iberia together with the 597 of the Causses outside Ardèche and Gard. It is evident at a glance that the overwhelming majority of these 1542 tombs are SR, with orientations to sunrise in the autumn and early winter predominating; and that most of those that are not SR are SC.

Further north in France, in the Loire Valley, we encounter a wide variety of tomb, including the monumental 'Angevin dolmens' found in greatest numbers near Angers. Every one of the 85 tombs measured in this area faced the eastern half of the horizon. Four (4.7%) faced anomalously north of midsummer sunrise, but the other 81 (95.3%) are SR/SC.[14]

When we cross into Brittany in the far northwest of France, we encounter an even greater variety of tomb.[15] There are a handful of outlying Angevin dolmens, and,

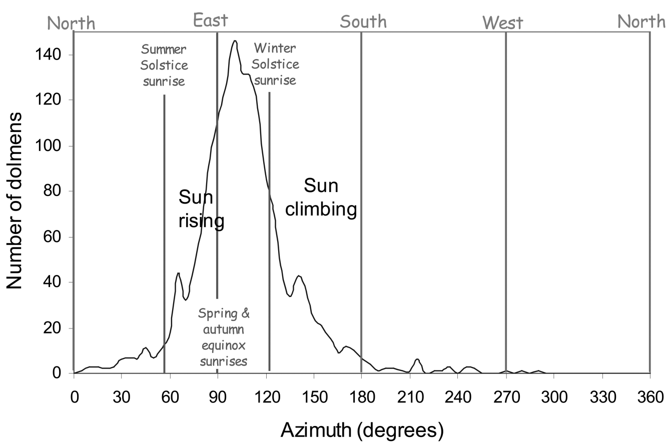


FIG. 3. Graph of orientations of 1542 tombs of Iberia and of the Causses excluding Ardèche and Gard (courtesy of David Le Conte).

along the south coast, a number of 'transcepted' tombs, and of these a minority faced westerly. When we consider the much more numerous Breton passage graves and the later *allées couvertes*, it proves helpful to divide the *départements* of Brittany into those in the south and east (and therefore nearest the Loire), and those in the north and west. Of the 68 passage graves measured in the south and east, all (100%) are SR/SC; of the 21 *allées couvertes* in the south and east, all (100%) are SR/SC; while the handful of dolmens 'à entrée latérale' in the south and east are all SR. In the north and west, however, although the majority of tombs of all types are SR/SC, a significant minority face westerly: the consensus is no longer overwhelming.[16] Meanwhile of the 31 measurable tombs in the nearby Channel Islands of Jersey, Guernsey, Alderney and Herm, 29 (93.5%) are SR and the remaining 2 (6.5%) are SC.[17]

This overview has so far taken into account over 1700 tombs spread over Portugal, Spain, southwest, west and northwest France, the French Causses, and the Channel Islands, a vast region extending some 1500 km from one extreme to the other. Throughout this region, when agriculture was developed and the local clan settled in one place, people everywhere decided to build communal tombs on the surface of the ground, tombs that often seem to be bold statements to the passer-by that the land has been occupied by the clan since time immemorial; and of these 1700 or so tombs, nineteen out of every twenty faced sunrise or the sun when it was climbing in the sky.

Along the French Mediterranean coast, however, things were very different, and many of the tombs faced westerly rather than easterly. Working on the principle that customs become increasingly relaxed at greater distances from their source (in both time and space), archaeologists have pinpointed the origin of the west-facing tombs — so anomalous in the broad European context — at Fontvielle, near Arles, close to the Rhône.[18]

The Fontvieille tombs were not prominent surface structures as in most other places; in fact, they were not surface structures at all. Instead, the long rectangular chambers were excavated out of the bedrock and then covered with roof-slabs. These slabs were carefully dressed on the interior, but the exterior was left in its natural state and cannot easily be distinguished from undisturbed bedrock; only the presence of discreet entrance steps to the chamber below betrays the existence of a tomb. In one place, where the rock was of poor quality, the trench was excavated as usual, and then a dolmen with drystone walls was built within it, below ground level and concealed from sight.

Not only were the tombs hidden from view, but they faced west rather than east. The number of tombs at Fontvieille is too small to permit a statistical proof, but the pattern of orientation is consistent with the tombs' being constructed to face the setting sun ('SS').

With increasing distance from Fontvieille, we find tombs that modify the structural form found there: the tombs are now constructed on the surface rather than below ground, the chambers are again rectangular but less extreme in length, and

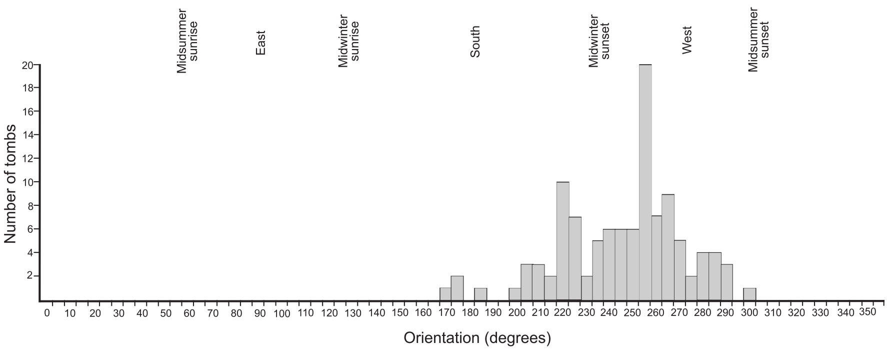


FIG. 4. Histogram of 84 Fontvieille-type dolmens in Provence (to the east of Fontvieille), and 26 in east Languedoc (to the west and northwest). Those in Provence have azimuths between 206° and 289°: they are uniformly SS/SD, and none of them is close to culmination. The five tombs with azimuths closest to south are all in Ardèche, where tombs of easterly-facing traditions are also found.

the sidewalls frequently alternate the fragile drystone with vertical slabs. And just as in Iberia the strict SR custom seems to have been relaxed with increasing distance to permit directions where the sun is climbing and so became SR/SC, so the SS custom of Fontvieille seems to have been relaxed to permit directions where the sun is descending ('SD') and so became SD/SS.

To the east of Fontvieille, throughout Provence in the direction of the Italian frontier, the tombs are uniformly SD/SS (see Figure 4). Influence in Provence, it seems, came solely from Fontvieille; and this is unsurprising, because in neighbouring *départements* of southeast France the SR/SC tombs widespread elsewhere are nowhere to be found. But to the northwest and west of Fontvieille, as far as the Spanish frontier and even a little beyond, the Fontvieille custom of westerly orientation was in conflict with the normal SR/SC custom found on the Causses, and in these regions there is a confusion of construction styles as well as of orientations.[19] The situation is particularly interesting in Ardèche and Gard, not far from Fontvieille. There the SR/SC tombs tend to face closer to south than usual, and the same is true of the SD/SS tombs (see Figure 4): it is as though the rival customs are seeking to downplay their differences.[20]

The picture that has emerged from our fieldwork, therefore, is of orientations to sunrise, or to the sun when rising, throughout Iberia and the southwest, west and northwest of France, as far as the Channel Islands; and to sunset, or the sun when descending, along the French Mediterranean coast east from Fontvieille and (but only in competition) west from Fontvieille as well.

BIBLIOGRAPHY

*Abbreviations*

| | |
|---|---|
| *AA* | *Archaeoastronomy* supplement to *Journal for the history of astronomy* |
| *JHA* | *Journal for the history of astronomy* |
| MH | Michael Hoskin |

*General*

MH, *Tombs, temples and their orientations: A new perspective on Mediterranean prehistory* (Ocarina Books, Bognor Regis, 2001); translated into Italian, with an added section on the Loire Valley, as *Stele e stelle: Orientamento astronomico di tombe e templi preistorici del Mediterraneo* (Ananke, Turin, 2006).

*Iberia*

[1] MH, "Orientations of megalithic sepulchres in Salamanca, Spain", *JHA*, xxiii (1992), 57–60.

[2] MH, Elizabeth Allan and Renate Gralewski, "Studies in Iberian archaeoastronomy: (1) Orientations of the megalithic sepulchres of Almería, Granada and Málaga", *AA*, no. 19 (1994), S55–82.

[3] MH, Elizabeth Allan and Renate Gralewski, "Studies in Iberian archaeoastronomy: (2) Orientations of the tholos tombs of Almería", *AA*, no. 20 (1995), S29–40.

[4] MH, Elizabeth Allan and Renate Gralewski, "Studies in Iberian archaeoastronomy: (3) Customs and motives in Andalucía", *AA*, no. 20 (1995), S41–48.

[5] MH and Toni Palomo i Pérez, "Studies in Iberian archaeoastronomy: (4) The orientations of megalithic tombs in Eastern Catalunya", *JHA*, xxix (1998), 63–79.

[6] MH and colleagues, "Studies in Iberian archaeoastronomy: (5) Orientations of megalithic tombs of northern and western Iberia", *AA*, no. 23 (1998), S39–87.

[7] MH and Carme Sauch i Aparicio, "Studies in Iberian archaeoastronomy: (6) Orientations of megalithic tombs of Badajoz and neighbouring Portugal", *AA*, no. 24 (1999), S35–40.

[8] MH and Toni Palomo i Pérez, "The orientations of megalithic tombs of Eastern Catalunya: Addendum", *AA*, no. 24 (1999), S89–90.

[9] Adriano Gómez Ruiz and MH, "Studies in Iberian archaeoastronomy: (7) Orientations of megalithic tombs of Huelva", *AA*, no. 25 (2000), S41–50.

[10] MH and colleagues, "Studies in Iberian archaeoastronomy: (8) Orientations of megalithic and tholos tombs of Portugal and southwest Spain", *AA*, no. 26 (2001), S45–64.

[11] MH, "Studies in Iberian archaeoastronomy: (9) An overview", *AA*, no. 27 (2002), S75–82.

*Southwest, West and Northwest France*

[12] Richard Iund, "Orientations of dolmens north of the eastern Pyrenees", *AA*, no. 27 (2002), S21–28.

[13] MH and Gail Higginbottom, "Orientations of dolmens of west-central France", *AA*, no. 27 (2002), S51–61.

[14] MH, "Orientations of Neolithic monuments of Brittany: (1) Context", *JHA*, xxxiv (2003), 401–20.

[15] MH, "Orientations of Neolithic monuments of Brittany: (2) The early dolmens", *JHA*, xxxviii (2007), 487–92.

[16] MH, "Orientations of Neolithic monuments of Brittany: (3) The *allées couvertes*", *JHA*, xxxviii (2007), 493–501.

*Southern France*

[17] Yves Chevalier, “Orientations of 935 dolmens of southern France”, *AA*, no. 24 (1999), S47–82.

[18] Gérard Sauzade, “Orientations of the Provençal dolmens”, *AA*, no. 25 (2000), S1–10.

*Channel Islands*

[19] David Le Conte, “Orientations of Channel Islands megalithic tombs”, *JHA*, xxxix (2008), 497–506.

REFERENCES

1. For a list, see the Bibliography (above).
2. In particular at Fontvieille, see below.
3. Most notably at Los Millares in Almeria, see MH, *Tombs*, 57. The tombs stemming from the Fontvieille tradition have rectangular chambers whose sidewalls are wholly or partly drystone, see MH, *Tombs*, 139, 144, 145; Bibliography, art. 18.
4. Most notably in the *arques amb vestíbul pou* of Catalunya, of which El Salt d’en Peió is the best example. See J. Tarrús and J. Chinchílla, *Els monuments megalítics* (Girona, 1992), 51, and the works by Font Cop cited in ref. 19 below.
5. MH, *Tombs*, 132; Bibliography, art. 17.
6. See Bibliography, art. 14.
7. See Bibliography, art. 16.
8. MH, *Tombs*, 95–100; Bibliography, art. 11.
9. MH, *Tombs*, 109, where there are arithmetical errors: for 335 read 323; for 346 read 334; and for 336 read 324.
10. MH, *Tombs*, 66–67; Bibliography, art. 2.
11. MH, *Tombs*, 59–62; Bibliography, art. 3.
12. MH, *Tombs*, 213, with arithmetical error corrected.
13. MH, *Tombs*, chap. 8; Bibliography, art. 17.
14. Bibliography, art. 13.
15. Bibliography, art. 14.
16. Bibliography, art. 15 and 16.
17. Bibliography, art. 19. These tombs are thought to date either to the middle of the fourth millennium B.C., or to the second half of the third millennium.
18. MH, *Tombs*, chap. 9; Bibliography, art. 18.
19. MH, *Tombs*, chap. 10. In an important doctoral thesis at Barcelona University, “Les orientacions dels sepulcres de corredor, galeries catalanes i arques amb vestíbul-pou de Catalunya” (2007), Josep Oriol Font Cop has revised and extended the present author’s work in Catalunya. See his *Els dòlmens catalans en el context de l’Europa Occidental* (Societat Catalana d’Arqueologia, Barcelona, 2007). Font Cop’s work will require minor adjustments to the data published by the present author and summarized here.
20. MH, *Tombs*, 148–50; Bibliography, art. 17, S60–61.

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NOTE

# A TEST OF THE "SIMULTANEOUS TRANSIT METHOD"

GEOFFREY KOLBE, Newcastleton, Scottish Borders

## 1. *Introduction*

This Note reports on experiments devised to test the accuracy of the "Simultaneous Transit Method" (STM) by which, according to Kate Spence,[1] eight Old Kingdom pyramids from Snofru to Neferirkare were aligned to the cardinal points. This STM involved the use of a plumb line and sighting vane to observe two circumpolar stars and fix the azimuth at the moment when one star was directly above the other. A small lamp, some distance away from the plumb line, was then positioned so that it had the same azimuth. The plumb line and the small lamp formed the two ends of a principal survey line from which the pyramid was aligned.

## 2. *Experimental Procedure*

For practical reasons, I tested the STM by two separate experiments, described in Sections 3 and 4 below.

The apparatus used for the experiments consisted of a light cotton rope for the plumb line, having a diameter 3.8mm. A tripod supported the line to a height of 2.8m. To mitigate against wind which would cause a traditional weighted plumb line to sway and oscillate (and not having the luxury of being able to wait for windless nights), I tethered the bottom of the line to a weighted board sitting on the ground. A lead-screw mechanism allowed the bottom of the line to be adjusted so that the line was vertical. I tested the verticality of the line using a theodolite, which viewed the line at the approximate azimuth of interest.

The sighting vane was made from a piece of sheet copper 1mm thick, into which a slit of width 3.8mm was cut. The sighting vane was attached to a block of wood using a wing-nut, so that it could be rotated and set with the slit vertical. A flat board was placed on the ground and packed so that it was firm. The sighting vane block was set upon the board and the copper sighting vane rotated so that the slit was parallel by eye with the plumb line. The block could now be moved about the board with the slit remaining vertical. For these experiments, the sighting vane was set up 2.7m behind the plumb line.

## 3. *Principal Survey Line Alignment*

This experiment was performed at Riccarton in Scotland, $55^\circ\,14.8'$N, $2^\circ\,42.8'$W. For this experiment, a small peg was driven into the ground 70m away from the plumb line. The azimuth of the peg from the line was $168^\circ\,53' \pm 1'$ as measured in the usual

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way with a theodolite, which was calibrated by taking timed sightings of the sun. A white light LED, 5mm in diameter, was set up directly over the peg. The sighting vane was adjusted (at night) so that the lamp was occluded by the line and then left in place. Following this, two observations were made of the times at which the star Sirius transited the peg and was occluded by the plumb line. On 23 November 2007, Sirius was observed to be occluded by the line for a period of six seconds. The mid-point of the period of occlusion occurred at 02:05:20 GMT, at which time the azimuth of Sirius was calculated to be 168° 52.2′. The second observation was made on 25 November 2007 when Sirius was observed to be occluded for a period of four seconds. The mid-point of the period of occlusion occurred at 01:57:32 GMT, at which time the azimuth of Sirius was 168° 53.1′. The average of these two observations is 168° 52.7′, which is in excellent agreement with the theodolite azimuth measurement of the peg from the line.

This experiment showed that by using the apparatus described here, it is possible to measure the azimuth of a survey line on the ground to an accuracy of ±1′ from an observed stellar transit. There is no reason why the reverse should not be true, allowing a survey line to be set with similar accuracy from an observed stellar transit.

### 4. *Observation of a Simultaneous Azimuth*

This experiment was performed in the Western Desert of Egypt, where there are clear skies and where the latitude was similar to that of Giza. Due to precessional drift, the star pair proposed by Spence, Mizar (ζ UMa) and Kochab (β UMi), are no longer suitable. It was decided instead to use the star pair Kochab and Alrai (γ Cephei). Their altitudes at the moment of simultaneous azimuth are very similar to those of Kochab and Mizar in Old Kingdom times and the simultaneous azimuth is only 6° away from True North.

An observation was made on the night of 3 November 2007. The location, as recorded from a GPS receiver, was 27° 28.3′N, 28° 59.6E. The method was to track Kochab (the lower and faster star) with the sighting vane, keeping it occluded until Alrai (the upper star) was also observed to be occluded. Both stars were observed to be simultaneously occluded by the plumb line for a period of 17 seconds. The mid-time of the observed occlusion period was 20:29:43 GMT, which was just three seconds earlier than the calculated moment of simultaneous azimuth at 20:29:46 GMT. Kochab had an azimuth rate of change of one minute of arc every 15.4 seconds, from which it can be determined that the sighting vane was set correctly to the plumb line with an accuracy of about 0.2′ and a precision of about ±0.6′.

### 5. *Conclusions and Discussion*

It has been shown that with apparatus of the scale and type used for the experiments described here, an accuracy of ±1′ should be easily achievable for the STM. A taller plumb line would have enabled the sighting vane to be set further back and so achieve greater sensitivity, although diffraction effects would limit the plumb line to sighting vane distance to about 10m.

Due to precessional drift, the simultaneous azimuth of Kochab and Mizar during the Old Kingdom period varied[2] by about 31′ per century. Spence[3] hoped that it would be possible to date the pyramids of Khufu and Khafre to ±2 years or better, which assumes that the pyramid surveyors had used the STM to lay down the principal survey line with an accuracy of about ±0.5′. On the basis of on the experiments described here, the STM would appear to be capable of accuracy of this order.

Such accuracy would be clearly better than the ±2′ general limit set by Belmonte[4] for possible alignment methods of Old Kingdom pyramids. Belmonte based his limit on a resolving power of 3′ for the unaided human eye. But the STM is by nature a null method, which does not require good visual acuity. (The apparatus is aligned with a star when the star is occluded by the plumb line and is *not* seen.) It is this property that gives the STM the potential for accuracy that would appear *prima facie* to exceed the capabilities of the human eye.

It should be emphasized that the accuracy of these experimental results does not depend critically on precision in the construction of any of the apparatus elements, or on the materials used. A version of the New Kingdom *merkhet* (for the plumb line) and *bay* (for the sighting vane) would have been quite suitable. It is only necessary that the slit width in the sighting vane should be smaller than that of the dark adapted pupil of the eye (about 7mm), yet wide enough that the star can still be clearly seen, and that the plumb line width should be close to that of the slit. For the lamp, the only requirement is that the angular width of the flame, as viewed from the sighting vane, be small compared to the required precision of the STM. This criterion would be easily met using a typical ancient Egyptian small oil lamp over the base length of a typical Old Kingdom pyramid.

There is little doubt that the Old Kingdom Egyptians could have built an apparatus as described here with the materials available to them and achieved similar accuracies to those reported here. Discussion of whether they did, or indeed would have used the STM to align their pyramids, is beyond the scope of this brief communication. However, this work is a necessary first step to answering these questions.

*Acknowledgements*

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REFERENCES

1. Kate Spence, "Ancient Egyptian chronology and the astronomical orientation of pyramids", *Nature*, cdviii (2000), 320–4.
2. Kate Spence, "Astronomical orientation of the pyramids", *Nature*, cdxii (2001), 700.
3. Spence, *op. cit.* (ref. 1), 324.
4. J. A. Belmonte, "On the orientation of Old Kingdom Egyptian pyramids", *Archaeoastronomy,* no. 26 (*Journal for the history of astronomy,* xxxii (2001)), S1–20.

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ESSAY REVIEW

# THE CORRESPONDENCE OF PIERRE GASSENDI: *LETTRES LATINES*, *OPERA OMNIA*, AND THE "PRIMAL ARCHIVE"

*Pierre Gassendi (1592–1655): Introduction à la Vie Savante*. Sylvie Taussig (Brepols,Turnhout, 2003). Pp. 454. €60 (paperback). ISBN 2-503-52182-7.

*Pierre Gassendi (1592–1655): Lettres Latines.* Transl. and notes by Sylvie Taussig (2 vols, Brepols, Turnhout, 2004). Pp. xxxiv + 622, x + 609. €175 (paper). ISBN 2-503-51353-0.

When Pierre Gassendi died he left a large legacy of letters. Best remembered as a mechanical philosopher, mitigated sceptic, and Epicurean atomist, Gassendi made his early reputation in astronomy, and by mid-career was known throughout Europe as one of the chief architects of the New Science. In retrospect, as Rochot remarked, Gassendi's influence in science was more philosophical and critical than technical and systematic. Gassendi was a gentle sceptic and eclectic humanist; his empiricism was hands-on, his theories scissors-and-paste. In France, the Académie Montmor made him patron-saint; in Italy, the Accademia del Cimento lionized his name, which appears almost as often as Galileo's in the *Saggi* (1666, 1684). In context, Gassendi's contemporary reputation in the Republic of Letters was based on his extensive correspondence network. Today, especially over the last several decades, his reputation has enjoyed an historical resurgence, although his published works and correspondence remain far less accessible compared to those of French contemporaries, Descartes, Pascal, or Mersenne. Given Gassendi's Baroque Latin, a key concern has been the lack of translations and absence of a critical edition of his complete correspondence. Finally, despite a dozen recent monographs on his thought and work, there is no modern biography of Gassendi. It is in this context, then, that the appearance of the works under review has been much anticipated by scholars seeking to situate Gassendi in the mainstream of early modern science.

The following essay seeks to evaluate and place in historical context the three volumes under review: Taussig's *Introduction* to Gassendi's life and her edition of the *Lettres latines*, which consists of the Latin letters in French translation and a companion volume of Notes. My second purpose is to sketch the historical circumstances that gave us Gassendi's *Opera omnia* (1658) while leaving hundreds of his letters unpublished. A brief overview of Gassendi's "primal archive" provides context for this review and desiderata for a future edition of Gassendi's Complete Correspondence.

*Introduction à la vie savante*

Despite his rich historiography, Gassendi remains an enigma. From the outset, biographers have troubled over the relationship between his public and private beliefs. While important biographical insights have been offered about this man of many parts — usually involving oppositions such as ancients–moderns, humanism–science, reason–faith, scepticism–dogmatism, mechanism–voluntarism — Gassendi, by acclaim, was remarkably allusive about his private beliefs, particularly those considered dangerous. Longstanding debates, some turning on the distinction between public and private, hint at the pivotal role of his personal papers. Unlike published works (so the argument goes) Gassendi's letters open a window on his private views about God, mind, freewill, and by natural extension, Copernicanism, atomism, and materialism. His letters also serve as a trace in time; they show ideas in flux and works in progress. Even Gassendi's view of atoms was not unswerving.

Tausig's *Introduction* to Gassendi's life has two apparent drawbacks. First, it is not a biography; second, in surveying Gassendi's scholarly career, it overlooks a vast scholarship. Instead of addressing longstanding issues directly or systematically, Taussig chronicles Gassendi's career by means of thematic summaries based on his *Lettres latines*. Ignoring his early life and education, the *Introduction* makes few claims to understanding "Gassendi the man" or to offering fresh insight into traditional areas of conflict. Taussig, however, makes no claim to having written a biography.

Instead, the *Introduction* provides an intelligent overview and analysis of Gassendi's career. Carefully keyed to the *Lettres latines* and arranged into seven chapters, the *Introduction* focuses on the last 34 years of Gassendi's scholarly life. As described in chap. 1, Taussig divides Gassendi's career into three major periods: 1621–37 (punctuated by two "silent years"), 1639–49, and 1650–55. The logic of the divisions is apparently based on Gassendi's loss of friends; first, the death of Peiresc (1637), and second, a series of deaths around 1649, including those of Gaultier (1647), Mersenne (1648), Luillier (1651), and Valois (1653). Given this framework, Taussig identifies a series of themes: Gassendi's entrance into the world of learning; the emergence of his publication plans; the establishment of his reputation; the expansion of his role in the Republic of Letters; and his cultivation of powerful patrons. Although these themes highlight Gassendi's ascent to European celebrity, more attention might have been paid to his troublesome legal battles, his consuming health concerns, and the cruel behaviour of his old friend, J.-B. Morin. Although the importance of key friendships are justly highlighted — Peiresc, Mersenne, Luillier, and Bernier — further focus might have been given to Boulliau, Chapelain, and the Brothers Dupuy. It may also be time for historians to focus on the personal relationship between Gassendi and Peiresc.

Although his friendships are only partly represented in his correspondence, Gassendi's letters tell us much about how his interests evolved. In chap. 2, one of the strongest chapters, Taussig offers important insight into the art, genre, and function of scholarly correspondence and the practical, philosophical, and utopian rules of engagement in the Republic of Letters. Libertinage, freethinking, and censorship are important subtexts. Often insightful, Taussig offers a shrewd analysis of early modern communication. If something is lacking, it is a clearer sense of the chronological scope and geographical distribution of Gassendi's network, perhaps in relation to those of a Mersenne, Descartes, or Boulliau. While Taussig rightly contrasts Mersenne's wide-ranging interests with Gassendi's more focused agenda, further analysis might be offered about the boundaries — geographical, political, and religious — that shaped Gassendi's network.

Chap. 3 is devoted to Gassendi's intellectual style in relation to authors ancient and modern. Trained in the classics, Taussig analyses Gassendi's citation patterns and thoughtfully calls into question his eclecticism and originality as a thinker. Chap. 4 draws on scientific themes from the *Lettres*, some relating to astronomy. These topics include Gassendi's fascination with telescopes, the comet of 1618, the size of the universe, and the problem of mathematical idealization. Perhaps most relevant for readers of *JHA*, this chapter is sometimes disappointing, particularly when too little context is supplied. Inevitably, errors and omissions appear. For example, Galileo's theory of comets is misconstrued, Gassendi's reputation-making observations on the transit of Mercury are quickly passed over, and he is wrongly credited with conducting the first falling-ball experiment from the mast of a ship. There is surprisingly little discussion of the great clash between Descartes and Gassendi (Monsieur Mind *v*. Monsieur Flesh) in the *Objections and replies*. More generally, some readers might welcome more discussion of Gassendi's views on Copernicanism, on Kepler's planetary theory, and on his extensive astronomical exchanges with Hevelius and Boulliau, which often included data and snippets of dialogue from correspondents across Europe.

If Gassendi had a single passion it was Epicurus. On this topic our hero embodied the view that books are never finished, merely abandoned. Predictably, chap. 5 on Gassendi and Epicurus is Taussig's most extensive. Here the *Introduction* and *Lettres latines* work together to tell the story "behind the book". Much like an intellectual diary, Gassendi's letters trace the evolution of his mature views while exposing his moments of doubt; uncensored, they betray his first inklings and second thoughts. An important focal point comes in the form of some 59 letters Gassendi sent to his patron, Louis de Valois (October 1641 to November 1642). Structured after his *De vita et doctrina Epicuri* and eventually published as his *Syntagma philosophiae Epicuri*, Gassendi's letters provide an advanced primer on Epicurean thought and, equally important, a working example of Gassendi's historicist views. Read alongside his *Lettres*, this chapter shows Gassendi's ideas in gestation, how his concerns with scepticism, empiricism, and language worked together where history, philosophy, and science converge.

In chap. 6 Taussig extends the issue of Epicurus to the problem of the prince and philosopher, specifically the relationship between Gassendi as pedagogue and Louis de Valois as pupil and patron. The epistolary exchange between the two is famously substantial: nearly half (over 800) of Gassendi's extant letters involve Valois. Despite the lopsided representation, Gassendi was not as close to Valois as to others, particularly Peiresc. Taussig suggests that Gassendi valued his independence, and further, that he saw Valois (who was neither dim nor unlettered) as a poor pupil. She concludes that Gassendi took Valois's failed political career to heart and, having failed as his tutor, later declined Queen Christina's overtures to join her court. But it seems more likely that Gassendi's final years were shaped by failing health and unmistakable pressure, self-imposed, to publish his life's work.

In her final chapter Taussig assesses Gassendi's views on history, on the proper use of the past and the appropriate role of the historian. As a form of research and mode of expression, history was the "lumière de la vie" for Gassendi. But as he well understood, historical writing is writ both large and small. According to Taussig, Gassendi distinguished between 'history' as philosophical or Baroque and as the daily chronicle of local and contemporary events. Living in Paris, Gassendi supplied numerous chronicles to Valois who lived in Provence; but Gassendi's local narratives, even of contemporary riots and rebellions, were largely descriptive and devoid of personal judgement. By contrast, speaking philosophically, Gassendi showed great sympathy for grander themes of war and peace, of heroic drama and Baroque spectacle. Taussig concludes that Gassendi was ambivalent about a linear or cyclic view of history. Alas, she offers little discussion of Gassendi's historicism.

Finally, on the conviction that scholarly apparatus is important, I address three sections that conclude Taussig's *Introduction*. For the record, I applaud the revival of several old-fashioned scholarly traditions — a timeline of Gassendi's life (pp. 287–92) and an appendix of short biographical sketches (pp. 293–412). Although the sketches are not without problems, they provide critical information about dozens of figures now forgotten. Given the number and esoteric content of the sketches, it is no surprise that many are dated, derivative, and not error-free. Some of these mistakes could have been eliminated by checking major figures in the *DSB* or minimized by cross-checking minor figures in the landmark sources.[1] The Bibliography also presents problems.[2] The first section ("Oeuvres de Gassendi"), apart from the *Opera omnia*, is limited to editions in French translation, and thus forfeits the opportunity to supply an authoritative list of Gassendi's original works. The second section ("Manuscrits de Gassendi") is surprisingly incomplete. Rather than attempt an updated list of manuscripts outside the *Opera*, selected items are quoted from René Pintard's published thesis (now over 60 years old), thus continuing old errors and omitting hundreds of little-known manuscripts. Finally, Section 3 ("Oeuvres des contemporains") omits books Gassendi is known to have read, including works cited in the *Lettres* themselves.[3] Section 6 ("Ouvrages consacrés à Gassendi") is stronger but with obvious omissions and numerous typographical errors.

In sum, the *Introduction à la vie savante* is a welcome addition to Gassendi studies. Despite its self-imposed limitations, it is a useful volume, consistently intelligent, and in important ways scholarly in the extreme. Read in concert with the *Lettres* *latines*, the *Introduction* offers a roadmap to the unfolding of Gassendi's thought across a variety of disciplines. Specialists may be hard pressed to identify how this volume changes our views of Gassendi; and non-specialists may feel unsteady with several interpretations, particularly given the minimalist approach to citation. On balance, the *Introduction* favours Gassendi's view of 'history' as chronicle rather than as Baroque.

*Lettres latines*

Gassendi's Latin letters (discussed more fully below) first appeared in print three years after his death, in vol. vi of his *Opera omnia* (1658). *Pierre Gassendi (1592–1655): Lettres latines* provides the first modern translation of a large portion of those letters (*from* Gassendi) found in the first half of *Opera*, vi. To be clear, Taussig's *Lettres latines* do not include letters *sent to* Gassendi, which occupy the second half of *Opera*, vi. Taussig's *Lettres latines* consists of two volumes — French translations of Gassendi's letters and commentary. In lieu of letters sent to Gassendi, brief summaries appear, as necessary, in the Notes.

The basic structure of the *Lettres latines* follows the original organization of *Opera*, vi. In presenting the translations and apparatus, Taussig has in some ways treated *Opera*, vi, as a work of literature whose format should be preserved. In practice, each letter has been assigned a Letter Number which is then keyed to the page and column of *Opera*, vi; for example, "Lettre no 307, 191a". A useful convention, this practice nevertheless disappears when an unbroken series of letters was sent to the same person. With the Valois sequences, for example, letters ("Au même") are assigned a Letter Number but lack the page and column reference (for example, nos. 201–19, pp. 262–84).[4] It should also be noted that Letter Numbers sometimes refer to an extract, incipit, or editorial note that indicates a letter is lost. While this practice is not unreasonable, some readers may feel the need for more editorial assistance.

Some editorial suggestions may be in order. Most readers would be well served if each translated letter were given a standardized heading. In addition to a Letter Number, each heading would include key particulars: sender and city; recipient and city; date (as it appears in the letter, whether Old Style, Roman, etc.); and New Style date, converted as necessary. Given the complexity of Gassendi's correspondence, the heading might also identify the parent manuscript (whether original, draft, copy, or printed version) along with locations (library, fond, folios). While headings of this kind might be considered a luxury, they would show that translations are based on authoritative texts and that manuscript sources have been identified and compared. Other information, usually best known to the editor, might also be included. The location of the sender or recipient, not always evident in the translated text, is significant and always useful. Dates present similar difficulties. During the seventeenth century a variety of competing calendric systems were at work (Julian, Gregorian, Roman, Ecclesiastical, Florentine, Pisan), not to mention problems with undated letters, corrected dates, multiple dates, and postscripts. Many of Gassendi's letters have been silently converted from Roman Style to New Style. Once again, some readers may feel the need for more editorial assistance.

On the other hand, the quality of Taussig's translations is excellent. She has done a masterful job with Gassendi's infamously difficult Baroque Latin. Correct but convoluted, Gassendi's Latin can make Kepler's look lithe and lively. A key contribution of Taussig's French translations is that they will make Gassendi more accessible and better known, and as these translations are more widely consulted, there will be pleas for more. Gassendi's intellectual talent, never in doubt, was not always perfectly expressed; Taussig has skilfully captured his sense with accuracy and grace. While these translations are no substitute for the original Latin texts, they represent a fresh avenue that surely will be well travelled.

As a trilogy of companion volumes, Taussig's *Introduction* and the *Lettres latines* themselves are buttressed by the volume of Notes. Though massive and impressive, the Notes are not without problems. Virtually the same length as its companion volume, the volume of Notes contains 609 pages and some 7491 endnotes in very small type. Traditionally designed for commentary and citation, the Notes are remarkably erudite, an essential ingredient in keeping pace with Gassendi. Surprisingly, despite notable displays of sophistication, the Notes offer few citations to other scholarly works. The undeniable strength of the Notes is the wealth of information they provide, not only on classical and philological concerns, but on a vast range of issues that span political, military, and diplomatic history, as well as science, philosophy, theology, history, and law. The Notes sometimes falter, however, when dealing with technical issues in the history of science. While some errors of fact might be noted, most involve minor misunderstandings, oversights, or limited detail. For example, readers would benefit from further elaboration concerning the Moon Illusion, Poisson's Problem (as it extends from optics to vision and "points and parts"), and the status of Gassendi's retinal image (which despite mention of the choroid is passed over in silence in notes 1782, 1948 and 3681). Elsewhere, when his *De apparente* (1652, 1658) is addressed, Gassendi's exchanges with Liceti, Naudé, and Chapelain (Aristotelians and literati) are discussed but a fourth letter, by a Keplerian, is omitted (note 1831). Readers of *JHA* may notice that slim mention is given to Gassendi's observations of the transit of Mercury (1631), with no references to guide inquiring readers. Boulliau's planetary theory, detailed in his *Astronomia philolaïca* (1645), is somewhat bungled (note 4342; *cf*. note 4872), as circular orbits are ascribed to the planets (not ellipses) and Mercury is once listed as a superior planet.

Gassendi's erudition, of course, presents difficulties for any editor. A clear strength of Taussig's Notes, signalled at the beginning of the volume, is an impressive list of Abbreviations (Notes, pp. v–x). It is perhaps telling that virtually all refer to classical authors. Any balanced review of Taussig's trilogy must underscore the new knowledge we have regarding Gassendi's classical debts. Punctilious and perceptive, Taussig's Notes demonstrate anew that the New Science was rooted in ancient texts, and that natural philosophers and classical scholars were often one and the same. Would that we had similar studies of Copernicus, Kepler, Peiresc, Schickard, Boulliau, Viviani, Halley, and Newton! Given her classical strengths, it is no surprise that Taussig's commentary is less steady on issues in science, though there are important historiographic weaknesses as well. A final concern is the absence of an Index. Most readers will quickly discover that they have a treasure but no map. A simple solution is to make all three volumes available in electronic format. A searchable digital text would eliminate the need for an Index and diminish other organizational concerns.

*Background: Gassendi's* Opera*, vi (1658)*

When Gassendi died in 1655, arrangements had already been made to publish his complete works. His last patron, Henri-Louis Habert de Montmor (1600–84, "Montmor the Rich"), assured Gassendi that his writings, the work of a lifetime, would be put into print, including a substantial portion of his Latin letters. Assisted by Jean Chapelain, François Henry, Samuel Sorbière, and Gassendi's last secretary, Antoine de la Poterie, Montmor financed a new edition of Gassendi's existing publications, edited his unpublished manuscripts, and helped prepare Gassendi's Latin letters for publication.[5] It is likely that François Bernier was also involved. Once edited, the texts were transported to Lyon, and after two years in press they appeared as Gassendi's *Opera omnia* (6 vols in folio, Laurent Anisson and Jean-Baptiste Devenet, Lyon, 1658).[6] Though he had intended seven volumes, Gassendi understood that many of his letters in Latin, and virtually all in French, would be excluded. The fate of these letters is discussed below.

Although Gassendi's *Opera omnia* represents a landmark event, it has since become an historical artifact. From the outset, the *Opera* was designed as a show-piece, a posthumous celebration of Gassendi's fame and the munificence of his patrons. Written in the universal language of learning, the Latin letters found in *Opera*, vi, were designed to demonstrate the cosmopolitan character of the Republic of Letters, a Commonwealth of Learning that defied political and religious boundaries. Few contemporaries could claim such a monument.

From a modern scholarly perspective, however, Gassendi's *Opera* is utterly outdated. Incomplete from the start, *Opera*, vi, is poorly organized, existing texts are marred by editorial errors and misprints, and the absence of an index makes navigation all but impossible. Consisting of 545 pages, *Opera*, vi, contains a total of 1204 letters divided into four sections, each arranged chronologically. As discussed above, the first section contains 686 letters written by Gassendi which, thanks to Taussig's efforts, are now in French translation. The remainder, containing letters sent to Gassendi, has not been translated. The second section includes 6 "exceptional" (re-grouped) letters from Queen Christina and her circle, all in French; the third consists of 331 Latin letters from Valois; and the last section contains 181 Latin letters sent to Gassendi from 67 correspondents.

*Gassendi's "Primal Archive"*

Although Gassendi's *Opera* represents a remarkable effort by his friends — and a rare moment in scholarly publication — little concerted effort has been made since his death to locate his remaining letters. As the leading Gassendi scholar Bernard Rochot noted, the "bulk of his [Gassendi's] extensive correspondence in French and Latin is far from entirely known".[7]

The search for Gassendi's letters begins with what we know. Gassendi's "primal archive" is a useful way to imagine what we want to know, namely, the totality of letters — originals, drafts, copies, and printed letters — that Gassendi sent or received. Patterns of exchange make it clear that many of those letters are lost, but equally important, that Gassendi's correspondence was far more extensive than currently believed. In reconstructing Gassendi's primal archive, I have identified some 1744 extant letters.[8] By any measure, this number is unexpectedly large, easily surpassing the respective tallies for Descartes, Pascal, Hobbes, Flamsteed, Newton, and even Marin Mersenne, the "Mailbox of Europe". The following overview of Gassendi's correspondence supplies historical context for his *Lettres latines* and a crude outline of what is needed for a future edition of Gassendi's Complete Correspondence.

Taussig's *Lettres latines*, as discussed above, are based on the first half of *Opera*, vi; in turn, *Opera,* vi, is based on Gassendi's autograph drafts at the Bibliothèque Nationale, Paris, NaL 2643.[9] This manuscript volume was originally entitled "Pierre Gassendi. Epistulae". As with other collections of the period, NaL 2643 is poorly organized and its content confused. In addition to competing pagination and foliation, repeated number sequences, broken chronologies, and a rash of cancellations and emendations, the content of NaL 2643 does not match *Opera*, vi. Careful comparison of the two volumes shows that each contains both more and less than the other. Most disturbing, numerous missing manuscripts have not been located, and while a careful survey of nineteenth-century sale catalogues has proven useful, it has not proved heartening.[10] Readers of either volume are not without assistance. In *Opera*, vi, omissions are usually identified with a brief editorial notice, extract, or incipit. Similarly, readers of NaL 2643 will find a smattering of annotations (dark black ink) that help link some parent manuscripts to printed versions in the *Opera*. Overall, *Opera*, vi, includes about a dozen letters not found in NaL 2643, while the manuscript volume contains an important set of French copies of Gassendi's letters to Luillier in 1632–33.[11]

The parent manuscript letters for the second half of *Opera*, vi, (letters sent to Gassendi) are also found in Paris. The bulk of the Latin letters sent to Gassendi (*Opera*, vi, 391–545) are found in NaL 1637 and most are originals. The letters in French sent to Gassendi by Queen Christina and by her circle (*Opera*, vi, 335–7) are found near the beginning of NaL 1637 and NaL 1638. The original letters of Valois to Gassendi (*Opera*, vi, 338–90), conserved and bound separately, are found in NaL 1638. Two related volumes, NaL 1635 and NaL 1636, contain important Gassendi manuscripts. The above volumes represent the parent manuscripts for most Latin letters published in *Opera*, vi.

Gassendi wrote a large number of letters in French. Although many are presumed lost, the best-known among the extant letters have been published. In addition to the Luillier letters noted above, the largest single group of French letters was exchanged with N.-C. Fabri de Peiresc, Gassendi's closest friend and first patron. Published by Tamizey de Larroque, the exchange consists of some 160 letters that are included as part of vol. iv of the *Lettres de Peiresc*.[12] Unfortunately, the selection is incomplete and the editing is shoddy.[13] Most of the parent manuscripts are found at the BN Paris in two key volumes: f.fr. 9536 contains letters from Gassendi to Peiresc[14] and f.fr. 12772 contains originals from Peiresc to Gassendi.[15] A Peiresc manuscript suggests that many letters in this exchange are lost.[16] Seldom cited, f.fr. 12270 contains some of Gassendi's earliest correspondence in French, as well as important exchanges with family members during his last years.[17] In addition, a handful of Gassendi letters in French, mostly administrative and some personal, can be found at Digne.

Gassendi's "primal archive" likely contained more letters in French than Latin. Many have yet to be located and are presumed lost. Of the numerous exchanges in French, as one example, some 40 letters of Gassendi and Boulliau remain largely unpublished. This particular exchange also suggests a general pattern; although many of Gassendi's letters have been preserved, fewer responses are extant. Similar patterns appear in Gassendi's other French exchanges, while dozens of Latin letters have yet to be identified and published, among them important exchanges with Hevelius. More generally, many of Gassendi's originals, sent out across Europe, have yet to be located, and equally disheartening, even letters from the illustrious cannot be found, including Galileo originals. They may have become meat wrap.[18]

Important questions remain about Gassendi's correspondence. What is not known is how Gassendi's letters were dispersed immediately after his death, how the publication of *Opera*, vi, may have contributed to the loss of letters, and how his manuscripts were scattered in subsequent centuries.[19] What we do know is that Gassendi merits further study. To that end, what we need are adequate charts and abscissae, a map of Gassendi's primal archive, and an authoritative edition of his Complete Correspondence.[20]

To conclude, thanks are clearly due to Taussig for introducing Gassendi to a new generation of interdisciplinary scholars. While her introduction to Gassendi's life is largely descriptive, it does provide a useful and intelligent guide to Gassendi's *Lettres latines,* and importantly, it opens new avenues of research. In the end, while Gassendi specialists will continue to base their claims on the Latin texts themselves, Taussig's French translation will doubtless direct new traffic to new topics. Drawn by the drama of Gassendi's private correspondence — often more telling than his published works — a new generation will give new meaning to the manuscripts of a dusty past.

University of Florida ROBERT ALAN HATCH

REFERENCES

1. Many of the historical figures do not appear in nineteenth- or twentieth-century biographical dictionaries and encyclopedias. One of the best sources is Louis Moréri, *Le grand dictionnaire historique* … (Paris, 1671, ..., 1759).
2. It is in seven sections: 1. Gassendi: Published Works; 2. Gassendi: Manuscripts; 3. Contemporary Works; 4. Works from Antiquity; 5. Reference Works; 6. Works about Gassendi; 7. General Works.
3. Beyond the works of Baliani, Bardi, Le Cazré, Fabri, Lobkovitz, and Morin, there is occasional reference to books known to be owned by Gassendi, among them works by Boulliau, Hevelius, and Kepler. For Kepler, as with primary texts listed elsewhere, only French translations are noted. Although some original texts are cited in footnotes, they are difficult to locate. There is no Index to the three volumes.
4. I use the term "Letter" generically, as most manuscripts letters from Gassendi published in *Opera*, vi, are autograph drafts (ADf; ADfS) not autograph originals (AL; ALS) actually sent by Gassendi and received by his recipients.
5. After his death, Gassendi left his manuscripts to Montmor. Happily, this key manuscript volume remained in the family, passing from Gassendi's nephew, also named Pierre Gassendi, to François Gassendi (1713). The volume then passed through later branches of the family and was finally donated to the BN Paris by the family Marey-Monge. See the Introduction by Bernard Rochot, *Lettres familières à François Luillier pendant l'hiver 1632–1633* (Paris, 1944). Rochot leaves no stone unturned in tracing the genealogy and provenance of this key manuscript volume.
6. Gassendi's *Opera* (Lyons, 1658) was later reprinted (Nicolao Averanio, Florence, 1727; re-paginated with minor corrections) and more recently in a facsimile reprint of the 1658 edition (Stuttgart, 1964). Gassendi's published works were epitomized in French by François Bernier, *Abregé de la philosophie de Gassendi* (Lyons, 1674–75), and recently in a facsimile reprint edition (8 vols, Fayard, Paris, 1992). Bernier's *Abregé* is not a translation; it follows Gassendi's original Latin unevenly, sometimes mistaking and embellishing his published views.
7. Bernard Rochot, "Gassendi (Gassend), Pierre", in *Dictionary of scientific biography*.
8. The trick is to locate and identify letters in a systematic way. The traditional practice is to identify all published versions, to create inventories of the major manuscript collections, and finally, to construct a working calendar of all original, draft, copy, and printed versions. Chronologically arranged, the calendar helps identify patterns of exchange; broken exchanges and gaps suggest possible locations for missing letters. This process also involves pursuing all catalogue listings, writing letters of inquiry, and conducting onsite research, which ranges from precise look-ups to informed "folio flipping".
9. NaL 2643 is not listed in the bibliography of Pintard or Taussig.
10. At least a dozen Gassendi letters appear in manuscript sale catalogues from the heyday of autograph hunters, 1840–60. About half of these letters now appear lost; others were acquired by libraries in Paris, London, New York, and Washington, D.C.
11. The series of letters in French (copies) from Gassendi to François Luillier are found in NaL 2643, ff. 55–77. The copies were published by Rochot as *Lettres familières* cited above.
12. Nicolas-Claude Fabri de Peiresc, *Lettres de Peiresc*, ed. by Philippe Tamizey de Larroque (7 vols, Paris, 1888–98). Most of the letters exchanged between Peiresc and Gassendi appear in vol. iv, 177–611, but this published exchange is incomplete. Most of the published manuscript letters are found at the BN Paris, some are scattered in public repositories, others remain in private collections.
13. Some of the editorial difficulties, but not all of the manuscript omissions, are discussed by Jean Charron, "Quelques rectifications et remarques concernant les lettres de Gassendi à Peiresc, publiées par Tamizey de Larroque dans sa collection: *Lettres de Peiresc*", *XVIIe siècle*, no. 68 (1965), 50–56.
14. The Gassendi letters are found at the BN Paris, f.fr. 9536, ff. 196r–254v, as well as in Naf 5173.

15. The Peiresc letters to Gassendi occupy the entire volume of f.fr. 12772 (211 folios). Other manuscript letters from the published exchange between Gassendi and Peiresc are found at Aix, Carpentras, and London; others, still unpublished, can be found in Digne, Florence, Vienna, and the United States. Tamizey de Larroque omitted several known letters from this exchange, perhaps because of the personal content.
16. See BN Paris, N.a.f. 5169.
17. The early letters date from 1623 from Jean Gassendi at Digne. Among the last letters are exchanges with Gassendi's family members, Catherine, his sister, and especially his nephew, also named Pierre Gassendi, who wrote during the 1650s from Avignon, Aix, and Digne.
18. Targioni Tozzetti relates an anecdote from the eighteenth century regarding one Giovanni Lami and several luncheon guests, among them the historian G. B. Nelli. After stopping at a market to purchase a fresh portion of mortadella, Lami's guests discovered their lunch had been wrapped in an original letter of Galileo. His appetite whetted, Nelli returned to the market to find still other Galilean wrappings. For this well-known tale, see J. J. Fahie, *Galileo, his life and work* (London, 1903), 427–8.
19. Some thirty years ago, in the course of doing something else, I started to take note of the Gassendi letters that showed up in my research in various libraries. If this is called serendipity, I later made systematic inquiries and on-site searches. I have since located Gassendi correspondence in some 50 archives in 11 countries.
20. The publication of Gassendi's Complete Correspondence, an important desideratum for the future, will likely appear in searchable electronic format. After a thorough search to identify all originals, drafts, copies, and printed versions, all letters to and from Gassendi, scholarly and personal, should be arranged in chronological sequence, including appended materials and selected association letters.

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## BOOK REVIEWS

# TYCHO AND HIS CORRESPONDENCE

*Bearing the Heavens: Tycho Brahe and the Astronomical Community of the Late Sixteenth Century*. Adam Mosley (Cambridge University Press, New York, 2007). Pp. xiv + 354. $99. ISBN 978-0-521-83866-5.

This book examines scientific communication during the early modern period, and focuses, in particular, on the correspondence of the Danish astronomer Tycho Brahe. Tycho has not received as much attention as others such as Galileo or Kepler; and most of the letters he exchanged with numerous learned people of his time have yet to be fully investigated. One feature of this book is to exploit this material to bring to the fore the daily life of the astronomer, the circulation of astronomical ideas and instruments, and the practices of the learned men of that period. Adam Mosley contributes to Tychonian studies through the epistolary corpus about which he shows a very deep knowledge.

Correspondence was an important component of the early modern period. The letters that circulated among different astronomers contained in-depth analyses which could shape or reshape theories and strategies of observation. In addition, these astronomers had practical problems to face, such as the transport of packages to their correspondents, and the burden of social etiquette to respect.

Mosley especially examines a selection of the *Epistolae astronomicae*, published in 1596, exchanged between Tycho and Christopher Rothmann, the official mathematician of the Landgrave of Hesse. Thanks to a very accurate analysis of the humanistic letter (divided into five parts: *salutatio*, *captatio benevolentiae*, *narratio*, *petitio* and *conclusio*), he tries to conjure up the social network in which the scholars were involved, the issues and the theories they debated, their relations with their patrons, as well as their disputes and conflicts. Mosley taps the wealth of information contained in this correspondence, which thus enables him to embrace the history of science simultaneously on several fronts.

Because the *Astronomical letters* were published in 1596, the question of the printing press and its meaning is raised. Tycho set up his own printing press about 1585; as a result, he has often been regarded as a hero of both the scientific and the printing revolutions. The point here is not, however, to assert once again the impact of the printed book on the emergence of astronomy, but rather to correct the exaggeration of the use of printing on the image of the astronomer. Mosley contends that by printing his letters, Tycho did not seek commercial profit; rather, he sought to reach a greater number of readers. To that end, the astronomer managed to enter the book trade networks in order to have his correspondence delivered or to get the relevant equipment for his presses. Tycho's printed *Letters* relate moreover to the Ursus affair. The imperial astronomer had been accused by Tycho of plagiarism. In this particular conflict, publishing the *Letters* was a good means for Tycho to become regarded as the father of the geo-heliocentric world system. Mosley also discusses the didactic role of the *Letters*, showing that Tycho intended his *Letters* to become a reference work for astronomy students.

In the last part of his book, Mosley presents the symbolism of astronomical instruments, and how they were produced and put into circulation. He does not limit his research to treatises such as the *Mechanica*, but delves into the scientists' correspondence for it provides more information on the instruments — their number, where they were located in Hven, and their exchanges between the other astronomical centres. The principal subject of the last part deals with the communication of knowledge, data and theories through the transfer of instruments. Adam Mosley's book recalls the works of Collins or Shapin and Schaffer on the transfer of technology. To illustrate his study, the author refers to very practical examples, such as the Blaeu cartographers, who were involved in the construction of the Tychonic astronomical globe.

Mosley thus proposes to consider the history of communication as a significant part of the history of science, in that it encompasses the transmission and evolution of techniques, as well as the sharing of data and ideas. *Bearing the heavens* provides a new perspective on the Danish astronomer and is definitely worth the reading.

Université de Paris X GÉRALD PÉOUX

# THE INVENTION OF THE TELESCOPE

*Galileo's Glassworks: The Telescope and the Mirror*. Eileen Reeves (Harvard University Press, Cambridge, MA, 2008). Pp. 240. $21.95. ISBN 978-0-674-02667-4.

News that a Dutchman had made an optical device that magnified distant objects reached Venice in November 1608. Galileo began the experiments that resulted in his version of the telescope in the early summer of 1609, when, he said, he first heard (or received useful information) about the Dutch invention. By the end of the year he had completed his observations of the Moon and early in 1610 he discovered the satellites of Jupiter. The splendour of this discovery and its time value in the patronage game recommended prompt publication; *Sidereus nuncius*, in which Galileo described the motions of the satellites and gave them, as the "Medicean stars", to the Grand Duke of Tuscany, appeared in March.

Eileen Reeves, professor of comparative literature at Princeton, asks why ten months elapsed between the arrival of the initial report in Venice and Galileo's first experiments with telescopic lens combinations. The answer, according to Reeves, is that Galileo and his friend Paolo Sarpi, who heard the first reports bruited in Venice, dismissed them as natural-magical boasts.

The great interest of Reeves's book lies in her reconstruction of the natural magic in question, which made use of a combination of a concave mirror as objective and a concave lens as eyepiece. According to a story as enduring as the legend of Archimedes and the Roman fleet, a device incorporating a mirror enabled observers at the Pharos of Alexandria to spy on actions taking place hundreds of miles away. The story, which Reeves retraces in detail, helped natural magicians of later periods to believe (or pretend to believe) that with mirrors sufficiently large and true they could duplicate the feats of the voyeurs of Alexandria.

Reeves credits reports of some telescopic effects that the natural magicians claimed to have achieved using undisclosed arrangements of mirrors. The argument of *Galileo's glassworks*, however, requires that Galileo and Sarpi reject these claims. For it was just their doubts about standard stories of magnification that, Reeves says, caused them to dismiss the Dutch telescope until informed that it did not employ mirrors.

In February 1609, before his energetic engagement with the Dutch device, Galileo hinted at a cupboard full of great inventions in the making. Although this inventory, which he drew up in natural-magical style to support his negotiations for a position at the Medici court, does not mention catoptrics, Reeves gives reasons to suppose that Galileo had some optical magic up his sleeve, indeed, a Pharos device of mirror and lens. (Does this conclusion not undercut the argument that suspicion of such devices delayed Galileo's engagement with the Dutch telescope?) Reeves supports her guess with another: Galileo had written a dedication giving the Medicis the myriad of stars visible through the telescope in the constellation Orion before he discovered the better present of the satellites of Jupiter. Techniques of literary criticism allow her to identify the ghosts of this premature effort in the definitive dedication of *Sidereus nuncius*.

The ghosts haunt the familiar rhetoric in which Galileo itemized the ways people memorialize heroes. Working upward from the most transient, Galileo mentions marbles, bronzes, statues, columns, pyramids, and cities named for those most worthy of everlasting commemoration. All these decay eventually. More enduring, sometimes, are written records, which, however, also perish. The most certain and secure vehicle of everlasting glory is a star — a planet for a Saturn or Venus, a constellation for a Perseus or Hercules. Fortunately for Cosimo, Providence had caused his would-be courtier to discover new planets for the Medici. Reeves interprets Galileo's recital of monumental genres from marbles to stars as veiled hints at ancient optical magic. For to what could the columns and pyramids, the city named for a great hero, and the destruction of books refer but Alexandria? And is it not clear that Galileo's explanation of the need for monuments — "such is the condition of the human mind that unless continuously struck by images of things rushing to it from the outside, all memories easily escape from it" — points to a device like the camera obscura and, by association, to Alexandria, to the Pharos?[1]

Among those who manipulated Pharos devices effectively were Jesuits and devils. Reeves gives examples of Jesuits. I can supply a devil. The commentary on the Evangelists by the very learned Spanish theologian Benito Arias Montano includes an elucidation of the temptation of Christ as recorded in Luke iv:5. We read, "And the Devil, taking him up into a high mountain, showed unto him all the kingdoms of the world in a moment of time." How the devil, asks Arias Montano, who was something of a natural magician, did the Devil do it? How did he manage to show Christ all the earth at once? "Hoc potuit effici prospectivae sive opticae artis vi, quam diabolus non ignorat; ut eadem arte à nobis conficiuntur inspicilla, quae longissimè distantes res oculis exactissimè subiicunt."[2]

This text dates from 1575. It was written in the Spanish Netherlands, where Arias Montano supervised the printing of the famous polyglot bible subsidized by his master King Philip II of Spain. Should we suppose a Dutch telescope before the Dutch telescope? A Pharos device? Some not-so-natural magic? At a minimum, Arias Montano's commentary suggests that a belief in effective telescopic devices existed before 1600 among the extensive and widespread readership of books published by the Plantin press, and that historians of early modern science should spend more time with their bibles.

University of California, Berkeley/Worcester College, Oxford J. L. Heilbron

REFERENCES

1. Reeves, *Glassworks*, 143; the passage from *Sidereus nuncius* comes from Albert van Helden's translation (Chicago, 1989), 29, which Reeves uses.
2. Benito Arias Montano, *Elucidationes in quatuor evangelia* (Plantin, Antwerp, 1575), 192, note e. "He could have done this with the help of the perspective or optical art, which the devil knows; as by the same art we make *inspicilla*, which bring very distant objects very exactly before the eyes."

## ASTRONOMY AS A MODEL?

*Astronomy as a Model for the Sciences in Early Modern Times*. Edited by Menso Folkerts and Andreas Kühne (*Algorismus* 59; Dr. Erwin Rauner Verlag, Augsburg, 2006). Pp. xviii + 498. €27.50. ISBN 978-3-936905-22-9.

This admirable volume comprises marginally revised versions of twenty-nine papers that were first presented in March 2003 at a symposium held at one of the leading European centres for the history of science, that at the Ludwig-Maximilians-Universität in Munich. The title of the symposium, originally phrased in German, ended in a question mark that the editors of the volume have dropped with some optimism. While not all of the contributors addressed the question directly, some of those who did so have shown how one might begin to demonstrate that astronomy was indeed a model for the sciences. More specifically, speakers were asked to consider the situation only after Regiomontanus. Since his death was just three years after the birth of Copernicus, those who want to follow the well-worn — but still not fully explored — historical path leading up to the early modern period had plenty of scope. An interesting aspect of recent research along that path has been the attention paid to individual schools and scholars previously regarded as somewhat off the beaten track. Fridericus Amann (d. 1465), the subject of Armin Gerl's paper, is one of several individuals about whom we learn much that is new, and the kind of things Amann did — in calculation and instrumentation — were certainly productive of a sound scientific mentality. There are very many other instances in the volume about which this could be said, and there would be no point in trying to list them all in a review, but it must be said that very few authors attempted to show systematically how the imputed influence came about, or could have come about, or failed to come about, or what precise form it took that gave it its value.

One could never say that Prague was off the beaten track, but Alena Hadravová and Petr Hadrava throw much interesting new light on what was going on there, and short as is their paper, they — like Gerl and a few others — remind us of the existence of the Middle Ages, and of the occasional need to break out of the historical compartments decreed by symposium organisers. Their passing mention of Tycho's planned expedition to Alexandria connects nicely with the introductory chapter by Sonja Brentjes, on early modern encounters across the Mediterranean Sea. She has numerous examples of the rather random movements of scientific ideas, as Catholic and Protestant, Ottoman and Safavid, and scholars of many other persuasions, jostled each other in their various searches. But searches for what? They were searching for so many different things that the bearing of her many fascinating examples on the theme of the symposium is again not easy to spot, and she confesses at the end of her extremely enjoyable paper that "astronomy appears to have functioned rather as a guide for orientation in cultural and social spaces than as a guide for acquiring pure knowledge". She is speaking, of course, only of her own *dramatis personae*.

The search for influence of the kind being sought comes closer to success with the several papers touching on the mathematics of astronomy, but there we rarely find authors prepared to go further than setting out mathematical details — perhaps because they were thought interesting enough in themselves. Mieczysław Markowski takes a bolder line, in a short but thoughtful paper presenting astronomy and astrology in pre-Copernican [*sic*] Cracow as a *Leitwissenschaft* for advances in a wider scholarly world. Michael Segre is likewise unafraid to mention the part played by astrology, citing Michael Polanyi's dictum that science could not avoid tradition, with all its irrational baggage. That idea, I imagine, was not in the minds of the symposium organisers when they phrased their question, but several other contributors show the importance of astrology to it. Richard Kremer does so in a thorough way, by focusing closely on the relevance of annual practica — by Achilles Pirmin Gasser and Joachim Heller — to the spread of Copernicanism. An important subtext to his chapter is the question of an increasing acknowledgement of the importance of accurate calculation. Another line of influence with a bearing on the symposium theme is that which was effected through what Owen Gingerich calls "the invisible astronomical network" after 1543. He has in mind, of course, an analogy with what Robert Boyle referred to as an Invisible College of natural philosophy, and presents the case for an astronomical network primarily through the medium of his study of the fortunes of Copernicus's *De revolutionibus*, the subject of his two well-known books on the subject.

Theory was not the only inheritor of the Renaissance astronomical tradition. Instrumentation during that period made great leaps forward in the West, and several authors touch hesitantly on the ways in which other physical sciences benefitted as a result — through telescopic optics, for example (Sven Dupré), and through the work of those whom E. G. R. Taylor called "mathematical practitioners", with their influence on navigation, geodetics, and cartography. Fernand Hallyn treats the last subject obliquely, through its repercussions on humanism, Uta Lindgren more directly, by looking at geodetic instruments. Yaakov Zik pursues a relatively original and interesting line, closer to the purpose of the volume, analysing Kepler's discussion of the problems instrumental observers faced, and proposing ways in which instruments in general could be refined in principle.

Since the book is arranged more or less chronologically, it is to be expected that the influences that are sought will be more conspicuous towards the end. Robert Hatch, for instance, is able to discuss Newton's inverse-square law and its roots in optics and astronomy, drawing on the writings of such astronomers as Kepler, Borelli, and Bouillau. Andreas Verdun takes things a stage further, considering the development of many of the methods of the modern exact sciences in the astronomical writings of Leonhard Euler. We are so used to hearing that the astronomically inspired Newtonian model — one that finally replaced a kinematical with a dynamical astronomy — was of supreme importance, after his time, for the mathematical modelling of the physical sciences more generally, that it is refreshing to come across a change of emphasis, placing the vital turning point in eighteenth-century mechanics and astronomy. There are many ways of deciding this question, however. Verdun argues his case by focusing on the complexity and richness of Euler's many new physical concepts, and on his analytical and notational advances, many of them still highly conspicuous in modern science. Those whose feathers are ruffled by what they might interpret as an attempt to play off one period of history against another might well reply along the lines that the acorn is not inferior to the oak. Verdun's essay, in short, will not please everyone, but I would have given him first prize for keeping the initial symposium question uppermost in his thoughts. The book is well worth buying, however, even for the contributions of those who did not.

Oxford JOHN D. NORTH

## *HARMONIA MACROCOSMICA* REPRINTED

*Andreas Cellarius, Harmonia Macrocosmica of 1660: The Finest Atlas of the Heavens*. Introduction and texts by Robert H. van Gent (Taschen, Cologne, 2006). Pp. 240. $150. ISBN 978-3-8228-5290-3.

What is Andreas Cellarius's *Harmonia macrocosmica seu Atlas universalis et novus*, published by Johannes Janssonius in 1660 and reprinted in 1661? The subtitle of Robert van Gent's stately volume, *The finest atlas of the heavens*, implies a twofold answer to this question. On the one hand, the *Harmonia macrocosmica* can be thought of as a book about astronomy, even if it fails to incorporate some of the most important developments of seventeenth-century astronomy (which Cellarius may have intended to consider in a subsequent book). Because of this, the work might well have been forgotten as time went by, despite Ernst Zinner's contention that it had had an important impact on the diffusion of Copernicanism. On the other hand, the *Harmonia macrocosmica* is splendidly illustrated and contains twenty-nine double-sided plates showing the Ptolemaic, Tychonic and Copernican world systems and maps of the heavens, which reflect the history and development of astronomy. Together with the frontispiece, these exquisite images, which in many copies were hand-coloured, turned the volume into a prestigious object to be displayed in parlours to impress visitors. Indeed, ever since their original publication in 1660, the illustrations in the *Harmonia macrocosmica* have been so popular that they have been frequently reprinted without the text. This kept Cellarius's name alive, and prompted Taschen, a company that specializes in producing lavishly illustrated coffee-table books, to publish the huge volume under review.

Measuring 32 × 53cm, Taschen's edition is larger than the original folio and has considerable physical weight. Like the earlier reprints, it contains facsimiles of the *Harmonia macrocosmica*'s frontispiece and plates, but does not include reproductions of the text. Robert van Gent's commentary begins with a short introduction, which covers the history of celestial atlases, celestial globes and world systems from Antiquity to Cellarius's times, and includes a brief description of the *Harmonia macrocosmica* and its printing history. This is followed by a discussion of the frontispiece and the plates, including their astronomical and iconographical content. The book presents three appendices: an overview and summarized history of the constellations depicted in the plates, the names of the stars, and a glossary of astronomical terms. The edition concludes with a short biography of Cellarius, about whom not much is known, and a fragmentary bibliography. All the texts appear in English, French and German.

This is an attractive edition, but historians of astronomy were not necessarily in desperate need of it since many copies of Cellarius's work are available, including an online-version (www.lib.utah.edu/digital/splash.php?CISOROOT=/Cellarius). But Robert van Gent has done a very good job of making Cellarius's work accessible to a more general public, and the merits of this should not be underestimated.

Johannes Gutenberg Universität Mainz VOLKER R. REMMERT

## EPICYCLES AND ECCENTRICS IN THE MIDDLE AGES

*Studies in Medieval Astronomy and Optics*. José Luis Mancha (Variorum Collected Studies Series, C852; Ashgate, Aldershot, 2006). Pp. xxii + 338. $120. ISBN 978-0-86078-996-3.

This volume assembles the substantial publications of the historian of astronomy and optics José Luis Mancha. As the author explains in the preface, each article in the volume is connected, often via the work of al-Biṭrūjī (*fl.* 1200) and Gersonides (d. 1344), to medieval astronomers' rejection of epicycles and eccentrics (p. vii).

According to quotations from Aristotle's *Metaphysics* and Simplicius's commentary on Aristotle's *De caelo*, the first astronomer to devise models of homocentric orbs, without epicycles and eccentrics, to account for available celestial observations was Eudoxus (*fl. c.* 370 B.C.). In the eleventh century, Ibn al-Haytham (d. *c.* 1040) proposed what appeared to be a Eudoxan couple of two orbs, one enclosing the other, as part of a mechanism to cause the planets' motion in latitude.[1] The absence of any real cause for the motion in latitude had been a lacuna of Ptolemy's *Almagest*. Mancha, in "Ibn al-Haytham's homocentric epicycles", concluded that Ibn al-Haytham's work might be the source for the homocentric models in *A treatise concerning the refutation of the eccentrics and the epicycles* by Henry of Hesse (d. 1387) and in Julmann's *Tractatus de reprobationibus epiciclorum et eccentricorum* (composed 1377) (VIII, p. 73).

Eudoxan ideas continued to surface in medieval astronomy. B. R. Goldstein's scholarship on al-Biṭrūjī (*fl. c.* 1200) found that al-Biṭrūjī's models were essentially Ptolemaic models placed on the surface of a sphere, and depended on Ibn al-Zarq_llu and other writers on trepidation. But the article entitled "Al-Biṭrūjī's theory of the motions of the fixed stars", argued that al-Biṭrūjī's model for the motion of the fixed stars was in fact Eudoxan, as E. S. Kennedy had first suggested (XI, pp. 143–4).

Mancha's "Right ascensions and hippopedes: Homocentric models in Levi ben

Gerson's *Astronomy*" shows that the *Astronomy* of Gersonides (d. 1344) included a detailed description (and rejection) of Eudoxan homocentric models to explain the motions of the planets in longitude and anomaly (VII, pp. 264–5). This text, in part, was available in Latin during Gersonides's lifetime, well before the late fifteenth- and sixteenth-century homocentric astronomies of Regiomontanus, Amico, and Fracastoro. Given F. J. Ragep's observation[2] that an astronomer as early as Ibrāhīm Ibn Sinān (d. 946) was aware of Eudoxan models, one might ask why al-Biṭrūjī, if his model for the motion of the fixed stars was Eudoxan, took a different approach in his homocentric models for the planets' motions. In light of recent scholarship[3] proposing new understandings of how Eudoxus could have been understood, Mancha's work tells us how pre-modern astronomers actually understood Eudoxan theories. Homocentric models *in toto* could be seen as part of a broad tradition of criticism of Ptolemy.

"The Latin translation of Levi ben Gerson's *Astronomy*" found, by examining variations between the Latin text and the Hebrew, that Gersonides worked with the Latin translator of the *Astronomy*, Petrus of Alexandria, on a translation from a Provençal intermediary that Gersonides had already produced (III, pp. 13–14). Mancha dated the Latin translation to the last few years of Gersonides's life (IV, pp. 15–18) and wrote more about Gersonides's relationship with Christian scholars in "Levi ben Gerson's astronomical work". Another article, "The Provençal version of Levi ben Gerson's Tables for Eclipses", showed how the Provençal version, being earlier than the extant Hebrew version of the tables, lends insight into Gersonides's intellectual biography (VI, pp. 269–73).

Because Gersonides chose to use epicycles and eccentrics, his determination of the parameters and dimensions from observations involved multiple observations and successive approximations. "Approximation procedures in Levi Ben Gerson's astronomy" focused on Gersonides's own analysis of heuristic reasoning as he determined, for example, the dimensions and parameters of the model for Saturn (V, pp. 22–30). Gersonides knew that he had the correct answer only when his observations verified his theoretical starting point. This article is intriguing because Gersonides was aware of how he had to rely on inductions from experience for information that, in a homocentric cosmos, could be more easily demonstrated (V, pp. 15–16).

Gersonides's development of an instrument, the Jacob's Staff, with a pinhole aperture, led Mancha to investigate work on pinhole images in two texts that preceded Gersonides's *Astronomy*. "Egidius of Basiu's theory of pinhole images" treats the production of circular images via angular apertures; "Astronomical use of pinhole images in William of Saint-Cloud's *Almanach planetarum*" considers a use of pinhole images to measure the solar diameter.

Finally, a laudable characteristic of Mancha's work is the presence of passages, with translations and analysis, from primary sources. Even a reader who questioned Mancha's conclusions would still be able to learn and develop new interpretations from these articles.

Bowdoin College ROBERT MORRISON

REFERENCES

1. F. Jamil Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy: al-Tadhkira fī 'ilm al-hay'a* (Berlin, 1993), 451–3.
2. *Ibid.*, 452.
3. E.g., Ido Yavetz, "On the homocentric spheres of Eudoxus", *Archive for history of exact sciences,* lii (1998), 221–78.

## COSMOLOGY DOWN THE AGES

*Conceptions of Cosmos, From Myths to the Accelerating Universe: A History of Cosmology.* Helge S. Kragh (Oxford University Press, Oxford, 2006). Pp. 276. £35. ISBN 978-0-19-920916-3.

Here is a very workmanlike review of ideas about the cosmos from Antiquity to the present. The publishers state that "It presents cosmology as a subject including scientific as well as non-scientific dimensions, and tells the story of how it developed into a true science of the heavens. Contrary to most other books in the history of cosmology, it offers an integrated account of the development with emphasis on the modern Einsteinian and post-Einsteinian period".

The first half of the book is devoted to the pre-relativistic epoch, starting with myths and creation stories of ancient Egypt and Mesopotamia and ending with the Copernican revolution, Newtonian ideas about cosmology, and the beginnings of an understanding of the true scale of the universe. The second half covers the epoch of relativistic cosmology from Einstein to the present, including the inflationary universe idea and the recent discoveries of dark matter and the acceleration of the universe. Kragh maps the transition from cosmological myth to evidence-based science and physical explanation, and concludes with more philosophical speculation. It is very useful to have collected in one place such a synoptic overview of cosmological theories, as it is easy for today's cosmologists to be ignorant of the larger context of thought in which their work takes place.

The very recent epoch is necessarily covered in rather sketchy fashion, with a variety of alternatives presented but in a rather non-critical style; for example variable speed-of-light cosmologies and cyclic universes are both problematic, and this is not well reflected in the text. Nevertheless Kragh offers sufficient material for those interested to be aware in broad-brush outline of many of the main cosmological proposals being made at present. Some significant ideas are however omitted, for example the Randall-Sundrum brane-world notion and the loop quantum cosmology programme of Bojowald and others.

Three things are of particular interest in this survey. First, some old themes recur through the ages. Did the universe have a beginning? If so what was there before (insofar as that question has a meaning)? Is the universe infinite in space? Will it have an end? Are there one or many worlds? These have been topics of speculation and dogmatic statement for thousands of years. And we still don't know the answers.

Second, huge progress, with various new themes coming into play, has been essentially enabled by advances in experimental and observational technology. The development of observational testing together with physical theories led to the discovery, successively, of the size of the observable universe, its expansion, simple models of the physical evolution of the universe as a whole, the way gravity leads to development of astrophysical structures, the way nuclear physics leads to a theory of element formation, and the way particle physics provides clues to the evolution of the early universe. All this is an impressive and coherent, evidence-based development.

A third theme is how resistance to many of these changes, even from scientists, held back understandings. For example, the idea of a static universe delayed understanding of the expanding universe for a decade. And the prediction of the existence of blackbody cosmic background radiation (as a result of examination of the thermodynamics and element formation in the early universe) was essentially ignored for fifteen years.

What is perhaps not made so clear in the book is the way that we are now again entering an era of cosmological myth, but this time of scientific rather than religious mode. By *myth*, I mean an explanatory story or theory that gives a means of understanding what happens but remains hypothetical rather than proven. It is not uniquely supported by empirical evidence; indeed, it may not be supported by any evidence at all. The multiverse idea is one major current example, another is the statement that physical infinities really exist in the physical universe, and yet others are various theories of creation of the universe 'out of nothing'. The first two are unprovable, and the third rely on as yet unclarified ontological assumptions about where or how the massive machinery of quantum field theory that underlies these explanations exists in some form pre-existent to the origin of the universe. That supposed pre-existence is not in any way testable. The overview presented in this book helps put these myths into proper historical and philosophical perspectives, which may be useful for their future development.

University of Cape Town GEORGE ELLIS

## ISLAMIC INFLUENCE ON COPERNICUS

*Islamic Science and the Making of the European Renaissance*. George Saliba (MIT Press, Cambridge, MA, 2007). Pp. xii + 315. $40. ISBN 978-0-262-19557-7.

Much attention is currently being given to the achievements and influence of medieval Islamic science. This book presents two major arguments. The first (original to the author) concerns the motivations for the ninth-century translation movement during which Greek science was rendered into Arabic. The second (building upon the work of earlier modern scholars such as E. S. Kennedy, Otto Neugebauer, and Noel Swerdlow) presents a case for the influence of thirteenth- to fifteenth-century

Islamic astronomers on Copernicus.

A remarkable program of translation occurred in Baghdad during the ninth century during which nearly all of the major scientific and medical Greek writings were rendered into Arabic, including the *Almagest* of Ptolemy. Not only were such treatises translated but immediately the data in the texts were modified. For example, the value ascribed to the precession of the equinoxes was corrected from Ptolemy's 1° per 100 years to 1° per 66 years or 1° per 70 years. The motion of the solar apogee (considered fixed by Ptolemy at 5;30° Gemini) was found to have moved eleven degrees by the early ninth century. For observation of the solar apogee, a new technique unknown to the Greeks was devised: observation of the daily declination of the Sun at the midpoints of the seasons. The value of the inclination of the Earth's axis was recalculated from 23;51,20°, as given in the *Almagest*, to either 23;33° or 23;35° depending upon which authority you read. These revisions — not just translations — were already reflected in tables compiled in Baghdad for the caliph al-Ma'mūn (reg. 813–33).

Saliba argues here that this very early activity of refining the values given in the Greek texts clearly indicates that science, in this case astronomy, was already fairly mature at the time the translations from the Greek were made. He contends that mere translators of entirely new material would not know how to go about verifying the data and procedures, much less be able to develop new techniques for use in critically checking them. And indeed virtually all of the early translators into Arabic were also scholars of considerable originality in their own right.

Saliba then intriguingly suggests that the impetus for the translation movement came from members of the government bureau of revenue (*dīwān*), who were already skilled in arithmetical and geometrical procedures (particularly surveying) and methods of computing solar years, but who needed to expand their skills and knowledge so as to maintain their dominance in the bureaucracy. He persuasively argues that fields of knowledge had in this instance become "tools of political power" (p. 77).

The second major argument of the volume gives rise to the title *Islamic science and the making of the European Renaissance*. In essence Saliba argues that: (1) prior to Nicolaus Copernicus (d. 1543) there was no tradition in Europe of criticizing and changing the mathematical models employed in Ptolemaic astronomy; (2) there was, however, a continuous and vigorous tradition in the Islamic world of doing just that; (3) Copernicus employed geometrical and diagrammatic techniques that appear identical to those originated by late medieval Islamic astronomers; and (4) therefore Copernicus must have had access to these Arabic works even though there is no evidence available today that they were translated into Latin.

There is no doubt that a number of medieval Islamic astronomers refined the mathematical models employed in the astronomy of the day, and many were adamant in their rejection of some geometric techniques (equants, deferents, eccentrics) employed by Ptolemy to account for varying angular speeds and latitudes of planetary orbits. Their objection to these geometric devices (which Saliba repeatedly calls "absurdities") was that they lacked consistency and violated the Aristotelian principle of uniform circular motion. The criticism of Ptolemy's geometry and the invention of new mathematical techniques employing combinations of circles each with uniform circular motion is particularly evident in the work of Naṣīr al-Dīn al-Ṭūsī (d. 1274) and his colleague at the Maragha observatory in northwest Iran, Mu'ayyad al-Dīn al-ʿUrḍī (d. 1266) as well as Ibn al-Shāṭir (d. 1375), a time-keeper at the Umayyad Mosque in Damascus, ʿAlī al-Qūshjī (d. 1474) working at the Samarqand observatory, and Shams al-Dīn al-Khafrī (d. 1550).

Ultimately, according to Saliba, there was so much questioning of the underlying mathematical principles in Ptolemaic astronomy that a foundational shift became evident in thirteenth- to fifteenth-century Arabic writings, with the "new astronomy" (as Saliba has termed it) rejecting many of the principles of Ptolemaic astronomy. Saliba's "new astronomy", however, should not be confused with the heliocentric astronomy of Copernicus and his supporters, which earlier historians often have termed "the new astronomy".

Regarding the influence upon Copernicus of the work of these Islamic astronomers, it should be kept in mind that none of them proposed the paradigm shift of placing the Sun at the centre rather than the Earth. Although critical of the failure to maintain uniform circular motion in the modelling, Islamic astronomers made no criticisms of the basic Ptolemaic geocentric scheme; they could, in fact, be viewed as being very conservative in their approach in that they wished to return to complete compliance with the Aristotelian view of perfection in the circle.

There was, however, undoubtedly a long, creative, and continuous tradition of Islamic theoretical astronomy. It is unclear how much this activity was driven by discrepancies between observed data and the predictive ability of models, and how much by the intellectual need to maintain circular motions and a desire for an agreeable mathematical model. As for the hypothesis that there was a causal link between the activities of the later Islamic astronomers and the development of Copernican astronomy, it remains only a hypothesis until the mechanism for such borrowing can be found. Yet the evidence is mounting for some form of connection, especially given the sudden appearance in Europe of technical geometric innovations that had a centuries-long tradition in Islam.

The volume is unfortunately flawed by tiresome repetitions and numerous spelling errors (examples of the latter being "Liones" instead of "Leonis", p. 80; "equinoxial" and "solsticial" for "equinoctial" and "solstitial", p. 82; "lied" instead of "lay", pp. 111 and 120). The volume would have profited from careful and judicious editing. Throughout it is evident that Professor Saliba has an agenda, which is to reveal the importance of late medieval Islamic astronomers to the development of European astronomy. In making his case, however, he has felt the need on occasion to overstate certain points, an example being the statement (p. 112): "By the beginning of the sixteenth century, no self-respecting astronomer would have continued to uphold the long-discarded and obsolete astronomy of Ptolemy." As always happens in such instances, these exaggerations tend to detract from a argument well worth consideration. A more dispassionate examination of the issues can be found in a recent study by F. J. Ragep in which he draws attention to the potentially revolutionary suggestion (overlooked by Saliba) made by the fifteenth-century astronomer ʿAlī al-Qūshjī that there was no need for astronomers to adhere to Aristotelian physics and uniform circular motion ("Copernicus and his Islamic predecessor: Some historical remarks", *History of science*, xlv (2007), 65–81).

One of the great values of this volume is that it argues, convincingly and passionately, that scientists and scholars in the Muslim world remained creative, original, and productive well into the sixteenth century, often at times when Europe was intellectually quiescent. Those of us who work in the history of Islamic science and medicine have long been aware of this fact, but many remain convinced that Muslim scholars did little after the twelfth century, even asserting that what these scholars did do was merely to pass on to Europe the earlier Greek science. This book should surely lay that myth to rest at last.

University of Oxford EMILIE SAVAGE-SMITH

## CALENDRICS IN THE ANCIENT NEAR EAST

*Calendars and Years: Astronomy and Time in the Ancient Near East.* Edited by John M. Steele (Oxbow Books, Oxford, 2007). Pp. 176. £25. ISBN 978-1-84217-302-2.

In 1975, Otto Neugebauer remarked that "historical chronology rests on an interplay of theoretical astronomy and historical conditions, far more intricate than professional historians usually realize — to the great detriment of their insight into the very foundations of their field".[1] The papers in this volume, originally presented at the 2005 Notre Dame workshop and edited by John M. Steele, address many of the intricacies of ancient Near Eastern (Mesopotamian and Egyptian) calendrics, attesting to the fact that the conversion of ancient to modern dates (e.g., ITI.AB 24 MU 13 ᴵDariamuš = 24th Tebetu year 13 Darius II = 12/13 Jan. 410 B.C.) rests not only on the establishment of a workable correlation of various ancient calendars with our own but also on an understanding of the many aspects of chronography that underpin them.

Two papers concern Egyptian calendrics. Sarah Symons analyses the diagonally arranged decan tables found on the inside of coffin lids from the IXth to the XIIth Dynasties and on the Abydos ceiling of the XIXth Dynasty Osireion, and adds to this corpus eight additional sources beyond the thirteen included in Neugebauer's and Parker's *EAT*, vol. i. She offers a new reconstruction of the list of decans and their order as well as a new typology (T and K Tables), and discusses the 365-day Egyptian civil year that underlies the tables. She rejects the old term 'diagonal star clock' on the grounds that the decan tables do not tell time at night but indicate parts of the night (called "hours") by means of stellar appearances, which, with the passage of years, do not represent actual situations as they require periodic and regular revision to be practicable. L. Depuydt seeks to clarify the foundations of the modern model for Egyptian chronology based on the consistent use of the 'wandering' (with respect to the seasons and the rising of Sirius/Sothis [Spdt]) 365-day calendar and provides a broad sweep of the historiography of the 365-day year from ancient Egyptian evidence of civil months (in Djoser's Step Pyramid) to Ideler's nineteenth-century work on ancient chronological systems.

The section on Mesopotamia consists of four papers of broad scope, dealing with calendrical systems attested in cuneiform sources from the Archaic period to the Seleucid Babylonia. Lis Brack-Bernsen shows that both cultic and civil calendars (where cultic events take place in relation to special days in the lunar cycle) and an administrative calendar (where each month had 30 days, not based on lunar phases, but which facilitated calculations and bookkeeping) were in existence from the earliest of historical times (Early Dynastic III, *c*. 2600–2300 B.C.), with evidence for the administrative calendar and its artificial year of 360 days going back as far as the Protoliterate period (3200 B.C.). Following Ur III times (*c*. 2000 B.C. onward) the lunisolar calendar is in evidence for dating documents, with the need for intercalation every three years on average (regularized by adoption of the 19-year cycle in the late Persian period). In addition, she argues that the old administrative calendar, based on 360 days, was continued as an ideal calendar used in astronomical schemes and calculation.

W. Horowitz focuses on the Babylonian "astrolabe" texts that divide each of twelve months into three parts defined by the rising of certain stars (and a few planets) in different parts of the sky ("roads" of the gods Anu, Enlil, and Ea), creating thereby a system of thirty-six stars to define the months of a schematic year. He discusses the textual development of the astrolabe tradition, beginning in the Kassite period and continuing into the first millennium, with exemplars from Neo-Assyrian and Late Babylonian periods. He argues for an internal relation between the "Astrolabe B" tradition and the Babylonian Creation Poem *Enuma Eliš*, both of which were composed *c*. 1100 B.C., and sees theological reasons for this intertextual relation relating to the exaltation of Marduk.

John P. Britton's centrepiece summary of the history of Mesopotamian calendars, intercalation practices and year-lengths brings together heretofore scattered materials (especially in the particularly welcome section on year-lengths). He sees the administrative calendar referred to by Brack-Bernsen as a bridge between civil and schematic calendars, a convenient accounting convention more than a calendar as it did not reckon time in the sense we normally attribute to that word. The historical development he traces of the intercalary schemes that end in the adoption of the 19-year lunisolar period relation 19 years = 235 months will probably be the last word on this issue for some time.

John Steele takes up the centrally important question of month length in the Babylonian calendar, which he tracks from the Neo-Assyrian through to the Parthian Periods. His research shows the consistent use of a calendar in Mesopotamia over the course of its 3000-year history which used the true lunar month (experienced as either 29 or 30 days), defined by and aligned with the lunar phases. By the seventh century B.C. attempts were being made to predict the new moon day but textual evidence shows this was not successful until the Neo-Babylonian period. By the last 300 years B.C., month lengths were being predicted a year in advance and a centralized control of the calendar from major cultic centres (Babylon and Uruk) to outlying areas seems indicated. The centralized calendrical function of these late period temple organizations may explain why astronomical archives are found in precisely these centres.[2]

A. Jones offers the sole contribution on Greek calendrics. He discusses calendars associated with Greek astronomers for which there are intercalary cycles, the Callippic cycle (76 years) being the only one that served as the basis for a calendar and this only in astronomical contexts. Other "astronomer's calendars" include the Egyptian and the strictly solar Dionysian (for a brief interval during the third century B.C.). Jones explores antecedents of the astronomical calendars, i.e., dating by astronomical phenomena, in pre-Greco-Roman non-astronomical contexts, *viz.,* Hesiod, Book 4 of the *Epidemics* in the Hippocratic corpus, the Peripatetics and parapegmatists (Euctemon and Geminus). He then discusses evidence for the use of the zodiacal (twelve-sign) calendar, first associated with parapegmata from the end of the fourth century (possibly introduced by Callippus, Jones suggests). In the context of the parapegmata, which relate stellar phases to weather, the zodiacal division of the year is not strictly speaking a working calendar for generating dates but an ideal framework for organizing seasonal fixed-star phases with other data, such as weather or the length of daylight. The only true zodiacal calendar, Jones notes, is that of the astronomer Dionysius, preserved in the *Almagest*, a calendar soon dropped in favour of the Callippic and the Egyptian calendars in later astronomical work.

With this collection of papers, Steele has assembled an essential foundation for the further study of calendariography and chronography in the ancient Near East and Egypt. Specialists and readers interested in ancient calendars alike will profit from this fine publication.

University of California, Berkeley FRANCESCA ROCHBERG

REFERENCES

1. Otto Neugebauer, *A history of ancient mathematical astronomy* (3 vols, Berlin, 1975), iii, 1071.
2. Further agreement and discussion of the Babylonian month can be found in Sacha Stern, "The Babylonian month and the new moon: Sighting and prediction", *Journal for the history of astronomy*, xxxix (2008), 19–42.

# NOTICES OF BOOKS

*Anubio, Carmen astrologicum elegaicum*. Edited by Dirk Obbink (Bibliotheca Teubneriana; K. G. Saur Verlag, Munich and Leipzig, 2006). Pp. x + 79. €58. ISBN 978-3-598-71228-9.

The editio princeps of a Greek astrological poem, only partially extant in papyrus fragments and briefly mentioned by several later authors. The extremely obscure Anoubion wrote the elegiac poem probably in the first century A.D., probably in Egyptian Thebes. Firmicus Maternus's widely known fourth-century astrological handbook, the Mathesis, appears to borrow heavily from Anoubion's text.

*The Oxford Guide to the History of Physics and Astronomy*. Edited by J. L. Heilbron (Oxford University Press, New York, 2005). Pp. xxii + 358. $42.95. ISBN 978-0-19-517198-3.

A compilation of concise articles for general readers, selected from the far larger (941 pp.) *Oxford companion to the history of modern science* (2002) prepared by the same editor. Although it treats physics more extensively than astronomy, this guide offers entries on topics such as the anthropic principle, ether, space and time, celestial mechanics, pulsars and quasars, and telescope, plus some fifteen biographical entries on the best-known astronomers since Copernicus. A detailed index greatly enhances use of the alphabetically arranged guide.

*Introducción a la Astronomía y la Geografía.* Jerónimo Muños, ed. by Victor Navarro (Consell Valencià de Cultura, Valencia, 2004). Pp. 354. ISBN 84-482-3709-9.

Jerónimo Muños (*c.* 1520–92) taught Hebrew and a wide range of mathematical subjects, first at Valencia and then at Salamanca. He published three works, including *Libro del nuevo cometa* (1573) on the nova of 1572, which he recognized as celestial, and left others in manuscript. This sumptuous volume, embellished with many colour illustrations, presents a transcription of the manuscript Latin text of *Astrologicarum et geographicarum institutionum libri sex*, together with a translation into Spanish.

*Lights and Shadows in Cultural Astronomy: Proceedings of the SEAC 2005, Isili, Sardinia 28 June to 3 July.* Edited by Mauro Peppino Zedda and Juan Antonio Belmonte (Associazione Archeofila Sarda, Via Dante 76, Isili 08033, Sardinia, 2007). Pp. 374. ISBN 978-88-901078-2-5.

The triennial meetings of the European Society for Astronomy in Culture (SEAC) go from strength to strength. This latest volume of proceedings contains some forty papers, ranging over many topics, given at Isili in 2005. The keynote paper by Stanisław Iwaniszewski explores the sources of some of the misunderstandings produced by science-based and humanities-based archaeoastronomies.

# NOTES ON CONTRIBUTORS

GEORGE ELLIS (ellis@maths.uct.ac.za) is a recently retired relativist and cosmologist at the University of Cape Town. He continues to be active in those areas; see his discussion with Bernard Carr on Multiverses in *Astronomy and geophysics*, xlix (2008), 2.29–2.37. He is also studying the emergence of complexity and the nature of the mind.

MIGUEL A. GRANADA (granada@ub.edu) is researching into the transformation of astronomy and cosmology between 1570 and 1620. He is preparing with Nicholas Jardine and Adam Mosley an edition and English translation of Rothmann's *Scriptum de cometa*, which is dedicated to the comet of 1585.

ROBERT HATCH (ufhatch@ufl.edu) teaches at the University of Florida in the Program for the History of Science, and currently serves as Interim Director of the Center for the Humanities. As sequel to his first book, *The collection Boulliau* (American Philosophical Society, 1982), he is now completing Part II of the trilogy, *The Boulliau correspondence*, and continues apace with Part III, *Boulliau's Europe: Science & learning in 17th-century France*. He is also writing a book on the Republic of Letters and the role of correspondence networks during the Scientific Revolution.

J. L. HEILBRON (john@heilbron.eclipse.co.uk) is professor emeritus of history and the history of science at the University of California, Berkeley. His current research interests centre on the history of the physical and historical sciences in early modern times.

MICHAEL HOSKIN (michael.hoskin@ntlworld.com) recently opened Centro Solar Michael Hoskin at the archaeological site of Antequera in southern Spain, so-named to mark his archaeological work in Iberia.

GEOFFREY KOLBE (geoffreykolbe@compuserve.com) obtained a Ph.D. in physics from Imperial College, London. He spent twelve years researching the atomic physics of hot dense plasmas at Imperial College and at Lawrence Livermore National Laboratory, University of California. He now runs a precision engineering business in the Borders of Scotland.

DAVID LE CONTE (dleconte@guernsey.net) has had a lifelong interest in astronomy, having worked at the Royal Observatory Edinburgh, the Smithsonian Astrophysical Observatory, and Kitt Peak National Observatory. Now living in the island of his birth, Guernsey, Channel Islands, he was stimulated by the interest of local archaeologists in studying the orientations of the islands' megalithic tombs. His current research concentrates on the life of the nineteenth-century Guernsey-born astronomer Warren De La Rue.

ROBERT MORRISON (rgmorris@bowdoin.edu) is Associate Professor of Religion at Bowdoin College and is the author of *Islam and science: The intellectual career of Nizam al-Din al-Nisaburi* (Routledge, 2007).

JOHN D. NORTH is the author of *Cosmos: An illustrated history of astronomy and cosmology*, published by Chicago University Press in June 2008.

PAOLO PALMIERI (pap7+@pitt.edu) teaches at the University of Pittsburgh. He does theoretical and experimental research on the history and philosophy of early modern science.

GÉRALD PÉOUX (gpeoux@u-paris10.fr) is an assistant professor (Maître de Conférences) at the University of Paris X–Nanterre. He recently earned a master's degree in history of science at the Ecole des Hautes Etudes en Sciences Sociales (EHESS-Centre Alexandre Koyré). In association with scholars at the Observatoire de Paris, he is currently studying sixteenth-century astronomy.

VOLKER R. REMMERT (remmert@mathematik.uni-mainz.de) teaches History of Science and History of Mathematics at the University of Mainz. His book on the role of frontispieces in the Scientific Revolution, *Widmung, Welterklärung und Wissenschaftslegitimierung: Titelbilder und ihre Funktionen in der Wissenschaftlichen Revolution*, appeared in 2005.

FRANCESCA ROCHBERG (rochberg@berkeley.edu) is Catherine and William L. Magistretti Professor in the Department of Near Eastern Studies and the Office for the History of Science and Technology, University of California, Berkeley. Her research concerns ancient Mesopotamian and Greco-Roman history of science, especially astronomy and astrology. Her most recent book is *The heavenly writing: Divination, horoscopy and astronomy in Mesopotamian culture* (Cambridge University Press, 2004 and 2007).

EMILIE SAVAGE-SMITH (emilie.savage-smith@orinst.ox.ac.uk) is Professor of the History of Islamic Science, The Oriental Institute, University of Oxford. Her recent publications include an edition and analysis of an eleventh-century cosmography (with co-author Y. Rappoport) available at www.bodley.ox.ac.uk/bookofcuriosities, *Medieval views of the cosmos* (with co-author E. Edson), and *Medieval Islamic medicine* (with co-author P. E. Pormann).

CURTIS WILSON (c.wilson002@comcast.net), tutor and dean emeritus of St John's College, Annapolis, is in process of completing a detailed study of "The Hill-Brown theory of the Moon's motion (1877–1955): Its coming-to-be and consequences".

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