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Alan C. Bowen

and

Francesca Rochberg

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Sources and Studies in the History of Science
Alan C. Bowen and Francesca Rochberg, Editors
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From the Editors

15 Dec 2020

The Institute for Research in Classical Philosophy and Science (IRCPS) is pleased to offer this inaugural volume, which marks a new beginning for *Aestimatio* now revised to become *Aestimatio: Sources and Studies in the History of Science*.*

This new *Aestimatio* will focus, as before, on the history of science from antiquity up to the modern period. This chronological span, however, is to be complemented by an extended geo-cultural one that takes into account cultures in Eurasia and Africa, recognizing that the spread of the traditions of knowledge and of ideas is a unifying characteristic of the chronological and geo-cultural scope of science in the Old World before the modern era.

In *Aestimatio*, we take science broadly to be the goals, methods, knowledge, and practices in what is presented as science in the historical sources. Accordingly, this new series aims to make fundamental texts and ideas in the history of science accessible to readers today through the publication of original research. It will also include assessments of books recently published that allow reviewers to engage critically the methods and results of current research. On occasion, there will be guest-edited thematic issues and supplementary volumes.

We are most grateful to William R. Bowen (University of Toronto), Luis Meneses of ETCL (University of Victoria), and Megan O'Connor (IRCPS) for their invaluable help in making this publication possible. We also thank the members of the journal's editorial board for joining us in this new venture. Their support is deeply appreciated.

Alan C. Bowen and Francesca Rochberg

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* This particular volume draws in part on *Aestimatio: Critical Reviews in the History of Science* and *Interpretatio: Sources and Studies in the History of Science*, both of which are now discontinued. Our intention in reissuing items from these journals is to grant them the greater attention that they deserve.

The Directions and Names of the Winds

by

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Abstract

The anonymous text *Ventorum situs et nomina*, once held to be by Aristotle himself, gives the local names of 10 topic winds as well as their directions. It is not an elaboration of the wind-rose that Aristotle, for example, describes in *Meteor*. 2.5, though many scholars have assumed this, but a presentation of a weather-map for the inhabited world (οἰκουμένη). What seems to be important to the author in collecting the local winds under the topic winds is not so much their direction as their time of year as well as the etymologies of the local names.

About the Author

Alan C. Bowen is director of the Institute for Research in Classical Philosophy and Science (Baysville, ON, Canada). He is the author of numerous articles and books focused mainly on the history of Hellenistic science, especially astronomy, and philosophy. His latest book, with Francesca Rochberg, is *Hellenistic Astronomy: The Science in Its Contexts* (Brill, 2020), listed as an Outstanding Academic Title of 2020 by *Choice*/*Choice Review*.

This brief, anonymous account of the winds, which has no explanatory introduction and a laconic conclusion, is perhaps best described as a Hellenistic contribution to a body of learning about the winds, weather, and signs to which many contributed in antiquity, including notably Aristotle and Theophrastus.

1. The subject of the *Vent. situs*

Consider the main title of this work, «Ἀνέμων θέσεις καὶ προσαγορίαι». The word «θέσις», like the Latin "situs", though usually rendered into English in this context by "location",[1] "position",[2] or "situation",[3] has the general sense "disposition". But, in the case of the winds, especially as presented in this account, the disposition in question is surely a direction. Hence, I translate the Greek title as "The Directions and Names of the Winds". This is consistent with, and appropriate to, the substance of Aristotle's *Meteor*. 2.6, and to the anonymous Περὶ κόσμου as well, given their focus on where the winds are from.[4] I will return to this in due course.

2. On its provenance

As for the subtitle of *De vent. situs*, the codices have «ἐκ τῶν Ἀριστοτέλους Περὶ σημείων», which I propose to render "From Aristotle's Writings on Signs".[5] But perhaps this should really be "From Aristotle's Treatise *On Signs*" instead. In either case, the implication is that it was not Aristotle himself who offered the remarks in *Vent. situs* but some later author. Of

1 See D'Avella 2007, 223.

2 In French, see Federspiel and Levet 2018, 28, 83.

3 See Hett 1936, 453.

4 Aristotle himself maintains that air in motion counts as wind when it is understood to flow from a source or origin [*Meteor*. 360a27–33: for the text transposed, see Lee 1952, 167]. Περὶ κόσμου 394b7–9, however, though it identifies winds in terms of their direction, defines wind itself simply as the flow of a connected mass of air.

5 The capitalization of «περί» is the work of some medieval copyist in the transition from majuscules to minuscules and so has no probative value in its own right.

course, in either case again, the question becomes, In what sense is the *De vent. situs* really *from* its source? Is it

(a) an abridgment, that is, a complete section of this source?
(b) a collection of passages taken from throughout that source? or
(c) indebted to its source in the more limited sense that it draws on some governing motif or project that is developed in ways not found in the source? [6]

Now, I will not tarry with speculation about who this author was, whether he was a Peripatetic, a Stoic, or whatever. Nor will I address the related question, Did Aristotle actually write a Περὶ σημεῖων?, beyond confessing that, in my view, the evidence is not of the sort that warrants the claim that he did. Instead, I propose to compare Aristotle's *Meteor*. 2.6, the Περὶ κόσμου, and the *Ventorum situs*, with the aim of highlighting two features by which the latter differs significantly from the other accounts of the winds, features that should, but do not, figure in recent discussions of its provenance.

Latitude North (φ°)	Distance (η°) from SSRP to VERP
0[a]	23.5
23.5[b]	25.77
37.1	30
45	34.33
55.67	45
60	52.89
66.5[c]	90

[a] The latitude at the equator.
[b] The northern limit of the torrid zone.
[c] The Sun touches the horizon but does not cross it.

Table 1. The variation of the distance of the summer solstitial rising point (SSRP) to the vernal equinoctial rising point (VERP).

[6] For Sider and Brunschön, who hold that Aristotle actually wrote a Περὶ σημεῖων, only the first two possibilities are in play [2007, 12].

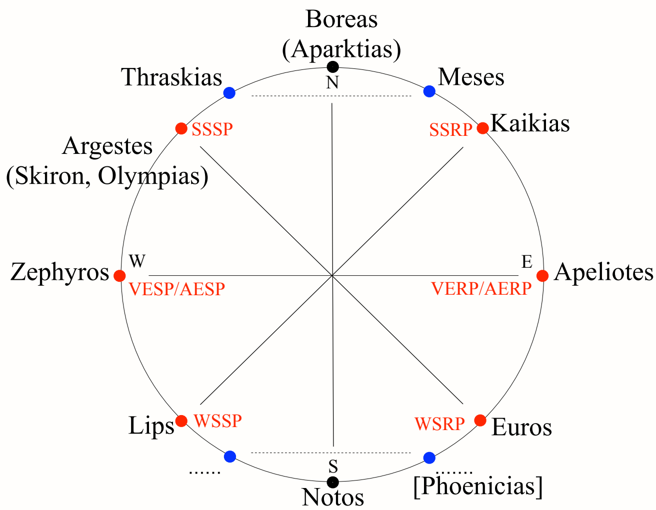

Figure 1. Aristotle's Wind-Rose [*De caelo* 2.6]

3. The winds and their directions

So, let us turn to the question, How is one to specify the directions of the winds? For Aristotle [*Meteor*. 2.6] and the author of the Περὶ κόσμου, these directions are to be specified with reference to the Sun's rising and setting points at the observer's horizon, when the Sun is at the cardinal points of its annual course through the heavens. These cardinal points are its positions on the days of solstice and equinox. Thus, for observers at less than lat. 66.5° to the north or south, there are, on the eastern horizon, the summer solstitial, the vernal/autumnal equinoctial, and the winter solstitial rising points; and, on the western horizon, there are the summer solstitial, the vernal/autumnal equinoctial, and the winter solstitial setting points. Now, the directions to these points on the observer's horizon are not to be identified by points on the compass, as Forster [1913] does. The reason, as Furley recognized [1978, 366 n*a*], is that the solstitial rising and setting points are not fixed for all observers but vary from horizon to horizon, that is, with the observer's latitude [see Table 1; Appendix, p. 17]. In fact, for observers at latitudes greater than 66.5° to the north or south, the Sun does not even cross the horizon on the day of solstice.

Thus, in *Meteor*. 2.6, when Aristotle offers a graphical representation of the winds [see Figure 1], this representation, which constitutes a wind-rose, involves marking out the horizon of some arbitrary observer in the northern inhabited lands by noting the Sun's cardinal rising and setting points during its annual course. As a refinement of the wind-rose offered in 2.6, Aristotle distinguishes the observer's real and ideal horizons, and remarks that both can be marked out in the same way.[7] Still, Aristotle gives no indication that this division obtains for only those horizons in which the Sun rises and sets on the day of solstice; or that, while the divisions in the real and ideal horizons are made in the same way, that is, with reference to the same cardinal points, the positions of the solstitial rising and setting points on the horizon vary with latitude. The same holds true as well of points on the horizon at or near the ever-visible circle—these are the small, solid blue circles in Figure 1, p. 5—in that the location of this circle varies with latitude as well [*Meteor*. 2.6, 363b27–364a4].

Nevertheless, this is not a critical problem: what it means is that one must take care in interpreting Aristotle's account. His causal theory of the Sun's action throughout the year on the Earth's two exhalations, the moist (vapor) and the dry, the latter being the source and nature of wind [*Meteor*. 2.4–5], suffices to guarantee that his wind-rose will hold, but only for those observers at latitudes greater than 23.5°, if Notos is to be a southerly wind [*Meteor*. 2.5, 362a31–b10], but yet no greater than 66.5°, the latitude where the wind-rose pattern ceases to hold.

There is no saying if the author of the *Vent. situs* recognized the nature and limitations of Aristotle's wind-rose. Still, we can see that he takes a different approach. Rather than use the Sun's cardinal rising and setting points for reference, he identifies the directions of winds by pointing to such geographical features as mountains, promontories, plains, and rivers, as well as to such political features as countries and their peoples. The typical entry in his catalog is of this sort:

> Topic wind.
> Alternative name for this wind, *N*, given in *X* (a town, country, town, or island), since it blows from a geographical or political feature *N′*.

[7] See Aristotle, *Meteor*. 2.6, 363a25–30:

> γέγρπται μὲν οὖν, τοῦ μᾶλλον εὐσήμως ἔχειν ὁ τοῦ ὁρίζοντος κύκλος· διὸ καὶ στρογγύλος. δεῖ δὲ νοεῖν αὐτοῦ τὸ ἕτερον ἔκτημα τὸ ὑφ᾽ ἡμῶν οἰκούμενον· ἔσται γὰρ κἀκεῖνο διειλεῖν τὸν αὐτὸν τρόπον. [Louis 2002]
> Now, the circle of the horizon has been drawn for the sake of greater clarity—which is why it is round. Although it is right to consider the [land] inhabited by us as a section different from [this horizon circle], it certainly will be possible to divide that [section] too in the same way.

Cf. Forster 1913, *ad loc*.

It is striking that a good number of the entries in the *Vent. situs* are concerned to state the connection between the alternative name *N* for the topic wind and the geographical or political feature *N′*. In some cases, such etymologizing is unexceptional: thus, for instance, the derivation of the name “Pagreus” from the fact that it blows from the Pagrica mountains, or of the name “Kaunias” from the fact that it blows from the town, Kaunos [Caunus].[8] In others, it is bizarre: the apparent attempt to derive «Ζέφυρος» from the word for evening and the west, «ἑσπέρα», is a case in point.[9]

But this brings to the fore the problem of understanding the topic winds themselves. These are pretty much the same as those found in *Meteor.* 2.6 and Περὶ κόσμου 394b10–35 [see Table 2, p. 9]. So, are we to understand them in the same way, that is, as winds also defined in some wind-rose?

Plainly, it would be a mistake to hold that the topic winds listed in the *Vent. situs*, though perhaps taken from those mentioned in some wind-rose, are understood to be specified in a wind-rose. After all, the *Vent. situs* neither alludes to nor needs a horizon circle. In fact, the diagram mentioned at its close is a circle of the Earth (ὁ τῆς γῆς κύκλος), that is, a circle enclosing the inhabited world. This circle cannot be a horizon. After all, there is no single horizon circle that includes parts of Asia Minor as well as northern Africa. That is, there is no observer on Earth who can see these lands without changing longitude and latitude. But what if the author presupposes a *standard* horizon, say, the one at lat. 55.67°, in which the summer solstitial rising point lies due northeast [see Table 1, p. 4], or a standard pattern that has the winds spaced equally at 30° intervals between the orthogonal north-south and east-west directions?[10]

Well, consider D’Avella 2007, 222.1–8 [973a1–8], where it is asserted that Borras has the names:

(a) “Pagreus” in Mallos (≈ long. 35;30°, lat. 36;45°) because it blows from the Pagrika mountains (≈ long. 36;15°, lat. 36;20°), and

[8] See p. 11below on transliteration and the presentation of place-names.

[9] Such etymologizing may indicate a Hellenistic provenance and raises the question, Was the author of the *Vent. situs* a Stoic? There certainly were Stoics who took Aristotle’s texts as important points of departure in the late second and first centuries BC [see Falcon 2012, 2015, and 2016]. Posidonius himself is reported to have written a commentary on the *Meteorologica* [Edelstein and Kidd 1989, fr. 18: cf. frr. 137a–b]. Furthermore, the Stoic school was in general given to using etymology to show the nature of things.

[10] This pattern figures in Ptolemy’s *Geographia*: cf. Berggren and Jones 2000, 15.

(b) “Kaunias” in Rhodes (≈ long. 28;00°, lat. 36;10°) because it blows from Kaunos [Caunus] (≈ long. 28;35°, lat. 36;50°).

Mallos is roughly 45′ to the west of the Pagrika and roughly 25′ to the north, which means that the Pagreus is a southeasterly to easterly wind [Talbert 2000, 67 B3–C4]. Rhodes, however, lies roughly 35′ to the west of Caunus and roughly 40′ to the south, thus making the Kaunias a northeasterly wind [Talbert 2000, 60 F3–G3, 65 A4, 1 I3]. So, if we take for granted at the outset that Borras is a northerly wind, it would follow that the *Vent. situs* is in error. Indeed, there will prove to be numerous errors of this sort. But all of them, I suggest, will be no more than a scholarly artifact of choosing the wrong starting point for interpretation.

So, let us not attribute either error to the author of the *Vent. situs*. That is, instead of reading *Vent. situs* as a supplement to some wind-rose, let us understand it as a report of the terms used by diverse peoples in naming the winds characteristic of different times of year at their locations. Specifically, my working hypothesis is that:

(a) the names of the 10 topic winds are selected in the light of some variant of the wind-rose [see Table 2];
(b) these topic winds are differentiated mainly by when they blow during the year and, perhaps, by the weather that they bring, something that the author does not explain but takes for granted, perhaps because it is common knowledge;[11]
(c) the winds listed under a topic wind are thus to be understood as winds that blow at roughly the same time of year as the topic wind and, perhaps, bring the same kind of weather;[12]
(d) the winds so listed need not have the same direction;[13]
(e) one of the author’s aims is to explain, whenever he can, the name of a listed wind in terms of the name of where it comes from;

[11] As Aristotle indicates, while the topic winds do not always bring the same kind of weather, there is a general tendency for this [*Meteor*. 364a4–24, b3–365a1: cf. Περὶ κόσμου 395a1–5].

[12] See Aristotle, *Meteor*. 2.6, 364a27–32, for the remark that winds coming from different directions that are not opposite may blow at the same time.

[13] Indeed, the same wind need not always have the same direction [*Meteor*. 2.6, 364b12–14, 365a7–13: cf. Περὶ κόσμου 394b36–395a1]. For our part, we in the northern hemisphere might explain this phenomenon by reference to the typical course of a seasonal, cyclonic weather-system, that is, a large weather-system rotating counterclockwise.

Aristotle *Meteor*. 2.6	Περὶ κόσμου	*Ventorum situs* Topic Winds
Boreas (Aparktias)	Aparktias	Borras
Meses	Boreas	..........[a]
Kaikias	Kaikias	Kaikias
Apeliotes	Apeliotes	Apeliotes
Euros	Euros	Euros
[Phoenicias][b]	Euronotos	Orthonotos[c]
Notos	Notos	Notos
..........[d]	Libanotos (Libophoenix)	Leukonotus
Lips	Lips	Lips
Zephyros	Zephyros	Zephyros
Argestes (Skiron, Olympias)	Argestes (Olympias, Iapyx)	Iapyx
Thraskias	Thraskias (Kirkias)	Thraskias

[a] "Meses" does not actually have its own entry. It is only listed as an alternative name for Borras. Forster [1913, *ad* 972a4 and n 1] assumes that *Vent. situs* describes a wind-rose and proposes that Meses is the missing topic wind.

[b] This is a purely local wind.

[c] See p. 14 n24 below.

[d] Aristotle maintains that there really is no wind contrary to Meses.

Table 2. Greek wind-names

The names in columns 1, 2 explicitly belong to a wind-rose; and the names in column 3 are in the order of their occurrence, assuming the omission of a wind after Borras [see note a].

(f) the disagreements that the author indicates about the topic wind under which a given named wind is to be placed are but indications of differences in linguistic usage; and

(g) the graphical representation mentioned at the close of *Vent. situs* amounted to a crude, composite weather-map showing the seasonal winds in different parts of the inhabited world in a typical year.

Finally, to develop this working hypothesis, I must also assume that

(h) the author had (access to) reliable practical knowledge of the directions from the locales that he mentions to the places named as sources of wind there; and

(i) modern inferences about geographical directions on the basis of archaeological evidence of the places as identified in Talbert 2000 are warranted.

One might imagine that it would be better to proceed in the light of ancient geographical knowledge. But it was not until the second century AD, when Ptolemy's *Geographia* presented the requisite theoretical basis for mapmaking in the modern sense and supplied a gazetteer, that it was even possible either to represent the inhabited world graphically or simply to locate a place in a way that was both precise *and* accurate. Moreover, even if the data of Ptolemy's treatise were an accurate guide to the state of our author's own geographical knowledge, thus allowing us by comparison with modern maps to detect any errors that he makes about the directions that the winds come from—recall that his date is unknown and that Ptolemy's work may thus not be pertinent—the gazetteer does not mention many of the places that figure in the *Vent. situs*.[14] Consequently, the best we can do is to assume that the author is correct about the directions that he identifies and that atlases such as Talbert 2000 are the best means available of determining them.

4. A Hellenistic weather-map

Readers will, of course, decide for themselves whether the *Vent. situs* is a lexical report that was, or can be, cast as an early kind of weather-map, by considering the text closely in relation to what is known today of places in the ancient world. But, if my the argument is correct, then we may infer that:

(a) since the *Vent. situs* is to be viewed as a proto-typical weather-map, it is unlike the accounts of the winds found, for example, in Aristotle, *Meteor*. 2.6, the anonymous Περὶ κόσμου, and even Strabo, *Geog*. 1.2.21.

Moreover, we may now raise the question whether

(b) the attention to linguistic usage and etymology in the *Vent. situs* signals a rejection of the causal theory advanced by Aristotle in *Meteor*.

[14] On the general state of geographical knowledge in the Greco-Roman world of Hellenistic times, see Geus 2020.

2.4–5 in favor of the thesis that to understand the nature of the winds it suffices to understand their names, that is, why the winds have the names that they have.

Clearly, there is a need to re-assess the claims that the *Ventorum situs et nomina* is a Peripatetic work that draws on Aristotle's writings—perhaps even his putative Περὶ σημεῖων—and that it is of a type found as well in the Περὶ κόσμου. But any such renewed inquiry into the provenance of this text I will leave to others.

5. The text

For the Greek text here translated, though I have consulted Bekker 1831, Rose 1886—Rose printed the same version three times (1863, 1870, 1886)—and Apelt 1888, which mostly follows Rose 1886, I have based my translation on the edition recently prepared by Victor D'Avella [2007], recording D'Avella's lineation in the left margin and Bekker's [1831, 973] in the right.

I have used the following *sigla* in the footnotes to the translation as a means of simplifying the presentation of the Greek text itself:

Apelt 1888	A
Bekker 1831	B
Rose 1886	R

6. Transliteration

The problem of how to present the numerous place-names in this text is real. One approach is simply to transliterate the Greek, a practice followed mostly in D'Avella 2007, 223, 225 and Federspiel and Levet 2018, for example. Another would be to latinize these place-names, as in Forster 1913, Hett 1936, and Furley 1978. Yet another would be to follow the policy for place-names adopted in the *Barrington Atlas of the Greek and Roman World*, by transliterating Greek forms in all instances except when there is a Latin form available and this form may be regarded as more familiar [Talbert 2000, xxv]. My solution is a hybrid that accommodates readers wishing to locate the places mentioned in the *Vent. situs*. Thus, while I have as a rule transliterated the terms for the winds and places from the Greek, I have inserted beside them in brackets the Latin name under which they may be found in the *Barrington Atlas* when this name differs from the transliterated Greek. The exceptions are "Crete", "Italy", "Rhodes", and "Sicily"; rather than transliterate the Greek in these instances, I have translated it and enclosed the terms in the *Atlas* for these places alongside in brackets.

TRANSLATION

The Directions and Names of the Winds from Aristotle's Writings on Signs[*]

Borras.[1]

In Mallos, this is Pagreus since it blows from great heights, that is, from two mountains lying alongside one another that are called the | Pagrika [Pagrica].[2] In Kaunos [Caunus], it is Meses. In Rhodes [Rhodos], it is Kaunias since it blows from Kaunos, disturbing their har || bor, Akanias.[3] In Olbia, the one by Magydos of Pamphylia, it is Idyreus since it blows from the island which is called Idyris.[4] Some, among whom are also the Lyrnatians, the ones in Phaselis,[5] think that [the Idyreus] is Borras.[6]

| Kaikias.

In Lesbos, this is called Thebanas, since it blows from the || plain of Thebe[7] [Thebai], the [plain] above the Elaitic Gulf of Mysias. It disturbs the harbor

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[*] On this subtitle, see section 2, p. 3above.

[1] 3 Βορρᾶς. Attic dialect for Βορέας.

[2] The Pagreus is a southeasterly to easterly wind [Talbert 2000, 67 B3–C4, 1 K3].

[3] 5 ἐνοχλῶν τὸν λιμένα αὐτῶν τὸν ἀκανίαν B] ἐνοχλῶν τὸν λιμένα αὐτῶν τῶν Καυνίων R, A (disturbing the harbor of the Kaunians themselves). See Goh and Schroeder 2015, *s.v.* Ἀκανίας. The Kaunias is a northeasterly wind [Talbert 2000, 65 A4, 1 I3].

[4] Talbert 2000, 65 E4 queries whether there was a town to the south of Olbia and Magydos called Idyros and a river Idyros nearby. No island named Idyris/Idyros has been identified yet.

[5] 9 Λυρνατιεῖς...Φασηλίδα. «Λυρνατιεῖς» is the name of some collective in Phaselis. Note that Talbert 2000, 65 E4 queries whether an island to the north of Phaselis (and the putative Idyreis) but to the south of Olbia was named Lyrnateia.

[6] This supports the author's inclusion of the Idyreus under the topic "Borras". The direction of the Idyreus, however, cannot be determined because the location of the island Idyris is not known.

[7] 11 θήβης: a place in the Troad.

of the Mitylenians, especially [the temple] of [Apollo] the Protector of Flocks.[8] But among some it is Kaunias, which others think is Borras.[9]

Apeliotes.

In Tripolis, the one of Phoinike [Phoenice],[10] this | is called Potameus. It blows out of a level plain which is like a great threshing-floor and is sur || rounded by the Libanos [Libanus] and Bapuros mountains.[11] For this reason, it is, in fact, called Potameus.[12] It disturbs [the shrine] of Poseidon.[13] In the Gulf of Issos [Issicus Sinus] and around Rhossos [Rhosos], it is Syriander. It blows from the Gates of Syria [Syrii Pulai], which the Tauros [Taurus] and Rhosian mountains demarcate.[14] In the | Gulf of Tripolis, it is Marseus || after the village of Marsos.[15] In Prokonnesos [Proconnesus], Teos, Crete [Creta], Euboia [Euboea], and Kyrene [Cyrene], it is Hellespontias. It especially disturbs the harbor, Kapheres, of Euboia and the harbor of Kyrene, which is called Apollonia. It blows from the Hellespont.[16] In Sinope, it is

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[8] 11 τὸν Μαλόεντα. «Μαλόεις» is an epithet of Apollo in Lesbos meaning "Protector of Flocks" [cf. Thucydides, *Hist*. 3.3.3] and may also designate his temple there (*scil.* τὸν Μαλόεντα ναόν). See line 17 [973a16] τὸ Ποσειδώνιον *scil.* ἱερόν.
The Thebanas is a southerly wind [Talbert 2000, 61 E2, 56 C3, and 1 I3].

[9] The author thus notes that the wind called Kaunias falls under both "Borras" and "Kaikias". This sentence is not misplaced, as Forster [1913, n 3 *ad* 973a24–25] supposes.

[10] 14 τῆς Φοινικῆς: *scil.* Asiatic Phoenicia.

[11] There is no entry for the Bapuros mountains in Talbert 2000. The Potameus is a southerly to southeasterly wind [Talbert 2000, 68 A5, 69 C2, 1 K4].

[12] 16–17: the etymology here is not explained. Forster [1913, n 3 *ad* 973a16] speculates that the plain may have been called Potamos.

[13] 17 τὸ Ποσειδώνειον: *scil.* ἱερόν. See Goh and Schroeder 2015, *s.v.* Ποσιδώνιος -α -ον. This shrine or temple is, presumably, in Tripolis. See line 11 [973a11].

[14] The Gulf of Issicus is to the north of Rhossos, which would make the Syriander a southerly wind. The Gates of Syria, however, are to the east and slightly north of Rhosos, which would mean that the Syriander is more easterly.

[15] There is no entry for Marsos in Talbert 2000.

[16] Hellespontias is a northerly to northeasterly wind [Talbert 2000, 57 B3 and 6, E6; 38 C1; 1 I3–4, H2–3]. Prokonnesus, however, is either a town or an island in the Propontis and thus to the northeast of the Hellespont. Thus, the Hellespontias there would be southwesterly.

Berekyntias because it blows from | regions in Phry || gia.[17] In Sicily [Sicilia], it is Kataporthmia because it blows || from the strait.[18] Some think that it is a25 b1
Kaikias and call it Thebanas.[19]

Euros.

In Aigai [Aigai(ai)], the one in Syria, this is called Skopeleus after the cliff (*skopelos*) of Rhossos [Rhosos].[20] In Kyrene [Cyrene], it is Karbas after | the foreigners || in Phoinike [Phoenice],[21] which is why some call it Phoinikias b5
as well.[22] There are some who also think that it is Apeliotes.[23]

Orthonotos.[24]

Some designate this Euros; and others, Amneus.

Notos.

Among all [peoples], it is called the same. Its name is on account of its being

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[17] The etymology of "Berekyntias" is left unexplained. The Berekyntias is a northwesterly wind [Talbert 2000, 87 A2, 1 I2–K2].

[18] 973b1 ἀπὸ τοῦ πορθμοῦ: *scil*. the *Fretum Sicilium* or Strait of Messina today. The Kataporthmia is a northeasterly wind. [Talbert 2000, 1 F3–G3].

[19] The author thus indicates some controversy about whether the Kataporthmia should be listed under "Kaikias" and identified as Thebanas or under "Apeliotes".

[20] The Skopeleus is a southerly wind [Talbert 2000, 67 B3–4, 1 K3].

[21] 29–30 ἀπὸ τῶν καρβάνων: *scil.* οἱ κάρβανοι, the Phoenicians themselves, viewed by the author as foreigners/barbarians [cf. Forster 1913, *ad loc*; Goh and Schroeder 2015, *s.v.* κάρβανος -η -ον]. In the next sentence, it is said that this wind is also Phoinikias, the wind from (Asiatic) Phoenicia. Apparently, the winds could be named after political features of the inhabited world and not just geographical ones.

[22] The Karbas/Phoinikias is an easterly to northeasterly wind [Talbert 2000, 67 B3–C2, 1 H4–K4].

[23] That is, some would think that the Karbas/Phoinikias should be listed under "Apeliotes".

[24] 32 Ὀρθόνοτος. Foster [1913, *ad loc*. and n5] has «Εὐρόνοτος». This emendation presupposes that those who used the name «Ὀρθόνοτος» must have understood it to mean "Due South", and thus fails to distinguish how a word is formed and its usage or what it actually means in practice. Federspiel and Levet [2018, 33] evince the same failure when they claim that the author does not provide an etymology for "Aparktias" and "Apeliotes" because it was obvious that the former comes from the Ἄρκτος (Ursa Major)—*scil.* from where it rises and sets—and the latter, from the (rising) ἥλιος. But, even granted that such was the *practical* meaning of "Aparktias" and "Apeliotes", neither etymology identifies a geographical or political feature on Earth and so including them would be out of character.

productive of illness and on account of its being rainy—in both senses, “notos”.[25]

Likewise || Leu | conotos.
Its name is from a property since it makes [the sky] clear....[26]

Lips.
This [wind has], in fact, this name after Libya, from where it blows.[27]

Zephyros.
This [wind has], in fact, this name on account of its blowing from the west. The west (evening?)....[28]

Iapyx.
In Taras [Tarentum], this is Skylletinos after the place Scyllantion [Scylletium].[29] In || Dorylaion, the one of Phyrygia, it is, in the words of some,[30] Pharangites since | it blows from some one of the canyons [*pharanges*] in Pangaion.[31] Among many, it is Argestes.

Thrakias.
In Thrakia [Thracia], it is Strymonias since it blows from the river Strymon.[32] But in the Megarid [Megaris], it is Skirron after the Skirronian Rocks.[33] In Ita || ly [Italia] and Sicily [Sicilia], it is Kirkias on account of its | blowing

[25] 33–34: the claim appears to be that this south wind is Νότος because it is νοσώδης or productive of νόσος (illness) and because it is νότιος or rainy (viz. κάτομβρος).

[26] 35 λευκαίνεται. Here a connection is made between Λευκόνοτος (λευκός (white) + Νότος)—a wind from the south—and the verb «λευκαίνω» (to brighten, make clear). The Leukonotos is a wind that clears the sky.

[27] Lips is a southerly to southwesterly wind, assuming locations ranging from Italy in the west to Phrygia in the east [Talbert 2000, 1 H4–I4].

[28] 37 ἀφ’ ἑσπέρας. ἡ δὲ ἑσπέρα...: Zephyros blows from the west (ἀφ’ ἑσπέρας) in the evening (ἑσπέρᾳ). Here the claim being made seems to be that «Ζέφυρος» derives from «ἑσπέρα».

[29] The Skylletinos is a southwesterly wind [Talbert 2000, 46 E4, 1 G2–3].

[30] 39 ὑπὸ δέ τινων. There is no need to supply the passive verb «καλεῖται». The use of «ὑπό» + genitive with intransitive verbs to indicate agency goes back to Homer.

[31] 40 τὸ Παγγαῖον: a mountain in Macedonia.The Pharangites is a westerly wind [Talbert 2000, 62 E2, 51 C3, 52 B4–H4, 1 I2–J3].

[32] The Strymonias is a westerly to southwesterly wind [Talbert 2000, 51 B2].

[33] 44 ἀπὸ τῶν Σκιρρονίδων πετρῶν. If these rocks, which are named after the mythical bandit Sciron, lie between Attica and Megara [Goh and Schroeder 2015, *s.v.* Σκιρωνίς -ίδος], then the Skirron would be an easterly wind. Given their association with

from Kirkaion.[34] In Euboia [Euboea] and Lesbos, it is Olympias; its name is after the Olympos [Olympus] of Pieria. It irritates the people of Pyrrha.[35]

I have also drawn for you their locations, how they lie and blow, by drawing the circle of the Earth,[36] so that || they may be set before your eyes as well. b25

| End of the names for winds.

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the Megarid, however, and the route traveled along its southern coast, it seems possible that they were between Megara and Corinth, which, in some quarters of the Megarid, would make the Skirron a westerly to southwesterly wind.

[34] 45 τοῦ Κιρκαίου: the promontory [Talbert 2000, 44 D3]. To have roughly the same direction in Italy and Sicily, the Kirkias would have to be northwesterly [Talbert 2000, 44 D3, 1 E2–3].

[35] The Olympias would seem to be northwesterly to westerly [Talbert 2000, 50 B4; 1 H2–3].

[36] 48 τὸν τῆς γῆς κύκλον: *scil.* the circumference of the inhabited world.

APPENDIX
THE SUN'S ORTIVE AMPLITUDE AT SOLSTICE

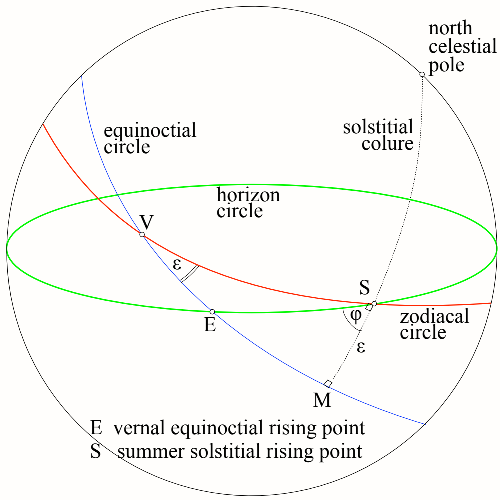

Figure 2. The ortive (rising)
amplitude of the summer solstitial point

Consider $\triangle EMS$, a spherical triangle on the unit-sphere on which all arcs are arcs of a great circle. The ortive amplitude of the Sun's rising point on the day of summer solstice is ES, the distance from the vernal equinoctial rising point E to the summer solstitial rising point S.

Since the vernal equinoctial point V on the celestial equator is a pole of the solstitial colure (the meridian circle through the poles of the zodiacal circle, the poles of the equinoctial circle, and the solstitial points), arc $VM =$ arc $VS = 90°$. Therefore, since $\angle MVS = \varepsilon$, where ε is the obliquity of the zodiacal circle to the equinoctial circle ($\approx 23.5°$), arc $MS = \varepsilon$.

Since $\angle ESM = \varphi$, where φ is the latitude of the horizon circle (*scil.* the elevation of the north celestial pole above this circle),

$$\sin ES = \frac{\sin \varepsilon}{\cos \varphi},$$

then

$$ES = \arcsin\Big(\frac{\sin \varepsilon}{\cos \varphi}\Big).$$

The distance from the equinoctial rising point to the winter solstitial rising point is the same.[37]

[37] See Van Brummelen 2013, 51–55.

BIBLIOGRAPHY

Primary sources

Aristotle. *Meteorologica*. See Louis 2002.

[Aristotle]. Περὶ κόσμου. See Bekker 1831, vol. 1.

[Aristotle]. *Ventorum situs et nomina*. See D'Avella 2007.

Ptolemy. *Geographia*. See Nobbe 1843–1845.

Strabo. *Geographica*. See Radt 2002.

Secondary sources

Apelt, O. 1888. *Aristotelis quae feruntur, De plantis, De mirabilibus auscultationibus, Mechanica, De lineis insecabilibus, Ventorum situs et nomina, De Melisso Xenophane Gorgia*. Leipzig.

Bekker, I. 1831. *Aristoteles graece*. vol. 2. Berlin.

Berggren, J. L. and A. Jones. 2000. *Ptolemy's* Geography*: An Annotated Translation of the Theoretical Chapters*. Princeton, NJ/Oxford.

D'Avella, V. 2007. See Sider and Brunschön 2007, 221–225.

Edelstein, L. and I. G. Kidd. 1989. *Posidonius: 1. The Fragments*. Cambridge, UK.

Falcon, A. 2012. *Aristotelianism in the First Century BCE*. Cambridge, UK/New York.

——— 2015. "The Reception of Aristotle in Antiquity". https://www.oxfordhandbooks.com/view/10.1093/oxfordhb/9780199935314.001.0001/oxfordhb-9780199935314-e-54?rskey=NlcCwC&result=1.

——— 2016. "Aristotelianism in the First Century BC". Pp. 101–119 in A. Falcon, *Brill's Companion to the Reception of Aristotle in Antiquity*. Leiden/Boston.

Federspiel, M. and J.-P. Levet. 2018. *Pseudo-Aristote. Du monde, Positions et dénominations des vents*. Paris.

Forster, E. S. 1913. "Ventorum situs et appellationes". See Ross 1913, 973a–b.

Furley, D. J. 1978. *[Aristotle]: On the Cosmos*. Cambridge, MA/London, UK.

Geus, K. 2020. "Hellenistic Maps and Lists of Places". Pp. 232–239 in A. C. Bowen and F. Rochberg edd. *Hellenistic Astronomy: The Science in Its Contexts*. Leiden/Boston.

Goh, M. and C. Schroeder. 2015. edd. *The Brill Dictionary of Ancient Greek by Franco Montanari*. Leiden/Boston.

Hett, W. S. 1936. *Aristotle: XIV. Minor Works*. London, UK.

Lee, H. D. P. 1952. *Aristotle: VII. Meteorologica*. Cambridge, MA/London, UK.

Louis, P. 2002. *Aristote. Météorologiques*. 2 vols. Paris.

Nobbe, C. F. A. 1843–1845. *Claudii Ptolemaei Geographia*. 3 vols. Leipzig. Repr. Hildesheim, 1966.

Radt, S. 2002. *Strabons Geographika. Band I. Prolegomena, Buch I–IV: Text und Übersetzung*. Gottingen.

Rose, V. 1886. *Aristotelis qui ferebantur librorum fragmenta*. Leipzig.

Ross, W. D. 1913. *The Works of Aristotle Translated into English*. vol. 6. Oxford.

Sider, D. and C. W. Brunschön. 2007. edd. *Theophrastus of Eresus:* On Weather Signs. Leiden/Boston.

Talbert, R. J. A. 2000. *Barrington Atlas of the Greek and Roman World*. Princeton, NJ.

Van Brummelen, G. 2013. *Heavenly Mathematics: The Forgotten Art of Spherical Trigonometry*. Princeton, NJ.

The Daimon in Hellenistic Astrology

by

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Abstract

A discussion of Dorian Greenbaum's *The Daimon in Hellenistic Astrology: Origins and Influence*.

About the Author

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The word « δαίμων » appears in the technical language of Hellenistic astrology in two contexts. On the one hand, two of the 12 topical places (τόποι), houses in modern astrological parlance, of the horoscope bear the traditional names "Good *Daimon*" and "Bad *Daimon*"; on the other, there is a calculated horoscopic point of the genre called lots (κλῆροι: parts) that is labeled the "Lot of *Daimon*". In both cases, this *daimon* is paired with fortune (τύχη).

Daimons are, of course, far more familiar from Greek mythology, theology, philosophy, and magic, especially in the form of a personal *daimon*, a supernatural entity acting as a guardian of an individual. These entities, it seems, often influenced astrology in its stricter or broader, more or less technical form when it was used to classify or describe *daimons* in order to communicate with them effectively or to find the personal *daimon* in an individual's nativity (birth-horoscope).

Furthermore, since *daimons* had a strong relationship with fate and destiny both in and outside technical astrology, and since astrology as a craft was meant primarily to be a study of fate, *daimons* and astrology were intertwined in antiquity in many ways. By singling out this relationship for the subject of her PhD thesis in the 2000s, Dorian Greenbaum found a promising area of research. The book under discussion here,*The Daimon in Hellenistic Astrology: Origins and Influence* [Greenbaum 2016], is an expanded and updated version of her dissertation of 2009.

The title itself of the book is somewhat misleading since it discusses not the *daimon* in Hellenistic astrology so much as the *daimon* and its intricate relationship with astrology; and the complexity of this subject is also reflected in the organization of the book. It is divided into three parts:

(1) Daimon and Fortune,
(2) Gods and Daimons, and
(3) Lots and the Daimon.

This structure might seem arbitrary but it is one of very few meaningful layouts that can organize the book's abundant sources and secondary literature. It also shows that the role of the *daimon* in astrology cannot be properly understood without the knowledge of the rich and complex cultural background in which astrology is embedded.

In the first part ("Daimon and Fortune"), chapter 1 surveys the themes of the *daimon*, fortune, and astrology through the lenses of two representative authors of the second century AD, Plutarch and the astrologer Vettius Valens. The investigation of the latter is easily justified by the fact that Valens is practically the only known astrological author who has anything to say about the issues of fortune and fate beyond technicalities. Besides the various treatises from Plutarch's *Moralia*, the spurious *De fato* from the same era is surveyed to provide a full image of contemporary thinking about the *daimon*, fortune, and fate.

Chapter 2 is devoted to the astrological pairing of "Good Daimon" and "Good Fortune", that is, the names of the 11th and fifth places of a horoscope, respectively. It offers an analysis of astrological works from Manilius (early first century AD) to Rhetorius (fifth or early sixth century), who is considered the latest representative of Hellenistic astrology. This discussion is introduced with an eye to the wider historical and cultural background, using Greek and Demotic sources. This theme is continued into chapter 3, which investigates the issues raised in the previous chapter in the other Mediterranean cultures, most importantly, in Egypt and Mesopotamia. A convincing and highly important conclusion is found at the end of this chapter [114]: Greenbaum raises the possibility that the Greek concept of immutable fate was mitigated in Hellenistic astrology by oriental influences that allowed negotiation about fate.

The first part concludes with chapter 4, which treats the "Bad Daimon" and "Bad Fortune" (the names of the 12th and sixth places in Hellenistic astrology) in much the same way as their positive counterparts earlier. In this instance, however, Greenbaum summarizes briefly Mesopotamian, Egyptian, Greek, Jewish, and Christian traditions regarding demons (that is, malevolent *daimons*) before discussing astrological ideas.

Comparison of chapters 2–3 with chapter 4 reveals similarities in the survey of astrological authors, though there are also some dissimilarities. Of the latter, the different descriptions of the cultural background are entirely justifiable, but chapter 4 includes a table of names and descriptions of the sixth and 12th places [143–145] which chapter 2 oddly lacks. Although this table is useful as an overview of the ideas, in practice it suffers from two shortcomings. First, a table exhibiting the diachronic development of the themes related to these two topical places would have served the reader better than this *potpourri* of keywords collected from different astrological authors. Second, it seems that the known Hellenistic interpretation of the places is the result of the amalgamation of two cognate but different streams of ideas: the δωδεκάτροπος (twelve-turning), covering all the 12 places, and the ὀκτάτροπος (eight-turning) associated with "Asclepius", which extends only over the first eight astrological places, including the fifth and the sixth, the equivalent of "Good Fortune" and "Bad Fortune" of the δωδεκάτροπος, respectively [Beck 2007, 44–45]. These different constituents, although known by Greenbaum [400n5], are left unmentioned, though they should have been analyzed more carefully to give the necessary insight into the intricacies.

The second part ("Gods and Daimons") consists of three chapters. In chapter 5, Greenbaum investigates Gnosticism and Mithraism to show how the role of *daimons* and their relation to gods are evaluated in harshly different ways within syncretic traditions in which astrological thinking is also found. At least two important achievements must be highlighted here: a new and sound suggestion to assign Gnostic «αἰῶνες»/«ἄγγελοι»/«ἐξουσίαι» to the zodiacal signs and planets [174–175] as well as an intriguing treatment of the so far neglected *thema dei* found in the Byzantine summary of the *Introductio* of Antiochus of Athens [187–193]. This latter gives further support to Roger Beck's hypothesis that this Antiochus is identical with C. Iulius Antiochus Epiphanes Philopappus, the eponymous archon of Athens in the late first century AD. He belonged to the family of the astrologers Thrasyllus and Balbillus, whose activities, and therefore Antiochus', may well be connected to the rise of the Roman mysteries of Mithras [Beck 2006, 253–254].

Chapter 6 extends this inquiry of good and evil *daimons* into the realm of magical papyri, the philosophical *Hermetica*, and the decan-lore originating from Egypt and eventually subsumed into astrology. Here, some astrological works are examined along the same lines taken in the first part. Overall, the content of chapter 6 is rather vague.

In contrast, chapter 7 investigates the role of the personal *daimon* in Neoplatonism with a special focus on Porphyry, who links the idea of a personal *daimon* to the astrological concept of the οἰκοδεσπότης (the master of the house), a type of a ruling planet in a nativity. This concept is not without problems, as «οἰκοδεσπότης» has different context-dependent meanings in astrological texts; but these concerns are excellently clarified here [256–257]. More problematic is Greenbaum's acceptance of the *Introductio ad Ptolemaei tetrabiblum*, specifically its mostly uncontested chapters, as a genuine text of Porphyry. This issue and the analysis of "Porphyry's" (in fact, Antiochus') method to find the οἰκοδεσπότης will be further explored below. As a final remark on this chapter, it is not clear how Greenbaum would like the reader to understand Iamblichus' five elements (στοιχεῖα) in finding the οἰκοδεσπότης [256]: she refers to Ptolemy's technique as an example of these "five steps", but the exact meaning remains uncertain.

The final part ("Lots and the Daimon") is devoted to the previously mentioned astrological lots, chiefly to the Lot of *Daimon*, its counterpart, the Lot of Fortune, and further lots derived from them, as well as to their cultural background. Both these lots are calculated by measuring the interval between the Sun and the Moon from the Ascendant clockwise or counterclockwise, depending on whether the horoscope is cast in daytime or in nighttime. Chapter 8 explores the notion of lot in Hellenistic culture, emphasizing the connection between the *daimon* and lots in Plato's Myth of Er. This chapter concludes with a survey of the doctrine of lots in astrology, but the exploration of the rather extensive material is sensibly narrowed down to topics having greater importance, such as Manilius' idiosyncratic Circle of Athla (a sort of alternative δωδεκάτροπος based on the position of the Lot of Fortune) and the lots found in the *Panaretus*, a lost book cited by the late fourth-century Paulus of Alexandria and attributed to Hermes. As is rightly pointed out, the names of these "Hermetic" lots (Fortune, *Daimon*, Necessity, Eros, Courage, Victory, and Nemesis) are all abstractions and have daemonic connotations [300]. Furthermore, the very important distinction between fatalism and determinism is raised here with the conclusion that Hellenistic astrologers in general, but at least Valens in particular, may have been determinists yet were definitely not fatalists [336].

Chapter 9 continues to investigate the two most important lots, those of Fortune and the *Daimon*, more closely, which makes this chapter perhaps the most technical in the book. Six carefully chosen case studies, mostly from Valens, illustrate the various usages of these lots as well as a derivative of theirs, the Lot of Basis. The chapter concludes with a section on the appearance of the two lots in the techniques of ascertaining the length of life. While the discussion is satisfactory in every detail, the usage of the Lot of Fortune in a katarchic context, for instance, in astrological thought-reading (see, e.g., Hephaestio, *Apot.* 3.4.14–18) might also have been mentioned.

Finally, chapter 10 adduces two more derivative lots (at least in a tradition separate from the Hermetic one), those of Love and Necessity. A section is devoted to the cultural background of the pairing of love and necessity and another one to their astrological role, supplemented with the assessment of all known horoscopes utilizing them, including a recently published horoscope on papyrus, P. Berlin 9825 [Greenbaum and Jones 2017], which, unlike the others, uses the Hermetic formulas. One notable achievement must be mentioned here: the association of the caduceus with the four lots, Fortune, *Daimon*, Eros, and Necessity.

The book ends with conclusions and several appendices, the first of which is a highly useful summary of astrological theory. The rest is mostly a collection of source-texts illustrating the various chapters. Conclusions also provide the reader with an excellent aid to discover the most important themes and threads of the book, which are often buried under the vast material.

What is deeply missed, however, is a chapter on methodology, even if it can be gleaned from the structure of the book that the aim is to read and utilize every piece of source material and scholarly literature related to the broader relationship of astrology and the *daimon*. Still, this barely conscious methodological approach results in a curious contrast between Greenbaum's handling of secondary literature and primary sources on astrology; while arguably all the accessible scholarly contributions are covered (the bibliography runs to 28 pages), the usage of the sources is rather haphazard.

In some cases, it is a mixed result of an uncritical acceptance of the accessible editions and ignorance of their recent re-evaluations. To give an example: texts from Antiochus' *Thesauri* (not *Thesaurus*, as referenced throughout the book) as edited by Franz Boll [1908] are cited six times, although David Pingree, in an article known and even cited four times by Greenbaum, warned that this attribution is largely mistaken [1977, 214–215].

Another problem of minor importance is that Greenbaum appears completely unaware of the syncretic tendency of astrological text-editions prior to the publication of the first volume of Hephaestio of Thebes by Pingree [1973]. Before that year, editors, in an attempt to reconstruct a hypothesized common ancestor of manuscripts, eliminated the boundaries between different recensions, re-workings, epitomes, and excerpts in order to create an idealized but in fact conflated text that had never existed yet might please the aesthetics of similarly inclined classical philologists. This discomforting fact was first emphasized by Pingree [1977, 203], and has been repeated and aptly illustrated by Stephan Heilen recently [2010, 301–303]. Certainly, no readers or reviewers ought to expect Greenbaum to reconstruct, for instance, the different versions behind Emilie Boer's edition of Paulus [1958] from scratch. But the fact that not even allusions are made to the existence of available parallel texts, as in the case of Hephaestio, is rather alarming. Fortunately, the interpretations of the passages are rarely affected by this deficiency.

Compared to these two issues, the third problem is by far more general and pervasive in the book. While the theories expounded by different astrological authors are frequently discussed in various chapters, the development of ideas as it is displayed in the source-texts is scarcely elaborated. I shall illustrate this claim with a randomly chosen example: the relationship of the fifth astrological place and children, discussed in chapter 2 [50–76].

Here, Greenbaum, assessing Manilius' poem, is astounded by his association of health-issues with the fifth place, which is "unlike traditional descriptions of the fifth, which stress fertility and children" [60]. The significations given by Antiochus, "both the acquisition of living beings (ἐμψύχων κτῆσις) and the increase of things pertaining to living" [65], are also received reluctantly. On the other hand, she concedes that many other astrologers associate children with this place.

Had she compared the texts giving descriptions both of the aforementioned δωδεκάτροπος and the ὀκτάτροπος, that is, the Michigan Papyrus and the works of Thrasyllus, Antiochus, and Firmicus Maternus, more carefully, she should have noticed that (except in the description of Firmicus Maternus, who is two or three centuries later than the other authors) while the ὀκτάτροπος-system does associate the fifth place with children, even calling it "the Place of Children", the δωδεκάτροπος-system does not. In the latter system, the fifth place either means some unqualified good fortune or is further elaborated in various ways by Manilius, Antiochus, and Valens [67]. Although one may argue that children can be interpreted as part of the broader context of Good Fortune (and, incidentally, also of the Good *Daimon*) in the δωδεκάτροπος, the interpretations of the planets lingering in the fifth place given by Valens [67] and Firmicus Maternus [70] have only to do with overall fortune and success, not with children.

Admittedly, there exists another tradition that does interpret planets in the fifth place as conveying indications exclusively for children, a tradition found in the works of Paulus and, of course, Olympiodorus [74], as well as in a poem cited in "Palchus" 134 as attributed, probably falsely, to Antiochus [Pérez Jiménez 2011].[1] Also, the amalgamation of the indications of the fifth place in the δωδεκάτροπος and ὀκτάτροπος is attested both in techniques related to the genethlialogical topic of children and in a description by Valens [*Anth.* 4.12.1], overlooked by Greenbaum, which calls the fifth place that "of children, friendship, partnership, slaves, freedmen, acquisition,[2]

[1] Greenbaum does not mention Pérez Jiménez 2011.

[2] Reading «περιποιήσεως» with MS Venice, BNM, gr. Z. 334, c. 55 on f. 181 [Kroll 1900, 158], for the «ἐκποιήσεως» of Valens' manuscripts.

some good deed or good service"—covering also many of the meanings of the 11th place.

This example illustrates how complex the development and transmission of astrological ideas was, and the significance of Greenbaum's failure to separate the distinct but interrelated threads. Her undeclared method of aggregating sources—which are sometimes barely reliable, and at other times attributed to certain authors without solid ground—with occasional oversight of relevant texts seems to have resulted in these three problems in her account.

Greenbaum also falls into the trap of building narratives, one being exceptionally grand and fragile: Porphyry's paramount role as a link between fate, the Platonic *daimon*, and astrology. Whereas Porphyry's importance in this context cannot really be denied, as was already mentioned, Greenbaum throws caution to the winds when she accepts the text entitled « Πορφυρίου φιλοσόφου εἰσαγωγὴ εἰς τὴν Ἀποτελεσματικὴν τοῦ Πτολεμαίου » (Latinized as "Introductio ad Ptolemaei tetrabiblum") as genuinely his. In truth, several arguments may be raised against his authorship beyond the ones mentioned [266–267n122; László 2021]. Most of the *Introductio* attributed to Porphyry is a slightly adapted copy of Antiochus' *Introductio*, which is seen in chapter 30, the very one analyzed and discussed by Greenbaum [268–273]. The investigation of the κύριος is postponed [Boer and Weinstock 1940, 207.28]; but this promise will be fulfilled only in Antiochus, *Epit. intro.* 2.3 [Cumont 1912, 119.22–33], the original of which is now lost. Therefore, this chapter, which for Greenbaum is the key text linking Porphyry's ideas of the personal *daimon* to astrological technicalities, is probably Antiochus' genuine text, otherwise summarized in *Epit. intro.* 1.28 [Cumont 1912, 118.9–22].

A final remark about Antiochus. The two major works associated with his name are the *Thesauri* and the *Introductio*. The *Thesauri* is extant in its fullest form as book 5 of Rhetorius, *Comp.* [Pingree 1977, 210–212]; whereas the *Introductio* is lost, save for a summary in *Epit. intro.* [Cumont 1912, 111–119], several chapters in [Porphyry]'s *Introductio*, and a few fragments. Since several chapters of the *Thesauri* overlap with what is extant of the *Introductio* and are mostly reworked [Pingree 1977, 207–208], it is reasonable to assume that, since Antiochus alone was the author of the *Introductio*, his name was attached to the *Thesauri* only as a mistake by Rhetorius, and that the chapters in Rhetorius' *Comp.* resembling the ones in the *Introductio* are barely adaptations [cf. Schmidt 2009, 21]. Certainly, one cannot entirely dismiss the idea that certain chapters of the *Thesauri* missing from the summary of the *Introductio* may have been authored originally by Antiochus, while their present form is obviously due to Rhetorius. Therefore, it seems more reasonable to associate the *Thesauri* with Rhetorius, not Antiochus.[3]

In the following, I record some minor corrections, additions, and remarks:

(1) 8 n28; 27 n44; 306 n14; 309 n24; 310 n30; 447–449: *CCAG* 1.160 is not genuine Antiochus, but Rhetorius, *Comp*. 5.47 ultimately stemming back to Paulus (as is also acknowledged).

(2) 21 n16 and 306 n14: *CCAG* 7.127 is Rhetorius, *Epit. IIIb* xvi; but it is in fact a copy of Antiochus, *Epit. intro*. 1.1 [Cumont 1912, 112.2–4 (Moon), 111.18–19 (Sun)].

(3) 50: the concept of Jupiter and Venus being the greater and lesser benefics, respectively, is medieval, postdating Guido Bonatti and Leopold of Austria (13th century), who do not mention it.

(4) 63–64; 279 and n4; 311: comparing Dorotheus, *Carm. astrol*. 1.24.6 to the available Latin translation of an Arabic version composed around 800 by al-Khayyāṭ [Heller 1549, d2v–d3], the word "fortune" (Arabic «saʿādah» [Pingree 1976a, 30.5]) most likely refers to material fortune, in the same manner as towards the end of the sentence [Pingree 1976a, 30.6].

(5) 65 n90: *CCAG* I, 157 is Rhetorius, *Comp*. 5.28, using Antiochus, *Epit. intro*. 1.18 [Cumont 1912, 116.3–6], which is found in another version as [Porphyry], *Intro*. 36 [Boer and Weinstock 1940, 209.19–21]. This latter is quoted here.

(6) 143–145 and 149 n159: *CCAG* 7.114–115 is not Antiochus, but Rhetorius, *Epit. IIIb* 21, deriving from Rhetorius, *Comp*. 5.59, which is quoted here in 149 n159. Therefore, delete "dog-men" and "epileptics" on 143. The referenced passage in the *Liber Hermetis* (more correctly, *De triginta sex decanis*) originates from Rhetorius.

(7) 146 n148: read Rhetorius, *Comp*. 5.57 = Rhetorius, *Epit. IV* 1.

(8) 146 n150; 148 n155; 148 and n157; 149 n161: Rhetorius draws on Firmicus, *Math*. 3.4.34, 3.5.39, 3.6.25–26, and 3.4.11, respectively.

(9) 148 and n158: *CCAG* 7.114 is not Antiochus, but Rhetorius, *Epit. IIIb* 21, deriving from Rhetorius, *Comp*. 5.56.

(10) 167 and then *passim*: in fact, the expression "Chaldean order" is an early modern derivation from Macrobius, *In somn*. 1.19.2, and was never used as such by Hellenistic astrological authors, who favor the expression "seven-zoned [sphere]" («ἑπτάζωνος [σφαῖρα]»).

[3] For a recent evaluation of the texts associated with Rhetorius, see László 2020. In the present discussion, however, the results published there are not utilized.

(11) 184 n115: Antiochus' authorship of the calendar, which is the second part of Rhetorius, *Comp.* 6.7 = Rhetorius, *Epit. IIIb* x, is contested [Pingree 1977, 215]. *CCAG* 1.163 is Rhetorius, *Comp.* 5.51. Whether it is from Antiochus is uncertain.

(12) 186 n119: Paulus, *Intro.* 37 is a late addition since it is omitted from the extant summary [Cumont 1912, 95–97; Boer 1958, xxi–xxiv], and not contained in several manuscripts. Its alternative *thema mundi* is probably translated or adapted from Arabic.

(13) 227 nn147–148 and 229 n157: the so-called "scholium 9" of Paulus is not a scholium but an addition to Paulus, *Intro.* 4 in branch β of Paulus' manuscripts [Boer 1958, xii] from Rhetorius, *Comp.* 5.10, which latter is also copied into [Porphyry], *Intro.* 47. It is probably not from Antiochus.

(14) 232 n168: "*Liber Hermetis*" in fact descends from the quoted Rhetorius passage. The difference is due only to misreading «λαμπρομοιρίαν» in a way that would result in «λαμπρὰ ὅρια». It refers to the doctrine of "bright degrees", which has different traditions. Rhetorius, *Comp.* 6.17 tabulates one, which will be later transmitted into Arabic astrology, while *De trig. sex. dec.* 3.1–16 describes a different system. There are many further variants [cf. Heilen 2015, 2.1320–1323].

(15) 257 n87 and 436–437: under "Palchus", the anonymous astrologer of the emperor Zeno must be understood. For No. L486 [436] see now Pingree's edition [1976b, 148–149]; No. L487 [437] appears, among others, as "Palchus" 87, and there is one more horoscope, dated to 479, also in "Palchus" 59, which uses «οἰκοδεσπότης» in meaning #1a [Cumont 1898, 104.15]. This latter is omitted from the *TLG*.

(16) 311 and n32: *CCAG* 1.161 is not Antiochus, but Rhetorius, *Comp.* 5.48.

(17) 311 and n33: *CCAG* 7.113 is not Antiochus, but Rhetorius, *Epit. IIIb* 20, deriving from Rhetorius, *Comp.* 5.65.

(18) 314 and n42: Antiochus, *Epit. intro.* 1.4 [Cumont 1912, 113.8–9], which is apparently a concise summary of [Porphyry], *Intro.* 44, does not use the Lots of Fortune and the *Daimon* in the zodiacal melothesia; however, Rhetorius *Comp.* 5.14, copied as [Porphyry], *Intro.* 50, does, referring to Rhetorius *Comp.* 5.61 = Rhetorius, *Epit. IV* 4, which in parts is clearly based on Valens, *Anth.* 2.37 [Pingree 1977, 214]. The source of the doctrine, therefore, is Valens.

(19) 376 and 480: the horoscopic fragment is probably an insertion into Olympiodorus' text since it appears in the middle of lists of lots [Boer 1962, 53–59] already inserted into the hyparchetype of the extant manuscripts [Burnett and Pingree 1997, 191].

(20) 387 n179 and 475: Abū Maʿshar's Lots of Affection and Love (*sahm al-ulfah wa-al-ḥubb*) and of Poverty and Lack of Means (*sahm al-faqr wa-qillat al-ḥayāh*) (ninth century) together with the other lots were

simply copied by al-Bīrūnī in the 11th century, only the English translations differ. The same is true in the case of his adaptation of the list of lots in his *Kitāb al-mudkhal (al-kabīr)* ([Great] Introduction), into the more concise treatise entitled "Mukhtaṣar al-mudkhal" (The Abbreviation of the Introduction), also known as the *Kitāb al-mudkhal al-ṣaghīr* (Little Introduction). The records for these works are badly confused in the index [551]. It must also be noted that John of Seville, a translator of the *Great Introduction*, interpreted the word « ḥayāh », meaning "life; faculty of growth, sensation or intellect", in a Mercurial way to produce "ingenium" [Lemay 1995–1996, 6.332.439]; see also Adelard of Bath in his translation of the *Mukhtaṣar* writing "useless concern" (6.8: *sollicitudo inefficax*) [Burnett, Yamamoto and Yano 1994, 128]. These lots, however, had already been known in the eighth century by Māshā'allāh: see *Liber Aristotilis* 3.xii.1.2 and 3.xii.3.3. The source is Dorotheus [Burnett and Pingree 1997, 194]; the history of lots is considerably more complicated than what Greenbaum's examination suggests.

(21) 399 n2: only the definition of the tropical zodiac is given here, although until about the fourth century astrologers used a certain type of sidereal zodiac exclusively [Jones 2010]. The reference to Antiochus should also be to Rhetorius, *Comp*. 5.proem.

(22) 400 n6: the description of the quadrant-system does not appear in the genuine text of Olympiodorus, only in the 14th-century reworking composed probably by Isaac Argyrus [Caballero-Sánchez 2013, 94–98].

(23) 404: the expression "Ptolemaic aspects" is a double misnomer in the Hellenistic astrological context. On the one hand, there seems to be no dedicated expression for "aspects" before Arabic astrology, save for words deriving from « σχῆμα » and verbs involving the notion of vision. On the other, the "classical" configurations are first called "Ptolemaic aspects" only in the 17th century, after Kepler's "invention" of the so-called "minor aspects" [*De fundamentis astrologiae certioribus*, thesis 38: [Kepler 1601, c1v]]. In this latter context, it reflects the false but widespread assumption that Ptolemy was the archetypal Hellenistic astrologer.

(24) 408 n22: read Rhetorius 5.7 for Antiochus. For the genuine description of Antiochus, see *Epit. intro*. 1.3 [Cumont 1912, 112.27–28], whose original is perhaps [Porphyry], *Intro*. 6.

(25) 417–418: Emilie Boer's edition of Paulus [1958] is a conflation of different recensions of Paulus' text, and consequently its apparatus must be closely followed. There is no room to cite all the non-trivial testimonies here; it is sufficient, however, to remark that the version found in Rhetorius, *Comp*. 6.30 on ff. 191–196 of MS Paris, BNF, gr. 2425 (Boer's ms *Y*) and the closely related but radically reworked version in Rhetorius, *Epit. IIIb* (Boer's ms family δ [Boer 1958, xii; Pingree 1977, 212–215])

use the language of indication («δηλόω», «σημαίνω», «[ἀπο]δείκνυμι») consistently, in contrast with the language of causation found in the other recensions whose readings are accepted in the edition.

(26) 429–431: for the new edition of Antigonus' examples, see Heilen's edition [2015]: for No. L40, see 1.160–161; for No. L76, see 1.130–131 and 133–137; for No. L113, IV, see 1.168–169 and 172–175.

(27) 433: Greenbaum's suggestion is an excellent and exemplary emendation of the defective text.

(28) 450–452: this is a part of Rhetorius, *Comp*. 5.54; cf. *De sex. dec*. 16.30–45 (seventh consideration) and 16.22 (fifth consideration), which originates in the same Rhetorius' text but provides the numerous emendations used here. Pingree's manuscript (also mentioned on xviii) is the above-mentioned Paris, BNF, gr. 2425, which provides books 5–6 of Rhetorius, *Comp*., including the summaries of Paulus and Antiochus' *Introductio*.

Apart from these deficiencies, mostly rooted in concerns about texts, there are many positive aspects of Greenbaum's approach. She understands Hellenistic astrology, including the perspective of a practitioner. She is sympathetic with features of astrology that are often blamed or ridiculed by others—for instance, the existence of myriads of techniques [301]—solely on the ground of preconceptions and ignorance. This is a refreshing advance beyond the occasional presentist biases of other scholars. At the same time, she laudably avoids, at least in the majority of possible cases, the pitfall of anachronism in astrological techniques, which could lead to confusion. The excellent quality of English writing must also be highlighted.

In summary, Greenbaum's *The Daimon in Hellenistic Astrology* will indubitably enthrall those interested in the difference between fatalism and determinism and in the solutions provided by astrologers of the past. Moreover, it yields insight into the technicalities and practices of Hellenistic astrology.

BIBLIOGRAPHY

CCAG *Catalogus codicum astrologorum Graecorum*.

TLG *Thesaurus Linguae Graecae* (https://stephanus.tlg.uci.edu/).

Beck, R. 2006. *The Religion of the Mithras Cult in the Roman Empire*. New York.

____ 2007. *A Brief History of Ancient Astrology*. Malden, MA.

Boer, E. 1958. *Pauli Alexandrini elementa apotelesmatica*. Leipzig.

Boer, E. 1962. *Heliodori ut dicitur in Paulum Alexandrinum commentarium.* Leipzig.

Boer, E. and S. Weinstock. 1940. "Porphyrii philosophi introductio in tetrabiblum Ptolemaei". *CCAG* 5.4: 187–228.

Boll, F. 1908. "Excerpta ex Antiochi thesauro et ex Heliodoro". *CCAG* 7: 107–122.

Burnett, C. and D. Pingree. 1997. *The* Liber Aristotilis *of Hugo of Santalla*. London.

Burnett, C.; K. Yamamoto; and M. Yano. 1994. *Abū Maʿšar: The Abbreviation of the Introduction to Astrology*. Leiden.

Caballero-Sánchez, R. 2013. "Historia del texto del *Comentario anónimo al* Tetrabiblos *de Tolomeo*". *MHNH* 13: 77–198.

Cumont, F. 1898. "Palchi capitula selecta". *CCAG* 1: 94–117.

—— 1912. "Librorum astrologicorum epitome Parisina". *CCAG* 8.3: 91–119.

Greenbaum, D. G. 2016. *The Daimon in Hellenistic Astrology: Origins and Influence*. Leiden/Boston.

Greenbaum, D. G. and A. Jones. 2017. "P.Berl. 9825: An Elaborate Horoscope for 319 CE and Its Significance for Greek Astronomical and Astrological Practice". *ISAW Papers* 12. https://dlib.nyu.edu/awdl/isaw/isaw-papers/12/.

Heilen, S. 2010. "Problems in Translating Ancient Greek Astrological Texts". Pp. 299–329 in A. Imhausen and T. Pommerening edd. *Writings of Early Scholars in the Ancient Near East, Egypt and Greece: Translating Ancient Scientific Texts*. Berlin/New York.

—— 2015. *"Hadriani genitura". Die astrologischen Fragmente des Antigonos von Nikaia*. 2 vols. Berlin/Boston.

Heller, J. 1549. *Albohali Arabis astrologi antiquissimi, ac clarissimi de iudiciis natiuitatu[m] liber unus, antehac non editus*. Nuremberg.

Jones, A. 2010. "Ancient Rejection and Adoption of Ptolemy's Frame of Reference for Longitudes". Pp. 11–44 in A. Jones ed. *Ptolemy in Perspective: Use and Criticism of his Work from Antiquity to the Nineteenth Century*. New York.

Kepler, J. 1601. *De fundamentis astrologiae certioribus*. Prague.

Kroll, W. 1900. Περὶ τῶν δώδεκα τόπων. *CCAG* 2: 158.

László, L. 2020. "Rhetorius, Zeno's Astrologer, and a Sixth-Century Astrological Compendium". *Dumbarton Oaks Papers* 74: 329–350.

László, L. 2021. “Revisiting the Authenticity of Porphyry’s *Introduction to Ptolemy’s* Apotelesmatics”. *Classical Philology* (in press).

Lemay, R. 1995–1996. *Albumasar. Liber introductorii maioris ad scientiam judiciorum astrorum*. 9 vols. Naples.

Pérez Jiménez, A. 2011. “Poésie et astrologie chez Antiochos”. Pp. 181–191 in I. Boehm and W. Hübner edd. *La poésie astrologique dans l’Antiquité*. Paris.

Pingree, D. 1971. “The Astrological School of John Abramius”. *Dumbarton Oaks Papers* 25: 189–215.

—— 1973. *Hephaestionis Thebani apotelesmaticorum libri tres*. Leipzig.

—— 1976a. *Dorothei Sidonii carmen astrologicum*. Leipzig.

—— 1976b. “Political Horoscopes from the Reign of Zeno”. *Dumbarton Oaks Papers* 30: 133–150

—— 1977. “Antiochus and Rhetorius”. *Classical Philology* 72: 203–223.

Schmidt, R. 2009. *Antiochus, with Porphyry, Rhetorius, Serapio, Thrasyllus, Antigonus et al.: Definitions and Foundations*. Cumberland, MD.

The Meaning of « ἐνὶ ὀνόματι » in the *Sectio canonis*

by

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Abstract

A new interpretation is proposed of the crucial expression «ἑνὶ ὀνόματι» ("in one name") as applied to ratios of the musical concords in the preface of the *Sectio canonis* ascribed to Euclid. A link is also established with the name of one of the irrational lines introduced by Euclid in *Elements* 10. Past interpretations of the expression are discussed and shown to be inadequate.

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1. An interpretative problem

The introduction of the *Sectio canonis* ascribed to Euclid ends by setting a correspondence between concordant notes and certain kinds of numerical ratios:

> πάντα δὲ τὰ ἐκ μορίων συγκείμενα ἀριθμοῦ
> λόγῳ λέγεται πρὸς ἄλληλα, ὥστε καὶ τοὺς φθόγγους
> M158.20 ἀναγκαῖον ἐν ἀριθμοῦ λόγῳ λέγεσθαι πρὸς ἀλλήλους·
> τῶν δὲ ἀριθμῶν οἱ μὲν ἐν πολλαπλασίῳ λόγῳ λέγονται,
> οἱ δὲ ἐν ἐπιμορίῳ, οἱ δὲ ἐν ἐπιμερεῖ,[1] ὥστε καὶ τοὺς
> φθόγγους ἀναγκαῖον ἐν τοῖς τοιούτοις λόγοις λέγεσθαι
> πρὸς ἀλλήλους. τούτων δὲ οἱ μὲν πολλαπλάσιοι καὶ ἐπι-
> μόριοι ἑνὶ ὀνόματι λέγονται πρὸς ἀλλήλους.[2]
> Γινώσκομεν δὲ καὶ τῶν φθόγγων τοὺς μὲν συμφώ-
> νους ὄντας, τοὺς δὲ διαφώνους, καὶ τοὺς μὲν συμφώνους
> μίαν κρᾶσιν τὴν ἐξ ἀμφοῖν ποιοῦντας, τοὺς δὲ διαφώ-

[1] In a multiple ratio, the greater term is a multiple of the lesser. In an epimoric ratio, the excess of the greater term over the lesser term is a part (i.e., a divisor) of the latter. In an epimeric ratio, this excess is “parts” of the lesser term: “parts” of a given number is any number less than the given one that is not a part of it. The current characterizations of these relations as fractions, as we shall see below, is simply misleading. I shall use the denominations “epimoric” and “epimeric” in place of the more common “superparticular” and “superpartient”.

[2] A look at the particles in this sentence suggests that something has gone wrong. The initial « δέ » is mildly adversative, as is the « δέ » at the beginning of the sentence opening the second paragraph. This is in line with the careful disposition of the *cola* in the whole introduction: independent, principal clauses are always introduced by conjunctive « δέ », and inside them the subclauses in contraposition are regularly marked by the canonical correlative « μέν...δέ ». Moreover, every « μέν » is answered by a « δέ ». The only exception is the « μέν » in the underlined sentence [lines 24–25]: a subsequent clause such as « οἱ δὲ ἐπιμερεῖς οὔ » (whereas epimeric do not) is surely missing. I regard the correction as certain, given the strictly analogous structure of the immediately following sentence. Nothing in the interpretation that I shall develop depends on this textual detail, however.

> νους οὔ. τούτων οὕτως ἐχόντων εἰκὸς[3] τοὺς συμφώνους M160.1 φθόγγους, ἐπειδὴ μίαν τὴν ἐξ ἀμφοῖν ποιοῦνται κρᾶσιν τῆς φωνῆς, εἶναι τῶν ἐν ἑνὶ ὀνόματι πρὸς ἀλλήλους λεγομένων ἀριθμῶν,[4] ἤτοι πολλαπλασίους ὄντας ἢ ἐπιμορίους. [Jan 1895, 149.8–24; Menge 1916, 158.18–160.4; Barbera 1991, 114.15–116.11]
>
> Now all things that are composed of parts are compared to each other in a ratio of number, so that notes too must be compared to each other in a ratio of number. Some numbers are compared in a multiple ratio, some in an epimoric ratio, and some in an epimeric ratio, so that notes must also be compared to each other in these kinds of ratio. And of these, the multiple and the epimoric are compared to each other in a single name.
>
> Among notes we also recognize some as concordant, others as discordant, the concordant making a single blend out of the two, whereas the discordant do not. In view of this, it is to be expected that the concordant notes, since they make a single blend of sound out of the two, are among those numbers which are compared to each other in a single name, being either multiple or epimoric. [Barker 1984–1989, 2.192–193, modified]

Two entangled problems in the argument have attracted the attention of commentators. The first is the status of the so-called "principle of consonance", namely, that concordant notes must be represented either by multiple or epimoric ratios.[5] I shall not discuss this issue here. The second is the meaning of the expression «(ἐν) ἑνὶ ὀνόματι» (in a single name): this is the characterization, admittedly rather cryptic, of multiple or epimoric ratios that allows setting any of them in correspondence with notes that make a single blend.[6]

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[3] εἰκός: notice the determination of likelihood in a place where in the first paragraph one finds two occurrences of a determination of necessity (ἀναγκαῖον). I would link this feature to a perceptibly less firm status of the assumed correspondence between notes and numbers. Compare the more precise statement occurring on the second line of the first paragraph: «τοὺς φθόγγους ἀναγκαῖον ἐν ἀριθμοῦ λόγῳ λέγεσθαι πρὸς ἀλλήλους».

[4] The *variatio* «(ἐν) ἑνὶ ὀνόματι» between lines 158.25 and 160.2 is very likely a scribal *lapsus*, even if it is not clear whether the mistake is a haplography or a dittography.

[5] The problem lies in the fact that the introduction of the *Sectio* apparently expresses the principle as a sufficient condition only, whereas in *Sectio* 11 the converse is explicitly applied.

[6] As the second underlined clause confirms [lines 160.2–3], the demonstrative «τούτων» in the line 158.24 refers to numbers and not to classes of ratios or of notes. As a consequence, what is qualified by the "single name" clause is each single ratio, not

2. Ancient commentators

The ancient commentators did not address the question of the "single name". Neither Porphyry nor Boethius, when reporting the introduction of the *Sectio*,[7] remains faithful to the received text.[8] Porphyry skips altogether the portion of the argument beginning with the first sentence underlined in the text. Boethius provides a paraphrase of the entire final part but does not render the occurrences of "single name" in his abridged version. This could mean either that they thought the meaning of "single name" unimportant or obvious or that they were too puzzled about it to point out the problem or to survey earlier (if any existed) interpretations.

3. Current interpretations

The interpretations of the expression "single name", which I shall call "current", derive from a proposal first elaborated in a paper by L. Laloy [1900], a proposal which has been rediscovered a few times since then. Laloy introduces his central claim when he explains «(ἐν) ἑνὶ ὀνόματι» by remarking that in ordinary usage ancient Greek has single words to denote each particular multiple and epimoric ratio only. As he observes, terms denoting epimoric ratios, being more complex in principle than terms for multiple ratios, are formed according to a fixed rule so that any such ratio can be easily named. But the ordinary language of ancient Greece does not offer similar terms for the other kinds of ratios. The occurrence of single words designating epimeric ratios in Nicomachus, *Intro. arith.* 1.20–21—at any rate much later a work than the *Sectio*—is restricted to a fairly technical context. Indeed, the very exposition in Nicomachus, Laloy says, suggests that he is really handling very uncommon terms or maybe even coining them.[9]

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whole classes of multiple or epimoric ratios (which would be a truism). The correspondence set forth in the introduction of the *Sectio* requires in fact that one single ratio be related to one single concord, since any of the latter makes a single blend. Of course, any single epimoric or multiple ratio stands for a whole class of equivalent ratios. For simplicity, I shall refer to each class as if it were one single ratio.

7 At Düring 1932, 90.7–23, and *De inst. mus* 4.1–2 [Friedlein 1867, 301.12–302.2], respectively. It should be noted that Porphyry does not mention the *Sectio* in his quote, whereas he expressly refers to it at Düring 1932, 98.19, when reporting an extensive initial segment of the deductive part of the same treatise.

8 We may exclude the possibility that the occurrences of "single name" are later additions to the introduction of the *Sectio*, since they are integral parts of the argument.

9 The shorter account by Theon of Smyrna [Hiller 1878, 78.6–22] employs only two- or many-word phrases to name epimeric ratios; elsewhere [109.15–110.18], Theon

As for the the omission of the phrase «(ἐν) ἑνὶ ὀνόματι» in Boethius' abridged translation, Laloy has this explanation:

> Le fait de langage auquel il est fait allusion est propre au grec: les mots *sesquiquartus*, *sesquiquintus*,...sont des mots savants forgés pour les besoins d'un ouvrage d'arithmétique: ils ne peuvent être invoqués comme des preuves. Euclide, au contraire, trouvait toutes formées, dans sa langue, des locutions usuelles qui sont à ses yeux des témoins irrécusables. [Laloy 1900, 239]

Scholars after Laloy have either sided with him or rediscovered his interpretation: so, for example,

P. Tannery 1904, 445,
C. E. Ruelle 1906, 319,[10]
E. Lippmann 1964, 154,
W. Burkert 1972, 383n63,[11]
A. Barker 1981, 2–3; 1984–1989, 2.192–193 nn6–8, and
A. Barbera 1991, 55–58.[12]

In her Italian translation of the *Sectio*, L. Zanoncelli [1990, 63–64] further qualifies Laloy's interpretation in asserting that the reference is to the single numeral appearing in the designation of a (multiple or) epimoric ratio,[13] such as «ἐπίτριτος» and so on.[14] Unfortunately, besides regularly formed terms for epimeric ratios such as, e.g., «ἐπιδίτριτος»,[15] which contains two

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introduces one-word denominations that are different from Nicomachus'. This means that the terminology was not fixed but does not entail that the terms were of recent coinage. Theon and Nicomachus were contemporaries.

10 In fact simply relying on Tannery's authority.

11 Burkert does not argue his claim but adduces (pseudo-)Aristotle, *Prob*. 19.34 and 41 as *loci paralleli*. Yet only the latter has a reliable text and, though it can be compared more properly to some propositions in the *Sectio*, it does not bear on the principles set forth in the introduction [see the translation in Barker 1984–1989, 2.95–96].

12 Barbera apparently came to know of Laloy's paper after a communication by A. Kárpáti.

13 The name of an epimoric ratio is always the name of the ratio in lowest terms identical to it. As an epimoric ratio in lowest terms is of the form $(n+1):n$, only one "number" (in Greek sense, hence excluding unity) has to be named. This is already pointed out by Theon of Smyrna, [Hiller 1878, 77.5–7]. A similar remark, this time pointing to the single number appearing in the anthyphairetic expression of an epimoric ratio, is found in Fowler 1999, 141.

14 This is "one third more" and corresponds to $4/3$ in least terms.

15 This is "two thirds more" and corresponds to $5/3$ in lowest terms.

numerals, there are alternative names of the same ratios containing one numeral, in this case «ἐπιδιμερής». Therefore, Zanoncelli has not isolated a characterization that can serve as a criterion for singling out multiple and epimoric ratios.

Alternative interpretations take different routes. Assuming that some precise word is referred to in the introduction of the *Sectio*, proposals for such a single word have been advanced by a number of scholars. Jan suggests "potior" (more powerful)—

> Porphyri...nomen illud commune affert, cum potiores (κρείττους) dicit has duas rationes: Euclides ea brevitate et dicendi inopia haec agit, ut excerpta potius dicas quam ipsa verba hominis sagacissimi. [Jan 1895, 118][16]
>
> Porphyry provides such a common name when he says that these two ratios are "more powerful". Euclid treats these things so succinctly and in so few words, that you would regard them more as excerpts than the words themselves of this most brilliant man.

—and Mathiesen puts forward "consonant" [Mathiesen 1975, 254n12]. But these alternatives are defended on the basis of an incorrect reading of a text by Porphyry, who asserts only that multiple and epimoric ratios are more powerful than epimeric in the same way as consonant and melodic notes are more powerful than dissonant ones, and concludes that one should thereby "fit" («ἐφαρμοστέον») multiple and epimoric ratios to consonant notes, epimeric ratios to dissonant notes.[17] Porphyry's explanation is in fact nothing but a slight restatement of the very passage in Ptolemy's *Harmonica* 1.5 on which he is commenting [see Düring 1930, 11.8–20]. Both Porphyry and Ptolemy are far from claiming that either "consonant" or worse yet "more powerful" is the single name referred to in the *Sectio*: neither mentions the "name" and Ptolemy even ascribes the whole argument expounded in 1.5 to the "Pythagoreans".[18]

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16 The absence of the "name" induced Jan to conjecture the existence of a richer version of the argument in another Euclidean treatise.

17
> Τῶν οὖν ἀνίσων λόγων οἱ μὲν πολλαπλάσιοι καὶ οἱ ἐπιμόριοι κρείττους τῶν ἐπιμερῶν, τῶν δ' ἀνισοτόνων κρείττους οἱ ἐμμελεῖς καὶ οἱ σύμφωνοι τῶν ἀσυμφώνων. ἐφαρμοστέον ἄρα τοὺς ἐπιμορίους καὶ πολλαπλασίους λόγους τοῖς συμφώνοις, τοὺς δ' ἐπιμερεῖς τοῖς ἀσυμφώνοις. [Düring 1930, 98.3–6]

18 Just after that, Ptolemy quickly summarizes formalized arguments—he asserts that they conclude γραμμικώτερον (more rigorously)—which are an abridgment of *Sectio* props. 11, 10, 12; and he refers to the results established in props. 3, 6, 13, and 16.

More interesting is the *mathematical* explanation provided by Ptolemy of the asserted superiority of multiple and epimoric ratios to epimeric ratios. The basic assumption, Ptolemy says, was that

> οἱ μὲν ἴσοι τῶν ἀριθμῶν παραβληθήσονται τοῖς ἰσοτόνοις φθόγ-
> D11.10 γοις, οἱ δὲ ἄνισοι τοῖς ἀνισοτόνοις, τοὐντεῦθεν ἐπάγουσιν, ὅτι καθάπερ
> τῶν ἀνισοτόνων φθόγγων δύο ἐστὶν εἴδη πρὸς ἄλληλα τὰ πρῶτα, τό τε
> τῶν συμφώνων καὶ τῶν διαφώνων, καὶ κάλλιον τὸ τῶν συμφώνων, οὕτως
> καὶ τῶν ἀνίσων ἀριθμῶν δύο γίνονται πρῶται διαφοραὶ λόγων, μία μὲν
> ἡ τῶν λεγομένων ἐπιμερῶν καὶ ὡς ἀριθμὸς πρὸς ἀριθμόν,[19] ἑτέρα δὲ ἡ
> τῶν ἐπιμορίων τε καὶ πολλαπλασίων, ἀμείνων[20] καὶ αὕτη τῆς ἐκείνων κατὰ
> τὴν ἁπλότητα τῆς παραβολῆς,[21] ὅτι μέρος ἐστὶν ἁπλοῦν ἐν αὐτῇ τῶν μὲν
> ἐπιμορίων ἡ ὑπεροχή, τῶν δὲ πολλαπλασίων τὸ ἔλαττον τοῦ μείζονος.[22]
> [Düring 1930, 11.9–17]
>
> Equal numbers should be associated with equal-toned notes, and unequal numbers with unequal-toned; and from this they argue that just as there are two primary classes of unequal-toned notes, that of the concords and that of the discords, and that of the concords is finer, so there are also two primary distinct classes of ratio between unequal numbers, one being that of what are called "epimeric"or "number to number" ratios, the other being that of the epimorics

Accordingly, Porphyry's transcription of a substantial part of the *Sectio*, with explicit reference to its title and mention of Euclid as the author [Düring 1932, 98.19], is but an expansion of Ptolemy's sketchy proofs. On the issue, see the discussions in Barker 1994 and Barker 2000, 54–73. Barker assigns the Pythagorean argument to Archytas.

[19] For the latter denomination, see Plato, *Tim.* 36b. It might be surmised that the former is a more recent and more technical term, the latter an archaic one. Alternatively, we might have here simply a quotation from Plato without technical implications. The Platonic expression is given a wrong explanation in Theon, *Exp.* [Hiller 1878, 80.7–14]: Theon asserts that the phrase singles out ratios different from those he has just described, not realizing that his own classification (which included multiple-epimoric and -epimeric ratios besides the usual ones) is exhaustive.

[20] Porphyry varies the term to «κρείττους» using the plural to refer to the ratios.

[21] Barker's translation [1984–1989, 2.285] has "comparison" (at the beginning of the quotation, the passive future of the related verb is rightly translated "associated"). But «παραβολή» (application) is here employed as a technical term coming from the theory of the application of areas: an area is applied to a straight line when the area is transformed into a rectangle having the straight line as one of its sides. In numerical context, «παραβολή» simply means "division" or the resulting "quotient", and the corresponding verb («παραβάλλω») means "to divide": for the verb, see, e.g., Acerbi and Vitrac 2014, 159n36 and Tannery 1893–1895, 2.278 *sub voce*.

[22] Porphyry's paraphrasis [Düring 1932, 98.7–13] simply makes the argument clumsier.

> and multiples; and of these the latter is better than the former on account of the simplicity of the application, since in this class the difference, in the case of epimorics, is a simple part, while in the multiples the lesser is a simple part of the greater. [Barker 1984–1989, 2.284–285, slightly modified]

Ptolemy's argument appears to imply that the "single name" is warranted not by language but by a mathematical property shared by both multiple and epimoric ratios.

A quick reading of this passage and of the paraphrase in Porphyry may underlie arguments that the "name" is "more powerful" or "consonant". But note that Ptolemy (or his "Pythagorean" sources) reverses the order of the main inference found in the *Sectio* by making the classification of ratios depend on that of concords. Moreover, since concords are defined on aesthetic grounds just at the end of the preceding chapter of the *Harmonica* [see Düring 1930, 10.25–28], the same semantic field is naturally at one's disposal to denote ratios too. For this reason, Ptolemy qualifies multiple and epimoric ratios as "better" (« ἀμείνων ») than epimeric ratios. Still, we should not mistake such a judgment as grounds for identifying the "name" in the *Sectio*.

A. C. Bowen [1991, 176–182] argues at length for "concordant" as the name, using an approach that is different from any of the others just described. The core of the argument is that the predicates "multiple" and "epimoric" can be applied directly to notes since in the *Sectio* phenomenal musical sounds (i.e., sounds as described by intervals related by certain ratios) and objective musical sounds (i.e., sounds analyzed as series of consecutive motions) are identified. This reading precludes from the very outset any reference to numbers and ratios as such, and the problem of the "single name" really evaporates since what we actually hear are the ratios. Solving a problem by dissolving it is an elegant way to cope with *aporias* but we shall presently see that a satisfactory answer can be given within the traditional interpretative framework, in which notes and ratios are kept distinct.

The interpretations of the "single name" phrase proposed by most modern scholars stress a linguistic feature, although one linked to a mathematical property. The basic weakness of all such proposals lies in the fact that in ancient Greek it was far from impossible to form one-word descriptions of epimeric ratios. On the contrary, ancient Greek is more than capable of doing this, as we have seen. Moreover, it is disputable that Nicomachus' denominations of epimeric ratios were his own invention: after all, he does not claim it as his own and the names are formed in accordance with a rule that is a natural extension of the one for epimoric ratios. Nor is it a problem that the first occurrences of names for particular epimeric ratios are found first in Nicomachus and in Theon of Smyrna, considering what has survived of ancient number theory.[23]

4. The concept of name (ὄνομα)

There is a very specific property of multiple and epimoric ratios making them suitable to be ranged under the extension of the same description. It is a mathematical and not a linguistic feature, even if the two aspects have a large overlap *because* the names of such ratios are in general built up looking at some mathematical property.

A first point, showing that the context is less specifically linguistic than usually believed, can be made concerning the verb « λέγειν ». It occurs six times in the introduction of the *Sectio*, in the passive and possibly qualified by « πρὸς ἀλλήλους » (to each other). The first four occurrences refer to notes or numbers that are in relation to each other by means of a ratio; the latter two refer to numbers that are in relation to each other "in a single name". The parallelism of the two verbal constructions is obvious. Translations of « λέγεσθαι » such as "to be spoken of"[24] load the expression with philosophical overtones and unduly stress the linguistic connotation of the verb. The most proper translation of « λέγεσθαι πρὸς ἀλλήλους » is "to be compared to each other" in all its occurrences here.

This is in line with one of the current meanings of « λέγω » [see Liddell, Scott, and Jones 1968, *sub voce* (B).I] and comparable to the usage in the preface to Archimedes, *De lineis spiralibus*:[25]

> τᾶν ἀνισᾶν
> γραμμᾶν καὶ τῶν ἀνίσων χωρίων τὰν ὑπεροχάν, ᾇ
> ὑπερέχει τὸ μεῖζον τοῦ ἐλάσσονος, αὐτὰν ἑαυτᾷ συν-
> H12.10 τιθεμέναν δυνατὸν εἶμεν παντὸς ὑπερίσχειν τοῦ προ-
> τεθέντος τῶν ποτ' ἄλλαλα λεγομένων.
> [Heiberg 1910–1915, 2.12.7–11]

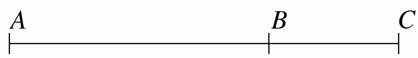
[23] Theon's account [Hiller 1878, 74.15–75.25] might suggest that older classifications knew only of multiple and epimoric as independently defined classes of ratios; but the closing of his exposition [1878, 75.17–21] seems to imply that Theon suggests this possibility only by way of rhetorical expedience.

[24] E.g., Barker 1984–1989, 2.192–193.

[25] A similar formulation is also found in the fifth assumption at the beginning of *De sph. et cyl.* 1.

> Of unequal lines and of unequal areas, the excess by which the greater exceeds the lesser, if added to itself, can exceed any proposed ‹magnitude› among those that can be compared to each other.

What is more, even if the term «ὄνομα» has an obviously prominent linguistic connotation, it also carries a peculiar and well-defined mathematical meaning. To see this, notice first that in general a ratio between two numbers can be represented as a divided line as follows:[26]



Let us suppose, without loss of generality, that AB is the greater segment of AC. It may happen that BC measures exactly AB. But by definition

$AC:BC$ is multiple whenever BC measures AC (and hence AB) exactly.
$AC:AB$ is epimoric whenever BC measures AB (and hence AC) exactly.[27]

Therefore, $AC:BC$ is multiple if and only if $AC:AB$ is epimoric; and this happens if and only if BC measures exactly AB. As a consequence, multiple and epimoric ratios are built upon a single reference number BC, let us call it a single "name", in the strong sense that BC is the common measure of all the numbers at issue in such ratios.

No other ratios share this property. Such a fundamental characterization of multiple and epimoric ratios is completely obscured by their usual representation as ratios of the form $mn:m$ and $(mn+m):mn$, respectively, or, if reduced to lowest terms as is usually and even more misleadingly done, $n:1$ and $(n+1):n$. In particular, what is lost is the key role played by the notion of "part" of a number in the ancient definitions of multiple and epimoric ratios.

The characterization just expounded is purely mathematical; for two reasons, it does not coincide with the one that was expounded in the preceding section and is an integral part of the "current" interpretation. First, in the latter, the "single name" of multiple and epimoric ratios derives from the (name of the) number corresponding to the greater segment AB, that is, number n in the ratios $n:1$ and $(n+1):n$. But in the interpretation

[26] Nothing in the following argument depends on the possibility of representing numbers by line segments.

[27] Cf. p. 39 n2 above. Ancient definitions can be found, e.g., in Theon of Smyrna, *Exp.* [Hiller 1878, 76.8–14 (multiple), 76.21–77.2 (epimoric)]. Less perspicuous definitions are in Nicomachus, *Intro. arith.* 1.18–19. Of course, the definitions state necessary and sufficient conditions.

just presented, the "single *name*" is the *number itself* (and not its name)[28] corresponding to the *lesser* segment BC (namely, number m in the ratios $mn : m$ and $(mn + m) : mn$). If we like, when dealing with a ratio, our focus can be either on the common measure of the terms of the ratio or on the pair of numbers by which one must multiply such a common measure to generate the terms themselves.[29] My proposal assumes the former point of view; the "current" interpretation surveyed above assumes the latter.

Second, since what is referred to in the ordinary names of multiple and epimoric ratios is the number corresponding to the greater segment, the present interpretation does not require that there be a predicate which answers to "concordant" and which singles out multiple and epimoric ratios.[30]

In my view, the phrase "single name" in the introduction of the *Sectio* should be taken as a reference to a "single *name*", i.e., to a mathematical object. Thus, I would render the sense of

> τούτων δὲ οἱ μὲν πολλαπλάσιοι καὶ ἐπιμόριοι ἑνὶ ὀνόματι λέγονται πρὸς ἀλλήλους [Menge 1916, 158.24–25]

by

> The multiple and epimoric numbers are compared to each other [*scil.* in ratio to each other] with respect to a single reference-number.

All of this would be just a refinement and a completion of Ptolemy's explanation κατὰ τὴν ἁπλότητα τῆς παραβολῆς (because of the simplicity of the application) [see p. 44, above], were it not for a lucky accident that permits adding some historical flesh that squares rather well with the proposed interpretation. This is the use of the term "name" for a mathematical object in the theory of irrational lines.[31]

[28] I shall henceforth use "name" in italics to denote a mathematical object denominated in this way.

[29] Of course, the two multiples are the terms of the ratio expressed in lowest terms.

[30] Unless the predicate is simply taken to be "having a single *name*" (i.e., being described by a single reference number). If we assume that the ratios are in lowest terms, we might even hold that there is in fact a common predicate to all multiple and epimoric ratios, namely, "having the unit as their *name*".

[31] I have not been able to find any relevance to our subject in the notion of "homonymous" parts and numbers at work in *Elem.* 7.37–39 and in Diophantus' *Arithmetica*. Apollonius' usage of «ὁμώνυμος» as reported by Pappus in *Coll.* 2.1–16 deserves a more careful assessment but appears to be irrelevant to our purposes.

In book 10 of the *Elements*, a binomial—in Greek, ἐκ δύο ὀνομάτων (from two *names*)—is a line formed by composition of two expressible lines that are commensurable in power only.[32] It is first defined at *Elem*. 10.36 and its *names* are expressly mentioned dozens of times in the rest of book 10. In a diagram analogous to the one set out above, 10.36 amounts to saying that a line *AC* is a binomial if it is obtained by composing two expressible straight lines *AB* and *BC* such that *AB* is incommensurable with *BC* but the squares on them are measured by a common area.

In a testimony whose reliability is controversial, however, Pappus, on the authority of Eudemus, assigns a seminal role to Theaetetus, who is reported to have introduced and named the three basic kinds of irrational lines (medial, binomial, and *apotome*), linking them to the three basic means (geometric, arithmetic, and harmonic respectively).[33] At 968b19–20, the Peripatetic tract *De lineis insecabilibus* mentions the binomial line as well as the *apotome*.[34] It is, therefore, almost certain that the denomination "binomial" was introduced before the composition of *Elem*. 10. Moreover, the lines from which an *apotome* is obtained by subtraction are expressly called its *names* in *Elem*. 10.112–114[35] and such *names* of an *apotome* are set in one-to-one correspondence with the *names* of a suitable binomial. This suggests that

32 An expressible line is any straight line set out as a reference-line or any line commensurable in power with it. Two lines are commensurable in power when the squares on them are commensurable. Lines commensurable in power are said to be "commensurable in power only" when they are not commensurable [*Elem*. 10.def.2]. On the notion of "expressible line", see also p. 52 n39, below.

33 Junge and Thomson 1930, 63: see also 138, where Eudemus is not mentioned. The authenticity of book 1 of Pappus' *Commentary* is doubtful: see Vitrac 1990–2001, 3.417–21. Pre-Euclidean interest in the theory of irrationals is of course attested in Plato's *Theaetetus*.

34 This small treatise is a product of the Peripatetic school. A work with the same title is included also in the list of Theophrastus' writings: see, e.g., Diogenes Laertius, *Vitae philos*. 5.42. Therefore, it is reasonable to assume that it was composed before the *Elements*.

35 The definition of an *apotome* in 10.73 is exactly symmetrical to the one of a binomial in 10.36: an *apotome* is a line formed by subtraction of two expressible lines that are commensurable in power only.

the *names* had a more widespread application than the one that the extant sources attest and that they lasted well beyond Euclid's times: since *Elem.* 10.112–114 are absent in the Arabo-Latin tradition, we may infer that they were introduced later into the text, very likely after Apollonius and certainly before Pappus, who read them.[36]

An even later tradition, which surfaces in the Theonine manuscripts and in the medieval Greco-Latin translation of the *Elements*, designates the segments from which other irrational lines are formed as *names*. This happens in the enunciations of *Elem.* 10.43–47, e.g., where a corrector in the unique pre-Theonine ms. Vat. gr. 190 has put the same qualification in the text of prop. 10.46 as well [see Heiberg and Stamatis 1969–1977, vol. 3 *in app. ad locos*].

As the two lines composing the binomial are called its *names*, one is naturally led to assume that the existence of some well-defined and basic mathematical object called *name* should precede the choice of such a denomination as "from two *names*". But then, what was that *name*?

To clarify the point, it may be useful to refer briefly to Aristotle, *Meta.* 10.1, where he lists examples of things for which it is necessary to set out more than one reference-measure. The last items are «καὶ ἡ διάμετρος δυσὶ μετρεῖται καὶ ἡ πλεύρα καὶ τὰ μεγέθη πάντα» (both the diagonal and the side are measured by two <reference-measures> as well as all magnitudes) [*Meta.* 1053a17–18]. Surprisingly enough, commentators since Alexander have been at a loss in explaining such a transparent sentence.[37]

Very simply, all Aristotle says is that since side and diagonal (of a square) are incommensurable, by definition there is no common measure to them

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36 But it is likely that the *names* for the *apotome* were introduced to mimic the attested Euclidean usage for the binomial, not as a reference to a longstanding tradition harking back to earlier investigations.

37 Alexander of Aphrodisias:

> For if <the diagonal> is measured, say, by a finger, the finger is twofold: the essence and the form of the finger and this <finger> here itself measuring it; and similarly also the side is measured by two since it is a magnitude. [Hayduck 1891, 610.4–6]

Aquinas:

> Similiter etiam est diameter circuli vel quadrati, et etiam latus quadrati: et quaelibet magnitudo mensuratur duobus: non enim invenitur quantitas ignota nisi per duas quantitates notas. [Cathala 1935, liber 10, lectio 2, §1951, 561b]

and, hence, to measure both of them one has to set out two independent reference-measures.[38] The generalization to all magnitudes is straightforward when they are geometrical and simply a matter of analogy when they are not. Nor should the syntax of the sentence bewilder us [Ross 1924, 283]: when a clause has two subjects, referring the verb (hence put in the singular) to the first subject and then adding the second subject paratactically is not an unknown pattern in Greek prose [cf., e.g., Smyth 1920, §966]. The Aristotelian allusion entails that setting out two different reference-measures in the field of irrational lines was a matter of course. Aristotle calls each of them the μέτρον but this was clearly a most generic denomination, dictated by the very subject of the second part of *Meta*. 10.1.

Let us return to the binomial. The two segments that compound such lines are incommensurable. Thus, it follows that two reference-lines are needed to measure them. My hypothesis is that the two *names* in the denomination of the binomial refer exactly to this feature.

H. Bonitz:

> hoc videtur significare, et rationem quae diagonalem inter et latus intercedit, et cuiuslibet planae figurae magnitudinem non definiri una linea mensurata, sed duabus mensuratis et mensurae numeris inter se multiplicatis. [Bonitz 1849, 418]

W. D. Ross:

> the diagonal is conceived as consisting of two parts, a part equal to the side, and a part which represents its excess over the side. [Ross 1924, 283]

In following Göbel, Ross deems the mention of “the side” as “the gloss of an over-zealous copyist”.

T. L. Heath:

> the relative lengths of the diagonal and the side can be approximated to by forming the successive approximations to $\sqrt{2}$ in accordance with Theon of Smyrna’s rule: these are 7/5, 17/12, 41/29, etc. If therefore we took the side to be 1, we could say that the diagonal was one of these fractions, so that two numbers (one divided by the other) are required to measure it. [Heath 1949, 218–219]

Of course, Heath is bound to accept Ross’ excision of “the side”. Only in Burkert 1972, 462n74 does one find a correct assessment of the passage. However, Burkert refers quite misleadingly to the setting out of two reference-measures as an “expedient of practical geometry”.

[38] This was in fact a commonplace point: cf. Plato, *Parm*. 140b–c and the first scholium to *Elem*. 10 in Heiberg and Stamatis 1969–1977, 5.2 at 84.21–85.1.

Of course, one may well take the two segments themselves that compound the binomial as reference-lines; and in this sense the binomial may appropriately be said to be composed "from two *names*". All of this, however, is at variance with the introduction in *Elem.* 10 of a single ῥητή (expressible) line as a reference-line. Both *names* of a binomial are in fact expressible lines, even if they are commensurable in power only. As a consequence, one single ῥητή is needed as a reference to build up a binomial, though the ῥητή itself is not a common measure of the *names*. This shows that the use of « ῥητή » in *Elem.* 10 should not be taken as coming from the same developments that yielded the coinage of "from two *names*" for the binomial.

The "metrological" conception of the reference-line as an standard of measurement was the one in use in the pre-Euclidean theory of irrational lines. This can be argued on the basis of a series of testimonies [see Acerbi 2008], including the well-known passage at *Theaet.* 147d–148b containing Theodorus' lesson and Theaetetus' definitions of "lengths" and "powers", and a handful of Aristotelian texts. It should then come as no surprise if the introduction of the peculiar notion of "expressibility" that we find in book 10 were original with it (we should, of course, suppose that the *Sectio* draws on a much earlier tradition).[39] Since to build up a binomial just one expressible line is required while two *names* were apparently needed, the introduction of the former notion might well have been devised as a simplifying feature.

5. Conclusion

Can we connect the "names" of some irrational lines with the "single name" in the introduction of the *Sectio*? From the preceding discussion a unified view of the two notions emerges naturally. The *name* of multiple and epimoric ratios is the single number that is the common measure of the two terms of such ratios: this we can surmise on the basis of the passage from Ptolemy's *Harmonica*. On the other hand, the *names* in a binomial irrational line are the two incommensurable lines needed to measure the two segments from which the binomial itself is obtained by composition. The tradition reports that the term "name" was used to denote also the components of other irrational lines. A *name*, I surmise, was a reference-measure, both in a geometrical and in a number-theoretical context. Going beyond these remarks would be rash. However, the interpretation advanced here has at

[39] For a thorough discussion of the ancient debate concerning the notion of expressibility in *Elements* 10, see Vitrac 1990–2001, 3.43–51.

least the virtue of proposing a unified view of two hitherto unrelated objects in Greek mathematics denoted by the same *name*.

BIBLIOGRAPHY

Acerbi, F. 2008. “In What Proof Would a Geometer Use the ποδιαία?” *Classical Quarterly* 58:120–126.

Acerbi, F. and B. Vitrac. 2014. edd. *Héron d’Alexandrie,* Metrica. Mathematica Graeca Antiqua 4. Pisa/Rome.

Barbera, A. 1991. *The Euclidean* Division of the Canon*: Greek and Latin Sources*. Lincoln, NB/London.

Barker, A. 1981. “Methods and Aims in the Euclidean *Sectio Canonis*”. *Journal of Hellenic Studies* 101: 1–16.

____ 1984–1989. *Greek Musical Writings*. 2 vols. Cambridge, UK.

____ 1994. “Ptolemy’s Pythagoreans, Archytas, and Plato’s Conception of Mathematics”. *Phronesis* 39: 113–135.

____ 2000. *Scientific Method in Ptolemy’s* Harmonics. Cambridge, UK.

Bonitz, H. 1849. *Commentarius in Aristotelis metaphysicam*. Bonn.

Bowen, A. C. 1991. “Euclid’s *Sectio canonis* and the History of Pythagoreanism”. Pp. 164–187 in A. C. Bowen ed. *Science and Philosophy in Classical Greece*. New York/London.

Burkert, W. 1972. *Lore and Science in Ancient Pythagoreanism*. Cambridge, MA.

Cathala, M.-R. 1935. ed. *S. Thomae Aquinatis doctoris angelici O. P. in metaphysicam Aristotelis commentaria*. Torino.

Düring, I. 1930. ed. *Die Harmonielehre des Klaudios Ptolemaios*. Göteborg.

____ 1932. ed. *Porphyrios Kommentar zur Harmonielehre des Ptolemaios*. Göteborg.

Fowler, D. H. 1999. *The Mathematics of Plato’s Academy*. 2nd edn. Oxford.

Friedlein, G. 1867. ed. *De institutione arithmetica libri duo, De institutione musica libri quinque. Accedit geometria quae fertur Boetii*. Leipzig.

Hayduck, M. 1891. ed. *Alexandri Aphrodisiensis in Aristotelis metaphysica commentaria*. Commentaria in Aristotelem Graeca 1. Berlin.

Heath, T. L. 1949. *Mathematics in Aristotle*. Oxford.

Heiberg, J. L. 1910–1915. ed. *Archimedis opera omnia cum commentariis Eutocii*. 3 vols. Leipzig.

Heiberg, J. L. and Stamatis, E. S. 1969–1977. edd. *Euclidis elementa*. 5 vols. Leipzig.

Hiller, E. 1878. ed. *Theonis Smyrnaei philosophi platonici expositio rerum mathematicarum ad legendum Platonem utilium*. Leipzig.

Jan, C. 1895. ed. *Musici scriptores graeci*. Leipzig.

Junge, G. and W. Thomson. 1930. edd. and trans. *Pappus: Commentary on Book X of Euclid's* Elements. Cambridge, MA.

Laloy, L. 1900. "Un passage d'Euclide mal interprété". *Revue de philologie, de littérature et d'histoire ancienne* 24: 236–241.

Liddell, H. G.; H. R. Scott; and H. S. Jones. 1968. *A Greek-English Lexicon*. Oxford.

Lippmann, E. A. 1964. *Musical Thought in Ancient Greece*. New York/ London.

Mathiesen, T. 1975. *An Annotated Translation of Euclid's* Division of a Monochord. *Journal of Music Theory* 19: 236–258.

Menge, H. 1916. ed. *Euclidis phaenomena et scripta musica*. In J. L. Heiberg and H. Menge edd. *Euclidis opera omnia*. vol. 8. Leipzig.

Ross, W. D. 1924. ed. *Aristotle's* Metaphysics. Oxford.

Ruelle, C. E. 1906. "Sur l'authenticité probable de la *Division du canon musical* attribuée à Euclide". *Revue des études grecques* 19: 318–320.

Smyth, H. W. 1920. *Greek Grammar*. Cambridge, MA.

Tannery, P. 1893–1895. ed. *Diophanti Alexandrini opera omnia cum graecis commentariis*. Leipzig.

——— 1904. "Inauthenticité de la "Division du Canon" attribuée à Euclide". *Comptes rendus des séances de l'Académie des Inscriptions et Belles Lettres* 4: 439–445. Repr. pp. 213–219 in J. L. Heiberg and H. G. Zeuthen edd. *Paul Tannery. Mémoires scientifiques*. vol. 3. 1915. Paris.

Vitrac, B. 1990–2001. trans. and comm. *Euclide d'Alexandrie,* Les Éléments. 4 vols. Paris.

Zanoncelli, L. 1990. *La manualistica musicale greca*. Milan.

Aratus' *Phaenomena* beyond Its Sources

by

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Abstract

In this article, I compare the astronomical poem by Aratus called *Phaenomena* (third century BC) with the citations of a work of the same name by Eudoxus that are found in Hipparchus' only extant work, *In Arati et Eudoxi phaenomena* (second century BC). I argue that, contrary to what most scholars maintain, Aratus' poem is not a mere versification of Eudoxus' work but a version enriched in style, language, and content. Indeed, Aratus' *Phaenomena* is a paradigmatic reflection of the astronomical knowledge of the period in which it was written and a comprehensive, non-technical presentation of the celestial phenomena known in his time. It was, as I show, a very popular work, so popular that Hipparchus was moved to correct it in the hope of establishing himself as the authority in astronomy and prose as its proper medium.

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Aratus' *Phaenomena* is an astronomical poem of the third century BC that remained immensely popular until the Middle Ages. Despite the longevity of the *Phaenomena*, it has taken modern scholars many years to appreciate Aratus' role in the history of literature: only in the last few decades has the *Phaenomena* been roused from its hibernation and put into the bigger picture of Hellenistic poetry. This has in turn involved studying the poem as a representative of the didactic genre[1] and as a product of Stoic influences. It has also been compared to other Hellenistic poems, to the works of Homer and Hesiod, and so forth.[2] Even though scholars have yet to understand the dimensions of the poem's popularity, it seems that they all suppose that Aratus "neither was nor pretended to be a scientist" [van Noorden 2009, 256] and that he was not an astronomer. Indeed, as Marrou puts it,

> he was essentially a philosopher and a man of letters, one of the wits at the court of Antigonus Gonatas, and all he did was put two prose works into verse and join them together—Eudoxus of Cnidus' *Phaenomena* and Theophrastus' mediocre Περὶ σημείων....There are errors in his observations: as Hipparchus mentioned in his commentary.... [Marrou 1956, 184: cf. Clarke 1971; and Gee 2013, 4]

Marrou's view has indeed become a topos and the consensus is that Aratus' poem bears no scientific astronomical value and that it is merely because of the author's poetic skills that both he and the *Phaenomena* became famous throughout the centuries.

Yet, if we take a closer look at this consensus that Aratus' work was merely a copy that Hipparchus evaluates, and so has no real place in our understanding of Hellenistic astronomy, we will see that it is problematic. In fact, as I will show, Aratus is the liaison between the astronomical knowledge

[1] But see Mastorakou 2020 for an argument that this characterization is misleading, if not incorrect.

[2] Apart from two editions with translation and commentary, Martin 1956 and Kidd 1997, and in addition to the citations in this article, I have found the following selection especially useful: Hunter 1995, Hutchinson 1988, Fakas 2001, and Fantuzzi and Hunter 2004.

of his time and the general public.[3] Indeed, it is my thesis that to deny or even downplay the poem's astronomical content and its own contribution to celestial knowledge is to strip from it the materials of which it is made and thus to leave our current histories of astronomy incomplete and puzzling.

1. Aratus' and Eudoxus' *Phaenomena*

1.1 *A few words on Hipparchus' commentary*

Eudoxus (408–355 BC) and Hipparchus (*flor*. third quarter, second century BC) hardly need an introduction. The former was a mathematician and an astronomer who, according to Aristotle [*Meta*. Λ.8], proposed a combination of nested revolving spheres to account for the motion of the planets. He also wrote the acclaimed works *Phaenomena* and *Enoptron*.[4] Hipparchus, for his part, took some Greek hypotheses of planetary motion and, by using Babylonian data, specified their parameters in order, it seems, to adapt them for quantitative prediction.[5]

The only extant treatise by Hipparchus, however, is his commentary on Aratus' and Eudoxus' *Phaenomena*. Dicks observes that this work is "usually dismissed as an early, youthful work of no importance"; but then adds, "This, however, is hardly correct" [1960, 16–17]. Hipparchus' commentary was written after at least two of his major works, *On Simultaneous Risings* and *On the Rising of the Twelve Signs of the Zodiac*, both of which he mentions. What is more, this commentary, which alone survives, is the one for which Hipparchus gained his reputation outside the small circle of experts in antiquity.

In his commentary, Hipparchus compares Aratus' *Phaenomena* with Eudoxus' *Phaenomena* and *Enoptron* as well as with Attalus' own commentary on Aratus' *Phaenomena*. Hipparchus' goal is to correct the information that these works provide about the heavenly bodies, a goal which requires him

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3 For a brief history of the *Phaenomena*'s reception, see Possanza 2004, 79–103. On Aratus' place in the history of astronomy and his depictions in art, see Mastorakou 2020.

4 Hipparchus [*In Arat*. 1.2.2] says that Eudoxus wrote two books, the *Phaenomena* (Appearances) and *Enoptron* (Mirror), which, he says, were not very different from each other, and that Aratus followed the *Phaenomena* in writing his poem.

5 For information about Hipparchus' life and works, see Dicks 1960, 1–18; Toomer 1978.

to quote numerous lines from Aratus' and Eudoxus' works.[6] Once this comparison is completed, he proceeds to list his own very specific data for the first and last stars to rise and set in each of 42 constellations, along with the degree of the zodiacal circle at the horizon and at the meridian at the moment when each of those stars rises or sets. Finally, he divides the celestial sphere into 24 equinoctial hours and states, beginning at the summer solstice, which stars are separated by one, or very close to one, equinoctial hour.

Hipparchus disregards not only poetry in general but also the poetry in Aratus' composition in particular [*In Arat*. 1.1.7], as well as anything that its commentators write about its poetic character. He recognizes that the poem has been commented on many times before and has consequently been widely discussed by the time that he is writing; and adds, "...but the most careful exposition...is that of Attalus, a mathematical astronomer (μαθηματικός) of our own time" [*In Arat*. 1.1.3].[7] Nevertheless, as Hipparchus sees it, Attalus, one of Aratus' several commentators, makes many mistakes about the heavenly bodies and sometimes even changes things in Aratus' poem that are correct. Still, in Hipparchus' view, Attalus' commentary remains the best, although it is not clear whether it is the best in relation to those by other mathematical astronomers or in relation to those not written by mathematical astronomers. Certainly, as Hipparchus notes, the best astronomers to distinguish which of Aratus' statements were consistent with the actual phenomena and which ones were not are experienced professionals [*In Arat*. 1.1.4]. In that category, Hipparchus distinguishes himself from all the others:

> Eudoxus wrote the same treatise about the phenomena as Aratus but in a more expert way. It is reasonable, then, that [Aratus'] poetry is considered trustworthy from the agreement of so many and such great mathematical astronomers (μαθηματικοί). It is perhaps not fair to blame Aratus even if he happens to stumble in some things, since he wrote the *Phaenomena* following Eudoxus' composition, but without making observations or declaring that he was going forth according to his own mathematical judgement[8] in celestial matters and making mistakes in them. [Hipparchus, *In Arat*. 1.1.8]

[6] For a discussion of Hipparchus' agenda in his preface and in commenting on Aratus, see Mastorakou 2020.

[7] All the translations of Hipparchus, *In Arat*. are my own. Translations of Aratus' *Phaenomena* are taken from Kidd 1997.

[8] Manitius 1894, 6.11–12 κατ' ἰδίαν μαθηματικὴν κρίσιν.

Hipparchus thus puts himself on a level superior to all on the grounds that he can correct previous astronomical views and reveal the truth about the heavens. Below him is Eudoxus, who, although a good mathematical astronomer, is wrong in many instances. After Eudoxus comes Attalus, just a mathematical astronomer, who again is often wrong. Finally, there is Aratus, who is often wrong yet again but whom we should not blame because he is merely a poet trying to follow the work of great mathematical astronomers. It is a great advantage for us to have Hipparchus' commentary in our hands, since this allows us to check for ourselves Hipparchus' claims and to see how Aratus based his poem on Eudoxus' *Phaenomena*, especially since Eudoxus' work has not survived to present times. In what follows, then, I will use Hipparchus' commentary to explore the astronomical knowledge in Aratus' *Phaenomena* and to compare it to that in Eudoxus' work, with the aim of assessing rigorously whether the poem is worthy only for its literary qualities, as many scholars today maintain.

1.2 *Comparing the style of Aratus' and Eudoxus'* Phaenomena

1.2.1 *The Cepheus-group* When someone browses through the texts of Aratus and Eudoxus that describe the constellations of the Cepheus-group without examining them in detail, it is easy to spot the difference in the order in which each lists the members of this group. Eudoxus describes the constellations in this order: Ursa Minor, Cepheus, Serpens, Cassiopeia, Andromeda, Pisces, Aries, Delta, Pegasus,[9] Perseus, Pleiades [*In Arat*. 1.2.11–15]. Aratus, however, deals with the group in this order: Cepheus, Cynosura, Draco, Cassiopeia, Andromeda, Pegasus, Aries, Delta, Pisces, Perseus, Pleiades [*Phaen*. 179–267]. The main difference here is that Aratus jumps from Andromeda directly to Pegasus, while Eudoxus comes to Pegasus from Andromeda gradually.

Both Aratus and Eudoxus agree that the star at the tip of the tail of Ursa Minor makes an equilateral triangle with the two feet of Cepheus:

> Eudoxus
> Below the tail of Ursa Minor, Cepheus has his feet, making an equilateral triangle with the tip of her tail. His middle is near the bend of Draco between the Ursae. [Hipparchus, *In Arat*. 1.2.11]
>
> Aratus
> The line that extends from the tip of her tail to each of his feet equals the distance from foot to foot. And you have only to look a little way past his belt

[9] Ἵππος/Equus (Horse): *scil*. Pegasus.

> if you are searching for the first coil of the great Draco. [Hipparchus, *In Arat*. 1.2.12; Aratus, *Phaen*. 184–187]

But Hipparchus does not agree with Aratus and Eudoxus and says:

> Next, concerning Cepheus, they all[10] err [in holding] that his feet form an equilateral triangle with the tip of the [lesser] Ursa, as Aratus says.... The reason is that [the line] between the feet is smaller than each of the others, so the triangle produced is isosceles and not equilateral. [Hipparchus, *In Arat*. 1.5.19]

A close examination of the language that Eudoxus and Hipparchus are using to describe the night-sky compared to that of Aratus brings to light significant differences. Although Eudoxus and Aratus agree about the position and the type of the triangle, they use different terminology. Aratus does not use the phrase "equilateral triangle" that is found in Eudoxus but writes more simply that "the line that extends from the tip of her tail to each of his feet equals the distance from foot to foot" [*Phaen*. 184–185]. Such avoidance of technical terminology serves to make his work more accessible to common people or non-experts. Eudoxus, for his part, uses the phrase without explaining it and Hipparchus not only shows no concern about how familiar this term was to his readers, he adds yet another, "isosceles" [*In Arat*. 1.5.19].[11]
Further differences in vocabulary are also striking. Eudoxus calls a constellation Serpens (ὁ δια τῶν Ἄρκτων Ὄφις or ὁ Ὄφις), while Aratus calls it Dragon (Δράκων). The latter name first appears in Aratus [Kidd 1997, 192], whom, interestingly enough, Hipparchus follows [*In Arat*. 1.4.2]. This is another instance of the attention that Aratus pays in making his poem clear and easy to follow. In my view, Aratus changed the name from «Ὄφις» to «Δράκων» in order to avoid the confusion with the other Ὄφις (the Serpent) introduced earlier in the poem at *Phaen*. 82, a change that everyone after Aratus adopted.
This is not the only occasion in which Aratus changes the name of a constellation. This happens too when he talks about the two Ursae. Eudoxus uses the names «ἡ Μεγάλη Ἄρκτος (Ursa Maior)» and «ἡ Μικρά (Ursa Minor)» [*In Arat*. 1.4.2], and Aratus changes them to «Κυνόσουρα (Cynosura)» and «Ἑλίκη (Helice)» [*Phaen*. 36–37]. "Cynosura" was probably an older name meaning "Dog's Tail", but we find the name "Helice" for the first time in

[10] Manitius 1894, 52.1 πάντες: Aratus and Eudoxus at least but perhaps other commentators as well.

[11] Later in his poem when he writes about Triangulum, Aratus again does not make use of the more mathematical term "isosceles" but instead just says that two of the triangle's sides appear equal [*Phaen*. 235].

Aratus [Kidd 1997, 188], a name which is most probably meant to capture the wheeling movement of that constellation around the North Celestial Pole, "the Twister". One can thus see that the names preferred by Aratus are more descriptive and, hence, more helpful to his readers. He implicitly refers to this difference with Eudoxus when he writes, "One of the Ursae, men call Cynosura by name, the other Helice" [*Phaen.* 188]. The choice of these specific names also fits with the mythological descriptions that Aratus incorporates into his poem.[12]

In his grouping of constellations in the myth of Cepheus, Aratus introduces his subject as "the suffering family of Cepheus" [*Phaen.* 179] which cannot "be just left unmentioned: their name also has reached the sky, for they were akin to Zeus" [*Phaen.* 180–181]. This group of constellations is interesting because all the figures are part of one myth. In fact, it is the only myth to be represented fully among the constellations.[13]

1.2.2 *The Cynosura-group* When Eudoxus and Aratus describe the Cynosura-group, they again place the constellations in the sky in a similar way but their accounts are very different.

> Eudoxus
> In front of Cepheus is Cassiopeia, and in front of her is Andromeda, whose left shoulder is over the more northerly Piscis; her girdle is above Aries, except that Triangulum is in between [Aries and the girdle of Cassiopeia]. A star in her head is common to the belly of Pegasus. [Hipparchus, *In Arat.* 1.2.13]

> Aratus
> In front of him revolves the tragic Cassiopeia, not very large, but visible on the night of a full Moon. [Hipparchus, *In Arat.* 1.2.14; Aratus, *Phaen.* 188–189]
>
> There too revolves that awesome figure of Andromeda, well defined beneath her mother. [Hipparchus, *In Arat.* 1.2.14; Aratus, *Phaen.* 197–198]

[12] Aratus uses mythology throughout the first part of his poem: see, e.g., *Phaen.* 30–35.

[13] There are different traditions regarding the family tree of Cepheus but Aratus chooses the one that relates to Zeus. So Cepheus, a descendant of Iasus, was the son of Io [*Phaen.* 179], a king of Ethiopia, and husband of Cassiopeia, who was mother of Atymnius by Zeus and of Andromeda by Cepheus. We may assume that people in Aratus' period were familiar with the plays entitled "Andromeda" by both Sophocles (496–406 BC) and Euripides (480–406 BC), and, thus, that they were also aware of the myth of Cepheus, since these plays were very popular in Athens at the time, as we can tell by the references in Aristophanes and the frequent portrayal of scenes from them on Attic vases. Thus, we may also assume that Aratus' readers were familiar with the mythology that he depicts in the heavens.

> there shines a star that is common to its navel and the head at her extremity. [Hipparchus, *In Arat*. 1.2.14; Aratus, *Phaen*. 206–207]
>
> but you can still identify it from the girdle of Andromeda: for it is set a little way below her. [Hipparchus, *In Arat*. 1.2.14; Aratus, *Phaen*. 229–230]
>
> Let Andromeda's left shoulder be your guide to the more northerly Piscis, for it is very close to it. [Hipparchus, *In Arat*. 1.2.14; Aratus, *Phaen*. 246–247]

Figure 1. The Cynosura group on the Kugel Globe (third century BC).

In the middle from left to right: Piscis, Triangle, Andromeda, Cassiopeia, Cepheus

The differences concern mainly the vocabulary that each author chooses and the picture that they give us. Eudoxus uses the verb "to be" (εἶναι) to indicate where Cassiopeia and Andromeda are as well as to say where the northerly Piscis is, while Aratus uses the verb "to revolve" (προκυλίνδεσθαι). The difference between Eudoxus' two-dimensional and motionless picture of the heavens and Aratus' rotating sky with three-dimensional figures that are alive and move might be expected: it is definitely one of the features that separate the former's prose and the latter's poetry. Such use of mythology and anthropomorphism is typical of Aratus' descriptions. But what we would not necessarily expect is to see how many of Aratus' descriptions and notions became standard practice among his successors. For example, the name «Δράκων» appears for the first time in Aratus, and Hipparchus adopts it instead of Eudoxus' «ὁ διὰ τῶν Ἀρκτῶν Ὄφις» or simply «Ὄφις».

In addition to his preference for a moving, three-dimensional cosmos is Aratus' introduction of more stars than Eudoxus in his description of each constellation and his focus on the shape and brightness of the constellations and the stars.[14] This extra information is crucial for Aratus' audience: they can learn how the constellations and the stars should appear to them, how well defined they are, and how easily they can spot them depending on their brightness:

> there shines a star that is common to its navel and the head at her extremity. [Hipparchus, *In Arat*. 1.2.14; Aratus, *Phaen*. 206–220]
>
> only a few zigzagging stars adorn her [Cassiopeia], giving her all over a distinct outline. [Aratus, *Phaen*. 190–191]
>
> the three other stars mark off lines of equal length...they are beautiful and bright. [Aratus, *Phaen*. 208–210]
>
> Aries itself is faint and starless. [Aratus, *Phaen*. 228]

But the *Phaenomena* not only guides its readers in exploring the night-sky, it actually urges them to do this. Aratus actually addresses his readers by using the second person. Examples from his descriptions of two groups of constellations discussed above are as follows:

> you can still identify it. [Aratus, *Phaen*. 229]
>
> I do not think you will have to look all round the night sky in order to sight her very quickly. [Aratus, *Phaen*. 198–199]
>
> you have only to look a little way past his belt if you are searching for the first coil of the great Draco. [Aratus, *Phaen*. 186–187]

In this way, Aratus calls upon his readers to see for themselves, presenting his observations as something accessible to everybody, where this accessibility is effected by means of the terminology that he chooses. The use of mythology and the correlation of groups of constellations to specific groups of mythological characters helps as well. It is not only that the poem becomes more approachable and vivid to the reader but that, on top of this, mythological names and scenes also help them to find the constellations more easily and to memorize them. Aratus' verbal star-map is one to be remembered.

1.3 *Comparing the content of Aratus' and Eudoxus'* Phaenomena I will now present examples to support my argument that Aratus changed his source not only by using different terminology and addressing the needs of an observer of the night sky, but also by changing specific astronomical data. When going through the description of the night sky in a comparative way,

[14] See, e.g., the star(s) in: Draco [*Phaen*. 55–57], Arctophylax [94–95], Virgo and Ursa Maior [136–146], Taurus [170–176], Cassiopeia [190–195], Pegasus [206–214], Pisces [244–245], Sirius or the Dog-Star [329–337, 339–341]. See also the unnamed stars in *Phaen*. 367–385, 389–401.

one sees that Aratus actually changes the content of Eudoxus' account either by placing the constellations differently or by mentioning that different parts of them rise and set with particular zodiacal signs. The following analysis goes hand in hand with the changes that Aratus made to update the astronomical information in Eudoxus' *Phaenomena* with the knowledge of his time, i.e., that there is no star at North Celestial Pole, and his treatment of the observer's eye as the center of the cosmos.[15]

1.3.1 *The celestial circles*

My first example is the group of celestial circles described by both writers and, in particular, the Tropic of Cancer. Eudoxus discusses the solstices [Hipparchus, *In Arat* 1.2.18, 1.2.20, 2.1.20], the equinoxes [2.1.20.], the Arctic Circle [1.11.1, 1.11.5.], the colures or circle passing through the celestial poles and the equinoctial points [1.11.17, 1.11.19, 2.1.21], and the zodiacal band [*In Arat*. 1.9.1–2]. Aratus, however, omits the colures and deals with the solstices [*Phaen*. 480–510], the equinoctial circle [*Phaen*. 511–524], the zodiacal band [*Phaen*. 525–558], and the Milky Way [*Phaen*. 525–558]. The latter is absent from Eudoxus' description, perhaps because such a circle, though definitely interesting for any lay-observer of the night sky, may not have been very interesting to the astronomers of his time.

1.3.2 *The Tropic of Cancer*

As for the Tropic of Cancer, the celestial circle on which we have the summer solstitial point, both authors agree that the left shoulder and the left leg of Perseus, the knees of Auriga, and the heads of Gemini lie on this circle [Hipparchus, *In Arat*. 1.2.18; Aratus, *Phaen*. 480–496]. Eudoxus additionally mentions the right hand of Heracles[16] and the nape of Serpens [Hipparchus, *In Arat*. 1.2.18], which Aratus omits altogether. Notice too that Eudoxus goes on to say that on the Tropic of Cancer lies the head of Ophiuchus [Hipparchus, *In Arat*. 1.2.18], though Aratus mentions only the shoulders of that constellation [*Phaen*. 487]. Furthermore, Eudoxus mentions that the right hand of Andromeda and the distance between her feet lie on the circle [Hipparchus, *In Arat*. 1.2.18], while Aratus maintains that

> Andromeda's right arm [is] above the elbow; her palm lying above it, nearer the north and her elbow inclining to the south. [Aratus, *Phaen*. 484–486]

[15] For discussion of these changes, see Mastorakou 2020.

[16] In Greek, this is ὁ Ἐγγόνασιν (the Kneeler) *scil.* Heracles; in Latin, Ingeniculatus (the Kneeler) *scil.* Hercules.

Co-Rising Constellations		Co-Setting Constellations	
Aratus	Hipparchus	Aratus	Hipparchus
◦ Orion with his belt and two shoulders [all of the River]	◦ the whole of Orion	◦ half of Corona ◦ as far as the spine of the northern Piscis ◦ the parts up to the belly of Heracles ◦ Ophiuchus as far as his shoulders [from knees to shoulders] ◦ the Serpens as far as its neck [close to the neck] ◦ the bigger part or half of Boötes	◦ half of Corona ◦ the head of the northern Piscis ◦ all of Heracles ◦ the head of Ophiuchus ◦ the tail of the Serpens ◦ the head of Boötes

Table 1. When the constellation Cancer rises [Hipparchus, *In Arat*. 2.2.2–30]

Finally, Eudoxus says that the feet of Pegasus and Cygnus'[17] nape and left wing are on the tropic of Cancer [Hipparchus, *In Arat*. 1.2.18]. In Aratus' poem, it is the hooves of Pegasus and Cygnus' neck [Aratus, *Phaen*. 487]. More differences yet have to do with the constellations that rise and set when Cancer and Aquarius rise, according to Aratus and Eudoxus. I have schematized the two accounts to make the differences clearer. In brackets are the differences between the fragments of Aratus' *Phaenomena* presented in Kidd's edition [1997] and the same fragments in Hipparchus' commentary. Table 1 shows how extensively Aratus' work differs from changed Eudoxus'. Except for Corona—both agree that half of it sets as Cancer rises—everything is quite different. One might think that Eudoxus and Aratus may be describing different phases of the rising and setting of the constellations. For example, Aratus mentions the part that has already gone, and Eudoxus, the part that is setting. But that hardly works for most of the constellations

[17] The constellation Ὄρνις (Bird) is thought to be Cygnus.

Co-Rising Constellations

Aratus	Hipparchus
◦ the head and the feet of Pegasus ◦ the back of the Centaur ◦ Hydra's head until her first coil [Hydra's neck-coil and all the stars in its head]	◦ Horse ◦ Centaur ◦ Hydra ◦ Cassiopeia ◦ Delphinus

Table 2. When the constellation Aquarius rises [Hipparchus, *In Arat*. 2.3.4–10]

which they mention.[18] The obvious conclusion is that Aratus differentiates himself from Eudoxus by presenting his reader with more recent thinking about the celestial sphere.

Beyond mentioning different parts of setting and rising constellations, Aratus also omits whole constellations that Eudoxus includes in his account [see Table 2].

Although Aratus and Eudoxus mention that the same constellations set when the Aquarius rises, there is the important difference that Eudoxus mentions two additional ones, namely, Cassiopeia and Delphinus, which Aratus completely omits. Here again Aratus changes Eudoxus' account, and Hipparchus' version agrees. Indeed, Hipparchus says, first, that Cassiopeia sets with Sagittarius and Aquarius; and, second, that the Delphinus as a whole sets with Sagittarius. Aratus thus avoids the erroneous information that Eudoxus includes in his work, something that Hipparchus does not acknowledge.

Intriguingly, for his own reasons, Hipparchus does not usually credit Aratus for correcting information found in Eudoxus' work. Perhaps, as I mentioned earlier, it is because, in his hierarchy of technical competence or understanding of the heavens, Eudoxus is superior to Aratus. There are, however, a few instances when Hipparchus does admit that Aratus is right and that

[18] I am not aware of two different traditions of describing the risings and settings of the constellations but it would be interesting to investigate this further. It might be something similar to the two different ways of depicting the constellations on celestial globes, viz. from the front or the rear or a mixture of both.

Eudoxus or Attalus is wrong, for instance, when he comments that the simultaneous risings recorded by both Eudoxus and Aratus are more correct for the division of the zodiacal band assumed by Aratus [*In Arat*. 2.2.6].

In general, Hipparchus is selective in his reports of Aratus' work, perhaps because he is primarily interested in describing where each of the constellations is and has little interest in anything else. It should not surprise us, in any case, that Hipparchus does not include Aratus' mythological descriptions, the similes that he deploys, the meteorological references and weather-signs, the role of Zeus, or even information about the stars' sizes and their brightness, or how one can find a constellation in the sky. All these omissions have, I think, to do with Hipparchus' focus in his work and and the attendant style. Despite claiming in the preface that he wants to correct Aratus' work for the benefit of everybody, Hipparchus is very careful to exclude aspects of astronomy that do not fit the discipline as he sees it: for him, this discipline is mathematical astronomy and his targeted readers are, like himself, its practitioners. The result is that he did not really aim to reach a general educated public (beyond impressing it with his expertise). This is suggested, for instance, by his omitting to tell his reader how to find the constellations in the sky or his assuming that his reader already knows how to do that. It could be said that, since this is a commentary on Aratus, the reader is assumed to be familiar with Aratus' poem already; so there would be no point in Hipparchus' re-stating this sort of information. But overall, one gets the strong sense that Hipparchus is trying to create a specific picture of Aratus which is inextricably linked to the one that he wants to create for himself. By focusing for the most part on Aratus' incorrect statements, Hipparchus shows that he wants to emphasize the difference between a good, professional mathematician/astronomer and someone who only writes poems following mathematical works by others. That is why, although Hipparchus mentions that Aratus and Eudoxus agree on one description, when he wants to say that he disagrees with that account, he typically sets himself in opposition only to Aratus, even though both Aratus and Eudoxus are wrong. He writes, "as Aratus says" [e.g., *In Arat*. 2.2.31–35] and not for instance "as they both say".

Aratus' account of the heavens, then, is the one that Hipparchus is trying to correct and eventually replace.

This means that with Aratus we have the close of one era of celestial knowledge and the start of another in the second century BC with Hipparchus. Curiously, such a gap between Aratus (315–240 BC) and Hipparchus (190–120 BC) is evident in the the sequence of the major contributors to astronomy up to and including Hipparchus that is acknowledged by ancient writers. Be that as it may, there is evidence enough that Aratus' poem marks the close of an era culminating in the wide dissemination and popularity of astronomical knowledge [Mastorakou 2020] and that Hipparchus, in order to establish his own account, undertook not only to re-present the facts but also to re-cast their presentation in prose, a goal that apparently required "correcting" Aratus' *Phaenomena* and diminishing any role that it had played in the history of that science.

2. Conclusion

I have drawn attention to Aratus' and Eudoxus' works on the fixed stars. On looking closely at the content and presentation style of the two works, it is clear that Aratus not only changes the language of his source, he also modifies the actual content of the prose-work on which his poem is based. Both Eudoxus and Aratus locate the constellations in relation to one another spatially but Aratus also exhibits an interest in their appearance and brightness as well as in the legends associated with them. The result is a vivid poem, which attracts and holds the reader's attention on the night-sky and all its wonders. When it comes to the actual astronomical detail that the poem provides, there are again changes in the content, changes either in line with the updated knowledge of Aratus' time or omissions whenever Eudoxus' information was incorrect or ambiguous. In effect, we see Aratus providing an account that would be easier for non-experts (who are in the majority) and thus more readily transmitted to the next generations. Aratus seems to be the last in a long astronomical tradition. He is the one who sums up the non-technical astronomical knowledge of his period to give it to the general public. But note: Aratus did not write a poem on popular astronomy; he wrote an astronomical poem through which astronomy became popular. Indeed, astronomy had a prominent place in Hellenistic education—in contrast to mathematics for example—and it kept this role and commanded high popular interest for many centuries.

BIBLIOGRAPHY

Clarke, M. 1971. *Higher Education in the Ancient World*. London, UK.

Cuvigny, H. 2004. "Une sphère céleste antique en argent ciselé". Pp. 345–381 in H. Harrauer and R. Pintaudi edd. *Gedenkschrift Ulrike Horak (P. Horak)*. Florence.

Dicks, D. R. 1960. *The Geographical Fragments of Hipparchus*. London, UK.

Fakas, C. 2001. *Der Hellenistische Hesiod. Arats Phainomena und die Tradition der Antiken Lehrepik*. Wiesbaden.

Fantuzzi, M. and R. Hunter. 2004. *Tradition and Innovation in Hellenistic Poetry*. Cambridge, UK.

Gee, E. 2013. *Aratus and the Astronomical Tradition*. New York.

Gillispie, C. C. 1970–1990. *Dictionary of Scientific Biography*. 18 vols. New York.

Hunter, R. L. 1995. "Written in the Stars: Poetry and Philosophy in the Phaenomena of Aratus". *Arachnion* 2: 1–34.

Hutchinson, G. O. 1988. *Hellenistic Poetry*. Oxford.

Kidd, D. 1997. *Aratus: Phaenomena*. Cambridge, UK.

Manitius, C. 1894. *Hipparchi in Arati et Eudoxi phaenomena commentariorum*. Leipzig.

Marrou, H. I. 1956. *A History of Education in Antiquity*. London, UK.

Martin, J. 1956. *Arati Phaenomena*. Florence.

Mastorakou, S. 2020. "Aratus and the Popularization of Hellenistic Astronomy". Pp. 383–397 in A. C. Bowen and F. Rochberg edd. *Hellenistic Astronomy: The Science and Its Contexts*. Leiden/Boston.

Possanza, C. M. 2004. *Translating the Heavens*. New York.

Toomer G. 1978. "Hipparchus". Gillispie 1970–1990, 15.207–224.

van Noorden, H. 2009. "Aratus' Maiden and the Source of Belief". Pp. 255–275 in M. A. Harder, R. F. Regtuit, and G. C. Wakker edd. *Nature and Science in Hellenistic Poetry*. Leuven.

Before Copernicus and Copernicus

by

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Abstract

A discussion of *Before Copernicus: The Cultures and Contexts of Scientific Learning in the Fifteenth Century* edited by Rivka Feldhay and F. Jamil Ragep.

About the Author

MATJAŽ VESEL is a senior research fellow at the Institute of Philosophy in the Research Centre of the Slovenian Academy of Sciences and Arts (Ljubljana, Slovenia). His research focuses mostly on historical epistemology in the period from the late Middle Ages to Newton. He has published monographs on Nicholas Cusanus, Nicolaus Copernicus, and Galileo Galilei, as well as numerous articles on medieval and early modern science and philosophy. He is also the editor of the series Historia scientiae (Založba ZRC, the publishing unit of RC SASA), dedicated to publishing scholarly translations of the key texts from the history of human knowledge (*scientia*).

Despite extensive and increasingly nuanced scholarly research, the work of Nicholas Copernicus, one of the most iconic names in the history of human thought, is still controversial. Before addressing some of the controversies and *Before Copernicus* [Feldhay and Ragep 2017] in this context, allow me to note some fairly uncontroversial, basic facts about his life and astronomical work.

Copernicus, who was born in Toruń in 1473, enrolled as a student of liberal arts at the University of Cracow in 1491, which he left without a degree in 1495. In 1496, he moved to the University of Bologna to study canon and civil law. In 1500, he briefly visited Rome and then returned to his native Warmia. Shortly after that, in 1501, he returned to Italy, this time to the University of Padua, where he was supposed to study medicine. He was awarded a doctorate in canon law from the University of Ferrara in 1503. Upon returning home, he started working as his uncle's physician and subsequently also as a church administrator. Sometime around 1510 (before 1514 and possibly as early as 1508), he drafted his earliest attempt at a heliocentric, geokinetic astronomy and cosmology in a text later known as *De hypothesibus motuum caelestium a se constitutis commentariolus* and referred to in short as the *Commentariolus*. This text presumably circulated among his friends but was not published during his lifetime. His next astronomical text was the very short (semi-) private *Letter to Werner*. Having been persuaded by Rheticus and some other friends, Copernicus finally published his major work *De revolutionibus orbium coelestium* in 1543. He died in the same year.

The aspects of his work that are still debated are many and, due to the difficult, sometimes technical subject matter and substantial scholarly output, tend to be very nuanced and sophisticated. The famous Copernican question is really a bundle of different but interrelated questions. The more general ones, such as Was there really such an event as the Copernican revolution?, clearly depend on how we understand the concept of "science" (to put it anachronistically for the sake of brevity) and its multifaceted continuous transformations, and—no less importantly—on how well we understand Copernicus' immediate or less immediate "scientific" context, against which his achievements and contributions are to be assessed. This naturally leads to an examination of more specific details of his work: What exactly was the question that he was trying to answer? How, why, and when did he become a Copernican? What is the nature of the orbs mentioned in the title of his *De revolutionibus orbium caelestium*? Are his astronomical models the result of an independent development in Western thought or did he borrow them from his Islamic predecessors? These are just a few examples. It is generally understood, first, that these and other questions are in themselves very complex and divisible into myriad sub-questions that demand studies of considerable historical and epistemological breadth, length, and depth; and second, that sometimes seemingly insignificant details can turn the whole narrative completely upside down, since, as is usual in such complex matters, the whole depends on its parts as much as the parts depend on the whole.

The aim of *Before Copernicus* is to address some of the above-mentioned issues by examining Copernicus' intellectual and social background. The book is divided into three parts:

Part 1 covers Copernicus' 15th-century European social and political context;

Part 2 is dedicated to his 15th-century European intellectual and scientific context; and

Part 3 explores the multicultural astronomical background to the Copernican revolution.

Although the book, true to its title, focuses on the period before Copernicus, i.e., on the "long fifteenth century",[1] its authors keep one eye on the value of this period for understanding Copernicus' work, especially his *Commentariolus*, which is set as the endpoint of the discussion.

With this in mind, I will divide my review into two sections. In the first, I will summarize the introduction, which sets the stage and defines the main coordinates of the discussions with several important "observations" (the editors' term) and conclusions. I will then attempt to summarize the main points and the most important results of each chapter. While I, together with the editors and contributors to the book, believe that Copernicus' work—or any other work of any significance, for that matter—can be fully appreciated only when set within a sufficiently long as well as adequately studied historical context, I will pay much closer attention to the chapters and chapter-sections that discuss issues that are in my view "closer" to Copernicus and, therefore, more relevant to an understanding of his *Commentariolus*. In

[1] The interval from the mid-14th century to roughly 1525, according to Christopher Celenza [17–18].

the second section of my review, I will provide a critical appraisal of the book with special emphasis on the question of how the book as a whole and each of the chapters succeed in making the genesis and nature of Copernicus' *Commentariolus* (and in some cases *De revolutionibus*) more understandable. At the same time, I will point out some conclusions that I find questionable and suggest alternative interpretations. I will also suggest what I believe still needs to be done to advance our understanding of Copernicus' astronomy and cosmology.

1. Summary

1.1 *The introduction*

Rivka Feldhay and Jamil Ragep, the editors of the book and the authors of its introduction, explain the need for an examination of Copernicus' social and intellectual background by the fact that it is little understood. According to their outline of the most important issues discussed during the last half century (or so) of Copernican scholarship, he has sometimes been portrayed as a lone genius without history and without context. This changed with Thomas Kuhn's *The Copernican Revolution* and his thesis about the crisis that prompted the revolution. Kuhn did not manage, however, to explain the exact nature of this crisis, which

> remained elusive, in large part because the 15th-century background to Copernicus was and remains to a large extent *terra incognita*. [3]

A major step forward was taken by Otto Neugebauer, who showed how much the mathematical details of Copernicus' work are connected to both the "Western" tradition and, crucially, the "Eastern", Islamic tradition.
In continuing Neugebauer's work, Noel Swerdlow arrived at even more important conclusions. His detailed analysis of the *Commentariolus* brought to light more evidence of Copernicus' debt to Islamic astronomers. Copernicus' mathematical models, which were supposed to solve the so-called "equant problem" (among other things), were very similar or identical to those of his Islamic predecessors.[2] Swerdlow stressed the importance of Copernicus' adherence to physical astronomy, i.e., to the astronomy of real, solid orbs. And finally, he voiced speculation about Copernicus' path to heliocentrism. He

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[2] To the observer stationed on the motionless Earth at the center of the cosmos, the five planets along with the Sun and Moon exhibit nonuniform velocity during their courses through the zodiacal band. Ptolemy tried to solve this problem with the concept of the equant, a mathematically established point or *punctum equans* about which each body was supposed to move uniformly. This solution was deemed unsatisfactory and problematic.

posited that Copernicus had come to his heliocentric cosmology by a technical route, that Copernicus turned to heliocentrism because he believed that the planets are carried around by solid spheres and because he adhered to the principle of the uniform and circular motion of the heavenly spheres. Copernicus' search for an alternative that avoided Ptolemy's violation of the second principle (the equant problem) led him to a "Tychonic" cosmography that had the Sun moving about the Earth while being more or less at the center of the orbs of the retrograding planets. Since, in this system, the solid orbs of the Sun and Mars intersect, Swerdlow speculated that Copernicus opted for one with a static Sun and a moving Earth in which all the orbs were discretely nested.

Swerdlow's publication incited discussion of Copernicus' belief in solid spheres and his debt to his Islamic predecessors for his mathematical models. Critics of Swerdlow's reconstruction, who include Feldhay and Ragep, as we shall see later, claimed that there must be "more to this monumental cosmological shift than a strictly mathematical/astronomical explanation" and that there "were certainly other ways to deal with the problem of the equant and other Ptolemaic violations" [4]. Al-Shāṭir, for example, from whom Copernicus apparently borrowed extensively in the *Commentariolus*, dealt with the Ptolemaic difficulties while retaining a geocentric cosmology.

There have indeed been other proposals that pretend to provide "the missing cause or motivation" for Copernicus. Mario Di Bono drew attention to the Paduan Aristotelians, Andre Goddu to the Cracowian Aristotelians, and Robert S. Westman to the astrological "crisis" caused by questions about the planetary order. But Feldhay and Ragep are uncomfortable with the predominant attempts to reduce the Copernican question "to one of finding the univocal explanation that somehow supersedes all others" and with the fact that "the most recent discussions of Copernicus have taken a Eurocentric turn, with the question of cross-cultural influence mostly set aside" [5]; and so they have assembled scholars to discuss the background to Copernicus in a multicultural and multidisciplinary way. With the *Commentariolus* as the endpoint, these discussions were guided by a set of observations from which several conclusions were reached. Let me cite these seven observations in full here:

1. Copernicus' stated purpose in the *Commentariolus* is to find "a more reasonable model composed of circles...from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires".

2. Copernicus does not refer in the *Commentariolus* to the "marvelous symmetry" brought on by his new ordering of the planets, as he does in *De revolutionibus*. Although one must be cautious when speaking of motivation, it is curious that Copernicus does not explicitly put forth in the *Commentariolus* what is perhaps his most compelling argument.
3. Copernicus' models (taking into account both the *Commentariolus* and *De revolutionibus*) contain both eccentrics and epicycles.
4. There is strong evidence that Copernicus adheres to solid-sphere astronomy.
5. There is no indication that Copernicus ever resorted to a strictly Aristotelian, Averroist, Biṭrūjian, or Paduan "homocentric" astronomy. Copernicus does insist on a single center for his main orbs and otherwise uses only epicycles in the *Commentariolus*, whereas he uses eccentrics with their multiple centers in his *De revolutionibus*.
6. The number of similarities between the planetary models in the *Commentariolus* and those advanced by Ibn al-Shāṭir (14th-century Damascus) is significant.
7. Discussions of the possibility that the Earth is in motion can be found in both Islam and Christendom prior to Copernicus. [5–6]

While Feldhay and Ragep admit that "any number of conclusions may be drawn from these observations" [6], they propose the following:

(1) Copernicus' initial motivation was to address the violation of the principle of perfect motion, that is, of its uniformity. The *symmetria* of the cosmos achieved by the heliocentric ordering of the planets in *De revolutionibus* was *post hoc*. They are, therefore, not convinced by Goldstein [2002] and Westman [2013] that the ordering of the planets was a motivating factor (from 1 and 2).

(2) Copernicus' work falls within the tradition of Ptolemy's *Almagest* and *Planetary Hypotheses*, the *hay'a*-tradition of Islamic astronomy, and the 15th-century revival of Ptolemaic astronomy and cosmology as found in Peurbach's *Theoricae novae planetarum* and in Regiomontanus' *Epitome of the Almagest* (from 3, 4, and 5).

(3) In his early career, Copernicus was concerned with some kind of quasi-homocentrism (from 5).

(4) He was significantly influenced by post-1200 Islamic astronomy (from 6). The existence of a longstanding criticism of Ptolemy and alternative models that were developed within the geocentric cosmology highlight, however,

> that it was not necessary for Copernicus to make his momentous transformation in order to satisfy his stated goal of a cosmography with

> uniform circular orbs. It thus seems that there were aspects of Copernicus' intellectual and cultural context that led him to his decision to put the Earth in motion. [6–7]

(5) Copernicus may have been aware of, or influenced by, discussions about the motion of the Earth in prior Christian and/or Islamic traditions (from 7).

Feldhay and Ragep's point of departure was their dissatisfaction with Swerdlow's technical reconstruction of Copernicus' conversion to heliocentrism. Copernicus might, they reaffirm,

> have fulfilled his stated goal of a reformed astronomy with uniform, circular motions within a geocentric framework. This latter approach was, after all, the one that a number of Islamic astronomers had already employed to a large extent. [7]

Accordingly, they are not convinced that the response to the Copernican question is "through one correct derivation of a model that necessarily led to a coherent and true astronomical-cosmological picture" [8]. Instead, they see Copernicus' system as a result of many practices

> that included attempts to deal, mathematically, with violations of physics found in Ptolemy's models, discussions of the relation of natural philosophy and mathematics, and epistemological forays into the "true" cosmology and the human capacity to discover it. [8]

They likewise believe that 15th-century astronomy was

> the outcome of multiple transformations along different paths that crystallized in the work of Copernicus into some kind of coherent whole that differed enough from the preceding astronomical discourse to open the door to additional, enhanced transformations. [8]

1.2 *Part 1. Social and political contexts* Christopher Celenza ("What Did It Mean to Live in the Long Fifteenth Century?" [17–28]) discusses some characteristic features of the 15th century that could have shaped Copernicus' world. Celenza reflects on the political life of the time and points out that in order to find some personal safety as well as to advance their intellectual activities, the scholars of Copernicus' period sought personal patronage. Celenza sees Copernicus as a member of the group of traveling scholars in search of patronage and briefly examines his studies at the universities of Bologna and Padua, stressing their "secularism", that is, their lack of an organic link between concern with the arts and theology, on the one hand, and their link to Italian humanism, on the other, where humanism meant

> a willingness to question authority....Given this situation, Copernicus' willingness to entertain divergent techniques (like the Ṭūsī-couple) and possibly revolutionary viewpoints (like heliocentrism) becomes more understandable. [20]

Celenza also shows that in Copernicus' time intellectual elites still believed in supernatural powers.

The most important section of the chapter, however, is perhaps the one dedicated to the way in which information was gathered and transmitted. One of the characteristics of the 15th century was a collaborative approach to knowledge. There were many different varieties of reading and writing practices and

> a number of them make it likely that [Copernicus] may well have come across a theory like the Ṭūsī-couple without feeling the characteristically modern need to record precisely where, when, and in what format he encountered it. [28]

Nancy Bisaha ("European Cross-Cultural Contexts before Copernicus" [29–41]) focuses on the political realities relevant to the transmission of knowledge. Her basic question is

> [W] hy did Copernicus and his contemporaries say nothing about recent Islamic astronomers if they were so heavily indebted to them?...How and why did such astronomical knowledge travel great distances in the early modern era, only to have its origins vanish so effectively that scholars did not discover them until the last few decades? [29]

She draws a picture of the complex, multifaceted relations between Latin Europe, the Ottoman Empire, and Byzantine refugees in Europe. The exchanges that took place among European, Asian, and Byzantine scholars were characterized by connections and tensions at the same time. Muslims, for instance, "were extremely wary of travelling in Christian Europe, with the exception of Venice, throughout the period" [32]. Her key examples that illustrate this situation are the books *Europe* and *Asia*, often printed together and read as one piece called the *Cosmographia*, written by Aeneas Silvius Piccolomini, Pope Pius II (1458–1464). These two texts reflect the crystallization of a European identity *vis-à-vis* the perception of Asia as "the other". Bisaha considers three possible explanations of why Copernicus did not acknowledge his borrowings from Islamic astronomy:

(1) The Islamic origins of Copernicus' ideas were obscured at some point by Greek refugees, who

> > found the provenance a sensitive subject given their adamant calls for crusade and the rhetoric of Ottoman barbarism that was so fashionable in western Europe. [40]

(2) "Copernicus knew the origins and chose not to note them for fear of unpleasantness or a harsh reaction from the papacy" [40].

and

(3) This lack of provenance could be "simply due to an innocent omission at some point in the transmission" [40].

Bisaha points out one common denominator that emerges despite all of this uncertainty. These new ideas

> travelled westward and were used, but they were changed or cloaked consciously or unconsciously, perhaps to make them fit with the growing belief among Europeans that their current scholarship had surpassed that of the East. [41]

1.3 *Part 2. Intellectual and scientific contexts* With Edith Dudley Sylla's chapter ("The Status of Astronomy as a Science in Fifteenth-Century Cracow: Ibn al-Haytham, Peurbach, and Copernicus" [45–78]), we focus more closely on Copernicus; more exactly, on his *Commentariolus* and its background, which can, according to Sylla, be found in Copernicus' years as a student in Cracow (1491–1495). Two eminent teachers, John of Głogów and Albert de Brudzewo, were active there at that time. Głogów probably lectured on Aristotle's *Posterior Analytics* and, in 1499, when Copernicus had already left Cracow for Bologna, published a commentary thereon. He also wrote a commentary on Sacrobosco's *Sphere*. Brudzewo wrote a commentary on the most popular and progressive textbook of the day in astronomy, Peurbach's *Theoricae novae planetarum*, which was also printed after Copernicus' departure. It is very likely, however, that Copernicus was familiar with all three texts either through manuscripts or through lectures (not necessarily by Głogów and/or Brudzewo) based on these manuscripts.

Sylla develops two lines of investigation. One is the development of the theoretical and narrative, i.e., non-demonstrative, astronomy that was intended as introductory and is found in the so-called *theorica*-tradition. This was physical astronomy, an astronomy that proposed the physical bodies that might lie behind the observed motions described mathematically in Ptolemaic astronomy. She links Ibn al-Haytham's *On the Configuration of the World* (transmitted to Latin-speaking Europe at the latest by the end of the 13th century) and the *hay'a*-tradition of Islamic astronomy with the European tradition of *theorica*-astronomy, and this in turn with Peurbach's *Theoricae novae planetarum*, and the *Theoricae novae* with Copernicus' *Commentariolus*. The second line of her investigation concerns the status of astronomy as a science as this was understood in the commentaries on Aristotle's *Posterior Analytics*. She approaches this question through the medieval opposition of *antiqui versus moderni* and, closer to Copernicus, by an analysis of the above-mentioned texts by John of Głogów and Albert de Brudzewo.

Copernicus' *Commentariolus* lies firmly in the same tradition as Peuerbach's *Theoricae novae planetarum*. It is "theoretical rather than practical, narrative rather than demonstrative, and based on the assertion of hypotheses or principles" [45]. The *Commentariolus* mirrors the *Theoricae* in starting with a statement of principles. In Copernicus' work these principles are called postulates (*petitiones*) and in Peurbach's work they are the *theoricae* (figures) themselves together with their descriptions of planetary orbs. Copernicus' *petitiones* represent

> hypotheses derived from experience, which are to be accepted as true, even though they could be wrong given that astronomy is a science still in the process of development. [49]

The orbs of the *Theoricae* (three-dimensional, three-part spherical shells) are the identifying DNA of the configuration that it shares with Ibn al-Haytham's *On the Configuration of the World*. Ibn al-Haytham and the Islamic *hay'a*-tradition understood these orbs as rigid, not fluid bodies. They included deferents and epicycles, and, while the planets are held tightly in place, they can rotate uniformly but without ever exceeding the place or cavity they are in. Moreover, these orbs spin. Brudzewo's commentary on Peurbach's *Theoricae novae* establishes what he understood to be the proper principles of *Theoricae novae*. All five principles are of a physical rather than mathematical nature, such as, for instance, the second: "Of any simple body there is only one simple motion proper to it naturally". These "principles have a relation to Peurbach's *Theoricae novae planetarum* similar to the relation of Copernicus' *petitiones* to his *Commentariolus*" and are ultimately derived "from thinking about observations and how they could be explained by underlying reality" [53]. This format, however, was not unique to theoretical astronomy. Many scholastic philosophers before Brudzewo

> put their theories or parts of their theories into a structure in which there are suppositions, principles, or premises (i.e., hypotheses) on which conclusions are based. [54]

These principles are usually physical rather than mathematical and are held to be derived from experience.

Although Sylla believes that the predominant influence on the *Commentariolus* was that of the conception of astronomy in the *Theoricae novae planetarum*, she thinks that

> the background of Aristotelian philosophy at Cracow also helps to explain why Copernicus might have proposed a new configuration of the world in the *Commentariolus*. [60]

This leads her to discuss the concepts of science in general and astronomy in particular as formulated in different commentaries on Aristotle's *Posterior Analytics*. She situates the discussion within the medieval Aristotelian opposition between a conservative *via antiqua* and a progressive *via moderna*, and argues that

> the conception of astronomy as a science that Copernicus encountered as a student at Cracow University, the one reflected in the *Commentariolus*, was closer to the attitudes of the *moderni* than to those of the *antiqui*. [59]

This is confirmed by Głogów's texts (*Commentary on Sacrobosco's* On the Sphere of the World and *Commentary on the* Posterior Analytics), which are consistent with the views of the *moderni*. In his *Commentary on the* Posterior Analytics, Głogów, for example, in answering the question of whether it is possible to know something *de novo*, opposes Plato in claiming that we can have scientific knowledge and that it can be new rather than always something that we knew previously but forgot. One of the important features of his commentary on *On the Sphere* is a distinction between what is mathematical (hence imaginary, hence dependent on human thought) in astronomical theories and what is physical. The same is the case with Brudzewo's *Commentary on the* Theoricae novae planetarum. He, too, has a clear conception of astronomy as partly physical and partly mathematical. He repeatedly differentiates between physical orbs and mathematical/imaginary circles. Brudzewo argues that astronomers are not to dispute the basic principle of astronomy, that is, the uniform circular rotation of the celestial bodies. He also claims explicitly that the equant is not a physical thing since there is no corresponding aetherial sphere in the heavens. Despite that, astronomers used it for the purposes of practical astronomy (i.e., astrology) to support prognostications concerning the effects of the heavenly bodies on Earth.

What, then, did Copernicus learn while studying in Cracow? The main thing was Peurbach's *Theoricae novae planetarum*, which served as a model for the status of astronomy as a science. Copernicus was exposed to the idea of theoretical (not demonstrative) astronomy according to which the astronomer "can start by stating principles or postulates upon which the following exposition will be based" [53]. This had certain consequences for astronomers. Knowing that "principles are not proved and that the processes by which they are arrived at are not logically rigorous" [54], astronomers could be led to think about a reformation of principles. And this, according to Sylla, is exactly what Copernicus says at the very beginning of the *Commentariolus* before he lists his seven postulates (*petitiones*).

Since Copernicus, like the authors of *theoricae planetarum*, starts with physical principles, he must have "conceived his research program within the *theorica planetarum* genre". Copernicus also learned "that astronomy was both mathematical and physical and that, although it had many real achievements, it might still be improved by new insight into the hidden physical structures behind the appearances" [55]. The physical side of astronomy was represented in real three-dimensional orbs; the mathematical side was represented in *theoricae*/figures that were two-dimensional geometrical circles and lines. These figures were understood as products of mathematical constructions or human imagination and not as real things existing in the external world. The task of physical astronomy was to find physical bodies that might lie behind the observed motions described mathematically in Ptolemaic astronomy. In Copernicus' period, this task of finding physical configurations consistent with mathematical regularities had not been completed. There was, therefore, a constant need for new and better physical hypotheses, better physical configurations. Astronomy was, therefore, conceived as a progressive scientific discipline in which principles were "derived *a posteriori* from experience and hence could be received from new or added experience" [59].

Michael Shank ("Regiomontanus and Astronomical Controversy in the Background of Copernicus" [79–109]) discusses the life of the most important and advanced astronomer before Copernicus, Johannes Regiomontanus, his approach—or better, approaches, as we shall see—to astronomy, and his impact on Copernicus. Two important personalities had a strong influence on Regiomontanus' career. One was the astronomer and humanist Georg Peurbach, author of the *Theoricae novae planetarum*, with whom Regiomontanus worked in Vienna. The second was Basilios Bessarion, a Greek *émigré*, originally a Byzantine orthodox and a student of the Platonist George Gemistos Pletho, who became a cardinal of the Roman Catholic Church and was instrumental in procuring the *Epitome of the Almagest*, the book that Copernicus preferred over the *Almagest*.

One of Regiomontanus' earliest astronomical manuscripts is a copy of Peurbach's lectures of 1454 on his *Theoricae novae planetarum* at the *Bürgerschule* in Vienna. The "New (*novae*)" in its title signaled the fact that it presented the real, physical configurations and motion of the spheres, as opposed to merely mathematical ones. When Regiomontanus edited it for the first time in 1474, partial spheres of the planetary models, being physical, were filled in with black ink or striking colors, while the purely geometrical diagrams were thin black-on-white lines. Regiomontanus' astronomical interest did not stop with his mentor's work. While in Vienna, he also studied Henry of Langenstein's *De reprobatione ecentricorum et epicyclorum* (1364), which stimulated his openness to homocentric possibilities. He later formulated similar proposals and objections when criticizing Ptolemy's approach in the *Almagest*. Regiomontanus was also aware of the earlier homocentric system of al-Dīn al-Biṭrūjī's *De motibus celorum* (translated into Latin from the Arabic in 1217 by Michael Scot) and his unorthodox arrangement of the inferior planets according to their synodic period: Venus above the Sun and Mercury below it.

In 1461, Regiomontanus left Vienna for good in the company of Cardinal Bessarion. His association with Bessarion was connected with a long controversy between Bessarion and another Greek *émigré* in Italy, George of Trebizond (1396–1472). George had translated Ptolemy's *Almagest* from Greek into Latin in order to replace Cremona's 12th-century Latin translation from the Arabic but his new translation and the commentary were judged less than satisfactory. The commentary itself was full of errors and Bessarion was angered by George's attacks on Theon of Alexandria's commentary, which Bessarion recommended as a guide. The relationship of the two men deteriorated even further for philosophical reasons. In 1455, George published *Comparatio philosophorum Aristotelis et Platonis*, an *apologia* of Aristotle and an attack on Plato and his followers, especially Pletho and Bessarion. During his diplomatic visit to Vienna (1460–1461), Bessarion convinced Peurbach and Regiomontanus to write an epitome of the *Almagest* that would displace George's work on the subject. Peurbach started, finished half of the *Epitome of the Almagest*, and then died suddenly in April 1461. When Bessarion left for Italy, Regiomontanus accompanied him and remained a member of the Cardinal's *familia*, improving his Greek, revising Peurbach's first half, and writing the remainder of the *Epitome*. He completed the task in about 1462. The *Epitome*, however, remained a manuscript with limited circulation which became wider after it was printed in Venice in 1496, the year of Copernicus' arrival in Bologna.

The *Epitome* is a detailed, sometimes updated, condensed, and clearer exposition of Ptolemy's *Almagest*. Its format follows the general structure of the *Almagest* but has a more Euclidean layout. Along the lines of the *Almagestum parvum*, each book is organized into propositions, many followed by proofs. It sometimes comments on post-Ptolemaic developments. On the other hand, the summary of book 1—the most natural-philosophical part of the *Almagest*—leaves the discussion in the second century and says nothing about the late-medieval natural-philosophical debates about the rotation of the Earth. Among the problems of Ptolemy's astronomy, the *Epitome* notes the problems with its lunar theory. Another intriguing feature is the proof of the equivalence of the epicyclic and eccentric models for the second anomaly of the planets in book 12.

After finishing the *Epitome*, Regiomontanus dived into Bessarion's library, which contained 1,000 Greek and Latin manuscripts and included several Greek *Almagest*s, Proclus' *Hypotyposis astronomicarum positionum*, Theon of Alexandria's *Commentary on the Almagest*, and Theon of Smyrna's *Mathematical Knowledge Useful for Reading Plato*. It is worth noting that Proclus, in his *Hypotyposis astronomicarum positionum*, refers to the proof of the equivalence between the eccentric and epicyclic models.

In 1463, Regiomontanus entered into a correspondence with the Italian astronomer Giovanni Bianchini that demonstrates his mathematical skills, his dissatisfactions with the existing tables and mathematical models, and his expectations of consistency in physical and mathematical predictions, all being consistent with his hopes for the advent of a homocentric astronomy. His *Defensio Theonis contra Georgium Trapezuntium*, a work intended to destroy George's *Commentary on the* Almagest, reveals Regiomontanus' desire for an astronomy that would integrate physical and mathematical considerations. The *Defensio* shows his conflicting sympathies: Ptolemaic, homocentric, and Peurbachian. Regiomontanus "faced a trilemma that left unresolved the tensions between the pros and cons of his three options" [97]. In this text, Regiomontanus also treats the order of the planets as an unsolved problem and illustrates it by citing the different positions taken by Ptolemy, Martianus Capella, Geber, Biṭrūjī, and others:

> Copernicus would work on precisely this problem and was thrilled to see that reordering the planets (and the Earth) around the mean Sun gave their spheres a necessary order. [97]

After some time spent in Hungary, Regiomontanus moved to Nuremberg and set up the first printing press devoted primarily to the mathematical sciences.

What about Copernicus' use of Regiomontanus' work? Copernicus owned and used several works by Regiomontanus, especially his *Epitome of the Almagest*, in many ways. The earliest traces of the language of the *Epitome* are in Copernicus' "computations of planetary spheres that preceded the conversion to heliocentrism before the *Commentariolus*" [102] but they also pervade the detailed quantitative implementation of his new theory in his

De revolutionibus. Another point of considerable significance is that the *Epitome*

> stressed some of the unfinished business of astronomy, such as the order of the Sun and the inferior planets, to which Regiomontanus explicitly ascribed "no certainty" (*nulla certitudine*) at the beginning of Book 9. [102]

But the most important impact of the *Epitome* on Copernicus is that it

> stands behind Copernicus' move to his new astronomical system, which placed not the physical Sun but the mean Sun at the center of the Earth's orb. [102]

Another significant sign of Copernicus' faith in the *Epitome* is his

> following Regiomontanus in *not* undertaking to derive his astronomical models themselves from observations. Both men believed that, whatever their problems from a physical point of view, Ptolemy's models were basically adequate to their task from the geometrical and predictive points of view. [108]

Rivka Feldhay and Raz Chen-Morris ("Framing the Appearances in the Fifteenth Century: Alberti, Cusanus, Regiomontanus, and Copernicus" [110–140]) analyze different conceptualizations of appearances (*phaenomena*) in the 15th century and their possible relevance for Copernicus. In an often overlooked passage of the *Commentariolus*, Copernicus denounces the philosophers' defense of the immobility of the Earth as being founded upon appearances; and in his later *De revolutionibus*, he explains the phenomena of the movements in the heavens, such as the risings and settings of the zodiacal signs and the fixed stars, the stations of the planets and their retrogradations, by the motions of the Earth "which the planets borrow for their own appearances" [Rosen 1992, 18]. Copernicus' claim, in other words, is that the immobility of the Earth, one of our most basic visual experiences, is just apparent (visible but not true), while at same time he affirms that the mobility of the Earth—not experienced, invisible—is a reality that explains the apparent motions of the stars and the planets. How could he have come to such a conclusion? Or, to put it differently, "[W] hat enabled the competent, cautious astronomer Nicholas Copernicus to embrace the idea of an invisibly moving Earth?" [114]. In line with the introduction, Feldhay and Chen-Morris are critical of Swerdlow's technical reconstruction of Copernicus' path to heliocentrism. Why did Copernicus, they ask, find a heliocentric conversion of an eccentric model of the second anomaly for the inferior planets attractive (i.e., the element, according to Swerdlow, that is crucial in the transition to a heliocentric cosmology), whereas Regiomontanus simply stopped short of all that?

> If Regiomontanus was very likely aware of the possibility of a heliocentric conversion, as Swerdlow maintains, one may rightly assume that there was

> no mathematical-technical reason for him to reject it. Likewise, there was no mathematical-technical reason for Copernicus to adopt it and infer further the motion of the Earth. [114]

There is “no clear answer to such a question” [114], but Copernicus’ claim about his engagement with something “beyond appearance” (*praeter apparentia*) encourages an investigation of the conceptualizations of the relationship of appearances to their “beyond” in 15th-century Europe.

Feldhay and Chen-Morris search for an answer to their question in the works and practices of Leon Battista Alberti (1404–1472), Nicholas of Cusa (i.e., Cusanus) (1401–1464), and Johannes Regiomontanus (1436–1476). These three important figures, plus Paolo Toscanelli (1397–1482), were connected through a social network: Regiomontanus, Toscanelli, and Cusanus even met personally at Bessarion’s villa in Rome, while Alberti, a member of the papal curia since 1420, was a constant visitor to the villa—which

> testifies to the existence in Italy of a cultural field in which mathematicians...as well as philosopher-theologians like Cusanus took a position and articulated their critique of each others’ views. [113]

Copernicus probably acquainted himself with this field when he came to Bologna in 1496, and “this field may have inspired his daring to experiment with the idea of a moving Earth” [114].

Alberti’s *De pictura* (1435–1436) laid the foundations for the theory of artificial perspective. Feldhay and Chen-Morris see it

> as an ambitious project to broaden the scope of the visible that challenged the accepted boundaries between the natural and the artificial. [113–114]

His enterprise concerned the question of how a

> sensible and mathematical, yet invisible, grid of perspective constitutes the spatial relationships on the surface of the painting and offers a new perception of beauty radiating from things represented to the observer’s understanding. [113]

According to Alberti, the artist does not imitate and represent nature itself but aims at the forms of beauty that are “lurking beyond the phenomena and concealed behind them” [116]. Painting on a two-dimensional surface brings forth Alberti’s ideal of beauty, such as the “symmetry” and “harmony” between the different parts of the painting.

The desire to see what is beyond appearances found similar expression in the theologian Cusanus, who elaborated Alberti’s project by different means. In his major works, from *De docta ignorantia* to *De possest*, Cusanus attempted to explain how one can “view things that were invisible before” and how the mind can be presented "with a vivid image of the invisible unification of opposites (*oppositorum coincidentia*)" [117], i.e., God. One of the methods that he used for such purposes was speculative mathematics, with which he tried to solve the quadrature of the circle. He wrote 11 mathematical treatises dedicated to the quadrature and corresponded about it with fellow mathematicians, philosophers, and theologians. He tried to find a "visible" geometrical point that would represent the "invisible" coincidence of opposites (i.e., an intellectual vision of God). According to Feldhay and Chen-Morris, Cusanus' writings on quadrature

> engaged the best European mathematicians of the period—whom he personally knew—in a conversation about the quadrature across disciplinary and professional boundaries. The echoes of this conversation were likely to have reached Copernicus in Bologna and Ferrara some decades after they took place among Cusanus, Regiomontanus, Toscanelli, and perhaps even Alberti. [121]

For Cusanus, mathematics was not just a method but a model used in the constitution of the world for human understanding. His statement that

> the intellect is to truth like the polygon is to the circle in which it is inscribed [reveals] the motivation behind his investigations of the quadrature problem, namely to observe critically, from an imagined divine point of view, the limitations of the human intellect. Applying the results of his investigations to the theological realm, Cusanus broadened Alberti's discourse on the visible-invisible relationship and provided new kinds of legitimization for naturalizing the invisible within the discourse on human knowledge. [113]

Cusanus' conceptualizations of the mathematical conclusions in theological terms belong to the history of "invisibles" "that may have made possible Copernicus' later leap into a cosmological invisible such as the motion of the Earth" [121].

Cusanus' preoccupation with mathematical procedures came to the notice of Johannes Regiomontanus, *via* the Italian mathematician Paolo Toscanelli, a common friend. Regiomontanus wrote a series of texts on the quadrature of the circle, criticizing Cusanus' "speculations". Regiomontanus' distance from Alberti's and Cusanus' projects of representing invisible and abstract entities in a visual form is also manifest in his views on the required astronomical reform and the place of observation within it. Regiomontanus constantly complained of the erroneous observations of his predecessors and put his trust in those astronomers ready to make new observations and compare them with sound and good calculations. He himself barely bothered to improve the situation.

What is the stance of traditional astronomy regarding appearances? Since antiquity, astronomy had been based on what the astronomer saw and appearances were assumed to be valid and authentic regardless of the specific theory suggested.

> All there was to be explained was in front of the astronomer's eyes, and these explanations were supplied under the assumption of order. [134]

Appearances are true; they are not illusions and have to be explained in accordance with the assumption that the motions of the heavenly bodies are by nature uniform and circular. For a static observer situated at the center of the universe, the planets really do retrograde. The task of the astronomer is to find

> a system of circles to explain why the planets move in such peculiar ways without damaging the cognitive value of the observer's ocular experience. [134]

Either an eccentric circle or an epicycle would do the job but they are both "calculated in relationship to the point of view of an observer situated at the center of the universe" [135]. This dependence of mathematical theory on visual experience is clear from Ptolemy's presentation of the equant as an explanation of the anomalies of the planets. The equant is a point that is not directly related to the observer but to a "point bisecting the line joining the center of the ecliptic and the point about which the ecliptic has its uniform motion" [135]. Ptolemy admits that this procedure is not taken from any apparent principle. It is without proof: its only justification is that it is in agreement with the phenomena. For Ptolemy, the coherence of the models is less important than saving visual experience, which has to be realized in accordance with the more basic principle of preserving uniform circular motion without exception. The specific feature of the equant is that it "implies that the point from which planetary motions can be viewed as uniform is an imaginary point unrelated to the position of the observer" [135].

But, while the eccentric spheres are physically real and calculated with regard to the observer's central position, the equant is, according to Peurbach, based on an imagined circle around the equant point, i.e., around the point on the line of the apogee as far from the center of its orb as this center is distant from the center of the world. The basic characteristic of traditional astronomy, upheld also by Regiomontanus, was that it assumed the reality of celestial appearances. There is no doubt about what one sees. Astronomers apply invisible spheres and circles only to substantiate the authenticity of their observations. Alberti and Cusanus, however, challenged this traditional conception of astronomy on several levels by probing the demarcation between the phenomenal realm and the realm of invisible structures:

(1) the position of the observer is not predetermined and static; appearances are relative to one's point of view, and
(2) it is possible "to peer beyond appearances to gauge invisible structures and entities through the use of different kinds of devices" [135–136].

"These two notions", claim the authors, "may have shaped Copernicus' propensity to accept the invisible motion of the Earth as a basic principle of his system" [136]. In both the *Commentariolus* and *De revolutionibus*, Copernicus continuously points out that appearances misled astronomers into ascribing the wrong motions to the celestial bodies and that one should adopt a critical attitude toward the testimony of the eyes. The interpretation of visual experience has to take into account the position(s) of the observer's own actual viewpoint (no longer central) and his or her location within the entire universe (there are constant changes due to the Earth's motions).

> Going beyond one's local and immediate point of view entailed the realization that appearances are a function of the observer's location. The new forms of visibility proposed by Alberti's techniques of perspective and by Cusanus' geometrical visualizations were part of a more general cultural reassessment of the role of perception in the cognitive process leading to knowledge. This role had special relevance to the epistemological status of astronomy, the observational science par excellence....The core of Copernicus' argument is the limits of sense perception and the need to surpass them. [140]

Whether the Earth moves or not

> cannot be derived from one's sense experience, as these phenomena presuppose the observer's point of view. By calculating the observer's position, Copernicus can transcend visual experience and gauge a new invisible point of view from where a new picture of the universe is revealed. These calculations incorporate novel mathematical techniques coming from the East, yet Copernicus mobilizes these techniques to answer the challenges that Alberti's artificial perspective and Cusanus' theological speculations offered to visual experience in the preceding century. [140]

1.4 *Part 3. Copernicus' multicultural background* To open part 3, Sally Ragep ("Fifteenth-Century Astronomy in the Islamic World" [143–159]) paints a fascinating canvas concerning the number of students and practitioners of mathematical sciences (some contemporaries referred to roughly 500 students) in 15th-century Samarqand. This number and the enormous quantity of manuscripts that survived are testimony to how entrenched a scientific education was within Islamic society. Roughly 120 authors wrote some 489 treatises during the long 15th century. Their works are represented by several thousand extant manuscripts located throughout the world. The subject matter of these works was both theoretical and practical astronomy, and it included cosmology (both celestial and terrestrial realms), instruments, handbooks, tables, calendars, timekeeping, and astrology. S. Ragep focuses in particular on theoretical astronomy, i.e., the tradition of *hayʾa*. Works in this tradition belonged to a genre of astronomical literature termed *ʿilm al-Hayʾa*, which attempted to explain the configuration (*hayʾa*) or physical structure of the universe as a coherent whole; thus, for celestial bodies, it included cosmography and for terrestrial bodies, geography. This tradition brought into a single discipline the unchanging celestial realm of aether and the ever-changing realm of the four elements, the world of generation and corruption. This tradition can be traced back to the 11th century when the term « hay'a » was adopted, particularly in eastern Islam, as the general term for the discipline of astronomy which did not include astrology. The *hayʾa basīṭa* literature was influenced by Ptolemy's *Almagest* (omitting its mathematical proofs) and by his *Planetary Hypotheses*, and usually included discussions of the sizes and distances of the stars and planets. The main emphasis of the *hayʾa*-tradition was on translating mathematical models of celestial motion into a bodily representation in order to show the configuration (*hayʾa*) of the universe as a whole. It focused on external aspects of cosmology, on issues related to *how* the celestial and terrestrial realms operate, not on questions as to *why*.

Another tradition of Islamic astronomy provided a range of accounts of various aspects of Ptolemaic spherical astronomy and planetary theory. It reworked Ptolemy's *Almagest* and sometimes included original material, such as there is in Ṭūsī's *Taḥrīr al-Majisṭī* (Recension of the *Almagest*) as well as treatises devoted to criticizing and reconciling inconsistencies in Ptolemaic astronomy and to reforming certain models, such as Abū ʿAlī al-Ḥasan ibn al-Haytham's *al-Maqāla fī hayʾat al-ʿālam* (Treatise on the Configuration of the World). This treatise attempts to explain how the various components of the Ptolemaic models worked and ultimately fit together. It strives to match the mathematical models of the *Almagest* with physical structures in order to explain the various motions of the celestial bodies.

From these and other examples, it is clear that Islamic astronomy in the 15th century was not an isolated event or episode but was built upon centuries of scientific work. This was also the astronomy "that most likely provided the immediate context of transmission to a bourgeoning European astronomy"

[156] through the institutions of the Ottoman Empire. It is, as S. Ragep affirms, "through these Ottoman institutions that one finds the connection between Islamic astronomy, Copernicus, and his immediate Latin predecessors" [156]. A certain Moses ben Judah Galeano (Mūsā Jālīnūs), the subject of the final chapter of *Before Copernicus* by Robert Morrison, was especially important in this transmission: he traveled, among other places, between the Ottoman court and Italy.

The last question posed by S. Ragep is why Islam, despite thriving scientific traditions and stunning achievements in astronomy, did not give rise to a Copernicus. She claims that the reason lies exactly in these traditions:

> Scientific change may be far more difficult when the traditions...are so entrenched....Thus, paradoxically, the strength of a scientific tradition, such as that in Samarqand, may have been a hindrance to adopting new, revolutionary ideas. Perhaps the lesson we then take from this cross-cultural comparison is that proposing revolutionary ideas may be easier for someone, such as Copernicus, whose scientific context was less rigid and was, in many ways, a work in progress. [158]

F. Jamil Ragep ("From Tūn to Toruń: The Twists and Turns of the Ṭūsī-Couple" [161–214]) takes up the case of the transmission of arguably the most famous astronomical device of Islamic astronomy, the so-called Ṭūsī-couple, which was invented by Naṣīr al-Dīn al-Ṭūsī (d. 1274) to amend Ptolemy's use of the equant. The Ṭūsī-couple is actually not a single device or model but a general concept that encompasses several different mathematical devices serving different purposes. There are several versions:

(1) the mathematical rectilinear version, which consists of two uniformly rotating circles that can produce oscillating straight-line motion in a plane between two points;
    (a) a physicalized version of (1);
(2) the two-equal-circle version, which is a curvilinear version meant to produce a linear oscillation on a great circle;
(3) the three-sphere curvilinear version, consisting of three additional orbs enclosing the epicycle that are meant to produce a curvilinear oscillation that results in motion in latitude; and
    (a) the two-sphere curvilinear version, which is a truncated version of the full three-sphere curvilinear version.

Ṭūsī elaborated different versions of the device at different stages of his career and used them to solve different technical problems. The first one and its physicalized version, for example, were used with the aim of replacing the equant in planetary models. The second one was meant to account for Ptolemaic motions requiring a curvilinear oscillation on a great circle.

Within the Islamic context, the Ṭūsī-couple was subject to further development and discussion over many centuries. Since there are no translations of Ṭūsī's writings on the couple in non-Islamicate languages, J. Ragep postulates transmission though non-extant texts and/or non-textual transmission and thus bases his case "on plausibility rather than direct evidence" [174]. He argues, given the various types of evidence of transmission, that "independent rediscovery, especially multiple times, becomes much less compelling" [175].

There were several appearances of the Ṭūsī-couple outside Islamic societies. The first occurred in Byzantium around 1300. It is found in the work of a certain Gregory Chioniades of Constantinople, the translator of a number of astronomical treatises from Persian (or perhaps Arabic) into Greek. One of them, which is dubbed *The Schemata of the Stars*, uses the Ṭūsī-couple in the lunar model and thus seems to derive from Ṭūsī's earlier Persian (not Arabic) works.

> [T]here can be no question that some of Ṭūsī's innovations had made their way into Greek by the early fourteenth century, and the existence in Italy of the only three known manuscript witnesses strongly suggests that the transmission of this knowledge had made it into the Latin world by the fifteenth century. [176]

In Latin Europe, the Ṭūsī-couple appeared several times—the first was in the 14th century. Here follows a list of authors in whose works it can be found: Avner de Burgos, Nicole Oresme, Joseph ibn Naḥmias, Georg Peurbach, Johann Werner, Giovanni Battista Amico, and Girolamo Fracastoro (*Homocentrica*, 1538), who refers to a device for producing rectilinear motion but does not incorporate it into his astronomy.

Copernicus used the Ṭūsī-couple in both his *Commentariolus* and his *De revolutionibus*. In the *Commentariolus*, he used the truncated two-sphere curvilinear version for the latitude models and the physicalized rectilinear version to vary the radius of Mercury's orbit, but in a truncated, two-sphere version without the enclosing/maintaining sphere. It seems, J. Ragep assesses,

> that Copernicus was attempting to provide actual spherical models for the two versions of the Ṭūsī-couple he uses in the *Commentariolus* but that he cut a corner or two by not dealing with the disruption of the contained orb. [184]

In *De revolutionibus*, Copernicus relies only on the two-equal-circle version, which is a mathematical, not a physical, model.

Although it seems that the majority of historians of early astronomy have accepted to a lesser or greater degree the influence of late-Islamic astronomy on early modern astronomers, particularly Copernicus, there are some (Di Bono and Goddu, for instance) who demand more evidence of transmission. In order to provide such evidence, J. Ragep summarizes the past 25 years on the issue. Dealing first with the critics of transmission (Veselovsky, Di Bono, Goddu), he then provides empirical evidence of transmission.

There is evidence that the Ṭūsī-couple first made its way into another cultural context through Byzantine intermediaries, first and foremost through Gregory Chioniades. This transmission occurred through an adapted translation from Persian into Greek. The circumstances under which Gregory's *Schemata* itself was further transmitted are less clear. The *Schemata* is currently witnessed by three manuscripts: two in the Vatican and one at the Biblioteca Medicea Laurenziana in Florence. These sources provide evidence that the work, with diagrams, was available in Italy as early as 1475. Swerdlow and Neugebauer favor this Italian route for the transmission of the Ṭūsī-couple to Copernicus. Since Copernicus spent part of the Jubilee year 1500 in Rome, this opens up the possibility that he had access to the *Schemata*.

There may also have been another channel of transmission—the Spanish connection—which could have brought the new astronomy of 13th-century Iran to the Latin West. There was considerable ongoing diplomatic activity between the Spanish court of Alfonso X of Castile and the Mongol Īlkhānid rulers of Iran.

And there is yet another possibility, the Jewish link. Tzvi Langermann and Robert Morrison have shed light on a host of personalities involved in the transmission of astronomical models from Islam to Christendom through Jewish scientists and mathematicians. Langermann has shown that in 15th-century Italy, Mordecai Finzi knew the *Meyashsher ʿaqov* of Avner de Burgos, in which it is proved that a continuous straight-line oscillation could be produced by means of a Ṭūsī-couple. That Finzi knew of the *Meyashsher ʿaqov* is indicated by his copying of some interesting technical details in Avner's text. It seems reasonable to assume, as J. Ragep claims, that Finzi "knew the other parts of the *Meyashsher ʿaqov*, including the Ṭūsī-couple proof" [190]. Finzi also had extensive contacts with Christians. Finzi is an example of "a Jewish scholar who most likely knew of the Ṭūsī-couple in contact with north Italian mathematicians a generation or so before Copernicus would be in the neighborhood" [190].

The last piece of empirical evidence of transmission discussed by J. Ragep is the sheer number of the manuscripts containing one or other of the versions of the Ṭūsī-couple. In this context, it is significant

> that the critical proposition that Swerdlow has claimed was used by Copernicus to transform the epicyclic models of Mercury and Venus into eccentric models, which is found in Regiomontanus' *Epitome of the Almagest*, was put forth earlier in the 15th century by ʿAlī Qushjī of Samarqand. [191]

It is not known how Qushjī's treatise came to be known by Regiomontanus but a very likely candidate for transmitter is Cardinal Basilios Bessarion.

Robert Morrison ("Jews as Scientific Intermediaries in the European Renaissance" [198–214]) takes up the role of Jews in the circulation of scientific knowledge. Morrison argues against a solely European context for Copernicus' work and discusses the criticism and modifications of Ptolemaic astronomy in both Renaissance Europe and Islamic societies, and how Copernicus could have learned of the achievements of astronomers from Islamic societies. The focus of the chapter is the Ṭūsī-couple and how a text in astronomy, *The Light of the World*, which was written by the Jewish astronomer Joseph ibn Naḥmias (*fl. ca* 1400) and composed in Judaeo-Arabic (a dialect of Jews in the Arabic-speaking world), and a recension of it written in Hebrew characters, could have interested Renaissance astronomers.

Morrison points out several parallels between *The Light of the World*, an attempt to improve Nūr al-Dīn al-Biṭrūjī's (*fl.* 1200) *Kitāb fī al-Hayʾa* (On the Principles of Astronomy), which was translated into Latin by Michael the Scot, and the works of early modern European astronomers interested in the revival of homocentric astronomy. Naḥmia supposes that all celestial motions occur on the surface of an orb and accounts for these motions by means of a set of homocentric orbs with the Earth at the precise center of that orb or set of orbs. His models improved on the predictive accuracy of Biṭrūjī's models, although not completely. Regiomontanus and other Renaissance astronomers, working and/or interested in the tradition of homocentric astronomy, would certainly be interested in his models due to their philosophical consistency. Since there is no evidence of the presence of theories from *The Light of the World* in the Veneto as early as 1460, Morrison agrees with Swerdlow that—despite certain similarities between Regiomontanus' homocentric models and the Hebrew recension of *The Light of the World*—it did not influence Regiomontanus..

One of the interesting technical features of *The Light of the World*, adopted in the Hebrew, is the improvement of the reciprocation mechanism. In addition to this development of the mechanism for reciprocal motion, both the Arabic and Hebrew versions contain another hypothesis that is mathematically equivalent to the curvilinear version of the Ṭūsī-couple in Ṭūsī's *al-Tadhkira fī ʿilm al-Hayʾa*. They both suggest the elimination of the circle of the path of the center and the inclined circle carrying the circle of the path of the center from the solar model. This is the model that appeared in Giovanni Battista Amico's (d. 1538) *De motibus corporum coelestium*, written in the 1530s in Padua. Another reviver of homocentric astronomy, Fracastoro, referred to the double-circle hypothesis but did not incorporate it into his astronomy. There is a real possibility that Amico and Fracastoro could have learned of the double-circle hypothesis from *The Light of the World*.

Morrison continues by presenting specific connections between Islamic, Jewish, and European scholars and routes by which Jews became intermediaries between Islamic astronomers and European Renaissance intellectuals. Morrison focuses on two possible channels. One of the possible mediators, probably the key one, was Moses ben Judah Galeano (Mūsā Jālīnūs, d. after 1542), who was present at the court of the Ottoman Sultan Bāyazīd II (1481–1512) in Istanbul. Galeano composed a Hebrew text entitled *Ta ʿalumot ḥokmah* (Puzzles of Wisdom) around 1500 and finalized it in the 1530s. *Ta ʿalumot ḥokmah* mentions the astronomy of ʿAlāʾ al-Dīn ibn al-Shāṭir, whose models figure extensively in Copernicus' work and explains that *The Light of the World* was a text about homocentric astronomy. It also describes Galeano's visit to Venice around 1500, during which he met with the prominent printer Gershom Soncino. Another possible route for the passage of *The Light of the World* was from al-Andalus to Istanbul and from there to Padua. Linguistic evidence suggests that Galeano's own text on homocentric astronomy found in the Topkapi Library was translated from Hebrew or transcribed from Judaeo-Arabic. It is, therefore, plausible that the extant Arabic text by Galeano is a translation or transcription carried out in Istanbul of a now lost Hebrew or Judaeo-Arabic version of *The Light of the World*, which was probably made before Galeano left Istanbul for Venice. In any case, the contents of *The Light of the World*, if not the complete manuscript, clearly found their way to Istanbul.

The striking parallels between Ibn Naḥmias' theories and those of the astronomers in Padua, Galeano's voyage to Venice, and the much later report of *The Light of the World*'s being at Padua make it highly likely that scholars at Padua such as Amico and Fracastoro were aware of the contents of *The Light of the World*. The career of Moses ben Judah Galeano helps to explain the numerous parallels with Ibn al-Shāṭir's theories in Copernicus' work.

Another question regarding scholarly exchange is whether any Jews knew what contemporary European Renaissance astronomers were doing. As proven by translations of Averroes' (Ibn Rushd's) corpus into Latin, there was an area of contact between Jews and Christians in Europe: Jews translated three-fourths of Averroes' writings into Latin from Hebrew translations of the original Arabic. Furthermore, there is some evidence that the last Jewish Averroist, Elijah Delmedigo (d. 1493), knew of recent efforts to develop new theories in astronomy. While his commentaries on Averroes' Latin *Metaphysics* and on his *On the Substance of the Celestial Orb* do not refer explicitly to Ibn Naḥmias or even to Biṭrūjī's work, Delmedigo's Hebrew commentary *On the Substance of the Celestial Orb* makes "a clear connection between the dismissal of eccentrics and epicycles and Renaissance Averroism's interest in the physical world" [210]. In the same commentary, Delmedigo also makes a reference to attempts to reform Ptolemaic astronomy in the face of the familiar Averroist criticism that Ptolemy's eccentrics and epicyclic orbs contradict the roots of natural science. He complains that some later astronomers were trying to save Ptolemy by positing bodies without any function except for filling the void. Morrison suggests that Delmedigo here refers to Ibn al-Haytham or Jābir ibn Aflaḥ, critics of Ptolemy, cited in Ibn Rushd's *Talkhīṣ al-Majisṭī*. Since Delmedigo's manuscript was probably composed in 1485 and copied in 1492, that is, before Delmedigo returned from Italy to Crete, it is possible that "the attempts to save Ptolemy to which Delmedigo referred were attempts by European astronomers such as Regiomontanus, not the work of recent Islamic astronomers" [211]. This would provide evidence

> that a prominent Jewish scholar may well have known of developments in 15th century European astronomy, providing more indications that Galeano would have known that there were European astronomers interested in the news he was bringing from the Ottoman Empire, and/or it is evidence that another Jewish scholar in Galeano's milieu knew about important achievements in Islamic astronomy. [211]

But even if the referent were earlier critics of Ptolemy, this text would have alerted the reader to the interest of scholars in Europe (which is where Delmedigo was writing) in models based on perfectly homocentric orbs as solutions to the known problems of Ptolemaic astronomy. The role of Jews from both Andalusia and the Ottoman Empire in the scholarly exchanges is also evident from their role in the composition of astronomical tables.

2. Critical assessment

Before Copernicus is a rich book in terms of both scope and depth.[3] The result of a project extending more than 15 years and four workshops held at different academic institutions, the book brings together eight chapters written by some of the leading experts in the field who can claim a substantial number of important publications. Most of the chapters, if not all, make very handy summaries of the previous research and publications by the authors and other scholars, adding at the same time fresh and nuanced details and insights. Many chapters are illustrated by very useful tables, diagrams, and images. No summary, no matter how extensive, can do complete justice to the wealth of detail, technical and historical nuance, and profound analysis based on a close examination of the vast number of primary sources, while keeping the results of previous research in mind.

In general, I consider the following to be the major strengths of *Before Copernicus*. The first is its very topic: before Copernicus. There had been, despite significant previous research and publication, a need for a comprehensive and up-to-date reexamination of the numerous topics that focus on the immediate and less immediate contexts of Copernicus. We now have a general overview of the basic features of the long 15th century and European attitudes toward the Islamic world as well as a handy and comprehensive study of:

- the development of physical astronomy and different concepts of astronomy as a science during the Middle Ages and Renaissance;
- Regiomontanus' approaches to astronomy and his impact on Copernicus, an intriguing chapter on the different conceptualizations of appearances and their "beyond";
- Islamic mathematical scholarship in Samarqand and elsewhere;
- the Ṭūsī-couple and its possible transmission channels; and finally
- the role of Jews as scientific intermediaries.

The second is the book's collaborative nature. Authors with different preoccupations, specialists in their own areas of pre-Copernican and Copernican scholarship, concentrate on clearly defined topics (the social and intellectual background to Copernicus' *Commentariolus*). Due to the complexity and enormous range of the issues, this is—as I have experienced myself—hardly a task for one person.

[3] All critical remarks and suggestions that follow are based on my research on Copernicus and his context, which was published in Vesel 2014.

Its third is its "multicultural" approach. Although the influence of Islamic astronomy on the Latin West, including Copernicus, has been known and widely acknowledged, some scholars still doubt it, especially when it comes to Copernicus. Copernican astronomy is even nowadays sometimes—completely anachronistically and perhaps also ideologically, to use a mild word—supposed to be a pure European achievement. "They", the "others", allegedly have nothing to do with his genius. Opposition to such an attitude runs the risk, though, of making Copernicus more indebted to Islamic astronomy than he really was. Putting aside J. Ragep's brief reference to Islamic discussions on the motion of the Earth [see note 3 above], his chapter and the others that discuss the Islamic influence on Copernicus avoid this pitfall.

Its fourth is its multidisciplinary approach. On several occasions, Feldhay and Ragep in their introduction and Feldhay and Chen-Moriss in their chapter make it clear that Copernicus' heliocentric cosmology was not achieved by a purely technical route. There is, as Feldhay and Ragep put it nicely, "more to this monumental cosmological shift [i.e., from a geocentric to a heliocentric cosmos] than a strictly mathematical/astronomical explanation" [4].

With that said, let me now address the question, Does the book explain the nature of Copernicus' *Commentariolus* and his work in general? I believe it does—but only to a certain extent. It leaves out, unfortunately, some of its essential aspects. If the social and intellectual background that shaped the astronomy and cosmology of the *Commentariolus* (and consequently *De revolutionibus*) are to be understood correctly, many issues that should be addressed are either missing or not adequately treated in this volume. These issues range from the treatment of Copernicus' studies and his work after his final return home from Italy to more theoretical reflections on what Copernicus actually says in the *Commentariolus*, which was, I believe, to a large extent a result of his years in Italy and his work after he returned home. *Before Copernicus* treats his Italian years and what he had learned—the possibilities that had opened up for him there—very superficially. Its focus is mainly on his years in Cracow and, within this framework, only Aristotelian influences are taken into account. A more theoretical problem is that the *Commentariolus* is treated very selectively. When it is cited and discussed, many nuances are overlooked. One would like to understand specifically how Copernicus' context is linked to his text(s). Let me illustrate my reservations by following the structure of the book, starting with the introduction.

I find the last five observations, numbers (3) to (7) [see pp. 76–77 above], and the conclusions reached therefrom to be more or less sound. I also very much agree with Feldhay and Ragep that Swerdlow's technical reconstruction of Copernicus' conversion to heliocentrism is not conclusive. I have, however, some reservations about "observations" (1) and (2) regarding the principle of uniform motion and the absence of the *symmetria*-argument stated in the *Commentariolus* and the conclusion(s) that they derive from them. It is of course indisputable that Copernicus' first stated purpose in the *Commentariolus* is, to put it briefly, to satisfy the principle of perfect, uniform, and circular motion. It is also true that Copernicus here does not refer *explicitly* to the "marvelous symmetry" of the world. But it is not clear to me what exactly is the point of the editors' conclusion(s) reached from numbers (1) and (2) [see p. 77 above], i.e., that Copernicus' initial motivation was the equant problem and that the justification from the *symmetria*[4] achieved by a heliocentric cosmology was *post hoc* and that, as a consequence, it did not play a motivating role. Motivation to do what? To start working on the problems of Ptolemaic astronomy? To reform astronomy in such a way that it would be brought into line with the principle of uniform, circular movement? To reform it along heliocentric and geokinetic lines? Or something else?

It could well be that Copernicus was primarily moved to tackle the reform of Ptolemaic astronomy by "irregularities" contravening the principle of circular uniform motion. Or by any other "irregularity" that he might have learned of from the astronomical literature at his disposal. It is completely plausible and reasonable. But if that alone were the case, Copernicus would have stayed within a reformed geocentric system. As Feldhay and Ragep nicely explain, this

> would have secured his fame, earned him the gratitude and admiration of his contemporaries and successors, and spared him and those successors a considerable amount of grief. [7]

Copernicus does not rest with a reformed geocentric cosmos, however. Just a few paragraphs after his complaint about these "irregularities" and after he lists seven (heliocentric) *petitiones*, he argues for the order of the cosmos on the basis of the so-called distance-period principle [see Goldstein 2002], the same principle that he also uses in his mature *De revolutionibus*, where he claims that in this way a marvelous *symmetria* (or *harmonia*) of the world is achieved. In the heliocentric order of the spheres, Copernicus affirms in the *Commentariolus* that "one [planet] exceeds another in rapidity of revolution

[4] Note that Copernicus used Greek in his *De revolutionibus*.

in the same order in which they traverse the larger or smaller perimeters of [their] circles" [Swerdlow 1973, 440]. Saturn makes its period in 30 years; Jupiter, in 12; Mars, in two, while Earth has a one-year period; Venus, nine months; and Mercury, three months. The only difference between the *De revolutionibus* and the *Commentariolus* is that in the latter Copernicus does not explicitly mention *symmetria* (or *harmonia*). But the principle and the results of that principle are already there. Thus, the ordering of the planetary spheres *was*, then, an important motivating consideration already in the *Commentariolus*. So, if the aim of the book is to render the *Commentariolus* understandable, it should not avoid discussing this issue. But, as it stands, this essential feature is left unaddressed.

The question, as I see it, is, therefore, What connects the issue of the principle of perfect motion and, as it was subsequently called, the harmonious order of the planets? Since Copernicus did not arrive at heliocentrism by a technical route, linearly, so to speak, from the equant problem to the problem of the *forma mundi*, there must be some conceptual common denominator of both issues. What exactly is the "more" from Feldhay and Ragep's claim that there must be "more to this monumental cosmological shift than a strictly mathematical/astronomical explanation"? Which aspects of his "intellectual and cultural context...led him to his decision to put the Earth in motion"? [6–7].

2.1 *On part 1* While the first two chapters depict some of the matters that could be relevant to Copernicus, they remain on a very general level and are, in my view, of relatively limited use for understanding his *specific* astronomical and cosmological enterprise. Bisaha provides some possible explanations of Copernicus' silence as to his Islamic sources, among which the "innocent omission at some point in the transmission" seems the most appealing to me. Celenza in his turn does mention Copernicus' study at the Universities of Bologna and Padua but devotes very little attention, almost none, to the curricula there. He does not say anything about the books that Copernicus purchased at the time and there is nothing on the people with whom he may have discussed burning astronomical and astrological questions (the astrological "crisis") [see Westman 2013, 76–105]. Moreover, there is nothing about Copernicus' learning the Greek language nor about his visit to Rome where he may have had access to Bessarion's library (mentioned by J. Ragep), and so on. In Padua, for instance, Copernicus very probably learned Greek with Nicholas Leonicus Tomaeus, an acquaintance of Callimachus (they met in Venice in 1486), who was very active in Cracow. Tomaeus read Plato in Greek at the University of Padua from 1497 to 1506 and translated a portion of Plato's *Timaeus* 35a–36e along with Proclus' commentary on the same passage. Girolamo Fracastoro, author of the *Homocentrica* (1538), who was in Padua at about the same time as Copernicus, first as a student and then as a teacher of logic, reported that the homocentric revival initiated by Giambattista Della Torre was somehow related to Plato's *Timaeus*. In his dedication to Pope Paul III in the *Homocentrica*, he explains that Della Torre, on his deathbed, told him to recall the circles from the *Timaeus* in the shape of the letter X [Fracastoro 1538, "Sanctissimo Pavlo Pontifici Maximo"]. Fracastoro refers here to *Timaeus* 36b–c, which is included in the part translated by Leonicus Tomaeus.

2.2 On part 2 This neglect of Copernicus' student years is partly amended by Sylla's chapter. She thoroughly discusses three important books of two of the most remarkable teachers of Cracow, both with interests outside astronomy, and sets them in a broader context. Her discussion of the history of physical astronomy in the long period from Ibn al-Haytham through the Middle Ages to Copernicus' years in Cracow, and of the status of astronomy as a science as debated by *antiqui* and *moderni* as well as in the three texts by Głogów and Brudzewo, is very thorough, interesting, and useful. One becomes aware of many matters previously unknown or known only partially. Among many useful insights, I would point out Brudzewo's understanding of the equant as mathematical (hence, imaginary) and not as physical.

There are several problems, though, which I see in her account. The first two are more general in nature but with important consequences for understanding the *Commentariolus* (and *De revolutionibus*). She limits her discussion to Copernicus' studies in Cracow and makes several remarks that at least imply—if not directly affirm—that those years constitute the decisive background to his *Commentariolus*. What about his subsequent studies in Bologna and Padua? Did they not contribute anything to the genesis of the *Commentariolus*? And what did Copernicus do after he returned to Warmia but before he wrote the *Commentariolus*?

Sylla also directs her attention only to the Aristotelian tradition and completely ignores the humanist and Platonist current(s) of Cracow's intellectual life. This is strange since there is plenty of evidence thereof. Filippo Buonaccorsi, called Callimachus Experiens (1437–1496), as already mentioned, was very active in Cracow. He corresponded with the Platonist and translator of Plato's *Opera omnia*, Marsilio Ficino (1433–1499), who called Callimachus "my fellow Platonist". Callimachus was constantly traveling from Cracow to Italy and Constantinople. In 1485, one of Cracow University's reading rooms was called Plato's and Albert of Brudzewo was mentioned in that connection. Even John of Glogów, who appears to have mostly drawn on the Aristotelian tradition, was well versed in other philosophical schools of thought, including Plato and Platonism. In his manuscript *In metaphysicam* (or *Quaestiones super duodecim libros metaphysicae Aristotelis*), to give just one example, he mentions Plato approvingly several times. While in Cracow, Copernicus was also closely connected with Laurentius Raabe Corvinus, another Platonist, one of the most important members of the Cracow's humanist Vistulan Literary Sodality. After Copernicus' return from Italy, Corvinus helped him publish his Latin translation of Theophylactus Symocatta's Greek *Epistolae morales, rurales et amatoriae*.

There is no doubt in my mind that Copernicus (and those of his contemporaries who read it) understood *Commentariolus* as a *theorica*. It is a theoretical astronomy, using physical astronomy (the three-orb compromise) and partly mathematical astronomy. It also fits quite nicely into the practice of some *theoricas* by establishing some physical principles on which the subsequent astronomy is based. According to Sylla, Copernicus mirrors these physical principles with his *petitiones*; namely, Copernicus claims that he could solve the problem "if some postulates, called axioms (*petitiones quas axiomata vocant*) are granted to us" [Swerdlow 1973, 436]. Sylla calls these *petitiones* hypotheses or principles, puts them on a par with scholastic suppositions, principles, or premises, and claims that they are "derived from experience" [49]. She also claims that in the *Commentariolus* these principles are stated postulates (*petitiones*), while in Peurbach's *Theoricae novae* they are the *theoricae* (figures) themselves.

Despite some similarity between the *Commentariolus* and Brudzewo's *Commentary on Theoricae novae* in the matter of the physical principles, I believe that an epistemological distinction is in order. Copernicus' postulates or axioms are neither derived from experience nor have exactly the same epistemological status as suppositions, principles, or premises. How, for instance, can the fifth postulate—

> Whatever motion appears in the sphere of the fixed stars belongs not to it but to the earth. Thus the entire earth along with the nearby elements rotates with a daily motion on its fixed poles while the sphere of the fixed stars remains immovable and the outermost heaven. [Swerdlow 1973, 463]

—be derived from experience? And if it were—let us allow this for the sake of the argument—from which experiences or observations exactly? There are approximately 70 documented observations by Copernicus, and he occasionally does refer to observations and measurements of the positions of the stars from which ancient philosophers worked out their planetary theory. But I am not aware of any instance when he did so in reference to himself.

As noted by Shank, Copernicus was "following Regiomontanus in *not* undertaking to derive his astronomical models themselves from observations" [108]. It would be very useful to make a list of all of his observations and analyze them to determine what precise purposes he had in using them.

I also do not understand how the statement of principles in the *Commentariolus* can mirror—this time, specifically—that in Peuerbach's *Theoricae novae*. Why would Brudzewo need to "establish" principles in his commentary, as Sylla claims he did [53], if they were already established by Peurbach himself (figures/*theoricae*)?

I believe that the key to the secret of Copernicus' axioms or postulates is to be found elsewhere and that it is Copernicus himself who reveals where. In one of his annotations to Plato's *Parmenides* in Ficino's translation, he writes "what needs to be known about hypotheses (*quid aduerti oporteat circa hypotheses*)". Copernicus obviously understood hypotheses, axioms, and postulates in Platonist terms. This is further confirmed when we compare the *Commentariolus* and Proclus' *Commentary on Plato's Timaeus* 2.3. In this passage, Proclus explains that Plato is not an empiricist: Plato does not start with experiences and then draw conclusions. Plato's method (μέθοδος) is hypothetical or, rather, Plato uses the method of hypothesis. He sets out fundamental axioms (ἀξιώματα) and hypotheses (ὑποθέσεις) and draws conclusions. Proclus first presents a list of five axioms and then follows with another list of seven. Describing Plato's "hypothetical method", Proclus does not refer to Plato's own description of the hypothetical method but explicitly refers to the method used by geometers. They first postulate, define, and name their key principles before proceeding to their demonstrations. And he cites an example from Euclid. On the basis of fundamental principles or hypotheses, Plato's *Timaeus* then proceeds, in Proclus' reading of the text, to a number of demonstrations (ἀποδέξεις) required in order to solve the problems. Copernicus' method in the *Commentariolus* is highly reminiscent of Proclus: he first establishes seven *petitiones quas axiomata vocant* and then promises to provide mathematical *demonstrationes* in a larger book.

I find Shank's chapter to be one of the highlights of *Before Copernicus*. In a very well written, exciting exposition, Shank depicts the interrelatedness of seemingly unrelated issues—astronomical (the controversy regarding Ptolemy's *Almagest*), religious, and political (the Crusades, Orthodox/Roman Catholic Christianity)—that played a part in the life and work of Regiomontanus, the most advanced astronomer before Copernicus. From his exposition, it is abundantly clear that Copernicus was working not in a void but in a period of vigorous institutional development in astronomy that was to a large extent due to Regiomontanus' work and his printing activities, themselves in turn the result of long and multifaceted dispute. The main characteristic of Regiomontanus' work is its search for a philosophically (i.e., physically) adequate astronomy. He also makes it clear why Regiomontanus was justly considered the most advanced astronomer in the second half of the 15th century as well as to what extent and regarding what particular details Copernicus relied on and used his work.

I have only one remark here. Shank complains that while intellectual historians are familiar with George of Trebizond's attacks on Plato and Cardinal Bessarion's defense of the latter, "the astronomical and astrological dimensions of that conflict are poorly integrated into the history of astronomy" [87]. As are, I would like to add, the philosophical dimensions. What do I mean? Copernicus bought and annotated a book by Cardinal Bessarion, *In calumniatorem Platonis*, in which he read praise of Plato as a mathematician. In book 4, chapter 12, for example, Bessarion defends Plato against the accusation that mathematics was to be taught to those who wanted to become divine. He declares that, according to Plato, mathematics was truly the subject most worthy of study by a free man and continues, paraphrasing the *Epinomis*, that the easiest way to ascend to the divine was through mathematics. He concludes the chapter by referring the reader to books 7 and 10 of the *Laws*, to the *Epinomis*, as well as to books 5, 6, and 7 of the *Republic*. This is relevant to the question addressed by Chen-Morris and Feldhay: How did Copernicus end up going "beyond the appearances"? While this is the right question, however, their answer, I am afraid, is not correct. I share with them numerous epistemological conclusions about Copernicus' work. I strongly agree that Swerdlow's reconstruction of Copernicus' path to heliocentrism is not satisfactory and I also agree that we should ponder the question of the relationship of appearances to their "beyond". In this context, Copernicus' astronomy questions the role of vision in the cognitive process leading to knowledge, which has special relevance to the epistemological status of astronomy. The very essence of Copernicus' argument is to limit vision and surpass it. Copernicus transcends visual experience and establishes a new point of view, whence a new picture of the universe is revealed. But I fail to see how any connection between these insights with Alberti's artificial perspective and Cusanus' theological speculations can be established.

It is Plato who demanded, specifically in reference to study of the heavenly motions, that astronomy should go beyond the visible motions of the corporeal universe. Plato makes this demand in the *Timaeus* and he is especially clear about it in the *Republic* 7.528e–530c. There, he instructs that astronomy must be learned differently from the way in which it is learned at present. We should consider the ornaments in the heavens as the best and most exact visible things. But we should at the same time admit that these motions fall short of the true ones:

> those motions which the real speed and the real slowness in [their] true numbers and in all [their] true figures move relatively to each other and carry along whatever is in them, these things are for reason and understanding, not for sight, to discern. [Vlastos 1980, 2]

The decorations in the heavens are just models, an excellent starting point to discover the real movements of the stars, but not by any means their real motions. It is just as if someone came upon some thoroughly well-drawn and perfected diagrams of some skilled craftsman or artist, such as Daedalus. He or she would consider them beautifully crafted but would "think it laughable to scrutinize them zealously, expecting to find in them true equality or duplicity or any other relation of *symmetria*" [*Resp*. 529e–530a: Vlastos 1980, 3 lightly modified]. The True Astronomer would feel the same when looking at the motions of the stars. He would find the tracings beautiful but it would be absurd for him to seek to obtain the truth "of the relation of [the] *symmetria* of night to day, of these to months, and of the [periods of the other] stars to these and to one another from the visible appearances" [Vlastos 1980, 3 lightly modified]. According to Alexander Mourelatos, the Real Astronomer "does not dismiss questions concerning the *symmetria* of celestial periods" [1980, 39]. On the contrary, Plato demands that the True or Real Astronomer discovers the true *symmetriai*—that is, the commensurable proportions—of celestial periods, which exist beyond visible motions; the Real Astronomer "realizes that the *aletheia* concerning these *symmetriai* cannot be elicited from the observed periods of the celestial bodies" [Mourelatos 1980, 39].

2.3 *On part 3* I find S. Ragep's chapter very informative and well documented. The extent of mathematical scholarship and the technical innovations of Samarqand and the other astronomers that she depicts is impressive. I also like her more general warning about the "danger of putting forth explanations based on the heroic individual scientist in search of knowledge" [156]. The same goes for Morrison's chapter. I think that it shows convincingly the possible passages of Islamic astronomy through Jewish scholars. J. Ragep's chapter, another highlight of the book, clearly explains the concept and development of the Ṭūsī-couple and discusses channels through which it could have been brought, together with other Islamic materials, to Latin Europe and to Copernicus. Given all the evidence of transmission, I think it safe to agree with J. Ragep that independent rediscovery of all these materials, especially many times, is much less compelling.

All I should like to add regarding the third part of the book are some other possibilities for the transmission of Islamic astronomy to the Latin West. First, it seems to me that Bessarion's legacy, which includes his own books as well as the books and manuscripts of his library, deserves fuller and much more thorough research. I have already mentioned his *In calumniatorem Platonis* and its impact on Copernicus; but the books included in his library, those mentioned by Shank and cited above (by Proclus, Theon of Alexandria, and Theon of Smyrna) as well as possibly many others, should be read with renewed interest. The same goes for the manuscripts that he brought with him. Next, Callimachus was constantly traveling from Cracow to Constantinople and Italy (Venice, Rome, Padua, and Florence). Could he not have brought some materials? Finally, while in Padua, Copernicus lived in the house of Girolamo Della Torre. Della Torre was subsequently praised by Girolamo Fracastoro in his *Homocentrica* (published in 1538 in Venice) as his inspiration for the revival of homocentric astronomy. Fracastoro, as I mentioned earlier, was in Padua at about the same time as Copernicus and mentions the Ṭūsī-couple in his book. He studied literature, mathematics, astronomy, and philosophy (the latter under the guidance of Pietro Pomponazzi and Nicholas Leonicus Tomaeus), and received his doctorate *in artibus* on 2 November 1502. One of his promoters at the conferment ceremony was Gabriele Zerbi (1435–1505), a professor of theoretical medicine and a humanist who discovered several medieval scientific manuscripts and had contacts with the Ottoman Empire. This is, I believe, another possible route deserving of further study.

My closing remark on the topics of transmission: given that the astronomical models in the *Commentariolus* and *De revolutionibus* differ rather significantly, it would be good to examine whether Copernicus worked on the basis of one manuscript, one set of manuscripts, or many manuscript or sets of manuscripts. Did he obtain any new material after the *Commentariolus*, and if yes, how?

3. Conclusion

Feldhay and Ragep claim in the introduction that Copernicus' system is a result of many practices

> that included attempts to deal, mathematically, with violations of physics found in Ptolemy's models, discussions of the relation between natural philosophy

> and mathematics, and epistemological forays into the "true" cosmology and the human capacity to discover it. [8]

They likewise believe that 15th-century astronomy was

> the outcome of multiple transformations along different paths that crystallized in the work of Copernicus into some kind of coherent whole that differed enough from the preceding astronomical discourse to open the door to additional, enhanced transformations. [8]

I could not agree more. The question is, however, whether *Before Copernicus* covers the *essential* "transformations" that led to Copernicus' work and whether they are treated adequately such that they explain his work as "some kind of coherent whole". It is clear from the reservations and critical comments stated above that I do not believe that is the case. In particular, the issue of the aspects of Copernicus' intellectual and cultural context that led to his decision to put the Earth in motion is, for the reasons given above, not treated adequately.

According to the editors [8–10], three kinds of transformation lie in the background to the Copernican system:

(1) transformations in the body of knowledge;
(2) transformations related to the image and status of astronomy (the older order of the disciplines being more or less accepted in both Islamic and Christian environments for centuries); and
(3) transformations in the paths of the transmission of knowledge, in its carriers and their identities.

In what follows I will use their scheme as a point of departure and suggest some changes that, according to my research, are more appropriate to Copernicus' work.

The first category of transformation concerns the body of knowledge and is subdivided into three subcategories:

(a) the transformation of Ptolemaic two-dimensional circles into physical, three-dimensional orbs, as proposed by many scholars;
(b) new types of models, i.e.,
    (i) the transition from the epicyclic models for the second anomaly of the inferior planets to their eccentric models (ʿAlī Qushjī and Regiomontanus), and
    (ii) the Ṭūsī-couple and the construction of non-Ptolemaic models;
(c) conceptual transformations related to a moving Earth, "new ways of seeing".

I think that it can be affirmed without any reasonable doubt that Copernicus' work was a crystallization of the long period of transforming mathematical models into physical ones, and of many transformations within the astronomical models themselves, i.e., the inventions of new types. As is clear from my previous comments, I also agree that something "more" than just a technical/mathematical explanation is needed for Copernicus' affirmation of the invisible motions of the Earth. But this one should be linked not with Alberti's artificial perspective or Cusanus' speculative mathematics but with Plato and a Platonist understanding of astronomy.

This brings us to the transformations within the second category, that of the image and status of astronomy, that is, its place in the order of disciplines:

(a) the transformations of Ptolemy's two-dimensional mathematical circles into a three-dimensional physical astronomy resulted in a discussion about whether astronomy was to be understood as a mathematical science, a physical science, or both;

(b) New categories for classifying the nature of astronomy—theoretical but non-demonstrative astronomy *versus* demonstrative theoretical astronomy—thus emerged and enhanced reflection about the epistemic status of its procedures and conclusions.

> The epistemic status of astronomy was questioned once the empirical-observational origins of astronomy's "first principles" [was] addressed following the "physicalization" of astronomy by Islamic astronomers. [9]

In the long 15th century there were, of course, discussions about the mathematical *versus* physical nature of astronomy, and the "physicalization" of astronomy did indeed lead to epistemological reflections on its status and procedures. But these, I would argue, were far from decisive for Copernicus. Copernicus' heliocentric choice did depend on a "new way of seeing", on looking at the celestial appearances "with both eyes", the corporeal eye and the mind's eye. Yet this was a result of the conceptual change in the status and abilities of astronomy and not *vice versa*. This change also had little to do with the "physicalization" of Ptolemy's mathematical models. The transformation of a mathematical model of a certain planet into a physical *theorica* had nothing to do with the arrangement of the planets. The order of the planets was strictly speaking not an astronomical problem. One was able to predict the positions of heavenly bodies in geocentric and Copernicus' heliocentric cosmos equally well. The order of the planets was an astrological and natural-philosophical problem, a problem within philosophy especially for Plato and the Platonists. The Platonist understanding of the status of astronomy and its goals was radically different from that in the Aristotelian traditions.

And finally, the last category of transformations in the paths of the transmission of this knowledge:

(a) Basilios Bessarion (the new translation of Ptolemy's *Almagest* from Greek to Latin, the *Epitome of the Almagest*, his library);
(b) Jews expelled from the Iberian Peninsula who resettled in the eastern Mediterranean and traveled to Istanbul or Italy;
(c) the diffusion of the *Configuration of the World* and the tradition based on it in medieval Europe; and
(d) the circulation of knowledge within informal, intellectual-artistic circles that associated around a site of knowledge (Bessarion's library in Rome).

There were many possible paths for the transmission of knowledge from the Islamic world to Latin Europe. I have added some new possibilities. But we also should not forget other transmissions of knowledge: those, namely, that were a result of the renewed humanist interest in Plato and Platonism as reflected in the Latin translation and diffusion of Plato's *Opera omnia* as well as the works of different Platonist and commentators on Plato (including doxographers), in readings of his work in the original Greek, and so on. One can find much of this already in Bessarion's library.

Let me conclude on a positive note. Despite my reservations and critical remarks, I certainly would have benefited from having *Before Copernicus* at my disposal before writing my own book on Copernicus.

BIBLIOGRAPHY

Anton, J. 1980. ed. *Science and the Sciences in Plato*. New York.

Bessarion. 1469. *In calumniatorem Platonis libri IV*. Rome. See Mohler 1927.

Copernicus. 1543. *De revolutionibus orbium coelestium*. Nuremberg. See Rosen 1992.

Feldhay, R. and F. J. Ragep. 2017. edd. *Before Copernicus: The Cultures and Contexts of Scientific Learning in the Fifteenth Century*. Montreal, PQ/Kingston, ON.

Fracastoro, G. 1538. *Homocentrica*. Venice.

Goldstein, B. 2002. "Copernicus and the Origin of His Heliocentric System". *Journal for the History of Astronomy* 33: 219–235.

Mohler, L. 1927. ed. *Kardinal Bessarion als Theologe, Humanist und Staatsmann*. Paderborn.

Mourelatos, A. P. D. 1980. "Plato's 'Real Astronomy': *Republic* VII. 527d–531d". See Anton 1980, 33–73.

Rosen, E. 1992. *Copernicus:* On the Revolutions. Baltimore, MD/ London, UK.

Swerdlow, N. 1973. "The Derivation and First Draft of Copernicus' Planetary Theory: A Translation of the *Commentariolus* with Commentary". *Proceedings of the American Philosophical Society* 67: 423–512.

Vesel, M. 2014. *Copernicus: Platonist Astronomer-Philosopher: Cosmic Order, the Movement of the Earth, and the Scientific Revolution*. Frankfurt am Main.

Vlastos, G. 1980. "The Role of Observation in Plato's Conception of Astronomy". See Anton 1980, 1–31.

Westman, R. 2013. *The Copernican Question: Prognostication, Skepticism, and Celestial Order*. Berkeley, CA/Los Angeles, CA/ London, UK.

Wilbur R. Knorr on Thābit ibn Qurra

A Case-Study in the Historiography of Premodern Science

by

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Abstract

There was a widespread belief among historians of science of my generation that high competence with regard to content and languages alone can guarantee better, more reliable results than can good philology combined with high competence in history or the other human sciences. In my case-study of Wilbur R. Knorr's analysis of several medieval Arabic and Latin texts on the balance, or steelyard, I highlight a variety of factors that compromised time and again his understanding and interpretation of his chosen texts. I conclude that a greater openness to more complex historiographical assumptions and more sophisticated methodological approaches as well as a greater willingness to contextualize documents in numerous dimensions before coming to conclusions about their specific meaning is crucial if we are to correct and improve upon work such as Knorr's analysis of the *Kitab al-qarastun*, ascribed to Thābit ibn Qurra, and the *Liber de canonio*. The way forward is to enhance and temper philological analysis with solid analysis of scientific content within its relevant contexts.

About the Author

Sonja Brentjes is a historian of science specializing in Islamicate societies between the eighth and the 17th centuries. She works on the history of the Arabic translations of Euclid's *Elements* and the place of the mathematical sciences at the madrasas. She also studies forms of narration in the sciences in Arabic historical sources, the cross-cultural exchange of knowledge, maps, forms and languages of patronage, dictionaries, and European travel accounts about early modern Islamicate societies. She is currently a visiting scholar at the Max Planck Institute for the History of Science in Berlin.

In what follows, I present arguments for the need to go beyond both the positivist and the postmodernist approaches to the history of science in Islamicate societies. While positivist research practice often was and is focused exclusively on scientific content, postmodernist practice often avoids the analysis of this content and focuses instead on a narrow language of contexts. I think that good historical practice needs to aim at a solid analysis of scientific content within its relevant contexts. Analysis of content without paying attention to the conditions of and motivations for its creation can at best be the very first step in our labor. Context analysis without interest in the nature of scholarly practices and their results loses its basis and transmutes all too often into specious respect for a different culture. In my view, the professional goal of our activities is not the subjugation of historical objects to the power of our own worldviews and academic profiles. Our academic self-representation and legitimation, if taken seriously and honestly, should aim instead at our being competent seekers for reliable and trustworthy interpretations of the material that we study. Truth about the past in this sense, however, can only be established if we consider the objects of our research as self-contained, valuable products constructed by independent human beings of earlier times who, though they differ from us in their knowledge and values, remain worthy of our respect, appreciation, and our honest effort to discover them and their worlds. Even as the hybrid cultural creations that many of them are, these works are always more than simple containers of yet another past which we happen to esteem more or less because of the stories that we tell about our own history.

I will make my arguments on the basis of a book published 31 years ago by a senior and serious scholar of good repute. I chose this example because it is a very elaborate technical product, which leaves no doubt that its author, Wilbur R. Knorr, spent much time and labor to work out his positions. Yet Knorr's *Ancient Sources of the Medieval Tradition of Mechanics: Greek, Arabic and Latin Studies of the Balance* [1982] is characterized by the following three flaws which are found all too often also in current publications:

(1) the undue impact of prior beliefs and prejudices on questions, arguments, and conclusions;

(2) the deleterious role of methodological limitations in the choice of an interpretive framework; and
(3) lack of expertise.

I am also moved to write about Knorr's study because it forced me to confront some of the problems resulting from our standard approach to the analysis of Arabic scientific texts written in the ninth century or of Arabic translations of Greek texts executed in this period, that is, to face the problems that arise when we treat these works as isolated texts without any contextualization.

At the same time, an analysis of this book serves to highlight that many of our biases are deeply anchored in our education and training, in the political alignments and ideological commitments of our teachers and, thus, in our own academic values, convictions, and beliefs. Such deep-seated biases are often very difficult to recognize, even more difficult to acknowledge as severe shortcomings, and extremely difficult to overcome because of the demands that changes in working practices have necessitated. As Dagmar Schäfer remarked in a conversation about issues of contextualization:

> It is already a difficult endeavor to read, translate, and understand a complex medieval text in any language. It is much more difficult to analyze its textual contexts, if these contexts are unknown and the relevant texts unpublished. But it is nearly impossible to investigate the entire non-textual contexts in which the medieval text, which is the primary goal of study, was created.

In the case of Arabic, Persian, and Ottoman Turkish manuscripts, the challenges do not merely include the search for manuscript copies in sometimes almost inaccessible libraries and then overcoming problems with handwriting, dating, and the interpretation of content. It also means breaking away from the traditionally sanctioned habit of analyzing a historical sequence of texts, starting either in antiquity or in ninth-century Baghdad, which considers them only in relation to one another, by turning first and foremost to a study of contemporaneous authors, their works, and their networks. This has been undertaken so far only in fairly limited ways, my own work included, of course.

1. Knorr's working practices

In order to understand Knorr's analysis of a number of Arabic and Latin medieval texts on the steelyard, one should remember the goals of textual studies 30 years ago, the values attached to ancient and medieval sciences and mathematics, and the methods taught and valued in that period. Editions of Greek, Latin, or Arabic scientific texts aimed to (re)produce the genuine text of an author on the basis of critical comparisons among the extant copies and their errors. Interpretations of a text's content proposed to establish its scientific or mathematical results, preferably, but not exclusively, in a more recent language than that of the text itself. Chronology and authorship were additional important issues that were pursued, the first prevalently within the oeuvre of the individual author, the second mostly within the specific text at hand or in comparison with other texts on the same topic. Once these tasks were completed, the historian's duty ended. Of course, there were always colleagues who invested time and effort in the study of larger disciplinary or institutional themes. But such questions were certainly considered to go far beyond the investigation of a single text.

This methodological stance was not new in the 1980s; it has been well established since the 19th century. What emerged in the 1970s and 1980s was the conscious and explicit opposition between two research positions and their respective goals: the study of content alone by the so-called internalists and the study of the external conditions of the sciences, including mathematics, by the so-called externalists. Most historians of mathematics and almost all historians of ancient and medieval mathematics and the exact sciences subscribed strongly to the internalist position and rejected or even disparaged the pursuit of externalist inquiries. As my discussion of Knorr's work will show, the strong belief in the exceptionality of the sciences in comparison to other domains of human society did not merely prevent the study of the mechanisms that created interest in, and support for, scientific problems in any given society. It did not attend to the study of textual content or of the subsidiary questions of chronology and authorship beyond the simplest understanding of how a text was produced, read, and reproduced. How producers and readers of texts communicated through such texts within their immediate environments and how they created meaning were issues only accepted much later in the history of science.

Hence, in contrast to today's much broader array of methodologies available for the study of texts, the conditions in which Knorr worked in the 1980s were more restricted. Even if he had wished to approach the issue of authorship, which is central to his study of the texts on steelyards, in a different manner, he could have done so only by contravening practices current at the time for the study of scientific and mathematical texts by classicists and medievalists. While Knorr had shown in earlier works on ancient and medieval geometry, Archimedes, and Euclid that he was willing to reshuffle beliefs held earlier, his iconoclastic tendencies did not include issues of methodology. In this respect, he was a representative of the dominant approach of his time and day—he was clearly an internalist. This basic methodological stance underlies his entire analysis and all of his arguments in *Ancient Sources of the Medieval Tradition of Mechanics*.

1.1 *A caveat* Those who read my critique of Knorr's results and my different interpretation of the same texts must take into account the enormous changes in working practice, values, and goals that have taken place between 1982 and 2020 in order to avoid an anachronistic reading of Knorr's book. My main critique does not in fact center on the conceptual and methodological differences between then and today, although my new interpretative results are clearly the product of these changes. Instead, it focuses on those misinterpretations or even clear missteps that belong to the internalist framework of textual studies. It is in regards to these points that I will argue that Knorr's interpretive and analytical shortcomings were caused by unquestioned assumptions and beliefs about authorship, the development of ancient and medieval mathematical texts, and the relative qualities of ancient and medieval scholars of the mathematical sciences as well as of modern historians of science and mathematics from the US, Europe and the Middle East. As my analysis indicates, Knorr too often broke the "rules" of an internalist textual study because of his larger desires, prejudices, and assumptions. Additional interpretative difficulties were the direct result of his limited control of the Arabic language and his failure to subject his decisions about the merits of different interpretive options to critical examination of the criteria by which these decisions were made.

2. Issues of authorship

As I have said, the central problem of Knorr's study comes to the fore in his determination of the authorship of several Arabic and Latin texts (complete as well as fragmentary) on the steelyard translated or written in the ninth, possibly 10th, and 13th centuries. His analysis yielded three conclusions concerning the texts ascribed to Thābit ibn Qurra (d. 901):

(1) the Arabic *Kitāb al-qarasṭūn* (Book of the Steelyard) was not written by this Sabian scholar;
(2) the Latin *Liber karastonis* (Book of the Steelyard) is rightfully seen as his work but of a different textual tradition than the *Kitāb al-qarasṭūn*; and
(3) an addition (*ziyāda*) to MS Beirut, St Joseph University, 223, one of the two manuscripts available to Knorr, was not of Arabic origin.

In regard to the *Liber de canonio* (Book on the Beam), an anonymous Latin text on the steelyard, Knorr confirmed earlier evaluations by Duhem [1905], Moody and Clagett [1952] that this text was a translation of an ancient

Greek ancestor. Knorr's final, main conclusion states that all the Arabic texts which he analyzed and the *Liber de canonio* were but fragments of one major ancient Greek text on balances and that its author was Archimedes in his youth.

In order to settle the questions raised by Knorr for the Arabic and Latin texts, I studied carefully each and every claim and its demonstration in order to understand their soundness and to decide what further work was needed. The only point that I excluded from my analysis concerns the issue of the young Archimedes: I am not an expert in Archimedean studies, let alone of the young Archimedes, whose writings are not extant. The result of my analysis is that only one of Knorr's conclusions is valid, namely, the one about Thābit's authorship of the *Liber karastonis*. All other conclusions are insufficiently backed by primary source evidence or rest on faulty or one-sided arguments.

Problems that played an increasing role in my own investigations, but that understandably were not addressed in a comparable manner by Knorr, concern our beliefs about authorship—beliefs that have changed substantially in the last 30 years. In the 1980s, we believed that an ancient or medieval text ascribed to a concrete person could be either the work of this person, or the work of someone else ascribed to this person by mistake, or a forgery. The idea of multiple authorship, for instance, where many people contribute to the production, transformation, and dispersion of a text, while only one, if any, is named as its author, had not yet been put forward. In its more complex form, this concept of multiple authorship allows for a group production of a text at a specific time and location as well as a series of subsequent contributors, previously often thought of as "mere" commentators or interpolators, to a living text. Such an understanding of the concept of multiple authorship turns out to be fruitful for reformulating Knorr's claim that Thābit had not composed the *Kitāb al-qarasṭūn* as a question of the sense in which Thābit had contributed to the content and form of the extant text and, hence, the sense in which he could be credited or not with authorship. This type of question, as I will show below, is not an effort to hide the fact that Thābit was not the originator of the *Kitāb al-qarasṭūn* in its entirety. Indeed, it is clear that Thābit was not the sole author of this text. But against any such traditional sense of authorship, we must note that this text would not have come into being without Thābit. It is, in fact, a new product and, hence, his role in producing it deserves proper recognition as such.

2.1 *Knorr on the* Kitāb al-qarasṭūn Knorr's rejection of Thābit ibn Qurra's authorship of the Arabic *Kitāb al-qarasṭūn* is directed against

Khalil Jaouiche, the modern Arabic editor and translator of this text [1976]. Jaouiche had accepted Thābit's authorship on the basis of the three manuscripts known then, i.e., MSS London, BL, India Office Library, 461; Beirut, St Joseph University, 223; Cracow, Jagielonska University Library, Mq 559. He had had access, however, only to the first of them, which prevented him from discussing the appendix found only in the Beirut copy. In regard to the *Liber de canonio*, Jaouiche challenged its interpretation as a Latin translation of an ancient Greek text. He held that this text was written after the *Kitāb al-qarasṭūn* and was willing to grant a Byzantine origin. Jaouiche also rejected the false translation of the preface of the *Liber karastonis* provided by Duhem [1905] and accepted by Moody and Clagett [1952], and presented his own and better reading of this part of the Latin translation, which he achieved in cooperation with a medievalist. Duhem's false reading of the preface stipulated another ancient text, a so-called *(Liber) causae karastonis*, as the original text behind the *Liber karastonis*. Knorr accepted Jaouiche's rejection of this so-called *(Liber) causae karastonis* but upheld Duhem's overall reading of the preface as correct. Knorr ignored Jaouiche's proposal of a Byzantine origin of the *Liber de canonio* and focused entirely on his question of whether the author of this text could have been a Latin scholar of the 13th century.

Knorr's differentiated replies to the interpretive positions of his predecessors suggest—in addition to the existence of conceptual issues regarding authorship, originality, commentators, and the like—the role of beliefs about the relative merits of scholarly works written by ancient Greek, Byzantine, and Arabic-writing scholars. I will return to this point [see p. 125, below]. The fact that Knorr did not recognize Duhem's clear and, in some instances, even simple mistakes in translation in the case of the *Liber karastonis* [1905] and his striking disregard of Jaouiche's arguments about the *Liber de canonio* may reflect some puzzling biases. His rhetorical treatment of both groups of colleagues is slightly different. Duhem, Moody, and Clagett are mostly treated with respect, with only a few strong expressions of criticism such as "Duhem's view being irrelevant". Jaouiche, in contrast, is more often described in strong or emotive terms, the latter carrying a negative subtext in the internalist framework: for example, "but Jaouiche's denying the existence of any such Greek text", "although he is emphatic", "Jaouiche's desire", "Jaouiche wishes to assign". A few times, Knorr also misrepresents Jaouiche's claims or ignores the explanations given in his footnotes, while this is not the case as far as Duhem, Moody, and Clagett are concerned. Although I know from my own experience that such missteps in reading occur more easily than one might wish, such small differences in treatment may indeed be more than simple accidents.

Knorr's method for clarifying the problems of authorship was to study the philological and scientific features of texts taken by themselves. No other approach was considered necessary at the time. In the case of the *Kitāb al-qarasṭūn*, his focus was on matters such as proofs, arguments, and methods. As I have already indicated, Knorr was trained as a classicist and historian of mathematics but not as an Arabist. His limited expertise in Arabic along with his prior beliefs about the mathematical capabilities of Thābit ibn Qurra as opposed to Archimedes, Greek writers of late antiquity, and post-ninth-century writers in Arabic had a clear, negative impact on his interpretation. I will provide a few examples to confirm this in the following two sections.

For now, I will note that, surprisingly, Knorr did not undertake a study of philological features of this text, either to establish arguments against Thābit's presumed authorship or to determine features that might have spoken in favor of its character as a translation from Greek. Somehow he was satisfied to rest his case for authorship and character on the analysis of a limited range of mathematical statements, a few proofs, and a few perceived mistakes, which, like it turned out, were mostly his own. Knorr gives no reason for his limited exploration of the text. He was clearly inconsistent in his working practice in this book, since his main arguments concerning the *Liber de canonio* are taken from a philological analysis, as I will show below. Beyond this internal inconsistency of methods and conclusions, this lack of any justification for the differences in his analysis of the various texts at issue contravenes the standards for research of his own time.

Granted, a traditional philological approach to the issue of authorship, which Knorr knew and practiced well in his other papers and books, would have provided him initially with additional arguments for a Greek ancestry of the *Kitāb al-qarasṭūn* because it uncovers Graecisms in the Arabic text. But a proper, comprehensive philological analysis of this work would have alerted Knorr that his belief in a single text as a predecessor of the *Kitāb al-qarasṭūn* was in all likelihood erroneous since these Graecisms do not occur in the same manner in all parts of the text but differ in kind and frequency.

2.2 *Knorr on the* Liber de canonio In contrast to the analysis of the *Kitāb al-qarasṭūn*, the confirmation of Duhem's, Moody's, and Clagett's identification of the ancestor text of the *Liber de canonio* as an ancient Greek text rests primarily on the analysis of its philological properties. When summarizing the theorems of the *Liber de canonio* and the addition (*ziyāda*) in the Beirut manuscript, Knorr discussed certain of their aspects but not with the same comprehensiveness as in the case of the *Kitāb al-qarasṭūn*. For instance, he paid only very little attention to the text's axiomatic-deductive structure; its references to definitions, axioms, or proofs in its proofs; and the lack of physical arguments, which are, however, an important key for understanding, for example, the relationship of these two texts to the one presented in the *Kitāb al-qarasṭūn* or for understanding the relationship between the first and the second part of the *Liber de canonio*.

Knorr's philological analysis identifies a number of Graecisms, a single Arabism, and a number of philological features that could be identified as either of the two and so are undecidable. Given this, he finds it more plausible to consider the undecidable cases as favoring an ancient Greek ancestor. This result is surprising since a brief glance at the Latin text uncovers without any difficulty many more Arabisms than Knorr recognized. It shows too that Graecisms, Arabisms, and undecidable forms are unevenly distributed throughout the complete text. Graecisms are concentrated in the first half, while Arabisms dominate the second half. Undecidable forms can be found in both parts. Although Knorr allowed at the beginning of his discussion for the possibility of some other identification of the source text—for instance, its translation in Sicily, or even an Arabic ancestor (because of the existence of similar theorems at the end of the Beirut manuscript of the *Kitāb al-qarasṭūn*)—he does not spend time weighing such alternatives seriously. The purely rhetorical character of these alternative interpretations is of a piece with Knorr's failure to see the contradiction between another of his claims, namely, the largely correct, if slightly too general, assertion that Latin translations of Greek texts (and texts composed in Latin) prefer singular verbal forms over plural forms in contrast to Arabic translations of Greek texts and texts newly composed in Arabic—and the numerous plural verbal forms found in the second half of the *Liber de canonio*.

It is very difficult to believe that Knorr did indeed miss all 28 instances of a first person plural in six printed pages of Latin text. However, there is no indication in his text to suggest that he intentionally misconstrued the argument. Hence, I am inclined to think that he was in fact blinded by his belief in the ancient Greek origin of the *Liber de canonio* and simply did not see the many plural forms in its second part. There are other, indisputable Arabisms in the *Liber de canonio* and here it is much easier to understand why he missed them. Recognizing them presupposes a much broader familiarity with Arabic translations of Greek mathematical texts than Knorr had. Such intimate familiarity with unpublished Arabic translations of Greek mathematical texts is requisite if one is to render properly the pair "greater"/"smaller" in Greek, Arabic, Greek-to-Latin, and Arabic-to-Latin texts. Greek texts and Greek-to-Latin as well as certain Arabic translation texts only use one pair corresponding to "greater"/"smaller": respectively, « μείζων »/« ἐλάσσων », "maior"/"minor", and « aʿẓam »/« aṣghar ». In other Arabic translations and Arabic texts derived from them, there appear three pairs corresponding to different types of objects. The three pairs are « aṭwal »/« aqṣar » (longer/shorter) for lines, « akbar » or « akthar »/« aqall » (bigger or more/smaller or less) for areas, solids, or numbers; and « aʿẓam »/« aṣghar » (greater/smaller) for numbers and angles or similar magnitudes. In Arabo-Latin translations, these three pairs are represented as a rule by the following two pairs: "longior"/"brevior" and "maior"/"minor".

Since the *Liber de canonio* mixes "longior" with "maior" and "brevior" with "minor" in its second part, it is impossible that the direct predecessor of this part was an ancient Greek text. It is also unlikely that it was a pure Arabic translation of such an ancient Greek text. In my experience, the mixing of these terms occurs predominantly in commentaries, editions, or newly composed texts. This philological phenomenon goes beyond idiosyncratic usage by an individual. It reflects the coming into being of different sets of technical vocabulary during the process of translation, their social acceptance by the scholarly community, and their merger into one technical language with several options to express one and the same point. Thus, the second part of the *Liber de canonio* suggests strongly that its basis was an Arabic text, which may have derived from an earlier Arabic translation of an ancient Greek text or a newly composed Arabic text in which such usage was accepted. Whether a Byzantine intermediary was situated between this Arabic basic text and the Latin final product cannot be decided on the basis of this and other Arabisms in the second part of the *Liber de canonio*.

But what of the first part? The overwhelming presence of Graecisms in it might seem to contradict this. But after a closer look at Latin translations of Arabic and Greek texts in the 12th and 13th centuries, four possible explanatory hypotheses compete with each other:

(1) there was indeed a Byzantine intermediary between the Arabic ancestor of the *Liber de canonio*, whose producer paid significantly more attention to Greek style and grammar in the first part than in the second;

(2) the Arabic ancestor was translated in Sicily by a trilingual translator who paid significantly more attention to Greek style and grammar in the first part than in the second;

(3) the first part of the *Liber de canonio* represents a Graecisized Arabic-to-Latin translation, while the second part was left in the original form of translation;
(4) the first and second parts derive from a single source translated by two translators or from two different sources translated by one or two translators.

The second part of hypothesis 4 may easily be excluded by virtue of the consistency in content, procedures, types of arguments, and sources used or referred to in both textual parts. This consistency militates against the existence of two different texts that were intentionally or accidentally fused to form one new text by one or two translators. The remarkable philological differences between the two parts appear then to be results of a difference in the intention of either one or two authors translating a single text. The overall philological properties of this single text favor the hypothesis of a single translator at work.

This raises the question: Which of the two parts was philologically altered? When we consider the two language components in the Latin text and the historically possible cultural environments (Sicily, Iberian Peninsula, southern France, Crusader states) of the translation, it seems more plausible to assume an editorial modification towards a more "Greekish" appearance than one which would increase an "Arabicizing" outlook. The distribution of the two language components also supports the hypothesis that the modification of the translated text consists in the introduction of the Greek terms and forms. Reworking a text from its beginning instead of starting with such changes in its middle sounds not merely practically more plausible, it also makes more sense with regard to the effect such a change may have meant to achieve.

A further argument for abandoning hypothesis 4 altogether and privileging hypothesis 3 instead comes from properties of other Latin texts translated from Arabic. At least two Arabic-to-Latin texts, one a translation (Theodosius, *Sphaerica*), the other a compilation (Euclid's *Elements*, labelled Adelard III by Clagett and ascribed to Robert of Ketton by Busard and Folkerts [1992]), are well-known examples of the use of Graecisms in Arabic-to-Latin translations. Translations made from Arabic at Sicily are few and not known for this, so far as I know, and Byzantine intermediaries of Arabic-to-Latin texts are not known at all. Hence, until more material is found, the most likely interpretation of the fascinating philological contrast and its uneven distribution through the text of the *Liber de canonio* is hypothesis 3 [Brentjes and Renn 2016].

2.3 *Knorr and the question of context* As I have emphasized, Knorr was an internalist. Hence, contextualization, even in the limited form of textual contextualization, was not something that he would have pursued as a means necessary for putting the analysis of authorship on solid footing. The fact that social, cultural, epistemic, and other contexts were not considered to the degree that they are today led Knorr, as it had other, previous scholars who studied the *Kitāb al-qarasṭūn*, to ignore the explicit statement in one of the two manuscripts that he worked with according to which the extant text of the *Kitāb al-qarasṭūn* had been dictated by Thābit b. Qurra. Such a statement generally indicates a teaching text. The *Kitāb al-qarasṭūn*, in the form in which we have it, is thus not the result of Thābit's research or of his editing one or more Arabic translations of one or more Greek short texts on the balance. It is rather a text that Thābit prepared for classroom work. This is important for several reasons. First, given that historians of education and codicology in Islamicate societies claim that such dictation, teaching certificates, or auditing certificates and the like can be found only in literary texts from the late 10th century onwards and in texts on religious matters after the early 11th century, the *Kitāb al-qarasṭūn* would appear to be one of the earliest, if not the earliest, extant document for formal teaching activities [Gacek 2005, 55; Witkam 2012, 157–160]. Given the fact that it is the only Arabic or Persian text on mechanics known so far that carries this kind of information and that such statements indeed become more prominent only in later centuries, remaining always much less a feature of scientific literature than of religious and literary texts, there is no reason to suspect falsehood in these references to teaching. I, at least, cannot think of any good reason for explaining such falsehood. Hence, in absence of arguments and evidence to the contrary, I consider as a true report the claim that Thābit dictated the *Kitāb al-qarasṭūn*.

We know next to nothing about the teaching of the mathematical sciences in the ninth century. Thus, the statement that Thābit had dictated the *Kitāb al-qarasṭūn* is most welcome. Not only does it confirm that the mathematical sciences were taught in the ninth century outside the court and beyond its patronage, it also suggests through its similarity to later such statements in texts taught at the *madrasa* or in mosques that the methods of teaching that we are aware of may already have been in place during the ninth century. That is, Thābit's statement confirms that it was the practice in his time to write out a complete text which was then to be read out loud to students who were to copy it meticulously and who then received confirmation from their teacher if their note-taking was correct. Indeed, a fourth manuscript, which I found in the late 1990s in the Biblioteca Laurenziana at Florence, Or. 118, contains a different form of such teaching statements, e.g., an audition certificate. An audition certificate signifies exactly what I just summarized, i.e., that the teacher read his text to students who listened to him and wrote down (correctly) what he had said. Since I do not see any good reason for assuming that either form of the two teaching statements is a falsification, we may be fairly certain that the *Kitāb al-qarasṭūn* is the result of Thābit's holding classes on the steelyard. This means that the specific character of the *Kitāb al-qarasṭūn* as a teaching text will have impacted its form and structure. We should, therefore, expect and look for explanations, a less rigid axiomatic structure, different types of demonstrations, and other didactic devices. This means that parts of the text considered by Knorr as interpolations may now be understood, for example, as remainders of oral explanations by Thābit given to his students when reading his prepared text to them [1982, 8–9, 63–72, 78–87]. Other oral features appear to exist when the text is studied from such a perspective.

A second aspect of the identification of the *Kitāb al-qarasṭūn* as a teaching text is that we may now also understand other textual features as a reflection of the manner in which Thābit presented the text in class and not as interpolations in the classical sense. One instance is the appearance of postulate-like statements after the first theorem and not at the text's beginning. Knorr proposed to consider these two statements plus a subsequent theorem as an interpolation. One explanation for this decision is his disagreement with Jaouiche's choice to understand their presence as a misplacement through copying, which induced Jaouiche to emend the manuscript text [see Knorr 1982, 63n15; Jaouiche 1976, 146–147, 171]. Another reason seems to have been the absence of these two statements plus the subsequent theorem from the *Liber karastonis*, where this difference between the two texts is obviously understood not as a decision made by Thābit but as an indicator for the "better" or "purer" quality of the Arabic text that forms the basis of the *Liber karastonis*. A third reason will have been Knorr's lack of access to the Berlin and Florence manuscripts of the *Kitāb al-qarasṭūn*, since these two texts confirm the presence of the two postulate-like statements at exactly the same place where they are found in the London manuscript. Had he known this, Knorr might have chosen a more cautious interpretation of the text's provenance and its circumstances. Instead, Knorr buttressed his interpretation with a rash as well as inconsistent identification of the two postulate-like statements with two postulates in the pseudo-Euclidean *Kitāb fi l-mīzān* (Book on the Balance) [1982, 79, 81]. I say rash because his judgment is exclusively based on the similarity of content and does not take into account the substantial differences in their formulation as well as expression. I say inconsistent because he modifies his first evaluation of the parallelism of the two texts by the later qualification that none of them depends on the other but that “they must be viewed as independent translations from closely related, if not identical, sources” [1982, 81].

In contrast to Knorr, I have the impression that the apparently misplaced postulate-like statements were indeed presented in class by Thābit after the first theorem. In addition to the didactic character of the work, this idea is based on the differences in content, concepts, and terminology that demarcate boundaries between different parts of the text. The internal philological analysis of the text regarding its possible relationship to Greek predecessors reveals these borderlines in regard to specific Graecisms. It is thus most likely that Thābit presented successively different bits and pieces from Greek texts on the steelyard to his students. The partially incomplete nature of these pieces suggests that the idea of discussing them in this way with his students may have been a consequence of the fact that Thābit had come across several Arabic translations of Greek fragments, which he wished to interpret. This fits well the preface of the *Liber karastonis*, where he reports in a continued discussion with an unnamed friend on his engagement with faulty and partially incomprehensible translations or copies of collections of theorems on the balance and his efforts to solve the problems of transmission. Moreover, it is not at all true that all Greek texts on theoretical geometry start with their axioms, postulates, or definitions placed exclusively at their beginning. Archimedes, for one, often introduces such new statements after he has already proven theorems. Euclid did the same in book 10 of the *Elements*. Knorr was well aware of this textual practice of ancient Greek scholars. As I see it, his insistence on the interpolated character of these two postulate-like statements in the *Kitāb al-qarasṭūn* is symptomatic of the presumptions that he brought to his conclusions concerning Thābit’s authorship of the *Kitāb al-qarasṭūn*.

2.4 *Advantages of contextualization* As is well known, contextualization may occur in different ways and on different levels. I will begin my discussion with textual contextualization. Contextualization of this sort is the lowest possible level and should be acceptable to most students of past scientific or mathematical texts. A textual contextualization provides clues for understanding parts of a scholar’s working practice and intellectual environment in addition to those which can be derived from the analysis of the particular text being studied.

A systematic check of all of Thābit's published works confirms that he was not a solitary writer. He exchanged letters on scholarly themes with colleagues and friends. He wrote short introductory texts for courtiers and a more general public, a feature that whoever attached the title to his treatise on Aristotelian natural philosophy and metaphysics made explicit. He composed at least one other sufficiently difficult text, this time on astronomy, as the result of repeated discussions with friends and students.[1] Thus, identifying the *Kitāb al-qarasṭūn* as one part of a complex project on the study of the balance, which was headed by Thābit b. Qurra and included friends, students, and apparently his patrons, the Banū Mūsā (ninth century), and not as an isolated single text created in antiquity and translated into Arabic, is very plausible. Other remainders of this project are the *Liber karastonis*, the extract of Thābit's text on the properties and causes of the equal-armed balance produced by ʿAbd al-Raḥmān al-Khazīnī (d. after 1130) in Merv (then northeastern Iran, today southern Turkmenistan), and perhaps, but not likely, a further text attributed to Thābit also called *Kitāb al-qarasṭūn*. Thābit's efforts to understand various, partly contradictory and faulty Greek fragments on the balance and to transform them into a consistent explanation of the conditions of equilibrium of an unequal-armed balance formed the center of this project, as the content of these texts along with the preface and prologue of the *Liber karastonis* and several remarks in the treatise on the properties and causes of the equal-armed balance shows.

A higher level of contextualization concerns issues beyond the texts of one author. Such contextualization can produce further insights into the sociocultural nature of authorship and the intellectual interests shared among different groups of scholars in a certain period and location. This at least is the case for the discussions on equal- and unequal-armed balances and the issues of weights that took place in Baghdad in the ninth century. It also applies to scholars in the 10th and 11th centuries and helps to explain the presence of such texts and intellectual interests in western and northeastern Iran in the early 12th century. However, only a few of the contextual elements of Thābit's and other texts on balances and weights that are at the heart of these two claims are new discoveries. In and of themselves, they were known to individual researchers since the 19th century. But they were never brought together nor questioned for their relevance regarding

[1] Sabit ibn Korra 1984, 12, 20–21, 24, 243–247, 278–284, 321–328, 353–355, 365–367, 380–381; Lorch 2008, 43, 47, 49, 51.

the issue of Thābit's authorship. To these long-known elements belong facts noted on specific copies of manuscripts:

(1) one of the manuscripts of the (pseudo-Euclidean?) *Kitāb fi l-mīzān* once belonged to the Banū Mūsā;

(2) in the 10th century, this manuscript came into the possession of the astronomer/astrologer of the Buyid court, ʿAbd al-Raḥmān al-Ṣūfī (d. 986);

(3) it was finally copied by another scholar of the mathematical sciences in the 10th century, Aḥmad b. Muḥammad b. ʿAbd al-Jalīl al-Sijzī;

(4) the Banū Mūsā owned the only extant copy of the Arabic translation of book 8 of Pappus' *Collectio*;

(5) Thābit edited the anonymous Arabic translation of the other (pseudo-Euclidean?) text on issues of weight, this time specific weight, with the title *Kitāb fi l-thiqal wa'l-khiffa* (Book on Heaviness and Lightness).[2]

Other long-known facts concern translations, newly written treatises, and patronage of Greek texts related to balances and weights. Among them are:

(6) Qusṭā b. Lūqā's (d. *ca* 912/3) translation of Hero's *Mechanics* around 860;

(7) Qusṭā's text on weights used in medicine for an unnamed patron in Baghdad with medical interests (identified in some manuscripts as one of the Banū Munajjim); and

(8) Sanad b. ʿAlī's (ninth century) treatise on the unequal-armed balance.

Finally, since Josef van Ess' magisterial oeuvre, *Theologie und Gesellschaft im 2. und 3. Jahrhundert Hidschra* [1991–1997], we know not only that there was a vivid debate on the meaning of the balance in the Qur'an among religious scholars of the eighth and ninth centuries but that in particular Muʿtazili authors were also interested in the question of why an unequal-armed balance needed only a small counterweight to balance a much heavier body [van Ess 1991–1997, 3.64].

What do such long-known contextual data signify for the issue of authorship of the extant *Kitāb al-qarasṭūn*? They buttress the claim that Thābit was embedded in an environment interested in how equal-armed and unequal-armed balances function and what ancient Greek authors had to say on this

[2] Woepcke 1851, 225, 232; MS Paris, BnF, Arabe 2457; Jackson 1970, 113, A78; Ahlwardt 1893, 5.353 no. 6.

and other mechanical questions, as well as in who collected manuscripts of translations of such works and who received commissions for writing summaries of these issues. In this sense, they imply that the attribution of the *Kitāb al-qarasṭūn* to Thābit b. Qurra is not at all implausible. Furthermore, the data show that the interest in this particular text and its topics continued into the 10th century. The preservation and acquisition of texts from the libraries of leading scholars of the ninth century was an important part of the scholarly practices in the mathematical sciences during the 10th. Finally, these larger contextual data draw attention to the explicit sociocultural nature of a theoretical text and its genesis.

Finally, I wish to stress that contextualization is indeed beneficial for solving even such classical questions as that of authorship. In order to develop my own position on whether Thābit might be the author of the *Kitāb al-qarasṭūn*, not only did I compare this work with the *Liber karastonis* and the treatise on the properties and causes of an equal-armed balance, I also compared it with all published mathematical and astronomical works attributed to Thābit as well as with Qusṭā's translation of Hero's *Mechanics*, the anonymous translation of book 8 of the *Collectio* by Pappus, the extant Arabic fragment of the *Problemata mechanica*, Archimedes' works and their Latin translations by William of Moerbecke, and the Greco-Latin translation of the *Elements*. The goal of this extensive comparative analysis was to determine philological properties of the various texts in order to find at least preliminary answers to three questions:

(1) Did Thābit b. Qurra write all his works in a consistent style with a stable vocabulary?
(2) Do other texts contain the philological and conceptual idiosyncrasies of the *Kitāb al-qarasṭūn* and do they form clusters in regard to content, time, or origin (author, translator)?
(3) Which parts of the language of the *Kitāb al-qarasṭūn* are shared by other translators or editors of mechanical and related mathematical texts, and do such relations reveal the existence of parallel or even competing technical languages that can be linked with some caution to identifiable groups of translators or authors during the course of the ninth century?

Knorr's denial that Thābit was the author of the *Kitāb al-qarasṭūn* was a denial of authorship in the classical sense: he took for granted that the text had but a single author and maintained that it was not Thābit. But once one allows that a text can have multiple authors where one is singled out above the others as explained above, it is clear that Thābit was indeed the text's author.

Thus, the medieval attribution of the *Kitāb al-qarasṭūn* to Thābit b. Qurra is to be accepted not merely as an expression of beliefs held then but also as a result of my systematic analysis of the text's features. Thābit, that is, compiled the *Kitāb al-qarasṭūn* from several fragments translated from Greek into Arabic, fragments which represent different Greek scholarly traditions—Aristotelian, Archimedean, Heronian, and possibly mixtures of those with other school or commentary literature. Philological, symbolic, conceptual, methodical, and demonstrative particularities leave no doubt that it was not a single ancient Greek text translated into Arabic (perhaps by the anonymous colleague to whom Thābit refers in the preface of the *Liber karastonis*) as one of Knorr's many contradictory hypotheses would have it [1982, 37, 48]. The fact that the *Kitāb al-qarasṭūn* shows undeniable traces of numerous Greek traditions highlights the usefulness of the larger concept of multiple authorship. It also illuminates, as said before, that Thābit respected the forms of the fragments that he encountered when he compiled this particular text. (When he later reworked it into a text now lost but translated from Arabic to Latin by Gerard of Cremona, he no longer respected these forms but changed them quite substantially.) The *Kitāb al-qarasṭūn* is, thus, not an extract of earlier works that summarizes their main content. Still, it is true that Thābit was not the immediate author of any of the parts of the *Kitāb al-qarasṭūn*, as is shown by the omissions and oddities in some parts of the *Kitāb al-qarasṭūn* such as the incomplete proof of theorem 2 or the circularity in the proof of theorem 5. A second argument comes from its comparison with the *Liber karastonis*. There, Thābit expresses his frustration with the difficulties that he encountered in the translations and copies, their proofs and explanations, and describes some of his efforts to understand the ancient texts.

On the other hand and against Knorr's belief that both works represent different textual traditions, a stepwise comparison of the elements that both texts share leaves no doubt that the *Liber karastonis* is a carefully modified, edited, corrected, at times simplified version with explanations of the *Kitāb al-qarasṭūn*. Thābit clearly invested much effort into improving the extant Arabic version. It is in this altered text that he dealt with the deficits of the Greek ancestors of the *Kitāb al-qarasṭūn* by deleting two of the postulate-like statements and one theorem, introducing a new theorem and modifying the proof of its subsequent theorem, while he left these parts unchanged in the compilation. He also added explanatory statements and numerical examples within the various theorems, which can be easily traced. In addition to these clearly visible mathematical and methodical interferences into the previously compiled *Kitāb al-qarasṭūn*, Thābit's personal voice is also much clearer and stronger in the *Liber karastonis*. Hence, Thābit's role as an author differs between the two texts, although he wrote neither of the two fully on his own. This difference is reinforced by Thābit's explicit claim to authorship in the case of the *Liber karastonis*, while the claim to authorship in the *Kitāb al-qarasṭūn* comes from his students who wrote the text down in his class.

This comparison also shows very clearly that both texts constitute a textual unity and elucidates Thābit's working practice and concerns. It is this feature of interconnectedness and continuous dialogue, already visible to some degree in the elements surrounding the *Kitāb al-qarasṭūn*, and the leading role of Thābit in this continuous debate with unnamed contemporaries that allow us to attribute not merely the *Liber karastonis* but also the *Kitāb al-qarasṭūn* to Thābit as an author who chose their individual elements and decided how to present and share them and in which manner to interact with them.

The contextualized philological analysis of the *Kitāb al-qarasṭūn* reveals several other important features. This text shares central parts of its vocabulary, style, and grammar with many of the texts for which Thābit's authorship is accepted. Conspicuous terminological idiosyncrasies are shared with a translation of the *Almagest* made in the 820s as well as with early and late ninth-century translations of Aristotle's *Meteorology* and *Physics*. Symbolic idiosyncrasies link the *Kitāb al-qarasṭūn* to the environment of Hero's *Mechanics*. The *Kitāb al-qarasṭūn* and the translations just referred to agree in a specific and, in mathematical and astronomical texts, not widely spread choice of words for drawing or generating the path of a moving object. This idiosyncratic expression is "cutting out (or through) space or distance". Its particular context in the *Kitāb al-qarasṭūn* is that of producing a sector of a circle. It appears in the same form and with the same mathematical meaning in the extant fragment of the *Problemata mechanica*. This does not merely suggest that the *Problemata mechanica* was translated probably in the early ninth century and belonged to the collection of texts on mechanics available to the Banū Mūsā and Thābit b. Qurra before the 870s. The differences in detail between the two texts also show that Thābit did not copy directly from this Aristotelian text.

The same applies to the two texts on the balance and on heaviness and lightness attributed to Euclid. They share philological, conceptual, and representational elements with the *Kitāb al-qarasṭūn*, all without being identical. There is even sufficient reason to assume that Thābit worked with an older version of the *Kitāb al-mīzān* than the one extant today or with some other, very similar fragment. Thus, by comparing in this way the

Kitāb al-qarasṭūn with a substantial number of other mathematical texts translated, edited, or newly written during the ninth century, we confirm that the enriched and enlarged concept of multiple authorship applies well to this text. Thābit clearly used Arabic translations of Greek fragments, which he fused without remedying their shortcomings. But he also had access to a broader range of such texts and preferred certain formulations of principally similar subject matters to others. If he reformulated some of them on his own—which is possible but difficult to prove—he took care not to deviate recognizably from the language of his source(s). Textual fidelity was thus an important aspect of Thābit's authorship but had a different, richer nature than we tend to suppose.

The central force of my critique of Wilbur Knorr's arguments against Thābit b. Qurra's authorship of the *Kitāb al-qarasṭūn* is that we must engage in meaningful textual contextualization. This means considering all the other types of data available in the text at stake, along with the manuscripts containing its copies as well as their meaning for this text, if we wish to understand the working practices, values, and goals of the scholar to whom the text is ascribed. Furthermore, such contextualization yields insights into the sociocultural richness of the very concepts of authorship and textual fidelity for the text under analysis and, thus, may not merely answer specific questions but also help us to gain deeper insights into the scholarly climate and practices in a given culture at a given location and time. In the case of Thābit's connection to the *Kitāb al-qarasṭūn* and its Greek components, such contextualization also works against our importing any prejudices and generalizing assumptions about the value of different scholarly cultures and the capabilities of their members.

3. Peculiarities of Knorr's analysis

One outstanding peculiarity of Knorr's analysis of authorship is his continued modification of claims and positions, unaccompanied by any final decisions regarding which of his various ideas he considers at the end to be the most plausible. It makes it difficult for the reader to understand the relevance of individual arguments for or against each of these ideas. Moreover, these idiosyncratic oscillations obstruct the clarity of the proofs for or against Thābit's or young Archimedes' authorship.

A further methodological problem follows from Knorr's basic assumptions about authorship in general and for the case of the *Kitāb al-qarasṭūn* in particular. These assumptions summarized above cohere with his overlooking alternative hypotheses to his idea that the *Kitāb al-qarasṭūn* is an edition of an Arabic translation of a single ancient Greek text. And once set on this unilinear track, his particular readings of individual passages of the Arabic text were almost predetermined.

4. Issues of expertise

In this section, I will present evidence that Knorr's analysis did not merely suffer due to the limitations of his methods and his commitment to an internalist reading but was adversely affected as well by his misunderstanding of some of the Arabic words, expressions, or grammatical features. His analysis also suffered because of his limited engagement with the problems that he saw in the mathematical content of the *Kitāb al-qarasṭūn* and the *Liber karastonis* as well as with the issue of the relation between the content of these two texts, the *Liber de canonio*, and the appendix of the Beirut manuscript. It is not always clear whether his omitting to study these points more closely and his misinterpretation of some of them was due to his biases, which I will discuss in the last section, or whether they also were due to his difficulties with Arabic.

One case where the issue of philological competence played a decisive role is the interpretation of the *Liber de canonio* as a Latin translation of an ancient Greek text. As I have argued, one reason for Knorr's not seeing the many Arabisms in the second part of the *Kitāb al-qarasṭūn* is his limited familiarity with unpublished Arabic translations of Greek mathematical texts and their philological peculiarities. This kind of shortcoming applies to all of us. But not all of us are equally aware of the possible implications of our limited knowledge for our analysis and conclusions. Knorr certainly was more confident than I am. Another, and greater, part of his denial of Arabisms in the *Liber de canonio* (except for one) is his blindness to the many instances in which the second part contains verbal forms of a first person plural as well as conventional formulas for stating that something was proved or would be done similarly and so forth. I doubt that this astonishing fact reflects Knorr's philological problems, although one cannot exclude this entirely. It is more likely that it is the professional blindness that many of us will have experienced in our own work, a blindness which prevents us from seeing things in a text because we are so bound by our biases or questions as to overlook them.

4.1 *Philological misunderstandings and misrepresentations* Cases of true misunderstanding of Arabic occur when Knorr identifies phrases or sentences as corrupt or false against either classical grammar or medieval dictionaries. Their interpretation as simple philological errors remains nonetheless difficult since they are occasionally also part of his misrepresentations.

For example, there is his discussion of one of the passages in the Beirut manuscript of the *Kitāb al-qarasṭūn* which he took to be scholia. The London manuscript, the only other text available to Knorr, does not contain this part. Neither does the third manuscript, originally in the possession of Berlin's State Library but preserved since the final stage of WWII in Cracow. However, the shorter Florentine version of the *Kitāb al-qarasṭūn* presents this part after claiming that the *Kitāb al-qarasṭūn* had ended [MS Florence, Biblioteca Laurenziana, Or. 118, f 72a]. Thus, this new copy may indeed support the view that this particular passage is a scholium. It also offers some valuable alternative readings for weighing Knorr's interpretations of the corresponding passage in the Beirut text.

Knorr's main quest is, as in the case of the *Kitāb al-qarasṭūn*, for the (unknown) author of this passage. He admits that no clear evidence can be found in the text itself for providing a definitive answer. But he feels that

> certain awkward or unclear expressions...together with problems of its logical ordering, recommend viewing it to be a translation, rather than an original composition. [1982, 68–69]

In alleging the logical problem and construing one of the expressions as awkward, Knorr shows that he misunderstood the Arabic here. The so-called "unclear expression" likewise highlights his limited familiarity with Arabic scientific literature. I will discuss this "unclear expression" momentarily. As for the problem of logical order, Knorr describes it as the failure to point out that "the problem of balancing the unevenly divided weighted beam" is "a logical consequence of (the) general principle of equilibrium for the weightless beam" [1982, 68]. The alleged lack of logical ordering is the product of Knorr's misinterpretation of «wa-dhālika annahu» as «wa-dhālika innahu» and his literal understanding of this expression as "and that is what it...". But «wa-dhālika innahu» does not exist, while «wa-dhālika annahu» means "this is the reason why", "because" or "since". It also can be translated simply as "which means" or "to say it more precisely" or simply "namely" or "to wit". Thus, there is no logical problem here. Read correctly, the Arabic text makes clear that the problem of the material beam can be treated on the basis of the knowledge provided for the immaterial beam with the additional consideration of the role of the beam's materiality. There is no need to treat this formulation "as an inadequate translation" [1982, 69].

Knorr's first type of "awkward expressions" occurs in two instances of stating—incorrectly, according to Knorr—"the condition of parallelness of the beam". According to Knorr's discussion in the main body of his book, the text expresses the equilibrium as obtaining when "it (*sc*. the scale-pan) is parallel to the horizon with the beam" [1982, 69]. In the appendix, however, the passage is translated as "if it is suspended at the end of the smaller part, it is too small to make the beam parallel to the horizon..." [1982, 187]. Ignoring here the rendering of « aqṣar » as "smaller" rather than "shorter" and the reading of « qaṣara ʿan » as "being to small to make" rather than as "being unable to, failing to reach..."—these two different possible translations reflect two different "identities" of the verb « qaṣara »—the translation in the appendix is in principle correct. Thus, I fail to understand Knorr's lengthy discussion of a deviating and false rendering of this as well as a second expression of analogous kind and their description as "awkward", "clumsy", or "ungainly". Neither is it clear to me why he chastised the two Arabic expressions by writing "But of course it is the beam, not the counterweight, which can be parallel" [1982, 69–70]. Had he forgotten his translation in the appendix or did he believe so strongly in his intended result, i.e., in the fact that we have here "again an imprecise rendering of an absolute expression from the Greek" that he sacrificed this translation?

Furthermore, the incorrect statement detected by Knorr in the expression "it (*sc*. the scale-pan) is parallel to the horizon with the beam" reflects difficulties in understanding the function of the preposition « bi » in two Arabic phrases [Knorr 1982, 186] which I translate as follows:

اذا علقت بطرف القسم الاقصر قصرت عن ان يوازي بالعمود الافق

> If it [*scil*. the scale-pan] is suspended at the endpoint of the shorter arm, it fails to make the beam parallel to the horizon.

ثم يتعرف وزن ما يحتاج اليه مع وزن الكفة لموازاة الافق بالعمود

> Then the weight is to be learned [i.e., determined], which is needed together with the scale-pan for the parallelism of the beam to the horizon.

Knorr took both instances to signify that the author of the scholium speaks "in each instance of the counterweight, the scale-pan" and expresses equilibrium as obtaining when "it (sc. the scale-pan) is parallel to the horizon with the beam" [1982, 69]. The second passage, however, does not concern the weight of the scale-pan alone but a sum which is responsible for the equilibrium, namely, the weight of a part of the material beam together with the weight of the scale-pan. In short, Knorr's description of the problem is inadequate. But is it correct to interpret even the first passage as meaning that "it (the scale-pan) is parallel to the horizon with the beam"? As my translation indicates, I do not think that this is a correct reading of the Arabic phrase. Knorr goes astray here because he mistakes the meaning of the preposition « bi ».

In classical Arabic, «bi» indicates *a connection with something* or *the object with which something happens* [Fischer 1972, 136: §§294, 294.1]. Knorr obviously selected the first meaning, though the second is the appropriate one. Furthermore, in one of its sub-forms, «bi» is called the «bi» of transitivity. This means that it either transforms an intransitive verb into a transitive one or strengthens the transitivity of an already transitive verb. It is this grammatical function that «bi» has in the two instances given above. Hence, «bi'l-ʿamūd» in both cases can either be translated simply as a part of a genitive construction, viz. the parallelism of the beam to the horizon, or as an object that is made parallel to the horizon. Given the minor differences between the two formulations, I have given both in my translation. In sum, the Arabic of these formulations is not faulty. And there is no cause to mark them as the product of a bad Arabic translator or to speculate about the existence of a Greek ancestral text.

4.2 *Identifying diacritical marks* Other philological problems with the passage just discussed in the previous subsection concern the identification of the letters in an Arabic word without diacritical points [1982, 183]. The lack or misplacement of diacritical points is a constant technical problem of medieval texts in Arabic script. It is not always easy to ascertain the correct placement of these points and, thus, to identify the verb and its grammatical form. Mistakes are easy. Their avoidance necessitates in difficult cases careful reflection and at times tedious comparison with other, similar formulations within the same text or, if one encounters a particularly ambiguous statement, with other texts.

In the two cases within one sentence that I will present here, the difficulty rests not merely in the lack of diacritical points but in the changes evident in the text in the Beirut manuscript. Knorr could not fully comprehend these changes, since he was not aware of the Florentine manuscript. Nonetheless, his first choice of diacritical marks should at least have made him suspicious of the passage since, in order to make sense of the text, he had to assign the verb that he settled upon a meaning which is not supported by our lexica. Moreover, he clearly recognized that the reference to some previous theorems (where his forced translation of this verb appears) posed a problem in so far as the Beirut text refers to theorems which were not yet presented. But rather than make allowances for a problematic Arabic text, he took this feature to signify that the passage was interpolated from an Arabic translation of a different Greek text where it actually had made sense.

وذلك انه اذا اخذ عمود متساوي الغلظ فقسم بقسمين مختلفين على نقطة وجعلت المعلاق فانه يتهيا بالاشكال التي قد علمت ان يوخذ مقدار الثقل الذي اذا علق بطرف القسم الاقصر اعتدل العمود اذا علق بمعلاقه على موازاة الافق.

[Knorr 1982, 182]

> And that is that if there is taken a beam, uniform in thickness, and it is divided into two different parts at a point and this is made its suspension, then it results from the theorems which have just been learned that there can be taken the quantity of weight, which, if it is suspended at the end of the smaller part, the beam is in balance if it is suspended from its suspension in parallel to the horizon. [Knorr 1982, 183]

I have highlighted the two verbs without diacritical points and their interpretation by Knorr in red text and the reference to previous theorems in dark red text. In both verbs, three letters are unidentified in the Arabic text. In the first, Knorr chose to interpret them as «y», «t», and «y». In the second, he opted for «y», «kh», and «dh». In this latter case, he knew that the alternative was «y», «j», and «d». In the first, he does not present any alternative reading, which in my view is given by «n», «b», and «n». Knorr's reading of the first verb is «yatahayya'u», which means literally "it is prepared" or "it is ready". But this does not fit the context as can be seen in his translation above. Hence, he altered it to "it results" [1982, 84–185]. My alternative identification of the consonants yields «nabihnā» for the first verb, which means "we note". This modifies the translation meaningfully without overstating the content of the Arabic verb. This new translation is, however, only possible thanks to my access to a fourth Arabic text preserved in Florence.

In the case of the second verb, the Arabic text available to Knorr allows for two possible readings: «yu'akhudha» meaning "to take" or «yujada» meaning "to exist" or "to be found". The text transmitted in the Florentine manuscript [see MS Florence, Biblioteca Laurenziana, Or. 118, f 72a,4–6] offers a substantial variant to the Beirut text and thus opens the way for an altogether different understanding of this passage.

فانه نبهنا ان يجد بالاشكال التي عملها ابو الحسن مقدار الثقل الذي اذا علق بطرف القسم الاصغر اعتدل العمود اذا علق بمعلاقه على موازاة الافق.

> ...then we note that the quantity of the weight, which equilibrates the beam in parallel to the horizon, if it (i.e., the weight) is suspended at the end point

> of the smaller part (and) if it (i.e., the beam) is suspended in its suspension, is found with the theorems, which Abū l-Ḥasan has produced.

It is easy to see that the verb of the second passage together with «an» appears much earlier in the Florentine text than in the Beirut version and has, thus, a different point of reference. This difference in placement implies a different understanding of this passage and, by virtue of further deviations between the two texts, allows us to solve the problem of the referent of the previous theorems. The Florentine variant specifies that these are theorems which Abū l-Ḥasan, i.e., Thābit b. Qurra, had produced. This explanation shows, moreover, that the passage came into being in all likelihood after Thābit b. Qurra had compiled and taught the *Kitāb al-qarasṭūn* or at least the parts prior to theorem 2.

The problems that I have just addressed not only indicate Knorr's struggles with classical and middle Arabic, they also highlight the difficulties of interpreting such passages on limited textual bases. They warn us to be more cautious and to avoid drawing over-grand conclusions from too small features, a failing of mine for many years in my studies of the Arabic *Elements*. This insight into my own shortcomings has convinced me of the need to contextualize documents textually at the very least.

5. Issues of prior beliefs

Our explicit beliefs and deep-seated prejudices can be a persistent obstacle in our research. They guide our interpretations and conclusions and, thus, typically mislead us in our study of texts, images, or material objects. The way to limit their impact is well known today—critical reflection. In comparison to today's attention to historical epistemologies, there was not so much awareness of the importing of modern notions to historical sources at the time when Knorr wrote his book; not, at least, among historians of premodern mathematics and other exact sciences. At that time, we believed, myself included, that we could and ought to be objective and neutral and that, if we did introduce values, they should work in favor of the people and the works that we studied. We did not believe that our scholarship included and inevitably brought to bear values that we did not question but took for granted. Thus, for example, we believed without question that doing science for science's sake was the most noble and, indeed, the only right way of doing science. Likewise, we also assumed that good science relied on objective, rational, and verifiable principles, methods and theories; and that in contrast to other domains of human activity, science was free of biases and subjectivity. One consequence of these beliefs was a scholarly practice that privileged the study of theoretical themes, primarily in texts, by scholars deemed first-class, in periods and regions regarded as leading intellectual centers. All other products, scholars, periods, and regions were more or less overlooked with the exception of astronomical, mathematical, and geographical material on timekeeping, the determination of the *qibla* or direction to Mecca for prayer, and related religiously sanctioned problems.

5.1 *The putative superiority of ancient Greek geometers* Knorr's claims against Thābit's authorship and in favor of a single ancient Greek ancestral text with the young Archimedes as its author are anchored in two beliefs, the first of which I will discuss in this section and the second in the next. The first is that Hellenistic geometers were intellectually superior to medieval scholars writing in Arabic. The second is that mathematical texts developed or evolved from a higher, more advanced level to a lower, more elementary level as a result of the explanatory and exemplifying interpolations introduced over centuries of teaching those texts. The belief in the intellectual superiority of ancient Greek scholars was first formulated by humanists. It was particularly rampant during the 19th century when claims to astronomical or mathematical creativity by scholars from Islamicate societies were greeted, for instance in France, with disbelief or even derisive laughter, as Charette has argued in his analysis of the respective positions among European writers about the exact sciences in Islamicate societies [1995, 101–142].

In the course of the 20th century, especially since the 1960s, historians increasingly began to argue for the innovative and creative achievements of medieval scholars from Islamicate societies. Other historians, in particular classicists and European medievalists, continued, however, to uphold the older "sandwich thesis" according to which the only or major role that scholars from these societies had played consisted in their translating ancient Greek texts and preserving them in this way for their later translation into Latin.[3] Knorr's belief in the intellectual superiority of Hellenistic geometers was less crude in that he recognized that from the 10th century onwards there existed gifted masters of theoretical geometry among the scholars from the classical Islamicate societies.

In the case of the *Kitāb al-qarasṭūn*, Knorr's conviction derives from his profound familiarity with Archimedes' works and his superficial understanding of Thābit b. Qurra's oeuvre. The mere fact that Knorr did not try to

[3] Sabra termed this position as straightforwardly "reductionist" [1987, 224–225].

compensate for his limited exposure to Thābit's works by a careful analysis of at least all treatises by Thābit that contain aspects relevant to the various issues discussed by Knorr in the *Kitāb al-qarasṭūn* indicates the guiding power of his belief in the higher quality of classical and Hellenistic mathematical works. His constant willingness to ascribe all kinds of perceived or actual shortcomings in the Arabic text to translators at large or to Thābit b. Qurra in particular, and to use these failings as indicators of the mishandling of Greek source texts, further manifests the power of this belief. Likewise, Knorr's seizing on the contradictory and inconsistent treatment of individual points in the *Kitāb al-qarasṭūn* as hints of an Archimedean background or as evidence of Thābit's "pedantic" or "pedestrian" but "competent" work as a geometer is yet a third instance of his biases at work.

The clearest cases of Knorr's biases and their interpretive consequences appear in his analyses of theorems 3–5. But, before I turn to them, I must draw attention to his inconsistency in formulating his main interpretations, something which I have already mentioned. He changes these formulations often and in a substantive manner, as I will show below. Again, it is unclear why he proceeded in this manner, given the difficulty in supposing that he did not understood the differences between his various statements.

The inconsistency and contradictory manner of Knorr's presentation of his belief in the Greek origin of the *Kitāb al-qarasṭūn* surfaces on numerous occasions. He oscillates between stronger and weaker forms of this belief. This would not have been a problem if he had expressed his uncertainty clearly and presented the arguments in a manner clarifying the problems that he saw in regard to any of his proposed positions. But he does not do so. Instead, he leaves the reader with the impression that he either remained unaware of these variations or did not recognize the methodological problems that they entail.

Knorr starts his discussion by allowing that Thābit had composed "his *Kitāb al-qarasṭūn*" or at the very least the proof of theorem 5 [1982, 31, 33]. Then, he proceeds to the claim that, in this proof, the relationship between this text, the Latin *Liber karastonis*, the *Liber de canonio*, and the short appendix (*ziyāda*) in the Arabic text of the *Kitāb al-qarasṭūn* in MS Beirut, St. Joseph University, 223 shows that the *Kitāb al-qarasṭūn* must have been written by "an author different than Thābit" and that Thābit "prepared an improved edition of this prior treatment and appears to have had access to the theorems in the Beirut appendix to guide his effort" [1982, 37]. He repeats this when he claims:

> The Arabic manuscripts [of the *Kitāb al-qarasṭūn*] appear to derive from a work by one of Thābit's colleagues; it is not impossible that it was a prior draft on the *qarasṭūn* made by Thābit himself. It depended in an important way on materials translated from a Greek work not now extant. [Knorr 1982, 48]

But, after discussing some of the material, he no longer hesitates to offer a strong form of his thesis of its Greek origin: "...the Greek source, of which the Arabic manuscripts of *K. Qar*. are an edited translation,..." [1982, 86]. A few pages later, he goes a step farther and writes: "This strengthens our view that *K. Qar*. presents to us the edited remnant of an Archimedean work" [1982, 93].

Accordingly, Knorr takes the position that the text extant in the two copies of the *Kitāb al-qarasṭūn* which were known to him (Beirut, London) is but a single text derived from another single text by way of translation from Greek into Arabic and editing in Arabic. Two men were the main contributors to this textual sequence: Archimedes and Thābit b. Qurra. The other persons whom he touches upon during his discussion, Thābit's "anonymous colleague" and the author of the possibly pseudepigraphic Euclidean fragments of *On the Balance* and *On Heaviness and Lightness* have faded into the background.

5.2 Knorr on the devolution of mathematical texts The second belief that shaped Knorr's analysis and, hence, his arguments and conclusions concerns whether there was in the main a single direction of development in mathematical texts during antiquity and the Middle Ages. Rommevaux, Djebbar, and Vitrac [2001] have already described Knorr's view [1996] of this in their analysis of his article about Heiberg's edition of Euclid's *Elements*. They concluded, in somewhat different words, that Knorr believed that in ancient Greek texts there was in general a devolution which went from more complex or advanced mathematical works to simpler and longer ones where (almost) every simple step of the original has been spelled out. According to their analysis, Knorr saw this line of development as a result of the use of texts in teaching and of continuous editing and commenting. In his book on Greek, Arabic, and Latin texts on the balance, Knorr does not formulate this belief explicitly. But, as my analysis of his treatment of a part of the proof for theorem 3 in the *Kitāb al-qarasṭūn* shows, it was one element that guided his choice between two alternative interpretations.

Knorr's strong thesis about the Archimedean origin of the *Kitāb al-qarasṭūn* and its fragmentary textual nature is based on a mixture of beliefs about how mathematics developed in ancient and medieval times as well as about the mathematical skills of ancient Greek and early Abbasid scholars. In addition, he develops and proposes to justify it, as I will show below, through references to a number of Archimedean works and an analysis of some of the mathematical as well as a few philological features of the two copies of the *Kitāb al-qarasṭūn* available to him [1982, 47–48, 76–86]. These beliefs and his selective working practice precluded considering interpretations of material alternative to his strong thesis. In short, his conclusions are not always derived from an open-minded investigation of what was available to him in the 1980s. In several instances, no firm conclusions can be drawn from the evidence presented by Knorr, sometimes not even from the broader evidence that I have collected. The processes that led to the texts extant today may have evolved in more than one way. The material available does not allow us to determine one historical sequence of steps. Indeed, it is possible to conjecture one set of steps as a sequence taking the one or the other direction or as a parallelism of events, independent or not from each other. Instead of allowing for questions that could not be answered fully or problems that must be left unresolved, Knorr wished to do the impossible—to reconstruct a fully lost text of which we possess no more than a small selection of titles provided by Heron and Pappus. In his effort to reach this goal, Knorr did not attend to the extant texts and determine their individual features with care and caution.

Lest one think that this is peculiar to Knorr alone, I must confess that I have been told on several occasions by a friend and colleague that I was trying to achieve too much in my analysis of the Arabic translations and editions of Euclid's *Elements* made during the ninth century. Having believed for more than a decade in the medieval narrative of two main bodies of translations and editions, one undertaken in the early ninth century by al-Ḥajjāj b. Yūsuf b. Maṭar (d. after 827), the other by Isḥāq b. Ḥunayn (d. 911) in cooperation with Thābit b. Qurra, I tried to compile a collection of fragments of al-Ḥajjāj's work. Eventually I was forced to admit that all the extant texts of these two different traditions of the ninth century derive, in the case of books 3–9 at least, from the work of only one of these translators, given that they share idiosyncratic vocabulary and mistakes. Many of our specific beliefs about the work of these three scholars and their terminology stand in need of revision as well. Whether it will be possible to determine the translator or editor of this interrelated set of texts remains an open question: I suspect that I may never be able to sort things out in a manner that will allow me to formulate at least a credible hypothesis. Such differences in research goals, as illustrated by the differences between Knorr's and my own aims, signal fundamental changes in epistemic values over time, even within the lifetime of a single scholar.

5.3 *Theorem 3 of the* Kitāb al-qarasṭūn Theorem 3 describes a weightless beam with one weight appended to its extremity, and with two equal weights on the other side, one of them at the extremity and the other one closer to the fulcrum. Assuming that this configuration is in equilibrium, the theorem states that these two weights can be replaced without disturbing the equilibrium by a weight of the same amount as the two taken together, positioned at the midpoint between them [Jaouiche 1976, 152–155].

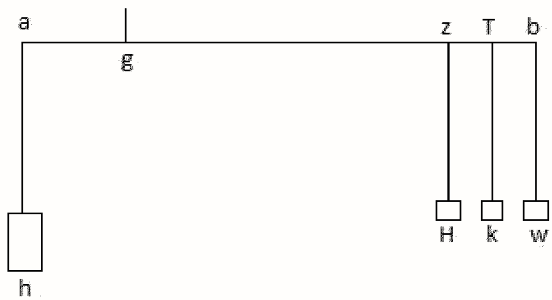

Figure 1. Theorem 3
(the weightless beam)

In the two manuscripts available to Knorr, there are differences in how the proof of this theorem begins. I summarize the relevant steps as follows:

The London manuscript

(1) For w (it is the case that) (part of h)$:w = bg:ga$
(2) For H (it is the case that) (part of h)$:H = zg:ga$
(3) $w = H \;\rightarrow$ (part of h)$:w = zg:ga$
(4) Now add $\rightarrow h:w = (bg + zg):ga$
(5) Equally (it is the case that) $h:(w + H)$, since $w + H = 2w$, $= (bg + gz):2ga$
(6) Also: $= \frac{1}{2}(bg + gz):\frac{1}{2}(2ga)$
(7) As for $\frac{1}{2}(bg + gz)$, this is gT
(8) As for $\frac{1}{2}(2ga)$, this is ga
(9) $\quad \Rightarrow h:(w + H) = Tg:ga.$

The Beirut manuscript

(1) For w (it is the case that) (part of h)$:w = bg:ga$
(2) For H (it is the case that) (part of h)$:H = zg:ga$
(3) Now add → all of $h:(w+H) = (bg+gz):2ga$
(4) Equally (it is the case that) (part of h)$:(w+H)$, since $w+H = 2w$, $=$ ½ $(bg+ga)$; this is gT.
(5) As for ½ $(2ga)$, this is ga.
(6) $\Rightarrow h:(w+H) = Tg:ga$.[4]

Anybody who encounters such a divergence needs to decide which of the two versions is most likely the earlier one. There are no strict criteria to apply, only rules of thumb deriving from the search for mistakes, contradictions, modernizations, interpolations, philological peculiarities, and comparison with similar passages within the given text as well as with other works of an author, translator, or commentator. Often such investigations clarify the sort of divergence that one sees here in theorem 3. Occasionally, though, the best one can offer is an informed guess about which is earlier. But is this the case here?

A quick comparison between the two step sequences reveals two key differences. First, step 3 of London is missing in Beirut and step 4 in Beirut differs in form and content from that in London. Second, the last part of London's step 4, all of steps 5 and 6, and the beginning of step 7 are missing in Beirut. This second difference leaves no doubt that Beirut has lost a part of its text. So, is this also the case for the first difference? There is here, however, no clear sign to show which version has lost text. Yet, one would seem to be a modification of the other. The question only is, then: Which version modifies the other?

Knorr decided it was Beirut that preceded London, i.e., that the London version modifies the Beirut version. What are his arguments? He claims that, in addition to some minor differences, the texts of the Beirut and London manuscripts are separated by one subtle but substantial difference. He believes that the move from steps 1 and 2 in the Beirut manuscript to step 3 necessitates the silent assumption of a lemma about the addition of proportions. He finds the same move in Archimedes' theorem 1 of *Conoids and Spheroids* and he calls Beirut's step 3 "daring" [1982, 59].

[4] My use of the letters here corresponds literally to the Arabic. In the following, however, I will stick to Knorr's usage of Latinized Arabic letters as is the convention among historians of science in Islamicate societies.

For the sake of clarity, I will quote here the entire passage where Knorr presents this evaluation. Before I do so, however, I have to point out that a mistake of copying is part of this quote. In the Arabic text of the proof of theorem 3 (called by Knorr "proposition IV" after the *Liber karastonis*) in appendix E, Knorr correctly gives the verb in Beirut as «jumi'a» and in London as «jama'nā», which he correctly translates as "to combine" with their specific grammatical forms [1982,194–195]. In the passage that I will quote now, however, he transformed the Arabic verb «jama'a» into «ja'ala», translating the latter incorrectly as "to compose" [1982, 58–59]. In the quote, I highlight in red important interpretative sentences.

After pointing out that step 3 of London is missing in Beirut, Knorr writes:

> the texts now come back in agreement, save for a key difference at the end of the next line:
>
> | (London) | (Beirut) |
> |---|---|
> | And if we compose, the ratio of weight E to weight W is as the ratio of the ratio $bg + gz$ to ga. | And if they have been composed, the ratio of *all* of weight E to weight WH is as the ratio of $bg + gz$ to *twice* ga. |
>
> Here the differences are minor: "compose" and "have been composed" are a matter of scribal differences («ja'alna» and «ju'ila»). The appearance of "all" in Beirut is important, in that it alludes to the procedure by which the subsequent proportion has been derived: namely, by adding parts of E which have been viewed as counterbalanced separately by W and H. We note also the appearance of "twice" in Beirut, missing from London. These discrepancies result from two rather different modes of "composing" the ratios. To see this, let us write E_w for the part of E counterbalanced by W, and E_h for that part balanced by H. Then, $E_w : W = bg : ga$ and $E_h : H = zg : ga$. In the London ms. we are to introduce the substitution $H = W$ in the second proportion. Since the denominators of our two proportions are now identical, we may add the numerators, obtaining $(E_w + E_h) : W = (bg + gz) : ga$. This step is not stated in this form, but as $E : W = (bg + gz) : ga$, it being obvious that E is the sum of the parts E_w and E_h. In the Beirut ms. an operation of a subtly different sort is performed. On the same initial terms, $E_w : W = bg : ga$ and $E_h : H = zg : ga$, it is at once deduced that $(E_w + E_h) : (W + H) = (bg + zg) : (ga + ga)$, that is, $E : W = (bg + gz) : 2ga$. Is this justified? As it happens, the step, daring in appearance, is actually covered precisely by the theorem on proportions which Archimedes proves as *Conoids and Spheroids*, 1—we only need the condition that the four numerators or the four denominators are in proportion, e.g., $W : H = ga : ga$. This is manifestly true here since $W = H$. [Knorr 1982, 58–59]

Once Knorr "recognizes" in the variant presented in the Beirut manuscript an Archimedean ancestor unknown to scholars in Abbasid Baghdad, he reverses the argument. He now presents the silent application of the Archimedean lemma just diagnosed as a marker for an Archimedean character of the *Kitāb al-qarasṭūn* and supports this argument by referring to the subsequent theorem:

> The automatic assumption of a lemma on proportions of this sort, proved and applied only in Archimedean works not available to Arabic scholars, is reminiscent of the Archimedean features we have perceived in *K. Qar*. VI. [Knorr 1982, 58–59]

Thus, in seeing in the Arabic *Kitāb al-qarasṭūn* the remainder of a single text of Archimedean provenance, Knorr ignores the circularity of his argument and overlooks the fact that other readings are possible and more plausible, if one abandons the Archimedean thesis and tries to understand the Arabic text on its own terms. This might entail, for instance, searching for hints that the slow, step-by-step procedure of London constituted the original version, while Beirut's allegedly daring recourse to Archimedes' *Conoids and Spheroids* theorem 1 was the result of editing London's text. Or it might involve asking whether the proof given in the *Liber karastonis* contributes to the understanding of this small textual difference.

If one tries to understand the Arabic text on its own terms and looks to its language, it becomes evident that Knorr's understanding of the two variants of the proof of theorems 3 in the Beirut and London manuscripts is predicated on three more simple mistakes that are relevant for answering the question of which variant is the older of the two. The first of these two mistakes Knorr shares with Jaouiche. Both did not recognize that the letter «waw» in one occasion signified the mathematical symbol of one of the weights as it does on other occasions in this theorem. As a result, Knorr's translation is incorrect.

The phrase in question is «idhā kānā mithlay waw». Its correct translation is: "since the two are twice the same as *waw*", i.e., $W + H = 2W$. Jaouiche [1976, 155] understood the phrase to mean «lorsque ces deux derniers sont égaux», i.e., $W = H$. He overlooked the «waw» and ignored the fact that the form of the dual of «mithl» (the same in this phrase) is a *status constructus* (*mithlay*) due to the following «waw», not a *status indeterminatus* («mithlayni») as demanded by his translation. Knorr [1982, 59] rendered this phrase as "since they are equal", thus agreeing tacitly with Jaouiche.

The paragraph in full where this expression occurs runs as follows:

London

وكذلك تصير نسبة ه الى و ح مجموعين اذا كانا مثلي و كنسبة ب جـ
جـ ز مجموعين الى مثلي جـ ا ونسبة نصف ب جـ جـ ز الى نصف مثلي
جـ ا. فأما نصف ب جـ جـ ز فهو جـ ط. وأما نصف مثلي جـ ا فهو جـ ا.

And equally, the ratio of E to W, H, the two being added, since the two are twice the same as W, will be as the ratio of bg, gz, the two being added, to twice the same as ga (as well as) the ratio of half of bg, gz to half of twice the same as ga. As for half of bg, bz, this is gt. As for half of twice the same as ga, this is ga.[5]

Beirut

وكذلك نظير نسبة ه الى و ح مجموعين اذا كانا مثلي و كنسبة نصف
ب جـ جـ ا مجموعين فهو جـ ط. واما نصف مثلي جـ ا فهو جـ ا

And equally, the corresponding of ratio E to W, H,[6] the two being added, since the two are twice the same as W, (is) like the ratio of half of bg, ga, the two being added, this is gt. As for the half of twice the same as ga, this is ga.

The text in the two Arabic manuscripts differs in the third word. MS London uses « taṣīru » (will be, becomes, *vel sim.*). MS Beirut has « naẓīr » (same, like, corresponding, equivalent *vel sim.* or in correspondence to, in return of, for, *vel sim.*). Knorr correctly suggests considering the spelling in the Beirut ms. as a scribal mistake [1982, 59n4].

Knorr's second mistake in this passage comes in his translating « kadhālika » as "and for that (reason)" [1982, 59]. This translation of « kadhālika » is simply false from the semantic point of view. But, surprisingly, Knorr also misinterprets the content of this sentence. Step 5 of the London variant and the garbled step 4 of the Beirut manuscript are clearly not the consequence of their respective predecessors. In the London variant, step 5 is the result of the multiplication of both denominators W and ga by 2 and the argument that $2W = H + W$. It is here that Knorr's previous mistake concerning

5 I did not add the twice missing « majmūʿayn » (the two being added) after bg, gz in the second half of the passage, since such an elliptical mode of speaking was not uncommon in Arabic mathematical texts of the period. The corresponding passage from Beirut shows, however, that the term was not missing in an earlier stage of textual transmission.

6 *scil.* what corresponds to $E:(W+H)$, i.e., $(bg+gz):2ga$. The Arabic word « naẓīr » is, however, a scribal mistake for « taṣīru ».

« waw » impedes a proper understanding of the garbled sentence in the Beirut manuscript. But, in trying to interpret the sentence as he has read it, Knorr makes a third simple mistake by explaining the obvious loss of steps in Beirut as due to *homoioteleuton* [1982, 59–60]. But in so doing, Knorr is forced to emend the Arabic ga to the English gz, i.e., "the half of $bg + gz$", which appears twice in quick succession:

> (Beirut) And for that (reason) the equivalent of the ratio of E to $W + H$, since they are equal, is as the ratio [of the half of $bg + gz$ to half of twice ga. As for] the half of $bg + gz$, it is gt, and as for the half of twice ga, it is ga. [1982, 59–60, Knorr's emphasis]

The Arabic text has, however, "the half of $bg + ga$" [1982, 194.13 (middle column)]. Thus, it is necessary to assume two losses. The first occurred between $bg+$ and ga. It consists of "gz to half of twice". This is not due to a *homoioteleuton*. The second loss occurred between ga and the description of an addition followed by "this is gT". It consists of "as for the half of bg, bz". This *is* the result of a *homoioteleuton*. The complete ancestral text of Beirut in this passage would then be almost identical to the one in London:

> Equally it is the case that $h{:}(W + H)$, since $(W + H) = 2W$, $= ½(bg + [gz){:}½(2]ga)$; [as for $½\,(bg + gz)$,] this is gT.

When we ponder the significance of this restored passage within the entire part of the Beirut text for the question of which of the two variants is the younger, we find an additional argument for the Beirut manuscript's being the one which was modified. The point is that this step fits perfectly well into the slow procedure of the London text but is superfluous in the Beirut variant, since in the Beirut text step 3 has already produced the proportion $h{:}(H + W) = (bz + gz){:}2ga$, which is the purpose of step 5 in the London text and of the restored form of step 4 in the Beirut manuscript.

The mistakes which Knorr makes in interpreting these two sentences result, on the one hand, from his problems with Arabic and his extending the semantic range of Arabic words too broadly and, on the other, from his identification of supposedly Archimedean features in the Arabic text and his unwillingness to investigate alternative interpretations.

My conclusion that the Beirut manuscript is more recent than the London manuscript is reached without any preconceived notion about the character of the *Kitāb al-qarasṭūn*. It is also confirmed by the philological congruence of London's steps 7 and 8 and the latter's equivalence with Beirut's step 5. The agreement between these steps and their elementary content also contradicts Knorr's assumption that the first step in Beirut must, by virtue of its supposed boldness, represent the older textual stage. The elementary character of this part of the proof, where the author found it necessary to state that $\frac{1}{2}(bg + bz) = gT$ after he had just stipulated this and that $\frac{1}{2}(2ga) = ga$, does not support Knorr's characterizing the start of Beirut theorem 3 as a "daring" step. Rather, one must either explain away the later steps as an interpolation or abandon the idea that Beirut's first step is Archimedean and prior to London's elementary building up of the proportions needed. If one values a minimalist invasion into a transmitted text to "make it fit" some idea of correctness, one will have difficulty seeing any credible alternative to considering the text in the London copy as the earlier stage of the proof, with the constraint that it contains certain features that are clearly the result of copying, e.g., the disappearance of the stipulation that bz, gz or two other quantities need to be added.

This interpretation of London's priority is strengthened by the fact that the proof of this proposition in the *Liber karastonis* proceeds like that in the Beirut manuscript. The *Liber karastonis* is, as I have said, a clearly recognizable edition of the *Kitāb al-qarasṭūn* by Thābit b. Qurra [see Moody and Clagett 1952, 96, 98]. Accordingly, it seems more likely that Beirut's variant is a modification of the original text introduced by some copyist on the basis of Thābit's edited Arabic version of his compilation of Arabic translations of Greek fragments on the steelyard, a compilation which is only extant in Latin translation as *Liber karastonis*.

As I have already mentioned, Knorr's interpretation of the textual differences in the manuscripts of theorem 3 confirms what Rommevaux, Djebbar, and Vitrac have already learned from their analysis of his article on Heiberg's edition of Euclid's *Elements* about his view of the development of ancient and medieval mathematical texts [Rommevaux, Djebbar, and Vitrac 2001, 244–246]. His unwarranted interpretation of step 3 in the Beirut manuscript as a marker of an Archimedean ancestry reflects partly this belief in a "downhill" change in mathematical treatises. In the case of theorem 3, the reason for this change is purportedly due to editorial work:

> While the discrepancies between the manuscripts are minor, they are nevertheless instructive. Most important are the differences occasioned by the slightly different conceptions of the "composition" of the proportions. The Beirut ms. adopts a rather more sophisticated method, reminiscent of a technique peculiar to Archimedes. But the London ms. is here quite correct, despite the changes made. These changes are thus not inadvertent, but deliberate, the work of an editor who perceived a step in his text, assumed there without explicit justification, and so sought to clarify it by making minimal changes. It is evident here that the manuscript tradition of *K. Qar*. represented by the Beirut ms. must be

> prior to that represented by the London ms., since the former adopts a proof technique not likely to have been familiar to an Arabic editor. [Knorr 1982, 60]

The third and the last sentences in this quotation highlight Knorr's beliefs about Archimedes' exceptional methods and about the dependence of Arabic scholars on ancient Greek methods and theories. The quotation also confirms my analysis of the shortcomings in Knorr's reasoning and the fact that they are due to such prejudices. Note too that Knorr's reasoning is circular. He looked at this brief passage, decided that Beirut is more sophisticated, recognized the method in Archimedes' *Conoids and Spheroids*, which had not been translated into Arabic, and concluded that the Beirut variant must be the older textual level derived from an Archimedean text most likely unknown to an Arabic editor. My analysis of theorem 3 indicates, in contrast, that no Archimedean predecessor is warranted and that the "daring", "peculiar", "Archimedean" technique actually seems to have been introduced by a later copyist on the basis of Thābit's modifications of this proof in his revision of the *Kitāb al-qarasṭūn*.

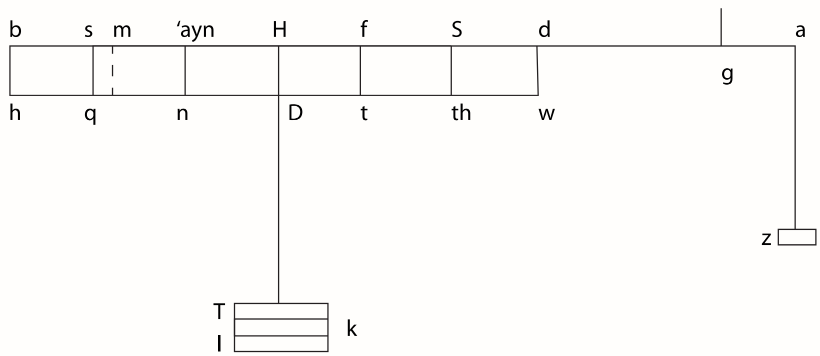

Figure 2. Theorem 4
(the weightless beam)

5.4 Knorr on Theorems 4 and 5 Theorem 4 (see Figure 2) states that equilibrium is not disturbed if a uniformly distributed weight on an (immaterial) balance is replaced by an equal weight suspended from the middle point of that distributed weight. The proof is a sophisticated demonstration *ex contrario* using Archimedean-style techniques of proof [Jaouiche 1976, 156–165].[7]

[7] Figure 2 represents the first part of the proof. For the diagram of the second part, see Jaouiche 1976, 160–161.

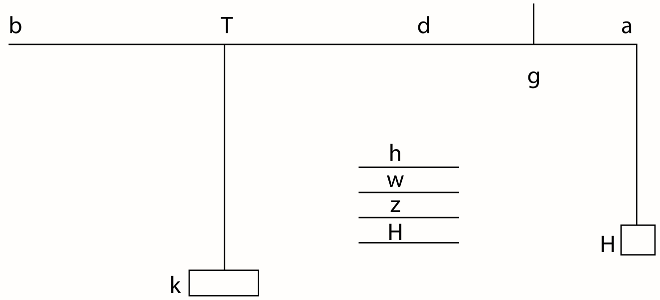

Figure 3. Theorem 5
(the material beam)

Theorem 5 (see Figure 3) treats a balance with a material beam. It determines, in the form of a problem, the weight that must be attached to the shorter end of a material beam that is not suspended from its middle point, in order to keep the beam in equilibrium. The proof explicitly refers to calculation techniques of practitioners. It also makes explicit use of Theorem 4 [Jaouiche 1976, 164–169].

Knorr's treatment of these two theorems represents a second example of the impact of his beliefs and prejudices on his description, analysis, and interpretation of this Arabic text. Knorr describes the first of these two proofs as standing "firmly in the tradition of the finest Archimedean convergence arguments" [1982, 53]. Three elements are particularly emphasized: the division of the beam into equal weights by parallel sectioning, the procedure of distributing an assembly of equal weights at equal intervals and then aggregating them at the midpoint of the whole interval, and, finally, the application of the Archimedean axiom in an indirect proof of the exhaustion type. He states: "we find among known Archimedean works several places which provide exact models for portions of *K. Qar*. VI [i.e., theorem 4—SB]" [1982, 53]. Indeed, as already pointed out by Jaouiche and acknowledged by Knorr, the proof of theorem 4 possesses clear similarities with methods and arguments made by Archimedes in his *Quadrature of the Parabola* and in *Plane Equilibria I* [see Jaouiche 1976, 94–101; Knorr 1982, 53n6]. Knorr does not take this immediately as a proof of Archimedes' authorship of this proof. He acknowledges that the two texts are not known to have been translated into Arabic. This was the belief commonly shared in 1982. He also discovers subtle differences between the extant Archimedean texts and their procedures and the proof of theorem 4 [1982, 54–55]. He concludes:

> All these observations thus compel us to recognize the author of the Arabic *K. Qr*. VI [i.e., theorem 4—SB] as a master of the application of formal geometric techniques in the analysis of mechanical theorems. [Knorr 1982, 55]

But Knorr sees himself faced with a major conundrum when comparing the proof of this theorem with that of theorem 5: "This impression, as we have seen, is yet utterly belied by the uninspired treatment of the Arabic *K. Qar*. VIII [i.e., theorem 5—SB] [1982, 55]." In his analysis as well as later comments, he labels the proof of theorem 5 with a string of very negative terms, the denigrating force of which he reinforces several times by adding qualifiers such as "lamentably uninspired" or "remarkably inept" [cf. 1982, 55, 31–33, 37, 53, 55].

I will give examples of the sort of language that Knorr chooses in evaluating theorems 4 and 5 by several quotes because they elucidate his prejudices and their impact on his analysis. I begin with a quotation concerning theorem 5:

> In the Arabic version the proof of this rule [i.e., the rule for the calculation of the counterweight—SB] is clumsy and confused.... This outline [of the proof—SB] only begins to suggest the labored line of this proof. Each step, however patent, is justified in detail. Yet the essential idea—that the weight F equals $W_a - W_b$ so that the extended portion $A - B$ can be replaced by F suspended at its midpoint—is virtually submerged in a flood of trivia. The wonder is that this proof, so inexpertly conceived, should still be quite correct and that the text has suffered not even a single scribal error.
>
> But if the essential line of the proof is here obscure, nevertheless the author's procedure is entirely clear. He has constructed the proof by working backward from the formula. This is in striking contrast to the approach in *L. Can*. and the Beirut appendix which derive the rule from the two or three essential aspects of the problem. It would thus appear that the author of the Arabic proof had before him a statement of the computational rule without its proof and set out to verify it by means of a safe, "brute-force" method. While such inelegance can be found in Thābit's work, one still begins to doubt that he could have been responsible for such an ill-framed method. [Knorr 1982, 33]

Knorr defends his claim that Thābit's mathematical methods were "inelegant" with the following comment:

> In his treatment of the quadrature of the parabola, for instance, Thābit plods through fifteen lemmas on arithmetic summations before coming to the properties of the parabola. The determination of the area takes five propositions.... So inelegant was Thābit's method that his grandson took up the problem for his family's name's sake to devise a better proof.... Ibrāhīm clears up the whole matter in four propositions. By a comparable method Archimedes had required two lemmas on the parabola, one on summation and four propositions. [Knorr 1982, 33n3]

Knorr is not bothered by the fact that despite its lengthiness Thābit's determination of the area of the parabola is of a much higher degree of difficulty and complexity than the proof of theorem 5, which indeed is simple but neither "inept" nor "ill-framed". Knorr's comparative claim of "inelegance" is misplaced. Given Knorr's own mathematical skills, his evaluations of Thābit's mathematical skills in his treatise on the quadrature of the parabola and of the character of the proof of theorem 5 are hardly accidental. They are either an intentional misrepresentation of the respective degrees of difficulty or mere sloppiness.

There are other cases of lack of care, at times serious ones, in Knorr's book: for instance, when he argues for the ancestry of the *Liber de canonio* and the appendix to the Beirut manuscript in relation to the *Kitāb al-qarasṭūn* on grounds of the content of the first two texts. I will return to this below. For now, I observe that Knorr's assessment of Thābit's mathematical capabilities is hardly compelling, since he does not analyze these capabilities on the basis of *all* of Thābit's extant texts, texts which use, like the proof of theorem 4, methods of exhaustion, partitions, and the axiom attributed to Eudoxus and Archimedes.

The same negative evaluation of the proof of theorem 5 (which he calls *K. Qar*. VIII) appears in Knorr's summary of his (equally false) analysis of the corresponding content in the *Liber karastonis*, the *Liber de canonio*, and the appendix to the Beirut manuscript:

> The rules proposed in *L. Can*. III and in *K. Qar*. VIII are in essence the same. Yet the Arabic proof of *L. Qar*. VII is remarkably inept, apparently the effort by an editor who knew the rule and by proceeding backward from the rule to the givens of the problem attempted in a most cumbersome way to compose its proof. By contrast, the proof adopted in the Latin version, *L. Kar*. VIII, is well framed, much in the manner of *L. Can*. [Knorr 1982, 37]

This denigrating language is repeated in Knorr's account of theorem 4:

> ...the Arabic version [of the proof of theorem 4—SB], for all its length and complexity, is as precise and as tightly conceived as it could be. It is firmly in the tradition of the finest Archimedean convergence arguments. While this has already been recognized by commentators on Thābit's work, the consequent paradoxes have not been appreciated. First, how could the author of the inept proof of the Arabic *K. Qar*. VIII [i.e., theorem 5—SB] have come up with such a profoundly accurate proof in VI [i.e., 4—SB]? Further, if that author were an Arabic scholar, perhaps Thābit, what could his technical model for the proof have been? [Knorr 1982, 53]

This last quotation does not merely show Knorr's contrasting evaluation of the proofs of theorems 4 and 5, it also expresses very clearly his beliefs about the fundamental differences between ancient Greek and medieval Arabic scholars. The author of the proof of theorem 5 is only conceived as an Arabic scholar. There is no reflection on how the analysis might change if a Greek author of this proof is assumed. But if the author of the proof of theorem 4 was perhaps an Arabic speaker, he must, in Knorr's view, have used an ancient Greek, technical model for his work. An independent invention of the proof by an author of the ninth century who wrote in Arabic is apparently unthinkable for Knorr. Again, for Knorr, that the author of the proof of theorem 4 might also have been the author of that of theorem 5 seems unthinkable. This is remarkable because at the end he ascribes the entire text to the young Archimedes. He can do so only by proposing that the Arabic text was derived from an incomplete Greek text and that the extant proof of theorem 5 was produced by its unknown Arabic translator.

This perception of such a substantial difference in quality between the two proofs leads Knorr to the following move, which, like many others of his ideas and arguments, is highly problematic for its patent dependence on biases and falsehoods:

> How are we to account for this radical discrepancy? We appear required to assume two authors for the Arabic *K. Qar*.: the one a geometer of amazing insight, who could draw freely from ancient technical works inaccessible to others in late antiquity and the Middle Ages; the other a competent but pedestrian commentator. While any number of ancient and medieval commentators known to us still were capable of translating a technical text and explaining its difficult points, so fitting the latter description, there is none to name, not even Thābit, who might fit the former. Only a century after Thābit do we come upon this sort of formal but creative geometer. But such a level of expertise seems unlikely as early as the 9th century, the first generation of Arabic scholarship in the formal tradition of geometry. This problem leads us to the view that what we have in the Arabic *K. Qar*. VI [i.e., theorem 4—SB] is the translation from a Greek mechanical writing. It is still a problem to determine who might have produced such a work, since the few writers from the later Hellenistic period who concern themselves with formal geometric methods manifest little originality. We return to this question later.
>
> But given such an ancient work, any one of the Arabic scholars we have named was fully able to produce an accurate translation. This accounts completely for the technical idiosyncrasies of the proof of *K. Qar*. VI [i.e., theorem 4—SB], and also for its length and complexity. An Arabic editor, by contrast, would certainly have striven to produce a simpler and shorter treatment of his own. As for the weaknesses we have detected in *K. Qar*. VIII [i.e., theorem 5—SB], we may recall that the most direct logical order for the treatment of the weighted beam is to establish the replacement theorem (*K. Qar*. VI) [i.e., theorem 4—SB]

> and then use it for the determination of the counterweight, as in *Liber de canonio*. What we have in *K. Qar*. VIII is merely an alternative expression of the result in *L. Can.* III. Presumably, the Greek manuscript containing the proof of the replacement theorem was defective, missing the theorems of *L. Can.*, or possibly bearing them out of place—perhaps as an appendix, as they appear in the Beirut manuscript of the Arabic *K. Qar.*, but still asserting the solution of the counterweight in its alternative form. This would appear as a corollary whose proof would be "obvious" in the context of the complete work, but far from clear within the defective manuscript. The Arabic translator would thus be required to provide his own proof for *K. Qar*. Apparently, the first attempt to produce such a proof, as we have it in the Arabic *K. Qar*. VIII, was correct, but far from perceptive. Faced with this Arabic edition of the *qarasṭūn*, Thābit set out to improve it, revising the theorem on the counterweight (*L. Kar*. VII and VIII) to good effect, but abridging the proof of the replacement theorem (*L. Kar*. VI) in a way that misconstrues a key feature of the argument. [Knorr 1982, 55–56]

This passage is saturated with highly problematic statements that are not grounded in a careful analysis of Arabic texts extant from the ninth century nor formulated in view of what was argued about the mathematical proficiency of scholars like Thābit b. Qurra in the 1970s by other historians of mathematics. It is not clear why Knorr believed that Thābit was incapable of applying formal geometric techniques, since he knew Thābit's text on the parabola [1982, 33]. Despite the fact that he considered this proof "inelegant" because it needed almost thrice as many lemmas and propositions as Archimedes, who needed twice as many as Thābit's grandson, this perceived lack of elegance does not entail that Thābit did not master the design and proof of a correct exhaustion method and the application of the so-called Eudoxus-Archimedes axiom. Seemingly characteristic for Knorr's working practice here is the fact that he did not consider Thābit's three other treatises which use exhaustion methods and the Eudoxus-Archimedes axiom, i.e., Thābit's works on parabolic bodies of revolution, on two lines that meet each other when they include an angle different from a right one, and on the trisection of an angle. Lack of familiarity with Arabic manuscripts does not excuse Knorr's unfriendly evaluation of Thābit's skills as a geometer. These texts namely were known to be extant in MS Paris, BnF, Arabe 2457 long before Knorr's study of the *Kitāb al-qarasṭūn*. He did not even have to work with this collection of texts in manuscript form since the three treatises had been published or studied in Suter 1916–1917, 16–17; al-Dabbagh 1966; and Hogendijk 1981. While Knorr may not have had access to al-Dabbagh's thesis in Russian, he was familiar with the two other works. Hence, at least one comment with an argument to the effect that the methods used by

Thābit in the three works do not warrant praise for their expertise but only disparagement as the work of a "competent but pedestrian commentator" would have been in order. But do the studies of other colleagues support such a negative evaluation of Thābit's treatises and his skills as a geometer?

An investigation of all published mathematical works of Thābit b. Qurra that focuses on his use of the Eudoxus-Archimedes axiom and the method of exhaustion makes it clear that Thābit fully deserves to be recognized for his talent as a geometer. Such an investigation also shows that he used the axiom in most cases and the method in all cases in a different manner than that in the *Kitāb al-qarasṭūn*.[8] The one case of identical usage of the axiom is found in postulate 5 of Archimedes' *On the Sphere and Cylinder* [Sabit ibn Korra 1984, 184; Heiberg and Stamatis 1972–1975, 9], a text which Thābit knew. In the other cases, Thābit uses the axiom in the form of theorem 1 in book 10 of Euclid's *Elements* [Heiberg and Stamatis 1972, 72.2–3]. In contrast to the equidistant partition of the thick segment mounted at a beam, Thābit partitions the diameter of the segment of a parabola or a paraboloid according to the sequence of odd numbers beginning from 1 [Sabit ibn Korra 1984, 184–185, 195].

As for the methods of exhaustion, Thābit uses in his other works variants of what Dijksterhuis has baptized the "method of approximation" [1956, 130–133: cf. Jaouiche 1976, 95]. In the *Kitāb al-qarasṭūn*, the method used is the variant that Dijksterhuis has labeled the "method of compression". Jaouiche [1976, 94–101, 135–137] has argued convincingly that it is the form also found in theorem 16 of Archimedes' *The Quadrature of the Parabola*. This Archimedean work was, however, not translated into Arabic, as far as we know.

This brief survey brings to light that the technical elements of the method of exhaustion in Thābit's published mathematical works differ from the method in the *Kitāb al-qarasṭūn*. Hence, one may conclude that there is no direct, immediate link between these two methods and the aforementioned texts. Thus, the conclusion to be drawn is that, given the existence of other partitioning methods in Thābit's works as well as another form of the Eudoxus-Archimedes axiom and his use of the method of approximation, Thābit was a competent geometer who was capable of working with advanced concepts and methods which are known to us, but perhaps

[8] Sabit ibn Korra 1984, 70, 149, 184–185, 195, 239–240, 334n7, 342n21, 343n32, 343–347n3, 345nn61–63, 348n85, 351–353n13, 353n15.

not to him, from Archimedes' works. If the information about a text on centers of gravity ascribed explicitly to Archimedes in works of the 10th century is correct, and so far there is no reason to doubt its veracity, we may suppose that Thābit may have known Archimedes' *Plane Equilibria*. But this would mean that he was capable of understanding Archimedean reasoning and techniques, and applying them to new problems. Rozenfel'd, in his evaluation of the first two works, goes beyond this: as he sees it, Thābit's work on paraboloids demonstrates that in comparison to Archimedes' *On Conoids and Spheroids* (theorem 22) Thābit "solved the more complicated problems of determining the volumes of cupola with straightened and indented cusps" [Sabit ibn Korra 1984, 344–345]. Since Knorr confirms his familiarity with Jushkevitch's *Les mathématiques arabes*, his downplaying of Thābit's geometrical skills without any discussion of this counter-evidence is inappropriate at best.

But did Thābit apply Archimedean methods independently to the problem discussed in theorem 4 as Jaouiche surmised? This seems unlikely, not because of any doubt about Thābit's mathematical abilities but because, as a philological analysis shows, there are a few Graecisms in this theorem. It is thus possible that Thābit worked with an unknown or as of yet undetermined Greek text on the balance in Arabic translation. The limited number of such Graecisms and the lack of mistakes in the Arabic text suggest that Thābit might also have edited this Arabic translation. Analysis of the Arabic theorem 4 does not, however, support Knorr's speculation that an Arabic translator was responsible for the length and complexity of the proof as well as those features perceived by Knorr as idiosyncrasies. Knorr's other speculation concerning the role of the anonymous Arabic translator in completing a fragmentary proof of theorem 4 and invention of the proof of the rule for the counterweight (theorem 5) is equally unfounded. The extant text of the *Kitāb al-qarasṭūn* does not provide any evidence for it. On the contrary: there are components in both proofs that connect them with each other and suggest that they derive from the work of a Greek scholar. In addition, the proof of theorem 5 shows traces of Thābit's interference, while a comparison of this proof with the corresponding theorems 7 and 8 in the *Liber karastonis* indicates that Thabit's willingness to alter the text of the translated Greek fragments was very limited when he compiled the *Kitāb al-qarasṭūn*.

Two elements connect the proofs of theorems 4 and 5:

(1) the repeated physical arguments in Aristotelian language, and
(2) the use of theorem 4 in the proof of theorem 5.

Knorr overlooked (1) and seems to deny (2) in the quotation given above [see p. 155], where he highlights the use of theorem 4 only for the *Liber de canonio*. In all likelihood, Thābit did not introduce the physical arguments into the proof since they disappear completely in the *Liber karastonis*. While this observation is of limited use in the case of theorem 4 because it is unclear who the author was of the proof in the extant form of the *Liber karastonis*, it applies to theorem 5 and its two corresponding theorems 7 and 8 in the *Liber karastonis*. In effect, Thābit strengthened the purely geometrical character of the treatment of the steelyard when he transformed the *Kitāb al-qarasṭūn* into the Arabic text of the *Liber karastonis*. In this transformation, he almost completely eliminated any physical argument. If this observation based on the comparison of the two texts reflects correctly Thābit's conceptual goals, then it is not very likely that he would have introduced the prominent physical arguments in the proofs of theorems 4 and 5. It is more plausible to assume that they were a genuine part of the Greek ancestor text of the two theorems. If this is a correct evaluation of the two theorems, then this shared peculiar feature of the two proofs speaks for one author of both. Whether this author was the inventor of the two proofs or an editor of two proofs invented by two different scholars cannot be decided in the absence of good evidence. The fact, however, that the goal of theorems 3–5 consists in determining the quantity of the counterweight needed for balancing a material beam implies rather one inventor than two of the two latter theorems.

Knorr's evaluation of the proof of theorem 5 rests on three claims that are evident in the various quotations that I have adduced:

(1) This proof is so simple that it cannot be part of an Archimedean heritage.

(2) The theorems found in the *Liber de canonio* were originally part of a single Greek text that also contained theorems 3 and 4. (This is another of Knorr's false conclusions, as I will show.)

(3) Someone else created the proof of theorem 5: an inept, pedantic scholar who encountered the rule for the counterweight without a proof. Although Knorr does not say this explicitly, his argumentation makes it clear that in his view the proofs of theorems 4 and 5 could not have had one and the same author. He oscillates between ascribing this proof to Thābit, the anonymous translator into Arabic, and some other unspecified Arabic author.

To address these claims, I will begin with the fact that Knorr, following Jaouiche [1976, 166–169], has misunderstood and misrepresents the simple proof of theorem 5.[9] This proof may be summarized as follows, with L signifying length and W or w, weight; z and h, auxiliary quantities; and H, the counterweight (see Figure 3 on p. 152):

> Material beam ab, suspended at point g, part $gb >$ part ag. Cut off ag from gb; this is bd; $L_{bd} \times W_{ab}$. Let the result be h. Let $h : L_{ab}$ be w; let $w \times L_{ab}$ be z; let $z : 2L_{ga}$ be H.
> I say: H is the magnitude of the heavy body that, if it is suspended in point a, balances the weight of the beam parallel to the horizon.
> $h = L_{bd} \times W_{ab}$ and also $h = L_{ab} \times w$
> [MS Mq 559, f 223v.12]: because $h : L_{ab} = w$
> $L_{bd} : L_{ab} = w : W_{ab}$,
> $w = W_{bd}$.
> But this is so because the thickness of the segment bd of beam ab together with all the beam is equal among each other and the substance of the whole is one. Hence, the heaviness of all of its parts (*ajzāʾ*) is equal among each other [i.e., the weight of each part is the same—SB].
> Also $z = w \times L_{ab} = H \times 2L_{ag}$
> and $z = w \times L_{ab} = H \times 2L_{ag}$ because $z : 2L_{ag} = H$.
> $H : w = L_{ab} : 2L_{ag}$
> bisect bd at point T.
> $L_{gT} = ½\, L_{ab}$, because $ag = gd$
> $L_{Tg} : L_{ag} = L_{ab} : 2L_{ag}$.
> But we had explained that $L_{ab} : 2L_{ag} = H : w$.
> $L_{Tg} : L_{ag} = H : w$.
> If we now imagine that H is a weight suspended at point a and if k is a heavy body with weight w suspended at point T and we imagine ab as a straight line or as a straight beam without weight, so that the heaviness of H counterbalances the heaviness of k, then the weight of beam ab is balanced parallel to the horizon given the preceding fundamental statement (*aṣl*) [i.e., theorem 4—SB].
> But w, as we explained, is the weight of segment bd of the beam, if we gave weight to the beam ab. The heaviness of the beam's segment bd, if we imagined it [i.e., the segment—SB] suspended in point T, so that it counterbalances the heaviness of H suspended at point a, will equilibrate the weight of beam ab parallel to the horizon.
> Likewise, we also imagine it [i.e., the segment bd—SB] spread out and expanded in evenness and connectedness in its attachment between the two points b, d. It is clear that segment gd, (which is) also part of the beam, counterbalances segment ag of it because the two are equal to each other in length and thickness and substance and in sum are equal to each other in weight.

[9] See MS Cracov, Jagielonska University Library, Mq 559, ff. 223r.10–224r.15.

> The entire part gb thus counterbalances the beam ag and the weight H. Hence, the weight of beam ab will be parallel to the horizon. QED

This summary contradicts clearly Knorr's claim that

> (e)ach step, however patent, is justified in detail. Yet the essential idea—that the weight F [i.e., w—SB] equals $W_a - W_b$ [i.e., W_{bd} —SB] so that the extended portion $A - B$ [i.e., bd —SB] can be replaced by F suspended at its midpoint—is virtually submerged in a flood of trivia. [Knorr 1982, 33]

There is no "flood of trivia" but merely two auxiliary quantities h and z, which structure the proof neatly and thus look like didactic devices, and three explanatory statements that repeat things as given in the exemplum, one of which is repeated once. Neither should one call these very short justifications of the type "because $x = y$" detailed; nor is the "essential idea" "obscured", as Knorr would have it, since it is explicitly stated in the passage "But w, as we explained, is…". Having wavered above in my description of Knorr's evaluation of Thābit's mathematical skills as either sloppiness or intentional denigration, I think that his excessively negative evaluation of the proof of theorem 5 is intentionally misleading. The proof of theorem 5 is simple, no doubt, except for two points—the use of theorem 4, which at least the inventor of the proof fully understood and who is thus not rightly described "inept", and the use of physical theory in order to make the transition from the immaterial beam as proved in theorem 4 to the material beam discussed in theorem 5. But even in its simple parts, theorem 5 is well structured in that it uses the didactic device of auxiliary quantities and is to the point.

Knorr's strong condemnation of the proof of theorem 5 was predicated on a misunderstanding of several of its elements. He did not recognize the didactic device, which explains an apparently absurd feature in the formulation of the rule, namely, the immediate sequence of a multiplication and a division by the same quantity. Neither did he see that the proof's claim "But we had explained that $L_{ab} : 2L_{ag} = H : w$" is actually false. The proof does not explain why the factor $L_{ab} : 2L_{ag}$ is correct for obtaining H from w. It merely justifies the product $w \times L_{ab} = H \times 2L_{ag}$ with a reference to the labels provided in the exemplum. This lack of a true justification of the definition of the counterweight is something Thābit apparently chose not to correct in compiling the various fragments that constitute the *Kitāb al-qarasṭūn*. But he remedied this mistake later by introducing a new theorem in his text extant today as the *Liber karastonis*, namely, theorem 7. There are other elements in this proof that Knorr misunderstood but I will abstain from discussing them too in order to focus more closely on Knorr's claims about the dependence of the *Kitāb al-qarasṭūn* on the *Liber de canonio*.

6. On the relation of the *Kitāb al-qarasṭūn* and the *Liber de canonio*

Knorr's claim that theorem 5 (rule and proof) is derived from theorem 3 in the *Liber de canonio* is also false. First, the forms of the rule as expressed in these two texts as well as in the appendix to the Beirut manuscript do not agree, contrary to what Knorr suggests [see p. 163, below]. Second, theorem 3 of the *Liber de canonio* is proved with explicit references to axioms and theorems in Euclid's *Elements* because it works with similar triangles. It is, thus, on a higher level of mathematical complexity than the proof of theorem 5. All physical arguments of the proof of theorem 5 are missing in theorem 3 of the *Liber de canonio* as is the didactic device of theorem 5. Third, the *Liber de canonio* splits the rule proved as a package in theorem 5 into two parts. Theorem 1 proves the proportion for the weight of the material segment *bd*, while theorem 3 deals with the proof of the proportion for the counterweight. Moreover, theorem 3 justifies in its first part this proportion with the help of similar triangles and so avoids, or perhaps repairs, the mistake of theorem 5 of the *Kitāb al-qarasṭūn*. Hence, it makes no sense to assume that theorem 5 (rule and proof) was designed on the basis of the *Liber de canonio*.

It is difficult to understand what motivated Knorr to make such an ill-considered claim, if not his desire to understand these texts as remnants of one and the same ancient Greek source composed by the young Archimedes. That this is not another instance of sloppiness can be seen in the manner in which Knorr rewrites the rule for the counterweight according to theorem 5, the appendix to the Beirut manuscript, and the *Liber de canonio*. The resulting statements are equivalent to, but different from, their original forms in the three texts.

Knorr's translation of the prescription for the counterweight

Be the material beam divided into two segments a and b. Then L and W with their indices denote the length and weight of the respective segments. W without an index labels the counterweight.

Kitāb al-qarasṭūn

$W = (L_a - L_b) \times (W_a + W_b) : (L_a + L_b) \times (L_a + L_b) : 2L_b$ [1982, 31]

Addition (*ziyāda*) after the text in MS Beirut

$W = 1/2 (L_a + L_b) \times (W_a - W_b) : L_b$ [1982 18]

Liber de canonio

$W = (W_a - W_b) \times (L_a + L_b) : 2L_b$ [1982 18]

I will now give a literal presentation of this rule in the three texts. The letters "N" and "P" stand for "numerus" and "productus", both belonging to the set of Arabisms of the *Liber de canonio*.

Prescription of the counterweight as expressed in the three source texts

Kitāb al-qarasṭūn
$W = (L_a - L_b) \times W_{ab} : L_{ab} \times L_{ab} : 2L_b$ [Jaouiche 1976, 166–167]
ziyāda, MS Beirut
$W = ½\, L_{ab} \times W_{a-b} : L_b$ [Knorr 1982, 160]
Liber de canonio
$(W_a - W_b) \times N\{L_{ab}\} = P$ and $P : N\{L_{2b}\} = N\{W\}$ [Moody and Clagett 1952, 68–69]

The comparison between these two sets of formulas shows that the formulations in the three texts contain no additions due to their different labelling of the various parts of the steelyard. Furthermore, it shows that the *Kitāb al-qarasṭūn* is recognizably distant from the two variants in the *ziyāda* to the Beirut version and the *Liber de canonio*, while the latter two show structural similarities without being identical. Knorr's idea that the variant in the *Kitāb al-qarasṭūn* was derived from an Arabic version of the *Liber de canonio* is thus plainly unwarranted.

7. Knorr's lack of precision

Cases of a clear lack of care in Knorr's analysis appear always when he speaks of literal coincidence between parts of different texts. The example that I have chosen to back up this judgment is closely connected to the discussion of the proof of theorem 5 and its relationship to the *Liber de canonio*. It deals with the relation between this Latin text and the Arabic *ziyāda* to the *Kitāb al-qarasṭūn* in the Beirut manuscript. Knorr suggested that the *ziyāda* was derived from a larger Greek text, a text which, according to him, was also the source of the theorems found in the *Liber de canonio*. His first argument rests on a putative "literal coincidence" of the enunciations of theorems 1–3 of the *Liber de canonio* and the last two theorems and the corollary to proposition 4 (3b) of the *ziyāda* [1982, 15–17]. This claim is, however, far too grand. While the enunciations describe the same content and so do indeed possess shared features, they are not in literal agreement.

Knorr's second argument states that "(w)hile the proofs do not agree literally as texts, their arguments are the same, step for step in the same order" [1982,15–16]. This too is too grand a claim, since it obliterates important differences between the proofs. In my discussion of Knorr's concept of "literal coincidence”, I will consider only the enunciations of the different theorems since this is Knorr’s point of reference.

7.1 *Relation of the* Liber de canonio *and the* ziyāda

Study 1

Theorem 1, *Liber de canonio*

> *Si fuerit canonium symmetrum magnitudine, et substantie eiusdem, et dividatur in duas partes inequales et suspendatur in termino minoris portionis pondus quod faciat canonium parallelum epipedo orizontis, proportio ponderis illius ad superhabundatiam ponderis maioris portionis canonii ad minorem, est sicut proportio longitudinis totius canonii ad duplam longitudinis minoris portionis.* [Moody and Clagett 1952, 64]
>
> If there is a beam of uniform magnitude and of the same substance, and if it is divided into two unequal parts, and if at the end of the shorter segment there is suspended a weight which holds the beam parallel to the plane of the horizon, then the ratio of that weight, to the excess of the weight of the longer segment of the beam over the weight of the shorter segment, is as the ratio of the length of the whole beam to twice the length of the shorter segment. [Moody and Clagett 1952, 65][10]

Theorem 3 of the *ziyāda*, Beirut

> اذا كان عمود متساوي الغلظ متشابه الجوهر وقسم بقسمين مختلفين وعلق من طرف القسم الاقصر ثقل فاعتدل العمود على موازاة الافق فانه تكون نسبته الى ثقل فضل القسم الاطول على القسم الاقصر كنسبة نصف طول العمود جميعه الى طول القسم الاقصر.
>
> [Knorr 1982, 146]
>
> If there is a beam, (which is) equal in itself in thickness, equal in itself in substance, and it is partitioned in two different parts and a weight is suspended at the end of the shorter part so that it balances the beam in parallel to the horizon, then its ratio to the weight of the surplus of the longer part over the shorter part is like the ratio of half of the length of the beam in its entirety to the length of the shorter part.

These two enunciations represent the same content. They are closely related but not identical. They differ in their statement of the second part of the proportion and they show some differences in language. The *ziyāda* does not speak of magnitude but thickness. It uses a second term, « mutashābih », for

[10] Moody and Clagett translate the Latin “minor” and “maior” by “shorter” and “longer”. A literal translation would be “smaller” and “greater” or “larger”.

describing the property of the substance, which is not present in the Latin text. Instead of saying that the weight suspended at the end of the shorter part of the beam makes the beam parallel to the place of the horizon, it prescribes that it is of such a kind that the beam balances itself in parallel to the horizon. In view of my earlier argument on the Arabisms in the second part of the *Liber de canonio*, let me point out here that it is only the Arabic text that speaks of *shorter* and *longer* parts of the beam. The Latin text speaks of *smaller* and *greater* or *larger* parts. Unfortunately, in his translation, Knorr obliterates this important terminological difference. He chose to translate "shorter" by "smaller" and "longer" by "greater", thus following Moody and Clagett [1982, 139, 141]. He does the same in the remaining theorems [cf. 1982, 143, 147, 149, 153, 155, 159, 161].

The two terms «mutasāwin» and «mutashābih» used for describing the quality of the beam in terms of thickness and matter mean both "equal" and "similar" in Arabic. There is a clear preference in Arabic mathematical text for using the first for equal and the second for similar. Thus, Knorr translated them in this manner [1982, 139]. In the given context, it is clear though that similarity is not meant literally but in the sense of having the same property. This ambiguity reflects the use of «ἴσος» and «ὁμοίος» for respective terms in Greek. It is, however, useful to remember that the *Kitāb al-qarasṭūn* does not use «mutashābih» or its verb at all in the sense meant here, i.e., for equality or evenness, but exclusively in the sense of "similar" [Jaouiche 1976, 146, 148]. Neither does the *Liber karastonis* [see Moody and Clagett 1952, 108, 110, 112].

Study 2

Theorem 2, *Liber de canonio*

Si fuerit proportio ponderis in termino minoris portionis suspensi, ad superhabundantiam ponderis maioris portionis ad minorem, sicut proportio longitudinis totius canonii ad duplam longitudinis minoris portionis, erit canonium parallelum epipedo orizontis. [Moody and Clagett 1952, 66]

If the ratio of the weight suspended at the end of the shorter segment, to the excess of the weight of the longer segment to the weight of the shorter one, is as the ratio of the length of the whole beam to twice the length of the shorter arm, then the beam will hold parallel to the plane of the horizon. [Moody and Clagett 1952, 67][11]

[11] The second mention of weight in the second term of the proportion is supplied by Moody and Clagett. The Latin text does not have it.

Theorem 4, *ziyāda*, Beirut

وجعلت نسبة الثقل الى ثقل فضل القسم الاطول على ثقل القسم الاقصر كنسبة نصف طول العمود كله الى طول القسم الاقصر فان العمود يعتدل على موازاة الافق.

[Knorr 1982, 154]

If there is a beam, (which is) equal in itself in thickness, equal in itself in substance and partitioned in two different parts and (if) a weight is suspended at the end of the shorter part and the ratio of the weight to the weight of the surplus of the longer part over the weight of the shorter part is made like the ratio of half of the length of all of the beam to the length of the shorter part, then the beam equilibrates itself in parallel to the horizon.

Again, the content of both theorems is the same and the two enunciations are similar but not identical. Their difference is greater than in the previous case because the *Liber de canonio* does not repeat the description of the properties of the beam and the suspended weight, and so has to integrate the latter into the description of the proportion. It differs from the *ziyāda* also in regard to the placement of the term "weight" in the description of the second term of the proportion. The *Liber de canonio* uses the term only once, that is, after the surplus and before the longer part. The *ziyāda* uses it twice, once before the surplus and once before the shorter part. While the formulation in the *Liber de canonio* is imprecise but comprehensible, the formulation of the *ziyāda* is comprehensible but false. It is most likely the result of a scribal error as may be the sloppy form of the *Liber de canonio*. Again, it is only the Arabic text that uses "shorter" and "longer", while the Latin text works with "smaller" and "greater" or "larger".

Study 3

Theorem 3, *Liber de canonio*

Atque ex hoc manifestum est, quoniam si fuerit canonium symmetrum in magnitudine et substantie eiusdem, notum longitudine et pondere, et dividatur in duas partes inequales datas, tamen possible est nobis invenire pondus quod, cum suspensum fuerit a termino minoris portionis, faciet canonium parallelum epipedo orizontis. [Moody and Clagett 1952, 68]

But from this it is evident that if there is a beam, symmetrical in magnitude and of uniform substance, whose length and weight are known, and which is divided into two given unequal parts, it is still possible for us to find the weight which, when suspended from the end of the shorter segment, will make the beam hold parallel to the plane of the horizon. [Moody and Clagett 1952, 69]

Porism, *ziyāda*, Beirut

وهنالك استبان انه اذا كان عمود متساوي الغلظ متشابه الجوهر يقسم بقسمين مختلفين وننقص من القسم الاطول مثل القسم الاقصر ويضرب نصف طول العمود في وزن ثقل فضل القسم الاطول على القسم الاقصر وقسم ما اجتمع على طول القسم الاقصر فان ما خرج من القسمة يكون ثقلا اذا علق بنقطة طرف الاقصر اعتدل العمود على موازاة الافق.

[Knorr 1982, 160]

And herewith it is clarified that if a beam, (which is) equal in itself in thickness, equal in itself in substance, is partitioned in two different parts and we take away from the longer part the same as the shorter part and half of the length of the beam is multiplied by the weight («wazn») of the weight («thiql») of the surplus of the longer part over the shorter part and that what results is divided by the length of the shorter part, then that what comes out from the division is a weight («thiql») [that], if it is suspended in the point at the end of the shorter [part], balances the beam in parallel to the horizon.[12]

The main difference between these two propositions is caused by their different format. The Latin statement presents the task in the form of a problem. The prescription of how to determine the weight sought follows afterwards. The Arabic statement is formulated as a porism and so consists of the prescription of how to find this weight. This difference signals clearly that the Latin text belongs in genetic terms to a later developmental stage than the Arabic text. In order to evaluate the overall relationship in language, we must consider the statement of the prescription as given in the *Liber de canonio*.

Statement of theorem 3 of the *Liber de canonio*

Hoc est, ut sumamus superhabundantium ponderis maioris portionis ad minorem, et multiplicemus eam in numerum longitudinis totius canonii, et productum dividamus per numerum longitudinis duple minoris portionis, et quod exierit est numerus ponderis quod, suspensum a termino minoris portionis, faciet canonium parallelum epipedo orizontis. [Moody and Clagett 1952, 68]

The method is to take the excess of the weight of the longer segment over that of the shorter, and to multiply this by the number representing the length of the whole beam, and then to divide this product by the number representing twice the length of the shorter arm; and what results is the number representing the

[12] The Arabic text printed by Knorr has a few minor, probably scribal, errors: «wazn» before «thiql», the shift in tense and person between the verbs.

> weight which, if suspended from the end of the shorter arm, will make the beam hold parallel to the plane of the horizon. [Moody and Clagett 1952, 69]

Four points come to light when comparing these two passages from theorem 3 in the *Liber de canonio* with the Arabic porism.

(1) There is the small difference of the numerical factor used by the two (2 in the denominator *versus* ½ in the numerator) and the order of the two terms at the beginning of the prescription is changed.
(2) Of more substance is the addition of "numerus" in the Latin text, since this is conceptually improper.
(3) The repeated use of "symmetrum in magnitudine" (commensurable in magnitude where size is at issue) in the Latin text cannot be found in the Arabic version, which regularly uses « mutasāwī l-ghilaẓ » (equal/even in thickness). In contrast, the use of « mutashābih al-jawhar » (literally: equal/similar in substance) is not precisely reflected in the Latin formulation of the example but can be found elsewhere in the *Liber de canonio*.
(4) There is the probably insignificant difference between the two texts regarding the standard concept of the plane of the horizon *versus* the horizon *simpliciter* and the perhaps slightly more important difference in the verb used for expressing the parallelism, i.e., "facere" as opposed to « iʿtadala ».

These four points appear to be of minor relevance when compared to the philological coincidence of the two texts which is clearly visible despite their formulaic differences as a problem and a porism. But the differences listed confirm what can be easily discovered by comparing the proofs, namely, that none of the two texts is a translation of the other.

In sum, in the light of our studies of the relations between the enunciations of the theorems in the *Liber de canonio* and the *ziyāda* of the Beirut manuscript, Knorr's claim of their "literal coincidence" is clearly too strong or, as one says in German, "The wish was the father of the thought." I accordingly regard Knorr's claim as an instance of a lack of care in carrying out his analyses.

8. Instead of conclusions

Here is not the place to identify in further detail the steps that contributed to Knorr's misconstruals time and again of the *Kitāb al-qarasṭūn*, its scholia, the *ziyāda* in the Beirut manuscript, and the *Liber de canonio* as bits and pieces of a single, coherent, ancient Greek text on the steelyard, whose author was, in Knorr's view, none other than the young Archimedes. Still, I hope that I have illuminated the dangers that arise from interpreting *any* text, whether highly technical or more narrative, without carefully exploring its content as well as its various contexts. Additional difficulties impeding an analysis that does justice to the extant textual and other material arise from the biases that typically guide our own perceptions of language, images, and values. A third type of problem results from the limitations of our own philological, scientific, mathematical, philosophical, and historical skills and knowledge. Humility is always the better path to truth than hubris in the case of a mathematical text or to a well-balanced evaluation in the case of any other type of text, because, as we all know, even here pride comes before a fall. In short, self-critical control is not only needed in regard to our beliefs and convictions but also towards our own scholarly abilities.

In consequence, for example, to date there is available no truly micro-historical study of any subject matter in the history of the sciences in any Islamicate society. Today, many historians of science or philosophy in Islamicate societies feel compelled to situate their topics much more explicitly in a chain of predecessors or even in a chain of predecessors and successors. This practice applies primarily to scholars and topics from the classical period of Islamicate societies, i.e., to the time before *circa* 1200. The price paid for this is akin in principle to that paid by Knorr, where his attention to the contemporaries of a scholar is visibly less than that to the scholar's predecessors and successors. This is not to say that it cannot be worthwhile to study the place of a scholar in some chain of ideas. But this always entails a substantial loss of insight into the intellectual environment of the scholar studied if such contextual considerations are not also taken into account.

Such work has to face a series of immensely more difficult questions:

- How can we recover information about the intentions, purposes, goals, or values of historical scholars?
- How can we unveil or penetrate the views that different groups of people held on the sciences of their times and then proceed to determine whether these groups engaged one another in a supportive or hostile manner and what that meant for other groups in their environment or their society at large?
- How can we move from such local studies to understanding the regional or even the bigger picture?

But these will have to wait, at least in historical studies of science in Islamicate societies, until we have a series of well-researched micro-histories of a broad range of topics at our disposal and learn which questions we need to ask beyond the clarification of authorship and which methods and theoretical fundamentals we need to develop. The one central point that my analysis of Knorr's book as well as the group of texts that he had studied brings to the fore, in addition to those three which I have discussed (issues of concepts, methodologies, and methods; issues of expertise and its lack; issues of beliefs and assumptions), consists in the insight that the narrowly defined set of questions that Knorr studied in order to produce a history of the steelyard in antiquity and the Middle Ages does not suffice for reaching this goal. At the very best, it is a preliminary preparation of the ground from which to start. Many other questions need to be raised and serious efforts made to answer them. Among them, contextual issues will be of primary importance.

In the present case, these contextual issues will concern the transfer and potential transformation of the material steelyard from Byzantine times to the Umayyad and then to the Abbasid dynasties. Unfortunately, only two specimens seem to be known from either of these two periods. More material is available for weights. Hence, we need ideas about how to link the study of weights and their specific properties to the study of the steelyard. We will also require a better knowledge of the development of long-distance trade in the Abbasid Empire, the emergence of merchant communities and their impact on Abbasid trade policies as well as scholarly patronage. We need to try, following studies in other areas of the history of science, medicine, or technology in other pre-modern societies, to understand what issues of authorship meant to scholars in the ninth or any other century and which functions the category or title of author had for the production of texts, the teaching of the sciences, or the pursuit of a successful career in the administration, at an educational institution, or at court.

There are many more questions that we need to address in studying scientific and other texts. This article certainly is not the place to formulate more of them, let alone most of them, except for one, since the *Kitāb al-qarasṭūn* contains one explicit statement pointing in this direction: How did scholars of the Greco-Arabic sciences and the practitioners of more practical domains of knowledge such as surveyors or calculators relate to each other and communicate with one another? Was there a spillover between these two spheres of knowledge? It seems to me that we are now poised not only to raise these kinds of questions but also to revise our concepts of what knowledge meant in the classical period of Islamicate societies and thus to question any facile belief in the dominance of Greco-Arabic theoretical knowledge over all other forms of knowledge.

BIBLIOGRAPHY

Manuscripts

MS Beirut, St Joseph University, 223.

MS Cracov, Jagielonska University Library, Mq 559.

MS Florence, Biblioteca Laurenziana, Or. 118.

MS London, British Library, India Office Library, 461.

Secondary sources

Ahlwardt, W. 1893. *Verzeichnis der Arabischen Handschriften*. Bd. 5: Ethik, Naturwissenschaften, Medizin 5387–6456. Die Handschriften-Verzeichnisse der Königlichen Bibliothek zu Berlin. Bd. 17. Berlin.

al-Dabbagh, J. al-D. 1966. *Razvitie infinitezimal'nyh metodov na srednevekovom arabskom Vostoke*. Moscow.

Brentjes, S. and J. Renn. 2016. "A Re-Evaluation of the *Liber de Canonio*". Pp. 119–149 in P. Caye, R. Nanni, and P. D. Napolitani edd. *Scienze e rappresentationi saggi in honore di Pierre Souffrin. Atti del convegno internazionale Vinci, Biblioteca Leonardiana, 26–29 Settembre 2012*. Florence.

Busard, H. L. L. and M. Folkerts. 1992. *Robert of Chester's Redaction of Euclid's* Elements*, The So-Called Adelard II Version*. 2 vols. Basel.

Charette, F. 1995. *Orientalisme et histoire des sciences. L'historiographie européenne des sciences islamiques et hindoues, 1784–1900. Mémoire présentée à la Faculté des études supérieures en vue d'obtenir du grade de Maître ès arts (M.A.) en histoire*. Montreal.

Dijksterhuis, E. J. 1956. *Archimedes*. Copenhagen.

Dodge, B. 1970. *The Fihrist of Ibn al-Nadim*. 2 vols. New York.

Duhem, P. 1905. *Les origines de la statique*. 2 vols. Paris.

Fischer, W. 1972. *Grammatik des klassischen Arabisch*. Wiesbaden.

Gacek, A. 2005. *Arabic Manuscripts: A Vademecum for Readers*. Handbook of Oriental Studies: Series 1. The Near and Middle East 98. Leiden.

Heiberg, J. L. and E. S. Stamatis. 1972. edd. *Euclides. Elementa*. vol. 3. Leipzig.

——— 1972–1975. edd. *Archimedis opera omnia*. 3 vols. Stuttgart.

Hogendijk, J. 1981. "How Trisections of the Angle were Transmitted from Greek to Islamic Geometry". *Historia Mathematica* 8: 417–438.

Jackson, D. E. P. 1970. *The Arabic Version of the Mathematical Collection of Pappus Alexandrinus Book VIII: A Critical Edition*. PhD dissertation, University of Cambridge, Cambridge, UK.

Jaouiche, K. 1976. *Le livre du Qarasṭūn de Ṯābit ibn Qurra*. Paris.

Knorr, W. R. 1982. *Ancient Sources of the Medieval Tradition of Mechanics: Greek, Arabic and Latin Studies of the Balance*. *Supplemento agli Annali dell'Instituto e Museo di Storia della Scienza* 2.

—— 1996. "The Wrong Text of Euclid: On Heiberg's Text and Its Alternatives". *Centaurus* 38: 208–276.

Lorch, R. 2008. *Thābit ibn Qurra: On the Sector-Figure and Related Texts*. Augsburg.

Moody, E. A. and M. Clagett. 1952. *The Medieval Science of Weights*. Madison, WI.

Rommevaux, S.; A. Djebbar and B. Vitrac. 2001. "Remarques sur l'histoire du texte des *Éléments* d'Euclide". *Archive for History of Exact Sciences* 55: 221–295.

Sabit ibn Korra. 1984. *Matematicheskie traktaty*. Vol. 8 in B. A. Rozenfel'd, and A. P. Jushkevich edd. *Nauchnoe Nasledstvo*. Moscow.

Sabra, A. I. 1987. "The Appropriation and Subsequent Naturalization of Greek Science in Medieval Islam: A Preliminary Statement". *History of Science* 25:223–243. Repr. in pp. 3–27 of F. Jamil Ragep and S. P. Ragep edd. *Tradition, Transmission, Transformation. Proceedings of Two Conferences on Pre-Modern Science held at the University of Oklahoma*. Leiden/New York/Cologne, 1996.

Suter, H. 1916–1917. "Über die Ausmessung der Parabel von Thâbit b. Kurra al-Harrânî". *Sitzungsberichte der physikalisch-medizinischen Sozietät zu Erlangen* 48–49, 65–86.

van Ess, J. 1991–1997. *Theologie und Gesellschaft im 2. und 3. Jahrhundert Hidschra. Eine Geschichte des religiösen Denkens im frühen Islam*. 6 vols. Berlin.

Witkam, J. J. 2012. *Formation of the Classical Islamic World*. Burlington, VT/Farnham, UK.

Woepcke, F. 1851. "Notice sur des traductions de deux ouvrages perdus d'Euclide". *Journal Asiatique* 18: 217–247.

The Dependence of Ancient Greek Geometry and Metaphysics on Craft-Culture

by

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Abstract

A discussion of Robert Hahn's *The Metaphysics of the Pythagorean Theorem: Thales, Pythagoras, Engineering, Diagrams, and the Construction of the Cosmos out of Right Triangles*.

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The study of ancient philosophy is never more fascinating—or more frustrating—than when it deals with thinkers who left behind nothing in writing. This category includes three of the most famous names in Greek philosophy: Thales, Pythagoras, and Socrates. Without a fixed text or even fragments of such a text to work with, any scholarly attempt to interpret their doctrines—to explicate their details, reconstruct how they arose, and study how they were applied—will always be shadowed by fundamental doubts about their actual nature. At least in the case of Socrates, we have a great deal of indirect evidence at our disposal in the massive Platonic corpus, together with other literary works, like Xenophon's memoirs and Aristophanes' *Clouds*, which can serve as a check on Plato's testimony. But when it comes to Thales and Pythagoras, we are much less fortunate; for each thinker, fewer than a dozen pieces of testimony survive that date to within two human lifespans of their deaths, most no more than a few sentences in length. Since what we have is so limited, any new insight into the nature of their thought or teachings, however slight it may be, is potentially of great interest.

In his new study, *The Metaphysics of the Pythagorean Theorem: Thales, Pythagoras, Engineering, Diagrams, and the Construction of the Cosmos out of Right Triangles*, Robert Hahn [2018] proposes that such insight can be had if we are willing to explore the implications of the geometrical discoveries made by Thales and Pythagoras. His specific hypothesis is that the two men not only laid the foundations for geometry as a formal, deductive science by revising the mensuration-techniques of Greek and Egyptian craftsmen, they also endowed it with a new, metaphysical meaning. Hahn is here reprising and extending an approach that he developed in previous studies of Anaximander, which aim to show how contemporary craft-practices provided early Greek philosophers with mental models and other habits of reasoning that, once directed at the natural world, helped give rise to natural philosophy. His first book, *Anaximander and the Architects* [Hahn 2001], centered on the proposition that the construction of monumental column-drums by such contemporary architects as Theodorus and Rhoecus prompted Anaximander to think in analogous terms about the form and proportions of the cosmos; hence, the cosmologist's striking assertion that the Earth "resembles a stone column" [Hippolytus, *Ref*. 1.6.3] and his further claim that the Earth is one third as deep as it is wide, its proportions thus strikingly similar to those of a lone column-drum. Hahn's "thick description" of architectural practice during Anaximander's lifetime—construction-plans, models, building techniques, Egyptian influences—made these little fragments come alive, and gave weight to his plea that architecture be granted as much attention as politics or literacy when questions about the origins of Greek philosophy are raised.

In a follow-up study, *Archaeology and the Origins of Philosophy* [Hahn 2010], Hahn pursued this line of inquiry further, arguing that Anaximander's famous Sun and Moon "wheels"—two massive, mist-wrapped wheels of fire which define the orbits of the two bodies—were influenced conceptually by the massive wooden wheels used to transport building stone in Ionia. He also showed how archaic smelting technology informed Anaximander's comparison of the visible faces of the Sun and Moon to a furnace's vent-pipe. The book concludes with a theoretical justification for this focus on archaeology, with texts from Dewey, James, and Putnam brought in to support the claim that knowledge is always embedded in material realities and, thus, that close study of material culture should be an essential part of any reconstruction of ancient philosophy. To this roster of modern authorities, Hahn could also have added Aristotle, who in his account of the development of different forms of human knowledge placed the wisdom of ἀρχιτέκτονες or master builders just one step below that of philosophers proper [*Meta*. 1, 981a24–b24].

A reader of Hahn's first two books—both of them lucidly written and richly illustrated—is apt to come away persuaded that Anaximander engaged in serious reflection on contemporary craft-culture, and that many compelling and original features of his cosmology owe something to that reflection. In describing the philosophical significance of this material, however, the books sometimes go too far. The position which Hahn argues for is not just that a confrontation between archaeological and doxographical evidence can be fruitful, but that architectural thinking lay at the core of Anaximander's vision of the cosmos. Now, the surviving doxography for Anaximander gives pride of place to the doctrine of the Ἄπειρον, an originary being from which the elements emerge and to which they eventually return. In his treatise, Anaximander further sought to account for the creation of the existing world, the cycling through of various κόσμοι or world-orders, the formation and eventual disappearance of the ocean, the creation of the first human beings out of fish-like creatures, and the physical causes of wind, rain, and lightning. These important doctrines are, unfortunately, not illuminated in any way by an understanding of architectural practice. Only those facets of his cosmology that involve structure, measure, or form benefit in this way. So, unless natural philosophy is seen as something *primarily* concerned with the study of cosmic structures, it is going too far to treat craft-based thinking as instrumental in the formation of his core teachings. Study of the impressive remains of Ionian temples or archaic technology is still very valuable, but chiefly for the way in which it can make our reconstructions more grounded, meaningful, and accurate.

In his new book, Hahn again aims high, aspiring to show not just that Greek geometry as practiced by Thales and Pythagoras developed from Egyptian techniques of mensuration, but that they endowed it with metaphysical significance. Before reviewing the particular arguments for this, I would note that nearly half of the pages in this book are given over to clear, step-by-step explications of numerous Euclidean propositions—the "Pythagorean theorem" [*Elem.* 1.47] together with its "enlargement" [*Elem.* 6.31], and several other theorems from books 1, 2, 6, and 10—all illustrated with large, attractive, color diagrams. These expositions are meant to show how much of Euclidean geometry centers on problems involving the application of areas, the scaling up and down of similar shapes, and the theory of proportions. Hahn's commentaries on these theorems are sensible and make for rewarding reading. In some ways, this material constitutes one book—an introduction to the fundamental principles of Euclidean geometry—that has been folded into a second one exploring the origins of Greek geometry and its metaphysical implications. The first of these "books" is cautious and conservative, while the second is much bolder and full of imaginative leaps, not all of which the reader may feel safe taking.

Eudemus of Rhodes, in his authoritative *History of Geometry* [Proclus, *In Euc.*: Friedlein 1873, 65.7], reported that Thales was the first to introduce Egyptian geometrical science to Greece. According to Hahn, Thales learned three things during his Egyptian sojourn:

> (1) formulas and recipes for calculating the area of rectangles and triangles, volumes, and the height of a pyramid...(2) from the land surveyors, he came to imagine space as flat, filled by rectilinear figures, all of which were reducible ultimately to triangles to determine their area; (3) watching the tomb painters and sculptors, he recognized geometrical similarity: the cosmos could be imagined as flat surfaces and volumes articulated by squares, and each thing can be imagined as a scaled-up smaller version. [12]

In his lengthy introduction, Hahn walks us through the technique of Egyptian land-surveying, a few representative problems from the Rhind Mathematical Papyrus, and the wall-painters' practice of laying out grids to define the proportions of figures. A good general case is made here for the Greek inheritance of these techniques from Egypt. Yet, it must be said that none of our sources expressly credits Thales with the introduction of rules for calculating areas or dissecting shapes; all they suggest is that Thales understood how the power of geometrical similarity could be used to solve problems in mensuration. Thales' method for determining the distance of ships at sea seems to have rested on a construction involving similar triangles [108–113]. He also reportedly used similar triangles to measure the height of the Great Pyramid at Giza, treating the vertical axis of the pyramid and its shadow as sides of an isosceles triangle similar in proportion to a smaller triangle formed by a gnomon and its shadow.

In the course of what must have been a fascinating study-abroad visit to Egypt, Hahn had a group of students replicate this measurement [97–107]. While their efforts proved successful, they discovered that there are only a handful of days during the year when the Sun reaches the requisite altitude of 45° in the sky while standing due south, east, or west; on other days, the shadow is either shorter than the base of the pyramid or not aligned with its major axes, situations which render the measurement impossible. Hahn is to be applauded for documenting the attempt and the difficulties that he encountered. To my mind, however, the difficulties feed a suspicion that the story is apocryphal—the earliest source for it, Hieronymus of Rhodes, was a collector of miscellanea from the third century BC.

Nevertheless, the account of Thales' measurement of distance at sea goes back to Eudemus, our most reliable authority for early Greek geometry, and we have no good reason to reject it. Hahn's argument that Thales discovered the principle of geometrical similarity by studying the use of grids in art, either in Egypt or, perhaps, in Ionia, where sculptors in his day were already employing it [Diodorus Siculus, *Bib. hist*. 1.98.5–9], is quite plausible.

In his introduction, Hahn also draws on archaeological research to argue that Greek geometers were using lettered diagrams as early as the middle of the sixth century BC [35–41]. Here he is mounting an explicit challenge to Reviel Netz' claim [2004] that such diagrams did not come into use until about a century later. For evidence, Hahn cites the famous tunnel dug through Mt. Castro on Samos by the Megarian engineer Eupalinus during the reign of Polycrates in *ca* 530 BC. On its walls was painted a series of Greek letters, spaced every 20.6 meters, which served to mark the length of the tunnel.

Near its midpoint, the tunnel makes a curious triangular zigzag. Kienast's explanation for this feature [2005], which Hahn follows, is that ancient diggers had encountered an area of soft stone and, in order to avoid it, deviated westward, then bent back towards the east before resuming their original course. The detour resulted in the tunnel being an extra 17.6 meters long. Someone who thought this fact worth recording marked off an interval of 17.6 meters on one wall, accompanied by the inscription «ΠΑΡΑΔΕΓΜΑ». Hahn argues, to my mind persuasively, that the deliberate way in which this detour was marked implies that Eupalinus was working with a master sketch or diagram that featured the same letters as those painted on the wall.

That said, the fact that Eupalinus apparently made use of a line-diagram with letters on it does not constitute a counterexample to Netz' claim. Lettered diagrams in geometrical texts differ considerably from this putative drawing in their pragmatic function. As Netz has explained in great detail [1999], such diagrams were designed to complement and complete the verbal statement of a proof for a given proposition; their letters serve as indicial marks, designating the particular points (and, by extension, lines and angles) that are named in the verbal account. By contrast, the purpose of Eupalinus' drawing was, one presumes, to provide an objective visual record of the progress of the tunneling. Technically, the marks should be regarded not as letters or indices but as *numerals*, counts of the 20.6-meter measures in the tunnel; the marks are in fact considered the earliest known deployment of alphabetical numerals [Kienast 1995, 148–160]. So Eupalinus' tunnel does not provide clear evidence that diagram-based geometry was already being practiced in the time of Thales or Pythagoras. Hahn would have been on firmer ground had he argued that the classic lettered diagrams of Greek geometry *evolved* from engineering drawings like Eupalinus'; confirmation for such a claim might even be forthcoming some day, if excavations should turn up more examples of lettered plans dating to the early fifth century BC.

The other claims that Hahn puts forward in this study revolve around geometrical metaphysics and the broad thesis that Thales and Pythagoras both understood the structures of the world to be composed of triangles—in particular, right triangles. The anchor for this line of argument is the famous passage in the *Timaeus* [53c–55c] where Plato asserts that the material continuum of space constitutes a tiling of microscopic triangles, which, when clustered, form the polygonal faces of five regular solids, each complete solid representing an elemental particle (save for the dodecahedron, which is somehow linked to the cosmos as a whole). It is natural to wonder whether this theory of geometrical atomism was wholly Plato's brainchild or whether it might represent an elaboration of a doctrine held by prior thinkers. Pythagorean precedents have long been suspected, given that the Pythagorean Ecphantus of Syracuse (*ca* 400 BC) expounded a teleological atomism, and that Pythagoras—or perhaps his student Hippasus—reportedly discovered the regular solids. Hahn takes this Pythagorean background as given and also regards as true Proclus' claim that the ultimate goal of Euclid's *Elements* was to teach the reader how to construct the five regular solids [198–201]. By his reading, much of the early tradition of Greek geometry was in the service of this larger project. He then interprets Pythagoras' discovery of the regular solids with the help of the passage in the *Timaeus*, arguing that the discovery arose from an attempt to explain how the world could be composed out of right triangles [198–212]. Thales is brought into this picture as the source for the insight that all rectilinear shapes can be reduced to collections of triangles [29–32 ff.]. Finally, it is argued that Thales and Pythagoras read metaphysical significance into the fact that geometrical shapes can be scaled up and down, and areas of constant size transformed from one shape into another [82–89 ff.].

Attributing the All-is-Triangles thesis to Pythagoras does motivate his apparent interest in the regular solids, which is otherwise rather hard to account for. But the shortcomings of such a reconstruction are rather severe. Even if we prefer Leonid Zhmud's Pythagoras [2012, 270–283] to Walter Burkert's [1972, 447–465] and see the Samian as making significant contributions to geometry, there is no direct evidence linking him to the triangle-hypothesis or to its metaphysical interpretation. Over a century ago, the influential historian of science Paul Tannery put forward a proposal similar to Hahn's, positing a Pythagorean geometrical atomism that was the target of criticisms made by Zeno [1887, 258–261]. Tannery's hypothesis was further developed by Cornford and others, but no longer has defenders. The reasons for its abandonment are sound. As for Thales, no source ascribes to him the doctrines with which he is credited here. For want of direct testimony, Hahn argues that Thales must have drawn many geometrical diagrams—"to begin to understand Thales and his geometrical speculations, we have to understand that he must have made countless diagrams" [96]—in the course of which these insights would have become all but inevitable. But, to my mind, his conclusion that Thales must have known an early version of the Pythagorean theorem [116–133] highlights the risks rather than the advantages of such a way of proceeding. The assertion is also made that Thales was inspired to develop a geometrical metaphysics in order to quiet critics who were sceptical of his assertion that water was the fundamental substance [29]. On this reading, Thales' triangle-hypothesis was designed to make his theory of water more palatable; but how it would have done so is left wholly unclear. Much more plausible is Hahn's running argument that the earliest Greek geometers (whom, following Netz, I would date to the fifth century, not the sixth) were deeply fascinated by the principles of geometrical equality, similarity, proportion, and magnitude. A book less focused on metaphysics might have been able to draw more interesting connections between craft-practice and the theoretical study of these elementary concepts.

This book's more audacious claims run far beyond the surviving evidence, and the effort to tease them out as implications is not carried off successfully. Nevertheless, its discussions of Euclid, the quality of its layout and presentation, and the investigations of archaic material culture make the book worthwhile. Hahn's deep dives into the τέχνη-tradition represent a substantial contribution to scholarship; few researchers have traced the links between technology and philosophy in pre-Aristotelian thought with such care. Our understanding of the world in which Thales and Anaximander worked is sharpened by Hahn's discussion of contemporary techniques of design, even if his attempt to bring Pythagoras into clearer focus falls short.

To conclude, I would observe that many early philosophers besides Thales and Anaximander found the crafts "good to think with". Is it too much to hope for a future monograph with a title along the lines of *Empedocles' Lantern, Heraclitus' Game-Board, and Plato's Fish-Trap*? There are not many scholars who would be in a better position to write it.

BIBLIOGRAPHY

Burkert, W. 1972. *Lore and Science in Ancient Pythagoreanism*. E. L. Minar Jr. trans. Cambridge, MA.

Friedlein, G. 1873. *Procli Diadochi primum Euclidis Elementorum librum commentarii*. Leipzig. Repr. Hildesheim, 1967

Hahn, R. 2001. *Anaximander and the Architects: The Contributions of Egyptian and Greek Architectural Technologies on the Origins of Greek Philosophy*. Albany, NY.

____ 2010. *Archaeology and the Origins of Philosophy.* Albany, NY.

Hahn, R. 2018. *The Metaphysics of the Pythagorean Theorem: Thales, Pythagoras, Engineering, Diagrams, and the Construction of the Cosmos out of Right Triangles*. Albany, NY.

Kienast, H. J. 1995. *Die Wasserleitung des Eupalinos auf Samos*. Bonn.

—— 2005. *The Aqueduct of Eupalinos of Samos*. Athens.

Netz, R. 2004. "Eudemus of Rhodes, Hippocrates of Chios and the Earliest Form of a Greek Mathematical Text". *Centaurus* 46: 243–286.

—— 1999. *The Shaping of Deduction in Greek Mathematics: A Study in Cognitive History*. Cambridge, UK.

Tannery, P. 1887. *Pour l'histoire de la science Hellène*. Paris.

Zhmud, L. 2012. *Pythagoras and the Early Pythagoreans*. K. Windle and J. Ireland trans. Oxford.

Translating Sanskrit Mathematics

by

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Abstract

A discussion of Jean Michel Delire's *Les mathématiques de l'autel védique. Le* Baudhāyana Śulbasūtra *et son commentaire* Śulbadīpikā.

About the Author

Satyanad Kichenassamy is professor of mathematics at the University of Reims Champagne-Ardenne. He was born in Paris in 1963, graduated from the École Normale Supérieure in Paris in 1986, and obtained his thesis in 1987 under the direction of Haïm Brezis. He works in mathematics, history and Indology, and has solved several long-standing problems in each of these fields. He introduced the method of Fuchsian reduction in nonlinear PDEs and the method of conformal snakes in computer vision. In history, he introduced the notion of apodictic discourse—motivated and conclusive discourse—in which rigorous proofs are encoded in a discursive structure.

The work to be discussed here, *Les mathématiques de l'autel védique. Le* Baudhāyana Śulbasūtra *et son commentaire* Śulbadīpikā [Delire 2016], is devoted to the translation from Sanskrit into French of a late commentary on an ancient Indian mathematical text, the *Baudhāyana Śulvasūtra*[1] (*BŚl*). This text is dated *ca* 800–400 BC.[2] It opens with a discourse on geometry, possibly the earliest mathematical discourse[3] from India still extant. It continues with applications to the building of structures of very specific shapes required for "solemn" ritual purposes, by arranging and stacking bricks according to elaborate rules: these are the Vedic altar(s) of the title. There are mathematical constraints on the shapes of the bricks, on the overall shape of the structures, on the number of bricks and the total area that they cover, and on the relation between consecutive layers. The area-constraint in particular requires the elaborate tools described in the opening discourse.

Among Indian texts of the same class, *BŚl* is the most complete and systematic, and in it we recognize ideas that were developed in later Indian mathematics. P.-S. Filliozat states in his preface that "[n]o text, in the immense mathematical literature in Sanskrit, better shows the originality of Indian Science" [vii–xi], an assessment not inconsistent with current scholarship.[4]

After recalling some of the mathematical aspects of *BŚl* in §1, I summarize the contents of *Les mathématiques de l'autel védique* and relate it to earlier

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[1] Also spelled "Śulbasūtra". Thibaut's sectioning of the text into three parts will be used, following established usage.

[2] For the arguments, see the introduction of Sen and Bag 1983. We give another, possibly new, argument for relative dating at p. 191 n22below.

[3] Constructions are prescribed in earlier Indian texts, but they do not seem to have been woven into a connected discourse specifically devoted to geometry, emphasizing mathematical coherence and generality.

[4] The back cover, however, claims that the

> mathematical skills (*savoir*) of that time [*scil.* the first millennium BC] were comparable to the knowledge (*connaissances*) of civilizations of the same period as to content, but very different as to form, which reveals its oral character.

works (§2). A few specific remarks on individual chapters follow (§3). Possibly because this book was written for Indologists rather than for historians of science, the mathematical concepts at work are not analyzed; in fact, the very existence of rigorous mathematical reasoning in India appears to be ignored, or even vigorously denied.[5] The analysis of a typical example shows how essential aspects [§4] were missed by focusing on a commentary that failed to account for the mathematical content of *BŚl*, and by performing incorrect *mathematical transpositions* of the correctly construed text. It seems that this neglect of mathematical issues reflects some aspects of the early historiography of the subject [§5]. The review closes with a summary of the conclusions in a form hopefully useful to historians of science, whatever their area of interest.

1. The mathematical content of the *Baudhāyana Śulvasūtra.*

The *Śulvasūtras*[6] or *Aphorisms of the Cord*[7] deal, as their name intimates, with constructions performed ultimately on the basis of a single cord that

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[5] The quotation opening the chapter entitled "The Mathematics of the *Baudhāyana Śulbasūtra*" [63] refers to Hindus in general (*les Hindous*) in the following terms that we unfortunately must reproduce:

> I can only compare their mathematical and astronomical literature, as far as I know it, to a mixture of pearl shells and sour dates, or of pearls and dung, or of costly crystals and common pebbles. Both kinds of things are equal in their eyes, since they cannot raise themselves to the methods of a strictly scientific deduction. [Sachau 1910, 1.25]

Delire quotes a French translation of the same judgment [Monteil 1996, 51–52]. Such inflammatory language may reflect the author's fear that an essential preconception is at threat. It could be, in this case, the belief that there is only one type of legitimate (mathematical) discourse.

[6] Four have been translated: the *BŚl*, the *Āpastamba Śulvasūtra*, the *Kātyāyana Śulvasūtra*, and the *Mānava Śulvasūtra* [Sen and Bag 1983]. They belong to four eponymous Vedic schools, each of which had its own *Śulvasūtra*. These four *Śulvasūtra*s display significant differences. The third is very likely to be much more recent than the first two, and the last may be corrupt. Other texts of this class are described in Michaels 1978, and there is a word-index in Michaels 1983.

[7] As Michaels has argued, « śulva », which may mean "cord" in general, must be taken in this context to refer to the topic, cord-geometry, rather than to the instrument; in fact, the latter is called *rajju* or *spandyā* in *BŚl*, rather than *śulva*. We express this by capitalizing "Cord". For an analysis of this and other technical terms, see Michaels 1978, 156–170.

defines the unit of length, all auxiliary lengths being derived from it. Constructions are performed on the ground, points being materialized by poles. The cord may be divided into any integral number of equal parts[8] and may receive marks at distinct points. The unit-area is determined by the square whose side has unit length. The cord serves the purpose of both (marked) ruler and compass, and also enables one to determine perpendiculars. Symmetry with respect to an axis plays a central role. The isosceles trapezium is the most important figure after the oblong, and seems to be the substitute for the scalene triangle.[9] The primacy of quadrilaterals (preferably symmetric) over trilaterals is still apparent in much later mathematical texts. All figures are ultimately exact transformations of squares, with the exception of the circle, for which rules for approximate quadrature/circulature[10] are given. Thus, any figure is determined by the sequence of operations required for its construction, starting from the unit-cord. Because each figure is defined by such a sequence, the scaling of figures is accomplished simply by changing the unit of length and by going through the same sequence of operations. Here, number is embedded in geometry through the scalable unit of length. Much attention is devoted to transforming one figure into another without a change of area. Since figures are obtained by area-preserving or scaling transformations, or by starting from squares of prescribed areas, the area of every figure is determined by its very construction. Baudhāyana never uses angles, parallels, or a calculus of fractions.[11] A *scale-calculus* serves as a substitute for the latter [Kichenassamy 2006]. The possibility of carrying out

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8 In a later section, alternative constructions involving a bamboo rod with holes bored at distinguished points are described [*BŚl* 3.13–15]. The restriction to the cord in the opening section seems, therefore, to be deliberate.

9 An isosceles trapezium is divided by a diagonal into two scalene triangles with the same height.

10 That is, rules for transforming a circle into a square of the same area, and conversely.

11 In other words, at no point is a magnitude associated with the intersection of two lines. Angles do not seem to occur even in later texts [Kichenassamy 2010, 2012a, 2012b]. They are never needed: relations between oblongs or quadrilaterals, or the trilaterals that they contain, provide all the required tools. For instance, the Indian sine and cosine—attested from the middle of the first millennium AD onwards—are obtained by associating to an arc of a circle the sides of the obvious “right triangle”. The standard argument for the Indian origin of our sine function may be found for instance in Filliozat 1988, 261. As was stressed in the French (Bourbaki) school, the measure of an angle is by no means a primary or elementary notion: it ultimately requires the rectification of an arc of a circle.

geometric operations without error is taken for granted in *BŚl*, as in Euclid's *Elements* for that matter.

Like most important works of Indian mathematics, the *Śulvasūtra*s are discourses, typically unwritten and meant to be memorized. This feature seems to have been conducive to the abstraction of mathematical concepts, and to account for the absence of diagrams in *BŚl* and all major Indian mathematical texts. Baudhāyana is thought to have introduced the notion of *paribhāṣā* (meta-discourse), a discourse comprising statements that govern the way other statements are to be understood:

> [T]he innovation [of his] that would turn out to be most important, at least through its indirect effects, is that of the *paribhāṣā*,...axioms that must be present in the user's mind.... Baudhāyana may have been the first to introduce *p[aribhāṣā*s*]*, as they seem to play [in his works] a more necessary role than elsewhere. [Renou 1963, §15, 178–179]

The introductory section, *BŚl* 1.1–62, seems to be such a meta-discourse. Units and subunits of measurement are defined first, stressing that some of them may be redefined at will [1.1–21]; this freedom is the basis for the scaling of figures. Next, the text describes how to construct a square, an oblong, or an isosceles trapezium, and a special type of isosceles triangle.

Proposition 1.48[12] expresses that the diagonal cord of an oblong makes by itself what the two dimensions[13] of the oblong separately make. In other words, first construct one figure[14] by taking one side of the oblong as unit of length. Then, construct another figure by performing the same sequence of operations with the other side of the oblong as unit of length. Next, produce a third figure using the diagonal cord as unit-cord, with again the same sequence of operations. The conclusion is that the third figure is equivalent in area to the first two figures together. This proposition is applied to the construction of a square with an area equal to the sum (or difference) of two given squares.

These methods of sum and difference are relevant for the transformation of a square into any one of a class of figures without a change of area. Approximate rules for the circulature of a square and its inversion, the quadrature of the circle, are also given [1.58–60]. The meta-discourse closes with a famous

[12] *dīrghacaturaśrasyākṣṇayārajjuḥ pārśvamānī tiryaṅmānī ca yatpṛthagbhūte kurutastadubhayaṃ karoti.*

[13] Literally, the side-measure and the cross-measure (*pārśvamānī tiryaṅmānī ca*).

[14] Possibly a square, but the text does not spell this out.

approximation of the diagonal of the square [1.61–62] that is accurate to four places (in modern terms); its place here is logical, since it is a consequence of the derivation of the rules for quadrature [1.59–60: Kichenassamy 2006].

The text continues with a detailed exposition of how, on the basis of these general results, one may construct brick structures that may be described as multilayered jigsaw puzzles of precise shapes and prescribed areas. They are often referred to as altars in the secondary literature because of the central place of fire in the ritual. The pieces are square or oblong kiln-fired bricks or subdivisions and combinations of the same.

2. The content of *Les mathématiques de l'autel védique*

As its full title shows, the work under review approaches the text through one of two extant commentaries, designated as *Śulbadīpikā* (*ŚD*), by Dvārakānātha Yajvan. *ŚD* appears to have been composed between AD 1434 and 1609.[15] There is general agreement that the commentator's remarks do not shed light on Baudhāyana's *modus operandi*. Rather, they illustrate how this *sūtra* was reinterpreted in a particular school, with emphasis on its applications to ritual. *Les mathématiques de l'autel védique* also attempts to draw parallels with other cultures, but no clear structure or hypothesis about transmission emerges from it. The work seems to be intended for Sanskrit readers, as is suggested by the use of the Nāgarī script for the edited text, including the footnotes.

Les mathématiques de l'autel védique is an update of three earlier works:

(1) the edition of *BŚl* and *ŚD*, and the translation of *BŚl* with comments by Thibaut [1875a, b];

(2) the edition by Bhaṭṭācārya [1979] of two commentaries on *BŚl*, including *ŚD*, with a more extensive set of diagrams; and

(3) Sen and Bag 1983,[16] with remarks on commentaries as well as a modern commentary.

[15] Delire's argument for this dating is as follows [150–160]. It appears that the commentator "borrowed" from Sundararāja's commentary on the *Āpastamba Śulvasūtra*, although not in a "slavish" manner [146]. There are two manuscripts of the latter, one from 1581 and the other from 1588 [150]. Although Datta [1932, 18] considers Sundararāja to be the later of the two commentators, Delire opines with Gupta [1993] that Sundararāja's work is earlier than Dvārakānātha Yajvan's but later than the *Śulba-Vārtika* (1434) by Rāma Vājapeyin. On the other hand, there is a copy of Dvārakānātha Yajvan's commentary that is dated to 1609.

[16] See p. 186 n6 above.

It differs from them in three respects:

(1) it takes into account a greater number of manuscripts;
(2) it provides a French translation of the commentary; and
(3) it includes a more complete set of diagrams—in particular, it addresses in some detail the relative position of the various brick structures within the ritual area [42–55]. The diagrams are, of course, an editorial addition.

This volume is an expansion of the author's thesis [Delire 2002] "elaborated under the supervision of P.-S. Filliozat".

The first part [1–191] contains four chapters devoted respectively to:

(1) technical and social aspects of ritual [3–61],
(2) the mathematics of *BŚl* 1.22–62 [63–123],
(3) the mathematics of the commentators [125–160], and
(4) the manuscripts taken into account and the editorial choices made [161–191].

The second part [193–363] gives the (French[17]) translation of the text and commentary. It also provides a transliteration of *BŚl* in roman script. There is no running commentary by the editor in this part.

The third part[18] contains the Sanskrit text [369–515], followed by the editor's diagrams [519–578]. Thibaut's sectioning is used. The 21 sections marked off by Bag and Sen are also indicated in part 2. There is also a further, intermediate sectioning.[19]

A name and place index [581–587], a partial[20] Sanskrit index [598–597], a list of references (works cited and manuscript catalogs [601–613]), and a table of contents [615–620] close the work.

The edition was established by basing the first two parts of *BŚl* on 13 manuscripts, selected from about 30 manuscripts, in addition to Thibaut's edition of the text and commentary [1875a, b], which was itself based on three

[17] The few peculiarities of Belgian French (such as «nonante» for "ninety") do not pose any difficulty.

[18] Page numbers in this part are also given a numbering in Nāgarī characters.

[19] To take a typical example, Bag and Sen group Thibaut's 1.29–35 as 1.5. In the volume under review, they form two unnumbered groups: 1.29–31 are listed on three consecutive lines, each preceded by «sū» (for «sūtra»), followed by a paragraph of commentary preceded by «dvā» (for «Dvārakānātha Yajvan»). Then come 1.32–33, similarly grouped together.

[20] As compared with Michaels 1983.

manuscripts of text and commentary, and a fourth one with the text alone. He did not have access to all of the manuscripts mentioned in the work but gives full particulars including location for all of them. There is no *stemma codicum*.[21] A few emendations for *BŚl* itself are proposed, mostly for part 3 [162–166]. These generally confirm Thibaut's suggestions or correct misprints and "obvious errors" («erreurs manifestes») that are readily detected by carrying out the constructions or the implied computations.

3. Analysis and specific remarks

The title of part 1—"Mathematical Methods in the Architecture of Solemn Sacrifice (*sacrifice solennel*) of Ancient India"—makes the outlook of the work clear. The focus here is on public sacrifices (as opposed to domestic rites) involving brick structures, performed by householders [16] and considered as requiring methods akin to mathematics and architecture. The more complex public rituals are organized by hired experts who act on behalf of the *yajamāna*, whose needs or personal desires are the primary motivation for the rite. The *Śulvasūtra*s are manuals for those experts who may not have the same outlook or desires as the *yajamāna*. Since these rites require larger structures than the domestic ones, they may require greater precision. It appears that the need for precision, together with ritual exactness, was instrumental in the development of a new, more rigorous geometry. Delire refers to Seidenberg's speculation about a possible ritual origin of Greek and Indian geometry [65: see, e.g., Seidenberg 1962]. *Les mathématiques de l'autel védique* also explicitly excludes from consideration the two later stages of life beyond the stage of householder, stages generally associated with the philosophical investigation of the meaning of texts and the reinterpretation of ritual [16].[22]

Chapter 1.1 is entitled "The Sacrificial Ground". It contains a description of ritual structures, focusing on their interpretation in the commentary that is translated in this work—there is some variation among authors—together with a collection of comparisons that have been made in the past with elements of other cultures. A political interpretation of ritual seems to be

[21] Perhaps the implication is that all manuscripts belong to a single family.

[22] This would have given an argument for relative dating: *Kaṭha Upaniṣad* 1.1.15 [Radhakrishnan 1953, 601] refers to the introduction of another brick structure, not mentioned in *BŚl*. If it is an innovation, this proves that Baudhāyana's geometry predates the *Kaṭha Upaniṣad*.

suggested, perhaps unwittingly: "When the Vedic nation (*le peuple védique*)[23] settles somewhere, it takes possession of the territory by a sacrifice" [15]. On the same page, we read: "[O]ne of the altars (*foyers*)...symbolizes conquered and managed (*conquis et exploité*) territory." The question whether those social aspects were essential ingredients in the emergence of geometry does not seem to be addressed.

Les mathématiques de l'autel védique mentions the existence of patterns involving circles, the intersections of which are the vertices of squares, in the Indus Valley and in Heraklion, suggesting that similar patterns "most certainly led to" (« ont très certainement débouché sur ») an exact construction of a square in *BŚl* [69–71]. The implied thesis is not clear: Did Baudhāyana create an abstract discourse on the basis of ornamental patterns in order to improve ritual performances? Or is mathematical discourse an outgrowth of solemn ritual, a response to challenges to this ritual. Or is it only incidentally associated with it? There are indeed suggestions that the *Śulvasūtra*s were an outgrowth of the geometry and architecture of an earlier culture, such as the Indus Valley Civilization, or some other with a sophisticated kiln-fired brick technology [Converse 1974; Staal 1999 and 2001]. Whatever its remote forerunners, it appears at the present time that Baudhāyana's approach, by its discursive structure, not only differs from extant texts from other cultures, but also represents a new stage in the evolution of Indian tradition.

Chapter 1.2 is devoted to Baudhāyana's mathematics and presents a translation of the results into modern symbols, together with speculations about their possible origins, collecting some of the opinions that have been put forward in the past. *BŚl* 1.22–62 are termed "mathematical *sūtras*" (in the title of section 1.2.1), implying that this part of the text qualifies as mathematics while the rest would be ritual. The missing part of the meta-discourse, *BŚl* 1.1–21, is described in the chapter on ritual [§1.1.3]. This part introduces the variability of the unit of measurement, which forms the basis of the scaling of figures in *BŚl*. Delire does recognize in it "a principle of proportionality enabling one to construct objects similar to others by simply adjusting the base measure" [19], suggesting that this part, too, is mathematical. It is true that the commentators also missed most of the mathematical issues and did not realize that their own conceptual framework differed from Baudhāyana's.

[23] The existence of such a well-defined Vedic ethnic or political entity, let alone its bellicose nature, is highly controversial. The existence of similarities between Indo-European *languages* is not. For a recent discussion of this issue, see Demoule 2014.

This chapter also contains a collection of some of the earlier suggestions about the possible derivation of Baudhāyana's results. The author mentions Piaget's analysis of the stages of learning observed in some children as a possible model for the evolution of Indian mathematics, and reads earlier derivations based on dissection methods in this light [90 ff.]. But Baudhāyana is working within a complex tradition that he has already assimilated; we are not dealing with the infancy of mathematics but with its coming of age. Ancient mathematics does not seem to have been performed by children, even in the remote past. Also, Piaget's *praxis*-driven model, as presented by Delire, does not account for the discursive dimension of *BŚl*. Mention of dissenting views on these controversial issues, such as those of Chomsky or Lacan, would have been welcome.

Chapter 1.3 is devoted to "the commentators' mathematics". Their results seem to have been obtained by using the methods that have been standard in India since Āryabhaṭa (AD 499). This chapter records inconsistencies "certainly to be attributed" to borrowings from other sources, without double-checking [144]. It closes with a detailed comparison of parallel passages in the commentary edited here and with Sundararāja's commentary on the *Āpastamba Śulvasūtra*, leading to Delire's proposed timeframe for the commentary [150–160].[24]

Some aspects of the translations may be misleading to the non-specialist. Some of them are perhaps due to carelessness and have the effect of hiding conceptual problems from view. Here are three examples.

(1) The archaic term «praüga» for the isosceles triangle obtained from a square by joining the middle of the top side to the ends of the lower side is translated by "triangle" [*BŚl* 1.56: 208]. Now, words equivalent to "triangle" or, more precisely, "trilateral" («tribhuja») are absent from *BŚl*;[25] so is the very notion of a scalene triangle.

(2) Single terms are not always translated uniformly: «pāśa» is translated by «boucles» (loops) in 1.27 and in the commentary to 1.30, but by «noeud» (knot) in 1.30 itself. The technological issue is how, given a cord of known length, one may fit loops, or perhaps nooses, at its ends in such a way that, by stretching the cord between two poles, one is guaranteed that the distance between them is equal to the length of the original cord. Knotting a cord slightly reduces its length.

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[24] See p. 189 n15 above.

[25] According to Michaels 1983.

Such points confirm the lack of emphasis on practical issues in *BŚl* that were perhaps to be left to the care of specialized staff. Similarly, « vidha » is translated as « unité » unit) and as « sorte » kind, "type") [see *BŚl* 2.11–12, 2.14]. Bag and Sen translate it as "fold" because, for instance, « saptavidha » means sevenfold: it qualifies the figure obtained from a given one by increasing its area sevenfold. This technical term reflects the conception of scaling of figures by the mere change of the fundamental cord [see §2, p. 189above]. The translation of « tiryaṅmānī » and « tiraścī » for a transverse dimension [1.54, 3.281]) as « transversale » is also misleading because of the existence in modern mathematics of the "théorie des transversales", in which a *transversale* is a line that cuts *through* several others. On page 81, Delire had correctly translated the first of these words as « mesurée en travers » (measured across).[26] Readers already familiar with the subject will hopefully make the necessary adjustments.

(3) The very first line of the commentary is a prosternation to Gaṇeśa (« śrī gaṇeśāya namaḥ »: « śrī » is honorific). In the translation, this clause is moved *after* 1.1 and translated approximately by "Glory to Gaṇeśa". It is a prosternation and not praise; and it is essential that it should come *first* since it is a standard way for authors to ward off, at the outset, obstacles of any kind that might arise in the course of the work.

We now turn to the basic questions outlined in the introduction about the neglect of the conceptual and discursive dimensions of the text.

4. The problem of mathematical transposition

4.1 *An example of mathematical transposition*

As a typical example of transposition in *Les mathématiques de l'autel védique*, consider Baudhāyana's rule [1.59] for the (approximate) quadrature of the circle. We read:

> Let us note at the outset that Dvārakānātha [the commentator] did not feel any difficulty in understanding Baudhāyana's quadrature. Indeed, he transforms the fraction[27] $1-\frac{28}{8\times 29}-\frac{1}{8\times 29\times 6}+\frac{1}{8\times 29\times 6\times 8}$ —*for this is indeed how* sūtra *(I.59) is*

[26] An oblong constructed symmetrically with respect to an axis has two dimensions, one along this axis, the other one across it.

[27] Here and in the next sentence, the wording is ambiguous. The French verb used is « comprendre »; it can mean "to understand" or "to comprehend". The commentator construed the sentence correctly in the mere grammatical sense, but he did not comprehend it, as we shall see.

> *to be understood* [my emphasis]—into $\frac{7}{8}+\frac{1}{8}\left(\frac{41}{1392}\right)$, then further into $1-\frac{1}{8}\left(\frac{1351}{1392}\right)$, thus showing his mastery of the calculus of fractions, even [when they are] not unit[-fractions].[28] [142]

The implication is that

(a) Baudhāyana's text may be written in a form in which a possible allusion to "Egyptian fractions" is apparent, thus introducing unit-fractions that are not in the text; and

(b) since the commentator could handle general fractions, there is no need to investigate whether Baudhāyana worked with this concept.

However, point (a) is incorrect: this is *not* how the *sūtra* is to be understood. To see this, consider Thibaut's translation of 1.59—the way in which Thibaut construed the text has never been challenged, not even in the volume under review, since the Sanskrit is quite clear. His translation reads:

> If you wish to turn a circle into a square, divide the diameter into eight parts and one of these parts into twenty-nine parts: of these twenty-nine parts remove twenty-eight and moreover the sixth part (of the one part left) less the eighth part (of the sixth part). [Thibaut 1875b, 1.59]

Taken literally, and with the same notation as *Les mathématiques de l'autel védique*, the text would correspond to the expression:

$$1-\frac{1}{8\times 29}\left(28+\frac{1}{6}\left(1-\frac{1}{8}\right)\right).$$

Thus, in terms of fractions, one would have to deal with a compound expression of which the numerator could itself be a fraction—in no sense is this mathematical object a sum of unit-fractions. Now, there is general agreement that a general calculus of fractions with reduction to the same denominator is not attested at this time. And all attempts to account for 1.59 by means of a calculus of fractions lead to inconsistencies [Kichenassamy 2006]. The question is: What mathematical tool, possibly absent from modern mathematics, was used by Baudhāyana in those situations where *we* would be tempted to use general fractions or "Egyptian" fractions? The work under review and the commentary missed this question because they performed an *incorrect mathematical transposition* on top of the unproblematic literal translation.[29] This transposition made it impossible to see

[28] A unit-fraction is one of the form $1/n$, where n is integral. Calculations with aliquot parts are found in Egyptian mathematics; hence, the name "Egyptian fractions" for expressions involving only sums of unit-fractions.

[29] Thibaut also performed this mathematical transposition, although he did point out some of the anachronistic aspects of the commentary.

the problem. Recall that, according to the back cover [cf. p. 185 n4 above], the author considers that all works of the same time frame are essentially similar in content. The mathematical transposition is driven by the illusion that the text must involve unit-fractions.

Now, the mathematical object involved in 1.59 is *not* a combination of fractions such as $^{13}/_{15}$, even though it is determined by pairs of numbers such as (13, 15). One may think of each of them as a "pairs of divisors", in which none of the elements is distinguished as the numerator. Such pairs express a correspondence between lines or, rather, (portions of) cords [Kichenassamy 2006, 2011]. For instance, 1.60 states: "after having made fifteen parts, remove two". That is, to 15 parts of one cord correspond 13 $(15 - 2)$ of another. This pair is not a fraction because the two numbers play symmetric roles. If there is only one such pair, it is readily inverted *without* reference to fractions. In this case, it suffices to divide the latter cord into 13 parts and to *add* two of these parts to recover the length of the first cord.

More generally, two cords, a and b, would be related by giving a pair p, q of divisors if the following holds: if one divides a into p parts, then q of them make up b. And if one divides b into q parts, then p of them make up a. If we read the text closely with this idea in mind and remember that the unit or length may be redefined in the course of the argument, we see that the text lists, in a remarkably compact yet transparent way, the steps of a derivation of 1.59 and of the following few propositions, using only tools attested in the text [Kichenassamy 2006, 172–180]. This derivation differs from all those proposed so far, and it cannot be recovered by mere transposition from some modern derivation. It accounts for the very specific numbers in the text, as well as the order of the words in the sentence, and is, to date, the only one that accounts for the text as it is.

Thus, *Les mathématiques de l'autel védique*, by relying on the commentary, is affected by the belief that mathematical transposition may be made without loss of content. However, transposition is by no means tautological.[30] That Indian commentaries make use of a form of transposition does not make it legitimate in historical work. Change of notation, however, can be harmless provided that the operations performed on the new symbols reflect those of

[30] Transposition may be useful in the study of mathematical problems to gain new insight, but becomes objectionable when it leads to attributing one's own ideas to someone else.

the text.[31] Modernized notation becomes dangerous only when it suggests relations that could not have been suspected without it.

4.2 *Is mathematical transposition unique?* It has been argued[32] that mathematical transposition is nevertheless a legitimate tool in the analysis of mathematical texts, not only because it has been performed in some ancient texts, but because it is allegedly the only way to make sense of a text. To our knowledge, the only example on this score is the algebraic interpretation of four "lost" books of Diophantus in Arabic sources of the late ninth century, in which Diophantus is turned into al-Khwārizmī's "heir" («successeur»)[33] (*sic*). This text was further reinterpreted in terms of 20th-century algebraic geometry, occasionally requiring spaces of more than three dimensions. Mathematical transposition is claimed in this case to be not only convenient but necessary because it is *unique*. But in fact, it is not. This transposition requires the introduction of several unknowns not attested in the text, but we know that Brahmagupta (in the seventh century) introduced several literal unknowns. Moreover, we find, for example, in a ninth-century commentary,[34] an equation with six unknowns labeled by letters (*yā, kā, nī, pī, lo, ha*) that are the initials of a conventional set of words and bear no connection to the quantities represented.[35] Thus, a literal algebra with several unknowns, unrelated to the conception of a space of more than three dimensions, is attested at the same time as our Arabic text. We must, therefore, wonder, regardless of any possible hypothesis about transmission, why one particular transposition was preferred by some modern readers to another. At any rate, this proves that mathematical transposition into 20th-century mathematics is not the only possible transposition. We also

31 An example is provided by the introduction, in the analysis of *BŚl* 1.59 above, of the pair-notation for the benefit of the modern reader. The derivation in Kichenassamy 2006, however, does not use it and does not introduce other symbols.

32 We thank Karine Chemla for bringing this problem to our attention. Chemla 1986 gives an overview and is careful not to jump to conclusions.

33 Chemla 1986, 368.

34 Colebrooke 1817, 355 *et pass*. See also 139 n1 for details on this multi-literal algebra and its development.

35 "Letter" here translates «varṇa». This word also means "color", hence, the use of the initials of names of colors, as here. Other lists of letters as symbols are also attested. Those letters are further analyzed into phonemes in Indian grammars, but this is not relevant here.

see in this example that appropriation through mathematical transposition is by no means a recent phenomenon.

5. Other reasons why conceptual issues in Indian mathematics were neglected

The belief that mathematical transposition is harmless fosters the feeling that texts do not constrain our readings of them, that internal analysis is not necessary. Leaving aside prejudice and disregard of axiological neutrality, there seems to have been three further reasons for the relative dearth of textual analyses of Indian texts in their own terms:

(1) the existence of undetected errors in the texts,
(2) the (related) assumption that results found in Indian texts were derived from unacknowledged sources, and
(3) the belief that ancient mathematical discourse may be understood on the basis of much later sources of the same tradition.

I examine them in order.

(1) The existence of errors[36] propagated by commentaries suggested that some results

> were handed down as received truths, with the result that incorrect theorems were not identified as a matter of routine by any student who checked the proofs. [Bronkhorst 2001, 54]

Some commentaries were blamed for striving to justify the incorrect ones [Bronkhorst 2006]. However, undetected errors and ideologically driven discourses are not unheard of, even in modern mathematics. The issue is, therefore, whether such commentators are representative of the entire tradition and, indeed, whether there may not have been several mathematical cultures in India.

(2) It was assumed that Indian mathematics was influenced by Hellenistic mathematics, which may be true to some extent for late authors, just as

[36] A famous example is Āryabhaṭa's rule that appears to give an incorrect formula for the volume of the sphere [*Āryabhaṭīya* 2.7]. The error was not spotted in the oldest extant commentary, by Bhāskara I (AD 629, translated in Keller 2006, 1.xxxii–xxxiii): Keller points out that the commentator seems to work with a faulty version of the text [2006, 1.35 nn209–210]. Since there is an ingenious way to make sense of the passage [Elfering 1975, 71–76], we must conclude that the commentator missed the error *and* failed to propose a mathematically correct reading of the text, even though one was possible.

Indian mathematics influenced other cultures. Hellenistic influence[37] on genethliacal astrology is documented and acknowledged in the texts, but interpretative astrology—the subject of a vast literature in India as elsewhere—does not seem to be discussed at all in mathematical texts. Also, the absence of the notions of angle and parallel in India shows that, for instance, the conceptual framework of Brahmagupta's geometry (AD 628) does not seem to have a counterpart in other cultures. The transmission hypotheses formulated so far do not seem to account for Brahmagupta's text. More generally, it is essential to refrain from speculating on issues of transmission before the content of the texts has been thoroughly studied. Issues of priority must not become a priority.

(3) Since ancient Indian mathematical texts were preserved faithfully by tradition to this day, their meaning may perhaps be inferred from late commentaries. However, this is not always warranted. To take an example, the existence of several schools with non-equivalent conceptual frameworks[38] is indicated by a passage in which Bhāskara II (12th century) criticizes Brahmagupta's formula[39] for the diagonals of a cyclic quadrilateral as unnecessarily complicated. He gives a simpler formula that does not, however, apply to all the cases covered by Brahmagupta's [Colebrooke 1817, 80–81]. It seems established [Kichenassamy 2012b] that there were partial breaks in the continuity of the Indian mathematical tradition, so that texts were passed down to further generations but their conceptual framework or the associated *modus operandi* was partially lost in the process.

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[37] Probably before the seventh century AD. The date and nature of this influence have recently been reexamined, and an error in the reading of an important text was discovered in the process. See Mak 2013; Filliozat 2016.

[38] The existence of two distinct schools in India—one that deals exclusively with cyclic quadrilaterals; another that never considers them—seems to have been first clearly singled out as a fundamental issue in Sarasvati Amma 1999, 81.

[39] Many Indian texts describe in words general formulae—for the determination of lengths, areas, or volumes for instance—where variables are represented by words, as is appropriate for versified texts. The existence of separate names for parts of a figure makes the correspondence with modern formulae unambiguous. This system coexists with literal or symbolic algebra among authors who also deal with the theory of equations.

6. Conclusion

Les mathématiques de l'autel védique is a contribution to the study of an important text, the *Baudhāyana Śulvasūtra*, and will be of interest to those Indologists already familiar with the basic texts of ancient Indian mathematics and the issues that they raise. However, the very existence of rigorous mathematical reasoning in this text is not apparent in this study because Delire focuses on a late commentary that failed to address conceptual issues, introduced mathematical transpositions in terms of a much later framework, and did not account for the text itself.

We attribute this state of affairs to two main causes. First, the *Baudhāyana Śulvasūtra*, while an apodictic discourse, is not dogmatic: it requires the reader to think with the author rather than to be submissive. Second, there were partial breaks in the mathematical tradition: the conceptual framework of one school was forgotten while its texts were passed down; its results were thus fitted to the Procrustean bed of another school, resulting in inconsistencies that indirectly cast a shadow on the original works.

However, the correct conceptual framework of the *Baudhāyana Śulvasūtra* may be understood by textual analysis because the text was composed with great care. Insofar as text and context are correlated in this case, internal analysis provides strong evidence for the context that is more reliable than second-hand information. And the *mathematical coherence* of this text is a very strong constraint on its reading, as it is for the reading of any mathematical text. The notion of apodictic discourse that includes all forms of rational argumentation to establish a result within a shared framework seems relevant to the analysis of texts from other cultures as well.[40]

The following conclusions appear to be of relevance to the analysis of all cultural areas.

(1) *Mathematical transposition* from one conceptual framework to another is a form of tampering with the text. By contrast, *transcription* into modern notation is sometimes admissible, provided that the operations permitted are never lost sight of, and may help communication with modern readers.

(2) *Priority is not a priority.* Transmission or issues of priority should not be discussed before analyzing and understanding the texts themselves.

[40] See Kichenassamy 2015 for an application to an Italian text of the Renaissance.

(3) Consistent scientific discourse, ancient or modern, takes the form of an *apodictic discourse* that need not take a deductive form, unless one wishes to suppress motivation and stress verification.

(4) There may be *mathematical pluralism within a culture*.[41] In particular, a text and a commentary on it may not share the same conceptual framework. Any plural tradition will perforce appear incoherent or inchoate at best, if one attempts to interpret individual differences as forms of variability within categories implicitly taken as universal.

The analysis of mathematical discourse, guided by the demands of the internal mathematical coherence of each individual text and strict axiological neutrality, is similar to ordinary communication: other peoples' discourses are seldom entirely transparent and are understood through a process of gradual adjustment, provided that we accept that we do not know beforehand what others mean. It is possible to understand others without becoming similar to them or forcing them into assimilation. In this sense, the process of analysis advocated here provides a framework for the understanding of diversity.

BIBLIOGRAPHY

Bhaṭṭācārya, V. 1979. *Baudhāyanaśulbasūtram, with Two Commentaries: «Bodhāyanaśulbamīmāṃsā», by Śrī Vyaṅkaṭēśvara Dīkṣita, and «Śulbadīpikā», by Śrī Dvārakānātha Yajva*. Benares.

Bronkhorst, J. 2001. "Pāṇini and Euclid: Reflections on Indian Geometry". *Journal of Indian Philosophy*, 29: 1–2, 43–80.

____ 2006. "Commentaries and the History of Science in India". *Asiatische Studien / Études Asiatiques*, 60: 773–788.

Chasles, M. 1837. *Aperçu historique sur l'origine et le développement des méthodes en géométrie*. Brussels. Reprint: Sceaux, 1989.

Chemla, K. 1986. "II. Étude mathématique". See Chemla, Morelon, and Allard 1986, 361–375.

____ 2016. "La diversité des cultures mathématiques. Un passé et quelques futurs possibles". *Gazette des Mathématiciens* 150: 16–30.

[41] For a very recent example of pluralism, see Chemla 2016, 2018. She points out the lack of definition of the term "mathematical cultures" [Chemla 2016, 1]; the notion of conceptual frameworks may provide a useful substitute.

Chemla, K. 2018. "How Did One, and How Could One Have Approached the Diversity of Mathematical Cultures?" Pp. 1–61 in V. Mehrmann and M. Skutella edd. *Proceedings of the European Congress of Mathematics*, Berlin, 18–22 July 2016. Paris.

Chemla, K.; R. Morelon; and A. Allard. 1986. "La tradition arabe de Diophante d'Alexandrie. À propos de quatre livres des *Arithmétiques* perdus en grec, retrouvés en arabe". *L'Antiquité Classique* 55: 351–375.

Colebrooke, H. T. 1817. *Algebra, with Arithmetic and Mensuration, from the Sanskrit of Brahmegupta and Bhascara*. London. Reprint: Delhi, 2005.

Converse, H. S. 1974. "The Agnicayana Rite: Indigenous Origin?" *History of Religion* 14: 81–95.

Datta, B. B. 1932. *Ancient Hindu Geometry (The Science of the Śulba)*. Calcutta. Reprint: Delhi, 1993.

Delire, J.-M. 2002. *Vers une édition critique des Śulbadīpikā et Śulbamīmāṃsā, commentaires du Baudhāyana Śulbasūtra . Contribution à l'histoire des mathématiques sanskrites*. 2 vols. PhD dissertation, Université libre de Bruxelles. Brussels.

____ 2016. *Les mathématiques de l'autel védique. Le* Baudhāyana Śulbasūtra *et son commentaire* Śulbadīpikā. Geneva.

Demoule, J.-P. 2014. *Mais où sont passés les Indo-Européens?* Paris.

Elfering, K. 1975. *The Mathematik des Āryabhaṭa I. Translation from Sanskrit and Commentary*. Munich.

Filliozat, P.-S. 1988. "Calculs de demi-cordes d'arc par Āryabhaṭa et Bhāskara I". *Bulletin d'études Indiennes* 6: 255–274.

____ 2016. "La nature des planètes selon le *Yavanajātaka* 'L'Horoscopie grecque' de Sphujidhvaja et le Bṛhajjātaka 'La Grande Horoscopie' de Varāha Mihira". Pp. 341–359 in J. Jouanna, V. Schiltz, and M. Zink edd. *La Grèce dans les profondeurs de l'Asie*. Paris.

Gupta, R. C. 1993. "Sundararāja's Improvement of Vedic Circle-Square Conversions". *Indian Journal of History of Science* 28: 81–101.

Keller, A. 2006. *Expounding the Mathematical Seed: A Translation of Bhåskara I on the Mathematical Chapter of the* Āryabhaṭīya. 2 vols. Basel.

Kichenassamy, S. 2006. "Baudhāyana's Rule for the Quadrature of the Circle". *Historia Mathematica* 33: 149–183.

____ 2010. "Brahmagupta's Derivation of the Area of a Cyclic Quadrilateral". *Historia Mathematica* 37: 28–61.

Kichenassamy, S. 2011. “Textual Analysis of Ancient Indian Mathematics”. *Gaṇita Bhāratī* 33: 15–28.

—— 2012a. “Brahmagupta’s Propositions on the Perpendiculars of Cyclic Quadrilaterals”. *Historia Mathematica* 39: 387–404.

—— 2012b. “L’analyse littéraire au service de l’histoire des mathématiques. Critique interne de la géométrie de Brahmagupta”. *Comptes-Rendus des Séances de l’Académie des Inscriptions et Belles-Lettres* 2: 781–796.

—— 2015. “Continued Proportions and Tartaglia’s Solution of Cubic Equations”. *Historia Mathematica* 42.4: 407–435. For an introduction, see http://www.cnrs.fr/insmi/spip.php?article1477.

Mak, B. M. 2013. “The Date and Nature of Sphujidhvaja’s Yavanajātaka Reconsidered in the Light of Some Newly Discovered Materials”. *History of Science in South Asia* 1: 1–20.

Michaels, A. 1978. *Beweisverfahren in der vedischen Sakralgeometrie*. Wiesbaden.

—— 1983. *A Comprehensive Śulvasūtra Word Index*. Wiesbaden.

Monteil, V.-M. 1996. *Le livre de l’Inde. Selected excerpts, translated from Arabic, with annotations*. UNESCO. Arles/Paris.

Radhakrishnan, S. 1953. *The Principal Upaniṣads*. London. Reprint: Atlantic Heights, NJ, 1992.

Renou, L. 1963. “Sur le genre du sūtra dans la littérature sanskrite”. *Journal Asiatique* 251: 165–216.

Sachau, E. C. 1910. *Alberuni’s India*. 2 vols. London.

Sarasvati Amma, T. 1999. *Geometry in Ancient and Medieval India*. 2nd edn. rev. Delhi.

Sarma, K. V. 2008. *Gaṇita-Yukti-Bhāṣā of Jyeṣṭhadeva. With Explanatory Notes by K. Ramasubramanian, M. D. Srinivas, and M. S. Sriram*. Berlin.

Seidenberg, A. 1962. “The Ritual Origin of Geometry”. *Archive for History of Exact Sciences* 1: 488–527.

Sen, S. N. and A. K. Bag. 1983. *The* Śulbasūtras *of Baudhāyana, Āpastamba, Kātyāyana and Mānava, with Text, English Translation and Commentary*. New Delhi.

Staal, F. 1999. “Greek and Vedic Geometry”. *Journal of Indian Philosophy* 27: 105–127.

Staal, F. 2001. “Squares and Oblongs in the Veda”. *Journal of Indian Philosophy* 29: 257–273.

Thibaut, G. 1875a. “On the Śulva-sūtra”. *Journal of the Asiatic Society of Bengal* 44: 227–275.

—— 1875b. *Baudhāyana Śulva-sūtra. Edited and translated with commentary by Dvārakānātha Yajvā. The Paṇḍit*, os 9–10 (1874–1875), ns 1 (1877). Benares. Thibaut’s translations and comments were reprinted as *Mathematics in the Making in Ancient India*. Calcutta, 1984.

Remarks on the Historiography of Mathematics

by

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Abstract

In this paper, I examine aspects of the methodological debate that originated in 2010, when the distinguished historian of mathematics Sabetai Unguru reviewed Roshdi Rashed's edition of the Arabic translation of Apollonius' *Conics*. In his review, Unguru criticized what Rashed calls "l'usage instrumental d'une autre mathématique pour commenter une oeuvre ancienne". I consider this debate very important and will try to place it within in the discussion of the so-called "geometric algebra" that goes back to the seventies, by tracing the contributions of the main figures who took part in it.

About the Author

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The discussion that I will address in these pages was prompted by the methodological decisions taken by Roshdi Rashed in his memorable edition, translation, and commentary on the Arabic/Islamic mathematical tradition. I will concentrate on this discussion after I have placed it in a larger context that goes back to a distant past. Since I believe that this discussion is of great relevance for historians of mathematics (and more generally for cultural historians), I will limit my personal opinions to a minimum. Instead, I will try to do justice to all the points of view expressed in this discussion.

The discussion was prompted by Sabetai Unguru and his review [2010] of Rashed's *Apollonius de Perge. Coniques. Tome 2.2. Livre IV: Commentaire historique et matématique* [2009]. Rashed stated his historiographical point very clearly in the first volume of his editorial project. I report some of the excerpts quoted by Unguru. They will give a first idea of the nature of this discussion:

(1) Le recours aux termes de la géométrie algébrique risque de déplaire. ...Il s'agit bien d'une théorie géométrique des sections coniques: point de géométrie algébrique, point de géométrie différentielle. Et pourtant, nous avons pris la liberté de recourir dans nos commentaires à la géométrie algébrique, encourant ainsi, en toute connaissance de cause, un reproche d'anachronisme de la part des gardiens du temple. [Unguru 2010, 34]

(2) Il s'agit plutôt, nous semble-t-il, de l'effet du choix délibéré d'un style d'écriture de l'histoire, par élucidation rétrograde, telle que le pratiquait Bourbaki: partir du présent pour restituer le passé; et aussi d'un souci didactique: s'adresser aux contemporaines dans la langue de leurs mathématique. [Unguru 2010, lvi]

(3) Pour lire une œuvre mathématique ancienne, il nous a donc semblé nécessaire de solliciter l'aide d'une autre mathématique, à laquelle on emprunte les instruments qui pourront en restituer l'essence. Un modèle construit dans une autre langue mathématique permet d'aller plus loin dans l'intelligence du texte, particulièrement lorsque cette langue est celle d'une mathématique plus puissante, mais qui trouve dans l'œuvre commentée l'une des sous sources historiques.

Pour les *Coniques*, c'est la géométrie algébrique élémentaire qui fournit ce modèle. [Unguru 2010, 35]

(4) Dans le cas des *Coniques*, on observe, à partir du IXe siècle, une extension de certaines de leurs chapitres, ainsi que leur application aux domaines les plus divers et leur contribution, essentielle, à la création de la géométrie algébrique élémentaire. Il suffit pour s'en convaincre de lire l'*Algèbre* d'al-Khayyām, les *Équations* de Sharaf al-Din al-Tūsi, la *Géométrie* de Descartes, la *Dissertation Tripartite* de Fermat. Négliger le contexte des successeurs conduit inévitablement à tronquer l'histoire de l'œuvre. Même s'ils transforment son sens, les successeurs permettent en effet à l'historien de voir l'œuvre avec d'avantage de clarté et de profondeur. Cette préoccupation a été la nôtre ailleurs. [Unguru 2010, 36]

The excerpts above may give the impression that Rashed shares the approach of the so-called geometric algebra promoted by Heath and Zeuthen. This is also suggested by Unguru, who says, "This is how Heath and Zeuthen proceeded when appealing to geometric algebra" [Unguru 2010, 34]. It is, therefore, useful to make a brief excursus and recall another, older debate, revived in the 1970s, in which Unguru himself took part.

The label "geometric algebra" has been defined as the attempt to interpret part of Greek mathematics, typified by book 2 of Euclid's *Elements*, as a translation of Babylonian algebraic identities and procedures into geometric language [Berggren 1984]. In reality, geometric algebra is based on a much older tradition. Some of the first protagonists of the birth of analytic geometry used algebraic methods in geometry (e.g., Viète, Descartes, and Newton, among others). They thought that books 2 and 6 of Euclid's *Elements* were actually the translation in geometrical fashion of pre-existing algebraic theorems. In particular, Newton, in the appendix to his *Arithmetica universalis*, says "geometria excogitata fuit ut expedito linearum ductu effugeremus computandi tedium". In other words, contrary to the views of some of his contemporaries, Newton held that geometry was not merely a kind of coating on algebraical calculus but rather an achievement destined to overcome the calculating complexity of algebra and arithmetic.

Of course, these were not historiographical considerations; rather, they reflected the attempt of the creators of a new mathematical language to make contact with the language (and, indeed, the results) of those who had preceded them. The introduction of this way of thinking into the historiographical tradition is usually attributed to the Danish mathematician

Hieronimus Zeuthen in the second half of the 19th century, after the discovery of Egyptian and especially Babylonian mathematical materials by Otto Neugebauer and Barthel Van der Waerden. With the work of these two eminent mathematicians, the theory of "geometric algebra", adopted by various historians of mathematics (including Heath and Boyer), was established.

It should be stressed, however, that this was a precise historiographical thesis. As such, it was not meant to be a methodological proposal, even though it was based on specific methodological choices, which were nevertheless different among the various proponents of this thesis. It became a thesis about the method to be used in historical research after the publication of the seminal article by Unguru, "On the Need to Rewrite the History of Greek Mathematics", first published in *Archive for the History of Exact Science* in 1975.[1] After describing in critical terms the theory of geometric algebra, Unguru tries to identify the cause that has led to what he characterizes as a scandalous situation. He writes:

> It is in truth deplorable and sad when a student of ancient or medieval culture and ideas must familiarize himself first with the notions and operations of modern mathematics in order to grasp the meaning and intent of modern commentators dealing with ancient and medieval mathematical texts. With very few and notable exceptions, Whig history is history in the domain of the history of mathematics; indeed, it is still, largely speaking, the standard, acceptable, respectable, "normal" kind of history, continuing to appear in professional journals and scholarly monographs. It is the way to write the history of mathematics. And since this is the case, one is faced with the awkward predicament of having to learn the language, techniques, and way of expression of the modern mathematician...if one is interested in the historical exegesis of premodern mathematics; for it is a fact that the representative audience of the mathematician fathering "historical" studies consists of historians...rather than mathematicians....As to the goal of these so-called "historical" studies, it can easily be stated in one sentence: to show how past mathematicians hid their modern ideas and procedures under the ungainly, gauche, and embarrassing cloak of antiquated and out-of-fashion ways of expression; in other words, the purpose of the historian of mathematics is to unravel and disentangle past mathematical texts and transcribe them into the modern language of mathematics, making them easily available to all those interested. [Unguru 2004, 386]

[1] This article and others that followed in the ensuing debate are now collected in Unguru 2004.

The rest of this article is a critique of the thesis of geometric algebra, in which the perceived errors of this approach are linked to what is regarded as a mistaken methodological conception of the history of mathematics.

Especially offensive to historians who had been (or were still) first-class mathematicians, but who had dared to venture into the field of history, was the use of sociological or biographical considerations. Unguru writes:

> Let me only suggest again...that the fact that the history of mathematics has been typically written by mathematicians might have something to do with it...they were mathematicians who have either reached retirement age and ceased to be productive in their own specialities or became otherwise professionally sterile. However, both of these categories had something in common: in order to serve humanity and expend untapped remnants of scholarly energy, they decided to employ their creativity in a field, history of mathematics, "half" of which—the history—was too alien and exotic while the other "half"—the mathematics—was, alas, too familiar to them; the underlying assumption being that history does not really require any training, its narrative, reportorial methods and techniques being common-sensical and self-evident; and since they were highly proficient in mathematics they had all which was required to become successful historians of mathematics!...the reader may judge for himself how wise it is for a professional to start writing the history of his discipline, when his only calling lies in professional senility which bars him from encroaching on more friendly, familiar and hospitable territory! [Unguru 2004, 405]

The reader will forgive me for these long quotations, but it seems to me essential to establish the frame of reference in which to insert Unguru's harsh criticism of the methodology adopted by Rashed in his commentary on the Arabic versions of Apollonius' text.

The controversy raised by the Israeli historian provoked both more or less violent replies and a rich debate that lasts until today. Firm responses came from mathematicians targeted (and, indeed, offended) by Unguru's words; among them, I mention Van der Waerden, Hans Freudenthal, and André Weil.[2] Their replies prompted a debate that lasted until 1979, when Unguru himself replied with an intervention that was unfortunately rejected by the *Archive*.

It is not my task to give a full account of this important debate, which touched upon historiographical problems (the consistency of the hypothesis

[2] The replies by Van der Waerden and Weil, as well as the response by Unguru, are collected in Christianidis 2004. Freudenthal's reply is reprinted in Freudenthal 1977. Another important contribution to this debate is by Weil in his speech offered at the plenary session of the International Congress of Mathematics held in Helsinki in 1978 [Weil 1980].

of "geometric algebra"), more general themes (the very definition of the concept of algebra), and properly methodological questions (the legitimacy of a modern reading of classical texts and its usefulness toward their interpretation). I limit myself to offering a quotation from Weil 1980 because it characterizes well the methodological questions at stake:

> How much mathematical knowledge should one possess in order to deal with mathematical history? According to some little more is required than what was [ed. needed] to [ed. understand] the authors one plans to read about; some go so far as to say that the less one knows, the better one is prepared to read those authors with an open mind and avoid anachronisms. Actually, the opposite is true. An understanding in depth of the mathematics of any given period is hardly ever to be achieved without knowledge extending far beyond its ostensible subject-matter. More often than not, what makes it interesting is precisely the early occurrence of concept and methods destined to emerge only later into the conscious mind of mathematicians; the historian's task is to disengage and trace their influence or lack of influence on subsequent developments. [Weil 1980, 231]

Weil rejects the charge of anachronism:

> [A]nachronism consists in attributing to an author such conscious knowledge as he never possessed; there is a vast difference between recognizing Archimedes as a forerunner of integral and differential calculus, whose influence on the founders of calculus can hardly be overestimated, and fancying to see in him, as has sometimes been done, an early practitioner of calculus. [Weil 1980, 232]

In the four decades since 1979, virtually no text on the methodological issues related to the study of ancient mathematics (Egyptian, Babylonian, Greek, and Arabic) has failed to refer to the debate reconstructed here. For the sake of the interested reader, I compiled a list (albeit incomplete) of some works that refer to this debate [see p. 220below]. In the most recent ones, the reader will find additional bibliographical information. Here, I am content to recall two points that have emerged.

David Rowe writes that "Alexander Jones told me that Unguru's position could now be regarded as the accepted orthodoxy" (« le gardien du temple » mentioned by Rashed) [Rowe 2012, 37]. Evidently, according to many scholars, the position defended by Unguru has gained ground and is consolidated to the point of being perceived as a sort of orthodoxy. Jens Høyrup makes the following remark:

> As analysis of the writings of the actors involved shows, these have rarely read each other's works with much care. That already holds for many of those who have claimed inspiration from Zeuthen, but those who have criticized the idea

> have felt even less obliged to show that they knew what they spoke about. [Høyrup 2016, Abstract]

This dispirited assessment shows that the debate is far from over.

Such is the context for Unguru's critical review of the commentary by Rashed on book 4 of Apollonius' *Conics*. I believe that this context helps us to understand why the excerpts from Rashed were deemed inadmissible by Unguru. It is time now to turn to Unguru's objections.

With respect to excerpts 1 and 2 on page 207, Unguru recalls the main lines of his criticism of geometric algebra:

> This is how Heath and Zeuthen proceeded when appealing to geometric algebra in their elucidation of the *Conics* and this is also the "historical" methodology of Bourbaki.... Still, Rashed's reasons for calling on "algebraic geometry" (*sic*!) as his main historical interpretative tool are different, one being instrumental and the other historiographic. [Unguru 2010, 34]

It is worth recalling that, in his avant-propos to the first volume of his edition of the *Conics*, Rashed had distanced himself from the interpretations linked to the hypothesis of geometrical algebra:

> Th. Heath n'a pas hésité à lire les *Coniques* à la lumière de la géométrie algébrique. Plus encore, il a justifiè cette lecture par la fameuse doctrine de "l'algèbre géométrique des Grecs", déjà défendue par Zeuthen et Tannery, et selon nous historiquement insoutenable. [Rashed 2009, viii]

Furthermore,

> Dire que les *Coniques* sont un livre de géométrie, c'est enfoncer une porte ouverte. Il suffit de jeter un coup d'œil sur ce traité pour y constater l'absence de tout équation d'un courbe plan et, d'ailleurs, du moindre concept algébrique. On vérifiera, par exemple...que le concept central de symptoma n'est nullement équivalent à celui d'équation. [Rashed 2009, vii]

Unguru has clarified this. He also makes the following remark in a footnote: "Surprisingly, and inconsistently, it seems to me, Rashed rejects the legitimacy of geometric algebra". This is an odd remark. Here I am content to recall Høyrup's point that "as soon as Unguru sees the word "algebra" [and I would add "geometric algebra"] he stops reading the explanations of the writer" [Høyrup 2016, 32]. What appears to be a contradiction has been clarified by Ivo Schneider:

> [I]t is, for example, necessary to distinguish whether an author represents the contents of a Greek mathematical text in algebraic dress while referring to the underlying geometrical argumentation of the original, or he claims the algebraic representation to correspond to the proper thought of the Greeks. [Schneider 2016, vii: trans. Høyrup 2016, 8]

This is exactly what is at stake, in my view. While Rashed presents Apollonius' text in a geometric argumentation, he does not ever derive the consequence that this (or something similar to this) is the "true" intention of the Greek mathematician and that he, too, dressed his algebraic reasoning in a geometric argumentation. Rashed himself makes this very clear when he speaks of his "instrumental" use of geometry:

> Bref, si l'usage instrumental d'une autre mathématique pour commenter une œuvre ancienne nous a semblé indispensable, c'est surtout en raison de ce rapport diffus d'identité et de différence qui les unit l'une à l'autre. Que l'instrument, le modèle, ne soient pas l'objet, c'est un truisme. Ils ne relèvent pas de la même Mathesis. [Rashed 2009, ix]

It is here, in my view, that we see the main reason behind Unguru's harsh criticism. Unguru thinks that this position is conceptually self-contradictory. It is an attempt, as it were, "to eat the cake and keep it too". Here Unguru's obsession with geometric algebra resurfaces:

> And, by the way, what exactly is, for Rashed, the difference between "geometrical algebra", which he rejects and, "algebraic geometry", which he embraces, though, at times...he seems to conflate and confuse them? [Unguru 2010, 38]

It seems to me that there are two issues that must be kept separate (according to what Schneider also indicates):

(1) the historiographical hypothesis that attributes modern methods and ideas to authors of another era—methods and ideas that are different only in some linguistic aspects from our own;

(2) the practice of translation into modern language of the mathematics of another epoch which only serves to help us better understand the mathematical contents expressed with notions very different from ours. It is not at all true that the latter implies the former, or that it is contradictory to use the latter while rejecting the former. In other words, one can discuss the usefulness of a translation but certainly not its legitimacy.

Let us quote, one more time, from the recent article by Høyrup: "[L]ater (well after 2001, perhaps in 2011) he [Unguru] told me that even he had to start with symbolic algebra in order to grasp Apollonius" [Høyrup 2016, 32]. This is exactly the need which Rashed has tried to address. While it is more than legitimate to discuss the method chosen by Rashed, I do not think that it is useful to attribute to him aims and ideas different from those he has in fact expressed. Unguru makes an analogous, and even harsher, criticism with respect to excerpts 3 and 4 on page 208 above. He describes them as an "unbelievable statement" [Unguru 2010, 35].

There is another point concerning what is perhaps the most significant statement made by Rashed, that is, the statement that the reference translation used for the first four books of the *Conics*, namely, that by ibn al-Haytham, is more reliable than the original redaction of Apollonius, the one preserved in Greek by Eutocius, to which reference had so far been made. On this point, Unguru is more open to dialogue, although with reservations:

> His text differs from the Eutocian Greek text in both trivial and substantive matters.... With the publication of this book, any student of book 4 of the Conics had at his disposal a welcome and necessary addition to the preserved Greek text, ultimately stemming from another, and better, manuscript tradition than that available to Eutocius. Sadly, this is served in the framework of an unacceptable historical approach. [Unguru 2010, 36]

We thus get a summary of Unguru's assessment of what Rashed has done.

Almost at the same time as Unguru, Nathan Sidoli expressed a much harsher criticism on this very issue: "His [Rashed's] procedure for this is quite incredible" [Sidoli 2011, 539]. Sidoli's review concentrates on this issue but is largely favorable. It is worth noting, however, that there is no trace in this review of the methodological issues that are so important to Unguru.

In subsequent years, two other reviews were published in *Aestimatio* (old series) on the edition and translation of classical works of Arabic mathematics by Rashed (in one case, written in collaboration with Hélène Bellosta). The first was by Clemency Montelle [2011] and the second, by Jeffrey Oaks [2014]. Both reviews take a critical stance with respect to the methodology used in the commentaries. This stance is similar to that of Unguru, but is much less "ideological". In fact, Montelle is cautious. Speaking of the careful study done by Rashed and Bellosta of the second-degree equation that algebraically translates the problem studied by Apollonius, she writes:

> Being careful to caution that this approach is worlds apart from the original conception, the algebraic orientation allows them, they maintain, to explore the structure of the work and investigate the systematic character and completeness of the approach of Apollonius. But while one can appreciate, with some effort, the intricacy of this work and its mathematical scope, such an orientation does not directly address the original issues the authors raised at the outset, such as motivation, exposition, and approach in the Greek geometrical context. [Montelle 2011, 184]

Later on, she adds:

> The parallel processes of analysis and synthesis, the very organizing feature of Apollonius's treatment of each configuration become muted as a result of this algebraic transformation. The documentation of investigation of the details and nuances of these processes in this context remains then for future scholarship. [Montelle 2011, 185]

Since I do not believe that Rashed and Bellosta thought that the aim of the algebraic translation was to give answers to the actual use of the methods of geometrical analysis by Apollonius, Montelle's review can be situated in the debate on the methodology used by the authors.

Another interesting observation, partly taken up by Oaks, concerns the lack of interest on the part of Rashed and Bellosta in engaging with the rest of the scientific community on the topics discussed in the book: "One notable absence in this publication is an engagement with the contemporary scholarly community" [Montelle 2011, 186]. This is an interesting observation, the discussion of which goes beyond the scope of this review.

Oaks' lengthy review takes up Unguru's theme of the perceived anachronism of the reconstructions offered in a modern language. He writes:

> We are used to this from Rashed. He has exhibited a string of publications in which he gives a modern reading of premodern mathematics, always careful in a preface to give a brief warning that the modern models are anachronistic. Yet, in practice, he treats them as if they are equivalent to the originals. [Oaks 2014, 43]

In support of this, Oaks cites the book reviews by Unguru and Montelle.

The belief that the modern reading of ancient texts is the origin of all evils, and that any other interpretation springs from this "original sin", appears to be central to this whole discussion. Of course, Oaks knows and cites Rashed's position on the subject; but, it seems to me, he believes that such warnings are purely a façade. (It will be remembered that Unguru, in this regard, believes that Rashed's theory and practice are contradictory.) It is crucial, therefore, to understand what exactly Rashed means by "instrumental". I will come back to this in my conclusion.

At any rate, Oaks deals with other issues that seem to me more substantial and deserving of a more in-depth discussion. I will mention some of them. To begin, Oaks expresses a position radically different from that of Rashed about the essence of Arabic medieval algebra. For Oaks,

> Medieval Arabic algebra was part of arithmetic. As a technic for solving numerical problems, it was practised alongside methods like a single and double false position, working backwards, and analysis. In these methods, one calculates

> directly with the numbers given in a problem to get the answer. What distinguishes a solution by algebra...is that an unknown number is named and an equation is set up and then solved. [Oaks 2014, 27]

It follows as a criticism that Rashed would be "turning arithmetic into algebra" [Oaks 2014, 33]. Instead, Rashed's position consists in underlining the elements of discontinuity between the arithmetic and the algebraic tradition inaugurated by al-Khwārizmi and developed by Abū Kāmil, which he finds in the very collocation of the study of the six canonical equations.[3] It is a classificatory study that precedes the resolution of individual problems and is logically independent from them. To put it in Rashed's words:

> Ce n'est pas lors de la résolution des problèmes qu'al-Khwārizmi trouve ces équations: la classification précède en effet toute problème. Celle-ci est résolument introduit comme première étape obligé de la construction d'une théorie des équations des deux premiers degrés, destinée à devenir le cœur d'une discipline mathématique. [Rashed 2007, 24]

Of course, this does not preclude that al-Khwārizmi was influenced by his predecessors:

> Cette démarche, à l'évidence inspirée par ses prédécesseurs et contemporaines dans d'autres disciplines, est doublement irréductible à ce qu'on peut rencontrer dans d'autres traditions: babylonienne, diophantienne, héronienne, celle d'Aryabhāta, ou celle de Brahmagupta. [Rashed 2007, 24]

Once the purely verbal problems are removed, this debate appears to be of great interest. In connection with this debate, I find Oaks' remark odd. While the invented algebraic versions are criticized (with regard to the solution of the equation $x^2 + 10x = 39$ in notes 10, 12, 17, and 23), he claims that in notes 9, 11, 13, and 18 we are given "purely arithmetical and, thus, more appropriate explanations for Abū Kāmil's procedures for finding the *māl* (x^2) directly". Perhaps an in-depth discussion of this apparent contradiction would have allowed a better understanding of their respective points of view. However, it seems to me that the difference between the two ways of treating the question goes back to the same Abū Kāmil who used al-Khwārizmi's

[3] The six equations are (in Rashed's translation of al-Khwārizmi):

> [D]es carrés sont égaux à des racines ($ax^2 = bx$); des carrés sont égaux à un nombre ($ax^2 = b$); des racines sont égaux à un nombre ($ax = b$); des carrés plus des racines son égaux à un nombre ($ax^2 + bx = c$); des carrés plus un nombre sont égaux à des racines ($ax^2 + b = cx$) ; des racines plus un nombre sont égaux à des carrés ($ax + b = cx^2$). [Rashed 2007, 98, 100]

resolutive formula to derive the root, and his own formula to derive the square (*māl*).

If the equation is given as $x^2 + ax = b$, his formula gives

$$x^2 = \frac{a^2}{2} + b - \sqrt{a^2 b + \left(\frac{a^2}{2}\right)^2}.$$ [Rashed 2012, 152]

This result is demonstrated by means of geometry. The algebraic translation of the first formula is direct and corresponds, as already mentioned, to what was presented by al-Khwārizmi. Rashed presents it through the geometrical steps of Abū Kāmil translated into algebraic notation. A second demonstration is more arithmetical. Rashed presents it only in its final form. Personally, I think that the meaning and the different demonstrations given by Abū Kāmil of the two equivalent formulas could have given rise to a much more interesting discussion than the polemics on the use of an algebraic/symbolic translation.

Another point of disagreement has to do with Rashed's statement that

> c'est dans ce livre [le troisième de l'algèbre de Abū Kāmil] en effet que l'on rencontre la première étude délibérément et entièrement consacrée à la l'analyse indéterminée rationnelle [Rashed 2012, 145].

This statement concerns a controversy of considerable historical importance—namely, the relations between the Arabic algebra and the work of Diophantus. This controversy is resumed, and somehow extended, in a subsequent review. Oaks writes:

> Diophantine analysis, according to Rashed, does not originate with Diophantus. This is a consequence of Rashed's claim that algebra was invented by al-Khwārizmi as a science of equations in the early ninth century. Since algebra is necessary for Diophantine analysis, Diophantus could not have practiced either one. [Oaks 2015, 105]

Oaks concludes his analysis as follows:

> Rashed denies indeterminate analysis to Diophantus by emphasizing superficial differences with Abū Kāmil, and by distorting the premodern arithmetic and algebra by rewriting everything with modern algebraic symbols. Then, by interpreting Abū Kāmil's text through these symbols, he invokes a grossly anachronistic interpretation of the solutions in terms of modern projective geometry. [Oaks 2015, 105]

So, again, the root of all misinterpretations would be in the translation of pre-modern texts into modern symbolism.

I will not elaborate on this issue and will not expand on Oaks' criticism [2015] of Rashed's interpretation of the work by Viète, which goes beyond the scope of these remarks.

The final point of Oaks' harsh criticism concerns the translation (in the commentary) of the indeterminate problems of Abū Kāmil in terms of algebraic geometry. In this case, the criticisms are similar to those of Unguru. As said before, I believe that to understand (even without possibly sharing) Rashed's position, it is necessary to read his definition of the concept of instrumental reading carefully. I will return to this in the light of what is offered in Rashed 2012.

Rashed relies on the following assumption: even if a philologically rigorous reconstruction is indispensable, the idea that it is possible to interpret an author who lived several centuries ago relying exclusively on this philological rigor is illusory:

> Rédigés il y a plus de onze siècles, ces traités le furent dans un contexte totalement étranger au nôtre, que nous ne connaissons pas et qui ne nous est que partiellement accessible. La tentation la plus immédiate, à laquelle certains n'ont pas résisté, est d'interpréter Abū Kāmil à l'aide de ses propre mots. Illusion d'un apprenti-philologue. [Rashed 2012, ix]

Instead, Rashed proposes an alternative approach in which ample use of mathematical models that are based on modern language is made:

> Il s'agit…de combiner une analyse philologique sûre, une histoire de l'élaboration du texte et des pratiques et procédés mis en œuvre par son auteur pour le rédiger, et, enfin, des modèles mathématiques construit à partir des disciplines que ce texte a contribué à fonder et, donc, appartenant à des mathématiques postérieures à celui-ci, modèles aptes à révéler la mathesis de l'auteur. [Rashed 2012, x]

This must go hand in hand with the utmost care not to confuse the model with the original text:

> Mais le recours à ces modèles n'est que instrumental: indispensable, en raison de ce rapport diffus d'identité et de différence que relie les contextes, l'algèbre de Abū Kāmil aux disciplines modernes, l'instrument ne se substitue pas à l'objet, cela va de soi. Il relève d'une tout autre mathesis. L'historien doit donc le manier avec prudence et sagacité, pour ne pas attribuer au texte ancien les notions véhiculées par l'instrument : le modèle. [Rashed 2012, ix–x]

In conclusion, I can say that it is unavoidably necessary for any historian—at least, it seems so to me—to read an ancient text first by translating it into modern terms so as to grasp its profound mathematical meaning, and only then to look for the thread that, in the given historical circumstances, the author could have followed concretely. This makes understanding of the ancient text easier for the modern reader, even if not a specialist ("adding a mathematical commentary; this will allow the modern reader to follow more easily, without problems in language or overlong descriptions" [Rashed 2013b, 34]). At the same time, it enables the reader—thanks to the commentary, the sole goal of which is "mettre en lumière le visée de la recherche géométrique menée par Apollonius" or the other relevant authors to plumb "la profondeur de ses concepts et de ses résultats et en apprécier la richesse, per cui il nous a fallu…emprunter d'autre modèles mathématiques inventés plus tard" [Rashed 2009, v].

With reference to the *Conics*, Rashed claims that

> les objets géométriques étudiés dans les Coniques possèdent bien ces propriétés, qui ne seront appréhendées et révélées que par les successeurs d'Apollonius, depuis Desargues. C'est donc en restant fidèle à la pensée du mathématicien alexandrin que l'historien peut s'inspirer de ces propriétés, pour mieux pénétrer cette réalité mathématique que celui-ci abordait les moyens de la géométrie de son temps. [Rashed 2009, 78]

This is a historiographical picture that places historical research in direct relation to the past both with respect to the work studied (the past that has supplied to the author "les moyens de la géométrie de son temps") and with the potential developments contained implicitly in the work and which can be explicated in a dialectical relationship with the creation of new means of analysis.

Thus, in my opinion, the algebraic reading made by the 17th-century mathematicians (Viète and Descartes, among others) of the second book of Euclid's *Elements*, even if it led to controversial and partly unacceptable historiographical hypotheses, has certainly thrown a new and clearer light on the *Elements* and provided a new key to reading them. Without the new methods of Monge and Poncelet, it would have been impossible for historians to frame the works of Desargues, Pascal, or La Hire correctly. Therefore, the work of mathematicians and that of historians of mathematics appears to be in close relationship, without blurring their respective specificities.

The debate on these issues would have been extremely fruitful had it not been vitiated by purely ideological prejudices. I would like to conclude by quoting the opinion of a friend and colleague who summarizes the question very well:

> It is necessary, in my opinion…to explain things in a more modern language, for two reasons: first because the use of a more refined language highlights merits and defects of the original view; this places the original view in a more exact scientific and historical perspective and, ultimately, makes us better understand what the authors at the time were trying to do; second because if this is not done, the original view remains incomprehensible to the vast majority of today's mathematicians, which is contrary to what is said that should be done, namely, to bring the two communities of historians of mathematics and professional mathematicians together.[4]

4 C. Ciliberto, personal communication, 13 Aug 2017.

APPENDIX

I list a few works on the debate about "geometric algebra", with a special concentration on the last 15 years. This incomplete list is organized chronologically. Where bibliographical information is missing, it can be found in the Bibliography which follows.

J. L. Berggren 1984.

W. R. Knorr. 1990. "Nouvelles approches des mathématiques. Sources, problèmes et publications". *Impact* 159: 237–251.

D. Rowe. 1996. "New Trends and Old Images in the History of Mathematics". Pp. 3–16 in R. Calinger ed. *Vita Mathematica*. Washington, DC.

M. Galuzzi. 2002. trans. "Premessa". Pp. i–vii in A. Weil. *Storia della matematica: come e perché e come*. Milan.

R. Netz. 2004. *Problems to Equations*. Cambridge, UK.

S. Unguru 2004.

N. Sidoli. 2006. Review of Christianidis 2004. *Historia Mathematica* 33: 491–493.

D. Rowe 2012.

L. Corry. 2013. "Geometry and Arithmetic in the Medieval Traditions of Euclid's Elements: A View from Book II". *Archive for the History of Exact Science* 67: 637–705.

N. Sidoli and G. Van Brummelen. 2014. *From Alexandria, through Baghdad*. New York.

V. Blåsjö. 2016. "In Defence of Geometric Algebra". *Archive for the History of Exact Science* 70: 25–359.

M. Sialaros and J. Christianidis. 2016. "Situating the Debate on 'Geometrical Algebra' within the Framework of Premodern Algebra". *Science in Context* 29: 129–150.

I. Schneider 2016.

J. Høyrup 2016.

BIBLIOGRAPHY

Berggren, J. L. 1984. "History of Greek Mathematics: A Survey of Recent Research". *Historia Mathematica* 11: 394–410.

Christianidis, J. 2004. ed. *Classics in the History of Greek Mathematics*. New York.

Freudenthal, H. 1977. "What Is Algebra and What Has It Been in History". *Archive for the History of Exact Science* 15: 189–200.

Høyrup, J. 2016. "What Is 'Geometric Algebra', and What Has It Been in Historiography?" Preprint 15 January 2016. http://akira.ruc.dk/ jensh/Publications/2016_What%20is%20'Geometric%20Algebra'_S.pdf. In print: *AIMS Mathematics* 2 (2019) 128–160. Citations are from the preprint.

Montelle, C. 2011. Review of Rashed and Bellosta 2010. *Aestimatio* os 8: 183–187.

Oaks, J. 2014. Review of Rashed 2009. *Aestimatio* os 11: 24–49.

——— 2015. Review of Rashed 2012. *Aestimatio* os 12: 105–109.

Rashed, R. 2007. *Al-Khwārizmi. Le commencement de l'algèbre*. Paris.

——— 2008. *Apollonius de Perge. Coniques. Tome 1.1: Livre I texte grec et arabe établi, traduit et commenté*. Berlin

——— 2009. *Apollonius de Perge. Coniques. Tome 2.2. Livre IV: Commentaire historique et matématique*. Berlin.

——— 2012. *Abū Kāmil. Algèbre et analyse diophantienne*. Berlin.

——— 2013a. *Histoire de l'analyse diophantienne classique. D'Abū Kāmil à Fermat*. Berlin.

——— 2013b. *Ibn al-Haytham and Analytical Mathematics. Translated by S. Glynn and R. Wareham*. Abingdon, UK/New York. Translation of the French edition of 1993.

Rashed, R. and H. Bellosta. 2010. *Apollonius de Perge*. La section des droits selon des rapports. Berlin.

Rowe, D. 2012. "Otto Neugebauer and Richard Courant: On Exporting the Göttingen Approach to the History of Mathematics". *Mathematical Intelligencer* 34: 29–37.

Sidoli, N. 2011. "Apollonius' *Conics*: The Greek and Arabic Traditions". *ISIS* 102: 537–542.

Schneider, I. 2016. *Archimedes*. New York.

Unguru, S. 1975. “On the Need to Rewrite the History of Greek Mathematics”. *Archive for the History of Exact Science* 15: 67–114.

——— 2004. “Methodological Issues in the Historiography of Greek Mathematics”. See Christianidis 2004, 383–461.

——— 2010. Review of Rashed 2009. *Aestimatio* os 7: 29–42.

Weil, A. 1980. “The History of Mathematics: Why and How”. *Proceedings of the International Congress of Mathematics, Helsinki 1978. Academia Scientiarum Finnica* 1: 227–236.

Understanding the Science of Other Cultures

by

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Abstract

A discussion of *Science in the Forest, Science in the Past* edited by Geoffrey Lloyd and Aparecida Vilaça.

About the Author

PAUL T. KEYSER studied physics and classics at Duke University and at the University of Colorado at Boulder, where he earned doctorates in physics and in classics. After some years of research and teaching in classics at the University of Alberta (Edmonton), Cornell University, the Center for Hellenic Studies (Washington DC), and other places, he returned to his first love, programming. He worked as a software engineer at the IBM Watson Research Center, then at Google, Climate, and Bridgewater. He is currently a software engineer at Relativity. His publications include work on gravitational physics, stylometry, and ancient science and technology. He is co-inventor on some patents in computer science. Three co-edited books have appeared: *Greek Science of the Hellenistic Era: A Sourcebook*, *Encyclopedia of Ancient Natural Scientists*, and *Oxford Handbook of Science and Medicine in the Classical World*. Moreover, he has published a monograph, *Recovering a Late-Antique Edition of Pliny's* Natural History. Current projects include papers on ancient mechanics and on Plato's astronomy, as well as books on the evolution of ancient science and on "classic" lineages.

Those whose occupation it is to study histories of sciences begin, like Aristotle, with wonder: "What could they mean by that?" Some of those studies consider the sciences of people from long ago, whereas others consider the sciences of more recent people but from cultures different from those of the student. Participants in such efforts mostly know to expect a conceptual chasm and yet hope to cross it. Moreover, even when studying sciences within one's home culture, there are arresting moments of defamiliarization and dizzying chasms open before our footsteps.[1] Conversely, philosophers and theologians have often made hegemonic claims for their approach, arrogating titles such as "Queen of the Sciences". What then to say when a diverse tribe of scholars sets out to explore "Science in the Forest, Science in the Past", as presented in a special issue of *HAU* [Lloyd and Vilaça 2019]?

First, a little context. Some early Greek scientists eagerly explored the conceptual worlds of the "alien" cultures to which they had some access; Babylonians, Egyptians, Indians, Persians, and Scythians are attested as informants or teachers. ("Alien" of course cuts both ways, as Xenophanes famously remarked [Diels and Kranz 1951, frr. 21B15–16], speaking about how foreigners depict the gods—that is, like themselves.) No doubt, the attempts of those Greeks to explore (or exploit) the scientific ideas of those neighbors would not pass muster in a contemporary department of anthropology. But the activity attests to a human belief that other peoples' ideas may be commensurate with, and even relevant to, our own concerns. The Romans went further, of course, and besides the fascination many of them felt for Celtic, Etruscan, or Punic wisdom, there was a broad-based "translation movement" that rendered Greek science, or some parts of it at least, accessible in Latin to interested readers [Keyser 2010].

Travelers may import new ideas and ways of thinking, easing the task of an anthropologist of science, but narrowing any results to what the travelers happen to import. Such down-the-line trade has long been a feature of human cross-cultural interaction, and allows for a good deal of assimilation and transformation. The remark by Francis Bacon—that the greatest

[1] E.g., Kidder 1981 and Traweek 1988.

modern inventions are printing, gunpowder, and the magnetic compass, but no-one knows their origin—exemplifies that sort of assimilation and transformation [Bacon 1620, 147–148: cf. Boruchoff 2012, esp. 138]. It also amuses, if only because we know that all of them came west from China.[2] The long and rich interaction between the scientific cultures of the Islamic caliphates and those of the Latin west displays another kind of trade in ideas and sciences. Translation was essential to that set of enterprises, starting with the translations of Greek scientific literature into Syriac and Arabic in the eighth century AD, but including also the numerous later renderings of Arabic and Greek texts into Latin.

So we find ourselves immersed in a long-running stream of cultural interaction around science. That stream as I have described it embodies an activity that assumes the possibility of translation and communication. Moreover, it is a "mercantile" style of interaction, in which all parties extract from the sciences of the respectively "alien" culture(s) mostly what they themselves expect to be "useful" for their own interests. That limits the degree to which "alien" science can be understood because technologies are more fungible than ideas.[3]

The idea that understanding the science (or poetry) of an "alien" culture might be of interest and worthwhile for its own sake is radical and rare in human history, as it seems. When the Romans or the Arabs translated Greek science, it seems that they expected to learn something useful about the world. In either case, it is debatable to what extent the dominant culture believed that Greek literature or culture was of value *per se*.[4] Romans were certainly fascinated by Greek culture and some Romans at least felt that that their conquest of the Greek world had enriched the Roman world by more than mere territory or *Macht*.[5] Modern enterprises such as ethnobotany or

[2] Perhaps we should add eyeglasses, which are first attested in the west around AD 1300? Laufer 1907 argues for a Chinese origin, but Rosen 1956 and Needham 1962, 118–122 reject this: see also Ilardi 2007, 3–50.

[3] Medical anthropology is indeed highly pragmatic: Pfeiffer and Nichter 2008; Goodson and Vassar 2011; Joralemon 2017; and Singer, Baer, Long, and Pavlotski 2020.

[4] The earlier case of Assyrians studying Sumerian literature might reflect a similar response. On this activity, see Oppenheim 1977, 16–24, 235–238, 249, 255–256; and Michalowski 2017, esp. 205–207.

[5] Cicero describes Greeks as excelling Romans in all forms of literature [*Tusc*. 1.3], and Horace remarks that conquered Greece took Rome captive, thus bringing *artes*

ethno-agriculture operate at least in part with a similar goal of (possibly mutual) benefit.[6]

None of that is anthropology, which I understand to be occupied with the study of "alien" cultures *per se*. That is, cultures become topics of study not because they might provide something useful, but because they are of intrinsic interest. (That distinction is not absolute: learning about another culture in an appreciative way will naturally lead to reflections and reconsiderations about one's own culture.) But that long history of cultural exchange, whether between neighbors as when Greek scientists reached out to Egypt or Mesopotamia, or between a conquered ("colonized") people and their new overlords, runs as an undercurrent beneath all our modern attempts to perform anthropology.

I am no anthropologist, but we hope that the silos of scholarship are not opaquely incommensurable. Moreover, I hold that it is best when there is "free trade" and open dialog between disciplines. (Classicists, historians of ancient science, and other students of ancient cultures may be seen as practicing a kind of time-traveling anthropology [cf. Holmes 2020].) Given that Geoffrey Lloyd was a leading participant within the flash-tribe that gathered at the conference to explore these questions, I think that readers can have confidence that some degree of communication was both a goal and an outcome. The scholars pursued various paths into the forest, but a chief discursive frame encompassed the issue of "ontologies". Some of the papers were more explicitly concerned with that frame. Others followed a path around mathematics. A third, smaller cluster of papers explores some aspects of artificial intelligences, or as I would prefer to label them, cyborgs.[7]

1. Ontologies

Although invoked as a guiding inquiry of the conference, the "clash of ontologies" did not deeply engage many of the participants, as Lloyd and Vilaça remark [179–180] in their closing essay. Nevertheless, the issue is latent in many of the papers and is worth exploring. One simple example

to Rome [*Epist*. 2.1.156–157]. Somewhat differently, Vergil [*Aeneid* 6.847–853] predicts that Rome shall excel in rule, let others excel in arts.

6 See Prance, Chadwick, and Marsh 1994; Minnis 2000; Soejarto, *et al.* 2005; and Voeks 2018.

7 Two papers in this volume fall outside these categories and definitely outside my expertise, so I will keep silent: Kuper, "Deconstructing Anthropology" [10–22] and Herzfeld, "What is a Polity?" [23–35].

of the problem would be the classification of animals, which for modern science involves distinctions between mammals, birds, and fish (among others). However, a more ecocentric ontology might exploit categories like "flying creatures" or "creatures dwelling in Air" (and thus bats, bees, and finches are close relatives) as well as "swimming creatures" or "creatures dwelling in Water" (and thus carp, dolphins, and shrimp are close relatives). So the two distinct ontologies, ecocentric and phylocentric, encode different concepts—but the ontologies are not incommensurable or incommunicable.

Vilaça, in the contribution "Inventing Nature: Christianity and Science in Indigenous Amazonia" [44–57], addresses contrasting the ontologies of humans and animals of the Wari' and of modern science. For the Amazonian Wari', animals and humans share a great deal, whereas for some strands of European and Mediterranean thought, humans are radically distinct from animals. Likewise, there is a contrast between the meanings assigned to singularity and duality: for the Wari', singularity (the number one and related concepts) is lonely and incomplete, whereas duality (the number two and related concepts) is richer and more potent. That contrasts with a tradition in European thought (found among Pythagoreans, as well as Neoplatonists and monotheists) that "the One" is primal, original, and Good, whereas "the Dyad" is the opposite of those. But traditions in western, or even modern, sciences about the significance of numbers, or the relation of humans to animals, are themselves not unitary. Descartes' view that animals are simply bionic machines was never the only choice, and there is a rich array of debate and tradition in European and Mediterranean science and philosophy about the ontology of animals vis-à-vis humans [Sorabji 1993]. (Moreover, I would respectfully but strongly dissent from the claim that modern western science has "Christian foundations" or "is monotheist" [49]. Science hardly began with the 17th-century "Scientific Revolution", and several other contributions to these proceedings emphasize that point [see Lloyd, p. 37] and especially the contributions on mathematics, below.)

Translation, too, implicates ontologies, and necessarily so. Any translation is an assertion of semantic proximity, which in turn is an assumption of overlapping ontology. As Lloyd argues in "The Clash of Ontologies and the Problems of Translation and Mutual Intelligibility" [36–43], even such "simple" words as "fire" and "water" are slippery to translate. He is taking those as terms that are not "highly theory-laden" [38], but I think that his own discussion shows that they are actually theory-laden. He cites translations of those words among Chinese, English, and Greek—and at least in Greek and Chinese, the chosen example terms refer to fundamental "elements" or "phases" of matter. To translate ancient Greek «ὕδωρ» (*hydōr*) or Chinese «水» (*shuǐ*) into English "water" is both "obvious" and yet missing many resonances; likewise in translating Chinese «火» (*huǒ*) or Greek «πῦρ» (*pūr*) as "fire" [Lloyd 2012, 85–89]. Other "obvious" terms may be translated with no more—and no less—risk of ontological clash, such as "book" or "city," or even "food" or "school". Any effective translation will arrive accompanied by a host of adjutants, serving to qualify, nuance, or clarify.

Lloyd, as he has done elsewhere, takes an optimistic position on translation. He holds these claims to be foundational [36]:[8]

> (1) no translation is ever perfect and complete, all are provisional and revisable; (2) there is indeed no perfect, complete, mutual understanding, even when all interlocutors share the same natural language. On the other hand, (3) some understanding is always possible, even across divergent systems, and even across incommensurable paradigms, even if (4) there is no neutral vocabulary in which it can be expressed. This depends (5) on allowing that the terms in any language exhibit what I call "semantic stretch".

As Lloyd goes on to argue [39, 41], there is no neutral or universal language in which to disambiguate terms and semantics; one just has to work it out tentatively and provisionally. He points out that "incommensurability" is not a threat, but is instead an opportunity [41]. I would go further, and claim that an apparent "incommensurability" is only provisional, and is always a sign that can elicit wonder and curiosity, and thus reflection, engagement, and exploration.

I offer an enlightening example from modern science of a semantic stretch that is also an issue of apparently clashing ontologies. Chemists often speak of chemical bonds [Pauling 1960] and the usual initial distinction is between the typical bond of "inorganic" chemistry and the "covalent" bond, as found in "organic" chemistry. The "ionic" bond is between two atoms, in which one or more electrons are entirely transferred from one atom to the other. The canonical example is salt, in which a single sodium atom yields an electron to a single chlorine atom. In simplistic contrast to this is the "covalent" bond, that is, in compounds of carbon, hydrogen, oxygen, and nitrogen (primarily). In the covalent bond, there is no wholesale transfer, and the atoms participating in a bond share one or more electrons. One simple example is water, in which each of two hydrogen atoms shares its electron

[8] Lloyd here reprises 1987, 172–214, esp. 174–181, citing Porzig 1934 as similar, and 208–214: cf. also Lloyd 2002, 123, where again Porzig 1934 is credited.

with a single oxygen atom. (These terms originated in the 1930s, although the concepts were being explored 20 years prior.)

But in fact, the ontology is unstable, since the ionic or covalent character of a bond is a matter of degree, not dichotomy. Moreover, other types of bonds also exist, such as the "hydrogen bond", in which a hydrogen atom participates both in its canonical single covalent bond and in a weaker bond with a third atom that has some electrons on its surface that are not participating in any other bond. This bond-type is responsible for many of the remarkable properties of water. Further, compounds of boron and hydrogen (known as "boranes") display yet another type of bonding, in which the single electron of a hydrogen atom is shared among three atoms, namely, two boron atoms and the hydrogen atom itself. The complexities ramify, and there are, for example, "clathrates"—compounds in which a large molecule forms a "cage" in which a smaller molecule is bound. All of this shows how even within a single scientific discipline and in a single language, there is an instability, or at least complexity, of ontologies. That seems to chime well with Lloyd's advice [41] that investigators allow for the "multidimensionality of the explananda".[9]

The essay by Jardine, "Turning to Ontology in Studies of Distant Sciences" [172–178], employs the useful covering term "distant science(s)" to refer alike to sciences of the past and to those of "alien" cultures. Jardine argues for a pluralist view of science(s), so that, in his example, "indigenous practices of pigment preparation" would cohere with western industrial lab chemistry. Indeed, many journals are devoted to understanding indigenous or ancient practices of pigment preparation, along with many other "chemical" techniques: e.g., *Archaeometry* (1958–). Such work exemplifies some aspects of the practice of translation, that is, of commensurability, for materials science(s), across cultures and time. The concluding remark [176] is well worth quoting:

> For however deep the understanding we may achieve by "going native" in the forest or the past, we owe it to ourselves and our audiences to provide comprehensible interpretations.

Jardine calls it "the principle of responsibility," evoking a strong commitment to working hard to perceive the nature of the commensurability, and to translate that for readers.

9 Lloyd has very insightfully explored ontologies, and the issues of translation around them, in 2015, 88–108.

2. Mathematics

Turning now to the papers that followed a path around mathematics, we have a contribution by de Almeida asking "Is There Mathematics in the Forest?" [86–98], plus three contributions on each of three literate cultures: Chinese, Greco-Roman, and Indian. Those three are, respectively, "Different Clusters of Text from Ancient China, Different Mathematical Ontologies" by Chemla [99–112]; "Mathematical Traditions in Ancient Greece and Rome" by Cuomo [75–85]; and "Shedding Light on Diverse Cultures of Mathematical Practices in South Asia" by Keller [113–125]. These contributions exist within a larger framework of "ethnomathematics", itself a problematic term, and an active set of fields.[10] Those fields offer studies of mathematical notation in literate cultures [see Chrisomalis 2010], studies of mathematical practice in specific communities,[11] and plenty of studies of learning styles.[12]

De Almeida argues for "the existence of universal mathematical capabilities," supported by evidence in the form of "recursive rules used to produce consistent patterns that are transportable across distinct domains of thought and action" [86]. Even without the restriction "recursive", that would be a proper definition of the work of mathematicians in any culture. Detecting recursion is a pleasant extra accomplishment, and not just because recursion is a concept of modern western mathematics that is widely used in writing computer code. It also foregrounds a fundamental human capacity, visible also in the structures of human language. The primary and extended example concerns how kin relations can encode abstract maths, among the Cashinahua (better, "Huni Kuin") of Acre state in western Brazil and nearby Peru [90–93]. As de Almeida convincingly demonstrates, kinship structure encodes formal mathematical statements, such as multiplicative identity ($f * e = f = e * f$, with "e" the identity element for the operation "*", and "f" any element of the set over which the operation is defined). This encoding represents the rules for combining kinship terms, such as $epa * betsa = epa$ (translated as "same-sex parent * same-sex sibling = same-sex parent"). The vocabulary and grammar of the kinship system also encodes the self-inverse property ($f * f = e$), as well as others.

[10] See especially Barton 1996, Vithal and Skovsmose 1997, and Rivera and Rossi Becker 2008.

[11] Many such, e.g., Millroy 1991 and Chahine and Naresh 2013.

[12] Widely cited is Eisenhart 1988.

To demonstrate further that cross-paradigm translations are possible [93–94], de Almeida provides a translation involving irrational roots (of 2, 3, and 6) across the chasm between Euclid and Dedekind.[13] De Almeida shows how the proof is valid both in Euclid's paradigm of irrational values and in Dedekind's paradigm for thinking irrational numbers (the "Dedekind cut", which defines an irrational number as the limiting boundary between a pair of disjoint sets of rational numbers). Another, more briefly drawn translation involves Euclid, *Elem.* 9.20, which proves that, given any list of prime numbers, there exists a prime not on the list, and thus that the set of primes is unbounded. As de Almeida says, we must pay close attention to what Euclid does, and does not, argue; and because of Euclid's careful language, the argument takes the same form, even after a paradigm shift in the theory of infinity, because it does not implicate any specific theory of infinity [94]. Another point also requiring careful attention is that the proof asserts that the number composed by adding 1 to the product of the primes in the list is either prime or else has a prime factor that is not in the list. To see that 1 plus the product of the primes in the list need not be prime itself, start with a list of the primes 3 and 5, and find that $(3 \times 5) + 1 = 16$, where 16 has a prime factor not on the list, namely, 2. Likewise, starting with the first six primes, namely 2, 3, 5, 7, 11, and 13, one finds that 30,031 has the prime factors 59 and 509, not in the initial list.

Chemla's contribution on Chinese culture considers school texts of the 7th century AD, and tomb texts from "last centuries BC"—the two clusters "testify to two different ways of practicing mathematics, which related to different material practices" [99]. As Chemla says, using actor-created corpora is a better way to investigate ontologies in that it is both more principled and more effective. Such corpora reflect their underlying ontology in their technical language and material practices [100]. Chemla shows in detail that texts in the later cluster all regularly use rods for computing that are laid out on a surface in decimal place-value arrangements [100–109]; this is explicit in the *Mathematical Canon by Master Sun*, and implicit in other texts of the same later corpus.[14] In contrast, the algorithms described in two tomb scrolls from *ca* 200 ± 15 BC, as well as some Qin-era texts in Beijing, also use rod-numerals; but they do not describe the operations of division

[13] Here, de Almeida follows Stillwell 2016, 156–157.

[14] The contribution here relies upon the valuable work of Chemla 2013 and Volkov 2014.

and extracting roots in words that reflect the same ontology as in the commentaries [109–110]. Instead, the earlier mathematical texts "seem to reflect the use of operations as means to reach a result rather than as processes to be pondered" [109].

Cuomo's contribution on Greco-Roman culture considers the tradition(s) of Greek mathematics: the "theoretical" tradition and the allegedly contrasting "practical" tradition. The distinction is ancient and starts, as Cuomo demonstrates, with Plato and other authors. The "theoretical" tradition is mathematics as conceived by Plato, or as practiced in the pages of Euclid's *Elements*; the "practical" tradition is mathematics as seen in the corpus of Heron of Alexandria (mid-first-century AD). Cuomo views the dichotomy as unstable and shows how practices migrated across the very permeable boundary, and how modern attempts to maintain the distinction founder [75–81]. Instead, an approach using "situation-specificity, or situated learning" is to be preferred, along with "code-switching" [81]. That is, any given mathematician might produce more theoretical work in one situation and more practical work in another. Likewise, the language of a Greek (or any) mathematical work might vary between "theoretical" and "practical" depending on the intended audience or expected use of the work.

Moreover, Cuomo argues, an analysis of mathematical behavior in terms of situations is more responsive to details of the work and opens up more avenues for comparison, since similar situations might arise in quite distinct times and places. I would point out that the Archimedean corpus contains both "theoretical" works (such as *Spiral Lines* or *Sphere and Cylinder*) as well as "practical" efforts (such as *Division of the Circle*). Nor do the *Cattle Problem* or the *Stomachion* (however interpreted) easily fit into some binary classification. Likewise for Eratosthenes, both the "mean-obtainer" (*mesolabon*, a kind of slide-rule for extracting roots) and the *Geography* seem "practical" (or at least not "theoretical"); whereas the attested but lost work *On Means* would likely have been "theoretical".

Keller's contribution on Indian culture considers two contrasting practices of numbers, measures, and computations in South India [113]. One is documented in early Sanskrit mathematical treatises and commentaries (of the 7th to 12th centuries), the other in elementary mathematical educational texts in Tamil (of the 17th to 20th centuries). The Sanskrit mathematical texts present abstract mathematics, in which calculations are performed on "pure" (unitless) numbers, and decimal place-value numerals are used

[115–116]. The Sanskrit texts also present themselves as delineating a timeless discipline; that is, any given text claims to be “the reframing of a preceding treatise or of an orally transmitted doctrine” [115]. In contrast, the Tamil texts use Tamil numerals, which are decimal and non-positional, and the computations are made with units attached to the numbers [115–116]. Keller’s analysis focuses on two common kinds of computations found in both sorts of texts:

(1) computations of areas [116–120], and
(2) computations of gold fineness [120–121].

As Keller shows, the two corpora are not utterly distinct, and some specific problems or methods appear in both [122].

All three of these contributions on literate cultures conclude, analogously, that the allegedly distinct or dichotomous corpora are not in fact separated by an incommensurable chasm. Greek “theoretical” and “practical” mathematics, Chinese Tang-dynasty school-texts, and Qin- or Han-dynasty tomb-texts, as well as Indian Sanskrit texts and Tamil texts, all show communication across the chasms.

3. Cyborgs

Turning finally to the (small) cluster of papers that explore some aspects of artificial intelligences, we have Blackwell, “Objective Functions: (In)humanity and Inequity in Artificial Intelligence” [137–146], and McCarty, “Modeling, Ontology and Wild Thought: Toward an Anthropology of the Artificially Intelligent” [147–161]. In both cases, I think that the full perspective here is better described using the word “cyborg”. The artificial intelligences are considered under the same defamiliarized perspective as are the “distant” cultures of ancient China or contemporary Amazonia (to borrow the term from Jardine, as above). That is, the artificial intelligences are imagined as members of some “alien” culture that to be sure bears a rather special dependent relation to modern western culture but is nonetheless imagined as distinct or on the far side of a chasm. To express that uncanny relation, I want to use the word “cyborg”.

Blackwell focuses on “the subjectivities embedded in these mechanical systems, and the human satisfactions and ambitions in constructing them” [137]. Two different approaches to those subjectivities are made. The first is to examine, briefly, the perhaps surprising procreative aspect of cyborgs [138]. Blackwell writes that the artificial construction of simulated humans in fiction seems often to become powerfully gendered, perhaps alluding to the gendered nature of all human procreation. The figure of the AI engineer building sexy robots and falling in love with them has many fictional precursors, including that of Pygmalion. Indeed the Turing Test itself was first posed as an Imitation Game in which the challenge assigned was not for a computer to imitate a man but for a man to imitate a woman.

Blackwell sharpens the point by suggesting that such creations "often" result in some excess and some retribution, as if such involvements transgress some well-defined moral order. Certainly some cyborg fictions have such an element, and perhaps the transgression is that the creator mates with (usually) his creation, thus violating the taboo against incest. (Indeed, here the use of the word "cyborg" enables sharper focus on the problem.)

But I do not think that the (surely fictional) "singularity" is either inherently retributive or necessarily sexual. It certainly smacks of the divine to hypothesize that some being(s) would gain such extreme, even infinite, power. The imagined "singularity" is an overly-simplified extrapolation of current trends, without any physical model to explain or validate the specific direction or degree of extrapolation. Even without an actual infinity, we may imagine a growth of cyborg power to an unpleasant or risky degree—just as one might extrapolate (on well-grounded assumptions) three more familiar catastrophes: nuclear, biological, or climatic. On the one hand, nation-states or others might increase the number and power of nuclear weapons and thus run the risk of an extremely destructive war. Or, new kinds of zoonoses, whether natural or artificial, might increase in number and fatality rate, until some apocalyptic plague breaks out. Or, thirdly, the degree of global warming might increase to such an extent that the structures of modern global society would crumble. But such extrapolations are at least founded on scientific measurements and experiments, which thus provide means of analysis and form a basis for attempting to evade hypothesized bad outcomes.

Blackwell also engages in a second line of investigation about subjectivities by examining the language used to describe certain aspects of the making of cyborgs [139–144]. Here he addresses three specific phrases or labels:

(1) "objective function",
(2) "logistic regression", and
(3) "oracles" and "ground truth" (two terms that regularly travel together).

The terminology is not usually used by practitioners in an ambiguous way, but, indeed, as Blackwell says [141], many computer scientists are poorly trained in basic principles of epistemology, while many philosophers are poorly trained in basic principles of engineering, meaning that they happily talk at cross-purposes with the aid of ambiguous terminology that neither properly understands.

So there is the potential for the perception of an incommensurability or clash of ontology. An "objective function" is a kind of component of many pieces of software, and would likely be used to create any eventual cyborg [139–140, 142–144]. As Blackwell says, one example is the objective function that evaluates the relative goodness of search results from any search engine (whether Google, Bing, or DuckDuckGo). Such a function is a mathematical transformation that defines how closely a given measurable result (of a computation) adheres to some defined goal. The "objective" in the phrase is, as Blackwell says, the goal being sought; so an "objective function" might better and more clearly be called a "goal-function". It is unfortunate that, by the usual ambiguity of language, an "objective" function can seem to refer to something that is "objective", i.e., in contrast to something "subjective". So here the actual issue of cyborg subjectivity concerns the goal-functions used to program the eventual cyborg, which were of course developed by the programmers who presumably used their subjective best estimates of what would work well in addition to whatever evidence they accumulated by testing proposed goal-functions.

The second label, "logistic regression", refers to a mathematical procedure that fits data to a "yes / no" model, or indeed to any categorical model [140–141]. That is, in trying to evaluate data to see if, for example, the data are more consistent with one outcome (from a list of distinct outcomes) than with other outcomes (on the same list), this procedure is used. It is not perhaps a well-named procedure, but it is widely used in data-analysis. The procedure is not very specific to the creation of cyborgs but would likely be used to program some of their behavior. Again, the actual issue of cyborg subjectivity concerns the lists of distinct outcomes used to define any logistic regressions in the eventual cyborg, which were of course developed by the programmers who presumably used their subjective best estimates of what would work well in addition to whatever evidence they accumulated by testing proposed outcome-lists. (It is something of a red herring to suggest that logistic regression is tainted by its origin in eugenics, as Blackwell does [140], citing a paper on eugenics from 1947. Logistic regression is a mathematical technique, possibly valuable, that is independent of any early uses of it [see Cramer 2010 or Simonoff 2003].)

Third, there is the problem of "oracles" and "ground truth" [141]. As Blackwell writes, "supervised learning" depends on humans having labeled data or outcomes, so that the machine has a defined goal. The sense of "supervised" is that the data are human-labeled, as if "...; item #456, an outcome type "A"; item #457, an outcome type "D"; ...". Such labeling can be very labor-intensive when the quantity of relevant data is huge, as it often is. Sometimes instead, an existing system or database can be used. In any of these cases, the reference to an "oracle" or to the "ground truth" points at the human-labeled "right answer". So here again, the subjectivity within the cyborg is actually composed from the subjective judgments of the humans who tagged the data or outcomes.

Last, but hardly least, there is McCarty's contribution [147–161]. McCarty by his subtitle—"Toward an Anthropology of the Artificially Intelligent"—grabs the cyborg by its uncanniness. The key insight here is that the cyborg requires a model, i.e., an ontology, of the domain to be affected [147]. Moreover, McCarty addresses the defamiliarization of the "person" *via* the creation of mechanical "persons", i.e., cyborgs, as well as how those types of persons relate to one another, and the key role of Wiener's approach to cybernetics in enabling the comparison [147–148]. That is, Wiener saw that something like a control system (feedback loop with a sensor to detect the difference between the actual state of the system and the desired state of the system) would be a good model for cyborgs as well as for humans [Wiener 1966]. Now McCarty asks readers to imagine a Turing-test-like conversation with an actual cyborg and announces that we would feel alienated, that we would find ourselves faced with the chasm of incommensurability [148–149]. He writes that the cyborg would be "enigmatically and unresolvably both like and unlike us". How, I ask, is that situation different from what we manage every day, talking with the aliens all around us? It may differ in degree but it is not different in kind. The "anthropology" in McCarty's title both foregrounds the problem to be faced in dealing with cyborgs and also indicates the response. Indeed, he concludes that machine intelligence is commensurable with ours, but that we should not underestimate the difficulty of communication [154–155]. McCarty argues [155–156] for a slow evolution of "bridgeheads" of mutual understanding [citing Lloyd 2010]. In the end, he says that to talk about cyborgs is to talk about "an emergent manifestation of ourselves differently constituted" [156].

Less convincing is McCarty's intervention on the "plurality of ontologies" within computer science [149–153]. Taking as his point of departure the observation that work on computers regularly creates a multiplicity of ontologies, McCarty argues that this plurality shows that "the ontological question was from the very beginning implicit in the design of the stored-program computer" [150]. If the multiplicity of ontologies is intended to refer to the various object-hierarchies that constitute the structure of many programs, then this multiplicity would not be very meaningful. These object-hierarchies, which are also known as class hierarchies (with "class" here meaning something very like "category" or "type"), are created by the programmers *ad hoc* in order to organize their own thoughts and understandings about the program they are creating. Moreover, this mode of thought was not actually implicit in programs or computer architecture. Early programming languages, such as assembler, FORTRAN, ALGOL, or COBOL, had no notion of type-hierarchies. More recent languages include many that are constructed in terms of type-hierarchies; but even in those, the programmer can ignore that aspect of the language and write programs that do not reflect it at all. On the other hand, if the multiplicity of ontologies is intended to refer to the many object-hierarchies that organize the data being analyzed by the program, then again, this is not very meaningful. Such hierarchies are also *ad hoc* in that they are invented for the specific small set of problems being addressed in the current work of any given set of collaborating programmers. As McCarty says, such an ontology is "a practical inventory in a schema" [150]. One monistic attempt to create a hierarchy of everything has attracted adherents and criticism, namely, Cyc [https://www.cyc.com/], but has not yet produced any cyborgs.

4. Conclusion

Aliens of three kinds, then, have been encountered by the explorers whose reports grace the pages of this issue of *HAU*, a name that, as I understand it, refers to a gift. The volume is indeed freely available, and well worth taking the time to read. I encourage engaging and reflecting, and further reporting.

BIBLIOGRAPHY

Bacon, F. 1620. *Instauratio Magna*. London, UK.

Barton, B. 1996. "Anthropological Perspectives on Mathematics and Mathematics Education". Pp. 1035–1053 in A. J. Bishop, K. Clements, C. Keitel, J. Kilpatrick, and C. Laborde edd. *International Handbook of Mathematics Education*. Dordrecht.

Boruchoff, D. A. 2012. "The Three Greatest Inventions of Modern Times: An Idea and Its Public". Pp. 133–163 in K. Hock and G. Mackenthun edd. *Entangled Knowledge: Scientific Discourses and Cultural Difference*. Munster.

Chahine, I. C. and N. Naresh. 2013. "Mapping the Cognitive Competencies of Street Vendors and Bus Conductors: A Cross-Cultural Study of Workplace Mathematics". *Revista Latinoamericana de Etnomatemática* 6: 7–35.

Chemla, K. 2013. "Ancient Writings, Modern Conceptions of Authorship: Reflections on Some Historical Processes that Shaped the Oldest Extant Mathematical Sources from Ancient China". Pp. 63–82 in M. Asper ed. *Writing Science: Medical and Mathematical Authorship in Ancient Greece*. Berlin. https://dx.doi.org10.1515/9783110295122.63.

Chrisomalis, S. 2010. *Numerical Notation: A Comparative History*. Cambridge, UK.

Cramer, J. S. 2010. "The Origins and Development of the Logit Model". Pp. 149–157 in J. S. Cramer ed. *Logit Models from Economics and Other Fields*. Cambridge, UK. https://dx.doi.org/10.1017/CBO9780511615412.010.

Diels, H. and W. Kranz. 1951. *Die Fragmente der Vorsokratiker. Griechisch und deutsch*. 6th edn. Berlin.

Eisenhart, M. A. 1988. "The Ethnographic Research Tradition and Mathematics Education Research". *Journal for Research in Mathematics Education* 19:99–114.

Goodson, L. and M. Vassar. 2011. "An Overview of Ethnography in Healthcare and Medical Education Research". *Journal of Educational Evaluation for Health Professions* 8.4. Unpaginated. https://dx.doi.org/10.3352/jeehp.2011.8.4.

Holmes, B. 2020. "At the End of the Line: On Kairological History". *Classical Receptions Journal* 12: 62–90. https://dx.doi.org/10.1093/crj/clz027.

Ilardi, V. 2007. *Renaissance Vision from Spectacles to Telescopes*. Philadelphia, PA.

Joralemon, D. 2017. *Exploring Medical Anthropology*. 4th edn. New York.

Keyser, P. T. 2010. "Science". Pp. 859–881 in A. Barchiesi and W. Scheidel edd. *Oxford Handbook of Roman Studies*. Oxford. https://dx.doi.org/10.1093/oxfordhb/9780199211524.013.0055.

Kidder, T. 1981. *The Soul of a New Machine*. Boston, MA.

Laufer, B. 1907. "Zur Geschichte der Brille". *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften* 6: 379–385.

Lloyd, G. E. R. 1987. *Revolutions of Wisdom: Studies in the Claims and Practice of Ancient Greek Science*. Berkeley, CA.

—— 2002. *Ambitions of Curiosity: Understanding the World in Ancient Greece and China*. Cambridge, UK.

—— 2010. "History and Human Nature: Cross-Cultural Universals and Cultural Relativities". *Interdisciplinary Science Reviews* 35: 201–214. https://dx.doi.org/10.1179/030801810X12723585301318.

—— 2012. *Being, Humanity, and Understanding: Studies in Ancient and Modern Societies*. Oxford.

—— 2015. *Analogical Investigations: Historical and Cross-Cultural Perspectives on Human Reasoning*. Cambridge, UK.

Lloyd, G. E. R. and A. Vilaça. 2019. edd. Special Issue: "Science in the Forest, Science in the Past". *Hau: Journal of Ethnographic* Theory 9.1. Chicago.

Michalowski, P. 2017. "Literary Journeys from Babylonia to Assyria: Second Millennium Copies of a Bilingual Poem Concerning Ninurta". Pp. 205–230 in L. Feliu ed. *The First Ninety Years: A Sumerian Celebration in Honor of Miguel Civil*. Boston, MA/Leiden. https://dx.doi.org/10.1515/9781501503696-013.

Millroy, W. L. 1991. "An Ethnographic Study of the Mathematical Ideas of a Group of Carpenters". *Learning and Individual Differences* 3: 1–25.

Minnis, P. E. 2000. ed. *Ethnobotany: A Reader*. Norman, OK.

Needham, J. 1962. *Science and Civilisation in China*. vol. 4.1. Cambridge, UK.

Oppenheim, A. L. 1977. *Ancient Mesopotamia: Portrait of a Dead Civilization*. Chicago, IL

Pauling, L. 1960. *The Nature of the Chemical Bond and the Structure of Molecules and Crystals*. 3rd edn. Ithaca, NY.

Pfeiffer, J. and M. Nichter. 2008. "What Can Critical Medical Anthropology Contribute to Global Health?" *Medical Anthropology Quarterly* ns 22 :410–415.

Porzig, W. 1934. "Wesenhafte Bedeutungsbeziehungen". *Beiträge zur Geschichte der deutschen Sprache und Literatur* 58: 70–97.

Prance, G. T.; D. J. Chadwick; and J. Marsh. 1994. edd. *Ethnobotany and the Search for New Drugs*. Chichester, UK/New York.

Rivera, F. and J. Rossi Becker. 2008. "Ethnomathematics in the Global Episteme: Quo Vadis?" Pp. 209–225 in B. Atweh, A. C. Barton, M. C. Borba, N. Gough, C. Keitel, C. Vistro-Yu, and R. Vithal edd. *Internationalisation and Globalisation in Mathematics and Science Education*. Dordrecht.

Rosen, E. 1956. "The Invention of Eyeglasses". *Journal of the History of Medicine and Allied Sciences* 11: 13–46, 183–218.

Simonoff, J. S. 2003. *Analyzing Categorical Data*. New York.

Singer, M.; H. Baer; D. Long; and A. Pavlotski. 2020. *Introducing Medical Anthropology: A Discipline in Action*. 3rd edn. London, UK.

Soejarto, D. D.; H. H. S. Fong; G. T. Tan; H. J. Zhang; C. Y. Ma; S. G. Franzblau; C. Gyllenhaal; M. C. Riley; M. R. Kadushin; J. M. Pezzuto; L. T. Xuan; N. T. Hiep; N. V. Hung; B. M. Vu; P. K. Loc; L. X. Dac; L. T. Binh; N. Q. Chien; N. V Hai.; T Q. Bich; N. M. Cuong; B. Southavong; K. Sydara; S. Bouamanivong; H. M. Ly; T. Van Thuy; W. C. Rose; and G. R. Dietzman. 2005. "Ethnobotany/Ethnopharmacology and Mass Bioprospecting: Issues on Intellectual Property and Benefit-Sharing," *Journal of Ethnopharmacology* 100: 15–22. https://dx.doi.org/10.1016/j.jep.2005.05.031.

Sorabji, R. 1993. *Animal Minds and Human Morals: The Origins of the Western Debate*. Ithaca, NY.

Stillwell, J. 2016. *Elements of Mathematics: From Euclid to Gödel*. Princeton, NJ.

Traweek, S. 1988. *Beamtimes and Lifetimes: The World of High Energy Physicists*. Cambridge, MA.

Vithal, R. and O. Skovsmose. 1997. "The End of Innocence: A Critique of 'Ethnomathematics'". *Educational Studies in Mathematics* 34: 131–157. https://dx.doi.org/10.1023/A:1002971922833.

Voeks, R. A. 2018. *The Ethnobotany of Eden: Rethinking the Jungle Medicine Narrative*. Chicago, IL.

Volkov, A. 2014. "Mathematics Education in East- and Southeast Asia". Pp. 55–72, 79–82 in A. Karp and G. Schubring edd. *Handbook on the History of Mathematics Education*. New York. https://dx.doi.org/10.1007/978-1-4614-9155-2_4.

Wiener, N. 1996. *God and Golem, Inc.: A Comment on Certain Points Where Cybernetics Impinges on Religion*. Cambridge, MA.

Science et exégèse. Les interprétations antiques et médiévales du récit biblique de la création des éléments (Genèse 1, 1–8) edited by Béatrice Bakhouche

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Biblical exegetes from Antiquity and the Middle Ages continuously confronted the cosmogonic narrative offered in Genesis with the scientific cosmological theories of their times. Besides addressing theological questions raised by the text, most exegetes of the past were occupied with harmonizing the biblical cosmogony with current scientific knowledge or dealing with their manifest discrepancies. As noted by Anastasios Brenner in the closing chapter of the present volume, a chapter which proposes a reflexive look at our contemporary scholarly attitude toward such exegeses, we generally adopt a post-Kantian position on the issue of religion and science. We tend to think that the Bible belongs to the domain of belief and that the attempt at its harmonization with scientific knowledge is nothing but naive and dogmatic. Nevertheless, the proliferation of studies and congresses dedicated to the exegesis of the opening verses of Genesis could be seen as a symptom of our continuous fascination with a text that contributed, along with the scientific disciplines of physics and metaphysics, to shaping the Western worldview. To mention only French-speaking academia—the volume gathers contributions in French only, except for one in Italian—at least three volumes of proceedings of congresses on the subject have appeared in the last decades, as recalled by the editor, Béatrice Bakhouche:

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> This volume is the proceeding of a congress, which took place in Montpellier in 2013, dedicated specifically to the place of scientific considerations regarding the creation of the physical elements in the interpretation of Gen 1:1–8. It stands out by the number of contributions (22) and the length of the period covered, from the Antiquity (actually the very redaction of the cosmogonic narrative of Genesis) to the late Middle Ages. The main stress though is put on the Antiquity, treated in three of the four parts of the volume. [5: cf. CERL 1973; Vannier 2011 and 2014]

The first section, "Founding texts", gathers contributions on the Hebrew text of the Bible (Dany Nocquet, Jan Joosten), its Greek translation (Gilles Dorival), and its rabbinic interpretation (Ron Naiweld). The section "Receptions in the Hellenistic world" includes contributions on Philo (Jérôme Moreau), Gregory of Nissa (Claudio Moreschini), Origen (Christophe Leblanc), gnostic literature (Chiara Ombretta Tommasi), Ephrem and Narsai (Colette Pasquet), and Cosmas Indicopleustes and John Philoponus (Marie-Hélène Congourdeau). The section "Receptions in the Roman world" deals with Roman Patristics (Paul Mattei), Augustine of Hippo (Jérôme Labgouanère), and Jerome (Cécile Biasi). The fourth section is dedicated to a selection of "Medieval readings": Bede (Alessandra Di Pilla), a series of Carolingian commentators (Raffaele Savigni), Bernward Doors (Isabelle Marchesin), 12th-century monastic exegetes (Annie Noblesse-Rocher), Meister Eckhart (Marie-Anne Vannier), and a selection of representative 13th- and 14th-century exegetes (Gilbert Dahan).

The impressive variety of authors and texts that are treated makes a detailed discussion of each contribution impossible. But the vast period encompassed by the volume allows one to track the constitution of an exegetical tradition that is articulated around central questions. What emerges in the course of reading is recognition of a long-lived inquiry about whether the biblical cosmogony is to be read literally or allegorically and, more precisely, where the dividing line between history and allegory should be put. Two names emerge as cornerstones of this tradition. Augustine of Hippo, notably with his *De Genesi ad litteram*, set a theoretical framework of long-lasting influence, according to which scripture and science were two ways to access the truth that should be harmonized, and thus required that verses should be interpreted figuratively if their literal meaning contradicts scientific knowledge. As noted by Jérome Lagouanère, this model was still invoked by Galileo in his defense against his religious persecutors [188]. The second figure of the Christian exegetical tradition is Origen, who continued Philo of Alexandria's method of biblical allegorical interpretation in the Christian tradition.

But the issue of the harmonization of Bible and science is not dependent on the choice of one of these hermeneutical methods. For example, the Cappadocian Fathers address the exegetical problem of whether the firmament separating the lower and the upper waters on the second day of creation is to be taken as a physical body or as a metaphor of the border between the world of ideas and intellects and the material world. The former position was notably defended by Basil of Caesarea; the latter, by Gregory of Nyssa, whose views are analyzed by Claudio Moreschini. According to both, a confrontation with science is involved: physics alone for the former, physics and metaphysics and their respective boundaries for the latter. The precise extension and definition of "science" are at stake in this confrontation with the biblical text. The scientific disciplines of physics and metaphysics, and even theology (viewed as a science at least after Aquinas) and ontology (in the case of Meister Eckhart, as Marie-Anne Vannier's contribution shows) are not only *used* in order to understand the biblical text, but also partly *built* through this confrontation with scriptures.

Besides major authors, the volume highlights less expected literary corpora such as that of gnostic exegesis (in a chapter by Chiara Ombretta Tommasi), which constituted a type of interpretation to be excluded and which, therefore, had a negative but still important role in the formation of the exegetical tradition. Colette Pasquet's chapter on the question of what was created *ex nihilo* and what *ex aliquo* in the Syriac texts of Ephrem and Narsai elucidates a Syriac terminology (*men medem* for *ex aliquo*, *men lo medem* for *ex nihilo*) that probably influenced the terms used later in Arabic (*min shai* and *min lā shai/lā min shai*) and Hebrew (*mi-davar* and *min lo davar / lo mi-davar*) theological discussions of this issue [see Wolfson 1948].

The transmission of ancient exegetical material to the Middle Ages and the process by which the basic constituents of medieval Christian exegesis in the West were selected are illuminated in interesting contributions on the exegetical genres that flourished in Late Antiquity. In this period, various literary tools were used to spread the biblical cosmogony and worldview. The example of the poetical *Hexameron* of Dracontius, studied by Paul-Augustin Deproost, is shown both to introduce exegetical elements taken from Augustine and to recast biblical discourse in a way accommodating the scientific ideas of the intellectual elite of the fifth century. Such poetry can, therefore, be understood as a tool in the process of the Christianization of the Roman world in that period. The same is true of the genre of the poetical *epos* that developed in the fifth and sixth centuries, the subject of the chapter by Michele Cutino. These versified rewritings of biblical texts were specifically addressed to the *rudes*, those who were not acquainted with the Bible but who were very cultivated and thus sensible to poetical forms [246]. In these chapters, the reader comes to sense how such transitional and didactical genres were associated with the specific exegesis of Bede in the seventh century (studied by Alessandra di Pilla), and such exegetes in the Carolingian period (presented by Raffaele Savigni) as Raban Maur and Remigius of Auxerre, in the process of crystallizing a standard exegesis that led to the redaction of the *Glossa ordinaria*. The *Glossa* itself, though, would have deserved a chapter of its own given the important role that it played in the medieval reading of the Bible.

The question of the channels by which biblical exegeses were transmitted is also addressed in a contribution on the Bernward Doors, the 11th-century monumental bronze doors of the Hildesheim cathedral. According to Isabelle Marchesin, they call for a reconsideration of the role of the plastic arts in the diffusion of knowledge among the illiterate masses.

Several contributions go beyond the limits of the topic announced by the title of the volume, i.e., the confrontation of the Bible and science regarding the creation of the elements. This is clear in Jan Joosten's discussion of the Hebrew text of Genesis. He argues that the specific feature of this biblical text among the cosmogonies of the Levant is that it presents a God who creates a world, and more specifically a human being, because he seeks a partner with whom to associate. Moreover, in several contributions, the issue of science and exegesis thus meets existential and spiritual considerations. Christophe Leblanc claims that, in the case of Origen, their confrontation led him to understand the world as a text to be read rather than to view the Bible as a certain representation of the world. In her chapter on 12th-century monastic exegesis, Annie Noblesse-Rocher adopts a conception of intertextuality that is fruitfully conceptualized in the works of Gérard Genette, and shows that such intertextuality, as generated by reading the Bible mainly through Augustine's commentary, brought the monks to a process of "impersonation" in which they identify with biblical characters and actually "live" the biblical text.

Despite the variety of corpora treated, the volume is almost exclusively dedicated to Christian exegesis. The chapters dedicated to the Hebrew Bible or the Rabbinic tradition, which feature a section entitled "Sources", reflect the Christian-oriented perspective of the volume: for example, Céline Biasi's chapter on Jerome, which refers to the Hebrews as witnesses of historical meaning of the text [192]. The question of the confrontation of the biblical narrative of creation with scientific knowledge in the Jewish exegetical tradition as such would certainly have enriched the volume. In his contribution on classical rabbinic literature (Talmud and Midrash), Ron Naiweld shows that, by assigning to the Torah the role played by *Logos* in a Neoplatonic and Stoic topos of the time, the rabbis oriented the Jewish exegetical tradition in an existential-juridical direction and manifested precisely their lack of interest in harmonizing the Bible with scientific knowledge. Indeed, a chapter on medieval Jewish exegesis could have shown how this endeavor became central among Jewish rationalist thinkers, such as Saadya Gaon (10th century) and, even more so, Maimonides (12th century) and his disciples. With his repeated affirmation that "the *Account of the Beginning* is identical with natural science" [Pines 1963, 6 *et passim*], Maimonides really introduced in the West the interpretation of biblical cosmogony as an allegory of Aristotelian physics. Chapter 2:30 of his *Guide of the Perplexed* offers a continuous reading of Gen 1:1–8 in line with Aristotelian elemental physics. Maimonides' introduction of Aristotelianism in biblical exegesis later influenced such Christian authors as Albertus Magnus and Thomas Aquinas and, at least indirectly, those exegetes of the 13th and 14th centuries whose interpretations of the two narratives of creation in the first chapters of Genesis (from Robert Grosseteste to Nicholas de Lyre) are here studied by Gilbert Dahan.

Through its numerous contributions, this volume introduces new perspectives on the constitution of the Western exegetical tradition and reflects the dynamism and variety of research in France and Italy concerning the history of science and biblical exegesis.

BIBLIOGRAPHY

CERL Centre d'étude des religions du Livre (CERL). 1973. *In principio: interprétations des premiers versets de la Genèse*. Paris.

Pines, S. 1963. trans. *Moses Maimonides: The Guide of the Perplexed*. Chicago.

Vannier, M.-A. 2011. ed. *La Création chez les Pères*. Berlin.

——— 2014. ed. *Judaïsme et christianisme dans les commentaires patristiques de la Genèse*. Berne.

Wolfson, H. A. 1948. "The Meaning of Ex Nihilo in the Church Fathers, Arabic and Hebrew Philosophy, and St. Thomas". Pp. 355–370 in U. T. Holmes Jr and A. J. Denomy edd. *Medieval Studies in Honor of Jeremiah Denis Matthias Ford*. Cambridge, MA.

The Oxford Handbook of Science and Medicine in the Classical World edited by Paul T. Keyser and John Scarborough

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The volume under review is a recent addition to the Oxford Handbooks series, which

> offer[s] authoritative and up-to-date surveys of original research in a particular subject area. Specially commissioned essays from leading figures in the discipline give critical examinations of the progress and direction of debates, as well as a foundation for future research. [dustcover]

This review is written from the standpoint of someone fairly new to the fields of ancient science and medicine, who teaches an undergraduate survey of them and would like to be brought up to date on recent discoveries, interpretations, and approaches. To that end, this book is a fantastic resource and a major achievement. And at just over 1,000 pages, there is a lot in it: much that readers might reasonably anticipate, but a lot that they might not. The title suggests a broad scope—science and medicine in the classical world—but we get considerably more.

Core topics—cosmology, astronomy, mathematics, geography, anatomy, pathology, and pharmacy—receive ample coverage. But so do topics that are less commonly treated in handbooks or overviews, such as harmonics, optics, surgical tools, and physiognomy. Most strikingly, the volume opens with four groups of chapters treating Mesopotamian, Egyptian, Indian, and Chinese science and medicine. The scholarship on Greek science and medicine has often looked beyond the Greek world to understand precursors and influence. Yet handbooks (or even monographs) rarely look beyond the ancient Greek and Roman worlds for their own sake.

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Contributors to the volume frequently cite the *Encyclopedia of Ancient Natural Scientists* (*EANS*), of which Keyser is also an editor,[1] and we might understand this volume to be a companion that offers context for the entries in *EANS*. It is, similarly, a collaborative effort: for this volume, Keyser and Scarborough have gathered an international team of 44 contributors. The editors are well-placed to have taken on this project: Keyser has also co-edited Routledge's *Greek Science of the Hellenistic Era: A Sourcebook* [Irby-Massie and Keyser 2002], while Scarborough is the contributor to, and editor of, multiple volumes as well as a leading figure in ancient pharmacology and medicine more generally. Their contributors are a mix of established figures with long records of research and up-and-coming scholars. The volume is also a natural companion to *The Oxford Handbook of Engineering and Technology in the Classical World*, edited by John Peter Oleson [2009].

Keyser and Scarborough have structured their volume around broad disciplines or spheres of activity, much more so than the *Companion to Science, Technology, and Medicine in Ancient Greece and Rome*, edited by Georgia Irby [2016], whose 60 chapters each have a narrower focus. While the two books have much in common—they are comparable in length and temporal coverage, and even share some contributors—importantly, Irby's *Companion* also includes chapters on technology. Keyser and Scarborough's inclusion of chapters on areas outside the classical world (notwithstanding Irby's chapter 56) signal their awareness of future directions in the study of classical science and medicine.

Keyser's introduction sets up some helpful parameters and guiding principles for the volume. It is here that we learn the volume's chronological divisions: excepting part A, in which the timespan for each contribution is less fixed, science and medicine are considered from Homer through AD 650, divided into four rough periods at what Keyser terms "natural joints" [5]. Part B runs from Homer through Plato; part C, through the Hellenistic period; part D, the Greco-Roman period; and part E, late antiquity and early Byzantium.

Since each chapter offers a summary of its topic—and given that there are 49 chapters, plus an introduction—I will simply pick out highlights of each. Most chapters include the following elements: the major source material for their topic, key players, ancient and modern approaches, and essential bibliography.

[1] See Keyser and Irby-Massie 2008.

Part A, “Ancient Scientific Traditions beyond Greece and Rome”, contains 10 essays on ancient scientific and medical traditions outside Greece and Rome. As Keyser acknowledges in his introduction, these contextualizing essays on Mesopotamia, Egypt, India, and China are not comprehensive, largely because of a lack of contributors. The gaps signal areas for future scholarship. Other parts of the world had science too, as Keyser acknowledges; but their lack of texts, or the difficulty of working with them, made comparable essays impossible.

Jens Høyrup’s contribution, “Mesopotamian Mathematics”, introduces the key themes of the volume: the tension between theory and practice; the importance of scribal activity and textual transmission; and the social and cultural prestige of the topic. The close of the essay, which problematizes the Greek inheritance of Near Eastern mathematics, provides a useful corrective to often oversimplified and misleading presentations of intellectual inheritance. In “Astral Sciences of Ancient Mesopotamia”, Francesca Rochberg points out that astronomy and astrology were not distinguished as a science and a pseudo-science, a theme picked up several times later in the volume. There are helpful summaries of key compendia, including *Enūma Anu Enlil* and *MUL.APIN*, and here, as elsewhere, mathematics is emphasized as a key underpinning of other sciences. JoAnn Scurlock’s lively essay, “Mesopotamian Beginnings for Greek Science?”, focuses on medical practitioners and remedies. In rationalizing and psychologizing the role of magical practices as part of healing, she argues that Mesopotamian healing was more rational and effective than Greek. Scurlock also discusses the Greco-Babyloniaca: texts in Akkadian that used the Greek alphabet to give Greek-language scholars access to Akkadian literary and scientific texts and that reflect significant cultural encounters between the Greek and Mesopotamian worlds.

Moving to Egypt, Annette Imhausen’s “Mathematics in Egypt”, which emphasizes textual transmission and our lack of sources, includes the sobering statistic that “only six chance finds of mathematical texts have survived” [54]. This essay is a model of clarity; her explanation of Egyptian fractions [51–52] is admirably lucid. Joachim Friedrich Quack’s “Astronomy in Ancient Egypt” surveys star-clocks, the *Book of Nut*, and simple formulae for calculation. Quack occasionally looks forward in time to the Greco-Roman period and helpfully anticipates the later chapters of the volume. In her survey of Egyptian medicine, Rosemary David emphasizes current and recent research. She argues for a greater presence of rational elements than irrational, citing preliminary findings from the University of Manchester’s

Pharmacy in Ancient Egypt project. In a long section on sources, she surveys the possibilities offered by physical remains, as well as their current limitations.

Much of the material on Mesopotamia and Egypt is well known to experts on the Greek and Roman side. But India and China will be less familiar, and the essays that follow are a real boon to all classicists, pointing us to possibilities for comparative work and alerting us to a multiplicity of ancient sciences and medicines.

Toke Lindegaard Knudsen's accessible "Mathematics in India until 650 CE" picks out just a few elements of interest, including large numbers, the place-value decimal system, Indians' use of the Pythagorean Theorem, and Pascal's Triangle. While classicists should avoid the temptation to approach science and medicine outside the ancient Mediterranean by looking for relative chronologies in discovery, Knudsen does provide details for ancient texts (and translations) that will help classicists track contemporary modes of thought. "Sanskrit Medical Literature" by Tsutomu Yamashita takes a source-based approach to argue that rational medicine originated in irrational religious texts. In the final section, Yamashita provides a careful and lucid introduction to physiology and pathology and points up the distorting tendency among scholars to fit Āyurvedic theories to those of Greek sources.

Moving to China, we are made aware of an enormous and complex scholarly tradition. In "Ancient Chinese Mathematics", Alexei Volkov supplies specific examples of problems that interested Chinese mathematicians, including the "remainder theorem" and calculations of pi and the volume of a sphere. According to Xu Fengxian's "Astral Sciences in Ancient China", there were two driving forces: calendar-making and astrology. Fengxian's discussion of how the Chinese conceptualized and observed the structure of the heavens (with 28 constellations or *xiu*) reminds us that core conceptualizing frameworks, such as the zodiac, are not inevitable.

These opening contributions give a sense of universal themes, which are helpful for the instructor trying to guide undergraduate students away from notions of Greece and Rome being special or different. The essays on Egypt and Mesopotamia attend to influences on and between peoples, and it would have been helpful to have some discussion on external influences—or the lack of them—on Indian and Chinese science and medicine (for example, the influence of Hellenistic texts on Indian astronomy).

Part B, "Early Greek Science", takes us from Homer to Plato in four chapters. In "Pythagoras and Plato", Andrew Gregory takes a biographical approach—the essays in the volume arrange their material biographically, topically, chronologically, and around key texts—to explore early Greek treatments of a few topics. Other chapters, too, will be selective, favoring depth over coverage. Investigation is a key theme of Gregory's chapter, and indeed approach—theoretical *vs* empiricist—is important in the volume overall. Mention of Philolaus is welcome; his pyrocentric model of the universe can be presented to students alongside Aristarchus' heliocentric model as alternatives to the dominant geocentrism.

Leonid Zhmud's "Early Mathematics and Astronomy" is a dense chapter. We learn that competition existed among early Greek scientists—giving rise to proofs as evidence of excellence—but emphasis on "firsts" comes later from Eudemus of Rhodes, who exerted enormous influence on the form and focus of the history of Greek science. The terminology employed in this chapter could have been clearer: "astronomy" is not clearly defined, and *mathemata*, a term used throughout the chapter, is not defined until the final section. Zhmud's clear explanations of various mathematicians' attempts to square a circle are valuable.

In "Early Greek Geography", Philip G. Kaplan surveys Homer's and Hesiod's approaches to space through early cosmogonies and genealogies, and traces the shift in Greeks' conceptions of space from itinerary-based to cartographic, as apparent in Herodotus. Kaplan presents Herodotus as a geographical innovator who describes distance using units of measurement, not time (stades *vs* days' walk). This section of the chapter is especially helpful to graduate students, providing them with another context in which to think about a writer otherwise approached as a historian.

"Hippocrates and Early Greek Medicine" by Elizabeth Clark contains a broad introduction to the Hippocratic Corpus and early medical thought. Clark also briefly considers similarities between Āyurvedic medicine and early Greek medicine, and raises the possibility of the movement of ideas, along with people and goods, especially around the Black Sea. She notes that Greek mechanical views of the body (as containing fluids that might need to be unblocked when gathered in excess in one place) are also identified in contemporary Chinese medicine.

Moving to Part C, "Hellenistic Greek Science", which receives the most attention of any period (16 essays), we pick up with Aristotle, whom Joachim Althoff suggests we should regard as a scientist first and philosopher second.

His "Aristotle, the Inventor of Natural Science" is a well-written chapter that does an admirable job of connecting Aristotle's key areas of inquiry and approaches. Little is said about Aristotle's intellectual context, though Althoff stresses Aristotle's towering influence on the Hellenistic period (and later). Teun Tieleman's brief essay "Epicurus and His Circle" can be regarded as something of a companion piece that similarly treats Epicurus and his successors.

Fabio Acerbi's "Hellenistic Mathematics" is one of the most imaginatively presented essays in the volume. He opens with an intriguing section on the stylistics of mathematical writing (which might be a nice addition to graduate-level courses on Greek prose style). Acerbi summarizes Hellenistic mathematics—no mean feat—by characterizing it as concerned with lines, and then goes on to define those various lines and to sketch various individuals' concern with them. The survey is highly technical and condensed, but offers a neat approach to what might otherwise have been a long and unwieldy section. Acerbi helpfully points out that the notion of a collective endeavor to solve the classic three problems (duplication of the cube, squaring of the circle, and trisection of an angle) has arisen from succeeding traditions of commentary and compilation that have downplayed the breadth and independence of mathematicians' work.

In "Hellenistic Astronomy", Alan C. Bowen surveys ancient conceptions of *astrologia*, or work on the heavens (which encompasses modern astronomy and astrology). He stresses the need to acknowledge the literary nature of Hellenistic astronomical texts, of the "facts" chosen and presented to support their author's literary intent. For Bowen, the Hellenistic period's main contribution to *astrologia* lies in its establishment of a framework for the work that is to follow.

Duane Roller's "Hellenistic Geography from Ephorus through Strabo" is a masterly and fascinating survey of the development of geography as a discipline. Roller points out that Polybius viewed himself more as an "explorer" than a historian (another useful corrective, to set aside that concerning Herodotus). It is good to see mention of Hestiaia of Alexandria, who wrote on topography [330].

T. E. Rihll's essay on "Mechanics and Pneumatics in the Classical World", a *tour de force*, marks an important shift in the volume, to the immediately practical and sometimes utilitarian. But as Rihll notes, "Academic subjects and the world of work were less separated in antiquity than they are today" [339]. Despite the reputation of Greeks and Romans for engineering, Rihll points out that the erroneous notion of "natural motion", deriving from Aristotle, hampered progress in mechanics for many centuries, though practical applications of the mechanics of moving objects were not altogether stymied. Rihll matches descriptions of catapults and other machines with archaeological finds, arguing against labeling devices not immediately realizable as "armchair devices" and noting that the gap between written explanations and final execution has always existed. This chapter, which is more accessible than most because of the familiarity of so many of the devices being described, could be assigned to a senior undergraduate interested in the topic. There is also a survey of ancient theoretical explanations for machines, many of which were unsuccessful—a nice counterpoint to the positive impression of ancient mechanical understanding given by the archaeological record.

Fabio Stok's "Medical Sects" surveys and carefully differentiates Herophileans, Erasistrateans, and Empiricists, stressing the development of their approaches over time. This essay nicely anticipates that of Lauren Caldwell later in the volume.

Glen M. Cooper's chapter, "Astrology (The Science of Signs in the Heavens)" provides an introduction to its subject. There is a significant section on skepticism (along with astrologers' rejoinders), and a brief but fascinating section on Christianity's uneasy but sometimes accommodating relationship with astrology. The essay closes with an analysis of Hadrian's horoscope, a neat way to explain facets of prediction and to introduce key explanatory texts (and their contradictions), including Ptolemy's.

In "The Longue Durée of Alchemy", Paul Keyser defines his subject as the "science of materials" [409], a definition which he acknowledges as broad, and which enables consideration of alchemy both as a precursor to chemistry and also as a philosophically driven set of practical and spiritual practices. Claiming the former as alchemy's primary goal through the Hellenistic period, Keyser focuses on work with pigments and metals. Readers will benefit from his explanation that modern categories based on physical properties (e.g., metals *vs* minerals) did not exist in the ancient world

Klaus Geus and Colin Guthrie King's chapter, "Paradoxography", is a fascinating survey of Greek and Latin accounts of phenomena considered outside what is normal or expected. Paradoxographical accounts of phenomena rely on their sources for credibility, not the judgment of their collector. As such, they demonstrate the broad point that ancient epistemological premises differ from those of today. The chapter exemplifies the volume's emphasis on the otherness of ancient science and medicine, and its concern to consider ancient areas of interest according to their ancient definitions and goals.

The inclusion of the chapter “Music and Harmonic Theory” is likewise in keeping with the editors’ concern to conform to ancient definitions and conceptualizations. This a highly technical chapter, one that is hard to penetrate without some familiarity with music theory. Stefan Hagel helpfully reviews Aristoxenus’ main achievements, including his attempts to reconcile musicians’ and mathematicians’ ratios, as well as Ptolemy’s attempts to do the same, which were apparently too technical or difficult to be picked up by either fellow theorists or musicians.

Philip Thibodeau’s chapter, “Ancient Agronomy as a Literature of Best Practices”, marks a significant shift for the volume. His focus is not on archaeological evidence but on texts that communicate the most economically beneficial practices and share marvels of farming. This chapter might sit more obviously in a volume on ancient technology, though Thibodeau points out that some agronomers organized their material around the calendar and basic astronomical observations, and, as Keyser points out in the introduction, the editors have used a broad definition of science.

“Optics and Vision” raises the intriguing question, What is vision? In this chapter, Colin Webster tracks the various ancient definitions that came into vogue and their proponents. Most of the names are ones already encountered in the volume, and Webster briefly connects their thoughts on vision to their wider concerns with issues of matter, perception, astronomy, and geometry.

“Pharmacology in the Early Roman Empire: Dioscorides and his Multicultural Leanings” is a compelling chapter. In it, John Scarborough has arranged his material by simples, which he has chosen to reflect both the contemporary empire in which Dioscorides worked and traveled, with its varied geography and flora, and the history of pharmacology. There is plenty in this chapter for the instructor: information on pharmaceutical uses of silphium, castoreum, and sea urchins, as well as poisons and narcotics. While authors of other chapters have tried to avoid applying modern definitions or explanations lest they seem to be making judgments from a modern sensibility or knowledge, Scarborough does supply modern explanations for ancient remedies which help the reader to understand better that remedies were often the result of empiricism. Scarborough’s description of Pliny’s *Natural History* [520–521] is delightful.

A chapter devoted to dietetics, "Dietetics: Regimen for Life and Health", is a welcome surprise, opening with a nod to the importance of experimental archaeology to some scholars of ancient food and diet. Most compelling in this chapter is Mark Grant's survey of ancient understanding regarding food's interaction with the body, e.g., in digestion and in cures for madness; he sets out the connections between qualities (hot, dry, cold, moist) and the foods that were believed able to correct the imbalance that had caused illness.

"Greco-Roman Surgical Instruments: The Tools of the Trade", with its interest in archaeology, follows neatly from the previous chapter. Lawrence J. Bliquez organizes some of his material by tool and notes the consistency between archaeological finds and written descriptions of instruments.

Moving to Part D, "Greco-Roman Science", and Philip Thibodeau's "Traditionalism and Originality in Roman Science", we might ask whether there was such a thing as Roman science. Thibodeau answers by defining it as science written in Latin and identifying some of its achievements. Those are often hard to recognize because Romans liked to place themselves in traditions and credit discoveries to early figures, notably Numa Pompilius and Pythagoras, rather than single themselves out as originators or significant developers. Thibodeau surveys such figures as the Elder Cato, Nigidius Figulus, and Varro.

By invoking Pythagoras, Roman scientists acknowledge their debt to the Greeks, a theme picked up by Pamela Gordon in "Science for Happiness: Epicureanism in Rome, the Bay of Naples, and Beyond". Gordon explores the extent to which Lucretius, Philodemus, and others developed Epicureanism, in a broad survey that brings together medicine, physics, and evolution. This essay reflects well the scope of the volume, encompassing theory and philosophers alongside physical evidence and practitioners.

Lauren Caldwell picks up on Stok's earlier essay in her "Roman Medical Sects: The Asclepiadeans, the Methodists, and the Pneumatists". She offers a sketch of the sects and their key positions or approaches, acknowledging the problem of scholars' necessary over-reliance on one source: Galen. Highlights of this chapter include Caldwell's overview of what Empiricist and Methodist doctors might offer patients—carefully considered plans of treatment from the former, efficiency and value for money from the latter—and her consideration of medical education (the first chapter in the volume to do so). She also explores the extent to which doctors consciously adhered to a sect and how united those sects were. This is a lucid, well-written, and highly readable chapter that brings together scholarship and carefully chosen ancient sources, such as Aelius Aristides' *Sacred Tales*.

In "Science and Medicine in the Roman Encyclopedists: Patronage for Praxis", Mary Beagon tackles the importance of polymathy for ancient scientists. In her treatment of Vitruvius, Beagon identifies a "Roman holistic attitude to learning, whereby human need, utility, and aesthetics make the study of nature more than the literal sum of its elemental parts" [666]. Her discussion of Pliny works towards a definition of a Roman approach to science, with its emphasis on practicality, utilitarianism (in support of profit), personal authority, and a "medico-magico-religious" approach from a Roman tradition that can be set alongside the Greek tradition [673]. Added to that is the Roman attitude towards knowledge as a corollary to power, which is exemplified in the encyclopedists.

Teun Tieleman's "Stoicism and the Natural World: Philosophy and Science" focuses more on philosophy than on science, though a highlight is his discussion of Stoic responses to developments in medical thought.

John Scarborough's "Scribonius Largus and Friends" is the companion to his earlier chapter on Dioscorides. According to Scarborough, the precision and complexity of Largus' recipes for remedies ensured that they would become neglected, in contrast to Dioscorides' far simpler text. (Scarborough's acknowledgment of the importance of the reader complements Caldwell's earlier discussion of patient experience.) In addition, as Scarborough notes, Galen favored Dioscorides. Scarborough analyzes one of Scribonius' recipes, carefully presenting how it was (and was not) efficacious—a powerful example of Scarborough's training in pharmacy and history. The final section of the chapter, which describes the effects wrought by the recipe (including, alarmingly, kidney poisoning) is a salutary reminder of what the capabilities of ancient medicine were.

In "Distilling Nature's Secrets: The Sacred Art of Alchemy", Kyle Fraser revisits the history of alchemy in order to correct and complicate Festugière's influential claim that alchemy became less scientific and more mystical over the centuries. The section on Maria, a figure often mentioned only in passing, is a significant contribution. Presented usually as a designer of apparatus, Maria developed her *kerotakis*, a sealed still used to collect heated gases, with the goal of transmutating a base metal by complete transformation of all its properties.

Mariska Leunissen's "Physiognomy" is an excellent addition to the volume. Though no longer a modern science, thanks especially to its notorious employment in the early 20th century, physiognomy nevertheless has a long and important history. As Leunissen points out, Greek and Roman philosophers used the body to understand character, while physicians used character to understand the body.

Galen has appeared throughout the volume thus far, but Ian Johnston's "Galen and His System of Medicine" is devoted entirely to him. Galen's predecessors are identified as chiefly Hippocrates and Plato. The chapter emphasizes his philosophical training and interests as an intentional basis for thinking about methods of diagnosis. He wrote on philosophical topics, an aspect of his work that this volume could have overlooked but happily did not. There are excellent accounts of Galen's positions on, for example, elemental *vs* atomistic views of anatomy and his classifications of disease. At the close of the chapter, Johnston sets out his list of answers to the question, "What relevance does the study of Galen have today?" This should be a go-to list for anyone teaching a course on ancient science or medicine.

James Evans' chapter is an elegant introduction to the wide-ranging work of Ptolemy. A standout from this long chapter is the discussion of Ptolemy's claim that the Earth cannot be moving because items thrown into the air do not continue to move along with it—a helpful example of ancient explanations for what we understand as Earth's gravitational pull. The section on Ptolemy's geography provides an example of one of the strengths of this volume: Evans' discussion, focused on cartography, is oriented quite differently from that of Duane Roller, which focuses on explorers and historical writers. (Compare also Evans' discussion of refraction with that of Colin Webster, and his less technical treatment of harmonics with Hagel's.) The closing section is one of the most important in the volume, raising the issue of instrumentalist *vs* realist approaches to science among the ancients.

Paul Keyser's "Science in the 2nd and 3rd Centuries CE: An Aporetic Age", which closes this part of the volume, helpfully puts Ptolemy and Galen in context and affords their contemporaries some attention. According to Keyser, there are three characteristics of science writing in this period: adoration of the past, a tendency to produce compendia or summaries rather than wholly original work, and the cultural importance of claiming wide intellectual authority, all of which will be important in the last part of the volume.

In Part E, "Late Antique and Early Byzantine Science", the volume continues through the sixth century AD. This editorial decision was made, perhaps, in the spirit of inclusivity, and I hope classicists will pay it due attention. They should certainly read “Plotinus and Neoplatonism”, for as Lucas Siorvanes reminds us, Neoplatonist texts make up about 58% of all extant Greek philosophical texts. With scholars increasingly relabeling those texts as simply Platonist, perhaps familiarity with them will increase. The importance of (Neo)platonism is underlined in the closing summary of its influence on later scientists, most notably Kepler.

The brief and clearly demarcated sections of Alain Bernard’s “Greek Mathematics and Astronomy in Late Antiquity” are a good fit for a handbook and make his arguments easy to find and follow. Unlike most other chapters, Bernard emphasizes his subject’s social and intellectual contexts, which are far different from those in previous parts of the volume; for example, mathematics’ newly increased importance to late antique philosophy would justify its importance in the future. Commentaries are emphasized, as indeed they are through the remaining chapters, as gatekeepers for the mathematical tradition and venues for new ideas.

Commentaries are the focus of Michael Griffin’s “Greek Neoplatonist Commentators on Aristotle”. Griffin emphasizes the originality of late antique commentators on Aristotle, who were concerned with reconciling those of his texts that are in contradiction and picking out shared ideas. They also refined Aristotelian thought and approaches. Griffin supplies the example of Philoponus, who develops Aristotle’s notion that a javelin thrower imparts movement to the air that then propels the javelin; Philoponus posits that the thrower is giving force to the javelin.

In “Byzantine Geography”, Andreas Kuelzer reminds us that information was drawn not only from older Greek authorities, most notably Strabo and Ptolemy, but also from texts from Nisibis and from Jewish and Christian texts of the third century and later—a salutary reminder of the strands of thought that should stand alongside the more familiar (to us) texts of Ptolemy *et al*. Especially helpful in this connection is Kuelzer’s discussion of Christian opposition to notions of the Earth and universe as spherical.

In “Byzantine Alchemy, or the Era of Systematization”, the focus returns to commentaries and collections and the processes of compiling and editing. A notable feature of Cristina Viano’s chapter is the section on material evidence for alchemy, including the black patina on some statues that may be the famed “black bronze” of some alchemical recipes, and the remains of gold mining sites at Samut in Egypt. Here, the emphasis on the practical work of alchemy nicely echoes Keyser’s earlier chapter.

Svetla Slaveva-Griffin describes a new area for research in her "Byzantine Medical Encyclopedias and Education". These encyclopedias are little mentioned in regular scholarly surveys of the period, but medical practitioners and medical historians are drawing attention to their significance, especially as syntheses that were of immediate use to medical practitioners.

In "Late Encyclopedic Approaches to Knowledge in Latin Literature", David Panagua surveys those works in Latin, from the third century AD to Isidore, that present *omne scibile*, everything knowable, such as Lucius Ampelius' *Liber memorialis*. But what is worth knowing? The example of Augustine's abandonment of secular learning as incompatible with Christian education highlights one of the myriad threats to the later transmission of ancient science. Yet Cassiodorus' educational program provides an encouraging counterpoint.

Louise Cilliers' "Medical Writing in the Late Roman West" provides a fitting end to the volume; this is the period in which, as Cilliers points out, the majority of Latin medical texts were produced. Cilliers describes how Greek scientific and medical texts were being translated into Latin for a Roman West that was in the fourth to seventh centuries—and onwards—increasingly Latin-speaking rather than bilingual. The philosophical and theoretical aspects of translated texts were excised, leaving only practical instruction. The chapter would have benefited from a longer discussion of Alexander of Tralleis, who, as Cilliers acknowledges, is termed by modern doctors "the third Hippocrates".

As these summaries indicate, in this volume there is a wealth of information and analysis, far in excess of what one might expect from a handbook or introduction. However, in several chapters, especially those by Rochberg, Zhmud, and Hagel, the information has been presented so densely that a reader unfamiliar with the topic would need to do some background reading in order to understand it fully. Other chapters (most notably that by Rihll) are accessible to the non-specialist.

There is an inconsistency in references to Pythagoras' theorem: it is thus named in Volkov's and Gregory's chapters, but in Lindegaard Knudsen's it is the "Pythagorean theorem". The difference is important: the theorem was not Pythagoras', though it was perhaps proven by him. "Pythagorean theorem" more elegantly reflects that fact and might have been adopted through the volume. Another inconsistency is that Maria, the alchemical authority discussed in Fraser, has become Mary the Jewess in Viano.

In 49 chapters, there are, inevitably, overlaps in subject matter (for example, between Zhmud's and Gregory's discussions of Pythagoras). When those are treated with different approaches, appropriate cross references would benefit the reader. Some chapters do contain cross references, most notably those of Gordon, Cooper, and especially Bernard, who seems to have read other chapters carefully and taken pains to engage with them. The paucity of cross references in some chapters is not a source of criticism, rather a missed opportunity. Johnston's treatment of the medical sects does not refer to similar treatments in Stok and Caldwell, and Grant and Caldwell do not refer to each other's contributions, despite the overlap in their material. Scarborough, in his chapter on Scribonius, discusses Philodemus and Epicureanism at Herculaneum, but does not reference Gordon's chapter. Given that Scarborough is one of the volume's editors, it seems likely that contributors were not encouraged to reference others' essays.

Division of the Greek and Roman material into four parts (early Greek, Hellenistic, Greco-Roman, and late antique and early Byzantine) broadly reflects intellectual developments, along with developments in politics and culture. As Keyser notes in his Introduction, Part C, "Hellenistic Greek Science", covers "the long Hellenistic era generally", and Part D, "Greco-Roman Science", is "somewhat overlapping" [5]. As a result, chapters in the same part of the volume might not have the same temporal bounds, an inconsistency that was disconcerting to this reader. For example, in Part C, while Althoff and Tieleman focus on the fourth and third centuries BC, the chapter that follows by Acerbi ranges as late as the first century AD. The title of Scarborough's chapter on Dioscorides, "Pharmacology in the Early Roman Empire", was a confusing choice for a chapter included in Part C. Similarly, Bliquez's "Greco-Roman Surgical Instruments: The Tools of the Trade" surveys instruments that date to the late Republic/early Empire, yet because, as Bliquez notes, they were used by Greek doctors, the chapter was included in Part C. In Part D, Thibodeau has an end point of the first century AD, but Gordon and Caldwell, in the chapters that follow, span as far as the third century. Finally, in Part E, the distinction between late antique and early Byzantine is hard to determine: for example, Kuelzer's "Byzantine Geography" references texts dating as early as the second century AD and written in Latin, but also ranges as late as the 11th. While some contributors (such as Bowen, Viano, and Cilliers) do an excellent job of stating clearly their beginning and end dates, others (including Rochberg) are less clear.

A handbook on science and medicine will, quite reasonably, offer only brief historical narrative or supporting detail, but sometimes supporting evidence or references were needed for them. For example, Cooper presents assertions about the emperors' use of astrology and astrologers without textual references that would have helped the reader evaluate those claims. Cilliers, in an otherwise excellent chapter, refers to "the deposition of the last Roman emperor in the West in 476" [1013], though, as some scholars are keen to point out, Julius Nepos clung to his imperial title until 480. Her characterization of the fourth to the seventh centuries as "the twilight years of the western Roman Empire, passing over into the Dark Ages" [1030] catches the reader's attention, but feels dated.

Readers of handbooks are often looking for good bibliographies, and this volume does an excellent job of providing judicious lists of editions (and, where necessary, translations) and of seminal and recent scholarship. The bibliographies of Panagua and Webster are even divided helpfully into sections (though the latter does not key his in-text references to those sections), and Volkov offers two lists of publications, in "oriental languages" and "western languages". Acerbi's "Onomasticon" is a boon to the reader, though a reference to it early in the chapter would have made it more useful. In addition, the contributors do an admirable job discussing important individual works of scholarship.

Editors of handbooks are faced with the difficult choice between, on the one hand, sacrificing space for the sake of clarity in presenting complex material and, on the other, keeping discussions short and relying on references to relevant detailed discussions elsewhere. The editors and their contributors have achieved an effective balance, largely through judicious selection of exemplary material. The volume would have benefited from a full discussion (perhaps not a chapter) somewhere of atomism as treated by Leucippus, Democritus, and others. Also desirable would have been a rigorous discussion of where scholars have stood and currently stand on the role and status of magic in ancient science and medicine, a topic that admittedly exercises non-specialist classicists more than it might the volume's contributors.

To the classicist who must incorporate them into teaching or research, ancient science and medicine can seem impenetrable and intimidating, largely because the sources are unfamiliar and rarely available in the usual collections. Keyser and Scarborough are therefore to be commended for the fact that their contributors emphasize sources—both textual and material—throughout the volume.

In his introduction, Keyser tackles the issue of definitions of science. He acknowledges that what qualifies as science develops over time and, in viewing science as a broad church, hopes to encompass both ancient and modern definitions. This seems a reasonable approach, especially for a handbook that must somehow divide its material to suit ancient conceptions, modern expectations, and scholars' areas of expertise. There are some significant, perhaps unintended, consequences for the volume. For example, Keyser himself, in his chapter on alchemy, takes a modern scientific approach to the topic by privileging the exoteric over the esoteric (and, in so doing, can argue for its success in modern scientific terms). By contrast, Fraser acknowledges that a modern distinction between what is science and what is not threatens an anachronistic and misleading view of alchemy.

The inclusion of both Bowen's chapter, which doggedly sticks with ancient conceptions of *astrologia* (in which astronomy and astrology are often enmeshed), and Cooper's, which focuses on astrology, suggests that the editors had either not sought to impose definitions and approaches or wanted contributions that would reflect different definitions and approaches. Some contributors question definitions in such a way that justifies their choices. For example, Beagon opens by wondering whether there is such a thing as an ancient encyclopedia or even encyclopedic writing; the chapter that follows suggests she has established criteria that suggest there are. Beagon's anxiety over generic definitions is modern, though the parameters of her chapter are then structured around the very definitions she questions.

The classicist new to the fields of ancient science and medicine will benefit from the questioning of long-standing assumptions and over-simplifications in many of the chapters—for example, that Greek science simply emerged from and continued Near Eastern work. The following, from Zhmud's chapter on mathematics, might stand as a programmatic statement on how to deal with transmission of ideas across space and the problem of parallel evolution of scientific ideas: "Real or assumed isomorphism between two mathematical theories, formulas, or methods often gives rise to common-origin hypotheses, but only the theories placed in a specific historical setting with identifiable ways of transmission survive the tests" [184]. Another assumption—that the canons of authors and authorities passed down to us are historical—is also widely tackled. For example, Scarborough reminds us that Dioscorides enjoys a higher reputation than does Scribonius Largus thanks to Galen; and Galen himself dominates discussions of Roman-era medicine simply because he wrote so much (and so much survives).

There are a few typographical errors: p. 89, “patters” for “patterns”; p. 120, “(Needham and Wang 1959)” for “Needham and Wang (1959)”; p. 152, *testmonia* for *testimonia*; p. 391, “though” for “through”; p. 466, “Xenophon’” for “Xenophon’s”; p. 615, “Laërtius, Major scholars” for “Laërtius. Major scholars”. There are some proofing errors too: p. 322, “Aethiopia ~~Ethiopia~~”; p. 486, “have attempted harmonize”; p. 629, “Furley1999”; p. 682, “Craftsmancraftsman”; p. 822: “Ptolemy’s regarded his theories”; p. 937 “(*see*”. (a reference has dropped out); p. 945, “soma” should be italicized; p. 951, “he would also commented”. Clagett 2000 (cited on p. 54) and Schürmann 1991 (cited on p. 340) are not included in the relevant bibliographies. But these errors are few in a book of over 1,000 pages, and the overall production quality is high. The editors are to be commended that all Greek text presented in the volume has been transliterated, and all Greek and Latin text is translated.

There is a wealth of information in this volume, much more than I anticipated. It comes at a literal cost: the list price for the volume is $175, which is steep for a graduate student or the classicist looking for an introduction to science and medicine (though far less than Irby-Massie’s *Companion*). But should they take the plunge: this is a fascinating and absorbing volume that will expose them to aspects of the ancient world still too little considered by many in the field.

BIBLIOGRAPHY

Irby, G. L. 2016. *A Companion to Science, Technology, and Medicine in Ancient Greece and Rome*. 2 vols. Chichester, UK/Hoboken, NJ.

Irby-Massie, G. L. and P. T. Keyser. 2002. edd. *Greek Science of the Hellenistic Era: A Sourcebook*. London, UK/New York.

Keyser, P. T. and G. L. Irby-Massie. 2008. edd. *Encyclopedia of Ancient Natural Scientists*. Abingdon, UK/New York.

Oleson, J. P. 2009. ed. *The Oxford Handbook of Engineering and Technology in the Classical World*. Oxford, UK/New York.

Plato's Timaeus *and the Latin Tradition* by Christina Hoenig

Cambridge, UK/New York: Cambridge University Press, 2018. Pp. xvii + 331. ISBN 978–1–108–41580–4. Cloth €120.95

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After enumerating several recent works on the *Timaeus*, all in English and almost all belonging to the same interpretative family, Christina Hoenig explains in her introduction that "the present examination focuses on the development of Platonic philosophy at the hands of Roman writers between the first and the fifth century BCE" [5]. This is the period when Platonists cut off their connections with the probabilist New Academy, and in which a new dogmatism was established, with Greek philosophy continuing to enjoy great popularity within the Roman élite.

This books contains five chapters: the first is on the *Timaeus* and its interpretation, while the others are on Cicero, Apuleius, Calcidius, and Augustine.

In the first chapter, the *Timaeus* is situated in a dualist metaphysical context which considers that true reality is to be found at the level of the intelligible, of which things are mere images. The narrative method of the *Timaeus* remains ambiguous: one cannot choose between λόγος and μῦθος, for the story concerns the origin of the sensible world, which is a mere image (εἰκός) of the intelligible. Adopting a position on this question requires choosing between a literal and a metaphorical reading of this story about the origin of the world. A similar ambiguity concerns the identity of the demiurge, who is considered either as a separate intellect or as the intellect of the soul of the world. Finally, χώρα, bereft of any property, is considered as the basic substrate of change. Ultimately, "the Timaean narrative portrays the universe as a teleologically structured whole" [17]. These are interpretative presuppositions that should be discussed within the context of a commentary on the *Timaeus*, but which cannot all be taken into consideration in the context of this book.

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Cicero succeeds in reconciling his activity as an orator with his translation of the *Timaeus*, of which only fragments remain. His reading of *Timaeus* 29b2–d3 allows him to give to the term «εἰκώς» the meaning of «πιθανόν» (*probabile* or *veri simile*) in accordance with the definition of rhetoric in Plato's *Gorgias* and, especially, in Aristotle's *Rhetoric* [1356b ff.]. Nevertheless, one must not separate what remains of Cicero's translation of the *Timaeus* from what we find in his philosophical treatises. In both cases, we encounter the previous controversies concerning this treatise on the origin of the world, taking into account the criticisms by Aristotle, by Stoics, and by Epicureans. Cicero advocates an interpretation of the term «ἀρχή» that tends toward a temporal origin of the world, which implies the hypothesis of a new design (*novum consilium*) in a demiurge who is supposed to be an immutable divinity. Cicero thereby distinguishes himself from the probabilism of Carneades, and seems closer to thinkers like Philo of Alexandria.

Apuleius takes his place within the dogmatic interpretation of Plato that was customary in the second century AD. For him, Platonic doctrine develops according to a well-defined program which moves from ethics to physics, and finally to theology, that is, to metaphysics. The acquisition of philosophical knowledge is assimilated to the celebration of the mysteries, as is implied by the vocabulary of the *Phaedrus* or the *Symposium*. Apuleius, who was a rhetor, thus becomes the high priest of this cult [112]. We therefore find in him a mixture of rhetoric, philosophical dogmatism, and religion, which can be explained by the fact that he assimilates dialectic to genuine rhetoric. Nevertheless, in the treatise *On the World*, attributed to Aristotle, which Apuleius was said to have translated, we find the essential points of the interpretation that he proposes for the *Timaeus*, in that he attempts to carry out a synthesis between a temporal origination of the world and its everlasting existence. It is the insertion of the harmony between the elements that ensures the eternity of the world, a harmony that is maintained by providence and which implies a highly elaborate demonology.

We find a similar interpretative scheme in Calcidius, whose identity is impossible to determine. It seems that Osius, Calcidius' sponsor, had merely ordered him to translate the *Timaeus*. Yet Calcidius, whose mother tongue must have been Greek, translated only the cosmological part of the dialogue, to which he added a commentary in order to shed light on its subject matter. The commentary reveals the influence of Numenius and probably of Porphyry, the disciple of Plotinus, who was accused of having plagiarized Numenius. As a Christian, Calcidius could not help but militate in favor of a temporal origin of the world. For him, the whole problem consisted in reconciling the image of a transcendent divinity with the idea of a material world structured by providence. In interpreting *Timaeus* 28c3–5, Calcidius discovers a triadic structure: at the summit one finds the *summus deus*; then comes providence, which imitates the goodness of the first god and introduces it into the world; finally comes fate, which depends on providence, which the soul of the world obeys. This is, moreover, why demons no longer play their traditional role of carrying out the designs of providence and destiny. Calcidius finds clear confirmation of this in the passage from the *Timaeus* concerning the four kinds of living beings that must be included within the complete living being [39e10–40d5]: a celestial kind [39d7–8], that of the demons, and three terrestrial kinds—living beings that fly, those that swim, and those that walk the Earth. The demons are rational, immortal living beings, subject to passions and made of aether, who take care of human beings. As is the case in the *Epinomis*, there are several kinds of demons living in different places.

Augustine uses Cicero's translation of the *Timaeus* to corroborate the Christian tradition of creation and to oppose the interpretation of the Platonists. What Augustine says about the creation of the world and the salvation of the soul is inspired by the interpretative tradition of the *Timaeus* in Cicero and in Apuleius. Moreover, his interpretation of Genesis 1:1 features several points that are akin to what one finds in Calcidius, which suggests that he may have made use of the same source. Basically, Augustine believes that Plato, who defends a coherent system, borrows his physics and his theology from Pythagoras, and his ethics and dialectics from Socrates. In Augustine, creation features three moments. The first stage of creation is atemporal, since it is Jesus Christ, son of God the Creator, who cannot be situated within time. What follows, however, is temporal: on the first day, God creates the angels; then, during the following days, comes the turn of sensible things. The first two stages, described as creation (*conditio*), are beyond human sensation and knowledge. The third stage, in which sensible things appear, is called *administratio* and is partially open to human knowledge. As we can see, this account of creation is a patchwork which associates Christian revelation with the essential elements of Platonism. In particular, it allows one to reconcile the transcendence of the Creator with providence, a problem which the Middle Platonists had to confront. To solve this metaphysical problem, Augustine makes the figure of Christ a mediator between the divine and the human world. This mediating status of Christ leads Augustine to devalue the beings that established a bridge between the sensible and the intelligible. This is why he undertakes to show that what

Apuleius says about demons as mediators between the mortal and the divine must be rejected, as must the tripartite division god, man, and demon.

This book, which partially takes up a thesis defended at Cambridge (UK) in 2012, is less rich than the one by Stephen Gersh [1986] because it deals with a smaller number of authors and focuses only on the interpretation of the *Timaeus*, which was the paradigmatic dialogue for Platonists at the time. Yet this work is well written, well structured, and very clear. It contributes a great deal on the history of the influence of Platonism among Latin philosophers. The translations, which the author has made of Greek and Latin texts, printed in two facing columns, are very useful for following the course of the exposition, which definitely shows how a translation from Greek into Latin is based on an interpretation which in turn makes the manner of translating the text evolve. Finally, it should be noted that most of the contemporary interpreters of Plato's *Timaeus* often understand the dialogue as the Middle Platonists did, which gives this volume a genuine currency.

BIBLIOGRAPHY

Gersh, S. 1986. *Middle Platonist and Neoplatonism: The Latin Tradition*. 2 vols. Notre Dame, IN.

Everyday Magic in Early Modern Europe edited by Kathryn A. Edwards

Farnham, UK/Burlington, VT: Ashgate: 2015. Pp. 296. ISBN 978–1–4724–3350–3. Cloth US$119.95

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Historians have long known that the efforts of religious reformers, both Catholic and Protestant, to challenge the magical beliefs of ordinary people in early modern Europe met with limited success, and that a rich stratum of unorthodox supernatural beliefs survived well into the 18th century. This welcome collection of essays addresses the negotiations and compromises between official religion in its various forms and the vibrant world of popular magic during the "long Reformation".

The study of magic has often meant the study of witchcraft. Inevitably perhaps, many of the contributors to this book draw on the records and historiography of witch trials, and often with illuminating effect. The treatment of witches was an important area of tension (and accommodation) between religious authorities and the magical assumptions of ordinary people. But this collection seeks to move beyond the European witch trials to examine the larger and generally less dramatic "lived experience" of early modern magic. In this enterprise it achieves considerable success.

One obvious point of divergence between official and folkloric assumptions about the occult arose from the practical nature of magic. For many ordinary people, the effectiveness of magic was at least as important as its nature. In a fascinating discussion of the "magical lives" of villagers in Catalonia, Doris Moreno Martínez observes that a witness in a case of alleged healing magic in 1649 did not know, or greatly care, whether the healer derived her power from God or the Devil. Elsewhere, ordinary people were careful to defend magical traditions that had practical utility, though they were mindful of the need to stay within the accepted boundaries of religion. Raisa Maria Toivo suggests that communities in 17th-century Finland negotiated the

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border between legitimate and illicit magical activities with the Lutheran authorities, often preserving local customs in the process.

In perhaps the most memorable essay, Johannes Dillinger considers the activity of magical treasure-hunters in early modern Europe. Again, the practical context of their supernatural work appears to have been crucial. Despite the interaction with demons that was often part of their business, treasure-hunters did no harm to others, and were seldom condemned for witchcraft as a result. Also their operations did not involve the transfer of wealth to themselves from the rest of the community, as the fortunes they sought were otherworldly. In a strangely haunting detail of the kind that characterizes the collection as a whole, Dillinger adds that the success of magical digs depended on the solemn silence of their participants.

Two other essays explore attitudes towards divination and dreams. Jason Coy considers the hostility of many Protestant thinkers to fortune-telling of all kinds—apart from the reading of special providences practised by the reformed clergy. In contrast, Jared Poley surveys more sympathetic ideas about the interpretation of (possibly) predictive dreams. In the process, he offers a fascinating glimpse into the dream literature of Tudor and Stuart England.

The nature of various kinds of spirit, and the proper human attitude towards them, has recently received much scholarly attention. Here, the essays by Antoine Mazurek and Kathryn A. Edwards make valuable contributions. Mazurek considers the delicate status of guardian angels in early modern Catholicism. Edwards offers a penetrating analysis of the haunting of the house of the Huguenot minister François Perrault in 1612. She observes that Perrault moved between an orthodox Calvinist interpretation of the spirit as a demon and wider, folkloric understandings of its nature. This ambiguity, she suggests, was probably common among even devout people faced with such supernatural encounters.

Linda Lierheimer also notes the belief in deceiving spirits in the context of "false sanctity" in 17th-century French convents. But she observes that cases of spiritual imposture were often attributed to fraud or the vivid imagination of young women, and dealt with quietly within their institutions. In the final chapter, Sarah Ferber explores the relationship between "everyday magic" and more extreme (or notorious) events within late medieval and early modern Catholicism. She notes the crucial role of the church as an arbiter of acceptable varieties of "magic", and the considerable porousness between legitimate and illicit activities.

This collection will be welcomed by scholars and students of the supernatural in the period. In the range and subtlety of their work, the writers make an important contribution to an already rich and exciting field.

MondSymbolik – MondWissen. Lunare Konzepte in den ägyptischen Tempeln griechisch-römischer Zeit by Victoria Altmann-Wendling

Studien zur spätägyptischen Religion 22. Wiesbaden: Harrassowitz, 2019. 2 parts. Pp. xxviii + 1098. ISBN 978–3–447–11136–2. Cloth €198.00

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This publication comprises the slightly revised version of the author's doctoral dissertation, which was submitted in 2017 to the Philosophical Faculty of Eberhard Karls University, Tubingen. The study concerns the embedding of lunar phenomena and the Moon's cycle in religious contexts, while the calendrical aspect plays only a secondary role. The timeframe of the investigation is concentrated on the Ptolemaic-Roman Period.

Part 1

In section A, Altmann-Wendling makes some preliminary remarks. As she notes, in Egypt, the terms «iʿḥ» or «iwn-ḥʿʿ» could be used for the Moon [2]. She recapitulates the history of research on the Moon's role in chronology [5–13] and its mythological dimensions [14–19], reaching back in part to the work of R. Lepsius. Some astronomical facts about the Moon are imparted, among which is its motion at a mean distance of 384,400 km from the Earth [29]. She then presents the relevant astronomical knowledge of the Graeco-Roman Period, Aristarchus of Samos gaining special mention as the first proponent of the heliocentric view of life [32].

Section B deals with the temples, starting with Dendera. The scene with the catching of the *udjat*-eye on the astronomical ceiling of the pronaos is interpreted as an increasing lunar phase [38]. In the texts on the eastern margin line of the ceiling, the healing of Osiris is similarly associated with the increase of the lunar phase [58]. The decoration of the pronaos was devoted to the astral Hathor, the key theme being the New Year [69]. In the

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lunar staircase scenes of Dendera, the process of the Moon's waxing to Full Moon is reproduced by the actions of the 14 gods on the steps [105]. The hymn to the Moon on the staircase has several parallels: see pBM EA 10474v Z. 13–19, pBerlin P.157 62a, 10–14; and on the southern door jamb of the outer side, in Bigeh and Kom Ombo 202 [128–133].

The texts in Edfu are introduced next. In the description of the lunar synodic month of roughly 30 days on the pronaos in Edfu, the movement of the Sun runs from east to west, while the Moon proceeds from west to east [277]. The minerals and plants in the pylon texts of Edfu and Philae can be interpreted as integral parts of the filling of the Moon-eye [314]. The bearers with their offering are recruited from the nine hood of Heliopolis,[1] with some additions [315]. The processions of the barques of Hathor and Isis on the northern side of the east tower of the pylons in Philae are comparable to the scene with the two barques of Horus and Hathor in Edfu [337]. The chapel of the leg in Edfu (Room J) is treated next. The sole, clear scene concerning the Moon appears in the second register of the western wall with the handing over of the crown by the king to the god Chons [346].

The procession of the Moon's worshippers on the Euergetes gate in Karnak is next in line for discussion. In the middle of the frieze, there is a Full or crescent Moon to which gods and the royal pair are striding from both sides [352]. The Moon may be venerated by those who worship the Sun as a nocturnal substitute [360]. The choice of the direct object of veneration is interpreted by the author as a hint at the completely round form of the Full Moon [360], which is simply difficult to understand. The eastern side of the frieze on the gate in Karnak deals with the Full Moon, while the western side touches on the New Moon and new light [398]. The scenes of the two bulls on the Euergetes gate are then brought into the picture. The central text contains one of the most exact descriptions of the Moon's cycle, without being limited to one of the two halves of the month [405]. The two bulls perhaps embody the waning and waxing Moon [405].

The lunar inscriptions in the temple of Chons in Karnak follow next. The lunar scenes in the temple from the time of Ramses III are all Ptolemaic redecorations [527]. The investigation is continued by texts in the temple of Opet in Karnak. The appearance of the lunar epithets in the Opet temple is connected with the regeneration and rebirth of Osiris depicted there [549].

[1] *scil.* a group of nine gods.

The tombs of priests in the Bahria oasis may be considered to show some of the earliest examples for lunisolar scenes [648].

Section C describes papyri with lunar aspects. The myth of Horus and Seth presents the basis for the description of the lunar cycle in the chapters on the Moon and planets in the *Book of Nut* [679].

Part 2

Section D treats the Moon as celestial body and god. The representation of the Moon as a celestial body occurs for the first time in the New Kingdom [699]. The most frequent way of representing of the Moon is as an *udjat*-eye or the combinations built with its help [703]. The representation of the Moon as an ibis is considered very rare [707]. The gods in the lunar processions of gods are collected mostly from the great Theban nine hood with 14 or 15 individuals who embody the days of the lunar month [732]. In Esna, the scene with 28 gods in two registers as an embodiment of the wholeness of the Moon's illumination during a lunar month is exceptional [734]. The scenes are mostly executed in the east-west direction [735].

Section D3 gives insight into the names of the Moon. The expression «wꜣḥ ḳd=f» (who takes off his form) is accepted as a term for the decrease of the Moon under certain conditions [752]. D4 inquires about the Moon as a goddess. The goddesses Hathor and Isis were associated with the Moon [761]. But two and three dimensional representations of the Moon as a goddess are very rare [766]. Section D5 broaches the topic of animals representing the Moon. The Moon was symbolized as a bull [769–770], ibis [770–772], ape [772–773], or cat [773]. The hostile animals representing the Moon include the Oryx gazelle [774] and the pig [777–780].

The most complete inscription about the actions of an Egyptian astronomer is on the Ptolemaic statue of Harchebis [786]. The explicit interpretation of eclipses as omens is documented in Egypt after the first century AD [792]. The first introduction of astrological concepts from the ancient Near East is dated in the Persian Period [795].

Section E addresses the Moon cult. E1 deals with the ancient observations of the Moon. The earliest attestations of star gazers come from the fifth dynasty [782]. In section E2, lunar feasts are examined. The only ritual with a recitation text is the "Book of the New Moon Feast", written down on two funerary papyri [799]. Since the Old Kingdom, the days of the lunar month occur as dates of offerings for the deceased [802]. Section E3 scrutinizes the names of the days of the lunar month. The terms for the days of the Egyptian lunar month exist in complete form only since the Ptolemaic Period [811]. E4 discusses the filling of the Moon-eye on the sixth day of the lunar month.

The day of the coincidence of Osiris with the Moon and the filling of the *udjat*-eye is important [833]. The origin of the filling on this day goes back to Sun-myths from Heliopolis [835]. Section E5 takes into account the centers of the cult of the Moon. The Theban provenance of this cult can be deduced from the prominence of the Moon in the names of the Ahmosidic family [848–849]. The existence of a cult of the Moon in Heliopolis, however, is not supported by hard evidence [853].

Section F evaluates lunar concepts. In F2, short forms of the text passages are presented. F3 discusses pictures, identifications, and metaphors. The expression «iwn-ḥꜥꜥ» (jubilating pillar) for the Moon dates from the third to the second century BC [878]. The identification of the Moon as bull can probably be founded on the association of the lunar crescent with the horns of a bull [881]. The metaphorical conception of the Moon as an eye may be the oldest means of accounting for the cycle of lunar phases [888]. The frequent connection between the Moon and Osiris can be put down to the power of the Moon to regenerate [894]. The theme of the Moon's rejuvenation is expressed most clearly by its identification with a child [900]. Section F4 samples some aspects of the Moon and the principle of *maat*.

I offer the following as an aid to the reader:

page 45	For «ꜣmś» (sceptre of Osiris), see Rickert 2011, 145.
50	For the introduction of the *artabe* measure in the Persian Period, see Chaveau 2018, 3–5.
130	For the relationship between «śštꜣ» and «śšd», see Fischer-Elfert 1997, 19.
131	For «brbr» instead of «bnbn», see Jansen-Winkeln 2005, 134 [19]
161	The translation "the cloud is driven away" for «dr(.w) igp» is grammatically impossible: the correct version is "who drive away the cloud (from the «rḏw»-outflows)".
490	«ḫpi n=f ṯꜣw nḏm n mḥii.t rꜥ nb» has to be translated by "for whom the sweet breeze of the north wind comes every day". For «ṯꜣw» (breeze of the north wind), see, e.g., Assman 1999, 393 (no. 167); Luft 2018, 573.
532	«ḫśr=k ḥꜣ.ti» should be subordinated as "while you are destroying the clouds".
622	For «nḫb» (lamp), see Scheele-Schweitzer 2014, 498; Jansen-Winkeln 1985, 47 (5).
849	The translation "who satisfies the *udjat*-eye" for «śmnḫ wḏꜣ.t» must be corrected to "who makes perfect the *udjat*-eye".

This book will be of value to students of Egyptian conceptions of the Moon and its use as a symbol. With only a few exceptions, the translations are accurate and the interpretations turn out for the most part to be very clear. Moreover, the sources are well mastered. I warmly recommend it.

BIBLIOGRAPHY

Assman, J. 1999. *Ägyptische Hymnen und Gebete*. 2nd edn. Freiburg.

Chaveau, M. 2018. “La plus ancienne attestation démotique de l’artabe?”. Pp. 3–8 in K. Donker van Heel, F. A. J. Hoogendijk, and C. J. Martin edd. *Hieratic, Demotic and Greek Studies and Text Editions: Of Making Many Books There Is No End. Festschrift in Honour of Sven P. Vleeming*. Leiden/Boston.

Fischer-Elfert, H.-W. 1997. *Lesefunde im literarischen Steinbruch von Deir el-Medineh*. Wiesbaden.

Jansen-Winkeln, K. 1985. *Ägyptische Biographien der 22. und 23. Dynastie*. Wiesbaden.

——— 2005. “Denkmäler einer thebanischen Offiziersfamilie der 22. Dynastie”. *Studien zur altägyptischen Kultur* 33: 125–146.

Luft, D. C. 2018. *Osiris-Hymnen, Wechselnde Materialisierungen und Kontexte. Untersuchungen anhand der Texte “C 30”/Tb 181, Tb 183, “BM 447”/Tb 128 und der “Athribis”-Hymne*. Tubingen.

Rickert, A. 2011. *Gottheit und Gabe. Eine ökonomische Prozession im Soubassement des Opettempels von Karnak und ihre Parallele in Kôm Ombo*. Wiesbaden.

Scheele-Schweitzer, K. 2014. *Die Personennamen des Alten Reiches, Altägyptische Onomastik unter lexikographischen und sozio-kulturellen Aspekten, Philippika*. Wiesbaden.

The Arabic, Latin and Hebrew Reception of Avicenna's Physics and Cosmology edited by Dag Nikolaus Hasse and Amos Bertolacci

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Avicenna occupies a unique place in the history of philosophy in Islam. Not only was the synthesis that he elaborated one of the culminating points of classical Islamic culture; it also constituted fertile ground for the flourishing of various intellectual trends in the post-classical period of Islamic history (from the early 13th century onward). His corpus, and especially his central works *The Cure* (*Kitāb al-Shifāʾ*), *The Salvation* (*Kitāb al-Najāt*), and *Pointers and Reminders* (*al-Ishārāt wa-al-tanbīhāt*), inspired generations of Muslim theologians and philosophers, and were the object of a long and rich commentary tradition that extended up to the 19th century. But his impact was not by any means restricted to a Muslim audience. Avicenna holds the rather unique privilege among medieval thinkers of having (like Aristotle) profoundly shaped the development of Latin, Hebrew, and Arabic philosophy and theology. But while his metaphysical legacy has been appreciated for some time and has been the focus of considerable scholarly research, the physical theories that he bequeathed to posterity have not been extensively studied.

Bearing this in mind, the present volume is a rich and important contribution to the history of Avicenna's physics and its critical reception in medieval Jewish, Christian, and Islamic intellectual history. As a companion piece and sequel to the previously published *The Arabic, Hebrew and Latin Reception of Avicenna's Metaphysics* (prepared by the same editors and publisher) it

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effectively complements our assessment of the impact that Avicenna had on later philosophical activity in the Islamic world and medieval Latin Europe.

The book is a collection of 13 articles written by specialists in their respective fields covering a large array of issues, with an emphasis on the physical notions of place, time, and motion, as well as on meteorology. The studies successfully combine philological expertise with insightful analyses of the main philosophical theories articulated in the works of Avicenna and his commentators. Since in many cases these studies tread new ground and delve into hitherto unexamined texts, they do not always make for easy reading and often assume a highly technical character. Accordingly, the volume is aimed primarily at graduate students and specialists in the field of medieval intellectual history rather than at a lay readership seeking to learn more about medieval philosophy.

There is a cluster of themes that run through the volume and unify its various contributions. One of them has to do with the systematic and sometimes scholastic nature of Avicenna's reception in later philosophical circles. The studies in the volume uniformly testify to the high level of philosophical reasoning and argumentation that were deployed to make sense of Avicenna's ideas, as well as to elaborate or amplify his theories and, at times, also to question or even criticize his position on specific points of doctrine. The book showcases some of the main actors and figures involved in the dissemination and interpretation of Avicenna's philosophy in the Middle Ages. In most cases, these thinkers approached Avicenna's physics in a rather programmatic manner and with a specific aim in mind, either as part of an effort to interpret Aristotle or from within the tradition established in some school of commentary on the master. In other instances, the aim was to provide a harmonizing synthesis of various philosophical sources or to refute him or even to explain some passages of scripture in a rational or naturalistic manner by relying on his works. Regardless of the specific intention orienting these readings of Avicenna, medieval thinkers in general had direct access to at least some of his principal physical works, notably, his treatises on meteorology and *Physics of* The Cure (*al-Samāʿ al-ṭabīʿī*), which lie at the heart of the volume.

The rigor and technicality of the later responses to Avicenna, as well as their dialectical and sometimes scholastic style and format, are particularly well brought out in the articles by Jon McGinnis, Jules Janssens, Peter Adamson, and Andreas Lammer. These studies suggest that Avicenna, by the late 12th or early 13th century, had begun to occupy a position in the Arabic tradition comparable to that of Aristotle in the Greek commentary tradition of Late Antiquity. Just as it was inconceivable for an aspiring late-antique philosopher not to grapple with the views of the main authorities, Plato and Aristotle, so it would have been very difficult for an Arabic scholar from the 12th century onward not to engage directly with Avicenna's writings. This point also applies to the Latin West in the aftermath of the translations of Avicenna's works from Arabic to Latin, which unfolded in particular in the city of Toledo in Spain.

As the various articles focusing on the Latin reception of Avicenna emphasize, the master's legacy proved crucial in orienting discussions on meteorology and physics in medieval Europe. Jean-Marc Mandosio's article documents the reception of Avicenna's meteorological treatises in the Latin West and argues that Avicenna became an authority in this field, to such an extent that his writings were sometimes used to fill gaps in the Aristotelian corpus. As Cecilia Trifogli's comparative study convincingly shows, Avicennian physics underpins many of Roger Bacon's (d. 1292) most important theories in his *Communia naturalium*, such as those focusing on nature and change. Katrin Fischer for her part exposes the similarities and differences between William of Auvergne's (d. 1249) and Avicenna's conceptions of efficient causality, particularly in how it relates to eternality and to God as a cause of the world.

Yet, the fact that Avicenna achieved an authoritative status in post-classical Islamic intellectual history and in the Latin West should not divert our attention from the very vivid critiques that his philosophy inspired among certain groups. Cristina Cerami's article, which systematically maps the various objections that Averroes had to Avicennian physics, is a welcome proviso regarding Avicenna's legacy, which was not always received positively or constructively. Through a meticulous analysis of Avicenna's and Averroes' physical texts, Cerami shows that Averroes' responses to Avicenna were systematic in nature and part of a general strategy aimed at purging Aristotle's philosophy from these external "Avicennizing" elements. (This thesis is also put forth in Bertolacci's study.) Likewise, the articles by Janssens and Adamson focus on the great polymath and Ashʿarite theologian Fakhr al-Dīn al-Rāzī (d. 1210) and tease out Rāzī's critical attitude and free philosophical spirit, as well as his willingness to depart from Avicenna on key physical issues.

In general, however, one observes a rather conciliatory and constructive attitude towards Avicenna's legacy. Medieval scholars deployed a variety of means to interpret, adapt, and integrate Avicennian material into their systems, often in an attempt to harmonize it with religious considerations. McGinnis' article, for example, stresses the long-lasting impact of certain

Avicennian ideas that trickled through various layers of commentaries in the later Islamic tradition, and which in general were accommodated within a larger religious framework. It also bears testimony to the fact that later commentators did not hesitate to resort to Avicenna's logical and metaphysical theories in order to contextualize or explain his physical ideas. Resianne Fontaine shows that Abraham ibn Daud, a 12th-century Jewish scholar involved in the translation movement of Arabic to Latin in the Iberian Peninsula, most likely relied on Avicenna's *The Cure*, as well as on Ghazālī's (d. 1111) summary of philosophical doctrines entitled *On the Aims (or Doctrines) of the Philosophers* (*Maqāṣid al-falāsifa*), to elaborate his own doctrine. Like many other medieval thinkers, he sought to reconcile scripture and philosophy,[1] and Avicenna's theories played a key role in that process. Gad Freudenthal provides a thought-provoking analysis of how various Jewish thinkers grappled with the problem of "the formation and perseverance of dry land". Freudenthal's study reviews an array of "fideist and rationalist interpretations" articulated by Jewish scholars of the 13th and 14th centuries. Remarkable in this regard is Samuel ibn Tibbon's (d. 1232) willingness to borrow Avicenna's cosmological and meteorological arguments in order to argue for the periodic flooding of dry land by the sea and to provide a philosophical exegesis of certain passages of the Book of Genesis. And, while Cecilia Trifogli shows that Roger Bacon's involvement with Avicennian physics was primarily philosophical and intellectual in nature, Katrin Fischer clearly brings out the religious dimension of William of Auvergne's evaluation of Avicennian theories, particularly with regard to the controversial and religiously sensitive topic of the creation of the world; even then, William did not shy away from integrating key Avicennian concepts in his system.

Occasionally, the priority was to reconcile Avicenna with other philosophical views. As Amos Bertolacci shows lucidly, Albert the Great, in his commentaries on the *Physics* and *Metaphysics*, implemented a threefold strategy ("material", "stylistic", and "doctrinal") aimed at harmonizing Avicenna's and Averroes' physical theories, a synthesis which in turn forms a cornerstone of his own philosophical system. This feature of Albert's approach to philosophy suggests a certain evolution in his understanding of Avicennism and Averroism while at the same time underscoring his reliance on these Arabic thinkers.

[1] This is indicated by the very title of Abraham's main work, *The Book of Exalted Faith That Brings Agreement between Philosophy and Religion*.

Thus, in addition to providing engaging case studies of Avicenna's influence, the volume offers a more fine-grained assessment of the reception of the three great philosophical authorities in the Middle Ages, Aristotle, Avicenna, and Averroes, that shows complex and shifting patterns of influence in the works of individual Latin scholars. And, while it has long been known that Avicenna's logic and metaphysics exercised a profound influence on medieval Jewish and Christian thinkers located in the Western Mediterranean and in Europe from the 12th century onward, the studies gathered in this volume corroborate the hypothesis that Avicenna's physics was also an important source of inspiration for these philosophers.

One of the book's great merits is to dwell on and illuminate some of the key mechanisms at play in the reception of Avicenna's works. It is fascinating to realize that the process of interpreting Avicenna inevitably led to doctrinal transformation and adaptation as well, a phenomenon that is very well brought out in the volume. When it comes to physics in particular, it was common for medieval thinkers to adhere to Aristotelian and Avicennian theories, while at the same time grounding these physical theories in a cosmological and theological paradigm that was often inspired directly by the religious texts. This shows the great extent to which science—in this case physics, but the same applies to astronomy—could be reconciled with a religious worldview without preventing creative and experimental thinking.[2]

In other words, one could be an Aristotelian or an Avicennian regarding specific issues of physics, while otherwise upholding the tenets of divine creation or the temporal finitude of the world, and one could even rely on Avicenna to interpret specific aspects of scripture. These dynamics between Avicennian physics and religious views are examined in detail in the volume, which sheds considerable light on strategies of textual adaptation, assimilation, and harmonization, as well as on Avicenna's (largely involuntary) role in what A. I. Sabra once called the "naturalization" of science in an Islamic setting.

In this connection, the book also provides valuable information regarding exactly which Avicennian works were instrumental in shaping the later tradition of physics in Hebrew, Latin, and Arabic. This textual problem is more difficult than it first appears, since medieval scholars rarely acknowledge their sources. In particular, the issue of the putative influence of the sections of *The Cure* and *Pointers and Reminders* bearing on physics are explored in

[2] For an example of the latter, see Adamson's chapter [65–100].

detail, with a complex picture arising. The reliance on either was due in part to geographic and temporal circumstances, but mostly to the way in which the Avicennian corpus was preserved and transmitted, a topic that remains only partially understood to this day.

In general, it was the part on physics in Avicenna's *Pointers* that enjoyed the most popularity in the eastern swaths of the Islamic world, although even there it was occasionally superseded by the *Physics of* The Cure. In the Latin West and in medieval Hebrew circles, where the *Pointers* remained unknown, *The Cure* (*Meteorology*, *On the Heavens*, *Physics*, and so on) represented the main text and was sometimes read in conjunction with Ghazālī's summary of Arabic Peripatetic philosophy, the *Maqāṣid*.[3]

In this context, the volume also usefully explores dynamics of textual transmission and translation from Arabic to Hebrew and Latin. This is the case notably in the article by Hasse and Büttner, which seeks to "lift the anonymity" of many Arabic to Latin translations by relying on computational stylometry and a careful lexical analysis of the extant translations. Their study confirms many of the hypotheses proposed by earlier scholars (notably Manuel Alonso and Charles Burnett) regarding the authorship of some important translations and supports highly plausible theories regarding other problematic texts (including Avicenna's *Physics* of *The Cure*, the translation of which into Latin Hasse and Büttner attribute to Gundisalvi). Ultimately, their study expands the corpora of translations attributed to key figures such as Dominicus Gundisalvi, Michael Scot, and Gerard of Cremona.

I have a few qualms with the volume. One of them is that the editors nowhere provide a sustained and satisfactory explanation of the term "cosmology". This is problematic inasmuch as classical Arabic does not have a word that neatly corresponds to it. What may approximate it best is the expression «ʿilm al-hayʾa», which eventually came to designate "astronomy" in the Arabic tradition, especially in post-classical times, but which during the classical period co-existed with a variety of other locutions such as «ʿilm al-nujūm» and «asṭrunūmiyā», with which it bears an ambiguous relation (notably when it comes to the place and legitimacy of astrology). In this connection, the editors' proposal in the introduction [1] to construe the expression «ḥikma mutaʿāliya» as meaning cosmology seems unconvincing,

[3] It should be noted that the latter was sometimes erroneously perceived as a genuine philosophical work, when Ghazālī in fact had probably intended it as a premise to his critical onslaught on the Arabic philosophical position as embodied in his *Incoherence*.

and this idea is at any rate not explored in detail in the article by Gutas that deals exclusively with the meaning of this phrase. Gutas' erudite study unravels the syntactic, lexical, and terminological problems associated with «al-ḥikma al-mutaʿāliya» (a *hapax legomenon* in the Avicennian corpus). Thanks to a detailed philological analysis of the later Arabic commentaries on Avicenna, it provides an illuminating case study of the relation between language and philosophical meaning. At any rate, it would have been worthwhile for the editors to devote more space to the notion of cosmology, all the more so since it is distinguished from physics in the title of the book, and since most of the articles deal with the sublunary world rather than with the heavens and heavenly phenomena *per se* (arguably the first sense of cosmology).

Furthermore, although one can only applaud the breadth of the volume and the high quality of its individual contributions, a critical reader may remain skeptical at the attempt to address the Jewish, Christian, and Islamic traditions in a single stroke. Although the book succeeds in corroborating Avicenna's position at the confluence of these three traditions, and thus also in stressing some of the textual and intellectual commonalities that connect them, it inevitably only scratches the surface of what appears to have been an extremely complex, widespread, and multifaceted phenomenon, one, that is, which seems too broad to fit within the covers of a single volume. In this regard, if the Muslim reception of Avicenna is adequately represented (six articles, two of which focus on the works of Fakhr al-Dīn al-Rāzī, which seems justified given his sheer stature in Islamic intellectual history and his pivotal role in the later interpretation of Avicenna), the Christian reception is less well represented (five studies, or rather four, since one article deals with technical issues of translation); and the Jewish reception, inadequately so, with only two studies focusing on this theme.

Moreover, this approach also leads to some lacunae and glaring omissions relative to the tradition to which Avicenna himself belonged. For example, one regrets the absence of a study on the Arabic Jewish philosopher Abū al-Barakāt al-Baghdādī (d. 1165), who often adopted a highly original approach to physics, and whose works effectively underscore the complex dynamics of borrowing and departing from Avicenna. Likewise, one misses a study on Naṣīr al-Dīn al-Ṭūsī (d. 1274), one of the great exponents of Avicennian philosophy in the 13th century and a towering theorist of Shīʿī theology, or of Mīr Dāmād (d. 1631) and his pupil Mullā Ṣadrā (d. 1640), whose evaluations of Avicennian physics and metaphysics in the 17th century represent a fascinating aspect of the reception of Avicenna's ideas, but one not explored in the volume.

In view of the limited time span covered by the book, its title may strike one as somewhat overstated and perhaps better adapted to a multi-volume publication. At any rate, a chronological pointer inserted in the title would have been a welcome addition. Perhaps somewhat inadvertently, therefore, the volume raises some acute methodological and terminological questions that derive from the very subject that it tackles: How can we cogently and systematically study a phenomenon as broad and rich as the reception of Avicenna's philosophy in three distinct religious traditions? Should we not distinguish between different Avicennian or Avicennizing trends, that is, between various Avicennism*s*? Are general notions such as cosmology at all meaningful when applied to such varied endeavors and interpretations? More insight into these questions would have been welcome.

In spite of these minor shortcomings, the volume is an essential contribution to the history of Avicennian and post-Avicennian philosophy. It treads new ground, and there can be little doubt that the various avenues of research it opens will be thoroughly explored in the decades to come.

BOOKS RECEIVED 2019–2020

Troels Pank Arbøll. *Medicine in Ancient Assur: A Microhistorical Study of the Neo-Assyrian Healer Kiṣir-Aššur*. Ancient Magic and Divination 18. Leiden/Boston: Brill, 2020.

Markus Asper ed. *Thinking in Cases: Ancient Greek and Imperial Chinese Case Narratives*. Science, Technology, and Medicine in Ancient Cultures 11. Berlin/Boston: De Gryuter, 2019.

Joël Biard and Aurélien Robert edd. *La philosophie de Blaise de Parme. Physique, psychologie, éthique*. Micrologus Library 96. Florence: SISMEL – Edizioni del Galluzzo, 2019.

Jean-Patrice Boudet. *Astrologie et politique entre Moyen Âge et Renaissance*. Micrologus Library 102. Florence: SISMEL – Edizioni del Galluzzo, 2020.

Petros Bouras-Vallianatos and Barbara Zipser edd. *Brill's Companion to the Reception of Galen*. Brill's Companions to Classical Reception 17. Leiden/Boston: Brill, 2019.

Alan C. Bowen and Francesca Rochberg edd. *Hellenistic Astronomy: The Science in Its Contexts*. Brill's Companions in Classical Studies. Leiden/Boston: Brill, 2020.

Christián Carman and Rodolfo P. Buzón. *Aristarco de Samos. Acerca de los tamaños y las distancias del Sol y del la Luna. Estudio preliminar, revisión del texto griego y traducción al castellano*. Barcelona: University of Barcelona, 2020.

Giovanni Catapano and Onorato Grassi edd. *Rappresentazioni della natura nel Medioevo*. Micrologus Library 94. Florence: SISMEL – Edizioni del Galluzzo, 2019.

José Chabás. *Computational Astronomy in the Middle Ages: Sets of Astronomical Tables in Latin*. Madrid: Consejo Superior de Investigaciones Cientificas, 2019.

Annie Collognat and Bernadette Chopin. *Ex Machina. Robots et machines de l'Antiquité*. Paris: Les Belles Lettres, 2020.

Sten Ebbesen. *Anonymus Cantabrigiensis. Commentarium in Sophisticos Elenchos Aristotelis*. Scientia Danica. Series H: Humanistica 8.19. Copenhagen: Royal Danish Academy of Sciences and Letters, 2019.

Markham J. Geller and Strahil V. Panayotov edd. *Mesopotamian Eye Disease Texts: The Nineveh Treatise*. Die babylonisch-assyrische Medizin in Texten und Untersuchungen 10. Berlin/Boston: De Gruyter, 2020.

Pantelis Golitsis and Katerina Ierodiakonou edd. *Aristotle and His Commentators: Studies in Memory of Paraskevi Kotzia*. Commentaria in Aristotelem Graeca et Byzantina 7. Berlin/Boston: De Gruyter, 2019.

Dag Nikolaus Hasse and Amos Bertolucci edd. *The Arabic, Hebrew and Latin Reception of Avicenna's Physics and Cosmology*. Scientia Graeco–Arabica 23. Berlin/Boston: De Gruyter, 2018.

Stephan Heilen. *Konjunktionsprognostik in der Frühen Neuzeit*. Baden: Verlag Valentin Koerner, 2020.

Christoph Helmig ed. *World Soul – Anima Mundi: On the Origins and Fortunes of a Fundamental Idea*. Topics in Ancient Philosophy 8. Berlin/ Boston: De Gruyter, 2020.

G. O. Hutchinson. *Motion in Classical Literature*. Oxford: Oxford University Press, 2020.

David Juste, Benno van Dalen, Dag Nikolaus Hasse, and Charles Burnett edd. *Ptolemy's Science of the Stars in the Middle Ages*. Ptolemaeus Arabus et Latinus: Studies 1. Turnhout: Brepols Publishers, 2020.

P. Kalligas, C. Balla, E. Baziotopoulou-Valavani, and V. Karasmanis edd. *Plato's Academy: Its Workings and Its History*. Cambridge, UK: Cambridge University Press, 2020.

Paul T. Keyser and John Scarborough. *The Oxford Handbook of Science and Medicine in the Classical World*. New York: Oxford University Press, 2019.

Ekkehard Kopp. *Making up Numbers: A History of Invention in Mathematics*. Cambridge, UK: Open Book Publishers, 2020.

Stavros Lazaris ed. *A Companion to Byzantine Science*. Brill's Companions to the Byzantine World 6. Leiden/Boston: Brill, 2020.

David Lemler. *Création du monde et limites du langage. Sur l'art d'écrire des philosophes juifs médiévaux*. Études de Philosophie Médiévale 106. Paris: Librairie Philosophique J. Vrin, 2020.

Geoffrey Lloyd and Aparecida Vilaça edd. Special Issue: "Science in the Forest, Science in the Past". *Hau: Journal of Ethnographic Theory* 9.1. Chicago: Society for Ethnographic Theory, 2019.

Stefan M. Maul. *The Art of Divination in the Ancient Near East: Reading the Signs of Heaven and Earth*. Waco, TX: Baylor University Press, 2019.

Michiel Meeusen. *Ancient Greek Medicine in Questions and Answers: Diagnostics, Didactics, Dialectics*. Leiden/Boston: Brill, 2020.

Robert Middeke-Conlin. *The Making of a Scribe: Errors, Mistakes and Rounding Numbers in the Old Babylonian Kingdom of Larsa*. Why Sciences of the Ancient World Matter 4. New York: Springer Nature, 2020.

Nigel Nicholson and Nathan Selden edd. *The Rhetoric of Medicine*. Oxford: Oxford University Press, 2019.

Vivian Nutton. *Galen: A Thinking Doctor in Imperial Rome*. Routledge Ancient Biographies. London, UK/New York: Routlege, 2020.

Alessandro Palazzo and Anna Rodolfi edd. *Prophecy and Prophets in the Middle Ages*. Micrologus 103. Florence: SISMEL – Edizioni del Galluzzo, 2020.

Strahil V. Panayotov and Luděk Vacín edd. *Mesopotamian Medicine and Magic*. Ancient Magic and Divination 14. Leiden/Boston: Brill, 2018.

Mariassunta Picardi. *Il geroglificio della natura. Filosofia, scienza e magia in John Dee*. Micrologus Library 95. Florence: SISMEL – Edizioni del Galluzzo, 2019.

Christine Proust and John M. Steele edd. *Scholars and Scholarship in Late Babylonian Uruk*. Why Sciences of the Ancient World Matter 2. New York: Springer Nature, 2019.

Andrew Riggsby. *Mosaics of Knowledge*. Oxford: Oxford University Press, 2019.

Josefina Rodríguez-Arribas, Charles Burnet, Silke Ackermann, and Ryan Szpiech edd. *Astrolabes in Medieval Cultures*. Leiden/Boston: Brill, 2019.

Jacques Sesiano. *An Ancient Treatise on Magic Squares*. Boethius 72. Stuttgart: Franz Steiner Verlag, 2020.

Anthony Spalinger. *Feasts and Fights: Essays on Time in Ancient Egypt*. Yale Egyptological Studies 10. New Haven, CT: Yale Egyptological Institute, 2019.

Oleg Voskoboynikov. *Michael Scot. Liber particularis, Liber physonomie. Édition critique, introduction et notes*. Micrologus Library 93. Florence: SISMEL – Edizioni del Galluzzo, 2019.

William Wians ed. *Logoi and Muthoi: Further Essays in Greek Philosophy and Literature*. Albany, NY: SUNY Press, 2019.

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