AESTIMATIO

Critical Reviews in the History of Science

Aestimatio

Critical Reviews in the History of Science

AESTIMATIO

Critical Reviews in the History of Science

Volume 6

2009

Edited by
Alan C. Bowen and Tracey E. Rihll

A Publication of the
Institute for Research in
Classical Philosophy and Science
Princeton, New Jersey

Co-Published by

gorgias press

Co-Published by Gorgias Press LLC
180 Centennial Avenue
Piscataway, NJ 08854
USA
Internet: www.gorgiaspress.com
Email: helpdesk@gorgiaspress.com

ISBN 978–1–61143–555–9

This volume is printed on acid-free paper that meets the American National Standard for Permanence of paper for Printed Library Materials.

Printed in the United States of America

Aestimatio: Critical Reviews in the History of Science is distributed electronically free of charge at http://www.IRCPS.org/publications/aestimatio/aestimatio.htm.

To receive automatic notices of reviews by email as they are posted on the Institute website, readers are invited to subscribe to *Aestimatio-L* by sending an email message to majordomo@list.library.utoronto.ca containing in the body of the message the single command line ‘subscribe Aestimatio-L’. Alternatively, they may subscribe to the RSS feed on the Recent Reviews page (http://www.ircps.org/publications/aestimatio/rreviews.htm).

Though many of the reviews published in *Aestimatio* are solicited by special invitation, readers are welcome to volunteer to write a review by sending an email message to the Editor (bowen@IRCPS.org) that lists the title and author of the book they wish to review and gives a brief indication of their qualifications to undertake this review.

Contents

Preface

Aestimatio is founded on the premise that the finest reward for research and publication is constructive criticism from expert readers committed to the same enterprise. It therefore aims to provide timely assessments of books published in the history of what was called science from antiquity up to the early modern period in cultures ranging from Spain to India, and from Africa to northern Europe. By allowing reviewers the opportunity to address critically and fully both the results of recent research in the history of science and how these results are obtained, *Aestimatio* proposes to advance the study of pre-modern science and to support those who undertake this study.

This publication, which was originally intended to exist primarily online has grown nicely; and, while it will remain available online free of charge, it is now available in print as well from Gorgias Press. In addition, it is distributed electronically by EBSCO and registered in both the Directory of Open Access Journals and the Standard Periodical Directory.

Alan C. Bowen

Tracey E. Rihll

On Philosophy and the Sciences in Antiquity

Andrew Barker

andrewqbarker@hotmail.com

Robert Sharples' *Philosophy and the Sciences in Antiquity*[1] collects the papers delivered at a colloquium at University College London in 2003. No matter how precisely the organizer defines the subject to which such a colloquium is dedicated, the collected papers that emerge from it rarely add up to a unified whole; contributors go their own ways, sometimes with scarcely a nod to the theme that was intended to unify their efforts. The title 'Philosophy and the Sciences in Antiquity' is enormously capacious, and in itself points to no integrated set of questions and no one line of enquiry, so that readers looking for a cohesive treatment of a single theme may well come to it—in the words of Sydney Smith—'with no very lively hope of success'.

In fact, however, the agenda set for the authors of these papers was more precisely outlined than the book's title suggests. The editor, Robert Sharples, explains it as follows:

> The aim of the present volume, and of the colloquium from which it took its origin, is to examine the relation between philosophy and the individual sciences from the perspective of the ancients themselves, in so far as this is possible. How did they understand this relation, and how did they make use of it in argument and debate? Considering this will also throw light on the process by which, historically, specialist areas of study of the natural world—'sciences'—became detached from philosophy and obtained an autonomy of their own. It may indeed. . . be more accurate to describe the process as one by which philosophy itself came to have a more clearly defined agenda. [3]

[1] R. W. Sharples, ed. *Philosophy and the Sciences in Antiquity.* Keeling Series in Ancient Philosophy. Burlington, VT/Aldershot, UK: Ashgate 2005. Pp. vii + 168. ISBN 0–7546–5171–1. Cloth $89.85.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 1–34

A quibbler might object that several contributors to the volume have allowed themselves some latitude in interpreting this specification of the project, by focusing on relations between particular sciences rather than on those between the sciences and philosophy. But the objection can be dismissed. Negotiations between one scientific discipline and another are part of the process by which they 'obtained an autonomy of their own'; and in any case, the collection attains a surprising level of thematic coherence. Each writer pursues a different set of issues; and the essays address a very diverse selection of sciences, differently conceived varieties of philosophy, and chronologically widespread phases of the disciplines' development. No clear and coherent set of conclusions emerges from their reflections, unless, perhaps, it is to do with the powerful and progressively increasing influence of mathematics both on philosophy and on the natural sciences. But on the basis of the work done in these papers, one can build up an enlightening picture of the intricate network of interactions that took place between the various disciplines, the ways in which barriers were erected between them and broken down, the raids made by one specialism on the territory of another, the claims made by some to authority over others, and, over the centuries, the gradual molding and remolding of the profiles of each of them, especially the most elusive and Protean of them all, philosophy.

Of the seven papers included in the volume (together with Sharples' admirable introduction), the most wide-ranging is the first, an essay by André Laks entitled 'Remarks on the Differentiation of Early Greek Philosophy' [8–22]. Laks first draws attention to an ongoing debate about whether, and if so to what extent, 'pure' science and 'pure' philosophy were treated as distinct specialisms in the sixth and fifth centuries BC, and studied only (or mainly, or often) by dedicated 'professional' specialists [8–9]. He cites Zhmud [1994] as a champion of the view that 'the specialization of science and philosophy happens in Greece astonishingly early', and Lloyd [2002] as one of its notable opponents; but he notes, correctly, that the issue between the two sides is complex and slippery, and that they may not in fact be as radically divergent as they appear at first sight. Some of the teasing ambiguities and complexities are well brought out in later parts of the paper.

Among the difficulties facing anyone looking for a definitive solution to the problem is the fluidity of the boundaries between disciplines. Laks argues[10], if I understand him rightly, that we shall get nowhere if we treat a situation in which these borderlines are indeterminate as flatly incompatible with one in which distinct specialisms are recognized and their autonomy is proclaimed, and if we then try to decide which situation existed in the time of the Presocratics. Worthwhile results are more likely to be achieved if we concentrate instead on the dynamics of the process by which the various disciplines gradually and in different degrees acquired distinct identities, even though the boundaries they assign to themselves may continue to be contested and the territory they claim repeatedly invaded by others. He notes also, in this context, that philosophy is a special case. Though sciences such as medicine and mathematics change dramatically over the course of time, they are still recognizably concerned with the same subject matters. Philosophy, by contrast, had to be invented from the ground up; earlier and later versions of it are not simply different approaches to the same subjects. The question 'What is philosophy and what is it about?' is vigorously alive to this day; and it would be rash to assume that when fifth-century Greeks referred to φιλοσοφία and drew distinctions between it and other intellectual enterprises, they invariably had the same conception of it or its subject matter in mind.

Laks asserts the right 'to describe as "philosophical" some brand of intellectual activity that antedates the appearance of the word itself' [11]. But as a way of identifying the kinds of activity that were regarded in this light, he next considers three of the earliest passages in which the term φιλοσοφία itself appears: chapter 20 of the Hippocratic *On Ancient Medicine*, Gorgias' *Encomium of Helen* 13, and Plato's *Euthydemus* 305c (which is included as a chronologically relevant allusion because its remarks about philosophy are attributed to the sophist Prodicus) [11–15]. In each of these passages, philosophy is pointedly distinguished from some other discipline or disciplines; but on what basis? In the *Euthydemus*, the answer seems fairly straightforward. The distinction is between the theoretical activities of the philosopher and the practical activities of the politician: the former aims at understanding; the latter, at appropriate action. Gorgias' view is more elusive: he contrasts the λόγοι of the 'meteorologists' (whom Laks plausibly identifies with students of nature) and the contests (ἀγῶνες) of judicial arguments with the argumentative competitions (ἅμιλλαι) of philosophers. Competition is evidently involved in both the second and the third, and as Laks says, it is 'certainly not incompatible with meteorology'; and he suggests that philosophy is really being distinguished by the openness of its subject matter, '*any* subject which might point to semantic and logical problems' [13].

This conclusion seems at best uncertain, and I am still less convinced by Laks's reading of the passage from *On Ancient Medicine*. He interprets its contrast between philosophy and medicine as essentially a distinction between the theoretical and the practical; medicine, unlike philosophy, aims at action, 'changing the state of the world' [12]; hence, it corresponds closely to the distinction drawn in the *Euthydemus*. But that is not what the writer says, or not in this passage. The thesis that he disputes is ὡς οὐκ ἔνι δυνατὸν ἰητρικὴν εἰδέναι ὅστις μὴ οἶδεν ὅ τί ἐστιν ἄνθρωπος ('that it is not possible to know medicine if one does not know what man is'). What proponents of this view are talking about, he says, is philosophy, and he states his contrary opinion as follows: νομίζω δὲ περὶ φύσιος γνῶναί τι σαφὲς οὐδαμόθεν ἄλλοθεν εἶναι ἢ ἐξ ἰητρικῆς ('I think that in order to have some precise knowledge of nature, there is no other source than medicine'). This opinion, as it seems to me, could only be plausible if the 'nature' to which it refers is, or includes, that of a human being, and in that case the principal goal being attributed to both philosophy and medicine is the same, knowledge of 'what man is'. Whatever one might conclude from other parts of the treatise, the distinction made here between the two disciplines is not between their subject matters or their aims, or between the theoretical and the practical; it is to do with their methods, their approach to the issues that concern them both. According to the philosophers, as I understand them, one must know what man is *before* one can become expert in medicine; according to the writer, knowledge of the nature of man can only emerge from the study of medicine itself.[2] Laks interprets the writer's comments on Empedocles and the enigmatic allusion to γραφική (painting), which intervene between the two statements, in a way designed to support his claim to detect a contrast between the theoretical and the practical. It would take too long to investigate his interpretations here, and I shall say only that I find them unpersuasive.

I have devoted a disproportionate amount of space to these opening pages of Laks's paper, despite my resistance to some of his views, because they provide a useful introduction to the problems discussed in the remainder of the volume. It is natural to assume that any attempt to examine the relations between the various disciplines must begin by identifying them unambiguously and distinguishing one from another. Whatever else may be said about Laks's remarks, they and the texts he cites bring out very clearly the difficulties into which this assumption will lead us, and why in the case of the early period at least, an approach presupposing that each nameable discipline has, as it were, a definable 'nature' or 'essence' is sure to fail. If our purpose is to reach a view of these intellectual activities 'from the perspective of the ancients themselves', as Sharples puts it, we must focus as steadily as possible on the ways in which the Greek writers actually represent the relevant distinctions, however nebulous or perverse they may seem. It would be a serious error to elide their inconsistencies or apparent eccentricities, to force precision on distinctions that they leave vague or to give sharp outlines to notions which in their hands are malleable and amorphous, or to allow our interpretations to be colored by our own conceptions of the disciplines and their boundaries—or by those of Greek writers working in different periods from the one with which we are currently concerned. It is to the credit of the scholars represented in this collection that they rarely succumb to any of these seductive temptations, or not without explaining what they are doing and why.

---

[2] This is not far from the position which Laks later attributes to Diogenes of Apollonia, though they are arguably not identical:

> One could say that in Diogenes' case, doing philosophy implies doing medicine, in the same sense that in Aristotle's case, doing first philosophy implies doing astronomy—up to a certain point, perhaps. [18]

The distinctions that Laks goes on to draw between 'professionalization', 'specialization' and 'differentiation' [15–18] are ones that anyone working in this area would do well to bear in mind; they will help to keep a number of potential confusions at bay. 'Professionalisation', in his usage, presupposes an institutional framework of some sort, a 'school', and is unlikely to be relevant to the context of the sixth and fifth centuries; it also implies a substantial degree of 'dedication to a single activity'. A 'specialist' is someone who claims a particular field of competence. It need not be his only one (Empedocles and Diogenes, for instance, would probably have claimed several), and it implies no institutional affiliations. Both professionalisation and specialization are categories that apply to persons; 'differentiation', in Laks's sense of the word, is not. It applies to disciplines or fields of competence, and these may be differentiated even when there are no people who are specialists in them. When there are differentiated fields the same person may be a specialist (or we might say, an 'expert') in several, a qualification emphatically claimed for himself by the sophist Hippias, for example.

Laks points out also that fields of competence which are externally differentiated from others may also be internally differentiated into various sub-disciplines (whose status in the field and whose borderlines may themselves be subjects of dispute). The relations between the sub-disciplines and the larger field become especially complex and elusive in the case of philosophy, since some of the disciplines which an exponent asserts or implies that it contains may be ones which other writers, or the same writer elsewhere, represent as distinct from philosophy. Thus, philosophy may at one level be externally differentiated from medicine, and yet it seems that in the view of Diogenes of Apollonia doing medicine is an essential part of a philosopher's business.

Hence, the dichotomy between external and internal differentiation is less straightforward than one might suppose; and it becomes even more complex when we turn to the work of fourth-century philosophers, especially Aristotle. At the end of his paper [18–21], Laks tries to shed some light on the ways in which science and philosophy are differentiated in Aristotle's writings and elsewhere by considering the relation in which the two of them are said to stand to a third category, myth. He finds in a passage of Epicurus the implication that 'myth is not a genre, but a function', which he describes as 'extremely appealing'; and he suggests that the same may be true of science and philosophy. He amplifies these rather gnomic remarks by characterizing 'science' and 'philosophy' as 'alternate and legitimate descriptions of the same activity, depending on how you construe science and philosophy' [19]. This seems entirely unobjectionable. But except in so far as it sounds a renewed warning against the allure of fixed and determinate categories into one or another of which each discipline unambiguously falls, I do not see that it greatly advances the discussion of these troublesome issues.

In his opening paragraph, R. J. Hankinson sketches the problem he addresses in his paper, 'Aristotle on Kind-Crossing' [23–54].

> In a number of places, Aristotle seems to state unequivocally that no science can make use of the principles of any other science in its demonstrations. Elsewhere, however, he seems not only to countenance such borrowings, but on occasion to make them an essential feature of the construction of scientific explanations. And since science is, for Aristotle, fundamentally an explanatory exercise, this is a particularly uncomfortable position to be in. In this paper I seek to offer an interpretation of Aristotle's views on the issues that tries at least to minimize the tensions involved. [23]

Here, then, we are not concerned with the relations between the sciences and philosophy but with those between the individual sciences, and specifically with the ways—or the senses—in which they can and cannot draw on one another's principles when performing their explanatory tasks. The problem that Hankinson identifies arises mainly from passages in the *Posterior Analytics*. It is notoriously troublesome and has often been tackled before,[3] but no consensus about its solution has been reached.

I shall say little about the first two sections of the paper [23–43], valuable though they are. They give a very clear account of Aristotle's general theory of scientific demonstration (ἀπόδειξις), and a meticulous analysis of the passages from which the difficulties arise.

[3] See for instance Lennox 1986, McKirahan 1992, and the commentary in Barnes 1994, all of which are cited, with many others, in Hankinson's bibliography; one might now add Barker 2007, 353–361.

One important point which Hankinson brings out [esp. 38–40] is that the cases in which a science seems to borrow principles from outside its own domain are of two sorts. In one kind of case, science A is subordinate to science B and draws on some of B's principles in constructing its demonstrations; harmonics, for instance, is subordinate to arithmetic in this way, and optics to geometry [e.g., *Post. an.* 75b16, 76a10, 24, 78b37–8]. In the other, demonstrations in several sciences which do not fall into such a hierarchy make use of more general principles which are specific to neither of them, as geometry and arithmetic both make use of the axiom of equality (particularly 76a37–b2). In their different ways, both kinds of relation seem to fly in the face of Aristotle's repeated insistence that all the principles used in a scientific ἀπόδειξις must be proper and peculiar to the domain of the science in question.

At the beginning of the third part of his paper [43–47], Hankinson sums up the situation as he has analyzed it:

> The principles of any science will be proper to that science. They will consist in part of I_1 predications[4] which are by definition... proper to it; and if they also make use of existence assumptions... those assumptions too will be tied to the domain in question (there is no room in anthropology for propositions like 'there are frogs'). Hence, the sciences ought to be (and Aristotle argues that they are) hermetically-sealed; and there will be no kind-crossing.

Yet somehow there can be, at least 'in a way', as is indicated at *Post. an.* 75b8–11:

> The domain must either be the same without qualification, or at least in a way, if the demonstration is going to cross; and it is clear that it is impossible in any other way, since the extreme and the middle terms must be from the same domain.

[4] Hankinson [27] adopts the label 'I_1 predication' from Barnes [1994, 112–14]; 'I' abbreviates 'in itself'. Barnes, following Philoponus, formalizes it as follows: in an I_1 predication, 'A holds of B in itself =df. A holds of B and A inheres in the definition of B'. This is distinguished from an I_2 predication (in which A holds of B and B inheres in the definition of A), but as Hankinson says, 'it is the I_1 cases which are the more important'.

Hankinson points out that the extreme and middle terms include *all* the terms figuring in the demonstrative syllogisms, and that if they are not all from the same domain there will be no scientific explanation. 'Now', he continues,

> if the domain is 'the same without qualification', then there are no kinds to cross; but there may be if it is the same 'in a way'. What might that mean? [43]

This, as it seems to me, is exactly the right question to ask, since it keeps in view a point that might easily be missed. Even when kind-crossing is in play in a legitimate demonstration, all the terms employed will still in some way belong to the same domain, even though in some other way they do not. In no case are we faced with a successful demonstration using terms which belong without qualification to different domains. The challenge is to work out what the relevant qualifications could be.

In moving towards his solution to the problem, Hankinson focuses mainly on one of the two types of case, that in which several sciences which are not subordinate to one another draw on principles which—in one perspective at least—are peculiar and proper to none of them.

> Of the things which are used in the demonstrative sciences, some are proper to each science while others are common; but common in virtue of analogy, since they are useful only insofar as they belong to the domain which falls under the science. [*Post. an.* 76a37–40]

Hankinson explicates this through an example.

> In other words, if I make use of the equals axiom in an arithmetic proof, I make use of it in its arithmetic form
>
> (EA) Equal numbers subtracted from equal numbers leave equal numbers,
>
> rather than in that of its Euclidean generalization
>
> (EG) Equals subtracted from equals leave equals. [45]

He next considers and rejects the objection that in such a case EA will lack a feature that is essential to the premises of a demonstration; 'EA is not primitive and immediate, since it can be shown to be a consequence of EG'. He argues that even if EA can be derived from EG, this cannot be done within the science of arithmetic itself. Suppose that the first premise of such a derivation runs like this (Hankinson apologizes for the cumbersome formulation):

> being such that, when equal and when subtracted from equals, equals remain, belongs to all magnitudes,

and that the second is:

> magnitude belongs to all numbers.

From these we can indeed infer something equivalent to EA. But the first premise is clearly not proper and peculiar to arithmetic, and within the restricted domain of that science alone EA cannot be demonstrated, or 'thickened' by the insertion of a middle term. If there is a science to which the first premise belongs it is the more general science of quantity or magnitude, and it will hold *per se* in that science, not in arithmetic as such. Hence, within arithmetic, EA is underived, primitive, and immediate; and when various different sciences use their own versions of EG, the principles that they use are not identical but are related by way of analogy, as Aristotle says; 'their domains are different, but certain separate facts about the separate domains are structurally isomorphic with one another' [46].[5]

This leads Hankinson to an important conclusion:

> Whenever anyone derives EA as a special case of EG, he does so not as an arithmetician, but as a quantity-theorist—and similar strictures hold when one employs geometrical reasoning in optics or mechanics, or arithmetical reasoning in harmonics. Moreover, this analysis has the further advantage of minimizing the distance between the subordinate cases [i.e. ones such as those just mentioned] and those involving co-ordinate science (such as arithmetic and geometry in the case of proportional alternation): for the latter can now be seen to be a complex type of the former—there is a superordinate

[5] Here Hankinson notes the prominent use of 'analogy' in Aristotle's biological works, saying that the sense in which 'analogy' is employed there is the same as the one involved in *Post. an.* 76a. If he is right about that (he does not argue the point), a thorough exploration of the biological analogies might help to clarify further his thesis about the relation between scientific domains and perhaps to give it additional support.

> science, quantity-theory, which provides the explanation as to why the interpreted principles hold in two subordinate sciences. [47]

As he states it, this conclusion seems too weak; so far as I can see it is not just that the distance between the two types has been minimized but that there is now no significant difference between them at all. In every case, a subordinate science makes use of a principle proper to a higher science, but uses it only in a form restricted to the contents of its own domain; and in every case the 'higher' science is also more general, and can be used in parallel ('analogous') ways by several sciences of more restricted scope.[6]

The fourth and final part of Hankinson's paper [47–52] addresses a problem which his interpretation must face, as indeed must any other; it is set in front of us most directly (though not only) by *Post. an.* 78b35–79a6. Here Aristotle asserts, among other things, that in at least some cases where one science is subordinate to another, it is the task of the lower science to present the facts, and for the higher to provide the explanations. Since the higher science (e.g., geometry) makes no reference to the factual data that are the special province of the lower (e.g., optics), it cannot explain them by itself; and the lower science, it now appears, can provide no explanations at all. But there must be something wrong with this *scenario*, since Aristotle regularly insists that all the sciences as such are in the business of explaining their data. Hankinson, therefore, follows Ross in his contention that the subordinate discipline, which is merely a collection of empirical data, 'is only by courtesy called a science' [48–49: cf. Ross 1949, 555]; to put it more bluntly, it is not really a science at all.

This seems an unhappy conclusion, not least because in passages where Aristotle mentions such disciplines he seems to have no qualms about representing them as sciences (ἐπιστῆμαι) without qualification. In the passage we are discussing, for instance, he tells us that the reason (τὸ διότι) differs from the fact (το ὅτι) in that—in the cases under consideration—'each of them is studied by way of a different science' (τῷ δι' ἄλλης ἐπιστήμης ἑκάτερον θεωρεῖν) [78b34–

[6] This will remain true even if scientists actually recognize only one science subordinate to it; since its domain is wider than that of the lower science, it is logically bound to make room for at least one other subordinate, even if in fact none is practised.

5]; and it would seem, at least on the face of it, that on Ross's view, every one of the disciplines which Aristotle treats as 'subordinate' in this sense must be expelled from the catalogue of the sciences. We should surely hesitate before consenting on Aristotle's behalf to so draconian a purge. Might it be the case, for instance, that though there are facts falling within the subordinate science's domain which it cannot explain without help from above, there are some such facts which it is capable of explaining unaided, and that its explanatory power in those cases is enough to preserve its scientific credentials?

This suggestion leads to fairly obvious difficulties of its own. I shall not pursue them or argue in favour of the hypothesis here; but perhaps the problems it encounters are no more vexing than those that Ross's involves; and I mention it only by way of an indication that when we are looking for a way of understanding the status of the subordinate disciplines, there might be alternatives to the strategy that he proposes. Now while he accepts Ross's view about this, Hankinson also adds a new twist. He notes that

> Aristotle appears to countenance a three-stage hierarchy of at least some of the sciences: just as optics stands to geometry, so 'the study of the rainbow' stands to optics in general; and similar hierarchies seem to be constructible for harmonics and possibly also for astronomy. [48][7]

As an example, he suggests an analysis of an explanation of the phase-structure of the Moon.

[7] This contention might be challenged in the case of harmonics. In the relevant passage [*Post. an.* 78b35–79a2], Aristotle says first that harmonics is subordinate to arithmetic, and then that 'harmonics based on hearing' (i.e. empirical harmonics) is subordinate to mathematical harmonics. This may seem to encourage Hankinson's postulation of a three-part hierarchy, and it would perhaps be quibbling to object that Aristotle does not specify that it is *mathematical* harmonics which is directly subordinate to arithmetic. The real difficulty is that he presents the second statement as specifying one of several cases in which 'some of these sciences' (ἔνιαι τούτων τῶν ἐπιστημῶν, that is, some of the pairs of sciences that have just been mentioned) have 'almost the same names'. This seems to imply that in the present context the references to arithmetic and harmonics on the one hand and to mathematical harmonics and harmonics based on hearing on the other are alternative designations of the same pairs of sciences; and in that case the three-part hierarchy evaporates.

> The first stage is simply to observe and record its regularity (this would belong to observational astronomy). Next comes the hypothesis that the moon is spherical, which, along with other facts... will account for the appearances. That hypothesis (since it concerns *the moon*) is proper to astronomy, but to mathematical rather than observational astronomy—and it explains the fact in the familiar manner. But it does so by applying a perfectly general theorem of geometry.... [49]

We are to understand, of course, that it does not apply it in its full generality, but in a way restricted to the astronomical domain; in its restricted form it is related to the general theorem in the same way that the proposition labelled EA above is related to EG.

Thus, mathematical harmonics is 'intermediate between observational astronomy and pure geometry'. That seems a reasonable conclusion and the overall picture seems to capture much of what Aristotle says, though it will still leave observational astronomy, harmonics, and so on in a non-scientific limbo. But there is perhaps a residual problem about the complex 'intermediate' science. Hankinson points out that Aristotle is right in saying that an exponent of the highest of the three disciplines need not be aware of any of the facts stated by its subordinates, since its propositions are set in a generalized form which refers to none of them. But it is not clear that the same can be true of mathematical astronomy in relation to its subordinate, observational astronomy. The mathematical astronomer's demonstrations must refer to the facts to be explained, that is, to the facts collected by his observational counterpart; and if the demonstrations mention and explain these facts, the demonstrator must surely 'know' them, as indeed Hankinson implicitly concedes: 'Unlike the observational astronomer, who... merely knows the facts, the mathematical astronomer understands them' [49]. If he understands them, I presume, he must know them. Yet just after mentioning the two kinds of astronomy, Aristotle has said that in such cases

> it is for the observational sciences to know the facts and for the mathematical ones to know the reason why; for the latter possess the demonstrations of the reasons why, while they are often unaware of the fact. [*Post. an.* 79a2–4]

I do not know how this difficulty should be resolved. In much of the remainder of Hankinson's paper, he focuses on a rather strange example of kind-crossing in which an explanation appeals to propositions of two sciences, medicine and geometry, neither of which, so Aristotle asserts, is subordinate to the other, prefacing his assertion with the remark that there are many cases of this sort [*Post. an.* 79a13–16]. Hankinson shows, convincingly, I think, that such cases can be acquitted of breaching Aristotle's rules in much the same way as the others.

> The argument supplying the reason for the fact will have geometrical content; but in exactly the same way as in the cases of the genuine subordinate sciences such as optics, that content will be formal only; the material will be specified by the domain, and the formal principle... particularized to medicine. [51]

If the suggestions he makes about the subordinated sciences are acceptable, there seems no good reason why they should not be stretched to accommodate these cases too.

James Lennox begins his paper, 'The Place of Zoology in Aristotle's Natural Philosophy' [55–71], with a barrage of questions. How are Aristotle's books about animals related to one another? How are they situated within his natural philosophy? Does he think of them as a unified investigation, and if so how should we conceive this unity? How are all these studies related to his other investigations of nature? Should various other writings, the *De anima* and the *Parva naturalia*, for instance, be included among his works on animals? Here, then, we move from examinations of the relations holding between philosophy and the sciences in general to questions directed to a single, though at this stage uncertainly defined group of writings by a single author; and the spotlight also shifts away from Aristotle's logically motivated rules governing relations between the principles and demonstrations of different sciences (though it will return to them in the paper's closing pages), to the broader conceptual relations and forms of classification which emerge, less directly, from his actual investigations in one scientific domain.

Since Aristotle nowhere addresses these questions head-on, Lennox proceeds on the basis of clues given in passages of certain particular types. Passages of the first type that he discusses [56–59] are those which give cross-references from one work to another within the corpus concerned with the study of animals. He dismisses the common notion that these can provide a guide to the works' chronology; what he is looking for is 'a better understanding of the internal conceptual and methodological relationships' that Aristotle envisages between his various zoological enterprises [56]. In practice, he discusses in detail only two passages at this stage, both from the *De partibus animalium* [640a10–22, 689a4–20]. These passages refer us to studies undertaken in the *De generatione animalium*; and Lennox' first important point is that all 10 such references in *De part. an.* look *forward* from that work to the other, suggesting that *De part. an.* is in some sense prior to *De gen. an.*, though this entails nothing about the order in which the studies were carried out or the treatises written.

The reason why *De part. an.* is prior to *De gen. an.*, Lennox argues, is that 'the study of generation must be posterior to the study of that which is to be generated—*generation is for the sake of being*; being is the cause, coming into being the effect. . . '; and this 'reflects Aristotle's peculiarly teleological understanding of the methodological/conceptual structure of animal inquiry' [57]. This represents faithfully what Aristotle says in the passage at 640a, which ends with his well-known criticism of Empedocles for reversing the true order of priority.[8] But the relation seems to become more complex in the passage at 689a. Here Aristotle twice explains features of animal anatomy by reference to facts which, he says, we must assume here and prove later, and which are elaborately established in *De gen. an.* As Lennox says, these allusions clearly point forward from *De part. an.* to *De gen. an.*, like the remarks at 640a; but he says nothing here about their implications concerning the conceptual relations between the two enterprises. Readers may be left wondering how explanations in *De part. an.*, which is concerned with the final form, can depend on demonstrations that will be provided in *De gen. an.*, whose business is with the processes whereby that form is reached, if indeed 'being is the cause, coming into being the effect'. At a superficial

[8] Empedocles' error is exemplified by his explanation of the segmented character of the backbone: it gets broken by being twisted during the process of generation. Features of the generative process are thus used to account for the animal's final form, whereas in fact, according to Aristotle, it is the final form to be achieved that explains this process' characteristics and the course it takes.

glance, reliance on facts to do with generation to explain facts about the fully-formed animal's structure might seem to involve the Empedoclean error. Some of Lennox' later remarks, however, may help to clarify the issue (see further below).

He draws an important conclusion from the closing lines of the passage. Aristotle refers there to two other investigations relevant to the subject of generation in addition to those contained in *De gen. an.*: the 'enquiry about animals' (i.e., the studies recorded in the *Historia animalium*) and the 'dissections' (the topic of a work that is lost). These two 'will make apparent (i) the arrangement of the internal organs connected with generation and (ii) their differences from one group of organisms to another'. These facts will then be reported (not 'explained') in *De gen. an.*, 'which will then define the male and female contributions to generation and demonstrate their properties'. The task remaining for the investigations recorded in *De part. an.* is to 'show that the configuration of these parts is determined in relation to their activities' (or as Aristotle puts it, 'the configuration of these parts is necessarily for their activity' [*De part. an.* 689a19–20]). Hence, Lennox concludes, 'each of the four works makes a distinct contribution to an understanding of the reproductive organs of blooded organisms' [58].

In elaborating this conclusion, he points out that the 'histories' and 'dissections' are not referred to as sources of explanation, but in effect as databases organized with a view to their purposes within a larger scientific framework.[9] By contrast, as we have seen, *De part. an.* is clearly in the business of explanation: it explains teleologically facts recorded in those databases, and also on occasion takes for granted facts to be proved in *De gen. an.*, even though that work 'is to be studied after *De part. an.*' [58–59]. Perhaps Lennox would say, then, that the status of these facts is closely comparable to that of those taken from the databases, and that the difficulty raised by this passage's earlier cross-references from *De part. an.* to *De gen. an.* is only apparent. The former's reliance on facts that 'will be proved' (δειχθήσεται) in the latter does not undermine *De part. an.*'s methodological and conceptual priority, since it still has the role of providing the fundamental explanations, even for facts 'proved' in

[9] Gotthelf 1988 and Lennox 2001 make important contributions to an understanding of their mode of organization.

De gen. an. In the relevant passage of *De part. an.*, they are used to account for other anatomical facts and in that sense they are relied on; but they do not account for those facts teleologically and, therefore, do not explain them in the required sense. Similarly, the proofs in *De gen. an.* do not amount by themselves to adequate explanations, and none of these facts will have been properly explained until we have understood why they are necessary for the animals' activity. In proposing this interpretation, however, I am (very tentatively) putting words into Lennox' mouth; it is a pity that he does not pursue the issue explicitly himself.

Lennox now asks readers to suppose that the ingredients of Aristotle's zoological investigation are indeed so integrated as to form a 'distinct scientific domain', and moves to his next question: how are we to conceive the relation in which it stands to other enquiries into nature? This introduces the longest and most fascinating section of his paper [59–65], based on clues provided by a second group of texts, 'those in which an investigation of animals is explicitly discussed, whether on its own or in comparison with other investigations' [60]. He focuses initially on passages from *Meteorology* 1.1 [339a5–9] and from *De part. an.* 1.1 and 1.4 [639a12–15, 644b16–20]. The first of these looks ahead to studies of animals and plants for which, Aristotle tentatively proposes, the work of the *Meteorology* may have prepared us: 'For having given an account of these things, we may perhaps have reached the goal we set before ourselves at the outset'. The second asserts that the purpose of *De part. an.* is to set certain standards (ὅροι) for the enquiry into nature (τῆς περὶ φύσιν ἱστορίας), 'such that by referring to them one can appraise the manner of its proofs'. The key point in the third is that Aristotle now claims to have said how the 'systematic study of nature' (τῆς περὶ φύσιν μεθόδου) should be judged, and that neither here nor in the previous passage does he limit the scope of his assertion to the study of animals.

> Concretely, it is clear that Aristotle's focus is entirely on animals; but it is the investigation of *nature* for which he claims to be providing standards of judgement. [61]

How, then, could Aristotle justify his apparent thesis that it is the study of this special class of natural beings, those that are alive, which can set standards for the study of nature as a whole? Lennox proposes [62] that his viewpoint is similar to one memorably enunciated by G. G. Simpson, in a passage which struck me very forcefully when I first read it some 40 years ago.

> I suggest that both the characterization of science as a whole and the unification of the various sciences can most meaningfully be sought... not through principles that apply to all phenomena but through phenomena to which all principles apply.... I have, I believe, sufficiently indicated what those latter phenomena are: they are the phenomena of life. Biology, then, is the science that stands at the center of all science. [Simpson 1964, 107]

As applied to Aristotle, the point is that the study of living things must depend both on the methodological 'standards' or principles that apply to the study of nature in general and all other domains within it, and on principles peculiar to itself. This is true of no other field of enquiry. Hence, 'only a fully articulated *zoological* method will provide us with a complete set of standards for natural science' [62]. This, as it seems to me, is a very appealing interpretation of the Aristotelian evidence; and at the same time, as Simpson believed, a worthwhile (though no doubt highly debatable) challenge to modern scientific orthodoxies.

In the remainder of this part of the paper [63–65], Lennox considers the relations between Aristotle's zoological studies, his work in the *Meteorology*, and his investigations in the *De anima* and the *Parva naturalia*. I shall not examine the details of his discussion, which revolves mainly around passages from *On Length and Shortness of Life* [467b5–9], from *De sensu* [436a1–4],[10] from *Meteor.* [389b23–8], and from *De part. an.* [390b15–22], in addition to some on which he has already drawn. Summarily, his conclusion is that

> Aristotle sees the study of animals as both continuous with certain other natural investigations and yet distinct in subject matter, methods and principles.

The *Meteorology* provides information about 'elemental compounds and their emergent dispositions and interactions' which is essential

[10] This passage, as Lennox notes, seems to pose 'insuperable problems' for the view he proposes; but he resolves them persuasively [63–64].

to the zoological works; but since it says nothing about these compounds' roles in living things or the functions or purposes of the animal parts formed from them, it is a 'prolegomenon' to the study of animals rather than a part of it. As to the *De anima* and its sequel, the *Parva naturalia*, which examine the nature and activities of the soul as such and the activities of the complex entity we might call 'ensouled body',

> *Parva Naturalia* is characterized both as dependent upon *De Anima* and, at least in part, a sort of capstone of the investigation of animals. [65]

This last statement seems distressingly vague, and the details of Lennox' discussion do little to dispel the obscurity; he returns briefly to the issue at the end of his paper.

The penultimate section of the paper [66–68] considers a question suggested by the relation between the *Meteorology* and the studies of animals. The former is not included among the latter, but it provides resources on which the latter draw. How, then, are we to understand 'this sharing of resources across disciplinary boundaries'? Lennox addresses the problem on the basis of three texts which refer to a relation between two other 'distinct but related disciplines', natural philosophy and medicine [*De sensu* 436a18–436b2; *De part. an.* 653a1–3, 8–10; *De respiratione* 480b22–31]. The general upshot of these passages is that it is the role of a natural philosopher to investigate, theoretically, the principles of disease and to provide the science of medicine with the results of this enquiry. It is important to notice that this task is not hived off to a putative discipline of 'theoretical medicine' [66]. It is an intrinsic part of the business of natural philosophy, which will

> study not only the causes of the proper functioning of the human organs, but also the causes of their malfunctioning or premature decay, and in general the causes of disease. These causes will be referred to and cited in medicine, but establishing these causes as the causes of disease is the task of the medically oriented *physikos*. [67]

These observations return us to the topic of Hankinson's paper, since Lennox suggests that the relation identified here between natural philosophy and medicine is much like the one posited in the

Posterior Analytics between geometry and optics or between arithmetic and harmonics. 'The physician studies the fact, the *physikos* supplies the reason why' [67–68].[11] Thus, there are connections as well as differences[12] between Aristotle's thoughts on the life-sciences and on the mathematical sciences to which the *Posterior Analytics* refers; but the conclusion Lennox draws about the relation between *Hist. an.* and the other zoological enquiries is different and perhaps more interesting. As he noted earlier, *Hist. an.* provides facts which 'causal investigations' such as *De part. an.* use and explain, and this makes the relation between them look identical with that between medicine and natural philosophy. But here the situation has changed. *Hist. an.*, like the other zoological works, is concerned specifically with animals; the *genos* with which each is concerned is the same, and Aristotle makes no suggestion that two distinct sciences are involved here, one of which supplies first principles for the other. No borrowings from different domains are involved. Hence, there is a strong sense in which, by the criteria of the *Posterior Analytics*, the investigations undertaken in *Hist. an.*, *De part. an.*, and *De gen. an.* all belong to the same science [68]. But does the same hold of the *De anima* and its sequel, the *Parva naturalia*? Lennox leaves this question hanging, noting only that at least one part of their enquiry is clearly outside the scope of natural science.

The next two papers, by Philip van der Eijk and by Geoffrey Lloyd, take medicine as their principal subject. Van der Eijk's essay, 'Between the Hippocratics and the Alexandrians: Medicine, Philosophy and Science in the Fourth Century BCE' [72–109],[13] focuses on a phase of Greek medicine to which scholars have typically paid less attention than to the Hippocratics and the Alexandrians; neither the relevant philosophical texts nor the fragmentary works of the strictly medical writers of this period have been well served.[14] On pages 73–78 he lists 35 known authors (and one compilation, the

[11] Lennox does not return to the relation between the *Meteorology* and the zoological works which prompted this part of his paper; but fairly clearly he would construe it in the same way.

[12] One of the most important differences is indicated in the concluding part of the paper [69].

[13] He includes a seven-page bibliography.

[14] Van der Eijk mentions various honorable exceptions to this scholarly neglect [72–73nn1–3].

Problemata attributed to Aristotle) and the titles of a large number of their works; and the list, as he says, is not exhaustive. Clearly there is no shortage of material to be studied, even if much of these authors' work survives only in fragments or indirect allusions, or is completely lost.

An important point that emerges from this survey is that the interests of 'philosophers' and 'medical' writers overlap to a considerable, but also a variable extent. Van der Eijk issues a warning that the similarities and differences between them 'have to be identified and assessed from one individual case to another', and that it is inadequate and sometimes misleading to distinguish them under such general headings as 'practical vs. theoretical', 'clinical vs. scientific', and so forth [78]. He notes also that neither 'philosophy' nor 'medicine' unambiguously identifies any one definable project, and that ancient authors themselves not only quite often treated certain 'philosophers' as authorities on medicine, but were also well aware that the contours assigned to this discipline were changeable and continually disputed [79–80].

After these introductory remarks, the first topic considered is the treatment of medicine and the life-sciences in the fourth-century Academy and Lyceum [80–83]—or so the title of this section promises, though van der Eijk's comments on Plato are restricted to a reference to recent work by Vegetti [1995] and Lloyd [2003, 142–175], and Plato's successors in the Academy are not mentioned at all. Nor does he discuss the writings of Peripatetics other than Aristotle himself. His central theme, in fact, is the extent, diversity, and importance of Aristotle's own work in the field of medicine and on matters closely related to it. It made or inspired major empirical discoveries; it created a theoretical framework for the study of the workings, failings, and reactions of the human body; it made valuable contributions to the methodology of medical investigation and to the repertoire of concepts used to systematize and communicate medical knowledge; and it laid the foundations for an understanding of the discipline's historical development.

Van der Eijk offers several considerations which might help to explain how it came to do all this. First, Aristotle's familiarity with earlier medical thought led him to acknowledge the extent to which doctors had contributed (and were still contributing) to the study of nature, and to comment frequently on their ideas. Then, there is the intimate connection, touched on in Lennox' paper, which Aristotle himself recognizes between medicine and natural science. Thirdly, van der Eijk contends, we should take seriously the fact that later writers credit Aristotle with several specifically medical works and a number of medical doctrines, arguing that there is no better reason to reject their authenticity than

> a tacit distinction between 'philosophy' and 'science' and the assumption that these writings were too 'specialized' and 'unphilosophical' for the mind of Aristotle.

After all, as he points out, there is plenty of 'specialized' or 'technical' matter in Aristotle's surviving treatises [82–83]. The section ends with a catalogue of the major benefits that would be gained if scholars studying later phases of medical history were to give full weight to the impact made on it by Aristotelian thought; conversely, a well-grounded understanding of Aristotle's medical work would help us to appreciate the ways in which later developments in medicine affected the interpretation of his writings in general, both inside and outside the Aristotelian tradition. All this is admirable, and points forward to research which, he tells us, is ongoing but still in its early stages. I can only add that it will not be complete until it also takes fully into account the work done in the Academy throughout the same periods of history.

In the remainder of his paper, van der Eijk brings into the center of the picture the people normally classified as fourth century 'medical writers' rather than 'philosophers'. After a general survey of relevant features of their work, he devotes separate sections to each of three individuals, Diocles, Praxagoras, and Mnesitheus, followed by more detailed discussions of their most significant contributions (and those of some others) to debates outside the strictly medical sphere. These include refinements of the procedures of classification and division; the development of a classificatory terminology; analyses of the distinctions, in pathology, between signs, symptoms, and causes; advances in empirical research; and negotiations between the claims of reason and experience as sources of knowledge. His comments on all these matters are solidly grounded in the surviving texts, several of which—including some of the most intriguing—will probably be unfamiliar to non-specialist readers.

I shall not go into the details. It is enough to say that van der Eijk makes a very compelling case for the thesis that a substantial amount of these doctors' work was devoted to issues well entrenched in the philosophers' agenda, particularly in epistemology. Whether they contributed original and worthwhile insights of their own in these areas is another question; the evidence presented here is not enough to underwrite a confident affirmative answer (except, perhaps, in so far as Diocles' delicate balancing-act between theoretical and empirical criteria of judgement can be viewed in that light); but it is certainly enough to encourage further research into the matter. Van der Eijk's paper as a whole, in fact, challenges students of ancient philosophy to push their investigations out into this underdeveloped territory. In doing so, I suggest, they should take proper account of a point that he makes [86] which may seem tangential, that these writers were in the business of expanding their field of operation not only into philosophy but into many other, less intellectually high-profile areas too, writing on topics as diverse as child-rearing, cookery, etiquette, flower-wreathing, and seafaring. Though their interest in philosophy no doubt had deeper roots, it is worth considering the extent to which their pronouncements upon it, like these apparently more trivial exercises, were motivated by their wish 'to have a finger in a large number of pies and to address a wider clientele', so 'disseminating their ideas more widely and having greater influence on society'.

G. E. R. Lloyd's paper [110–130] is entitled 'Mathematics as a Model of Method in Galen'. The problem that it addresses can best be brought out in his own words:

> On the one hand he [Galen] often expresses his admiration for the mathematicians' methods. They provide his star examples of the highest type of demonstration, 'epistemonic *apodeixis*', securing certainty. On the other hand to illustrate those methods Galen gives a bewildering array of examples that do not all by any means conform to the patterns set by Euclid in the *Elements*... The issue I wish to explore here is how clear a grasp of mathematical method does Galen have? Or rather, since we should look at the problem from his point of view, what counts as 'mathematical method' in Galen's eyes? The question derives its importance from the frequency with which mathematics is held up as an ideal: but

> quite what that ideal comprises is more difficult to pin down than is generally recognized. [100]

Lloyd begins [111–117] by examining passages in which Galen professes his allegiance to mathematical methods of proof,[15] sometimes explaining also why, in certain cases, he has not exploited them. One reason why he does not always provide such proofs, as he says in various passages of *On the Usefulness of Parts*, is that many of his potential readers cannot abide such things: he is inclined to avoid loading his writings with 'demonstrations requiring astronomy, geometry, music, or any other logical discipline, lest my books should be held in utter detestation by physicians' [*De usu part.* 10.14 in Helmreich 1907–1909, 3.837.9–12]. In other cases he admits his inability to find such a proof, either by questioning other philosophers or through his own efforts.[16] In all the relevant passages, however, Galen treats mathematical proof as the ideal to which scientists should aspire, repeatedly assigning this status, specifically, to proofs in geometry, and distinguishing such demonstrations sharply from arguments of a dialectical, rhetorical, or sophistic sort. The central points that Lloyd derives from these texts are that not all the arguments to which Galen approvingly refers are strictly geometrical, and—more importantly—that although those of Galen's own arguments to which he assigns the status of 'epistemonic *apodeixeis*' may be logically sound, there is nothing specifically mathematical about them.

Lloyd now looks more closely at details of Galen's comments on mathematical arguments, and of the specimens that he chooses to exemplify them, paying particular attention to the status of their premises. The texts which he cites first show clearly that Galen understood the requirement that scientific demonstrations, in medicine as elsewhere, must be based on premises that are indemonstrable, 'evident', and self-justifying, and more generally that he seems to have had 'an impressive grasp of axiomatic-deductive reasoning, proceeding from indemonstrable primary premises, via valid arguments, to

---

[15] A notable example is in ch. 11 of *On my own Books* [Helmreich 1884, 115.21–117.20], in which he recalls how his grasp on mathematics rescued him from the perils of scepticism.

[16] See ch. 6 of *On the Construction of the Fetus* [Kühn 1821–1823, 4.695.1–696.3].

incontrovertible conclusions' [117–118]. But there are troublesome complications. Sometimes, it turns out, Galen is prepared to attempt proofs of propositions that he has claimed as primary and evident, thus falling foul of Aristotle's carefully argued contention that the primary premises of a demonstration must be incapable of proof [*Post. an.* 72b5ff.]. Elsewhere, his illustrations of mathematical method frequently involve observational premises, which of course cannot be evident and self-justifying in the same way as mathematical axioms, and in some cases are not even evident 'in the way that Galen claims that what is actually hot is evident to the sense of touch' [121].[17] Various other peculiarities arise too, mainly from the mixture of mathematical and empirically based propositions in the supposedly exemplary demonstrations. As Lloyd points out,

> the recurrent problem... is that once axiomatic deductive demonstrations are attempted in such fields as physics, cosmology, or medicine, finding good-looking principles that can be claimed as *self*-evident is extremely difficult;

the medical 'principle' he mentions here, for example, will turn out, on different interpretations, to be 'either controversial or vacuous' [120]. An especially striking oddity in Galen's approach appears in passages where he calls on observation to *confirm* results predicted by mathematical reasoning [123–124]. One might wonder why such reasoning should stand in need of empirical confirmation, and in what sense the whole of the resulting demonstration, including the confirming observation, could be considered 'mathematical'. But it is worth noting (though Lloyd does not mention the point) that Galen's strategy here has affinities with the procedure adopted by Ptolemy in his *Harmonics*, and might perhaps be understood in a similar way.[18] Calculations based on mathematical principles will generate indisputable results; but it is only if these results tally with our observations of

---

[17] The passage cited here (from *On the Opinions of Hippocrates and Plato* 8.1) involves astronomical observations made with the help of a dioptra.

[18] Likewise, one might also recall the Stoic concern with the use of logic and mathematics in addressing the problem of getting knowledge of objects that are not immediately evident, a use expounded in part by Cleomedes in his *Caelestia*, for example.

the phenomena that we can be sure that they were the right principles to apply in this particular case.[19] In *Harm.* 3.3 [Düring 1930, 91.22–94.20], Ptolemy represents the study of harmonics, conducted in the manner which his treatise commends and exemplifies, as an intrinsic part of mathematics, and reflects on the relations between reason and the senses of sight and hearing in such enquiries; and it would be interesting to compare the conception of the mathematical disciplines outlined in this passage with the one implicit in Galen's usage.

Lloyd considers, finally, 'the most sustained stretch of applied geometry in the Galenic Corpus' [124–127], the discussion of optical problems at the end of *De usu part.* 11. He notes the impeccable credentials of the geometry itself; but here again the passage runs into difficulties in its application of the theorems to the cases in hand. Most problematically, the forms and arrangements of the eye's parts and the course of the optic nerves which are implied by application of the geometry to them are not always consistent with one another; and on several occasions—and in several respects—the geometry fails to correspond to physical facts of which Galen was fully aware. In these cases, it is hardly possible to treat the geometrical models even as idealizations of the empirical facts, as may sometimes be appropriate elsewhere.

As we have come to expect, Lloyd guides his readers confidently through the tangled forests of the texts he discusses, pointing out and identifying the specimens of unusual fauna and flora encountered along the way and lucidly explaining their peculiarities. This marriage of medicine and mathematics has evidently produced some strange hybrids. Readers may reasonably wonder whether similar monsters emerge also from other scientific projects of the period, and if so, just how similar they are. Perhaps, as my allusion to Ptolemy

[19] Thus, Ptolemy finds no fault in the mathematical reasoning of the harmonic theorists whom he calls 'Pythagoreans'; but the fact that their conclusions do not correspond to the observed phenomena shows that the principles from which their reasoning flowed were inappropriate to the subject matter in question [see, e.g., Düring 1930, 6.1–5, 13.1–25]. Cf. Barker 2000, 26–29.

may have suggested, Galen's apparently cavalier treatment of mathematics reflects a general conception of the discipline that might have seemed less odd to his contemporaries than it does to us.[20]

The last two papers in the collection examine relations between musical science (primarily harmonics) and philosophy in the work of the Neoplatonists. In the first few pages of his erudite essay, 'The Music of Philosophy in Late Antiquity' [131–47], Dominic O'Meara emphasizes the influence of 'Pythagorean' and, hence, mathematical thought on late forms of Platonism, noting the pivotal role played by Nicomachus in setting its musical and mathematical agenda and putting in place the distinction between the 'empirical' harmonics of Aristoxenus and the 'rational' mathematical harmonics of the Pythagoreans [131–133]. He sketches Boethius' (and probably therefore Nicomachus') distinction between cosmic, human, and 'instrumental' music—where 'human' music concerns the structures of soul and body and the relation between them, and 'instrumental' music is music in our normal sense of the term—and he explains why it is the third of these that is the main subject of Pythagorean analysis.[21] He then explores the Nicomachean and Neoplatonist account of the place of harmonics among the mathematical sciences [133–135], from which it emerges *inter alia* that it is subordinate to arithmetic, and that whereas arithmetic is concerned with number or plurality in itself, 'Pythagorean music deals... with relations between numbers or finite pluralities' [135].

This leads to an important question: 'Pluralities of what?' Despite the fact that Iamblichus speaks of music as articulating relations between sounds, O'Meara denies that he means that sounds are the object of this science's studies, relating it instead, in a fascinating passage [135–136], to Iamblichus' discussion of the objects of the mathematical sciences in general [*De comm. math.* 10], with the help of explanations offered later by Syrianus and Proclus. Mathematical objects are concepts articulated through arithmetic and geometry.

[20] One might also usefully recall Aristotle's example of an application of geometry to medicine, and Hankinson's discussion of it at the end of his paper.

[21] I am unconvinced, however, by his use of Porphyry [Düring 1932, 5.21–27] in the course of his explanation. There is no reason to assume, as he does, that the ideas reflected there are Pythagorean; the relevant part of the passage is in fact a close paraphrase of Aristoxenus [see Da Rios, 1.5–8].

They are 'discursive conceptual projections of higher truths, present to the soul as above discursive reason and as innate in the nature of soul'. They 'exist in soul, but are not identical with soul: they are the way soul elaborates a discursive scientific knowledge of its own constitution'. This can be understood fairly straightforwardly so far as the objects of musical science are concerned, on the basis of material in the *Timaeus* where it is clear that in exploring musical relations the soul is developing discursive knowledge of its own structure. Thus, although the same relations may be expressed in audible sounds, they are not what 'Pythagorean music', as O'Meara calls it, is really about.

This part of the paper concludes [137–139] by explaining the mathematical procedures involved in this science, and discussing some of their key concepts and their sources (not always their earliest sources, but those that formed the immediate background to the Neoplatonists' work). The remainder [139–146] considers questions about the ways in which music, so conceived, was used in Neoplatonist philosophy. One is in philosophical education, on which O'Meara [139] quotes Calcidius: 'Music orders the soul rationally, calling her back to her former nature and making her at last into what she was when god at first made her' [Waszink 1975, 273.2–3]. In Neoplatonist thought, this 'assimilation of the soul to divine life' is the goal of philosophy, and the various disciplines in their philosophical curriculum are 'stages in a progressive scale aimed at the transformation and divinization of the soul', starting from education in the commonplace forms of moral virtue and proceeding upwards to the purificatory and intellectual virtues. Music contributes at two levels, both in the initial stages of ethical training and as one of the higher, theoretical sciences [140].[22]

Since the lower educational level is the subject of Anne Sheppard's paper in this volume, O'Meara restricts his comments on it to identifying its role in Neoplatonist thought as a preparatory sort of

[22] O'Meara notes that these contributions correspond roughly to those identified by Plato: (i) to the role of music in *Republic* 2–3 and (ii) 'to the role assigned to mathematical science in the image of the line at the end of book VI'. It strikes me as odd that he does not mention the more detailed and extremely relevant discussion of the mathematical sciences, including harmonics, in book 7.

'moral edification', and citing two familiar stories (about the use of music by Pythagoras and by Hypatia of Alexandria to cure people of erotic passion) to illustrate the way in which it supposedly worked [140–141]. As to the higher level, the role of music, like the other mathematical sciences, is to help us to bridge the gap between knowledge of the physical world and the knowledge of immaterial being to which philosophy aspires. Iamblichus and Proclus develop the idea that

> mathematics anticipates, foreshadows, the science above it, metaphysics, as if an image of it, just as mathematics represents itself as a kind of paradigm of the sciences below it.

Here the Neoplatonists are very clearly building on the picture sketched in Plato's image of the line. O'Meara finds indications in these philosophers' writings of the significance of musical science for two of the strictly philosophical disciplines, theoretical ethics and physics [142].

Central concepts in Iamblichus' account of ethics are those of measure, completeness, unity, proportionality, and harmony, all of which can be related to musical thought, and in particular to 'the Pythagorean concept of concord' (though one might add that many of the relevant facets of this conception are by no means restricted to the Pythagorean tradition). In elaborating this point through the examination of passages in Proclus and Damascius [143–145], O'Meara shows how Neoplatonist writers sometimes attempted to establish close correlations between particular virtues (specifically, those discussed by Plato in *Republic* 4) and particular musical concords, inspired in part by Plato's own words in *Rep.* 430e and 431e, while drawing also on the Pythagorean representations of these concords as ratios of numbers. It also emerges very clearly that Proclus' and Damascius' views on the matter are irreconcilably different; and the issue between them seems undecidable. For O'Meara, however,

> the important point... is that in approaching ethical concepts, the late Neoplatonists could find in music a theory of relations, of structure and in particular of unification, which influenced the way in which they saw the moral life, a life whose paradigms, they believed, were to be found in a higher theoretical science, in music. [145]

His closing comments on the relation between music and physics [145–146] are brief but important. The central point is that the main task for which Pythagorean musical theory is used in this context, notably by Proclus, is in the service of attempts to understand 'the extraordinary richness of Plato's harmonical accounts of the constitution of the soul and of the world' in the *Timaeus*: witness the fact that Proclus' vast commentary on the dialogue 'is to a considerable extent devoted to presenting Pythagorean music in connection with explaining the production of soul and of the world' [145]. A full reconstruction of the Neoplatonist understanding of musical theory and its integration with their accounts of these matters is yet to be attempted.

Greek harmonics, and late Platonist treatments of it in particular, are unfamiliar ground to many students of ancient philosophy and science, and I sympathize with the need O'Meara has evidently felt to devote much of his paper to basic essentials of a sort that can be dispensed with in studies of better-known areas of Greek thought. This has inevitably squeezed out many details which might fruitfully be examined.[23] But anyone embarking on a fuller exploration of this relatively uncharted territory will find the paper a valuable point of departure.

Anne Sheppard's paper, 'Music Therapy in Neoplatonism' [148–155], originated as a response to O'Meara's. Its scope is a good deal wider than its title suggests, since she considers an extensive range of ways in which Neoplatonists supposed that music can affect our emotions and dispositions; and she treats its educational uses as the central case.[24] At least some Greek theorists drew distinctions, as we might ourselves, between strictly therapeutic procedures and others. Thus, Aristides Quintilianus in his *De musica* (whose second

[23] Apart from closer study of the issues that he mentions, I miss in particular any reference to the epistemological and logical problems arising in connection with harmonic science, which interest Porphyry so deeply in his commentary on Ptolemy's *Harmonics*. Whether these interests are reflected in similar contexts in the writings of later Neoplatonists I do not know; it is another question that invites further research.

[24] She even includes uses of music which we might describe as only circumstantially beneficial, as when Synesius is said to have routed an army of barbarians by musical means when they were attacking Cyrene [153]. There is evidently nothing 'therapeutic' about this.

and third books place him at least in the penumbra of Neoplatonism) divides ethical education παίδευσις into two types, therapy (τὸ θεραπευτικόν) and 'cultivation' (τὸ ὠφελητικόν), each of which is divided in two again. The objective of therapy is to cure actual evils in the soul (in either of two ways); that of cultivation is to instill in young people virtues that they have not yet developed, or, in its second guise, to strengthen and preserve good dispositions that are already in place [Winnington-Ingram 1963, 68.22–69.1]. Education, as conceived in *Republic* 3, clearly falls under the latter heading; Plato says nothing there about eradicating vices already present in the soul. One might argue, of course, that for a Neoplatonist ethical education is indeed a kind of therapy, helping to release the soul from the afflictions brought upon it by its embodiment. But it would be perverse, in any case, to complain about the paper's extensive scope.

Sheppard begins [148–149] from a passage of Proclus' commentary on the *Republic* [Kroll 1899–1901, 1.56.20–60.13: esp. 1.58.27–59.3] in which he attributes to Plato a classification of μουσική into four types. The first two are philosophy and inspired poetry; Sheppard [148–149] identifies the third with the 'Pythagorean music' discussed by O'Meara, and the fourth as music of the lower kind to which he briefly refers [140–141], whose role is the education of the emotions. This is clearly right; Proclus, as she says, relates the fourth kind of music explicitly to Plato's discussion in *Rep.* 3.

Before any further consideration of Neoplatonist texts, Sheppard surveys ideas current in the fifth and fourth centuries BC about the uses to which music's emotional power can be put (mentioning Damon, Aristophanes, Plato, Aristotle, and Theophrastus in particular). 'Somewhere in the period between Theophrastus and the Neoplatonists', she goes on, 'the idea that music can be used for the therapy of the passions became associated with Pythagoreanism' [150]. It is just possible, I suggest, that the association originated with Plato's immediate successors in the Academy; but her comment is a salutary reminder of the unreliability of our evidence for such an idea in the early Pythagorean tradition. She then offers arguments, based on a hypothesis of Thomas Mathiesen [1983] about Aristides Quintilianus and on her reading of a passage in Iamblichus *De mysteriis*, for the view that the conception of two kinds of music, Proclus' third and fourth, was introduced into Neoplatonism by Porphyry; and while the case is not proven, I find her reasoning persuasive.

Her brief discussion of another passage of Proclus [Kroll 1899–1901, 1.60.14–63.15] deserves a special mention [152]. Proclus is considering the *harmoniai* and rhythms which Plato thought useful in education, and trying to answer Aristotle's criticism of Plato on the topic of the Phrygian *harmonia* [*Pol.* 1342a–b]. She notes that it amounts to 'a typical piece of late Neoplatonist exegesis, assuming unity in Plato's thought and seeking to reconcile Plato and Aristotle'. Quite so. But her more intriguing and very well taken point is that Proclus' argument turns in part on ideas about religious ritual and divine possession, which are prominent in Iamblichus, as she has already said [151], and indeed pervasive in Neoplatonism, but of course make no appearance in Plato's discussion at this point in the *Republic*. It is a fascinating example of the ways in which Neoplatonists wove together threads drawn from a very wide range of original contexts—both within the writings of the philosopher they were studying and outside them—to create their extraordinary philosophical synthesis.

Sheppard's closing remarks [154] are worth quoting.

> A full account of Neoplatonic views of music would need to cover both the scientific and the therapeutic kind. Music is, after all, a complex phenomenon: regarded nowadays as one of the arts, it is nevertheless susceptible both to scientific analysis and to philosophical study. It should come as no surprise that the Neoplatonic view of music reflects that complexity.

To echo the final words of Lennox' paper [70], 'Amen to that'.

This has been a long review of a short book. As I said at the start, no compendious conclusions emerge from it. Its signal virtue is that each of its contributors has outlined, in more or less detail, a set of problems concerned with the relations between the sciences and between them and philosophy; and both the overall issues and the problems which they identify are of genuine importance. None of them, I think, would claim to have definitively solved them; what they do is to set a massive agenda for further and urgently needed research. The book will be of great value to anyone who tries to undertake any part of it.

BIBLIOGRAPHY

Barker, A. 2000. *Scientific Method in Ptolemy's Harmonics.* Cambridge.

________ 2007. *The Science of Harmonics in Classical Greece.* Cambridge

Barnes, J. 1994. *Aristotle: Posterior Analytics.* 2nd edn. Oxford.

Da Rios, R. 1954. *Aristoxeni elementa harmonica.* Rome.

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

________ 1932. *Porphyrios Kommentar zur Harmonielehre des Ptolemaios.* Göteborg.

Gotthelf, A. 1988. '*Historiae I: Animalium et Plantarum*'. Pp. 100–135 in W. W. Fortenbaugh and R. W. Sharples edd. *Theophrastean Studies.* New Brunswick.

Helmreich, G. 1884. *Claudii Galeni scripta minora.* vol. 2. Leipzig.

________ 1907–1909. *Galeni de usu partium.* 2 vols. Leipzig.

Kroll, W. 1899–1901. *In Platonis rempublicam commentarii.* 2 vols. Leipzig.

Kühn, K. G. 1821–1823. *Claudii Galeni opera omnia.* 22 vols. Leipzig.

Lennox, J. G. 1986. 'Aristotle, Galileo and the "Mixed Sciences"'. Pp. 29–51 in W. A. Wallace ed. *Reinterpreting Galileo.* Washington, DC.

________ 2001. *Aristotle's Philosophy of Biology.* Cambridge.

Lloyd, G. E. R. 2002. 'Le pluralisme de la vie intellectuelle avant Plato'. Pp. 39–54 in A. Laks and C. Louguet edd. *Qu' est-ce que la philosophie présocratique / What is Presocratic Philosophy?* Lille.

________ 2003. *In the Grip of Disease.* Oxford.

Mathiesen, T. J. 1983. *Aristides Quintilianus: On Music, in Three Books.* New Haven.

McKirahan, R. 1992. *Principles and Proofs.* Princeton.

Ross, W. D. 1949. *Aristotle's Prior and Posterior Analytics.* Oxford.

Sharples, R. W. 2005. ed. *Philosophy and the Sciences in Antiquity.* Burlington, VT/Aldershot, UK.

Simpson, G. G. 1964. *This View of Life: The World of an Evolutionist.* New York.

Vegetti, M. 1995. *La medecina in Platone.* Venice.

Waszink, J. H. 1975. *Calcidius. Commentarium in* Timaeum. London/Leiden.

Winnington-Ingram, R. R. 1963. *Aristides Quintilianus. De musica.* Leipzig.

Zhmud, L. 1994. 'Die Beziehungen zwischen Philosophie und Wissenschaft in der Antike'. *Sudhoffs Archiv* 78:1–13.

Nicolas de Cues. Les écrits mathématiques. Présentation, traduction et notes by Jean-Marie Nicolle

Textes de la Renaissance 132. Paris: Honoré Champion Éditeur, 2007. Pp. 507. ISBN 2–7453–1573–1. Cloth CHF 143.36

Reviewed by
Eberhard Knobloch
Technische Universität Berlin
eberhard.knobloch@tu-berlin.de

Nicolaus von Kues war der überragende Denker, Philosoph und Theologe der Renaissance. Sein Einfluss auf die folgenden Jahrhunderte kann kaum überschätzt werden. Die Mathematik mit ihren Zeichen und Figuren diente ihm dazu, sich dem unerkennbaren Gott zu nähern, sich über ihn zu äussern. Deshalb ist diese erste vollständige französische Übersetzung aller mathematischen Schriften von Jean-Marie Nicolle zu begrüßen. Eine deutsche Übersetzung von Josepha Hofmann mit Erläuterungen ihres Ehemannes Joseph Ehrenfried Hofmann erschien 1951.

Nicolle hat sich seit 1996 in neun Veröffentlichungen als Kenner der Materie ausgewiesen. Bei der Vorbereitung seines Bandes war ihm bewusst, dass Menso Folkerts, München, für die Heidelberger Akademie der Wissenschaften eine Neuedition der mathematischen Schriften vorbereitet [68].

Die Einleitung nimmt zunächst zu den philosophischen und mathematischen Quellen von Nicolaus Stellung, das heisst insbesondere Plotin, Augustin, Boethius, Proklos, Ramón Llull bzw. Euklid (in sehr eingeschränktem Maße), vielleicht Archimedes, Campanus von Novara, Bradwardine, Johannes de Muris, Nicole Oresme. Die zwölf mathematischen Schriften befassen sich alle mit dem Kreis, dessen Quadratur oder der Rektifikation des Umfanges. Zur (Nach-)Wirkung zählt Nicolle dreizehn Zeitgenossen und spätere Mathematiker auf. Nicht genannt sind z.B. Gottfried Wilhelm Leibniz oder Otto von Guericke, da Nicolle offensichtlich manche englische, französische oder deutsche Arbeiten zum Einfluss des Cusanus auf Galilei, Leibniz oder Guericke nicht bekannt geworden sind.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 35–36

Das folgende kurze Glossar ist leider durch viele Fehler beeinträchtigt: ‚abscision' statt ‚abscisio', ‚addition' statt ‚additio', ‚complementis linea' statt ‚complementi linea', ‚isoperimetris' statt ‚isoperimetrus' usf. Die Aufzählung der Handschriften richtet sich nach Hofmann. Der lateinische Text der zwölf Schriften ist im wesentlichen der Basler Ausgabe von 1565 entnommen. Von der Übersetzung, der knappe Anmerkungen beigegeben sind, gilt, was Nicolle über die deutsche Übersetzung sagt [69]: ‚d'une grande qualité, mais n'est pas exempte de critiques', freilich aus anderen Gründen. Es gibt Ungenauigkeiten, Auslassungen, auch klare Missverständnisse. Einige Beispiele mögen diese Bemerkung verdeutlichen.

- ‚figuram ultimam quam ibi posui, brevitatis hic praetermitto' [130f.] wird zu ‚je passe à la dernière figure que j'ai posée ici pour aller vite' statt ‚omets ici la dernière figure que j'y ai posée pour aller vite';
- (Archimedes) ‚videns illam [*sc.* quadraturam circuli] attingi non posse' [234f.] wird zu ‚qui, voyant qu'il ne pourrait pas l'atteindre' statt ‚qu'elle ne peut pas être atteinte', das heisst die Kreisquadratur ist nicht möglich, nicht nur nicht für Archimedes;
- ‚Multiplicatio primae lineae in medietatem peripheriae, aequatur embado polygoniae' [246] wird zu ‚La multiplication de la première ligne par la moitié du périmètre est égale à deux polygones' statt ‚à la superficie du polygone'. In der Tat ist die Fläche des Quadrates (des Polygons), das dem Kreis mit dem Radius r umbeschrieben ist, $r \times \frac{1}{2}(8r) = 4r^2$.
- ‚nemo unque scire potuit' [27]) wird zu ‚personne ne pouvait connaître', unque (*jamais*) bleibt unübersetzt.
- ‚rectae curvaeque quantitati' [432] wird zu ‚de la droite et de la courbe'. Quantitas bleibt unübersetzt, obwohl quantitas, quantum, non-quantum Kernbegriffe des cusanischen Denkens sind. Deshalb hätte ‚quantité' im kurzen Index rerum nicht fehlen dürfen.

Eine bestimmte Vorsicht bei der Benutzung der Übersetzung ist also nötig, auch wenn sie weithin zutrifft. Als Anhang hat Nicolle eine französische Übersetzung des Dialogs Johannes Regiomontans über die Kreisquadratur des Nicolaus von Kues angefügt. Den handlichen Band beschließt ein Personen- und ein Sachverzeichnis sowie eine Bibliographie.

Jacopo da Firenze's Tractatus Algorismi *and Early Italian Abbacus Culture* edited by Jens Høyrup

Science Networks—Historical Studies 34. Basel/Boston/Berlin: Birkhäuser, 2007. Pp. xii + 482. ISBN 978–3–7643–8390–9. Cloth € 99.00

Reviewed by
Warren Van Egmond
Tempe, Arizona
warrenve@gmail.com

The Italian Renaissance is remembered best for its magnificent works of art and architecture. The words themselves evoke the paintings and sculptures of Michelangelo, Leonardo, Raffaello, Botticelli, and the other great masters of the period, while the churches, palaces, and plazas of Italy continue to inspire wonder to this very day. The Renaissance is also remembered for certain masterpieces of modern literature and philosophy such as the essays of Petrarch, the stories of Boccaccio, and *The Prince* of Machiavelli; but the other accomplishments of the period are largely forgotten.

One of those achievements that is least remembered is the Renaissance contribution to mathematics. Although only a few experts are aware of it, the Italian Renaissance created the style and manner of doing mathematics that has become the common heritage of Western Europe and modern world culture. The mathematicians of the period, although virtually unknown by name, determined the way in which we write and calculate with numbers, the types of problems we solve, the manner in which we approach them, and, most significantly, the way we do algebra [see Van Egmond 1986].

The records of these achievements are preserved in a large body of documents known collectively as the 'abbaci', a name derived not from the more familiar reckoning device (the abacus, written with one 'b') but from the title of the fundamental work of the genre, the *Liber abbaci* of Leonardo Pisano or Fibonacci, which was composed in Italy in 1202.[1] More than 400 such documents survive from the

[1] There is now a complete English translation by Laurence E. Sigler [2002], which was reviewed by Serafina Cuomo in *Aestimatio* [2004].

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 37–47

broad Renaissance period itself, from about 1300 to 1600, in both printed and manuscript forms.[2]

Unfortunately, the elementary and inelegant form of these simple mathematical texts has kept them from receiving the kind of editions and printings that would attract the attention of other Renaissance scholars, let alone the general public. Those editions of the *abbaci* that do exist are largely typescript transcriptions cheaply reproduced and distributed by academic centers and small publishers.[3]

The current volume, an edition of Jacopo da Firenze's *Tractatus algorismi* prepared by Jens Høyrup of Roskilde University in Denmark, is the first to aim at a larger audience. It is the first text of its kind to be published by a major academic publisher, and the first to be translated into English in its entirety. This makes it the only example of an *abbacus* book that will be readily available to the wider academic and general public, and on this score alone it is worthy of some note.

The quality of the production is excellent. The typography is clear and readable, the paper and binding are of the highest quality, and all the drawings and diagrams have been redrawn to increase their readability. The English translation is printed in parallel columns so that it can be easily compared with the original Italian. The editor's deliberate decision to render a highly literal translation means that the English reads somewhat awkwardly at times; but since we are dealing with a technical mathematical text where content is more important than literary style, this is of little consequence.

The text itself is also well chosen. It provides a representative sample of what a normal *abbacus* book contained, including sections on how to write and calculate with the Arabic numerals, an exposition of the principle of place value, a large number of multiplication and division tables, and many practical business problems on pricing, exchange, interest and discount, partnerships, and the like, plus additional sections on practical geometry, algebra, and the alloying of

---

[2] For a nearly complete catalog of almost all known *abbacus* books and manuscripts, see Van Egmond 1980.

[3] See, e.g., the long series of 'Quaderni' published by the Centro Studi della Matematica Medioevale at the University of Siena, or the many editions of Gino Arrighi listed in the bibliography of the book under review.

metals. There is also a short list of the common coins in use at the time and their values in terms of their precious metal content. Thus, anyone who is interested in seeing what a typical *abbacus* book looked like can now readily refer to this volume.

The transcription is accurate though not perfect. I have checked the entire text against the original manuscript and found, on average, about one error per manuscript page. These are all confined to single letters or words and none of them affect the meaning of the text. The most annoying error is a systematic rendering of the numeral '1' as the lowercase letter 'j', a mistake so obvious and so persistent that I can only attribute it to some uncorrected computer glitch. The only error that even slightly affects the sense of the text is the rendition of the word 'terza' in the explanation of the rule of three on page 237 as 'altra'. This loses the significance of using the 'third' number that gives the rule its name.

The original manuscript is full of colorful drawings of buildings, objects, and people that illustrate many of the problems being posed. While these are quite crude and of no artistic merit, they add much to the charm of the original document. Unfortunately, apparently for technical and financial reasons, these were not reproduced but were all redrawn by hand by the editor and reproduced as black and white photographs. Again, these are quite accurate and do not affect the content of the text. However, I did note a failure to reproduce the numeral forms on page 196 accurately and to space the tables of continued division on pages 221–226 correctly. The former gives the wrong antique forms of the numerals 3 and 4 and omits a 1 written before the zero. This, when combined with the reformatting of the tables, might give the impression that the author wrote the zero separately and not always as part of the number 10.

These minor errors do not detract from the value of having a complete *abbacus* text available with a full English translation for the first time. Unfortunately, the advantage offered by having such a fine edition produced by such a reputable publisher is offset by the editor's unfortunate decision to use the youngest and least reliable copy of the text as the basis of the edition. His concomitant decision to relegate the earlier texts to an appendix without the benefits of a standard textual *apparatus* renders the edition largely useless to advanced scholars in the field, while his extensive efforts to justify the priority of the inferior text burdens the commentary with arcane arguments that will likely mystify and repel any general reader who might be interested in Jacopo's work. The result is a book that satisfies no one.

As is often the case with books in the *abbacus* genre, there are multiple copies of this particular text, all claiming to be written by Jacopo da Firenze at Mons Pesulanus [Montpellier] in the year 1307. Two versions are clearly later copies. Since they are both written on paper, they can be readily dated by watermarks, ink, handwriting, and production styles to the 15th century. One copy now in the Trivulziana Library in Milan (ms. 90) is datable to *ca* 1410; the second copy in the Vatican library (Vat. Lat. 4826) dates to *ca* 1450. A third copy, now in the Riccardiana Library in Florence (ms. 2236), is written on vellum and so cannot be precisely dated; but the fact that it uses vellum (which was largely abandoned for writing common texts by the middle of the 14th century), combined with its ink, handwriting, language, and style, make it clear that it was written in the early 14th century, and thus must be accepted as the oldest text.

Of course, chronological priority does not necessarily establish textual priority. A scholar must also examine the details of the text, looking in particular for the kinds of omissions and errors that signal a derivative copy. In this case, the Vatican copy contains many such omissions and errors, all of which can be corrected by looking to the two earlier copies. Indeed, six complete paragraphs/problems found in the earlier Florence and Milan copies are missing from the Vatican version, which shows that its copyist was carefully selecting what he wanted to include. The only novelties in the later copy are the insertion of a short explanatory paragraph in the multiplication tables, a reformulation of six geometry problems, the insertion of an entirely new section on algebra, and the addition of a large set of miscellaneous problems at the end of the book.

None of this is at all unusual. The *abbacus* books as a whole are noteworthy for the variability of their texts. Authors and copyists often took problems, passages, and entire sections from other books without ever giving credit or even noting that fact. The focus was always on learning how to do mathematics and solve problems, not on crediting one's predecessors or preserving 'sacred texts'. Modern ideas of editing and textual integrity simply did not exist at this time. Compilers felt free to revise, add, omit, or mix their sources in whatever way they wanted. Thus, it is not at all surprising that a 15th-century copyist, while working primarily from an older source, might have omitted some problems that were of no interest to him, added or revised a few paragraphs, and inserted a discussion of algebra, which had become a common feature of other *abbacus* books by that time.

Unfortunately, early in his study of Jacopo's work, the editor became convinced that the Vatican copy, which was clearly written last (he does not dispute this fact at all), nonetheless represents the most authentic text and must be given priority over the two older copies. Elsewhere he reports that he came to this conviction in 1997 when he first examined the algebra section in the Vatican manuscript and noticed how different it was from the traditional presentations of algebra that derived from the tradition of Mohammed bin Musa al-Khwarizmi [Høyrup 2006, 5]. Later comparison with an earlier version of the text that does not contain the algebra led him to believe that the algebra was 'really due to Jacopo' and not a later insertion [2006, 5].

This conclusion was first presented at a conference in Beaumont in 1999, and subsequently published in the proceedings of that conference in 2001 [Høyrup 2001]. Meanwhile, he had published the text of the algebra section alone in *Centaurus* in 2000. His arguments were further elaborated at a second conference in Barcelona in 2003, which were published in *Historia Mathematica* in 2006. Many of the diagrams and discussions found in the present book are taken from these earlier publications, most with few changes.

The grounds that the editor gives for preferring the youngest copy are primarily linguistic and stylistic. I think that he does a better job of presenting them in his first conference paper; but in the current volume he summarizes them by saying,

> this [the Vatican] manuscript is very coherent in style as well as regarding the presence of [*sic*] various idiosyncratic features both in the chapters that are shared with [the Milan] and [Florence manuscripts] and in those that are not. [5]

Now style does offer one way to establish a relationship between manuscripts, but it is certainly the least reliable and the hardest to prove scientifically. This is particularly true for texts in Renaissance Italian, which had not yet become a standard literary language. Indeed, at the time that this text was copied, it can be said that Italian was not yet a language at all but rather a range of local dialects, differing not only from region to region and province to province but even from town to town. Without any standard vocabulary, spelling, or grammar, a copyist might feel free to change the text into the words, forms, and phrases that were familiar to him, or he might stick literally to his exemplar. Several styles could easily become mixed, depending on how attentive he was to his task. The fact that a text seems 'coherent' only indicates that the copyist was being consistent; it says nothing about the state of the original.

Unfortunately, it is clear in hindsight that the editor allowed his first impression of the section on algebra to color all of his subsequent investigations of Jacopo's work and to value weak stylistic impressions above the hard evidence of direct textual comparison. Sadly, he allowed this personal prejudice to affect his entire presentation and treatment of the text, markedly limiting its value to both scholars and the general reader. Thus, the transcription of the younger Vatican text is presented on pages 193–376 as the authoritative text, entirely by itself, with the parallel English translation, even though it is clearly an inferior copy that omits many words and phrases together with the six complete paragraphs that had to be supplied from the Florence and Milan copies. Even a cursory scan shows the large number of omissions that had to be supplied from these earlier copies or corrected by the editor, not to mention the many duplications and insertions made by the Vatican copyist himself. Indeed, while comparing the text with the original, I found that the editor had omitted all of the corrections that the copyist himself made, perhaps because there were so many.

The transcription and translation of this flawed text is then followed by a second transcription of the two earlier texts on pages 383–456, added as if they were an inferior appendage and presented in a very unusual way using different font styles, underlining, and subscripts that is not at all standard for editing variorum texts in modern textual studies. The editor himself calls it a 'semi-critical edition' [379]. I found it extremely difficult to read and essentially useless for scholarly purposes. Moreover, there is no common numbering for the paragraphs or sections of the text, so one cannot readily compare the texts in the two sections; nor is there a comparative table of contents that would show how the three texts compare in their organization and selection of problems. All of this hinders any effort to examine the editor's hypothesis independently and makes it very difficult for serious scholars of the texts and language of the period to use the book to advance their research in these fields.

But by far the greatest failing of the edition is the complete absence of a glossary, a list of technical terms and their variant forms, a summary list of problems, an index of problem types, a list of coins, or any of the other tools that have become essential to the scholarly presentation of medieval and Renaissance texts. Such *apparatus* have long been standard in the field, and were first applied to an *abbacus* text in Kurt Vogel's edition of the Columbia algorism in 1977, a work that the editor cites and certainly was aware of. In short, the edition lacks the basic standards and tools that are fundamental to modern scholarship in this field.

Other failings clearly follow from the editor's determined effort to establish the priority of the Vatican copy, including exaggerations, misrepresentations, unsupported claims, and a blindness to contrary evidence. For example, on page 6 the editor states that the Vatican manuscript 'is a meticulous (yet not blameless) library or bookseller's copy made from another meticulous copy', when in fact, as we have already seen, it is in fact a very poor copy. It is full of errors, omissions, insertions, and corrections, and is clearly inferior to the two older copies. On page 5, the editor says that

> reducible fourth-degree equations were solved routinely in Arabic algebra at least since al-Karaji's time and therefore were no innovation, neither in 1307 nor in the late fourteenth century.

No source is ever given for this very expansive claim, and the editor himself, after an exhaustive comparison with 13 Arabic algebras listed on page 154, not only fails to identify any such source but states, 'We do not know the kind of Arabic algebra that provided him [Jacopo] with his ultimate inspiration' [159], i.e., there is no Arabic source for the equations in the Vatican manuscript.

This fruitless search for a foreign source is driven by the early date of Jacopo's original work, 1307, which places it 20 years before the oldest known vernacular algebra text, that of Paolo Gerardi [see Van Egmond 1978]. Since the editor steadfastly maintains that the algebra section found only in the 1450 copy of Jacopo's work is original, he is forced to devote an entire chapter [147–182] to a search for sources that predate Gerardi. In addition to the above-mentioned Arabic texts and Gerardi himself, he looks at several other 14th-century Italian algebras, including those of Dardi da Pisa and Giovanni di Davizzo as well as some anonymous texts now in Parma and Luca. A simple stemma first offered on page 145 grows on pages 167 and 176 to become an incredibly complex nest of manuscripts and links that miraculously leaves the Vatican copy untouched at the top. Clearly some form of Occam's razor ought to apply to textual studies, so that the simplest explanation, in which the Vatican manuscript is a late copy of Jacopo's 14th-century algorism with the insertion of a later algebra section, would be preferred.

And indeed, such a source is readily found. Two late 14th-century algebra texts now in the Biblioteca Nazionale Centrale di Firenze, Fond. Prin. II. V. 152, folios 153r–166r, and Conv. Sopp. G. 7. 1137, folios 110r–111v, give exactly the same equations as the Vatican text in exactly the same order [Van Egmond 2008, 313]. Moreover, they are the common sources for the algebras found in a number of *abbacus* books written in Florence in the middle of the 15th century, which probably stem from the school of maestro Biagio dell'abbaco, who died in 1397, but whose work was carried on by Lucha di Matteo and Calandro Calandri and his sons and students [Van Egmond 2008, 313]. Clearly, the copyist of Vat. Lat. 4826, while revising an old copy of Jacopo's *Tractatus algorismi*, merely inserted a section on algebra that was being widely circulated in his own day, and then further added the additional collection of problems that follow the end of Jacopo's original text. Had the editor not been so firmly wedded to his early conviction that the algebra section in Vat. Lat. 4826 had to be original to Jacopo, he might have discovered this and produced a far better book. As it is, he allowed his initial impression to become a bias that adversely affected the quality of his final work. The result is distorted scholarship, the unwarranted separation of the early and later texts, the lack of a common reference system, and the absence of the standard scholarly *apparatus*, all of which severely reduce the book's value for the serious scholar in this field.

His obsession with proving the authenticity of the Vatican text also diminishes the book's appeal for a more general reader, such as a student of Renaissance history or someone merely curious about the state of Renaissance mathematics. The title of the book promises a discussion of 'Early Italian Abbacus Culture', and, as the first edition of an *abbacus* book aimed at a broader public, one might have hoped for a general introduction explaining the economic and social background that led to the composition of the *abbacus* books, the role that mathematics played in Renaissance society and business, and some illustration of how Renaissance men actually did mathematics, wrote numbers, and solved problems. Unfortunately, anything that might fit this description is limited to about three pages [27–29]. The two-page introduction [3–4] is devoted to a dull review of the scholarly history of the Vatican manuscript, and the description of the three manuscripts that follows [5–25] becomes fully occupied with the editor's complex linguistic arguments over why the Vatican text must have priority over the other two. The detailed discussion of alternate spellings, words, phrases, and word ratios will bore anyone but the most dedicated student of Italian linguistics. The chapter titled 'The Abbacus Tradition' [27–44] quickly turns into a detailed summary of the obscure 13th-century *Livero de l'abbecho* and a comparison with the *Liber abbaci*. The long analysis of the mathematical content of the Vatican manuscript that follows this [45–146] is overly technical and will be impenetrable to the general reader. There is no question that this section is an intellectual *tour-de-force*, as the editor displays his wide knowledge of early mathematics by identifying similar problem types in Latin, Greek, Arabic, Indian, and even Chinese problem texts; but it is an effort that will be appreciated only by the most narrow specialist and is already readily available in other well-known sources.[4]

Thus, the editor's conviction that the youngest manuscript contains the most authentic text has resulted in a severely flawed book. For the serious scholar in the field, it has led him to split the texts, disregard the scientific standards of textual editing, and omit the basic tools of scholarly analysis. For the more general reader or Renaissance scholar, has it buried what could have been a very entertaining and illuminating document beneath a pile of arcane scholasticism. Any attempt to make the book accessible to a more general reader or

[4] The best is Tropfke 1980, 513–660.

even the wider class of Renaissance scholars was lost in the pursuit of the editor's personal passion.

The overall value of having a complete edition and translation of an entire *abbacus* text available in a quality edition for the first time is undeniable, and this book will retain its value for this purpose alone despite its many other failings. But one can only regret that the editor's fixation on proving his narrow thesis led him to compromise so much else. This book could have been so much more.

BIBLIOGRAPHY

Cuomo, S. 2004. rev. Sigler 2002. *Aestimatio* 1:19–27.

Høyrup, J. 2000. 'Jacobus de Florentia, *Tractatus algorismi* (1307), the Chapter on Algebra (Vat. Lat. 4826, fols. 36v–45v)'. *Centaurus* 42:21–69.

__________ 2001. 'The Founding of Italian Vernacular Algebra'. Pp. 129–156 in *Commerce et mathématiques du moyen âge à la rénaissance, autour de la Méditerranée. Actes du colloque international du Centre International d'Histoire des Sciences Occitanes (Beaumont de Lomagne, 13–16 mai 1999).* Toulouse.

__________ 2006. 'Jacopo da Firenze and the Beginning of Italian Vernacular Algebra'. *Historia Mathematica* 33:4–42.

Sigler, L. E. 2002. *Fibonacci's* Liber Abaci*: A Translation into Modern English of Leonardo Pisano's Book of Calculation.* New York.

Tropfke, J. 1980. *Geschichte der Elementarmathematik, 4. Auflage. Band I: Arithmetik und Algebra; vollständig neu bearbeitet von Kurt Vogel, Karin Reich, Helmuth Gericke.* Berlin.

Van Egmond, W. 1978. 'The Earliest Vernacular Treatment of Algebra: The *Libro di ragioni* of Paolo Gerardi (1328)'. *Physis* 20:155–189.

__________ 1980. *Practical Mathematics in the Italian Renaissance: A Catalog of Italian Abbacus Manuscripts and Printed Books to 1600.* Annali dell' Istituto e Museo di Storia della Scienza, Monografia N. 4. Florence.

Van Egmond, W. 1986. 'The Contributions of the Italian Renaissance to European Mathematics'. Pp. 51–67 in Istituto Nazionale di Alta Matematica Francesco Severi, *Symposia mathematica* 37. London/New York.

——— 2008. 'The Study of Higher-Order Equations in Italy before Pacioli'. Pp. 303–320 in Joseph W. Dauben *et alii* edd. *Mathematics Celestial and Terrestrial. Festschrift für Menso Folkerts zum 65. Geburtstag.* Acta Historica Leopoldina 54. Halle (Saale).

Vogel, K. 1977. *Ein italienisches Rechenbuch aus dem 14. Jahrhundert (Columbia X 511 A13).* Veröffentlichungen des Forschungsinstituts des Deutschen Museums für die Geschichte der Naturwissenschaften und der Technik, Reihe C: Quellentexte und Übersetzungen 33. Munich.

Historical Dictionary of Ancient Greek Philosophy edited by Anthony Preus

Historical Dictionaries of Religions, Philosophies, and Movements 78. Lanham, MD/Toronto/Plymouth, UK: Scarecrow Press, 2007. Pp. xxiv + 345. ISBN 978–0–8108–5487–1. Cloth $85.00

Reviewed by
Joshua J. Reynolds
Center for Hellenic Studies
joshua@mail.chs.harvard.edu

This work, as its title indicates, is historical in focus. Its purpose is to provide general information about the lives and times of the ancient Greek philosophers, their professional associations and schools, the questions that they addressed, their basic doctrines, and the reception of those doctrines by subsequent thinkers in antiquity. Extensive interpretive discussions and the critique of modern scholarship are omitted. Preus' dictionary is thus best suited for undergraduate or beginning graduate students who require knowledge of the historical issues involved in the study of ancient philosophy.

Being a dictionary, this work contains an abundance of Greek terms. All the Greek, however, has been transliterated. The author begins with an extended 'Note' on transliterating and pronouncing the letters of the Greek alphabet (including diphthongs and breathing marks) to accommodate those who need to find the appropriate entry for a term that they might encounter elsewhere in Greek characters. Following the note on transliteration, a chronology includes the life spans of individual philosophers along with the dates of relevant social and political occurrences. This timeline has been divided into Hellenic, Hellenistic, and Roman Imperial periods, although the dictionary as a whole extends its coverage to the Medieval period.

A unique feature of this dictionary is its introduction: a concise narrative of key figures and ideas in their historical contexts. This survey emphasizes intellectual influences, associations, departures, successions, and traditions. Endnotes include handy references to the standard editions of the relevant texts, whether Greek, Latin or English. The general subjects and some specific topics that the introduction covers are as follows:

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 48–51

(1) The Presocratic Period (4 pp.): natural philosophy and the beginning of metaphysics; source problems; Pythagoras on philosophy; Xenophanes' epistemological scepticism; Heraclitus; Parmenides on being; Zeno on motion and dialectic; the elements of Empedocles and Anaxagoras; the Atomists; Sophists on teaching politics and rhetoric.
(2) Socrates and Plato (2 pp.): the historical Socrates, sources for his life, his firm belief in the possibility of objective definitions; Plato, the chronology of his corpus, the unity of his thought; the Academy; Forms as objects of knowledge.
(3) Aristotle (1.5 pp.): biographical information; the Aristotelian corpus; Aristotle's concern with empirical research; Aristotle on forms and the soul.
(4) Hellenistic Philosophy (2 pp.): successors in the Lyceum and Academy; Pyrrho of Elis; Epicurus' Garden, atoms, and death; Zeno of Citium and early Stoics; the Library at Alexandria and its scholars.
(5) The Academy Becomes Skeptical (2.5 pp.): Arcesilaus against the Stoics; Athenian philosophers in Rome; Panaetius' 'revisionist Stoicism'; Lucretius and Cicero; the Platonic dogmatism of Philo and Antiochus; changes to Stoicism under Posidonius.
(6) Philosophy in the Roman Imperial Period (8 pp.): Platonism in Alexandria; Neopythagoreanism; Philo's synthesis of the Torah with the *Timaeus* and Stoicism; Roman court philosophers; Plutarch; second-century texts (AD) from varying traditions (e.g., Chaldean Oracles, Ptolemy's *Almagest*, the *Meditations*); Middle Platonists; Alexander of Aphrodisias; thinkers of the late second to early third centuries (e.g., Galen, Sextus Empiricus, Diogenes Laertius, Hippolytus); Clement and Ammonius; the Neoplatonists; Calcidius' Latin translation of the *Timaeus*; the Cappadocian philosophers; fourth and fifth century Neoplatonists in Athens; Jerome and Augustine; other important writers up to the close of the Athenian school in 529 (e.g., Macrobius, Stobaeus, Theodoret, Boethius).
(7) The Survival and Transmission of Greek Philosophy in the Medieval Period (5.5 pp.): the fate of Greek philosophy from the seventh to 12th centuries; higher education in Constantinople; medieval Latin Neoplatonic theology; the manuscripts of Plato and Aristotle; the synthesis of Greek philosophy with Islamic thought;

> Charlemagne and the transmission of Latin texts; Anselm and other 11th-century contributors to philosophy in western Europe; Latin translations; preview of the renaissance and modern world.

This overview should make it clear that, in terms of coverage, the introduction inclines towards the Imperial period and late antiquity. This stands to reason, as students tend to be least familiar with the thinkers, ideas, and traditions of these eras. Philosophically, the particular discussions in the introduction vary in helpfulness. For instance, the discussion of Parmenides on 'what is' is effective, but the discussion of Heraclitus simply states that he was concerned with *logos* and the ambiguity of language. One final note concerning the introduction: in the dictionary proper, the author has highlighted in bold print the various terms under a given entry that have their own entries as well. A similar device would have been helpful in the introduction.

The dictionary proper follows the introduction. It contains 250 pages of entries and covers an impressive range of topics: proper names, places, philosophical notions in English (e.g., Abstraction, Account, Chance, Law), schools, transliterated Greek terms ('Adēlon', 'Ousia', 'Sympaschein'), including technical phrases, such as 'ti esti' and 'to ti ēn einai'. A glossary provides brief definitions for all of the terms highlighted in the dictionary, whether Greek or English.

Some of the dictionary entries contain no more than the information from the introduction. For instance, the introduction on Jerome reads,

> In the Roman world, Jerome (Eusebius Sophronius Hieronymus, 347–420) translated the Bible into Latin [19],

while the dictionary reads,

> **Jerome (347–420 CE)**. Eusebius Sophronius Hieronymus, translator of the Bible into Latin [148].

Most entries, however, expand upon the introduction (e.g., 'Cosmos', 'Logos'), while some provide information that the introduction does not mention at all (e.g., 'Alcmaeon of Croton', 'Aulos', 'Egyptian Origins of Greek philosophy'). Finally, several entries simply contain cross-references, such as 'Cognition. See Dianoia; Katalēpsis; Noēsis; Nous' [76].

One virtue of the dictionary is that most entries provide references to relevant primary texts and passages. A possible shortcoming, however, concerns entries that consist of English translations of Greek terms. Typically, only one or two translations have been provided for a given Greek term, so students might run into difficulties locating the appropriate entry if their source uses a translation that the dictionary does not use. For example, the entries 'Purpose' and 'End' direct the reader to the entry 'Telos', but there is no entry for 'Goal', an equally valid translation of the Greek. Likewise, 'Virtue' directs to 'Aretē', but there is no entry for 'Excellence'.

Students will find a great benefit in the bibliography, which contains not only references to the fundamental scholarly works for a given topic, but also a preliminary discussion on using standard research tools. This discussion covers both traditional and online materials, such as *L'Année Philologique*, journal indices, major journals in English, Greek lexica, encyclopedias (including an evaluation of the reliability of Wikipedia), databases (such as Perseus), and online sources for primary texts in translation.

In sum, the *Historical Dictionary of Ancient Greek Philosophy* offers convenient access to a broad range of considerations that are essential to an historically sensitive study of ancient philosophy. No doubt it would have been especially welcomed and extensively utilized by the present reviewer had it been available when he began studying Greek philosophy in college.

Pseudo-Aristoteles (Pseudo-Alexander), Supplementa Problematorum*: A New Edition of the Greek Text with Introduction and Annotated Translation* edited by S. Kapetanaki and R. W. Sharples

Peripatoi 20. Berlin: Walter de Gruyter, 2006. Pp. 301. ISBN 978–3–11–019140–0. Cloth €82.24

Reviewed by
Andrea Falcon
Concordia University, Montreal
afalcon@alcor.concordia.ca

The *problemata* edited in this volume are traditionally known as *Problemata inedita*. They were first published with this title in 1857 by Bussemaker, who added them to his edition of the *Problemata*.[1] Sophia Kapetanaki and Bob Sharples think that it is anachronistic to continue to refer to these *problemata* as 'unpublished'. Instead, they suggest calling them *Supplementary Problems* (*Supplementa problematorum*). This title avoids any anachronism and at the same time conveys the message that these *problemata* are best understood as an addition or a supplement to the main corpus of Aristotelian problems.

Although Bussemaker included the *Problemata inedita* in an edition of Aristotle, he alerted the reader that his Greek manuscripts were divided in attributing the *problemata* to Aristotle and to Alexander of Aphrodisias. The connection with Alexander is reflected in the indirect tradition as well. Some (but not all) of these *problemata* are transmitted in the Latin translations of Alexander of Aphrodisias' *Problemata* produced by Giorgio Valla (1488)[2] and Theodore

[1] Both the *Problemata* (38 books) and the *Problemata inedita* (3 books) edited by Bussemaker can be found in the Didot edition of Aristotle [see Dübner, Bussemaker, and Heitz 1848–1869, vol. 4].

[2] Alexander Aphrodisiensis, *Problemata* 2.2–62 and 5.1–56 with #61566; *Problemata inedita* 2.60–124 and 2.126–186. I have used a copy of the 1488 edition available for consultation in the Osler Library at McGill University. The Valla translation was reprinted in 1501. Kapetanaki and Sharples have used this reprint, which has book 5 renumbered as book 3.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 52–58

Gaza (1453).[3] Bussemaker did not make much of the connection with Alexander. In fact, he concluded his *praefatio* with a distinction between Aristotelian *problemata* and Alexandrian *problemata* [xviii–xix]. Only two years later, in 1859, the connection with Alexander was given a completely different interpretation by Usener. On the basis of transcripts of the manuscripts entrusted to him by Brandis, Usener produced an edition of the first two books of these *problemata* in which he argued that these two books were a continuation of two extant books of medical puzzles and physical problems attributed to Alexander of Aphrodisias. He made this connection explicit by speaking of Alexander's *Problemata* 3 and 4 [Usener 1859]. Note that an edition of Alexander's *Problemata* 1 and 2—always attributed to Alexander and never to Aristotle—had already been published by Ideler [1841–1842, 1.3–80]. According to Usener, Ideler's books 1 and 2 plus Usener's books 3 and 4 (= Bussemaker's books 1–2) constituted a collection of medical puzzles and physical problems circulating under the name of Alexander of Aphrodisias.

This whole question is revisited by Kapetanaki and Sharples. Their edition not only offers a new and improved text of the *Supplementary Problems*, it also provides a detailed discussion of their textual tradition, including the complicated transmission of books 1 and 2 (Bussemaker's 1–2 = Usener's 3–4). Their edition is based on the collation of 32 Greek manuscripts[4] and a careful study of the indirect tradition. The indirect tradition includes not only the Latin translations by Valla and Gaza of the *problemata* attributed to Alexander of Aphrodisias, but also the eighth century collection of Latin problems attributed to Aristotle and known as *Problemata Bambergensia*,[5] and a paraphrase by Michael Psellus (1018–1097) drawing on the first two books of our *problemata* plus the two books

[3] Alexander Aphrodisiensis, *Problemata* 2.78–135 = *Problemata inedita* 1 and 2.1–38. The Gaza translation can be found in the Juntine edition of Aristotle and Averroes [Giunta 1562–1574, 7.169–204.

[4] Bussemaker based his edition of the *Problemata inedita* on six Greek mss plus the Latin translations of Valla and Gaza. Usener based his edition of Alexander's *Problemata* 3 and 4 on 14 Greek mss plus the Latin translations of Valla and Gaza.

[5] This collection is also known as *Problemata vetustissima*. For the *editio princeps*, see Rose 1863, 665–676.

of *problemata* unanimously attributed to Alexander of Aphrodisias (= Ideler's 1 and 2).[6]

Regarding the attribution of the first two books of the *Supplementary Problems*, Kapetanaki and Sharples have come to the conclusion that 'it cannot be demonstrated that the attribution of books 1 and 2 to Aristotle is secondary and that to Alexander primary' [16]. While there is clear evidence that these books were united into a single collection and were circulating under the name of Alexander of Aphrodisias, their attribution to Aristotle is equally well attested. But there is no reason to suppose, I hasten to add, that these *problemata* are the work of either Aristotle or Alexander. According to Kapetanaki and Sharples, their association with Alexander is no closer and no stronger than their connection with Aristotle. It may well be that 'we are simply dealing with just one more example of the tendency for texts to be ascribed to famous individuals' [27].

Although the processes that led to the formation, arrangement, and even rearrangement of the material that ended up in the *Supplementary Problems* cannot be fully reconstructed, Kapetanaki and Sharples help us to appreciate fully the complexity of these processes. In order to see this, it will be useful to take the second book of the *Supplementary Problems* as a case study. A short preface not only introduces the second part of the book [2.39–192] but also separates it from the first part [2.1–38]. This preface can be taken—and indeed was taken by Bussemaker and Usener—as evidence of a change of source. But it is clear that the short preface, which announces the study of the common symptoms—defined in the preface as the symptoms that can occur at all ages, for instance dizziness—does not prepare the reader for the final zoological section [2.127–192]. It is telling that in a 15th century manuscript [Modena, MS gr. Alpha V7.17 (= K)] this section is introduced with the title *Various Problems and Solutions by the Same Author Concerning the Four-Footed Animals, Book 5*.[7] The author in question is to be understood as Alexander of Aphrodisias and '*Book 5*' is to be taken as an indication that, in this branch of the tradition, the zoological section was

6 The reader will find Psellus' paraphrase in Duffy and O'Meara 1989–1992, 1.241–283.

7 Valla, who used this manuscript as a source for his translation of Alexander's *Problemata*, has the title *Doubts and Solutions about Four-Footed Animals*.

treated as a separate book. Note also that this book belonged to a compilation of *problemata* consisting of *at least* five books.[8] It turns out, however, that one of the *problemata* in this section [2.156] is attributed to 'Aristotle's *aporemata*' in an Oxyrhynchus papyrus of the second century AD [P. Oxy. 2744]. It is certainly significant that this problem existed in the second century AD (or even earlier); but it is even more significant that it existed as part of a collection of *problemata* (or *aporemata*),[9] and that this collection was attributed to Aristotle as early as the second century AD. We cannot rule out that this 'Aristotle' collection was subsequently incorporated into an 'Alexander' collection. But it is clear that other sections of our book circulated as (parts of) 'Aristotle' collections. For one, section 2.1–38 certainly circulated under the name of Aristotle, since a few of these *problemata* can be found, often in an abbreviated form, in the *Problemata Bambergensia* as well as in the Arabic tradition.[10]

This leads to the question of the possible relation between the *Supplementary Problems* and the *Problems* attributed to Aristotle. Kapetanaki and Sharples note that certain sections of the second book of the *Supplementary Problems* have significant parallels with the Aristotelian *Problems*.[11] They explain this overlap by assuming that our material was at least in part excerpted from the Aristotelian *Problems* [8–9]. But they also warn the reader that this explanation cannot be applied to the sections of *Supplementary Problems* that have few or no points of contact with the Aristotelian *Problems* (or, for that matter, with the problems unanimously attributed to Alexander). In fact, they argue that the *problemata* that ended up in our

[8] I stress 'at least' because only three books from this collection can be found in this manuscript (books 1, 2, and 5).

[9] Both 'puzzles' (*aporemata*) and 'problems' (*problemata*) are acceptable titles for a collection of problems. For example, the first book of the *Supplementary Problems* is entitled 'Natural Puzzles and Medical Problems, Selections'.

[10] For the Arabic tradition of the *Supplementary Problems* (still described as *Problemata inedita*), see Flius 2006. Interestingly enough, *Supplementary Problems* 2.1–38 is the only section of the *Supplementary Problems* that was known in the Arabic world.

[11] *Supplementary Problems* 2.39–53 (dizziness), 2.83–97 (voice and hearing), 2.98–104 (smell).

collection cannot be explained solely on the basis of a process of selection from previous sets of problems. Rather, we have to allow for 'the composition of new [sets of] problems...and for the development of collections [of problems] by a process of accretion rather than selection' [11].

The *Supplementary Problems* are not just a collection of *problemata*. The first book begins with a very interesting prologue in which Hippocrates is introduced as a helper that a provident god sent to the human race at a time when it was being destroyed by a succession of diseases. It is not obvious how this prologue is connected to the *Supplementary Problems*. In fact, it is not clear that it was originally written to introduce the first two books (or the first book) of the *Supplementary Problems*.[12] Bussemaker did not include this prologue in his edition of the *Problemata inedita*; but Usener included it in his edition of Alexander's *Problemata* 3 and 4. However, Usener printed the text in square brackets because he thought that this prologue was originally written for a commentary on the *Aphorisms* of Hippocrates. Flashar thinks that our text could have been written as an introduction to our *problemata*. Furthermore, he argues that the language of our prologue betrays Stoic influence and suggests that this text could provide evidence, alongside works such as the Aristotelian *De mundo*, of a Stoic presence in the Peripatetic tradition [see Flashar 1962a and 1962b, 363]. Kapetanaki and Sharples print the prologue in their edition of the *Supplementary Problems*. However, they refrain from telling us whether this text was written for our *problemata*. Here we reach, presumably, the

[12] This prologue cannot be taken to introduce the third book. While books 1 and 2 circulated widely under the name of Aristotle or Alexander—and even under the name of Aristotle *and* Alexander—book 3 is found in only one manuscript [Paris, BN, ancient gr. 2047A (= A)]. Moreover, although this manuscript transmits book 3 along with books 1 and 2, the latter are attributed to Alexander; whereas the former is presented as a separate collection and its *problemata* are not attributed to any specific author. One may even raise the question whether this book should be edited together with books 1 and 2. Kapetanaki and Sharples address this question in their introduction. They acknowledge the lack of connection but tell us that they have decided to include the compilation 'influenced by the similarity in manner between 3.1–45 and 2.39–192 and by the presence in 3.9–29 of a possible Theophrastean connection' [6].

limits of what can be confidently said on the basis of the information in our possession.

Kapetanaki and Sharples print the Greek text of the *Supplementary Problems* alongside an English translation and copious footnotes. The footnotes contain detailed information about parallel texts not only in the extant corpus of *problemata* but also in Aristotle, Theophrastus, and the ancient medical tradition. This information suggests that, while the driving force behind the *Supplementary Problems* may have been intellectual curiosity, this curiosity was guided, and indeed controlled, by general principles of natural philosophy and medical knowledge. The theoretical framework of the *Supplementary Problems* is of a Peripatetic character. The reader will find references in the footnotes not only to Aristotle's zoological works (*Historia animalium*, *De partibus animalium*, *De incessu animalium*, and *De generatione animalium*) but also to Theophrastus. The section on dizziness draws on the *De vertigine* by Theophrastus. But Kapetanaki and Sharples suspect that more Theophrastean material from works that we no longer possess may be present in the *Supplementary Problems.*[13]

By focusing on the *Supplementary Problems*, Kapetanaki and Sharples have given us a model for any future study of the history of a philosophical genre which originated in the Peripatetic tradition, and indeed with Aristotle, but which remained popular well beyond the boundaries of antiquity. Scholars working on the *fortuna* of this genre in antiquity and beyond will find this very fine book of great help in the context of their own research.

BIBLIOGRAPHY

Dübner, F.; Bussemaker, U. C.; and Heitz, E. 1848–1869. edd. *Aristotelis opera omnia, graece et latine.* Paris.

Duffy, J. M. and O'Meara, D. 1989–1992. *Michaelis Pselli philosophica minora.* 2 vols. Leipzig.

---

[13] For a discussion of the possible Theophrastean origins of the section of nails and hair [2.54–82], excretion and purgation [2.105–126] and wine and olive-oil [3.9–29], I refer the reader to Sharples 2004.

Flashar, H. 1962a. 'Beiträge zur Spätantiken Hippocratesdeutung'. *Hermes* 90:402–418.

——— 1962b. *Problemata Physica.* Berlin.

Flius, L. S. 2006. 'The Genre *Problemata* in Arabic'. Pp. 44–52 in P. De Leemans and M. Goyens edd. *Aristotle's* Problemata *in Different Times and Tongues.* Leuven.

Giunta, L. 1562–1574. *Aristotelis opera cum Averrois commentariis.* 9 vols. and 3 supplements. Venice. Repr. Frankfurt a.M., 1962.

Ideler, J. L. 1841–1842. *Physici et medici graeci minores.* 2 vols. Berlin. Repr. Amsterdam, 1963.

Rose, V. 1863. *Aristoteles pseudepigraphus.* Berlin. Repr. Hildesheim/New York, 1963.

Sharples, R. W. 2004. 'Evidence for Theophrastus *On Hair*, *On Secretion*, *On Wine and Olive-Oil*?' *Bulletin of the Institute of Classical Studies* 47:141–151.

Usener, H. 1859. *Alexandri Aphrodisiensis quae feruntur problematorum libri 3 et 4.* Berlin.

Galileo's Glassworks: The Telescope and the Mirror by Eileen Reeves

Cambridge, MA/London: Harvard University Press, 2008. Pp. 231.
ISBN 978–0–674–02667–4. Cloth $21.95

Reviewed by
A. Mark Smith
University of Missouri
smitham@missouri.edu

Precisely when it occurred to someone to insert a convex lens into one end of a tube and a concave lens into the other to form the first refracting telescope is anyone's guess. We are considerably more certain about when the first attempt to patent such a device was made. On October 2 in 1608, the States-General of the Dutch Republic discussed and eventually denied the patent-application that Hans Lipperhey, a spectacle-maker from Middelburg in Zeeland, had previously submitted to his provincial council. Since this is the first documented evidence of the refracting—or 'Dutch'—telescope, Lipperhey has been credited with its invention, even though there are at least two, perhaps as many as four, competing claims from around the same time.

News of the device spread quickly and so, evidently, did the device itself: a refracting telescope with a cracked lens was on offer at the Frankfurt Fair in autumn of 1608. By late November of the same year, Galileo's friend and supporter, Paolo Sarpi, had got wind of the invention at Venice; and within a matter of months the Dutch telescope had become fairly commonplace as more and more samples of the device were presented to various aristocrats by men seeking patronage or preferment. Indeed, by April 1609, the Dutch telescope was commonplace enough to be commercially available in Paris. Anyone with a keen interest in optics and optical devices should therefore have known of the Dutch telescope by no later than the beginning of 1609.

Galileo was certainly among this group; yet in the introductory portion of his *Starry Messenger*, which was published in March 1610, he claims that he had only learned of the device some 10 months earlier, presumably no earlier than May of 1609. Why it took him

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)

so long to get the news is puzzling for several reasons. First, there is good reason to believe that Galileo had become deeply interested in optics well before 1609 and that his interest was focused on how best to magnify distant objects for close scrutiny. Second, when it came to things scientific, Galileo had his ear continually to the ground through a network of correspondents and friends. Third, and most puzzling, one of those friends was none other than the Venetian, Paolo Sarpi, who already knew of the Dutch telescope by November of 1608. How could he not have shared that news with Galileo, who was ensconced in Padua, just a stone's throw from Venice and under Venetian rule at the time?

According to Reeves, the unaccountable lag between the invention of the Dutch telescope and Galileo's first intimation of its existence is not unaccountable at all. Galileo, she surmises, probably did hear of the device before May of 1609, but until that time he misunderstood what he had heard. That misunderstanding was based in part on the vagueness with which the instrument was initially described to Galileo and in part on his expectation that it would consist of some sort of mirror-lens combination. Galileo thus had no idea that the Dutch telescope consisted entirely of lenses until May or June of 1609; and only then, when he finally realized his mistake, was he able to embark on the path that led to the publication of the *Starry Messenger* in March 1610.

Why was Galileo fixated on a mirror-lens telescopic device before learning of the actual composition of the Dutch telescope? The delightfully shandean story that Reeves offers in response takes us back to the legendary Pharos of Alexandria and the miraculous concave mirror mounted at its top. This mirror had such magnifying power that it could reveal ships at sea 500 miles away. Not only that, but it could focus sunlight on distant, hostile fleets with such concentration as to burn them long before they reached the harbor. With all its supernatural powers, this legendary mirror apparently served as the model for a host of subsequent fictional spy-mirrors, such as the one supposedly deployed by Caesar to scope out Britain from across the English Channel or the Chinese version described in the late 12th-century *Letter of Prester John.* Through the proliferation of such legends, the powerful spy-mirror, often given exotic origins and magical powers, had become a cultural trope in later Medieval and Renaissance Europe.

While late-Renaissance Europeans were lost in wonder at the marvelous capabilities of such legendary mirrors, the more mundane, yet still wondrous, capabilities of real concave spherical mirrors were capturing the interest of certain theorists and instrument-makers during the second half of the 16th century. Such interest may well have been piqued by improvements in the technology of mirror-making, although it is unclear that such improvements extended to the formation of concave mirrors. Whatever the case, by the second half of the 16th century, the optical focal properties of concave spherical mirrors had been recognized and exploited to enhance real images projected inside the *camera obscura.* Giambattista della Porta had even suggested the addition of a convex lens at the aperture. Moreover, plane mirrors had proved exceptionally useful in surveying devices, so it stood to reason that, if properly formed and deployed, concave mirrors might prove equally useful for surveying at great distances.

The chase was therefore on. Some researchers concentrated on formation, seeking to perfect the curvature of their mirrors while extending focal length as far as feasible in the hope of improving both magnification and image-clarity. Others concentrated on deployment, adding a convex sighting-lens to produce what is essentially a reflecting telescope. Although these efforts failed to yield satisfactory results, many researchers exaggerated the effectiveness of their particular device in the hope of attracting a wealthy patron, often publishing accounts extolling the merits of their invention while providing tantalizingly vague technical explanations of design and implementation. Perfection always seemed to be just around the corner. The long and short of it, according to Reeves, is that the mainstream of telescopic research at the beginning of 1609 was focused on concave spherical mirrors, either by themselves or in combination with convex lenses. And Galileo fell squarely within this mainstream until he finally learned in May or June of the actual composition of the Dutch telescope and reconfigured his research-program accordingly. Moreover, as Reeves points out in chapter 5 (‘The Afterlife of a Legend’), Galileo’s successful deployment of the Dutch telescope did not immediately put paid to the promise of an effective mirror-based telescope. After all, some, like Giovanni Magini, had a vested interest in such a device and were eager to protect that interest not only by extolling the merits of their alternative but also by deprecating Galileo's lens-based telescope and its observational results. Vicious priority-disputes also blossomed, as claims and counter-claims to originality were staked and defended, often on an *ad hominem* basis. Adding to the confusion were clashes over which 'nation' should get final credit for the invention.

I have no doubt that Reeves would be the first to admit that her explanation of why Galileo was so laggard in grasping the construction of the refracting telescope is plausible but not definitive. That, however, is somewhat beside the point. What makes this book so compelling (and so much fun) is the way Reeves embeds that explanation in her account of the social and cultural context of the refracting telescope's invention and dissemination. The result is a lively tale of seductive ideas, false hopes, serendipity, overweening ambition, partisan squabbling, astounding credulity, knaves, fools, and *agents provocateurs*—a story, in short, that is all too human.

Islamic Science and the Making of the European Renaissance by George Saliba

Cambridge, MA/London: MIT Press, 2007. Pp. xi+315. ISBN 0–262–19557–7. Cloth $40.00, £24.95

Reviewed by
Michael H. Shank
University of Wisconsin-Madison
mhshank@wisc.edu

The ambitions of this book are to rewrite the historiography of Islamic science in light of recent research and to transform our understanding of its relation to Copernicus' Sun-centered theory and the subsequent growth of European science. Although Saliba occasionally discusses other topics (e.g., medicine, mechanics, optics, instrumentation), his primary case study is astronomy, which he takes to be paradigmatic for Islamic science as a whole—as the first two words of his title indicate. The 'and' that follows conceals one of the most tantalizing cross-cultural questions in the history of late medieval and early modern science: How did Copernicus learn about the geocentric astronomical models from 13th-century Maragha and 14th-century Damascus that he recycled as heliocentric ones in his *De revolutionibus*? And this specific question opens up nothing less than the high-stakes problem of modern 'Western' science itself, which is often fathered on Copernicus. Throughout the book, Saliba's argument has a notable 'science and society' component, as he seeks to anchor his explanations in societal needs. Even when he tackles detailed questions, he is thinking about their place in a narrative of *longue durée* associated with the rise and decline of various scientific cultures and their interactions, up to and including the present.

Saliba self-consciously adopts the *persona* of an *agent provocateur* in the best sense of the term, a role that he performs admirably and with obvious relish (he refers to 'my follies' in the preface). His book will certainly be controversial among specialists in Islamic science, as Saliba takes on many of his colleagues. Just as clearly, he is also intent on reaching a wide audience, including readers who are approaching these issues for the first time. For the most part,

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 63–72

Saliba has succeeded in offering an accessible narrative (including a nice summary of Ptolemy's models, for example) and in keeping to a minimum the sections that will be difficult for the proverbial 'general readers'. Thankfully, the hidden scholarly barbs and controversies will largely be subliminal for this audience, which is likely to take away a valuable picture of Islamic science as told by one of the leading participants in revising it.

Saliba's revisionism proceeds in opposition to a 'classic story' that lumps together a century of scholarship and interpretation. As outlined in chapter 1 ('Questions of Beginning I'), this classic narrative may be summarized as follows. Islamic science, seen largely as a body of ideas, emerges under the early ᶜAbbāsid caliphate in the late eighth and ninth centuries, thanks to a translation movement that is either left unexplained or traced to Byzantine or Persian stimuli. Building on this newly translated Greek material, Islamic science thrives between the ninth and 11th centuries. Following this brief flowering, Islamic science declines after either the 11th century for internal reasons (symptom: al-Ghazālī's *Incoherence of the Philosophers*) or the mid-13th century for political ones (when the Mongols bring the ᶜAbbāsid caliphate to an end). The grand narrative of the history of science then moves on to Europe.

Saliba's targets here are many—from Roland Carra de Vaux to our own contemporaries, from Toby Huff's widely criticized nonspecialist meta-argument for European exceptionalism to such canonical specialist theses as A. I. Sabra's association of decline with the 'naturalization' of the Greek scientific tradition and Dimitri Gutas' argument for ᶜAbbāsid emulation of Persian philhellenism. To call these historians 'strange bedfellows' would be an injustice, as some do not even fit in the same room. Fitting all these accounts into a single composite 'classic story' requires simplifications that, however useful pedagogically and polemically, sometimes verge on oversimplification, as Saliba himself is no doubt aware. This foil allows Saliba to set up his revisionist alternative in 'Questions of Beginning II', which pushes the beginning of Islamic science/astronomy back in time, extends its heyday into the 16th century, and adopts an explanatory framework based on societal and competitive needs.

With respect to the translation movement from Greek and Syriac, Saliba believes that the 'classical narrative' is at best wrong and at worst based on 'essentialist features of Islamic religion' or 'the racial composition of early Islamic society' [71]. Saliba reproduces and translates some of the canonical early sources that address the emergence of Arabic science. His detailed exegesis of them in effect makes them out to be myths that historians have trusted uncritically instead of seeing in them the 'legendary' expressions of self-interested parties [e.g., 49].

As an alternative to these accounts, Saliba argues that the translations began well before the ᶜAbbāsid revolution, and for practical reasons. The first reported translation turned a Greek alchemical text into Arabic, an event that Saliba reads as motivated by contemporary political issues associated with the minting of coins. He suggests that subsequent translations under both the Umayyads and the ᶜAbbāsids were driven by competition, most notably for courtly attention, between the Arabic-language specialists and the multi-lingual scholars interested in the philosophical ideas of the Greeks. The word 'competition' recurs often to explain the motivations of leading protagonists.

In the lengthy chapter 3 ('Encounter with the Greek Scientific Tradition'), Saliba surveys the translation movement from Greek and Syriac into Arabic, which he starts in the Umayyad period and frames as a continuously critical enterprise. On his account, that critique engages neither the Byzantine nor the Sassanian scientific traditions. He therefore argues that the latter are effectively irrelevant to Arabic developments, which were stimulated by unmediated interaction with the Greek classical tradition (his evidence includes the correction of long-term parameters from Greek rather than other sources).

Saliba minimizes the extent to which either the Byzantines or the Persians could have sparked, directly or indirectly, an Arabic interest in Greek materials. This part of Saliba's argument has the kind of classic 'Renaissance' structure that makes a medievalist like myself wince. Just as the traditional Renaissance was a revival of classic materials that owed nothing to the preceding millennium, so Saliba's translation movement reaches back to the classical Greek corpus, untainted by Byzantium or Persia. Looking at a historical map, however, one wonder how the Islamic empires could possibly have swallowed cities like Damascus, Alexandria, Nishapur, and many others while remaining aloof from Byzantine and Persian culture.

The sophistication of al-Hajjāj b. Mattar's first known translation of the *Almagest* (829) is an important exhibit in Saliba's argument for an Umayyad translation movement. To explain the excellence of this work, terminologically and otherwise, Saliba postulates that 'several generations of earlier translators of elementary sciences must have paved the way' [83]. That this translation is more sophisticated than some later ones poses a problem for Saliba's argument. If, starting from al-Hajjāj's translation, one ranks extant *Almagest* translations according to the criterion of sophistication, one does not get a slowly ascending chronological order. On the contrary, the most sophisticated one comes first. So, in the absence of any evidence for earlier translations, where is the warrant for postulating a slowly ascending terminological sophistication *before* al-Hajjāj's translation?

Likewise, Saliba deems it improbable that Hajjāj coined the new technical terms found in his translation, of which he gives only three examples ('apogee', 'perigee', and 'horizon'), which do not seem too onerous for one person. Saliba nevertheless postulates piecemeal coinages by many individuals over several generations, thus hypothetically pushing the translations back. Yet he offers no evidence for the early period of lexical competition that one would expect as multiple coinages sorted themselves out. This phenomenon is well attested in Greek medical terminology, as multiple Hellenistic 'coiners' vied for supremacy, a process discussed in G. E. R. Lloyd's *Ambitions of Curiosity* [2002] and also evident in 12th-century Latin astronomical translations. Conversely, there is good evidence that Nicole Oresme singlehandedly coined more than 100 Middle French astronomical and philosophical terms in his late 14th-century *Livre du ciel et du monde*, a vernacular translation of and commentary on Aristotle's *De caelo*. Why, then, should it be implausible to credit al-Hajjāj and his circle with the leap forward?

Chapter 4 ('Islamic Astronomy Defines Itself: The Critical Innovations') outlines the characteristic features of Arabic astronomy—its high technical competence at the quantitative and observational levels as well as its concern for consistency between these results and 'the cosmological presuppositions of the universe' [167]. Here Saliba sees a fundamental inconsistency between Ptolemy's *Almagest* and his *Planetary Hypotheses*, works that he reads—problematically, to my mind—as describing 'a universe completely composed of Aristotelian spheres' [134]. On this account, the *Almagest* sought consistency with Aristotle (‘his guiding cosmology’ [138]), but failed to attain it, most notably in the invention of the equant, a problem that would also engross Copernicus (words like ‘absurdities’ occur repeatedly in these pages).

The polarity between Ptolemaic astronomy and Aristotelian cosmology pervades Saliba’s account of Ptolemy. By Aristotelian cosmology, however, he often seems to mean no more than the requirement of using uniform circular motions when constructing planetary models. It is not clear to me that, in aspiring to uniform circular motions, Ptolemy was in fact trying to conform to peculiarly Aristotelian principles or to be an orthodox Peripatetic. It is, therefore, far from clear that Ptolemy overlooked or remained deliberately silent about the ‘other Aristotelian conditions’ [138] to which his supposed allegiance demanded adherence, as if he were trying to trick his readers by sneaking past them. The overall direction of Saliba’s argument is that Arabic astronomy was so deeply critical of Ptolemy’s *Almagest* that it in effect constituted an anti-Ptolemaic revolution.

In chapter 5 (‘Science Between Philosophy and Religion: The Case of Astronomy’), Saliba contends that the fundamental premises of Islam gave Islamic astronomy a cast fundamentally different from that of its Greek predecessor, despite the similarities between the two. Different societies have different needs: ‘societal forces . . . required new disciplines to be created’ [171]. Among these were *ᶜilm al-hay’a*, the new discipline concerned with the ‘configuration’ of the universe, which salvaged the palatable aspects of Aristotelian cosmology while setting aside its controversial aspects (especially astrology). This selectivity meant that *hay’a* could enjoy broad support in political circles and among religious scholars, and interact fruitfully with both philosophy and religion. Although he mentions occasional tensions with religion at the fuzzy interface between astronomy and astrology, Saliba argues that Islamic civilization does not evidence what he calls ‘the European paradigm of conflict between science and religion’ [191]. Saliba’s contrast, however, works only if the Galileo Affair is indeed paradigmatic of the European situation, a point that an increasing number of scholars doubt.

Given his overall goal, Saliba’s choice of astronomy as the archetypical science is a clever one, for it is Copernican astronomy that opens the classic narrative of the Scientific Revolution and the beginning of modern science. Chapter 6 (‘The Copernican Connection’) summarizes the scholarship of the last 50 years on the striking parallels between Copernicus' heliocentric models in his *Commentariolus* and *De revolutionibus*, on the one hand, and various earth centered 13th- and 14th-century Islamic models (notably those of Tūsī, Urdi, and Ibn al-Shātir). Since convincing connections have eluded historians thus far, the order of the day remains hypotheses about the way in which these models may have reached Copernicus. Although he alludes to the standard hypothesis of a Byzantine route of transmission (that is, a westward migration of Greek manuscripts containing summaries or translations of Islamic astronomy), Saliba suggests that translation into Greek or Latin may in fact have been superfluous. He points to a small cohort of 15th- and 16th-century Europeans who read Arabic, from Andrea Alpaga to Guillaume Postel and others. To support this hypothesis, Saliba is perhaps too eager to inflate manifold the size of this group, on the grounds that 'one has to conclude' that the 1000 Arabic copies of Tūsī's *Euclid* sold by the Medici Oriental Press went to a domestic market [228–229]. The necessity of this inference is far from obvious, however; in the face of such contingency, some evidence would be nice.

Even so, the historical interest of even a small number of European Arabists remains considerable, all the more so as some had astronomical interests. But improving the statistics by multiplying the known handful by 100 or more will not satisfactorily answer the question, What did that one man Copernicus read or see, and when? Here we must mind the law of small numbers. I can no more infer reliably from box office statistics what movies Saliba has seen than he can infer from 1000 putative European Arabists that Copernicus saw Ibn al-Shātir's models thanks to one of them. Only careful detective work into Copernicus' various circles and contexts will answer that question.

Another prong of Saliba's argument in this chapter is motivational. It consists in attempting to show that Latin astronomy had no internal reasons for criticizing Ptolemy; hence, the problems raised by a critique and the solution to them must have come from elsewhere (that is, from Islamic civilization). Here Saliba uses highly schematic stereotypes of the Renaissance to magnify the differences between Copernicus and his own context:

> [Maraghan astronomical works were] written expressly to counter Greek astronomical thought rather than to preserve

> it. So why would any Renaissance scientist be interested in them, if the purpose of the Renaissance intellectual project was the recovery of the sources of classical Greco-Roman antiquity as we are so often told? [211]

Saliba also refers here to the 'rupture of the Aristotelian universe by the Tūsī couple... [which could] now demonstrate that circular motion could produce linear motion and vice versa' [213]. In this chapter and in these quotations, we can glimpse the *telos* of Saliba's portrayal of Islamic astronomy as revolutionary. Since, by definition, Copernicus' revolution cannot have grown out of an antiquity-worshipping movement, the stimulus must have been external [e.g., 232].

This is a provocative argument on which the law of small numbers once again casts doubts. Even if the latter did not apply, one would have to be very cautious. Despite the stereotypes about the Middle Ages and the Renaissance, the Latin natural philosophy and some of the astronomy of the 14th and 15th centuries do much more than rehash Aristotle uncritically. Years ago, Claudia Kren [1971] pointed out that Nicole Oresme's commentary on Sacrobosco's *Sphere* effectively describes a Tusi couple to make precisely Saliba's point about the mutual production of circular and rectilinear motions. Moreover, there is no good evidence that astronomers like Regiomontanus in the generation before Copernicus valued antiquity over what they took to be the truth about the heavens. If these individuals were stereotypical humanists, this judgment is best reached inductively rather than deductively.

Not least (and here graduate students should pay attention), Saliba seems to assume that we already know what there is to know about the context and background of Copernicus. This is so far from being the case that, although Europe had dozens of universities, even historians of astronomy would be hard pressed to name ten 15th-century Latin astronomers, to say nothing of characterizing their work. Giovanni Bianchini, the leading astronomer of Italy in the 15th century and a student of the *Almagest*, has no entry in the *Dictionary of Scientific Biography*, while the 600-odd folios of controversy about the *Almagest* between George of Trebizond and Johannes Regiomontanus (1450s–1470s) have yet to receive sustained attention. Counterintuitive though it may seem, Latin astronomy in the 15th century has yet to be mapped, both in general and in detail.

The book's concluding chapter 7 ('The Age of Decline') challenges earlier chronologies of decline in Islamic astronomy specifically, and in Islamic science generally. Simply put, some of the best work in Islamic astronomy falls between the 13th and 16th centuries, squarely after both al-Ghazālī and the Mongol conquests, the two leading and competing benchmarks for earlier accounts of the decline of Islamic science. Saliba thus postpones the 'age of decline' to the later 16th century for reasons far more fundamental than science, as we shall see. Here it is important to notice that astronomy has once again become normative. Indeed, apart from its cosmological portions, Saliba has relatively little to say about the fortunes of the vast enterprise of natural philosophy in Islamic civilization, except as it relates to astronomy. Here one suspects that the trend represented by al-Ghazālī may be more important to the overall story than Saliba allows. What are the reasons for such suspicion? Ghazālī alone would seem to count against Saliba's claim that astronomy and natural philosophy follow the same trajectory and chronology, since Ghazālī himself treated the two endeavors very differently, allowing the one while being suspicious of the other. Saliba's decision to make astronomy the paradigm and to generalize from it leads to interesting questions, but it is does not address directly A. I. Sabra's broader 'decline thesis', which, as I read it, concerns the scientific enterprise as a whole, including the full range of natural philosophy. Indeed, Sabra was writing in full awareness of the significant astronomical developments between the 13th and 15th centuries, from Maragha to Samarkand.

Saliba's reflections on the problem of decline remain valuable and make several points that bear on the wider historiography of science. The first is his general definition of scientific decline as 'an age in which a civilization begins to be a consumer of scientific ideas rather than a producer of them' [248]. The second, using this definition, is his rejection of the commentary genre as a traditional symptom of decline. This problem is not unique to Islamic historiography: it surfaces in discussions of the decline of Greek science and is implicit in the stereotypical image of the Latin Middle Ages as an era of perpetual stagnation (commentaries as far as the eye can see).

Scholars who work on commentaries will easily agree with Saliba that the genre was in fact a leading medium for the production of new ideas, and played a role analogous to that of specialized periodical literature today. One could extend this fruitful insight by noting that to write a commentary or a super-commentary on a specialized text is in effect to appeal directly to the specialized audience interested in the original text. Saliba's insights into the commentary suggest that his definition of decline may require revision. Despite its *prima facie* plausibility, it undersells the consumption of scientific ideas, a rubric that arguably encompasses both translation movements and education. It is hard to see how a high consumption of the best such ideas, even in the absence of much new production, could constitute an unalloyed scientific decline.

At the end of his last chapter, Saliba turns to the problem of modernity and the role of science in it. He notes that the discovery of the New World coincides with the division of the Muslim world into three empires (Ottoman, Safavid, and Mughal), events which, he argues, rerouted trade to the west, thereby gradually cutting out the Islamic world and setting it on the path of economic decline. Europe, in contrast, pulled in new wealth, drew on slave labor, and thrived economically. For Saliba, it is not a coincidence that royal and princely houses channeled some of this wealth into new institutions—scientific academies and societies—that he sees as responsible for the Scientific Revolution (note, however, that the Royal Society was royal only in name, not in munificence). In short, Saliba advances the grand thesis that

> the major scientific developments in Europe during the 16th and 17th centuries were the product of this dynamic cycle of wealth, mostly initiated by the 'discovery' of the New World. [253]

Overall, Saliba's book is certain to be an influential one, whether it conjures up support, opposition, or—more likely—a complex blend of the two. While it offers vast prospects on more than eight centuries of astronomy in Islamic civilization, it also advances bold explanations for these developments. Saliba deserves our gratitude for raising to a new level the debates about this central episode in world history.

BIBLIOGRAPHY

Kren, C. 1971. 'The Rolling Device of Nasir al-Din al-Tusi in the *De spera* of Nicole Oresme'. *Isis* 62:490–498.

Lloyd, G. E. R. 2002. *The Ambitions of Curiosity: Understanding the World in Ancient Greece and China.* Cambridge/New York.

Diogène d'Apollonie. Edition, traduction et commentaire des fragments et témoignages. Deuxième édition revue et augmentée by André Laks

International Pre-Platonic Studies, 6. Sankt Augustin: Academia Verlag, 2008. Pp. 336. ISBN 978–3–89665–440–3. Cloth € 44.50.

Reviewed by
David Sider
New York University
david.sider@nyu.edu

The first edition of André Laks' book (his Lille dissertation) in 1983 was welcomed by all students of the Presocratics, and was generally reviewed favorably, although only once in English.[1] Long out of print, it now appears in a second edition (having lost the puzzling but attractive Jean-Cocteau-like line drawing on its cover) in the series International Pre-Platonic Studies, which has already published updated versions of Marcovich's edition of *Heraclitus* [2001] and my *Anaxagoras* [2005], as well as a reprint of Diels' *Parmenides. Lehrgedicht* [2003] and original monographs on (so far) Gorgias [Robbiano 2006] and Parmenides [Mazzara 1999]. No surprise to those who know the first edition or to those who know only his later work: Laks' text and commentary remains and will remain for years to come the best study of Diogenes of Apollonia, whose interesting teleology is often overlooked and who is often gently damned for being eclectic. The primary purpose of this review, therefore, is merely to acknowledge its publication and to record some changes between the two editions.

Laks has dropped, as too naïve, his original subtitle, *La dernière cosmologie présocratique*, although Theophrastus *apud Simplicium* (if it is not Simplicius himself) said much this very thing of Diogenes

[1] By George Kerferd [1990]. The other reviewers were Schwabl [1984], des Places [1984], Longo [1984], Duvernoy [1985], Romeyer-Dherbey [1985], Pasqua [1986], Janda [1987], and de Sousa Barbosa [1988]. Of those that I have seen, only Oddone Longo's is somewhat unfavorable, criticizing Laks for an introduction that is 'mediata, ma forse non sufficientemente sviluppata' and for a commentary that is too 'tradizionale'.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 73–79

[T4 Laks = Theophrastus fr. 226a in Fortenbaugh, Huby, Sharples, and Gutas 1992].[2] Between editions, however, he has given much thought to the nature and characteristics of Greek philosophy before Plato; note, e.g., the scare guillemets in the title of his *Introduction à la « philosophie présocratique »* [2006], and the question mark in the proceedings of a conference that he organized in Lille, 'Qu'est-ce que la philosophie présocratique?' (Villeneuve-d'Ascq, 2002).[3] Little of this has been imported into the second edition, though: 'des formulations ont été changées,' but for the most part, these changes are more of emphasis. There are also additions to the original text, the introduction being particularly rich in additional thoughts, which are set off with double asterisks (**. . . **).

Most of the changes occur in the introduction and the additional notes, sometimes simply by moving from the latter to the former; thus, the assemblage of the texts from Simplicius, who not only, as often, is our main source of *ipsissima verba* (having explicitly said that Diogenes' book *On Nature* has 'come down to him,' an odd phrase), but who also embeds these quotations into longish passages on Diogenes.[4] Likewise, the chapter on Diogenes' writings (how many, what titles?) has been moved to the front. Two new chapters have been added: 'La question de l' influence de Diogéne' and 'Diogenes revisited,' which is Laks' (English) contribution to Curd and Graham 2008 and which provides an *entrée* into Laks' book for those more comfortable in English than French.

Laks has kept his rearrangement of the fragments (whose text is dependent on earlier editors, chiefly Diels' edition of Simplicius). This rearrangement differs somewhat from Diels' in properly downgrading B9 [see Diels and Kranz 1951, §64] to testimony level and adding three one- or two-word fragments embedded in Simplicius' and Theophrastus' discussions of Diogenes:

[2] Fortenbaugh, Huby, Sharples, and Gutas 1992 should be added to Laks' bibliography for those less familiar with Theophrastus. Laks' thorough commentary on this testimony foreshadows his important later work on Theophrastus.

[3] The proceedings are pubished in Laks and Louguet 2002.

[4] Some readers will want to note they have been translated into English in Guthrie's chapter on Diogenes [1965].

frr. 2 σοφισταί
3 πολλὴ νόησις
11 διασκίδνασθαι

all of which Diels had already put within quote marks. Fr. 2 σοφισταί and fr. 3 πολλὴ νόησις deserve this new status, the former also its low number, although the latter need not have come so early in Diogenes' work as Laks argues. I am less convinced by fr. 11 διασκίδνασθαι, which is not quite as purely Ionic as Laks argues [cf. Thucydides, *Hist.* 6.98.4, Euripides, *Hec.* 917, [Aristotle] *Prob.* 933a31, 943b7].

In the longer fragments there are some few places where one can disagree or prefer another way of explaining things; e.g., B7 Diels = Fr. 5 Laks καὶ αὐτὸ μὲν τοῦτο καὶ ἀίδιον καὶ ἀθάνατον σῶμα, τῷ δὲ τὰ μὲν γίνεται, τὰ δὲ ἀπολείπει. Laks may be right to maintain τῷ Simplicius^{DE} (τό Simplicius^{F}) against the Aldine's τῶν (approved by Diels), but is he right to regard its reference as αὐτό? ὁ δέ even without a preceding μέν, almost always signals a new reference.[5] Laks' vague reference to 'l'emploi épique' [see, e.g., Homer, *Il.* 2.188] is insufficient to justify taking τῷ δὲ to have the same reference in such a prosaic prose author as Diogenes. Yet, after resisting Laks (who, like Guthrie, follows Diels in his edition of Simplicius) for a while, I now think that he is right and that the way to explain this is not so much by recourse to standard Greek, but to take full account of Diogenes' notably primitive prose style. Here, first, as he does elsewhere, he oddly employs a neuter pronoun to refer to ὁ ἀήρ; but, more important for the fragment's meaning, the contrast lies not in αὐτὸ and τῷ having distinct references, but rather between the unchanging 'selfness' of the former and the role it plays in bringing about (apparent) change, as shown by the dative case of the latter. One might also note in support Simplicius' θαυμαστόν in introducing this fragment, which probably is occasioned by his amazement that *aer* could have the characteristics stated in both μέν and δέ clauses.

I agree with Laks and others who place Diogenes after Anaxagoras—the former's νόησις does indeed look like a conscious advance on the latter's νοῦς—but when it comes to the writing of Greek, one

[5] See Ruigh 1971, §§130–132, which surveys usages of δέ in prose as well as poetry. There is much fascinating stuff in this modestly titled book.

would think the order was reversed. Indeed, Diogenes' use of repetition (*kyklos*), exegetical *καί* and hendiadys, explanatory apposition, and amphiboly is both frustrating, fascinating, and a challenge to the reader. This can best be illustrated by B2 Diels = Fr. 4 Laks, where attention to style leads me to disagree slightly with Laks' *plan du fragment*. What follows is largely his text; but the slashes (/) indicate where I would add commas, the setting of one phrase in parentheses is mine, and I have added superscripts to facilitate reference. Letters in boldface are Laks', to indicate the logical division of the argument: **H/h**(ypothesis = protasis) and **C**(onsequence = apodosis).

> ἐμοὶ δὲ δοκεῖ/ τὸ μὲν ξύμπαν εἰπεῖν/ πάντα τὰ ὄντα ἀπὸ τοῦ αὐτοῦ ἑτεροιοῦσθαι[a] καὶ τὸ αὐτὸ εἶναι. καὶ τοῦτο εὔδηλον· **H, h1** εἰ[1] γὰρ τὰ ἐν τῷδε τῷ κόσμῳ ἐόντα νῦν, γῆ καὶ ὕδωρ καὶ ἀὴρ καὶ πῦρ καὶ τὰ ἄλλα ὅσα φαίνεται ἐν τῷδε τῷ κόσμῳ ἐόντα, εἰ[2] τούτων τι ἦν ἕτερον[b] τοῦ ἑτέρου[b] (ἕτερον[b] ὂν τῇ ἰδίᾳ φύσει), **h2** καὶ/ μὴ τὸ αὐτὸ ἐὸν/ μετέπιπτε πολλαχῶς καὶ ἡτεροιοῦτο[b], **C** οὐδαμῇ οὔτε μίσγεσθαι ἀλλήλοις ἠδύνατο, οὔτε ὠφέλησις τῷ ἑτέρῳ <γενέσθαι ἀπὸ τοῦ ἑτέρου> οὔτε βλάβη, **C*** οὐδ' ἂν οὔτε φυτὸν ἐκ τῆς γῆς φῦναι οὔτε ζῷον οὔτε ἄλλο γενέσθαι οὐδέν, **H*** εἰ[3] μὴ οὕτω συνίστατο ὥστε ταὐτὸ εἶναι. ἀλλὰ πάντα ταῦτα ἐκ τοῦ αὐτοῦ ἑτεροιούμενα[a] ἄλλοτε ἀλλοῖα γίνεται καὶ εἰς τὸ αὐτὸ ἀναχωρεῖ.
>
> ἑτερ-[a] = (only) apparent change/difference (possible)
> ἑτερ-[b] = real change/difference (impossible)

TRANSLATION

(without angle brackets and with additional explanations)

> To speak of the whole matter, it seems to me that all existing things change[a] from the same thing and are the same. And this is quite clear: **H, h1** for if[1] the things now existing in this cosmos (earth, water, aer, fire, and all the other things that seem to be existing [i.e., *scil.* εἶναι]—if[2] one of these were truly different[b], one from the other[b] (being truly different[b] in its own nature), **h2** and, were it not the same, it changed and altered[b] in many ways, then **C** in no way would mixture of one with the other or mutual benefit or harm be possible, nor **C*** could a plant grow from the earth nor an animal

> or anything (grow), unless **H*** it were so constituted as for (everything) to be the same. But (in fact), all these things are changed[a] from the same thing and come to be different things at different times and return to the same thing.

Diogenes' prose proceeds in fits and starts, constantly going back on itself and clarifying, in part to acknowledge its ambiguous use of 'change'. Its model for both thought and style here seems to be Melissus B8 [see Diels and Kranz 1951, §30]. As the superscripts indicate, sometimes the stem ἑτερ- indicates real change (ἕτερον[b] ὂν τῇ ἰδίᾳ φύσει), other times only the false change that has led people, like Empedocles, to think that earth, air, fire, and water are absolutely different. **h1** begins with a general statement, εἰ[1], and then, before a verb appears, backtracks to apply itself to individual items, εἰ[2] (so far, largely Laks), which contains two clauses, the second of which contains its own subordinate protasis in the form of a negated conditional particle, μὴ τὸ αὐτὸ ἐόν, which does nothing but rephrase the first part of the protasis. Laks' rendering seems to miss this last point: 'et qu'il ne fût pas vrai que, étant le même, elle se transforme . . . ,' which seems to misplace the negative. (μετέπιπτε [as in Melissus B8] πολλαχῶς καὶ ἑτεροιοῦτο is a hendiadys.)

Given this manner of composition, I see no reason to follow Laks (who here follows Diels and Schneidewin) in adding logical clarity and syntactic regularity in the form of the clause inserted in angle brackets; Diogenes himself inserted this clause in his usual fashion in order to clarify the preceding one, in which ἀλλήλοις was probably thought sufficient to justify the use of only one instance of ἑτερ- in the next. οὔτε ὠφέλησις . . . οὔτε βλάβη should be taken as a polar expression roughly equivalent to what later philosophers would term πάθος.

Nor is there anything wrong with the syntax: μίσγεσθαι, ὠφέλησις, and βλάβη are all subjects of the verb [cf. Homer, *Il.* 10.173 ἢ ὄλεθρος Ἀχαιοῖς ἠὲ βιῶναι, Kühner and Gerth 1890–1904, 2.3]. One can now take issue with Laks' distinction between **C** and **C***. To me the latter seems like yet another of Diogenes' re- or paraphrasing, although Laks is right to note that **C** is from the point of view of the interaction of existing things, whereas **C*** is from that of the genesis of things.

One final, small, point: Laks follows Simplicius[F] in printing ἡτεροιοῦτο, which might at first appear the better choice, but ἑτεροιοῦτο (Simplicius[DE], Diels) is probably correct [cf. Rosén 1962, 152]. My point here is to demonstrate that before finding advances in thought in Diogenes' work, one must first learn to appreciate his primitive prose style.

For me, one of the hallmarks of a good commentary is that it lays out the reasons for the editor's choices on all matters so thoroughly that it gives the reader all the evidence with which to disagree with these choices. Laks' book passes with flying colors.

BIBLIOGRAPHY

Curd, P. and Graham, D. 2008. *Handbook of Presocratic Philosophy.* Oxford.

de Sousa Barbosa, M. J. 1988. rev. Laks 1983. *Euphrosyne* 16:443–445.

des Places, E. 1984. rev. Laks 1983. *Revue de philologie* 58:112.

Diels, H. and Kranz, W. 1951. edd. *Die Fragmente der Vorsokratiker.* 3 vols. Zurich.

________ 2003. *Parmenides. Lehrgedicht. Mit einem neuen Vorwort von Walter Burkert und einer revidierten Bibliographie von Daniela de Cecco.* Sankt Augustin.

Duvernoy, J. F. 1985. rev. Laks 1983. *Études philosophiques.* 419–420.

Fortenbaugh, W. W.; Huby, P.; Sharples, R. W.; and Gutas, D. 1992. *Theophrastus of Eresus: Sources for his Life, Writings, Thought and Influence.* Leiden.

Guthrie, W. K. C. 1965. *A History of Greek Philosophy.* vol. 2. Cambridge.

Janda, J. 1987. rev. Laks 1983. *Eirene* 24:145–146.

Kerferd, G. 1990. rev. Laks 1983. *Ancient Philosophy* 10:269–270.

Kühner, R. and Gerth, B. 1890–1904. edd. *Ausführliche grammatik der griechischen sprache.* 3rd edn. 4 vols. Hannover.

Laks, A. 1983. *Diogène d'Apollonie. La dernière cosmologie présocratique.* Cahiers de philologie 9. Lille.

Laks, A. 2006. *Introduction à la « philosophie présocratique »*. Paris.

Laks, A. and Louguet, C. 2002. edd. *Qu'est-ce que la philosophie présocratique? What is Presocratic Philosophy?* Villeneuve-d'Ascq.

Longo, O. 1984. rev. Laks 1983. *Rivista di Filologia e Instruzione Classica* 112:206–209.

Marcovich, M. 2001. *Heraclitus.* Sankt Augustin.

Mazzara, G. 1999. *Gorgia. La retorica del verísimile.* Sankt Augustin.

Pasqua, H. 1986. rev. Laks 1983. *Revue philosophique de Louvain* 84:242–248.

Robbiano, C. 2006. *Becoming Being: On Parmenides' Transformative Philosophy.* Sankt Augustin.

Romeyer-Dherbey, G. 1985. rev. Laks 1983. *Revue de métaphysique et de morale* 90:276–277.

Rosén, H. G. 1962. *Eine Laut- und Formenlehre der herodotischen Sprachform.* Heidelberg.

Ruigh, C. J. 1971. *Autour de « τε épique »*. Amsterdam.

Schwabl, H. 1984. rev. Laks 1983. *Wiener Studien* 18:233.

Sider, D. 2005. *The Fragments of Anaxagoras*. 2nd rev. edn. Sankt Augustin.

The Libraries of the Neoplatonists edited by Cristina D'Ancona

Philosophia Antiqua 107. Leiden/Boston: Brill, 2007. Pp. xxxvi + 531. ISBN 978–90–04–15641–8. Cloth € 149.00, $221.00

Reviewed by
Peter Adamson*
King's College London
peter.adamson@kcl.ac.uk

Here's a statistic for you: of the nearly 11 million words of extant Greek philosophical texts now available in the *Thesaurus Linguae Graecae*, about 58% were written by Neoplatonists and another 13% were written by Alexander and Themistius. This means that much more than half of the directly extant Greek philosophical tradition consists in original works of Neoplatonists, Neoplatonist commentaries on Plato and Aristotle, and other late ancient commentaries on Aristotle. The Neoplatonists and commentators are mostly what remains to us of what one might call the Greek 'philosophical library'.

I take this information from a delightful article by R. Goulet in the volume under review. His statistical analysis is open to various caveats. It only counts Greek and so leaves out such authors as Lucretius and Cicero. And it does not count all Greeks: the voluminously extant Galen does not figure in the tally, even though some of Galen's works should be considered philosophical. Still, Goulet's point is a telling one. Plato and Aristotle, with their relatively extensive and inexhaustibly fascinating writings—they make up respectively 6% and 9% of the total extant Greek—will always attract the most attention from readers of ancient philosophy. But there is a vast corpus of late antique philosophical literature which has only begun to be explored seriously in the past few decades. The corpus becomes even more vast when one looks beyond the late ancient Greek evidence and considers the Neoplatonic inheritance in Byzantium

* Though we do not usually solicit reviews from contributors to collections under review, we thought that the importance of this volume to the readers of *Aestimatio* and its almost exhaustive list of expert contributors warranted our asking Dr Peter Adamson, a contributor himself, to undertake a review for us. We are most grateful for his agreeing to do this.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 80–88

and in non-Greek traditions. Among these the Arabic corpus is most extensive, but also important is the intermediary Syriac tradition. As one article here shows, Armenian literature too rewards study.

Obviously it would take a very large book by many scholars to do justice to this material; and that is just what we have here. Drawing on a research grant from the European Science Foundation, the editor Cristina D'Ancona and her collaborators staged a major conference in Strasbourg in 2004, of which these are the proceedings. The volume includes a substantial introduction by D'Ancona and 27 articles in a variety of languages (15 in French, 8 in English, 2 in Italian, 2 in German). The first 12 articles deal with the Greek tradition itself, focusing especially on the transmission of Greek texts and on the Byzantine reception; the second part, comprising 15 articles, is devoted the Armenian, Syriac, Arabic, and Hebrew receptions. The articles range from fairly general to extremely specific, and it would be the rare reader who finds that every piece commands their attention equally. But given the uniformly high standard, anyone with an interest in Neoplatonism will find the volume well worthwhile. (In my general assessment of the volume, I exclude one contribution which is by myself, and which I will leave others to evaluate.)

Goulet's aforementioned piece is a good place to start in approaching the first half of the book: he provides a useful overview of the cultural and material conditions for the transmission of Greek thought to the modern period. As he points out, no late ancient work is really 'preserved', except for the occasional papyrus scroll dug up from, say, Herculaneum or the sands of Egypt. Rather, ancient philosophy is *transmitted* to us, thanks to copying by hand. What survives in this way is only what has managed to avoid the perils of fire, water, lack of interest, and the upheaval caused by changes in the technology of writing and reading (e.g., from papyrus roll to the codex). In light of this, issues and problems surrounding textual transmission cannot be neatly separated from issues and problems of philosophical analysis. So it is useful that the first half of the volume devotes so much attention to transmission, and even more useful that a central case is given attention by several of the articles.

This is the case of the so-called 'philosophical collection', a set of now separated manuscripts that were produced in the Byzantine period, probably during the ninth century in Constantinople. The opening article of the volume, by H. D. Saffrey, discusses the history of one manuscript from the collection held in Paris, which contains numerous works by Plato. This manuscript and the collection as a whole embodied the transmission of Greek works from Alexandria to Byzantium and beyond. The question of who collected these works in Byzantium and why is a vexed one, but G. Cavallo argues here [158] that it could have been the work of a group of learned scholars and scribes (as opposed to having been a top-down decision by the political elite, as recently argued by Marwan Rashed [2002]). What is in any case striking about the collection is, first, its precious and extensive evidence for the philosophical tradition—not only Plato and Aristotle, but also Theophrastus, Alcinous, Proclus, Damascius, Alexander and other commentators, and so on. And second, the inclusion of works we would consider non-philosophical: not only is Aristotelian science well represented, but one manuscript (now held in Heidelberg) collects geographical works, which is the subject of a piece here by D. Marcotte.

Apart from these treatments of the 'collection', this part of the volume also includes detailed textual studies by C. Luna and M.-O. Goulet-Cazè. Luna discusses the commentary of Syrianus on the *Metaphysics* and its textual history. She incidentally makes the point that an edition of this commentary needs to preserve the lemmata of the *Metaphysics* as Syrianus quoted them: with the lemmata, the often hostile commentary becomes a kind of dialogue between the Neoplatonist and Aristotle [124]. Goulet-Cazè's attention is directed to the two sections of Plotinus, *Enneads* 4.7, which are not preserved in the direct tradition but are found in Eusebius. Though it has been thought that these derive from a pre-Porphyrian 'edition' of the works of Plotinus, Goulet-Cazè argues that Eusebius may have had access to a more complete copy of Porphyry's edition than we have [84, 89]. A final paper on the pre-Byzantine Greek tradition is by B. Reis. He argues that the roots of the Neoplatonist curriculum of reading Plato's dialogues are to be found earlier, among the 'Middle' Platonists. The first moves towards a thematic division of the dialogues may have been made in order to counter an Aristotelian accusation that Plato was insufficiently systematic.

This brings us to four papers on the Byzantine tradition. A useful general piece by M. Cacouros sets the scene by explaining the continuities, and lack thereof, between the late ancient and Byzantine philosophical ‘libraries’. In general, the continuity is more striking than the discontinuity. As Cacouros puts it, ‘le nèoplatonisme ètait avant tout prèsent’ in Byzantine philosophical literature [179]. But, like those who used Greek texts in Syriac and Arabic, Byzantine readers could be selective: they devoted much attention to the logical corpus and often to only the early parts of the *Organon*. It must be said that this too can be traced back to the late ancient period. Neoplatonists started their students on the *Isagoge* and *Categories*, which assumed a disproportionate importance in teaching contexts, with predictable results for what commentaries were written and were deemed useful enough to survive. It must also be said that the next three pieces, interesting though they are, do not exactly amount to a strong case for intellectual innovation among Byzantine authors. E. Delli discusses the topic of the pneumatic vehicle in Psellos, showing his treatment to be derived largely from Philoponus, albeit with some small changes. Yet, even some of these changes seem to be steps backward: note, for instance, Psellos’ insensitivity to the role of this vehicle in mediating between the physical and intellectual worlds [216].[1] A. Papamanolakis similarly discusses sources, but there is not much in the way of new philosophical insight regarding schemes of the virtues in Psellus and Eustratius; they draw on schemes found already in Plotinus and Porphyry (interesting here is a discussion of how the scheme of virtues may have been used to structure Marinus’ *Life of Proclus*). Finally, P. Golitsis discusses Nicephorus Blemmydes (13th century) and his use of Simplicius’ commentary on the *Physics*. This section as whole certainly shows that Byzantine texts are important, if only because their quotations can help us to establish a better text for the Greek sources they cite. But it would be interesting to see more in the way of distinctive philosophical ideas in the Byzantine tradition. We do get occasional hints of how such ideas could have emerged from the need to reconcile Neoplatonism with Christianity [e.g., 226, 250–251].[2]

The reconciliation between revealed religion and pagan philosophy becomes something of a *leitmotif* in the second part of the volume. Of course, the process of reconciliation had begun already in late antiquity with the first generations of Christian commentators

[1] On this point, see recently Zambon 2005.

[2] On the general topic, see Ierodiakonou 2004.

on Aristotle. These commentators, often difficult to distinguish from pagans like Ammonius and Simplicius in terms of their approach to Aristotle, included most famously John Philoponus but also such figures as David 'the Invincible'. The latter loomed much larger for the later medieval traditions than he does for us. C. Ferrari mentions in her piece on Ibn al-Tayyib, an 11th century Christian Arabic commentator, that his commentary on the *Categories* is closer to that of David or Elias—the attribution is disputed [270ff]—than to any other Greek author [472]. The Armenian tradition produced several translations of David, and this evidence is discussed by V. Calzolari. As in Byzantium, philosophy in Armenian was strongly influenced by the late ancient Neoplatonic curriculum; but there are interesting divergences. Of these the most striking to me is the selection of Plato's dialogues that were chosen for rendering into Armenian, which included the *Euthyphro*, *Apology*, *Minos*, *Timaeus*, and *Laws* [262].

Equally new for most readers will be the Syriac tradition, which is well served here with pieces by H. Hugonnard-Roche, S. Brock, and V. Berti. Hugonnard-Roche, probably the leading figure on Syriac philosophical literature, supplies a general discussion of the extant Syriac evidence, which like the Byzantine tradition leans strongly towards the Aristotelian logical corpus and within that, towards the first few texts of the *Organon*. On the other hand, Berti's very interesting treatment of Timothy, patriarch of the Syrian Church in the eighth to ninth centuries, mentions his interest not only in the *Topics* but also the *Poetics*. (The Syriac tradition, as the Arabic tradition, followed late ancient authors in including the *Poetics* and *Rhetoric* as part of the *Organon* [see Black 1990].) Brock, another leading Syriacist, takes up the question of whether there was a Syriac version of the works of Plotinus which stands behind the Arabic version of parts of the *Enneads* and thus behind what, notoriously, became known as the *Theology of Aristotle*. Brock agrees with an emerging consensus that the Arabic Plotinus was translated directly from Greek, not *via* Syriac, but points out that Plotinus was known to some extent in Syriac [296] and that the intellectual culture of Syriac-using monasteries may have been an influence on those who produced the Arabic Plotinus [305]. This is certainly plausible, given that the translator hailed from Syria.[3]

[3] For a similar recent assessment, see Bucur and Bucur 2006.

The Arabic Plotinus is also the subject of a promising piece by D. Gutas, who announces the project of re-editing this material and already makes numerous suggestions for emending the text. He points out that our best manuscript for the *Theology* is the one in Istanbul—this was already noted by G. Lewis [1957, 298] in a review of Badawi's edition [1955]—and provides a useful (if 'provisional') stemma [379] for the whole Arabic Plotinus tradition down to some manuscripts of the short version. The Arabic Plotinus was produced within the so-called 'Kindi circle', a group of translators gathered around the philosopher al-Kindi in the ninth century. G. Endress, whose seminal study, *Proclus Arabus* [1973], did so much to clarify the achievements and methods of this translation circle, offers an overview of the Greek sources known to al-Kindi. This provides a good entry to the volume's section on Arabic, which includes numerous studies of Greek works in Arabic translation. In addition to the *Theology*, which is discussed by Gutas, the volume covers translations, re-workings or commentaries of Aristotle's *De anima*, his *Categories*, Proclus' *Elements of Theology*, and of Palladius on the Hippocratic *Aphorisms*.

To take these in reverse order: H. H. Biesterfeldt discusses the Arabic version of Palladius, an important text not only for what it tells us about the Arabic translation movement but also for the more basic reason that this commentary is lost in Greek [386]. While this topic may seem out of place in a volume on Neoplatonism, it's important to remember the close ties between medicine and philosophy in both the Greek and Arabic traditions. An indication of this is that commentaries on medical works use the same set of opening questions about title, topic (*skopos*) and so on, as the commentators used for Plato and Aristotle [391–392]. More obviously relevant to Neoplatonism are the fortunes of Proclus' *Elements*, which was reworked to become the so-called *Book of the Pure Good* in Arabic, the basis for the influential Latin *Liber de causis*. Here E. Wakelnig discusses another version of the *Elements* written by the 10th-century Platonist al-ᶜAmiri. After discussing the complex set of Proclean materials now extant in Arabic, she sets out a claim defended at greater length by Wakelnig [2006] that there must have been a larger 'Ur-*Liber de causis*' which is now lost, and which spawned several incomplete versions. As for Aristotle, M. Sebti announces the important discovery of a new manuscript for a paraphrase of the *De anima*, which was edited and discussed in an extraordinary study by R. Arnzen [1998]. And C. Ferrari discusses the handling of the category 'relation' in Ibn al-Tayyib's massive commentary on the *Categories* [see Ferrari 2006].

The cultural and intellectual reception of Greek philosophy in Arabic is also treated in several offerings. In my own piece, I discuss what I refer to as the 'Kindian tradition', a line of Neoplatonic authors associated with the aforementioned al-Kindi. This tradition is well represented in a fascinating manuscript held in Oxford, Bodleian Or. Marsh 539, discussed here by E. Cottrell. It contains, among other things, bits of the Arabic Plotinus, sayings of other ancient philosophers, and remarks by Neoplatonists writing in Arabic such as Miskawayh and al-ᶜAmiri. I was particularly struck by Cottrell's argument that the manuscript provides evidence for comments on the *Theology* by al-Kindi's student, Abu Zayd al-Balkhi [438–440]. This is more evidence for an abiding fascination with the Neoplatonic translations among the Kindian authors. But works like the *Theology* also had influence beyond these Kindians: they had, for instance, a major impact on the Shiite tradition. Here a useful overview by D. De Smet discusses the use of Greek philosophical literature by the Ismailis. And J. Montgomery suggests that authors and patrons with Shiite tendencies may have been involved with numerous works of the Graeco-Arabic tradition, including several anonymous or pseudonymous works like the *Opinions of the Philosophers* of pseudo-Ammonius [455]. Montgomery's excellent discussion, taking off from al-Jahiz, is an important reminder of the wider cultural and political forces that motivated and shaped the reception of Greek philosophy in the Arabic-speaking world.

The book concludes with a final survey piece by S. Harvey, who looks at the question of which Greek works (and in what versions) were known to Jewish authors. Among other things, Harvey emphasizes the close links between the Jewish and Muslim philosophical traditions. As he says, 'the Jewish Aristotelians knew Aristotle very well, but their knowledge for the most part came from Averroes' commentaries' [504].

As this dash through the volume shows, D'Ancona has brought together an impressive group of scholars to deal with an extraordinary range of authors, texts, and linguistic traditions. One is hard pressed to think of another volume which tackles such a large swathe of the Neoplatonic tradition. Inevitably, the resulting book ranges widely in terms of theme, approach, and level of specificity; yet, from it all emerges a sense that we are indeed dealing with a single tradition here. Of course, seventh century Christian authors writing in Armenian, 10th-century Muslim authors writing in Arabic, and 13th-century Jewish authors writing in Hebrew had access to different texts from the Greek corpus, read these texts once they had been translated into different languages, and approached them with different preoccupations in mind. Yet, it is striking how many things remain constant throughout, ranging from fundamental Neoplatonic metaphysical convictions to strategies for reading Aristotle and organizing his corpus. D'Ancona is to be congratulated for her success in the appropriately Neoplatonic task of bringing some degree of unity to a bewildering multiplicity of sources and problems, many of which are rarely discussed at all, never mind together in one place.

BIBLIOGRAPHY

Arnzen, R. 1998. *Aristotles' De Anima. Eine verlorene spätantike Paraphrase in arabischer und persischer Überlieferung.* Leiden.

Badawi, A. 1955. *Aflutin* ^{c}*inda l-*^{c}*Arab.* Cairo.

Black, D. L. 1990. *Logic and Aristotle's Rhetoric and Poetics in Medieval Arabic Philosophy.* Leiden.

Bucur, C. and Bucur, B. G. 2006. '"The Place of Splendor and Light": Observations on the Paraphrasing of *Enn.* 4.8.1 in the *Theology of Aristotle*'. *Le Musèon* 119:271–292.

Endress, G. 1973. *Proclus Arabus. Zwanzig Abschnitte aus der Institutio theologica in arabischer Übersetzung.* Beirut.

Ferrari, C. 2006. *Der Kategorienkommentar von Abu l-Faraj* ^{c}*Abdallah Ibn at-Tayyib.* Leiden.

Ierodiakonou, K. 2004. *Byzantine Philosophy and its Ancient Sources.* Oxford.

Lewis, G. 1957. rev. Badawi 1955. *Oriens* 10:395–399.

Rashed, M. 2002. 'Nicolas d'Otrante, Guillaume de Moerbeke et la « Collection Philosophique »'. *Studi Medievali* 43:693–717.

Wakelnig, E. 2006. *Feder, Tafel, Mensch. Al-^{c}Amiris Kitab al-Fusul fi l-Ma^{c}alim al-ilahiya und die arabische Proklos-Rezeption im 10. Jhr.* Leiden.

Zambon, M. 2005. 'Il significato filosofico della dottrina dell'*ochêma* dell'anima'. Pp. 305–335 in R. Chiaradonna ed. *Studi sull'anima in Plotino.* Naples.

Las tablas alfonsíes de Toledo by José Chabás and Bernard R. Goldstein

Toledo: Diputación Provincial de Toledo, 2008. Pp. 423. ISBN 978–84–96211–37–7. Paper € 24.00

Reviewed by
Beatriz Porres de Mateo
Brussels, Belgium
bporres@skynet.be

The first rigorous analysis of the origins and transmission of the Alfonsine astronomical corpus was published in English by José Chabás and Bernard R. Goldstein in 2003. The core of their book was the edition with commentary of the 'canons', or instructions for use, of a set of astronomical tables composed in Toledo during the mid 13th century by two Jewish astronomers, Isaac ben Sid and Juda ben Moshe ha-Cohen, working under the patronage of Alfonso X, king of Castile (1221–1284). These canons and tables came to be called (too simplistically as the authors prove) 'Alfonsine Tables'. For reasons well developed by Chabás and Goldstein, this work was fundamental for the history of Latin medieval astronomy in Western Europe. Together with other scientific works produced under Alfonso, it was also fundamental for the development of Spanish as a scientific language. As it happens though, the transmission of this work was precarious: the tables themselves are not preserved and the canons are extant in a single manuscript, nowadays kept in the Madrid National Library under number 3306. The manuscript is a 16th-century copy that gathers several astronomical treatises in Latin and Spanish, the Alfonsine canons among them.

There were very good reasons for publishing these canons. In the first place, the only previous edition was a very bad one, published in the 19th century by a Spanish doctor and professor of physics, Manuel Rico y Sinobas [see 1863–1867]. Rico, who was a cultivated man with a strong interest in archaeology and history of science, was nevertheless not instructed in the basic rules of paleographic transcription. His poor edition, fairly criticized by Chabás and Goldstein, is regrettable also because the manuscript, already badly preserved

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 89–94

when he read it, suffered further damage afterwards. Had Rico been more accurate, he could have transmitted to posterity much more of the text than we possess today. Thus, Chabás and Goldstein aimed to improve on Rico's edition as much as possible, given the current state of the source materials. Next, another aim of their earlier edition was to place the Alfonsine canons into their proper scientific and historical framework. The editors traced the origins of the canons back to the Toledan and Andalusian context of early medieval astronomy written in Arabic and Hebrew; and their careful research follows the threads that link these tables to subsequent Latin astronomy as was practiced first in Paris and then in the rest of Europe from the beginning of the 14th century until the Copernican revolution.

Both of the editors' objectives were fully achieved, as one would expect considering the deep knowledge and abundant research already published by Chabás and Goldstein in the history of Hebrew, Arabic, Spanish, and Latin astronomy in medieval times. The edition of the Spanish canons is flanked by a glossary of the Spanish scientific terminology used in it, an astronomical comment to each canon, an investigation of the scientific and historical context of the Toledan Alfonsine Tables, and a study of their dissemination into Latin scientific production in Europe.

Several years later, the editors decided to re-publish their book, this time in its Spanish translation. I have no doubt that this decision is mainly due to the linguistic sensitivity of one of the editors, José Chabás, a Catalan native speaker who spent some years of his life as a professional Spanish translator for the European Union administration. This might seem a surprising decision against the background of the predominantly English-speaking environment of current research into the history of medieval science. But it is no less surprising than the decision of King Alfonso himself, who deliberately and for the first time promoted scientific production in Spanish within a then dominant context of either Arabic or Latin science.

The goal of disseminating the history of science in the original language is equally shared by the publisher of this book. The Diputación Provincial is a public authority administering the Province of Toledo. One of its aims is cultural promotion, including publication of studies and research on local literary works. In the case of Toledo, the Diputación has inevitably produced a huge number of historical publications, as befits the major role of Toledo in the history of medieval Spain. The cultural life of the city in medieval centuries was unparalleled both under the culturally refined Muslim period and then under Christian times, when it became the main city of the Kingdom of Castile and León, and the only city to gather a sufficient number of scientific books and highly educated men of Muslim, Jewish, and Christian persuasion. Thus, it was only natural that the idea of publishing a Spanish translation of Chabás and Goldstein 2003 on the Toledan Alfonsine Tables immediately attracted the interest of the publishing services of the Toledan Diputación.

The Madrid manuscript was copied for a scholar of astronomy (perhaps Francisco de Morales himself, the clerk who signed the Spanish translation of John of Saxony's canons in the same book?) no earlier than the 16th century—Chabás and Goldstein rightly correct the date of catalogs in the 15th century. The date of the canons in the text is 'in the first decade of the fourth centennial of the second millennium of the Era of Caesar', i.e., between 1301 and 1310. This means that the drafting of the canons is to be dated between AD 1263 and 1272, as the Era of Caesar, which was predominantly used in medieval Spain, started 38 years before Christ. They were composed for tables starting 10 years earlier, in 1252, the year of Alfonso's coronation. The copyist was a professional scribe and clearly not an astronomer, as he makes mistakes in the transcription of technical terms that no astronomer would make, such as writing 'opinion' instead of 'oposicion'. The transcription of the Spanish text by Chabás and Goldstein, without being purely philological, is nevertheless a very accurate work. It is especially praiseworthy not only because it has been done not only from a badly preserved manuscript, but also because, since this manuscript is a *unicum*, it is not possible to collate it against copies in other manuscripts, which is always a useful tool for an editor.

The glossary is exhaustive and especially useful for those of us used to the astronomical Latin terminology but not to these then-newly created Spanish terms. However, it lacks an explanation for some of the terms included such as 'arco de la vista', 'arco del ponimiento', and 'echamiento de los rayos', whose meaning in explained only in the astronomical commentary in chapter 4. Some others might have been added as well, e.g., 'padron' (the starting value in a calculation [canon 51]) and 'planetas de suso' meaning 'superior planets' [canon 27:19]. An explanatory note for some terms incomprehensible nowadays to Spanish speakers such as 'sobrehas/sobrehaz' ('surface') might have been added too. But these are petty details when confronted with an impressive number of more than 300 terms.

The glossary also has an important bonus: it has been compared with four other astronomical texts written in Spanish in the Alfonsine *milieu*, two of them by Isaac ben Sid himself: the *Tratado del quadrante sennero*, Azarquiel's *Almanac*, the translation of al-Battani's *zīj* (attributed to Isaac ben Sid without further explanation [245]); and the *Lapidario.*[1] In this way, the lexical coherence of the Alfonsine canons *vis-à-vis* contemporary works, and thus the pertinence of their attribution to that same *milieu*, is demonstrated. The rigorous approach of the authors in entering the territory of comparative lexicography is again to be praised.

This book is not just a translation of the first English version. It adds some information here and there that reflects the continued work of the authors on the history of Alfonsine astronomy. Thus, we find in the Spanish version that the chapter on John of Vimond's tables is more detailed than in the English version, and that further information on the tables for the mean movements of the inferior planets has been added. The research on Vimond's work is a fascinating one, as the book illustrates. His tables have 10 March 1320 as starting date (which means that the composition of the tables themselves was probably done later, as was usually the case). He seems to have worked in parallel with the well known Parisian astronomers who disseminated their own versions of the Alfonsine Tables (the so-called Parisian Alfonsine Tables): John of Lignères, John of Murs, and John of Saxony. The relation between the astronomical productions of these men is far from clear. Vimond seems to take in an intermediate position between Azarquiel, Castilian Alfonsine astronomy, and the Parisian version: his model for precession and trepidation is close to the Parisian Tables but, like the Castilian ones, his mean motions are sidereal; his table for planetary equations adds a column for planetary velocities that seems to match the description of the Castilian canons; his tables for planetary latitudes of inferior

[1] The first three works are kept in a single manuscript, Paris, Bibl. de l'Arsenal 8322. The fourth one is preserved in El Escorial, Bibl. del Monasterio, h–I–15.

planets add a third column that has its only precedent in canon 22 of the Toledan Alfonsine Tables; and, as in the Toledan Tables based on Azarquiel, he accepts the existence of proper movements for the apogees of the Sun and the planets. On the other side, his list of stars shows a precession of 17;52°, which does not match the Parisian catalog. Chabás and Goldstein clearly reject the total precession of 17;8° as the standard value for the Alfonsine star list and affirm that, contrary to what has always been said, there is no homogeneity in the star catalogs that can be found in the Alfonsine corpus.

The Parisian tables present mean movements in sexagesimal days, signs of 60°, a new model for trepidation, and tropical mean motions. By contrast, the Toledan Alfonsine tables use sidereal mean motions presented in *anni collecti* at 20-year intervals, and signs of 30°. It is not easy to understand how the Parisian tables could have been influenced by the Toledan ones; but the authors prove that an undeniable link exists, based on some shared characteristics: 1252, the year of Alfonso's coronation, as starting era; Toledo as meridian of reference; the presence of several calendar tables for the calculation of different eras; a value of 2;10° as maximum solar correction; and no proper movements for the apogees of the Sun and the planets.

To demarcate the exact perimeter of a set of medieval astronomical tables is usually a frustrating exercise, and the Alfonsine Tables are no exception. The complexity of manuscript traditions and permanent 'contamination' of sources is a reality that all scholars dealing with medieval astronomy have to accept. Being well aware of this, Chabás and Goldstein have already introduced in the English version of this book a terminological and conceptual distinction between 'Toledan (or Castilian) Alfonsine Tables' and 'Parisian (or Latin) Alfonsine Tables' that tries to identify general trends or groups within a specific kind of astronomical practice. The basis for this distinction relies on issues such as the tables' layout, their underlying parameters and models, and the internal coherence between tables and canons. On a higher level, they speak of an 'Alfonsine corpus' that comprises 'the totality of the astronomical works that ultimately derive from Alfonso's court' [2003, 6]. In this way they put forward a different and certainly more honest approach to the question, and issue a general warning against any attempt to identify tables and authors through categorical affiliations.

Introducing such conceptual distinction is one of the major aims of this book, and we think that it is a successful one. But beyond that, the work of Chabás and Goldstein provides excellent scientific value, especially in two remarkable chapters: the astronomical commentary in chapter 4 and the chapter dealing with the legacy of Alfonsine astronomy. They are a fine example of scholarly work both on the mathematical and the historical side. The history of astronomy has gained a contribution that will be difficult to surpass.

BIBLIOGRAPHY

Chabás, J. and Goldstein, B. R. 2003. *The Alfonsine Tables of Toledo*. Archimedes: New Studies in the History and Philosophy of Science and Technology 8. Dordrecht.

Rico y Sinobas, M. 1863–1867. *Libros del saber de astronomía del Rey D. Alfonso X de Castilla*. 5 vols. Madrid.

Astronomy and Astrology in al-Andalus and the Maghrib by Julio Samsó

Variorum Collected Studies Series CS887. Aldershot, UK/Burlington, VT: Ashgate, 2007. Pp. xiv+366. ISBN 0–7546–5934–1. Cloth $124.95

Reviewed by
J. L. Mancha
University of Seville
mancha@us.es

Over the last 30 years, Julio Samsó and his colleagues from the University of Barcelona, most of them his former students, have continued the work of Millás Vallicrosa in the first half of the 20th century and have substantially modified our knowledge of the history of astronomy and its related sciences in the Iberian Peninsula during the Middle Ages. We are much indebted to them for their efforts in making new textual evidence available, since this is the first task of historians.

This interesting collection of articles is the second, and very welcome, volume of Samsó's papers in the Variorum series. The first, *Islamic Astronomy and Medieval Spain*—20 papers published between 1977 and 1994 (four of them co-authored with M. Comes, F. Castelló, H. Mielgo, and E. Millás)—covered a wide range of topics: the survival of Latin astronomy and astrology in al-Andalus, Eastern influences in Andalusian astronomy and trigonometry, astronomical theory (mainly the work of Ibn al-Zarqālluh and his school on access and recess[1] and solar theory) and the presence of Islamic materials in the works sponsored by Alfonso X. In this collection, two papers on 'eccentric' subjects are, in my opinion, especially worth mentioning: the one devoted to a homocentric solar model described by Abū Ja^{c}far al-Khāzin (d. *ca* 965), and 'On al-Biṭrūjī and the *hay'a* Tradition in al-Andalus', which emphasizes the Neoplatonic components in al-Biṭrūjī's physics and challenges the view of al-Biṭrūjī's *Kitāb fī-l-hay'a* as the Aristotelian culmination of the (not well understood but often mentioned) 'Andalusian revolt' against Ptolemy.

[1] The theory of access and recess, or trepidation, accounted for supposed long-term oscillatory changes in stellar longitudes.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 95–104

The volume under review assembles 16 papers (of which chapters 2, 4, 5, and 8 are co-authored with M. Castells, H. Mielgo, H. Berrani, and E. Millás, respectively) published between 1994 and 2004. Seven of them are still devoted to Andalusian astronomy; but the rest deal with the Maghribī tradition of astronomical tables from the 13th to the 17th centuries, a tradition that depends strongly, at least until the beginning of the 15th century, on the work of Ibn al-Zarqālluh (d. 1100). Contrary to what the volume's title suggests, only two articles relate directly to astrology.

Chapter 2 concerns the lists found in old texts such as *De mensura astrolabii* (ms. Ripoll 225, classified by Kunitzsch as Type III), which is attributed to the school of Maslama al-Majrīṭī (d. 1007), of the names and coordinates (*latitudo* and *altitudo*) of 27 stars. Previous research by Kunitzsch and North established that the values for *latitudo* are equivalent to those of the column labelled *mediatio* in the star list of Type I (Maslama, *ca* 978); they concluded, after taking into account the low level of accuracy attained in deriving the values for *altitudo* from the declinations in Maslama's list, that these values were obtained neither from observations nor from derived calculation, but probably from measurement on the *rete* of an astrolabe. Samsó presents a new derivation from Maslama's declinations (although it does not provide exact agreement) and consequently argues that the list was composed after 978.

Chapter 3 provides an edition (with translation and commentary) of seven chapters of a lost *zīj* (astronomical tables with instructions for use) by Ibn al-Ṣaffār (d. 1035), a disciple of Maslama, which have been preserved in Arabic written in Hebrew characters in a manuscript of the Bibliothèque Nationale in Paris (Heb. 1102, 1r–5r). The text deals with eclipses, the equation of time, and the determination of astrological houses. Similarities between this text and the canons on the use of the *Almanac* of Ibn al-Zarqālluh and some passages of Adelard of Bath's translation of al-Khwārizmī suggest a common source for all these texts.

'Ibn al-Haytham and Jābir b. Aflaḥ's Criticism of Ptolemy's Determination of the Parameters of Mercury' deals with two Andalusian texts from the 12th century. The first is by Ibn Bājja, the founder of Spanish Aristotelianism, who is mentioned by Maimonides in his *Guide* [2.24] as the author of astronomical models without epicycles; and the second, by Jābir b. Aflaḥ.

Jābir's *Iṣlāḥ al-Majisṭī* (*Improvement of the Almagest*) is the most original astronomical text in the 12th century al-Andalus: it exhibits a knowledge of Ptolemy's mathematics that is exceptional in the Middle Ages.[2] Samsó analyzes in detail passages in book 7 in which Jābir points out a flaw in Ptolemy's determination of the apogee of Venus and of Mercury [*Alm.* 9.7, 10.1]—Ptolemy assumes without proof that equality of maximum morning and evening elongations indicates symmetry of the positions of the epicycle with respect to the apsidal line[3]—and proposes his own solution.

Studies of Jābir show that, except when it is based on an error in the manuscript of the *Almagest* that he used, his criticism is sound. Thus, in my opinion, to speak of Jābir's 'mathematical scruples' [7.218] is a bit unfair. Ptolemy's derivation of the eccentricity and the apogees of the outer planets [*Alm.* 10.7] requires, in modern terms, the solution of an eighth degree equation; and the problem of finding $\sin a$ from $\sin 3a$ in the construction of the table of chords [*Alm.* 1.10], a cubic equation. In both cases, Ptolemy resorted to iterative procedures which, according to him, did not constitute rigorous demonstrations. Ptolemy's criteria are Euclidean, and the Middle Ages shared without objection his view that approximation methods do not provide proofs reducible to apodeictic syllogisms.

The text by Ibn Bājja (d. 1138) is a letter to Abū Ja^{c}far Yūsuf ibn Ḥasdāy, calling attention to a mistake made by Ibn al-Haytham [*Doubts* 1.9] in his criticism of Ptolemy's determination of the eccentricity of the equant of Mercury and Venus [*Alm.* 9.9, 10.3]. For, in attacking Ptolemy's account, Ibn al-Haytham stated that the line joining the center of the Earth and the mean Sun always passes through the center of Venus' epicycle. This was indeed an elementary error, and Ibn Bājja concluded that Ibn al-Haytham 'only studied astronomy in a superficial way'.[4] Samsó wonders if Ibn al-Haytham's

---

[2] Unfortunately, neither modern translation nor edition of the complete Arabic text are still available. Gerard of Cremona's Latin translation was published in 1534. As noted by Samsó in the addenda, Josep Bellver's Ph. D. dissertation (Barcelona, 2005) was devoted to Jābir's analysis of Ptolemy's solar and lunar theory. Bellver 2006, 2007, 2008a, and 2008b provide much new light on Jābir.

[3] This was also pointed out by Sawyer [1977], who was unaware of Jābir.

[4] Samsó [7.204] considers Ibn al-Haytham's error 'understandable' since in the *Zīj al-Shāh* (as well as in Ḥabash and al-Battānī) the apogees and the

unsound criticism might have had any influence on Andalusian scholars, and his answer is negative: neither Averroes in his *Epitome of the Almagest* nor Jābir allude to it. In my opinion, since Ibn Rushd used Ibn al-Haytham's *Doubts* profusely in his *Epitome*, his silence rather suggests that he knew Ibn Bājja's argument and agreed with it. Ibn Bājja's letter was surely very influential: even al-Biṭrūjī, who had an extremely poor knowledge of the *Almagest*, states twice in his *Kitāb fi'l-hay'a* that the centers of the epicycles of Venus and Mercury have only two conjunctions with the mean Sun every year [Goldstein 1971, 1.129,1.141].

Samsó also underlines that Ibn Bājja ascribes to Ibn al-Zarqālluh a non-extant treatise on the invalidity of Ptolemy's method to determine the position of Mercury's apogee, and concludes that these texts confirm that in 11th- to 12th-century al-Andalus 'there was a certain awareness of the existence of an error' in Ptolemy's value for the longitude of Mercury's apogee. In fact, Ptolemy's longitude of 190° is in error by about 30°. Samsó alludes here to his hypothesis formulated in 1994 [ch. 4] according to which the longitude of Mercury's apogee preserved in Ibn al-Zarqālluh's *Almanac* (210°) is derived from a new determination by the Toledan astronomer. But this hypothesis was apparently abandoned in 1998 [ch. 8.267], once it was established that Ibn al-Zarqālluh's disciples had transmitted a longitude close to 198°.

In the second section of the book, Samsó focuses entirely on Maghribī astronomy, adding much important information to what was known before. Chapters 11–12 provide an outline of the history of the Maghribī *zīj*es from the 13th century onwards. The earliest extant *zīj* was composed by Ibn Isḥāq (beginning of the 13th century); it was also the most interesting and editions of it were prepared by Ibn al-Bannā (1256–1321) and Ibn al-Raqqām (d. 1315). The Andalusian school—namely, Ibn al-Zarqālluh, Ibn al-Kammād (fl. 1115) and Ibn al-Hāim (fl. 1205)—is the predominant influence in all them; it is also evident in two other *zīj*es by two 14th century astronomers of Constantine, Abū l-Ḥasan ᶜAlī ibn Abī and Abū l-Qāsim ibn ᶜAzzūz. Both chapters shed light on the role played by observation in the abandonment of the main feature of the Andalusian school (the theory of

equations of center of Venus and of the Sun coincide. But even if the corrected solar longitude and the corrected argument of Venus are equal, Ibn al-Haytham's claim is wrong.

trepidation) and its replacement by uniform precession as found in Eastern *zījes*.

Chapter 8 is an analysis of the parameters and methods of computation in Ibn al-Bannā's *Minhāj zīj*. With Zarqallian roots and independent of those in al-Kwārizmī's *zīj*, Ibn al-Bannā's mean motions and mean longitudes depend on Ibn Isḥāq's. The same can be said for the apogees derived for the superior planets from al-Battānī, although the correction for precession used is not evident. Samsó is inclined to think that when the parameters of Ibn Isḥāq, Ibn al-Bannā, and Ibn al-Raqqām seem unrelated to known sources (mean motions in longitude of Saturn and Mars, motion in anomaly of Venus, lunar nodes, apogees of Venus and Mercury), they reflect research undertaken by Ibn al-Zarqālluh himself after the completion of the Toledan Tables. Ibn al-Bannā's *zīj* is more original in the presentation and methods of composition of the tables: he was, apparently, the first Western Islamic astronomer to use tables of 'displaced' equations of center (always positive by addition of a constant to facilitate the computation), well known in the East since the 9th century. Other characteristics of his *zīj* are the use of the method found in the *Handy Tables* to compute the lunar anomaly in calculating the equation of anomaly of Saturn and Jupiter, and the extension of the motion of the solar apogee discovered by Ibn al-Zarqālluh to the apogees of the inferior planets. (This is in agreement with Ibn Isḥāq; whereas in the *zījes* of Ibn al-Kammād and Ibn al-Hāim, the motion of the solar apogee affects all the planets).

Chapters 9–10 are devoted to the work of Abū l-Qāsim ibn ᶜAzzūz al-Qusanṭinī (d. 1354). The first is a detailed description of his *Muwāfiq zīj*. In the introduction, Ibn ᶜAzzūz explains the reasons which motivated his revision of Ibn Isḥāq's tables: the disagreement between the calculated times of past events using *tasyīr* techniques[5] and the historical data. To correct this divergence, Ibn ᶜAzzūz claims to have made observations with an armillary sphere in 1344 which led him to modify Ibn Isḥāq's parameters (although this revision, in

[5] *Tasyīr* and the projection of rays are the subject of chapter 5, 'World Astrology in Eleventh-Century al-Andalus: The Epistle on *tasyīr* and the Projection of Rays by al-Istijī'. The text of al-Istijī's letter (with translation and commentary) has been published in Samsó and Berrani 2005 and reprinted in Samsó 2008.

my opinion, is only a return to the Toledan and Ibn al-Kammād's tables). It would be interesting to obtain additional evidence confirming Ibn ^{c}Azzūz's use of the 31 years lunar cycle known to us from David Bonjorn's tables (epoch: 1361).

In chapter 10, 'Horoscopes and History: Ibn ^{c}Azzūz and His Retrospective Horoscopes Related to the Battle of El Salado (1340)', Samsó provides a detailed analysis of the data of four horoscopes contained in the second part of Ibn ^{c}Azzūz's *Kitāb al-Fuṣūl fī jam*^{c} *al-uṣūl* (*The Book of the Chapters on the Totality of the Principles*), corresponding to the vernal equinox of 1305, the Sun-Moon opposition preceding the Saturn-Jupiter conjunction of 1305, and the vernal equinoxes of 1340 and 1344. Recomputation of the textual values has been made using computer programs provided by the late J. D. North [1986] for the longitudes of the points (cusps) at which the astrological houses begin and by E. S. Kennedy (revised by H. Mielgo and J. Casulleras) for planetary positions, using Ibn ^{c}Azzūz's parameters.[6]

I agree with Samsó that this *zīj* deserves a detailed study, but I am not sure about Ibn ^{c}Azzūz's claim that his modifications of Ibn Isḥāq's parameters were based on observation. The text does not say at which date these horoscopes were cast. Samsó achieves good recomputations of Ibn ^{c}Azzūz's numbers using parameters of the *Muwāfiq zīj*, but some doubts remain on the role played by observation in those changes. For example, the position and date given in the text for the Saturn-Jupiter conjunction of 1345 are computed, not observed, values: thus, for February 25, the sidereal longitude is 303°(actual: March 24, 319°, tropical), which is very close to those we can obtain with the Toledan tables. In fact, there are no significant differences for these planets in their mean motions, mean longitudes, and apogees between the Toledan (or Ibn al-Kammād's) values and Ibn ^{c}Azzūz's [ch. 9.98–101, Tables 1 and 2]. The same can be said regarding Ibn ^{c}Azzūz's position for Saturn on March 24, 1344 (Capricorn 25°, sidereal; actual: Aquarius 308°, tropical) [Tuckerman 1964]. Now, at the end of the 13th century the Toledan

[6] Although it is said [10.13] that the column of true longitudes in the appendix is computed from the formula 'manuscript second *markaz* + [recomp. corrected apogee] + [recomp. equation of anomaly]', the column is indeed computed from 'mean longitude + equation of center + equation of anomaly').

longitudes for Saturn were already systematically low by more than 2°, as observers of the time record and modern computation shows.[7] Moreover, there is no trace in Ibn ᶜAzzūz's tables of anything like the correction in Saturn's radix introduced by the tables of Toulouse (before 1240) to correct this deficiency. Better contemporary computations of that conjunction required changes in mean motions and radices (John of Murs, March 21, with the Alfonsine Tables) or mean motions and apogees (Levi ben Gerson, March 28, with al-Battānī's) [Goldstein and Pingree 1990]. But without details of the computer program used by Samsó, it is is difficult to see how Ibn ᶜAzzūz avoids the Toledan errors for Mars (e.g., for March 1305), taking into account that he seems to have made the worst choice for its parameters (apogee 119; 41°, as in Ibn al-Kammād's tables, instead of Ibn Isḥāq's 122;13°; mean longitude 211;7,57°, closer to the Toledan one than to Ibn Isḥāq's 210;37,25°). Note also that Ibn ᶜAzzūz adopted Ibn al-Kammād's table for trepidation, whose maximum is 9;59°.

I should like also to add some comments on the appendix of chapter 9 ('On the Epoch of the Star Table of Ibn al-Kammād and Ibn ᶜAzzūz'), in which Samsó echoes discussions among scholars who in the last years have resorted to the first two trepidation models described in chapter 5 of Ibn al-Zarqālluh's *Treatise on the Motions of the Fixed Stars* [Millás 1950, 315–319][8] in order to explain the different numerical values found in texts or tables composed by Ibn al-Zarqālluh's and his followers. These models have thus supported 'reconstructions' for Ibn al-Kammād's trepidation table (model 2, with parameters not mentioned by Ibn al-Zarqālluh), Ibn al-Kammād's star list (model 1A), as well as Ibn al-Kammād's trepidation table (model 2B), and even the planetary apogees in Ibn al-Zarqālluh's treatise on the construction of the *equatorium* (model 1B). That Ibn al-Zarqālluh himself or his followers could have used these models so indiscriminately is a *guess* and a historically implausible one at that; yet, that Ibn al-Zarqālluh ultimately accepted only one, the third, and explicitly rejected the others, is a *fact*. Ibn al-Zarqālluh's

---

[7] See Duhem 1958–1959, 4.16 (according to William of Saint Cloud's report of his observation, the Toledan error for the date of the Saturn-Jupiter conjunction of 1285 exceeded 20 days) and Gingerich and Welther 1977.

[8] To account for observational data, Ibn al-Zarqālluh considers three models and gives two sets of parameters for the two first and only one for the third (hereafter, 1A, 1B, 2A, 2B, and 3).

reports a set of data that he considered soundly derived from observations according to which the amount of precession since Hipparchus was 16;56°(=$\Delta\lambda$), with differences resulting from the sum of the differences 2;46° (interval from Hipparchus to Ptolemy), 11;36° (Ptolemy to al-Battānī), and 2;34° (al-Battānī to al-Zarqālluh).[9] The models yield the following results when parameters are provided to account for these data:

	1A	1B	2A	2B	3
	2;22	2;47	2;52,30	2;48	2;45,45
	10;56	11;57	11;35	11;34	11;35,41
	2;27	2;42	2;34,20	2;34	2;34,19
$\Delta\lambda$	15;45	17;25	16;41,30	16;56	16;55,45

After concluding that model 3 provides the best results, in chapter 6 of the same treatise, Ibn al-Zarqālluh mentions additional reasons to reject the first two: in model 1, the amplitude of the motions of access and recess cannot be equal; and in model 2, as he writes, 'the positions should be increased since the beginning of the motion of the small circle until our time, but we have investigated this matter and we do not found them increased' [Millás 1950, 320–321]. Thus, the 'explanation' provided by appealing to models 1 and 2—indeed to the underlying formulae, or to numbers from columns 1A–2B deprived of their context—is in fact an explanation *per obscurius*: why would astronomers who recognized Ibn al-Zarqālluh as the greatest authority refuse to accept his conclusions?

'On the Lunar Tables in Sanjaq Dār's *Zīj al-Sharif*' closes the Maghrib section of the volume and analyzes the solar and lunar tables in this late *zīj* (end of the 16th century), the main characteristic of which is the use of double argument tables of equations for the Moon. This is the first instance in the Maghrib of this kind of table, though it is well documented in the East since the time of Ibn Yūnus (d. 1009). Mean motions and equations derive from Ulugh Begh's *Zīj i-Sulṭanī* (15th century).

The last three papers of the collection are a thorough complement to a recent monograph on Abraham Zacut (1452–1515) [see

[9] Millás 1950, 316. Millás gives also 2;47°, 11;32°, and 2;37°[1950, 297–299].

Chabás and Goldstein 2000]. Samsó offers a minutely detailed account of the assimilation and adaptation of the *Almanach perpetuum* (1496) in the Muslim world from the 16th century onward, and of the role played in the process by the two Arabic versions by Moses Galiano (Istanbul, *ca* 1506) and Aḥmed b. Qāsim al-Ḥajarī (Marrākush, *ca* 1624). Samsó's exhaustive survey of the copies of these versions preserved in Eastern and Western libraries also contains useful information on the transmission of other Jewish, Andalusī, and Maghribī astronomical and astrological materials to the Mashriq. These manuscripts not only certify the long survival of Ḥajarī's version, which was still in use at the end of the 18th century, but also (*pace* Samsó's sceptical look to social history) to the deep decline of science and society in these Islamic countries during a time in which European astronomy had long ago forgotten Zacut's work.

BIBLIOGRAPHY

Bellver, J. 2006. 'Jābir b. Aflaḥ on the Four Eclipse Method for Finding the Lunar Period in Anomaly'. *Suhayl* 6:159–248.

________ 2007. 'Yābir b. Aflaḥ en torno a la inclinación de los eclipses en el horizonte'. *Archives Internationales d'Histoire des Sciences* 57:3–23.

________ 2008a. 'Jābir b. Aflah on lunar eclipses'. *Suhayl* 8:47–91.

________ 2008b. 'On Jābir b. Aflaḥ's Criticisms of Ptolemy's *Almagest*'. Pp. 181–189 in E. Calvo, M. Comes, R. Puig, and M. Rius edd. *A Shared Legacy: Islamic Science East and West.* Barcelona.

Casulleras J. 2007. 'El cálculo de aspectos o la proyección de rayos en la astrología medieval árabe'. *Archives Internationales d'Histoire des Sciences* 57:25–46.

Chabás, J. and Goldstein, B. R. 2000. *Astronomy in the Iberian Peninsula: Abraham Zacut and the Transition from Manuscript to Print.* Philadelphia.

Díaz Fajardo, M. 2008. *Tasyīr y proyección de rayos en textos astrológicos magrebíes.* Ph. D. dissertation, University of Barcelona.

Duhem, P. 1958–1959. *Le système du monde.* 10 vols. Paris.

Gingerich, O. and Welther, B. 1977. 'The Accuracy of the Toledan Tables'. Pp. 151–162 in Y. Maeyama, W. and G. Saltzer edd. *Prismata. Festschrift für Willy Hartner.* Wiesbaden.

Goldstein, B. R. 1971. *Al-Biṭrūjī: On the Principles of Astronomy.* 2 vols. New Haven/London.

Goldstein, B. R. and Pingree, D. 1990. 'Levi ben Gerson's Prognostication for the Conjunction of 1345'. *Transactions of the American Philosophical Society* 80.6.

Millás Vallicrosa, J. M. 1950. *Estudios sobre Azarquiel.* Madrid.

North, J. D. 1986. *Horoscopes and History.* London.

Samsó, J. 2008. *Astrometeorología y astrología medievales.* Barcelona.

Samsó, J. and Berrani, H. 2005. 'The Epistle on Tasyīr and the Projection of Rays by Abū Marwān al-Istijī', *Suhayl* 5:163–242.

Sawyer, F. W. 1977. 'On Ptolemy's Determination of the Apsidal Line for Venus'. Pp. 169–181 in Y. Maeyama, W. and G. Saltzer edd. *Prismata. Festschrift für Willy Hartner.* Wiesbaden.

Swerdlow, N. M. 1987. 'Jābir ibn Aflaḥ's Interesting Method for Finding the Eccentricities and Direction of the Apsidal Line of a Superior Planet'. Pp. 501–512 in D. A. King and G. Saliba edd. *From Deferent to Equant. A Volume of Studies in the History of Science in the Ancient and Medieval Near East in Honor of E. S. Kennedy.* New York.

Tuckerman, B. 1964. *Planetary, Lunar and Solar Positions A.D. 2 to A.D. 1649 at Five-Day and Ten-Day Intervals.* Philadelphia.

Ancient Scepticism by Harald Thorsrud

Ancient Philosophies. Berkeley/Los Angeles: University of California Press, 2009. Pp. vxi + 248. ISBN 978–0–520–26026–9. Paper $24.95

Reviewed by
Katja Maria Vogt
Columbia University
kv2101@columbia.edu

Harald Thorsrud's introduction to ancient scepticism is written for an audience of undergraduate students and non-specialists who want a knowledgeable, trustworthy, detailed, and nevertheless accessible introduction. Thorsrud is so successful at this that, in effect, his book will also be valuable for graduate students and scholars.

An introduction to ancient scepticism is no easy task—one must explain the arguments of a wide range of thinkers, whose names often mean next-to-nothing even to educated readers. First, there are the sceptical philosophers, among whom are Pyrrho, Arcesilaus, Carneades, Philo, Cicero, Aenesidemus, Agrippa, and Sextus Empiricus. Next, there are their contemporary interlocutors and opponents: various Stoics, Epicureans, Platonists, Peripatetics, and so on. Finally, there are the doxographers and commentators through whose writings we have much of our information about ancient scepticism. Accordingly, an introduction to ancient scepticism must be informed by a nuanced understanding of large parts of classical philosophy, of the goals and idiosyncrasies of various lesser known authors, as well as of the longstanding history of some of the basic intuitions, often reaching back to Presocratic philosophy.

A good introduction to ancient scepticism is, thus, a considerable achievement. Every chapter in Thorsrud's book displays the kind of nuance and judgment that can only come from detailed study of more than the immediately relevant writings of ancient sceptics. At the same time, the book is utterly readable and engaging—it has the potential to steer students towards an otherwise seemingly inaccessible field, and capture their philosophical imagination.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 105–110

Thorsrud's discussions involve detailed acquaintance with and exposition of the scholarly literature. Thus, they do not only introduce the ancient material. They are also an excellent starting-point for further study. At many points, Thorsrud does more than explain the controversies among interpreters; he makes suggestions that constitute interesting contributions to these debates.

The book divides up into a plausible sequence of nine chapters:

1. Introduction
2. Pyrrho and Timon
3. Arcesilaus
4. Carneades
5. Cicero
6. Aenesidemus

7.–9. Sextus Empiricus

and comes with very useful additional material (chronological tables, indices, extensive bibliographies for further study).

There are two ways in which I think a new introduction to ancient scepticism might have done more, drawing on the surge of research on Hellenistic philosophy in recent years. First, there could have been even more attention to the different concepts employed in ancient as compared to modern scepticism; second, there could have been greater engagement with the Epicurean camp of anti-sceptics. I shall explain both of these points briefly.

Study of ancient scepticism has long been impeded by the fact that scholars were much better acquainted with early modern scepticism than with the more inaccessible ancient versions. From this perspective, it seemed obvious that scepticism must be about doubt, certainty, and knowledge—the key conceptions of modern scepticism. Who else is the sceptic but someone who doubts things? And what else is she calling into question if not knowledge, or certainty? As surprising as it may seem, none of these terms is central to ancient scepticism. The ancient sceptics have an intuition that is mostly absent from modern discussions: if one sees that one should not claim to know something (say, because there are countervailing considerations), then one should also not believe it. In every belief, we make a truth-claim. But why should one claim that p is true, if it might be false? Accordingly, ancient discussions quickly turn from the concept of knowledge to the concept of a criterion of truth, and thus to the question of whether there are impressions (perceptions, appearances, or thoughts) that can be recognized as true. If it is hard to establish a criterion of truth, then perhaps we should hold back from forming beliefs. In this line of thought, certainty and doubt play no role. Indeed, neither are there words for these ideas nor is there conceptual space for them. Ancient scepticism is importantly motivated by the question of whether and how one can identify truths as truths. This question is different from how one can find something that is certain so as to build on it.

To his credit, Thorsrud speaks of doubt only in his introduction [10], employing more precise vocabulary once he explains particular sceptical philosophies (at the end of the book, he points out that suspension of judgment should not be mistaken for doubt [182]). Things are somewhat more complicated when it comes to knowledge and certainty. Thorsrud describes matters as if the notions of the 'absolutely certain' and of 'knowing with certainty' figured in ancient discussions [47, 43]. However, it is not clear that any of the participants in ancient epistemological debates would see a plausible distinction between knowledge and certain knowledge. Thorsrud also speaks of 'isolated bits of knowledge' and 'isolated bits of certainty' when describing the debates between Arcesilaus (the first Academic sceptic) and Zeno (founder of the Stoa) [47–48]. It is a particularly intriguing and difficult aspect of Stoic epistemology (one of the dogmatic theories that the sceptics engage with in great detail) that such a thing is impossible. One does not have knowledge until one has a whole system of knowledge. Knowledge is 'unchangeable by argument' [Sextus, *Adv math.* 7.151]; that is, to know something means to hold it to be true in such a fashion that one shall not change one's mind. But no single truth-claim is unchangeable if it is not part of a body of unchangeable truth-claims. Thorsrud's choices could be considered harmless glosses, justified by the aim to provide an accessible introduction. However, in so far as he aims to explain the distinctiveness of ancient (as opposed to later) scepticism, they are not always helpful. Perhaps as a consequence of explaining matters in terms of certainty, Thorsrud devotes somewhat less attention to a central concept in Hellenistic epistemology, the criterion of truth, than one might expect.

However, Thorsrud is impressively subtle in many other respects that concern precisely such matters. For example, when considering whether the Pyrrhonian sceptic has beliefs, Thorsrud makes it clear that, whatever our answer to this question, it cannot invoke mental states, understood as something genuinely different from facts about the world [175–180]. Thorsrud explains that Sextus' sceptic confines herself to appearances. For example, the sceptic does not deny that the honey appears sweet, but does not claim that the honey is sweet [*Pyrr. hyp.* 1.20]. If the sceptic has any beliefs, then these will have to be explained in terms of what appears to her. Appearances are something like affections of the mind, and so they might be described as mental states. Does this mean that the sceptic has beliefs in so far as she has beliefs *about* her mental states? Thorsrud does not make the point that I think we should mention first in this context: that the idea of a reflective turn of the mind upon itself does not figure in Greek scepticism. But he explains in a very clear fashion why the 'mental states interpretation' cannot be convincing. First, the sceptics do not claim that there is such a thing as mental states (which would be a dogmatic thesis). Second, if there were mental states, they would count as part of how the world is. Accordingly, this move does not provide the sceptic with beliefs that would differ from the beliefs she does not have—beliefs about how things are in the world. An important difference between ancient and modern scepticism is implicit in Thorsrud's argument: from the point of view of Hellenistic discussions, there is no difference between the mind and the world such that the mind would not be part of the world.

Consider next the role of Epicurean philosophy. Thorsrud's introduction, as nuanced as it is in other respects, is perhaps somewhat conventional here. Scholars usually see the Stoics as the main philosophical interlocutors and opponents of the sceptics. There are many respects in which the sceptics seem to engage Stoic premises directly or to respond to Stoic objections. This observation has been central to the so-called dialectical interpretation of scepticism, according to which sceptics, rather than putting forward any views of their own, argue from the premises of their interlocutors, leading them to conclusions based on their own assumptions. Perhaps it seems easier to explain this mode of argument if there is one prime interlocutor. However, the dialectical approach ultimately works just as well if there are several philosophers for the sceptic to talk to.

Pyrrho and Epicurus apparently knew of each other: from the very beginning, scepticism and Epicurean epistemology are antagonists. Some of the more extreme Epicurean theses such as 'all perceptions are true' are perhaps formulated in such radical fashion because they engage with scepticism. Where the sceptics see conflicting appearances, the Epicureans only see difference. They display the mind-set of natural scientists: every perception has its causal history and is, therefore, explicable. In so far as the physics and physiology of perception account for it, it is a fact; and in that sense, it is true. There are different perceptions, but no conflicting perceptions.

The relationship between scepticism and Epicureanism is an under-explored topic, and it is probably not the task of an introduction to ancient scepticism to remedy this. However, some of Thorsrud's own arguments would have been helped by supplying the missing link that, as I would suggest, can be found in sceptical engagement with Epicureanism. Here are three examples. First, Thorsrud analyzes Arcesilaus' response to the question of whether the sceptic finds the bath when wanting to go to the bath, as if Arcesilaus were responding to a Stoic [51]. But this is not a compelling interpretation. In the relevant text, Plutarch moves from sceptic engagement with Stoic premises to an exchange between the sceptics and Colotes, an Epicurean [*Adv. Col.* 1122a–d]. Colotes asks the sceptics how they find the bath, and how they find the door when they leave a room. In response, the sceptics employ Epicurean, not Stoic, premises: that it is one thing to have one's perception of the door available to one, and another to form a belief based on it.

Second, Thorsrud notes that the Stoics are not the obvious interlocutors (or not obviously the sole interlocutors) for Carneades, the second major Academic sceptic [81]. Carneades develops a criterion by which his sceptic is guided in her actions, the convincing (τὸ πιθανόν). In matters of greater importance, the sceptic adheres to a stricter criterion, the convincing and undiverted. In matters of the greatest importance, she adheres to her strictest criterion, the convincing, undiverted, and thoroughly examined. This approach is structurally analogous to Epicurean methods for examining perceptions [cf. Sextus, *Adv. math.* 7.211–216]. Thorsrud's observation, that we need to think of a different interlocutor here than the Stoics, or of an additional interlocutor, seems right. We should think of Epicurus.

Third, consider the assumption that arguably figures in Aenesidemus' scepticism: if x is by nature F, then it affects everyone as F [111–116]. Thorsrud [197–199] mentions that Sextus also employs this premise in his discussions of ethics [*Pyrr. hyp.* 3.179–187, *Adv math.* 11.110–166: cf. *Adv math.* 8.189]. He reconstructs Sextus' arguments as if they were directed at Stoic ethics, based on what I think is a somewhat forced account of a Stoic position instead. A more straightforward account, however, can be given if we think of Epicurus as the sceptic's opponent. Epicurus argues that pleasure is the good because it affects everyone as good, just as fire affects everyone as hot, snow as white, and honey as sweet [Cicero, *De fin.* 1.29–32]. That is, he provides precisely the kinds of argument and examples that the sceptics engage.

But these are points of detail. In sum, *Ancient Scepticism* is to be highly recommended. Thorsrud's interpretations are based on subtle analyses both of the ancient texts and their modern interpretations. The book is a joy to read as well as philosophically engaging and broad in scope. With very few exceptions, Thorsrud does not simplify things in any problematic ways, which is a rare achievement in a book that genuinely functions as a lively and accessible introduction.

Asklepios, Medicine, and the Politics of Healing in Fifth-Century Greece: Between Craft and Cult by Bronwen L. Wickkiser

Baltimore: Johns Hopkins University Press, 2008. Pp. xiv + 178. ISBN 13–978–0–8018–8978–3. Cloth $55.00

Reviewed by
Vivian Nutton
The Wellcome Center for the History of Medicine, UCL
ucgavnu@ucl.ac.uk

This is a revised version of part of a 2003 Austin thesis, directed by Lesley Dean-Jones. Despite the restrictions in its title, it offers new ideas about a variety of healing cults in later Greece, and even in Italy, although its main focus is on the burgeoning of the cult of Asclepius and specifically its arrival in Athens between 421 and 417 BC. After decades in which scholars largely contented themselves with quoting the literary evidence assembled by the Edelsteins in their deservedly famous *Asclepius* [1945], the last 20 years have seen a revival of interest in ancient healing religions, led principally by archaeologists and epigraphists. The range of easily accessible material has expanded enormously; new journals dealing with ancient religion like *Kernos* have sprung up; and young scholars in Italy, Germany, France, and the USA have challenged many of the older presuppositions about healing cults in Classical Antiquity. Above all, there has been a welcome return to setting ancient religion within a civic, and often a political, context. From being a dully antiquarian study, ancient religion has become a very controversial topic, with new and important contributions constantly appearing. Although Wickkiser has done her best to integrate some very recent studies into her revision, she has at times been unable to do little more than add a footnote reference; and one would have liked to hear more of her views on Melfi's *I santuari di Asclepio in Grecia* [2007] or on Riethmüller's massive *Asklepios. Heiligtümer und Kulte* [2005], a fundamental survey of the archaeological, epigraphic, and numismatic evidence for shrines of Asclepius in the Ancient World.

But these weaknesses are unavoidable in a fast-changing debate and do not seriously detract from the value of this short book. Elegantly written, and with a sound command of the original Greek, it

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 111–115

provides an excellent introduction to the rise of Asclepius' cult in Athens.[1] It also promotes a clear and challenging thesis. In her view, the spread of Asclepius' cult is in large part the result of the rise of medicine as a craft in the late fifth century; and, at Athens in particular, it was deliberately fostered by leading Athenians and by the Athenian state for their own political reasons. The introduction of Asclepius into the city in 420/419 BC was not a private initiative but one sanctioned at the highest civic level. It was not a response to the plague of 430–426, but part of a political rapprochement with neighbors across the Saronic Gulf to gain allies against Sparta. Neither thesis is entirely new, but Wickkiser provides some fascinating perspectives, even if some of her conclusions require modification.

Wickkiser rightly rejects the traditional dichotomy between religion and healing, and refuses to see Athenian, or Greek doctors, for that matter, deliberately setting out to create an anti-religious system of healing. Her arguments are compelling, not least because of the part played by doctors in endowing healing shrines and, as at Athens, participating in certain cult rites and practices. But she goes too far in claiming that Asclepius' cult burgeoned as a reaction to the rise of medicine, and as a response of patients faced with doctors who were now encouraged to avoid treating the sick as part of their new professionalism. Asclepius thus stepped in when doctors abandoned their patients. The god cured, because doctors in a sense allowed him to.

But this is a difficult thesis to sustain for several reasons. The first is simply the absence of evidence. There is nothing to show that earlier doctors did not regularly refuse to treat patients whom they considered incurable, or that the desire of the sick to be healed by whatever means, including the divine, was not prevalent also in the sixth century. Some patients and their families grumbled when doctors refused to help (just as they do today), but the author of the *Art* expected to persuade them that only the incompetent doctor expected to cure every single case—a sentiment that might well

[1] Misunderstandings are few. At p. 57, Wickkiser mistakes a reference to Asclepius as Galen's 'ancestral god' (i.e., both come from Pergamum) for a claim to his being a direct descendant, an Asclepiad like Stertinius Xenophon (who also boasted descent from Hercules, another healing god). The epigraph on p. 10 syncretizes Herophilus of Chalcedon and Erasistratus of Ceos.

have been shared by the wider public. Secondly, the evidence itself, when it exists, is far from convincing as to what happened. Some of the texts cited by Wickkiser date from well after the fifth century, a fact that is still likely to trip up the unwary reader of Edelstein's *Ancient Medicine* [1967]; and one must be careful not to read back into history documents that may not have been written until centuries afterwards.[2] Much of the advice comes from prescriptive texts on how a doctor should behave—others, by contrast, also discuss palliative care—and it is not always easy to see how these recommendations were put into practice. The situation of the isolated sufferer from *phthisis* in Isocrates' *Aegineticus*, abandoned by friends, most of his family, and doctors hardly depends on recent developments in medical ethics or professionalisation, but reflects a typical human reaction when faced with a distressing, chronic, and fetid illness. The arrival of Asclepius, at least in Attica and perhaps elsewhere, also seems to overshadow existing healing cults, too often forgotten in the story.

Wickkiser's second thesis is more convincing, even if her rejection of any influence from the recent experience of the plague may be excessive. She follows Parker, Clinton, and others in emphasizing that in Classical Greece religion was not just a private matter. The arrival of Asclepius' cult, whatever the role of Telemachus, was sanctioned by the Athenian authorities: it was not some private whim. Wickkiser develops the observations of Clinton about the interactions of Asclepius' cult with both the Eleusianian mysteries and the cult of Dionysus to demonstrate in a clear and convincing manner that the location of the shrine of Asclepius placed it at the very center of Athenian imperial ambitions. Unlike the Asclepieion of Rome, which was on an island in the Tiber and not quite in the city, the Athenian shrine lay on the slopes of the great religious center of Athens, the Acropolis, and visitors to one of its major festivals, the Dionysia, took their seats in the theatre immediately below the walls of the shrine. Even if a slightly different political context for the arrival of Asclepius can be envisaged, and even if the later rapid spread of

[2] *Pace* p. 131, the *Oath* seems to have been known, and disliked, by Cato in the early second century BC, since he sees it as proof of a conspiracy of doctors against their patients. Wickkiser, like many other scholars, seems not to know the fragments of Galen's commentary on the Hippocratic *Oath*, published by F. Rosenthal [1945], although Galen's observations contain much of relevance.

Asclepius' cult may owe less to Athenian political and military power than Wickkiser implies, her neat demonstration of the interplay of religion and politics is convincing. The thesis itself is not new, but the evidence and arguments that are used by Wickkiser to support it expand our understanding of the whole development.

Given that Wickkiser pursues some of her researches in this book well into the Hellenistic period, it is somewhat surprising to find nothing of the role of Asclepius' cult at Messene [see Riethmüller 2005, 1.141–143, 2.156–167]. Here in the revived city, the shrine of Asclepius was erected in the main square in the center; and legends grew up insisting that Asclepius was a local hero god, not an import from Epidaurus or Thessaly. Here Asclepius' cult is used to establish, or re-establish, a city's political and cultural identity. Isyllus' hymn to Asclepius at Epidaurus has also recently been placed in a political as well as a religious context by Antje Kolde [2003]. One might also wonder whether the relatively limited influence of the cult of Asclepius at Tricca was not also the result of that region's political impotence throughout the whole of Classical Greek history [see Aston 2004].

This is a valuable book, even if in its over-eagerness to push its theses it seems rather unconvincing at times. It shows how much the history of ancient healing cults has developed since the Edelsteins, and provides the Anglophone reader with a sound guide to the introduction of the god Asclepius into Athens and Attica. There are indications of how Wickkiser might approach Asclepius' cult in other regions around the Mediterranean, or characterize it in relation to Judaism and Christianity, and it would be good to see them developed further in another book.

BIBLIOGRAPHY

Aston, E. 2004. 'Asclepius and the Legacy of Thessaly'. *Classical Quarterly* NS 54:18–32.

Edelestin, L. and Edelstein, E. J. 1945. *Asclepius: Collection and Interpretation of the Testimonies.* Baltimore, MD. Repr. 1998.

Kolde, A. 2003. *Politique et réligion chez Isyllos d'Epidaure.* Basel.

Melfi, M. 2007. *I santuari di Asclepio in Grecia.* Rome.

Riethmüller, J. W. 2005. *Asklepios. Heiligtümer und Kulte.* 2 vols. Heidelberg.

Rosenthal. F. 1956. 'An Ancient Commentary on the Hippocratic Oath'. *Bulletin of the History of Medicine* 309: 52–87. Repr. in Rosenthal 1970.

——— 1970. *Science and Medicine in Islam: A Collection of Essays.* Aldershot, UK/Brookfield, VT.

Temkin, O. and Temkin, C. L. 1967. edd. *Ancient Medicine: Selected Papers of Ludwig Edelstein.* Baltimore, MD.

A Response to Van Egmond on Høyrup, *Jacopo da Firenze's* Tractatus Algorismi

Jens Høyrup
Roskilde, Denmark
jensh@ruc.dk

It is with little pleasure that I sit down to formulate my objections to the review, written by an appreciated colleague, of my edition and study of Jacopo da Firenze's *Tractatus algorismi.*[1] However, the misrepresentations and distorted arguments in the review are so dense and so serious that I feel obliged (to myself, to the publisher and editorial board, and to the scholarly field in question) to respond.

I have no complaints about the fact that the reviewer would have liked me to write a different book directed at the general and not a specialist public. If he thinks that a competitor to Frank Swetz' *Capitalism and Arithmetic* [1987] is needed (and it may well be), he should be in the optimal position to write it himself.

To start with the positive: I am grateful to the reviewer that he has discovered my mistaken transcription and translation of the rule of three; my mind must somehow have been infected by the ensuing identification of this third thing as 'the other that remains'.[2]

[1] Since the reviewer speaks of me almost solely as 'the editor', I shall refrain from mentioning his name except in quotations.

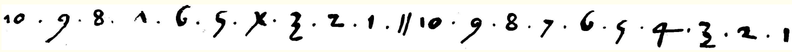
[2] In full, the rule runs as follows:

> *<S>e ci fosse data alcuna ragione nela quale se proponesse tre cose, sì debiamo multiplicare sempre la cosa che noi vogliamo sapere contra a quella che non è simegliante, et parti nela terza cosa, cioè, nell'altra che remane.*
>
> If some computation should be given to us in which three things were proposed, then we should always multiply the thing that we want to know against that which is not similar, and divide in the third thing, that is, in the other that remains.

Contrary to what the reviewer states, the rule gets its name *of three things* from the initial '*three things* [that] were proposed', not for the appearance of the third thing within the rule.

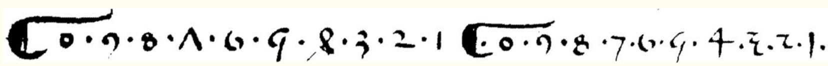
ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 116–126

I am also glad that he noticed that my reproduction of the shapes of numerals on Høyrup 2007, 196 is wrong—not because I could not draw them correctly but because I mixed up two computer files with almost identical names. The shapes that are rendered on my page 196 are those of the Trivulziana manuscript (**M**, for Milan), and accordingly reappear on page 385. However, this manuscript (and thus what I render) does not omit the 1 written before the zero in the indication of the old shapes; it writes it to the right. But in the indication of the new shapes, it does omit it. The shapes in the Vatican manuscript (**V**) are:



The 'old' and 'new' shapes of the Arabic numerals according to **V**

The Riccardiana manuscript (**F**, for Florence), the one which the reviewer considers by far the oldest, omits the 1 in both places:



The 'old' and 'new' shapes of the Arabic numerals according to **F**

What the original author did is thus not clear at all.

According to the reviewer, the omission of the 1

> when combined with the reformatting of the tables [of continued division], might give the impression that the author wrote the zero separately and not always as part of the number 10. [39]

The 'reformatting' of which he speaks refers to a greater spatial separation of columns that have nothing to do with each other, and thus can give no impression of the kind. And indeed, the tables with continued divisions contain many remainder zeros, transferred to a separate column in the next row (in all three manuscripts). So, here the reviewer is mistaken on both accounts.

What he says [39] about 'a systematic rendering of the [Arabic] numeral "1" as the lower case letter "j"' is equally mistaken, and shows that he has not read the pages just before the edition itself explaining that this shape (simply a long "i" and not a separate letter “j”, which was only invented centuries later) is rendered “j” ‘when it represents the Roman numeral 1 and stands as the last in a sequence (thus j, vij, xiij, etc.)’ [Høyrup 2007, 190]. This follows the convention of the epoch and of the manuscript; and I cannot imagine that the reviewer does not know it. What I render “j” is everywhere a Roman numeral in the manuscript; it is long, and it is prolonged below the line; comparison with the correlated writings of numbers in Roman and Arabic style on fol. 2v leaves no doubt. At times, it is provided with a ‘phonetic complement’, a small “o” written above the numeral (for typographic convenience I omitted this from the edition, which was perhaps a mistake).

Another complaint also comes from the reviewer’s failure to read what I say about my editorial principles (and from misreading the edition). He writes that

> while comparing the text with the original, I found that the editor has omitted all of the corrections that the copyist himself made, perhaps because there were so many. [42]

This is simply nonsense. On the same page as before, he would have found that

> passages in < > repair copyist’s omissions, in the translation also copyist’s errors; the occasional superscript letters (< >$^{\text{M+F}}$, < >$^{\text{M}}$, < >$^{\text{A}}$) refer to a manuscript or manuscript group on which the restitution is based. Letters, words and passages in { } are present in the manuscript by error; those that are deleted by the copyist are struck out in the text edition and omitted from the translation; words or passages that were at first omitted by the copyist and afterwards inserted above the line are marked ˆ ˆ, whereas insertion in the margin is marked * *. Editorial comments are in [ ], added words in the translation in ( ). Passages in italics in the edition correspond to the use of red ink in the manuscript. [Høyrup 2007, 190]

I cannot guarantee that I have not overlooked one or two corrections—editors make errors—but the reviewer speaks of omission of ‘all of the corrections’, which shows that he can have read very little of the text since he has not stumbled on any passage marked in this way.

Yet another complaint based on similar failure is that

> there is no common numbering for the paragraphs or sections of the text, so one cannot readily compare the texts in the two sections [containing, respectively, the editions of the Vatican and of the Trivulziana + Riccardiana manuscripts]. [43]

The reviewer has obviously not read page 380 [Høyrup 2007] (just before the edition of **M** and **F**, where indication of such things should be expected). There I write that

> for the numbering of paragraphs in **M**, I use those of my transcription of **V**; this should facilitate a comparison of these two manuscripts. Paragraphs that have no counterpart in **V** are assigned the number of the previous paragraph with an added letter A (and B if necessary); paragraphs that are displaced in **M** with respect to **V** are treated similarly, but the corresponding number in **V** is added in parenthesis.[3] For **F**, I indicate Simi's numbering.

Besides not reading the explanation of editorial principles, the reviewer has not even tried to compare the editions, since in this case he would have discovered that the numbering is the same for the Vatican (**V**) and the Trivulziana manuscript (**M**) to the extent that the differences make it possible. That I also indicate Annalisa Simi's numbering in her edition of the Riccardiana manuscript just below the corresponding number for **M** should not produce confusion but only facilitate comparison with her edition.

A final result of the reviewer's not reading the explanation of editorial principles is that he finds it 'extremely difficult to read' the edition of **M+F** [42]. For reasons explained in my book, it was reasonable to choose **M** as exemplar and to correct it where the reading of **F** was clearly better; this should be quite standard. Since only two manuscripts are involved, I then chose to indicate by super- and subscripts where one of the manuscripts deviates from the text that I had established in this way. This was intended to make it easier for the user to locate the deviations than if the apparatus had been put into footnotes. If the reviewer had read an italicized sentence on Høyrup 2007, 379–380:

[3] I omit the footnote in the original that gives examples.

> *Neglecting all superscript and subscript, one thus essentially gets a text which is close to the common archetype for the two manuscripts.*

he ought to have had no difficulty.

The reviewer is further dissatisfied that I did not make a critical edition of all three manuscripts. Actually, Annalisa Simi and the late Jean Cassinet had already prepared a critical edition of the Riccardiana and the Trivulziana manuscripts. As Jean Cassinet told me in 1999, they found the Vatican manuscript so different from the others that it was meaningless to make an edition of all three manuscripts—a claim that I still endorse. The expected appearance *in print* of this edition made my choice to prepare an edition of the Vatican manuscript obvious. But, as it turned out, the edition of **M** and **F** never did appear: the publisher lost the manuscript (after having brought the project so far that subscriptions were paid!), and those who took care of Cassinet's *Nachlass* did not find a copy [see 121n6 below]. At a late moment, I therefore decided to include what I call a 'semi-critical' edition of **M** and **F**—called thus because for **F** I relied on Simi's edition and not on the manuscript. This (except Cassinet's reason not to include **V**) is explained on pages 5 and 379 of my book. The reviewer's speculations and accusations in this respect are yet again built on a failing ability or will to read the work that he was supposed to review.

In other places, the reviewer has at least read enough to misrepresent what is written in the book. For instance, he writes that I

> came to this conviction [*viz.* that **V** represents the most authentic text] in 1997, when [I] first examined the algebra section in the Vatican manuscript and noticed how different it was from the traditional presentations of algebra that derived from the tradition of Mohammed bin Musa al-Khwarizmi. [41]

If that were the case, I would be a fool. If the reviewer's oft-repeated belief in the derivation of the *abbacus* tradition from the *Liber abbaci* were true, the differences should rather suggest a long development and thus a late date. Now, I still shared this belief with him in 1997, and only gave it up reluctantly years later.[4] What I wrote is indeed

[4] The 'detailed summary of the obscure 13th-century *Livero de l'abbecho* and ... comparison with the *Liber abbaci*' [45]—actually, not only a summary

something different, namely, that I realized that the algebra of this manuscript 'might have astounding implications for our understanding of the origins of European vernacular algebra' [Høyrup 2007, vi]. This has nothing to do with the Jacobean authenticity or the exact date of the text, and precise investigation of any orderly *abbacus* presentation of algebra might have served the same purpose. (I disregard the chapter copied from Fibonacci's *Liber abbaci* in Benedetto da Firenze's encyclopedic *Trattato* and a few similar encyclopedias, but not Benedetto's own presentation). The Vatican manuscript just happened to contain the first *abbacus* algebra that I worked on in depth.

Admittedly, all of this is peripheral, even though the last point is connected to the reviewer's main complaint: my 'obsession with proving the authenticity of the Vatican text' [45]. This accusation, however, can easily be turned around.

The first scholar to describe **V** was Louis Karpinski [1929]. Since he had not seen the other manuscripts, he took it to represent Jacopo's original treatise. The next scholar to look at it was apparently the reviewer himself who, as I wrote [2007, 5],

> inspected it in the mid-seventies during the preparation of his global survey of Italian Renaissance manuscripts concerned with practical mathematics [1976; 1980]. [...] Van Egmond noticed that the manuscript which Karpinski had examined (Vatican MS Vat. Lat. 4826, henceforth **V**) could be dated by watermarks to the mid-fifteenth century, and that the algebra chapter (and certain other matters) were missing from two other manuscripts which also claim to contain Jacopo's *Tractatus algorismi* (Florence, Riccardiana MS 2236, undated;[5] henceforth **F**; and Milan, Trivulziana MS 90, c. 1410; henceforth **M**).[6] Because **M** can be dated by watermarks to c. 1410,

but an analysis is presented—serves to show that the only argument that has ever been advanced for this generally accepted dependency of the *abbacus* tradition on Fibonacci is a fallacy.

[5] Høyrup 2007, 5n5:

> Van Egmond's dating [1980: 148] is misleading, since it is merely the date of Jacopo's original (which is given in all three manuscripts), not that of the manuscript.

[6] Høyrup 2007, 5n6:

> some 40 years before **V** (yet still a whole century after 1307), and since **V** contains rules for the fourth degree not present in the algebra of Paolo Gherardi's *Libro di ragioni* from 1328, Van Egmond decided (personal communication) 'that the algebra section of Vat. Lat. 4826 [was] a late 14th-century algebra text that [had] been inserted into a copy of Jacopo's early 14th-century algorism by a mid-15th-century copyist'.

The reviewer was apparently not aware that 'reducible fourth-degree equations were solved routinely in Arabic algebra at least since al-Karajī's time'. In his review, he calls this 'a very expansive claim' for which 'no source is ever given' [43]. It is indeed well known by everybody working on the history of Arabic algebra, and *should* also be known by anybody speaking about the 'achievements' of the *abbacus* masters and interested in distinguishing their innovations from their borrowings. Since the reviewer does not seem to know, I urge him to start with Roshdi Rashed's biography of al-Karajī [Rashed 1973, 243 col. B (the last six lines)].

Until recently, one of the reviewer's main arguments was based on his contention that **F** was from 1307.[7] Perhaps because of my objections [see 120n4], he has now understood that this claim cannot be upheld. Instead (and perhaps because of an 'obsession with

---

> A transcription of **F** was made by Annalisa Simi in [1995]. A critical edition of **F** and **M** by the late Jean Cassinet and Annalisa Simi was almost finished in 1999, but it got stuck with the publisher and is not going to appear (Maryvonne Spiesser, personal communication), for which reason I give a transcription of **M** with indication of all not merely orthographic variants with respect to **F** in the Appendix.
>
> As pointed out by Karpinski and Robbins [1929: 170], **F** had already been mentioned by Boncompagni in 1883 and by B. Lami, librarian of the Biblioteca Riccardiana, in 1754; however, they had not seen **F** and, therefore, could not know that it differs from **V** on important points.

[7] In an earlier paper, the reviewer refers indeed to 'an early 14th-century *Tractato de algorismo* [*sic*] found in Ricc. 2236 and therein specifically dated to the year 1307' [2008, 313]. In the same article and on the same page, he also transforms **M** into 'several later copies' of that manuscript which 'do not contain any algebra', without noticing that mistakes in **F** that are not found in **M** exclude this affiliation. **M** can thus not be a copy of **F**; the two must come from a common archetype.

proving the authenticity of' **F** on his part), he now explains that this manuscript

> is written on vellum and so cannot be precisely dated; but the fact that it uses vellum (which was largely abandoned for writing common texts by the middle of the 14th century), combined with its ink, handwriting, language, and style, make it clear that it was written in the early 14th century, and thus must be accepted as the oldest text. [40]

Yet his own catalogue of *abbacus* manuscripts shows that two thirds of the conserved *abbacus* manuscripts written on vellum are from the 15th or the early 16th century, and that the *corsiva gotica cancelleresca* (the script of **F**) was used in *abbacus* books at least until the very end of the 14th century.[8] I cannot judge the ink, and I fear that the reviewer is in no better situation. Moreover, I can see nothing in the language which could not just as easily belong to the early 15th as to the early 14th century. This really looks to me like 'weak stylistic impressions' [42], whereas my references to the stylistic homogeneity of **V** and its partial agreement with features of **M** and **F** build precisely on 'the hard evidence of textual comparison' which the reviewer then characterizes as 'the editor's complex linguistic arguments [and] detailed discussion of alternate spellings, words, phrases, and word ratios' which 'will bore anyone but the most dedicated student of Italian linguistics' [45].[9]

---

[8] See Van Egmond 1980. 15th- and 16th-century manuscripts on vellum are mentioned on pages 73, 96, 143, 158, 165, 168, 173, 175, 178, 232, 247, 257, 261 (twice), 262 and 275; *corsiva gotica cancelleresca* used after 1350 is mentioned on pages 48, 137, 138, 211 and 250. Quite apart from what I may have overlooked, both lists are likely to be incomplete because dates stated in the manuscript or derived from internal evidence belong with the original and not with the actual copy.

[9] As I explain [Høyrup 2007, 55], similarities with a *Trattato di tutta l'arte dell'abacho* apparently written in Avignon in 1334 (as argued convincingly by Jean Cassinet) indicate that the compiler of the shared archetype for **M** and **F**, if not working in Provence, used material which was produced there—and indeed during the first half of the fourteenth century. However, the obvious deviations from this common archetype are at least as many in **F** as in **M**. Even if **F** should be written before (say) 1340, it is therefore not to be considered better than **M**.

The other main argument concerns the algebra contained in the Vatican manuscript. The reviewer sticks to his original opinion that it is a mid 15th-century insertion into a late copy of Jacopo's treatise, though he now adds arguments developed in Van Egmond 2008. He states that

> two late 14th century algebra texts [from the 1390s] now in the Biblioteca Nazionale Centrale di Firenze, Fond. Prin. II. V. 152, folios 153r–166r, and Conv. Sopp. G. 7. 1137, folios 110r–111v, give exactly the same equations as the Vatican text in exactly the same order [Van Egmond 2008, 313]. [44]

for which reason they must be regarded as sources for the Vatican algebra. At this moment of writing, I do not have access to the latter manuscript. But in the paper to which the reviewer refers, he himself states that it deals with 22 equations (read 'equation types'), not with 20 as does the Vatican manuscript algebra. Moreover, concerning the former manuscript, the same article states (correctly) that it contains 25 equations (i.e., equation types). So, already on this elementary level, the reviewer is unable to remember what he published two years ago. Worse is that 'the hard evidence of textual comparison' would have destroyed his claim completely. What he compares are just abstract equation types, rather than the level of the treatises or their words or their examples (the actual equations). Florence, Fond. Prin. II. V. 152 is a very advanced treatise. Its last three equations are of types $ax^3 + bx^2 = n$, $ax^3 = bx^2 + n$, and $bx^2 = ax^3 + n$; and it is shown how to reduce these to equations without a second-degree term—exactly the trick Cardano used 150 years later. The treatise also contains a discussion of the sequence of algebraic powers and schemes for the multiplication of polynomials, all of which is absent from the Vatical algebra (and certainly far beyond its author's horizon).

If we look at the examples given in the Vatican algebra and in Florence, Fond. Prin. II. V. 152, they are also very different.[10] On the other hand, the Vatican examples are shared with various algebras from the earlier 14th century (Gherardi and others), as shown in

[10] One, a very popular type, is shared; but the same type is also shared with Gherardi. The numerical parameters of the three are different.

a scheme given on Høyrup 2007, 160. Moreover, a *Trattato dell'alcibra amuchabile* (included in Florence, Riccardiana 2263) written *ca* 1365 (as dated by watermarks by the reviewer in 1980) contains everything from the Vatican algebra in so identical a form that it would not only be sensible but also very easy to make a critical edition of the two. In one place, however, the Vatican algebra leaves spaces (stating that its original did so) where it should have transformed $4\sqrt{54}$ into a pure square root. Here, the *Alcibra amuchabile* has $\sqrt{864}$, showing that it represents a more developed form of the treatise. The *Alcibra amuchabile* contains a few more equation types and for these it agrees with Gherardi with one exception; and where the Vatican algebra contains no examples, the younger treatise also has the same examples as Gherardi. However, the agreement with Gherardi's formulations is not nearly as close as with those of the Vatican algebra.[11] All of this is described in my book.

In conclusion, the Vatican algebra can be safely ascribed to the first half of the 14th century. The reviewer's neglect of all evidence showing this vitiates his objections.

A third argument against the genuineness of the Vatican manuscript is the reviewer's rejection of my characterization of this manuscript as 'a meticulous (yet not blameless) library or bookseller's copy made from another meticulous copy'. He protests that it contains a number of erasures and insertions of forgotten words between the lines and in the margin. But he overlooks that in the era when no corrections in proof could be made, this is in fact evidence of meticulous copying. On occasion, the copyist even corrects one spelling (which he has used elsewhere) into another one which is also used elsewhere, showing that he is trying to follow the orthography of his original.

I shall stop here, even though other distortions could be listed. Readers who are interested in what is really to be found in my book and cannot afford the exorbitant price or get it from a library may (at least for the time being) find the first 48 pages on the Google

[11] In Van Egmond 2008, 305, the reviewer claimed that this algebra repeats Gherardi's 15 equation types 'in exactly the same order'. This is simply not true: Gherardi's no. 8 is no. 11 in the *Alcibra amuchabile*, and his no. 14 is missing from the other treatise. This is not the only incorrect statement in that article.

books website. Preprint versions of my text editions and related papers can also be found at http://www.akira.ruc.dk/~jensh/Selected themes/Abbacus mathematics/.

BIBLIOGRAPHY

Høyrup, J. 2007. *Jacopo da Firenze's* Tractatus Algorismi *and Early Italian Abbacus Culture.* Basel/Boston/Berlin.

Karpinski, L. 'The Italian Arithmetic and Algebra of Master Jacob of Florence'. *Archeion* 11:170–177.

Rashed, R. 1973. 'Al-Karajī (or Al-Karkhī), Abū Bakr Ibn Muḥammad Ibn Al Ḥusayn (or Al-Ḥasan)'. Pp. 240–246 in C. C. Gillispie ed. *Dictionary of Scientific Biography.* vol. 7. Detroit.

Simi, A. 1995. 'Trascrizione ed analisi del manoscritto Rice. 2236 della Biblioteca Riceardiana di Firenze'. *Universita degli Studi di Siena, Dipartimento di Matematica. Rapporto Matematico* 287.

Swetz, F. 1987. *Capitalism and Arithmetic: The 'New Math' of the 15th Century, including the Full Text of the Treviso Arithmetic of 1478.* Translated by D. E. Smith. La Salle, ILL.

Van Egmond, W. 1976. *The Commercial Revolution and the Beginnings of Western Mathematics in Renaissance Florence.* PhD Dissertation, Indiana University. Bloomington, IN.

________ 1980. *Practical Mathematics in the Italian Renaissance: A Catalog of Italian Abbacus Manuscripts and Printed Books to 1600.* Florence.

________ 2008. 'The Study of Higher-Order Equations in Italy before Pacioli'. Pp. 303–320 in Joseph W. Dauben *et alii* edd. *Mathematics Celestial and Terrestrial: Festschrift für Menso Folkerts zum 65. Geburtstag.* Halle.

________ 2009. rev. Høyrup 2007. *Aestimatio* 6:37–47.

Aristotle on Life edited by John Mouracade

APEIRON 41.3. Kelowna, BC: Academic Printing and Publishing, 2008. Pp. x + 197. ISBN 978–0–020980–97–2. Paper $28.95

Reviewed by
Mariska Leunissen
Washington University in St. Louis
mleuniss@artsci.wustl.edu

In a famous passage from the *Parts of Animals*,[1] Aristotle encourages his students not to shy away from the study of even the least attractive of animals, because—quoting the words from Heraclitus—'there are gods here too.' Unfortunately, it has taken Aristotelian scholarship many centuries to take his advice. It was not until a few decades ago that scholars started to see the importance of Aristotle's investigations of life, both for their own merits (Aristotle is the first to study living beings in a scientific manner and is also the founder of philosophy of biology) and for their pervasive influence on Aristotle's philosophy, in particular his philosophy of science and metaphysics [see, e.g., Gotthelf and Lennox 1987, Devereux and Pellegrin 1990, and Lennox 2001a].[2]

[1] *Parts of Animals* 1.5.645a7–23:

> For even in the study of animals disagreeable to perception, the nature that crafted them likewise provides extraordinary pleasures to those who are able to know their causes and are by nature philosophers.... For this reason we should not be childishly disgusted at the examination of the less valuable animals. For in all natural things there is something marvelous. Even as Heraclitus is said to have spoken to those strangers who wished to meet him but stopped as they were approaching when they saw him warming himself by the oven—he bade them enter without fear, 'for there are gods here too'—so one should approach research about each of the animals without disgust, since in every one there is something natural and good. [trans. Lennox, 2001b]

[2] For this development, the works of David Balme, Robert Bolton, David Charles, Allan Gotthelf, Wolfgang Kullmann, James G. Lennox, G. E. R. Lloyd, and Pierre Pellegrin may be singled out as having been of particular importance.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 127–138

The collection of seven papers assembled by John Mouracade in this special volume of *Apeiron*—which originated in the conference 'Aristotle on Life' convened at the University of Alaska in August 2007—continues this fairly recent re-appreciation of Aristotle's study of life. Its central aim is to display the unity underlying Aristotle's treatment of life by combining two approaches, one physical and the other metaphysical, to this issue, thereby collectively contributing to our understanding of Aristotle's 'ontology of living beings'.

In the introduction to the volume, Mouracade promises us an interesting, important, and thought-provoking set of papers. Overall, readers will find that the collection lives up to that promise. Each of the papers defends a radically new interpretation of some aspect of Aristotle's study of life, many of which I expect to generate considerable discussion in the field. In addition, there is a noticeable effort—perhaps strongest in Mouracade's own contribution to the collection—to make Aristotle's ideas about life relevant to modern philosophers and scientists, which makes the collection of potential interest to a larger audience. (Note that all the primary texts in this volume are offered in translation and that the use of Greek is relatively sparse). And finally, some of the authors raise methodological problems concerning the interpretation of Aristotelian texts, and their proposed solutions ought to be of relevance to all scholars of Aristotle. (For instance, Julie Ward theorizes about how to reconcile different texts within the Aristotelian corpus and whether one ought to postulate extra-theoretical claims in order to explain away inconsistencies [78–79].)

As is common with edited volumes of this type, the volume is less successful when viewed as a unified collection. The introduction offers no further theoretical background on the central theme of the papers, nor is it always clear how some of the papers contribute to this theme. On a few occasions, the authors make conflicting claims (for instance, Devin Henry and Margaret Scharle present opposing views on the question of whether for Aristotle material-efficient causes can operate independently of formal-final causes) or discuss similar texts (Aristotle's account of inheritance in *Generation of Animals* 4.3, for instance, plays a key role in the arguments of both Henry and Katayama): it would have been interesting to see more explicit interaction between the contributors in their papers.

The first paper, by Paul Studtmann, offers a conceptual analysis of Aristotle's notion of form (εἶδος). Studtmann starts off with an overview of the difficulties in Aristotle's characterizations of form in *Meta.* 1013a27–29 and 1013b20–25, followed by a six-page list of examples of the meanings of 'form' occurring in Aristotle's *Metaphysics*. The list illustrates Studtmann's general claim that there are 'bewilderingly many uses of "form" in Aristotle' [3]; but it is not entirely clear how the reader is supposed to evaluate these examples, as many are quoted out of context and do not appear to be examples of meanings of 'form' at all (surely, 'form' does not *mean* 'art', or 'a this', and so on).[3]

From this list, Studtmann selects 14 central meanings of 'form' and reduces these meanings to two basic conceptions: just as Aristotle distinguishes between the subject (ὑποκείμενον) as 'composite of form and matter' and the subject as 'matter', so too we are urged to assume that he distinguishes between the form as the form of a composite ('form-c', being a universal) and the form as the form of matter ('form-m', being a particular). Studtmann illustrates the form-m by reference to Aristotle's conception of the soul as a capacity (a conception which Studtmann takes to be of a particular form) and to the pairs of contrary capacities characterizing the four sublunary elements (where the matter that is being informed is—rather controversially—understood to be prime matter). Both souls and contraries are sources of the dynamical activities of the composite they inform, which makes form-m a form that informs matter, a particular, and a capacity-like entity that is a source of activities. Form-c is—without further argumentation—identified with Aristotle's concept of species. The relation of form-m and form-c is then characterized as one of functional determination: form-m is that which is necessary and sufficient to make something a member of a certain species, which is form-c.

Studtmann's next move is to group the 14 meanings of 'form' under these two basic concepts, and he finally concludes that both concepts fall under the general genus of order. This conclusion is

---

[3] I found some of the translations confusing: for instance, Studtmann translates οὐσία in *Meta.* 1013a27 as 'essence' [2]; but in his list of examples 'essence' translates τό τι εῖναι, whereas 'substance' is used to translate οὐσία [3–8].

not so much argued for (nor supported by any textual evidence), but rather, as Studtmann concedes, inferred from 'plausible interpretative assumptions' [26]—the plausibility of which is simply assumed as well. However, it is not certain that many readers will agree; without further argument, I do not find Studtmann's final taxonomy of form to be convincing.

In the second paper, Margaret Scharle argues in a very illuminating way that Aristotle's defense of natural teleology does not just pertain to living organisms but also to all natural phenomena, including the operation of the four sublunary elements. This means that for Aristotle, '*all* material and efficient causation in nature depends on formal and final causation' [29], and that this is ultimately the source of disagreement with his predecessors.

Her argument proceeds in three parts. First, she argues that Aristotle believes that material causes in nature are dependent on formal causes in nature. Although Scharle never specifies this, the dependency relation that she has in mind is presumably an ontological one, meaning that, at least in natural substances, matter cannot exist without form, and *vice versa*. Aristotle's critique of his predecessors then pertains to their failure to realize that even at the elemental level, material natures cannot operate independently of formal causation, and that their concept of material nature itself was, therefore, inherently confused.

Next, Scharle argues that for Aristotle efficient causes always require a formal and final cause, meaning that efficient causes cannot exist—let alone operate—independently of formal and final causes. The reason why Aristotle's predecessors failed to acknowledge this is that they thought that only manifest entities could be efficient causes, whereas Aristotle identifies efficient causes with internal, nonsubstantial principles. Natural substances, on this account, 'are efficient causes *only* because of the formal principles at work in them' [36]. And these formal principles are—if they are intrinsic efficient causes of their outcomes rather than mere accidental efficient causes—themselves dependent on the end and final cause to which they are directed.

Finally, Scharle argues that for Aristotle only intrinsic efficient causes count as efficient causes in a strict sense, and that accidental efficient causes are always dependent on efficient causation in the strict sense, which amounts to saying that there is never any efficient causation that is independent of final causation. It is here that the radicalism of Scharle's ontological dependency thesis becomes apparent, and where, for me at least, her argument loses some of its plausibility. Aristotle's natural treatises are full of explanations in terms of non-accidental efficient causes that act independently of final causes. The most famous is perhaps the example of the Moon's moving in between the Sun and the Earth as the efficient cause of an eclipse:[4] the Moon does not interpose itself for the sake of causing an eclipse, but Aristotle nevertheless thinks that this phenomenon can be explained scientifically.

Ultimately, Scharle hopes that her radical interpretation 'shows his [i.e., Aristotle's] views to be more relevant today', as they highlight certain *a priori* issues about the nature of material and efficient causation that 'cannot simply be settled by contemporary scientists' [43]. Although I am not convinced that a pan-glossian portrayal of Aristotle will help to increase contemporary interest in his natural philosophy, Scharle is surely right to stress the importance of studying Aristotle's views on the teleology of the four sublunary elements.

Devin Henry, in the third paper in this volume, presents a rich account of the complex causal relationship between a biological substance's material nature and its formal nature (identified with the living being's soul) in the generation of animals.

Henry starts by demonstrating that Aristotle's appeal to non-intelligent natures as causes of natural development is not, as Galen has argued, vacuous. Henry shows that Aristotle's references to a thing's nature are not explanatorily basic but rather imply more fundamental causal powers or δυνάμεις, because 'organismal natures are themselves a kind of *dunamis*' [50]. This δύνάμις, however, is not some kind of *virtus dormitiva*: Aristotelian δυνάμεις are real causal factors, whose effects can be tested empirically, and which—even from the perspective of modern science that allows capacities to enter into scientific explanations (Henry refers to the work of Nancy Cartwright here)—are potentially explanatory. In explaining the causal role of material natures in animal generation, Henry focuses

[4] See, e.g., *Posterior Analytics* 2.12. Devin Henry discusses some examples from biology [55–59].

on both their negative and positive role. Sometimes, the indeterminacy of matter 'impedes to [*sic*] the teleological efforts of the formal nature in the construction of the embryo' [55], and thereby causes the occurrence of birth defects and monstrosities. For the most part, however, the material and formal natures of the organism interact with each other and produce functional structures. In such cases, the formal natures co-opt the material natures, and material necessity gets subordinated to 'conditional necessity' (i.e., some materials are necessary *given* the development of a particular goal). And finally, there are a few cases where material necessity alone, independently of the teleological actions of the formal nature, produces functional features.

Next, Henry addresses the question whether formal natures are species-specific or individual-specific. Traditionally, scholars have held that the form that is transmitted in sexual reproduction only contains the species-specific features, and that the individual differences among members of the same species are due to material or environmental influences. Henry, on the other hand, defends a rather controversial reading (first proposed in the 1980s by scholars such as Balme and Cooper, but which 'failed to convince the general populace' [60]) that biological forms may include individual features. Henry discusses hitherto unexamined evidence from Aristotle's discussion on inheritance in *Generation of Animals* 4.3, which strongly suggests that individuals have at least some properties that are heritable, because there exist δυνάμεις in the individuals for the formation of just those properties. Since these δυνάμεις are generative capacities of the individual's soul, they must be part of that individual's formal nature, and forms must be individual specific in exactly this sense. Under this interpretation, individual features such as blue eyes and snub noses need not be material accidents; they are rather the outcome of intrinsic efficient causes —even if they do not serve a specific function. Henry concludes by suggesting tentatively, and perhaps even more controversially, that Aristotle might have had a concept of a species nature, which would imply that species are in fact individuals.

Julie Ward, who contributes the fourth paper in this collection, focuses on the question whether Aristotle uses the term 'human' synonymously across various social and political groups or homonymously. Even though, metaphysically speaking, all humans are equal with regard to their substantiality, Aristotle's remarks in the *Politics* about the restricted rational capacities in women and natural slaves suggest that there are some members of the human species that lack an intrinsic property of being human, namely, the capacity to deliberate. The remarks in the *Politics* also suggest a possible psychological inequality among human beings in so far as they entail (*pace* Aristotle's general account of habituation in the *Nicomachean Ethics*) that there are humans who cannot ever become virtuous through habituation.

Ward tackles this problem by first giving an overview of the various possibilities of homonymous and synonymous predication, while paying special attention to what she calls 'systematic homonymy'. The concept of human resists the usual diagnostic tests for homonymy (as described in *Topics* 1.15), but Ward argues that Aristotle's remark in the *Politics* that not all humans have the deliberative capacity (or at least not have it in the same way) nevertheless suggests that 'human' is to be taken as a homonymous term. In favor of preserving the synonymy of human, Wards develops three different solutions. First, she argues that, if the deliberative capacity belongs to reason, then the absence of deliberation might imply the absence of reason itself, which means that 'human' cannot be considered synonymous for all individual humans. Under this interpretation, 'human' can only apply synonymously to all mature, male, Athenian citizens. Ward calls this the 'restrictive range synonymy'. Second, if deliberation and reason are not co-extensive capacities, then it is possible for some being to have reason but not deliberation and still to be considered human in a synonymous sense. Ward calls this the 'de-linking strategy'. Third, she proposes that we allow for some plasticity in the concept of deliberation: all humans are rational and deliberative, but not to the same degree of completeness. Ward calls this the 'dual deliberation synonymy'. If none of these solutions works, she claims, we ought to conclude that Aristotle's concept of *human* is indeed homonymous.

Ultimately, Ward argues effectively that we should accept a modified version of the 'dual deliberation' view. Assuming that there are different ways in which humans can partake in the same activity, some humans only engage in an everyday type of deliberation, whereas others—the free male citizens of Athens—also engage in the specialized kind of practical reasoning that is necessary for becoming virtuous. Under this reading, which finds support in Aristotle's discussion of the different levels of potentiality and actuality in *De anima*, Aristotle can consistently hold that human beings are metaphysically equal, while being psychologically different.

In the fifth paper, Errol Katayama argues cogently that not all living beings exhibit the same degree of unity characteristic of substances. A typical organism exhibits both essential unity in number ('numerical substantial unity') which pertains to the unity between its soul and body, and unity in form ('formal substantial unity') which pertains to the unity between its formal, final, and efficient cause; but—so Katayama argues—hybrids and spontaneously generated animals possess neither of these forms of unity. Therefore, these latter kinds of living beings are not substances.

Katayama's argument proceeds by a detailed analysis of the relevant senses of essential unity Aristotle distinguishes in *Meta.* 5.6 and 9.1. In these investigations it is assumed that Aristotle in fact presents a criterion for the identification of substances in the *Metaphysics*, and that 'substance' refers both to composite organism and to the form of this composite. While the criterion of formal substantial unity identifies form as substance, the criterion of numerical substantial unity identifies composite substances; and something is a composite substance only if its form is a form-substance. Second, Katayama assumes that among the four senses of unity (i.e., unity of an organism, its form, its genus, and its λόγος), unity in one sense implies unity in all the other senses. And finally, Katayama identifies reproduction as the key unifying activity of living beings *qua* substance.

Katayama then singles out sterility as an example of a form of 'deformity' in animals that affects the substantiality of the animal as a whole, and traces the source of this deformity back to a defect in the nutritive part of the soul. All hybrids and spontaneously generated organisms suffer from such a defect, and Katayama explains how each of these kinds of animals fails to be a substantial unity in all the possible relevant senses. Based on an analysis of *Generation of Animals* 4.3, Katayama shows that there is a spectrum of deformities, with monstrosities lacking individuality (and, hence, substantiality) on one extreme end of the spectrum and female organisms, which have both individuality and universality (and, hence, substantiality) on the other end. Hybrids and spontaneously generated animals resemble monstrosities in that they too are not individuals and, therefore, fail to be substances.

Christopher Shields, in the sixth paper of this collection, offers a subtle and intricate defense of W. D. Ross' interpretation of Aristotle as excluding artifacts from attaining the 'dignity of substance'. Aristotle states explicitly that only living beings qualify as substances; but as Shields points out, it is not easy to see why Aristotle thinks that this is the case, especially because Aristotle *also* sometimes lists artifacts among examples of substantiality. Some commentators have proposed a 'paradigmatic reading', entailing that while there exist non-living substances such as artifacts, Aristotle considered living beings alone to be paradigmatic substances. Against this interpretation, Shields advances a more radical, exclusivist reading, entailing that—at least on the basis of Aristotle's theory of substance as presented in *Meta.* 7–9—*only* living beings are capable of existing diachronically as separate and determinate entities and, therefore, that only living beings are substances.

Shields' route to the exclusive interpretation is based on Aristotle's argument that only things with natures qualify as substances. All things with natures have an internal principle for motion and rest; but in order to be substantial the natural thing also needs to have the capacity to initiate, stop, and reverse the motion, and also to control the directionality of the motion. Thus, for natural things to count as substances, they have to be sufficiently 'cybernetic', which is 'already close to being living systems' [139]. For living beings, the cause of their systemic directionality is the soul and the soul is a substance. Souls as substances are, so Shields points out, sortally-determinate, that is, they are the cause of different kinds of living beings' being the kind of thing they are. In addition, souls are causes of the existence of living beings as unified beings, that is, souls are the internal organizing principles of the characteristic activities of the living being in question. These activities, in turn, are also unified in that all are for the sake of one single identifiable end. On this account, substances turn out to be 'irreducibly teleological systems with specifiable intrinsic goods' [143], which is co-extensive with being alive. This, then, explains why neither artifacts nor elements can count as substances.

The question then remains to what ontological category artifacts belong if they are not substances (Shields does not return to the question why Aristotle sometimes mentions artifacts as examples of substances). Shields argues that for Aristotle this question is misconceived, since it is based on the false supposition that artifacts are something determinate, whereas they are not. Only things 'which exist determinately require consideration for categorical membership' [144]. The indeterminacy of artifacts becomes clear in Shields' discussion of Aristotle's analysis of the Platonic paradoxes concerning growth. If we are to distinguish which entity is growing and which entity disappears in the process of growth, one of the two needs to be a substance that has non-conventionally specified ends. Since only living beings have non-conventionally specified ends, artifacts are unable of existing diachronically as separate and determinate things; thus, they lack the kind of determinacy and stable identity required for counting as a substance.

John Mouracade contributes the final and perhaps most ambitious paper in the collection. His main purpose is to set up Aristotle's concept of form—which is ultimately to be identified with DNA—as the theoretical link necessary for bridging the gap between biology and metaphysics in contemporary, non-reductive materialist views about personhood.

Mouracade first offers a rather quick overview of non-reductive materialist theories of persons. Grouping supervenience, emergence, and constitution theories together, Mouracade argues that these theories all fail to make clear how the person 'comes to be' from the body. Simply referring to biologists or pointing to the complexity of the body will not suffice to complete these theories. Animalism, on the other hand, entails an immediate connection between biology and metaphysics; but, as Mouracade points out, that does not mean that there is no more work left for the metaphysician: it still remains to determine what it is that makes something an organism. Mouracade then reviews theories of organisms by Eric Olson and Peter van Inwagen, but again concludes that these theories are ultimately defective. In order to complement these theories, he claims, one ought to invoke Aristotle's hylomorphism.

In the next section, Mouracade discusses the basics of Aristotle's hylomorphism and his theory of form. Here the most important point is that, in the natural treatises, form is ultimately developed as soul, which is the internal cause of unity that structures individuals—and which, according to Mouracade, is exactly the notion that is needed to complete the non-reductionist account. This Aristotelian concept of form is then linked to DNA, by which Mouracade means 'the entire "genetic complex"' [169n46]. DNA is well suited to play the role of formal cause, because, at least so Mouracade argues, it is formal; it is an internal cause of unity, self-regulation, and self-maintenance; and it combines efficient and final causality. It also provides the basis for both diachronic and synchronic identity.

Mouracade ends with a defense of Aristotelian teleology: once distinguished carefully from Platonic or vitalist theories, a naturalistic theory of teleology could be acceptable in biology and 'would allow for the understanding of DNA as a paradigmatic case of Aristotelian form' [175]. It remains to be seen whether biologists will concur with Mouracade's optimism, but the suggestion is an interesting one.

In sum, this collection presents a broad and diverse perspective on Aristotle's (meta-)physics of life, and forms a welcome addition to the growing scholarship on this topic. I recommend it to all those interested in ancient philosophical theories of form, substance, and life, and in its potential intersections with contemporary debates in the life sciences.

BIBLIOGRAPHY

Balme, D. M. 1980. 'Aristotle's Biology was not Essentialist'. *Archiv für Geschichte der Philosophie* 62:1–12. Repr. in Gotthelf and Lennox 1987, 291–312.

Cartwright, N. 1989. *Nature's Capacities and their Measurements.* Oxford.

Cooper, J. M. 1990. 'Metaphysics in Aristotle's Embryology'. Pp. 55–84 in Devereaux and Pellegrin 1990.

Devereux, D. and Pellegrin, P. 1990 edd. *Biologie, logique et métaphysique chez Aristote.* Paris.

Gotthelf, A. and Lennox, J. G. 1987. edd. *Philosophical Issues in Aristotle's Biology.* Cambridge.

Lennox, J. G. 2001a. *Aristotle's Philosophy of Biology: Studies in the Origins of Life Science*. Cambridge.

_________ 2001b. *Aristotle on the Parts of Animals I–IV, Translated with an Introduction and Commentary*. Oxford.

Ancient Greek Divination by Sarah Iles Johnston

Blackwell Ancient Religions. Malden, MA/Oxford: Wiley-Blackwell, 2008. Pp. xiv + 193. ISBN 978–1–4051–1573–5. Paper $27.95

Reviewed by
Joshua J. Reynolds
Center for Hellenic Studies, Washington, DC
joshua@mail.chs.harvard.edu

This book provides an overview of Greek divination as a religious phenomenon. In particular, the author seeks to describe and explain both the details of Greek divinatory practices and how the ancients conceptualized those practices. As the title suggests, the discussion is restricted to divination as practiced in the Greek world, although the author does make abundant use of evidence from a much wider variety of sources and time periods, including Roman and Christian writers. The straightforward writing, logical organization, and absence of footnotes make the book accessible to a general audience; while the erudition, critical approach to prior scholarship, and thorough bibliography accommodate both classicists in general and specialists.

The book contains five chapters: an introduction, two chapters devoted to institutional oracles, and two chapters covering independent diviners (including magicians).

In chapter 1, the author sets out to justify her study in terms of the pervasiveness of divination, not only in ancient times but in modern cultural contexts as well. She points to the desire for divinatory knowledge as a 'basic human need' [4]. The difference, however, between moderns and ancients is the degree of theoretical reflection among the latter. The ancients, Johnston argues, were theoretically inclined towards divination because the practice allowed mortals the possibility of conversing with the gods, as opposed to other religious practices, such as prayer or sacrifice, which did not return immediate answers.

An important part of this theoretical interest was the attempt to explain how the gods communicate with mortals and, thus, how mortals might participate in such communication most effectively.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 139–144

Johnston surveys several attempts to answer these questions, beginning with such early examples as Hesiod's catalogue of lucky days, Prometheus' list of types of divination in the *Prometheus Bound*, and Plato's distinction between the most preferable form of divination, enthusiastic madness, and the sane, non-divine forms that are less reliable due to the involvement of human judgment in interpreting signs. Johnston notes a precedent here for the later distinction between 'natural' and 'technical' divination, but she emphasizes the artificiality of this distinction given the tendency of diviners to practice both types as circumstances demanded.

Johnston goes on to survey several later, intellectual attempts to explain the mechanics of divination, including Stoic συμπάθεια, Neoplatonist 'chains' connecting the higher and lower realms of the cosmos, and the idea of intermediaries (δαίμονες) which Plutarch and some Christian fathers used to explain how gods can send messages without contacting the mortal realm themselves. A history and critique of 20th century scholarship concludes the first chapter. The author's main point here is to explain why scholars of the late 1960s and 1970s tended to focus on magic to the exclusion of divination. The reason, she argues, lies in the fashions of the time: scholars saw divination as rational, religious, and inclusive (and, therefore, uninteresting), while magic was seen as an example of the Greeks' darker, irrational side and as one of the ways in which they imposed 'otherness' on outsiders.

Chapter 2 begins the discussion of institutional oracles. Its focus is Delphi and Dodona. After considering the importance of location, the author goes on to investigate how these oracles worked and how inquirers saw them. In the case of Delphi, Johnston discusses several ancient sources that try to explain the Pythia's ability to prophesy. Johnston then goes on to consider the real cause of the phenomenon. She slightly revises the recent theory that geological fault lines beneath the temple caused ethylene to fill the room where the Pythia sat upon her tripod, thus producing in the priestess an altered state of consciousness. In order to explain why witnesses do not seem to have been similarly affected, Johnston suggests that the presence of ethylene was sufficient only to trigger in the Pythia a psychologically altered state rather than to cause one, just as the swinging of a pendulum can trigger hypnosis.

For heuristic reasons, Johnston classifies the Pythia's inspired, but characteristically ambiguous, prophecies as an example of a 'conversational' oracle, according to which the reply is intended to address the specific situation of the inquirer. In contrast, the author also discusses the use of 'binary' forms of divination which used mechanical devices, such as the drawing of lots, in order to provide a simple 'yes' or 'no' answer to the inquirer's question. Both of these types of divination, Johnston argues, co-existed at Delphi, despite the lack of archaeological evidence for the latter. The lot oracle, she suggests, provided an economical alternative to enthused prophecy and allowed the inquirer to sidestep the ambiguity for which the oracle was famous.

Johnston's discussion of Dodona concerns the wide assortment of divinatory methods that the ancients attributed to the oracle there: she considers, for instance, divination through prophets (interpreters called Selloi), sacred doves (πέλειαι) (whether female doves or priestesses called 'Doves'), a talking oak tree, a sacred spring (of which no traces remain today),bronze cauldrons—Christian writers believed that their harmonious ringing was what sent a priestess into a trance—and finally, lead tablets (the only method for which there is archaeological evidence). Johnston argues that the tablets, a form of 'binary' divination, served as an alternative to inspired prophecy as did lot divination at Delphi; but, she continues, 'It is anyone's guess as to how frequently, compared with enthused prophecy, the procedure was used at either place' [72]. The significance here, rather, is that two divinatory methods that scholars tend to separate could in fact work together at the same location.

Continuing the topic of institutional oracles, chapter 3 focuses on Claros and Didyma. The author admits that information on both oracles is scarce, but she does manage to assemble some evidence regarding their foundation myths, formal procedures and prophetic methods, oracular personnel, and the concerns of the surviving oracles themselves (in particular, a growing interest in theological questions). An interesting section on Claros discusses the Cynic philosopher Oenomaus of Gadara, who discovered that the reply which he had received was identical to those delivered to other inquirers and thus set out to debunk the oracle. Against Oenomaus, however, Johnston argues that standard answers were part of the usual process at Claros and that more personalized attention was possible when appropriate.

Noteworthy in the discussion of Didyma are the author's treatment of the foundation myth and her reconstruction of the Hellenistic temple.

Chapter 3 also discusses several other divinatory methods available at institutional oracles. Foremost is the discussion of incubation, a process during which the inquirer slept in a temple or other sacred place and awaited dream visitations from the gods, typically, although not always, in regard to a medical problem. Some locations discussed here are the Asclepieion at Epidaurus, the Amphiareion at Thebes (or Oropus, depending on the source), and the oracle of Trophonius at Lebadea in western Boeotia. Johnston follows up with brief discussions of empyromancy (interpretation of the flames of a sacrifice) at the oracle of Zeus at Olympia, catoptromancy (divination with mirror reflections) and hydomancy (divination with reflections in water) at the oracle of Demeter in Patrai, and oracular consultation with dice, which various locations offered for the client's convenience. Johnston concludes the chapter with an entertaining discussion of a famous scam reported by the satirist Lucian which involved a certain Alexander who established an oracle for his talking snake at Abonuteichos.

Chapter 4 begins the discussion of independent or 'freelance' diviners. The author begins by considering how one became a diviner (μάντις). She discusses several possible methods, including inherited talent, acquired skill, and divine gift; but she concludes that 'there was no single, overarching model for how a *mantis* became a *mantis* and no single concept of what sort of thing mantic ability was...' [112]. For the Greeks, mantic ability, like skill in the medical and magical arts, could come from any or all of these sources. The uniqueness of diviners (μάντεις), however, was that they were 'much more firmly incorporated into myth' [113]. Johnston explains this characteristic in terms of the extraordinary: almost anyone, she argues, could master medicine, while magic was too extraordinary even for the world of myth. The diviner's art, however, fell in between these two extremes in that it maintained, as do most myths, contact between the divine and human worlds.

The remainder of chapter 4 is devoted to what the μάντις did and how he did it. In response to the former question, the author surveys a range of functions, including the resolution of a crisis during battle, the diagnosis of the cause of an illness and the prescription of a remedy, and the purification of a disease or pollution through magical techniques. In regard to the latter question, Johnston discusses both the practical details as well as the theoretical explanations of prophecies that diviners generated by means of the entrails of a sacrificial animal (especially the liver), birds and other ominous animals, and the involuntary motions of people (i.e., cledonomancy).

One might think that divination through heavenly bodies should enter the discussion here; but, as Johnston points out, prior to the first century BC the Greeks, in contrast to the Babylonians, were not concerned with the systematic observation of the heavens. Johnston attributes this lack of interest to the facts that the level of literacy required for the compilation of detailed records reached Greece relatively late; and that, when it did arrive, the Greeks did not make use of it since there was no scribal culture in place. The chapter concludes with brief discussions of an assortment of diviners, including dream interpreters, oracle collectors (χρησμολόγοι) who appealed to previous divinatory responses to resolve new problems, and 'belly-talkers' (ἐγγαστρίμυθοι) who claimed to host δαίμονες within their own bodies.

Chapter 5 concludes the treatment of the independent diviner. In particular, the author focuses on the diviner, his relationship to the magician, and why the ancients often associated the two. The bulk of the evidence here comes from the Greek magical papyri, which the author notes provide uniquely specific information about mantic practices, procedures, and ritual flexibility. In regard to this last issue, Johnston notes that even the ancients saw magic, in contrast to religion, as unusual, despite its essential similarity to the practices of mainstream religion. Any differences, however, were economical: both the priest and the magician sought to gain the cooperation of the gods; but the latter capitalized on oddness and innovation to suit the needs of his clients, and thus to enhance his reputation and earnings.

Following a discussion of the Greek magical papyri as texts and the changes of the times in which they were composed (e.g., greater cultural interaction, the rise of utopian religions, and increased attempts to eradicate diviners and magicians), Johnston proceeds to examine the content of the texts. She argues here that the basic point of the spells was to provide a close encounter (σύστασις) with the gods. Certain spells even provided a code of conduct or instructions to allow the magician to sustain the god's appearance. Among the methods for achieving encounters that the author covers are the process of leading divine light into the soul (φωταγωγία), lychnomancy (divination by lamp), divinatory statues and symbols, and dream-sending (ὀνειροπομπεία) which was meant to compel someone to do what he or she otherwise would not do, especially in the concerns of Eros.

Chapter 5 ends with a discussion of necromancy, the consultation of the dead. Johnston argues that there is little evidence that the Greeks ever engaged in this practice. She notes that only eight of the 600 spells from the Greek magical papyri advise on how to consult the dead for information. Johnston's explanation of this lack of interest is simple: the dead know nothing. When the Greeks sought prophetic knowledge, rather, they appealed to the gods. At the end of the chapter, Johnston again raises the question of why the ancients often associated divination and magic. In response, she points out that both shared the goals of 'extraordinary knowledge of ritual techniques and the power they could bring', and that both were 'pursuits in which professional specialists could make a living' [177]. Johnston also notes practicality: both diviners and magicians were 'willing to expand their repertoire as their clientele demanded' [177]. Consequently, we should think of both practitioners as selling the 'supplemental religious expertise' that mainstream religion was not willing to provide [177].

Johnston's book has many merits, the most important of which have been summarized in the opening paragraph of this review. The preceding summary of the range and detail of the study should also suffice to demonstrate its value. One should also highlight the author's refusal to impose rigid conceptual schemes upon her subject. It is, in fact, difficult to find fault with this work. But one criticism does come to mind. It is true that the author's intent is to approach Greek divination strictly in terms of myth and religion. One disadvantage of this approach, however, is that it neglects to consider divination in literature. A section on how authors of various genres use divination for their own purposes (whether literary, philosophical, historiographical, rhetorical, moral, and so on) would have been helpful, as would one on how such contexts affect our picture of divination as a historical reality.

Plato's Ghost: The Modernist Transformation of Mathematics by Jeremy Gray

Princeton, NJ/Oxford: Princeton University Press, 2008. Pp. x + 515. ISBN 978–00–691–13610–3. Cloth $45.00

Reviewed by
Fernando Q. Gouvêa
Colby College
fqgouvea@colby.edu

The period from 1870 to 1930 was a time of significant cultural change. In art, literature, architecture, and music, these changes led to new ways of doing things that went under the name of 'modernism'. Jeremy Gray's central argument in *Plato's Ghost* is that the same concept may be usefully applied to the history of mathematics in this period.

Modernism arose in reaction to Late Romanticism, in which previous ideas and techniques seemed to have reached their limits. Realism in literature could not, it seemed to many, be pushed any further than it had been by Zola. The flirtation with fantasy and fairy tales during the Victorian era also seemed not to lead anywhere. In art, Impressionism had called everyone's attention to the role of the perceiving eye as mediator between reality and the human observer. Wagner's music was felt to have pushed emotional intensity as far as it could go.

Science and philosophy also contributed to the unsettling of the old ways. Once the Christian consensus fell apart, the foundations of Western thought began to seem shaky. Philosophers began to wonder how to justify our conviction that we know anything about the world. Marx questioned the whole economic structure of Western civilization in a way that seemed, if not persuasive, at least worthy of consideration. Darwin's discovery of evidence for evolution led to a revolution in biology. Physicists dealing with electromagnetism ran into more and more difficulties. Finally, technology was becoming a bigger part of the everyday life of most Europeans and Americans,

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 145–154

changing the way that people interacted with their physical environment and thereby removing from nature the mystical aura that had so enthralled the Romantics.

The modernists called into question the whole structure of the culture that they had inherited, highlighting the tensions. They questioned the link between art and reality, and the idea that reality trumps human perception and imagination. Houses designed for no other purpose than to be lived in began to be admired as objects in and of themselves. Music migrated from homes to concert halls, art moved to museums, and it became fashionable to speak negatively of 'mass culture'. The modernists seemed unconvinced by the naïve progressivism of the 19th century. The deep past interested them more than recent times, but they wanted to preserve this legacy only by embedding it into a completely new framework.

The modernists were acutely self-conscious, probably more so than any other cultural and artistic movement. This was the time of manifestos, of schools and programs, of the proliferation of new 'isms'. More people than ever before learned to read and to appreciate art and music, but popular approval became identified with lack of quality. The cultural products of the modernist school seemed to be aimed at a select few, to those who understood their ideological and artistic background, to those who could appreciate the technical difficulty of much modernist art. To appreciate the new styles, one also needed to be aware of the past, to be able to understand the quotations and ironic misquotations of past works that filled the new art.

First art, then also music, literature, and architecture became abstract. Rather than continue to use longstanding conventions about how the perceiving subject interacts with cultural products, the modernists produced art that broke all the rules. The new cultural products seemed to say 'Forget your expectations, take me or leave me as I am.'

By the 1920s, modernism dominated the world of art. In literature, the new approach never quite achieved that kind of unanimity, especially in the realm of the novel, where new and old continued to survive and in fact to influence each other. Modernist music faced (and still faces) an uphill battle, given that the public for the most part refused to go along. Modernist architecture won the day when it came to monumental (and often government-funded) buildings, but most people's houses remained much as they always had been. One way or the other, by the 1930s modernism was the establishment.

This account of the modernist moment is, of course, vastly oversimplified. Historians still bicker about the details, but most seem to agree that 'modernism' is a useful way of understanding the period. The transformation that hit several kinds of artistic and cultural production during this period was real, and the changes were sufficiently similar in spirit that they all deserve a common name. There have been many attempts to describe what exactly 'modernism' consists of; most would probably agree with the following:

- Modernists were much less concerned about the connection between their art and external reality than the artists before them.
- There was a break with traditional forms (verse became 'free'; music, atonal; novels, experimental; and art abandoned the goal of being beautiful).
- The greatest achievements of the past were not abandoned, but they were systematically reinterpreted, reclaimed, and transformed.
- An acute self-consciousness led to aggressive agendas and manifestos.
- Artists wanted the support of the masses and of governments, but felt that only the truly educated had any right to pass judgment on their work.
- Form (which includes deliberate lack of form) and technique became more important than content. The idea that artists could be called to task for what they said was viewed as repressive.

No one doubts that mathematics (and physics) went through an equally dramatic transformation between 1870 and 1930. This was the time of the popularization of non-Euclidean geometries, of Relativity and Quantum Theory, of debates about the foundations and ultimate reliability of mathematics. This period saw the advent and triumph of abstraction as a mathematical technique, with new attention being paid to logic and axiomatics. It was also the time when mathematics became a profession, when universities began to focus on research and professional societies were formed.

One can also point to a kind of 'late Romantic' crisis in mathematics. What was one to make of the surfeit of formulas to be found in the work of Kummer and Jacobi? (The impenetrable first chapters of Gray's book on *Linear Differential Equations and Group*

Theory serve as a good example of this problem.) Things seemed to be getting so complicated that progress required a completely new approach. This helped lead to the new mathematics of the late 19th and early 20th centuries.

At the core of *Plato's Ghost* is the thesis that these two transformations can be usefully correlated. Between 1870 and 1930, mathematics became dramatically more abstract, more concerned with 'structure' than with specific examples of phenomena, and much less connected to the sciences. Abstraction required standards of proof that were new and much more formal than ever before. Mathematicians ceased to worry about the approval of society or even of scientists, creating their own journals and their own standards for what constitutes good work. And, as never before, they argued about foundational issues, about what was acceptable mathematics and what was not, about the relationship between mathematical truth and the real world.

The nature of the transformation at issue can be illustrated by a famous anecdote. In his study of integral equations, David Hilbert formulated a notion of proximity for functions that allowed him to make sense of the idea of producing solutions by approximation. Mathematicians more modern (or more modernist) than Hilbert took this notion and formalized it, creating a concept they called 'Hilbert space'. The anecdote tells of Hilbert attending a seminar in which the speaker began talking about a certain Hilbert space. The eminent mathematician whispered to a nearby colleague, 'Can you tell me what a Hilbert space is?'

It is precisely this sense of the old made new, of *ad hoc* techniques being turned into formal structures, of theory triumphant over problem solving, that we see throughout mathematics in these decades. Talk about 'objects' and 'spaces' replaced talk about 'formulas' and 'equations'. There was an almost total transformation of algebra, as anyone who compares the tables of contents of, say, Perron's *Algebra* (1927) and van der Waerden's *Moderne Algebra* (1930) cannot fail to note. (In fact, the adjective 'modern' became firmly attached to the new approach to algebra, and continues to be used in this sense.)

It would have been inconceivable for a mid-19th century mathematician (say, Bernhard Riemann) to have the attitude toward applied mathematics that we find in G. H. Hardy's *A Mathematician's*

Apology. (This despite the fact that Hardy's mathematics is hardly 'modernist'!) For Riemann, mathematics and physics were tightly linked, and the idea that one should (or even could) do mathematics without any concerns about what is 'useful' would probably have struck him as bizarre.

To most mathematicians today, the 19th century arguments about geometry seem equally strange. The discovery of non-Euclidean geometry generated a passionate argument about what the 'true' geometry was. For the scholars in question, geometry was about describing the real world, the three-dimensional space that we perceive. Today, that seems absurd. For us, geometry is an abstract bit of mathematics like any other, and we can prove theorems about the geometry at hand without worrying whether it corresponds to anything in the real world. When it comes to applying all this to reality, one simply chooses the most convenient geometry for the problem at hand. And just as we find them hard to understand, they would probably be puzzled by our talk of 'a space' or, worse, of 'spaces'.

Of course, that does not mean that mathematicians feel that they are playing an abstract game with no intrinsic rules. However abstract an object the Riemann zeta function may be, most mathematicians will argue that its value at $s = 3$ is a *specific* number, whose properties (e.g., is it a fraction?) are *investigated* rather than created. Philosophically, this seems very problematic. What kind of reality is there to a number that can only be specified *via* a complicated (and potentially infinite) convergence process? Do we 'have' a number if we cannot (even in principle!) do more than produce approximations to it? When one talks about that number, is one talking about one specific *thing* or about the approximation process itself? Given that mathematics seemed more and more loosely connected to science, the question of what warrant is available for mathematical 'facts' presents itself very forcefully.

Today, most mathematicians are happy to leave such questions to the professional philosophers. (I suspect that, as Henri Lebesgue suggested long ago, the philosophers are grateful for this.) In the early 20th century, however, mathematicians were deeply involved in such arguments. More remarkably still, they allowed such questions (and their proposed answers) to affect their practice of mathematics. The most dramatic example of this is L. E. J. Brouwer, who proposed a radical purge: infinitary arguments should simply be abandoned. The crisis provoked by this proposal plays a large role in *Plato's Ghost*, reflecting the large role it played at the time. In fact, Gray's discussion of philosophical issues is much more extensive and detailed than most mathematicians today (I include myself) can really stomach. In fact, such philosophical questions at times threaten to take over the argument. It was that way in the 1920s as well.

One could easily multiply examples of the difference between the mathematics of the mid-19th and of the 20th century. Of course, there are also counterexamples, including some areas of mathematics where fairly traditional work continued to be done. Gray cites differential equations, for example: in this field one sees *both* very concrete and traditional work and fancy reinterpretations in terms of linear operators, sheaves, and D-modules. There are many other examples, but most of them are similar, with both 'modernist' and 'classical' work happening side by side. (Beyond the purview of this book, one might note that in recent decades a move toward concreteness may have begun, spurred in part by the computer revolution.)

It is clear, then, that there was a transformation of mathematics during this period. It is also clear that the new mathematics shares many characteristics with artistic modernism: it is more abstract, less concerned with 'reality', more formal, harder to learn and appreciate. One even sees echoes of the modernists' rather tiresome fondness for manifestos and arguments about the nature of what they were doing.

Historians have, of course, long been aware of many of these changes. Some, for example, have emphasized the professionalization of the field: the establishment of 'mathematician' as a specific identity, the creation of mathematical journals and national mathematical societies, the rise of the research seminar and of the Ph.D. as the required certification. Gray's description of the new mathematics as 'modernist' is an attempt at a new understanding—and perhaps also a new explanation—of these changes.

In order for the notion of 'mathematical modernism' to be useful, however, one needs a far deeper analysis. It is necessary to examine the period carefully to see exactly what changed and how, in order to be sure that we are not selecting our evidence to fit our thesis. If 'modernist' were just a period label to be attached to whatever we find in the mathematics of the time, then very little would have been achieved. One hopes, in fact, for more: not only must the details fit the overall picture; the new concept of 'mathematical modernism' should also shed new light on the conceptual changes in the mathematics of the period. *Plato's Ghost* attempts to provide such analysis and to argue for its clarifying value. Jeremy Gray is admirably suited for such a task. Over the last decades, he has put together a substantial body of work on the history of mathematics in the 19th and early 20th centuries, focusing especially on geometry, mathematical physics, and the philosophy of mathematics. He has written less on analysis, algebra, and number theory; but he has read widely. *Plato's Ghost* is in many ways a summing-up.

Gray has chosen to address his book to historians of science in general; in particular, he tries not to require of his readers the kind of knowledge of mathematics that a professional mathematician would have. This is probably the right choice, given that mathematicians tend to be interested only in a utilitarian sort of history, in history of the kind that sheds some light on current mathematics. Since Gray wants to argue an eminently *historical* thesis, it is to historians that he directs himself.

This choice does, however, have its costs. The central one is easily grasped: in order to argue that there was a fundamental change in *mathematics* (rather than, say, in the practice of mathematics, or the philosophy of mathematics) one must look at mathematics itself. Since by 1870 mathematics was already very much a specialist topic, this creates real difficulties for non-mathematicians. Gray deals with this issue in three ways. First, he tries to explain some (in general well chosen) mathematical questions of the time and to give the reader some idea of the way they were dealt with. Second, he gives a lot of attention to mathematicians' writings *about* mathematics. Finally, he spends a great deal of time on topics that his readers may have some knowledge of (mathematical physics, non-Euclidean geometry, logic, language) and much less on the more abstract reaches of mathematics (algebra and topology, for example).

The first decision is commendable, the second a little worrying, the third something to be regretted. It is frustrating to see Gray avoid precisely those topics, especially algebra, which are the best examples of the transformation that Gray seeks to understand and document. The deliberate and self-conscious way in which Emil Artin and Emmy Noether remade algebra in the 1920s fits very well into the category of modernism. It is a pity, then, not to read more about them. Gray does give some attention to Dedekind's creation of 'ideals' and to Hensel's p-adic numbers, but those do not really represent the full blooming of modernist algebra. Hensel, in particular, was a transitional figure who does not seem to have ever really adopted (or understood) the newer style.

It is inevitable that Gray would focus on writing *about* mathematics in this period, both as a way of keeping non-mathematician readers on board and because this was a particularly fertile period for such writing. But it does raise questions, in particular the question of the relation between theory and practice. Gray himself notes, in his discussion of David Hilbert, that his mathematical practice was mostly rather traditional while his *philosophy* of mathematics was quite modern. There are other examples in which it seems that the theory is *post hoc*, intended to justify an approach to mathematics chosen, perhaps, for other reasons. Hermann Weyl is an unusually reflective example of this: after flirting with Brouwer's radical intuitionism, he abandoned it because it turned out that one needed infinitary arguments to do quantum theory.

It may be that philosophers will appreciate Gray's detailed attention to these writings. I will admit that I found some of the discussion either boring or irrelevant. Do discussions about language and linguistics (and the late 19th century fascination with artificial languages) really matter to a history of mathematics? Overall, however, one is impressed by the deep knowledge on display and the thoroughness with which Gray surveys the scene. He has convinced me that speaking of 'modernist mathematics' is more than a *façon de parler*, that it can be a useful way of thinking about the transformation of mathematics in these decades.

Gray is particularly strong on the connection between the new mathematical modernism and the foundational crises that shook the philosophy of mathematics in the period. Once geometry was no longer about physical space and mathematics was unmoored from physics, it became important to explain where the reliability (if any!) of mathematical results came from. The naïve answer was a form of

Platonism: there was a mathematical reality 'out there' that mathematicians can investigate. Such a position is hard to justify philosophically (especially for non-theists), and perhaps it was this that stimulated the new philosophical currents of the time. Some attempted to reduce mathematics to logic, others decided to discard all the mathematics that seemed philosophically dubious, still others attempted to base the potentially dubious part of mathematics on arguments using only what was universally accepted. Hilbert, for example, wanted to find finite intuitively acceptable arguments that would establish that the mathematics of infinity was free from contradictions and, hence, should be accepted.

As Gray explains, this project turned out to be unsuccessful. After an explosion of interest in such issues, their widespread discussion died out. The philosophical questions were left to the philosophers, and mathematicians mostly went back (unless pressed) to their naïve Platonism. Gray argues persuasively that these changes in mathematical epistemology are directly connected to the modernist project.

Gray has laid out the outline of a research program in this book. He has sketched out the landscape; but, as he says in the first chapter, there are many issues still to be addressed. The most obvious question, which Gray explicitly declines to discuss, is about the cause of the similarities between modernism in the arts and mathematical modernism. Was there influence of one on the other? (William Everdell, for example, includes Dedekind and Cantor among his *First Moderns*, which, given the chronology, puts them at the origin of modernism.) Or was it that the cultural conditions that lead to one also lead to the other? An initial step towards such an investigation would be to try to find out whether the crucial 'modernist mathematicians' were interested in the arts, and in what way. One might ask the same about the artists.

Other issues, perhaps more accessible, beg for investigation. How aware were these mathematicians of the 'modernist' character of their mathematics? Did they feel that the new approach was the inevitable way to proceed, or was there a conscious radicalism in play?

I expect to see many papers investigating how the transformation played itself out in particular areas. Algebraic topology and probability theory should be investigated. I would be especially interested in a close look at what happened in algebra. Also worth investigating are the pockets of resistance, such as the theory of differential equations and combinatorics. One might also look at individuals. Benoît Mandelbrot, for example, has always presented himself as a kind of anti-modernist; but is the claim justified? It is characteristic of good ideas that they are fertile in this way.

What does all this have to do with Plato and his ghost? The reference is to Yeats' 'What Then?' which appears as an epigraph to the book. In the poem, Plato's ghost repeatedly calls into question the achievements of the main character. Gray's introduction refers this to his own work; but, of course, it also points to the Platonist implications of modernist mathematics and to the unfinished task of sorting them out.

There have not been very many historians of mathematics willing to hazard overarching historical theses. Jeremy Gray is to be commended for having taken the risk. *Plato's Ghost* is an impressive achievement; I expect it to become a touchstone for future research on this period and to bear many fascinating children.

The Astronomical Tables of Giovanni Bianchini by José Chabás and Bernard R. Goldstein

History of Science and Medicine Library 12. Leiden/Boston: Brill, 2009. Pp. x + 150. ISBN 978–90–04–17615–7. Cloth € 75.00, $111.00

Reviewed by
Max Lejbowicz
Université Paris 1 Panthéon-Sorbonne
Max.Lejbowicz@univ-paris1.fr

Avant d'entreprendre la lecture de ce livre hautement technique, un néophyte éventuel pourrait ressentir le besoin de connaître les apports de Giovanni Bianchini (vers 1405–après 1469) à l'histoire de l'astronomie. S'il consulte un manuel reconnu d'histoire des sciences médiévales, par exemple le recueil d'études paru sur la direction de David C. Lindberg [1978], il déchantera vite : Giovanni Bianchini y est absent, alors que des astronomes contemporains, Georg Peurbach (1423–1461) et Regiomontanus (1436–1476), y sont cités à plusieurs reprises. La même absence se constate dans l'anthologie des sciences médiévales parue quelques années plus tôt sous la direction d'Edward Grant [1974]. Notre néophyte aurait tort d'en déduire que José Chabás et Bernard R. Goldstein ont choisi d'écrire une monographie sur un scientifique de second plan ou même inférieur en qualité à l'auteur des *Theoricae novae planetarum* et à celui du *De triangulis omnimodis*. Les deux plus jeunes astronomes cités ont utilisé dans leurs travaux les tables astronomiques réalisées par leur aîné ; et l'un d'eux, Regiomontanus, a correspondu avec lui, dont il a même recopié les Tables, comme en témoigne l'actuel manuscrit Nuremberg, Stadtbibliothek, Cent V 57. Il faut se rendre à l'évidence : les principales contributions de Bianchini à l'histoire de l'astronomie ne peuvent se réduire à des généralités aisément communicables. Elles reposent sur des apports spécialisés et ne sont comprises et appréciées que par le lecteur qui fait l'effort de se couler dans l'univers mental des astronomes du XVe siècle pour adopter leurs préoccupations intellectuelles et leurs méthodes de travail ; autrement dit, pour adopter les critères de scientificité d'un autre temps. Bianchiani est un de ces brillants praticiens de la « normal science » de Thomas Kuhn, un praticien qui travaille avec constance et acharnement dans le cadre

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 155–161

du « paradigm » ptoléméen (ou de la « disciplinary matrix », selon le dernier état de la réflexion kuhnienne).

Second préalable, avant d'entrer dans le vif du sujet : Chabás et Goldstein évoquent dans leur premier chapitre [13–22] les autres travaux de Bianchini. Pour ce faire, ils s'appuient pour l'essentiel sur les études pionnières de Grażyna Rosińska [1984, 1996, 1997, 1998 et 2006]. Bianchini occupe une place non négligeable dans l'émergence, en arithmétique, du séparateur décimal et, en astronomie, dans la généralisation de la notation décimale. Son commentaire de l'*Almageste* comporte une partie algébrique intéressante. Dans un autre de ses textes, il relate les observations d'éclipses lunaires et d'une éclipse solaire qu'il a faites sur une période d'une vingtaine d'année, à Ferrare, où l'université le compte parmi ses enseignants, tandis que, parallèlement, il administre la fortune des maîtres de la ville, la famille d'Este. Si l'un des membres de cette maison fut élevé au titre de duc de Modène et de Reggio en 1452 et de duc de Ferrare en 1471, c'est toute la lignée qui s'est illustrée dès le XIVe siècle dans le mécénat en soutenant l'école de peinture de Ferrare et en investissant dans l'agrandissement et l'embellissement de cette même cité. Pour Bianchini, le maniement des nombres et des grandeurs est devenu une seconde nature ; et il sait s'aventurer hors des sentiers battus de la numération, tout en bénéficiant de la stabilité que lui procurent des activités professionnelles éminentes.

Mais auparavant, dans leur introduction [1–12], Chabás et Goldstein résument les connaissances acquises au cours de ces dernières décennies sur les Tables astronomiques médiévales, tant au plan technique que dans le domaine historique. Elles apparaissent en Europe quand, pendant ce XIIe siècle si décisif pour le développement intellectuel du continent, ses lettrés entrent en contact avec la science arabophone et tout spécialement avec les *zīj*s.[1] Cette courageuse avant-garde prolonge les Tables astronomiques arabes en les adaptant aux réalités géographiques et culturelles de la chrétienté, avant que les générations suivantes s'en émancipent. L'indépendance des Latins

[1] Ce mot d'origine persane correspond au grec *κανών*. Il s'est imposé dans l'astronomie arabophone pour désigner dans un premier temps un ensemble de tables de mouvements des astres, précédées par des figures qui en explicitent la composition. Le sens s'est ensuite élargi et le mot désigne des traités d'astronomie qui contiennent des tables.

est acquise dans les années 1320 grâce aux Tables alphonsines parisiennes [North 1977 ; Poulle 1980 et 1984 ; Saby 1987 ; Chabás et Goldstein 2003, 2004, et 2009], dont la diffusion est favorisée par la mise au point de l'imprimerie peu après le milieu du XVe siècle.

La mécanique céleste est à cette époque une conquête à venir : les Tables qui permettent de retrouver les positions des astres à une date donnée, se réduisent à une description cinématique des corps célestes à partir de l'astronomie qui occupe alors le devant de la scène savante, celle des épicycles. L'astronomie des épicycles asservit le mouvement des planètes, soleil et lune inclus, au diktat des apparences et à deux *a priori* théoriques : le géocentrisme, cette donnée immédiate de la perception, est pensé par l'intermédiaire de planètes qui orbitent chacune sur un cercle spécifique à vitesse constante. Cette triple contrainte imposée à la réalité d'un système héliocentrique d'orbites elliptiques que les planètes parcourent à des vitesses variables, aboutit à une construction géométrique hautement sophistiquée. Chabás et Goldstein la rappellent très succinctement dans leur introduction ; sans doute, trop succinctement pour le lecteur qui n'est pas déjà initié : mais il est vrai qu'ils apportent dans leur deuxième chapitre maintes élucidations complémentaires qui devraient en permettre une connaissance plus concrète.

Ce deuxième et dernier chapitre est intitulée *Analysis of the Tables*. C'est le cœur de leur étude. Il occupe 109 pages dans un livre qui, avec sa préface, son introduction, son premier chapitre, son tableau de notations, sa bibliographie et son index, en compte au total 160. Il repose sur l'analyse successive des 112 tables dont l'ensemble constitue les Tables de Giovanni Bianchini — 112 pour autant qu'est prise en compte cette caractéristique déroutante des Tables astronomiques médiévales, bien connue des spécialistes et excellemment décrite par l'un d'eux : ce sont

> des ensembles aux contours flous. Constituées de sous-ensembles plus ou moins développés, eux-mêmes plus ou moins homogènes ..., les Tables se présentent, au gré de la tradition manuscrite, comme des lots de composition variable, les copistes, qui en furent aussi très souvent les utilisateurs, en ayant facilement largué tels de ces sous-ensembles ou en ayant au contraire récupéré tel autre, ayant négligé par ici un élément pour enrichir ailleurs leur recueil avec des additions ou des emprunts. [Poulle 1984, 5]

Dans des conditions aussi singulières, Chabás et Goldstein dégagent trois traditions manuscrites et éditions imprimées des Tables connues sous le nom de Tables de Giovanni Bianchini. L'une comprend les 68 premières tables et est représentée par le manuscrit de Naples, Biblioteca Nazionale, VIII.C.34 et par l'édition de 1495. Une autre regroupe les tables 69 à 86 et s'incarne dans trois des 20 manuscrits recensés (celui de Naples déjà cité, plus ceux de Rome, Biblioteca Casatense 1673 et du Vatican Biblioteca Apostolica, Pal. Lat. 1375). La troisième enfin réunit les tables 87 à 112 et est le plus complètement matérialisée par l'édition de 1526. Cette dernière tradition élargit très vraisemblablement le texte original de Bianchini, qui, en l'absence d'une copie autographe, est hors d'atteinte de l'historien, pour Chabás et Goldstein. Avec leurs 112 unités, les Tables sous examen sont probablement l'ensemble le plus volumineux produit en Europe dans le genre avant les temps modernes. Dans la mesure où le travail de l'astronome de Ferrare s'inscrit dans la continuité des Tables alphonsines, cet accroissement fait mieux comprendre et mieux apprécié le travail accompli par les astronomes du roi Alphonse X de Castille dans la deuxième partie du XIIIe siècle et les innovations dont il a été l'objet de la part des astronomes travaillant à Paris dans les années 1320.

Les auteurs invitent donc leur lecteur à un voyage de 112 stations construites selon le même schéma. Chacune d'elles comporte un rappel des manuscrits et des éditions qui contiennent la table sous examen, avec leur titre particulier. Puis elle reproduit la table dans une typographie actuelle, le plus souvent sous forme d'extraits. Enfin, elle en donne un commentaire qui peut éventuellement s'appuyer sur une figure spécialement dessinée pour l'occasion ou sur une table signalée par une lettre après le numéro : celle-ci provient d'une autre source que les Tables de Bianchini, ou a été dressée par les auteurs soit qu'ils donnent un arrangement différent de la table en cours d'analyse, soit qu'ils complètent leur propos par une table originale. Ces 112 stations se distribuent logiquement en trois grandes étapes qui recouvrent les trois traditions mentionnées plus haut [tables 1–68 aux p. 28–93 ; tables 69–86 aux p. 93–114 et tables 87–112 aux p. 114–132].

L'analyse technique est à tout point de vue excellent et ne prête pas à redire. Elle met bien en valeur les apports de Bianchini (tables des latitudes, tables à double argument). Tout au plus, un examen plus serré des 51 chapitres des canons donnés aux p. 16–18 aurait sans doute était souhaitable afin de savoir s'ils ne fournissent pas le moyen de cerner plus précisément les contours des tables originales de Bianchini. De même, un panorama de la vie culturelle et universitaire de Ferrare aurait été bienvenu : il aurait permis de mieux connaître l'environnement dans lequel Bianchini s'est formé et a vécu, et d'approcher, à la fois, les implications sociales de l'astrologie et l'inventivité des praticiens. Sur ce point le précédent livre des deux auteurs [Chabás et Goldstein 2003] est plus équilibré, qui marie heureusement l'histoire des sciences et l'histoire culturelle. La couverture du présent ouvrage reproduit l'enluminure de Giorgio d'Alemagna qui orne le frontispice d'un des manuscrits des Tables de Bianchino — frontispice qui est reproduit pleine page à l'intérieur du livre, en face de la page de titre. L'enluminure montre, à gauche, l'Empereur Frédéric III, assis, en train d'échanger un blason contre les Tables astronomiques qu'agenouillé, Bianchini lui tend, tandis qu'au centre se détachent Borso d'Este debout, et à droite, trois serviteurs de ce dernier, également debout. Elle a attiré l'attention des historiens de l'art [Gundersheimer 1993 ; Simons 1997] et le résumé de leurs travaux aurait utilement éclairé les rapports que Bianchini entretenait avec son mécène et ceux que ce dernier avait noués avec l'Empereur. La lecture terminée, l'impression demeure que l'ensemble forme davantage un article copieux destiné à une revue spécialisée qu'un livre au sens plénier du terme ; à savoir, une étude qui, centrée sur un sujet, l'aborde de tous les points de vue possibles pour tenter d'en dissiper tous les mystères.[2]

Il reste une analyse systématique de l'astronomie des épicycles telle qu'elle se pratiquait dans la deuxième moitié du XVe siècle par un esprit ingénieux et inventif. La prouesse est d'autant plus notable qu'il a fallu 190 ans pour passer du jugement sommaire d'un illustre historien de l'astronomie : « (Les Tables de Bianchini) n'ont de mérite que leur étendue, qui diminue le travail du calculateur » [Delambre 1819, 261] à celui, circonstancié et pertinent, de Chabás et Goldstein :

[2] J'ai relevé une coquille amusante, p. 135. Le nom de la revue où Goldstein et Chabás ont fait paraître leur article « Ptolemy, Bianchini, and Copernicus: Tables for Planetary Latitudes » est *Archive for History of Exact Sciences*, non *Journal for the History of Astronomy*. Le reste est sans changement.

> . . . our respect for Bianchini has shifted from volume to value. Although not innovative in their building blocks, his tables reflect a well defined approach to astronomy, a practical way to present it, and a solid computing ability. [viii]

Manifestement, l'histoire des sciences progresse moins vite que les sciences elles-mêmes. Voilà une autre excellente raison pour apprécier le minutieux travail de José Chabás et de Bernard Goldstein.

BIBLIOGRAPHY

Chabás, J. and Goldstein, B. R. 2003. *The Alfonsine Tables of Toledo.* Dordrecht/Boston/London.

________ 2004. 'Early Alfonsine Astronomy in Paris: The Tables of John Vimond (1320)'. *Suhayl* 4:207–294.

________ 2009. 'John of Murs's Tables of 1321'. *Journal for the History of Astronomy* 40:297–320.

Delambre, J.-B. 1819. *Histoire de l'astronomie du Moyen Âge.* Paris.

Grant, E. 1974. ed. *A Source Book in Medieval Science.* Cambridge, MA.

Gundersheimer, W. 1993. 'Clarity and Ambiguity in Renaissance Gesture: The Case of Borso d'Este'. *The Journal of Medieval and Renaissance Studies* 23:1–17.

Lindberg, D. C. 1978. ed. *Sciences in the Middle Ages.* Chicago/London.

North, J. D. 1977. 'The Alfonsine Tables in England'. Pp. 269–301 in Y. Maeyama and W. G. Saltzer edd. *Prismata. Naturwissenschaftsgeschichtliche Studien. Festschrift für Willy Hartner.* Wiesbaden. Repr. J. D. North 1989. *Stars, Minds and Fate: Essays in Ancient and Medieval Cosmology.* London/Ronceverte. ch. 21.

Poulle, E. 1980. 'Jean de Murs et les tables alphonsines'. *Archives d'histoire doctrinales et littéraire du Moyen Âge* 47:241–271.

________ 1984. *Les tables alphonsines avec les canons de Jean de Saxe. Édition, traduction et commentaire.* Paris.

Rosińska, G. 1984. 'Identification of Copernican Tables of the Latitudes of the Planets'. *Qwartalnik Historii Nauki i Techniki* 29:637–644.

——— 1996. 'The Fifteenth-Century Roots of Modern Mathematics'. *Kwartalnik Historii* 41:53–70.

——— 1997. 'The "Italian Algebra" in Latin and How it Spread to Central Europe: Giovanni Bianchini's *De Algebra* (ca. 140)'. *Organon* 26–27:131–145.

——— 1998. 'The Euclidean *spatium* in Fifteenth-Century Mathematics'. *Kwartalnik Historii* 43:27–41.

——— 2006. ' "Mathematics for Astronomy" at Universities in Copernicus' Time: Modern Attitudes toward Ancient Problems'. Pp. 9–28 in M. Feingold and V. Navarro edd. *Universities and Science in the Early Modern Period.* Dordrecht/Boston.

Saby, M.-M. 1987. *Les canons de Jean de Lignères sur les tables astronomiques de 1321.* Ph.D. dissertation (unpublished), École Nationale des Chartes, Paris. A summary appeared as 'Les canons de Jean de Lignères sur les tables astronomiques de 1321'. École Nationale des Chartes, 1987: *Positions des thèses*, 183–190.

Simons, P. 1997. 'Homosociality and Erotic in Italian Renaissance Portraiture'. Pp. 29–51 in J. Woodall ed. *Portraiture: Facing the Subject.* Manchester/New York.

La médecine dans l'Antiquité grecque et romaine by Helen King and Véronique Dasen

Lausanne: Éditions BHMS, 2008. Pp. xii + 130. ISBN 978–2–9700536–6–8. Paper € 18.66

Reviewed by
Janet Downie
Princeton University
jdownie@princeton.edu

In the preface to his monumental edition of the Hippocratic Corpus, the 19th-century philologist and physician Émile Littré compared his task as translator to that of a doctor engaged in bedside consultation: 'il a fallu souvent essayer un diagnostic rétrospectif, qui n'est pas entouré de moindres obscurités que le diagnostic au lit du malade' [1839–1861]. It was, of course, Littré's explicit intention that his work enrich the medical practice of his own time. But by evoking the complexities of philological 'diagnosis', Littré registers his awareness of the vast conceptual distance that separated him and his contemporaries from Greco-Roman culture. His work marks a turning point in the study of ancient medicine: long read by doctors as the work of a fellow professional, the Hippocratic texts now began to be claimed for philology [Jouanna 1982]. Several generations on, interest in the body as an index of social dynamics of power and expertise has extended this trajectory of estrangement. Increasingly self-conscious about how our readings of the ancient world are conditioned by our own intellectual and cultural history, scholars have also pushed back the boundaries of the evidence, setting archaeological evidence and para-literary texts alongside the staples of medical literature from the Hippocratic corpus to Galen.

Helen King and Véronique Dasen, in their brief and lively introduction to ancient medicine, propose to address this complex diagnostic *scenario*. When it comes to the basic question King poses in her introduction—'Did the Greco-Roman practice of medicine resemble our own?' [3]—the aim of both authors is to remind readers that in weighing the differences and similarities, there are no simple answers. Understanding ancient medicine, they propose, requires an

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 162–166

act of scholarly imagination that comes from full engagement with the range of evidence for Greco-Roman experience of the body.

The book has three parts. The first and longest is Dasen's French translation of King's *Greek and Roman Medicine*, originally published in 2001 in Bristol's Classical World Series. However, readers already familiar with this work in English will want to note that King has added a chapter on women and ancient medicine, 'Les femmes et la médecine antique' [1.8], and that Dasen has expanded the scope of the volume as a whole with two new dossiers of primary source material—all of which makes the French edition even richer and more useful than the earlier English version.[1] The dossier of texts in part 3 offers a judicious, if limited, assortment, ranging over a mere seven pages from the Hippocratic *Oath* to an excerpt from Gargilius Martialus' fourth-century AD *Remedies*. But it is the collection and discussion of visual images [Part 2. 'Médecine et iconographie: le discours d'images']—from vase paintings to votive reliefs and terracotta statuettes—that constitutes the crucial extension of King's close engagement with literary sources. In this volume, King and Dasen together succeed in offering general readers and beginning students a solid introduction to the history of ancient medicine, its theory, and its methodology. They also offer direct access to the archaeological materials and primary texts that support a multifaceted inquiry.

Given the brevity of the book and its broad scope, the great strength of King's account lies in her gift for distilling the distinctive features of the body as imagined by different sects and individuals and then showing what is at stake in the various attempts to map the human system and understand its pathologies. Since the Hippocratic body was one of humors in constant flux, the art of negotiating a balanced 'mode de vie' (δίαιτα) was medicine's central concern. Exercise, one's emotional state, and sleep were all part of 'dietetics', which means that to understand the reasons behind medical

[1] Also new in the French edition are a timeline of authors and works cited in the text (from the fifth century BC to the 19th century AD), a brief list of significant modern discoveries and inventions (from the microscope in the 17th century to the mapping of the human genome in 2003), and a general bibliography of primary sources (mostly in French translation) and secondary scholarship.

interventions, we need to understand whole constellations of cultural reference. The same substances might be both food and medicine in the Greco-Roman world, and it was often the resonance of myth and ritual that imbued φάρμακα with a symbolic power comparable in its workings to our modern placebo effect [1.7].

What happens, though, when humoral theories of individual self-regulation confront the sort of large-scale health crisis we would now call an epidemic? Thucydides' account of the fifth-century Athenian plague offers a case-study [1.3]. While retrospective biomedical diagnosis of the Athenian plague has long fascinated medical historians, King takes a different tack, investigating Thucydides' account to show that where explanatory models fall short, illness becomes a lightning-rod for issues of public morality. Women's medicine especially suggests that conceptions of order and disorder within the human body reflect strategies of social order; and as King suggests [1.8], our readings of the sources reflect, in turn, our own cultural biases. Scholarly attempts to identify a specifically female knowledge about ancient women's medical issues bear the imprint of post-classical medicine, whereas in fact, King argues, certain ideas about the female body—such as the notion that the womb wanders in search of hydration—were widely shared by female and male practitioners as well as their patients.

Since part of King's aim is to encourage awareness of the preconceptions modern scholars bring to ancient evidence, it is a perfect extension of this project to direct readers towards their own encounter with the primary sources, written and visual. From uterine amulets to a red-figure *aryballos* that depicts the inside of a doctor's office Dasen explores, in the 11 short chapters of part 2, the range of 'gestures that open, wound, and heal' [81], presenting a set of images with commentary that offer an inventory of the Greek collective imagination. Some of the most striking pieces in the visual dossier offer glimpses of the ancient body that go beyond written sources. A Sicilian terracotta statuette appears to represent a man with hemimelia [2.10], a condition that like most disabilities and congenital deformities is almost entirely absent from the medical literature. In other cases, the visual evidence extends or complicates what we know from literary sources. A vase painting contemporary with the Athenian plague shows a man in distress crouching before a small fire with a temple structure and a herm in the background [2.7]. Scholars are divided: does this figure represent the object of a scapegoat ritual evoked to rid Athens of the plague? Or is he a plague victim seeking refuge near a sanctuary and trying to purify the air of *miasma* with his small fire? Can we distinguish between ritual and medical gesture?

As Dasen points out [94], much of the iconography of illness betrays a certain reticence about probing the body's interior. While scenes of wounding are widespread in fifth-century vase paintings, for example, the perspective is social rather than medical. The heroic wound in visual culture is clean, expressing the dynamics of human interaction and the ethics and aesthetics of war instead of exposing the messy interior [2.3]. Anatomical votives from Greece [2.6] likewise tend to represent the surface of the body (with the exception of the womb) rather than its internal pathology and rarely depict the ailment for which divine help is sought. Other healing votives emphasize the social context of the dedicant, showing his family or illustrating the ritual circumstances of the encounter with the god [2.5]. Compared with the graphic display of human entrails that characterizes the famous Etruscan anatomical votives [2.2], votives in the Greek context are notably restrained. Images of sacrifice remind us, on the other hand, that the ritual offering of animals provided the most regular spectacle of 'la géographie corporelle' [86] in the ancient world. Hippocratic thought-experiments in human dissection, Dasen suggests, recall gestures familiar from animal sacrifice that—with very few exceptions—could be transferred only imaginatively to the human body.

In sum, the authors offer an engaging introduction to Greco-Roman medicine—one that is grounded in the primary sources, written and visual, and deploys a sophisticated analytical lens. My only slight criticism is bibliographic: because King's text is, appropriately, footnote-free, an annotated or topic-specific bibliography of secondary literature for part 1 (on the model of the succinct 'Pour en savoir plus' that accompanies each section of part 2) would probably have helped novice undergraduate, graduate, and general readers interested in following up a particular line of inquiry. This is, obviously, a minor point in view of the volume's great strengths. Balanced, insightful, and stimulating, this small book will be useful to those who teach ancient medicine in a French-language context, or in a bilingual setting, where the English original could easily be used alongside. As a stepping-stone to further study, it should make readers alert to the kinds of questions that we might ask about medicine in the ancient world—and prepared for the complexity of the answers.

BIBLIOGRAPHY

Jouanna, J. 1982. 'Littré, éditeur et traducteur d'Hippocrate'. *Revue de Synthèse* IIIe s. nos. 106–108:285–301.

King, H. 2001. *Greek and Roman Medicine.* Bristol.

Littré, E. 1839–1861. ed. *Oeuvres complètes d'Hippocrate.* Paris.

The Ancient Commentators on Plato and Aristotle by Miira Tuominen

Berkeley/Los Angeles: University of California Press, 2009. Pp. xii + 324. ISBN 978–0–520–26027–6. Paper $24.95, £16.95

Reviewed by
Lloyd P. Gerson
University of Toronto
lloyd.gerson@utoronto.ca

This ambitious monograph is the sixth in a series published simultaneously by the University of California Press and Acumen Publishing in Britain. The series intends to provide introductory studies of the major figures and themes in ancient philosophy. Among those already published, the volume on the Cynics by William Desmond and the volume on Neoplatonism by Pauliina Remes take up topics not normally treated with any degree of rigor in introductory texts. Forthcoming in the series are volumes on the non-Western philosophical traditions in Islam, Confucianism, and Indian Buddhism. This volume presents material with which even many specialists in ancient philosophy are unfamiliar. It is an altogether laudable effort to provide a clear and accurate introduction to the last great phase of ancient philosophy.

The 15,000 pages or so of the works of the ancient commentators on Plato and Aristotle written between, roughly, AD 200 and 600, comprise a good deal more than half of all the works of ancient philosophy that exist today. For this reason alone, it is unfortunate that this vast and complex body of work is so little known even among specialists in the field. That is perhaps slowly changing owing in part to the heroic efforts of Richard Sorabji and a dedicated team of translators who have over the last 20 years or so worked to provide scholarly English translations of the most important of these works. To date, about 70 volumes have been published with another 30 or so planned. Sorabji has published in 2005 a most valuable and convenient bridge to these works in a three volume sourcebook containing a large amount of the material arranged thematically give reference.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 167–170

Tuominen, quite reasonably, relies heavily on the division of material provided by Sorabji. After an introduction to the commentary tradition and the methodology of the commentators, there are chapters on epistemology [41–69], science and logic [70–117], physics [118–157], psychology [158–199], metaphysics [200–236], and ethics [237–279]. A concluding brief chapter summarizes the major issues discussed. The commentators whose positions are the principal focus of these chapters are Alexander of Aphrodisias (second–early third century), Themistius (*ca* 317–388), Porphyry (234–*ca* 305), Simplicius (*ca* 490–560), and John Philoponus (*ca* 490–570). Plotinus, Iamblichus, Proclus and a few others make occasional guest appearances. The title of the book is somewhat misleading because the actual extant commentaries on the works of Plato—few though they may be—are hardly discussed at all. Nevertheless, someone seeking to get an overview of the range of subjects taken up by the Aristotelian commentators will find in this book a good beginning. There is an especially helpful and substantial discussion of what was, during this period, the state-of-the-art regarding what we would call philosophy of science and formal logic. These subjects were, not surprisingly, the focus of much commentary material on the works comprising Aristotle's *Organon*, but they are seldom treated in much detail in the general histories of ancient philosophy.

Tuominen follows a fairly perspicuous format: a brief introduction to the philosophical issues under each heading, and then a survey of the views of the main commentators selected. Those unfamiliar with this material will no doubt discover in this book numerous challenges to contemporary received wisdom about what Aristotle is getting at or what are the problems he faced.

I have two main problems with this book. The first is that the author does not attend sufficiently to the Platonic principles that these mainly Platonic commentators on Aristotle brought to their work. Although there is a brief mention of this in the introduction, Tuominen does not keep before the reader's mind the fact that the reason for the extensive commentaries on Aristotle (with the possible exception of Alexander of Aphrodisias) was to provide an introduction to Platonism, to the so-called higher mysteries, according to Proclus and others. So, in order to appreciate the frequent criticisms of Aristotle made by the commentators, it is necessary to bring to the fore the Platonic principles which constituted the starting-points for the treatment of Aristotle. This the author does only sporadically. For example, since Plato's Demiurge is seldom invoked, a spurious distinction between the Demiurge and the Neoplatonic Intellect is assumed [131].

A related criticism, though more important, is the rather cavalier and inaccurate presentation of Aristotle's own philosophical positions. As Simplicius notes at the beginning of his commentary on Aristotle's *Categories*, the commentator on Aristotle must have read all of Aristotle and assimilated it before attempting to comment on a single work. This is I think equally true for an expositor of the commentary tradition.

Here are some examples of where I think the author has simply gotten Aristotle seriously wrong, or at least has presented his views in a most misleading fashion. Tuominen suggests [43] that Aristotle endorses in a qualified way the last definition of knowledge (ἐπιστήμη) in Plato's *Theaetetus*, according to which knowledge is true belief plus a λόγος. This definition is rejected in that dialogue. It is not true that Aristotle endorses it; in fact, in both the *Posterior Analytics* and his *Nicomachean Ethics*, Aristotle says that there is no knowledge of 'things that can be otherwise', that is, of the objects of belief (δόξα). This error colors the discussion of the commentators, when it is averred that they do not intend to 'restrict knowledge to permanent, necessary and unchanging facts' [93]. But in fact, following Aristotle, this is exactly what they do. The error is further magnified throughout a rather misleading discussion of the putative empiricism of the commentators. Another example is the treatment of *De anima* generally and Aristotle's view of human and animal cognition. It is not, I think, true to claim that the self-reflexivity of cognition that Aristotle ascribes to humans is also possessed by animals [162]. I do not understand what it means to say that 'something analogous to inference is attributed to them'. It is the immateriality of intellect that allows for self-reflexivity in Aristotle (and the commentators) and there is no evidence (despite the passages cited from *De anima* and *De somno*) that Aristotle thought that animals had immaterial intellects. It is at least misleading to say that φαντασία is, for Aristotle, a capacity that we share with animals [184]. For although animals do have this, Aristotle in the *De anima* clearly distinguishes the 'rational' imagination that we possess from the nonrational imagination of animals.

The treatment of Aristotle's *Metaphysics* which prepares the way for the discussion of the commentaries on, among other things, the *Categories* (which is not, of course, a work of metaphysics for Aristotle) is very odd indeed. According to Tuominen, Aristotle's *Metaphysics* offers an analysis of the sensible [201]. I suppose that this is in some sense correct; but it can hardly stand when coupled with the claim that, for Aristotle, 'the structure of being must be the structure of sensible reality' [210]. The ancient commentators were certainly not alone in understanding that Aristotle did not identify the primary focus of the science of being with sensible substance. Yet, supposing this, it is natural that the author would include the discussion of the categories of sensible reality under metaphysics.

These mistakes serve to undermine somewhat an otherwise admirable effort to erect some signposts for new travelers on what is now the last frontier of ancient Greek philosophy. The prodigious work involved in assembling this survey will no doubt be received with gratitude by many students in the field.

BIBLIOGRAPHY

Sorabji, R. R. K. 2005. *The Philosophy of the Commentators, 200–600 AD: A Sourcebook.* 3 vols. Ithaca, NY.

Essays on David Hume, Medical Men and the Scottish Enlightenment: 'Industry, Knowledge and Humanity' by Roger L. Emerson

Science, Technology and Culture, 1700–1945. Farnham, UK/Burlington, VT: Ashgate, 2009. Pp. xx+295. ISBN 978–0–7546–6628–8. Cloth £65.00, $124.95

Reviewed by
Rachel L. Dunn
University of Durham
r.l.dunn@durham.ac.uk

The Scottish Enlightenment was a period of the 18th-century in which Scotland, driven by utility and rationality, embraced intellectual, social, and scientific developments. This book builds on many years of scholarship by Roger Emerson and is an intriguing collection of revised conference papers and new essays. Emerson intends to involve the reader in a holistic approach to the topic, echoing Hume and the subtitle of the book that 'Industry, Knowledge and Humanity' are forever joined and flourish in times of refinement. Through 10 chapters, Emerson examines the context of the Scottish Enlightenment, the interaction between the Scots and their European neighbors, medical education, and the characters central to the Enlightenment such as David Hume and the third Duke of Argyll.

The first chapter seeks to examine the circumstances of the Scottish Enlightenment by reflecting on the countries that influenced Scotland and the movement towards making Scotland 'culturally respectable'. Emerson makes the point that at the beginning of the 18th-century constraints on travel meant that Scots often associated with the French and the Dutch. Scots traded in continental Europe and the professional classes were frequently educated abroad, as prior to 1726 neither medicine nor law could be studied completely in Scotland. Emerson makes good use of recent research in this field as he describes how Scottish professionals furthered their education in places like Leiden and La Flèche. He is also keen to contradict Roy Porter's suggestion that the Scottish Enlightenment was part of an English one [Porter 2000, xvii–xviii], and argues instead that it was a

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)

distinctly separate affair. The subsequent chapters defend this position, proving that the expressive and tolerant Enlightenment changed Scottish society so that it could compete in a rapidly evolving world.

Chapter 2 chapter provides detailed insight into Archibald Campbell, third Duke of Argyll, who was a fierce patron of the Scottish Enlightenment. The essay is based on Emerson's forthcoming biography. He suggests that more time should be devoted to researching patrons like Campbell who made the careers of enlightenment figures possible. As little has previously been written about him, this is a welcome chapter which is not only a condensed biography but also describes the Duke's methodology for patronizing certain causes. The Duke's influence was felt in many sectors and he fought to maintain Scottish independence. The extent of his patronage is acknowledged as a major factor in the progress of the Scottish Enlightenment. To reinforce this sentiment, Emerson asks, 'What would Scotland and the Scottish Enlightenment have been without a patron such as Argyll?' [38]. The answer is left for the reader to decide; but from the information put forward, I would suggest it may not have been as far-reaching or as swift.

Recognizing that there were relatively few people involved in the European enlightenments, chapter 3 attempts to estimate the number of Scots that were enlightened. It is a useful exercise, as it points out not only that women were almost excluded but also that the enlightened few were centered around the cities of Aberdeen, Edinburgh, and Glasgow. After much analysis and some guesswork, Emerson emerges with an effective enlightened community of no more than 1,300 between the three municipalities. He further reduces this figure to about 700 people in 1760 who would be considered the visible enlightened. The social and political elite was well represented in this group. Given that the upper classes feared that the enlightenment could prove socially disruptive, Emerson acknowledges the importance of the social historical aspects in understanding how national enlightenments arose and were adopted.

Chapter 4 provides insight into education as it focuses on what 18th-century Scottish students read at both school and university (college). Emerson explores the changing face of children's literature citing John Locke as the mentor of British writers in the early 18th-century, followed by Jean-Jacques Rousseau. Books produced by authors such as Anna Laetitia Barbauld promoted piety and morality as they reflected the religious overtones of the school curriculum. Moving on to what university students read, Emerson gives a number of possible authors. However, it is often difficult to decipher what books were used because professors recommended many books, but infrequently assigned a key publication. To extend their education many students undertook extracurricular reading. By inspecting club libraries and personal collections, Emerson notes that much of this reading can be traced. This chapter provides a useful introduction to the reading habits of 18th-century students, but it is in the footnotes that gems of inspiration can be found.

The next four chapters concern David Hume (formerly Home). By devoting over a third of the book to Hume, Emerson signifies his importance within the Scottish Enlightenment. During his lifetime Hume was primarily known as a historian and philosopher, occupations that most scholars focus on; however, he was also an important political economist. Hume's political economy is the focus of chapter 7 and fills a gap in Hume scholarship. Using extracts from Hume's correspondence, Emerson creates an image of Hume's life and work that is perceptive and recognizes the anxiety which he felt as a younger man. One aspect of chapter 5 that follows nicely from the previous chapter deals with Hume's changing reading habits. Hume had become bored with his law studies and had taken to reading literature and philosophy, becoming something of a Stoic.

Chapter 6 is a response to M. A. Stewart's essay, 'Hume's Intellectual Development 1711–1752' [2005]. Emerson addresses the intellectual development of Hume the historian, recognizing how history had fascinated him from an early age. By speculating on how the topographical features of his boyhood homeland could have fostered interest in local history, Emerson explores the sources of information such as the local kirk that would appeal to an inquisitive young mind. The analysis of what Hume read dovetails quite nicely with the discussion of what 18th-century Scottish students read that concerned chapter 4.

The exploration of Hume's historical writings and political economy is perceptive and thorough. Emerson considers the problems of structure and direction in Hume's work and highlights the religious views and the undercurrent of pessimism that he believes ran through Hume's work. Given the detailed pictures created throughout the text, images of Hume and the third Duke of Argyll would have provided an additional dimension to the narrative.

The penultimate chapter echoes chapter 3 as it attempts to determine how many Scots were trained in medicine and surgery. Such an exercise is interesting as it not only assists in the history of medical education but also emphasizes the economic and social importance of the trainee medics. During the discussion of how many non-Scottish medics were educated in Scotland, there is no mention of the number of English Dissenters excluded from Oxbridge who crossed the border to train. An investigation into such figures would provide a useful addition to the chapter.

The final chapter examines the utilitarian nature of the movement. Emerson recognizes that since 1985 there has been increased interest in the 18th-century and the Scottish Enlightenment. The resultant scholarship has shed new light on the enlightenment and led to the birth of societies like the 18th Century Scottish Studies Society. However, Emerson remarks that studies are often flawed as little attention is given to the social context of enlightenments, something that his work has addressed.

Throughout the book, Emerson makes references to both primary and secondary sources, which are detailed in both footnotes and listed in an extensive bibliography that includes many forthcoming publications. When transcribing Hume's correspondence, it is good that the original spelling and punctuation has been retained. The numerous footnotes add richness to the narrative of each chapter, often providing intriguing nuances and additional detail. One criticism that I have is that on some pages the spatial arrangement is skewed as the footnotes outweigh the body of the text. However, it is better to have information in footnotes than to turn constantly to the end of the book to retrieve it.

Individually the essays are interesting and insightful, and they force the reader to reflect upon the relationship between intellectual and social developments. Together they form an impressive collection that provides outsiders with a detailed overview of aspects of the Scottish Enlightenment. Owing to the book's composition, it has the potential to feel disjointed. Emerson manages to avoid this, even integrating a chapter on the number of Scottish medics which at first glance appears out of place towards the end of the book. By locating the count in chapter 9, it builds on the economic and political discussions of previous chapters and avoids the creation of a dense block of statistical analysis that could have disrupted the flow of the narrative. Overall the author's detailed research and clear prose paid off as the book was entertaining and stimulating; it certainly achieves its aim of embracing 'industry, knowledge, and humanity'.

BIBLIOGRAPHY

Porter, R. 2000. *The Creation of the Modern World: The Untold Story of the British Enlightenment.* New York /London.

Stewart, M. A. 2005. 'Hume's Intellectual Development 1711–1752'. Pp. 11–58 in M. Frasca-Spada and P. J. E. Kail edd. *Impressions of Hume.* Oxford.

The Other Mathematics: Language and Logic in Egyptian and in General by Leo Depuydt

Piscataway, NJ: Gorgias Press, 2008. Pp. xxii+373. ISBN 978–1–59333–369–0. Cloth $182.00

Reviewed by
Micah T. Ross
Kyoto Sangyo University
micah.t.ross@gmail.com

Despite its title, *The Other Mathematics: Language and Logic in Egyptian and in General*, this book by Leo Depuydt addresses the field of Egyptian grammar more directly than the topic of Egyptian mathematics. Yet, although Depuydt addresses grammarians more directly than historians of science, *The Other Mathematics* takes the work of George Boole as an unexpected point of departure for an analysis of conditional sentences in Old, Middle, and Late Egyptian as well as Coptic—but not Demotic: although Depuydt has published Demotic texts, *The Other Mathematics* omits this phase of the Egyptian language. Historians of science and mathematics will not find presentations of familiar texts from Egyptian mathematics, or new editions of previously unpublished texts, or even a reinterpretation of various enumerations, lists, and tables as a type of mathematical structure. Rather, the title refers to 'attribute mathematics' in which 'all things sharing an attribute together form a class or set' [16]. By definition, then, *The Other Mathematics* excludes numbers and focuses on symbolic logic, 'nothing more or less than... a kind of mathematics' [40]. However, because this approach applies modern logic only to ancient grammar, *The Other Mathematics* has next to no relevance to the idea and practice of science within an Egyptian context and only a limited bearing on the idea and practice of science outside of Egypt.

Depuydt provides a clear key to the reader when he summarizes the contents of his book [11–13] and identifies the first five chapters as a logical unit which establishes the differences between two sentence types, conventionally translated as conditional sentences. Chapter 1 establishes the grammatical structure of these two sentence types. Chapter 2 reviews the development of symbolic logic,

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 176–179

but be forewarned: this recapitulation is limited to six pages. Chapter 3 occupies a single page and provides a definition of the difference between the two sentence types with reference to the types of statements categorized by Boole and Venn. Chapter 4 summarizes the logical properties which must be addressed by the two Egyptian sentence types. Chapter 5 collects examples which express these properties from the corpus of Egyptian literature. Depuydt provides a clear overview to these chapters in the contents and has composed the chapters according to a rigorous logic. The ease of reference and clarity of structure outweighs the criticisms of uneven, choppy, or repetitive writing.

The ‘second unit’ contains five chapters which depend on the conclusions of the ‘first unit’. Chapter 6 establishes the logical certainty of the conclusions of the first unit. Chapter 7 considers the condition *sine qua non* as a special case of the conditional sentence. Chapter 8 presents important information for grammarians of the Egyptian language: an exhaustive compilation of ‘balanced sentences’. In Chapter 9, Depuydt presents a remarkably lucid and readable account of the historical development of grammatical forms from the decipherment of hieroglyphics to modern debates. Chapter 10 considers a special case of the *sḏm.f* verb. Chapter 11 argues that the Egyptian language increased in complexity and sophistication of expression as it developed over time. Chapter 12 derives rules from the first unit and explains away several commonly accepted features of the Egyptian verb.

Four appendices follow the second unit. The first two appendices contain articles which have appeared elsewhere; the third relates the mental acts associated with conditions and premises to circuits and switches; and the final one collects errata to Depuydt’s previous publications.

The methods and philosophical underpinnings of *The Other Mathematics* merit direct consideration. Depuydt never specifically addresses the topic of Structuralism but because the Boolean ‘Laws of Thought’ serve simultaneously as a point of departure, as an absolute mathematical truth, and as the means of verification, *The Other Mathematics* may be described as the first Structuralist grammar of the Egyptian language. Depuydt praises Boolean logic because it is demonstrable, certain, and internally consistent [22–23]. Oblique references to the usefulness of Boolean logic in the fields of computer science [10] and electrical engineering [back cover text, appendix 3] pepper the work. Finally, Depuydt notes that Boolean logic 'supplanted' Aristotelian and scholastic logic [40]. An undeclared Structuralism may explain the potentially anachronistic subjugation of Egyptian grammar to Boolean logic.

Because the topic of language (whether in Egyptian or in general) has limited relevance to the idea and practice of science, the portions of *The Other Mathematics* which treat logic demand closer scrutiny. Perhaps the utility of Boolean logic to computer scientists and electrical engineers has supplanted Aristotelian and scholastic logic in the European tradition, but Depuydt neglects to contextualize this development against the larger backdrop of other logical systems. Indeed, Depuydt's presentation of logic largely limits itself to Boole and Venn, with some additions by Shannon. Depuydt does not discuss the development of logic in non-European contexts. No mention is made of the grammatical rules of Panini, the inferences of Gotama, or the *tetralemma* of Nagarjuna. Likewise, no discussion of the Mohist School of Names appears; nor are Hui Shi or Gongsun Long introduced. An uninformed reader of *The Other Mathematics* might conclude that although some early work on logic had been done by Aristotle [39], Anaximander [43], or Cicero [242], Boole defined the field and all logical systems agree with him. In fact, one interesting result of the development of symbolic logic has been that symbolic logic has enabled paraconsistent logic to be understood as a separate logical system rather than as a fault of translation or a linguistic artifact.

If the topic of logic in general is sidestepped, the topic of logic in Egyptian could be expanded considerably. Depuydt treats only conditional sentences, but what could be said about each of Boole's logical operators? Negation is not a simple matter in the Egyptian language and a discussion of the various negations, rendered into symbolic logic, might prove both illuminating and entertaining. Another problem that demands attention is that of union and disjunction. Depuydt has already written on this topic in *Conjunction, Contiguity, Contingency: On Relationships between Events in the Egyptian and Coptic Verbal System* [1993] but neither expands this work to non-verbal cases nor reports the results as relevant to the topic of logic in general. Still another interesting topic, from a logical perspective, would be a discussion of the range of the use of the Egyptian word ‘ky’ (‘other’). Potentially rewarding topics could be multiplied and perhaps Depuydt will visit them in future publications.

BIBLIOGRAPHY

Depuydt, L. 1993. *Conjunction, Contiguity, Contingency: On Relationships between Events in the Egyptian and Coptic Verbal System.* New York/Oxford.

Michael Psellos. Kommentar zur Physik des Aristoteles, editio princeps. Einleitung, Text, Indices by Linos G. Benakis

Corpus Philosophorum Medii Aevi: Commentaria in Aristotelem Byzantina 5. Athens: Academy of Athens, 2008. Pp. ix + 94* + 450. ISBN 978–960–404–124–4. Cloth

Reviewed by
David Bloch
Copenhagen, Denmark
bloch@hum.ku.dk

The first commentators on Aristotle began their work very soon after the edition that Andronicus of Rhodes made in the first century BC. The surviving works of ancient scholars such as Alexander of Aphrodisias (fl. 200), Porphyry (third century AD), Simplicius and Philoponus (both sixth century AD) were edited well over 100 years ago under the auspices of the Academy of Berlin with Hermann Diels in charge of the project. They are still much used by modern Aristotelian commentators, and quite a few studies have appeared in recent times.[1] In contrast, there has been until recently little care for the Byzantine commentaries on Aristotle. However, things seem to be changing.[2] Granted, it may be that the level of philosophy and philosophical interpretation in these commentaries is not as high as in the works of their predecessors; and it is certainly true that the Byzantine Aristotelian commentators relied heavily on previous commentaries, sometimes to an extent that we would call 'plagiarism'. But these commentaries are still very important in at least two respects: they contribute to our understanding not only of the personal traits of the individual scholars of Byzantium, in this case the prominent

[1] See, e.g., Sorabji 2003–2005 and Tuominen 2009 with bibliographies. Also, Richard Sorabji is editing the series of translations, Ancient Commentators on Aristotle.

[2] Note, in particular, the two relatively new series Commentaria in Aristotelem Byzantina (of which the present book is part) and Commentaria in Aristotelem Graeca et Byzantina.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 180–187

thinker Michael Psellos (1018–after 1081),[3] but also of the general level of education, philosophy, and science in the Byzantine period. Linos Benakis' edition of Psellos' commentary on Aristotle's *Physics*, which is one of the oldest extant commentaries [11* with n15] if it is actually by Psellos [see below], is a very welcome contribution in both respects, and an excellent scholarly work in its own right.

The book under review consists of two main parts: a careful introduction [3*–94*] and the text itself [1–430]. Indices [431–440] and eight photographic reproductions [443–450] occupy the last pages of the book. It is, then, primarily a critical edition of a previously inaccessible text, and as such it is obviously valuable.

In the introduction, Benakis discusses the *Stand der Forschung* concerning the commentary [3*–20*], Psellos' biographical details as they bear on the commentary [21*–*25*], the nature and character of the commentary [26*–46*], and the textual transmission [47*–64*]. At the end, Benakis provides us with a substantial bibliography [65*–94*]. I find the introduction excellent, providing, as it does, all the necessary information and tools to read the commentary. Two general and important points seem, however, not to have been settled.

First, it is an important question concerning this commentary on the *Physics* whether it was written by Psellos at all. The very fact that this problem can be raised unfortunately limits our possibilities of saying anything about Psellos' character based on the commentary. And, even more importantly, it may have serious consequences concerning the date of the commentary. In 11th-century Constantinople, Aristotelian philosophy and science were apparently studied almost exclusively in the form of compendia that comprised Aristotelian thought in more accessible form; at least that is the general impression from the available material. Only the works on logic were normally studied in the original, and compendia were used for these as well. But if Psellos was indeed the author of the commentary on the *Physics*, then obviously this work was also read—and not only by Psellos himself but more generally in school, as Benakis points out [25*]. Therefore, Benakis spends a number of pages [5*–10*] on

[3] On Psellos, see, e.g., Kriaras 1968; Duffy 2002; Moore 2005; the essays in Barber and Jenkins 2006; and, of course, the introduction to the book under review.

Pantelis Golitsis' view [2007][4] that the author was in fact Georgios Pachymeres (1242–*ca* 1310). It is, however, not easy to discern the truth here; and more can certainly be said about the problem: Golitsis' arguments on paleographical and codicological grounds are not substantially addressed and, to my mind, Benakis' own arguments based on parallel passages in the works of Psellos and on a stylistic comparison of Psellos with Pachymeres need further substantiation.[5] If Benakis is right, the commentary is extremely important for our understanding of 11th-century studies of the Corpus Aristotelicum and for our knowledge of the multi-talented Psellos; if Golitsis is right, it is further evidence of a period of Aristotelian studies that is much better known, and contributes to our understanding of another exceptional figure, Pachymeres. Surely, more work on this important topic can be expected in the near future. In any case, for the sake of convenience—and because Benakis certainly does have a case—I shall in the following refer to the commentary as being Psellos' work.

Second, one of Benakis' main points in the introduction is that much can still be learned from the commentary. In fact, he seems to say that Psellos' commentary could well be used on a par with the ones from late Antiquity as well as those from modern times [see, e.g., 11*, 28*, 37*–40*], thus adding effectively a third reason for studying the commentary to the two mentioned above. Certainly, a commentary is usually better than no commentary when studying Aristotle; but the use of Psellos, or any other Byzantine commentary, for the sole purpose of understanding the text seems unreasonable.

---

4 Apparently accepted, albeit with slight hesitation, by Ierodiakonou and Bydén [2008].

5 For instance, the parallel passages between the commentary and Psellos' other works are only valid evidence in this context if it can be shown that they are particularly Psellian, and Benakis does not do this. Also, it seems somewhat unfair to compare the techniques and style of the *Physics*-commentary with Pachymeres' *Epitome*, which is obviously a different kind of 'commentary' or rather 'philosophical work'. Also, it does seem strange that an introductory commentary written in a period in which Aristotelian works, apart from the ones of the *Organon*, were little used makes casual references to Aristotelian treatises such as *De caelo*, *De anima*, *Metaphysics*, *Nicomachean Ethics* and others: see 6.10, 11.16–17, 63.6–9, 70.21–23, 155.4–20, 192.4–5, as well as Benakis' comments on 37* and 437–439 of the index.

First of all, we have excellent modern commentaries, as Benakis well knows [67*–68*], that are much more accessible; second, contrary to Benakis, it might well be claimed that Psellos' commentary on the *Physics* does not reach the standards of ancient and modern commentaries [see below]; and finally, all Byzantine commentaries depend heavily on ancient ones, and this tends to make the individual commentaries much more eclectic and uneven in quality, style, and point of view. I also believe that the few passages which I describe below show that the commentary is not likely to be of much help at a high scholarly level. Still, Benakis is undoubtedly right that if any Byzantine commentator brings us an independent, coherent, and interesting interpretation of Aristotle, it would be Psellos [see, in particular, 29*–31*]. This is also clearly brought out by Benakis' analysis of Psellos' rather unique personality and philosophy [21*–25*]. Furthermore, its brevity compared to other commentaries does in some respects make it more accessible for beginners.

Much can be gathered, then, from Benakis' introduction, which is very useful for the understanding of Psellos' commentary. It is also to be applauded that the book is furnished with an introduction that is longer and more careful than can usually be expected from a critical edition. As will be clear from my comments above, I believe a number of important problems will remain disputed; but Benakis has given the reader extremely good tools for tackling the commentary and making up his or her own mind about these problems.

The commentary itself is too long to be treated in detail—but, on the other hand, it may be noted that it is much shorter than, e.g., the corresponding works of Simplicius and Philoponus. On the philological side, Benakis is probably more familiar with the contents of the commentary than anyone else today: he has critically evaluated the modern scholarly literature and he knows the manuscripts extremely well.[6] In the light of Golitsis' research [2007], one might dispute Benakis' basic choices of manuscripts for the edition; but the resulting text would in any case most likely look very similar to Benakis'. Moreover, the printed edition is certainly solid and a very welcome addition to the accessible Byzantine literature. The only thing missing, from my point of view, is a simple description of Benakis' methods of editing.

[6] For Benakis' work on these and similar matters, see also Benakis 2002.

As regards the contents of the commentary, a brief discussion of the introduction and the first part of the commentary proper will illustrate Psellos' approach. (I suspect that books 1 and 2, and book 1.1 in particular, will also be the most interesting for a modern audience.) The eight books of Aristotle's *Physics* may not be science in the modern sense of the word; but as a work of *Aristotelian* science it is an extremely important treatise since it provides the basic concepts of his views on the natural world. Book 1 is concerned with the fundamental principles of Aristotelian natural science. Books 2–4 examine, in particular, the concepts of nature, movement, cause, time, space, and void, which are crucial in Aristotelian natural science. Books 5–8 delve deeper into the problems connected with movement, this concept being the defining feature of natural science according to Aristotle. The commentary proceeds in orderly fashion through all of these, and Benakis has added an *apparatus criticus* and an *apparatus fontium*.

Psellos' introduction

Psellos explains that the work he is about to comment on is by Aristotle and is entitled 'Physics' (φυσικὴ ἀκρόασις) [2.4–5]. This is a theoretical science concerned with the basic principles of nature [2.1–4], and as such it is the most difficult among the treatises on natural science/philosophy [2.5–9]. Principles, Psellos continues [2.10–19, 3.1–6], can be conceived both as principles of things (πράγματα) and as principles of cognition (γνῶσις), that is, they can be ontological or epistemological. In the *Physics* they are both, according to Psellos. These brief comments constitute the content of the introduction; and it is fair to claim that they are simply minimum requirements for any student of *Physics*. In short, there is nothing new or particularly exciting here. Indeed, it is clear already from the introduction that the commentary is an elementary work designed obviously and explicitly [1.7–13] for students who have worked their way through the logical writings (the *Organon*) but who have not necessarily read any other works by Aristotle.

Psellos on *Physics* 1.1

In book 1, Aristotle identifies the most basic principles of natural science and discusses the views of his predecessors. Psellos' procedure is similar to, but not quite the same as, that of the ancient commentators: he inserts brief quotations from Aristotle's text (*lemmata*) and then comments on and paraphrases both the *lemma* and text following and related to this *lemma*. Through such comments and paraphrases, he explains, analyses, discusses, and elucidates the contents of the entire Aristotelian text. The first *lemma* reads 'Since to know (τὸ εἰδέναι) and to know (τὸ ἐπίστασθαι) ...',[7] and thus the first problem to be explained under this heading is rather obvious. What is the difference between the two Greek words for 'to know'? There are two possibilities, he says: either εἰδέναι is simply a more general term than ἐπίστασθαι (and in that case the latter is used to narrow down the concept of knowledge in this context); or εἰδέναι is simple and general knowledge of the kind that everybody has, whereas ἐπίστασθαι is knowledge proper, that is, (scientific) knowledge of things that cannot be otherwise than they are. These suggestions are possible; but in Psellos' description the difference between the alternatives is not clear. Moreover, as it turns out, the suggestions are not Psellos' own, but a truncated version of those found, discussed, and determined in the commentaries of Simplicius and Philoponus.[8]

In fact, these ancient commentators are both clearer and much more thorough in their treatments of the problem. Philoponus explains that the difference implied by Aristotle's wording is indeed the one that Psellos also describes, but he clarifies it further by saying that the second solution suggests a difference between demonstrative and non-demonstrative knowledge. This is important, since the reader can now see the actual difference between the two solutions. In addition, Philoponus' interpretation provides students who have read the *Organon*—which Psellos' students are supposed

[7] Aristotle, *Phys.* 1.1 184a10–12: 'Since to know (τὸ εἰδέναι) and to know (τὸ ἐπίστασθαι) occur in every investigation/science (περὶ πάσας τὰς μεθόδους) of which there are principles (ἀρχαί) or causes (αἴτια) or elements (στοιχεῖα), by cognizing these [*scil.* principles, causes and elements] ...' (my translation); treated by Psellos on pages 3.8–23 and 4.1–3.

[8] For Philoponus, see Vitelli 1887; for Simplicius, see Diels 1882, 12.14ff.

to have done before reading the *Physics*—with a better understanding of the text, and enables them to place these species of knowledge in Aristotle's overall theories of knowledge and science as found in the *Organon*, particularly in the *Posterior Analytics*. Simplicius is even more thorough and distinguishes the individual cognitive components (perception, opinion, and so forth) of the two Greek words.

Similar problems arise immediately afterwards with three other words in the same sentence when one asks what is the difference between principles, causes, and elements [see 185n7 above]. Again, Psellos is much briefer—and, to my mind, less clear and certainly less deep in his analysis—than his ancient predecessors; and he has again taken much of his argument and descriptive vocabulary from them. Furthermore, he oddly fails to say anything about elements.[9]

These brief passages are, I think, representative of the commentary in general. It is elementary and rather heavily dependent upon the earlier commentary tradition. In some instances, one would even benefit from supplementing it by looking also in Philoponus and—especially—in Simplicius. But it is not a sloppy work; and the author, whether Psellos or Pachymeres, is obviously very well acquainted with his material. This also means that it is extremely important when one is examining the kind of basic scientific training that students were given in Byzantine times—whether in the 11th or in the 13–14th centuries, although the former would naturally be the more interesting.

In conclusion, this book is a valuable addition to our understanding of the scholarly and scientific methods and standards of the Byzantines. The introduction equips the reader with the necessary tools, and the commentary itself opens the door to the science of the Greek Middle Ages. I am not convinced that high-level scholars would then, not to mention now, benefit much from it in their usage and understanding of Aristotle; but it certainly reveals the training that a first-rate teacher would give his students. No one able to read

[9] There is, in fact, a rather obscure brief note on 'element' in the manuscripts, but Benakis deletes it—rightly I believe—as being a secondary intrusion.

Greek and interested in the science and scholarship of this period will want to ignore this volume.

BIBLIOGRAPHY

Barber, C. and Jenkins, D. 2006. edd. *Reading Michael Psellos.* Leiden.

Benakis, L. G. 2002. *Texts and Studies on Byzantine Philosophy.* Athens.

Diels, H. 1882. ed. *Simplicii in Aristotelis physicorum libros quattuor priores commentaria.* Commentaria in Aristotelem Graeca 9. Berlin.

Duffy, J. 2002. 'Hellenic Philosophy in Byzantium and the Lonely Mission of Michael Psellos'. See Ierodiakonou 2002, 139–156.

Golitsis, P. 2007. 'Un commentaire perpétuel de Georges Pachymère à la *Physique* d'Aristote, faussement attribué à Michel Psellos'. *Byzantinische Zeitschrift* 100:637–676.

Ierodiakonou, K. 2002. ed. *Byzantine Philosophy and Its Ancient Sources.* Oxford.

Ierodiakonou, K. and Bydén, B. 2008. 'Byzantine Philosophy'. In *The Stanford Encyclopedia of Philosophy* (Summer 2008). http://plato.stanford.edu/entries/byzantine-philosophy/.

Kriaras, E. 1968. 'Psellos'. Pp. 1124–1182 in *Paulys Realencyclopädie der classischen Altertumswissenschaft*, Supplementband 11. Stuttgart.

Moore, P. 2005. *Iter Psellianum: A Detailed Listing of Manuscript Sources for All Works Attributed to Michael Psellos, Including a Comprehensive Bibliography.* Toronto.

Sorabji, R. 1990. ed. *Aristotle Transformed: The Ancient Commentators and Their Influence.* London.

________ 2003–2005. ed. *The Philosophy of the Commentators, 200–600 AD: A Sourcebook.* 3 vols. Ithaca, NY.

Tuominen, M. 2009. *The Ancient Commentators on Plato and Aristotle.* Berkeley/Los Angeles.

Vitelli, H. 1887. ed. *Ioannis Philoponi In Aristotelis physicorum libros tres priores commentaria.* Commentaria in Aristotelem Graeca 16. Berlin.

From Summetria *to Symmetry: The Making of a Revolutionary Scientific Concept* by Giora Hon and Bernard R. Goldstein

Archimedes 20. New York: Springer, 2008. Pp. xvi + 335. ISBN 978–1–4020–8447–8. Cloth € 149.95

Reviewed by
Joseph W. Dauben
City University of New York
jdauben@att.net

The basic idea of this fascinating book is that while symmetry has often been regarded as an innate concept of the human mind, there is no historical evidence to support this; and that in fact, the understanding of symmetry is basically a product of the 18th century. As the authors argue, there are two major aspects to this matter, one aesthetic, the other mathematical, both converging on the figures of Adrien-Marie Legendre, who was the first to formulate an exact mathematical definition of symmetry in terms of what he called 'incongruent counterparts', and Gaspard Monge, who was the first to use the term 'symmetry' in a textbook on statics written for students in the French naval academy (wherein symmetry was applied to the problem of determining the center of gravity of ships). In their consideration of the aesthetic aspects of the history of symmetry, the authors consider such thinkers as Plato and Archimedes, Galen, Vitruvius, Alberti, Dürer, Perrault, Montesquieu, and Diderot; whereas the mathematical side of the story includes the works of (again) Plato and Aristotle, Euclid, Archimedes, Boethius, Oresme, Kepler, Galileo, Barrow, and Newton, among others. Noteworthy is the authors' attention to such matters as the subject of harmony and its relations to symmetry in studies of the impact of Vitruvius on Copernicus and the architectural conception of a planetary system, Galileo and the significance of harmony in music, Kepler and Descartes on the structure of snowflakes, and the extent to which both Kepler and Leibniz regarded harmony as a fundamental concept in astronomy and metaphysics. The authors also consider the appearance of symmetry in natural history, specifically in the contexts of botany, crystallography, and zoology.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 188–190

Undoubtedly the one aspect of this book that will cause the greatest concern among historians of mathematics is the extent to which (or whether) the concept of symmetry can be considered as truly 'revolutionary'; and if so, in what sense we are to understand the concept of 'revolution'. This problem has been treated extensively since the work of Thomas Kuhn, whose analysis of *The Structure of Scientific Revolutions* seemed to suggest that revolutions could not occur in mathematics due to its cumulative nature.[1] After Goldstein and Hon consider the 'revolutions' that did not happen in the works of Euler and Kant with respect to symmetry, they devote an entire chapter to Legendre's 'revolutionary definition of symmetry as a scientific concept', whereby he regards it as a relation, not a property, by drawing on Robert Simson's critical commentary on Euclid. Here the problems of symmetrical polyhedra and mirror images in optics played their parts in generating Legendre's thinking about symmetry. Early responses to Legendre's definition of symmetrical solids by Lacroix, Garnier, Hirsch, and Cauchy bring the book to an end, with a final chapter dealing with applications of symmetry in mathematics and physics in the period 1788–1815, where the book concludes with considerations of bilateral symmetry and its significance as an abstract concept in terms of events (probability) and functions (algebra).

The authors maintain [49] that Legendre's definition deserves to be regarded as 'revolutionary' because the 'pace of usages of symmetry accelerates: new applications of symmetry appear in a variety of scientific domains'. They argue that with his 'explicit definition of equality by symmetry which he embedded in the proof structure of his *Éléments de géométrie* (1794)', subsequent systematic application of the concept of symmetry in diverse areas of science 'took a dramatic turn'. Because there was no evolution of concepts from the past that led to Legendre's novel concept, they consider Legendre's definition as a break with past tradition regarding symmetry, and therein lies its revolutionary character in mathematical terms. Understanding symmetry as a transformation which leaves something invariant, the authors again stress the meaning of symmetry in the sense of relation rather than property.

[1] See the extensive consideration of this matter in the collection of essays edited by Donald Gillies [1992].

This is a book full of technical detail, but with plenty of interesting information to engage readers well beyond the circles of mathematicians and historians sure to be interested in this account of symmetry.

BIBLIOGRAPHY

Gillies, D. 1992. ed. *Revolutions in Mathematics*. Oxford. Repr. 1995.

Aristote au Mont-Saint-Michel. Les racines grecques de l'Europe chrétienne by Sylvain Gouguenheim

Paris: Éditions du Seuil, 2008. Pp. 281. ISBN 78–2–02–096541–5. Paper € 21.00

Reviewed by
Gad Freudenthal
CNRS, Paris
freudent@msh-paris.fr

When this book appeared in March 2008, it immediately raised an immense uproar, not only among the directly concerned students of medieval philosophy and science but among many intellectuals too. Newspapers carried articles for or against, petitions were signed, the author was prevented from teaching at his institution of higher learning, and what not. It was *un scandale à la française*, as only France can produce it. Why?

Gouguenheim's book advocates a strong thesis concerning the contribution of the Islamic/Arabic world to Western medieval culture, and, hence, to the emergence of modernity in Europe. According to him, in recent decades the thesis that medieval European culture is strongly indebted to the Arabic/Islamic civilization has become a dogma; and he sets out to dismantle it. In other words, he would wish us to go back to the earlier, 19-century dogma defended most eloquently by Ernest Renan (1823–1892), who contended that the ancestor of modern European civilization is Greece alone, and that the medieval Arabic-Islamic culture contributed next to nothing to the advancement of science and philosophy.[1] Gouguenheim similarly thinks that European culture has no Islamic roots: 'Europe, and Europe alone, has created modern science,' he states [23]. Nineteenth-century euro-centrism has been replaced by orientalo-centrism, he further complains [17].

[1] E.g., in a famous lecture, 'L'Islamisme et la science', delivered at the Sorbonne on 29 March 1883 and first published in *Journal des Débats* on 30 March 1883 [see Psichari 1947, 946–965]. This lecture includes such statements as:

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 191–193

Gouguenheim maintains that the Greek heritage, especially Aristotle's writings, did not reach the West primarily *via* translations from Arabic (beginning in the 12th century) and that the Arabic/Islamic mediation in the reception of Greek science and philosophy was of secondary importance. Rather, after the fall of Byzantium, philosophical and scientific works continued to be studied and copied in the West in Greek throughout the centuries; and, therefore, they did not have to be 'rediscovered'. They were also translated into Latin directly from Greek without the mediation of Arabic. The latter came later and its importance was secondary. Gouguenheim particularly emphasizes the role of James of Venice, who translated Aristotelian works from Greek into Latin in the second quarter of the 12th century. The conclusion is that Scholastic science, and, further down the road, modern science, would have emerged even if there had been no Arabic-into-Latin translations.

To make the point that Europe's roots are Greek and owe nothing to the Orient, Gouguenheim devotes a chapter to arguing that the 'esprit grec' did not at all gain footing in the Arabic/Islamic world and remained an artificial implant, the sole occupation of a few intellectuals. Put differently: the Arabic/Islamic civilization does not provide a favorable context for the development of science; that is the sole privilege of the West.

Is Gouguenheim's antagonism directed against the Arabs or against Islam? 'Arab' of course includes not only Muslims but also Christians, Jews, and Pagans. Gouguenheim goes out of his way to emphasize time and again that scholars who played an important role in the development of science in Arabic were Christians. Clearly, he takes issue not with the Arab Orient but with Islam: inasmuch as Islamic civilization was Islamic, it contributed to science only little;

Tel est ce grand ensemble philosophique, que l'on a coutume d'appeler arabe, parce qu'il est écrit en arabe, mais qui est en réalité greco-sassanide. Il serait plus exact de dire grec; car l'élément vraiment fécond de tout cela venait de la Grèce.... La Grèce était la source unique du savoir et de la droite pensée.... [1883, 951]

This then is the great philosophical corpus which is usually called 'Arabic' because it is written in Arabic, but which in truth is Greek-Sassanian. It would be more exact to say 'Greek'; for the truly fruitful element in all this came from Greece.... Greece was the only source of knowledge and of right thought.

and where contributions emerged within it, this was mostly due to non-Muslim individuals. Gouguenheim's conception of Islam and of Islamic civilization is entirely limited to a superficial reading of the *Qur'an* and devoid of any sociological dimension.

Gouguenheim is aware that his subject has political dimensions and immediate implications for contemporary politics: it is part of the long *face-à-face* of Islam and the West, he writes [14]. In Europe, the subject of the West's indebtedness to Islam gains in visibility and in urgency on account, first, of the increasing presence of Muslims in many European countries and, second, of the looming question of Turkey's entry into the European Union. Gouguenheim's book not only can be used in this ideological-political struggle, it was written in order to contribute to it. This is perhaps the most disturbing and irritating aspect of this book: that it is written and argued as a pamphlet and not as a scholarly book (its 16 pages of 'selective bibliography' notwithstanding). This style, together with the innumerable factual errors and bad-will interpretations, are what has caused the uproar.

Readers may be interested to know that there came to light in September 2009 an argued rejoinder to Gouguenheim edited by Irène Rosier-Catach, Alain de Libera, Marwan Rashed, and Philippe Büttgen.

BIBLIOGRAPHY

Rosier-Catach, I.; de Libera, A; Rashed, M; and Büttgen, P. 2009. edd. *Les grecs, les arabes et nous. Enquête sur l'islamophobie savante.* Paris.

Psichari, H. 1947. ed. *Ernest Renan. Oeuvres completes.* vol 1. Paris.

Darwin Studies: A Theorist and his Theories in their Contexts by M. J. S. Hodge

Farnham, UK/Burlington, VT: Ashgate Publishing. Pp. xviii + 336. ISBN 978–0–7546–5939–6. Cloth $144.95 (online $130.46)

Reviewed by
Sandra Herbert
University of Maryland Baltimore County
herbert@umbc.edu

Ashgate's Variorum Collected Studies Series collects and republishes the shorter works of senior scholars. The Ashgate volumes differ from the traditional Festschrift but have a related function. This function is particularly important in the case of M. J. S. Hodge, for he has worked at the length of articles rather than books, and some of the articles first appeared in obscure places. Hodge's articles covering the pre-1800 period were contained in an earlier Ashgate volume, published in 2008. His articles in this volume all have to do with Charles Darwin.

Hodge's overall orientation as a historian of science is as a historian of ideas. The title of his Ph.D. dissertation from 1970 indicates his scope: 'Origins and Species: Study of the Historical Sources of Darwinism and the Contexts of Some Other Accounts of Organic Diversity from Plato and Aristotle on'. Thus, while the 10 articles or reviews that appear in this 'Darwin Studies' volume all pertain to Darwin, they rest on Hodge's assessment of an intellectual tradition going back to the Greeks. As Hodge put it, 'Philosophy learns from history how to relate the short run to the long run' [VII.249]. Thus, in a contribution from 1985 to a French symposium, Hodge began discussion by commenting on Aquinas, noting that, for Aquinas, no new forms may come into matter within what Hodge termed the 'administrative course of nature, because such initial introductions are completed in the constitutional work' [VII.229]. Hodge's approach has had more in common with such scholars as Stephen Toulmin and John Greene, both of whom were active when Hodge began his work, than it does with younger historians, many of whom are more oriented towards social history, or younger philosophers, many of

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 194–195

whom are more inclined towards prescription or analysis. Hodge's orientation towards intellectual rather than political and social history was also indicated by his reprinted reviews of the work of Robert Young, Adrian Desmond, and Jim Moore.

The bulk of the volume is devoted to articles that reflect Hodge's great strength: close descriptions of the process of reasoning shown by Darwin as he absorbed and evaluated such influences as the work of his teacher at Edinburgh Robert Grant and the monumental writings of Charles Lyell. In separate articles, Hodge covered Darwin as a 'lifelong generation theorist,' as a follower of Lyell ('the Lyellian origins of his zoonomical explanatory program') and (in an article written with David Kohn) as a reader of Malthus ('the immediate origins of natural selection'). Hodge's focus was primarily, though not entirely, on the early Darwin—the medical student at Edinburgh, the naturalist on H. M. S. Beagle, and the London theoretician. As perhaps befits the inherent interest in continuity that underwrites the discipline of the history of ideas, Hodge avoided making any one period in Darwin's development supreme. For example, he viewed Darwin's stay at the Galápagos as important rather than pivotal.

Since the centennial year for the *Origin of Species* was celebrated in 1959, there has been a magnificent outpouring of scholarship devoted to understanding Darwin's work and its intellectual context. M. J. S. Hodge has been a well-read and constructive member of the community of scholars working on that subject. His collected writings are unique, engaging, and permanently valuable contributions to scholarship.

The Art, Science, and Technology of Medieval Travel edited by Robert Bork and Andrea Kann

AVISTA Studies in the History of Medieval Technology, Science and Art 6. Aldershot, UK/Burlington, VT: Ashgate, 2008. Pp. xiv + 225. ISBN 978–0–7546–6307–2. Cloth $99.95

Reviewed by
Iain Macleod Higgins
University of Victoria
imh@uvic.ca

The medieval millennium is not normally considered a great age of travel. We look back at it across 500 or so years of European global expansion, a period characterized by systematically pursued exploration, trade, colonization, missionary activity, emigration and immigration, grand tours, and tourism, not to mention the mass displacements caused by war, famine, and ethnic cleansing. Yet, even leaving aside the large-scale medieval movements of peoples ('Germans', Vikings, Magyars), one can say that, despite genuine differences in scale and scope, people travelled a great deal in the medieval world and more than a few of them did so extensively, sometimes even in considerable numbers. Not just pilgrims, but also missionaries, scholars, and merchants made their way by land, river, and sea to destinations both far and near.

Marco Polo is only the most famous medieval traveller nowadays, but there were others who made remarkable journeys of their own: Margery Kempe from England to Jerusalem, Rome, Compostella, and Prussia in the early 15th century, for example; or Friar Odoric of Pordenone from Italy to Khanbaliq (Beijing) in the early 14th century; or Leifr Eiríksson from Norway to Vinland around the year 1000. Nor should one forget the great Muslim travelers like Ibn Battuta and Ibn Jubayr, or the Jewish travelers like Abraham ben Jacob and Benjamin of Tudela. Many medieval practices, technological developments, and attitudes, moreover, persisted into the early modern period—for instance, travel to the Holy Land, the portolan chart and the magnetic compass, and various degrees of Christian hostility to non-Christians—and these helped shape those travels that

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 196–206

radically altered the world at the end of the 15th century, when the post-Viking European encounter with the Americas exploded the old geography shared by the Ancients and medieval Christians, Muslims, and Jews alike.

There have been studies of many elements and modes of medieval travel, especially of pilgrimage; but an integrated history of (Christian) European travel from the time of the Bordeaux Pilgrim's and Egeria's religious journeys in the fourth century to the era of Portuguese and Spanish expansion, or even from the First Crusade to just before the scientific voyages of the later 18th century, remains to be written. While waiting for such ideal studies to appear, anyone interested in medieval travel will find the present book of value, even though travel as such is only one of its concerns and the quality of its contributions is somewhat uneven. Specialist readers will no doubt take issue with specific claims, details, or arguments in individual essays; but the collection as a whole gives a fair indication of the richness and complexity involved in studying travel in the medieval era, and is perhaps most useful as an overview of the issues that one might confront in thinking about medieval travel. In the words of its editors, the essays gathered here 'offer a series of complementary perspectives on the practice of medieval travel, with particular emphasis on artistic, scientific and technological developments' [7].

Following a general introduction, the book's 13 'complementary perspectives' are arranged in four parts: 'Medieval Vehicles and Logistics' (four essays), 'Medieval Travel and the Arts' (three essays), 'Medieval Maps and Their Uses' (four essays), and 'Medieval Navigational Instruments' (two essays). This arrangement neatly frames the visual within the practical and the theoretical; but a strong case could be made for placing navigation first or second, then proceeding to the maps and ending with the arts, since that would have moved book and reader alike through theory and practice to representation, with maps providing a polyvalent bridge between the more distant parts. Despite the title and the claim just quoted above, artistic developments receive the least attention in this collection, since maps and buildings are not really considered in their artistic dimensions.

Robert Bork and Andrea Kann's introduction begins with a brisk overview [1–7] of travel in the medieval world, discussing practices, attitudes, and documents, then proceeds to summarize the individual contributions [8–13] to show how this collection reveals 'the [medieval] dialog between theories and practices of travel' [7]. The latter part of the introduction, although it largely recapitulates the book, is in fact the more helpful of the two introductory sections, since its concise summaries allow the reader both to see the collection as a whole and to consider the various ways in which the different essays complement one another. The potted history of travel, in contrast, even though it does its job, requires more space to be effective.

Bernard S. Bachrach's erudite 'Carolingian Military Operations: An Introduction to Technological Perspectives', an extended footnote to his debated *Early Carolingian Warfare* [2001], opens the collection by raising the central issues of concern in the first four essays: technological constraints on travel and the variable influence of economic, political, cultural, and ideological factors on the technology of travel. In raising these issues, Bachrach turns to a form of travel not usually discussed as such: the coordinated movement of military forces. Roads, mapping, vehicles, and centralized bureaucratic control are all shown here to be crucial to Carolingian military success, which built on Roman legacies but put them to specifically Carolingian uses. In Bachrach's view, the combined weight of disparate evidence from sources such as the capitularies allows one 'to begin to think that Charlemagne was pressing an agenda of standardization' [29] that encompassed weights, measures, containers, and vehicles (especially the *basternae* or heavy-duty carts used to supply the troops) and thus allowed him to make effective use of the Roman inheritance of roads and military mapping based on written itineraries. In this reading of the evidence, then, the creation of the Carolingian Empire can be seen as a practical, indeed technological, achievement as well as an ideological one, and an achievement to which innovation and conservation alike contributed significantly.

Vehicle design, an important part of the complex Carolingian military system, is explored at length in the second essay, John E. Dotson's informative 'Everything is a Compromise: Mediterranean Ship Design, Thirteenth to Sixteenth Centuries'. Focusing on 'that archetypical Mediterranean vessel, the galley', Dotson shows 'how competing design demands, along with technological and natural limitations, shaped <its> evolution' [31]. Like the previous essay, this contribution asks its readers to think about what is overlooked or taken for granted in thinking about travel: in this case, the practical meaning of speed in the usual distinction between warships and merchant ships. Concluding that speed in the present-day understanding would have been ‘an almost unimaginable, and likely irrelevant, concept for a medieval seaman’ [32], Dotson argues persuasively that a combination of military, technological, and practical or economic factors (the cost of skilled oarsmen, for example, *versus* that of cargo) shaped the late 13th-century ‘revolution in galley design’ [35] that saw the trireme replace the bireme. Like Bachrach’s contribution, this essay shows that the technology of travel cannot be understood as ‘merely’ technological and that planning and design—whether of a campaign or of a warship—are almost always subject to multiple, competing influences.

Julian Munby’s detailed ‘From Carriage to Coach: What Happened?’ returns from the much-studied subject of sea travel to the lesser-known one of road transport. In a revealing contrast to the first two studies, though, this essay shows that social and technological change can also happen entirely independently of each other—an insight that has implications for the ways in which we might generalize about the technology of travel. Briefly, Munby argues that between the 13th and the 16th centuries no significant technological changes occurred in carriage technology—despite the introduction of a carriage suspended body—but that an important sociological transformation occurred nevertheless. Travel by carriage, which ‘had largely been an aristocratic and feminine domain’ [42], was rapidly taken up by men, especially after the appearance of the coach in late-15th-century Hungary (the vehicle took its name from its Hungarian context, deriving from Kocs, a small town between Budapest and Vienna). Even an old technology, then, ‘whose foundations were laid in the Bronze Age’ [53], is subject to changing uses that are sometimes independent of technical considerations. In this context in particular, Munby’s essay serves as a salutary reminder of the complex relations between the history of technology and other historical domains.

The final contribution to the opening section, David H. Kennett’s ‘Caister Castle, Norfolk, and the Transport of Brick and Building Materials in the Middle Ages’, is another helpful re-examination of the obvious: in this case, the assumption that heavy building materials were necessarily delivered by water rather than land. Using documents associated with the construction of a mid-15th-century brick house ‘set on relatively higher ground among the marshes and creeks of <Norwich's> River Bure' [55], Kennett notes that four different words were used to record the transportation of building materials ('carriage', 'carting', 'freight', and 'freightage'). While some materials (plaster of Paris and Caen stone, for instance) were brought by water; others, such as timber and bricks, may well have been brought by land. Moving outwards from Caister Castle, this study provides complementary documentary evidence for both water and land transport in order to argue that 'the transport of bricks on medieval roads' is 'much more common than might be first considered', given the potential hazards of water travel [67]. Influencing decisions about transport were factors such as local topography and the practicalities of loading and unloading vehicles, whether waterborne or land-based. As so often in the medieval world, whatever the domain, one finds widespread practices being adapted to specifically local conditions and demands.

From this stimulating cluster of essays on the complex intersection of technological, practical, economic, and ideological elements affecting both travel and transport, a reader would do well to turn to the book's last section, 'Medieval Navigational Instruments', since it is most closely linked to the first. In contrast to the development of vehicles, though, that of navigational tools shows theory weighing more heavily than practice until the 15th century. The first of the two useful essays gathered here is Richard A. Paselk's 'Medieval Tools of Navigation: An Overview'. After a concise, informative discussion of the compass, which includes an account of the Chinese as well as the western development and uses of an instrument that marked 'the greatest single advance in navigation' [170], Paselk turns to the navigational tools for measuring altitude (the quadrant, the mariner's astrolabe, and the cross-staff), focusing on their development first in the Portuguese context in the 15th century under the sponsorship of Prince Henry the Navigator and then in northern Europe during the 16th century. Politics and ideology were highly important here, as instruments were developed so as to allow sailors greater freedom from expert local knowledge, thereby enabling imperial exploration. Like the discussion of the compass, this concise account is also highly informative for the non-specialist who wants to understand how medieval tools both developed and were used.

Sara Schechner's concluding essay on 'Astrolabes and Medieval Travel' is more narrowly focused on one instrument but even more informative, considering the tool's Muslim as well as Christian uses and tracing its development from costly astronomer's implement to shipboard essential. After an impressively clear presentation of the nature and uses of a device that was 'both an observing instrument ... and a portable analogue computer ... used to solve astronomical, astrological, and geometric problems' [181], and that also served 'as a teaching tool' [184], Schechner takes up the question of whether this widely travelled instrument was itself used by travelers. Examining material remains along with textual and visual sources, she concludes that there is little evidence of the astrolabe in use on the road or at sea until the late 15th century when a 'new sea astrolabe' was developed by the Portuguese. Stripping the instrument 'of all its non-essential and most costly parts' [207], scholars and sailors developed a device that could be readily used aboard a rocking, windy ship. This more practical astrolabe was an important tool in all the major voyages of expansion from the 1490s on and was further refined for ease of use—a development showing once again the ways in which technological developments are variously linked to economic, political, and ideological factors.

A different sort of navigational tool was provided by medieval maps, whose form and uses varied much more than those of devices like the astrolabe or the compass, and maps are the focus of the four essays in part three of this collection. Nigel Hiscock's 'Mapping the Macrocosm: Christian Platonist Thought behind Medieval Maps and Plans', which draws on the author's recent book, *The Symbol at Your Door: Number and Geometry in Religious Architecture of the Greek and Latin Middle Ages* [2007],[1] briskly examines maps, *rotae*, and plans within the framework specified by its title. This is the weakest essay in the book, leaping in its 12 well-illustrated pages from the *Timaeus* to the Ebstorf *mappamundi* and other related maps to Vitruvian man and cruciform church design. The complex spatial theories discussed here receive no consideration as possibly influential on travel, and that is the question that one would really like to have seen taken up. Did anyone, for instance, leave any evidence whatsoever of having moved through a landscape or a church mindful of a Christian Platonist sense of space? How far did such theoretical concerns actually impinge on individual or collective experience?

[1] Reviewed in *Aestimatio* by Indra Kagis McEwen [2008].

The ‘parallel ideas’ linking various forms in relation to each other as macro- and microcosmic analogues, says Hiscock, ‘*must have* provided people with an appreciation of being an integral part of the universe ... that can only be envied today’ [126, emphasis added]. Hiscock’s casual concluding ‘must have’ avoids precisely what should be the central question: the historically possible uses of specific ideas, texts, tools, and buildings.

Fortunately, historically possible practices are the subject of the next essay, Dan Terkla’s much more satisfying ‘Informal *Catechesis* and the Hereford *Mappa Mundi*’; and the frustrated reader is able to turn to a genuine historical and theoretical engagement with the medieval evidence *in situ*. Like Schechner and unlike Hiscock, Terkla does not simply assume use of any sort, and his essay undertakes to argue for his claim that the famous Hereford world map could have been used as a ‘teaching tool’ in a very specific spatial context (a familial mortuary complex within the Hereford cathedral). The third of a planned series of five essays on this encyclopedic map, ‘Informal *Catechesis*’ investigates

> what ... it mean<s> to say that a medieval viewer *read* a mural or word-and-image hybrid like the Hereford, Ebstorf, or London Psalter maps. [129, emphasis in the original]

Such an investigation entails thinking systematically not only about medieval maps and medieval theories of text-picture relations, but also about the physical evidence itself, right down to individual scratches on the map’s surface. Acknowledging that there is ‘nearly’ no ‘hard evidence’ that the map was explained to visitors by trained clergy, Terkla nevertheless persuasively puts together a plausible case for the ways in which the map might well have been mediated for pilgrims as a complex ‘semiotic enclave’ [129] capable of creating various ‘emotional, mnemonic, and intellective’ responses [141].

If scholars are uncertain how medieval audiences might actually have used Christianized world maps like the Hereford *mappamundi*, the same is no less true of more modern-looking cartographic documents. Nick Millea’s ‘The Gough Map: Britain’s Oldest Road Map, or a Statement of Empire?’ thus begins by acknowledging that ‘very little is known of <the map’s> creation, its purpose and its audience’ [143]. Essentially a report on the current state of scholarship on the oldest surviving road map of Britain (made *ca* 1360), this essay offers an inventory of the document's topographic and cartographic features so as to discuss the research opportunities that its recent digitization for the Bodleian Library have made available to scholars.[2] These include geo-rectification to analyze the nature and extent of the map's accuracy. Despite its eastern orientation, this map is a remarkably accurate representation of the British Isles south of Scotland; and its practice of naming, Millea suggests, makes it looks very much like a secular document made for English administrative, possibly even imperial, purposes. Indeed, it may even be a kind of palimpsest, the different hands suggesting that it was 'regularly updated' [154] by clerks with local knowledge.

From a single map with unknown uses and users, the collection turns to a single medieval literary figure with a professed interest in maps whose works include a travel guide to the Holy Land almost exactly contemporary with the Gough Map. While considering her 14th-century Italian author's 'geographical consciousness' [163] generally, Evelyn Edson's 'Petrarch's Journey between Two Maps' focuses especially on what can be learned from the text of his pilgrim's guide, speculating on the types of maps that he might have consulted in writing it. The guide itself, written for his friend Giovanni Mandelli, contains no maps and there is no evidence that Petrarch actually drew on any maps for his information. From the text itself, though, and making inferences from remarks in Petrarch's letters and other writings, Edson comes to the conclusion that his sense of geography shares something with both the Christianized *mappaemundi* and the portolan charts. This is hardly a surprising conclusion, and since the same thing can also be said about the unknown author of *The Book of John Mandeville* (*ca* 1360) and even about Columbus as well, this essay in effect demonstrates how widely shared certain basic geographical ideas were in the later Middle Ages. Yet even if this genial tour of Petrarch's interest in geography tells us little about the possible uses of medieval maps, it does show a distinctively medieval side to the work of a poet more often celebrated as a renaissance man responsible for transforming the ways in which the educated thought about ancient authors.

[2] http://www.bodley.ox.ac.uk/users/nnj/goughmap.htm.

Besides the Holy Land, to which Petrarch's guide was partly devoted, Rome and Compostella were the other major medieval sites of Christian pilgrimage; and travelers to them had to pass through many variously less important sites *en route*. One of these was Siena, whose location on the Via Francigena, the principal route from northern Europe to Rome, ensured that it saw considerable pilgrim traffic. Michelle Duran-McLure's 'Pilgrims and Portals in Late Medieval Siena' examines the ways in which civic officials after 1287 worked to link their city visually with its patron saint, the Virgin Mary, so as to make it seem a type of the New Jerusalem. Art and architecture, particularly the city gates, were consciously used to give the city 'a unified aesthetic' [74]; and festivals like the Feast of the Assumption were directed towards creating a unified religious and civic community. Thus, on Duran-McLure's reading of the evidence, spatial analogies of the sort discussed by Hiscock were in fact put into practice in the later medieval world, but in highly specific, locally shaped ways.

Annette Lermack's 'Spiritual Pilgrimage in the *Psalter of Bonne of Luxembourg*' likewise shows something similar at work in northern Europe, but this time in the domain of virtual pilgrimage. The program of texts and images in an early 14th-century devotional work [Metropolitan Museum of Art, New York, Cloisters Collection MS 69.86] made for the Duchess of Normandy can be interpreted, she argues, as 'designed to lead readers on a repeatable spiritual journey' that culminated in 'the contemplation of relics' [97]. Lermack focuses in particular on three devotional miniatures—an allegory of the Six Degrees of Charity and two visualizations of Christ's wounds—to show how they develop the metaphor of pilgrimage so as to move the soul affectively rather than the body physically. The wound imagery in particular can be read as appealing especially to women, whose experience of childbirth could be linked to Christ's suffering. Emotion, in other words, can be thought of as a virtual form of travel, and it may well be that virtual travel was at least as important for medieval Christians as actual movement through physical space.

More potentially conflictual modes of travel are the subject of Anne McClanan's 'The Strange Lands of Ambrogio Lorenzetti', which focuses on two frescoes painted by Lorenzetti in Siena: one known as 'Good Government in the Countryside' (1337–1340); the other, as the 'Martyrdom of the Franciscans' (1335–1345?). The latter shows friars before what may be a Mongol khan, while the former shows apparently Asian wayfarers in the Sienese countryside. McClanan uses these two paintings to discuss 'one particularly fecund way in which identity was constructed for the Sienese through their visual culture' [83]: the setting of the local in relation to the foreign. 'Good Government', McClanan argues, recalls the importance of slaves in 14th-century Tuscany and its landscape may even suggest Ilkhanid influence. Against this presence of the foreign in the local, the essay sets its antithesis, the local in the foreign, as found in the 'Martyrdom'. In both cases, McClanan suggests, we can see 'many of the same key markers of establishing identity', a fact which suggests that 'illustrating heterogeneity was a way of rendering the vitality of the commune' [95]. Thus, if Duran-McLure's essay suggests that communal identity might be enhanced through a program of unification, McClanan's complementary study speculates on the ways in which a program of differentiation might also serve the same end. Neither essay tells us much about travel as such, and each is linked to different modes of travel (pilgrimage *versus* missionary and mercantile travel); but taken together, the two studies, along with Lermack's on the *Psalter*, reveal how influential travel could be both imaginatively and ideologically.

The value of this collection, its editors assert, is that, in addition to demonstrating the physical and other constraints on medieval travel itself, it makes clear 'the importance of travel in catalyzing fruitful medieval developments in artistic, scientific, and technical fields' [13]. That claim seems a fair assessment. For every Margery Kempe, Marco Polo, or Bartolomeu Dias, there were countless others who would have journeyed no farther than their local shrine or fair; but even their local worlds could not have escaped the changes wrought by the influence of travel and travelers. Uneven though its case studies are, and as limited in certain respects as the whole collection is (the 14th century and the Mediterranean loom especially large), the book makes clear how fruitful interdisciplinary, international collaboration can be and could be in helping scholars understand the importance of travel and its manifold legacies in the premodern world.

BIBLIOGRAPHY

Bachrach, B. S. 2001. *Early Carolingian Warfare*. Philadelphia.

Hiscock, N. 2007. *The Symbol at Your Door: Number and Geometry in Religious Architecture of the Greek and Latin Middle Ages*. Aldershot, UK/Burlington, VT.

McEwen, I. K. 2008. rev. Hiscock 2007. *Aestimatio* 5:76–80.

Encyclopedia of Ancient Natural Scientists: The Greek Tradition and Its Many Heirs edited by Paul T. Keyser and Georgia Irby-Massie

London/New York: Routledge, 2008. Pp. x + 1062. ISBN 978–0–415–34020–5. Cloth $360.00

Reviewed by
William Wians
Merrimack College
william.wians@merrimack.edu

What are the boundaries of ancient science? What subjects were pursued as part of the study of nature? Who should be counted among ancient scientists? How did they conceive of themselves and their activities? Where were they from, especially those who inherited traditions long after ancient Greece lost its independence?

Historians of ancient science have increasingly recognized the importance of such questions, even as they have learned how difficult they are to answer. Research in recent decades has paid extensive attention to areas once excluded from studies of science—everything from applied technologies to magic, alchemy, and astrology—even as fuller and more honest accounts of central fields have acknowledged that individuals long celebrated as heroes of rational inquiry regularly delved into formerly suspect areas and often failed to live up to their carefully crafted self-presentations as fully rational inquirers.

In the face of the vastly increased complexity of the study of ancient science, Paul T. Keyser and Georgia Irby-Massie have done a superlative job in putting together the *Encyclopedia of Ancient Natural Scientists: The Greek Tradition and Its Many Heirs* (hereafter *EANS*). Shepherding a team of over 100 scholars of ancient science and writing half of the entries themselves, the editors have produced a resource of remarkable breadth and value, reflecting the best current thinking in the history of ancient Greek science in all its inclusive diversity. In a single volume of just over 2000 entries filling 1000 pages, they have provided a comprehensive guide to a range of materials far beyond what previous editors have attempted or would have thought necessary—a point over which they show justifiable pride [5]. There are, of course, concise and informative entries covering every

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 207–211

major figure in ancient science. More remarkably, they have included hundreds of names that appear in no other reference work—names mentioned sometimes only once in sources combed and sifted from more than a millennium of surviving literary remains. Joined to this exhaustive list of named figures, the editors also include 200 pages devoted to place names, timelines, topics, a glossary, and indices (including women scientists, rulers, and the ancient names of plants). *EANS* will certainly become the standard starting point and often the only readily accessible source for research in ancient science.

Despite its remarkable breadth, *EANS* is highly focused. This is indicated by the volume's subtitle. The editors concentrate on Greek and Greek-based natural science. By Greek, they mean works written in Greek (even if known only by reference in later writers) or works clearly indebted to Greek writers produced up until *ca* AD 650. These inheritors are found mainly in Latin sources; though again as a sign of their inclusiveness and completeness, reference is made to works in Armenian, Celtic, Gothic, Egyptian, Persian, Sanskrit, and a host of Semitic languages. By natural science, they mean abstracted descriptions of nature that attempt to explain it rationally, without recourse to divine personages or an uncritical reliance on tradition. One might worry that such a definition begs many questions. But the editors recognize the arbitrariness of disciplinary boundaries and have tried to be inclusive of figures and works on the margins. They have excluded areas of philosophy not bearing directly on a science of nature, most theology (including divine cosmogonies), and mere records of technological wonders. But one finds references not just to physics, cosmology, biology, and mathematics, but also to geography, pharmacy, the study of stones, astrology, alchemy, cosmetology, and many other formerly non-standard disciplines and activities.

Given the breadth of the coverage and the clarity of their goals, I offer the following observations not in criticism of the editors' policies but as an indication of the precision of their focus.

The oldest named figures are Homer (as the starting point of geography) and Hesiod (primarily for his moralistic tone and agricultural calendar, not for his cosmogony and possible Babylonian influences). Plato and Aristotle are covered in entries clocking in at the 2000-word maximum length, both of which focus on their scientific ideas. There is a brief entry on Socrates the younger, but there is no entry for ‘the’ Socrates. The historians Herodotus and Thucydides are included, the former for the sake of a fuller understanding of *historia*, the latter for his account of the Athenian plague of 430, and both for their contributions to geography. Xenophon of Athens also merits an entry, apparently because of his writings on applied military arts. Twelve pages are devoted to various Greek papyri dealing with mathematical problems, alchemical recipes, and medical issues. But there is no entry for the ancient Egyptian Edwin Smith medical papyrus or on Egyptian medicine generally. There is, however, an entry on the infamous Egyptian Queen Cleopatra that focuses on a work *On Cosmetics* attributed to her. Babylonian astronomy is discussed; but the emphasis of the very interesting article is on the assimilation of its later, exact phases into Greco-Roman astronomy of the Hellenistic period.

While the Greek tradition serves as a strong organizing principle, it functions less clearly as a theme of individual entries. The editors have developed a system for cross-references which is easily learned and can be useful. But articles often do not place their subjects in the larger tradition, so that the relative importance and influence of various figures is hard to assess. This may be an unfair criticism. *EANS* is, after all, an encyclopedia, not a history. And while the sense of promise of an overview of a long tradition conveyed by the editors’ introduction seems unfulfilled, *EANS* provides countless discoveries and delights for the curious browser. This should not be surprising given the volume’s unprecedented coverage.

There are two more serious criticisms. The first is the general failure of contributors to distinguish between works that are extant and those that are not. The editors remark in the introduction that more works of Greek science survive than any other genre. But they also note how arbitrary was the survival of particular works and how much has been lost. It is regrettable that so many entries provide little if any indication as to whether the works mentioned in connection with an author are extant and, if not (which I sense is often the case), what is the provenance of our knowledge of the work in question. Second, entries can be uneven in emphasis and in level of detail. By this I mean that some entries may devote up to half their length to biographical information, while others of equal length overall say little or nothing of a figure’s life (including whether anything is known at all). This corresponds to varying levels of detail in the entries, with some entries attempting to give fairly detailed summaries of important ideas and arguments, while others offer only very general summary statements of content. In general, I would prefer being given fuller descriptions of a figure's contribution to science where it is possible to do so.

Each entry concludes with a very brief list of sources, including critical editions where available, with which the reader may begin further research. These are necessarily highly selective regarding major figures and so quibbling about what is included or left out is beside the point. I would say that the sources listed for Hesiod seemed especially well judged, making me wish that they had served as a model for other entries. In the citations, frequent use is made of abbreviations of the sort classicists like. While these are no doubt important space-savers in a book that is already long and expensive, I would have preferred to see the keys to this scholarly shorthand listed in the contents under a separate heading, rather than being contained without separate notice in the concluding 14 pages of the introduction [13–26].

Following the entries devoted to scientists are 200 pages of supplementary material. The gazetteer [855–909] lists all 290 sites and 35 regions mentioned previously in the text. Each entry contains a brief historical sketch highlighting important events such as a city's founding and conquest by non-Greeks (especially the Romans), followed by a list of scientists born there, plus further references. There follows a 25-page glossary of ambiguous terms used at least three times in the encyclopedia. Entries cover many ancient scientific and technical terms, but also the names of institutions (Academy, Garden) and scientific movements (Atomism, Methodists, Epicurean), plus a list of scientists associated with the terms. The entries are often very basic. Thus, a key term such as *phusis* can be given a much shorter treatment than many less important terms (*hudrophobia*, *ikhthuokolla* or 'fish-glue'). There is a separate index of plants at the end of the volume [1039–1062] listing the Greek or Latin popular name along with those scientists who mentioned them, and a separate listing of modern binomial names where identification is possible.

Fifty pages of timelines cover nearly all of the figures included in encyclopedia entries, divided into two columns. The left-hand column groups figures in 35-year spans (a notional generation) where more precise dating of a figure is possible, while the right-hand column uses a span of 105 years to list those names for whom greater precision is not possible. Moving between the two columns in search of a particular name takes some getting used to, but the columns allow the editors to avoid the ancient convention of a figure's acme.

Very interesting and useful is a topics index, which classifies every figure in the encyclopedia under modern categories such as agriculture, alchemy, biology, doxography, encyclopedia, *lithika*, pharmacy, and so on. The editors allow multiple listings for figures with wide-ranging activities. There is also an index that lists figures under headings such as female scientists, rulers, and non-scientists who are nevertheless frequently mentioned. Though one might wonder at why some of the headings were chosen, one can imagine that they would be useful starting points for various research projects.

Despite the enormous labor that *EANS* must have cost them, the editors speak of their hope of someday producing an improved edition. No doubt specialists in many sub-specialties will feel that this or that entry could be strengthened, just as I have noted points which I think could be improved. But this should not obscure the outstanding achievement that *EANS* represents. In its unrivaled scope and the quality evident on every page, Keyser and Irby-Massie have given us an essential reference work.

La sphère et l'intervalle. Le schème de l'harmonie dans la pensée des anciens Grecs d'Homère à Platon by Anne Gabrièle Wersinger

Collection Horos 12. Grenoble: Éditions Jérôme Millon, 2008. Pp. 381. ISBN 978–2–84137–230–0. Paper € 30.00

Reviewed by
Sylvain Perrot
Université Paris IV, Sorbonne
sylvain.perrot@paris-sorbonne.fr

In 2008, two books were published about ἁρμονία (harmony) in ancient Greece. We must admit that until then no book had dealt with the question of ἁρμονία so precisely as these books, which are consequently welcome in this research field of ancient Greek philosophy. *The Science of Harmonics in Classical Greece* by Andrew Barker, who is well known as a great specialist of ancient Greek music, examines the Greek science of music in classical times: its purpose is to understand how the ancient Greeks dealt with questions of musicology in the fifth and fourth centuries BC. Barker's interest is with musical theory. So we may underline two important differences with the book by A.-G. Wersinger.[1] First, she is a philosopher, not a musicologist; and the concept of ἁρμονία is, for her, not limited to music. Second, she aims at understanding how the notion of ἁρμονία was born in Greece and developed from its beginnings until classical times, though mainly in archaic times. In my view, both books are complementary and very important for modern scholars who study philosophy and musicology as well as mathematics, because all of these sciences were studied together in antiquity.

Wersinger's book presents two big difficulties that she herself points out. Most archaic Greek texts (except Homer's and Hesiod's epics) are fragmentary, and so her project entails reconstructing a whole way of thinking largely from ashes. But this is even more problematic than it seems: these fragments are mostly extracts chosen and quoted by later ancient authors, e.g., Plato or Aristotle. Consequently, we cannot always be sure that these texts were quoted

[1] The table of contents of this book can be found on the editor's website, http://www.millon. com/collections/histoire/horos/spheretm.html.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 212–221

accurately and fairly: ancient authors typically want to prove something particular and so may edit or even falsify their sources. But Wersinger is conscious of these problems and is very cautious, so that her method and her results are absolutely convincing.

Wersinger considers the evolution of the notion of ἁρμονία in Greek thought. She proceeds by studying a different philosopher or philosophy in each chapter, broadly moving forward in time. The book itself is divided into two parts answering to the title: the sphere and the interval. In this way, the author aims to prove that this distinction lets us see two ways of understanding ἁρμονία in ancient Greece. As she says: 'at the beginning, the scheme of ἁρμονία is the circle and the infinite signifies perfect circularity; in the end, the infinite has become the interval between more and less, whereas ἁρμονία is identified with limit and unity' [11]. But we have to be very careful with the notions of Presocratic philosophy, since, for many of these notions, there are no equivalent words in our modern languages.

As expected, the author begins with ἁρμονία in Homer's epics. Of course, there is not any theory of ἁρμονία in archaic poetry. But Wersinger succeeds in finding many clues in descriptions of 'archaeological' objects (wheel, shield) and ritual events (the crane's dance, also called the dance of the labyrinth). We might think that ἁρμονία is to be seen in a perfect circle, but it was not actually so in those times: ἁρμονία was viewed more narrowly as the junction between both ends of the circle. There is a bond, a connection, but it is invisible. Aristotle says that the circle is infinite because there is not any end. For archaic writers, the circle is infinite in that there is no join for the eye to see. Consequently, the circle, formed by bending a straight line so that its ends meet, is, like ἁρμονία, the result of two main processes: tension and articulation. These terms belong first to physiology: body is at once fibrous or stringy (μέλος) and articulated into limbs (γυῖα). In Greece, a μέλος is also music or melody. Each sound has a particular tension, an inflection or pitch; it is not yet considered as a part of a musical interval. For archaic poets, ἁρμονία results from an articulation of sounds, whereas Pythagoras' and Aristotle's schools describe it as a succession of intervals.

Empedocles, the first of the Presocratic philosophers, uses the same terminology as Homer but in a new framework. For Homer, the σῶμα is a corpse; whereas for Empedocles it is a body. Articulation and tension are no longer only properties of body but of a whole nature. For Empedocles, body is a degree of ἁρμονία because it forms a unity due to the mutual articulation of members and organs; Homer, however, thinks body is made of multiplicity. Wersinger notices opportunely that Empedocles' theory of ἁρμονία appears in his poetry, what she calls 'harmonization of *melea*' [67]: it corresponds to repetition of set expressions, which characterizes Empedocles' style. Repetition builds a circle, a unity between all the verses of a poem: it is composed of several important moments comparable to peaks. Repetition is like a path that connects all the summits. There is another relevant metaphor in Empedocles' poems, the κώδων [see Diels and Kranz 1951–1952, 31B99]. The κώδων is a Greek bell, a percussion instrument, and at the same time a trumpet bell; it is also for Empedocles the bell in the ear which transfers sounds inside the head. It is not only a resonator but also a musical instrument which plays what it has heard. So there is repetition. Unity is the purest form of ἁρμονία but it is not its principle: ἁρμονία is a kind of net made of juxtaposition.

Heraclitus introduces a new concept into the definition of ἁρμονία: community. The junction of both ends of a circle is thought of as a union with common elements, a kind of fastening. In Homer's epics, a ἀρμονία could be a pact or agreement: Heraclitus shows that ἀρμονία forms a community of interests in politics or a community of principles in ontology. Like his predecessors, he thinks that infinity is in fact invisibility but for him it is due to density: there is a hidden circularity in universe. Therefore, contraries are bound together like day and night in the circle of 24 hours. Heraclitus' reflection about ἁρμονία is first a reflection about astronomy, particularly the transition between day and night. Heraclitus' astronomy rests on four new propositions:

- the Sun's journey through the sky no longer fixes the limits of night and day;
- the Sun no longer goes under the Earth (during the night);
- there is not any exclusive difference between day and night (only a variation of hot and cold exhalations, whose ratio is to be understood in relation to distance from the Sun); and
- the Sun does not form a unity.

Night and day are like tenon and mortise, bound together in themselves. Consequently, Heraclitus builds a theory of the whole universe by organizing the four elements in a circle [see Diels and Kranz 1951–1952, 22B31]: fire is changed into water by condensation, water into earth and air by solidification, and then earth into water by dissolution, water into fire by rarefaction. In fact, it is a circle of fire, which appears to be the most important element in Heraclitus' system. Wersinger examines too a fragment about music, Diels and Kranz 1951–1952, 22B10, which refers to the heptachord, the seven-note system of the seven-stringed lyre in archaic times. This heptachord is joined in that the seven notes follow one another without any 'break'—in our modern notation, this would be

$$ABCDEFG \text{ or } CDEFGAB.$$

In this case, the octave is not heard and so is 'invisible'; but if you restore the missing note, you obtain a circle and thus ἁρμονία. Archaic lyres had seven strings: three of them were pegged to the right, four to the left. So there is a difference: concordance (the octave) comes from difference. In fact, the heptachord (the octachord with the invisible note) is made of two tetrachords. The central note (*mese*) is common to both: from this community you have musical ἁρμονία (by adding the invisible note). ἁρμονία is at once visible and invisible and that is Heraclitus' style: it is made of argumentative prose (where the theory of ἁρμονία is visible) and aphorisms (where it is invisible).

A new conception of the circle may be found in Parmenides' fragments. As before, the circle is formed by the junction of two ends; but it is also geometrically defined in relation to its center, the circumference being conceived as a limit. So Parmenides poses an ontological problem: being is something limited. Empedocles and Heraclitus had their own style: Parmenides for his part composes many circles and each of them has a center. Limit is associated with identity and indivisibility: limit contains and maintains each being. It does not mean that limit is between more and less because that would amount to saying that being is made of multiplicity, which is not possible for Parmenides. If being were a multiplicity, it would disappear. Only later is there limit between more and less, as far as being able to grow or shrink. From Homer to Parmenides, the idea of the circle has been retained to define ἁρμονία. But the nature of the junction of both ends has been interpreted differently: for Parmenides, this union is a kind of universal binder, a coalescence which permits integrity.

The second part of Wersinger's book is devoted to the notion of interval and how the archaic vision of harmony as circle was changed into that new notion. Wersinger thinks that the missing link is to be found in Anaximander's philosophy. There is only one relevant fragment [Diels and Kranz 1951–1952, 12B1], which Wersinger analyzes grammatically, morphologically, and philologically. For Anaximander, the ἄπειρον (infinite) is at the beginning of generation, but there is no circle because the philosopher does not speak of corruption at the end of being. He invents the notion of γόνιμον, that is, the separation of two opposite qualities from the ἄπειρον. The ἄπειρον is a kind of 'panspermic' marrow. It combines the forces of differentiation and combination, Chaos and Eros. In consequence, it is the model of ἁρμονία, like sap, with a circular and discriminatory structure—for the sap of a tree both creates a living periphery and causes death (wood) at the center, thus combining two contraries, life and death, to form a tree. This is not the model of κόσμος which is an arrangement of different astral wheels with their hubs on the same line: so center is very important. In Homer's epics, the center is the place of conflict, of hard battle; in Anaximander's philosophy, it is the place of measure, of symmetry and balance. The whole universe is organized in circles, and so by the number three, which is in fact the best approximation at that time of the number π. Wersinger notices that Anaximander's reflection is inspired by Doric architecture, particularly the circular drums of a column. So Anaximander theorizes two forms of ἁρμονία: ἄπειρον (where ἁρμονία combines opposites) and κόσμος (where ἁρμονία is symmetry).

The Pythagorean school introduces its conception of number into the problematic of ἁρμονία. But modern scholars have to understand properly what number represents in those times: Is it the thing itself or just an instrument of knowledge? Wersinger answers that Pythagorean philosophers do not revere numbers but think that numbers are in a relationship with παθός (affection of being). So they said that the whole sky is ἁρμονία and number: there is the rhythm of the stars' movement and the ἁρμονία of astral sounds. There are two ways of interpreting numbers: the 'arithmo-geometric' one and the 'logistic' one. The first corresponds to the theory of ψῆφοι in which the little stones by which the ancient Greeks voted are used to figure numbers. The number 5, for example, is figured by five ψῆφοι which are arranged in two parallel lines each of two stones with the remaining stone in between the lines, thus signifying that this number is odd. So you have a constellation of identical arithmetic units. The second model, the 'logistic one', corresponds to the theory of λόγοι (ratios). A unit is composed of limit and ἄπειρον: the best example is that of the λογοὶ ἐπίμόριοι, superparticular ratios defined by the form (n + 1):n (where *n* is a whole number). One part can be measured and another one cannot. The first model seems to ignore ἄπειρον. That is why Zeno's argument about Achilles and the tortoise is against this arithmo-geometric interpretation: for Zeno, being is continuous and arithmo-geometric numbers cannot reveal that continuity. The question is what is between two units? Between two opposites? Many Pythagoreans, therefore, invented a table where there are two columns of absolute opposites, the συστοιχίαι. But, for other Pythagoreans, there is between beings a διάστημα, an interval, which lets one distinguish various fields in the ἄπειρον. As far as numbers are concerned, there is the 'geometric progression', but Archytas theorized two others: the arithmetic and the harmonic. Intervals may vary according to one's point of view. Intervals can limit or be infinite, as observed in music. The problem is to divide the tetrachord: the tetrachord is delimited by the interval of a fourth, which in modern terms is viewed as two tones and a half; for the Pythagoreans, the fourth is composed of two tones (9:8) and a λεῖμμα, literally, the rest of the interval (which is only approximately a half tone).[2] So an interval can be limiting (the tone, 9:8) or indefinite (the λεῖμμα).

Infinite and limit are constitutive of the circle for Pythagoreans: circumference is infinite whereas radius is limiting. Consequently, a sphere, like a circle, is created by an interval, in the relationship between the center and periphery. Many of them consider that the sky dome results from 'pulling' the external curve to the center by the means of the radius. However, the philosopher Philolaus held that the universe (κόσμος) is like a sphere: it results from harmonization of the Indefinite (τὸ ἄπειρον). The ἄπειρον is divided to produce the center of a spherical space: the center is made of fire and the

[2] The ratio of this interval, 256:243, is superpartient.

periphery of air, the envelop of sky. To harmonize is to divide the ἄπειρον (which is made of more and less) into intervals that demarcate degrees. So Wersinger thinks that Philolaus is a Pythagorean in so far as he holds that ἁρμονία combines opposites, but that he departs from that school in understanding that intervals do not definitively limit ἄπειρον: this is particularly clear in his conception of music. There is an interval between high-pitched tones and the low register. But inside this interval, there are other intervals and so on: interval is at once infinite and limited. As we have seen, Pythagoreans conceived music as mathematical ratios and a particular ratio is associated with each interval. All intervals are not fixed: the *diesis*, for example, is only approximately a half-tone. Therefore, limit and ἄπειρον are always in a relationship with one another. This relationship permits ἁρμονία: Philolaus is said to have invented the disjunct heptachord, also called 'Pythagoras' octachord'. In the disjunct heptachord, the highest-pitched note (the *nete*) is one tone higher than in the conjunct heptachord; so Philolaus creates a bigger interval from the *mese* to produce the octave with only seven notes (heptachord). But it is not yet the standard octachord because one note is 'mute' due to the organization of octave. For Philolaus, the octave was

$$EFGABCD.$$

Since he wanted to have an octave with only seven notes, he created the sequence

$$EFGABD$$

so that the C is mute and there is a tone and half between B and D.

Philolaus theorizes superparticular ratios from the octave, which is typically for him musical ἁρμονία. The octave is made of a fourth and a fifth. All these intervals have for Pythagoreans superparticular ratios: the octave is 2:1; the fourth, 4:3; and the fifth, 3:2. The tone articulates the octave, as far as it is the link between two fourths, and so the difference between the fifth and the fourth, a difference obtained by division (3:2/4:3 :: 9:8). The octave is like a circle whose center is the *mese*, the central note which creates limit; both extremities of the octave are also limits. The infinite is the interval which envelops the transition from conjunct to disjunct heptachord: one interval persists in another during transition. In this case, there is a 'redistribution' of notes inside the second tetrachord so as to maintain the same number of notes in a bigger interval. Therefore, the disjunct heptachord, Philolaus' ἁρμονία, is made of limits and ἄπειρον (tonic intervals and *dieseis*). For Wersinger, Philolaus' ἄπειρον is 'active diversification' [301]: there are unlimited possibilities to place *dieseis* inside the tetrachord.

Archytas has yet another point of view: he wants to measure all the differences and thinks that whole universe is made of proportions, like the great sculptor Polyclitus in his *Canon*: all the measures of the human body are proportional to the smallest phalanx in the little finger. But Archytas fails to find a geometrical average in the octave: he can only find an approximation, because it is in fact $\sqrt{2}$. It is typically the problem of ἄπειρον. For Archytas, the ἄπειρον is not measurable: there is no symmetry or visible proportionality. Since he does not want to see ἄπειρον in melodic ἁρμονία, he has a hard problem to solve. However, Philolaus admits the ἄπειρον in ἁρμονία; it is even one of its principles. Thus, ἁρμονία is the interval between the ἄπειρον and limit and at the same time it is the result of this bond, viz. a κόσμος.

Anaxagoras, Pericles' famous teacher, is the last philosopher whom Wersinger examines. He represents the last step before Plato and Aristotle in the question of ἁρμονία. For him, the universe (κόσμος) is just a blend of every quality. Infinite and limits are not separated. But how is it possible to conceive identity when everything is mixed? The answer is the theory of homeomery: following Barnes [1982, 20], we can say that a property P is homeomerous if it is the case that when x has P, every part of x has P. Anaxagoras thinks that the infinite is indeed an infinity of parts. It is not extensively infinite, but the number of parts is infinite; furthermore, opposite qualities are extended into one another. So, the ἄπειρον is relative, a circularity that is always at the same time more and less big. The whole universe is always between more and less: one could say 'everything is in everything'. Wersinger opportunely compares Anaxagoras' philosophy with theater scenery in the fifth century BC. (what a spectator sees depends on the place where he sits) and acoustics (you can speak with a high-pitched voice to sound like people who scream from a distance). According to Anaxagoras, we only see differences: the more you look at microscopic level, the more things seem similar.

Therefore, the infinite is a swirl of all differences. Anaxagoras introduces nothingness into being. Parmenides thinks that there is no infinite because being is limited. For Anaxagoras, there is not any limit or else there would be nothingness.

To conclude, Wersinger describes the evolution of the notion of ἁρμονία as 'leaving multiplicity' [335], which is not chronological, because most of the Presocratic philosophers lived at the same time. But each one belongs to Greek culture which begins with Homer and variously interprets this heritage. For the famous blind poet, the infinite corresponds to the invisibility of the bounds that bind the circle. Empedocles invents the notion of unity. Heraclitus thinks that invisibility is not enough to explain the ἄπειρον: for him, it is the expression of a unity which contains a certain multiplicity. Unity is the ἁρμονία of multiplicity. Parmenides and the others try to leave multiplicity: the continuous is identified with the indivisible. For Parmenides, limit is the key; for Zeno, the ἄπειρον is made of more and less and multiplicity leads to nothingness, chaos. When one conceives the ἄπειρον as made of more and less, the notion of interval is used. So it is easy to imagine that there are intermediate positions between both extremities of the interval. And so ἁρμονία is not represented as circle any more, but as an interval.

In sum, I would say that Wersinger's work consists in trying to isolate Presocratic philosophy from all the Pythagorean, Platonic, or Aristotelian elements. These schools have studied the Presocratic philosophers but have interpreted them in their own way. I personally think that Wersinger succeeds in understanding how the ancient Greeks elaborated this very difficult notion of ἁρμονία. Her method is meticulous, her knowledge of Greek philology and philosophy indisputable. For other sciences like musicology, she has consulted great specialists, which validates her results: the bibliography is complete and the historiography well digested. Of course, this book is sometimes difficult to understand because of the complexity of the subject, but the author tries to help her reader: each chapter concludes with a clear recapitulation of the most important points of the argument.

For all these reasons, I warmly recommend Wersinger's remarkable essay.[3]

BIBLIOGRAPHY

Barnes, J. 1982. *The Presocratic Philosophers.* Boston.

Diels, H. and Kranz, W. 1951–1952. *Die Fragmente der Vorsokratiker.* 6th edn. 3 vols. Zurich.

[3] While completing this review, I learned that Wersinger's book has been awarded the Prix François Millepierres by the Académie Française.

Tablettes mathématiques de la collection Hilprecht by Christine Proust with Manfred Krebernik and Joachim Oelsner

Texte und Materialen der Hilprecht Collection 8. Wiesbaden: Harrassowitz Verlag, 2008. Pp. x + 166, 44 plates, CD-ROM. ISBN 987–3–447–05705–9. Cloth € 74.00

Reviewed by
Duncan J. Melville
St. Lawrence University
dmelville@stlawu.edu

In the late 19th century, the University of Pennsylvania sponsored a series of excavations at the ancient city of Nippur on the banks of the Euphrates. Among the finds were some 50,000 cuneiform tablets or fragments, of which almost 1000 were mathematical. Under the laws of the time, the finds were divided equally between the state (the Ottoman Empire) and the excavators. Consequently, most of the tablets ended up in Istanbul or at the University of Pennsylvania. Hilprecht, the excavation leader, retained some tablets for his personal collection; and after his death, they passed to the University of Jena, where they remain.

Hilprecht [1906] himself published some of the mathematical texts in his excavation reports, one of the earliest attempts to understand Old Babylonian mathematics; the remainder have largely rested undisturbed in the museum collections until recently. Eleanor Robson [2001, 2002] has published some of the Philadelphia Nippur tablets from later excavations and is preparing a full text edition of the Hilprecht Nippur tablets with I. Marquez. Christine Proust [2007] published the tablets in Istanbul;[1] here she treats those at Jena. In the earlier volume, Proust used the occasion to provide an in-depth overview and update of the current understanding of Old Babylonian mathematics and the conclusions that she drew there were based on the complete corpus of Nippur tablets. In this volume, she is principally concerned with presenting a classic, comprehensive, text edition of the collection; and so she limits herself to providing fairly

[1] See my review, Melville 2008.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 222–226

brief introductory remarks orienting the reader along with a useful set of appendices containing composite texts of the metrological and numerical lists and tables, glossaries, and word indices.

The core of the volume is a meticulous edition of the 81 tablets that have not already appeared as joins to tablets in other collections. Each tablet is presented with a full-size copy (black and white line drawing) as well as several color photographs on the accompanying CD[2] All except the numerical tables are given complete transliterations (this is unnecessary for the numerical tables, given the hand copies), a physical description, and commentary on noteworthy points. Several of the tables mix mathematical or metrological content with literary and lexical exercises; the non-mathematical content is treated in a separate chapter.

The first two of the tablets are from earlier periods than Old Babylonian (*ca* 2000–1600 BC). Both of these tablets have been published before; but Proust republishes them in full, summarizes previous commentary, and adds her own observations. The first is a Sargonic tablet containing an exercise in calculating the width of a rectangular field, given the length and area. This exercise belongs in a collection of similar problems that have sparked a heated debate concerning mathematical procedures in the Sargonic period. The issue revolves around whether the problems were conceived as arithmetical procedures and so provide indirect evidence for the use of sexagesimal notation or were considered from a more geometrical, cut-and-paste point of view. Proust summarizes the arguments on both sides and judiciously declares the available evidence to be inconclusive.

The second earlier tablet is an Ur III (*ca* 2100–2000) table of inverses, recently published by Oelsner [2001]. This is one of a very small corpus of tablets that have conclusively demonstrated the introduction of the sexagesimal place-value system in the Ur III period. Proust has had access to some more unpublished exemplars and establishes a typology to differentiate these tablets from those in the succeeding Old Babylonian period. As a consequence of her analysis, she makes the intriguing suggestion that these early inverse tables belonged in the province of scholars but had not yet passed into use in general education.

[2] These photographs are also available at the Cuneiform Digital Library Initiative website at http://cdli.ucla.edu/.

The remaining 79 tablets are Old Babylonian, and include one that Hilprecht apparently bought that did not come from Nippur. As these formed part of Hilprecht's personal tablet collection, one can imagine that the selection was not arbitrary. There are quite a few extremely nice specimens here; and the contents of the tablets span the known range of metrological and mathematical lists and tables and include some calculations, but no problem texts. Proust organizes the tablets according to the standard sequence of the reconstructed Nippur curriculum. The derivation of this curriculum is detailed in her earlier book. The division falls into the categories of metrological lists; metrological tables; numerical tables; calculations; and small, unidentifiable fragments. As noted above, the lexical and literary material is treated in a separate chapter.

The bulk of a student's elementary education at this time consisted of learning Sumerian, then a dead language, mostly by writing out long lists of Sumerian words, phrases, and sentences. Within this context, the first exposure to quantitative information came with the metrological lists. There were four such lists: capacity (called 'grain', where the units are not derived from length units), weight (called 'silver'), area (called 'field'), and length (apparently unnamed). In each case, the list proceeded from the smallest unit on up to multiples of the largest unit. The notation for quantity was not abstract but depended on the type of unit being counted. The current volume contains 16 examples of such metrological lists, ranging from a large tablet that (originally) contained the complete set of all four lists to small fragments that have only a few entries.

The next phase of elementary education, which may not have been pursued by all students, began the bridge between metrology and computation. The metrological tables contain all the same entries as the metrological lists, but in each case the metrological quantity is also written down as a multiple of a base unit using the abstract sexagesimal system. The metrological tables add a fifth set to the four of the lists, with a collection of heights used for the computation of volumes. Old Babylonian units for volume had the same names as units for area, but were 1 *kuš* or cubit thick. Heights and depths thus had a different base unit from that of lengths, and so had a new table. The Hilprecht collection features 15 tablets with metrological tables with some very fine exemplars, such as HS 242 (a table of weights) and HS 243 (a table of heights) as well a number of other nice, clear examples.

The next two chapters of Proust's book cover what she refers to as 'numerical tables'. The sexagesimal place-value system facilitated computation, principally multiplication and division, of abstract numbers. The information was organized into 'inverse tables' giving inverse pairs whose product was (a power of) 60, a long series of multiplication tables giving multiples of some principal number, as well as a smaller set of squares and square roots. Students gained familiarity with the system by copying out the tables, practicing each one, and then reviewing by writing out long series of tables in abbreviated form on large tablets. The Hilprecht collection includes 37 tablets of these types. Due to the repetitive nature of multiplication tables and the fact that complete copies of each tablet are given in the plates, Proust merely summarizes the content in the her text, showing the organization of multiplication tables on large tablets while giving the smaller tablets in full. Among these tablets, the large HS 246 is unusual in that the multiplication tables are written out in the full style rather than the more usual abbreviated form. HS 208, containing multiplication tables for 12, 10, 9 along with 8,20 ($8\frac{1}{3}$) and 8, is a particularly fine specimen. Meanwhile, HS 209 shows the human side of mathematics instruction. It starts out with beautifully clear, nicely laid out entries on the obverse and ends with rushed, cramped entries in the last columns of the reverse. Perhaps the student was running out of time as well as space. The collection also includes a good selection of well-preserved smaller tablets containing just one multiplication table as well as two tables of squares and four of square-roots.

Exactly how calculations were performed in the Old Babylonian period is still something of a mystery. Sources and solutions tend to get preserved better than intermediate computations. However, at least parts of some computations were noted on palm-sized tablets, of which this collection contains four. These tablets are often frustratingly difficult to interpret. One of the four is a very nice example of a well laid-out multiplication, in fact a squaring, similar to a number of other exemplars. Another is just a fragment that Proust suggests possibly contains an inverse pair. The traces are unclear and there is an error, but it is an intriguing suggestion as the proposed pair is not in the standard table nor in the usual sequence of larger pairs.

The third tablet contains numerous numbers, some of which may belong together as an attempted square root extraction by factorization. The fourth is another example of the difficulties presented by this type of source. The tablet bears numerous traces of erasures and has obviously been re-used. Proust does not see a relationship between the numbers. If some of the 'ones' are interpreted as the heads of column dividers, as seems plausible from the photograph, then we are left with the sequence 10, 20, 30, 40, and 50 on the next line. The layout of the tablet, however, leaves the purpose opaque.

The section on the lexical and literary extracts is of lesser mathematical import. The entries provide some new and variant readings, although most of the texts come from standard compositions.

Christine Proust is to be commended for having produced a handsome volume revealing for the first time the Hilprecht collection of mathematical tablets from Nippur. Together with her earlier publication of the Istanbul tablets and the hoped-for publication of the Philadelphia tablets, this important collection of Old Babylonian sources can finally be studied as a whole, more than a century after excavation.

BIBLIOGRAPHY

Hilprecht, H. V. 1906. *Mathematical, Metrological and Chronological Tablets from the Library at Nippur* (BE 20,1). Philadelphia.

Melville, D. J. 2008. rev. Proust 2007. *Aestimatio* 5:23–33.

Oelsner, J. 2001. 'HS 201 – Eine Reziprokentabelle der Ur III-Zeit'. Pp. 53–59 in J. Høyrup and P. Damerow edd. *Changing Views on Ancient Near Eastern Mathematics.* Berlin.

Proust, C. 2007. *Tablettes mathématiques de Nippur.* Varia Anatolica 18. Istanbul.

Robson, E. 2001. 'The Tablet House: A Scribal School in Old Babylonian Nippur'. *Revue d'Assyriologie* 95:39–66.

——— 2002. 'More than Metrology: Mathematics Education in an Old Babylonian Scribal School'. Pp. 325–365 in J. M. Steele and A. Imhausen edd. *Under One Sky: Astronomy and Mathematics in the Ancient Near East.* London/Münster.

BOOKS RECEIVED

Aestimatio 6 (2009)

Peter Adamson, *Al-Kindī.* Oxford/New York: Oxford University Press, 2007.

_________ ed. *In the Age of al-Fārābī Arabic Philosophy in the Fourth/Tenth Century.* London/Turin: The Warburg Institute/Nino Aragno Editore, 2008.

Anna Akasoy, Charles Burnett, and Ronit Yoeli-Tlalim edd. *Astro-Medicine: Astrology and Medicine, East and West.* Florence: SISMEL-Edizioni del Galuzzo, 2008.

Linos G. Benakis, *Michael Psellos. Kommentar zur Physik des Aristoteles.* Athens: The Academy of Athens, Research Center for Greek and Latin Literature, 2008.

Marco Beretta, *The Alchemy of Glass: Counterfeit, Imitation, and Transmutation in Ancient Glassmaking.* Sagamore Beach, MA: Science History Publications/USA, 2009.

Charles Burnett, *Arabic into Latin in the Middle Ages: The Translators and Their Intellectual and Social Contexts.* Burlington, VT/Farnham, UK Ashgate, 2009.

José Chabás and Bernard R. Goldstein, *The Astronomical Tables of Giovanni Bianchini.* Leiden/London: Brill, 2009.

Kevin Corrigan, *Evagrius and Gregory: Mind, Soul and Body in the Fourth Century.* Farnham, UK/Burlington, VT: Ashgate, 2009.

Patrick Gautier Dalché, *La Géographie de Ptolémée en occident (IVe-XVIe siècle).* Turnhout, Belgium: Brepols, 2009.

Isabelle Demars and Giles Maloney edd. *Une leçon d'Hippocrate. La santé dans un environment nordique.* Québec, Canada: Les Éditions du Sphinx, 2008.

Leo Depuydt, *The Other Mathematics: Language and Logic in Egyptian and in General.* Piscataway, NJ: Gorgias Press, 2008.

Roger L. Emerson, *Essays on David Hume, Medical Men and the Scottish Enlightenment: 'Industry, Knowledge and Humanity'.* Farnham, UK/Burlington, VT: Ashgate, 2009.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 6 (2009) 227–230

Ruth Glasner, *Averroes' Physics: A Turning Point in Medieval Natural Philosophy.* Oxford/New York: Oxford University Press, 2009.

Michèle Goyens, Pieter De Leemans, and An Smets edd. *Science Translated: Latin and Vernacular Translations of Scientific Treatises in Medieval Europe.* Leuven: Leuven University Press, 2008.

Nigel Hiscock, *The Symbol at Your Door: Number and Geometry in Religious Architecture in the Greek and Latin Middle Ages.* Aldershot, UK/Burlington, VT: Ashgate, 2007.

Sarah Iles Johnston, *Ancient Greek Divination.* Malden, MA/Oxford: Wiley-Blackwell, 2008.

George Gheverghese Joseph, *A Passage to Infinity: Medieval Indian Mathematics from Kerala and Its Impact.* New Delhi, India: Sage Publications, 2009.

Helge Kragh and Kurt Møller Pederson, *The Moon that Wasn't: The Saga of Venus' Spurious Satellite.* Basel/Boston/Berlin: Birkhäuser, 2008.

Max Lejbowicz ed. *Une conquête des savoirs. Les traductions dans l'Europe latine (fin du XIe siècle — milieu du XIIIe siècle). Actes du colloque organisé à la Fondation Singer-Polignac le jeudi 27 novembre 2008.* Turnhout, Belgium: Brepols, 2009.

Brice Maucolin, *Untersuchungen zur hippokratischen Schrift „Über die alte Heilkunst“.* Berlin/New York: Walter de Gruyter, 2009.

Ian Mueller, *Simplicius: On Aristotle On the Heavens 3.1–7.* London: Duckworth, 2009.

Ian Mueller, *Simplicius: On Aristotle On the Heavens 3.7–4.6.* London: Duckworth, 2009.

Chandra Mukerji, *Impossible Engineering: Technology and Territoriality on the Canal du Midi.* Princeton/Oxford: Princeton University Press, 2009.

Paolo Palmieri, *Reenacting Galileo's Experiments: Rediscovering the Techniques of Seventeenth-Century Science with a Preface by William R. Shea.* Lewiston/Queenston/Lampeter: Edwin Mellen Press, 2008.

David Pingree, *Historic Scientific Instruments of the Adler Planetarium and Astronomy Museum: 2. Eastern Astrolabes.* Chicago: Adler Planetarium and Astronomy Museum, 2009.

Kim Plofker, *Mathematics in India.* Princeton/Oxford: Princeton University Press, 2009.

Anthony Preus, *Historical Dictionary of Ancient Greek Philosophy.* Lanham, MD/Toronto/Plymouth, UK: Scarecrow Press, 2007.

Robert D. Purrington, *The First Professional Scientist: Robert Hooke and the Royal Society of London.* Basel/Boston/Berlin: Birkhäuser, 2009.

Roshdi Rashed ed. *Apollonius de Perge, Coniques. Tome 2.2: Livre IV. Commentaire historique et mathématique, édition et traduction du texte arabe.* Berlin/New York: Walter de Gruyter, 2009.

Roshdi Rashed ed. *Apollonius de Perge, Coniques. Tome 4 Livres VI et VII. Commentaire historique et mathématique, édition et traduction du texte arabe.* Berlin/New York: Walter de Gruyter, 2009.

Eleanor Robson, *Mathematics in Ancient Iraq: A Social History.* Princeton/Oxford: Princeton University Press, 2008.

Duane W. Roller, *Eratosthenes' Geography: Fragments Collected and Translated with Commentary and Additional Material.* Princeton/Oxford: Princeton University Press, 2010.

Sabine Rommevaux, *Thomas Bradwardine, « Traité des rapports entre les rapidités dans les mouvements » suivi de Nicole Oresme, « Sur les rapports de rapports ». Introduction, traduction, et commentaire.* Paris: Les Belles Lettres, 2010.

Sreerama Rajeswara Sarma, Takanori Kusuba, Takao Hayashi, and Michio Yano, *Gaṇitakasārakaumudī: The Moonlight of the Essence of Mathematics by Ṭhakkura Pherū. Edited with Introduction, Translation, and Mathematical Commentary.* New Delhi: Manohar, 2009.

John Scarborough, *Pharmacy and Drug Lore in Antiquity: Greece, Rome, Byzantium.* Farnham, UK/Burlington, VT: Ashgate, 2010.

Alfred Stückelberger and Gerd Graßhoff edd. *Klaudios Ptolemaios. Handbuch der Geographie. Einleitung, Text und Übersetzung, Index.* Basel: Schwabe Verlag, 2006.

Miira Tuominen, *The Ancient Commentators on Plato and Aristotle.* Berkeley/Los Angeles: University of California Press, 2009.

Glen van Brummelen, *The Mathematics of the Heavens and the Earth: The Early History of Trigonometry.* Princeton/Oxford: Princeton University Press, 2009.

Ruth Webb, *Ekphrasis, Imagination and Persuasion in Ancient Rhetorical Theory and Practice.* Farnham, UK/Burlington, VT: Ashgate, 2009.

Anne Gabrièle Wersinger, *La sphère et l'intervalle. Le schème de l'harmonie dans la pensée des anciens Grecs d'Homère à Platon.* Grenoble: Éditions Jérôme Millon, 2008.

Bronwen L. Wickkiser, *Asklepios, Medicine, and the Politics of Healing in Fifth-Century Greece: Between Craft and Cult.* Baltimore: Johns Hopkins University Press, 2008.