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 9

2012

Edited by

Alan C. Bowen and Tracey E. Rihll

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

ISSN 1549–4470

ISBN 978–1–934846–03–2

This volume is printed on acid-free paper that meets ANSI standard Z39.48, Permanence of Paper for Publications and Documents in Libraries and Archives.

Printed in Toronto, Canada

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

To receive automatic notices of reviews as they are posted on the Institute website, readers are invited to go to the journal's webpage (http://ircps.org/aestimatio) and establish a user account or subscribe to an RSS feed.

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) listing the title and author of the book that they wish to review, with 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. Volumes 1–8 are available from Gorgias Press (go to http://www.gorgiaspress.com/bookshop/c-144-aestimatio-1549-4470.aspx). The present and all subsequent volumes will be available through a print-on-demand service (go to http://ircps.org/aestimatio). All volumes are also 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

Epicureanism by Tim O'Keefe

Berkeley/Los Angeles: University of California Press, 2010. Pp. xvii + 206. ISBN 978–0–520–26471–7. Paper $24.95

Reviewed by
Monte Ransome Johnson
University of California, San Diego
monte@ucsd.edu

I have been looking for a short introduction to Epicureanism to recommend to students in my upper-division Hellenistic Philosophy course at UC San Diego. The students are required to read, in addition to my own translation (with D. S. Hutchinson) of Epicurus' *Letter to Menoeceus*, the entirety of Cicero's *On Moral Ends* and several dialogues and essays of Seneca. But Cicero and Seneca are both hostile sources of information about Epicureanism; and the *Letter to Menoeceus*, though a brilliantly concise summary, is an extremely brief and compact introduction to Epicureanism, which, of course, contains no indication of the subsequent importance of Epicureanism on the history of philosophy and science. And so one looks for a serviceable starting point for further consideration of the Epicurean position and, hopefully, deeper research into the arguments.

In the case of Stoicism, I have found a book in the same series (Ancient Philosophies) to be very useful for these purposes: John Sellars' *Stoicism* [2006]. Sellars' book is cheap, in print, and contains all of the following very useful tools: a list of abbreviations; a chronology; short accounts of all the leading Stoic figures (Zeno through Hierocles) and of the most important sources for reconstructing their philosophy (Cicero through Simplicius); an overview of the 'decline and loss of texts'; and chapters on the Stoic system, logic, physics, ethics, and also on the Stoic legacy (covering late antiquity through Deleuze, including many important points of detail in the early modern period). It also has a glossary of names and a separate glossary of terms (which includes transliterations of the Greek). It then has a 20-page guide to further reading that is broken down into primary and secondary sources, individual Stoics, and themes such

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 1–8

as ‘epistemology’, ‘physics and cosmology’, ‘fate and determinism’, and so on. This is in addition to the bibliographic list of references. It also contains a general index and an index of passages. This is useful because the work contains several extended inset quotations freshly translated by the author from a variety of ancient sources. It is a great starting point for further understanding of Stoicism and research into it. As A. A. Long states in a blurb, ‘Stoicism needs a new work of this kind.’ I can assign students to read a part of or the whole thing and then to begin further research with a checklist of ancient sources (about whom they can easily learn more) and modern literature on the theme in which they are interested.

Epicureanism, arguably, is in even greater need of a work like this, at least in English—the situation is much better in French and Italian. One recommends, of course, the parts on Epicureanism in Long’s *Hellenistic Philosophy* [1974] and Sharples’ *Stoics, Epicureans, and Skeptics* [1996]. Oddly, these modern classics are never mentioned in O’Keefe’s book (even in the section ‘Further Reading’). But as for a dedicated monograph providing a thematic overview and starting point for further research, we are still largely dependent on Rist’s *Epicurus: An Introduction* [1972].[1] So it is this need for an up-to-date, compact introduction to Epicureanism that Tim O’Keefe nobly intends to fulfill, as he states in a section entitled ‘How to Use this Book’: ‘this book is intended as a standalone introduction’ [viii]. Although I do not think that it succeeds at this task, I must say at the outset that I have found it useful in some other ways and I have found myself recommending it to some kinds of students wanting to learn more about Epicureanism.

After stating that he intends the work to serve as a standalone introduction, O’Keefe says: ‘I do not include extended quotations from ancient sources; instead, I usually summarize matters in my own words’ [viii]. For my purposes, this policy renders it unserviceable as a *standalone* introduction, something O’Keefe almost immediately acknowledges when he points out that the student will need an additional compendium of translations of Epicurean philosophers. O’Keefe recommends either the second edition of Inwood and Gerson’s *Hellenistic Philosophy: Introductory Readings* [1997] or the first volume of Long and Sedley’s *The Hellenistic Philosophers* [1987].

[1] Also never mentioned in O’Keefe’s book.

Strangely, he does not (until the notes) mention Inwood and Gerson's *Epicurus Reader* [1994], although this is not only cheaper but also more comprehensive of integral Epicurean texts. The usefulness of that book may be inferred from its inclusion in both the 'Notes' and the bibliography.

O'Keefe's introduction contains a six-page biography of Epicurus and then three pages on sources of Epicureanism, including less than two pages on 'later Epicureans' (namely, Lucretius, Philodemus, Diogenes of Oenoanda, and Colotes) and then about a page on non-Epicurean sources. Many details are missing. For example, there is no biographical information provided about Hermarchus (who succeeded Epicurus as head of the school) or Metrodorus of Lampsacus, although Metrodorus' views about sex and convention are later discussed on page 146; similarly for Polyaenus of Lampsacus (whose rejection of geometry is mentioned on page 24). There is no mention whatsoever of Idomeneus of Lampsacus or of any other of the disciples and adherents of ancient Epicureanism.

The main part of the book is divided into three parts:

(1) Metaphysics and Physics (which I would rather, following the sources, have referred to as 'Physics'),
(2) Epistemology (which I would rather, following Epicurus, refer to as 'Canonic'), and
(3) Ethics.

The sections are uneven: 72 pages on physics, 66 pages on ethics but just 21 pages on epistemology. The book also includes a glossary of terms but it consists of only 17 words, a confusing mixture of transliterated Greek terms,[2] a Latin term,[3] English terms,[4] and English phrases[5] The foreign term is not always provided; and when it is, it is sometimes as the headword, sometimes as a parenthetical expression. By comparison, Sellars' glossary contains three times as many words and consistently gives the Greek terms for all of them.

[2] '*aponia*', '*apraxia*', '*ataraxia*', '*eidola*'.
[3] '*minima*'.
[4] 'atom', 'canon', 'cosmos', 'physics', 'preconception', 'swerve', 'virtue', 'void'.
[5] E.g., 'cradle argument', 'katastematic pleasures', 'kinetic pleasures', 'teleological explanation'.

The 10 pages of 'Notes' in O'Keefe's book contain, for the most part, recommendations for further reading, which is awkward because they precede a five page chapter by chapter list of 'Further Reading' that is divided into ancient and contemporary sources and followed by a five page bibliography (presumably a list of references). One, therefore, has to look in three different places to follow up some point discussed in the book. But even then one is sometimes disappointed.

Consider, for example, a student looking for biographical information about Epicurus, the school of Epicureanism, and the sources for Epicurean philosophy. In the 'Notes', he or she is referred to the recent *Cambridge Companion to Epicureanism* edited by James Warren but not to his admirably concise monograph *Epicurus and Democritean Ethics* [2002],[6] a work that firmly situates Epicureanism in the tradition of Democritus and provides much background information about the *milieu* of Epicurus' education and predecessors. Several ways to follow up on the later Epicurean Philodemus are mentioned but nothing else. For non-Epicurean ancient sources, one is told next to nothing but advised to consult the index of sources in Long and Sedley 1987, vol. 1. In the section 'Further Reading', the student is referred to Diogenes Laertius, *Vitae* 10.1–16 (but not given any bibliographic information about how to find that work or a translation of it) and to Lucretius, *De rerum nat.* 1.1–135 (which contains no biographical information about Epicurus or any Epicureans, and nothing about any sources). For secondary sources, the student is referred to Diskin Clay's *Paradosis and Survival* [1988] and an important technical article by David Sedley. One could pick out relevant things from the bibliography, such as Bailey's *Greek Atomists and Epicurus* [1928] or Festugière's *Epicurus and his Gods* [1955], but one is not pointed to these in the 'Further Reading'.

Similar problems could be pointed out for the other sections. For example, there is no reference to the most important monograph on Epicurean psychology, David Konstan's *A Life Worthy of the Gods: The Materialist Psychology of Epicurus* [2008]. This is in fact the book on Epicureanism that I most often recommend to advanced undergraduates and graduate students, and it is wonderful that it has been updated and reprinted. But there is no notice of this work

[6] This is not even included in the bibliography.

in O'Keefe. In fact, the only reference to Konstan's extensive work on Epicureanism is to his translation of Philodemus (in the bibliography under the misspelling 'Kontan').

O'Keefe's book also contains no direction for investigating the importance and influence of Epicureanism on later philosophy and science. He briefly mentions the decline of Epicureanism in the Christian era; but in the same paragraph he refers to Gassendi, Newton, and Boyle, and their reviving 'versions of atomism directly based on Epicureanism' [5], yet fails to mention the circumstance that Gassendi was a member of the Catholic clergy and that Boyle and Newton were adherents of Christianity and proponents of natural theology, a philosophy completely at odds with Epicureanism. Similarly, O'Keefe in a different context compares an argument for the swerve to a '*kalam*-type cosmological argument for God's existence', with no further reflection on *kalam* atomism and how it adopted a version of atomism (perhaps directly indebted to Epicureanism) while at the same time embracing theological ideas diametrically opposed to Epicureanism. No mention is made of other philosophers who have made extensive use of Epicurean ideas and are interesting to students, such as Hobbes, Nietzsche, or Marx (who wrote his doctoral dissertation on the superiority of Epicurus' philosophy to that of Democritus). No mention is made of the topics covered by H. Jones' useful and interesting book *The Epicurean Tradition* [1989].

Almost all of my criticisms have been about things O'Keefe does not include that would make his book more viable as a standalone introduction and more useful to undergraduate and graduate students (things which Sellars' *Stoicism* did manage to accomplish). Despite this, there are certain strengths of O'Keefe's book and reasons why one might recommend it to certain kinds of students for certain purposes. Further research is not one of them. But O'Keefe's book is useful as an overview of Epicurean dogma and he makes a vigorous defense of the philosophy as a whole, including some of its least satisfactory parts. Although it contains very few examples of close readings of extended passages (since, as stated above, it does not translate or even quote any extended passages), it does provide an account of the various dialectical positions taken by Epicureans on a vast range of disputes, and a good number of examples which make many difficult positions much easier to follow. Occasionally, O'Keefe's prose is elegant and even seems to be inspired by a kind of Epicurean conviction. For example, in a discussion of the primary impulse he writes:

> A baby feels the pangs of hunger and cries out. She is picked up and sees the bottle nearby. She eagerly latches on and sucks, feeling the gratification of the milk rolling over her tongue, sliding down her throat and quieting her pangs, until she is content. [113]

It would be even more natural if the bottle were replaced by the mother's breast, as in Lucretius comparison of the milk provided by mother earth [*De rerum nat.* 5.810–815]. But here and elsewhere, O'Keefe writes admirably well in support of his points, as with these examples:

> we can criticize my son's desire to play with matches by saying that, even though it is fun, it will lead to painful burns and possibly skin grafts...the pleasure of shooting up heroin is good, but not worth choosing, and the pain of getting an abscessed tooth drilled is bad but worth undergoing. [114]

I have found myself using these examples to illustrate the same points in the classroom, and O'Keefe's book appears to contain some nutritious fruit cultivated from his own teaching experiences. A small issue, however, on a related point. The cover of the book contains a detail from a slab of the Tomb of the Diver (produced 480–470 BC) which depicts two reclining and barely covered lovers drinking and playing *kottabos*. This is a very odd choice for a book about a philosopher who (generations later) took pains to stress that

> it is not drinking bouts and continuous partying, or enjoying boys and women, or enjoying fish or the other delicacies of a luxurious table, which produce the pleasant life; what produces this is sober calculation, which searches out the reasons for every choice and avoidance and drives out the opinions which are the source of the greatest turmoil for our souls. [Diogenes Laertius, *Vitae* 10.132]

This is a key text and I was surprised to not find it referenced in O'Keefe's book.

I have, nevertheless, recommended O'Keefe's book to two kinds of student. The first are students who need a more direct and easy exposition of Epicureanism than can easily be gotten out of Cicero.

O'Keefe shows how the Epicurean philosophy is divided and how its different parts relate to each other as well as to other dialectical options. The book is clear enough, comprehensive enough, and short enough, that such students can benefit easily and quickly from it. There is very little else—other than the primary sources such as the *Epicurus Reader* (a collection of translations of primary texts, mentioned above) or translations of Lucretius—that works as such an overview and nothing like a comprehensive monograph that is in print. That is an awkward fact if you think about it, since Epicureanism was designed to be a philosophy easy to access, understand, and propagate. Epicurus wrote his own summaries of the philosophy to serve the same kind of purpose that O'Keefe's book does; and had those survived, we should probably need only translations to serve the purpose of grasping the outlines of Epicurean philosophy. But in the absence of such texts, O'Keefe's work is a useful synthesis of the various logical, physical, and ethical commitments of the Epicureans.

The second kind of student to whom I have recommended the book are those hostile to Epicureanism, who reject it as a shallow kind of hedonism or as having ridiculous views about physics (e.g., the swerve) or epistemology (e.g., the Sun's being as large as it appears). O'Keefe does an admirable job of defending Epicureanism against glib objections and often against more serious and difficult objections. He is effective at summarizing many of the attractive aspects of Epicureanism and at responding to the major objections. I am thankful for that and will continue to recommend his book for such purposes.

BIBLIOGRAPHY

Clay, D. 1988. *Paradosis and Survival: Three Chapters in the History of Epicurean Philosophy.* Ann Arbor.

Davie, J. and Reinhardt, T. 2007. *Seneca: Dialogues and Essays.* Oxford.

Festugière, A. 1955. *Epicurus and his Gods.* Oxford.

Inwood, B. and Gerson, L. 1994. *The Epicurus Reader: Selected Writings and Testimonia, with an Introduction by D. S. Hutchinson.* Indianapolis.

Inwood, B. and Gerson, L. 1997. *Hellenistic Philosophy: Introductory Readings.* 2nd edn. Indianapolis.

Jones, H. 1989. *The Epicurean Tradition.* London.

Konstan, D. 2008. *A Life Worthy of the Gods: The Materialist Psychology of Epicurus.* Las Vegas. A revised, expanded edition of *Some Aspects of Epicurean Psychology* (1973).

Long, A. A. 1974. *Hellenistic Philosophy.* London.

Long, A. A. and Sedley, D. 1987. *The Hellenistic Philosophers: 1. Translations of the Principal Sources with Philosophical Commentary.* Cambridge.

Marx, K. 1902. *The Difference between the Democritean and Epicurean Philosophy of Nature.* Marx-Engels Collected Works 1. Moscow.

Rist, J. 1972. *Epicurus: An Introduction.* Cambridge.

Sellars, J. 2006. *Stoicism.* Berkeley/London.

Sharples, R. W. 1996. *Stoics, Epicureans, and Skeptics.* London.

Warren J. 2002. *Epicurus and Democritean Ethics: An Archeology of* Ataraxia. Cambridge.

__________ 2009. *The Cambridge Companion to Epicureanism.* Cambridge.

Woolf, R. and Annas, J. 2001. *Cicero,* On Moral Ends *Translated with Introduction and Notes.* Cambridge.

Writings of Early Scholars in the Ancient Near East, Egypt, Rome, and Greece: Translating Ancient Scientific Texts edited by Annette Imhausen and Tanja Pommerening

Beiträge zur Altertumskunde 286. Berlin/New York: De Gruyter, 2010. Pp. xiv + 436. ISBN 978–3–11–022992–9. Cloth $154.00

Reviewed by
Markus Asper
Humboldt University
markus.asper@hu-berlin.de

After a great deal of discussion in recent decades about both the practical and theoretical aspects of translation,[1] most contemporary critics would concur that translation is impossible in theory but necessary in practice. While the translation of highbrow literature is the field that has contributed most to the discussion of translation in general, another field has long gone unnoticed, namely, the transmission of explicit or even scientific knowledge. What about the great texts of Greek mathematics, let alone Egyptian medicine or Mesopotamian omen literature?

Translation as a practice and a problem, especially in the humanities that investigate the past, is a natural, and thus well chosen, topic for such a collection. This one brings together scholars from ancient Near Eastern studies, Egyptology, Classics, and History of Science. The volume well illustrates the risks that truly interdisciplinary work in the historically oriented humanities has to face, but it also clearly demonstrates the great benefits that can emerge from such collaborative work.

Throughout the volume there is some variation concerning what exactly it is that the contributors investigate. According to the subtitle the unifying question is,

(1) How should one translate 'ancient scientific texts'?

[1] See, e.g., Gerzymisch-Arbogast *et alii* 2006, Vandevelde 2005.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 9–18

Although the main focus of interest is on contemporary translations, some contributors also trace ancient attempts at translation. Nevertheless, there are at least two more overarching questions:

(2) What is ancient 'science'? or Are certain ancient discourses or texts 'scientific'?, and

(3) What do certain 'scientific texts' actually do or mean, or, occasionally, even say?

While it is quite clear how questions (1) and (3) connect, question (2) and its possible answers, although hotly debated not too long ago, do not primarily concern the problem of translation. Rather, they concern the range of texts discussed in this volume. In what follows, I will discuss the contributions in relation to which of these problems that they mostly deal with, leaving the central one (1) to the end and starting with the most specialized one (2). The editors have structured the collection differently, namely, in sections on:

(a) scientific language (Cancik-Kirschbaum, Althoff, Quack, and Fögen),

(b) ancient translations (Taub, and von Lieven),

(c) medicine (Pommerening, Heeßel, Worthington, Hoffmann, and Totelin),

(d) astronomy/astrology (Depuydt, Brack-Bernsen, Heilen), and

(e) mathematics (Imhausen, Ritter, and Høyrup).

As one would expect, many papers touch upon more than one of the three aspects. The essay that tries to address all three most consistently is probably Annette Imhausen's 'From the Cave into Reality: Mathematics and Cultures' [333–347], which critically assesses the state of mathematical historiography as strongly colored by what one could call with Bourbaki a 'work-day Platonism' (and Eurocentrism, too). She then discusses the cultural backgrounds of Babylonian and Egyptian mathematics, ending with an account of the Unguru debate,[2] which applies to ancient Near Eastern or Egyptian

[2] The debate was kicked off by Unguru 1975, which provoked many harsh responses by eminent historians of science, (e.g., B. L. van der Waerden) and even mathematicians (e.g., A. Weil). The venom of the debate, which is exactly the one about translation between source text and target reader, is explained more easily when one realizes that this debate is about the identity and continuity of European-style mathematics.

mathematical traditions even more than to ancient Greek ones. Today, all contributors and most readers of this volume would, I suppose, firmly settle on Unguru's side of the controversy, which is the historical-mathematical variant of the many debates about the two mutually exclusive ways of translation: either source- or reader-focused.

Problem 2: What is 'science' and/or *Wissenschaft*?

Some contributors actually define what they mean by 'Wissenschaft'. For their definitions, they identify two to three factors, namely, knowledge, a notion of the 'systematic' presentation of that knowledge, and social organization.[3] Thus defined, the notion of science or *Wissenschaft* becomes wide and integrative but perhaps too wide to be useful, especially too wide still to be 'science'. Rather, it reflects negotiations and struggles within modern knowledge systems that investigate ancient cultures, structurally similar to the discussion about what 'literature' is.[4] One should hope that these are battles of the past (and battles that were won), at least in strictly scholarly contexts.[5] On the other hand, throughout this collection there is some consensus on the fact that 'etic' or observer's categories, although unavoidable, are often less helpful than they seem to be at first sight. Thus, instead of discussing, e.g., whether divination texts or lexical lists are tokens of a scientific method, one should focus on describing what their language is telling us about the rational practices in which these texts were embedded. This kind of description is exactly what the papers of Cancik-Kirschbaum, Althoff, Quack, and Fögen attempt, by carefully trying to avoid purely 'etic' perspectives on ancient discourses.[6]

Eva Cancik-Kirschbaum's 'Gegenstand und Methode. Sprachliche Erkenntnistechniken in der keilschriftlichen Überlieferung Mesopotamiens' [13–34], in what is the most theory-conscious paper in the collection, investigates how language itself becomes a tool of inquiry.

---

[3] See p. 15 (Cancik-Kirschbaum, recurring in Høyrup) and p. 69 f. (Quack).

[4] See, e.g., the positions summed up in Schmitz 2007, 19–21.

[5] Fundamental and often quoted in this volume are Larsen 1987 and Jeyes 1991–1992.

[6] For the distinction of 'emic' and 'etic' categories of description in ethnology and cultural studies, often used in this volume, see Goodenough 1970 and Harris 1976.

After a rich introduction that situates her project in the history of ancient Near Eastern studies, she discusses various aspects and levels of the role of language in articulating cognition. Her main point seems, to me, to be that language is more than an instrument and thus that one cannot really separate cognition from language. A classic example of this is metaphor. The second part of the paper discusses four different ways in which language operates in gaining knowledge. These are binary statements, the creation of terminology in early seventh-century Assyria, the coordination of language and visualization in bilingual lists, and mythological explications of speculative knowledge. As several authors in this volume, Cancik-Kirschbaum settles on divination in order to demonstrate how language structures the scientific corpus of Babylonian divinations that employs the principle of image-based relations. She ends her paper with short remarks on thought experiments in the divinatory corpus that take the form of adopting impossible parameters. Cancik's paper highlights the importance of language for rational-practice texts (Ritter's term, see below) and thus the difficulties that result from translation.

Jochen Althoff [47–68: 'Das Verhältnis von medizinischer Prognose zur religiösen Divinatorik/Mantik in Griechenland'] takes translation less literally and investigates transpositions of divinatory patterns into Hippocratic medicine. His paper throws light upon the emergence of 'rational medicine', especially prognostics, from preexisting discourses such as divination. Thus, the ubiquitous polemic against divinatory practices that one finds in the Hippocratic Corpus becomes understandable as one more instance of 'boundary-work'.[7]

Joachim F. Quack [69–71: 'Präzision in der Prognose oder: Divination als Wissenschaft'] follows in the same vain, stressing the systematic character of Demotic divination texts—'systematicity' is one of his criteria for calling a discourse *wissenschaftlich*, besides its intellectual elitism [70]. Of course, one can always suspect that such a method begs the question; at least, one would have liked to see an abstract account of what 'systematic' actually means. Nonetheless, Quack grandly succeeds in describing Demotic divination as science.

More linguistics-oriented is Thorsten Fögen's paper on Pliny the Elder [93–115: Zur Rolle des Fachwortschatzes in der *Naturalis historia* des Älteren Plinius], which has much of value to say about

[7] I borrow the term from Gieryn 1995, 394–407 and Hess 1997, 58.

problems of translation. Pliny himself discusses a great number of bilingual phenomena. Fögen gives a systematic, descriptive account of Pliny's stances toward terminology. In the last lines of the paper [112], we glimpse a truly fascinating project, namely, the presentation of terminological discussion within a moral agenda, that is, as part of Pliny's authorial self-staging throughout his discussion of terminology.

Problem 3: Reconstruction of ancient argument in its context

Another slate of papers concerns a prerequisite of translation, namely, a clear grasp of what a text actually means, which in the case of ancient 'science' can be rather difficult. Most of the papers that belong to this group do not actually discuss the 'scientificity' or translatability of their textual objects, but their structure and argument. In two thorough papers, Leo Depuydt [241–276: 'Ancient Egyptian Star Tables: A Reinterpretation of Their Fundamental Structure'], and Lis Brack-Bernsen [227–297: 'Methods for Understanding and Reconstructing Babylonian Predicting Rules'] attempt to see ancient astronomical lore through 'emic' categories [see 11n6 above], that is, they free the modern interpreter of ancient astronomy from his modern knowledge and concepts of stars and simply try to make sense of the texts in terms of what their authors could actually have seen. While Depuydt is led by his research towards revoking certain functions of Egyptian star tables that were assigned by modern scholars ('just tables, not also clocks' [251]), Brack-Bernsen is concerned with procedural texts that determine the lengths of Babylonian months. Especially the latter are a great example of the difficulties that later scholars have to face when ancient texts do not even wish to communicate the second-order discourse that governs the data preserved by the text. Neither Depuydt nor Brack-Bernsen turn this into an argument against translation proper but it is difficult to see how one can translate these texts in the traditional sense and still produce a meaningful text.

Jim Ritter [349–383: 'Translating Rational-Practice Texts'] and Jens Høyrup [385–417: 'How to Transfer the Conceptual Structure of Old Babylonian Mathematics: Solutions and Inherent Problems'] work on our understanding of the structure of, mostly, Mesopotamian mathematical texts. Ritter, who also discusses Egyptian mathematics, Babylonian medicine, and Assyrian law, adduces parallels from computer science and information theory, e.g., the ‘abstract command list’ or ‘flow diagrams’, in order to understand the structure of these ancient algorithms. His apparent intention to free us from the problematic term ‘science’ with respect to ancient societies and to understand a whole range of ancient practices as ‘rational practice’ is well illustrated by the set of examples presented. Now, however, the term ‘rational’ should come into focus. (I expect problems lurking there that are similar to the ones historians of science had and are still having with ‘science’). Høyrup’s paper focuses more on actual translation, approaching the scene, however, from the perspective of conceptual structure. After giving a highly instructive sketch of how research on mathematical cuneiform texts developed and of the inherent methodological problems it had to face, Høyrup presents a list of terms and operations, arranged according to mathematical operations, adding his standard translations [399–405]. Even for readers who are not closely acquainted with Babylonian mathematics, these lists illuminate mathematical terminology and procedure and provide much insight. Both papers can also serve as general introductions to the field of ancient Near Eastern mathematical texts.

Problem 1: Translation proper

The remaining papers discuss more specific problems, or perhaps one should say questions, that are centered on the actual linguistic problems of translating ancient ‘scientific’ texts. Among these, Liba Taub’s ‘Translating the *Phainomena* across Genre, Language, and Culture’ [199–137] is the only one that focuses exclusively on ancient translations, by looking, in the style of a case–study, at Aratus’ *Phaenomena* and its Roman versions (Cicero, Germanicus, and so on). Since Aratus exerted such an impact on Roman didactic poetry, Taub’s paper can also serve as an overview of research to ancient didactic poems. Especially instructive are her remarks on different kinds of readers. While she does not dwell on ancient techniques of actual translation, her thesis that these texts occupy a poetic-scientific space that is lost to us, is well made (and well worth serious consideration, not the least by readers of Presocratic texts).

The remaining contributions are presented by Egyptologists, Assyriologists, or Classicists, who are currently working on specific translations, and allow us to glimpse what is going on at their workbenches. Among the Egyptologists, Alexandra von Lieven [139–150: 'Translating the Fundamentals of the Course of the Stars'] describes what we know as *The Book of Nut*—the true title, which she has discovered, is 'Fundamentals of the Course of the Stars'—and the different versions of which allow insight into the philological practices of priests who compared different existing versions. While describing methodological and actual problems of the translation of specific language-structures, there comes into view a fascinating area of Egyptian literature comprised of a great variety of 'manuals', including instruction on how to run an ideal temple.

Tanja Pommerening [153–174: 'Von Impotenz und Migräne—eine kritische Auseinandersetzung mit Übersetzungen des Papyrus Ebers'] presents interesting data (see especially the diagrams on page 161) on how certain, highly problematic, translations, in this case of the ancient Egyptian medical Papyrus Ebers, have been the most influential ones (both 'impotence' and 'migraine' are overly precise identifications of ailments against which the papyrus provides help). The dominant position of less than apt translations is due to a combination of information sociology (availability, established traditions, and so on) and of the tastes of the targeted readers' tastes who have, understandably, in the past preferred pseudo-accurate identifications of medical phenomena rather than question marks. Pommerening here touches upon the well known crux of retrospective or palaeodiagnosis that is always either marred by complex anachronism ranging from the conceptual to the terminological or essentially impossible.[8] In the first instance, although theoretically faulty, it provides 'facts' for modern readers who are not experts of the source-culture, e.g., physicists or historians of more recent 'sciences'. In the second, it ends in *aporia* but is at least well aware of the historical dimension of the problem. This problem has been well explored, for example, with respect to Thucydides' description of the Athenian plague.

Friedhelm Hoffmann [201–218: 'Zur Neuedition des hieratisch-demotischen Papyrus Wien D 6257 aus römischer Zeit'] describes his

---

[8] See Grmek 1998, 6 f. and, more sceptical, Leven 2004.

work on a medical text from Crocodilopolis. Hoffmann gives a practitioner's account that shows quite well, besides the points which he actually wishes to make, how extensively matters of private taste infuse the edition and translation of 'scientific' texts [204: 'Ich nehme daher lieber eine hellgraue Unterlegung'].

Among the Assyriologists, Nils Heeßel [175–188: 'Rechts oder links—wörtlich oder dem Sinn nach?'] introduces his article with Rosenzweig's well put dictum that translating is like serving two masters and thus impossible. After a short discussion of the problem's theoretical side,[9] especially with respect to 'science', Heeßel concentrates on Babylonian diagnostic texts and presents three aporetic cases in which target-oriented and source-oriented translations are bound to clash. Martin Worthington's essay [189–199: 'The Lamp and the Mirror, or: Some Comments on the Ancient Understanding of Mesopotamian Medical Manuscripts'] discusses the evidence for scribal philology, including translation, that the manuscripts provide.

Two Greco-Roman traditions that have roots in ancient Near Eastern or Egyptian knowledge traditions, namely, pharmacology and astrology, are the subject of papers by Laurence Totelin [219–237: 'A Recipe for a Headache: Translating and Interpreting Ancient Greek and Roman Remedies'] and Stefan Heilen [299–329: 'Problems in Translating Ancient Greek Astrological Texts']. Totelin gives a great *tour d'horizon* of Greek and Roman pharmacological recipes, explaining their context, textual structure, terminology, and so on, choosing as her example the τροχίσκος ('pastille') of Antonius. Besides the obvious problem of translating terms for the many substances of *materia medica* and its quantities, she discusses the reconstruction of actual drugs and their efficacy, also considering approaches taken from ethnpharmacology. For the layperson, Totelin opens up a fascinating and, among classicists, much-avoided field. Recipes, however, have been studied, even from a literary point of view.[10]

Heilen gives a systematic account of the difficulties that a translator of Greek astrological texts will encounter, starting from the local (transmission, style, terminology) and ranging towards the global (conceptual, poetic mode of exposition). Most of the points made here find parallels in non-astrological traditions. The end of the

[9] See now Kitzbichler 2007.
[10] See most recently Telle 2003 and Asper 2007, e.g., 197 f.

article reads as if it was meant to illustrate Rosenzweig's statement (quoted by Heeßel, see above): translation (of astrological texts) is either impossible straightaway, because the texts as transmitted do not make sense (Heilen's section 1); or it is impossible without giving additional information (in 'boxes', as Heilen repeatedly explains) that provides context, which means in fact a commentary, marginal or not.

As should be clear by now, this volume has many attractions. The editors have mastered the noble task to of bringing together people from different philological-historical disciplines in order to discuss a problem fundamental to them all. Generally, the collection is very successful in illustrating all the different aspects of the problem. Second, it is also a great guide to the range of fields concerned with ancient 'science'. Many of the papers presented here would also serve well as a readable, up-to-date introduction to the fields that they discuss (Totelin, Heilen, and Ritter). Only occasionally does one get the impression that experts are talking to each other, well over the heads of their interdisciplinary readership (e.g., Høyrup [391]: 'for example, ZUR.ZUR (now read UL.UL and interpreted du7.du7)'). The volume both underlines the heterogeneity of ancient rational-practice traditions, the need to approach them across disciplines, the many practical obstacles, and the rewards of such approaches. Our modern institutionalized field of higher education and professional scholarship, by bringing about increasing differentiation, has made the study of such cross-disciplinary problems even more difficult. It is certainly not by chance that the two editors of this volume, Tanja Pommerening and Annette Imhausen, before becoming well established Egyptologists, had earned undergraduate and graduate degrees in pharmacology and mathematics, respectively. They should be congratulated on their achievement.

BIBLIOGRAPHY

Asper, M. 2007. *Griechische Wissenschaftstexte.* Stuttgart.

Gerzymisch-Arbogast, H., Heine, C., and Schubert, C. 2006. edd. *Text and Translation: Theory and Methodology of Translation.* Tübingen.

Gieryn, Th. 1995. 'Boundaries of Science'. Pp. 393–443 in S. Jasanoff *et alii* edd. *Handbook of Science and Technology Studies.* Thousand Oaks, CA.

Goodenough, W. H. 1970. 'Describing a Culture'. Pp. 104–119 in W. H. Goodenough ed. *Description and Comparison in Cultural Anthropology.* Cambridge.

Grmek, M. D. 1998. *Diseases in the Ancient World.* Baltimore.

Harris, M. 1976. 'History and Significance of the Emic/Etic Distinction'. *Annual Review of Anthropology* 5:329–350.

Hess, D. J. 1997. *Science Studies.* New York/London.

Jeyes, U. 1991–1992. 'Divination as a Science in Ancient Mesopotamia'. *Jaarbericht von het Vooraziatisch-Egyptisch Genootschap Ex Oriente Lux* 32:23–41.

Kitzbichler, J. 2007. 'Nach dem Wort, nach dem Sinn. Duale Übersetzungstypologien'. Pp. 31–45 in H. Böhme *et alii* edd. *Übersetzung und Transformation.* Berlin.

Larsen, M. T. 1987. 'The Mesopotamian Lukewarm Mind: Reflections on Science, Divination and Literacy'. Pp. 203–225 in F. Rochberg-Halton ed. *Language, Literature and History.* New Haven, CT.

Leven, K.-H. 2004. 'At Times These Ancient Facts Seem To Lie before Me like a Patient on a Hospital Bed: Retrospective Diagnosis and Ancient Medical History'. Pp. 369–386 in H. F. J. Horstmanshoff and M. Stol edd. *Magic and Rationality in Ancient Near Eastern and Graeco-Roman Medicine.* Leiden/Boston.

Schmitz, T. A. 2007. *Modern Literary Theory and Ancient Texts.* Malden, MA.

Telle, J. 2003. 'Das Rezept als literarische Form'. *Berichte zur Wissenschaftsgeschichte* 26:251–274.

Unguru, S. 1975. 'On the Need to Rewrite the History of Greek Mathematics'. *Archive for the History of Exact Sciences* 15:67–114.

Vandevelde, P. 2005. *The Task of the Interpreter: Text, Meaning, and Negotiation.* Pittsburgh, PA.

Divine Machines: Leibniz and the Sciences of Life by Justin E. H. Smith

Princeton/Oxford: Princeton University Press, 2011. Pp. xii + 380. ISBN 978–0–691–14178–7. Cloth $45.00

Reviewed by
Lea F. Schweitz
Lutheran School of Theology at Chicago
lschweitz@lstc.edu

Justin Smith's fine book opens with a subtle guide for the reader. The preface claims that Smith sees *Divine Machines* as a 'transitional fossil'. In an age of increasingly electronic, interdisciplinary, collaborative research, Smith acknowledges that a single–author, printed book is already something of a fossil. However, *Divine Machines* is a transitional fossil because it employs these methods in a form that belies the virtual connections and electronic resources that made it possible.

In *Divine Machines*, G. W. Leibniz (1646–1716) is treated as a transitional fossil, and this is to his credit. It is clear that Leibniz is a thinker-in-between for Smith. The opening pages situate him between Aristotle and Nietzsche. The body of the book holds Leibniz's 'forward-looking' metaphysics and mechanism together with the problems that he inherited in natural philosophy and theology. The task of this book is to make sense of this shifting ground.

Methodologically, this puts Smith's work in a growing field of scholars who see Leibniz as a dynamic thinker rather than as a representative of a fixed philosophical system. The result is an investigation into the chronological development of his engagement with the life sciences—from an early concern about the reform of medicine to later views about the organic structure of the world [15]. The innovation in Smith's text is to locate Leibniz's engagement with the organic world and his emerging life sciences at the center of his dynamic thought. In particular, he treats the problem of the structure and motion of the physical world, living entities, and lifeless things [parts 1–2]; the problem of coming into being or generation [part 3];

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 19–24

and, species [part 4] as biological problems rather than as linguistic ones.

This stands in contrast to recent literature in Leibniz scholarship which has been largely concerned with interpreting Leibniz's metaphysics on an idealism/realism spectrum. In contrast, Smith aims to change the terms of the debate by refocusing it on the phenomena of life and by re-conceiving Leibniz's theoretical concerns in fundamentally biological terms [6]. The picture of Leibniz that emerges is more empiricist than often assumed, and it focuses on the structure and organization of bodies rather than the ontological status or ground of bodies. On Smith's reading, Leibniz is a philosopher of life whose philosophy is 'of biology' in important ways; but these have been mostly neglected, until now.

Smith also recognizes that Leibniz's philosophy of biology is a philosophy of medicine. As such, his argument begins by taking seriously Leibniz's early and formative encounter with medicine. Here, as throughout, Smith is able to bring together aspects of Leibniz's wide-ranging interests and he provides very helpful, broad introductions to often neglected areas of Leibniz's thought. The analysis is contextualized both by current scholarship on Leibniz and views contemporaneous with Leibniz.

In chapter 1, Smith shows that Leibniz's early encounter with medicine began a lifelong engagement with the medical debates of his day. This area of Leibniz's thought draws together experimental methods from vivisection to microscopy, influences from chemistry, commitments to improving public health, and the overarching concern to understand the mechanics of animal bodies. The opening chapter on medicine complements the remainder of the book. In particular, Leibniz's medical texts provide a snapshot of a larger shift in Leibniz's thought. In medicine, Smith charts Leibniz's gradual movement from an interest in the macroscopic vivisection of animal bodies to the microscopic investigation of what these bodies contain, including microorganisms. The chapters that follow parallel this shift. They trace the development of Leibniz's understanding of animal bodies from the macrostructure and function of the 'hydraulico-pneumatico-pyrotechnical' machine of animal economy to the microstructure and organization of the 'machines within machines to infinity' of organic bodies.

In chapter 2, Smith provides a detailed description of Leibnizian animal economy. On his reading, animal economy is part of Leibniz's early attempts to describe the animal body as a special kind of machine. The key feature of this discipline is its intention to understand animal bodies as a structure that is economical in the sense of achieving the maximum effect with the minimum of organs and in the sense of providing explanations of vital phenomena without recourse to the soul [92–93]. Animal economy is important in Leibniz's thought as the initial stage of his lifelong project of understanding the mechanism of animal bodies. Smith argues that Leibniz's focus shifts from this initial interest in the macrostructure of animal economy to the microstructure of the organism of the body. The latter is the subject of the remainder of the book.

For Leibniz, 'organism' is a structural term that names a condition of organic bodies; its meaning is closer to organization than to a biological entity, although the two are related. As Smith writes:

> Organism is to natural machines what mechanism is to artificial machines, and this organism is not contrasted with mechanism, but rather is conceived as a variety of it. [106]

'Organics' is the term used to describe Leibniz's attempt to describe animal bodies as infinitely complex, natural machines. By emphasizing Leibniz's pervasive interest in organics, Smith is able to trace another overlooked development in Leibniz's thought. In addition to the shift in focus from the macrostructure to the microstructures, Smith's persuasively argues for a change at the level of the structure of bodies 'from finite structures decomposable into homogeneous masses, to infinitely structured machines, or bodies endowed with organism' [105]. In the course of this argument, chapter 3 provides a clearly articulated conceptual map of the items that populate Leibniz's natural world, including organic bodies, artificial and natural machines, corporeal substances, animals, and aggregates.

Organic bodies are distinct from mechanical bodies because they are infinitely complex [108]. On this view, organic bodies are distinguished from mechanical ones without the introduction of an immaterial vital principle. The difference between inorganic and organic is in complexity. By locating the difference at the level of complexity, Leibniz maintains his thoroughgoing mechanism. At the same time, organic bodies are conceptually distinct from the corporeal bodies within which they are always found.

In this discussion, Leibniz is a 'transitional fossil'. As Smith writes:

> Leibniz helps to open up the possibility of studying biological entities biologically, that is, independently of soul-based features such as unity and activity. This new possibility would ultimately help to stimulate a naturalistic conception of biological entities, which in turn, would come to underlie the newly independent science of biology: the study of vital phenomena without appeal to vital forces. [110]

Organics helps to isolate a feature of the natural world which can be investigated without recourse to vital forces. There is some debate as to whether this is appropriately understood as a naturalistic conception but the distinction Smith identifies and its importance are well argued. With this distinction in hand, Smith helpfully compares his reading to contemporary scholarship on Leibniz and Leibniz's view to positions contemporary to Leibniz himself, including in his remarks Henry More, Anne Conway, Ralph Cudworth, and Damaris Masham.

Chapters 4 and 5 turn explicitly to the scientific and theological contexts of Leibniz's theory of organic bodies. In the former, Smith examines Leibniz's innovative theory of nested individuality and his long engagement with microscopy; in the latter, Smith develops Leibniz's account of divine preformation. This combination of influences explains the title of Smith's book. Organic bodies emerge as infinitely complex machines that are neither naturally generable nor corruptible. Only God can bring them into existence or take them out of existence. As such, Leibniz argues that together organism and divine preformation are sufficient to explain the origins, structure, and motion of organic bodies. Hence, organic bodies are divine machines:

> divine, because initially generable only by God directly; machines, to the extent that one need take no recourse to God's constant concourse, nor to some subordinate God-like principle within the machine, in order to obtain an adequate understanding of it. [135–136]

With this understanding of Leibniz's theory of organic bodies, the final chapters of *Divine Machines* address two well known features of Leibniz's thought: spontaneity and species. In his treatment of spontaneity, Smith distinguishes between two early modern options:

(1) spontaneity in which states are determined solely by the intrinsic properties of the thing itself, and
(2) spontaneity in which states are undetermined by prior conditions and arise under the influence of mind-like powers without constraint by the material being influenced.

Leibniz makes the former a central piece of his philosophical project; he rejects the latter. Smith connects Leibniz's view of spontaneity to his theory of trait acquisition and generation in the emerging life sciences and to his explanation of fossils in the emerging geological sciences. Supported by his preformationism and pre-established harmony, Leibniz opts for a view of generation as heterogenesis and sees fossils as vestiges of organic bodies rather than 'games of nature'.

In the final chapter, Smith takes up the question of Leibniz's view of species and he finds that for biological species Leibniz is a species-fixist. Consistent with his view of preformationism, Leibniz holds that the natural species are fixed from the time of creation, even as they may undergo radical changes over the course of a lifetime. These commitments together make Leibniz a species realist and not a nominalist. In this debate, Leibniz's primary interlocutor is John Locke; but Smith helpfully contextualizes these discussions with Leibniz's contemporaries by engaging John Ray, Nathaniel Highmore, Anne Conway, Edward Tyson, and others. Smith argues that Leibniz views biological species membership as determined by generation and origin. Smith's approach allows him to include Leibniz's denial of the possibility of evolution and his universalist anthropology as relevant features of his view of species.

Again in these final chapters, the reader gets a sense of Leibniz as 'transitional fossil'. He offers a thorough-going mechanism that is at the same time replete with immaterial powers/forces. He recognizes transformative morphological change but denies the principle of evolution. The fact that Smith is able to chart this shifting space with such precision is one reason, among many, to recommend *Divine Machines.* In addition, the concluding appendixes make available in English a chronological sample of Leibniz's texts on medicine, animals, and botany. Most importantly, Smith integrative approach to

Leibniz's philosophy and his lifelong engagement with the emerging life sciences brings a new lens to early modern scholarship.

In some cases, this raises challenges. For example, in the discussion of species, Smith considers Leibniz's view of the great chain of being, which is understood as the continuous, hierarchical ordering of nature. Typically, this view is read as supportive of a nominalist reading of species. However, Smith claims that Leibniz can hold together the commitment in the infinite gradations between natural kinds and the possibility of real breaks in the continuum. These breaks provide a view of nature that is dense rather than continuous and, as such, it is supportive of Leibniz's species-realism. In this instance, Smith's argument does not settle the matter. The claim against Leibniz's nominalism in favor of species-realism is persuasive but it opens up a deeper debate about whether Leibniz can consistently hold a view of nature that is in some sense both dense and continuous. Even in the instances where the reader's doubts linger, Smith's scholarship makes a convincing case and one is required to look anew at Leibniz's most well known commitments. For the contributions it makes in our understandings of Leibniz and for the way in which Leibniz is integrated in the emergence of the life sciences, *Divine Machines* is highly recommended reading.

Physics and Philosophy of Nature in Greek Neoplatonism. Proceedings of the European Science Foundation Exploratory Workshop (il Ciocco, Castelvecchio Pascoli, June 22–24, 2006) edited by Riccardo Chiaradonna and Franco Trabattoni

Philosophia Antiqua 115. Leiden/Boston: Brill, 2009. Pp. vi + 317. ISBN 978–90–04–17380–4. Cloth $169.00[1]

Reviewed by
Marije Martijn
Vrije Universiteit Amsterdam
m.martijn@vu.nl

This volume contains some very interesting material on philosophy of nature in late antiquity. As the editors point out in their introduction, recent decades have seen a revaluation of the Neoplatonic physics and philosophy of nature, as opposed to the earlier slighting of these subjects because of the supposed purely metaphysical and theological character of Neoplatonism. The main aim of *Physics and Philosophy of Nature in Greek Neoplatonism* is to contribute to this revaluation in a very concrete way by discussing some of the abundant material on the topic.

The editors chose to include both 'physics' and 'philosophy of nature' in their title in order to distinguish between the Neoplatonists' understanding of nature and the place of that understanding in the overall philosophical system or discourse. Although this distinction does not figure as such in the book, it does serve to indicate the broadness of the topic thereof as well as to emphasize an important aspect of Neoplatonic thought about the natural world. As the editors point out in their introduction, the strong metaphysics of the Neoplatonists did not suppress other branches of philosophy but instead formed their conceptual framework.[2] 'Accordingly, it would not be wrong to speak of a Neoplatonic "metaphysics of nature",

[1] Work on this review was made possible by the Netherlands Organization for Scientific Research NWO, project 275–20–020.

[2] I do not agree, however, with the editors' suggestion [14] that besides the shared metaphysical character or approach, the Neoplatonists shared a single

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 25–52

"metaphysics of fate and providence", or "metaphysics of science and knowledge"' [14]. And accordingly, it does not suffice to discuss Neoplatonic views of natural phenomena in isolation: they must always be considered within their metaphysical context. As we will see, this is what the papers in the volume do.

The editors pose two general questions:

(1) 'Did Neoplatonic authors ever prove capable of developing a unified conception of physical reality?', and
(2) 'Was such an overall conception capable (at least in principle) of providing rational explanations concerning natural phenomena in all their complexity?'

The first question does not receive a straightforward answer but is used to create a contrast between the unity of the metaphysical framework used [see, however, 25n2 above] and the absence of a unified conception of nature. Different philosophers come up with different theories, each with its own problems and attractions.

Neoplatonic philosophy of nature cannot be compared to contemporary theories, as it is more interested in tracing back physical phenomena to their metaphysical causes and generalizing than in analyzing empirical detail. Therefore, according to the editors, the answer to the second question is 'No'. However, I would counter that that answer is given from the perspective of contemporary theory, as a Neoplatonist would no doubt reply that, of course, the only good rational explanation of natural phenomena in all their complexity is a theory which explains those phenomena in terms of their transcendent causes.

The 10 papers are presented in chronological order of subject matter. In a nutshell, the volume contains the following papers: we find Marwan Rashed (in French) analyzing the truth behind the Neoplatonic presentation of Xenarchus, Ptolemy, and Plotinus as all three criticize Aristotle's theory of the natural motion of the elements; Riccardo Chiaradonna (also in French) reconstructs Galen's *De demonstratione*; George Karamanolis defends Plotinus' notion of quality and more generally his ontology of the sensible; Robbert van

conceptual framework. As is clear even in the volume here discussed, e.g., in Russi's paper, there are important variations in the metaphysics of the Neoplatonists as well.

den Berg distinguishes two kinds of common notions on the basis of Plotinus' and Proclus' (and Augustine's) concepts of time; Christian Wildberg scrutinizes Plotinus' puzzling remarks on 'nature's contemplation'; Chiara Russi shows that there are a number of differences between Plotinus' and Proclus' views on causality in the natural world; Alessandro Linguiti describes Proclus' views on the relation between nature and fate; Jan Opsomer analyzes Proclus' theory of motion in the *Elementa physica* and shows how Aristotelian and Platonic material interact therein; Gerd Van Riel traces the many layers in Proclus' notions of matter and necessity, and their sources in different Platonic dialogues; and finally Carlos Steel presents Proclus' theology of the Earth.

After this very brief and general overview, let us dive a little deeper into the papers one by one (or skip straight to the Conclusion).

Marwan Rashed presents a clearly written but very dense 'background check' of Simplicius' claim that Ptolemy, Xenarchus, and Plotinus all rejected the Aristotelian theory of the natural rectilinear movement of the four elements and replaced it with the theory that the elements either are at rest or have a circular movement, in order to render a fifth element superfluous. Rashed's main aims are to reconstruct the actual positions of these three philosophers and thereby to emphasize that one should not disconnect philosophical theories from their contexts. His method is that of meticulous textual analysis (unfortunately, without always quoting the Greek passages involved) combined with some speculation where evidence is lacking.

Rashed starts from Xenarchus' well known position that a fifth element is not required because fire, in its natural place, moves in circles; and assumes what he calls the 'naïve' [18n5] position, namely, that Xenarchus' aim is to present the Aristotelian system in as good a way as possible—which fits his epithet 'peripatetic'. The method Rashed ascribes to Xenarchus is rather Ockhamist/modern, as it includes the reduction of principles through the experimental verification of a theory with empirical data. The downside of that approach, according to Rashed, is that the eternity of heaven, the second argument for the fifth element, is 'left dangling' ('un flottement') [19].

In the case of Ptolemy, things are less straightforward because of textual issues. After analyzing them, Rashed first concludes that in *Alm.* 1 Ptolemy proposes a cosmological model which distinguishes between two static relations: on the one hand, there is a homeomerous element,[3] which by its relative inertia dominates and 'holds back' (i.e., keeps in place) the non-homeomerous sublunary realm; and on the other hand, there is Earth, which is immobile and ever more compact due to the external pressure of heavy (composite) bodies.

On the basis of five known testimonies and one 'new' testimony concerning Ptolemy's Περὶ τῶν στοιχείων and Περὶ ῥοπῶν, which Rashed argues are one and the same work, Rashed then goes on to introduce Ptolemy's notion of 'inclination', that is, the tendency of composite bodies to move to their natural place. Once they have reached it (a low place for heavy bodies, and a high one for light bodies), they become immobile.

Rashed distinguishes two views of heavy bodies, namely, as bodies which tend to the center of the cosmos (as opposed to light bodies which tend to the periphery) and as bodies which, in their own place, are not prone to move (as opposed to light bodies which are easily moved). The eventual circular motion of fire and air are explained as the result of the 'sweeping' of aether's motion.

While emphasizing that not all details in Ptolemy's doctrine on the elements are clear, Rashed suggests by way of conclusion that Ptolemy's aim is to unify Aristotle's kinematics while maintaining the supremacy of the heavens. He refined Aristotle and used Xenarchus to plead for the fifth element: by emphasizing the immobility of the other elements, he reinforced the circular movement of the fifth.

About Plotinus, finally, Rashed states that he says neither that the elements move rectilinearly to their natural places nor that they are at rest or move in circles once they get there. Moreover, by ascribing such positions to Plotinus, Simplicius betrays Plotinus' representation of the sensible, according to which certain phenomena in the sensible world can only be explained by causes that transcend the corporeal. On the basis of *Enn.* 2.2 [14] and 2.1 [40], Rashed argues that at first (in 2.2) Plotinus maintained that the circular movement of heaven is a result of the combination of the rectilinear movements of the elements and the non-local reversion of soul. Later, however

[3] Rashed interprets this as corresponding to the fifth element but it is not clear to me what his arguments are.

(in 2.1), Plotinus took circular movement to hold the middle between rectilinear and intelligible motion.[4]

Rashed's analysis of the positions of the three philosophers grouped together by Simplicius as opponents of the Aristotelian theory of the fifth element shows that the supposed unity does not exist (Ptolemy staying closest to Aristotle, and Plotinus being most critical). Rashed suggests that the Neoplatonist's reason for presenting a not quite accurate unified opposition to Aristotle was that in doing so he wanted to create a front against the anti-Platonic traits of Aristotle's cosmology.

Riccardo Chiaradonna's paper is the one that fits the overall topic of the volume least. Its aim is to reconstruct the epistemological project of Galen's *De demonstratione* and to place the work in the context of the philosophical debates of the second and third centuries. In order to reach that goal, Chiaradonna reconstructs the structure and general sense of the lost treatise, then considers some traces of its posterity, and finally places the whole in the broader context of the transition from post-Hellenistic philosophy to that of late Antiquity. Galen, it turns out, is quite conservative in that we do not find in his epistemology the far-reaching 'ontologization' which is the core of that transition. Another aspect of Galen's thought that is emphasized by Chiaradonna is his general interest in epistemological debates and especially in questions regarding the foundation of knowledge.

So how does this paper on epistemology in the mid-second century fit a volume on philosophy of nature and physics in Greek Neoplatonism? With respect to physics, I guess the answer is that, as Chiaradonna shows, *De dem.* contained some, at first sight irrelevant, discussions of all kinds of issues relating to time, space, generation and corruption, matter, the elements, and so on. With respect to the Neoplatonic aspect of the volume, the connection is a bit more forced: to some extent, Galen is a Platonist—he himself states that Plato is

[4] I do not agree entirely with Rashed's analysis of *Enn.* 2.2.1 [36]. I would say that providence explains not so much why fire moves in circles in its natural place (the presence of soul explains that) but why fire stops moving in a straight line, which prevents it from dispersing entirely. That is, soul explains the kind of motion but providence explains the presence of soul and, hence, circular motion as opposed to dissipation.

one of his main sources. However, since Galen does not 'ontologize', he is certainly not a Neoplatonist.

In the longest section of his paper, the reconstruction of Galen's 'applied epistemology', Chiaradonna brings up the role of the digressions concerning non-medical issues. He points out that the purpose of *De dem.*—teaching doctors the method of demonstration, the use of which gave medicine a scientific status—does not sit well with those digressions. Chiaradonna reviews the different solutions given to that problem and comes up with an answer of his own: for Galen, logic is no more than the method of invention in science. It allows the scientist to gain general and structured knowledge of axioms and theorems, and to apply this knowledge in his own discipline. This goes for the physician as well, as medicine is an applied science founded on exact and verifiable theorems. Now, since logic is no more than a method of science, a treatise on logic cannot but contain specific examples of the application of that method. Thus, a digression on vision teaches us about natural criteria as the foundation of knowledge; the discussion on the eternity of the world is to be understood as part of the distinction between problems of which we can have scientific knowledge and problems of which we cannot;[5] and the discussion of time is presented in order to explain that some objects are primitive and cannot be defined.

Chiaradonna elaborates quite a bit on the digression on time. Simplicius and Themistius report Galen's view that time has no relation to motion. Instead, motion comes in when we think about time, since we do not think by 'immobile thought'. Moreover, Galen, as is well known, rejects Aristotle's definition of time as circular. Chiaradonna briefly distinguishes two strands in current discussions on Galen's view of time: those who place it in the context of the ancient debate on Aristotle's *Physics* and argue that Galen adopts the *Timaeus*' notion of time as a substance, and those others, notably, S. Fazzo, who maintain that Galen did not really have an anti-Aristotelian view. Chiaradonna partly sides with Fazzo and suggests that Galen was trying to give a satisfactory explanation of Aristotle's

[5] Or, as Chiaradonna states later [54], the discussion on the generation of the world was part of a discussion on the relation between truths of reason and truths of experience.

theory.[6] It seems to me, however, that there is something of a tension between this suggestion which implies intrinsic interest in the question discussed and the aforementioned role ascribed to the digressions as merely exemplifying epistemological issues. Galen could have tried to come up with examples from the discipline to which the logical method was to be applied, namely, medicine. Instead, however, he chose traditional discussions from physics and tried to contribute to those discussions.[7]

After reconstructing *De dem.* and the role of the digressions therein, Chiaradonna moves on to characterizing the philosophy presented in the treatise. He points out that the epistemology (the theory of definition, of its epistemic function and limits, and of the immediate evidence of primitive terms) is not founded on an ontology—thus, when Galen describes immanent universals, he presents a taxonomical, not an ontological, realism. As a consequence, the relation between logic and physics is not mediated by ontology. I am not entirely sure what that means. Should we understand that Galen was not interested in making explicit his ontological assumptions? Or rather that he thought that in the context of logic and physics there are no relevant ontological assumptions? Chiaradonna seems to prefer the latter but his careful formulation, 'il n'est peut-être pas trop hasardeux de reconnaître' [64], shows that the evidence is thin.

Chiaradonna ends by addressing the transition from the post-Hellenistic era to Neoplatonism. Pointing out once more that Galen is not interested in metaphysical speculation, Chiaradonna emphasizes that there are nonetheless interesting parallels between Plotinus and Galen, e.g., in their discussions of time, which might reveal, not necessarily that Plotinus read Galen, but rather that both took up currents that were common in their time.

Chiaradonna concludes that, although we have only fragments of *De dem.*, they do allow us to grasp the general character of Galen's

---

[6] But he admits that Galen's attitude to Aristotle was at least 'ambivalent' [58].

[7] By way of a tentative suggestion: Could it be that *De dem.* contains examples that do not find a home in the other works on method, *On the Doctrines of Hippocrates and Plato* or *On the Therapeutic Method*?

epistemology and to determine that Galen is on the conservative side of the ontologization occurring in the mid-second century.

Like its subject, the paper contains some digressions which seem irrelevant, such as that on Galen's dealings with Aristotle's theory of time and Themistius' replies to Galen. However, it is in those digressions that the paper is closer to the topic of the volume. Chiaradonna's paper is at times a bit messy, at others a bit bold in its conclusions, which are necessarily based on scarce evidence; but it addresses interesting issues relating to an important text.

George Karamanolis' contribution addresses the very complex issue of the notion of 'quality' in Plotinus and presents an interesting analysis of different aspects thereof. Karamanolis sets out to show that Plotinus has a coherent and quite distinct theory of quality, using primarily *Enn.* 2.6 [17] and 6.1–3 [42–44]. The main threat to coherence in the case of Plotinus' theory is the problem of the status of immanent forms: Are they qualities like any other?

Substances are found only on the level of the intelligible and they are the causes of qualities in the sensible world. The sensible contains aggregates of matter and qualities, no more. As a consequence, immanent Forms cannot be substances but have to be qualities like any other—there is no distinction in Plotinus between accidental and substantial qualities. Nonetheless, he does consider the immanent Form to have both a causal role in bringing along other qualities and an epistemic role in our recognizing a sensible entity as a specific thing.

Karamanolis maintains that Plotinus is not inconsistent here. So how does he reconcile the two sides of the story? He starts by bringing together two other aspects in addressing what is sometimes called 'the integration challenge': the epistemological and ontological roles of the Forms should not clash, in the sense that their epistemological role cannot involve ontological presuppositions which do not match their supposed ontological role and *vice versa*. Karamanolis presents the main problem of Plotinus' theory of quality, i.e., that of the status of the immanent Forms, as an example of the integration problem; but I think he is partly wrong in doing so. According to Karamanolis, the epistemic role of immanent Forms cannot be matched with the ontological claim that they are qualities like any other. In fact, however, both the epistemic role and the ontological role ascribed by Plotinus to immanent Forms, i.e., that of bringing along qualities such as weight, are problematic. It is perhaps better to separate ontology and epistemology in this case and to say that, just as the ontological role of the immanent Forms does not match their nature of being a quality like any other, so the epistemic role of the immanent Forms does not match the fact that our only information regarding sensible entities is a collection of perceptions.

Plotinus' answer to the epistemological problem, according to Karamanolis, is that we humans cannot perceive without the use of reason. We perceive a collection of images and resort to our contact with transcendent λόγοι to construct what the sensible object in question is. Do the immanent Forms play a special role in this process? Karamanolis thinks that they do not. Or to be precise, since perception relies on awareness of Forms in the soul, the initial stage of perception plays 'hardly any role' [89]. In that case, the epistemological side of the problem is resolved. This is not a satisfying conclusion, I would say, as it relies on not really answering the question whether or not the immanent Forms guide us to the transcendent λόγοι.

How about the metaphysical problem? Is the immanent Form an accidental quality or is it instead the source of such qualities but itself of a different nature? Karamanolis presents a discussion of the relation between substance and quality and different kinds of qualities in Plotinus. Substances are found only on the level of the intelligible. Those substances have substantial qualities, which are in their subject. Since there is no intermediary between substance and quality, and since substances can only produce lower entities, on the level of the sensible we only find qualities—of what kind? In 2.6, Plotinus distinguishes between two kinds of qualities: intelligible (i.e., the λόγοι which are the activities of the intelligible) and sensible (i.e., the manifestations of those activities in the sensible ream). Intelligible qualities are qualities only homonymously. This discussion contains some obscurities, such as the nature of the substantial qualities: Are they identical to the intelligible qualities (i.e., the activities of the substance)? If not, then there are intermediaries between substances and (sensible) qualities after all, namely, the intelligible qualities.

A further distinction is made in the sensible realm between qualities which complement a sensible 'substance' (i.e., which separate kinds) and qualities which are merely accidental (i.e., which differentiate entities of the same kind). The latter are pure qualities, the former are called 'properties' (the existence of which is denied by Plotinus at 6.2—but Karamanolis thinks this is no problem). According to Karamanolis, the source of the pure qualities cannot be found in the λόγοι.

In order to answer the question whether immanent Forms are qualities like any other, Karamanolis proposes a wide and a narrow sense of 'quality': wide when all features of a sensible are called qualities, narrow when only accidental features are called qualities. Obviously, this answer does not really solve our problem: claiming that a notion is sometimes used in one sense and sometimes in another does not render a theory coherent. More importantly (as coherence is not the be-all and end-all of philosophy), it leaves many questions unanswered: it does not tell us what exactly the difference is between an immanent Form and other qualities, whether the former indeed brings along the latter, and how we tell them apart when identifying some x as an elephant. Also, the status of the transcendent forming principles or λόγοι remains unclear. Something Karamanolis does make clear is how Plotinus' view on quality can shed light more generally on the relation between the intelligible and the sensible. All in all, this is a thought-provoking but not entirely satisfying paper.

Robbert van den Berg presents us with a well known problem of the notion of 'time': the definition of that notion is always put in terms of time. Van den Berg uses this problem to discuss an interesting epistemological issue, namely, the nature of the so-called 'common notions' and, more specifically, the common notion of time in Plotinus, Proclus, and Augustine. Although in all three we find the Epicurean sense of common notions as deriving from sense perception and coinciding with the meaning of words, they also distinguish another kind of common notions, namely, those based on intuitions of transcendent principles.

After an overview of Phillips' and Strange's views on common notions in Plotinus as criteria of truth and as either a comprehensive grasp of an innate idea (P) or a vague concept, early reminiscence, and idea of the many (S), van den Berg partially sides with Strange but points out that for Plotinus the ideas of the many can never be a criterion of truth. Van den Berg concludes that it was crucial to know the source of a common notion: perception or intuition. In the case under discussion, the notion of time, Plotinus follows Epicurus to quite some extent but also Plato's *Timaeus*: we do not have an inborn notion of time as of eternity but develop it on the basis of empirical reasoning. Thus, although he does mention a common notion which cannot be gathered from perception, namely, that of the omnipresence of god, we cannot conclude that for Plotinus all common notions are innate.

So what does this mean for the use of such non-innate notions in philosophical argument? According to van den Berg, Plotinus [*Enn.* 2.4 [12] 1 and 3.7 [45] 1] uses the Aristotelian distinction between a conceptual definition (the meaning of a word which coincides with the sense-derived notion) and a substantial definition (giving an account of the essence). Since time has a sense-derived common notion, we cannot use it to understand the essence of time. For that, we need to ascend to the metaphysical principles of temporal phenomena. That does not mean, however, that the common notion is useless in philosophy: any essential definition of time will have to accommodate the common notion. Something that remains implicit in van den Berg's discussion, but may be a problem for the use of conceptual definitions, is the fact that 3.7 [45] 1 suggests that we have a conceptual definition both of sense-derived notions (time) and intuitive notions (eternity) [113]. That is, a conceptual definition does not necessarily coincide with a sense-derived notion.

Proclus, van den Berg shows, does not agree with Plotinus on the definition of time; but he does use the same method of explaining physical phenomena through metaphysical principles, starting, however, from a 'shared sensation' of time. Interestingly, unlike Plotinus, Proclus also mentions a notion of eternity that is grasped only by wise men. According to van den Berg, both notions are derived from sense perception and Proclus 'obviously thinks little of them'. The latter statement is hardly warranted, I would say, considering that Proclus ascribes one of them, namely the notion of eternity, to wise men (οἱ σοφοί).[8] What Proclus does think little of—as van den Berg goes on to show—is people who refuse to look beyond the common notions derived from perception.

Like Plotinus, Proclus assumes that we also have another kind of common notions that are not derived from perception but from

[8] The passage that van den Berg adduces in 116n56 does not support his statement as it contains no value-judgment.

contact with the transcendent gods. And like Plotinus, Proclus also thinks that sense-based notions are useful for testing a theory: we should not 'destroy the phenomena' but accommodate them in our metaphysical theory.

About Augustine, van den Berg is quite brief. He points out that Augustine's well-known paradox of time points to the above-mentioned distinction between a conceptual and an essential definition. Augustine's *communes notitiae* are not so much like Stoic common notions as they are like those of Plotinus, as they reveal only accidental features. And like Plotinus and Proclus, Augustine wants to move beyond the common notion to an explanation of the essence of time.

Throughout his paper, van den Berg discusses many details of the Neoplatonic theories of time. I have chosen not to include them in this summary, as van den Berg's conclusion concerns only the epistemological side of his paper, which I therefore take to be its core. Of the two types of common notions—those based on sense perception and revealing accidental features *versus* those resulting from contact with the intelligible and revealing the essence—it is the former which we will find most of all in philosophy of nature and their function may be to test the validity of our theories.

Interesting issues that remain after reading this paper are the relation between the common notions of images (e.g., of time, the image of eternity) and those of their paradigms (eternity),[9] and the nature and function of sense-derived notions of things of which there is also an intuited notion (e.g., eternity according to Proclus).

Christian Wildberg gives us a clear and beautifully written description of Plotinus as philosopher of nature, paying special attention to the philosopher's cryptic remarks on 'the contemplation of nature'. Starting from the first lines of 3.8 [30], Wildberg sets out to show that Plotinus is not the hardcore metaphysician that he is made out to be but also a natural scientist—albeit an idiosyncratic one and, therefore, hard to interpret. The heart of his theory of the natural world is that it is not a realm in its own right, but 'the external and derivative aspect of an ideal world'.[10] It is physically manifest

[9] Cf. also 3.7.1 where Plotinus suggests that our understanding of eternity will help us to understand its image.

[10] Cf. the papers by Karamanolis and Russi.

as opposed to spiritually productive. The images and traces of the transcendent that the natural world contains are due to Nature's engagement in quiet contemplation of real entities.

Wildberg analyses this aspect of Plotinus' philosophy of nature not only for its content but also for its methodology, by pointing out how Plotinus prepares the reader for the notion of non-rational contemplation. After 'playing around', as Plotinus calls it, we understand that contemplation involves 'the beholding of a concept or an intellectual reality in such a way that one aims at, and gradually succeeds in, understanding it' [128]. When applied to Nature, such contemplation would probably have to be non-propositional, and a combination of introspection and expression—i.e., action. There are many natural things which in their actions actually aim at contemplation. According to Wildberg, Plotinus 'boldly asserts' that all the actions of human beings, too, are a form of contemplation.[11]

With regard to the 'somewhat obscure' last lines of 3.8.1, Wildberg addresses some problems of the notion of Nature's contemplation after giving a convincing analysis of the Greek. He discerns in this passage the answer to four different questions, the most interesting of which relates to the distinction between two kinds of ϑεωρία, one which Nature does and one which it does not have. The activity of Nature, like any activity, both begins and ends in contemplation. As an artist starts out with a plan, so Nature starts from λόγοι or forming principles, the activity of which is a kind of ϑεωρία appearing as perceptible phenomena. The 'ontological quantum leap' from the mental to the phenomenal, Wildberg maintains, is facilitated by matter. How that works, however, unfortunately remains rather implicit in Wildberg's analysis.

The question of the role of the obscure 'contemplation nature does not have' turns out to be fairly simple: nature's own contemplation (i.e., the contemplation which it does have), is inferior to another kind of contemplation which is causally involved in the former's coming to be. As Wildberg points out, the presupposition here is that action is the by-product of or weaker substitute for (real) contemplation.

---

11 What Plotinus actually says, however, is that this is *probably* the case (κινδυνεύει) [*Enn.* 3.8.1.13].

Wildberg ends by pointing out that the view of the mental or conceptual as necessarily evolving to something external to it, an expression of itself, explains that the phenomenal world can be understood as a lower variety of a higher contemplation. Such a 'world of thoughts', Wildberg proposes, could be considered 'the apex of Greek speculation about nature', as completely doing away with a true material substrate.

An aspect of this paper that bothers me is the 'quantum leap' from the mental to the phenomenal. Either there is a quantum leap or there is a world of thoughts. Since matter is none other than a correlate to form and a necessary consequence of the process of emanation, I guess that we should say that for Plotinus there is in fact no quantum leap between the intelligible λόγοι and the form-matter aggregates which make up the phenomenal world: the latter are merely lower forms of the former.

In Chiara Russi's paper, there is something of a discrepancy between the professed aims and what is actually delivered: Russi wants to show that the hypothesis of a smooth progression of a homogeneous Neoplatonism from Plotinus to Proclus is untenable, that Plotinus' Platonism constitutes an integral and consistent theory, and that Plotinus occupies an exceptional position in the Neoplatonic tradition. In order to reach these ambitious aims, however, she takes Proclus as the paradigm of the Neoplatonic tradition as a whole,[12] and systematically shows with respect to different aspects of specific—but crucial—elements of his thought that these principles cannot be found *in nuce* in Plotinus. Russi focuses on productive causality, nature and fate, and space and physical objects. Despite the fact that her analysis does not allow her to support her strong claims, and despite the fact that I do not always agree with her, Russi does present an interesting comparison of Proclian and Plotinian causality. Her conclusions regarding Plotinian causality are:

(1) Plotinus' 'dualism' is not one of formative principle and substrate but of increasing separation of the activities of an immaterial cause;

[12] Although we do find some references to other Neoplatonists in the section on space.

(2) production is a necessary precondition of actualization of power, not a perfection; and
(3) sensible objects have a phenomenal, non-ontological nature.

The most interesting part of Russi's paper is the section on 'the interaction of *dunameis* in the generation of living beings'. In causal processes in general, including those constituting the physical world, Proclus distinguishes two kinds of δύναμις: an active, perfect one and a receptive, imperfect one. Plotinus also distinguishes two potencies in the constitution of the natural world but they are both active, the difference being that one of them is more universal. Secondly, for Proclus, the emanations of the highest causal principles provide the substrate for emanations from lower principles—Russi forgets to mention here that those lower principles emanate from those same higher principles—whereas, for Plotinus, the producing causes are always the lowest in the ontological hierarchy. And finally, according to Proclus' causal principles, the most universal proceeds first and dries up last, whereas the most specific causal agent proceeds last and dries up first; for Plotinus, instead, the lower and more partial potency remains longer.[13]

Russi concludes that whereas in Proclus we find a dualism in monism (in the combination of a substrate and an ordering principle), in Plotinus we find instead a productive principle which comes forth from a connective principle (i.e., connecting the product with its transcendent cause).[14] Moreover, for Proclus, production is a sign of power (causes can produce) but for Plotinus it is merely a lower necessity (causes cannot not produce).

Russi's discussion of Nature and Fate in Proclus and Plotinus is somewhat disappointing.[15] It oversimplifies Proclus' problematic notion of Nature [see Martijn 2010] and the relation between Nature and Fate.

The main differences between their notions of Nature and Fate, according to Russi, are that, for Proclus, Nature is a hypostatic level, whereas, for Plotinus, it has no formal content or self-apprehension.

---

[13] I find Russi's argument [154] for this opposition unsatisfying as it obfuscates a distinction between the formedness of (dead) matter and the identification of a dead body as either what it was when alive or mere matter.

[14] Russi [161] describes this as the separation of two formerly united faculties.

[15] On these topics, see the paper by Linguiti in this volume.

Further, for Proclus, Fate is identical to Nature and coordinates bodily beings in their own realm while connecting them to a higher realm; whereas, for Plotinus, Nature cannot reconnect lower being with the higher or endow it with 'horizontal' order and is, moreover, not identical to Fate, which instead is the name of external causes.[16]

In the section on space (also place), Russi concludes that, whereas for Proclus space is a formal and active potency, and even a kind of body or vehicle[17] which serves as an intermediate between the intelligible and the sensible, for Plotinus space is merely a consequence of the 'existence' of bodies. Moreover, Plotinus does not consider space to be an active potency or something which receives the forms but as something that merely reflects them.

A problem in this section, which to some extent pervades the paper as a whole, is that Russi overemphasizes the supposed dualism of Proclus by maintaining that the substrate is an antagonistic factor in the causal system. This may be inspired by the image of Necessity limiting the Demiurge's options in the *Timaeus* but is otherwise a bit too Gnostic. Like everything else, Proclus takes the disorder of the substrate to be an emanation from higher principles—as we read in Gerd Van Riel's paper in this volume.

The same tendency is found in the conclusion, where Russi states that 'behind Proclus' monism lurks a dualism of antagonistic δυνάμεις'. Instead of 'antagonist[ic]', perhaps 'incompatible' would have been better: as Russi herself points out, the sensible world cannot be directly formed by the intelligible. Mediators are required (nature, place) to bring the two together but both ultimately derive from the

[16] Note that the texts adduced in favor of this interpretation of Plotinus are not apposite: the texts on p. 163 concern the plurality of causes and the immanent λόγος *versus* external influences, where the latter are not identified with Fate. Moreover, in the text on p. 164, fate (or actually destiny, which is the translator's choice for «εἱμαρμένη») does occur but is importantly in a qualified ('perhaps') and *conditional* ('at least for those who think') statement.

[17] Russi here refers to Simplicius, *In cat.* as stating that Proclus calls space the 'first body'. This cannot be right. *In cat.* is probably *In phys.*, where, however, we do not find such a remark. Maybe she's thinking of *In phys.* [Diels 1882–1895, 616] on the first corporeal cosmos.

same source. For Plotinus the sensible world is more emphatically the product of three external activities of one and the same essence.

One of Russi's important conclusions is that due to the fact that physical entities are the perfections of their causes, Proclus can maintain his interest in physical entities as such. The same, I think, can also be said of Plotinus, if we replace 'the perfections of' with 'lower manifestations of'.

If anything, despite itself, this paper points out mainly that, yes, the differences between Plotinus' and Proclus' notions of causality are many and, no, they are not merely skin-deep but are also too subtle to speak of a 'deep opposition' between the two thinkers. The most interesting distinction between the two is probably that between Proclus' symmetrical system in which the lowest effects result from the highest cause and Plotinus' pyramidal structure in which every level causes only the next. But against Russi, I should like to maintain that Proclus is no more of a dualist than is Plotinus.

Alessandro Linguiti addresses the complex relation between Fate and Nature, Necessity, and Providence in Neoplatonism, taking his cue primarily from Proclus' *De providentia* and subsequently from *Theologia Platonica* and *In Timaeum.* Neoplatonic Nature is a somewhat problematic entity, as (by the principles of 'vertical causation') it has to transcend its product, the natural world, and yet it cannot be transcendent because it is intimately connected to the corporeal. Platonic Nature is a universal principle which presides not only over individual entities, but also over the physical world as a whole. Whether it is transcendent or not does not become entirely clear in this paper, but it does not become entirely clear in Proclus' writings either [cf. Martijn 2010, esp. ch. 2].

In many cases, Neoplatonic (but also Peripatetic) Nature seems to be identified with Fate as 'a universal principle ruling the whole of the sensible world' [175] as well as single events, but not exceptional ones. Linguiti states that, in a Stoic fashion, in Proclus' *De prov.*, Fate is the transcendent cause of the connection of events. However, as is clear from his sequel, Fate does not in fact connect events (as the term εἱμαρμένη suggests) but *bodies.* That is, it not only moves them but also maintains their constitution and binds them together into a whole. Fate is, thus, very similar to Nature.[18] Further support for this reading of Proclus is found in his dealings with Peripatetic material. Although Proclus uses Aristotelian terminology and equates 'against Fate' with 'against Nature', he criticizes the Alexandrian theory that Fate is either individual soul/nature or the revolutions of the cosmos. According to Proclus, neither is powerful and encompassing enough.

Providence is closely related to Fate but not identical to it: as it is directly related to the Good, Providence is superior to, and in fact the paradigm of, Fate. The Necessity of the *Timaeus*, on the other hand, should not be identified with matter or the goddess of *Resp.* 10 but is instead what Sebastocrator calls 'natural necessity', and as such is identical to Fate.

When we turn to the *Platonic Theology* and *In Tim.*, matters become a bit confusing. Linguiti shows that in these works Proclus takes Fate to exceed Nature and not be identical to 'Nature simpliciter'. Instead, it is qualified Nature, where the qualifications emphasize the divine essence of Fate: 'Nature in its proper divine manifestation'. Linguiti proposes that they are one reality viewed in different ways. He goes on to adduce further evidence in support of the claim that Nature and Fate (and natural necessity) are identical, namely, Proclus' description of Nature transmitting properties to bodies in a way that recalls Alexander's description of Fate. Apart from ordering the natural world, Nature transmits properties to bodies which will manifest themselves in those bodies in a manner appropriate to the ontological level in question, namely, as secondary properties.

This difference between how properties appear in cause and effect leads to Linguiti's conclusion, which resembles those of Russi and Wildberg: the natural order is not independent of the transcendent causes. Instead, 'it is a necessary aspect of the divine order in its corporeal appearance.'

Jan Opsomer's paper on Proclus' theory of motion is a very rich piece of work. Besides an analysis of the relevant material, mainly from the *Elem. phys.*, Opsomer also presents a critical evaluation.

---

[18] An interesting aspect of this theory is that due to the analogy between macrocosm and microcosm, the cause of our being a unity, our soul, can also be called Fate.

What shows most of all from both Opsomer's analysis and evaluation is that Proclus uses predominantly Aristotelian material in his theory of motion but that his arguments rely heavily on implicit Neoplatonic metaphysical principles.

As Opsomer shows, Proclus combines the Aristotelian and Platonic solution to the infinite regress of motion (something setting something else in motion): the notion of an unmoved mover and the notion of self-movers, thus obtaining a series with unmoved mover, self-mover, and externally moved.

The movers are not limited to the physical realm, as in Aristotle; but instead, as in Plato, we find motion or dynamism also in the spiritual realm (soul and up). Proclus does, however, accept Aristotle's theory of motion for the physical world.

Both sides show in Proclus' combination of an efficient causal role for the first mover on the one hand, and the necessity of continuity in space and time for motion to be possible on the other. The first efficient cause must be incorporeal, so there must be active motion in the intelligible. But is there also passive motion? And continuity?

From the first book of the *Elem. phys.*, we gather that in order for movement to be possible, time, space and body have to be continuous. So, the quantitatively indivisible is unmoved. Moreover, everlasting motion can only be topical and circular. In the second book, Proclus opposes this divisibility with the indivisible and unmoved first efficient cause of motion—establishing the necessity of which is the aim of the *Elem. phys.* Opsomer clearly shows that the arguments which Proclus adduces are flawed to the extent that they rely quite heavily on a number of tacit assumptions and, as Opsomer points out at a later stage, the method chosen in the *Elem. phys.*, i.e., the deductive method, should exclude tacit assumptions. Opsomer also discusses a number of other objections that one could make against Proclus' arguments and answers most of them. The core of these objections is what I would call a 'physics' version of the biggest Platonic problem: the gap between the transcendent and the immanent or, in this case, the puzzling relation between an infinitely powerful source of motion and the limited capacities of the universe. Both the tacit assumptions and the answers to possible objections show that Proclus' account of Aristotle's kinematics reveals a strong presence of Neoplatonic metaphysics. That Proclus nonetheless bases his account of motion on Aristotle, Opsomer explains from the fact that Aristotle simply gives a fuller account than Plato.

A notable difference between Aristotle and Proclus is the prominence in the latter of the self-movers, which he uses among others to argue the difference between intellects and souls. There are problems involved in this argument, among them that Proclus does not prove that a self-mover cannot be the beginning of a causal chain and that the necessity of self-movers is not argued for. Or, more generally, there is the problem that we now have two efficient causes of motion. Opsomer proposes as a solution, not that self-movers are moved extrinsically by the unmoved mover (because souls need to be autonomous) but that they depend on higher principles for their existence. And at a later stage, Opsomer comes up with an ingenious argument for the necessity of self-movers: since they are the first moved entities in the hierarchy, they are the origin of the passive capacity of being moved—where in the case of the soul, which is indivisible, passive motion cannot be locomotion but seems to be the activity of thinking. The soul is indivisible in its essence but divisible and moved in its activities.

Combining the notions of moved by another, moved by itself, unmoved, self-mover, mover of others, and non-mover, Proclus reaches a hierarchy consisting of:

(1) the unmoved movers (intellects),
(2) the primary self-movers (souls), secondary self-movers (ensouled bodies),
(3) things moved by another and also moving others (enmattered forms), and
(4) things moved by another but not moving others (bodies).

An interesting element in this hierarchy is that of the enmattered forms or qualities which are moved from without and move bodies. Sometimes, this category is equated with Nature, the source of the φυσικοὶ λόγοι. According to Proclus, when body A hits body B, it is not A but the incorporeal qualities of A which move B.

The most problematic part of the hierarchy, obviously, it that of spiritual motion, i.e., the motion of everything which is not divisible. In order to explain how we are to understand that, Opsomer presents a section on mathematical being that is at first sight irrelevant.

Mathematical being, which is the soul's way of grasping higher objects, is intermediate between divisibility and indivisibility: geometric figures, for example, are not extended but they do have shapes. So, it is possible to distinguish parts of figures such as lines, and so on—I take it this is more than a merely external conceptual division—but the figures as such are indivisible in form. Likewise, mathematical being is unmoved in that it is invariable but moved in that figures can be generated by what Proclus calls a 'living motion', e.g., of lines. Moreover, there is a whole hierarchy of geometrical figures, with material artifacts at the low end and the 'hidden' figures of the gods at the high end.

When it comes to the geometer's practice, extension and, with it, divisibility are required, in order to bisect a line, for instance. That extension is available in our φαντασία, where particularized and pluralized images of the figures are produced.[19] It is clear how all this is relevant for Proclus' analysis of motion: since geometrical objects have their existence in soul, we may now understand better how divisibility and extension work on a spiritual level.

There is no mathematical continuum in the rational soul or the intellect. Nonetheless, Proclus ascribes—apposite kinds of—motion to these levels. On the basis of the first hypothesis of Plato's *Parmenides*, Proclus distinguishes alteration, which concerns internal changes, and locomotion, which concerns external relations. Interestingly, Opsomer shows that both kinds of motion are ascribed to both soul and intellect. Souls alter when they assimilate to the intelligible in their activities and they experience locomotion when moving about in intelligible space (whatever that is). Intellect alters in that it comes to participate in the intelligible and once actual it prefigures sensible motion κατ' αἰτίαν. In other words, the notion of motion is stretched by Proclus in order to cater for spiritual motion, which is beyond time and space but does require some kind of (discrete) multiplicity.

Spiritual motion, Opsomer concludes, is the cause of physical motion but is essentially different from it due to the absence of an isomorphic continuum. This conclusion is warranted, I think, but

[19] It seems that different kinds of motion are involved in geometry, then: the motions of the figures themselves on the intellective level and the motions of the geometrical operations on the level of φαντασία.

at the same time disappointing. Is Proclus' analysis of motion in the spiritual realm a mere scholastic exercise? Or if not, does the 'essential difference' between physical motion and spiritual motion reduce the continuity between the two realms to one of equivocity?

Some of the most puzzling concepts of Plato's cosmology are those of necessity and the disordered substrate. In his lucid yet complex paper, Gerd Van Riel discusses the interpretation of these concepts found in Proclus, who, along with Syrianus, Damascius, and Simplicius, maintained a literal reading of Plato's receptacle and its pre-existent traces of the elements [*Tim.* 52b–53d], and linked it with the 'necessity' introduced at *Tim.* 47e–48a. Van Riel asks how, according to Proclus, the ordering divine agent and the disordered substrate interact, how divine order translates into laws for the lower realm, what the substrate looks like, and what necessity it imposes on the 'legislator'. The result is a hierarchy of the lowest parts of Proclus' world, which turns out to have an unexpected analytic complexity. By delivering this, Van Riel offers a convincing justification for Dodds' suggestion that Proclus' reality is symmetrical: its bottom mirrors its top in structure.

The first notion analyzed by Van Riel is that of necessity. Damascius ascribes to Proclus three kinds of necessity: divine, material (both categorical), and aim-directed necessity (hypothetical, i.e., if you want to reach aim x, y is necessary). Van Riel points out that there are more kinds of necessity and that Proclus' notion of material necessity is more problematic than it might seem.

Although Proclus accepts Plato's characterization of the disordered lowest level of reality, he does not identify formed matter with ἀνάγκη. On the other hand, he does locate ἀνάγκη in the substrate as the lowest element of a chain of Ἀνάγκη, which starts from the 'mother of the three Fates'. Van Riel suggests that this move aims at preventing a dualistic reading of the *Timaeus* (which I think is a more plausible reading of Proclus than Russi's).

Material ἀνάγκη works as follows. The dynamic of the substrate determines which power is needed to keep it under control. The substrate itself does not actually do anything other than being passive, i.e., not easily 'persuaded' by form. It is, thus, primarily a weakness, which however also implies a receptivity.[20] Dualism is again prevented because material necessity is by nature receptive to operations from higher realms.

When we take a closer look at the substrate, it appears to fall into a stratification of many different kinds. As opposed to matter (ὕλη) as the substratum of forms, i.e., which is already formed, the receptacle is entirely without determination. In between these two, Proclus moreover distinguishes 'the visible' or 'corporeal' which 'has received traces' and the 'second substrate'. To find out which substrates these are Van Riel turns to a passage from *In Parm.* in which Proclus describes the different kinds of πέρας and ἀπειρία. The relevant kinds of lowest ἀπειρία (matter) are:

(2) body without quality or the first extended thing,
(3) qualities and
(4) genesis.

Of πέρας, most interesting are:

(7) permanence due to enmattered forms,
(8) material quantity,
(9) body without quality [cf. ἀπειρία (2)], and
(10) enmattered form.

Van Riel suggests that what Proclus calls 'the second substrate', the ἄποιον σῶμα or three-dimensional substrate of elementary qualities, (which is just above the completely unqualified substrate and just below the layer bearing traces of the forms) is ἀπειρία (2) or πέρας (9). This 'second substrate' is not without properties altogether, as it does have οὐσία, εἶδος, ἑτερότης (dimension), and ταυτότης (continuity), κίνησις, and στάσις. That is, Proclus ascribes the Sophist's greatest kinds to it and identifies it with the 'discordant and disorderly moving thing'—but not with ἀνάγκη.[21] The traces of the forms

[20] The analysis is not entirely clear here: it seems that Van Riel takes the necessity to be the lack of and need for form, though at times he sees it instead as the resistance to form [e.g., 239]. In itself, this combination is not problematic. However, the resistance (or devouring or corrupting, as Van Riel 2001, 132 terms it) does not sit well with the passivity of matter—unless we take them to be mere metaphors for the receptivity's being limited.

[21] Van Riel locates ἀνάγκη below the second substrate but his arguments are not very clear [245].

are to be located in 'the visible', i.e., the third substrate, which has body and qualities. This is the last unordered level before the forms enter.

One might wonder whether these levels aren't merely conceptual distinctions as opposed to ontological layers. Van Riel shows, however, that the different substrates[22] are all engendered and, moreover, engendered by different non-physical causes—causes which transcend the Demiurge. For example, where pure matter is caused by 'the Father alone' (One Being), the third substrate is caused by the 'Father and Creator' (third intelligible triad). Each of these causes has its own *modus operandi*, which Van Riel [251] presents in an insightful scheme.

As Van Riel points out, two things are noticeable about that scheme. First, the second substrate is not included by Proclus in his overviews of the causes and substrates. Van Riel suggests that Proclus subsumed it under the causative power of the first Father. Yet, if it has no non-physical cause 'of its own' and given that that was the criterion for being an ontological layer, I would say that the distinction of the second substrate turns out to be merely conceptual. Second, matter is shown by Van Riel to be brought about not only by the ἀπειρία of the One Being (which is responsible for its potentiality), but also by the ineffable ἀπειρία that is beyond the first intelligible being (and is responsible for its utter indeterminacy).

The hierarchy as a whole shows that for Proclus reality consists of an ontological order in which the lower reflects the higher, reversing the order and with decreasing generative power. Proclus builds this order starting from the *Timaeus*, adding the greatest kinds of the *Sophist*, πέρας and ἄπειρον of the *Philebus*, Plato's criticisms of monism, and the hypotheses of the *Parmenides*, and finally some nuances of his own(?) invention.

At the end of his paper, Van Riel briefly addresses the question why Neoplatonists felt the need to hold on to Plato's confusing account of matter. The answer, he proposes, is that Aristotle's view of

[22] Van Riel also speaks of 'stages of the material substrate' but it is not clear how 'stages' is to be understood here.

matter is not straightforward either, as it does not explain receptivity to form—in other words, because Aristotle does not have a clear theory of *prime* matter.

Carlos Steel closes the volume, very appropriately, with a paper on Proclus' veneration of the Earth, a topic which, before reading this volume, may seem an oxymoron. His main focus is Proclus' interpretation of Timaeus' hymnic description of the Earth in *Tim.* 40b–c. He starts from the interesting paradox that, although the Earth is not a heavenly body, it is nonetheless traditionally a divinity in Greek culture.

When studying Proclus' reading of the hymn, we have to take into account that he supposed Timaeus of Locri's treatise to have been Plato's source. Steel's brief juxtaposition of the relevant passage of the treatise with that of the *Timaeus* shows that the Neopythagorean Timaeus was more interested in the veneration of Earth as the most divine element than in the Earth as the center of the cosmos. Although Proclus focuses primarily on the latter, the distinction between element and sphere is not always clearly maintained.

For Proclus, there are many reasons to praise the Earth as the most venerable god 'within the heaven', despite its mass and compactness: for example, its creative power and its position in the center of the cosmos. Like the heavenly bodies, the Earth is praiseworthy as a living being, with a visible body, an ethereal body, a divine soul, and a transcendent intellect. Because particular living beings have these properties, the Earth must have them *a fortiori*. The most interesting aspect of Earth's being a living being, in my view, is its vital force, which shows in the variety of organisms it sprouts and harbors and explains what looks like spontaneous generation.

From among the predecessors in venerating the Earth, Steel highlights Plotinus, who also identifies a 'vital rational principle' at work in the Earth, a rational soul, and an intellect. The latter two he identifies as Hestia and Demeter. Proclus, in turn, picks up on this unexpected theological remark in Plotinus and elaborates on it by discussing the different divinities of the Earth and by describing the Earth as a corporeal counterpart of the monad of Being.

Steel discusses several other issues related to the Earth, such as the question whether it moves around its axis (Proclus says it does not). He also considers the Earth's role as guardian, as maker of night and day, and as nurse ‘perfecting our intellect’—a function which unfortunately remains quite cryptic.

In a very interesting section, Steel presents the ‘symphony between Earth and heaven’, which consists in Earth’s providing a center for the celestial spheres, in the winds and rains as intercourse between heaven and Earth, in the four elements’ being present everywhere ‘in the appropriate manner’, and in Earth’s spherical shape. It is this role of Earth and its cooperation with heaven which make it ‘the most venerable of all the gods within the heaven’.

By way of conclusion, Steel summarizes the general properties of the Earth which make it a divinity, as well as the particular chthonic divinities which live within the Earth. The harmony between heaven and Earth, as Steel shows by quoting Proclus’ *Sacrificia*, is what makes theurgy work.

Two questions that deserve some further attention, I think, concern the nature of Earth’s ethereal body and the influence of Earth’s soul and intellect on us. The ethereal body (or vehicle) connects the visible body of the Earth with its divine soul [266, on *In Tim.* 2.135.8–23] and animates the Earth. This means that it cannot be an ordinary body. Instead, it has to be either a kind of soul or perhaps nature. As for the influence of Earth on us humans, the soul of the Earth perfects our souls and the intellect ‘arouses’ (not ‘perfects’, *pace* Steel) our intellects. What does that mean? Steel’s suggestion that this could be a reference to the gods as teachers, a point mentioned by Plato in the *Menexenus*, deserves elaboration. The role of intellect is more easily explained than that of soul in this case. As Proclus says elsewhere [3.136.28], Earth sets our intellects in motion. This could mean that our intellects are aroused to curiosity for all that lives and grows on the Earth and are thus provided with a starting point for learning. Perhaps Proclus is thinking of *Tim.* 44b–c, which speaks of the right food (ὀρθὴ τρόφη) for the soul. Once we are fully grown and have undergone the influences of physical nourishment, it is time to reestablish the circular movement of the soul through ‘the right nourishment of education’ (ὀρθὴ τρόφη παιδεύσεως). Proclus explains this as πολιτικὴ ἀγώγη which perfects the natural capacities of the soul by providing (metaphorical?) nourishment for the irrational soul, so that it will obey the rational, and philosophical education for our intellect.

Conclusion

Although it is in the nature of conference volumes to present as a unified whole quite diverse material, this volume is a pleasant exception in that the papers both cover an extensive range of topics and authors, and display a thematic unity[23] without sliding to the other extreme of overlap and redundancy. Despite its length, this review cannot do justice to the amount of detail and analysis presented in the volume.

Apart from the comments on the separate papers, mentioned above, I have some small nits to pick with the volume as a whole. It could have benefited, I think, from some more explicit interaction between the papers which address the same or related topics—but of course the process of editing does not always allow such interaction. Also, it is something of a pity that, in a predominantly English volume, the first two papers are written in French. However, fortunately both Rashed and Chiaradonna have a very clear style. And finally, in such an expensive volume, one should expect flawless copyediting.[24]

In general, however, this is a very valuable volume, which contains a wealth of interesting material and, even if one may not agree with everything in it, a great number of thought-provoking discussions. With one or two exceptions, the papers presuppose familiarity with Neoplatonic philosophy and will, therefore, be suitable reading primarily for specialists. Them it will serve well both to give an impression of the richness of Neoplatonic philosophy of nature and to elicit further discussion.

BIBLIOGRAPHY

Diels, H. 1882–1895. ed. *Simplicii in physicorum libros commentaria.* Commentaria in Aristotelem Graeca 9–10. Berlin.

[23] The notable exception being the paper of one of the editors: as said above, Chiaradonna's paper covers some aspects of physics (esp. theories of time) and of Neoplatonism, but its main focus is Galen's epistemology.

[24] Lists of editorial mistakes are quite pointless in reviews, so I will satisfy the pedant in me by stating that I found at least 50 typos or similar mistakes in the volume.

Martijn, M. 2010. *Proclus on Nature: Philosophy of Nature and Its Methods in Proclus'* Commentary on Plato's *Timaeus*. Mnemosyne 62. Leiden.

Van Riel, G. 2001. 'Horizontalism or Verticalism? Proclus *vs* Plotinus on the Procession of Matter'. *Phronesis* 46.2:129–153.

Divination and Interpretation of Signs in the Ancient World edited by Amar Annus

Oriental Institute Seminars 6. Chicago, IL: The Oriental Institute of the University of Chicago, 2010. Pp. viii+351. ISBN 978–1–885923–68–4. Paper $27.95. Available online: http://oi.uchicago.edu/pdf/ois6.pdf

Reviewed by
Alex Nice
University of the Witwatersrand, South Africa/
Université Libre de Bruxelles
alextnice@gmail.com

This collection of essays has its origin in the Fifth Annual University of Chicago Oriental Institute Seminar, 'Science and Superstition: Interpretation of Signs in the Ancient World' (6–7 Mar 2009). The deliberately provocative colloquium title has been toned down for publication to reflect the focus of the volume, which is primarily on the interpretation of divinatory signs (omens, extispicy, and prophecy) and not on semiotics more broadly defined. As such, the volume is a welcome addition to the growing corpus of literature devoted to divination in the ancient Near East; and some of the contributors here will be familiar names to researchers in this field.[1] However, scholars new to the field will be advised to read Manetti 1993, 1–13 for a brief but thorough introduction to divination in ancient Mesopotamia.

Though the collection is adorned with an image of the bronze model of a sheep's liver from Piacenza, classical scholars will be disappointed to discover that its locus is securely in the ancient Near East. Only two papers deal directly with Greek and Roman signs (Allen and Jacobs). No paper attempts to survey signs in Etruscan society and culture. One wonders why the editor did not choose a Mesopotamian illustration, such as the Old Babylonian clay liver in

[1] The following are useful starting points: Bottéro 1974 and 1992, Manetti 1993, Koch-Westenholz 1995, Guinan 1996, Freedman 1998, Veldhuis 1999, Koch-Westenholz 2000, Nissinen 2000, Rochberg 2006, and Veldhuis 2006.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 53–65

the British Museum, London,[2] to avoid charges of misapplied symbolism.

The collection has three sections:

(1) Theories of Divination and Signs,[3]
(2) Hermeneutics of Sign Interpretation,[4] and
(3) History of Sign Interpretation[5]

bookended by an introduction[6] and a response.[7] The overall and internal principles of organization seem relatively arbitrary. It might have made more sense to have grouped articles synchronically or diachronically by subject matter. Nonetheless, what can be learnt from this volume that might be useful for Classicists?

One of the most significant themes running through the volume is that the ways in which the omen catalogues were composed cannot be divorced from the circumstances of their composition. This is a

---

2 Western Asia Collection #ME92688.

3 Francesca Rochberg, '"If P, then Q": Form and Reasoning in Babylonian Divination' [19–27]; James Allen, 'Greek Philosophy and Signs' [29–42]; Ulla Susanne Koch, 'Three Strikes and You're Out! A View of Cognitive Theory and the First Millennium Extispicy Ritual' [43–60]; Edward L. Shaughnessy, 'Arousing Images: The Poetry of Divination and the Divination of Poetry' [61–76]; Niek Veldhuis, 'The Theory of Knowledge and the Practice of Celestial Divination' [77–91].

4 Eckart Frahm, 'Reading the Tablet, the Exta, and the Body: The Hermeneutics of Cuneiform Signs in Babylonian and Assyrian Text Commentaries and Divinatory Texts' [93–142]; Scott B. Noegel, '"Sign, Sign, Everywhere a Sign": Script, Power, and Interpretation in the Ancient Near East' [143–162]; Nils P. Heeßel, 'The Calculation of the Stipulated Term in Extispicy' [163–176]; Abraham Winitzer, 'The Divine Presence and Its Interpretation in Early Mesopotamian Divination' [177–198]; Barbara Böck, 'Physiognomy in Ancient Mesopotamia and Beyond: From Practice to Handbook' [199–224].

5 Seth F. C. Richardson, 'On Seeing and Believing: Liver Divination and the Era of Warring States (II)' [225–266]; Cynthia Jean, 'Divination and Oracles at the Neo-Assyrian Palace: The Importance of Signs in Royal Ideology' [267–276]; JoAnn Scurlock, 'Prophecy as a Form of Divination: Divination as a Form of Prophecy' [277–316]; John Jacobs, 'Traces of the Omen Series *Šumma izbu* in Cicero, *De divinatione*' [317–339].

6 Amar Annus, 'On the Beginnings and Continuities of Omen Sciences in the Ancient World' [1–18].

7 Martti Nissinen, 'Prophecy and Omen Divination: Two Sides of the Same Coin' [341–351].

common issue for Classicists concerned, for example, with the psychological pressures that might lead to an increase in the number of prodigy reports in periods of crisis or with the considerations at work in the use of divination to promote or hinder political ambitions.[8]

Several articles remark on the derivation of the omens' authority from the gods (ultimately from Ea himself) and report that their contents were only to be seen by a select few who saw themselves as guardians of knowledge. For example, Veldhuis suggests that the commentary of Summa Sin Ina on *Enuma Anu Enlil* (the text authored by Ea) provided an extra textual layer negotiating between the authoritative word of Ea and the actual practice of the diviner at the royal court.

Winitzer also considers the relationship between theory and practice. Despite the authority of the gods, there is relative silence in the texts regarding divinities. He suggests that, while there was an interest in the divine presence, the growth of writing and the need to interpret the words themselves led to less emphasis on the divine presence; in turn commentaries were required to explain revelation which itself was also relegated.

In a slightly different vein, Richardson argues against the 'autogenetic' nature of human enquiry and maintains that the second millennium texts do not presume a continuous scholarly tradition. He suggests that there was an extispical oral tradition in Old Sumerian temple-cities in the south. It was not until the 19th–18th centuries BC that the north appropriated this knowledge in deliberately crafting omen compendia and deploying liver models, all within the context of the Mesopotamian state struggles of this period. Finally, in the late Old Babylonian and Kassite period, extispicy became more widely available through school texts and in reports for individual clients. Tied into Richardson's analysis is the need for kings to utilize forms of power that they could trust. Diviners operated in the secular world, which made them ideal for circumventing the inherited power structures within the royal court whilst acting as trusted advisors at the highest levels of power. The context of the 19th to 18th century power struggles invites comparison to the last years of the Roman Republic, where one may view military dynasts such

8 See, for example, Liebeschuetz 1979, 7–17.

as Marius, Sulla, and eventually Augustus, turning to diviners who had no allegiance to the traditional priesthoods (*pontifices*, *augures*, *XVviri sacris faciundis*) to foster their political ambitions.[9]

The application of modern terminology to ancient systems of thought and belief is fraught with danger. This is especially the case with terms such as 'science', 'superstition', or 'magic'. Nonetheless, Jean argues that the ways in which divination was supported psychologically, socially, and politically in the Neo-Assyrian world implies that it was indeed a 'science' in its broadest sense. She suggests too that there was a process of negotiation that involved the king and his advisors and concerned the validity of a particular sign, which indicates a desire to come to a well-assessed conclusion and is akin to the enquiries of modern-day scientists. Similarities in this regard may be drawn with the Roman Senate's desire on occasion to request a second opinion regarding a prodigy, as in the case of the rise of water at the Alban Lake in 396 BC (*haruspex* and Delphi), or the cooperation of *pontifices*, *XVviri*, and *haruspices* in 207 BC.[10] Of course, for a Greek or Roman audience the application of the terms 'ars' ('skill') and 'scientia' ('knowledge') to divination posed no such difficulties. It was just another method by which the mysteries of the universe could be unveiled.[11]

The notion of divination as a 'science' is also broached by Rochberg. She argues that the 'tight, logical structure' of the omen lists with their protases and apodoses (if x, then y) is no less scientific than modern definitions of the term because they provide essential

---

9 On Marius and Martha, the Syrian prophetess, see Plutarch, *Mar.* 42. On Marius' use of a *haruspex*, see Sallust, *Iug.* 63–64. On Sulla's use of Chaldaeans, see Plutarch, *Sulla* 37 [cf. 5]; Velleius Paterculus, *Res gestae* 2.24.3. On Sulla and the *haruspex* Postumius, see Cicero, *De div.* 1.72 [cf. Appianus, *Bell. civ.* 1.50]; Plutarch *Sulla* 9; Cicero, *De div.* 2.65, Augustine, *De civ. dei* 2.24; Valerius Maximus, *Mem.* 1.6.4. For an overview of Augustus and divination, see Nice 2000, 88–97.

10 On the Alban Lake, see Livy, *Ab urbe* 5.15.1–16.1, 16.8–17.4, 18.11–19.2, 23.1; and Engels 2007, 365 §52 with relevant bibliography. For 207 BC, see Livy, *Ab urbe* 27.37. Again there is copious literature on the subject: Engels 2007, 470 §127. For a well-balanced discussion, refer to Champeaux 1996.

11 See, e.g., Pease 1920–1923, Krostenko 2000, and Wardle 2006 on Cicero's *De div.* for the importance of divination as a subject for serious philosophical enquiry.

clues to the worldview of the Babylonian and Assyrian scribes, and to what their concept of knowledge, reasoning, and even 'truth' was. Further on, Frahm argues how the inherent polysemy and polyphony would imbue the omen texts with additional layers of meaning. Other contributions suggest more thoroughly that omen compendia should be read as texts in their own right.

Classical scholars familiar with the pronouncements of the *haruspices* (who 'sing' their pronouncements)[12] or the *vates* of the Augustan age, will not be surprised to discover on reading Shaughnessy's chapter that there are similarities in ancient China between divination (the *I Ching* or *Zhou yi*) and poetry (the *Shi Jing*). Nor will it be a total surprise to learn that the divinatory texts use association, analogy, and wordplay; and that, although thorough analyses of these terms exist for the Sibylline oracles,[13] there is still much more that could be achieved for the worlds of Etruria and Rome.

The act of writing and standardization may have assisted in the preservation of ancient forms of knowledge but those same texts were then subject to scholarly interpretation and rationalization, as Böck points out [209]. Her thorough analysis of the physiognomic texts—the one form of divination in which signs are seen from the client's point of view rather than the interpreter's—may well offer further clues to its practice in the Greek and Roman worlds.[14]

Students of Greek and Roman divination should be interested in the significance attached to extispicy in ancient Mesopotamia. Koch tests the ground between divination and magic to consider whether extispicy might be countered by apotropaic rituals. Since, she argues, extispicy covered both information gathering and aversive rituals, there was no need for further apotropaic measures. As at Rome, sacrifice could be repeated and there was an emphasis on correct observance rather than on the person of the diviner.

---

12 See Torelli 1975 and Hano 1986.

13 See Parke and Wormell 1956, and Fontenrose 1978.

14 Barton 1995 argues that physiognomy and rhetoric had much in common. Both emphasize the presentation of character (ethos) through antiquity's cultural categories and oppositions (man, not woman; citizen, not foreigner; man, not animal).

Heeßel demonstrates that extispicy also had a stipulated time frame of efficacy (one year). Like Koch he notes that extispicy functions in both directions and so proffers 'real communication' with the gods. Scholars will be confused to learn that this makes it 'unlike other forms of divination' [163]. In fact, this makes it very *like* other forms of divination such as augury, sortition, oracular divination, even the Etruscan fulgural discipline. These all offer a two-way process in which the human world asks the gods a question and they respond with a sign written *via* the birds, the oracular lot, the Sibyl's prophecy, or a specific answer to the question 'what if it thunders on such-and-such a day?'[15] The answer is not always 'yes' or 'no' but it is certainly two-way communication.

Classical scholars conversant with Cicero might be surprised to read in the introduction that 'prophecy and divination are historically related to each other more closely than is generally assumed' [12]. Prophecy features in Noegel's comparative approach to the divinatory systems of Mesopotamia, Egypt, and Israel. He argues that the relevant texts, their language and images, allow for a fuller understanding of the performative power of divination and its effects on the promotion of certain ideological and cosmological concepts. Of the three societies, he notes the greater emphasis on orality in the Israelite tradition as the prophets themselves become signs.

The point is then taken up more wholeheartedly in the article by Scurlock and in Nissinen's response. The former argues that the Uruk and Dynastic Prophecies are prophetic texts in the same way as Nahum and Isaiah 36–37, but lack the universal appeal of the biblical narratives. While Nissinen agrees that prophecy and omen divination belong to the same symbolic universe, he argues that a distinction should be made between prophecy and omen divination. The starting point for this assertion is 'most prophets probably had nothing to do with livers of sacrificial animals or with the observation of the movements of the stars' [343]. In other words, and following Cicero, they were not concerned with artificial divination, only with natural divination. The statement is particularly problematic when applied to seers from Greece and, to a lesser extent, the Roman world.

---

[15] On augury, see Linderski 1986. On sortition, see Champeaux 1986 and 1990. On oracles, see Parke and Wormell 1956, Fontenrose 1978, Parke 1988, and Stoneman 2011. On fulgural lore, see Thulin 1905–1909, 1.1–128.

When the action moves eastwards, the seer is both intellectual and prophet, able to glide easily from the highest to lowest social classes. The blind Teiresias is a case in point but so too are the historical seers from Greece.[16] From Rome we can point to the *gens Marcia* who are not only conspicuous as priests but also implicated in a prophetic tradition which allowed their predictions to be written down and collated with the Sibylline oracles.[17]

This is an appropriate point to turn to the two articles which deal directly with the Greek and Roman worlds. Allen challenges our assumptions regarding signs which are not easily compatible with the ways in which Greek philosophers viewed them. The opening section considers the term 'sign' as often utilized in modern-day English as well as Aristotle's understanding of those inferences that allow us to know the *that* and those which help us understand the *why*. A feature of the ancient view was that experience was insufficient to understand the underlying nature of things. Real art (τέχνη) and real knowledge (ἐπιστήμη) were required for a true understanding.

After a brief summary of Sextus Empiricus (whose followers regarded experience as totally sufficient, drawing a distinction between 'commemorative' and 'indicative' signs), Allen contrasts the Stoic and Epicurean views. The Stoic viewpoint is examined *via* Cicero's interpretation of natural and artificial divination. Observation and experience offer clues to understanding the will of the gods and, as such, there is no distinction between the natural and non-natural. These clues can be comprehended along 'purely empirical lines' in that signs are not only produced by the divine will but are *intended* to be recognized by the human interpreter (normally the diviner). The Epicurean position, as outlined in Philodemus' *De signis*, might be seen to overcome the limitations on experience in the debate between rationalism and empiricism [39], since the Epicureans omit to offer a contrast between the two. Their approach is characterized by

---

16 On the multifaceted abilities of Greek seers, see Dillery 2005 and Flower 2008, esp. ch. 2, pp. 22–71. On Aristander, see Nice 2005.

17 On C. Marcius Rutilus, the first plebeian pontiff and augur, see Livy, *Ab urbe* 10.9.2. On M. Marcius, *rex sacrorum*, see Livy, *Ab urbe* 27.6.16. On the prophetic Marcius or Marcii, see Cicero, *De div.* 1.115, 2.113; Livy, *Ab urbe* 25.32.3–4. See Rüpke 2008, 787–790 for a full prosopography.

a limited grasp of the natures and causes at work in which observations reveal that things have to be as they are observed to be. Non-philosophers may find this paper somewhat heavy going unless they are familiar with the prevailing views on the interpretation of signs among the different philosophical schools.

Jacobs seeks to find traces of the omen series *Šumma izbu* in Cicero's *De divinatione* and to explain the transmission of those traces. He notes that despite Cicero's general understanding of divination in the Near East, no Classical scholar has attempted to trace these influences.

A brief study of abnormal human births in the *De divinatione* is slightly flawed by Jacobs' analysis of the phrase 'visa est' because he equates 'videri' with 'somniare' [323]. Cicero's 'somniavit se peperisse satyricum' is an accusative and infinitive construction not a 'reflexive construction'[18] and the phrase 'visa est' does not have to indicate specifically a dream. The phrase is standard in the prodigy lists of Livy and Julius Obsequens to indicate the observation of any phenomenon which *seemed* to suggest a sign from the gods.[19]

The main argument centers around Cicero, *De div.* 1.121: 'if a woman gave birth to a lion, the country in which this happened would be overcome by a foreign nation.' The similarity of this dream to others concerning Pericles in Herodotus and Plutarch reflect concerns in a series of lion omens to be found in *Šumma izbu*, but particularly 1.5.

As Jacobs notes there is no clear evidence of transmission from Near East to Rome. Nonetheless circumstantial evidence allows a more generous conclusion. The coast of Asia Minor and its Greek colonies offer an immediate starting point. It was Burkert who first argued that the Sibyl of Delphi had much in common with the 'raving women' of Babylon and Assyria, and the interconnected stories of Calchas, Amphiaraus, and Mopsus offer another East to West association.[20] A comparison of Theophrastus, Pliny, and Artemidorus on the writings of Aristander of Telmessus, Alexander the Great's legendary seer, hint at a literary transmission from Near East to

18 See Glare 1983, 1790 *s.v. somnio* 1b.
19 See, e.g., Luterbacher 1904, 44; Engels 2007.
20 See Stoneman 2011, 77–80.

Hellenistic Greece and thence to Rome.[21] A close analysis of the surviving fragments of Etruscan brontoscopic calendars, such as that attributed to Nigidius Figulus,[22] with the protasis/apodosis form characteristic of the Mesopotamian omen lists might yield a step from Near East to Rome. More persuasive, however, is the Roman tradition for the transmission of augury from Persia to Italy *via* Cybele's favorite *silenus*, the Phrygian Marsyas and his envoy Megales.[23] And in his *Antiquitates rerum divinarum*, Varro suggested that *hydromantia* and *necromantia* were brought to Rome by the Persians, and that *hydromantia*, taught to Numa by Egeria, was how Numa learnt the secrets contained in the pontifical books.[24] Furthermore, Livy's Numa receives his religion from the Sabines and they, if the myths are correct, learnt augury from Megales and acquired their 'plain living and austerity' from their admixture of Spartan blood. Would it be too far-fetched to suggest that the false association of the Greek philosopher Pythagoras with Numa may conceal the intervening step in this process of divinatory transmission?[25]

As with any collection derived from a conference, papers vary considerably in length and strength. The articles range in scope from 9 to 50 pages. Some readers may be frustrated by those articles which lack a clear or strong conclusion (Allen, Noegel, Böck, Jean, Jacobs). Nonetheless, the point of research is not only to find answers to pressing questions but also to suggest possibilities for future enquiry. In this 'age of information', it is barely possible for individual scholars to assume that they can have read all the relevant literature or have all the answers.

---

[21] See Nice 2005, esp. 90–95 with nn19, 23.

[22] See Swoboda 1964 93–106.

[23] See Silius Italicus, *Pun.* 8.502–504 for the arrival of Marsyas in Italy; Pliny, *Nat, hist.* 3.12 (Cn. Gellius) for Megales imparting augury to the Sabines. On Marsyas, his role in augury, and his significance at Rome, see Small 1982; Torelli 1982, 99–106; Coarelli 1992, 91–123; Schertz 2005.

[24] Cardauns 1976, 36 = Varro, *Ant.* 1 app. iv.

[25] See Livy, *Ab urbe* 1.18.4 (on Numa and the Sabines), 2.49 (on Dionysius). On Numa and Pythagoras, see Livy, *Ab urbe* 1.18.2; Cicero *Resp.* 2.28 ff. Penwill 2004, 39 suggests that when the Pythagorean books of Numa were discovered in 181 BC [cf. Livy, *Ab urbe* 40.29], the problem with them was that they problematically revealed Numa's Roman religion not to be Roman at all but Greek.

Although there is little of direct relevance to the Classical scholar, it should be clear that there is much to be learnt from a fuller understanding of the Mesopotamian omen literature and its relationship to the worlds of Greece and Rome: for example, in matters concerning the circumstances of composition and the complexities of the literary narratives, as well as the psychological, social, and political significance of divination (including prophecy). Then too, those articles that approach divination diachronically—Richardson on the historical development of Mesopotamian liver divination; Shaughnessy on China; Noegel on Mesopotamia, Egypt, and Israel; Scurlock on Babylonia and Israel—are essentially comparative in nature and stress the importance of understanding not only of the similarities but also the contrasts in the function and importance of divination in different cultures and in different eras. Above all, this is a volume which argues for the significance of divination as a semiotic system which should not be relegated to the realms of 'superstition' or 'magic' but which, as Peek [1991, 2] has suggested, can be viewed as the 'primary institutional means of articulating the epistemology of a people'.

BIBLIOGRAPHY

Barton, T. S. 1995. *Power and Knowledge: Astrology, Physiognomics, and Medicine under the Roman Empire*. Ann Arbor.

Bottéro, J. 1974. 'Symptômes, signes, écritures en Mésopotamie ancienne'. Pp. 70–196 in J.-P. Vernant ed. *Divination et rationalité*. Paris.

________ 1992. *Mesopotamia: Writing, Reasoning, and the Gods*. Chicago.

Cardauns, B. 1976. *Varro. Antiquitates rerum divinarum*. 2 vols. Mainz.

Champeaux, J. 1986. 'Oracles institutionnels et formes populaires de la divination italique'. Pp. 90–113 in D. Briquel and C. Guittard edd. *La divination dans le monde étrusco-italique. Caesarodunum* Suppl. 54.2.

________ 1990. 'Sors oraculi. Les oracles en Italie sous la république et l'empire'. *Mélanges de l'École Française de Rome* 102:271–302.

———— 1996. ‘Pontifes, haruspices et décemvirs l’expiation des prodiges de 207’. *Revue des Études Latines* 74:67–91.

Coarelli, F. 1992. *Il foro romano.* 2 vols. Rome.

Dillery, J. 2005. ‘Chresmologues and *Manteis*: Independent Diviners and the Problem of Authority’. Pp. 200–209 in S. Iles Johnston and P. Struck edd. *Mantike: Studies in Ancient Divination.* Leiden.

Engels, D. 2007. *Das römisches Vorzeichenwesen (753–27 v. Chr.). Quellen, Terminologie, Kommentar, historische Entwicklung.* Stuttgart.

Flower, M. A. 2008. *The Seer in Ancient Greece.* Berkeley/Los Angeles/London.

Fontenrose, J. 1978. *The Delphic Oracle: Its Responses and Operations.* Berkeley.

Freedman, S. M. 1998. *If a City is Set on a Height. The Akkadian Omen Series Šumma Alu ina Mēlê Šakin.* Philadelphia.

Glare, P. G. W. 1983. ed. *Oxford Latin Dictionary.* Oxford.

Guinan, A. K. 1996. ‘Left/Right Symbolism in Mesopotamian Divination’. *State Archives of Assyria Bulletin* 10.1:5–10.

Hano, M. 1986. ‘*Haruspex* et *vates* chez Tite Live’. Pp. 101–121 in C. Guittard ed. *La divination dans le monde étrusco-italique. Caesardonum* Suppl. 3.

Koch-Westenholz, U. 1995. *Mesopotamian Astrology. An Introduction to Babylonian and Assyrian Astrology.* Copenhagen.

———— 2000. *Babylonian Liver Omens. The Chapters Manzāzu, Padānu, and Pān tākalti of the Babylonian Extispicy Series Mainly from Aššurbanipal’s Library.* Copenhagen.

Krostenko, B. 2000. ‘Beyond (Dis)belief. Rhetorical Form and Religious Symbol in Cicero’s *De divinatione*’. *Transactions of the American Philological Association* 130:353–391.

Liebeschuetz, J. H. W. G. 1979. *Continuity and Change in Roman Religion.* Oxford.

Linderski, J. 1986. ‘The Augural Law’. *Aufstieg und Niedergang der römischen Welt* 2.16.3:2146–2312.

Luterbacher, F. 1904. *Die Prodigienglaube und Prodigienstil der Römer*. Burgdorf.

Manetti, G. 1993. *Theories of the Sign in Classical Antiquity*. C. Richardson trans. Bloomington, IN.

Nice, A. 2000. *Divination and Roman Historiography*. Ph. D. Dissertation, University of Exeter. Exeter.

________ 2005. 'The Reputation of the *Mantis* Aristander'. *Acta Classica* 18:87–102.

Nissinen, M. 2000. 'The Socioreligious Role of the Neo-Assyrian Prophets'. Pp. 89–114 in M. Nissinen ed. *Prophecy in its Ancient Near Eastern Context: Mesopotamian, Biblical, and Arabian Perspectives*. Atlanta.

Parke, H. W. 1988. *Sibyls and Sibylline Prophecy in Classical Antiquity*. B. McGing ed. London.

Parke, H. W. and Wormell, D. 1956. *The Delphic Oracle*. 2 vols. Oxford.

Pease, A. S. 1920–1923. *M. Tulli Ciceronis de divinatione*. 2 vols. Urbana, IL.

Peek, P. 1991. 'Introduction: The Study of Divination, Present and Past'. Pp. 1–22 in P. Peek ed. *African Divination Systems: Ways of Knowing*. Bloomington, IN.

Penwill, J. L. 2004. '*De integro condere*: Rediscovering Numa in Livy's Rome'. *Scholia* 13:28–55

Rochberg, F. 2006. 'Old Babylonian Celestial Divination'. Pp. 337–348 in A. K. Guinan ed. *If a Man Builds a Joyful House: Assyriological Studies in Honor of Erle Verdun Leichty*. Cuneiform Monographs 31. Leiden.

Rüpke, J. 2008. *Fasti sacerdotum. A Prosopography of Pagan, Jewish, and Christian Religious Officials in the City of Rome, 300* BC *to* AD 499. D. Richardson trans. Oxford.

Schertz, P. 2005. *Seer or Victim? The Figure of Marsyas in Roman Art, Religion, and Politics*. Ph.D. Dissertation, University of Southern California. Los Angeles.

Small, J. P. 1982. *Cacus and Marsyas in Etrusco-Roman Legend*. Princeton, NJ.

Stoneman, R. 2011. *The Ancient Oracles: Making the Gods Speak.* New Haven/London.

Swoboda, A. ed. 1964. *P. Nigidii Figuli opera.* Amsterdam.

Thulin, C. 1905–1909. *Die Etruskische Disciplin.* 3 vols. Göteborg.

Torelli, M. 1975. *Elogia Tarquiniensia.* Firenze.

——— 1982. *Typology and Structure of Roman Historical Reliefs.* Ann Arbor, MI.

Veldhuis, N. 1999. 'Reading the Signs'. Pp. 161–174 in H. L. J. Vanstiphout ed. *All Those Nations: Cultural Encounters within and with the Near East: Studies Presented to Han Drijvers at the Occasion of His Sixty-Fifth Birthday by Colleagues and Students.* Groningen.

——— 2006. 'Divination: Theory and Use'. Pp. 487–497 in A. K. Guinan ed. *If a Man Builds a Joyful House: Assyriological Studies in Honor of Erle Verdun Leichty.* Cuneiform Monographs 31. Leiden.

Wardle, D. 2006. *Cicero:* On Divination Book 1. Oxford.

Aratus: Phaenomena*, Translated with an Introduction and Notes* by Aaron Poochigian

Baltimore: The Johns Hopkins University Press, 2010. Pp. xxxi + 72. ISBN 978–0–8018–9465–7. Cloth $50.00

Reviewed by
D. Mark Possanza
University of Pittsburgh
possanza@pitt.edu

The Hellenistic poet Aratus of Soli (*ca* 315 to before 240 BC)[1] is known primarily as the author of *Phaenomena*, a poem which describes the constellations and circles of the celestial sphere, and catalogues terrestrial, atmospheric, and celestial signs for forecasting the weather. The Greek word «φαινόμενα» in its most literal sense means 'things that appear' and by extension is applied, with the definite article, specifically to celestial phenomena. The word in the title of Aratus' poem applies to both celestial and meteorological phenomena ranging from the constellation Orion to the behavior of wasps and flies; and suggests that, as objects of sense perception, they are also objects of inquiry whose behavior can be studied by human reason. Ultimately, celestial and meteorological phenomena are revealed as more than 'things that appear': the regularity and prognostic function of their appearances make them signs (σήματα), signifiers of a natural order that is itself the product of divine reason. To observe the constellations and comprehend the geometry of the celestial sphere is to perceive in phenomena a manifestation of the divine in the universe.

The astronomical and meteorological content of the poem is based on prose sources: a treatise also called *Phaenomena* by the fourth century mathematician and astronomer Eudoxus of Cnidus (*ca* 390–*ca* 340 BC), the fragments of which are collected in Lasserre 1966; and a treatise called *On Weather-Signs* attributed to the Peripatetic philosopher Theophrastus of Eresus (372/1–287/6 BC), which

[1] See Hornblower and Spawforth 1996, *s.v.* Aratus. Soli is in Cilicia, the southeastern coastal region of modern-day Turkey.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 66–87

is now available in English translation with commentary in Sider and Brunschön 2007.

Although Aratus wrote poetry in addition to the *Phaenomena*, through the accidents of manuscript transmission only this work survives. From the time of its first appearance, the *Phaenomena* was admired as an exemplar of a particular literary aesthetic whose values included refinement, sophistication, the learned deployment of allusion to the poetic tradition, and the avoidance of trite and hackneyed themes and subjects. Because it also offered ancient readers a non-technical exposition of the constellations and celestial sphere in relatively short compass, it was adopted as a kind of guidebook to the heavens and became part of the reading list in the Greco-Roman system of education. The authority of the poem in matters celestial and its use for educational purposes generated a tremendous amount of exegetical commentary. As a result of his poetic and, presumably unintended, astronomical success, Aratus enjoys the distinction of having a secure place not only in histories of ancient Greek literature but also in histories of ancient science. Whatever the shortcomings of Aratus' chosen subject from a literary perspective, (and they are a very dull cliché in the long history of the *Phaenomena*'s reception), his artistry in the treatment of the celestial sphere and the weather-signs has captured the imagination of readers down through the ages. The honey of the Muses sweetened the findings of science and the accounts of the experts to produce a didactic poem that recreates the act of observing and discovering a well-ordered cosmos. Signs of the *Phaenomena*'s enduring success are plentiful.

On a recent trip to the British Library, I saw on display in the Ritblat Gallery the first printed European star map of the northern hemisphere (produced at Nuremberg in 1515). Three individuals collaborated on the map: the artist Albrecht Dürer, who drew the constellation figures; the cartographer Johannes Stabius; and the astronomer Konrad Heinfogel.[2] Their combined expertise created a memorable portrait of the night sky that is a continuation of the Greek intellectual habit of combining myth and science in the description and explanation of natural phenomena, with stars embodied in the human and animal forms of their eponymous mythological

---

[2] For more details, see Levenson 1991, 220–221 and Kanas 2009, 138–140.

originals, as in the ill-starred family of Cepheus, Cassiopeia, and Andromeda, who is menaced by the sea monster Cetus. Although the beautifully rendered constellation figures immediately captured my attention, I also found myself studying the portraits of four men holding celestial globes, each identified by name and occupying a corner of the printed sheet: clockwise from the upper left corner, they are Aratus the Cilician; Ptolemy the Egyptian, who wrote in Alexandria between AD 146 and *ca.* 170; Azophi the Arab, i.e., ʻAbd al-Rahman al-Sufi (AD 903–986), who revised Ptolemy's *Almagest*;[3] and M. Manilius the Roman, author of an astrological poem, *Astronomica* in five books written *ca* AD 10–20. Two bona fide astronomers, Ptolemy and Azophi, and two poets, one astronomical (Aratus) and the other astrological (Manilius), make up this pantheon of celestial authorities enstarred, so to speak, in the margins of the sky to commemorate their contributions to understanding the heavens.

The inclusion of Aratus in this group is particularly noteworthy in connection with the history of the transmission and diffusion of astronomical knowledge: his image, presiding with celestial globe in hand over a sky of anthropomorphic, zoomorphic, and inanimate figures, is an evocative reminder of the long lasting influence of his poem *Phaenomena* as a functional guide to the activities of observing and recognizing celestial phenomena. In the history of ancient Greek astronomy, Aratus' *Phaenomena* is the earliest extant complete description of the constellations of the northern and southern hemispheres and of the geometrical model of the celestial sphere. Even though Aratus was himself not an astronomer, his description of the constellations and celestial sphere is embedded in our discourse of the heavens and in their visual representation. And translation has been a powerful agent in establishing and enhancing Aratus' influence as an astronomical poet from the Greco-Roman period down through the Middle Ages and into the Renaissance and modern period.

Since the invention of printing in the 15th century, the field of Aratus-translation has not been a populous one, especially in English. The subject matter clearly presents a greater challenge to a writer's talents than combats on the windy plains of Troy or stratagems to defeat the appetites of the Cyclops or Circe. Even in the tame activity of linguistic transfer, the attractions of warriors and adventurers win

[3] See Rashed 1996, 1.50.

out over seekers after truth. Apart from neo-Latin versions, only a handful of *Phaenomena* translations in French, Italian, German and English were published from the 16th to the end of the 19th century; the first English translation did not appear until 1848.

A landmark among these works is the German version by the renowned translator Johann Heinrich Voss, *Des Aratos Sternerscheinungen und Wetterzeichen*, published in 1824. This work, with Greek text and facing page translation (which corresponds to the Greek line-by-line) with extensive explanatory notes and an *index verborum*, remains an important contribution to scholarship on the *Phaenomena* and is still a valuable guide for students of the poem. Interestingly, Voss originally intended to translate the *Phaenomena* into Latin but felt that the result was no better than a dry report of the original. In the end, he decided on German and was confirmed in the choice when he experienced a dramatic epiphany: Aratus himself, speaking in German, called out to him and encouraged him to complete the task [1824, v–vii].

More recently, important translations of the poem have appeared. Two scholars, who have made the *Phaenomena* their life's study, published major commentaries on the poem with translations: D. Kidd [1997] (in English) and J. Martin [1998] (in French). These translations are an essential reference for the interpretation of Aratus' Greek. Before the publication of Kidd's edition, readers looking for an English version of the *Phaenomena* turned to that trusty standby, G. R. Mair's translation 1921 in the Loeb Classical Library, which has given many years of good service and contains two fold-out star charts. I will have more to say on English translations of Aratus later in my review.

Now Aaron Poochigian continues this tradition of translation, a tradition that goes back to Marcus Tullius Cicero (106–43 BC), into the 21st century with a rendering of the *Phaenomena* into rhyming iambic pentameter couplets. The translation comes equipped with an introduction of 23 pages [ix–xxxi]; an appendix on 'Constellation Risings and Settings' [39–41]; a second appendix on the Bayer designations of the individual stars in the constellations, e.g., α Virginis, [42–43]; explanatory notes [45–70] on the poem's astronomy, meteorology, and mythology; and a bibliography of works cited [71–72]. The description of the constellations in lines 25–337 is accompanied by nine illustrations: Draco with Ursa Major and Ursa Minor, Serpentarius (Ophiuchus), Virgo, Cassiopeia, Equus, Pisces, Cycnus, Orion (who stands out for uniqueness of composition), and Cetus. These illustrations of the constellations are taken from a 15th-century edition of Hyginus' handbook on astronomy and star myths[4] entitled *Poeticon Astronomicon*, which was printed by Erhard Ratdolt (Venice 1482).[5]

An inquiring student who looks at the illustration of Draco and the Bears, and assumes that it has more than a purely decorative function, may well ask why the illustration does not agree at all with the details of the text and may even venture a more difficult question: 'What text, if any, inspired the picture?' No information is given about these illustrations and their relation to the text; there is only a brief acknowledgement of source on the copyright page. The cover illustration, an impressive celestial globe, receives the same treatment: only a photo credit is given on the back cover. Similarities in the illustration of Cepheus and in the letterforms for the names of the constellations indicate that it is a 19th century globe produced by Gilman Joslin (1804–*ca* 1860).[6] Since it is a safe assumption that pictorial representation preceded written verbal description in the formation of the constellation figures, the delineation of the figures in various media and the history of their transmission deserve as much as attention as the texts which describe them and record their movements.

The introduction provides helpful information on a variety of topics: Aratus' life; the literary background of the *Phaenomena* and its relation to Hesiod's *Works and Days*, its primary poetic model; Greek astronomy and Eudoxus' *Phaenomena*, Aratus' prose source; Stoic elements in the poem; the chief characteristics of Aratus' poetry in the context of the literary values of the Hellenistic poets; Latin and medieval translators of the *Phaenomena*; and translation methodology. The material presented here will prepare readers for what to

---

4 Probably second century AD.

5 The complete Ratdolt edition can be viewed at http://www.lindahall.org/services/digital/ebooks/hyginus/ (the constellation illustrations begin on d1 *recto*).

6 Those who are interested may find more information and pictures at http://libweb5.princeton.edu/visual_materials/maps/globes-objects/globe14.html.

expect in a didactic poem on astronomy and meteorology, and will give them a sense of the poem's high reputation in the Greco-Roman world. Since Poochigian does a fine job of showing that Aratus is a highly skilled and sophisticated poet, it is surprising to read the following comment made in discussing the poem's systematic structural organization: '...Aratus strives to create the impression that he is rambling on' [xii].

In the introduction, the section 'Ancient Astronomy' [xiv–xvi] focuses on Eudoxus' *Phaenomena*, which is generally agreed to be the source for the astronomical portion of Aratus' poem [*Phaen.* 19–732], although the authenticity of the treatise has been challenged.[7] A footnote [xxix n10] seems to suggest that our source for Aratus' dependence on Eudoxus' *Phaenomena* is a statement in one of the ancient biographies of the poet, though these are late compilations and often unreliable. In fact, Aratus' adaptation of the prose treatise is demonstrated in detail by the Hellenistic astronomer Hipparchus of Nicaea (*floruit* in the second half of the second century BC) in his *Commentary on the Phaenomena of Aratus and Eudoxus*.[8] Hipparchus compares corresponding passages of both texts in order to demonstrate Aratus' dependence on Eudoxus and establishes the general principle that in those places where Eudoxus is wrong Aratus will also be wrong. On page xv, another astronomical work by Eudoxus, the *Enoptron* (*Mirror*), which was probably a revised version of his *Phaenomena*, is mistakenly called *Entropon*, whose meaning is then glossed with the phrase 'on "cyclic" astronomy'. This mistake will leave the reader baffled by the reference [xxix n10] to a statement made in an ancient life that Aratus followed a treatise by Eudoxus called *Katoptron*. 'Katoptron', which also means 'Mirror', is most likely a slip by the author of the ancient life for 'Enoptron'. As Hipparchus' *Commentary* makes clear, Aratus adapted Eudoxus' *Phaenomena*, although in certain places he also consulted the *Enoptron*. The treatment of Eudoxus would have benefited from a consideration of Dicks 1970, 151–189; and a reference to Rihll's introductory account of Greek astronomy [1999, 62–81] might have led some readers to look further into the intellectual world of these explorers who mapped and modeled the celestial sphere.

---

[7] See Martin 1998, 1.lxxxvi–cii.

[8] See Manitius 1894.

In introductions to Aratus' poem, it is customary to identify Eudoxus' *Phaenomena* as the source of the astronomical material in order to inform readers that Aratus himself was not an astronomer. Seldom, however, are readers given a specimen of what Eudoxus wrote (as reported by Hipparchus). To give an example, here is the astronomer's description of the position of Draco relative to Ursa Major and Ursa Minor:

> Between the two Bears lies the tail of Draco. The tail holds the star at its tip above the head of the Great Bear. It makes a bend by the head of the Little Bear and extends under its feet; after making there a second bend back in the opposite direction, it tilts back and holds its head forward. [Lasserre 1966, fr. 15]

What does Aratus make of this description in the corresponding passage of his poem quoted by Hipparchus? In Douglas Kidd's translation [1997],

> It [Draco] reaches over one of them [the Bears] with the tip of its tail, and intercepts the other with its coil. The tip of its tail ends level with the head of the Bear Helice [Ursa Major], and Cynosura [Ursa Minor] keeps her head within its coil. The coil winds past her very head, goes as far as her foot, then turns back again and runs upwards. [*Phaen.* 49–54]

Eudoxus efficiently follows the figure of the snake from its tail to its head in relation to Ursa Major and Ursa Minor. Aratus creates a word pattern through repetition and association; he repeats the words tail and coil and through the repetition he associates the tail with Ursa Major and the coil with Ursa Minor. Through the artful description of these constellation figures, the poem itself is revealed as a sign (and signs, astronomical and meteorological are the poet's great theme) of that cosmic order ordained by a providential and benevolent Zeus.

After comparing these two passages, the reader may be surprised at Poochigian's observation that Aratus 'shifts the point of view from Eudoxus' mathematical and objective perspective to that of the observer' [xvi]. The key difference here is not between an objective, mathematical perspective and an observer's perspective—Eudoxus and Aratus are both observers looking at the same anatomical components of three constellations—but between a straightforward prose description that gives the shape and orientation of the constellations and the poet's studied elaboration through repetition and pattern that creates a sense of order and design. Eudoxus connects the dots; Aratus paints the picture.

Given that the target audience for this book will be readers with no Latin or Greek, the purpose of the section on Aratus' Latin translators [xxiii–xxvii] is unclear, especially since readers are given no information about where they might find English versions of these Latin versions of the *Phaenomena*, which survive complete or in fragments. The discussion of a substantial quotation (in English) from Cicero or Germanicus Caesar (15 BC–AD 19) would help to illustrate for the reader some aspects of translation practice at this foundational stage of bringing the *Phaenomena* into a different linguistic and cultural environment and would reveal how reception, interpretation, and the translator's own aesthetic values have influenced strategies of translation. Germanicus, for example, transforms Aratus' unadorned, five-line list of the zodiacal constellations [*Phaen.* 545–549] into an excursus of 33 lines on the myths that explain the origins of those constellations [532–564], a considerable departure from the wording of the source text that was made in response to contemporary literary tastes and expectations. The thumbnail sketches of the Latin translators do provide interesting information about Aratus' secure place on the reading list in ancient education but will not provide readers with enough information to understand, in the case of Cicero for example, the valid but unsupported claim that he crafted 'an independent work of art' in his version of the *Phaenomena* [xxiv]. In this same section, what will readers gain from knowing that the Emperor Gordian (AD 159–238) produced in his youth a translation of the *Phaenomena* that is no longer extant [xxvi]?

I postpone discussion of the section 'Translation Methodology' until after the review of the translation. In what follows I use for comparison D. Kidd's prose translation [1997], which is an accurate and, for the most part, literal rendering of the Greek. All line numbers preceded by the abbreviation 'Phaen.' refer to the line numbers of Kidd's Greek text and the corresponding lines of his translation; the line numbers of Poochigian's translation are given separately, prefixed with the letter 'P'.

The translation gets off to a worrisome start. In the famous hymnic proem [*Phaen.* 1–18], in which the poet praises the providential and beneficent deity of Zeus, Aratus announces his theme of Zeus-given signs which make manifest the god's immanence in the world by showing humans the right times of year for the performance of agricultural labors:

> ὁ [Ζεύς] δ' ἤπιος ἀνθρώποισι
> δεξιὰ σημαίνει, λαοὺς δ' ἐπὶ ἔργον ἐγείρει
> μιμνήσκων βιότοιο, λέγει δ' ὅτε βῶλος ἀρίστη
> βουσί τε καὶ μακέλῃσι, λέγει δ' ὅτε δεξιαὶ ὧραι
> καὶ φυτὰ γυρῶσαι καὶ σπέρματα πάντα βαλέσθαι. [*Phaen.* 5–9]

> He [Zeus] benignly gives helpful signs to men, and rouses people to work, reminding them of their livelihood, tells when the soil is best for oxen and mattocks, and tells when the seasons are right both for planting trees and for sowing every kind of seed.

Zeus-given signs coordinate agricultural activities with their appropriate seasons. In Poochigian's version, however, we read:

> He deigns to give
> Signs out of kindness to remind us rest
> Must yield to work. He shows which soil is best
> For cows and which for hoes, and oversees
> Seasons for sowing seeds and planting trees. [P4–8].

By substituting relative clauses ('which') for the temporal adverbial clauses ('when') of the Greek, the translator represents Zeus as an agronomist rather than as the cosmic deity who helps humans by means of signs. The mistranslation diverts attention from the calendric significance of the constellations for farmers, which is a major theme of the poem and is given a prominent place here at the beginning. The proem also emphasizes that humans are the beneficiaries of Zeus' providential care. In *Phaen.* 1–18 the words for 'men' (ἄνδρες) or 'human beings' (ἄνθρωποι) are repeated five times; e.g., Zeus gives helpful signs to humans [*Phaen.* 5]; he is a great boon to humans [*Phaen.* 15].[9] The collective effect of these repetitions is to make clear

[9] See also *Phaen.* 1, 3, and 13.

to readers the dependence of humankind on Zeus and to stress the universality of his power. Poochigian's use of 'we'/'us' does not achieve this same effect.

Poochigian's treatment of the proem raises concerns about the accurate representation of Aratus' meaning. These concerns are not allayed by the rest of the translation, in which one finds mistakes, imprecise expressions, and unwarranted additions to the text of the *Phaenomena.* Here follow some instances where the translator fails to convey the meaning of the Greek and, as a result, may leave readers with the impression that Aratus was not seriously interested in giving a reliable account of the celestial sphere. I will cite Kidd's translation [1997] first, then follow with Poochigian's.

In the description of Draco's position relative to Ursa Minor (Cynosura), Aratus [*Phaen.* 52] says 'Cynosura keeps her head within its coil' (σπείρῃ δ' ἐν Κυνόσουρα ἔχει). Poochigian writes: 'his coils grip the neck of Cynosure' [P50]. But the coil does not grip the neck; it bends around the head.

At *Phaen.* 93, Aratus refers to Ursa Major as the 'Wagon-Bear' (ἁμαξαίης Ἄρκτου), combining its two representations. This is translated 'plow-like bear' [P89], though Ursa Major is not referred to as a plow here.

At *Phaen.* 162, Aratus locates the Goat on the left shoulder of the Charioteer (σκαιῷ ὤμῳ). In the translation, the detail of the 'left shoulder' is omitted and we find 'collar' instead [P163]. This may seem a small detail; yet it is part of the poet's attempt to give clear directions for locating constellations.

There is similar imprecision of expression at:

- *Phaen.* 232 [P235]: Orion's 'belt' (ζώνη) is rendered as 'waist';
- *Phaen.* 461 [P483]: 'the circles of the fixed stars and their guide constellations in the sky' (ἀπλανέων τά τε κύκλα τά τ' αἰθέρι σήματα) becomes 'fixed signs and consistent things';
- *Phaen.* 595 [P623]: 'forefeet of the blazing Dog' (πρότεροί τε πόδες Κυνὸς αἰθομένοιο) is translated by 'the Dog's bright feet', with the omission of the precise detail 'forefeet'.

Aratus uses the names of the winds to give compass directions. When he is giving the location of the Southern Fish, he says that it is 'exposed to the winds of the south' (ὑπὸ πνοιῇσι νότοιο) [*Phaen.*

386]. This expression is amplified into 'as if blown over the billows by a southern squall' [P398–399], an image incompatible with a fish.

Aratus introduces the long section [*Phaen.* 559–732] on the importance of observing the constellations that rise and set simultaneously with the risings of the zodiacal constellations by indicating the purpose of such observation: 'if you are watching for daybreak' (δεδοκημένῳ ἤματος) [*Phaen.* 559] and want to measure the passage of time during the night. The observer's 'watching for daybreak' is omitted in Poochigian's translation [P581–583].

The treatment of the tale of Orion's combat with the giant scorpion deviates considerably from Aratus' account. Aratus begins the story with a plea to the goddess not to be offended by the tale of violence he is going to tell—'[m]ay Artemis be gracious' (Ἄρτεμις ἱλήκοι) [*Phaen.* 637]—and explains that it is a tale not of his own telling but one handed down by the ancients. Poochigian translates '[m]ay you at last forgive him [Orion], Artemis!' [P672], a sentiment which fits neither the wrath of the goddess nor Orion's death from the scorpion's sting and his perpetual flight in the sky from Scorpio's pursuit. Aratus [*Phaen.* 637–638] gives as the reason for Artemis' anger that Orion attempted to lay violent hands on her (οἵ μιν ἔφαντο / ἑλκῆσαι πέπλοιο). In the translation, however, we find a very different explanation: Orion attempted to steal the goddess' cloak in order to give it to his host, king Oenopion [P673–678].[10]

At *Phaen.* 748–749, Aratus explains that in the course of the stars' seasonal risings the Sun overtakes all of them (ἤτοι γὰρ τοὺς πάντας ἀμείβεται εἰς ἐνιαυτὸν / ἠέλιος μέγαν ὄγμον ἐλαύνων). Poochigian mistakenly restricts the poet's reference to just the constellations of the zodiac [P801].

At *Phaen.* 771–772, in an important passage that echoes the proem's assertion of Zeus' immanence in nature and the pervasiveness of his signs, Aratus writes:

> He [Zeus] certainly does benefit the human race openly, showing himself on every side, and everywhere displaying his signs
>
> ὁ [Ζεύς] γὰρ οὖν γενεὴν ἀνδρῶν ἀναφανδὸν ὀφέλλει
> πάντοθεν εἰδόμενος, πάντῃ δ᾽ ὅ γε σήματα φαίνων.

[10] See Poochigian's note on p. 63.

The impressiveness and thematic significance of these lines are undercut by the translation:

> Everywhere immanent
> In entrails, birds, storms, stars, he helps our race
> To help itself. [P824–826]

The list 'entrails, birds, storms, stars' includes two types of signs, birds and stars, and two items that do not belong in the context of the *Phaenomena*'s signs: entrails, which are appropriate to divination by extispicy; and storms, which are a manifestation of meteorological activity, not a sign thereof. Moreover, the translator's addition of this restricted list seems oddly inconsistent with the poet's confident assertion of Zeus' presence everywhere.

Aratus' account of the weather-signs characteristic of the days and phases of the Moon [*Phaen.* 799–818] is a challenge for readers of the Greek as well as of a translation, even when they have plenty of commentary and explanatory paraphrase to help them. To give an example from this section, in *Phaen.* 805–808, Aratus first observes that weather-signs given by the Moon at specific points in its cycle are valid only for a certain number of days within the month and then begins his account with signs from the period of the waxing crescent to the full Moon:

> σήματα δ' οὔ τοι πᾶσιν ἐπ' ἤμασι πάντα τέτυκται·
> ἀλλ' ὅσα μὲν τριτάτῃ τε τεταρταίῃ τε πέλονται
> μέσφα διχαιομένης, διχάδος γε μὲν ἄχρις ἐπ' αὐτὴν
> σημαίνει διχόμηνον.

> But the signs are not all established for you for all the days of the month: those that occur on the third and fourth days are valid up to the half-Moon, those at the half foretell right up to the mid-month.

From these lines and the remainder of the section, the general rule appears to be that signs given at or near the beginning of a quarter hold good for that quarter: from waxing crescent to half-Moon, from half-Moon to Full Moon, from Full Moon to waning crescent, from waning crescent to New Moon; and then the cycle repeats itself with the waxing crescent.

What, then, will a student make of the following in Poochigian's version of *Phaen.* 806–808, especially since he omits Aratus' introductory statement in 805 that to obtain weather-signs from the Moon it is necessary to observe it at different phases of its cycle?

> The third and fourth look to the seventh phase;
> The seventh the fourteenth. [P861–862]

If I understand his translation correctly, there is a confusion of days and phases: signs given by the Moon on the third and fourth days after the new Moon are valid for the weather forecast up to the half Moon or seventh day (not 'phase') after the new Moon. What he means by 'seventh phase' I do not know. Similarly, signs given by the Moon on the seventh day are valid until the Full Moon or 14th day after the New Moon. Whatever the translator may have gained by this striving for syntactic compression in explaining the complicated topic of lunar weather-signs is lost in the riddling quality of the result.

At *Phaen.* 1091, the poet expresses the hope that 'the stars above will always be recognizable' for farmers as a sign of good weather (οἳ δ' εἶεν καθύπερθεν ἐοικότες ἀστέρες αἰεί). In Poochigian's translation, '[m]ay all the planets and the stars be clear' [P1127], the addition of 'planets' is inexplicable since they do not have a role to play in weather forecasting in the *Phaenomena*, with the exception of the Sun and Moon, which are given separate treatment.

There are places where I find myself puzzled and even baffled by Poochigian's word choice. In an important passage of thematic significance [*Phaen.* 367–385, 387–397] that illustrates the operation of human intelligence in observing and reading the signs which Zeus placed in heaven, Aratus gives an account of how an anonymous inventor devised and named the constellation figures. In Poochigian's translation the activity of the inventor is described as follows:

> Some one of those no longer living found
> A way to *lump* stars generally and call
> A group one name. [P384–386; emphasis mine]

The verb 'lump', which corresponds to the Greek ἐφράσσατ' ἠδ' ἐνόησεν ('he devised and contrived') [*Phaen.* 374], undoes utterly Aratus' reconstruction of the inventor's deliberate method of organizing proximate stars into recognizable shapes and then naming those shapes so that they could be identified repeatedly on successive nights among the myriad stars that appeared. The role of discerning and articulating an order among the individual stars, so crucial to the inventor's activity, is erased. Kidd [1997] translates:

> τά[11] τις ἀνδρῶν οὐκέτ' ἐόντων
> ἐφράσσατ' ἠδ' ἐνόησεν ἅπαντ' ὀνομαστὶ καλέσσαι
> ἤλιθα μορφώσας. [*Phaen.* 373–375]
>
> constellations that one of the men who are no more devised and contrived to call all by names, grouping them in compact shapes.

The mental activity of devising and contriving is considerably more strenuous than lumping.

Other examples of peculiar word choice include the following. At *Phaen.* 408–410, Aratus explains how the appearance of the area around the constellation Altar can be interpreted as a weather-sign given by Night:

> ἀλλ' ἄρα καὶ περὶ κεῖνο Θυτήριον ἀρχαίη Νύξ,
> ἀνθρώπων κλαίουσα πόνον, χειμῶνος ἔθηκεν
> εἰναλίου μέγα σῆμα.
>
> Yet even round that Altar ancient Night, sad for the suffering of men, has set an important sign of storm at sea

This is rendered by

> Night is an old, old crone who pities us.
> She stuffs the Altar with conspicuous
> Advice...[P423–425].

Aratus gives a more dignified picture of personified Night as ancient or primeval (ἀρχαίη) and introduces Altar and its vicinity as an important sign of storm for sailors. It is difficult to see what is achieved by describing Night as an 'old, old crone' and by replacing the *Phaenomena*'s key word 'sign' («σῆμα») with 'advice', especially when the latter is the object of the verb 'stuff'.

---

[11] i.e. constellation figures.

A little later in this same passage, Aratus [*Phaen.* 433–434] says simply:

> ἀτὰρ μετόπισθεν ἐοικότα σήματα τεύχοι
> Νὺξ ἐπὶ παμφανόωντι Θυτηρίῳ.

> behind it [Centaur] Night is fashioning recognizable signs on the radiant Altar.

This becomes

> Night like a high priest
> Sends forth distinctive signals from behind
> Her sacrificial Shrine. [P452–454]

The phrase 'high priest' is an unwarranted addition; 'sacrificial Shrine' is an odd substitution for Altar; and 'from behind', construed with 'sacrificial Shrine', is a mistake for an adverb indicating that Altar rises behind Centaur.

When Aratus says that the head of Draco

> νεύοντι δὲ πάμπαν ἔοικεν
> ἄκρην εἰς Ἑλίκης οὐρήν [*Phaen.* 58–59]

> looks altogether as if it is inclined towards the tip of Helice's tail

Poochigian expands this into

> his head appears to nod
> At Helike's tail like an assenting god.

At *Phaen.* 164, where the naming of the Goat as Olenian is explained, Aratus' high-sounding phrase 'interpreters of Zeus' (Διὸς ὑποφῆται) is replaced with 'every scholar' [164]. But scholars are learned; Zeus' interpreters are inspired.

Although Aratus [*Phaen.* 253] describes Perseus as a runner,

> ἴχνια μηκύνει κεκονιμένος ἐν Διὶ πατρί

> he takes long strides as he runs in the realm of his father Zeus

in Poochigian's translation, he becomes a walker:

[he] walks his father Zeus' property. [P259]

At P977, Poochigian's use of the word 'weathermen' suggests a special group involved in interpreting weather-signs when Aratus speaks more generally of men (ἀνδράσι) [*Phaen.* 932] who may have difficulty in determining the meaning of a weather-sign.

These examples of peculiar word choice, which I assume were motivated by a desire to create a more vivid image or a more striking expression or to make a rhyme, are, in my judgment, signs of a failure to trust the simplicity of Aratus' style.

There are fine passages of poetry in the translation. I note in particular the description of the storm-tossed sailors [P293–308] as well as the sections on wind-signs [P953–969] and rain-signs [P986–998]. In a famous acrostic, Aratus spells out the adjective «λεπτή», the first word in *Phaen.* 783, with the first letters of lines 783–787: «λεπτή» means 'thin', 'fine' or 'delicate', and in a metaphorical sense, 'subtle', 'refined'. The adjective in context describes the appearance of the waxing crescent Moon but it also has an aesthetic resonance, identifying a valued quality in the poetry itself. The acrostic is skillfully rendered into English with the adjective 'slender' [P837–843].

Poochigian's habit of attributing to the constellation figures the forms of movement that belonged to the original humans and animals, although somewhat exaggerated in comparison to Aratus' more restrained language, is largely successful. However, even after generous allowance has been made for the translator's own aesthetic intentions and the tight constraints imposed on him by his chosen form, rhyming iambic pentameter couplets, it seems to me that he too often strays from an accurate rendering of the original. When Aratus specifies the left shoulder of Charioteer or the forefeet of the Dog, or gives detailed instructions for interpreting weather-signs given by the Moon in its phases, or points out that the Altar and the area around it function as an important weather-sign for sailors, then, in my judgment, the translator is obliged to find a way of communicating that information in the target language: it should not be subjected to improvisation or free invention. A more diligent study of the resources that are available for the understanding of Aratus' Greek, for the interpretation of the poem and for the explication of matters astronomical and meteorological, and a more vigilant review of the manuscript by the press' readers would have resulted in a more accurate and reliable translation. As it is, Poochigian's *Phaenomena* will give readers some impression of Aratus' poem as a finely wrought literary work but not as a didactic masterpiece that reveals an inspired vision of the cosmos and was deemed worthy of the learned attentions of no less an astronomer than Hipparchus.

In the section 'Translation Methodology' [xxviii–xxix], Poochigian offers the following enigmatic statement about English translations of the *Phaenomena*:

> The two most frequently read translations were intended as service translations or cribs for the original Greek. [xxviii]

Neither 'service' nor 'crib' is a complimentary term. I assume he is referring to the prose translations of G. R. Mair [1921] and D. Kidd [1997]. In my own view, these versions are more than cribs. In his prose, Mair maintains a stylistic dignity, especially in the proem, with the help of archaisms and mild dislocations in word-order that is well suited to the subject and leaves one with the feeling of having been edified by a good sermon about the divine order of the cosmos. And there is an artfulness in the simplicity of Kidd's prose that gives readers a sense of Aratus' knack for the clear description of celestial topography and his strategic use of pattern and repetition in laying out the constellation figures. These two 'service' translations deserve explicit recognition.

In addition to the prose translations of Mair and Kidd, Stanley Lombardo's poetic version, *Sky Signs: Aratus'* Phaenomena [1983], should also be mentioned. Lombardo is a distinguished translator who has a gift for composing verse that reads well aloud. His loosely metrical translation accurately conveys the astronomical and meteorological information while at the same time giving readers a good impression of the economy and austerity of Aratus' style. He is not afraid to repeat words of thematic significance. In addition, Lombardo's introduction offers a sensitive appreciation of the *Phaenomena* as a didactic poem on astronomy and meteorology, as well as a meditation on the recognition and meaning of Zeus-given signs. It is regrettable that this book is no longer in print.

It is also worth mentioning that Poochigian is not alone in translating the *Phaenomena* into rhyming pentameter couplets. The first complete English translation of the poem is written in the same poetic form, John Lamb's *The* Phenomena *and* Diosemeia *of Aratus*, published in London in 1848. It is a fascinating performance. Following the method of free adaptation employed by Aratus' Latin translators, Lamb makes additions to the text, often in the form of mythological references: Taurus is identified as the 'Tyrian Bull, Europa's treacherous beast' [*Phaen.* 180]; and the Swan is

> th' adulterous bird, they say,
> That lent his fair form Leda to betray. [*Phaen.* 283–284]

These allusions to Zeus as rapist in disguise have no place in a poem that exalts Zeus as a providential and beneficent deity. But, like others before him, Lamb felt that the constellations are in need of some erotic excitement to spice up the descriptions. Lamb anachronistically incorporates into the text the names used for individual stars, for example, 'Aldebaran's fire', i.e. α Tauri [*Phaen.* 183], and 'Scorpio's gem Antar', i.e., α Scorpii [*Phaen.* 320]. The diction is a stately mix of Shakespeare, Milton and the Romantic poets. At one point [*Phaen.* 114], Lamb actually inserts a line from Hamlet's 'To be or not to be' soliloquy, with one small change, into the story of Virgo. In keeping with the conventions of the time, Greek Zeus becomes Roman Jove. And, since in his introduction Lamb stresses that Aratus and St Paul were both natives of Cilicia and that the heathen poet was quoted by the Christian apostle [Acts 17.28], it is not surprising that the Jove of the proem is hymned like the God of the Psalms: 'Our Father—Wonderful—our Help—our Shield' [*Phaen.* 14]. The general format of Lamb's book may have served as something of a model for his successors: an introduction that focuses on the life of Aratus, the translation itself accompanied by illustrations, and explanatory notes that contain generous quotation from Aratus' Latin translators, from Manilius' *Astronomica*, and from the ancient commentaries on the poem preserved in condensed and abbreviated form in the scholia.

Three more English translations of the *Phaenomena* were published in the 19th century. In *The Skies and Weather Forecasts of Aratus* [1880], E. Poste, writing in a predominately iambic line that varies in syllable count, produced a more accurate translation than Lamb and, for the most part, employs a much simpler diction, eschewing the kinds of ornament and embellishment that misrepresent Aratus' stylistic austerity. Even when he waxes somewhat Miltonic, the epic elevation seems appropriate, as in the description of Orion's encounter with the Scorpion sent by Artemis [*Phaen.* 639–642]:

> She, dashing in twain the island's central mountain range,
> From the yawning gulf sent against him far other monster,
> The Scorpion, who him struck and slew, gigantic though he stood,
> Far more gigantic; because he outraged Artemis divine.

In many ways, Poste is the best of the 19th century poetic translators. He is especially good on technical passages and his notes, with Greek and Latin quotation kept to a minimum, are efficient, helpful and interesting on matters astronomical and meteorological. In *The* Phainomena *or 'Heavenly Display' of Aratos* [1885], Robert Brown rendered into blank verse the astronomical portion of the poem [*Phaen.* 1–732] and omitted the weather-signs. Brown states that he is placing 'before the English reader a faithful translation of the poem, as distinguished from a loose and inaccurate paraphrase' [1885, 2].

In taking up the banner of faithfulness in translation, Brown was apparently responding to what he perceived as the shortcomings of Lamb's version. Poste is certainly not guilty of the charge of 'loose and inaccurate paraphrase'. Brown does produce a faithful translation to the extent that a line-for-line blank verse rendering will allow. As a sign of his fidelity to the Greek original, he translates Aratus' unadorned list of the names of zodiacal constellations [*Phaen.* 545–549] as a list without adding descriptive epithets or other embellishments, a temptation that not all of Aratus' translators have resisted. His concern for fidelity also finds expression in a useful warning he offers at the end of his introduction. After commenting on the repetitive nature of Aratus' material and the limited scope for artistic achievement in a faithful rendering of the Greek, he observes that those considerations do not license attempts to improve the content:

> But the attempt to improve facts when it is our duty to reproduce them, constitutes one of those faults, which, however common, is, when applied to ancient art or literature, almost unpardonable. [1885, 7]

Sage advice to translators of the *Phaenomena*.

To close out the 19th century, C. Leeson Prince had printed for private distribution only *A Literal Translation of the Astronomy and*

Meteorology of Aratus [1895]. Unlike Lamb, Poste and Brown, Prince produced a prose version, apparently the first in English, which, like the versions of Mair and Kidd, aims at a close rendering of the Greek and avoids poetic adornments. Prince was a physician with a strong interest in astronomy and meteorology. In fact, he had already published in 1871, as part of a larger work, *The Climate of Uckfield, Sussex*, a translation of the section on weather-signs. In undertaking a translation of the whole poem, he was motivated by a desire to set before his contemporaries what he regarded as an important document in the history of the observation of celestial phenomena and to incorporate Aratus' weather-signs into his own meteorological research and collection of weather-signs [1895, iii]. Prince, clearly regarding Aratus as an important predecessor and model, continued the tradition of recording and organizing weather-signs into categories. His translation is followed by a section entitled 'Some Remarks upon Local and Other Weather Prognostics' [1895, 53–82], in the introduction to which he writes:

> However, for many years I have paid such close attention to most of the recorded prognostics that I am justified in endorsing the expressed opinions of the Ancients respecting them, and more particularly those which have stood the test of two thousand years and upwards. [Prince 1895, 54].

There is much to be learned from these translations of the *Phaenomena* about the reading and interpretation of the poem—Is it a practical guide to the stars? A literary *tour de force* with no practical application? A philosophical meditation on the cosmos?—about attitudes towards poetic discourse as a vehicle for the communication of scientific knowledge; about the literary trends, tastes and expectations of the translator's own cultural milieu; about strategies for turning the *Phaenomena* into an English poem or prose essay; and about the role of commentary and illustrations in supporting the translation and helping the reader through the details of astronomy and meteorology. But above all, these translations show that a poetic text of 1180 lines that offers instruction in fields of knowledge rather than historical or fictional narratives presents tremendous challenges to a translator working in verse or prose. The greatest challenge to translators of the *Phaenomena* is the challenge of trusting the poet and respecting the poem.

BIBLIOGRAPHY

Brown, R. 1885. *The* Phainomena *or 'Heavenly Display' of Aratos, Done into English Verse.* London.

Dicks, D. R. 1970. *Early Greek Astronomy to Aristotle.* Ithaca.

Hornblower, S. and Spawforth, A. 1996. *The Oxford Classical Dictionary.* 3rd edn. Oxford/New York.

Kanas, N. 2009. *Star Maps: History, Artistry and Cartography.* Berlin/Heidelberg/New York.

Kidd, D. 1997. *Aratus.* Phaenomena*. Edited with Introduction, Translation and Commentary.* Cambridge.

Lamb, J. 1848. *The* Phenomena *and* Diosemeia *of Aratus, Translated in English Verse with Notes.* London.

Lasserre, F. 1966. *Die Fragmente des Eudoxos von Knidos.* Berlin.

Levenson, J. A. 1991. *Circa 1492: Art in the Age of Exploration.* New Haven/London.

Lombardo, S. 1983. *Sky Signs: Aratus'* Phaenomena. Berkeley.

Mair, A. W. and Mair, G. R. 1921. *Callimachus,* Hymns and Epigrams*; Lycophron; Aratus.* Cambridge, MA/London.

Manitius, C. 1894. *Hipparchi in Arati et Eudoxi* Phaenomena *commentariorum libri tres.* Leipzig.

Martin, J. 1998. *Aratos.* Phénomènes*. Texte établi, traduit et commenté.* 2 vols. Paris.

Poste, E. 1880. *The Skies and Weather-Forecasts of Aratus, Translated with Notes.* London.

Prince, C. L. 1895. *A Literal Translation of the Astronomy and Meteorology of Aratus, with Some Bibliographical Remarks.* Lewes.

Rashed, R. 1996. ed. *Encyclopedia of the History of Arabic Science.* 3 vols. London.

Rihll, T. 1999. *Greek Science. Greece & Rome*: New Surveys in the Classics 29. Oxford.

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

Voss, J. H. 1824. *Des Aratos Sternerscheinungen und Wetterzeichen.* Heidelberg.

The Mechanical Hypothesis in Ancient Greek Natural Philosophy by Sylvia Berryman

Cambridge/New York: Cambridge University Press, 2009. Pp. x+286. ISBN 978–0–521–76376–9. Cloth $90.00

Reviewed by
Serafina Cuomo
Birkbeck College
s.cuomo@bbk.ac.uk

In 1972, P. M. Fraser wrote that mechanics was the 'Cinderella' of Greek Hellenistic science [1972, 425]. I doubt that many scholars would subscribe to that today. In recent years, ancient mechanics has experienced a historiographical shift. On the one hand, treatises once dismissed as 'technical', such as the pseudo-Aristotelian *Mechanica* or Philo of Byzantium's *Belopoeica*, are no longer relegated to the margins of scholarly investigation. Indeed, the validity of the very category of 'marginal' text has come under scrutiny. On the other hand, the role of machines—the products and at the same time the subjects of study of ancient mechanics—is being re-evaluated. For example, in contrast to the traditional dogma of economic *blocage*, recent trends in the history of ancient economy, particularly in the Roman period, have emphasized the role played by mechanical artifacts in the growth of production and urbanization.[1] At the intersection of these two strands of scholarship, Sylvia Berryman is well aware of developments in the latter but operates within the former area of scholarship, which is essentially context-aware history of ideas. She writes:

> Although the deployment and exploitation of technology might loom large in the eyes of economic historians, the *philosophical* reception of technological devices is a different matter. [41]

The two principal questions discussed in her book are

[1] See, e.g., Wilson 2002.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 88–92

> whether mechanical theory was applied to nature, and whether mechanical practice played a heuristic role in guiding investigation of the natural world. [22]

Berryman answers both questions in the positive and thus advances two main claims. One is that there was a mechanical hypothesis in antiquity, which is distinguished from what is known as the teleological view of the world and which is at the same time not to be assimilated to materialism or atomism. The mechanical hypothesis was a third way, so to speak, which acquires clearer connotations as the book goes along. Berryman's second main claim is that the mechanical theories and mechanical devices developed from the Hellenistic period onwards stimulated many Greek thinkers, who engaged with the very specific issues raised by mechanics in order to understand the natural world better.

In order for the two claims to be sustainable, a few mistaken notions need to be dealt with and some ground needs to be cleared. Consequently, Berryman begins by sorting out the terms of the discussion and reviewing the various senses in which 'mechanics' and 'mechanical' are used in the scholarship. This is all the more necessary, since references to ancient mechanics (or to the modern as opposed to the ancient 'mechanical' world picture) have long been common in analyses of the Scientific Revolution. Berryman wants to make clear that historians of science of the early modern period often refer to a picture of ancient mechanics which does not correspond to what we find in the ancient sources but is a later construct. The book's appendix is specifically dedicated to this question.

In fact, the initial part of the volume is, almost by necessity, devoted to negative argumentation, a *pars destruens*, as it were. Berryman goes through some well-trodden territory, mostly demonstrating that common interpretations of mechanics are arbitrary or at best limited. Despite paying some dividends in terms of insights into the historiography of ancient mechanics, the first few chapters felt to me like a preamble to the following chapters, where (I thought) the real meat of the book was.

In this first part, although eschewing further discussion of the epistemic status of analogy among other things, Berryman usefully distinguishes between analogies to artifacts and mechanical analogies: 'not every artifact analogy would count as specifically "mechanical"'

[31: cf. 37]. She establishes, convincingly in my view, that mechanics as a discipline really only emerged in the fourth century BC and that mechanical devices preceded both the theories that tried to explain them and the classificatory attempts to divide and subdivide branches within the field of mechanics. Berryman also articulates with clarity the interpretation of ancient mechanics, now steadily gaining scholarly consensus, according to which «παρὰ φύσιν» is to be read *not* as being 'against nature' but rather 'beyond' or 'above' it [44–48].

With chapters 4 and 5 we finally get into the swing of things. They explore mechanics within mechanical treatises and devote particular attention to the interplay of theory and practice. We begin with the Aristotelian *Mechanica* and continue with Ctesibius (through the medium of later sources), Archimedes, Philo of Byzantium, Vitruvius, Hero of Alexandria, and Pappus of Alexandria. Many questions emerge that are remarkable for their significance to natural philosophy: for instance, the analysis of compound motion. How can one break down a movement resulting from more than one force into its causal components, while at the same time keeping the analysis within a conceptual framework organized along the distinction between motion that is either 'natural' or 'beyond natural'? And is compound motion really a composite of forces moving in different directions or does the greatest force determine the eventual effect? Again, the nature of elasticity and resilience emerges, particularly in the context of discussions about the best material for catapult springs. What makes a bundle of sinews return to its shape after they have been forcefully twisted out of it?

There are interesting insights into the role of equilibrium. The conception of balance is shown to be crucial to Hero's mechanics. Of Archimedes' balance, Berryman writes:

> [Archimedes' technique] avoids the problem of measuring an awkward quantity—an area, in this case—by setting it equal to a known quantity in the context of a balance, the archetypal device for establishing equality. ...The balance is used to give intuitive content to the notion of 'setting equal' two quantities that cannot, strictly, be weighed. [123]

Thus, she draws a neat connection between strands of research that have both been identified as crucial to Archimedes' activities.

Models of the heavens are given their own subsection; and pneumatics, its own chapter [ch. 5]. Elasticity, to be understood in the context of pneumatics and pneumatic devices, is at the heart of Berryman's interpretation of Hero's theory of matter. Chapter 6, of a more substantial size than most of the others, analyzes philosophical texts that take on board insights provided by mechanics. This is probably the central chapter, in terms of argumentation—Berryman finally applies most of what she has said so far to philosophical texts and delivers the promise of the book's title. It is true, as she says [179], that the chapter traverses a long time span, including late ancient Christian and non-Christian authors, and by necessity extrapolates passages from wider treatises and discussions; but I also thought that the result was coherent and that it will indeed stimulate further consideration of the question.

The chapter is organized thematically, and chronologically within each subsection. The themes include the already-mentioned question of elasticity and recoil, as well as the limits of indefinite proportionality; this latter forms part of the account of how weights can be moved and under what conditions. By showing that indefinite proportionality is problematic, mechanics exposes the difficulties involved in applying mathematics to physical, real-life situations and prompts and deepens philosophical reflections, in authors like Simplicius, on this complex relationship. It is almost an understatement for Berryman to write:

> [T]he evidence shows that late antique natural philosophers acknowledged and engaged with some implications of the weightlifting branch of mechanics for natural philosophy. [191]

In chapter 6, Berryman also further explores the intersections of mechanics and medicine, and mechanics and astronomy. Both are not completely novel ground but she finds some interesting things to say, highlighting for instance how Galen's explanation of the function of the parts of the body is related to his understanding of the limits of mechanical explanation [205–209]. Or again, she argues that the mechanical hypothesis provided fuel for a specifically Christian reading of design in the universe, both macro- and micro-cosmos. It is only in this, the last, chapter, that Berryman ventures an overarching definition of mechanics:

> My proposal—and it is speculative—is that the unifying factor [of the field of mechanics] was simply the perception that certain devices have in common that they *do* something. [201]

Berryman's minimalist definition is, in my view, all more convincing, coming as it does after a thorough review of the evidence. The book's conclusion is exemplary, in that it actually does what it says on the box, as it were, in summarizing lucidly and thoroughly what the book says.

I have to admit that I found Berryman's style arduous at times, especially in the initial chapters; but I also thought that the book picked up speed and elegance as it went along. Her style of argumentation is very measured. Unlike what seems to be the norm in academic writing nowadays, she is happy to advance moderate claims and is rather modest in affirming the breadth and significance of what she is saying. While at times I would have liked to see her push the envelope a bit more, it was refreshing to read such an unboastful piece of scholarship for a change.

If I had to summarize the argument of Sylvia Berryman's book in one sentence, I would say that her claim is: ancient mechanics mattered. She makes a well-argued case that it mattered in antiquity and that it ought to give us food for philosophical thought today.

BIBLIOGRAPHY

Fraser, P. M. 1972. *Ptolemaic Alexandria.* Oxford.

Wilson, A. 2002. 'Machines, Power and the Ancient Economy'. *Journal of Roman Studies* 92:1–32.

On Sunspots: Galileo Galilei and Christoph Scheiner translated and introduced by Eileen Reeves and Albert Van Helden

Chicago/London: University of Chicago Press, 2010. Pp. xii + 418. ISBN 978–0–226–70716–7. Paper $40.00

Reviewed by
David Marshall Miller
Emory University
david.m.miller@emory.edu

Eileen Reeves' and Albert Van Helden's *On Sunspots* offers the first complete English translation of Christoph Scheiner's and Galileo Galilei's epistolary debate regarding the physical nature of sunspots. It thus comprises Scheiner's six letters, written above the pseudonym 'Apelles latens post tabulam' ('Apelles hidden behind the canvas') and published as the *Tres epistolae de maculis solaribus* and the *Accuratior disquisitio* in 1612, along with Galileo's three responses, published as *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti* in 1613. In addition, Reeves and Van Helden supply several chapters and appendices of original explanatory material, setting the debate in its contemporary contexts, summarizing its contents, formally reconstructing its arguments, and outlining its significance for the history of science. The sunspots debate was an important episode that well deserves the comprehensive treatment it receives. Reeves and Van Helden succeed in elaborating an important scientific performance by Galileo and Scheiner within their intellectual community. They have produced a handsome volume that will be of essential use to scholars and students of Galileo and of early modern science.

Once the telescope became known in 1609, its use to examine the Sun was inevitable. At first overcoming the Sun's brilliance by crepuscular sightings or viewing through clouds, observers were surprised to find that its face was continually besmirched with irregular spots. Large sunspots had been occasionally observed by the naked eye, even in antiquity, and were taken as portentous omens or as transits of inferior planets; but the telescope revealed the spots to be quotidian features of the Sun's appearance. They were seen to cross the solar body, sometimes joining together or splitting apart,

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 97–102

in about 15 days. Most observers agreed that the lack of parallax and the consistency of reports from different locations proved that the spots were neither instrumental artifacts nor local atmospheric disturbances and had to be closely associated with the Sun itself, if not on it. It was equally agreed, however, that the spots should not be there. The long-dominant Aristotelian natural philosophy held that the heavens were a realm of immutable perfection—of immaculate orbs rotating uniformly in place. A spotted Sun was a theoretical oxymoron. Hence, the question became, 'What were the spots? What did the appearances signify?' Scheiner, trying to accommodate the new observations to the prevailing philosophy, supposed that they were swarms of small bodies orbiting the Sun—like the moons Galileo had recently discovered orbiting Jupiter. Galileo, happy to overturn Aristotelian natural philosophy, thought the spots were on the surface of the Sun, comparable to terrestrial clouds.

This much formed the philosophical kernel of Galileo's and Scheiner's dispute, but it was set within a filigree of patronage and politics. The letters became a virtual courtly contest, where establishing prestige and authority over the claims of the rival was as important, or more so, than establishing the truth. Novelty, clever argument, witness testimonials, and *bons mots* were all darts in the authors' quivers.

In almost every respect, Galileo began on the higher ground. With the publication of *Sidereus nuncius* in 1610, he had gained fame for his discovery of lunar mountains and Jupiter's moons, and he subsequently announced the phases of Venus and the 'companions' of Saturn (not yet known to be rings). All this had secured international recognition as the authority on telescopic astronomy, a position at the Florentine court, and election to the exclusive Accademia dei Lincei, the premier collection of *avant garde* Italian literati, who in turn threw their weight behind the publication of his letters. Galileo had the privilege of responding to letters that he had already seen. He was also a more competent mathematician, a better stylist, and possessed the significant advantage of being right. Nevertheless, perhaps still insecure in his newfound eminence, Galileo aggressively pressed his position, sticking the knife in at every opportunity and often giving it a twist. Galileo dismissively responded to Scheiner's Latin letters in Italian—which the German could not read—even after the patron organizing the exchange gently complained of the difficulty of translation [252]. Scheiner was not a very skilled astronomer or geometer but Galileo haughtily harped on every mistake. Having pointed out an inconsistency in Scheiner's ordering of the planets in the solar system, for instance, Galileo gratuitously comments that Scheiner, as if lazy-minded,

> cannot totally free himself from those fancies previously impressed on him, fancies to which his intellect still returns from time to time, habituated to assent by long custom. [95]

Scheiner was understandably stung by Galileo's attacks and an increasing acrimony grew between them.

In retrospect, Galileo's animosity was unfortunate. For one thing, Scheiner was a Jesuit, whose order was committed to secular education alongside religious instruction and thus had an institutional interest in the mathematical and empirical sciences. As Reeves and Van Helden helpfully relate, Scheiner was part of an extensive network of observers and collaborators within the order, emanating from the leading mathematicians of the age—the mathematics faculty of the Collegio Romano, the Jesuit flagship institution. Moreover, the order was favorably inclined toward Galileo at the time. They saw him as an allied progressive opposed to more reactionary elements of the Counter-Reformation. Those same professors at the Collegio Romano had ratified Galileo's astronomical discoveries and lauded him in person in 1611. The wrangle with Scheiner began the process, aggravated by later disputes and European intrigues, that converted the Jesuits from potential allies into leading protagonists of Galileo's condemnation in 1632–1633.

The letters also reveal that Scheiner himself was open to persuasion. His letters begin in an earnest tone of modest but guileless pride in his observations. He seems a sincere scholar offering new knowledge and anything but a rigidly orthodox Aristotelian. In fact, he published anonymously without the express consent of his superiors precisely because he adopted modern, heterodox views. Scheiner admits that empirical observation and mathematics can be decisive in natural philosophy. He rejects the Ptolemaic universe in favor of a Tychonic system in which at least Mercury and Venus orbit the Sun and Jupiter has satellites of its own. His account of the solar spots also postulated a multitude of new celestial bodies moving nonuniformly around the Sun. Scheiner seems, therefore, to have been the perfect candidate for conversion, not alienation. In his sniping, Galileo misses the important fact that Scheiner had freed himself from Aristotelian 'fancies' and was thus already partly on his side. In the event, and to his credit, Scheiner actually accepted the substance of Galileo's criticisms. In the course of his letters, he admits that appearances required much more irregularly shaped solar satellites orbiting much closer to the Sun than he first thought; and later in his career, he would adopt Galileo's view entirely, placing the spots on the surface of the Sun, though he remained stubbornly, even spitefully, opposed to Galileo's Copernicanism.

All the while, the letters document important advances. Galileo and Scheiner standardize the method of sunspot observation by which the Sun's image is projected by a telescope onto paper. They report numerous observations and publish detailed images, here lavishly reproduced in large format. The letters also contain an early statement of Galileo's inertial principle, the announcement of Saturn's changing appearance, comments and predictions about Jupiter's moons, methodological discussions regarding the role of observation and mathematical argument in natural philosophy, and so on. Thus, the letters illuminate the production of scientific knowledge in the early 17th century. They show how evidence combined with rhetoric was used to establish claims and how the entire process was embedded in patronage and institutions. The letters also demonstrate the receptivity and awareness of Galileo and his work on the part of his contemporaries. They also reveal much about the personality of the correspondents.

Reeves and Van Helden clearly explicate all these aspects of the exchange. Their effort is greatly helped by their decision to present the letters in chronological order, so that Galileo's first two letters separate Scheiner's first three letters (the *Tres epistolae*) from his latter three (the *Accuratior disquisitio*), which are then followed by Galileo's last letter. The translation is further surrounded and interspersed with short informative chapters on the history of sunspot observations, Scheiner, the development of the debate, and its aftermath. Altogether, the arrangement helps the reader keep track of the discussion and makes the entire book fluid and compelling. The book ends with additional appendices presenting the front matter from the *Istoria e dimostrazioni*, formal reconstructions of some of the more technical arguments, additional correspondence, and a useful bibliography. All, especially the translations themselves, are gracefully written in luminous prose, with a concision that never interferes with comprehension.

However, there are quibbles; most of them—literally—at the margins. In the first place, the translation of the *Istoria e dimostrazioni* is missing the marginal postils published in the original volume. Moreover, there are no indications of the corresponding pagination in volume 5 of the Edizione Nazionale, where the original texts are collected. This makes it difficult for scholars to find parallel texts in the original and is surprising in a volume intended for serious use. Also, the footnotes are of uneven tone and purpose. Some are clearly elementary; others provide references for advanced scholars. Similarly, the explanatory chapters are basic. Suitable for undergraduates, they do not add anything novel to the literature. The formal appendix, meanwhile, will be of interest only to a few specialists.

Finally, and only because the book will surely become a standard reference, I feel compelled to question Reeves' and Van Helden's translation of Galileo's statement regarding conserved motion, which is perhaps the most famous passage in the entire correspondence. Here is the original:

> *[E] però, rimossi tutti gl'impedimenti esterni, un grave nella superficie sferica e concentrica alla Terra sarà indifferente alla quiete ed a i movimenti verso qualunque parte dell'orizonte, ed in quello stato si conserverà nel qual una volta sarà stato posto; cioè se sarà messo in stato di quiete, quello conserverà, e se sarà posto in movimento, v. g. verso occidente, nell'istesso si manterrà.* [Favaro 1890–1909, 5:134]

This is rendered:

> And therefore, with all the external impediments removed, a heavy body on the spherical surface concentric to the Earth will be indifferent to rest and to movement toward any part of the horizon, and it will remain in the state in which it has been put; that is, if it has been put in a state of rest it will remain in it, and if it has been put in motion, toward the west, for example, it will remain in the same state. [125]

In the last sentence, 'istesso' ('the same') should refer back to 'movimento' ('movement'), not, as Reeves and Van Helden have it, 'stato' ('state'). Compare this, for instance, to Drake's translation, 'it will maintain itself in that movement' [1957, 113] or to Finocchiaro's, 'it will remain in that motion' [2008, 98]. The trouble is that Reeves and Van Helden, by subsuming 'movement' into the 'state' of a body, efface the opposition between motion and rest that seems present in the text, suggesting that motion and rest are both mere 'states' of a body, continuous with one another. Of course, modern physics would eventually adopt this principle; but the translation seems to anachronistically impute that later development to the text.

These criticisms ultimately pale at the overall achievement of the book. *On Sunspots* is a welcome addition to the Galilean corpus in English. It will prove a useful and informative text to a wide range of students and scholars of a wide range of subjects. Best of all, it is a pleasure to read.

BIBLIOGRAPHY

Drake, S. 1957. *Discoveries and Opinions of Galileo.* New York.

Favaro, A. 1890–1909. ed. *Le opere di Galileo Galilei.* 20 vols. Florence.

Finocchiaro, M. 2008. ed. and trans. *The Essential Galileo.* Indianapolis.

Numerals and Arithmetic in the Middle Ages by Charles Burnett

Variorum Collected Studies Series 967. Farnham, UK/Burlington, VT: Ashgate, 2010. Pp. x + 370. ISBN 978–1–4094–0368–5. Cloth $180.00

Reviewed by
Jens Høyrup
Roskilde University
jensh@ruc.dk

After *Arabic into Latin in the Middle Ages: The Translators and Their Intellectual and Social Context* and *Magic and Divination in the Middle Ages: Two Texts and Techniques in the Islamic and Christian Worlds*, this third Variorum volume from Charles Burnett's hand collects papers dealing with the period and process of adoption of the Hindu-Arabic numerals. The collection shows us the intricacies of this process, a process which was probably the 'most momentous development in the history of pre-modern mathematics' [IX.15]. Intricacies are certainly not unexpected in a process of this kind; but their precise portrayal can only be painted by someone as familiar as Burnett with the original documents, their languages, their style and context.

Burnett combines this technical expertise with a keen eye for the broader questions to which it can be applied (without which the answers provided by even the best technical expertise can appear naive). It must be said, however, that technical matters and details take up most of the space in the majority of the articles in the volume. The reader with paleographic proficiency will enjoy the many reproductions of manuscript pages.

The volume contains 11 articles of varying length:

(I) 'The Abacus at Echternach in *ca.* 1000 A.D'. 14 pp. text, 4 pp. reproductions. Originally published in 2002.

(II) 'Abbon de Fleury, *abaci doctor*'. 11 pp. text, 2 pp. reproductions. Originally published in 2004.

(III) '*Algorismi vel helcep decentior est diligentia*: The Arithmetic of Adelard of Bath and His Circle', 40 pp. introduction, 37 pp. edition with translation, 12 pp. edition of *Anxiomata artis*

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 103–118

arithmetice, 22 pp. reproductions. Originally published in 1996.

(IV) 'Ten or Forty? A Confusing Numerical Symbol in the Middle Ages'. 7 pp. text, 2 pp. reproductions. Originally published in 2008.

(V) 'Indian Numerals in the Mediterranean Basin in the Twelfth Century, with Special Reference to the "Eastern Forms"'. 32 pp. text, 20 pp. reproductions. Originally published in 2002.

(VI) 'The Use of Arabic Numerals Among the Three Language Cultures of Norman Sicily'. 4 pp. text, 7 pp. reproductions. Originally published 2005.[1]

(VII) 'Why We Read Arabic Numerals Backwards'. 6 pp. text. Originally published in 2000.

(VIII) 'The Toledan *Regule* (*Liber alchorismi*, part II): A Twelfth-Century Arithmetical Miscellany' (in collaboration with Ji-Wei Zhao and Kurt Lampe). 8 pp. introduction, 34 pp. text edition, 33 pp. translation, 16 pp. mathematical translation and notes. Originally published in 2007.

(IX) 'Learning Indian Arithmetic in the Early Thirteenth Century'. 10 pp. text, 2 pp. reproductions. Originally published in 2002.

(X) 'Latin Alphanumerical Notation, and Annotation in Italian, in the Twelfth Century: MS London, British Library, Harley 5402'. 10 pp. text, 5 pp. reproductions. Originally published in 2000.

(XI) 'Fibonacci's "Method of the Indians"', 11 pp. text. Originally published in 2005.

The recurrent themes are summed up in the short preface [vii], according to which the volume

> brings together articles on the different numeral forms used in the Middle Ages—actually from the 10th through the 13th century—and their use in mathematical and other contexts. Some articles study the introduction of Hindu-Arabic numerals into Western Europe between the late 10th and the early 13th centuries, documenting in more detail than anywhere else the different forms in which they are found, before they acquired the standard shapes with which we are familiar today

[1] Greatly reduced in size: the footnotes are *ca* 5 pt. This should have been reset in spite of Variorum's normal principles.

> [articles I, V, VI, VII, VIII, IX, XI]. Others deal with experiments with other forms of numeration within Latin script, that are found in the twelfth century: e.g., using the first nine Roman numerals as symbols with place value [III], abbreviating Roman numerals [IV], and using the Latin letters as numerals [X]. Different types of numerals are used for different purposes: for numbering folios, dating coins, symbolizing learning and mathematical games, as well as for practical calculations and advanced mathematics. The application of numerals to the abacus [I, II], and to calculation with pen and paper (or stylus and parchment) is discussed [VII, IX].

As reflected in these words, Hindu-Arabic numerals were indeed not adopted merely because they happened to present themselves; they came together with practices (astronomy, astrology, commerce) where they served. For a long while it was not obvious that all of these practices were best served by the complete Hindu-Arabic system and not by one of the alternatives that were tried: that is, by counters inscribed with the Hindu-Arabic numerals used on an abacus board emulating the place value system (the 'Gerbert' abacus)—a place value system using Roman numerals 'I' through 'IX' instead of the unfamiliar Hindu-Arabic shapes—or by a Latin emulation of the Greek alphabetic notation. Nor was the shape of the Hindu-Arabic numerals clear and certain from the start, since those who adopted them initially were in contact with different regions of the Arabic world that used different styles.

In detail, article I describes a large parchment sheet from the Benedictine monastery of Echternach from *ca* AD 1000 that carries the earliest extant specimen of what has been known as a 'Gerbert' abacus. As pointed out by Burnett [I.92],

> nothing precise is known about the origin of this device but our testimonies rather associate a revival of its use with Gerbert d' Aurillac, especially with his period as a teacher at Reims (972 to 983).

According to Burnett, it

> seems likely that Gerbert introduced the practice of marking the counters with Arabic numerals (which he would have come across when he studied in Catalonia, before coming to Reims),

> and established a form of the abacus board that became an exemplar for most subsequent teachers of the abacus.

This assumption has the advantage over a presumed invention from scratch that it creates harmony between pre-Gerbertian references to the abacus and the ascriptions to Gerbert. As argued by Burnett, the Echternach abacus agrees so well with the description of Gerbert's own abacus made by his pupil Richer and with Bernelinus' prescriptions for its use that we may reasonably regard it as a faithful copy of Gerbert's own board. However, as noted, another apparently contemporary manuscript from Echternach[2] —'virtually a facsimile' [I.101]—may contain what is in itself an even more faithful copy but to which complementary commentaries have been added, commentaries which explain, among other things, how to calculate with Roman duodecimal fractions (a vestige of earlier medieval monastic computation not represented on the original Gerbert abacus as described and copied in the two manuscripts described here but soon fitted onto the board in three extra columns). The parchment sheet itself as well as the quasi-facsimile enumerate the three-column groups by means of Arabic numerals (in abacus shape), thus making obsolete Walter Bergmann's observation [1985, 212] that no positive evidence supports the traditional belief that the 'Gerbert' abacus made use of these already from the beginning.[3]

Article II raises the question whether the mathematical honor of Gerbert's contemporary Abbon de Fleury can be saved. Nikolaus Bubnov [1899, 203] concluded from the paucity of substance in the references to the abacus that we have from Abbo's hand that his competence on the instrument on which he declared himself a doctor

[2] Now MS Trier, Stadtbibliothek 1093/1694.

[3] Thus Burnett's polite report of Bergmann's stance. Actually, Bergmann's claim is much stronger (though based on very weak evidence), namely, that the late-10th-century abacus used counters carrying Greek letter-numerals and that the first use of Hindu-Arabic numerals on the counters is to be dated two generations after Gerbert; common use according to Bergmann belongs to the second half of the 11th century. This is now not only obsolete but directly falsified.

Article III, written earlier, still follows Bergmann (in the weak version) and accepts the claim that the earliest appearance of the Hindu-Arabic numerals on abacus counters is in the pseudo-Boethian Geometry II [III.227 with n28]. This of course has to be corrected in view of article I.

was quite restricted (unless the lines where this occurs were added by a copyist). Burnett goes through the evidence (including references to Abbo in manuscripts from pupils of his citing his teaching) and finds that all of it is concerned with the mystical properties of numbers and not at all with technical teaching. The lack of mathematical substance thus does not prove his incompetence; nor, it must be said, is any evidence for particular skill supplied by the sources.

Article III

> investigates the kind of arithmetic practised by Adelard of Bath, his colleagues, and his immediate successors. This will lead us to re-examine the introduction of the algorism into Europe and, incidentally, to make some comments on the terminology for, and use of, the zero, and on the authorship of the Latin versions of Euclid's *Elements* known as Version I and Version II. The key texts are Adelard's passage on arithmetic in his *De eodem et diverso*, his *Regulae abaci*, the versions of Euclid's *Elements* associated with the name of Adelard of Bath, glosses to Boethius' *Music* which mention Adelard, glosses to Boethius' *Arithmetic* in the same manuscript as those to Boethius's *Music*, the *Helcep Sarracenicum* of H. Ocreatus, and the contents of [a] Coventry manuscript [containing another copy of the latter text]. [III.222f]

As far as the early *De eodem et diverso* and *Regulae abaci* are concerned, the analysis substantiates what was already pointed out by Marshal Clagett [1970, 61f], namely, that they show no influence from the Arabic world. The analysis of sources connected to the various versions of the *Elements* leads Burnett to conclude that Version 1 'seems to be a direct translation from the Arabic made by Adelard himself (probably with the help of an arabophone)' [III.229],[4] whereas Version 2 is indeed an ongoing (branched) project rather than a single version:[5] evidence is offered that friends and/or students of Adelard were involved in the project while he was still alive.

The article is accompanied by an edition and translation of the *Helcep sarracenicum*, whose title means 'Saracen calculation' ('helcep', as it is argued, rendering Arabic 'al-ḥisāb'), and which explains

[4] Busard, in his edition of the text, did not feel able to determine the authorship [1983, 18].

[5] This is in agreement with Busard and Folkerts 1992.

the place value system and how to calculate within it. Remarkably, the whole treatise represents the digits by Roman, not Hindu-Arabic, numerals—a pretty exemplification of how the new numerals and place value system represented a double difficulty, and that it could therefore be judged adequate to introduce one of them without the other.[6] The treatise was dedicated to Adelard and, hence, written during his lifetime—and also, it appears, before its genre acquired the standard name 'algorism'. Burnett suspects its author (an otherwise unidentified 'Ocreatus', whose name appears, however, in various puns in writings from the same intellectual environment) to have been more competent than Adelard in Arabic and, hence, perhaps involved in the production of Version 1.

Article IV deals with a particular writing of '40' as a ligature 'X^{L}', often reduced (perhaps by scribal misunderstanding) to a mere 'X'. The origin of this ligature is in Visigothic script. Analyzing all mathematical and astronomical/astrological manuscripts where it is used,[7] Burnett reaches the conclusion that it occurs in particular in John of Seville's earlier translations—Seville later used Hindu-Arabic numerals—and that his use of it seems natural, since the ligature was in common use in his environment. Plato of Tivoli and Raymond de Marseille also employ it, even though it was probably foreign to the places where they worked (Barcelona and Marseille, respectively); they can be presumed to have been influenced by John's writings. Use of the ligature by Gerard of Cremona in his translation of the *Almagest* (where Roman numerals are employed) is doubtful. Other 12th-century translators based in Aragon and Navarra but coming from elsewhere seem not to have used it (unlike John, indeed, they had not been brought up with it). In general, as formulated in the conclusion [IV.87], Burnett maintains,'When Hindu-Arabic numerals finally prevailed among mathematicians, the ligature disappeared altogether.'

The first part of article V presents the two principal ways to write Hindu-Arabic numerals, 'Eastern' and 'Western', together with the intermediate Palermitan way (on which more below). A table

[6] The terminology is also in debt to earlier abacus writings and to the Boethian tradition.

[7] The manuscripts, though not autographs, appear to reflect the originals faithfully.

shows their shapes in 53 manuscripts and on two coins (9 Arabic, 4 Greek, the rest Latin, dating from the 10th to the 13th century). The second part concentrates on the appearances of the Eastern type in Latin manuscripts. It finds that this type turns up in a few manuscripts that point back to Hugo of Santalla.[8] It is possible that Hugo's inspiration comes from manuscripts once belonging to the Banū Hūd library in Zaragoza. Manuscripts going back to Hugo's friend Hermann of Carinthia also use it (but here the Eastern form seems to be what the scribe is accustomed to himself). The earliest manuscript of the version of the *Elements* made directly from the Greek also uses the Eastern form.

However, all these manuscripts were probably written in Tuscany, which leads Burnett to Abraham ibn Ezra, who came from the region where Hugo and Hermann worked but whose essential work in the present respect—the Pisan Tables (if they really are his) and explanations of how to use them—were also written in Tuscany. The Eastern forms are also used in these commentaries. Still, after weighing the complete evidence Burnett comes to the conclusion that

> the use of Eastern forms in the Latin texts associated with Abraham ibn Ezra is probably due...not so much to Abraham himself as to his Latin associates, who were using the tables of Pisa. The combined testimony of these manuscripts strongly indicates that the Eastern forms were being used in Pisa and Lucca in the mid-twelfth century. [V.251]

Thus, even the Eastern forms used in the Hugo- and Hermann-manuscripts may say little about what the originals did. As pointed out, Pisan external connections were oriented at that moment toward Antioch and Constantinople—and even Greek writers using Hindu-Arabic numerals initially used the Eastern forms (the Western forms only turn up in 1252).

An appendix lists and describes 26 Latin manuscripts using Eastern and Palermitan forms.

The short article VI at first describes the particular character of the translations from Norman Sicily, where translations were made from the Greek as well as from the Arabic into Latin, and where some

8 Since the last copyist has difficulty in understanding them, he at least cannot have introduced the Eastern Hindu-Arabic numerals.

scholars at least knew all three languages; and notes the consequence that translations from the Greek were sometimes supplemented by Arabic material (thus the translation of the *Almagest* as well as of Euclid's *Optics*). After that, it describes the particular Palermitan forms of the Hindu-Arabic numerals—forms intermediate between the Eastern and the Maghreb style, as is the Arabic script of a trilingual psalter prepared at the Norman court. Burnett suggests as a common explanation that the Arabic scribes of the royal chancery (an institution perhaps emulating the chancery of the Egyptian Fatimids) had been taught in Egypt, but where the characters they had learned at home differed too much from those locally used (which were in Maghreb style) they adopted the latter.

Article VII, also short, discusses why (e.g.) 'twelve' is written '12' and not '21'. Initially, it is pointed out that there are two reasons for this. Firstly, this is the way in which the number is written in Arabic, where lower orders of magnitude are written first in the right-to-left reading direction; secondly, Greek alphabetic as well as Roman numerals write the higher orders to the left. However, as Burnett points out, the direction to be used was none the less uncertain at first and in need of explanatory justification. Early algorisms often speak of the position to the left as 'later' (perhaps translating an Arabic text directly), and when presenting the numerals in sequence they have '9' to the left (as Arabic texts would have it). By the early 13th century, according to Burnett, most algorisms had adopted what we would consider the normal orientation; but he points to a short algorism probably written shortly before 1250 where 'before' is still to the right.[9]

Article VIII is an urgently needed 'working edition' of the *Regule*, a miscellany of arithmetical texts glued to the *Liber alchorismi*,[10]

---

[9] According to the two editions [Curtze 1897, 2; Pedersen 1983, 176], Sacrobosco's *Algorismus vulgaris* also considers the position to the right as 'first' and gives the sequence of numerals as '9 8 7 6 5 4 3 2 1'. Even Jacopo da Firenze, in some debt to Sacrobosco but not copying, still has the sequence '10 9 8 7 6 5 4 3 2 1' or '0 9 8 7 6 5 4 3 2 1' in his Tuscan *Tractatus algorismi* of 1307, and his opinion about what is 'first' and what is 'last' is unstable [see Høyrup 2007, 196–202, 385, with Høyrup 2009, 117 for correction].

[10] The existing edition was made from one manuscript by Baldassare Boncompagni [1857b, 93–136]. André Allard, in his edition of the *Liber alchorismi*, only refers occasionally to a 'seconde partie' [1992, xvii, xix, xxxviii–xl]

made from what Burnett and his co-authors (Ji-Wei Zhao and Kurt Lampe) consider the best manuscript (Paris, BNF lat. 15461) and followed by English and mathematical translations.

The *Regule* consist of seven distinct textual elements, to which come multiplication tables for the orders of sexagesimal fractions and for the numbers 1 through 9, and a magic square. From the totality of manuscripts, the authors conclude that they were put together in Toledo (whence the name they give to the whole, 'Toledan regule'). They also point out an affinity with the *Liber mahamaleth* and with Gundisalvi's *De divisione philosophiae*.

The contents of the *Regule* cover various arithmetical rules concerning progressions, multiplication and division; abstractly formulated rules for the conversion of metrological units; the rule of three[11] and the partnership rule; the rules for the three mixed algebraic second-degree cases; and rules for finding a hidden number. Finally, there is a philosophical/numerological justification of the principles of Hindu-Arabic reckoning.

The treatise shares with the *Liber mahamaleth* (edited by Vlasschaert [2010]) as well as with the *Liber abbaci* the inscription of numbers for a calculation within a rectangular frame, probably corresponding to a dust- or clay-board (*takht* or *lawḥa* respectively).[12] Although the overlap in contents between the three treatises is limited, it cannot be neglected; and the *Regule* thus casts light on the environment that produced the two larger treatises.[13] For, since the algebra of the *Regule* is not taken from al-Khwārizmī (neither from known translations nor from the Arabic original), it can no longer be taken for granted that the lost algebra chapter of the *Liber mahamaleth*—and, for that matter, the algebra to which Abū Bakr refers in the *Liber*

---

without ever explaining in any way what these words refer to. Actually, this second part is identical with the *Regule*, present in all but one of the 10 manuscripts.

11 Understood as the answer to a riddle, not as a real-life commercial problem: somebody, 'concealing from you the fourth number', asks.... Obviously, the author is a scholar and not a clerk or a merchant school teacher.

12 Fibonacci speaks of it as a *tabula*: see Boncompagni 1857a, 118.

13 We should not forget that one of the earliest manuscripts of the *Liber abbaci* [Vatican, Pal-Lat. 1343, new foliation 47r] refers to a *magister castellanus* as the source for chapter 9, 'On Barter'.

mensurationum [see Busard 1968]—is identical with al-Khwārizmī's text. Any further study of these three texts (and many others until the 14th century) should henceforth take the *Regule* into account.

Article IX analyses two short introductions to an algorism, all three items to be found in a manuscript that also contains the *Helcep Sarracenicum*. They represent an intermediate stage of the development of the algorism genre, preceding the kind of codification achieved by Alexandre Villedieu's *Carmen de algorismo* and Sacrobosco's *Algorismus vulgaris* in the earlier decades of the 13th century[14]—part of the terminology is still inherited from the operations on the Gerbert abacus, and one of the commentaries applies to the abacus just as well as to algorism.

Article X deals with the

> use of the Latin letters in their alphabetic order as numerals, on the model of the notation for numerals which is normal in Greek, Arabic and Hebrew. [X.76]

This notation was not widespread. Indeed, Burnett locates it 'in a group of closely related works written by a certain "Stephen" and an "'Abd al-Masīḥ of Winchester"', two of which are dated 1121 and

[14] Burnett [IX.15] states that

> the acceptance of the algorism within the canon of European mathematics was ensured by the magisterial *Liber abbaci* of Leonardo of Pisa (Fibonacci)...and the more popular manuals of Alexander of Villa Dei...and of John of Sacrobosco.

I must object, however, that there is no evidence in favour such a role for Fibonacci. Apart from a barely possible reference to his solution to a problem about purchasing a horse in Jordanus of Nemore's *De numeris datis* [II.27 see Hughes 1981, which shares the numerical parameters with Boncompagni 1857a, 245–248 but speaks of the method as 'Arabic'], no school mathematician before Jean de Murs appears to have made use of or even known the *Liber abbaci*—and Jean uses the algebra and the treatment of roots, not the algorism [see l'Huillier 1990, 12].

Note 1 states that the texts of the *Carmen de algorismo* and the *Algorismus vulgaris* are available only in Halliwell 1841. Actually, a working edition of the *Carmen* is in Steele 1922, 72–80, while working editions of the *Algorismus vulgaris* are in Curtze 1897, 1–19 and Pedersen 1983, 174–201, the former based on a single manuscript, the latter on 4 manuscripts with control of 11 more (including the one used by Curtze).

1127, respectively, and which were both copied in Antioch. Stephen was from Italy and appears to have written for an Italian public. However, Burnett's article concentrates on the manuscript British Library, Harley 5402, where a planetary table using this notation is accompanied by a key, showing that its users were not expected to know the notation. These notes, written in a mixture of Italian and ungrammatical Latin, mention the date 1160 and refer to the tables of Lucca, which were derived from the above-mentioned Pisan tables. Since Abraham ibn Ezra, involved in these, had been in Lucca in the 1240s, it is suggested as a possibility, but not asserted explicitly, that the annotations might go back to Abraham.

From the linguistic point of view, the manuscript is important since it contains one of the earliest known examples of writing in Tuscan.

Article XI deals with a never-discussed puzzle contained in the oft-quoted introduction to Fibonacci's *Liber abbaci*. Fibonacci states that his father wanted him to stay and be taught 'for some days' in a 'calculation school'[15] in Bejaïa, where he was introduced to the 'art [of calculation] by the nine figures of the Indians'. The knowledge of this art pleased him so much that he learned all that he could about how it was studied in Egypt, Syria, Greece, Sicily, and Provence when going there for the sake of trade. But (this is the puzzle) he also writes:

> I reckoned all this, as well as the [Latin] algorism and the arcs of Pythagoras [the Gerbert abbacus] as a kind of error as compared to the method of the Indians.

As Burnett protests, both the Gerbert abacus and the algorism were also based on the nine figures of the Indians, and these were

15 XI.87n1 'Genitor meus...studio abbaci per aliquot dies stare voluit et doceri'. Burnett translates 'studio abbaci' by 'abbaco school' thereby intimating an institution of the same kind as is found in Italy a century later. While this may be an unwarranted jump if taken to the letter, the word 'doceri' ('be taught') at least guarantees that 'studio'must be taken in the meaning of 'school'.

known by Latin scholars since the mid-12th century.[16] The algorithms for computing were not the same in the three cases, he admits, but of course they had common features. So, is Fibonacci just showing off or self-advertising (the Indians being in odor of ancient wisdom)? This is Burnett's closing hypothesis.

This is indeed possible: we know that Fibonacci's use of references was strategic—he says nothing about his indubitable debt to existing Latin translations from the Arabic (al-Khwārizmī's *Algebra* [see Miura 1981] as well as Abū Bakr's *Liber mensurationum* [see Høyrup 1996, 55]). However, there is no reason to believe that Fibonacci speaks about the Hindu-Arabic numerals only. Indeed, the preface, as translated by Burnett, continues thus:

> Therefore, concentrating more closely on this very method of the Indians, and studying it more attentively, adding a few things from my own mind, and also putting in some subtleties of Euclid's art of geometry, I made an effort to compose, in as intelligible a fashion as I could, this comprehensive book, divided into 15 chapters, demonstrating almost everything that I have included by a firm proof, so that those seeking knowledge of this can be instructed by such a perfect method (in comparison with the others), and so that in future the Latin race may not be found lacking this (knowledge) as they have done up to now.

Apart from the Euclidean material and some unspecified contributions made by Fibonacci himself, the whole of the *Liber abbaci* was thus

[16] Regarding Syria etc., Burnett points out that Fibonacci says nothing about the Indian figures being used there and states that

> the most common forms of numerals used by merchants in the Mediterranean in the Middle Ages were derived from Greek alphanumerical notation.

However, whatever was done in commercial interaction and for accounting and notarial purposes does not reveal much about what was done when calculation was practised as an 'art'. Ibn Sīnā, as he says in his autobiography, was taught the use of Hindu numerals by a greengrocer [see Gutas 1988, 24], thus by a merchant, not by an astronomer or professional mathematician (which in the context amounted to much the same). In general, different purposes called for the use of different notations [see Rebstock 1993, 12; Rebstock 2008 27–29].

considered to present 'this very method of the Indians'. However, already on page 24 of the 459 pages of the Boncompagni edition we are introduced to the notations for ascending continued fractions and other composite fractions invented in the Maghreb or al-Andalus during the 12th century, notations totally unknown (according to extant documents, including the *Liber mahamaleth*) in the Latin world but used systematically and heavily by Fibonacci. Later there also follows a huge amount of 'practical arithmetic' (by far exceeding what was needed in commercial practice, of course), and even an algebra that goes well beyond what was known through the translations of Robert and Gherardo. According to Fibonacci's words, all of this belonged under the heading 'method of the Indians'. Much of it can be found in the *Liber mahamaleth*. But nothing suggests that Fibonacci knew that book; thus, he was entitled to believe that the Latin race had up to now been 'lacking this knowledge'.

The question remains why Fibonacci characterizes it as the 'method of the Indians'. He may, as Burnett proposes, just be self-advertising. But we should take note of his understanding that the whole subject matter of his book (the Euclidean and personal additions excluded, probably also chapter 15, part 1) constituted a single complex. This complex encompassed much material known not only from Arabic writings but also from Sanskrit mathematicians presenting and using the methods of 'the world'.[17] We know nothing about how the commercial community carried this knowledge structure between India and the Mediterranean but we may be sure that it did. Somehow, it may have been known in the environment that it was connected to India—or this may have been concluded mistakenly by Fibonacci because the complex encompassed 'Indian' numerals. Self-advertising remains a plausible explanation but alternatives are at hand (and one need not exclude the other).

To sum up, this collection of articles is immensely rich in insights—often so detailed that the reader may have to work through an article several times in order to get all the points. Often, by necessity, the conclusions drawn are tentative—but when they are, this is always made explicit; only rarely is it possible to suggest a more likely interpretation of the sources than what is proposed by

[17] This distinction between scholarly and 'lay' mathematics is made by Bhāskara I [see Keller 2006, 1.7, 12, 107f].

Burnett. The book can be recommended to anybody working on the matters which it deals with; but it can also be recommended that the reader go to the richness of its text with patience.

BIBLIOGRAPHY

Allard, A. 1992. *Muḥammad ibn Mūsā al-Khwārizmī, «Le Calcul indien» (Algorismus). Histoire des textes, édition critique, traduction et commentaire des plus anciennes versions latines remaniées du XIIe siècle.* Collection Science dans l'histoire: Collection d'Études classiques. Paris/Namur.

Bergmann, W. 1985. *Innovationen im Quadrivium des 10. und 11. Jahrhunderts. Studien zur Einführung von Astrolab und Abakus im lateinischen Mittelalter.* Sudhoffs Archiv Beiheft 6. Stuttgart.

Boncompagni, B. 1857a. ed. *Scritti di Leonardo Pisano matematico del secolo decimoterzo: I. Il Liber abbaci di Leonardo Pisano.* Rome.

——— 1857b. *Trattati d'aritmetica. I.* Algoritmo de numero indorum. *II. Ioannis Hispalensis* Liber algorismi de pratica arismetrice. Rome.

Bubnov, N. 1899. ed. *Gerberti postea Silvestri II papae opera mathematica (972–1003).* Berlin.

Busard, H. L. L. 1968. 'L'algèbre au moyen âge. Le «Liber mensurationum» d'Abû Bekr'. *Journal des Savants* Avril-Juin 1968:65–125.

——— 1983. ed. *The First Latin Translation of Euclid's '*Elements*' Commonly Ascribed to Adelard of Bath.* Studies and Texts 64. Toronto.

Busard, H. L. L. and Folkerts, M. 1992. ed. *Robert of Chester's (?) Redaction of Euclid's* Elements*, the So-Called Adelard II Version.* 2 vols. Science Networks 8. Boston.

Clagett, M. 1970. 'Adelard of Bath'. Pp. 61–64 in C. C. Gillispie ed. *Dictionary of Scientific Biography* vol. 1. New York.

Curtze, M. 1897. ed. *Petri Philomeni de Dacia in algorismum vulgarem Johannis de Sacrobosco commentarius, una cum algorismo ipso.* Copenhagen.

Gutas, D. 1988. *Avicenna and the Aristotelian Tradition.* Leiden.

Halliwell, J. O. 1841. Rara mathematica*, or a Collection of Treatises on the Mathematics and Subjects Connected with Them, from Ancient Inedited Manuscripts.* 2nd edn. London.

Høyrup, J. 1996. '"The Four Sides and the Area". Oblique Light on the Prehistory of Algebra'. Pp. 45–65 in R. Calinger ed. *Vita mathematica: Historical Research and Integration with Teaching.* Washington, DC.[18]

________ 2007. *Jacopo da Firenze's* Tractatus Algorismi *and Early Italian Abbacus Culture.* Science Networks/Historical Studies, 34. Basel.

________ 2009. 'A Response to Van Egmond on Høyrup, *Jacopo da Firenze's* Tractatus Algorismi'. *Aestimatio* 6:116–126.

Hughes, B. B. O.F.M. 1981. *Jordanus de Nemore,* De numeris datis*. A Critical Edition and Translation.* Publications of the Center for Medieval and Renaissance Studies 14. Los Angeles, CA.

Keller, A. 2006. *Expounding the Mathematical Seed: A Translation of Bhāskara I on the Mathematical Chapter of the* Āryabhaṭīya. Science Networks/Historical Studies 30. 2 vols. Basel.

l'Huillier, G. 1990. ed. *Jean de Murs. Le* Quadripartitum numerorum. Mémoires et documents publiés par la Société de l'École des Chartes 32. Genève/Paris.

Miura, N. 1981. 'The Algebra in the *Liber abaci* of Leonardo Pisano'. *Historia Scientiarum* 21:57–65.

Pedersen, F. S. 1983. ed. *Petri Philomena de Dacia et Petri de S. Audomaro opera quadrivialia: I. Opera Petri Philomenae.* Corpus Philosophorum Danicorum Medii Aevi 10.1. Copenhagen.

Rebstock, U. 1993. *Die Reichtümer der Rechner (Ġunyat al-Ḥussāb) von Aḥmad b. Ṯabāt (gest. 631/1234). Die Araber—Vorläufer der Rechenkunst.* Beiträge zur Sprach- und Kulturgeschichte des Orients 32. Walldorf-Hessen.

________ 2008. *Abū 'Abdallāh aš-Šaqqāq (st. 511/1118), Kitāb al-ḥawī li-l-a'māl as-sulṭānīya wa-rusūm al-ḥisāb ad-dīwānīya (Da*

[18] This contains upwards of 60 printing errors: the editor did not realize that computer conversion should be followed by proof reading.

umfassende Buch über die herrschaftlichen Tätigkeiten und Rechenvorschriften in der Staatsverwaltung). Islamic Mathematics and Astronomy 113. Frankfurt a.M.

Steele, R. 1922. ed. *The Earliest Algorisms in English.* Early English Text Society, Extra Series, 118. London.

Vlasschaert, A.-M. 2010. ed. *Liber Mahamaleth.* Wiesbaden.

From the Old Academy to Later Neo-Platonism: Studies in the History of Platonic Thought by Harold Tarrant

Farnham, UK/Birmingham, VT: Ashgate, 2011. Pp. xii + 356. ISBN 978–1–4094–0828–4. Cloth $165.00

Reviewed by
Andrew Smith
Platonic Centre, Trinity College Dublin
andrew.smith@ucd.ie

This volume, which is in the Variorum Collected Studies Series, contains 23 essays published by Harold Tarrant in various journals and books between 1979 and 1999. Tarrant is a distinguished contributor to the history of the Platonic tradition as it developed from the time of Plato through the various phases of the Platonic Academy until the last flowering of Greek philosophy and the closure of the Academy in the sixth century AD. He is a consummate interpreter of the intricate ways in which the work of Plato was read and understood over a period of 1000 years. It is an intriguing story, often documented by the most tenuous evidence, of a philosophical movement which may be traced through many changes of emphasis from metaphysical speculation to varying degrees of sceptical enquiry in the Hellenistic period and back again in the period of the early Roman Empire to renewed metaphysical and theological interests, becoming once again the dominant philosophical tradition that culminated in the Neoplatonism of Plotinus and Proclus. These essays neatly cover that ground and are divided into three sections:

(1) Socrates, Plato and the Old Academy,
(2) The Platonic Revival and the Second Century AD, and
(3) Later Neoplatonism.

The first section contributes to our understanding of some basic and stimulating problems about Socrates and Plato himself. Is the portrait of Socrates as a midwife bringing to birth ideas in others but having no positive ideas of his own a genuine trait of the 'historic' Socrates or an interpretation by Plato? How is it feasible that proponents of pleasure as the goal of life (the Cyrenaics) and their

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 93–96

opponents at the other end of the scale (the Cynics) could both claim Socrates as their inspiration? Here Tarrant skillfully demonstrates that Plato himself may be seen in his written works to uphold both pro-hedonistic and anti-hedonistic viewpoints, whilst in reality falling between the two, just as Cyrenaics and Cynics also were not quite as black and white in their attitude to pleasure as their more extreme statements might suggest. In his essay on the composition of Plato's *Gorgias*, Tarrant argues that there was a first and then a revised edition of the work which represents a turning point in Plato's own attitude to pleasure as he became more interested in the thought of Pythagoras after his first visit to Sicily.

Tarrant is a keen observer of the ways in which Plato sought to communicate through different styles of dialogue and a changing mode of representing Socrates. The harsher, more abrasive Socrates of the early dialogues is gradually replaced by a more amenable figure, an indication that Plato wanted to make more apparent the difference between Socrates' style of philosophical enquiry and the point-scoring of the sophists.

Two studies [V, VI] examine the structure of the dialogues and attempt to analyze the method and effect of the purely dramatic dialogues (set out like a play with the names of each speaker in turn) compared with the narrative dialogues which allow for the complexity of an accompanying narrative which sets the scene and permits the author to 'comment' on the attitude of the speakers. Particularly intriguing here is the suggestion that the dramatic dialogues were originally intended for internal 'performance' in the school of Plato, where Plato would read them aloud himself and add narrative comments. The narrative dialogues, on the other hand, are intended for an external audience whose interpretation the author can to some extent control by his narrative framework.

Other themes which come up in this section include that of mature students. Plato, of course, in the *Republic*, deliberately reserved metaphysical instruction for students in their late years as he did not trust young students to act responsibly with such knowledge. But he could be equally dismissive of older students who could misbehave in the same way. Lastly, there is a balanced essay on the role of myth in the dialogues as an important form of discourse and communication.

In the second section, we have a number of closely argued articles on some key issues concerning the turning in Platonism of the first century BC away from the sceptical Academy to a more dogmatic Platonism and the gradual development of what we know as Middle Platonism. Tarrant makes a distinction in the latter between the early Middle Platonism of the first century AD with transitional figures like Plutarch and the Platonism of the second century with its greater interest in metaphysical principles, theology, and the life of the disembodied soul. Apart from Plutarch, the information we have about Platonists covered in this whole section is very fragmentary and their philosophical positions are highly nuanced but difficult to recover. I will pick out just a few of Tarrant's contributions.

The section begins with a careful and detailed examination of the epistemology of Philo of Larisa (early first century BC), a Platonist who, though adhering to a form of Academic scepticism, represents just the beginnings of a return to something more positive. A similar tendency to the more positive may be observed too in the Anonymous commentary on Plato's *Theaetetus*, which Tarrant persuasively places in the context of the renewed Platonism of the late first century BC and before Philo of Alexandria in the first century AD.

Another important text which, according to Tarrant, may be traced back to about the same period or slightly earlier is the so-called philosophical digression at 340a–345c in Plato's *Seventh Letter*. This digression, which is regarded as a later addition to the text—whether or not Plato is the author of the original letter—began, Tarrant argues, to be included widely in the Platonic text only later, since it seems to have been unknown to Philo of Alexandria and Plutarch.

Another indication of the movement from early Middle Platonism after Philo of Alexandria and Plutarch (in *De Iside*) is the disappearance of λόγος as a metaphysical principle. Tarrant neatly connects their use of λόγος as a structural principle, both transcendent and immanent, with what he calls the basic theory of 'transcended dualism' as seen in Eudorus of Alexandria (late first century BC), a theory which posits an ultimate principle, the One, above a dyad as constituent principles of the universe.

Yet another feature of this period is, according to Tarrant, a gradual return to the close reading of Platonic texts. He places the composition of the anonymous commentary on the *Theaetetus* (a large fragment preserved in a papyrus of the second century AD) in the period of Augustus/Tiberius. And he can demonstrate how Taurus, a Platonist of the second century AD, was a very clear and sophisticated reader of Plato's text. The evidence comes largely from Aulus Gellius' reminiscences in his *Attic Nights* of his student days in Athens when he studied under Taurus. Tarrant shows that even where Gellius does not mention Taurus by name it is easy to identify his work from the more stumbling efforts of Gellius himself.

Section 3 contains an essay on 40 λόγοι by Zeno that Proclus mentions in his commentary on Plato's *Parmenides* as well as four articles on Olympiodorus, a Platonist active in Alexandria in the sixth century AD. Long after the closure of the Platonic Academy in Athens, the pagan philosophical schools continued to operate in Alexandria, perhaps because it was a more liberal setting or perhaps because they avoided the public utterance of ideas that might be construed as subversive of Christianity. Tarrant points out that Plato's *Gorgias* was deemed by Olympiodorus to be about the demiurge. This, to us, rather odd characterization of the dialogue is based on the concluding myth. But Olympiodorus' point is that the demiurge is seen as a structuring principle for virtue as lived out in the world. In a similar vein, Tarrant restores (from remarks in his *Gorgias* commentary) Olympiodorus' interpretation of Plato's *Republic* as primarily concerned with ethics, i.e., with 'political virtue'—'political' in the sense of constitutive of harmony between the three parts of the soul. And he notes that Olympiodorus curiously shows no interest in the central analogies of Sun, Line, and Cave, that section of the *Republic* which interested earlier Neoplatonists. Tarrant also restores Olympiodorus' reputation as a historian and, not least, as a logician, demonstrating that some of the apparent mistakes in his interpretation of Plato's arguments (in syllogistic form) can be traced to the mistakes of the student whose notes constitute our text of his commentaries: in one case, there is clearly a mistake not of copying out from notes but of mishearing a word in the lecture hall (hearing ἀδικοῦσι instead of ἀτυχοῦσι).

It is the latter kind of really detailed scholarship and argument combined with a stimulating general grasp of the development of ideas in their intellectual context which makes this collection such a pleasure to read and a most useful work of reference.

Theodosius, Sphaerica*: Arabic and Medieval Latin Translations* edited by Paul Kunitzsch and Richard Lorch

Boethius 62. Stuttgart: Franz Steiner Verlag, 2010. Pp. x + 431. ISBN 978–3–515–09288–3. Cloth € 64.00

Reviewed by
Sonja Brentjes
University of Seville
brentjes@us.es

Theodosius lived probably during the first, maybe also the second, century BC in Bithynia, a region which belongs today to Turkey at the northwestern coast of the Aegean. According to Vitruvius, he was known for having built a universal sundial. Strabo lists him among the famous men of Bithynia. Three works on geometry and astronomy by Theodosius are extant today in various languages: *Sphaerica*, *De habitationibus*, and *De diebus et noctibus*. *De habitationibus*, soon to be published too by Kunitzsch and Lorch, discusses the phenomena caused by the heavenly revolutions as seen in a geocentric model of the universe. It explains which parts of the world the inhabitants of different zones can see. *De diebus et noctibus* deals with the different lengths of days and nights in the course of a year and explains their variations and other related phenomena. *Sphaerica*, the most important of Theodosius' three treatises, is about the geometry of the sphere. It consists of three books with 60 propositions (23 in books 1 and 2 each, 14 in book 3) preceded by a small number of definitions. Earlier texts on this subject were written by Autolycus of Pitane (*ca* 310 BC) and Euclid; a later and the most sophisticated ancient text (lost in Greek but extant in Arabic, Latin, and Hebrew translations) is Menelaus of Alexandria's *Spherics* (first/second centuries AD).

Theodosius' work is of historical significance for its depiction of the knowledge of spherical geometry in his period and for the manner in which it is presented. It shares its structural set up and type of proofs with Autolycus' two works. All three treatises reflect an approach closely related to Euclid's *Elements*. The similarities between the *Spherics* and the *Elements* go beyond this methodological aspect. Book 1 and book 2.1–10 of the *Spherics* appear to be a translation

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 119–123

of book 3 of the *Elements* from the circle to the sphere. Given the dearth of either direct or indirect early testimonies for the Euclidean *Elements*, this aspect of Theodosius' *Spherics* is very valuable. Moreover, since books other than the Euclidean *Elements* seem to have existed in the times of Autolycus and Theodosius, these similarities may inspire some future researcher to investigate in greater detail the traces of earlier works that these extant texts possibly contain.

Books 2.11–23 and 3 of the *Sphaerica* deal in purely geometrical form with matters of relevance to astronomy. This aspect explains why the *Sphaerica* became a sought after work when astronomy, astrology, and their mathematical foundations were taught in Late Antiquity, Islamic societies, medieval Jewish communities, and universities in various Catholic states of Europe. It found its stable position in a canonical set of textbooks which taught plane, solid and spherical geometry, planetary models, and the calculation of stellar positions. These textbooks were called in Antiquity the *Little Astronomy*, in Islamic societies the *Middle Books*. They were meant to be studied after Euclid's *Elements* and before Ptolemy's *Almagest*.

Other aspects of historical importance concern theorems that the *Sphaerica* shares with Autolycus' and Euclid's earlier texts and methods that are found only in later works. Although the positions of historians of ancient astronomy differ in regard to the interpretation of the relationship between Theodosius and his two predecessors, the possibility of using this textual overlapping as a point of departure for reflection on the preceding stages of spherical geometry should not be denied outright. The methods that Theodosius teaches only allow one to prove that some arc is greater than another one. In a few cases, he also determines ratios between arcs and compares them to ratios between line segments. These methods do not suffice however to solve practical astronomical problems such as finding the nightly hours from stellar positions. For the calculation of such quantities trigonometric methods are needed, and they seem to have been introduced shortly after Theodosius by Hipparchus (*ca* 190–120 BC). Hipparchus, apparently, was the first ancient astronomer to calculate a table of chords. On this basis, ratios between spherical arcs could be calculated. Thus, distances on the heavenly sphere could be determined quantitatively.

Theodosius' works are not only related to Euclid, Autolycus, Hipparchus, and Menelaus. They were also used in neighboring genres of astronomical literature such as the writings that included depictions of star constellations or provided surveys of astronomy, for instance in Geminus' *Introduction to the Phaenomena*.

The integration of Theodosius' three treatises into the corpus of textbooks for students of geometry and astronomy secured their survival for more than one and a half millennia. The number of Arabic, Latin, and Hebrew copies produced until the modern period testifies to their importance for classes taught at *madrasa* or universities and by private tutors. Kunitzsch's and Lorch's decision to edit one of the two Arabic translations and the shorter of the two versions that circulated since the 12th century in Latin is very welcome. Their work complements Claire Czinczenheim's edition of the Greek text [2000]. They provide an important basis for the study of these intermediary textbooks and their respective philological, codicological, textual, and class room properties.

The Arabic transmission of Theodosius' *Sphaerica* comprises two translations and three redactions. Kunitzsch and Lorch edit the anonymous translation represented by three manuscripts (Istanbul, Topkap, Ahmet III 3464, ff. 20v–53v; Lahore, private library M. Nabi Khan, pp. 185–281; Paris, BnF, hebr. 1101, ff. 1–53r, 86r–87r) [3-4]. The last one, as can be surmised from the *siglum*, is Arabic in Hebrew characters. The second copy describes its text at the end as having been revised by Thabit b. Qurra (died 901) but at the beginning of book 2 as his translation [2]. Its colophon claims also a relationship to a direct descendant of Thabit b. Qurra; it states that this earlier copy was transcribed in the Nizamiya Madrasa of Mosul in 554 h/1158 and that a century earlier (421 h/1030) some al-Hasan b. Sa'id had corrected the diagrams by collating his unreliable copy with a second manuscript [4].

The other translation into Arabic is ascribed once to Qusta b. Luqa and once, in all likelihood falsely so, to Hunayn b. Ishaq [2]. As usual with such ascriptions, things get more difficult over time. In the redaction of Theodosius' text that Nasir al-Din Tusi (1202–1274) completed in 1253, he claimed that Qusta b. Luqa translated the Greek text until proposition 3.5. Then, somebody else finished the work and Thabit b. Qurra revised it [2]. The two other redactions were made by Ibn Abi [al-]Shukr al-Maghribi (died between 1281 and 1291) and Taqi al-Din b. Ma'ruf (died 1585) [1].

Kunitzsch and Lorch did not pursue the issue of who translated and revised which parts of the extant Arabic texts. Their primary goal was to establish a critical edition of the anonymous Arabic version and its Latin parallel, and to explain the mathematical content of the Arabic text as well as particular features of the copies [7]. They established the Arabic text by collating the first two of three available manuscripts and comparing doubtful readings with the edited Greek text [6]. The diagrams of the Arabic text also underwent editorial procedures described and discussed by the two authors in detail in their notes on the diagrams [328–341]. The Latin text that they publish is a transcription of the oldest extant copy of the text (*ca* 1200) found in MS Paris, BnF, lat. 9335, ff. 12–19v corrected in the process of collation with 10 further manuscripts from the 13th and 14th centuries [5–6]. They identify it as a clear translation of the version of the three Arabic manuscripts mentioned above [5]. Due to its terminology and further linguistic characteristics as well as the inclusion of the *Sphaerica* among Gerard of Cremona's translations by his disciples, they identify this text as Gerard's work [5]. In their brief general remarks [7], Kunitzsch and Lorch direct the reader's attention to the fact that the Arabic and Latin texts contain extra material in book 1, definitions and early theorems not found in Czinczenheim's Greek edition [2000]. The established texts in the two languages are placed side by side in the book, which is of great advantage to the reader interested in comparing the translation practices.

The edition is followed by notes on the Arabic text in the second manuscript mentioned above by al-Hasan b. Sa'id, together with an English translation [313–315], several lemmas to 3.11 in the first and the third of the three extant Arabic copies, together with the Latin translation of the second lemma and two Latin notes on 2.dem.11. The mathematical summary [343–427] offers the reader who does not understand Arabic a translation of the definitions and enunciations of all propositions plus, for any reader who does not wish to do the labor herself, a summary of the main points of the proofs.

The editions, translations, and summary are carefully executed. They provide the interested researcher with a valuable text for further investigations. The two editors are to be congratulated for another fine result of their long years of cooperation.

BIBLIOGRAPHY

Czinczenheim, C. 2000. *Édition, traduction et commentaire des* Sphériques *de Théodose.* PhD dissertation, University of Paris 4 (Sorbonne). Paris.

Ancient Greek Music: A Technical History by Stefan Hagel

Cambridge/New York: Cambridge University Press, 2010. Pp. xx + 484. ISBN 978–0–521–51764–5. Cloth $115.00

Reviewed by
Alison Laywine
McGill University
a.laywine@mcgill.ca

Ancient Greek Music is an ambitious new book by Stefan Hagel. Its title happens to coincide with that of an ambitious book published by M. L. West with Clarendon Press in 1992. But the two books differ considerably in perspective. West set out to study 'the various elements that go to make up ancient Greek music as a performing art, as an object of theoretical inquiry, and as a cultural phenomenon' [1992, 327]. Hagel's book is more narrow in scope. This is perhaps signaled by its subtitle, 'A New Technical History'. The history that Hagel offers is not only technical in nature, it is also a history of technical problems and innovations in the actual music making of ancient Greece—at least to the extent that such music making is known to us. There seems to be no one technical issue that gives focus to the book, which ambles and rambles in a not especially direct or obviously coherent way. But there is one such issue that gives the book its point of departure: it is the one indicated by the Greek word «μεταβολή» and its affiliated forms.

In the most general sense, «μεταβολή» just means change, as when we speak of a change of fortune (τῆς τυχῆς) or changes of constitution (τῶν πολιτειῶν). Music can undergo change in many different respects but the change relevant here occurs in melody. We find this association of 'change' and 'melody' in book 2 of Aristoxenus' *Elements of Harmonics*:

> Ἐπει δὲ τῶν μελῳδουμένων ἐστὶ τὰ μὲν ἁπλᾶ τὰ δὲ μετάβολα, περὶ μεταβολῆς ἂν εἴη λεκτέον, πρῶτον μὲν αὺτο τί ποτ᾽ ἐστὶν ἡ μεταβολὴ καὶ πῶς γιγνόμενον—λέγω δ᾽ οἷον πάθους τίνος συμβαίνοντος ἐν τῇ τῆς μελῳδίας τάξει—ἔπειτα πόσαι εἰσὶν αἱ πᾶσαι μεταβολαὶ καὶ κατὰ πόσα διαστήματα. [Macran 1902, 38.3–8]

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 124–170

In English, this passage might be rendered as follows:

> Since some melodies are simple and others changing, it is needful to speak of change, i.e., to say first of all what change is and how it arises—I mean when a certain effect is brought about in the order of the melody—and then how many of these changes there are and at how many intervals.

Aristoxenus is speaking at such a high level of generality that the passage as such tells us only what any untutored listener knows already, namely, that melodies can change. Again we ask: 'In what respect? Is a change in dynamics or volume relevant?' If we consult the introduction to the *Harmonics* of Cleonides, a follower of Aristoxenus, we learn that a melody can undergo four kinds of change: that of genus (κατὰ γένος), 'system' (κατὰ σύστημα), τόνος (κατὰ τόνον), and musical composition (κατὰ μελοποιΐαν) [von Jan 1895, 13.20.1–2]. Change of τόνος is what Hagel takes to be relevant for his purposes (and what he ultimately takes to have been at issue for Aristoxenus as well).[1] But then the question is what is meant by 'change of τόνος', or, more generally, what is meant by τόνος as such?

If we consult Cleonides again, we learn that the word «τόνος» can be used in four ways: either to mean a note (φθόγγος), or an interval (διάστημα, i.e., presumably the interval of a whole tone as the difference between the fifth and the fourth) or the 'range of a voice' (τόπος φωνῆς) or pitch (τάσις) [von Jan 1895, 12.19.6–8]. The most obscure gloss, from our point of view, is the third. 'Range of voice' could be taken to mean register, as when we say that a little boy's vocal register is significantly higher than that of an adult man. But Cleonides cannot have anything quite so simple in mind because we could get away, for most purposes, with a crude distinction of four vocal registers: the very high, the very low—at the extremes of human singing—and then the not so high and the not so low, somewhere in between. Following Aristoxenus, however, Cleonides distinguishes 13 'vocal ranges' and he mentions them by name, starting with Dorian, Phrygian, Lydian, and so on. If it were just a matter of register, one would wonder why so many fine discriminations were necessary: they are indeed fine, since the 13 'ranges' are successively some kind of semitone apart from one another. But once the discriminations are

[1] Unfortunately Aristoxenus' treatment is lost. This is why we depend on Cleonides.

made, context indicates that melodies can move from one of these 'ranges' or τόνοι to another. Such movement will count as the kind of μεταβολή of special interest to Hagel. To signal its significance and peculiarity to melody, it is more usual to translate it into English not as 'change' but as 'modulation'. (Needless to say, 'modulation' would be an apt translation of «μεταβολή» in the other three senses, as well.)

If we try to get closer still to what modulation involves, we will be struck by the fact that, though the discriminations between τόνοι are fine and numerous, they are not indeterminate in number. It might be thought that the simplest melody, at least for the sake of argument, consists of two notes of different pitch. Since pitch is a quality of sound, whether musical or not, that is registered by the ear along a continuum of higher and lower with no apparent gaps, nothing prevents us from continuously modulating a simple two-note melody, i.e., such that the melody, *as a whole*, passes from a lowest given pitch to a highest without skipping any pitch along the way. As a matter of fact, this is what happens as a result of the so-called Doppler effect. Emergency vehicles in Germany today—whether ambulances or police cars on the chase—have sirens that repeatedly emit two notes exactly a fourth apart. If you have the misfortune of living on a busy street like Friedrichstraße in Berlin, you will hear such sirens racing towards and away from your domicile all day long. As siren and vehicle approach you, the two-note pattern will not only get louder, it will also rise in pitch—by about a semitone; as they get further away from you, the two-note pattern gets softer and lowers in pitch—again, by about a semitone. But though this semitone sets a limit on how high and low the two-note pattern can rise and fall, the shift in pitch within that semitone is continuous in both directions: it never appears to your ear that the two-note pattern misses a possible pitch along the way. If, then, the Doppler effect is a species of modulation, one might say that it produces 'fine' discriminations but that they will be indeterminate in number. By contrast, the fine discriminations associated with 'modulation', as presented by Cleonides, are exactly 13. That number is significant—all the more so, if we take the liberty of rounding it down to 12. Let me explain.

Given that we are accustomed to thinking of the octave as if it were a unit of length divisible into 12 equal lengths a semitone apart, it is natural to suppose that, for Cleonides, modulation involves taking a melody through each of the 12 semitones from one end of an octave to the other: or at least that it potentially could do so, if we really wanted our melody to make a stop at all 12 stations.[2] But having gone that far with the thought, we might just as well take Cleonides to be speaking of the circle of fifths. Dialing through the circle of fifths is equivalent to starting from a given note, rising an equal-tempered fifth and descending an equal-tempered fourth—a total of six times. If we keep our risings and settings within the compass of an octave, we will pass from one end of the octave to the other, making a stop at each of the 12 semitones in between. But if we now think of each of the stops we make, in precisely the order in which we make them, as a 'key' in our sense of the word, and if we think of our point of departure as the 'natural' key, then, with each subsequent stop we make, we will reach a key with one more accidental: from the natural key, we will move to a key with one sharp, then two sharps, then three, then four. If we continue in this way, we will pass through keys that are 'enharmonic' in our sense, i.e., the ones that we can think of indifferently as either having numerous sharps or numerous flats: five sharps-seven flats, then six sharps-six flats, then seven sharps-five flats. If we continue further still, we will reach keys with fewer and fewer flats, starting with four and moving successively to one, until we finally reach our point of departure, the natural key [see Figure 1]. The implication of invoking the circle of fifths is that, when Cleonides construes τόνος as 'range of a voice', he can be understood to speak of keys; and, when he speaks of 'modulation of τόνος' in this sense, he can be understood to mean change of key—as we do. At least, this is how Hagel understands Cleonides. But, what is more important, he takes the musical practice that Cleonides is responding to as having exploited modulation in just this sense [2000, 33–38].

[2] For the purposes of the exposition that follows, I am assuming equal temperament. Hagel himself believes that equal temperament was at the basis of modulation in ancient Greek music. At least, he takes Aristoxenus to have been operating with equal temperament, and he takes Aristoxenus to have been responding to the practice of modulation in ancient Greek music. See, for example, his earlier book *Modulation in altgriechischer Musik. Antike Melodien im Licht antiker Musiktheorie* [2000, 18–20]. I myself think that the question of equal temperament remains open.

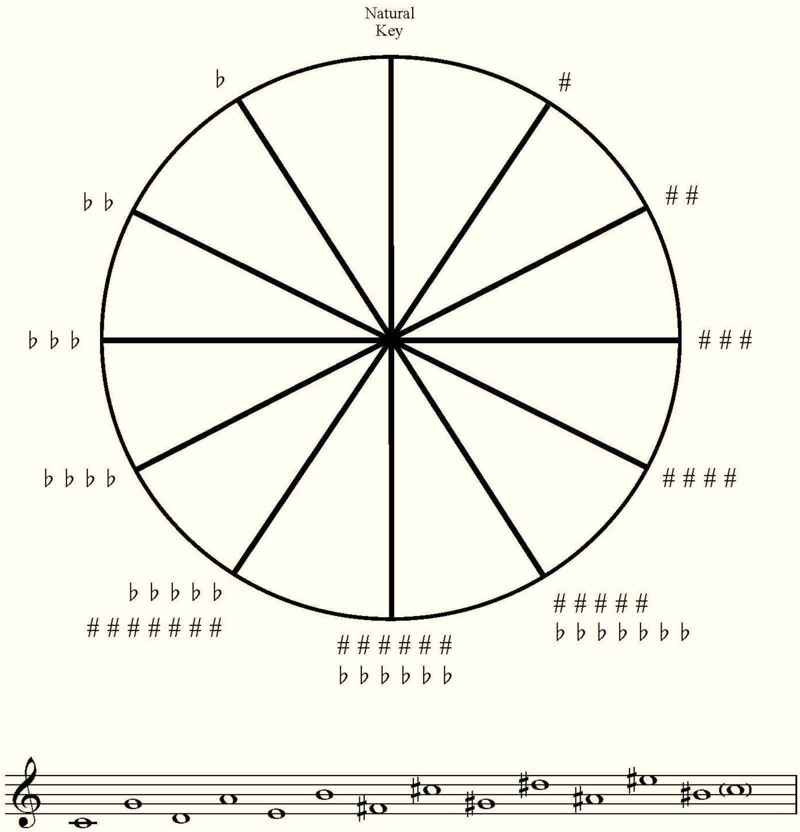


Figure 1 The circle of fifths

Having said all that, I should add that the modulation of a whole melody from one key to another is seldom of musical interest. Imagine a singer dialing ‘Frère Jacques’ through the circle of fifths. Since only the key is changed, the melody itself remains internally the same. From the listener’s point of view, the singer might just as well pick one key and sing the tune in it. This very natural thought suggests that modulation of key is of interest chiefly *within* a tune. As listeners, in other words, we will be more interested to hear a tune that starts in one key and switches midway to another. Imagine now that our singer sings a melody that starts in a natural key like our C-major. Imagine that the singer starts on the note that we take to be the tonic of the C-major scale, namely, C, ascends stepwise to the dominant

G but then modulates at G to the neighboring key of G-major by introducing the sharp that is characteristic of that key. She might do this, for example, by passing from G to F♯, only to return to G and rise to A. Perhaps she then descends stepwise from A back to C, but avoids F♯ in favour of F♮. If she does that, she will have modulated back to C-major. But though the modulation to G-major will have been short lived, it will have been enough to signal a break from the prevailing C-major environment and thereby introduce a little bit of variety. The distinctive mark of this escape will be that we can aggregate the relevant pitches so as to get three neighboring semitones where we would normally get only one. Had the singer remained in C-major, we would have got a semitone only between E and F. But having modulated from the one key to the other, we get semitones between E and F, then between F and F♯ and also between F♯ and G—though obviously not in that order, as I am imaging the tune. For Hagel, modulation of key in ancient Greek music was a phenomenon internal to melody, in the way I just characterized it. (He finds evidence for it in the surviving scraps of annotated music by looking for otherwise unexpected, implied sequences of successive semitones—like the relatively short, implied semitone train between E and G that I just found in the melody I imagined above.)

At this point, any reader interested in the subject and even minimally tutored in what, for lack of anything much better, I will call 'music of the Western World since the age of all those guys whose tunes we had to learn for childhood music lessons', will wonder what any of this could possibly mean. Does it mean that the Greeks had major and minor keys, as we do? Did they like to modulate from a given major key, say, to its related minor, or from a given minor key to its related major, as we do? Did their keys, whether major or minor, have degrees, as ours do? If so, did the first degree of their keys serve as the tonic and the fifth degree as the dominant such that if you played a tune in a certain key for a while and then ultimately moved from the dominant to the tonic, your listeners (if any) might well have the sense that you had come to the end (at least for now)? Did ancient Greek musicians employ openings and cadences that exploited the structure of their keys? The answer to these very reasonable questions remains (so far as I can tell): 'Who knows?'

What the evidence allows us to say is this. The theoretical writings that survive testify to the recognition in ancient Greek music of an ideal scale spanning an octave but indeterminate in pitch, and built up out of two tetrachords spanning a fourth and separated by a disjunctive tone.[3] The tetrachords of the ideal scale could be internally arranged in three different ways. From lowest to highest pitch, you could have intervals of quarter-tone, quarter-tone, ditone: this would give you 'enharmonic' tetrachords, in the Greek sense of the word. You would get 'chromatic' tetrachords, if the sequence of your intervals were semitone, semitone, tone-and-a-half. You would get diatonic tetrachords from semitone, tone, tone. The fact that there were three 'genera' of the tetrachord went hand in hand with the fact that the two notes bounding a given tetrachord were always fixed in pitch (whatever the pitch register of the scale as a whole), whereas the two inner notes could, in principle, vary in pitch, depending on the genus. The lower of the two moveable notes could vary by no more than a quarter-tone, whereas the higher could vary by a whole tone. The notes of the ideal scale all had names. The only one I will trouble my readers with is that of the lowest note of the disjunctive tone. It was called 'mesē' ('the one in the middle').

The scale as a whole was called the 'Greater Perfect System' [see Figure 2]. There was a second ideal scale called the 'Lesser Perfect System' [see Figure 2]. It was distinguished by the absence of the disjunctive tone. Hence, it spanned an octave less than a tone. The higher fixed note of the lower tetrachord was identical to the lower fixed note of the higher one. This note too was called 'mesē'. But to distinguish it from the 'mēse diezeugmenōn' of the Greater Perfect System, it was referred to as 'mesē synnēmenōn'—the 'one in the middle of the conjunctive notes' as opposed to the 'one in the middle of the disjunctive notes'. The interest of these two ideal scales is

[3] The ideal system that I have in mind here and later in this review is the so-called Greater Perfect System. But the Greater Perfect System had more than two tetrachords, as did the other ideal system which will also come into play in what follows. I should like to clarify that whenever I speak of the Greater Perfect System, I really mean the central octave of that ideal system; and that when I speak of the Lesser Perfect System, I really mean the center of that ideal system, i.e., the two central conjunct tetrachords that are missing the disjunctive tone characteristic of the central octave of the Greater Perfect System.

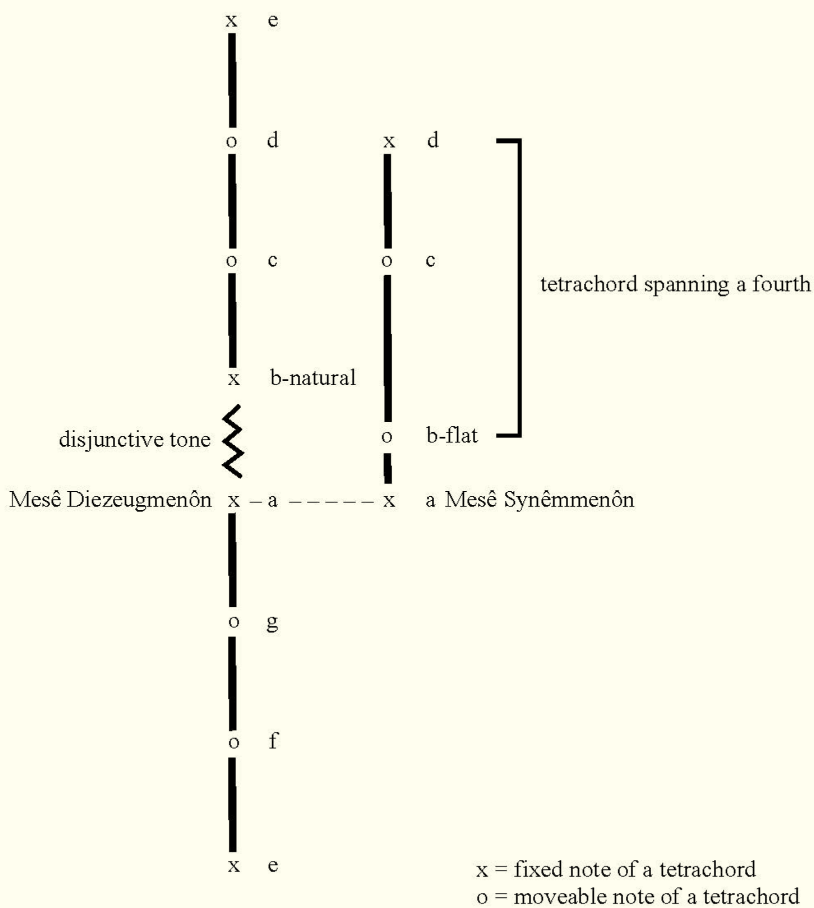


Figure 2. The Greater and Lesser Perfect Systems (given in the diatonic genus of the tetrachord)

that we can think of them as sharing a lower tetrachord and as thus forming a single path in the upward direction, until we get to *mesē*, at which point there is a fork in the road: the melody can either travel into the upper disjunct tetrachord of the Greater Perfect System or into the upper conjunct tetrachord of the Lesser Perfect System [see Figure 2]. For that matter, it can travel up into the one, retrace its steps and turn at *mesē* in order to travel up the other as well. Such a turn may be regarded as a 'modulation of key' in our sense of the phrase.

This can be seen if, say, we take the whole forked path, stipulate a genus for all three tetrachords—preferably the diatonic for convenience—and assign modern note names to the common lower tetrachord and the disjunct upper tetrachord of the Greater Perfect System in a 'natural' key. That will give us, in order of lowest to highest: e, f, g, a (*mesē diezeugmenōn*), b, c, d, e'. But in order to notate the conjunct upper tetrachord of the Lower Perfect System, we will need one accidental, namely b♭: e, f, g, a (*mesē synnēmenōn*), b♭, c, d [see Figure 2]. This will be like modulating to a key with one flat from a natural key. If a singer or some other musician were to perform the modulating turn on the forked path, analogous to the modulation from C-major to G-major in the little tune that I imagined earlier, there would be, in the neighborhood of a, that is, of the *mesē diezeugmenōn* and *mesē synēmmenōn*, two extra semitones that would not have come into play had the melody remained either in the Greater Perfect System or in the Lesser Perfect System: a-b♭, b♭-b♮ and b-c. Had the melody remained in the GPS (as I will refer to it henceforth), there would have been only the semitone between b and c. Had it remained in the LPS, there would have been only the one between a and b♭.

Here is an important implication of all this. Suppose that, after traveling upwards into the GPS, the melody explores the upper conjunct tetrachord of the LPS. If it advances to the upper fixed note of this tetrachord, a fourth above *mesē synnēmenōn*, it might naturally come to treat the disjunctive tone of the GPS as having now been shifted to this higher perch [see Figure 3]. In that case, we may regard the note *mesē diezeugmenōn* (MD) as having been raised a fourth. For convenience, let us refer to it as MD*. This is indeed equivalent to a 30° turn of the circle of fifths in the flatward direction and, hence, to a modulation to a key of one flat from the natural key [see Figure 1]. This is all to the good because it confirms the idea we have been exploring, namely, that the relation between the GPS and the LPS is such as to provide opportunities for 'modulation of key' in something like our sense of the word. But having gone this far with the idea, there is no reason not to push it as far as we can. For let us now regard the note a whole tone below MD* as the higher fixed note of the upper conjunct tetrachord of the LPS in a second appearance. The effect of this move will be to give us a *mesē synnēmenōn* a whole tone below its first sounding. For convenience, we may refer to it as

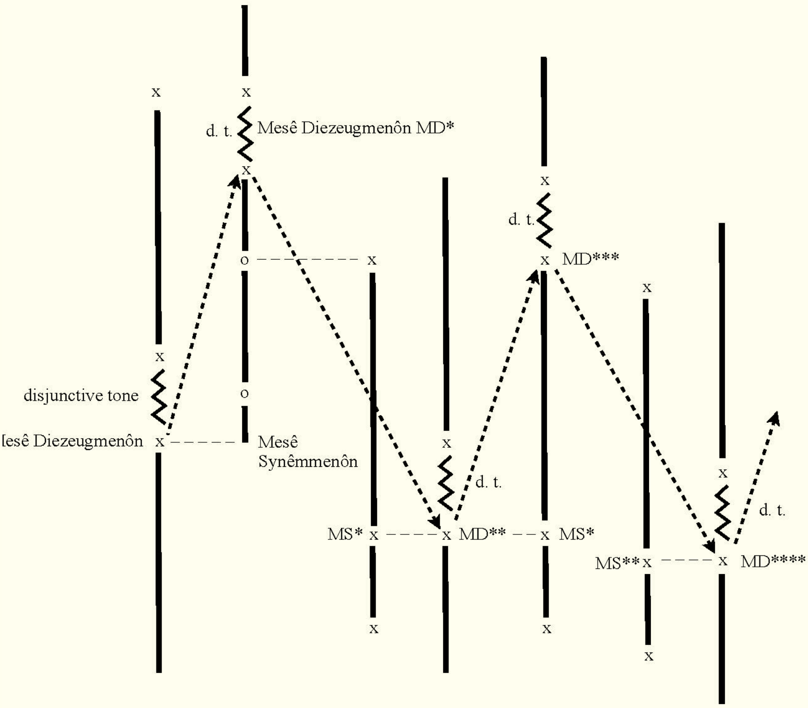


Figure 3. A sample modulation. Follow *mesē diezeugmenōn* (up a fourth, down a fifth, and so on) in the flat direction

MS*. If we now choose to think of this note instead as figuring in the GPS as MD**, we will have lowered MD* a perfect fifth—another 30° in the circle of fifths in the flatward direction, so that we now find ourselves in a key notated not with one flat, but two. We can keep shifting MD up a fourth and down a fifth. But each shift will bring us another 30° in the circle, i.e., another flat away from our point of departure in the natural key. We could conceivably shift MD up and down six times. If we did so, then, provided we took the octave to be divided into 12 equal semitones, we will have closed the circle of fifths.[4]

4 Again, let it be noted that all of this is presupposing equal temperament for the sake of exposition alone.

By spelling out, as I have, the implications of the fork in the path at the intersection between the GPS and the LPS, we get no closer to an answer to the reasonable questions about modulation in ancient Greek music that will come even to the mind of those minimally tutored in 'music of the Western World since etc.'. Sketching out the forked path and where it might lead to tells us absolutely nothing about how ancient Greek melodies might actually have moved along that path. All it tells us is that, in principle, we could assign a melody that travels far and wide a changing key signature with sometimes more or sometimes fewer accidentals. But knowing the number of sharps or flats in a key signature does not, all by itself, indicate anything about mode (major, minor, or what-have-you), much less what the musical conventions might be for establishing in the listener's ear melodic presence in a given key or departure for some other key. Still, even this much is interesting because it seems to support the hunch that ancient Greek music might be thought—perhaps even by ancient Greek theorists themselves—to 'change key' in a way that we would now recognize as dialing through the circle of fifths.

'Might be thought' is one thing, though; 'really was so thought by the players of the game' is another. Hagel tried to secure the second modality against doubt by looking for evidence of changing keys in the surviving musical fragments as they had been collected and presented by E. Pöhlmann in *Denkmäler altgriechischer Musik* in 1970. That project formed the basis of Hagel 2000, a stimulating and imaginative book. Its results are the point of departure for the book under review. For that reason, I cannot avoid discussing it in some detail.

The program of Hagel 2000 is worthwhile. Its hope is to learn *something* from the surviving musical fragments about how real melodies behaved, to see whether they behaved as the surviving theoretical treatments say they should and whether the theoretical treatments are illuminated by them in turn. (But our hope is that the shuttling from melody to theory and back again will not be such as to spin us in a vicious circle). The program faces the very serious obstacle posed by the paucity and fragmentary nature of materials that span almost 1000 years (fifth century BC to the fourth century AD). Still, something is better than nothing at all. For the purposes of Hagel 2000, the surviving scraps are enough to warrant a spirited stab at the question whether we can find evidence of deliberate modulation in them. The answer to the question is supposed to be 'Yes'.

The most interesting fragment for Hagel's purposes is Athenaeus' 'Delphic Paean' (128/7 BC). It gives us lengthy stretches of continuous melody, mostly—to relatively—free of lacunae until the end where it peters off into oblivion. Like all other surviving scraps of music, it follows the system of notation peculiar to ancient Greek music, which is quite different from ours. One way in which that system differs from ours is that it assigns a different set of signs for each τόνος or key (as we will suppose). Keys may share signs but no two keys have precisely the same set of signs.[5] From the notation of Athenaeus' 'Delphic Paean', we can tell that the first eight lines of the piece are in the τόνος or key called Phrygian. We can think of the melody as based on a scale with two notional tetrachords separated by the disjunctive tone, the lower note of which is Phrygian *mesē diezeugmenōn*. We cannot determine the genus of the lower tetrachord, because it is conspicuously defective. It is missing its upper moveable note—the note that normally disambiguates genus, since it occupies a different place in each of the three genera. From the listener's point of view, the lower moveable note could all by itself equally well figure in any one of them, either as the first moveable note in either the chromatic or the diatonic, or as the second moveable note in the enharmonic. The upper tetrachord is notated unequivocally as diatonic. It has

[5] I must beg forbearance of the reader. I can find no more perspicuous way to discuss the melodies of interest to Hagel—and others—than by reference to the ancient Greek system of musical notation. That will seem alien to anybody at home in modern note names and staff notation. But part of the problem is precisely how these melodies should be transcribed into our notation. So I will provide a diagram of the relevant keys, in their ancient notation, and their relationships to one another; and I will provide modern note names but only after I have provisionally (i.e., for the sake of argument) accepted certain assumptions. This means that I will provide modern note names only in my discussion of Athenaeus' 'Delphic Paean'. In the case of the Ashmolean fragments which I discuss later, where the note signs will, by then, be familiar to the reader, and where I will argue everything is up for grabs, I will not provide modern note names. One important source for the system of ancient Greek musical notation is the tables of Alypius, which can be found in von Jan 1895. I will frequently refer to these tables as the 'Alypian tables'. Our other source is Aristides Quintilianus, *De musica*, 1.11—more specifically, the so-called Wing Diagram in that chapter.

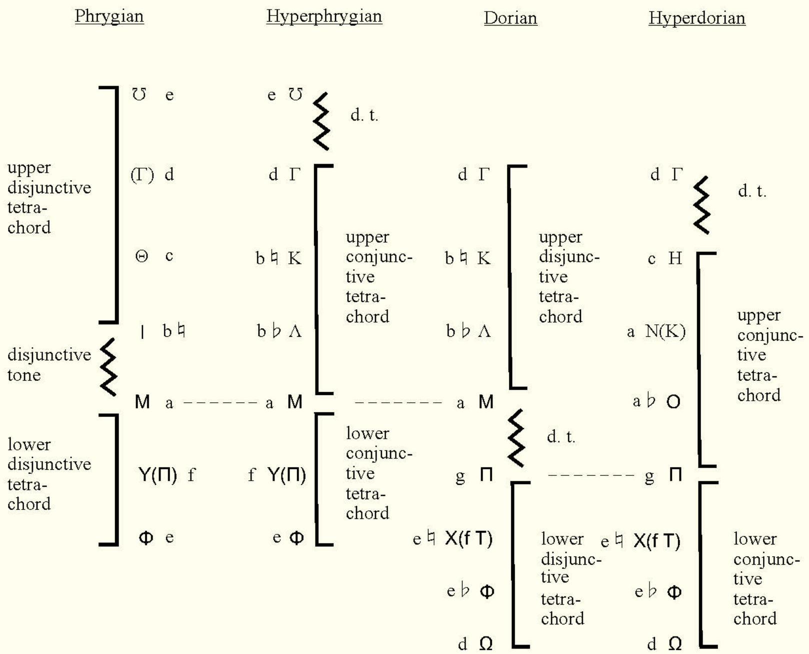


Figure 4. The scalar systems presupposed by Athenaeus' 'Delphic Paean' in Greek vocal notation (given in the chromatic genus of the tetrachord with alternative diatonic notes in parenthesis)

both lower and upper moveable notes. The upper moveable note is indicated by a sign that is specially reserved for the diatonic genus of this tetrachord. That sign looks like this: Γ.[6] Hagel thinks that Γ serves as a pivot point for modulation. Indeed, by the beginning of the second part of the paean, the melody has clearly migrated into the neighboring τόνος, the one called Hyperphrygian. This is plain from the notation. Is the change of τόνος here really a modulation in the sense that we have been discussing?

[6] That sign looks like the Greek letter «Γ» because it *is* the Greek letter gamma. Athenaeus' 'Delphic Paean' is transcribed in the notational system reserved for singers rather than instrumentalists, which uses the letters of the alphabet for the central vocal register.

The answer depends, at least in part, on the way the change is brought about and on the relation between Phrygian and its Hyper next-door neighbor. It is easy to see that the two τόνοι might be thought of as relating to each other as GPS and LPS [see Figure 4 for what follows]. Phrygian *mesē diezeugmenōn* and what we might conceivably regard as Hyperphrygian *mesē synnēmenōn*[7] are indicated by the same sign—M—and coincide in pitch. They would normally share a common lower tetrachord, reflected in the notation by the use of the same signs. But the upper tetrachord in Phrygian proceeds in the upward direction from the disjunctive tone, whereas its Hyperphrygian counterpart does not. This means that, though Γ is a fourth above M in both τόνοι, it is only in Hyperphrygian that they both serve as the two bounding notes of a tetrachord. Next door in Phrygian, Γ falls within the tetrachord set immediately above the disjunctive tone. For, as I mentioned earlier, Γ is the upper diatonic moveable note of this tetrachord according to the conventions of the notation. Passing through M would be one way for a melody to move from the Phrygian equivalent of the GPS to the Hyperphrygian equivalent of the LPS. But another way would be to have the melody travel upwards in the Phrygian equivalent of the GPS and then get off at Γ and descend from there through the Hyperphrygian equivalent of the LPS to M. Something like that is what we find in the transition from the first part of Athenaeus' 'Delphic Paean' to the second part.

I have to be cautious and say 'something like that', because the state of things here is messier than my remarks might otherwise suggest.[8] But it does seem on balance that Γ, rather than M, is the point of transition from Phrygian to Hyperphrygian. This means, however, that we have a shift from GPS to LPS. Assuming that the conjunctive tetrachord of the LPS, whose lowest bounding note we are regarding as Hyperphrygian *mesē synnēmenōn*, is chromatic and,

---

7 In fact, strictly speaking, it turns out to be Hyperphrygian *hypatē mesōn*, the lower bounding note of the tetrachord whose top note is Hyperphrygian *mesē diezeugmenōn*. But that detail and its implications do not affect the point here.

8 For one thing, there is a lacuna at the beginning of line 9—the first line of the second section—and because the last note of the first section is a fourth below M, there is a significant downward leap, after the upwards noodling in the Phrygian equivalent of the GPS in the first section and before the upward noodling resumes at the beginning of the second section.

given that the tetrachord set immediately above the disjunctive tone in the Phrygian equivalent of the GPS is unequivocally notated with diatonic note signs, the effect of this shift is to present the listener with three neighboring semitones, where he or she would otherwise have heard only one, or perhaps two. Had the melody remained in Phrygian, the listener would have heard a semitone only between the lowest bounding note of the upper disjunctive tetrachord and its lower moveable note. Had it been confined to Hyperphrygian, there would have been two semitones: the two lowest intervals—the so-called chromatic πυκνόν—of the *synnēmenōn* tetrachord bounded by M and Γ. But the movement from Phrygian to Hyperphrygian gives us all three, though not in succession. Not surprisingly, it becomes convenient as a result of this shift to transcribe the second section with one flat more in the key signature than the first section.[9] Indeed, this is what we find in the transcription of Athenaeus' 'Delphic Paean' printed in *Documents of Ancient Greek Music*, the new and improved version of Pöhlmann 1970, edited by Pöhlmann together with M. L. West and published in 2001, a year after Hagel 2000.[10]

Phrygian and Hyperphrygian are next-door neighbors. Modulating to the one from the other takes us no further than modulating from C-major to F-major. But the remarkable thing about Athenaeus' 'Delphic Paean' is that it modulates to more remote keys. At least that's the claim of Hagel 2000.

Everybody agrees that something is afoot here. The problem is how to understand what is going on. No sooner has the shift from Phrygian to Hyperphrygian taken place in line 10 [Pöhlmann and West 2001, no. 20] than a note is added to the mix that belongs to neither of the two original τόνοι. The note in question is indicated by the sign that looks like this: O. Pöhlmann and West characterize O as 'exharmonic' [2001, 73]. Hagel argues instead that we should take it as a modulation two doors down to Hyperdorian, i.e., at three flats removed from Phrygian. (In fact, he argues, at the end of the day,

[9] I want to insist that it is a matter of convenience, assuming that, for the purposes of transcribing the piece into modern notation, we want to avoid cluttering up the staff with lots of accidentals.

[10] I should note too that, for the purposes of my discussion of Athenaeus' 'Delphic Paean', I am referring to Pöhlmann and West 2001 rather than to Pöhlmann 1970. The paean is Pöhlmann and West 2001, no. 20.

that it is an 'honest-to-God' modulation to a key even more remote. But we will come to that in due course.)

The idea might be stated as follows. There is, in principle, a path that one can take from Phrygian to Hyperdorian, the τόνος in which O normally appears, simply by repeating the shift in the flatward direction that took us from Phrygian to Hyperphrygian the appropriate number of times [see Figures 4 and 3]. Yet another effect of modulating from Phrygian to Hyperphrygian was to transpose Phrygian *mesē diezeugmenōn* up a fourth. So when Athenaeus' melody went up the Phrygian upper disjunct tetrachord from M to Γ and prepared to modulate from Γ, it began to treat Γ as the lower note bounding the disjunctive tone. For since Phrygian Γ is the upper diatonic moveable note of its tetrachord, there is a whole tone between it and the note above it. That interval is preserved in Hyperphrygian. But since Hyperphrygian Γ is the top bounding note of a tetrachord, the whole tone above it becomes the disjunctive tone, which means that Γ becomes Hyperphrygian *mesē diezeugmenōn*. This is, as I say, to transpose *mesē diezeugmenōn* up a fourth. We could, as we did earlier, refer to it now as MD*. Suppose that we subsequently transpose it down a fifth. Then, as it turns out, we will have modulated from Hyperphrygian to a key with one more flat. That key is Dorian. In Dorian, *mesē diezeugmenōn* lies a whole tone below M. It is indicated in the notation by the sign: Π.[11] We could, as we did earlier, refer to this note as MD**. To get to our intended destination in Hyperdorian, a key with one flat more than Dorian, we would have to raise MD** a fourth. This gives us MD***, which falls a whole tone *below* Γ. It is notated by H.[12] The note indicated by O is the lower moveable note in the Hyperdorian tetrachord whose upper bounding note is H, taken to be MD***. If we assume that this tetrachord is chromatic, then O is a semitone in pitch above Π and a semitone in pitch below M.

That there is a modulating path from Phrygian to Hyperdorian is a trivial consequence of our reflections on the forked path defined by GPS and LPS taken as a single system. The question, which Hagel

[11] Note, however, that Π does not appear anywhere in what survives of Athenaeus' melody.

[12] Note, however, that H does not appear anywhere in what survives of Athenaeus' melody either.

wants to answer in the affirmative, is whether Athenaeus took this path—or, at any rate, something like it: as I say, the modulation Hagel takes Athenaeus to have carried out is, in fact, to a much more remote key.

Let me be clear about what is at stake. We can distinguish two different questions in the case at hand. The first is whether we can characterize Athenaeus as taking the multiple forked path to Hyperdorian (or perhaps beyond). The second is whether he deliberately took it, i.e., whether he would be willing to characterize himself as doing so. I take Hagel to be answering the second question in the affirmative (though ultimately this answer will pertain to the more remote modulation for which I have been sending up these red flags). Since Hagel takes Athenaeus' self-understanding to be correct, he—Hagel—necessarily answers the first question in the affirmative as well. But it is the second question that exercises him and upon which he expends his considerable energy and ingenuity.

The plausibility of Hagel's argument gets a boost from the notated melody itself. The first modulation of the piece—the one that occurs in line 10 of Pöhlmann and West 2001, no. 20, in the transition from the first section to the second—is brought into focus by the contrast between the Phrygian upper disjunct tetrachord and the Hyperphrygian upper conjunct tetrachord. The former tetrachord sits on top of the disjunctive tone, indicated in ascending order by these signs: M and I. The tetrachord itself is notated in ascending order by these signs: I, ϑ, Γ, Ʊ. For convenience, we can notate the whole sequence, from lowest pitch to highest, using modern note names in a natural key. That would give us: a, b, c, d, e. The Hyperphrygian upper conjunct tetrachord is indicated by these signs: M, Λ, K, Γ. In modern note names, that gives us: a, b♭, c, d. We can find unequivocally Phrygian strands of melody in lines 1 through 9. They stand out by their inclusion of b♮, indicated by ϑ. Thus, in the middle of line 6, we have ...M, I, ϑ, I, ϑ, Γ.... (In our notation, that would give us: a, b♮, c, b♮, d.) We can find an unequivocally Hyperphrygian strand of melody in line 10 at the beginning of §2. It stands out by its inclusion of b♭, indicated by «Λ». (Note: assuming that the Hyperphrygian conjunct tetrachord is chromatic, and assuming equal temperament of the whole business, it will turn out that Hyperphrygian K and Phrygian I coincide in pitch, namely, b♮, as I am notating it.[13] This is one respect in which the ancient Greek notation may have an advantage over our modern notation: it can indicate difference of τόνος even when, as here, we might well have two notes of the same pitch.) It is in the middle of this otherwise Hyperphrygian line 10 that the seemingly 'exharmonic' O (a♭) is introduced for the first time. Thus we have K, Λ, M, O, K, Λ, K, Γ...(In our notation, that would give us: c, b♭, a, a♭(!), c, b♭, c, d.) How is Hyperdorian O possible in this decidedly Hyperphrygian environment?

Just as Phrygian and Hyperphrygian have a tetrachord in common on the way up to M, so Hyperphrygian and Dorian share a tetrachord in common on the way down from Γ [see Figure 4]. In descending order, it is the familiar: Γ, K, Λ, M—the tetrachord distinguished by b♭, as indicated by Λ. In both Dorian and Hyperphrygian, Γ and M are both bounding notes of a tetrachord, namely *this* tetrachord. The difference between the two τόνοι is the placement of the disjunctive tone. In Hyperphrygian, the disjunctive tone sits on top of this tetrachord, and hence Γ is *mesē diezeugmenōn*. In Dorian, the disjunctive tone is suspended from M, so that *mesē diezeugmenōn* is a whole tone below M, as indicated by Π[14] —a note we may therefore render as 'g' in our notation. The path to O from Hyperphrygian is through this shared tetrachord with Dorian. For if we think of this shared tetrachord as Dorian, and if we think of it, moreover, as the GPS part of the forked path, then getting to O is no more complicated than hopping off at our intended LPS stop.

Now none of this sheds any light on Athenaeus' self-understanding. We know that his 'Delphic Paean' can be understood to travel along a path, from Phrygian to Hyperdorian, equivalent to a turn through the circle of fifths that leaves us with three flats more than we started with. But we do not know how Athenaeus himself understood what he was doing. All that anyone can do is to comb the evidence

[13] Having said that, I think I should add that it is an open question in my mind whether K and I *do* coincide in pitch. That will be so, if all the relevant tetrachords are chromatic and the semitones are all the same 'size'. But I do not think that we should take this for granted. I address this sort of issue below in my discussion of Hagel's analysis in his new book of the melodies from the Ashmolean fragments [Pöhlmann and West 2001, nos 5–6].

[14] But note, again, that Π does not appear anywhere in what survives of Athenaeus' melody.

for clues and think again about his melody in light of them. This is what Hagel does. The result is both imaginative and stimulating. But it is also totally speculative. Speculation can be a very good thing when it invites others to think hard and long about a question. But it can also lead into temptation. Hagel could not resist; we can and should.

Having made such a pronouncement, I am now obliged to discuss the problem of what can and cannot be said about Athenaeus and his 'Delphic Paean'. Though I did not set out to review both Hagel 2000 and the present book, Hagel 2010, I believe that this discussion is unavoidable, not just as a matter of fairness to the author but because of the intrinsic interest of Athenaeus' melody, because its analysis is so important to the project of Hagel 2000, and because this analysis is not reprised in Hagel 2010—not at length or in detail—which means that the reader of the new book who is innocent of German will be missing an important presupposition that the later book seems to rely on. Then, last of all, both books exhibit the same weakness for speculation.

The best way to convey my unease about Hagel 2000 is to focus on lines 13 through 16 of Athenaeus' paean [Pöhlmann and West 2001, 64]. The interest here is that the Hyperdorian O appears four times with Y, a note that, as I see it, may only be construed as the lower moveable note of the tetrachord shared by Phrygian and Hyperphrygian. Thus, we get:

YOM	line 13: f, a♭, a,
OYO	line 14: a♭, f, a♭,
YOM	line 15 and
ΛMOYOMΛ	line 16: b♭, a, a♭, f, a♭, a, b♭.

By the time we get to line 13, we will no longer be surprised to hear O in succession with M because we will already have heard a pairing of these two notes three times since the very first introduction of O in line 10. If we understand O as a modulation into Hyperdorian, we will understand M to be the note that facilitates this move since it is common to Dorian, Hyperphrygian, and Phrygian alike: it is the natural jump-off point to O from these more remote keys. (Note, however, that M does not appear in Hyperdorian: O is the only note in the whole piece from this key, which is part of what makes it so distinctive). By the very same token, it is not surprising to hear M and Y in the same melodic context since they are both shared by Phrygian and Hyperphrygian. The surprise is to hear Hyperdorian O sandwiched between them.

Hagel has an explanation for this. He says that Y can be understood as indicating an even more remote modulation—with the aid of O—into Hyperiastian (also known as Hyperionic), a key one semitone lower than Hyperphrygian and so much further in the circle of fifths that it has left the flats behind and has four sharps (assuming for the sake of convenience that Phyrgian, our point of departure in all this, is the natural key). He says, moreover, that this modulation to Hyperastian is clearly intended by Athenaeus. The issue is not merely how we can characterize the path taken by Athenaeus's melody; it is rather what is—or was –going on inside Athenaeus' head.

Hagel tries to support his claim with the following argument. It begins with the observation that O was first introduced in line 10 as the last note in a descending sequence of three semitones starting with the higher moveable note of the tetrachord shared by Hyperphrygian and Dorian: K, Λ, M, O (b♮, b♭, a, a♭). The next observation is that this is the last time in the piece, as it has survived, that we are treated to that many semitones in succession. This leads to the most important observation of all: in lines 12 through 16, O appears seven times in either an ascending or descending sequence of two and only two semitones, and indeed, the very same two semitones: O, M, Λ (four times: a♭, a, b♭); Λ, M, O (three times: b♭, a, a♭). The fact that these two patterns are repeated so often in such a short time suggests—to Hagel, at least—that they might be taken to be a 'chromatic pyknon' in their own right, i.e., the bottom of a chromatic tetrachord, with O serving as its lowest, fixed note and M and Λ serving as the lower and higher moveable notes respectively, such that O and M form some kind of semitone, and M and Λ form the next higher semitone.[15] If that is the case, then Y has a new function once it is introduced in line 13. It can no longer be taken as the first moveable note of a Phrygian or Hyperphrygian tetrachord with M as its higher fixed note (*mesē diezeugmenōn* in the first case; *mesē*

[15] Note that the ascending three note sequence of two successive semitones—M, Λ, K (a, b♭, b♮)—is the chromatic πυκνόν of the tetrachord shared by Hyperphrygian and Dorian; it appears in line 12.

synēmmenōn, as we have been thinking of it, in the second). It would now have to be treated as the higher moveable note in a tetrachord whose higher fixed note is a semitone lower than M, namely O. A quick look at the Alypian tables of keys shows that the tetrachord of this description can only belong to Hyperiastian, eight cranks of the circle of fifths away from our point of departure in Phrygian or five cranks away from the tetrachord shared by Hyperphrygian and Dorian.

It might be objected that, if the interval OY (a♭, f) indicates a modulation to Hyperiastian, we would expect Y to be noted with a sign at home in this key and not in Phrygian-Hyperphrygian. For, again, a quick look at the Alypian tables shows that the higher moveable note of this tetrachord in Hyperiastian is normally signaled by T. Hagel anticipates this objection. He points out that the presence of T would have caused hopeless confusion. For T also appears in the equivalent tetrachord of Phrygian and Hyperphrygian with the same function—that of a higher moveable note—but a semitone higher (f♯). (This is one of the disadvantages of the ancient Greek notational systems: notes of different pitch sometimes have the same sign.) Actually, it is worse than this in the case at hand because the two tetrachords also have the sign Φ in common. But while this note would be at the same pitch in both keys (e♮), if we are assuming equal temperament, as Hagel does, it clearly has a different function (as we saw above for Phrygian I and Hyperphrygian K): in the Phrygian-Hyperphrygian tetrachord, Φ is the lower *fixed* note; it is the lower *moveable* note in the Hyperiastian tetrachord. The result is that we cannot expect Athenaeus to have transcribed his Hyperiastian notes with Hyperiastian signs without risk of totally confusing his singers. Hagel's point here is perfectly cogent, so far as it goes. But it raises the question: 'How can we be so sure, just from the appearance of O and Y, that we are really in Hyperiastian?'

Hagel's answer is to appeal to the immediate melodic context in which Y is introduced for the first time in line 13 [Pöhlmann and West 2001, no. 20]. In the lead up, we find melodic strands that are Phrygian with a Hyperphrygian twist of K and Λ. Thus, from the last note on line 12, we have

K, Λ, Γ, M, Ʊ, ϑ, I, ϑ, Γ, ϑ, Y, O, M, Λ, M

(b♮, b♭, d, a, e, c, b♮, c, d, c, f, a♭, a, b♭, a).

For Hagel's purposes, the sortie back into Phrygian (Γ, M, Ʊ, ϑ or d, a, e, c) is significant because the Phrygian note immediately preceding Y is ϑ, which means that the interval between them is a downward leap by a fifth (namely, the fifth from c an octave above middle c to f). That is supposed to be significant because we can take ϑ to be equivalent to the Iastian H, the second chromatic moveable note in the Iastian tetrachord above the disjunctive tone, whose lower bounding note is O. Hence, the downward leap of fifth of ϑY, though notated in Phrygian, is equivalent to a leap from Iastian into a tetrachord it shares with Hyperiastian. The effect of this modulation between neighboring keys is to modulate from Hyperdorian to a key a semitone lower, i.e., to Hyperiastian, which is at a far greater remove in the circle of fifths—by no less than five cranks, as we saw. That this really is Hagel's argument can be seen on page 74 of his monograph. That it is supposed to get us inside Athenaeus' head is clear too. Hagel speaks explicitly of 'die Absicht des Komponisten' ('the composer's intention') and of 'die geplante Modulation um einen Halbtonschritt' ('the planned modulation by a semitone'). On page 73, in the introduction to this argument, he leads off with this:

> *Daß der Komponist nun tatsächlich eine solche entfernte implizite Modulation um einen Halbtonschritt im Sinn hatte....*

> Now that the composer did indeed intend such a remote, implicit modulation by a semitone....

The argument, however, establishes nothing of the kind. It shows only that a stretch of melody, whose notation indicates melodic travel from Hyperphrygian to Phrygian and back, with a seemingly out of place O in the middle of it all, can be redescribed as melodic travel from Hyperphrygian to Iastian to Hyperiastian by way of notes that the latter two keys have in common with Phrygian, Hyperphrygian's immediate neighbor and the key we are taking as 'natural'. But this raises the question whether we have any good reason to describe the melody this way. Since what remains of Athenaeus' 'Delphic Paean' begins unambiguously in Phrygian and carries on in this key for the first nine lines with a single Λ pointing ahead to the Hyperphrygian bits on the horizon, it is more natural to characterize the notes in lines 12 through 16 that are indicated by unambiguously Phrygian signs as—well, er, uhmm—Phrygian notes. For this will ensure something valuable in music, and that is coherence. If you sing the piece to yourself, you will still have the Phrygian notes in your memory when you get to line 13. You will almost certainly hear the Y introduced in the company of O from line 13 to line 16 as a Phrygian note (or possibly as Hyperphrygian, since Y appears in the tetrachord shared by Phrygian and Hyperphrygian whose top note is *mesē diezeugmenōn* in the one case, and *mesē synēmmenōn*, as we are thinking of it, in the other). Even if you, or your listeners (if you have any), do not know Phrygian from 'Schmygian', you will recognize, in lines 13 through 16, notes familiar to you from the opening. Though there is no way of knowing for sure, I would venture to say that even a real *Gelehrter* of Athenaeus' day would not have heard a modulation from Iastian to Hyperiastian in these lines, even if he could be brought to see that the bookkeeping could be understood to work out that way.

Aristoxenus says something relevant to all this:

> One should not overlook the fact that musical insight is at the same time insight into something that remains the same and something that changes, and that this holds for almost the whole of music and in each branch of it. [Macran 1902, 2.33.27–32]

If we possess musical insight, we can discern what remains the same in a melody and what changes. That presupposes, of course, that we can retain what we have heard in memory and recognize it as the same when we hear it again. Of course, we do this all the time when we listen to music: with greater or lesser insight, depending on how well informed we are. Aristoxenus gives a number of examples, one of which—not surprisingly—concerns modulation:

> And so too when, the same interval being put forth, a modulation comes to be in some cases but not in others. [Macran 1902, 2.34.9–11]

That is, we can discern, by musical insight, when a given interval occurs again in a melody and whether it has introduced a 'modulation', i.e., for the purposes of argument, a change of key in the relevant sense.

If we apply Aristoxenus' idea to the problem at hand, we will be interested to see if there is some interval that occurs before the introduction of O and then in the context of the piece where O, Y, and M allegedly play a Hyperiastian role. Then, we will want to consider how likely it is that musical insight would judge that this interval introduces a modulation and, if so, what sort. Now, the obvious interval to consider for this purpose is the downward leap of a fifth, ϑY, so crucial to Hagel's Hyperiastian construal of O. As it happens, we do not find this interval in what remains of the first eight lines of Athenaeus' piece, i.e., before O is heard for the first time. It should be noted that there are lacunae, one of which occurs in line 5 *after* ϑ. So it cannot be excluded that ϑY would already have been heard before line 13. But, even if it were not heard before line 13, which is what we should assume to be on the safe side, we can still enlist Aristoxenus' idea to see how musical insight might judge things and thereby test Hagel's argument. For the tonal material of line 6 and that of line 13 are strikingly similar. In line 6 we get:

Y, M, Y, M, I, ϑ, I, ϑ, Γ, Ʊ

(f, a, f, a, b♮, c, b♮, c, d, e).

In line 13, we get:

Λ, Γ, M, Ʊ, ϑ, I, ϑ, Γ, ϑ, Y, O, M, Λ, M

(b♭, d, a, e, c, b♮, c, d, c, f, a♭, a, b♭, a).

To be sure, our musical insight enables us to discern differences in line 13. There is the spice of O, in addition to the Hyperphrygian Λ. But those differences stand out against the things that our musical insight discerns as the same, notably the whole sequence of ϑ, I, ϑ, Γ (c, b♮, c, d). If we recognize that sequence in line 6 as Phrygian, surely we will recognize it as unambiguously Phrygian in line 13 as well, especially since that sequence is set off in both lines by two notes that may also be recognized as Phrygian, namely, M (a) and Ʊ (e). Then, notice that the downward leap of a fifth, ϑY, so important to Hagel's argument, is heard for the first time in the piece (so far as we know) immediately after the sequence of ϑ, I,ϑ, Γ. Thereupon, we get the first melodic figure alleged to be Hyperiastian: YOM. What would be the judgement of musical insight about this figure and its variants? To start with, what would be its judgement of Y?

Too much is the same in lines 13 and 16 for it to be likely that Y, a note that has by now so solidly established itself in our musical insight as belonging to the Phrygian-Hyperphrygian sphere of influence, could be understood as the Hyperiastian T even be it in the company of O, and even if O appear to be the lowest note of the Hyperiastian chromatic pyknon: O, M, Λ (a♭, a, b♭). Musical insight, in precisely the way it is characterized by Aristoxenus, is on the lookout for coherence. That does not mean that it cannot spot differences. It can and does, as when it distinguishes Λ (b♭) and I (b♮), and as when it notes the oddity of O. But it does so in such a way that it understands what we hear as coherent, i.e., it recognizes what is different against the backdrop of what it takes to be the same again. It will not matter to musical insight that we can re-describe what it discerns as the same again as having some functional identity other than the one that matters to it. Even if the alternative description is possible, it will not be plausible, at least not to musical insight. It is the presumed judgement of musical insight that matters here.

At this point, one may well wonder how musical insight would characterize O in lines 12 through 16, if it characterizes the other notes, including Y, as Phrygian with a dash of Hyperphrygian just for fun. The answer is very simple: it might very well characterize O as 'exharmonic', just as Pöhlmann and West do. But that need not be a big disappointment because there is more that we can say about what makes O exharmonic.

If we assume that the genus of our tetrachords is chromatic rather than enharmonic, then it is indeed the case that O is some kind of a semitone lower than M. This is interesting because no matter which key we understand M as belonging to in context—whether it be Phrygian or Hyperphrygian—it is a fixed note. In Phrygian, it is *mesē diezeugmenōn*; in Hyperphrygian, it is *mesē synēmmenōn*. For the purposes of analyzing lines 12 through 16, it is more likely to think of M as Hyperphrygian *mesē synēmmenōn*, i.e., the lower fixed note of the tetrachord: M, Λ, K, Γ. Then, precisely because Hagel is right to point out that O so frequently appears in the sequence, O, M, Λ—whether ascending or descending—we can say that what makes O so distinctively exharmonic is that it has the effect of surrounding the fixed note M with semitones on both its lower side and its upper side. That is interesting because the ancient Greek theoretical treatises on music allow a semitone on the upper side of the lower fixed note of a tetrachord (both in the chromatic and the diatonic) but apparently disallow one on the lower side of such a note. In other musical cultures, having some kind of a semitone on both sides of such a note is not only *not* a big deal—it is considered musically interesting. This is certainly true in Arab music and musical cultures related to it. O is therefore exharmonic to the extent that it breaks this rule. And that is what makes it so much fun too, as I am inclined to think, after improvising on the notes Υ, Ο, Μ, Λ, ϑ, Γ (f, a♭, a, b♭, c, d) on the *'oud* in the Arab style, i.e., by treating Μ and Γ as defining the boundaries of a Kurdī tetrachord (a, b♭, c, d) and Μ, together with Υ and Ο, as belonging to a lower, defective, conjunct Ḥijāzī tetrachord (<e♮ missing>, f, a♭, a). I always wondered whether actual ancient Greek musical practice rigidly adhered to the rule at issue here. Maybe the thing to say about Athenaeus is that he decided not to.

Now, I am not going to say that I know what went on in Athenaeus' head. My point is that the argument which Hagel musters to get inside his head is inconclusive because it depends entirely on the observation that certain melodic figures in lines 12 through 16 can be described as Iastian-Hyperiastian. They can indeed. But since they can also be described as Phrygian-Hyperphrygian, and perhaps more plausibly so, why should we believe that Athenaeus himself intended, planned, or conceived of them as Hagel says? You might just as well believe that the f♯ that I introduced into the little melody imagined earlier in this review [page 128] to illustrate change of key belongs to B-major rather than G-major and that I had thus effected a rapid modulation to a very remote key by briefly dropping my melody a semitone before restoring it to C-major—the key that it started out in. But that would be a crazy way to characterize things. Even if Hagel could show us that no description of the relevant melodic figures in lines 12 through 16 of the 'Delphic Paean' is possible except for the one he offers us, that still would not get us inside of Athenaeus' head. For it might still be the case that they are the residual side effects of the things that he, in fact, deliberately set out to accomplish in this piece (whatever they might be). One important difference between me and Athenaeus is that I am here to tell you that, in the little melody I imagined earlier to illustrate change of key, I deliberately introduced the single f♯ as a modulation from C-major in G-major. What Athenaeus deliberately intended in his melody and what is just an accidental effect of what he deliberately intended cannot be determined because he can no longer tell us himself. This matters a lot. If we cannot say that he deliberately intended Ο as a modulation to Hyperiastian, and if it is simpler, more elegant and more plausible to characterize Ο as 'exharmonic', then the only 'modulation' that occurs in his 'Delphic Paean' is the switch from the disjunct (Phrygian) tetrachord of the GPS to the conjunct (Hyperphrygian) tetrachord of the LPS. We already knew that that sort of thing happens in the ancient Greek musical fragments. Alas, in spite of everything that I said earlier, it is not even clear that that sort of thing would have counted as a genuine modulation by all Greek music theorists. The forked road that I laid out earlier in my review is sometimes referred to as the σύστημα ἀμετάβολον. That literally means 'the unmodulating system'.

I am now ready to take the reader into Hagel 2010 through the back entrance. If she or he asks why we are not going through the front entrance, the answer is very simple. There is no front entrance. No single, unified program pulls the book together. *Ancient Greek Music* is really a scrapbook of hitherto unsolved puzzles and riddles. The solutions offered by Hagel are not necessarily related to one another because the puzzles and riddles are not always related to one another. But there are some hunches, conjectures, and conclusions that run through the book like recurring leitmotifs. These leitmotifs were already audible in Hagel 2000. They include the idea, now familiar to us, that modulation to quite remote keys took place in ancient Greek music. That idea gets some more discussion in chapter 8 where Hagel reviews once again, but now in English, the existing musical fragments.

One important difference between Hagel 2000 and Hagel 2010 is that chapter 8 in the latter book follows the Pöhlmann/West collection of musical fragments [2001], which had come out in the meantime. This gives Hagel some new material to work over, notably Pöhlmann and West 2001, nos 5 and 6 from a collection of cartonnage scraps in the Ashmolean Museum that are believed to go back to the third or second century BC. West had published these fragments in 1999. But since they are never mentioned in Hagel 2000, I have to suppose that they came out as Hagel's first book was going to press. Very little of this material survives. We are lucky to have 13 notes in succession in line 6, column two of fragment 15 from no. 6. For the rest, all we have is an isolated note here and there or a sequence of three notes, occasionally four. But Hagel tries to argue in the new book that what we have is enough to give us another example of modulation to remote keys. The analysis he offers owes much to his earlier account of Athenaeus' 'Delphic Paean', which unfortunately receives little explicit treatment here. ‘Without Athenaeus’ Paean’, he says in the new book,

> any attempt to interpret the mutilated melodies[16] would be at a loss. With this piece as a guide, however, we learn from them that Hellenistic free modulation could go beyond what was sung at a traditional ceremony in Delphi. [269].

Maybe so. But the arguments that are supposed to get us to this conclusion are just as speculative as those in Hagel 2000. I will discuss them only to the extent that this will take us to the next assumption—or set of assumptions—shared by both books.

What survives of Pöhlmann and West 2001, no. 5 is, without doubt, an interesting piece (or set of notes) to stew over after Athenaeus’ ‘Delphic Paean’ because the note signs can all be understood to be Hyperphrygian (Υ, Τ, Π, Μ, Λ, Γ) with one seemingly exharmonic exception. This time the exception is Ν. It might be thought that Ν is the upper moveable note of the Hyperdorian tetrachord in ascending order: Π, Ο, Ν, Η. Η would be *mesē diezeugmenōn*, unless we had some special reason to take Π as *mesē synēmmenōn*. Whichever end of this tetrachord gets to be *mesē*, Ν is either chromatic or enharmonic by the conventions of the notational system. That means that it will either be a tone above Π (and thus identical in pitch—or roughly so—to Hyperphrygian Μ) or a semitone above Π and, thus, some kind of semitone lower than Hyperphrygian Μ. On the other hand, it could conceivably belong to other tetrachords, in which case it will likely be some kind of semitone higher than Hyperphrygian Μ. A quick look at the Alypian tables shows that it could just as well be the higher moveable note of either a chromatic or an enharmonic tetrachord in the following keys: Hypolydian, Hypoaeolian, Hyperiastian, and Iastian. What Hagel hopes to show is that all the notes in no. 5 that are ambiguous between chromatic and enharmonic turn out to be chromatic and, in particular, that Ν turns out to be a semitone lower than Μ and, therefore, a semitone higher than Π, which would make it the same in relative pitch as Ο in Athenaeus’ ‘Delphic Paean’. That would yield a string of *five* successive semitones:

[16] *scil.* of the Ashmolean papyri.

ΥΤ	the semitone between the two chromatic moveable notes of the Hyperphrygian tetrachord whose higher fixed note is Μ,
ΤΠ	the semitone between the higher chromatic moveable note of this tetrachord and the one unequivocally diatonic note of this tetrachord,
ΠΝ	
ΝΜ	and
ΜΛ.	

The value of that many semitones in succession—in the scalar system presupposed by the melody, if not in the melody itself—is that it would allow for rapid modulation to keys very remote from each other on the circle of fifths. But Hagel holds off from this conclusion until he gets to Pöhlmann and West 2001, no. 6. So bear with me.

No. 6 is from the same cartonnage as no. 5 and possibly even from the same roll. Pöhlmann and West believe that the fragments from both Numbers may come from a single 'music manuscript' but they say 'we should expect such a manuscript to have contained a number of different items...'. They say, moreover, that this is confirmed 'by the presence in two places of a *paragraphos* accompanied by a *coronis*' [2001, 38]. We must wonder, then, about the relation between the music in the two sets of fragments. On the face of it, it seems possible that what has survived could come from two different musical pieces or perhaps from more than two. But if we compare the note signs of no. 6 with those of no. 5, we will be struck by how many of them are the same. They are mostly Hyperphrygian and, indeed, the usual suspects: Υ, Τ, Π, Μ, Λ, Γ. Κ is missing but our seemingly exharmonic Ν is present, though less frequent. There are two Phrygian notes not found in what survives of no. 5. They are Ι and ϑ. Then, finally, there is one more seemingly exharmonic note that belongs neither to Phrygian nor to Hyperphrygian. It is Δ.

The overlap of note signs leads Hagel to say this:

> The resemblances between Number 5 and Number 6 are so striking that they [*scil.* Pöhlmann and West 2001, nos 5 and 6] can hardly be treated independently. [260–261]

'Independently' can, of course, be interpreted in many different ways. It is probably true that any observations we make about the note signs we find in the one set of fragments should be informed by what we find in the other set of fragments. But Hagel takes 'independently' in a much more literal way. He goes on to speak of the melody or melodies of nos 5 and 6 as figuring in the same 'tonal space'.This means, I think, that he is supposing that the same set of notes figured throughout the whole of the single 'music manuscript' of which we only have the fragments of nos 5 and 6. At any rate, he does nothing to caution us from taking him that way. So, however many individual melodies we may, in fact, be dealing with, Hagel apparently takes them all to have had all of the following notes (in order from lowest to highest, following Hagel):

$$\Upsilon, \mathrm{T}, \Pi, \mathrm{N}, \mathrm{M}, \Lambda, \mathrm{K}/\mathrm{I}, \vartheta, \Delta, \Gamma.$$

I am uncomfortable with this conclusion. It seems hasty to me. But I will hold off.

Hagel also claims that the interval between every two successive notes in the sequence from Υ to Γ that I just laid out, starting with ΥT, is exactly one equal-tempered semitone. So by the time he is finished with no. 6, he has no less than nine successive semitones. That is four more than he found in no. 5 all by itself. It is five more than he found running from O to ϑ in Athenaeus' 'Delphic Paean' back in 2000, as he himself points out on page 263 of the new book. The take-away lesson is supposed to be that the Ashmolean melodies must have had lots of very far ranging modulations. This is because four or more successive semitones gives us greater freedom to drop (or raise) a melody or melodic figure with a given arrangement of intervals by a semitone in the way exemplified by Athenaeus' 'Delphic Paean' according to Hagel 2000.

That modulation to a remote key requires fairly long trains of successive semitones is a point that can perhaps be made more concrete in light of the following example. Frère Jacques is a melody that spans the same *ambitus* as the material we have seen in the Ashmolean fragments: a major sixth, if we disregard the 'ding, dang, dong' bit at the end. Suppose that I sing it in C-major, a key with no sharps or flats. Then, suppose that I decide to raise the whole tune by just one semitone and sing it all over again in C♯ major, a key with seven sharps—very remote from the natural key on the circle of fifths. In order to sing 'Frère Jacques' the second time, I will need to ensure that I have available to me all the sharpened notes I need.

No big deal if I am singing, but a bigger deal if I am playing some instrument that has, say, a limited number of open strings. For the purposes of singing or playing the tune the first time round, I will only need the notes of the C-major scale from, say, middle c to a above middle c. Among these notes, there is only one semitone: the one between e♮ and f. If I were at the piano, I could play the whole tune on the white keys. But to sing or play the tune in C♯ major, I will need to divide all the whole tones between middle c and a above middle c, i.e., the one between c and d, the one between d and e♮, the one between f and g, and the one between g and a. Plus I will need an a♯. If I am sitting at the piano, that means I will need the five black keys laid out on the keyboard between middle c and b above middle c. As a result, my rendition of 'Frère Jacques' first in C major and then in C♯ major will require a train of ten successive semitones. Moreover, these will have to be equal-tempered semitones to ensure that the C♯ major version does not sound weirdly out of tune. Hagel claims that the train of nine successive semitones which he believes that he has found in the Ashmolean fragments clearly indicates that the melodies from these papyri must have modulated to keys very remote from one another: as exemplified by my repeat performance of 'Frère Jacques'.

Hagel puts it this way:

> The variety of notes itself is sufficient proof that what we have here is music of a very sophisticated style. Yet we are surprised by its narrow compass of a major sixth; and even of this sixth, the higher notes appear only rarely, so that the major part of the melody is restricted to a mere fourth [*scil.* that between Υ and Λ?[17] —AL]. Sophisticated melodies within so narrow a range are naturally impossible within a single scale. We must therefore expect that the music of the Ashmolean papyri is heavily modulating: which means that in the course of the melody the available notes must frequently rearrange themselves to new scalar patterns. [263]

[17] Actually, it is not clear which fourth Hagel means here. I take it to be ΥΛ, given what follows in Hagel 2010. But, of course, even if all that is true of the fragments from no. 6, it is not exactly clear what the 'musically effective tetrachord' of no. 5 is supposed to be.

Almost every clause of this passage is open to dispute. For starters, the first sentence is simply false. So too the claim that 'sophisticated' melodies are impossible within the compass of a fourth unless there is a lot of modulation going on, i.e., change of keys. In Arab music, and related musical cultures, a lot of very 'sophisticated' melodies take place within the compass of a fourth without any modulation at all. A friend of mine, a professional musician based in Montreal who grew up in Tunisia, likes to joke—usually in reference to the *'oud*-player Farīd al-'Aṭrash, but often as a universal principle—that all it takes for a really great melody is three notes (not exceeding a fourth).[18] However jocular the remark, it is at the same time not intended as hyperbole. This matters. The point concerns how we approach scraps of real music from the distant past. If we expect to find in it music familiar to us from our own culture, then that is precisely what we will find. But finding what we expect to find is not responsible scholarship. However much imagination it exhibits, it will never be anything more than the result of a fancy Rorschach test. Scholars who study the material in Pöhlmann and West 2001 should do so as good ethnomusicologists might, i.e., with the expectation that they could discover almost anything in it.

Second of all, why must we expect that 'the music of the Ashmolean papyri is heavily modulating'? If the answer is all those successive equal-tempered semitones, why be so sure that that is what we have? Here the answer starts with the claim that the Hyperphrygian and Phrygian moveable notes Υ, Τ, Λ, Κ/Ι, ϑ are chromatic. Π is unequivocally diatonic. So, then, we purportedly have the following sequence: Υ, Τ, Π, Μ, Λ, Κ/Ι, ϑ, Γ such that the only intervals that are not semitones are the whole tone ΠΜ and the whole tone ϑΓ. Then it is just a matter of inserting Ν half way between Π and Μ and then Δ half way between ϑ and Γ. But why should we accept these claims? Why, for instance, should we take for granted that the relevant moveable notes of the relevant tetrachords are chromatic rather than enharmonic? This question is not a quibble since the material at hand seems to go back to a time when the enharmonic genus of the tetrachord was still a live option. But then second of all, why must we think that Ν and Δ equally divide the whole tones ΠΜ and ϑΓ, respectively? Even if they do divide these whole tones, why

[18] Mohamed Masmoudi, founding member of Sokoun Trio.

suppose that that will get us all the semitones Hagel needs? Let us take the questions about N and Δ first, but let us focus on N.

If we go strictly by the conventions of the notational system, N will either be some kind of semitone higher than Hyperphrygian M,[19] some kind of semitone lower than Hyperphrygian M,[20] or N and Hyperphrygian M will be the same pitch (give or take).[21] Whether we take the tetrachord of which N is a part to be chromatic or enharmonic in genus, these are the only options. (N is never an unequivocally diatonic note. For if we adhere strictly to the conventions of the notational system, it is always the higher moveable note of a tetrachord, and it is never the one such note that can *only* be construed as diatonic.) There will be questions no matter which of the three options we pick.

If we suppose that N and Hyperphrygian M are exactly the same in pitch, we will then have to wonder why they are notated with different signs. Here there are two possibilities. One is that they may be the same in pitch but different in function. But then, since N is the only note in the surviving scraps from whichever key it may perhaps belong to, this difference in function remains a mystery. Perhaps, on the other hand, N and M are not quite the same in pitch and the reason N appears at all is to signal to the singer to adjust a bit upwards or downwards from Hyperphrygian M. Hagel rejects this possibility:

> Within this line of interpretation, there is room merely for a microtonal difference of tuning shade between M and N. But no ancient source recognizes a 'modulation of shade'. [260]

The talk about shade (χρόα) here concerns slight differences among tetrachords of the *same* genus. For example, Aristoxenus recognizes a 'soft' diatonic whose intervals arranged from lowest to highest are a half tone, three quarters of a tone and one tone and a quarter, as well as a 'tense' diatonic whose intervals are a half tone, tone, tone. The difference between the two is a difference of 'shade', which is effected by flattening the central interval of the tense diatonic by

---

[19] That would be in Hyperiastian chromatic and Hypolydian chromatic.

[20] That would be in Hyperdorian enharmonic and Hypoaeolian enharmonic.

[21] That would be in Hyperiastian enharmonic, Hypolydian enharmonic, Hyperdorian chromatic, and Hypoaeolian chromatic.

an enharmonic δίεσις. The discussion of 'modulation' in the surviving theoretical treatises certainly allows for change of genus, e.g., shifting from a diatonic tetrachord to an enharmonic tetrachord. As we saw earlier, this is one of the four kinds of 'modulation' that Cleonides explicitly mentions in the *Introduction to Harmonics* [von Jan 1895, 13.20.1–2]. Hagel is certainly right that there is no mention in Cleonides, or anybody else, of shifting from one shade of a given genus of the tetrachord to another. But that all by itself does not settle the question at hand. One of the questions at issue when we examine the surviving scraps of notated music is precisely what the relationship between theory and practice may have been. Perhaps some musicians liked changes of shade but such changes were not explicitly discussed in the treatises just because they were not taken to be significant enough changes to warrant any discussion. Perhaps such changes were discussed in treatises or parts of treatises that have been lost. Perhaps the conjectured microtonal difference between N and M in the Ashmolean papyri indicates something other than a change of 'shade' in the relevant sense. Here we could conjecture ''til the cows come home'.

If we now suppose that N is some kind of semitone higher than Hyperphrygian M, we will face the same sorts of questions all over again. The easiest way to bring them into focus is to restrict our attention for now to the fragments of no. 5. For we find in these fragments notes that can be construed as the Hyperphrygian tetrachord: ΜΛΚΓ. This tetrachord is either chromatic or enharmonic. If it is enharmonic, then it seems that, on the hypothesis now under consideration, K and N are the same in pitch. But if they are the same in pitch, why are they notated with different signs? Perhaps they vary in function; but once again that difference remains a mystery because we still do not know which key N comes from. On the other hand, perhaps they vary slightly in pitch, in which case we are back to speculating about differences of shade. This time the question will be how enharmonic ΜΛΚΓ varies from enharmonic ΜΛΝΓ. The same questions will arise if we take ΜΛΚΓ as chromatic, except that they will concern the relationship between N and Λ. If we now suppose that N is some kind of semitone lower than M, we will face another question, namely, whether, in fact, N belongs to Hyperdorian and, if so, why it is not notated as O, as in Athenaeus' 'Delphic Paean'.

In the face of all these questions, I think it is instructive to consider a different question, this time raised by Hagel himself: 'But should we presuppose a strong interest in notational logic on the side of the composer at all?' [260]. The answer is that perhaps we should not. If not, then perhaps Hagel is right to suggest that N was used for 'the next note below M'. Why below rather than above? Hagel must be thinking that even if the composer's interest in the 'notational logic' were not strong, it would not have so badly weakened as to fade away altogether. For all of the Ashmolean fragments use the vocal notation rather than the very different instrumental notation. The vocal notation is peculiar in that, for the central vocal register, it uses the letters of the Greek alphabet in alphabetic order. But the further in the alphabet we go, the lower we are in pitch. So if N is not to be thought of strictly according to the conventions of the notational system, namely, as a note that belongs to some specific key other than Hyperphrygian, but is to be thought of loosely according to these conventions, namely, as the next note after M, then the 'notational logic' suggests that N would be lower in pitch than M. But then by how much? Hagel says it would be lower by a semitone.

But why a semitone rather than something else? Hagel seems to assume that, if it were something else, it would have to be an enharmonic δίεσις. He also seems to assume that it would be an enharmonic δίεσις, if and only if the tetrachords of Pöhlmann and West 2001, nos 5–6 were enharmonic. The flip-side of this assumption seems to be that if the interval between N and M were a semitone, then the tetrachords of nos 5–6 would have to be chromatic. None of these assumptions can be justified for the simple reason that N is the note that does not belong, just like O in Athenaeus' 'Delphic Paean'. But they are very important assumptions for Hagel. For if he can find some reason for rejecting the enharmonic, he gets semitones all the way down. N will divide the whole tone ΠM. For that matter, Δ will divide the whole tone ϑΓ, assuming—perhaps not unreasonably, but who knows?—that Δ and N are a fourth apart. Then, to sweeten the deal, all the surviving Phrygian and Hyperphrygian moveable notes—Y, T, Λ, K, I and ϑ—will be chromatic (K and I will be identical in pitch). The result will be nine successive semitones from Y to Γ and, hence, freedom to modulate far and wide. The trouble is that it all just seems much too convenient. Why take for granted that, if NM is an enharmonic δίεσις, all of the tetrachords of nos 5 and 6 are

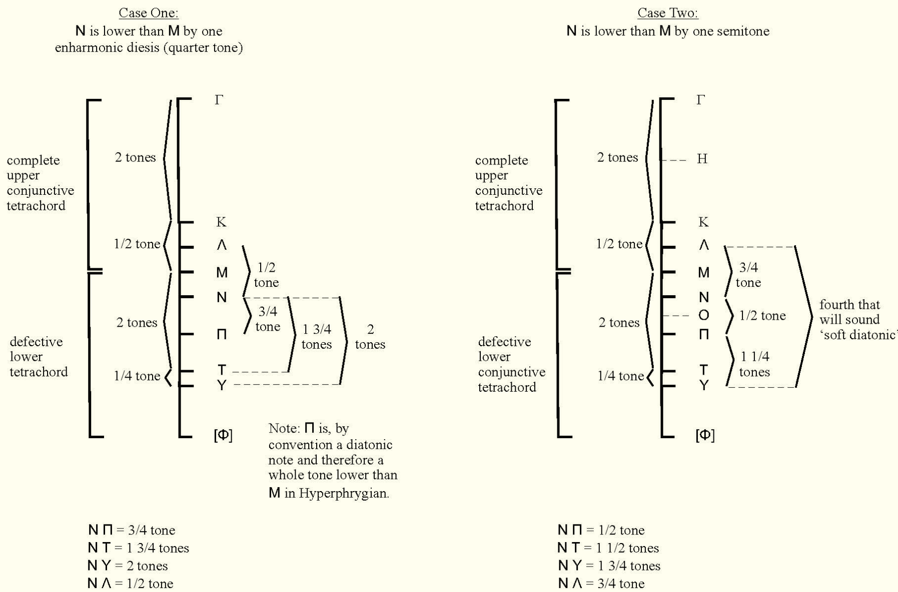


Figure 5. Two ways of construing the tonal material of Pöhlmann and West 2001, no. 5 on the 'enharmonic reading'

too? Why could the tetrachords of nos 5–6 not still be enharmonic even if NM were not an enharmonic δίεσις but rather a semitone after all?

To reject the enharmonic construal of the relevant tetrachords, Hagel helps himself to a claim made by Pöhlmann and West in their commentary on no. 5.

Pöhlmann and West also seem to take for granted that if the melody or melodies of no. 5 are enharmonic, then N is lower than M by an enharmonic δίεσις. But then they consider which intervals N forms with which of the surviving notes from these fragments [see Figure 5, case 1]. To start, N forms an interval with Y. Indeed, NY turns up five times in the first three lines of no. 5, fr. 1. We also find one occurrence of NΠ in line 4 of fr. 1, one occurrence of NT in line 9 of fr. 3 and one occurrence of NΛ in line 11 of fr. 3. With the relevant enharmonic assumptions in play, NY will be some kind of ditone, NΠ will be roughly three quarters of a tone, NT will be roughly one tone and three-quarters, and NΛ will be some kind of semitone. Pöhlmann and West disqualify the 'enharmonic reading' of no. 5 on the grounds that these intervals are 'clearly...less plausible' than the intervals we would get on the 'chromatic reading', i.e., on the assumption not only that Υ, Τ, Λ, and Κ are chromatic but that Ν is taken a semitone lower than Hyperphrygian Μ. On the chromatic reading, ΝΥ will be one tone and a half, ΝΠ will be some kind of a semitone, ΝΤ will be a whole tone, and ΝΛ will also be a whole tone. Hagel embraces the Pöhlmann/West claims without comment [260 and n12]. This seems to me much too hasty.

I take it that the intervals formed by Ν with Υ, Π, Τ and Λ in no. 5 are on the 'enharmonic reading' implausible not because of the difficulties that they might present to singers or instrumentalists. For the two that might seem oddest to those of us minimally tutored in 'the music of the Western World etc...' are ΝΠ at three quarters of a tone and ΝΤ at one tone and three quarters. But depending on how the quarter tones add up in practice, these intervals may well turn out to be intervals heard in Arab music and related musical cultures all the time. The Arab equivalent to ΝΤ is the interval distinctive of the 'Rast' tetrachord, i.e., the slightly wonky third formed by the Rast final, which might be c in the octave of your choice and its third degree, which is then an e neither flat nor natural but somewhere in between. The Arab equivalent to ΝΠ is the interval between the two inner notes of the Rast tetrachord: call them d in the octave of your choice and wonky e. Both of these intervals are easy to sing and vastly easier to sing than the weird and horrible seventh diminished by an enharmonic δίεσις that one finds three times in what survives of the Orestes fragment [Pöhlmann and West 2001, no. 3]! So the implausibility of ΝΠ and ΝΤ will turn on something else, namely, the alleged fact that they are not attested in the surviving theoretical accounts of the enharmonic genus of the tetrachord.

But lots could be said here. One simple conjecture would be that, though the tetrachords (explicit and implied) of no. 5 might well be enharmonic, perhaps Ν is indeed some kind of semitone lower than Μ and higher than Π [see Figure 5, case 2]. Indeed, perhaps the 'notational logic' is strictly adhered to such that Ν is a Hyperdorian note that belongs to the enharmonic tetrachord ΠΟΝΗ, with Η taken as *mesē diezeugmenōn* and thus a whole tone lower than Γ. Perhaps Ο and Η are, in fact, part of the melody or melodies of no. 5 but have not survived in what is left of the manuscript. If that were the case, we would have something not much more complicated than a modulation from disjunctive tetrachords to the neighboring conjunctive, *synēmmenōn* tetrachord. The only complication would be that, as we saw earlier in the discussion of Athenaeus' 'Delphic Paean', Hyperdorian is two doors down from Hyperphrygian in the flatward direction. Hence, the connection would have to be made either through the enharmonic πυκνόν shared by Hyperphrygian and Dorian MΛK or by one or both of the notes that Hyperphrygian, Dorian, and Hyperdorian all have in common, namely, Π and Γ. This would still give us a modulation in Hagel's sense, but it would take place between fairly close neighbors and not—as Hagel would have it—between remote keys in such a way as to raise or drop a melodic figure by a semitone. But what makes this conjecture interesting to me is that it keeps everything in conformity with the official playbook but it preserves the allegedly 'implausible' intervals that Hagel and Pöhlmann/West were prepared to rule out of court. It is just that those intervals will now be found between different pairs of notes than the ones predicted earlier. We now find the interval of three quarters of a tone not between N and Π but rather between N and Λ; and we now find the interval of one tone and three quarters not between N and T but rather between N and Y.[22] The upshot of all this is once again that we find a rush to judgement in Hagel—and not just in Hagel, but Pöhlmann/West too.

But there is something else that makes this conjecture worth considering—again in the interest of slowing the rush to judgement. This will also give us a point of contact, down the road, with no. 6.

Suppose again that the Hyperphrygian tetrachords of no. 5 are enharmonic, and that N is a semitone lower than M and a semitone higher than Π [see Figure 5, case 2]. But let us follow Hagel this time in supposing, moreover, that N does not belong to any specific key as such, but that it is just the 'next note below M'. Then, it is possible to understand what survives of no. 5 as showing no modulation at all, i.e., no change of key. If there is 'modulation' here, we might just as well understand it as change of genus, i.e., arrangement of the tetrachord.

[22] The semitone is now found between N and Π rather than between N and Λ, and the interval between N and T is one tone and a half. There is no ditone except between M and T.

This can be seen if we take the fourth bounded by Υ and Λ and then notice that, with the insertion of Π and Ν, we get the following successive intervals from lowest to highest: ΥΠ is a tone and a quarter; ΠΝ is a semitone; ΝΛ is three quarters of a tone. The interest of these intervals is that they are definitive of Aristoxenus' 'soft' diatonic. Now, according to the official playbook, the normal form of the tetrachord, whatever its genus, is that its smallest interval should be the lowest and its largest interval should be highest. The central interval may either equal the lowest, or it may equal the highest, or it may be larger than the lowest (but more usually smaller than the highest). That rule implies that, in a well formed soft diatonic tetrachord, the order of intervals from lowest to highest would be semitone, three quarters of a tone, a tone and a quarter. But all that follows from this rule as such is that ΥΠΝΛ is not itself a well formed soft diatonic tetrachord. It is, nevertheless, a sequence of successive intervals spanning a fourth that one might plausibly encounter in a soft diatonic melody. For suppose that a soft diatonic melody is based on a system of two conjunctive soft diatonic tetrachords, and that it moves about for a time within the fourth bounded by the higher moveable note of the lower tetrachord and the higher moveable note of the higher conjunct tetrachord. Then, the sequence of intervals from lowest to highest will be one tone and a quarter, a semitone, three quarters of a tone, i.e., the same sequence from lowest to highest that we get in ΥΠΝΛ. The musically trained listener who heard that melody as soft diatonic would presumably hear any part of the melody or melodies of no. 5 that moved in and through ΥΠΝΛ as soft diatonic.[23] But

[23] Here is another way to put it. The tetrachord ΥΠΝΛ does not fall within the bounds of Hyperphrygian fixed notes, contrary to what one might have expected. But one may well wonder why a melody of a certain τόνος could not establish itself between moveable notes. Pöhlmann and West 2001, no. 32, which is from one of the anonymous texts on music collected by Bellermann [Najock 1975, 33 (§104)], gives us an interesting parallel: a melody seemingly notated in Lydian that establishes itself within an octave between Lydian moveable notes rather than Lydian fixed notes. This does seem odd because we expect the fixed notes of a τόνος to have some kind of special melodic significance and that the melody should somehow insist on these notes. But, again, our expectations may not always be a good guide. It is instructive to note that Winnington-Ingram [1936] had such expectations and expresses his exasperation at the end of his treatment of the surviving musical fragments. He was looking, in particular, for evidence

now we remember that the melody or melodies of no. 5 also featured notes definitive of enharmonic tetrachords (enharmonic by hypothesis): ΜΛΚΓ and ΥΤΜ. The latter system, as it stands, is defective but it is at least suggestive of the full enharmonic tetrachord: ΦΥΤΜ. To the extent that the melody or melodies of no. 5 moved in and through these systems, the musically trained listener would presumably have heard them as enharmonic. The upshot is that nothing seems to stand in the way of understanding the melody or melodies of no. 5 as exhibiting modulation, not at all in the sense of change of key but rather as a change of genus of the tetrachord. I find nothing in Hagel's analysis that would exclude this possibility.

I should note that the Pöhlmann/West commentary on no. 6 takes the surviving notes from these fragments—Μ, Λ, ϑ, Γ—to form a soft diatonic tetrachord. The odd note here is ϑ because it is Phrygian while the other surviving notes—Υ, Τ, Π—are Hyperphrygian with the exception of Ν and Δ. If the tetrachord bounded by Μ and Γ were a normal (tense) diatonic, we would expect the second moveable note to be indicated by the sign Η. Thus, we would have ΜΛΗΓ.[24] Pöhlmann and West take the appearance of ϑ in the place of Η to signal the flattening of the diatonic note. Hagel naturally resists this idea because it would compromise his claim that the melodies in nos

that the fixed note *mesē* would be found to be modally significant in the fragments. But he could not find such evidence. He says:

> Thus, though the fragments give some support to the scheme of tonics and modal analysis that has been based on the Aristoxenian doctrine of the species of the consonances, this support is very incomplete.... [1936, 46]

Hagel too discusses modality in the new book. See, in particular, pages 219–229. His conclusions seem tentative.

[24] It may be a surprise that, even in the Hyperphrygian diatonic tetrachord, the first moveable note is indicated by Λ because we have already found that Λ could indicate the first moveable note in this tetrachord in both the enharmonic and chromatic. But, by the conventions of the notational system, the sign for the first moveable note of any given tetrachord is the same for all three genera.

5 and 6 were laid out on a grid of nine successive semitones.[25] He may well be right to say that Pöhlmann/West are wrong on the soft diatonic in no. 6. But I find his arguments inconclusive. The only thing that matters here for my purposes is this. Suppose for the sake of argument that Pöhlmann/West are right about the soft diatonic tetrachord in no. 6. Suppose too that the function of N and Δ in these fragments is to facilitate change to and from this genus to some other genus. If so, the interest of both nos 5 and 6 may well have been that they illustrated change of genus rather than change of key.

Hagel reviews many more fragments of ancient Greek music in chapter 8 of his new book. We have now seen enough to appreciate his perspective on this material. This will allow me to give the reader a better sense of some of the rest of the new book.

We have already seen that Hagel's claim to find evidence in ancient Greek music of modulation to and from keys remote from each other on the circle of fifths depends always on the claim to have found long sequences of successive semitones (indeed, equal-tempered semitones or roughly so). If his claims be accepted, and if modulation to and from remote keys was common practice, we should expect a preference for the (tonic) chromatic genus of the tetrachord where the two semitones of the πυκνόν are roughly equal. The idea is that if one can align tonic chromatic tetrachords in neighboring keys in the right way, it should be possible to get a train of successive semitones long enough to take shortcuts to the more remote keys. It is clear that a preference for the tonic chromatic is assumed in Hagel's analyses of Athenaeus' 'Delphic Paean' and the Ashmolean fragments. But the assumption was, in fact, stated explicitly in Hagel 2000 [72: cf. 87–88]:

> *Voraussetzung für die Konstitution einer Reihe von drei ‚Halbtonschritten' ist natürlich die chromatische Stimmung mit einem Pyknon aus (wenigstens ungefähren) ‚echten' Halbtönen...Nur mit der Grundlage einer solchen Stimmung kann auch eine Modulation um einen Halbtonschritt geschehen.*

---

[25] As he says elsewhere in the book, speaking of the 'soft' diatonic:

> such a division is in principle mutually exclusive with *synēmmenōn* modulation, which requires a stable whole tone at the top of the tetrachord. [141]

> A precondition for the setting up of a sequence of three 'semitone steps' is, in the nature of the case, the chromatic tuning with a pyknon of (at least roughly) 'true' semitones...A modulation by a semitone can occur only on the basis of such a tuning.

But this assumption raises a challenge for Hagel because the surviving theoretical treatises do not seem to privilege the tonic chromatic, and because they happily envisage alternatives to it: different shades of the chromatic, the enharmonic and its different shades, as well as the diatonic and its variants.

Actually, it is more challenging than that for Hagel because his account would lead us to expect that the tonic chromatic would at least find special favour with Aristoxenus—and this for a couple of reasons. First, Aristoxenus seems to have thought carefully and systematically about modulation in the sense of change of key, perhaps, as Hagel says, in response to musical innovations of the fifth century BC. It is unfortunate that his account is lost; but its broad outline is preserved in Cleonides. Second, Aristoxenus rejects numerical ratios of whole numbers as the way to define intervals and tunings. Third, he is naturally understood as committed to the idea that the octave is the sum of six whole tones each of which can be divided into equal semitones. This opens the door to equal temperament, otherwise closed to theorists who insist on numerical ratios.[26] But, for all that, Aristoxenus expresses an unequivocal preference for the then out of fashion enharmonic which has a ditone as its top interval presumably equivalent—though Aristoxenus would never put it this way—to an interval with a ratio of 81:64 produced by two Pythagorean whole tones (each 9:8).

This is a challenge for Hagel. He meets it in chapter 5 with a two-pronged strategy, if I really understand what is going on here. First, he takes Aristoxenus' preference for the out-of-fashion enharmonic as a 'hobby-horse' [155]. Second, he reviews all of the surviving theoretical discussions that define intervals and tunings by numerical ratios and divides them into the sheep and the goats. The goats are those that may be shown to be totally 'crackpot' from the point of

[26] Aristoxenus' countryman, Archytas, had shown, after all, that there are no natural numbers that could express as a ratio the interval that exactly divides the whole tone.

view of musical practice, like, for example, those who are directly and perhaps exclusively motivated by cosmological issues (Nicomachus and 'Timaeus Locri') or those who seem interested in the mathematics of numerical ratios for their own sake (possibly that is Hagel's judgement of Archytas, at the end of the day, though he does credit Archytas with a significant interest in the *aulos*). The sheep are those who can be shown to be responding to musical practice. In truth, it turns out that there are no sheep 'pure laine', as we might say in Québec, but rather only goats with certain sheep-like qualities that can be most clearly detected when they are forced to contort their mathematical commitments or quite possibly fudge their results (this is especially so in the case of Ptolemy). For the purposes of judging the ratio of goat-to-sheeplike qualities, the 'musical practice' of relevance here is, in the first instance, different from the one that I have been discussing. It will be the practice of performance on string instruments: the lyre and the cithara (no great surprise here because string-lengths can be readily compared in terms of numerical ratios). This is not to say that modulation did not take place on these instruments. It did. But first of all, Hagel says that the impulse for remote modulation came from another instrument, the *aulos*; and, second of all, the string instruments posed their own special problems, namely, how to ensure or maximize the richness of tone of those strings whose pitch could not, for whatever reason, be achieved by tuning through perfect fourths and fifths.

Didymus and Ptolemy are of the greatest interest here. Ptolemy all the more so, not only because his book on *Harmonics* survives but also because, as Hagel tries to argue, Ptolemy's program of squaring the judgement of ear and mathematical reason fails ultimately when it comes time to test the different divisions of the tetrachord familiar to the trained, musical ear against the findings of the eight-string canon. Here Hagel argues that we find either duplicitous fudging or—one would prefer to think—self-deception. This would consign Ptolemy to fully-fledged, irredeemable goathood were it not for two things. First, Hagel argues that it indicates an awareness of genuine problems in providing for maximally resonant intervals smaller than the fourth (pure thirds). Second, Hagel argues in chapter 4 that Ptolemy's report of contemporary lyre and cithara tunings may be regarded by and large as trustworthy (and, therefore, very valuable). I am not yet sure what I think about the details of Hagel's account of Ptolemy on the divisions of the tetrachord. But I think it is quite likely that Hagel is right to say that the outcome of Ptolemy's experimental tests will leave us shaking our heads; and I believe that figuring out the details here matter to our understanding of Ptolemy's legacy—and not only for music theory. Jamil Ragep [2009] has argued that the distinctive concern for observational precision in astronomy in the Islamic world was motivated at least in part by the awareness of, and irritation due to, the excessive neatness of Ptolemy's numbers. What a serious study of Ptolemy as both astronomer and music theorist may well show is just how hard it is to carry out the program of squaring mathematical reason and judgement of the higher senses.

Because almost all of the surviving ancient Greek treatments of music theory privilege numerical ratios, one could easily get a disproportionate sense of the significance of the string instruments for theory and practice alike. Part of the significance of Hagel 2010 is its effort to compare the significance of the string instruments (in chapters 4–5) and its oft overlooked competitor, the *aulos*. Chapter 9 collects and reviews the surviving literary and archeological evidence for this instrument and the way it may have developed over time. The special contribution of the *aulos* to the story is precisely what would have made people like Ptolemy turn away from it as a theoretical aid in preference for stringed- instruments, namely, its lack of precision in intonation. To be sure, it had finger-holes; once they were bored, they remained where they were until the instrument disintegrated. If you fully stopped those holes in the relevant ways, you would get a determinate pitch, all other things being equal. But in actual performance on a woodwind, all things are not equal. You can more or less cover a hole with your finger or incrementally cover it and thereby vary the pitch. Of course, *embouchure* can produce very fine changes of pitch. That is why the clarinetist, at the beginning of Gershwin's 'Rhapsody in Blue', can produce such a dramatic *glissando*, the like of which is impossible on the piano—much less on the ancient Greek lyre or cithara. So equal temperament—or thereabouts—and the possibility of modulation to remote keys found its home in performance on the *aulos*. The full actualization of this possibility is supposed by Hagel to have come about in the fifth century BC with a 'new music' documented only in literary testimony. All one can tell for sure is that this 'new music' was received at the time with bemusement and amusement, as new music almost always is. But Hagel believes that the literary evidence gives some (circumstantial) evidence that the new effects of this music were due to modulation. It is important, for his purposes, that the 'new music' was associated with the names of *auletes*. The significance of his analyses of the surviving music that I discussed is that, if he is right to say that they exhibit modulation to remote keys, then they can be understood to give us a sample of the 'new music' even though they were composed much later in time. Finally, I should add that Hagel takes the system of ancient Greek musical notation to have evolved to answer the needs of this modulating *aulos* music—the system does indeed follow the circle of fifths: Hagel provides a nice diagram showing this on page 13. He offers an account of its puzzles in chapter 1 and tries to reconstruct its evolution from the internal evidence of the system itself (as, say, documented in the Alypian tables). I have to say that I find this reconstruction totally farfetched. But be that as it may, one way of understanding what is going on in Hagel 2010, as a book rather than as a collection of essays, is that it tries to show how the coexistence of aulos and string instruments, uneasy though it may have been,was possible.

On balance, the thing to say about Hagel 2010 is that it is indeed a very stimulating book but at the same time very frustrating. The biggest source of frustration is its high tolerance for speculation that at times morphs into wishful thinking. But there are other significant frustrations. It was not well edited. The English is sometimes so clunky that it was easier to mentally back-translate it into German. Even then, I often had the impression that the author was talking to himself rather than to his reader. The book is also very badly organized. I could never tell where the discussion was headed. Often I could not tell how or whether earlier material was significant for later chapters. I initially took the attempt to reconstruct the development of the notational system in chapter 1 to be foundational for the rest: that was suggested by the presentation of the riddle associated with this system—Dorian is Hypolydian!—as being deep and fundamental. Fundamental though it may be, its solution did nothing to advance later discussion in the book. As a result, I could not understand the order or even the choice of topics until I read Hagel 2000. Even then, Hagel 2000 does not help motivate everything in Hagel 2010. The author never provides a map of his project in the new book or a single, self-contained, coherent statement of his overall motivations.

I have tried to do that in this review; but I am still not confident that I really have the big picture, much less the details. That may reflect on me but it also reflects in large measure on the author. The burden of communication falls on his shoulders at the end of the day. The last thing to say is that there is no index of subjects. In a book this messy and sprawling? *Ô stupeur!*[27]

BIBLIOGRAPHY

Hagel, S. 2000. *Modulation in altgriechischer Musik. Antike Melodien im Licht antiker Musiktheorie.* Quellen und Studien zur Musikgeschichte von der Antike bis in die Gegenwart 38. Frankfurt am Main.

________ 2010. *Ancient Greek Music: A Technical History.* Cambridge/New York.

Macran, H. S. 1902. *Ἀριστοξένου ἁρμονικὰ στοιχεῖα/The Harmonics of Aristoxenus. Edited with Translation, Notes, Introduction, and Index of Words.* Oxford.

Najock, D. 1975. ed. *Anonyma scripta de musica Bellermanniana.* Leipzig.

Pöhlmann, E. 1970. *Denkmäler altgriechischer Musik.* Nürnberg.

Pöhlmann E. and West, M. L. 2001. *Documents of Ancient Greek Music.* Oxford/New York.

Ragep. J. 2009. 'A Possible Islamic Reaction to Ptolemy's Imprecisions'. Pp. 121–134 in A. Jones ed. *Ptolemy in Perspective.* Berlin/Heidelberg.

von Jan, K. 1895. ed. *Musici scriptores graeci. Aristoteles, Euclides, Nicomachus, Bacchius, Gaudentius, Alypius et melodiarum veterum quidquid exstat.* Leipzig.

West, M. L. 1992. *Ancient Greek Music.* Oxford.

________ 1999. 'Sophocles with Music (?). Ptolemaic Music Fragments and Remains of Sophocles (Junior?), *Achilles*'. *Zeitschrift für Papyrologie und Epigraphik* 126:43–65, pl. IX–XII.

[27] I am grateful to my colleague Peter Schubert for helping me to understand more clearly the issues raised in this review and for kindly giving me feedback on this review. I also thank Mohamed Masmoudi and Matthew Provost.

Winnington-Ingram, R. P. 1936. *Mode in Ancient Greek Music*. Cambridge.

The Symptom and the Subject: The Emergence of the Physical Body in Ancient Greece by Brooke Holmes

Princeton/Oxford: Princeton University, 2010. Pp. xvvi + 355. ISBN 978–0–691–13899–2. Cloth $45.00/£30.95

Reviewed by
Susan H. Prince
University of Cincinnati
susan.prince@uc.edu

This is a humanist's book and readers of *Aestimatio* will want to know up front that the original claims are less about the scientific knowledge or reasoning of classical Greece than about the role of medical discourse in the development of mainstream Athenian classical thought, literature, and philosophy, as it has more traditionally been defined. Since most scholarship on the rich and well preserved Hippocratic corpus is all too isolated, and since ambitious syntheses of Greek intellectual history like those of Bruno Snell (in the mid-20th century) and G. E. R. Lloyd (early 1960s to present) can always be supplemented, not least by integration of the prolific research in ancient medicine from the last 30 years, Holmes provides a timely new avenue for putting ancient medicine centrally on the map of classical Greek thought.

Rosalind Thomas' *Herodotus in Context* [2000] can be compared; and Holmes exceeds Thomas in range and ambition by virtue of her forays into core ideas of tragedy and philosophy, where she establishes the influence of medicine, not for the first time, but as a turning point in a new master-narrative and in a setting accessible to a broad audience. Perhaps inevitably, given the range of the book and its proposal of a continuously developing story from Homer to Plato, Holmes' explanations can fall short of Thomas' precedent in precision and rigor. Yet these explanations are interesting and they intersect with modern issues that both are and ought to be philosophical in so far as we can accept Holmes' quest to historicize and so destabilize the conception of a passive, automatic human body continuous with inert matter (and so with robots and other artificial life forms), such as might be assumed in some modern schools.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 171–188

Specialists in ancient medicine should be glad to have such an articulate and intelligent advocate trying not only to bridge the gaps among subfields of Hellenic studies but making connections to Foucault and the 'mind-body problem' that we have inherited (as the story goes) from Plato and Descartes. Holmes' main goal, if it can be put so bluntly, is to pin the very origin (which is sort of a proto-origin in so far as it needed the later development by Plato among others) of the mind-body problem on the Hippocratic corpus.

Holmes keeps her main argument before the reader's mind in clear fashion.[1] She argues that 'the body' inherited by the Western Tradition (which is, overall, Plato's 'body' («σῶμα»), as we see it especially in the *Phaedo*, *Alcibiades* I and, more theoretically, in the core arguments of the *Sophist* and *Timaeus*) has a history that can be usefully traced, presumably in order to show that it is contingent on certain interlocutors rather than self-evident and universally true. The Greek term «σῶμα» is part of the argument: overall, Holmes thinks (against the complications brought up by Snell in *The Discovery of the Mind* [1953]), there is a simple, continuous trajectory from Homer to Plato whereby what was originally a term for organic bodies becomes a term for inert body, that is, matter. At the same time, Holmes hangs with one or two fingers onto Snell's implication that Homer's use of the word «σῶμα» only for corpses shows that there always was something dead and disparate about it. Her explanation for the semantic shift of «σῶμα» in the mid-fifth century BC, which she discerns from Melissus fr. 30B9 [Diels and Kranz 1952] in conjunction with a passage in the Hippocratic *On Regimen*, appeals to issues Plato cared much about, form and stability. Like Snell's own argument, hers seems to have a Platonic teleology. But the big picture does not depend essentially on the argument about Melissus. We have always known that Plato differs from Homer in ranking soul over body, and Holmes' case for a particular path through the intervening time and culture, whereby the Hippocratics become one key precipitating background for Plato, is convincing overall. (She encourages us to forget about the Pythagoreans, the more traditional answer: but it is likely that Plato's thought is a focus for many traditions.)

---

[1] See esp. pp. 2, 15, 22, 85–87, 189–191, 193–195, 225–227, 237–39, 273–274, 275–276.

Publishing a major book like this one within five years of the Ph.D. is a stellar achievement. Holmes (Ph.D. Princeton University, 2005) has read broadly in many languages on topics in Greek literature, Greek philosophy, modern art, and modern theory; and she has studied a wide array of Greek texts from Homer to Plato *via* the Presocratics and Hippocratics with a particularly keen eye for Euripides (evident already in an article of 2008 in *Classical Antiquity*). Her single most important interlocutor may be Foucault, although *The Use of Pleasure* [1985] and *The Care of the Self* [1986] do not dominate the book: indeed, the aim is to fill in more completely the story of origins that Foucault elides [5, 20n64, 177n119, 189–90]. From and through this previous scholarship and critical inquiry, Holmes tracks a course of her own, articulated nicely both in chronological terms and on the level of her prose. This is, to repeat, an achievement matched by few. That said, the reviewer's task is to evaluate the book, not the author. In Holmes' own words [87n9], in reference to Aristotle's history of his predecessors, one might expect, and even prefer, a 'healthy distrust' to any such master-narrative proposed as the account of the past. The following comments highlight Holmes' major claims and submit them to (some) critical examination. Since the book is so comprehensive, other readers may prefer to select other points for close criticism.

The book can be summarized under the three-part structure that Holmes gives [37–40]. First [ch. 1], Homeric poetry shows us how Greeks understood the boundaries of the ethical human self and the non-human 'other' before the emergence of naturalist thinking (Holmes renounces 'science' in its full sense) in the sixth and fifth centuries and of 'new medicine' in the late fifth and early fourth centuries. The human being was something complex, not just a soul and not just a body, but a being comprised of various working parts that could generally be seen and sensed. Surprising (and unseen) disruptions in the regular function of the human being, that is, both magical and uncaused events, were interpreted as divine actions; and the gods, who were intentional like humans, constituted the main field of 'the other' that interrupted the phenomenology of human life and so also constituted the main limit on human responsibility.

Second [ch. 2–4], the first Greek natural philosophers struck out in a new direction by conceiving of an extra-human world, or cosmos, as (quasi-)systematic, operating mostly by internal laws that allowed no choices or decisions and, hence, no ethical value. When 'new medicine' emerged, the Greek doctors, as well as some of the philosophers, used the naturalists' terms to explain the human being and so developed a kind of systematic explanation of the human being based on the more or less automatic interactions of internal, non-ethical stuffs such as humors and their quasi-chemical qualities or powers. The medical texts generally omit the ethical person, treating him or her as the object of the doctor's technical care. But because the nature under discussion in these technical treatises is the nature of the human being, we can trace out the authors' recognition (which is sometimes explicit, sometimes implicit) of the ethical person. This ethical person shows up mostly as one who is supposed to cooperate with the physician by taking care of him- or herself. This internal caregiver, who seeks what is objectively best against what might be subjectively pleasant, is the prototype for the new subject position, or the new 'ethical substance' (a quotation of Foucault), of the newly abstracted, non-embedded Greek ethics.

Third [ch. 5–6], the consequences of the new turns in medicine specifically are evident in philosophers and at least one tragedian, Euripides, of the late fifth and early fourth centuries. Philosophical ethics emerges as care of the self, where the self to be governed is equated or aligned with the body; and the ethical agent of care is equated with a thinking and planning capacity, whether the 'mind' or the 'soul' or 'deliberation' or another intellectual organ or faculty. The newly theorized body is both the 'foil' for the newly theorized psychological self, defining what it is not, and an analogy for the psychological self, offering a model for the hidden interior. The real person is thereby reduced to the soul, which is not a body but has (or plausibly could not have) a body. (This way of putting things can be contrasted with its converse, that the person is primarily a body, which has, or plausibly could not have, a soul.) This soul has both unity and anatomy like that of the body, by analogy. It can acquire disease and it can grow or nurture a disease automatically, so to speak, in its own internal cavity, whose workings are unseen but can be diagnosed and explained and either can be (in the most optimistic passages of the philosophers) or cannot be (in tragedy) treated and cured.

Close analysis should pay particular attention to the Hippocratic texts that Holmes calls into play. But first Homer. Holmes' picture of the Homeric background, in its focus on wounded and dying soldiers, might paint around and over certain matters that could be seen differently; but no one will dispute that abstract and technical accounts of the human, whether body or soul, are not found in Homer. Whether there is body-soul dualism in Homer (the answer depends on what 'dualism' exactly requires, whether different 'material', which is false and impossible for Homer, or just spatial separability and independent persistence of two components), this is relevant only at death and it has no role in the structure of the living human and no place in the ethics of living.[2]

The reader is sometimes uncertain, given the ethical evolution that she posits in the full run of the book, how Holmes places Homer's characters in the history of ethical subjects. Many readers have found the *Iliad* to be far more realistic and humanist, and far less interested in necessary (rather than rhetorically useful) divine causation or even a divine 'other' than it looks under Holmes' reading.[3] If there are not fully mature ethical subjects in the *Iliad* and *Odyssey*, it is hard to say what is missing. Government of the self is not abstracted in Homer as it is by Plato; but both Plato [*Rep.* 441b] and modern scholars have shown—as Holmes acknowledges in passing [60–61, 69–72]—how Homeric characters experience dilemmas and how they overcome parts of themselves, the fearful or emotional parts, through the operation of other parts of themselves, the parts that look for the best or the most advantageous outcome.[4] Holmes never claims really that ethics is missing from Homer (and she is very careful to keep Homeric characters' belief in their gods plausible). Rather, her clearest claim is that in the wake of the Hippocratics we get 'a new kind of "ethical substance"' [189, quoting Foucault 1985, 26–27]. But a main arch of her story [e.g., 226] is the claim that the 'daemonic other', against which the human ethical self is to be defined and judged after the Hippocratics have made their mark, is the body; whereas in Homer

[2] One might note that death is a major motivation for the body-soul dualism in most human cultures and so could be important to Plato, too, in and after the *Phaedo*, despite Holmes' efforts to sideline Greek eschatological traditions [30–31; 195n9 is a more positive footnote].

[3] Griffin [1977] showed that Homer, by contrast with other residues from the epic cycle, is generally not super-natural.

[4] E.g., Williams 1993 and Gill 1996, both of which Holmes uses at multiple points.

it had been the gods, who are somehow not fully to be distinguished from the person or not rigorously definable themselves: hence, ethics is not defined. This seems to diminish Homer. Perhaps one could get Holmes and Foucault both to agree that the new kind of ethical substance which we find in the philosophers might restrict the range of ethics even as it deepens the concepts by focusing so obsessively on control of the subject's own appetites. Of course, this change, for the better or for the worse, is handed down through Aristotle's practical syllogisms and Thomas Aquinas to mainstream European culture of the high Middle Ages and becomes a core (or the core) of ethics for several centuries of the pre-Cartesian, pre-Kantian, pre-Nietzschian, pre-Freudian, pre-Foucauldian background.

Let us turn to Holmes' history of automatic systems and inert body [ch. 2–4]. Surveying Anaximenes, Xenophanes, and Heraclitus, and including fifth-century receptions in Aristophanes, Euripides, and Plato of their ideas about cosmology, Holmes shows that by the late fifth century the 'laws of nature' can be considered to have 'a measure of autonomy' [98]. No one will dispute this. Meanwhile, thinkers such as Alcmaeon, Anaxagoras, Empedocles, and Democritus describe humans in physicalists' terms and address interactions and reciprocity between humans and non-human nature, including their birth, death, and sensation [99–101]. No one will dispute this either. Holmes' own claim is about just how the Greek word «σῶμα», which in or before Plato's works (esp. *Sophist* and *Timaeus*) is to become the earliest Greek word for inert matter, makes that semantic shift from organic body to inert matter.

The key idea, according to Holmes, is that the σῶμα is 'the site' of the physical reciprocity between organic bodies and inert cosmic matter [101] as organic bodies come to be and pass away: the fact that organic bodies have interchange with stuff outside themselves motivates the use of the term «σῶμα» for both.[5] The outcome of the semantic shift of «σῶμα» to 'inert matter' is apparent in a citation of Diogenes of Apollonia by Simplicius [Diels and Kranz 1952 fr. 64B7] and in several *testimonia* on Gorgias as well as directly in his *Encomium of Helen* [§8], evidence which probably pre-dates Plato.

[5] This is the process that Aristotle explores in *De gen. et cor.* and it is implied in passages of Plato from as early as the *Symposium*, but evidence in the Presocratics is lacking.

But the key figure for Holmes is Melissus (*flor. ca* 440 BC), who is attested using the term «σῶμα» in Diels and Kranz 1952, fr. 30B9. Given Melissus' presence in the opening of the Hippocratic *On the Nature of Man* as well as his own discussion of pain as a phenomenon to be denied to what-is [fr. 30B7], his bridging function is highly plausible. Older scholarship has already proposed that this fragment of Melissus is intermediate in the intellectual discovery (or invention, as one prefers) of non-material being.[6] Holmes finds insufficient Sedley's more recent view[7] that Melissus denies σῶμα to what-is because he is denying its anthropomorphism (and so the term is neither a novel metaphor nor a dead metaphor, but still stands on the earlier side of the semantic shift) and she proposes more: that key to Melissus' usage is an unrecognized component in the original, Homeric meaning of «σῶμα», the sense of corruptibility and change over time [104]. This meaning is supported from a sentence from *On Regimen* 1.28, which Holmes dates to '*ca* 400 BC' (40 years after Melissus is said to have flourished), and has obvious connections to Plato's view of aesthetic objects.

Maybe this solution is not impossible but it seems almost like a rabbit pulled out of the hat. In particular, the Homeric background that Holmes claims [104] seems wrong. Although Holmes lays the groundwork earlier in explicating Homeric «σῶμα» as 'flesh' and 'the point where form is yielding to formlessness' [34], this interpretation (which gains support from J.-P. Vernant [1991, as well as from Renehan's paraphrase 'bulk' [1979, 278]) misses what seems to me a crucial difference between «σῶμα» used as a so-called count noun, one that implies individuation and readily becomes plural, and «σῶμα» used as a mass noun, one that, like 'flesh' or 'blood' or 'bronze', usually remains singular because it names unformed stuff.[8]

Up to Melissus, «σῶμα» is a count noun and a group of dead organic bodies are σώματα, not σῶμα. It is unclear whether Melissus in fr. B9 uses σώματα as a count noun or a mass noun (Holmes negotiates this ambiguity by placing an article in parentheses); but,

---

[6] See Renehan 1980, 117 with references.

[7] Sedley 1999, reported and endorsed in Palmer 2003, 4.

[8] At the same time, the Pluralists could naturally speak of their stuff as plural σώματα that are not to be counted, as one can also speak of 'bones' and 'sinews' as organic matter.

since Simplicius uses no article in citing Melissus, one might want to presume the mass-noun sense. Or, if it is a count-noun applied (or, more correctly, denied) to the unique being the cosmos (or the singular what-is), the choice might vanish and this usage might help to bridge the semantic gap. In *On Regimen* 1.28, by contrast, we have the older use by this criterion, a count-noun used for an individual human body, whereas 'the soul' in this passage seems to be used as a mass-noun since it is the same for all ensouled beings. (It might be argued that 'the body' in its first occurrence is parallel, even as the point is that each being has a different body; but this is ambiguous in the same way it is for Melissus). Further, corruptibility has no resonance in Simplicius' context or in the (very inert) pseudo-Aristotelian treatise *On Gorgias, Melissus, and Xenophanes*, where the relation between σῶμα and the Eleatic what-is is also featured. One can say that the question deserves more investigation and that any use of «σῶμα» or «σώματα» in the mass-noun sense in the Hippocratic corpus would be relevant.

Meanwhile, the contrast between the varying bodies and the identical soul (of male and female, in this case) in *On Regimen* 1.28 is very interesting in relationship to Plato's ontology; but it is an unsolved problem exactly when most texts in the Hippocratic corpus were written and how much they were influenced by Plato himself. (I am on Holmes' side, generally in favor of the Hippocratics' independence of Plato.) More likely, both could have been responding to the same debates between Monists and Pluralists. When it comes to more subtle points of reasoning, rather than vocabulary in itself, a direction of influence from late fifth-century natural philosophy into medical physiology seems more plausible, in general, than the reverse direction that Holmes is proposing. We can agree to a special relationship between Melissus and medicine (shared by other philosophers such as Alcmeon, Empedocles, and Democritus), and possibly to Melissus' special role in the changing meaning of «σῶμα»; and this may be all Holmes needs since Melissus wrote text now lost where a clear link might have been found. In short, we see the interaction between natural philosophers and doctors in a more robust way than we otherwise might and we recognize how closely the organic body and cosmic nature (or what-is) were being compared and influenced each other's conceptualization.[9]

Holmes stops short of making organic bodies entirely inert or automatic, for there remains space in which the expert doctor will intervene (as she shows in detail in ch. 3). But she suggests that philosophical and medical theory comes close enough to doing this that the ethical subject is virtually omitted from the theory of the human. This is the 'invention' of the body or σῶμα that she has promised [16–21, 28] to map out.

Holmes sets herself up to announce the rebirth of the ethical subject in her fourth chapter; and her most original contribution to the study of ancient medicine lies here, in combing out the ethical subject that must be implied (as she sometimes seems to assume, although she also demonstrates why and how) amid the objectified, systematic, but non-transparent body in which he or she now resides.[10] Her readings of *On Ancient Medicine* and *On Regimen*, as well as of other texts, show that medical care is a reciprocal practice between doctor and patient in which the patient must communicate with the doctor about his or her 'biofeedback' [166] and must cooperate in making advantageous choices about what to eat and drink, when and how much to wrestle or sleep, and so on. Key to her point is that the right answers of medical technique, whether this is in the domain of doctor or patient, are, despite the 'biofeedback' term, non-transparent to the body as a whole, which therefore needs something else, the external doctor or the internal ethical subject, to direct it. The ideal ethical subject is subject of the symptom in a secondary sense, not by feeling it but by deliberate and educated response to knowledge of its causes; alternatively, the patient who fails to follow correct technique *versus* the symptom or is passive to it becomes 'a

---

9 G. E. R. Lloyd is responsible for most of the modern work in this area but he never fully accounts for Melissus' intersections with medicine: his most extended treatment of Melissus is in Lloyd 1979. Patricia Curd's subsequent work on the Eleatics laid the groundwork for distinguishing Melissus from Parmenides [see Curd 1993 and 1998].

10 One must remember, of course, that there were other ethical subjects alive in Athenian culture, such as in Sophocles' tragedies if not in the still beloved Homer, and that the ethical subject of Hippocratic care need not have displaced them all, although maybe it did eventually.

symptom himself' [176, elucidating the book's ambiguous title; also 189, 217].

But sometimes Holmes may be imputing too much non-transparency and passive mechanism to this body (or thinking ahead too fast to Plato and Aristotle). The issue comes to the fore several times in chapters 3–5. To take one example [167–169], in *On Ancient Medicine* §9 the writer appeals to αἴσθησις τοῦ σώματος (perception of the body) as the ultimate criterion for determining correct treatment. There has been scholarly debate over whether the genitive 'of the body' is subjective or objective, that is, whether the body perceives (e.g., its own warmth or cold, or pleasure or pain) or whether someone, such as the doctor, perceives the body by poking it or looking at it, for example. Schiefsky (most recently) has argued on the basis of parallels in other Hippocratic texts that the body does perceive changes as subject and he understands that there is a transparency implied from 'the body' to the person who reports 'bodily' sensations to the physician [2005, 188–189]. Holmes, too, concludes that the genitive is subjective; but because the body is incomprehensible without the expertise of the doctor (as she argues from *On the Nature of a Human Being* §2), someone must then interpret the body anyway and both the physician and the patient are in equal position to 'gather somatic data' and 'make inferences'. Since *On Ancient Medicine* seeks to defend medicine as an expert's field, however, not to make it a self-help field or 'democratic' (which is more the interest of *On Regimen*), Holmes concludes that the physician 'has the advantage' in interpreting the body's data; and so, although the genitive is subjective, we compound the diagnosis process from αἴσθησις to αἴσθησις plus judgment and get two subjects, thereby ending up with the same meaning we would have if we had read the genitive objectively. One cannot help suspecting that Holmes is presupposing the compound nature of judgment in, e.g., Plato's *Theaet.* 163b–c.

In chapter 5 [196–197], we do look into a text, *On Diseases* 4.39, where Holmes finds a 'rare counterexample' in which the body's needs are 'seamlessly and uncannily transformed' into the person's desires; and then we consider some 'probably...playful' similarities [201–202] in Plato's *Gorgias* and *Phaedo* (as they may well be, but especially if certain rivals thought they were serious). My own suspicion is that this is closer to normal than Holmes is allowing. Apart from this inclination to overlook transparent bodily 'perception', Holmes succeeds in making her overall point, which fills out and advances the insights of Foucault in *The Use of Pleasure* [1985, 99–116]. I will be among the first to agree that bodily 'perception' is several steps removed from ethical subjectivity or responsibility and that the questions merit further treatment at least in the Hippocratic corpus. Holmes maps out interesting territory for future work.

In her fifth chapter, where she turns to philosophical ethics, Holmes takes one last look at the medical corpus (*On the Use of Liquids* §2) to argue that the medical corpus itself has an inchoate notion of ethical error arising from conflict between the person's desire and what is in fact good for the body, that is, 'how desire—and especially desire for pleasure—comes to be articulated as an ethical problem' [200]. She may want to set out a precedent or parallel for Plato's conflict among parts within the soul by pointing to a Hippocratic writer's conflict among parts within the body, and to show that the Hippocratic 'person' is identified with a privileged one among these parts just as in Plato's *Republic* the 'person' becomes essentially the reasoning part of his soul. She also wants to ask, in consideration of a position like the Socratic denial of ἀκρασία, how the person ever comes to commit ethical error. She explains as follows:

> The author of the treatise *On the Use of Liquids* makes just this assumption—namely, that because we are estranged from the cavity and its needs, other motivating forces, more intimately felt, surge up in the conscious field. The author has been observing that different parts of the *soma* take pleasure in (ἥδομαι) or are vexed by (ἀγανακτέω, ἄχθομαι) heat and cold. He then turns to note that, although the cavity grows irritated when it is overpowered by cold, the person, being 'very far from feeling it' (πλεῖστον ἀπέχει τοῦ παθεῖν), sometimes develops a desire for [*scil.* to drink] something cold. Given that this desire is most proximate, it is only to be expected that the person takes pleasure in his cold drink, oblivious, at least initially, to any distress caused to the cavity. From one perspective, the (initially unfelt) conflict between the needs of the cavity and the needs of the person is just one possible example of conflict within the physical body's composite nature. At the same time, this conflict is singular, in so far as one 'body part,' that is, 'the person,' has the power to seek its pleasure at a significant cost to the

> pleasure of the other parts and, indeed, to the health of the whole. [200]

Liq. 2 is a difficult text, as one sees when one tracks it down.[11] Without going into full detail, it seems worth pointing out that 'the person' whom Holmes sees here and who is split off from experiencing his body is not in the Greek but is supplied as the subject of a third-person clause in the translations of Joly and Potter to make sense of the text. Littré, following the articulation of the Greek, understands the subject of «πλεῖστον ἀπέχει τοῦ παθεῖν» ('being very far from feeling') as 'the breast region and the cavity' itself. It seems to me also more natural to understand that the cavity is subject of the whole run of the sentence, the part that is both fatally conquered by the cold (drink) because it is least accustomed to cold (since it is located closest to the body core and so naturally warmest and most able to flourish in warmth) and at the same time farthest from experiencing the pathology of the cold because it most lacks the cold (and has ignorance of, but also desire for, what it lacks) and so takes pleasure in receiving the cold drink. In the same way, just earlier in the text, wounded lesions, which seem to know better what is good for them, take pleasure in warmth and, as Holmes reports, many body parts throughout the text have taken pleasure in the warm or the cold. The cavity has a special built-in liability, not because it is farthest from 'the person' who fails to experience his body but because, unlike superficial body parts like the skin, being deep inside, it does not have tolerance for variation, yet it does have a fatal desire for what it lacks. The medical writer might need to explain to the ethicist how it is that the cavity's pleasure in something new drives the person's mouth to imbibe a cold drink, but this writer does not even recognize a problem.

Philosophical ethics, Holmes goes on to argue in chapter 5, arise exactly on the precedent of, and sometimes in competition against, the medical ethics of regimen that she has traced out. In addition to Plato, to whom we return below, Holmes considers Democritus and Gorgias as ethicists who take on medical models. Basically, her goal seems to be to document an ethics that recognizes drug-like causal forces in the psychological realm and offers (or, in Gorgias' case, teases about offering) an expertise precisely in this situation

[11] Joly 1972, 166.15–167.5 = Littré 1927, 124.1–17.

without dismissing the body or resorting to dualism. As she tells us [214–215, 222–225], the precise way for attaining ethical freedom is not documented for these thinkers but we can reconstruct it. (Or for Gorgias, maybe ethics is all negative, a matter of resistance and rejection.) Just as Gorgias calls λόγος a drug for the soul [*Encom.* §14], so Democritus holds that 'thoughts can act, as it were, as drugs against potentially damaging desires' [223].

For Democritus, two special points seem worth querying before we accept either a connection to the Hippocratics or a non-dualist ethics. First, he does speak explicitly of the soul cloaked by a body [Diels and Kranz 1952, fr. 68B187] in a manner not unlike Pindar's fragment 131b, where Orphism has been suspected, or the dualist *Phaedo* of Plato. Moreover, his term for the body (here as in several other fragments) is 'tent' («σκῆνος»), not «σῶμα». If the soul-body analysis of the person in the late fifth century is the effect of the Hippocratic doctors' theorization and objectification of the σῶμα, why do we consistently find this odd term in Democritus? It suggests at least independence from the Hippocratics, to leave aside the question of dualism. Democritus did write texts whose titles are medical but was he in another tradition? Or did he explicitly reject the Hippocratics?

Second, the ethical therapy that Holmes reconstructs for Democritus is a top-down therapy from the psychological level to the physical, a rebalancing of the soul's atoms,[12] and not *vice versa*. It is not even the case that the psychological causation in the subject is steered by an external teacher since Democritus (in the preserved fragments) is offering self-help. If the psychological person is controlling the physical person—which admittedly could be a reversal of the more common relationship that Democritus seeks to correct—how is this different from the soul's government of the body recommended by Plato? Holmes might not claim that it is (she aligns Plato with these others [226]). But either the whole question of mind-body dualism vanishes as a significant feature of ethical theory, thus undermining the promises of the book, or the 'mind-body problem' turns into the full range of positions that are not rigorously determinist in the bottom-up direction, physics to psychology.

---

[12] Note «μεταρυσμόω» in fr. 68B33 and the uncompounded verb in 68B197 with Vlastos' interpretation [1945–1946].

When it comes to the influence of the medical writers' account of the person on Plato's ethical theory, Holmes is all too brief,[13] understandably enough, considering the pervasive and differential presence of the health metaphor, complexity of the soul, and soul-body dualism in the Platonic corpus as well as the considerable scholarship on Plato's moral psychology published in recent decades. Let us distinguish a few questions that one might ask.

First, one could hope to explain the very basis of the polarized ethical choice between the good and the pleasurable in Socraticism up to *Philebus*, and why some Platonic texts, such as the *Gorgias* and *Phaedo*, appear to imply a 'basically bipartite' soul [201]. Holmes is at her best here, not so much on the anatomy of the bipartition (which is not clearly made by Plato), but on the oddly polarized conflict between the advantageous and the pleasant and why the one is a value for the soul and the other for the body. Although Holmes does not fully exploit the centrality in the Hippocratic corpus of the vocabulary of the 'beneficial' (τὸ σύμφερον or the verb συμφέρειν) *versus* the 'harmful' (τὸ βλαβερόν, βλάβη, or the verb βλάπτειν), and the 'pleasant' (τὸ ἡδύ and so on) *versus* the 'painful' (τὸ λυπηρόν and so on)—she brings it up briefly on page 199–this seems highly relevant to the centrality of the same kinds of terms in the ethics of Plato's dialogues, especially in the *Republic*, along with Socrates' insistence that there is a fully objective basis to this vocabulary.[14]

Socrates' conviction that there is an objective science of virtue is very well supported by the Hippocratics' conviction that there is an objective science of medicine.[15] This is no new insight—Socrates says as much at, e.g., *Rep.* 353b–d and there have been previous studies of the medical craft in Plato—but Holmes presses and develops the point in ways that make it more serious ethically: within the *Republic*, the health analogy is presented as a second-best explanation for the

[13] There are scattered references in ch. 4 and six pages in ch. 5: 201, 206–211. She promises more in a separate study [Holmes 2010].

[14] This is by distinction with, e.g.,Thucydides' Melian dialogue, where 'advantage' is counter to justice and the conflict is between parties to the situation rather than between values or forces within one person's consciousness.

[15] Of course, we come up again against the possibility that a text such as *On Ancient Medicine* is post-Platonic and one hopes this question can be settled decisively.

objective nature of justice in case we do not buy the longer proof in the city-soul analogy and the dependence on the Form of the Good. Holmes defers various questions (for lack of evidence) that could shore up the connection more tightly, such as how physical diseases are like foul desires and to what degree the origin and development of each is parallel [e.g., 193, 196]; but it does seem that Plato sees a 'daemonic' inner system of the soul at work in texts such as the *Gorgias* and *Republic*.[16] The corrupt soul of the tyrant and its development along with the arguably self-directing and autonomous evolution of deviant constitutions in *Rep.* 8–9 might depend more closely on the medical corpus than we immediately see.

Second, one could hope to explain the more famous, and more detailed, tripartite soul of the *Republic* and *Phaedrus*, often recognized behind Freud's theory of the soul, where the notion of an unseen inner anatomy seems most vivid. Holmes does not touch this point, leaving mention of the tripartite soul to footnote 31 on page 202. Socrates' argument for the tripartite soul at *Rep.* 435b–441e, meanwhile, does use terms and vocabulary that one can connect to the medical corpus, although geometry is also evoked at 436d–e.

Third, one could hope to explain the extreme mind-body dualism of the *Phaedo*, where Plato's Socrates argues that the soul is a divine, eternal substance as opposed to the body in which it is temporarily entombed. This seems continuous with the semantic shift of «σῶμα» to inert matter seen in the *Sophist* and *Timaeus*, and may be the beginning of the 'Western Tradition' that Holmes is interrogating. But the dualism of the *Phaedo* is not addressed rigorously, although Holmes cites it as an outstanding example [e.g., 202n31]; and the dualism of the *Timaeus* provides the springboard for the conclusion [275] but is left hanging as a most extreme example of Plato's dualism. The story about how this notion gets shaped across the Platonic literary corpus was never really begun, let alone finished. But this is sooner a research agenda than a book topic. Holmes' book urges us to keep the Hippocratics on the agenda.

Holmes saves her final substantial chapter for Euripides' reflection of the medical writers' theory of the body. Although this is

[16] Since Holmes explains in her final chapter that the tragedians too derive this kind of thinking from the doctors, one might ask how we can decide whether Plato was influenced more by the tragedians or by the doctors directly.

disconnected from the philosophical and scientific history in that a tragedian may be more interested in complicating 'the answer' to ethical problems than in solving them, it answers the book's beginning in Homer and adds to Holmes' endeavor to restore the female subject omitted by Foucault [evoked, e.g., 20n64].[17]

Holmes offers sequential readings of the *Troades*, *The Madness of Heracles*, *Orestes*, and *Hippolytus* that show how consistently Euripides imagines madness and erotic affliction on the model of medical diseases. The differences between men's and women's diseases are not examined systematically but 'female nature' appears, as in the Hippocratic corpus, 'as a model for the daemonism that is buried in human nature' [262]. The 'magico-religious paradigm' of explanation for symptoms is not replaced by medicine, however, as it generally is in the naturalizing philosophical traditions; rather, Euripides explicitly overlaps natural explanation with religious explanation, rival accounts of the same problem. This is the best way to understand Euripides' theological outlook, Holmes proposes, as well as to map out compelling readings of his drama. Holmes is interested sooner in how medical discourse helps Euripides to articulate complex truths about the human condition and to write plays that have tension than in how tragedy itself is like medicine (as some of the Stoics thought, following a tradition that might be continuous from Hesiod) or how even tragedy, what we might consider the most 'artistic' and least technical of Athenian discourse, documents the importance of medicine in all registers of Athenian literature. For this broader treatment we have, most recently, Jennifer Kosak's book [2004] on Hippocratic thinking in Euripides.

Holmes succeeds on many levels. There remain all kinds of questions to be asked about the relationships between Plato's thinking and the Hippocratics', which Galen was not all wrong to align so easily. Whether or not we assume interdependence, the Hippocratics should be recognized better for their participation in the otherwise lost world of later 'Pre-Socratic' philosophy. Entities, powers, and causes permeate both the Hippocratics and Plato, sometimes in different idioms, sometimes in the same. New commentaries on Hippocratic

[17] She addresses the female body at pages 185–188 and briefly elsewhere.

texts, such as those by Jouanna, Craik, and Schiefsky[18] have made new syntheses such as this book by Holmes possible; and one hopes that there is more to come.

BIBLIOGRAPHY

Craik, E. 1998. *Places in Man.* Oxford.

________ 2009. *On Glands.* Leiden.

Curd, P. 1993. 'Eleatic Monism in Zeno and Melissus'. *Ancient Philosophy* 13:1–22.

________ 1998. *The Legacy of Parmenides.* Princeton.

Diels, H. and Kranz, W. 1952. edd. *Die Fragmente der Vorsokratiker.* 6th edn. Berlin.

Foucault, M. 1985. *The Use of Pleasure.* R. Hurley trans. New York.

________ 1986. *The Care of the Self.* R. Hurley trans. New York.

Gill, C. 1996. *Personality in Greek Epic, Tragedy, and Philosophy: The Self in Dialogue.* Oxford.

Griffin, J. 1977. 'The Epic Cycle and the Uniqueness of Homer'. *Journal of Hellenic Studies* 97:39–53.

Hankinson, R. J. 2008. *Cambridge Companion to Galen.* Cambridge.

Holmes, B. 2008. 'Euripides' Heracles in the Flesh'. *Classical Antiquity* 27:231–238.

________ 2010. 'Body, Soul, and Medical Analogy in Plato'. Pp. 345–386 in K. Bassi and P. Euben edd. *When Worlds Elide.* Lanham, MD.

Joly, 1972. ed. and trans. *Hippocrate.* Du régime des maladies aigués*. Appendice:* De l'aliment*,* De l'usage des liquides. Paris.

[18] Jacques Jouanna has published seven volumes in the Budé series (covering texts too numerous to list) from 1983 to the present. Elizabeth Craik has published *Places in Man* [1998] and *On Glands* [2009]. See also Schiefsky 2005. Paul Demont, Philip van der Eijk, Heinrich von Staden, and others have been advancing our research on the connections between older Greek medicine and philosophy beyond the fundamental work of G. E. R. Lloyd. Philosophical work on Galen may be in fuller force, as witnessed by R. J. Hankinson's *Cambridge Companion to Galen* [2008] and his other work.

Kosak, J. 2004. *Heroic Measures. Hippocratic Medicine in the Making of Euripidean Tragedy.* Leiden.

Littré, E. 1927. *Oeuvres complètes d'Hippocrate. Traduction nouvelle avec le texte grec.* vol. 6. Paris.

Lloyd, G. E. R. 1979. *Magic, Reason and Experience.* Cambridge.

Palmer, J. 2003. 'On the Alleged Incorporeality of What Is in Melissus'. *Ancient Philosophy* 23:1–10.

Renehan, R. 1979. 'The Meaning of ΣΩΜΑ in Homer: A Study in Methodology'. *California Studies in Classical Antiquity* 12:269–282.

________ 1980. 'On the Greek Origins of the Concepts Incorporeality and Immateriality'. *Greek, Roman, and Byzantine Studies.* 21:105–138.

Schiefsky, M. 2005. *Hippocrates: On Ancient Medicine.* Leiden.

Sedley, D. 1999. 'Parmenides and Melissus'. Pp. 113–133 in A. A. Long ed. *The Cambridge Companion to Early Greek Philosophy.* Cambridge.

Snell, B. 1953. *The Discovery of Mind: The Greek Origins of European Thought.* T. G. Rosenmeyer trans. Cambridge, MA.

Thomas, R. 2000. *Herodotus in Context.* Cambridge.

Vernant, J.-P. 1991. 'A "Beautiful Death" and the Disfigured Corpse in Homeric Epic,'. Pp. 50–74 in *Mortals and Immortals: Collected Essays.* Princeton.

Vlastos, G. 1945–1946. 'Ethics and Physics in Democritus'. *Philosophical Review* 54:578–592 and 55:53–64.

Williams, B. 1993. *Shame and Necessity.* Berkeley.

Idols in the East: European Representations of Islam and the Orient, 1100–1450 by Suzanne Conklin Akbari

Ithaca, NY/London: Cornell University Press, 2009. Pp. xii + 323. ISBN 978–0–8014–4807–2. Cloth $49.95/£33.95

Reviewed by
Charles Burnett
Warburg Institute
Charles.Burnett@sas.ac.uk

This is an interesting book written by a medieval literary historian about a subject which is currently much discussed. My first concern about the book, however, is whether the title gives the right impression to the prospective reader concerning the contents of the book. 'Islam' appears in the title but it is not until the fourth chapter (out of six) that Islam appears. The first chapter is about space—how the world has been divided into major parts, from Classical times until the late Middle Ages, the shift from north-south division into an east-west (orient-occident) division, the significance of climate and proximity to 'the region of the Sun' to physical and moral characteristics. The second chapter is about places—Jerusalem, India, and Ethiopia—especially as described in the medieval accounts of the campaigns of Alexander the Great. In this chapter, Jews are mentioned more than Muslims; and it is followed quite naturally by a chapter on the medieval representation of the Jew. This provides an archetype or point of contrast to the representation of the Muslim, which is dealt with first from the point of view of the characteristics of the Saracen's body [ch. 4], and secondly from the representation of the beliefs of Muslims [ch. 5]. The sixth chapter brings together Muslim, Jewish, and Christian belief about ideal places: paradise and the place of philosophy.

If one is looking for Muslims then, one might resent having to read 150 pages before reaching them. If one is interested in idols too, it is really only in chapter 4 that they are discussed, in respect to the alleged idols of the Muslims. There is a clear agenda in this book (on which see below) but it is not indicated in an obvious way in the title. The book is deliberately not the last in line of

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 189–194

a series inaugurated by Norman Daniel's *Islam and the West: The Making of an Image* [1960] and promoted most recently by John Tolan's *Saracens: Islam in the Medieval European Imagination* [2002]. Nor is it filling in the Middle Ages for Edward Said's *Orientalism* [1978]. Akbari orients her work carefully in respect to all three of these worthy predecessors and has a useful appendix on Renaissance and early modern orientalism [280–288]. But she seeks to go back to the most basic elements on which any concept of 'Oriental' and 'foreign' should be founded, namely, the orientation of the medieval viewer geographically and his or her conception of what constitutes differences between human beings. Perhaps because she wants to press home these basic considerations, she belabors her points and succumbs to repetition and reiteration (Europeans are 'bolde and hardy' at least three times within pages 42, 46, 47) or, at any rate, proceeds at a slow pace with much summarizing of arguments both before and after setting them out.

The basic thesis can be demonstrated clearly enough: that (according to the Western medieval sources) northern-western climes (the abode of Christians) are more conducive to reason, good morals, and a fair complexion; and southern-eastern climes are conducive to folly, bad morals, and a dark complexion. Jews and Muslims differ from Christians not only on an ethical level but also physiologically, though Saracen bodies are less immune to being changed into Christian bodies (for example, through intermarriage) than are Jewish bodies. The Jews offer a particularly complex case study, being both from the East (sometimes even identified with the races of Gog and Magog beyond the limits of civilization) and subject to melancholy, yet also dispersed and ever-present within western society. Yet, in spite of their inherited perverse dispositions, attention to Oriental peoples could provide not only entertainment but also edification. Medieval romances show remarkable interest in the Orient and positive views of its inhabitants, as indicated in the tales of Alexander the Great, the description of certain Ethiopians as 'white' [99], the 'white Saracen' female characters in the *chansons de geste*, and the detailed and approving portraits of several oriental women: e.g., Candace in the Alexander Romance and Floripas in the *Fierabras* story. Problematic is the outcome of a union between a Christian and a Saracen: in *Fierabras*, a lifeless lump, not animated by the seed of the Saracen father, can become alive by Christian baptism [188]. What the narratives, histories, and debates about Orientals provide is the opportunity for western Christians to see reflexions of themselves in others (whether Jews or Saracens) and draw lessons from their behavior.

Akbari attempts successfully to erase the differences alleged by modern scholars between the discourse about the Saracens in the *chansons de geste* and romances in which Muslims are portrayed as polytheistic idolaters, and the 'learned' tradition which focuses on the biography of Muhammad and the heretical nature of his religion [200–247]. There is, rather, a continuous interplay between the learned (Latin) sources and the literary narratives. The section of the book that responds most closely to the implications of the title gives a good account of how Muslims were regarded as idolaters not only because of worship of the false trinity of 'Mahom', 'Apolin', and 'Tervagant' (as in the popular tradition) but also because of their idolatrous attitude towards Muhammad and the Ka'aba in Mecca. In the 'Chanson de Roland', the Muslims abuse the statues of their defeated gods just as if they are men, and this parallels the Western accounts of Muhammad being disgraced and abused at the time of his death. On the other hand, the stories of Muhammad being 'suspended in mid-air by the power of the magnet' (according to the 'Chanson d'Antioche' [227]) and of the worship of Muhammad's footprint, which reflected the reality at the Dome of the Rock in Jerusalem, give the Prophet the nature of a statue. In reappraising western views of the Muslim paradise, Akbari draws parallels to the luxurious utopia depicted in the Land of Cockayne. The contrast between Western and Eastern conceptions is not necessarily black and white. The discourse of the book is shot through with the polarities of east and west, north and south, black and white, masculinity and femininity, self and Other, often with the intention of eliding their contrariety, though this concern sometimes taxes credulity. For example, to say that the traveller circumnavigating the world in *The Book of John Mandeville*

> suggests that, if the traveler enters far enough into alien territory, he finds himself at home; to put it another way, a close look at the Other shows the self [63]

draws a farfetched conclusion in respect to a society in which the spherical universe was accepted as the reality.

The largest part of the sources is vernacular encyclopedias, chronicles, and *chansons de geste*, in which the author's *forte* lies. Prominent among these are John Trevisa's translation of Bartholomew the Englishman's *On the Properties of Things*, *The Book of John Mandeville*, Thomas of Kent's *Roman de tout chevalerie*, *King Alisaunder*, *The Siege of Jerusalem*, the versions of *Fierabras*, *The King of Tars, Parzival*, 'La Chanson de Roland', *Le jeu de saint Nicolas*, *The Sowdene of Babylone*, Dante's *Divine Comedy*, and 'The Land of Cockaygne'.

There is clearly more insecurity about Latin texts as the numerous mistakes in citations and translations testify:

- 'orbus' for 'orbis' [20, 36];
- 'septentrio' interpreted as the 'north star' rather than the seven plough oxen of the Big Dipper [37];
- 'ortu solis' [44 *bis*], 'ortum solis' [68, 69 *bis*], 'ortus sol' [69, 72], should all be replaced by 'ortus solis';
- 'historia naturalia' for 'historia naturalis' [75, 84];
- 'signo in caelo' for 'signa in caelo' [80];
- 'iuxta Alexandro' for 'iuxta Alexandrum' [82];
- 'in fine iste libro' for 'in fine istius libri' [82];
- the 'Summa totius haeresis Sarracenorum' is the summary of the whole heresy of the Saracens', not 'of all Saracen heresies' [250];

and so on. It is safer to leave out Greek letters altogether because they are almost bound to be misprinted [83]. I wonder too whether 'meneliche' in 'meneliche disposed' has a pejorative sense [42] or rather, like the original Latin 'mediocriter', simply means 'in the middle'; and whether 'dessouz nous' [63] should mean simply 'beneath us', not 'directly beneath us'.

Some arguments are farfetched or rely on other people's farfetched arguments, such as that of Tuttle seeing a 12-fold division in the *Liber floridus* of Lambert of St Omer as reflecting the 12 books of the eighth-century commentary on the Apocalypse written by Beatus of Lièbana [76]. It is unlikely that 'prester' in 'Prester John' would have evoked the extremely rare Latin word 'prester' as 'snake' [87]. It would have been nice to have some contemporary evidence that Jonitus was identified with Prester John [87]. It is difficult to see Roger Bacon's 'multiplication of species' as being analogous to his idea of the history of philosophy [274]. And how can a prism be 6-sided [274]? On page 116, Acon is 'Acre' (modern 'Akko'), and 'Comagine' is the ancient Middle Eastern kingdom of Commagene (not Carthage), while the Marcomanni [119] are a well known Germanic tribe.

In respect to the main thrust of the book (the perception of the Oriental or Muslim), one must always bear in mind that, in the Middle Ages, scientific learning taken from Arabic sources was greatly respected and not regarded as 'Islamic'. Akbari has usually been careful to do this, adducing the example of Roger Bacon, who made ample use of Latin translations of philosophical works written by Avicenna, even to the extent that he makes Avicenna critical of tenets of Islam [272–273]. But one can go further in saying that it is not ironic that Bacon should condemn Islam whilst at the same time drawing from the Muslim astrologer Abu Ma'shar [275]. He avowedly cites Abu Ma'shar as the leading authority on astrology in the West to provide 'scientific' information on which he can build his own theory of the progression of religion. (For this progression, he cites 'Ovid' who is in fact the pseudo-Ovid, author of the poem *De vetula*, which is likely to have been composed in Bacon's circle). It is misleading to talk in terms of 'Islamic' philosophy in 'Bacon's presentation' [269], although this term is often used for philosophical works written in Arabic.

It is useful to have an up-to-date account of medieval conceptions of space and time, of *chansons de geste*, and romances on oriental themes, on the life of Muhammad [224], and of Muhammad's heavenly journey [252]. The reference [260] to John Mandeville's private audience with the Sultan of Babylon 'in which the comparative merits of Islam and Christianity are debated' (surely a reflexion of al-Kindi's *Apologia*, which gives the same scenario) joins a host of other references to an attitude of tolerant inquisitiveness towards Orientals. One has sympathy with the irenic intentions of this book, in contrast to literature in which the Jew and the Saracen are always betrayed with hostility and fear. Akbari has written a challenging and original account of East-West relations and her work should join those of Norman Daniel, John Tolan, and Dominique Iogna-Prat in any bibliography on these relations in the Middle Ages.

BIBLIOGRAPHY

Daniel, N. 1960. *Islam and the West: The Making of an Image.* Edinburgh.

Said, E. 1978. *Orientalism.* London.

Tolan, J. 2002. *Saracens: Islam in the Medieval European Imagination.* New York/Chichester.

Pierluigi Donini: Commentary and Tradition. Aristotelianism, Platonism, and Post-Hellenistic Philosophy edited by Mauro Bonazzi

Berlin/New York: De Gruyter, 2011. Pp. 466. ISBN 978–3–11–021872–5. Cloth $182.00

Reviewed by
George Boys-Stones
Durham University
g.r.boys-stones@durham.ac.uk

Pierluigi Donini's contribution to the field of ancient philosophy over the last 40 years is distinctive and important for a number of reasons. His work on the way in which philosophy developed in the immediate aftermath of the Hellenistic era has been pioneering—and all the more noteworthy since his earliest publications come from a time (the 1960s) when interest in the Hellenistic schools themselves was uncommon enough. Then, there is the fact that he is equally at home with the Aristotelian as the Platonic tradition. Everyone knows in principle that the two should be studied together, but the evidence for each throws up more than enough obstacles to understanding for a single lifetime and few scholars can claim to have a firm handle on both. Finally, there is his conviction that both Aristotelians and Platonists are part of a lively and sophisticated intellectual tradition—making him an inspiring guide through a field populated by scholars who often give the impression that (as someone once expressed it to me) they rather despise their material. In the best Italian tradition, Donini unites philological sensitivity with philosophical incision; and the results have been published in a prolific stream of game-changing studies, especially on Alexander and Plutarch, and especially in epistemology and metaphysics.

Commentary and Tradition reprints 22 of Donini's articles (15 in Italian, 3 in French, 3 in English, 1 in German), adding two previously unpublished studies (both in Italian), some cross-referencing between the articles, an English abstract for each, and an *index locorum* for all. The aims of the volume are to facilitate access to Donini's work and, by so doing, to promote the study and understanding of post-Hellenistic philosophy [7]. The result is an impressively broad-

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 195–201

ranging, yet surprisingly coherent, account of the period—enough so that it is possible to imagine this book as a way in to the study of the later Aristotelian and Platonist traditions. This is helped by the fact that one of the principal focuses of Donini's work is the characteristic medium of later Aristotelian and Platonist philosophy, the commentary.

In his seminal essay 'Testi e commenti' (reprinted here), Donini argued that commentary was the means by which Aristotelianism and Platonism sought to build coherent systems out of fragmented philosophical traditions; and most of the papers in this volume give substance to that thought or trace its implications—through the Aristotelian tradition in part 1 and the Platonic tradition in part 2. I list the individual papers and their conclusions at the end of this review: it is enough to give a sense of the volume if I add here that Alexander is a central figure in part 1, where the papers are mostly concerned with exploring and exemplifying the different techniques that he used to make sense of Aristotle, with a focus on how he dealt with apparent contradictions within the corpus or with places where expansion and elucidation was needed. Plutarch dominates part 2, especially the way in which his exegesis of Plato reflects his commitment to the unity of the Platonic tradition. Donini is excellent at teasing out the many layers that there are to Plutarch's discussions, particularly those on matters of physics—an imbrication which is convincingly read by Donini as an attempt to associate an appropriate level of ('Academic') uncertainty and caution with the views that we have of the sensible world while remembering that the ultimate answers are to be found in metaphysics and theology. Donini is never quite convinced that Plutarch really succeeded in his unifying mission [cf. 'Testi e commenti' at pp. 249–250]; and in more recent work represented here, he argues that we can see him grappling in particular with a 'Pythagorean' view of Plato which cannot be squared with the 'Academic'.

It is not clear to me why the volume starts with off four relatively minor pieces (including one of the new ones) on a scattering of issues in Aristotle himself. They do, of course, help to give a fuller sense of the scope of Donini's interests and their inclusion is appropriate in so far as the book is also a tribute to him. But they do nothing to promote the sense of thematic unity which is otherwise so strong. What is more, just in so far as they attest to additional strings to

Donini's bow, they risk drawing attention, by contrast, to areas in which he has done less work: for example, on Plato and also on the Hellenistic schools.[1]

As it happens, I felt the relative absence of the Stoics in any case. As a matter of methodology, Donini tends to concentrate on the way in which the Platonist and Aristotelian movements construct themselves through the exegesis of their foundational texts, rather than thinking about how their self-definition was shaped by conscious opposition to the Hellenistic schools. There is nothing wrong with that, of course; but there are times when it seems to me that it leads Donini to underestimate the importance, and continuing vitality, of schools such as the Stoa in the post-Hellenistic era [cf. Frede 1999, esp. 778–782]. It is symptomatic of this that Donini is keen to argue some of the Stoicism out of Seneca, for example, in favor of Platonizing tendencies mediated, he suggests, by Antiochus ('Le fonti medioplatoniche di Seneca'). But 'Platonic' motifs in Seneca can be explained in ways that do not compromise his Stoicism.[2] Indeed, he can easily be read as further evidence for the extent to which the Stoics engaged closely with Plato—and latterly with Platon*ism*—throughout the history of their school. Again, in the case of Antiochus himself, Donini plays down the importance of Stoicism in his thought, suggesting that it has been exaggerated by polemical sources [289–290]. But it is Antiochus' own spokesman in Cicero who calls Stoicism a 'correction' of Plato's original system [*Acad.* 1.35]. The thought is explicitly attributed to Antiochus at *Acad.* 1.43.

But in the end, this is only to say that there is a lot that remains to be done in this field. In particular, there are still many layers of debate and interaction that need to be excavated from the two-dimensional histories of philosophy by which they were occluded, and which Donini has done so much—perhaps more than anyone—to combat. His pupil, Mauro Bonazzi, who edited this volume, has crafted a fitting monument to Donini's groundbreaking work and, what is more to the point, a useful conduit for it.

[1] One can check this kind of generalization now, by the way, since the volume includes a bibliography of Donini's published works [453–458].

[2] Brad Inwood's work here is especially important: cf. the essays in Inwood 2005.

For the use that it might be, I end with a list of the articles in the volume, translating non-English titles and giving brief summaries of their conclusions. An asterisk marks the two articles published here for the first time.[3]

PART 1: ARISTOTLE AND THE ARISTOTELIAN TRADITION

(1) 'Book *Lambda* of the *Metaphysics* and the Birth of First Philosophy'

Λ marks the beginning of questions which led to Aristotle's distinction of physics and metaphysics.

(2) 'Tragic *Mimesis* and the Apprenticeship of *Phronesis*'

Aristotle's *Poetics* deals with tragedy in the formation of adult φρόνιμοι, complementing the discussion of character-formation of the young in the *Politics*.

(3) *'Causes, the Voluntary, and Decision in Aristotle, *Rhetoric* 1.10–15'

References to προαίρεσις in the *Rhetoric* usually assume the usage of contemporary forensic practice, though occasionally reference Aristotle's own analysis.

(4) 'Aristotle, *De motu animalium* 701a7'

The passage is to be construed so as to be about the practical syllogism.

(5) 'Alexander of Aphrodisias and the Methods of Philosophical Exegesis'

Alexander's tactics for dealing with inconsistencies in Aristotle, and for elucidating his thought.

(6) 'The Object of the *Metaphysics* according to Alexander of Aphrodisias'

Alexander usually thinks that the study of being *qua* being coincides with the study of first substances, but in his commentary on Γ suggests that they are two *different* types of first philosophy.

(7) 'ϑεῖα δύναμις in Alexander of Aphrodisias'

Alexander, *Quaest.* 2.3 experiments with answers to Platonist criticisms of providence in Aristotle, but does not get very far.

[3] The original contents page can be found through de Gruyter at http://www.degruyter.com/view/books/9783110218732/9783110218732.5/9783110218732.5.xml.

(8) 'Alexander's *De fato*: Problems of Coherence'

A degree of 'freedom' is possible—for non-φρόνιμοι only.

(9) 'Natural Gifts, Habits, and Characters in Alexander's *De fato*'

A person's nature is their fate: exceptionally few people can, through education, transcend this.

(10) 'Alexander of Aphrodisias *De an.* 90.3 ff.: concerning the νοῦς θύραθεν' (co-authored with Paola Accattino)

An emendation to a line athetized by Bruns makes the point that 'immortal intellect' is that *which is thought*—the νοῦς θύραθεν.

(11) 'Xenarchus, Alexander and Simplicius on Simple Movements and Sizes in the *De caelo*'

A reference to Aristotle in between two replies by Alexander to Xenarchus on simple motions is parenthetical, not an additional argument.

(12) 'Justice in Middle Platonism, in Aspasius and in Apuleius'

Aspasius' 'theoretical justice' is a Platonist concept; Apuleius' division of justice at *De Platone* 7 is based on *Republic* 4.

PART 2: PLATONISM AND POST-HELLENISTIC PHILOSOPHY

(13) 'The History of the Concept of Eclecticism'

Six senses of 'eclecticism' distinguished and their historiographical uses explored.

(14) 'Texts, Commentaries, Manuals and Teaching: The Systematic Form and The Methods of Philosophy in the Post-Hellenistic Age'

Aristotelianism and, in its wake, Platonism resorted to commentary to make coherent systems of themselves.

(15) 'Middle Platonism and Middle Platonist Philosophers: A Clutch of Studies'

Reviews articles on Middle Platonism in *Aufstieg und Niedergang der römishen Welt*, and reflects on attempts to characterize 'Middle Platonism'.

(16) 'Seneca's Middle Platonist Sources: Antiochus, Knowledge and Ideas'

Seneca's source for 'Platonism' in *Letters* 58 and 65 is Antiochus: this, incidentally, is the one piece which was not originally a self-standing work but an appendix to Donini 1979, a major study presumably too lengthy to be reprinted here in its entirety.

(17) 'Plutarch, Ammonius, and the Academy'

Plutarch learned his cautious Platonism from Ammonius, who is *not* contrasted with but *is* the 'Academy' of *De E* 387f.

(18) 'Science and Metaphysics: Platonism, Aristotelianism, and Stoicism in Plutarch's *On the Face in the Moon*'

Ways in which the *De fac.* signals that its scientific discussion of the Moon needs to be subordinated to a metaphysical/theological understanding.

(19) 'Foundations of Physics and the Theory of Causes in Plutarch'

Plutarch's preferred causes are god and matter; physics is subordinate to such explanations and invokes other causes.

(20) 'Plato and Aristotle in the Pythagorean Tradition according to Plutarch'

Plutarch's ethics relies on the same 'Pythagorean' tradition that he invokes to support his metaphysical dualism—a tradition which is at odds with his commitment to the Academy.

(21) 'The Heritage of the Academy and the Foundations of Platonism in Plutarch'

A response to Opsomer 1998: Plutarch's Platonism has a 'Pythagorean' as well as an Academic strand.

(22) *'Plutarch's *De genio Socratis*: The Limits of Dogmatism and of "Scepticism"'

The *De gen. Soc.* tries, without quite succeeding, to reconcile Pythagorean and Academic conceptions of Plato.

(23) 'Knowledge of God and Divine Hierarchy in Albinus'

The *Didaskalikos* recognizes two gods: a higher ineffable god and a lower demiurge.

(24) 'Socrates and his Daimon in the Platonism of the First and Second Centuries AD'

The interest in Socrates' 'daimon' comes from a dogmatic, 'Pythagorean' strain in Platonism, which ultimately prevails over the 'Academic' view of Socrates.

BIBLIOGRAPHY

Donini, P. 1979. 'L'eclettismo impossibile. Seneca e il platonismo medio'. Pp. 149–300 in P. Donini and G. F. Gianotti. *Modelli Filosofici e Letterari. Lucrezio, Orazio, Seneca.* Bologna.

Frede, M. 1999. 'Epilogue'. Pp. 771–797 in K. Algra, J. Barnes, J. Mansfeld, and M. Schofield edd. *The Cambridge History of Hellenistic Philosophy.* Cambridge.

Inwood, B. 2005. *Reading Seneca: Stoic Philosophy at Rome.* Oxford.

Opsomer, J. 1988. *In Search of the Truth: Academic Tendencies in Middle Platonism.* Brussels.

Defending Hypatia: Ramus, Savile, and the Renaissance Discovery of Mathematical History by Robert Goulding

Archimedes 25. Dordrecht/Heidelberg: Springer, 2010. Pp. xx + 201. ISBN 978–90–481–3541–7. Cloth $99.95

Reviewed by
Albrecht Heeffer
University of Ghent
Albrecht.Heeffer@UGent.be

The title of this book is somewhat misleading. Do not expect much on Hypatia or even on the history of mathematics. The figure of Hypatia is merely used in a metaphorical sense, as a virgin body exemplifying both the unity and the beauty of Euclid's *Elements*. This book deals first and foremost with the historiography of mathematics: how and why histories of mathematics are written. While the book contains six chapters, we can distinguish three main parts. The first part gives an overview of histories of mathematics written before 1570. A second part, mainly chapter 5, concerns the forgotten history of the conflation of Euclid of Megara and Euclid the mathematician. The third and most substantial part deals with the role of the history of mathematics in the understanding and teaching of mathematics by Petrus Ramus (1515–1572) and Henry Savile (1549–1622).

The first part, on the lineages of learning, provides the reader with a brief but useful overview of the historiography of mathematics before 1570. Goulding covers Diodorus Siculus (first century BC), Josephus (first century AD), Proclus (fifth century), Regiomontanus (1564), Vergil (1499), Cardano (1535), and Melanchthon (1536). This overview is particularly useful and the topic deserves more elaboration than it receives over 18 pages. Most historians in antiquity attributed great importance to the Chaldeans and the Egyptians. Participation in a long genealogy of mathematical learning would become an idea that the Renaissance humanist could not resist. Josephus added the role of the Jews to the narrative and was the source for the belief that the entire Mediterranean civilization was indebted to the Jews for the transmission of learning. Proclus was crucial for Renaissance historiography as he provided a model for the history of mathematics

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 202–207

as a process of progress. He saw Euclid as a culmination of ancient mathematics. This would lead to two distinctive but related views on ancient Greek mathematics. On the one hand, there was the belief of continuous progress—held by Regiomontanus and Savile—in which the Greeks perfected the achievements of mythical ancients as the moderns did with Greek mathematics. On the other hand, some authors such as Ramus held a cyclic view of degeneration and recovery, where Euclid was blamed for the degeneration of Platonist mathematics.

Goulding makes a strong case for the claim that Renaissance historiography of mathematics was not so much concerned with describing what actually happened but rather with justifying the very discipline of mathematics. Scholarly mathematics in the late Middle Ages was considered of little practical value, obscure, and indecorous. In order to justify the study of mathematics, humanists had to establish a 'rhetorically powerful, morally edifying historical narrative'. The humanist practice of mathematics was therefore by nature historically oriented. Humanists were concerned with analyzing and criticizing the newly discovered ancient texts, harmonizing them with prevailing knowledge and practices, establishing the authorship of texts, and tracing biographical data.

The second part [ch. 5] tells the curious and forgotten story of the conflation of the philosopher Euclid of Megara (*ca* 435–*ca* 365 BC) with the mathematician Euclid of Alexandria. The first Euclid was a student of Socrates and a friend of Plato, while the second Euclid was born after Plato had died. Although the first traces of the confusion date back to Valerius Maximus, it was mostly the humanist Bartolomeo Zamberti who was responsible for the Renaissance conflation by compiling a biography of Euclid for his Latin translation of Euclid's works, published in 1505. Many Euclid editions after Zamberti then included Euclides Megarensis on the title page, such as those by Pacioli (Venice, 1509), Faber (Paris, 1512), Hervagius (Basel, 1537), Finé (Paris, 1544), Scheubel (Basel, 1550), Tartaglia (Venice, 1565), de Foix Candale (Paris, 1566), as well as Sacrobosco's edition of *The Sphere* of 1527. It was only with Frederico Commandino's edition of 1572 that the matter was put straight. Goulding demonstrates that the humanists required a biography to establish the authority of Euclid. In the construction of an account of Euclid's life, Zamberti misappropriated the then unpublished commentaries by Proclus.

Goulding also convincingly shows that Petrus Ramus' meticulous collation of prosopographical data in the *Prooemium mathematicum* was the source for Commandino's corrections. Ramus had a good reason for doing so, as he wanted to disentangle Euclid the mathematician from the golden age of Plato's Academy. After Commandino,

> the Megarian error disappeared, as such obvious errors tend to do, into a kind of embarrassed silence. Euclid of Megara, the Platonic mathematician, simply ceased to exist. [142]

The third and major part of the book deals with the narratives on the history of mathematics by Ramus and Savile. Ramus' account of the evolution of mathematics took shape in three stages between 1555 and 1567 in the mathematical prefaces, the *Scholae mathematicae*, and the *Prooemium mathematicum*. Goulding's case of Renaissance history of mathematics as a justification of the discipline is well illustrated by the narrative developed in Ramus' works. He envisaged an educational program of mathematics at the University of Paris much as mathematics was in its formative beginnings. Ramus praised the kind of mathematics that was raised by Plato through abstraction to a philosophical level and by Archimedes and Heron to a useful kind of mathematics. Although both his own ambitions for the chair of mathematics as well as his reform program ultimately failed to be realized at Paris, his writings had a lasting influence on the course of mathematics in Europe. Savile's history of mathematics, taught at Oxford, was strongly influenced by the *Prooemium*. His ideals for the teaching of mathematics were well established through the Savilian Professorship founded in 1622 and would strongly influence mathematics education at Oxford. Goulding convincingly demonstrates the influence of Ramus on Savile through extant manuscripts of his lectures preserved at the Bodleian Library.

While *Defending Hypatia* is a valuable study contributing to our understanding of Renaissance historiography of mathematics, it suffers from two hiatuses. Two important Renaissance themes have not been explored by Goulding: the role of Arab translations on the understanding of Euclid's *Elements* and the place of algebra within mathematics. Both are essential in the motivations and directions taken in the humanist reform program of mathematics, including Ramus'. Let us take up the Arab influence first.

Goulding touches on the point in relation to the Theonine edition of Euclid [150–178]. At some stage, Renaissance scholars came to the conclusion that the original *Elements* by Euclid contained only the text of the propositions and believed that the demonstrations were the work of Theon of Alexandria (late fourth century), the father of Hypatia. While there circulated Latin editions based on Boethius' translation of the sixth century, they hardly found any readers: according to Menso Folkerts [1989], only Fibonacci and Campanus actually used them. Campanus' book of the 13th century became the first printed Euclid edition (1482 in Venice). This edition was based on a 12th-century Arabic-Latin translation by Adelard of Bath. Campanus' edition shows an influence of Arab commentaries by al-Nayrizi's and the *Arithmetica* by Jordanus, especially in the definitions of books 7 and 8. Hence, it is no exaggeration to state that almost all knowledge of Euclidean geometry in Medieval Europe was based on translations from the Arabic scholarly tradition.

It is only by the end of the 15th century that any serious work was undertaken to study the *Elements* beyond the first two books and to reconstruct the original text from Greek manuscripts. Regiomontanus started the task aided by Bessarion's Greek manuscripts [Folkerts 2006]. Giorgio Valla published books 14 and 15 in 1498 in Venice. Then came Zamberti in 1505 with a complete new translation based on Greek manuscripts. Goulding shows how Zamberti was primarily responsible for the division of the *Elements* into a part by Euclid with propositions and another part with demonstrations attributed to Theon. It is only since the late 19th-century discovery of a non-Theonine manuscript by Peyrard [Vatican gr. 190] that scholars such as Heiberg and Heath became able to pinpoint the extent of Theon's contributions, which were much more modest than was believed by Zamberti.

Goulding explains the role Ramus and Savile played in restoring Euclid's *Elements* as a single body of geometrical knowledge, exemplified in Hypatia's virgin body. However, he bypasses an important motivation of humanist mathematicians to restore the original Euclid on the basis of Greek manuscripts.

A second essential aspect of Renaissance historiography of mathematics is the question of the origin of algebra. This issue, which is related to the humanist concern with the contamination of Greek works by Arabic authors, is completely overlooked in the book. Shortly before delivering his *Oratio introductoria in omnes scientias mathematicas*, part of a series of lectures at the University of Padua in 1464, Regiomontanus reported his find of the six books of the *Arithmetica* of Diophantus in a letter to Giovanni Bianchini. In this *Oratio*, he introduces the idea that Arabic algebra descended from Diophantus' *Arithmetica*. His formulation is subtle. He does not claim that the Arabs learned algebra from Diophantus, but it can be—and it was—understood as if Arabic algebra was derived from the *Arithmetica*. Regiomontanus was one of the few men who had seen the Greek text of Diophantus in 1464 and he was aware of its importance. By then he was also well-acquainted with Arabic algebra. He owned a copy of the Latin translation of the algebra by al-Khwārizmī, possibly from his own pen (MS. Plimpton 188). He must have been aware of the very different nature of the two traditions [see Folkerts 1980]. The term he uses, the 'art of *rei* and *census*' is the typical Latin nomenclature employed only in the Latin translations of Arabic works. Here however, he uses this terminology to refer to Diophantus and claims this is known today as 'algebra, after its Arabic name'. The question of the Greek roots of algebra became central to the historiography of mathematics in the following centuries. As Jens Høyrup [1996, 1998] has pointed out, it divided authors into two opposing camps: those who acknowledged the Arabic origins of algebra and those who chose to deny any credit to Arabic authors. The first category included Luca Pacioli, Girolamo Cardano, and Michael Stifel; the latter, Ramus, Rafaello Bombelli, and François Viète. As Viète wrote in his dedication of the *Isagoge* to Princess Mélusine, he

> considered it necessary, in order to introduce an entirely new form into it, to think out and publish a new vocabulary, having gotten rid of all its pseudo-technical terms (*pseudo-categorematis*) lest it should retain its filth and continue to stink in the old way. [Klein 1968, 318–319]

In the *Scholae mathematicae*, Ramus [1569, 37] strengthens the claim that the Arabs learned algebra from the Greeks and adds Theon as a confirmation since he mentioned Diophantus.

Defending Hypatia is well researched and pleasingly written work. It broadens our understanding of Renaissance historiography of mathematics. Despite the erroneous claims made in the histories written by Ramus and others, their narratives turned out to be fruitful. Renaissance historiography allowed mathematicians and philologists to look at ancient Greek works, in particular the *Elements*, as historical texts which can be studied as such, and which facilitated an understanding of mathematics as historically contingent.

BIBLIOGRAPHY

Folkerts, M. 1980. 'Die mathematischen Studien Regiomontanus in seiner Wiener Zeit'. Pp. 175–209 in G. Hamann ed. *Regiomontanus Studien.* Sitzungsberichte der Österreichische Akademie der Wissenschaften, Phil.-hist. Kl. 364. Vienna.

________ 1989. *Euclid in Medieval Europe.* Winnipeg, Canada.

________ 2006. 'Regiomontans Euklidhandschriften'. *Sudhoffs Archiv* 58:149–164. Revised and translated as 'Regiomontanus' Approach to Euclid'. Pp. 1–16 in article VII of M. Folkerts, *The Development of Mathematics.* Burlington, VT/Aldershot/UK.

Høyrup, J. 1996. 'The Formation of a Myth: Greek Mathematics—Our Mathematics'. Pp. 103–119 in C. Goldstein, J. Gray, J. Ritter edd. *L'Europe mathématique. Mathematical Europe.* Paris.

________ 1998. 'A New Art in Ancient Clothes: Itineraries Chosen between Scholasticism and Baroque in order to make Algebra appear Legitimate, and Their Impact on the Substance of the Discipline'. *Physis* 35.1:11–50.

Klein, J. 1968. *Greek Mathematical Thought and the Origin of Algebra.* Cambridge, MA. Translated by E. Brann from Die griechische Logistik und die Entstehung der Algebra. Berlin, 1934–1936.

Ramus, P. 1569. *Scholae mathematicae.* Basel.

Plato on Music, Soul and Body by Francesco Pelosi. Translated by Sophie Henderson

Cambridge/New York: Cambridge University Press, 2010. Pp. viii + 228. ISBN 978–0–521–76045–4. Cloth $95.00

Reviewed by
Anne Gabrièle Wersinger
CNRS (Centre Pépin, UPR76)
wersinger@vjf.cnrs.fr

The relationship between body and mind is one of the traditional problems met by those who specialize in Plato's work. But recently scholars such as Sabina Lovibond have shown a fresh interest for a Platonic philosophy of mind freed from the dualistic view which used to predominate. It is from this perspective, which is profitable for modern cognitive research, that Francesco Pelosi's book (a revised version of a Ph.D. thesis in philosophy) should be read.

The author's method is particularly interesting because it tries to interpret Plato's philosophy of mind and his reflection on the relationship between soul and body in the light of his thought on music. One must be aware of the numerous musical metaphors that exist in Plato's dialogues such as that of the 'prelude and the song' that Socrates uses in *Rep.* 7. This metaphorical choice, made in order to define the difference between the scientific disciplines (μαθήματα) and dialectic, is striking. Also striking is Socrates' warning in the *Phaedo*, 'practise and compose music', which one should understand first of all as an indication of the double meaning of 'music', which has not only a common sense but also a philosophical and deeper sense, so that dealing with music means dealing with philosophy as well. If scholars such as Evanghelos Moutsopoulos have carefully studied Plato's theory of music, Pelosi's originality consists in considering music as providing, so to speak, 'laboratory conditions' for the study of the body and mind relationship in Plato's work. Pelosi's knowledge in the field of ancient music, which owes much to Andrew Barker's work, permits him to reconsider the psychological and epistemological relation between body and mind not only through Plato's later dialogues (*Timaeus* and *Laws*) but in the *Phaedo* as well.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 208–219

The book contains an introduction, four chapters, a conclusion, and some bibliographical references followed by an *index locorum*. Of course, it is not possible here to do justice to all the details of the author's argument, so I shall sum up its key results while studying the book critically.

Chapter 1 starts with the *Laws*, where the ancient practice of singing and rocking babies to make them sleep is compared to the curative rites of the Corybantes which are based on the movement of dance and irrational music in which 'madness has to cure madness' [16]. We then penetrate Plato's educative program in the *Republic*. Exploring the elements of the embodied soul, the author deals with the effect of musical education on sensibility [14–28] and tackles the theory of ἦθος and musical μίμησις [29–67].

One important point which Pelosi brings out is that Plato finds a place between the proponents of the ἦθος-theory («ἦθος» meaning 'character') who believe that it is possible to translate mental states and ethical contents into sounds and the formalists (such as those of the Hibeh papyrus) who reject the idea that music expresses feelings and emotions, and argue that only words carry an educative and psychagogic function. Pelosi rightly reminds us that Plato deals with a concept of μουσική where harmonies and rhythms are subordinated to words on the ground that music without words can corrupt the educative value of μουσική in that it is solely affective [61]—as, for instance, the New Music was said to, by focusing on the versatility of instruments in the interest of inducing affective states rather than ethical dispositions.

As ἁρμονίαι play an important role in the theory of ἦθος, Pelosi follows the usual and well known scholarly reconstructions of the ancient ἁρμονίαι based on witnesses such as Aristides Quintilianus and the Aristotelian *Problemata* [36]. We should remember that, as a ἁρμονία plays a role in the tuning of the chord, each tuning corresponding to a degree of tension, ἁρμονίαι are connected to the contrast between tension and relaxation. The technical meaning of 'tense' and 'slack' initially refer to the nerves of the body and to the strings of instruments, as in *Phaedo* 86b7. But «ἁρμονίαι» has a second meaning derived from its capacity to imitate the movements of the soul, that is to say, its emotions and passions. The author could have also mentioned here the fact that emotions have their physiological correspondence in the way that the heart is stressed or relaxed by movements [*Tim.* 69c1, 70d5].

But if Proclus, Pherecrates, Agathon, and Melanippides would seem to tell us about the 'slack' character of the New Music which is called 'chromaticism' and associated with the languid ἁρμονία rejected by Plato, against this stands Aristoxenus' testimony in which languid melody is associated with tense music. And in *Rep.* 411e4–412a2, where the two parts of the soul (the spirited and the philosophical) are compared to two strings on which music acts by slackening the first and making tense the second with the aim of tuning them, 'tense' and 'slack' have neither negative nor positive ethical meaning as they are both necessary to produce the equilibrium of the soul [40–41].

Given such an ambiguity, Pelosi argues, it is not surprising if other passages of the *Republic* [e.g., 411e2] show that a harmful effect of music on the soul does not derive from 'tense' or 'slack' ἁρμονίαι but from a 'repeated exposure' to music. But such a claim is not convincing: *Rep.* 410c8–412a2 concerns the effects of the practice of music or gymnastics—gymnastics is also necessary [410b5]—on the soul *and thought* (διάνοια) [410c8] to the exclusion of all else. Attention to music alone affects both the 'philosophical' [410d5–6] and the 'spirited' [411a9–b5]. The effects are described in the dynamics of slackness (ἄνεσις) and tension (τάσις). In 411a5–b5, Socrates describes the way the soft (μαλακαί), sweet (γλυκεῖαι) and mournful (θρηνωδεῖς) ἁρμονίαι influence the soul when music only is practiced. It does not mean an 'overdose' of music as Pelosi asserts but a process of excessive slackening, an operation precisely fulfilled by *bad* ἁρμονία. It does not mean that 'good' ἁρμονίαι, such as the Dorian and the Phrygian are forgotten in the process. Though Socrates is not explicit, nothing forbids hypothesizing that the first degree of slackening coincides with the Dorian and Phrygian. After all, Socrates says that the 'philosophical' is gentle before it becomes too slack [410d5–6] and that the 'spirited', before it melts and dissolves until it is completely liquified [411b2–5], is at first softened, losing its roughness (σκλήροτες) and becoming χρήσιμον [411b1] a word that means morally useful, obliging, and benevolent.[1] The continuous slackening supposes the

[1] See *Rep.* 413d5–6 where Socrates says that music renders man useful for himself and for the state.

faculty of modulating between all the ἁρμονίαι, an operation introducing πολυαρμονία or παναρμονία and which is connected to instruments that are πολυχορδότατοι ('with several *chords*', that is, 'with several forms or τροποί') such as the αὐλός.

Music without words, exciting pleasure, acts evasively on the soul.[2] The only music admitted by Plato is the one in which the different μιμήσεις linked to different characters are clearly specified and, of course, words play a capital role in this.

Pelosi uses Koller's concept of μίμησις, meaning 'representation' ('Darstellung') not 'imitation' and originally limited to music and dance [58]. For instance, in the *Laws*, music expresses ethical and emotive content through movements of the body and voice. Those movements 'represent' the attitudes assumed in particular situations, where such dispositions are defined in an ethical sense. Small variations of movements will be expressed by moderate individuals whereas cowardly individuals will have violent and greater movements [59]. In the *Republic*, the Dorian and Phrygian ἁρμονίαι 'represent' the sounds (φθόγγοι) and the modulations of the speaking voice (προσῳδίαι) of the courageous and the temperate by means of the φθόγγοι and προσῳδίαι themselves. Pelosi rightly asserts that μίμησις here is not the expression of an 'irritable and variable character' but of 'an intelligent and tranquil character'. There are two kind of μιμήσεις: one is a rich and varied expression of irrationality; the other is a simple, verbal one that represents positive and moral exempla, as do the Dorian and Phrygian [63].[3]

Chapter 2 begins by dealing with Plato's musical treatment of reason [68–89]. The immortal principle of the human soul is formed with the elements of the cosmic soul so that it derives from a musical nature [73]. The structure of the rational soul is that of two circular movements, the Same and the Different, the latter being divided into seven unequal circles that move according to precise ratios. The structural and ontological analogy between harmonious movement and the soul grounds the action of ἁρμονία on the soul through the

---

2 See page 222 and the second paragraph in ch. 1.

3 See *Rep.* 399a5–c3, where the Dorian and Phrygian are defined as imitating moral attitudes through exempla: one of a wounded warrior who bravely fights against adversity, the other of a man persuading, praying or being persuaded, with moderation.

sense of hearing. Rhythm plays a role as well because of the proportion that governs the motions of the seven circles of the Different.

The damage occasioned to the human psyche by contact with a body is described as a perturbation of the numerical and musical structure [*Tim.* 42e–44b]. Pelosi rightly outlines the *disharmony* that proceeds from incarnation [77], where 'disharmony' indicates the complete over-tuning of the embodied soul immersed in a sensory field. That is why the search for a harmony between soul and body lasts for the whole of existence and one must not neglect the important fact that any such harmony belongs to an incarnate soul [80].

Pelosi makes interesting remarks about how the ontological and moral hierarchy between the elements of the soul is projected within the body space [86] and notices that the mind-body interaction includes the conception of a psychic purpose for the corporeal organs and the production of physical effects by psychic activity. The marrow is an illustration of the mind-body connection comparable to the Cartesian pineal-gland [87]. Its proportioned structure (συμμετρία) [73c1] fits the proportioned rational soul [88]. Other illustrations show the reason why sensory stimuli can direct the rational soul towards correct movement [88–89].

The second part of chapter 2 [89–113] concentrates on the intellectual activity regarding the sensible and the rational, recalling the role of the two circles in the soul, the Different and the Same, so that one may notice first of all that it is through kinetic activity that the intellectual process is achieved. Music, in favoring the re-ordering of the circles of the soul, contributes by creating the conditions for the 'silent interior dialogue of the soul' which expresses a correct cognitive process [93]. Pelosi recalls the *Theaetetus*, the *Philebus*, and the *Sophist*, where the conditions of the interior dialogue and especially the role of difference and sameness are examined [91–92]. He takes notice of the fact that in the *Timaeus* what results from the damage caused by the contact of the soul with the body is the loss of the ability to correctly predict sameness and difference. But the embodied soul is nevertheless capable of taking on the data of the sensory world to a certain extent so that we may reconstruct the musical experience as a mute dialogue of the soul in which the circle of the Different carries the cognitive activity [94].

Pelosi addresses the problem of understanding how the embodied soul characterized as unreasonable [*Tim.* 44a8 ἄνους] could undergo a rational and *noetic experience*. The analysis of the perception of sound in 67b2–5 shows that the rational soul plays a role in the hearing of music. While, in most people, sound activates the circle of the Different, which thus provides an elementary decoding of the acoustic stimuli, in those who can recognize the logical component of the music, the circle of the Same permits the decoding of realities that are rational. There is a rational depth to music, the perception of concord [80b5–8] which brings pleasure in the unintelligent but delight (εὐφροσύνη) in the wise (ἔμφρονες) due to the representation of the divine harmony in mortal movements [97]. While gathering the super-sensory content of music, the soul seizes the affinity between that content and the original structure of the rational soul. Εὐφροσύνη is the emotion of musical experience indicated by «μετὰ νοῦ» [47d3]. There is a 'joy of knowledge', although the cause of the emotion resides in acoustic stimuli. The sensation of pleasure restores the natural condition and, in *Phil.* 31d8–10, this restoration is called a 're-harmonisation'. It proves that music remains a concrete experience for the wise also. That is why, in the *Timaeus*, it is not the study of harmonics to which the wise are invited but the concrete experience of listening to music [111]. The *Timaeus* should not be reduced to *Rep.* 7 for those reasons.

Chapter 3 concentrates on the musical education of rationality, starting with *Rep.* 7 [114–128]. This chapter is less original than other chapters of the book, the author here following what scholars have said on the subject. 'Higher education' is not a matter of knowing something new. Just as basic education does not aim at teaching something, it aims at making someone become something; what it fosters is another interior mutation [121]. As in Socrates' treatment of arithmetic and geometry, it is the method that is stressed in the last two μαθήματα, astronomy and harmonic science [123]. The astral figures have the same value as the geometric figures. They must act as paradigms of true astronomical objects. Working with the 'problems' of astronomy does not imply dismissing the stars but going from the trajectories of the visible stars to diagrams. Diagrams have to be considered for themselves without further recourse to the phenomena in order to assess their correctness [127]. In *Tim.* 40c3–d3, it seems that the use of an armillary sphere for studying celestial movements (though *Timaeus* says that it would be a futile undertaking) could illustrate the task defined in the *Republic*.

Pelosi's treatment of the Divided Line is rather evasive when he asserts that the bodies on which mathematicians work are images used to make out the intelligible realities that can only be assumed with διάνοια. It should be stressed here that Socrates chooses to present its object *via* a well known method, the geometrical proportion which belongs to λογιστική, a science specializing in the search for models of proportion in order to solve mathematical problems, of which Plato's dialogues offer us some illustrations such as the duplication of the square in the *Meno*. Key in architecture, sculpture, rhetoric and music, its function in the arts and the sciences [522c1–9, 522b9] makes it an exemplary method. But this method is only preparatory to dialectic and, though it helps to have an overview (σύνοψις) of the relationship between the sciences, this method must not be identified with dialectic [537c2–7]. In fact, as the the Divided Line occurs in the middle of a sequence where three attempts to reach the Good follow one another, beginning with the image of the Sun [506e] and ending with dialectic [532a–535a], its status is intermediate.

Pelosi could have noticed that in representing the work of the mathematicians through a device which itself is paradigmatically mathematical, Socrates recursively shows why mathematics remain inferior to dialectic, though aiming at the same ontological objects. But for the same reason we must also be careful not to take the Divided Line for a procedure capable of shedding full light on the matter.

The second part of chapter 3 [128–151] concentrates on harmonics. But the treatment seems to me unconvincing. For instance, Pelosi does not place in its right context Plato's allusion to the Pythagorean claim that astronomy and harmonics are kindred sciences, the first using the eyes while the second uses the ears [128–129]. And it is not true that the Archytas' remark in which the relationship between astronomy, geometry, arithmetic and music is asserted [Diels and Kranz 1956, 47A1] is comparable to *Rep.* 7, in which arithmetic and geometry are thought kindred because they concern fixed objects, while music and astronomy focus on mobile objects [129]. One has to reflect on the many criteria offered by Socrates in the passage: he tries to classify the preparatory sciences according to criteria of dimension, movement and rest, and perception of movement, in which the difference between seeing and hearing does not mean that harmonics and astronomy are at the same level at all. Moreover, the privileging of harmonics is not due to Pythagorean inclinations: one must be more attentive to the differences between Archytas' remark and what really happens in *Rep.* 7. Consequently, Pelosi misses also the explanation for another observation that he makes [148 and n57]: if the study of sounds becomes a study of numbers, the relationship that harmonic science has with the sensory is cancelled out and, consequently, the difference between the first three disciplines fades, leaving arithmetic as the only really effective science.[4]

Things are better when Pelosi stresses the fact that Plato accuses Pythagoreans of having a bad approach to harmonics. The author rightly recalls the comparison [531a–b] between the Pythagoreans and the those called ἁρμονικοί who are engaged with the καταπύκνωσις mentioned by Aristoxenus, that is, those theorists who are dedicated to study of the enharmonic genus in the search of micro-intervals and use diagrams to measure intervals. The comparison, born from Glaucon's misunderstanding, serves to condemn the empirical approach to music. The use of diagrams and sensible tools in the study of harmony making 'sight of sound' [134] is probably the background for the criticism of empiricism.

By contrast, the Pythagorean method that consists in translating sound into numbers appears as an alternative way of doing harmonics. The Pythagoreans' procedure is defined by a reciprocal measuring of sounds and perceptible concords [139]. Since the Pythagoreans connect numbers to sensible elements, they are still empiricists even if they are mathematicians as well [140]. The Pythagoreans thus failed to create a science of harmony whose contents are 'problems' and 'consonant numbers'.[5]

Pelosi rightly observes that this accusation of empiricism does not fit well with Ptolemy's view according to which the Pythagoreans are criticized for using an aprioristic approach and for rejecting empiricism [144]. In this view, the Pythagoreans show many points of contact with Plato's conception of harmonics. The figure of Archytas is

[4] On those problems, see Wersinger 2008.

[5] By the way, they do not understand that the audible consonants must be used as 'paradigms' for the study of the 'true' concords [143].

examined according to Ptolemy's testimony about Archytas' division of the tetrachord into three genera, which reveals a mathematical rigor in the study of musical practice [145].

Chapter 4 endeavors to examine the features of the presence of music in the body and the soul. The first part of the chapter considers Plato's treatment of acoustical phenomena and their perception [152–180]. *Tim.* 67a7–c3 offers a detailed definition of mechanisms tied to the acoustic sphere. The background of ancient acoustical theories is examined carefully [156]. In Plato's conception, sound is neither struck air, nor a movement of air, but a means of transmission of the impact emitted from a sonorous body.

The author pursues his examination of Plato's physiology of hearing. The first question is to understand the role of the brain and the blood. But there are textual difficulties to be solved first—the genitives «ἐγκεφάλου» and «αἵματος» can depend on «διά» or be objects of «πληγήν». Two interpretations, both referred to ancient accounts, are confronted. Then, Pelosi imagines a third possibility: the brain and the blood act as agents from which the impact derives and are not the end of the transmission of sound [158–159]. But as their role cannot be properly understood without examining the auditory process, the analysis of the textual difficulties goes on (the function of the two other genitives, «ὤτων» and «ψυχῆς», depending on two prepositions, «διά» and «μέχρι»). Pelosi feels reluctant to agree that sound should pass through the ears as through a funnel [160]. One has to understand that the ψυχή elaborates the sensations and is the end to which the affections tend. The elements comprised in the space delimited by «δι' ὤτων» and «μέχρι ψυχῆς», that is to say, brain and blood, are part of a psychophysical dimension, so that the passage of the *Timaeus* refers to the late Plato's 'psychologisation' of the perceptive act.

Pelosi follows Barker's argument that hearing (ἀκοή) is not sound [163] but rather the movement of the airy impact that is transmitted by the silent movements of the rational soul [*Tim.* 37b5–7] as this impact reaches the medial soul in the heart, the origin of the blood whose circulation [70b2 = 81b1–2] goes through all the parts of the body down to the liver [67b6], the place where the movements are reflected as appearances [71b4–6]. Thus, pitch [80a3–4] is due to hearing [165]. He then introduces some considerations concerning the correlation of pitch to the speed of propagation, a common error made in ancient acoustics except in *Sectio canonis*, which holds that it is the frequency of the vibrations that determines pitch [166–167].

Next he discusses other qualities that Plato recognizes in sound, with a special mention of ὁμοιότης that plays a central role in *Timaeus* 80a–b [168–170], a passage which is translated and studied with great care. It deals with the mechanisms of movement in the absence of void and with the propagation and perception of sounds of varied pitch, especially concords. The problem is to explain how the initial simultaneity of two consonant sounds is restored during the perceptual process. Pelosi again follows Barker, who has argued that Plato does not contrast συμφωνία (concordance) to διαφωνία (a musical but not concordant phenomenon) but to ἀναρμοστία (a non-musical pattern of attunement) [173]. It implies that Plato does not think that concords are perceived as a blend of two different notes, which appears as the softening of a discord. Sounds would undergo a slowing down during their journey in the body. The fast, high-pitched sounds would catch up to and be impeded by the slower sounds of any lower pitch. The catching up occurs at the moment in which the faster sounds that are slowed down proceed with a movement that is ὁμοῖος to that of the slower sounds. To avoid the embarrassing conclusion of a *glissando* in each perceived sound, one has to suppose that it is the speed with which the sounds reach the hearing that determines the perceived pitch. A concord is perceived as a fusion between high- and low-pitched sounds, and this occurs when the impulse of the slow introduces itself in a fluid manner into the kinetic process of the high-pitched that is thus transformed into a single consonant movement [179]. In this sense, Plato connects ὁμοιότης to concords defined by precise ratios, multiple or epimoric, and the acoustic characteristic of uniformity seems to be the translation in perceptive terms of mathematical excellence of consonances [180].

The second part of the chapter [181–201] explores the question of the soul as ἁρμονία, starting with the *Phaedo* [181]. Pelosi recalls Simmias' objection (the analogy of the type 'soul is to the body what harmony is to the lyre') to the last Socratic argument in which the soul is assimilated to the divine. This analogy is replaced by a materialistic one in which harmony of the soul is an attunement between corporeal elements, according to Alcmaeon and some Hippocratic treatises [182]. Socrates opposes two arguments: harmony follows the elements that compose it, while the soul dominates the corporeal elements; and if the soul is harmony, then virtue and vice cannot be explained [183]. Pelosi recalls how Plato reconsiders elsewhere the possibility of seeing harmony in the soul [185–187].

Justice too is expressed in a musical language where the three limits of ἁρμονία, the *nete*, the *hypate* and the *mese* are evoked to indicate the three parts of the soul. But the well known problem of what the 'intermediary notes' signify arises when Socrates mentions them in this passage. To answer it, Pelosi turns to what Socrates says in 612a3–5, where the soul is described by the word «πολυειδής» to indicate its complex structure with many aspects, thus perhaps recalling the *Timaeus* [188–189].

The last chapter ends by considering the structure of the world-soul in the *Timaeus* [190–195] The harmonic division is the means by which the Demiurge builds the structure of the soul, giving to it the form of a musical scale [192–193]. After many other scholars, the author outlines the 'extravagancy' of such an anomalous extensive scale in musical theory and practice, but fitted to an elaborate cosmological plan where 'musical mathematics' are the principal instrument of elaboration. The connection with astronomy is resumed also [194].

The rational soul contains a complex articulation and it looks like an image of fragility, carrying the potential of mislaying its tuning, a potential that embodiment would actualize. To that judgment, the author objects that it is not only the lack of unity that makes the immortal *human* soul susceptible to disorder but the inferior quality of its elements that makes the contact with the corporeal dimension insidious and harmful.

In his conclusion, where he recapitulates the aims and the results of his essay, Pelosi ends with the idea that if Plato seems to be aware that complex interactions between psychic and corporeal movements exist, he gives no definitive explanation in the dialogues of how these interactions are possible. We must content ourselves with an 'eikos mythos'. Figurative and metaphorical elements are the only way left to describe an intermediate reality between the sensible and the intelligible. But this seems an unhappy conclusion, if it implies that the metaphorical elements should be squeezed out as the negligible residue of an impossible rational account of reality. After all, figurative elements are part of Plato's writing and style that belong to the phenomenal body-mind dimension (as much as music does), so they should be taken as a material on which the scholar reflects seriously.

Overall, Pelosi's book proves that music is a fruitful and innovative tool for researchers on Platonic questions. I hope that it will invite further exploration in a field remaining unfamiliar to many students of ancient philosophy. The introduction is helpful in establishing the author's main challenges and ambitions. Some undeniable difficulties and obscurities remain unsolved in the book (such as the theory of ἦθος and the preparatory sciences in *Rep.* 7) and the bibliography misses some more recent works that bear on the author's research. Despite those omissions, students and scholars will find profit in studying this essay for its relevant and often precise analysis of the relationship between reason and sensibility seen in the light of acoustical theory and music.

BIBLIOGRAPHY

Diels, H. and Kranz, W. 1956. edd. *Die Fragmente der Vorsokratiker.* 6th edn. 3 vols. Berlin.

Wersinger, A. G. 2008. 'Pourquoi, dans la *République* de Platon, l'harmonique est-elle la science propédeutique la plus haute? Perspective nouvelle sur une question négligée'. Pp. 159–180 in J.-L. Périllié ed. *Platon et les Pythagoriciens. Hiérarchie des savoirs et des pratiques. Musique, science, politique.* Bruxelles.

Critical Notes on a Study of Galen's *On Critical Days* in Arabic or A Study in Need of Critical Repairs*

Y. Tzvi Langermann
Bar Ilan University
tlangermann@hotmail.com

Illnesses, and especially fevers—malarial fevers, to be even more precise—were observed to have critical turning points in their development; it was as if the patient was then being judged and the verdict could be recovery, death, or a prolonged illness with additional crises along the way. Moreover, these critical days, much like the paroxysms of the fever, were observed to occur at regular intervals. Hippocrates had already written about this phenomenon and Galen applied himself with his usual gusto, recording and analyzing the data, and sharply rebuffing sceptics and sophists. His investigations are recorded in *On Critical Days.* Toward the end of that book, Galen turns to the most difficult aspect of the topic, namely, the cause for the regularity of crisis. He addresses two explanations that were already in circulation, the arithmological account of the Pythagoreans and the astrological explanation. Neither of these was entirely to his liking but he had no way of entirely refuting any connection to the lunar phases. Galen's treatise underwent a significant revision in the so-called Alexandrian summaries, which circulated (in several versions) in Hebrew and Arabic. The full text as well was translated into Arabic by the prolific Ḥunayn ibn Isḥāq.

The subject of this essay is Glenn M. Cooper's edition, translation, and study of that complete version: *Galen,* De diebus decretoriis*, from Greek into Arabic: A Critical Edition, with Translation and*

* Research on this essay was supported by a grant from the German-Israeli Foundation for Scientific Research and Development, Grant I–1053–110/4/2009.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 220–240

Commentary, of Ḥunayn ibn Isḥāq, Kitāb Ayyām al-Buḥrān.[1] Cooper promises as well an edition of the Greek text to be accompanied by additional investigations.

Cooper's book is an important contribution to the history of science. I find it necessary to open my review with this statement because, in the following pages, I have many criticisms to make of Cooper's work, most of them serious. The length of this essay notwithstanding, I have not attempted to list all of the miscues or to call attention to all of the inaccuracies. Obviously I cannot propose better translations of all of the passages whose meaning, in my judgement, has been, let us say, blurred in Cooper's rendering. The examples presented in what follows ought to suffice. Nonetheless, I can say with all sincerity that this book, when used with caution, is a significant contribution and that I intend to refer to it over and over again in some work in progress.

The book opens with a detailed introduction followed by the annotated texts, English and Arabic in facing pages, both of which are keyed to page and line numbers in Kühn's text [1825, 769–943]. The first appendix contains the apparatus. There is considerable overlap between the material displayed there and the notes to the translation and Arabic text. I do not, in fact, fathom the method by which Cooper divided his textual comments, placing some as footnotes to his translation and relegating others to the appendix. But this is a minor criticism. In the second appendix, Cooper offers a 'working translation' of a text on critical days by al-Kindī, which is required to support his introduction. This text survives uniquely in a transcription of the Arabic into Hebrew letters and was published with a German translation by Felix Klein-Franke [1975]. Ursula Weisser later offered some constructive criticism of Klein-Franke's work, which she published in Sudhoff's Archiv [1982]. Cooper has taken Weisser's notes into consideration in preparing his English version. The third appendix has the Arabic text and English translation of a short treatise on the same topic by another early Arabic-writing scientist, Qusṭā ibn Lūqā, *Questions about the Critical Days in Acute Illnesses.*

---

[1] *Galen,* De diebus decretoriis, *from Greek into Arabic: A Critical Edition, with Translation and Commentary, of Ḥunayn ibn Isḥāq,* Kitāb Ayyām al-Buḥrān. Medicine in the Medieval Mediterranean. Farnham, UK/Burlington, VT: Ashgate, 2011. Pp. xx + 615. ISBN 978–0–7546–5634–0. Cloth $134.95.

Cooper publishes it here for the first time on the basis of the lone manuscript, Tehran 6188 (that is the number given to it in the library of the Iranian Parliament, where it is housed). Following the appendices one finds the bibliography, which is extensive [553–585]. At the end, there is a general index, an index of manuscripts, and an index of ancient sources. It is unfortunate that a book so well furbished with notes, bibliography, and so on, is missing one of the more important tools of this sort of research, i.e., an index of terms or a Greek-Arabic lexicon indicating as well the first occurrence.

In the introduction, Cooper labors to place the translation in its context, in line with the history of transmission presented in the widely cited book of Dimitri Gutas [1998] as well as with the book of his thesis advisor George Saliba [2007]. Moreover, he wisely incorporates into his discussion related texts such as the monographs of Qusṭā and al-Kindi, which, as noted, are conveniently included in appendices.

In my opinion, the level of detail that Cooper aims for is beyond the source material; but this too is a matter that can be left for future research to sort out. My focus in this essay will be on Cooper's handling of the text he presents from the perspective of philology (Cooper's skill in Arabic and, to a lesser extent, his handling of the Greek) and history (his understanding of a variety of topics such as astrology, music, and Pythagoreanism with regard both to the issues and to the published literature). Unfortunately Cooper's book will be found to be wanting in all of the above.

Cooper's translation is for the most part written in very good and clear English, close to the text yet not weighed down by literalism. Kühn exclusively is used for comparison between the Arabic and Greek. In the section of the introduction, 'The Textual Tradition', Cooper talks of [χ], Ḥunayn's 'autograph translation' which 'may be assumed to be an accurate (Arabic) representation of its Greek exemplar [Ψ]' [86]. Neither of these 'manuscripts' is extant: they are Cooper's hypothetical constructs and are, therefore, enclosed within square brackets as Cooper explains in note 381. In the critical apparatus [appendix 1, 505–528], Cooper mentions a single extant Greek manuscript, Venice, Marcian. app. gr. V, 8, which he describes as 'occasionally cited'. By my count, he cites this manuscript on only two occasions. With these exceptions, the only Greek text to which

Cooper compares the Arabic is that by Kühn [1825], whose editions are universally considered not to be up to standard.

Cooper finds some important pieces of text in the Arabic that are missing in Kühn's edition, citing them as 'om k': on page 59, he avers that the Arabo-Latin tradition has preserved better readings than 'the supposedly purer Greek tradition'. Who has supposed that the Greek tradition is purer? On page 252, in connection with the 20-day cycle [856.8–15], Cooper finds that the Arabic adds

> factual data from the Hippocratic writings not present in the manuscript(s) of the *Critical Days* in front of him.

However, Cooper offers no information about the Greek text other than Kühn's edition and Ḥunayn was not reading this edition. So how do we know that this information was not in the Greek manuscript(s) that Ḥunayn saw? Similar comments are found throughout.[2] More often than not, Cooper marks these divergences as omissions from the Greek supplied by the Arabic. Occasionally, however [e.g., 152n231], he declares them to be a clarification added by Ḥunayn. In one case [192n411], he asserts that a passage is both omitted from the Greek and a 'very significant addition' on the part of Ḥunayn. Sometimes he goes even further in the presentation of speculation as fact, e.g., that an omission in the Greek is not only an addition by Ḥunayn but also that Ḥunayn put in these comments for the benefit of his patron, Muhammad ibn Musa [437].

Rarely does Cooper confess to any doubt as to the source of the variant. One such case is 202n462, where, after marking an omission, he writes, 'Again, if this is not a genuine omission....'. In this particular case, it seems a safe bet that the cross-reference to the *Crises* is due to Galen but one would like to know for sure. At the end of his commentary to 812.8–17 [426], Cooper is more forthcoming:

> If this passage is an addition of Ḥunayn's and not ultimately from Galen, then it shows the translator's thorough understanding of ancient medical theory. Ḥunayn occasionally filled in lacunae based on his knowledge of medicine, but this passage is unusually long, and so is likely to have belonged to the original Greek.

[2] See, for example, 120nn104 and 107 or 122n108 and 112. Additional examples are discussed in detail below.

This is rather late in the book to begin a discussion of such an important issue: the additional material found in the Arabic ought to have been discussed in a separate section of the introduction. Philological tools—above all, the evidence of the Greek manuscripts—ought to be employed wherever possible in order to determine whether we are faced with an accretion or an omission. I do not see how length alone can be the determining factor, if it is a factor at all in our decision on this question. Only once does Cooper call attention to a lacuna in the Arabic text [431, to 819.12–13].

In sum, it is not clear whether the differences between the texts represent omissions from Kühn's text that are possibly found in manuscripts and hopefully to be corrected in a better edition of the Greek or rather a gloss of Ḥunayn that has insinuated itself into the text, or something else. Towards the end of this essay, when I turn to the vocabulary of the Pythagoreans, I will call attention to some more specific phrases where we would dearly like to know if Ḥunayn's translation has any basis in the Greek manuscript tradition.

Cooper's understanding of the transmission of some passages [Kühn 1825, 818.1–9] is not totally clear. The Arabic text appears to repeat itself. So if one of the two very similar passages is missing from Kühn's edition, must we interpret this as an omission from the original Greek? Is it not more likely that somehow the Arabic—not necessarily the translator, more probably a copyist—is responsible for dittography?

> But this is really impossible for him who cares about learning what Hippocrates said about this so that he grasps it before he attends patients, and diligently investigates their conditions. But, if he attends patients without learning *what Hippocrates also said about this discipline* then he is of no benefit at all, and his labor is in vain. *And if he cares about learning what the marvelous Hippocrates also said, so that he grasps it, and he is <not> lazy about tending to patients, and serving them, this will occur by itself.*

The italic text marks passages that are 'omitted' in Kühn's edition [see nn351, 354]. If we strike these passages from the translation of the Arabic, we are left with this:

> But this is really impossible for him who cares about learning what Hippocrates said about this so that he grasps it before he

> attends patients, and diligently investigates their conditions. But, if he attends patients without learning, then he is of no benefit at all, and his labor is in vain.

I submit that the ‘omitted’ passages (especially after correcting Cooper’s translation from the Arabic, which I shall do presently) add nothing and, hence, ought to be dismissed as intrusions. The point is simple: a serious physician must absorb Hippocrates’ teachings; then and only then will he be able to make a useful prognosis.[3]

Cooper’s treatment of the connection between the theory of critical days and ‘astrological medicine’, and his understanding of astrology overall, leave much to be desired. True, his conclusion, which is that

> Galen’s use of astrology is actually superficial, of mostly rhetorical value, and does not cohere with the rest of his medical science, except in an almost meaningless general way [69]

is not far off the mark. By ‘rhetorical’ Cooper is pointing to Galen’s effort to make his book, especially ‘his new theories and methods’, appealing to the educated Roman public by packaging it in keeping with their cultural expectations, which would have given an important place to astrology. To my mind, as a committed scientist, Galen felt it necessary to weigh seriously the astrological and arithmological explanations for critical days, even if he felt uneasy about them. The regularity (and occasional irregularity) of crises calls for explanation; and for all of Galen’s doubts about the theories of others, he himself was hard pressed to come up with a better alternative. But to return to the subject of this review: Cooper’s discussion of the issues throughout—introduction, notes, and commentary—is simply not up to standard.

Let us begin with the concept of critical days, which is Hippocratic—the definition from *On Medical Conditions* is cited by Cooper on 396n16 and has by itself no connection at all to the stars. Cooper himself correctly remarks, ‘That the critical days were part of common medical practice seems implied by the fact that Galen expects

[3] A small remark on the Arabic regarding 818.7 [182]: ‘bi-’aynihi’, means ‘exactly this’, i.e., ‘this is exactly what will happen’, not ‘by itself’, though the meaning is just about the same.

his readers to possess a certain basic knowledge of them' [403]. Again, near the end of his discussion of the scientific background, Cooper observes accurately, 'Astrology was not central to the scientific medicine of the Galenic tradition (as we have seen in the *Critical Days*)...' [58]. But he forgets these insights time and again when he gets carried away by disquisitions on 'astrology', which often have no grounding at all in the passage under discussion.

One key point must be made before turning to astrology. The importance of critical days for Galen, and Greek medical theory in general, has much to do with the fact that Greek medical theory developed largely in response to one disease, namely, malaria, which is marked by cyclic paroxysms and crises. This critical insight into the significance of malaria was made by the late Mirko Grmek in his *Diseases in the Ancient Greek World* [1989] but it has been largely overlooked, and not just by Cooper. Grmek's book is not listed in Cooper's bibliography. Indeed, the connection to malaria clearly lies at the heart of Galen's decision to take the onset of fever as the crucial factor in determining the beginning of the illness.[4]

Chapter 3 of the introduction, 'The Sciences in the Critical Days', which discusses 'the content rather than the history of the Arabic transmission', 'is included to assist the reader in understanding this treatise'. In fact, it seems to this reader that Cooper could use some assistance. For example, Cooper writes: 'Using basically an Aristotelian model, Galen offers a much more detailed hypothesis about lunar influences on patients' [61]. In n288, he observes that Ptolemy describes some supposed medical effects of the heavenly bodies in *Tetrabiblos* 3.10–14: 'but Galen makes an empirical argument, involving both induction from empirical data, and deduction from general principles'. Is this fair to Ptolemy? In fact, the first sections of the *Tetrabiblos* offer many arguments, not only that the heavenly bodies have effects but that these effects can be known, and that knowledge of them is beneficial. Indeed, the intimate connections between astrology and medicine with regard to issues such as the

[4] Cf. Cooper's comments on 797.10–16 [416], and especially on 798.7 [417], where he notes Galen's calling on Hippocrates' support 'for this important idea, namely, that, for purposes of calculating the series of critical days, the fever is identical to the illness'.

legitimacy of induction or the value of sciences whose success rate is not very impressive accompany the two arts throughout the ages.

Another slip-up, which in other circumstances could be written off as an oversight, reinforces my judgment that Cooper is not at all expert in the history of astrology. On page 24, Cooper notes that astrologers were consulted to select the propitious moment for the founding of Baghdad. He then adds: 'It would be interesting to know what the star chart of Baghdad looked like for that date...'. The foundational horoscope for Baghdad was published and discussed by David Pingree [see 1970, 104]. This is not the only instance where a desideratum passed on by Cooper for future research has already been discussed in accessible publications by leading scholars in the field.

Nonetheless, Cooper repeatedly introduces astrology into his comments, when there is no call at all to do so, for instance, with regard to 778.15 [112] where Galen opines that it is difficult, but not impossible, to distinguish the one who speaks the truth about critical days from the one who does not, adding that the matter 'is difficult only because it requires a long time and very thorough investigation'. Not the slightest trace of astrology here! But in his comment on this passage [401–402], Cooper volunteers an example of data that are hard to establish, such as the beginning of the illness (discussed by Galen elsewhere), compares the critical day as starting point to a person's astrological destiny, and spirals off into a disquisition on the parallel between prognosis by critical day and by astrology and between medicine and divination. None of this has the slightest bearing on Galen's text here!

Miscomprehension of astrology and its history are rampant. For example, in contradiction to Cooper's remarks at the top of page 58, mathematical detail in astrological medicine is consistent not just with Renaissance but with the medieval period too. However, the highly technical charts used in astrological medicine have nothing to do with the theory of critical days and, in fact, represent an alternative path to prediction. In his comment on 775.10 [398], Cooper offers an unjustified criticism of Vivian Nutton concerning Galen's dispute with the Methodists, claiming further that his own book 'will dispel future misconceptions regarding Galen's use of scientific method in connection with astrological prediction'—in a context where no astrology, or astrological prediction, has any relevance whatsoever.

It is not just in astrology, but in astronomy as well, that Cooper's grasp is insufficient. See, for example, his comment on 809.7–17:

> Galen is rather imprecise about what constitutes a day, which is odd, since his mathematical/astronomical exposition in Book III depends on precise values. Does he mean 'length of daylight' or does he mean something closer to the familiar twenty-four hour period in common use today?

Really! The *nychthemeron*, which is pretty darn close to 'the familiar twenty-four hour period', was in use in antiquity as well. But more to the point: if Galen has in mind the length of daylight or of night, then his computation will be off by a factor of two. Galen speaks of seven days, or 14 'lengths of day-or-night', not seven half-days. Cooper continues:

> This carelessness is another argument that Galen did not mean the astrology part of his theory to be taken seriously: astrology requires considering precise times and periods. Galen's near contemporary Ptolemy was far more precise about what a day is.

Indeed, he was.

On page 479, in commenting on 901.18, Cooper writes,

> Aristotle taught that without the motions of the heavenly bodies, whose influence churns the elements, no change, and hence, no life would be possible on Earth, since in the absence of celestial influences, the material elements would tend to seek their natural places—and stay there.

True enough—but does Aristotle speak of the 'influence' of the celestial bodies or rather of their motions?[5]

[5] In the following comment, Cooper writes that 'the Moon was thought to be closer—in the Aristotelian universe it is the heavenly body closest to the earth'. I cannot resist adding that the Moon is the closest heavenly body in the Ptolemaic universe and so also in the Copernican, Keplerian, Newtonian and contemporary universe. Cooper is not the only one to forget this simple fact.

> At K 912.3, Galen mentions a hypothetical horoscope in connection with a patient...He does not linger on details, but merely states that the sign that a patient is born under and the moon are related as follows....

But the value of a horoscope is in the details! If there are no computed values for the positions of the seven planets (at the very least), what kind of horoscope is it? Of course, in the passage in question, there is no horoscope at all but only some remarks of a very general nature concerning the 'sign' under which a person was born. To make matters worth, Cooper has mistranslated the passage that he refers to. Cooper's translation at 912.3 is not grammatical either; it reads:

> Suppose a patient was born in and good fortunes occur to him in Aries, and misfortunes in Taurus, then I maintain that when the Moon is in Aries, Cancer, Libra, or Capricorn, this patient's situation will inevitably be good.

But the Arabic conjunction 'wa-' in the phrase 'wulida wa-l-su'ūd fi al-ḥamal' means (as every beginning student of Arabic knows) not 'and' but 'when' or 'at the same time as'. So, a more precise translation would be:

> Suppose that a person is born when Aries is in good fortunes and Taurus is in bad fortunes, then, I say, that the situation of this person will doubtlessly be good whenever the Moon is in Aries, Cancer, Libra, or Capricorn.

On page 486, at the end of his comments on 913.6–15, Cooper writes:

> This connection between the lunar phases and the courses of illnesses was very important to the medieval physician, who carried charts to help him identify when the key lunar phases would occur.

Is there any evidence for the medieval physician carrying 'charts' of this sort with him when he made the rounds with his patients? This is another glib observation, thrown out to the reader without bothering to check, and without considering that the readership of this book will be exclusively trained scholars!

One more example should suffice:

> The patient's natal sign can be determined by observation, by tables, or, more reliably, by using as astrolabe (after these became available, long after Galen's time). [68]

It is not just the inaccuracy of these remarks—I do not know how the astrologer would 'observe' the natal sign; in any detailed horoscope, it is the ascendant point, not the sign, that signifies. I am annoyed not just by the inaccuracy of these generalizations but also by the way they are tossed off the cuff, as if this book was aimed at the most general audience, who would know no better.

Cooper's knowledge of astronomy as well is significantly lacking; this, combined with an insufficient command of the Arabic language, makes for some rough going. For example, Cooper writes in the introduction, 'Galen states that the relative positions of sun and moon during conjunctions are never the same' [63]. Obviously Galen would never say anything of the sort. In his note, Cooper refers to 906.15–907.5; and when one searches through those passages, one finds this sentence [332]: 'Consequently, the time in which the Moon appears distinctly is never the same'. But this is a mistranslation: the Arabic construction 'fa-laysa...dā'iman' means 'is not always'. So Galen is simply saying that 'the moment in which the Moon is distinctly visible is not always the same'. As he goes on to say, for a period of three days surrounding conjunction, the Moon is either not seen at all or seen. This is a somewhat cumbersome way of saying that the Moon will not be seen for a day or two, maybe three. It suffices for his purpose in the passage, which is to say that during those three days—even if the Moon may be occasionally visible, of course as a slim crescent barely above the horizon—its influence is practically nil.

At 906.11–15 Galen discusses the brief visibility of the Moon at the beginning of the month; just how long it is visible depends on a variety of factors such as the elongation, atmospheric conditions, the Moon's latitude, to name a few. Galen does not go into these details. Cooper writes [n984] in summing up Galen's discussion, 'So, the specific influences depend on whether the Moon is preceding or following the Sun'. But at the beginning of the month, the Moon is always following the Sun (i.e., to the west of the Sun). Moreover, Galen is here only describing the visibility of the Moon on the first day that it is visible; he is not saying anything at all about 'influences', whether specific or general. In the following note [332n985], Cooper again misunderstands the astronomy and adds an irrelevant astrological comment. Cooper notes, 'The Moon moves faster than the Sun, so its motion is a more significant factor in causing Earthly changes'. This may be true, but it is not what Galen is saying at 907.1–5. Galen is simply saying that the elongation, which depends on the swift and not very smooth motion of the Moon, is the strong variable in determining how long the Moon will be seen on the second day; not a word here about causing Earthly changes.

The following note as well reveals a complete lack of understanding of astronomy. In reviewing the causes for variation in the Moon's visibility, Galen lists also (citing for convenience Cooper's translation) the fact that 'due to the setting of the Zodiacal signs its interval is not equal', which Cooper glosses:

> This means that its path is not parallel to that of the signs. Therefore, reckoning its position along the Zodiac is problematic. [332n986]

Not at all. Galen is referring here to the 'setting time' of the Moon, which is determined by the setting of the arc drawn parallel to the celestial equator from the Moon to the horizon; in other words, it is evaluated by converting the altitude of the Moon into time degrees [see Pedersen 1974, 110–115].

More examples could be adduced but I will finish my critique of Cooper's handling of astronomy by citing just one more sentence from the comment on 905.11–16, this time without adding any counter-comment:

> This is because the lunar phases are constantly changing, and the fullest full Moon, when the Moon peaks in that phase mathematically speaking, lasts only a moment, and then swiftly begins to wane, like a point in time. [481]

In sum, the kindest thing one can say about Cooper's understanding of astronomy is that it is insufficient for the task he took upon himself in writing the commentary.

Cooper has something new to say about 'musical symbolism' in this treatise. The evidence for this is Galen's use of the verb «πλημμελέω», whose meaning Cooper gives as 'to play a wrong note in music', which serves to describe nature's going off course. I am not competent to assess just how significant Galen's choice of this word may be. But I am intrigued by Cooper's reference to Galen's 'application of rhythmic principles to his ground-breaking theory of prognosis via pulses', which, he avers, testifies to Galen's having no more than 'a superficial familiarity with some of the basic issues of music' [73]. Galen's statement that the pulse has a musical character was not lost on the Arabic tradition. It is repeated by Ibn Sina and developed in some of the numerous (and lengthy and involved) commentaries on that book.[6]

Music comes up again on pages 409–410 in comments on 789.6, where Galen says, in Cooper's translation from the Arabic, that physicians who investigate medical terms such as 'crisis' from the point of view of language 'understand as much of logic, grammar, and rhetoric as donkeys understand of music'. Cooper wants to make a nice point here: that the association of donkeys and music that is found in classical literature, in a negative sense of course, gets lost in the translation into Arabic. However, it is not clear just what is lost. Cooper says that 'Ḥunayn explains what is meant without the poetry'. It would be helpful here to display the 'poetry' found in Galen so that we can see just what has been left out. Cooper remarks later on the same page, 'Ḥunayn preserves the donkey/music image, which has a similar meaning in classical Arabic'. So has something been lost or has it not? I cannot figure it out. Cooper goes on to discuss some of the musical terminology in Greek and in Arabic. Again, a good thing to do. But for the Arabic 'īqā' he can supply only a few dictionary meanings, adding that 'an investigation of how this word is used, if at all, in the later Arabic musical treatises would be useful'. However, that investigation was carried out over 70 years ago by Henry Farmer [1929], the great pioneer in the history of Arabic music, and a discussion of the term in question can be found on page 49 of his *History of Arabian Music.*

I did not of course check Cooper's translation from beginning to end. I have already said that it generally reads well and appears to present correctly Galen's doctrines, as well as his unique style.

[6] I have a transcription of a long essay on this from the pen of Shlomo Ibn Ya'ish, the author of a multivolume gloss on the *Qanun* in Judaeo-Arabic, and patiently awaiting its turn in my work schedule.

However, I did stop for a closer look at some passages that caught my attention and in some cases I found some distressing errors.

For example, at the bottom of page 405, in a comment on 783.16 (misprinted as 783.14), Cooper offers some nice insights into Ḥunayn's translational skills but he has mishandled the phrase in question. Ḥunayn expands the Greek «κανόνες», which has no equivalent in Arabic (though the homonym 'qānūn' would be introduced into the language but with a different meaning) by a four-word phrase. Both my vocalization and translation differ from Cooper's. He reads 'li-yaṣbira bi-hā mā siwāhā' and translates it by 'in order that you might examine the others against those that resemble them'. I read 'li-yuṣbara bi-hā mā siwāhā' and translate it by 'in order that, by their means, other things may be examined'. 'Siwāhā' does not mean 'resemble' but 'other than' or 'different from'; I translate 'bi-hā' as 'by their means', expanding the instrumental suffix 'bi-', ordinarily translated 'by', to 'by...means' for added clarity.

Another example is the difficult passage, 798.1–6, on pages 146–148. Galen is talking about the beginning of the illness here. For him, this means specifically the beginning of the fever rather than the onset of symptoms such as insomnia or loss of appetite. But how precisely can this be determined? The physician must of course generally rely on the patient for this important datum. Galen's point here is that even the most insensitive, boorish, stupid person cannot be off by more than hour and that period of time is insignificant for determining the critical days. Here once again we must recall that by 'illness' and 'fever' Galen has in mind the disease that we call malaria, whose fever cannot be mistaken even by an ignorant brute.

Cooper translates the passage as follows:

> Suppose, moreover, that there is someone who fails to notice the fever when it began; then how many a patient is seen that it is possible for his fever to go unnoticed—which I consider to be impossible—and if the fewest people had perceived that an hour passes before it was perceived. Therefore, suppose that he thinks that the fever began in the tenth hour. But let its beginning really be not the tenth, but the ninth hour: then what harm is there for this person in knowing the critical days? Do you see how this is, for the most part, harmful in

> medicine, that some patients do not perceive their fever until an hour passes?

Cooper has completely missed the point of the passage and he has mishandled some of the Arabic phrases. His notes expand on his erroneous translation and are thus worthless. The correct translation, in my opinion, is:

> But it has happened that a person is unaware of his fever when it begins. How many a patient has been seen to be unaware of his fever as much as I reckon he is able to be, even if he were the least sensitive person, [which is] that an hour would pass before he became aware of it. So it would happen that he thought that the fever began in the tenth hour, whereas in truth it did not begin in the tenth but in the ninth. But what harm does this cause for the knowledge of the critical days? Do you not see that the maximum harm here for medicine is that some patients will not sense their fever until an hour has passed?

Cooper may not always have handled the Greek as well as he should have; his edition of the Greek will surely receive the scrutiny it warrants. But here are examples of some minor mishaps, which may be simple oversights; more serious errors will be discussed below in connection with Pythagoreanism.

- 190 regarding 821.7 'a bad mistake': 'mistake' here is '‘āri', the very same word that Cooper translated correctly in the preceding passages as 'accident'. It refers to an unforeseen event, one which can affect the natural course of events. For example, if the patient receives a piece of disturbing news or his attendants do not execute their duties properly, the patient's constitution will be affected and this in turn will put the development of the illness out of kilter. It is not a 'mistake', meaning an error of judgement or treatment on the part of the physician but an event that adversely affects the patient. This is indeed a difficult passage to translate and perhaps different English terms will be needed for the same Arabic term.
- 346 regarding 916.11: Cooper translates the Arabic as

> Therefore, in the strength of the weekly periods there is a marvelous thing, when so many factors cut up their illnesses and hinder them.

In n1039, Cooper explains that the many factors or reasons involved 'confuse the issue by presenting a complex situation'. I think that Galen intends here something very different: the fact that the weekly periods are so strong a factor—that is, that the 'week' remains the strongest variable despite the errors on the part of the physician, the patient, and others that foul up the natural process—is 'wondrous' and indicates that the temporal period is truly a potent cause.

- 398.3 in the comment on 776.6 regarding the Greek «τέλειαν», which is correctly translated into Arabic as 'tāmm': it means, however, 'complete', not 'incomplete' as Cooper has it.
- 404–406 'they have no rational principle (ἀλόγως)': to my taste, this should rather be presented as 'their theory is irrational (ἀλόγως)' or 'they have no rational principle (λόγως)', so that the Greek term in parentheses has a precise equivalent in the English.
- 422: in commenting on 806.6–16, Cooper calls attention to an important methodological choice made by Galen. Confronted with a number of possible causes for a phenomenon, Galen, in a thought experiment, fixes all but one and allows that one to vary in order to assess its effect on the entire system. This procedure was not unknown in medieval times. Levi ben Gerson used a similar method in order to determine whether a planet's influence varies with its altitude or its longitude. To do this, he compares their influence when at the two equinoxes, that is, when the longitude would be different but the altitude the same. On this basis, Levi decides that longitude is the strong variable. Moreover, he maintains that this can be determined empirically, and not just by a thought experiment [see Langermann 1999, 509–510].
- 427 regarding 813.11–18: 'mursalan' means 'without qualification', that is, 'not qualified in any way', in other words, 'in the most general sense'. It does not mean, as Cooper claims it does, 'absolute, unquestionable'.
- 431 regarding 819.14–820.11, where Cooper writes that Galen 'humorously' refers to death as a bad crisis: perhaps Cooper

means by ‘humorously’, in line with his theory of humours? I do not find this funny and doubt that Galen did either.

- 445 regarding 842.17: in fact the practice of consulting more than one physician continued in Islamic civilization. Maimonides complains of this [see Langermann 2004, 291–292].
- 452–453 regarding 853.7–11: the difference in opinion between Galen and al-Kindī is no evidence at all that al-Kindī did not see Galen’s book; it simply means that he chose his own path, as he did in all of the many fields within which he worked. Thinkers like al-Kindī are not the exclusive products of the books that they read and not every book that they read will exhibit its ‘influence’ in their writings. Al-Kindī is much more likely to have rejected Galen’s arguments and opted for Pythagoreanism.
- 480 regarding 903.11:

  > ...since the situation that was beginning to develop in the Islamic world in Ḥunayn’s time was for there to be a caliph, who had nominal authority, and a sultan, who had the real power and did the actual ruling.

  The most generous appraisal I can offer of this comment is that it is a tremendous oversimplification. In truth, it betrays a complete lack not just of understanding but of sensitivity to history. This is a minor point with regard to Galen but a major one concerning Cooper’s way of doing things. As in the case of music or the foundational horoscope for Baghdad, as we saw above, Cooper has made no effort at all to see what scholarship already exists on the subject. He would have done well, at the very least, to read the lengthy and learned entry in the *Encyclopedia of Islam s.v.* ‘sulṭān’, where he can learn that the first rulers to bear the title of sultan were the Seljuks.
- 498 regarding 934.12: Cooper once again betrays a fundamental unfamiliarity with the topic, here Pythagorean arithmology—and that is the correct term, not ‘the numerological approach to nature (‘numb-skull argument’)’, as Cooper writes in his commentary. Pythagorean arithmology has been studied intensively for some time, beginning at least with the investigations of Armand Delatte; but Cooper knows nothing of this. To be more precise, he evinces no knowledge of this

> in his discussion of the Arabic Galen. The correct term 'arithmological' is found in appendix 2 [530] but there is no further reference to the literature on the subject. Galen relates here some of the names that have been given to the numbers and Cooper misses the significance of just about all of them.

There is some rich material here for the textual history of *On Critical Days* as well as for the development and transmission of Pythagoreanism. In Table 1, I list the Arabic term in Cooper's edition, the Greek form found in Kühn [1825], Cooper's translation of the Arabic, and my own suggestion for translating the Arabic.

Arabic	Greek	Cooper	YTL
lā athar lahu	ἀμήτορα	[the One] has no mother	has no effect (Ḥunayn)
ṭalqan	τολμᾶν	[the Two is] unrestrained	unbound
ṣūra	ἰδέαν	[The One is] a form	form
unṣur ghayr mutanāhin	ὕλην ἄπειρον	[The Two is] a boundless essence	unlimited element
unṣur mutanāhin	πεπερασμένην	[The Three is] a bounded essence	limited element
niẓām	ἁρμονίαν	system	cosmos
adad tāmm awwal	ἢ τέλειον ἀριθμὸν	first perfect number	first whole (or full) number
mutajassam	ἢ στερεὸν	a (compound) body	a solid
basīṭ	ἢ ἐπίπεδον	a simple (substance)	a plane

Table 1

In the first definition or connotation of the monad, it is called ἀμήτορα. Oddly, Cooper chooses the reading of E ('lā athar lahu') for his Arabic text but he translates 'has no mother' in conformity with the reading of L ('lā umm lahu'), which is the correct translation of the Greek (and indeed Kühn's text here is cited in Liddell, Scott, and Jones 1968, *s.v.* ἀμήτωρ). But Ḥunayn (or perhaps the Syriac *Vorlage*, if there was one) read, or at least understood, the word to be «ἀμετέωρος», even though, as far as I know, no such word exists. 'Āthār' (the plural of 'athar') together with the adjective ''ulwiyya' ('higher up') was used to translate the title of Aristotle's *Meteorologica*: 'athar' means literally 'trace, effect' and in the context of Aristotle's book, refers to the effects produced in our atmosphere by the celestial bodies or the Earth's dynamic processes. This is clearly the meaning that the Arabic phrase has in our text. But where did it come from? Was this variant, or error, already found in any Greek manuscripts?

It is interesting that Ḥunayn chooses to translate «ὕλην» by 'unṣur', a term generally used to refer to 'element', most especially the four Empedoclean elements, rather than the basic material stuff before it is differentiated into elements. However, in Ḥunayn's translations of Galen, it means the simple, undifferentiated element: compare his version of *On the Elements according to Hippocrates* in Langermann 2009, 7. Eventually the Greek word was absorbed into Arabic as 'hayūlā'. In the Greek, «πεπερασμένην» modifies «ἁρμονίαν», so the meaning is 'bounded cosmos' or 'limited harmony' (*scil.* 'harmonious system'). But the Arabic has added here 'unṣur' ('element') and inserted the conjunction 'aw' ('or'), thus dividing the phrase into two alternative connotations. Is there any justification for this in the Greek manuscripts?

With regard to the various connotations of 3, something has gone awry again; and again, I have only Kühn's Greek text to consult, which reads «ἢ τέλειον ἀριθμὸν καὶ α′ ἢ στερεὸν ἢ ἐπίπεδον». Apparently Ḥunayn understood correctly that «α′» is shorthand for 'first' (or in his manuscript the adjective «πρῶτος» was written out) but either could not put the sentence together or else—and this seems very unlikely—came up with an Arabic sentence that conveys a different meaning. The Greek says, '[first] whole number, first solid, first plane'; in the Arabic, 'awwal' ('first') modifies only 'first whole number'. 'Basīṭ' can mean either 'simple' (as used in physics and metaphysics to refer to simple substances) or the geometric term 'plane', used also for numbers. Had Cooper consulted the Greek, he might have made a better choice for his translation. However, he did consult the Greek, or at least Kühn's Latin translation, for «τέλειον»: Kühn renders it 'perfectum'. Indeed, 'perfect' is the meaning given by Liddell, Scott, and Jones [1968] but it is inappropriate here. The first perfect number, that is, the number that is also the sum of its factors is 6, as indeed Cooper points out [498n16]. So here 'tāmm' clearly means 'whole' and signifies that according to this teaching, neither 1 nor 2 are considered to be numbers.

I believe that I have adduced enough examples of passages in this book that require critical repairs; more could be supplied. Clearly, Cooper ought to have done his research more carefully and so also his translation. Nevertheless, as I stated at the beginning of this essay, this a welcome and important addition to the scholar's bookshelf.

BIBLIOGRAPHY

Farmer, H. 1929. *History of Arabian Music.* London.

Grmek, M. 1989. *Diseases in the Ancient Greek World.* Baltimore, MD.

Gutas, D. 1998. *Greek Thought, Arabic Culture.* London/New York.

Klein-Franke, F. 1975. trans. 'Die Ursachen der Krisen bei akuten Krankheiten. Eine wiederentdeckte Schrift al-Kindī's'. Israel Oriental Studies 5. Tel Aviv.

Kühn, K. G. 1825. *Claudii Galeni opera omnia.* vol. 9. Leipzig.

Langermann, Y. T. 1999. 'Gersonides on Astrology'. Pp. 506–529 in S. Feldman trans. *Gersonides, The Wars of the Lord.* vol. 3. NewYork/Jerusalem.

________ 2004. 'L'oeuvre médicale de Maïmonide, un apercu général'. Pp. 275–302 in T. Lévy and R. Rashed edd. *Maïmonide Philosophe et Savant (1138–1204).* Leuven.

________ 2009. 'Islamic Atomism and the Galenic Tradition'. *History of Science* 47:277–295.

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

Pedersen, O. 1974. *A Survey of the Almagest.* Odense.

Pingree, D. 1970. 'The Fragments of the Works of al-Fazari'. *Journal of Near Eastern Studies* 29:103–123.

Saliba, G. 2007. *Islamic Science and the Making of the European Renaissance.* Cambridge, MA/London.

Weisser, U. 1982. 'Mondphasen und Krisen nach al-Kindī'. *Sudhoffs Archive für Geschichte der Medizin und der Naturwissenschaften* 66:390–395.

Apollonius de Pergé, Coniques. *Tome 4: Livres VI et VII. Commentaire historique et mathématique. Édition et traduction du texte arabe* by Roshdi Rashed

Berlin/New York: De Gruyter, 2009. Pp. xii + 572. ISBN 978–3–11–019940–6. Cloth € 137.00

Reviewed by
Aldo Brigaglia
Università di Palermo
brig@math.unipa.it

In memoriam Hélène Bellosta

With the publication of the fourth volume of the new edition of the *Conics* by Apollonius of Perga (*ca* 262–180 BC), Roshdi Rashed has completed his very important work on the edition of the Arabic text, its translation into French, and a vast mathematical commentary. Apollonius' treatise itself may well be considered one of the highest achievements of Greek mathematics at its most brilliant. In fact, together with the corpus of the mathematical work of Archimedes (287–216 BC), the *Conics* constitute the greater part of Greek *higher* mathematics.

Rashed's edition of the text of the *Conics* is the latest episode in the long and intriguing history of the transmission of this major mathematical work to us. The first four books arrived to Western mathematical culture through the edition by Eutocius (fifth century AD), which was translated into Latin in the 16th century by Johannes Baptista Memus, Francesco Maurolico, and Federico Commandino. Books 5, 6, and 7 of the *Conics* arrived in Europe only through the Arabic translations of the Greek text: the first text of the lost Greek books was contained in an Arabic compendium of the *Conics* written by Abu'l-Fath Mahmud al-Isfahani (second half of the 10th century). This text was given to Cardinal Ferdinando I de' Medici (later grand duke of Tuscany) by the Patriarch of Antioch as early as 1578 but was edited and translated by the Maronite deacon Abraham Ecchellensis (Ibrahim al-Haqilani) under the supervision of Giovanni

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 241–260

Alfonso Borelli only in 1661. This text was heavily manipulated by its Arabic editor.

In the ninth century, the Banū Mūsā brothers (Muhammad, Ahmad and al-Hasan) made great efforts to acquire, understand, and obtain a translation into Arabic of the complete text of the *Conics*. Books 1–4 were translated under the supervision of Ahmad by Hilal b. abi Hilal al Himsi, and books 5–7 by Thabit b. Qurra. (Book 8 is now considered to have been lost by this date). This Arabic translation was brought to Holland by Jacobus Golius in 1629 (it now is in the Bodleian Library in Oxford); but even though its existence was well known in Europe, it was published in a Latin translation by Edmund Halley only in 1710. Halley's edition remained the main reference for books 5-7 of *Conics* until recently and constituted the basis for the first English translation of these books [Heath 1896] as well as of the first French translation [ver Eecke 1923]. A more recent English translation (with the Arabic text) of books 5–7 was published in 1990 by Gerald J. Toomer.

It is worth noting that from Halley's edition on, the Banū Mūsā version has been used to give us the translation of the last three books only of the *Conics*, while the first four books were always published on the basis of the edition of Eutocius directly from the Greek text. As Rashed pointed out, this reveals some prejudices, among which I may cite:

- the idea that the edition by Eutocius provides us with Apollonius' exact text of the first four books of the *Conics*, and
- the idea that the Arabic translation of the first four books is that of this same edition by Eutocius.

The complete edition of the *entire* corpus of the Banū Mūsā version allows us to understand, for example, that there are many differences—and sometimes very profound ones—between the edition of Eutocius and the Arabic translation, mainly in book 4. Rashed points out some of these differences:

- in Eutocius's edition, book 4 consists of 57 propositions but there are only 53 in the Arabic translation;
- some propositions of Eutocius's edition are missing from the Arabic translation;[1]

[1] An attentive examination of these propositions shows that they may be

- there are two propositions in the Arabic translation that do not appear in Eutocius's edition;
- the order of propositions differs;
- the figures and their letters differ in a certain number of propositions; and
- there are different proofs and, moreover, some proofs are erroneous.

In any case, the Banū Mūsā 's edition of books 5–7 has always been reputed to be the principal source for that part of Apollonius' work and Rashed's edition makes a very important contribution to our knowledge of it.

Book 6 is concerned with the problem of defining equality and similarity between conic sections. The first part (up to proposition 27) treats what we can call the 'criteria for equality and similarity'. The second part (up to proposition 33) poses the main problem: how to cut a given right cone so that the result is a section equal (or similar) to another given one.

This poses an interesting conceptual problem: 'What is really meant by the terms "equality" and "similarity" between conic sections?' Rashed's commentary dedicates many pages to this matter. With regards to equality, Apollonius resorts to the idea of 'superposition': two conics are equal if they can be superposed on one another (by means of a motion). In the words of Apollonius as rendered in Rashed's translation,

> *les sections de cônes que l'on dit égales sont celles dont les unes peuvent se superposer aux autres et dont aucune n'excède l'autre.* [90]

Toomer [1990, 1.264] uses the term 'can be fitted' for Rashed's 'peuvent se superposer'. In any case, this is a usage that goes far beyond what was done by Euclid, who in his fourth common notion effectively says, 'Things which coincide with one another are equal to one another' [Heath 1956, 1.153]. The phrase written in Greek is «καὶ τὰ ἐφαρμόζοντα ἐπ' ἀλλήλα ἴσα ἀλλήλοις ἐστίν». The word «ἐφαρμόζοντα»

defective. For example, proposition 4.7 depends on a hypothesis which is supposed to have been given in the preceding proposition 4.6; but this hypothesis does not exist.

was translated by Heath as ‘coinciding’. The translation of al-Hajjai, reproduced by Rashed, is:

> *Celles (les choses) qui se superposent les unes aux les autres sont égales les unes aux autres.* [10]

While in the Euclidean definition we can thus discern the use of the term ‘superposition’, it is not at all clear how this is connected to the idea of *motion*. How is this common notion (note that in Euclid this is not a definition) to be used concretely? The absence of any postulate regarding the use of this notion and, in particular, the notion of the rigid motion that would lead the two figures to be superposed on one another, makes verifying the congruence of the two figures problematic.

Actually, Euclid prefers to make use of it in a very limited way. In the second proposition of the *Elements*, he constructs a segment equal to a given segment (thereby showing how to ‘move’ a segment) but he does not make use of congruence, and the verification of the equality of the two segments is entrusted to postulate 3 (‘All radii of a given circle are equal to each other’) and to common notions 2 and 3 (‘The addition/subtraction of equals to/from equals result in equals’). In the fourth and eighth propositions (criteria for the equality of triangles), he actually uses equality by superposition: from that point on, as far as the equality of polygons is concerned, no use at all is made of superposition. It is another matter with regard to the equality of arcs of circles, whose verification often requires reasoning based precisely on equality by superposition of figures since one cannot resort to equality between triangles as in the case of polygons.

The relevance and the meaning of this definition and its connection with Euclid’s common notion has also been discussed by Fried and Unguru [2001] in great detail; and although it might be worthwhile to compare their point of view with that of Rashed, this is beyond the scope of the present review. Still, it is perhaps worthwhile to underline, as Rashed does, the fact that from the point of view of modern criticism, in the absence of any postulate regarding rigid motion, Euclid’s reasoning does not appear to be rigorous. On the other hand, Hilbert showed that it is necessary to assume Euclid’s proposition 4 (the first criterion of equality of triangles) as a postulate, given that it is not at all a logical consequence of the Greek mathematician's postulates, axioms, and common notions.

Naturally, Apollonius, who is comparing segments of a conic, is forced to resort more than once to the criterion of equality by superposition. The differences in formulation between Apollonius and Euclid regarding equality by superposition are highlighted in Rashed's commentary. The use that Apollonius makes of the concept of superposition implies some idea (even though never explicated) of motion:

> *La définition de l'égalité par superposition...peut encore se dire ainsi: deux sections—ou portions—coniques sont dites égales si elles coïncident parfaitement une fois que l'une est amenée sur l'autre par un déplacement, de sort que leurs contours s'identifient.* [11]

In this definition:

(1) no concept of magnitude or measure is ever introduced;
(2) an idea of motion in the sense of a transformation is presumed but never explicated by Apollonius;
(3) there are no operating concepts, a fact which thus necessitates the integration of other properties whose use is more directly operative (the symptoms); and
(4) it is necessary to insert other procedures that integrate the concept of motion.

These observations will become clearer if we examine some of the first propositions of book 6.

Propositions 6.1 and 6.2 concern the equality of two conic sections (the parabola in the first and the hyperbola in the second). It is proven, for example, that two parabolas are equal if and only if they have the same *latus rectum.* Recall that the *latus rectum* of a parabola has the following property [see Figure 1]: the *latus rectum* is defined as that segment c, where B is a point of the parabola, C the corresponding point on the axis, and A the vertex of the parabola, such that CB is the mean proportional between AC and c. In modern terms, if we set $BC = x$ and $AC = y$, then we have $x^2 = cy$. This is the 'symptom' of the parabola.

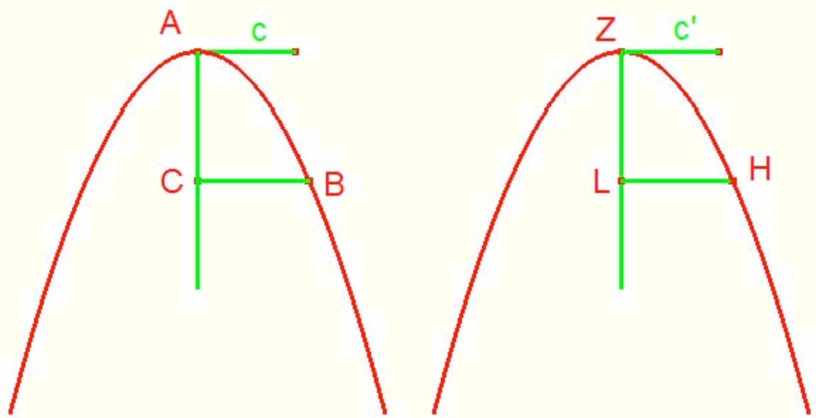


Figure 1

Apollonius' reasoning can be summarized in this way [see Figure 1]. If $c = c'$ and if we transport line AC so that it is superposed on line ZL such that A is carried onto Z, and if we call the point on ZL where C falls L (and thus $ZL = AC$), we will have $CB^2 = cAC = c'ZL = LH^2$. Thus, B too is superposed on H and the two parabolas are pointwise superposed. This reasoning can be clearly inverted.

In analogous fashion, Apollonius proceeds to find the conditions for the equality of two ellipses or two hyperbolas (the central conics), except that in this case what comes into play in addition to the *latus rectum* is either the axis [prop. 6.2] or an arbitrary diameter[corollary to prop. 6.2]: two central conics are equal if and only if their respective 'figures'—that is, the rectangles formed by the axis (or a diameter) and the corresponding *latus rectum*—are equal.

It should be noted that, once these propositions have been proven, Apollonius no longer needs to refer the equality of two conics to the poorly defined concept of 'superposition' but can refer instead directly to their 'symptoms', which in some way correspond to the equations of analytical geometry. Thus, for example, to see that two ellipses are equal it is sufficient to see that their *latera recta* and axes are equal. Rashed rightly notes:

> *La tâche qui est celle d'Apollonius dans le livre VI est donc, pour l'essentiel, de déterminer les conditions pour que les deux sections soient superposables...à l'aide des* symptomata, *sans*

> *toutefois s'intéresser à la nature même de ces transformations ponctuelles.* [6]

In other words, Apollonius, like Euclid, defines equality by means of superposition, which implies an idea, never explicated nor clearly defined, of motion. But he then tries to rid himself of that onerous condition through determining the equality of the conics by means of a simple comparison of magnitudes (segments or surfaces). As Rashed underlines,

> *de fait, au cours des démonstrations, l'égalité/superposition est doublée de l'égalité des aires—ou des longueurs. Apollonius recourt alors aux* symptomata. [11]

This process is completely analogous to that followed by Euclid: thanks to the theorems in the equality of triangles [*Elem.* 1.4 and 1.8], verification of the equality of two triangles (and, thus, of any two polygons) is reduced to the equality of segments and angles. A similar procedure is used in book 3 regarding circles, whose equality is attributed (in this case starting from the definitions) to the equality of the diameters.

It is interesting to note that, while this technique makes it possible for Apollonius to free himself from having to resort to superposition any time that two conics must be compared in their entirety, it loses its efficaciousness when he has to compare portions of conics: in this case, it is necessary to go back to the original definition of equality and thus to superposition. In this sense, the idea of superposition in book 6 of the *Conics* takes on a role and importance that it never assumed in either Euclid or in the other books of the *Conics*. This has been made clearly evident by Rashed [11]:

> *Malgré l'inspiration euclidienne patente, la définition de l'égalité/superposition recouvrira chez Apollonius plusieurs contenus.*

One instance of the role played by equality by superposition in this book can be found in the proof of what today we would call the symmetry of conics with respect to the axes. For example, let us examine prop. 6.4:

> If there is an ellipse and a line passes through its center such that its extremities end at the section [i.e. the line is a diameter] then it cuts the boundary of the section into two

> equal parts, and the surface is also bisected. [Toomer1990, 276]

The proof, which in this proposition is limited to the case in which the diameter is the axis, proceeds by *reductio ad absurdum* [see Figure 2]. Given axis AB, it is supposed by way of *reductio ad absurdum* that, after being turned over, the arc of ellipse AΓB does not coincide[2] with arc AEB, and that it is precisely point Γ where AΓB does not superpose itself on arc AEB. If from Γ we drop the perpendicular ΓΔ to the axis and extend it until it meets arc AEB in a point E, we find, by the definition of axis, that ΓΔE and, further, ΔE are perpendicular to AB; thus, after being turned over ΔΓ coincides with ΔE and Γ coincides with E, contrary to the initial hypothesis.[3]

I believe it evident that such considerations of Apollonius' proofs lead us to imagine a superposition achieved by some motion. Yet, in my opinion, it is not completely clear what Apollonius' idea of that motion was; but at the same time, there seems to be no doubt that, as Rashed shows amply, the point of view expressed in book 6 had a profound influence on later Arabic mathematicians. As Rashed puts it, we are dealing with 'proto-transformations ponctuelles, que les mathématiciens ne cesseront d'exhiber et de développer à partir du IXe siècle à Bagdad' [11].

The definition of similarity, however, is quite different. Apollonius wrote:

> And similar are such that, when ordinates are drawn in them to fall on the axes, the ratios of the ordinates to the lengths

---

2 Toomer uses the term 'coincide' in his translation, while Rashed uses 'tombe sur' ('fall on'). The two translations are comparable if we take 'coincide' to mean 'coincide after being turned over'. In any case, Rashed's translation provides a much clearer idea of motion than that implied by Toomer's.

3 It is also worthwhile observing that today we would have preferred an indirect proof rather than one by *reductio ad absurdum*. Such a proof might have proceeded in this way:

> given any point Γ on arc AΓB, we will show that after turning over ΓΔ with respect to axis AB a point on arc AEB is obtained.

But such a proof would have required considering an ellipse as being formed of infinite points, something that was far from the way in which geometric figures were conceived by the Greeks. However, considerations of this sort would inevitably take us too far afield.

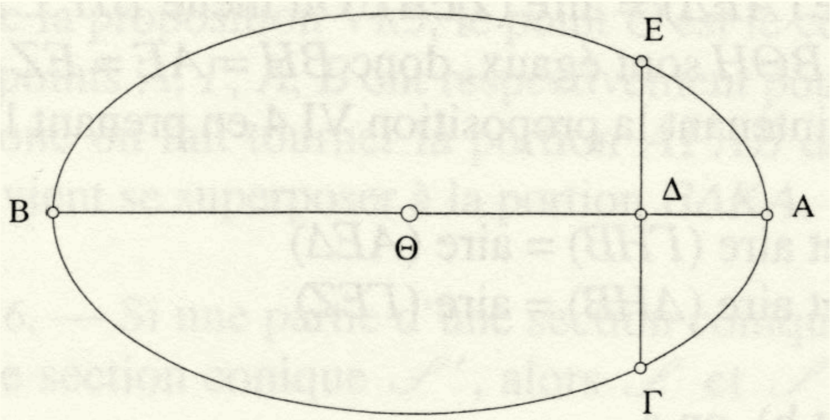


Figure 2

> they cut off from the axes from the vertex of the section are equal to one another, while the ratios to each other of the portions which the ordinates cut off from the axes are equal ratios. [Toomer 1990, 264]

In this case, we are dealing with a *functional* definition: to equal ratios between the abscissas correspond equal ratios between the relative ordinates. Rashed points out that the concept of similarity between conic sections is certainly present before Apollonius.[4] Archimedes stated that all parabolas are similar to each other and, thus, it is entirely plausible that Apollonius was aware of this fact [Apollonius, *Con.* 6.11]. But, as I believe, there is no difficulty in agreeing with Rashed's statement that

> *rien à notre connaissance ne permet d'affirmer qu'il y a eu une étude réglée de la similitude des sections coniques avant le livre VI.* [23]

In this case as well, Apollonius moves immediately to substituting the functional concept of similarity with his verification by means of the 'symptoms'. Two central conics are similar if and only if their respective figures—that is, the rectangles formed by the axis and *latus rectum* [*Con.* 7.12]—are similar. Thus, they are similar when, given d and d' as the respective axes and the *latera recta* c and c', $d{:}d' = c{:}c'$. The text continues with several generalizations

[4] E.g., in his book *On Conoids and Spheroids*: see Heath 1897, 99–150.

(taking into account any given diameters instead of axes) and then it is proved to be impossible for a conic to be similar to a conic of a different name (for example, a parabola can never be similar to a hyperbola, and so forth). Apollonius then deals with segments that are similar or equal in conic sections, first for similar sections and then for dissimilar sections. In this last case (dealt with in propositions 6.23–25), there is a beautiful result: there cannot exist similar segments in dissimilar sections. This result signifies, naturally, that similarity is a local property: if two conics have two similar segments (which are arbitrarily small, we would say), then they are entirely similar. Concluding the part regarding similarity, Apollonius proves that if a right cone is cut with two planes that are parallel to each other, the conics obtained are similar. As Rashed points out, what is in fact proven (using our terminology) is that in this case the two conics are homothetic from the vertex of the cone, with the ratio of homotheity equal to the ratio between the respective distances from the vertex itself.

In contrast, the final part of book 6 presents problems:

- given a conic section and a right cone, cut the cone with a plane so that the intersection is a conic equal to the given one; and
- given a conic section, find a cone similar to a given cone such that the given conic is a section of the cone found.

This kind of problem appears to be meaningful and may in some way provide a clue to Apollonius' aim in writing this book. In a certain sense, it is an inversion of what was done in the first book: while book 1 dealt with constructing the section of a cone as a plane curve, book 6 deals with cutting a given cone according to a given conic section in a plane.

Using the notations shown in Figure 3, where Α represents the vertex of the cone, Θ the centre of the circle of the base, and the end points of the diameter of the base Β and Γ, the condition under which it is possible to carry out the proposed construction (with d as usual as the diameter and c the *latus rectum*), is that

$$\frac{d}{c} \geq \frac{\mathrm{A}\Theta^2}{\mathrm{B}\Theta^2}$$

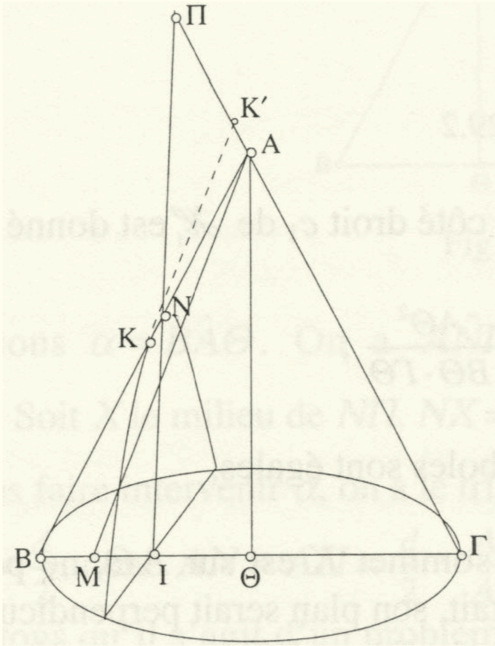


Figure 3

If this condition is satisfied, the construction is done by inserting between the extension of line AΠ and AB a segment (labelled NΠ in Figure 3) parallel to AΘ whose length is equal to d. Rashed makes two observations in this regard that I find particularly interesting.

(1) The first concerns the condition under which the construction can be carried out. The author notes that this condition

> *est équivalent à la condition selon la quelle l'angle entre les asymptotes de l'hyperbole ne doit pas être plus grand que l'angle 2α au sommet du cône. La recherche de la condition de possibilité dans le cas d'un cône oblique aurait été plus difficile: elle ne s'exprime pas en termes d'angle au sommet.* [66]

This observation raises a question which has already been posed by Zeuthen [1886], that is, 'Why is it, having in all preceding books set for himself the more general conditions of oblique cones, that here Apollonius always refers to right cones?' The answer is certainly not that provided by Toomer, who wrote, 'It is easy to see that his

[Apollonius'] solutions in book VI can be extended to the oblique cone' [1990, lviii]. As Rashed notes [66], this is not true at all and the solution becomes quite complicated, at least in this instance, in the passage to the oblique case [see also Brigaglia 1997]. In my opinion, the question remains open; but Rashed in any case provides, with reference to the problems that follow, an interesting hypothesis about this fact.

(2) As was seen earlier, the construction of the hyperbola requires inserting a segment of a given length that is parallel to one given line. This simple construction is completely absent in Apollonius. Rashed provides a complete proof of this fact, the absence of which appears not to have been noticed by the Arabic translators, although Halley did.

As we said, the final three propositions regard the construction of a right cone (similar to a given one) whose section is a given conic. Here again we can ask why Apollonius limited himself to the case of the right cone.[5] Rashed notes that while in the right case the problem is determinate; in the oblique case, it remains indeterminate. He concludes:

> *C'est précisément par ce caractère d'unicité de la solution que les propositions 31 à 33 diffèrent des propositions 49 et 50 du livre I. C'est ce même caractère qui semble expliquer le choix d'Apollonius du cône droit.*

It would be worthwhile to develop this interesting observation further.

Rashed's presentation of book 6 of the *Conics* ends with a section that is particularly original, 'Le sixième livre et la géométrie proto-transformationnelle'. He writes:

> *Le commentaire systématique du sixième livre révèle en effet qu'il s'agit indubitablement d'une géométrie où l'on procède pragmatiquement par mouvement et transformations ponctuelles.* [77]

The word 'pragmatiquement' is especially interesting: in this book, Apollonius makes ample use of concepts such as motion or transformation but without either defining them precisely or using them in a way that is altogether self-conscious. In fact, to find a fully self-

[5] See 'Remarques sur le propositions 31 à 33' [77].

conscious use of them, we will have to wait for the works of La Hire and then, another two centuries later, of Felix Klein. With regard to the works of La Hire, Rashed writes:

> *Ce regard, même s'il englobe celui d'Apollonius et l'éclaire, n'est cependant pas le sien: ses concepts, ses instruments et son langage sont en effet différents. Cependant, les objets géométriques étudiés dans les Coniques possèdent bien ces propriétés, qui ne seront appréhendées et révélées que par les successeurs d'Apollonius.... C'est donc en restant fidèle à la pensée du mathématicien alexandrin que l'historien peut s'inspirer de ces propriétés, pour mieux pénétrer cette réalité mathématique que celui-ci abordait avec les moyens de la géométrie de son temps. Aussi pour compléter le commentaire du sixième livre, allons-nous le considérer avec d'autres yeux que ceux d'Apollonius, ceux d'un lointain successeur.* [78]

This *lointain successeur* (distant successor) is, in fact, Felix Klein. The lengthy digression [78–83] in which Rashed reconstructs the entire sixth book from the point of view of projection and transformation groups may appear at first to be out of place, but this is not the case. The final lines [83] make the author's motivations clear:

> *Une interprétation de ce type permette de mettre en évidence les transformations ponctuelles sous-jacentes au travail d'Apollonius. Á partir du IXe siècle, ce livre VI, ainsi que les autres travaux d'Apollonius sur les lieux plans, ont incité les géomètres à concevoir les transformations ponctuelles de courbe à courbe (Thābit ibn Qurra et Ibn al-Haytham par exemple).*

To my mind it is precisely here that we find one of the aspects of greatest value in the new translation of the Arabic text of Apollonius, which can be inserted into the imposing context of the Arabic tradition of translating mathematical texts. Interpreted in this light, we can appreciate the work of the great Arabic mathematicians not only as transmitters of Greek thought, but also as original interpreters of the mathematics that was made available to them, interpreters who were capable, through new ideas, of opening new roads—even though a significant portion of them would receive their natural development only much later and in a different culture.

Before going on to a brief look at Book 7, I should like to go back to a central point in Rashed's formulation. Book 6 has traditionally been considered secondary in the context of Apollonius' work. As evidence of this, I cite Zeuthen, who says that no real geometric difficulty is overcome here. However, Apollonius himself had something to say about this book:

> We have enunciated more than what was composed by others among our predecessors....What we have stated on this is fuller and clearer than the statements of our predecessors. [Toomer 1990, 262]

Fried and Unguru [2001] have a different appreciation as they say that the importance of book 6 lies in the fact that:

(1) equality and similarity of conic sections is, for Apollonius, a far more subtle affair than we would like to think;
(2) the investigation of equality and similarity is necessary to clarify what is meant by a conic section being 'given'; and
(3) it does not merely elaborate ideas already elaborated in book 1 but complements those ideas somewhat in the way Euclid's *Data* complements the *Elements*.

Rashed, however, goes further. Indeed, without pretending to be completely original, Apollonius does give himself credit for providing a more complete and systematic organization of the material. This is precisely what Rashed claims. For him, Apollonius is in search of new means for extending the study of equality and similarity to curved figures:

> *Il fallut trouver les moyens de faire correspondre une section à une autre, différente, une portion à une autre, différente.*

Thus, Apollonius' aim was

> *trouver les moyens d'étendre aux sections coniques la recherche accompli pour les figures rectilignes et pour les arcs de cercle, et déterminer les conditions requises par une telle extension.* [5]

The historical importance of book 6, then, lies in its having paved the way later taken by numerous Arabic mathematicians:

> *les mathématiciens qui les premiers ont pris davantage de distance à l'égard de la géométrie des figures et ont introduit mouvement et transformations ponctuelles se sont précisément*

> *référés à ce livre VI—ainsi Thābit ibn Qurra, al-Sijzi, Ibn Hūd, Ibn Abi Jarrāda....* [7]

This is a point of view that I believe is novel, one which only someone like Rashed, who truly knows the contributions of these mathematicians, could provide and which deserves to be examined in greater depth.

The seventh book is quite another story. While the purposes of book 6 are extremely clear, book 7 appears quite difficult to read, not in the sense that it is mathematically difficult but in the sense of trying to understand the aims of its author. In the general introduction to book 1, Apollonius wrote, ‘another [*scil.* book 7] [deals] with theorems concerning determinations.’[6] In the accompanying letter from Apollonius to Attalus, he also wrote:

> Peace be with you.... In this book are many wonderful and beautiful things on the topic of the diameters and the figures constructed on them, set out in detail. All of this is of great use in many types of problems, and there is much need for it in the kind of problems which occur in conic sections which we mentioned, among those which will be discussed and proven in the eighth book of this treatise. [Toomer 1990, 382]

Here the word ‘diorismes’ (‘determinations’) signifies the determination of a problem’s conditions of solvability. Thus, we are dealing with a book in which are determined the range of possible variation for values relative to the diameters and *latera recta* of conic sections.

As Rashed rightly observes, it is very difficult to comprehend fully the significance of the choices made by Apollonius without having access to the eighth book (which, as mentioned, has been lost definitively), because it in fact appears that we are dealing with elements that are very closely tied to the solution of problems given in that eighth book. All of this is clearly highlighted by Rashed:

> *Quant à l’usage qui serait fait de ces théorèmes au huitième livre, nous l’ignorons puisque celui-ci est définitivement perdu et qu’aucun témoignage fiable ne nous est parvenu à son propos.* [241]

---

[6] ‘determinations’: *diorismes*, in Rashed’s translation.

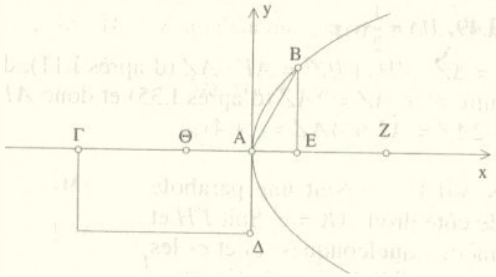


Figure 4

Thus, book 7 has to be read on its own, since no references to book 8 are possible. This is done in an exemplary way in the rest of the text that follows. The hinge of Rashed's interpretation is that it effectively consists in the study of the variation of several magnitudes tied to diameters and *latera recta* and, thus, to the determination of the maximum and minimum values that these can reach. This provides a point of continuity with book 5, which is dedicated to the determination of maxima and minima of magnitudes such as the distance of a point from the points of a conic section:

> *Le livre VII est dans une certaine continuité avec le livre V. Nous avons en effet montré que, dans ce dernier, Apollonius étudie la variation de la distance d'un point donné aux points d'une section conique. Mais cette continuité s'observe aussi dans la formulation des propositions...et dans la communauté du lexique.* [245]

It seems to me that this continuity is amply proven by Rashed's examination of the text, with perhaps one caveat: while book 5 is self-contained and its purpose lies in the search for maxima and minima of some magnitudes found in it, book 7 is completely oriented towards book 8 and is, therefore, much more difficult for a modern reader who does not have access to that last book, to grasp fully the beauty and depth of the theorems that are contained in it.

It is precisely these characteristics that prevent me from going into technical details. One central point of the second proposition in this book is the introduction of a new magnitude that Rashed translates as 'segment semblable en proportion' [251] (which Toomer, following Halley, translated as 'homologue'). This is what is at issue [see Figure 4]:[7]

> Let there be hyperbola H with transverse axis d_0 and *latus rectum* c_0, and let Θ be on diameter ΑΓ such that there is
>
> $$\frac{\Theta\Gamma}{\Theta\mathrm{A}} = \frac{d_0}{c_0} \ .$$
>
> We would call ΘA the segment 'similar in proportion'.

Five lemma-like propositions are dedicated to this magnitude. Book 7 then goes on with a group of propositions (from 6.6 to 7.20) dedicated to the determination of formulas relative to ratios between different magnitudes like

$$\frac{{d_0}^2}{(d-d')^2} \ .$$[8]

The second part of the book is the more substantial one, and is dedicated to the study of the variation of magnitudes such as diameters, associated *latera recta*, their sums, differences, products or ratios.

This is the part of book 7 that is most directly connected to book 5, itself dedicated to the determination of the maxima and minima that are (presumably) necessary for determining the conditions of solvability for the problems treated in the lost book 8. It is precisely here that we see the important role played by the attempts to reconstruct the long lost book 8 in order to understand the kind of interpretation that various readers have given to the theorems set out in book 7. Famous among such attempts are those of Halley and the 11th-century Arabic mathematician Ibn al-Haytham. Rashed refers in particular to the latter in the reconstruction proposed in this present volume.[9] Rashed cites two examples that I believe it is interesting to repeat here:

[7] I will discuss only the case of the hyperbola, the analogous one for the ellipse is given in 7.3.

[8] Here d_0 represents as before the transverse axis, while d and d' represent any diameter and its conjugate.

[9] He had earlier proposed and commented on this reconstruction in Rashed 2000.

(1) Given a central conic, find a point such that the diameter d drawn from this point and the associated *latus rectum* c satisfy the equation $dc = k$, with k given.
(2) Given a central conic with transverse axis d_0[10] and associated *latus rectum* c_0, find a diameter d and its associated *latus rectum* c such that $d + c = k$, with k given.

From the use that the Arabic mathematician makes of the propositions of book 7 to solve these and other problems, Rashed draws interesting conclusions:

> *Par « théorèmes relatifs aux diorismes », il semble donc que Apollonius entende deux choses à la fois. Il s'agit de propositions qui d'une part renferment elles-mêmes des diorismes, et qui d'autre parte interviennent dans la conception des diorismes lors de la construction des problèmes au moyen de l'intersection des coniques. Tel est bien le cas pour un bon nombre des propositions du septième livre. Or cette dualité de sens, seulement implicite, ne pouvait qu'intriguer les commentateurs.* [248]

To be sure, Rashed's rich commentary made it possible for me to retrace by following his text a magnificent itinerary through Greek mathematics filtered through Arabic culture.

Before finishing with book 7, I should like to note that, as he did in book 6, here too Rashed concludes an introductory commentary with a section ('Étude analytique de la variation des grandeurs associées à d, d', c, c'') that translates the text of the mathematician from Alexandria into modern language. This not only facilitates its comprehension by a modern reader but also makes evident the thread that ties the different mathematical languages together:

> *Grâce à ce modèle, la vérité des propositions se passe de l'appel constant aux figures, ainsi qu'à l'imagination des constructions auxiliaires. Plus importante encore, ce commentaire fait apparaître des liaisons entre les propositions, invisibles à la pure géométrie, et met en évidence des idées majeures qu'on ne pouvait saisir par la démonstration géométrique—ainsi les idées qui président à l'étude de la variation. Cette fois encore, et comme tous le géants qui jalonnent l'histoire des*

[10] Here I believe there is a typographical error because what is written is d_0'.

> *mathématiques, Apollonius n'œuvre pas seulement dans le présent, mais dans le futur mathématique, avec les moyens du présent. Situation éminemment féconde et extrêmement subtile, qui exige pour être comprise qui soient multipliés les commentaires.* [348]

This is precisely where the fascination of this edition lies: it unites philological rigor with a panoramic point of view which comprises successive developments of Apollonius' ideas without leading to anachronistic flights of fancy that depict mathematicians of classical antiquity as improbable precursors of modernity, but which highlights the thread of continuity that makes the history of mathematics a description of a fascinating adventure in search of those 'hidden harmonies' ('riposte armonie') of which Federigo Enriques spoke so convincingly.

BIBLIOGRAPHY

Brigaglia, A. 1997. 'La ricostruzione dei libri V e VI delle *Coniche* da parte di F. Maurolico'. *Bollettino di Storia delle Scienze Matematiche* 17:267–307.

Ecchellensis, A. (Ibrahim al-Haqilani). 1661. *Apollonius de Perga. Conicorum libri V, VI, VII, paraphraste Abalphato Asphahanensi, nunc primum editi.* Florence.

Fried, M. and Unguru, S. 2001. *Apollonius of Perga's* Conica*: Text, Context, Subtext.* Leiden.

Halley, E. 1710. *Apollonii Pergaei* Conicorum *libri octo et Sereni Antissensis* De sectione cylindri et coni libri duo. Oxford.

Heath, T. L. 1896. *Apollonius: Treatise on Conic Sections.* Cambridge.

________ 1897. *The Works of Archimedes.* Cambridge.

________ 1956 trans. *The Thirteen Books of the* Elements. Mineloa, NY.

Rashed, R. 2000. *Les mathématiques infinitésimal du IXe au XIe siècle: 3. L'achèvement de l'ouvrage des* Coniques. London.

Toomer, G. T. 1990. *Apollonius:* Conics *Books V to VII.* Heidelberg.

ver Eecke, P. 1923. Les Coniques *d'Apollonius de Pergé.* Bruges. Repr. 1959, Paris.

Zeuthen, H. G. 1886. *Die Lehre von Kegelschnitten im Alterturm.* Copenhagen.

Catalogue of the Exhibition 'Leonardo da Vinci: Anatomist' by Martin Clayton and Ron Philo

London: Royal Collection Publications, 2012. Pp. 260. 126 color illust. ISBN 978–1905686–39–1. Cloth £19.95/$25.00. Available as an iPad app (Touch Press) from Apple's iTunes Store, £9.99/$13.99

Reviewed by
N. D. Hodson
Durham University
n.d.hodson@durham.ac.uk

Visitors to the National Gallery's recent sell-out show, 'Leonardo da Vinci: Painter at the Court of Milan', were invited by director Nicholas Penny to 'suspend much of their curiosity concerning Leonardo as a "scientist" in order to focus on his achievements as a painter' [Syson 2011, 9]. However, trying to divide Leonardo's activities like this will always be fruitless, as he carried the same multifarious intellectual acuity into everything he did.

This is certainly true of his anatomical studies. In the current exhibition at the Queen's Gallery, Buckingham Palace, 'Leonardo da Vinci: Anatomist',[1] we find an anatomist whose achievements are the result of the same skills of observation and representation for which he was celebrated as a painter. All the works in the show are drawn from the holdings of the Royal Collection. And though displays of Leonardo's drawings from the rich collection at Windsor are not uncommon—indeed several of these drawings were featured in the National Gallery's show—this represents the most comprehensive exhibition to date of Leonardo's studies in anatomy, a field of inquiry which dominated his intellectual activity in the early 16th century.

'Leonardo da Vinci: Anatomist' focuses on the same period as the National Gallery show, around 1480 to 1514, much of which time Leonardo spent in Milan. Like the National Gallery exhibition, it poses a difficult task for its curator. Leonardo is so well studied and

[1] The Queen's Gallery, Buckingham Palace, London, 4th May to 7th October 2012.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 261–267

his drawings—even some of his anatomical studies—so well known, that the exhibition must aim for something more than simply comprehensiveness. Nonetheless, the sheer scale of the show is worthy of note: 87 pages from Leonardo's notebooks are on display, 24 sides of which have not previously been exhibited. Although the density of material on show makes the exhibition feel something akin to a day in an archive, browsing the walls of the gallery manages to be almost as absorbing as leafing through Leonardo's notebooks. Indeed, this is the real thrill of this exhibition: through immersing ourselves in his working notes, we learn much about Leonardo's mind and how he directed its energies. His ambitions were lofty: from planning to write a study on the body as part of a larger treatise on painting, Leonardo's interest in human anatomy gradually gained its own momentum and became a treatise project in its own right. Initially working from human bones and dissecting animals, Leonardo progressed to observing and undertaking human dissections, collaborating in his research with the anatomist Marcantonio della Torre. In this respect, the exhibition goes beyond an overview of Leonardo's anatomical career, as it traces both the development of his scientific knowledge and the progress of his anatomical thesis.

At the heart of the exhibition are two important staging posts in Leonardo's anatomical studies, known as Anatomical Manuscripts A and B. The earlier, Manuscript B, documents Leonardo's comprehensive dissection of a centenarian in around 1507:

> This old man, a few hours before his death, told me that he was over a hundred years old, and that he felt nothing wrong with his body other than weakness. And thus, while sitting on a bed in the hospital of Santa Maria Nuova in Florence, without any movement or other sign of mishap, he passed from this life. And I dissected him to see the cause of so sweet a death. [17]

Filling the pages of his notebook with drawings, diagrams and descriptions, Leonardo recorded with painstaking accuracy the man's muscular, nervous, and cardiovascular systems, including one of the earliest descriptions of a cirrhotic liver:

> The liver is desiccated and becomes like congealed bran broth in color and substance, so that when but a little friction is

> made on it this substance falls away in minute particles like sawdust. [82]

Although Leonardo's understanding of his dissections was limited, Clayton rightly draws our attention to a drawing in which he attempts, ambitiously, to synthesize his knowledge into a single diagram of the cardiovascular system and principal organs of a woman [see Figure 1]. Despite several omissions and flaws, this is a revolutionary drawing of the human body, immediately reminiscent of modern anatomical diagrams.

As if to underline the modernity of Leonardo's approach, the exhibition also includes a number of modern medical models offering direct comparisons with the drawings and demonstrating the accuracy of Leonardo's observations. As the exhibition goes on to make clear, this strong feeling of familiarity and recognizability is a red herring: Leonardo's anatomical treatise never came to fruition and his drawings languished unknown for centuries.

After Leonardo died in 1519, his manuscripts passed to his pupil Francesco Melzi and were later sold to the sculptor Pompeo Leoni, who had the notebooks and drawings bound into several large albums. One of these albums, which contained almost all of Leonardo's anatomical drawings, made its way into the collection of Charles II and makes up a large proportion of the Windsor collection. The drawings were removed from the album in the 19th century and are now mounted individually; but the empty binding of the album is included in the Queen's Gallery display, underlining the significance of the drawings' history. Hidden between the covers of this book, Leonardo's anatomical studies garnered little attention before they were studied by the physician William Hunter in 1773 [Clayton and Philo 2010, 208]. It would, nonetheless, be another century before an edition of Leonardo's anatomical works was published.

The studies which make up Anatomical Manuscript A (compiled in the space of a few months in 1510–1511) provide our only real glimpse of Leonardo's conceived anatomical treatise and make up some 24 heavily-annotated illustrations in the catalogue. The drawings begin with the superficial anatomy of the body drawn from a live subject, before moving on to detailed analysis and classification of the muscles, then bones, that are informed by Leonardo's growing knowledge gained from dissection. Leonardo goes beyond simply

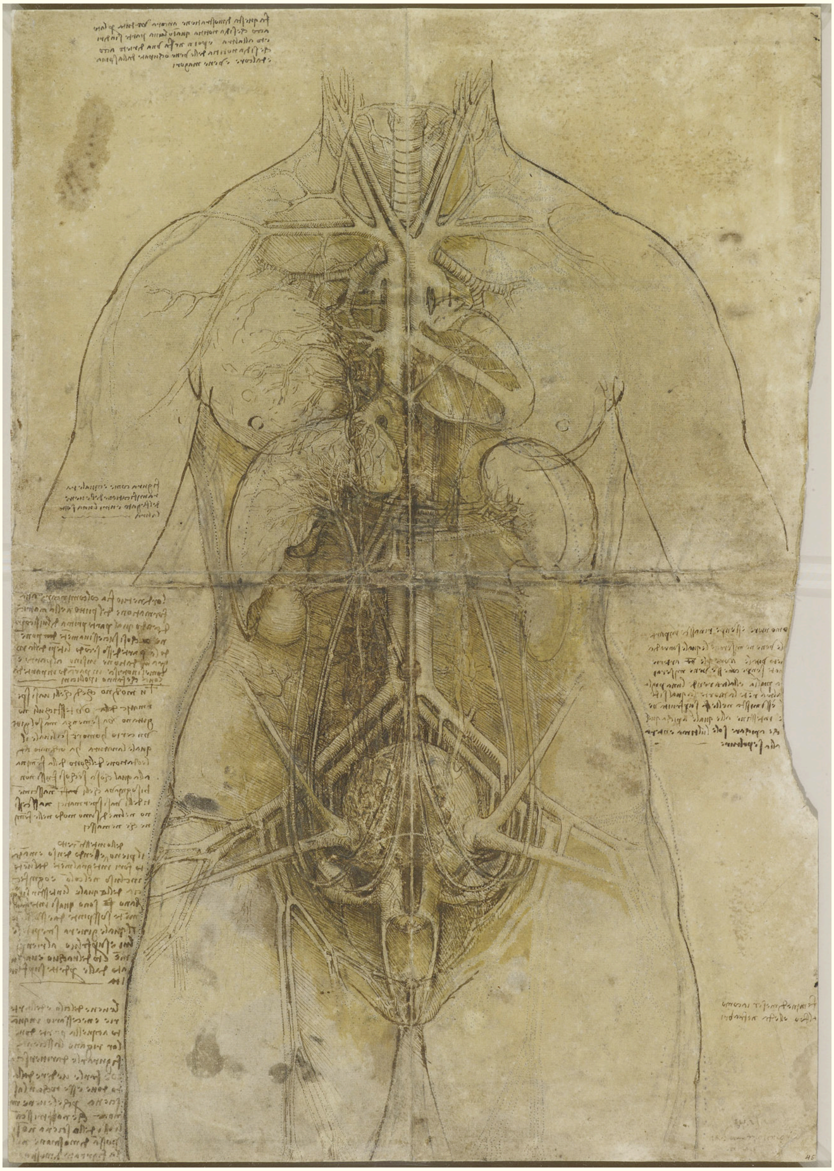


Figure 1. Leonardo da Vinci: The cardiovascular system and principal organs of a woman. Black and red chalk, ink, yellow wash on paper, *ca* 1509–1511

(Royal Collection Trust © 2012, Her Majesty Queen Elizabeth II) delineating the anatomy of the body, his notes by this stage filling every blank space and offering reflections on the identity, structure, and function of every muscle and bone. But it is Leonardo the artist whose ingenuity remains most fascinating: his drawings dismantle the body with a range of views and overlays surely impossible in the dissection room. Indeed, the question of how exactly Leonardo went about making such densely detailed drawings inside (or, more likely, outside) the dissection room remains a perplexing one. The skills earlier used in his compound view of a woman's cardiovascular system are well honed: many of the sheets employ a range of 'thread diagrams' (to employ Clayton's term) that successfully articulate the layers which make up the structure of the body. The anatomical inaccuracies and speculations in these studies, carefully noted throughout the catalogue, suggest that Leonardo worked as much from his own (informed) conception of the body as he did from observation.

Leonardo's drawings and investigations ended abruptly, and inconclusively, with his departure for Rome in 1513. The exhibition reflects this sudden suspension of activity, ending similarly abruptly with a closing group of studies of the heart. These studies follow on from Manuscript A and Leonardo's collaboration with the anatomist Marcantonio della Torre at the University of Pavia in 1510–1511, the period in which he had the greatest access to dissections. Leonardo's depictions of muscles from that period are amongst his most detailed and most accurate. His subsequent studies of the heart perhaps mark the apex of his anatomical career, providing the first descriptions of the atria and the blood flow through the aortic valve, although they also reveal his struggle to comprehend the results of his research; an understanding of the circulation of blood remained elusive. As Clayton reflects in the catalogue:

> There is a pervading sense in Leonardo's notes on the heart, running to many thousands of words, that he could go no further. Faced with an impasse between his physical understanding and the accepted physiology of the heart, he was doomed to keep on describing the motion of the blood through the valves in ever more detail. And there, apparently, his anatomical work came to an end. [24]

It is to the catalogue's great advantage that it is the product of a collaboration between a curator and a professor of anatomy;

engaging with Leonardo's work is always challenging and it is only expert guidance—in the form of notably detailed and explicatory catalogue entries—that prevents his anatomical studies from being overwhelming.

This is a great achievement; there is no escaping the fact that we are looking at Leonardo's drawings on a scale and with a focus usually reserved for scholars. In this respect, the show is timely: in the wake of the National Gallery exhibition, Leonardo is very much in the public consciousness and little introduction is necessary. Like the National Gallery exhibition, however, putting Leonardo in context remains a difficult proposition, and the nature of his collaboration with Marcantonio della Torre remains as unclear as his relationships with those contemporary Milanese painters who figured so strongly in 'Painter at the Court of Milan'.

Another important feature of the Queen's Gallery display is the use of a number of double-sided frames, which allow both sides of particularly interesting sheets to be seen, although it remains hard to escape the desire for a table, chair, and magnifying glass when studying such demanding drawings. It is for this reason that the very comprehensive exhibition catalogue and iPad app are so useful and deserve to be recommended highly. The well-produced catalogue follows the chronological structure of the exhibition, and includes high-quality color illustrations of every work exhibited. A number of photographs taken under ultraviolet light allow some additional detail to be seen on the more faded sheets, and a readable introduction gives a good overview of Leonardo's anatomical work and its significance. With such a great deal of material in the exhibition, the catalogue remains—perhaps understandably—a little light on context. This is best evidenced by an underdeveloped section on Leonardo's preparatory studies for the mural 'The Battle of Anghiari' commissioned in 1503 for the Palazzo della Signoria in Florence. Though the accuracy of musculature on the nude figures Leonardo sketched for the mural do owe something to his understanding of anatomy, the relation between his anatomical and artistic practice still needs greater treatment. The iPad app is of particular benefit, as it allows the user to browse the drawings with overlaid translations of Leonardo's dense textual notes.

Both catalogue and app are as well realized as the exhibition itself and underline the fact that this show—in many ways an elaboration of two earlier exhibitions of Leonardo's anatomical works curated by Martin Clayton[2] —is the result of decades of research. As an exploration of the unique scale of Leonardo's scholarly output, 'Leonardo da Vinci: Anatomist' offers the most detailed insight into a great Renaissance mind yet assembled.

BIBLIOGRAPHY

Clayton, M. and Philo, R. 2010. edd. *Leonardo da Vinci: The Mechanics of Man.* London.

Syson, L. 2011. ed. *Leonardo: Painter at the Court of Milan.* London.

[2] Namely, 'Leonardo da Vinci: The Anatomy of Man', Museum of Fine Arts, Houston, 1992 (this exhibition also travelled to Philadelphia and Boston that year and to Tokyo and Nagoya in 1995), and 'Leonardo da Vinci: The Mechanics of Man', Vancouver Art Gallery, Vancouver, 2010.

Arithmetic in Sixteenth-Century Muscovy by Mark A. Tsayger

Beersheba: Berill, 2010. Pp. 72. ISBN 987–965–555–455–7. Paper

Reviewed by
Rem Aleksandrovich Simonov
Moscow State University of Printing Arts
kafedra_kb@mail.ru

Translated by
Michael Weiss[1]
New York University
mlw369@nyu.edu

Arithmetic in Sixteenth-Century Muscovy by Mark Tsayger is dedicated to an important, complicated, and open research topic in the history of Russian mathematics—the *soshny* fractions.[2] I recall how, at one of the meetings of the Seminar on the History of Mathematics and Mechanics at Moscow State University, one well-known and respected scholar, an expert in this area and one of the presenters at the seminar, Professor Adolf Yushkevich (1906–1993), observed that the system of fractions obtained by successive divisions by two of fourths (*chets*) and thirds (*trets*) merits its own special research. Thus, it is without a doubt that Tsayger's recent work into this area is of substantial interest.

Yushkevich, in his fundamental book on the history of mathematics in Russia [1968, 16], wrote:

[1] This review was originally published in Russian in *Mathematics in Higher Education* 8 (2010) 135–142, a publication of Nizhny Novgorod University of the Russian Federation.

The translator acknowledges the support of the Algebraic Geometry Laboratory GU-HSE grant RF Government ag. 11 11.G34.31.0023 during the preparation of this translation.

[2] MW: the adjective 'soshny' in Russian refers to the tax unit, the *sokha*, corresponding to a variable amount of tilled land in 16th–century Moscow. In the existing literature, 'sokha' has been translated as 'plough'.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 268–279

> These two kinds of fractions played a role in the collection of taxes and constituted an important part of the *soshnoe pis'mo* (tillage accounting), which was the system describing the totality of methods for collecting taxes on parcels of land in the 16th and 17th centuries.

Tsayger's research on this topic is undoubtedly a significant milestone. One of the book's positive characteristics is the author's attempt to relate its subject matter partially to Old Russian mathematical culture. To this end, the book starts with a description of the archaic 'alphabetical' numerals, which remained in use in Rus' until the 18th century, when they were replaced with the modern Hindu-Arabic numeral system under one of Peter the Great's reforms. (As it is generally known, the latter form of numeration had already entered Russian life by the 17th century, chiefly through the handwritten version of *Numeral Calculating Wisdom* (*Tsifirnoi schetnoi mudrosti*), a manual on calculation arithmetic.)

Tsayger also discusses the original Russian method of denoting numbers 'in grids' ('v reshyotkakh'). This method was first elucidated in the historiography by the famous Russian scholar and well-known church figure, Metropolitan Evgeny Bolkhovitinov, in the first Russian-authored work on the history of mathematics [1813]. The method of denoting numbers 'in grids' has not been sufficiently researched. Indeed, discussion of this method of representing numbers is absent in the aforementioned book of Yushkevich, in the well-known mathematical history by B. V. Gnedenko [1946], and in the four-part *The History of Russian Mathematics* [Shtokalo 1966–1970]. This absence increases the value of Tsayger's book, in which the grid notation is considered in the requisite detail necessary for analysis of the sources regarding the *soshnoe pis'mo*.

The main sources of Tsayger's research go beyond the 'Books of Tillage Accounting' ('Knigi soshonogo pis'mo'), used during the 17th and 18th centuries, and also include the aforementioned *Numeral Calculating Wisdom* and *Arithmetic* (*Arithmetica*). These manuals have survived as handwritten texts and each copy is unique. In a number of them, there is a special section devoted to the so-called *doschany schot* (board abacus), a precursor to the well-known Russian/clerical abacus or *schoty*.[3] The principles of constructing a *doschany schot* were described and researched by the famous historian-numismatist Ivan Spassky [1952].

The first *doschany schot* was a rather cumbersome instrument comprised of four counting fields. These counting fields were separated by wooden frames, to which were fastened rods or cords lined with counting beads for the calculation of monetary and fiscal sums and measurements involving volumes and weights. As opposed to the modern *schoty*, the *doschany schot* had lower counting fields made up of rods or cords for calculations involving quarters and thirds along with their binary divisions.[4] In spite of the attention many historians of science have given to the *doschany schot*, it still has not been fully studied.

To a certain degree, Boris Gnedenko's comment back in 1946 is reasonably fair: 'By all appearances, the explanations of the uses of the *doschaty schot*[5] have been lost; only rather unclear descriptions of its implements survive' [1946, 48]. Thus, Tsayger's attempt to penetrate the secrets of the *doschany schot* merits the attention of historical-mathematical science. It is symptomatic that the author himself modestly believes that he has not succeeded in illuminating all of these secrets: he writes,

> Some special letter combinations in these schemes have up until now not lent themselves to deciphering.... Many of the topics which we have discussed are more assumption than demonstrated facts. [68]

Nonetheless, Tsayger's method of analysis is quite scientific and well deserving of further development and use by other researchers.

---

[3] MW: the Russian abacus is known both in the Russian and English literature as a *schoty*. It will be referred to as a *schot* when referencing the historical instrument and as a *schoty* when referencing the modern abacus used widely in Russia. In anticipation of the reoccurrence of these terms, it may be useful to provide their definitions here: 'doschany schot' designates a board abacus; 'dschitsi schotnie', auxiliary calculating tablets; and 'schot kost'mi', the abacus of beads (or loose abacus).

[4] MW: that is, in the case of thirds: sixths, twelfths, twenty-fourths, and so on.

[5] This term is used by Gnedenko for the method of calculation that we are currently examining, 'doschaty schot' (instead of 'doschany schot').

The reason for this lies in the fact that Tsayger 'takes a stab'[6] at explaining the arithmetical 'technology' in Russian governmental records and *fisc* or treasury holdings in the 16th century; there are essentially no direct resources about this calculating technology. For this reason, he uses a method of reconstruction that accepts as a basis the so-called *dschitsi schotnie* (auxiliary calculating tablets), which are found in Russian mathematical manuscripts from the 17th century (in even earlier sources they are completely absent). Historians of mathematics have analyzed the *dschitsi schotnie* before Tsayger. Indeed, the four-volume *The History of Russian Mathematics* interprets the *dschitsi schotnie* in the following manner:

> In some of the manuscripts from the 17th century, sketches are found with depictions of the *dschitsi schotnie*, which appear to be variations of the *doschany schot* of the 17th century.

Consequently, in that publication, *dschitsi schotnie* and the *doschany schot* are treated as interconnected but different mathematical phenomena. This relationship is also confirmed in Tsayger's book with his description of the distinction between the *dschitsi schotnie* and the *doschany schot*:

> *Dschitsi schotnie* differ from the *doschany schot* only in that they consist of counting fields divided into 13 or 12 straight lines, from which the six lower ones are divided in half. [Shtokalo 1966–1970, 115]

Spassky considered the *dschitsi schotnie* to be a prototype of the *doschany schot*. This follows from the caption that he placed with the depiction of the *dschitsi schotnie* taken from a Russian mathematical manuscript of the 17th century:

> Draft of the *doschany schot* in the handwritten *Arithmetic* from the middle of the 17th century. [1970, 124]

Spassky suggested that in the 16th and 17th centuries the *doschany schot* coexisted with the more archaic Russian *schot*—the *schot kost'mi* (abacus of beads or loose abacus), which did not have frames and whose beads were not threaded but loose. He believed that the

[6] MW: I have retained here, and throughout, Simonov's quotes that appear in the original review. In this instance, Simonov uses the verb 'zamaxnut'sya', which evokes his sense of the bold character of Tsayger's interpretive work.

final step in the transition to the *doschany schot* in Russia took place before the middle of the 17th century:

> But sometime before the middle of the 17th century, the *doschany schot* prevailed and became universal and widespread throughout the entire territory of the Russian state. [1970, 123]

Tsayger characterizes the *dschitsi schotnie* in the following way:

> Now we can answer the question 'What is a *dschitsa schotnaya*?'. It is a scheme painted on an auxiliary tablet depicting the way in which to mark a table for a *schot kost'mi*.... In the 16th century, these auxiliary tablets were indispensable for the persons performing calculations, preventing them from making mistakes when transferring the result of the calculation to paper in Slavonic numerals. Evidently, even later in the 17th century when many chalk-lined accounting tables were replaced by the *doschany schot*, and Slavonic numerals were replaced by Arabic numerals, the need for *dschitsi schotnie* did not immediately decline. [42]

Therefore, Tsayger, unlike Spassky, does not consider the *dschitsi schotnie* to be drafts of the *doschany schot* but rather auxiliary calculating references used with a *schot kost'mi*, which itself preceded calculating instruments (*schoty*) taking the name 'doschany schot'.

Tsayger's opinion has something in common with the point of view expressed in the multi-volume *The History of Russian Mathematics* [Shtokalo 1966–1970] that the *dschitsi schotnie* and *doschany schot* are alike but represent different methods of calculation. Moreover, the 1966 edition does not identify concretely to which method of calculation the *dschitsi schotnie* correspond. By the way, here there is no mention of the *schot kost'mi*: only the Western European 'line abacus' ('schot na liniyax') is considered, which is associated with the *schot kost'mi ili penyazi* (counter of beads or money) of the Russian mathematical manuscripts of the 17th and 18th centuries. (Actually, in the aforementioned title, there is a discussion of the 'line abacus'. However, Spassky believed that only the last part of the name ('schot...penyazi') corresponded to it, while the beginning part ('schot kost'mi') pertained to the original Russian *schot* in distinction from the line abacus.)

The history of the *doschany schot* is not clear. All the more interesting then, is Tsayger's attempt to investigate it. Foreigners who were living in or visiting Russia in the 16th and 17th centuries mentioned Russian calculators using the pits of fruits (plum and cherry) for counting rather than an instrument with wooden bars and threaded beads, that is, they were not using a *doschany schot*. To a certain extent, this contradicts Spassky's opinion that 'by the 17th century, the *doschany schot* (*schoty*) took over and became widespread throughout the entire territory of the Russian state.' If this were the case, the *doschany schot* should have had a certain degree of prevalence in Russia in the 16th century and the first half of the 17th century, and it is unclear how foreign observers did not notice it. Everything fits however, if we assume that foreigners observed a different kind of Russian *schot*—the *schot kost'mi*—to the extent that the *doschany schot* still was not in mass usage, and maybe it had such a limited distribution it was as if it had not existed at all.

Tsayger's idea that the *dschitsi schotnie* reflect the *schot kost'mi*, which itself preceded the *doschany schot*, may prove fruitful to mathematical history since there are really no other sources about the *schot kost'mi*. Almost every depiction of the *dschitsi schotnie*, of which there are many in Russian mathematical manuscripts, adds something to our understanding of the object and the individual calculating characteristics of the *schot kost'mi*.

Firstly, these manuscripts indicate that the *schot kost'mi* was constructed in a decimal system. We can conclude this because in the sketches of the *dschitsi schotnie* the counting pieces (beads) are depicted in quantities of 10s (rarely in nines) on each complete calculation level. Secondly, in this kind of *schot*, beads were used unthreaded or loose. Thus, they were drawn, as a rule, lying on the lines of the *schot* and not threaded through them. True, occasionally but rarely, one finds depictions with threaded beads, which could say something about the influence of the *doschany schot*. Thirdly, in some *dschitsi schotnie*, archaic 'alphabetical' numerals are used and in others, modern (Hindu-Arabic) numerals. This shows that *schot kost'mi* may date back to the 16th century, when the modern system of writing numbers began to take the place of the 'alphabetical' numeration. Fourthly, the lower portion of the *dschitsi schotnie* was divided into two parts for fourths and thirds, which were constructed by binary principles. This speaks to the fact that the corresponding binary fractions had entered the *schot kost'mi*.

Not long ago, discoveries were produced (about which Tsayger could not have known before the time of this book review) showing that binary fractions with a basis of half fourths and half thirds were apparently already in use in Rus' in the 16th century. This is evidenced in the deciphering of an Old Russian text (from 16th-century records), in which the corresponding system of fractions by halves was used with fractional divisions of time: half quarter past 5, one and a half quarter past 8, one and a half quarter till 11, a half quarter past half till 11, half minus half a quarter till 2, and so on [Simonov 2009, 106–108]. Indirectly, this fact supports Tsayger's hypothesis that the *dschitsi schotnie* date back to the 16th century. They might even date back to the border between the 15th and 16th centuries, if we factor in the date of 1495, which is assigned to the convoy of some of the texts which accompanied the Russian calculations of fractions of the hours [Simonov 2009, 108].

The uncovering of the specifics of Old Russian counting in the 'soshny fractions' system is important and substantial material in Tsayger's book. Spassky [1970, 123] wrote the following about this problem:

> There were special conversion tables in the instructions for Russian 'accountants', which allowed them to bring fractions of either base [RS: fourths and thirds] to a common denominator. It is remarkable how this monetary counting served them: it appears that one can express any fraction of either kind in the form of a monetary sum, after which adding or subtracting thirds and fourths is as easy as can be.

Those auxiliary resources, which Spassky calls 'special conversion tables', can produce case-specific formulas. Tsayger reproduces one of these formulas, which in the language of Old Russian *soshny* fractions sounds like this:

> A *chet'* [fourth] and a half *chet'* and a half-half *tret'* and a half-half-half *tret'* [third], sums to a *tret'* and a half *tret'* of a *sokha*.

In modern fractional notation, it can be expressed in the following equality:

$$\tfrac{1}{4} + \tfrac{1}{8} + \tfrac{1}{12} + \tfrac{1}{24} = \tfrac{1}{3} + \tfrac{1}{6}.$$

The purpose of the equality is the conversion of one *soshny* fraction into another, which was needed for the rationalization of calculations associated with the collection of tax on holdings. These holdings consisted of privately owned plots of land of varying sizes and values (ploughed fields, woods, hayfields, and so forth.) With this, the problem of whether the calculations were correct was also solved, and it was for this reason that a fast mechanism existed to verify them.

For example, the so-called 'Moscow counting' ('Moskovsky schot'), which Tsayger describes in his book, used such a verification. At the foundation of this method lies the definition of the calculating unit, the 8 *altyn*,[7] equaling 48 *dengas*. The author renders its meaning as such:

> To the extent that 8 *altyns* contained 48 *dengas*, the significance of thirds and fourths would seem to increases 48 times. The result is that the fractional summands seem to transform into whole numbers, with which the service class (*sluzhilie lyudi*) knew how to operate. After receiving the final result, it turns back into a fraction by that very same principle (i.e., out of the identity that one equals 8 *altyns*). [48]

To check the reproduced equation (assertion) we need to replace the one in the numerator with 8 (*altyns*) or 48 (*dengas*), put it in terms of uniform monetary units, and check the arithmetic of the equation. In this case, following Tsayger's calculations, we get:

In the left hand side of the statement:
quarter $= 2\,altyns$
half quarter $= 1\,altyn$
half-half third $= 4\,dengas$
half-half-half third $= 2\,dengas.$

$$2\,altyns + 1\,altyn + 4\,dengas + 2\,dengas = 3\,altyns + 6\,dengas = 4\,altyns.$$

[7] MW: the *altyn* and *denga* (plural in Russian *dengi*) were Russian monetary denominations used widely before, and in the case of the *altyn* into, the Soviet period. 'Dengi' is now the Russian word for money, while the meaning of the word 'altyn' has been the subject of speculation by academics, some suggesting that it comes from the Tatar word for 6.

> In the right hand side of the statement:
> third = $2\,altyns\,4\,degas$
> half third = $1\,altyn\,2\,dengas$.
>
> $$2\,altyns + 4\,dengas + 1\,altyn 2\,dengas =$$
> $$3\,altyns + 6\,dengas = 4\,altyns.$$
>
> The left hand side equals the right, i.e., the statement is correct. [52]

Actually it was a very simple method of verification and not borrowed at that but rather developed from an original Old Russian financial basis.

It seems that the designation of the method as 'Moscow' allows it to be dated. Divide 48 by 8, and we receive 6; this means that 1 *altyn* equaled 6 *dengas*. From the *Dictionary of Numismatics*, one can find that the *altyn* most likely had the above meaning only after the 14th century:

> *Altyn*, a Russian countable-monetary unit of the 14th century, equaling 6 *dengas*, later 3 kopecks. [Fengler, Gierow, and Unger 1993, 12]

The history of money circulation in Rus' shows that this data is not always absolutely exact. In actuality, in the last decade of the 14th to the first half of the 15th century, the *altyn* in Moscow equaled 3 *dengas*. Moreover, the minting of coins in Moscow and other Russian principalities was not unified. Such an unfavorable state for the development of the Russian economy ended partially, when, in 1420 in Novgorod, the Moscow norm of minting was accepted. After that, accounting was divided between the Novgorod and Moscow styles, a division that took place during the final years of the reign of the Grand Prince of Moscow, Vasily the Blind (1415–1462). The Novgorod *denga* retained its weight, adopted in 1420, while the Moscow *denga* became equal to half a Novgorod *denga* [Yanin 1970]. It follows, that only from this time the relationship 1 *altyn* = 6 *dengas* appeared, which had its primary use for the lighter Moscow *dengas*. It was named 'moskovka':

> 'Moskovka', beginning with the 16th century—name for the Moscow *denga*, which, although minted to a modest degree

> during the 16th and 17th centuries, was mentioned in a majority of commerce-related acts from that period. [Fengler, Gierow, and Unger 1993, 208].

It is possible that the name of the accounting as 'Moscow' follows from the connection with the *moskovka* currency, in which case the appearance and distribution of the 'Moscow counting' would date back to the 16th and 17th centuries.

There is some basis for more precisely defining the wide period of the first appearance of the 'Moscow counting'. The importance lies in that the *novgorodka*, a type of *denga* minted in Novgorod after 1534, changed its name and place of distribution: it became known as the kopeck and started being minted and used in Moscow:

> Kopeck, Russian silver coin, minted starting in 1534; its weight equaled the weight of a Novgorod *denga*, or *novgorodka*, which came into use in Moscow after the conquering of Novgorod by Ivan III (1462–1505) in 1478. [Fengler, Gierow, and Unger 1993, 141: cf. Spassky 1970, 111–113]

Consequently, the period between the last years of the reign of Vasily II, approximately 1462, and 1534, the year of the establishment of one united monetary system for the Russian state, was a much more convenient and suitable time for the appearance of the 'Moscow counting' (the name of which was formed from 'moskovka'). This was because, after 1534, the economical and political reasons for the division between Moscow and Novgorod minting systems had already disappeared, although the names of obsolescent monetary synonyms could have remained in usage for a long time.

Thus, there is some foundation to consider that the 'Moscow counting' could have appeared in the last decades of the 15th and early decades of the 16th centuries. I went into such extensive detail about the dating of the 'Moscow counting' because it is impossible to rule out the origins of the *schot kost'mi* (in the version which Tsayger reconstructs for the 16th century) from the 15th century. This has an important meaning for Tsayger's basic argument for the legitimacy of his reconstruction of the arithmetic knowledge of Muscovy in the 16th century based on sources (*dschitsi schotnie*, etc.) preserved in the records of a later period, the 17th and 18th centuries.

Of the many different arithmetic methods of 16th-century Russia reconstructed by Tsayger with which one may become directly acquainted by his book, I would like to touch upon the question related to approximations. The author, having dug deeper into the calculating material, noticed something remarkable in it: Old Russian mathematicians dealing with values less than $\frac{1}{48}$, 'simply threw them out, assuming that taking them into account would have no effect on the result' [57]. That being said, it is classified as a defect of the method ('the arithmetic had one disadvantage' [57]). Here our respected author acts as an expert who evaluates the phenomenon by its mathematical merits and not by the historical context of its emergence and functioning. Taking into account that the discussion is about a special arithmetic—the *soshny* fractions—it follows that attention should be given to the historical economic side of that arithmetic. The *soshnoe pis'mo* was intended for the realization of the governmental fiscal project, the Bol'shaya Sokha (the Great Plough) carried out and developed practically 'from scratch' by Ivan the Terrible in the middle of the 16th century regarding the levying of tax on huge land holdings of Russia. The task of collecting everything up to the last kopeck would require enormous expenses of resources and time for the education (which would have had to include mathematical training) of a huge army of tax collectors and for the training and maintenance of a security force for their protection and the wresting out of debts. With such a perspective, the activity of the financial service would get so bogged down in problems and stretch out into such long years that it would never reach its desired purpose.

Under these conditions it was better to view the task of realizing the Bol'shaya Sokha project as an optimization problem: 'How to reach the maximum potential tax revenue with the minimum costs?' *A priori*, for example, the problem could be solved by the development of simple methods for the approximate calculations of tax levies, whereby weakening the accuracy of the estimate would save one time and expenditure of mental energy. Alongside this should have been the ability to verify the calculations quickly and simply so as to reduce the number of taxpayer appeals and complaints about the dishonesty of the accountants and clerks. Perhaps Tsayger's reconstruction of the specifics of Russian *soshny* arithmetic from the 16th century is valuable, most of all, in that it answers this important question.

BIBLIOGRAPHY

Bolkhovitinov, E. 1813. 'O starinnoi slavyano-russkoi arifmetike'. *Vestnik Evropy* 71.17:47–54.

Fengler, H.; Gierow, G.; and Unger, W. 1993. edd. *Slovar' Numismata.* 2nd edn. Moscow. Russian trans. by V. M. Potin.

Gnedenko, B. V. 1946. *Ocherki po istorii matematiki v Rossii.* Preface and commentary S. S. Demidov. Moscow. 2nd edn. 2005.

Shtokalo, I. Z. 1966–1970. ed. *Istoria Otchesvennoi Matematiki.* Kiev.

Simonov, R. A. 2009. '[O chasekh] po-ruski zh v Novgorode v Velikom'. Drevnyaya Rus'. *Voprosi mediyevistiki* 3.37:106–108.

Spassky, I. G. 1952. 'Proiskhozhdenie i istoriya russkikh schotov'. *Istoriko-matematicheskie issledovaniya* 5:269–420.

__________ 1970. *Russkaya monetnaya sistema.* 4th edn. Leningrad.

Yanin, V. L. 1970. 'Den'gi i denezhnie sistemy'. *Ocherki russkoi kul'turi XIII–XV vekov. Part 1. Material'naya kul'tura*: 336–341.

Yushkevich, A. P. 1968. *Istoriya matematiki v Rossii do 1917 goda.* Moscow.

The Invention of Discovery, 1500–1700 edited by James Dougal Fleming

Farnham, UK/Burlington, VT: Ashgate, 2011. Pp. x + 217. ISBN 978–0–7546–6841–1. Cloth £55.00, $99.95

Reviewed by
Sean Roberts
University of Southern California
seanrobe@usc.edu

For historians of science, the age of discovery seems to have receded into the shadows, replaced by an early modern period pulsing with inventions and inventors, a landscape in which the social and cultural construction of newness in the world loomed large for natural philosophy, technology, and even the arts. The inevitable quotation marks surrounding the 'discovery' of the Americas are only the most visible (and surely the most justified) signal of this shift. Coincident with this now entrenched recognition of discovery's cultural chauvinism has been a growing awareness of invention's purchase as a rhetorical model and practical goal for early modern philosophers, historians, poets, painters, and travelers alike. The boundary between that which awaited unveiling and that which must be fabricated or conjured, a divide held fast at least in the modern vernacular imagination, appears in early modern Europe remarkably slippery and permeable. Given how firmly this revisionist conception has taken hold (for academics, if not for a larger reading public whose interest in feats of discovery remains unquenched), its own reappraisal is both welcome and overdue. James Dougal Fleming's *The Invention of Discovery* aims to problematize the hegemony of the invention paradigm not only within the history of science but across a wide swath of early modern disciplines and activities.

Fleming's volume revisits these entwined concepts with an observation and an ambitious structuring challenge. The rubric of discovery which we have treated with great scepticism was both prevalent and unshakeable in early modern Europe (as prevalent, indeed, as that of invention). In light of this fact, Fleming asks his contributors to

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 280–289

turn their attention to the ways in which the very concept of discovery was itself one of early modern Europe's most potent inventions. More than just a witty turn of phrase, 'the invention of discovery' represents a recognition of the rubric's conceptual emergence and its rapid acquisition of tenacious strength in early science. Crucially, Fleming's introduction and afterword insist that this is not merely a historicist challenge (or at least not a problem that pure historicism can solve) but is also hermeneutic in nature. For the editor,

> the issue before us is not just how to understand a given historical object...but how to understand, *kurz*. [186]

The history of early modern science is, for Fleming, a hermeneutic project not least because we seek precisely to understand models of understanding.

In reevaluating these hermeneutic alternatives, the volume's 11 essays provide fruitful rumination on discoveries and inventions in a variety of fields and in a number of distinct early modern moments. Topics discussed range from the pure philosophy of Francis Bacon to the poetics of John Donne and from the introduction of the printing press in France to the staging of John Fletcher's *Knight of Malta* (1617). The chronological and geographic sweep of these essays, taking in Quattrocento Florence, Elizabethan England, and even colonial North America, amply justify the broad appellation of 'early modern' and this breadth exemplifies the spirit of Ashgate's series, Literary and Scientific Cultures of Early Modernity.

The Invention of Discovery's strongest essays shed new light on components of early modern knowledge formation that we often think we understand well, restaging acts of discovery and invention for contemporary readers. Fleming's own contribution, 'The Undiscoverable Country', provides the most sensible account of 'occult qualities'—a perennial bugbear for historians of science and the new philosophy—currently available. Fleming deftly extricates these scholastic phenomena from those theories of influence and affinity (including their Neoplatonic and Paracelsian versions) to which they have seemed closely bound. Though the occult qualities or properties of objects have often been designated as 'unknown', Fleming shows that they were instead 'unknowable' for some early modern thinkers. While we are familiar with the former as the assumed object of normal science, prophetic revelation, and magic alike, the latter proves a fascinating (and to most readers now alien) component of scholastic knowledge—that which cannot be revealed, which indeed it is folly to investigate. Much of the success of this essay lies in Fleming's careful attention to the contours of these oft-parodied scholastic and Aristotelian modes of inquiry, a deep engagement that characterizes other recent works including Lodi Nauta's reevaluation [2009] of Lorenzo Valla's engagement with that legacy. Fleming also explores the relationship between secrecy and the occult within competing and collaborating systems of thought divided along the lines between scholasticism, the new philosophy, and esotericism. Horatio's imaginary response to Hamlet's rumination on the things of heaven and earth [69] both inspired this reviewer to laugh and brilliantly illuminated the point at hand—the too-often unexplored divide between the unknown and the unknowable. For Fleming, the very idea of discovery, the successful probing of the unknown, is revealed as one of early modernity's most powerful inventions.

Anthony Russell's exploration of the vital forces of early modern genius (and especially of poetry) surprises the reader by probing the reliance of John Donne's *Anniversaries* (1611–1612) on Ficinian ideas of creation (through the unlikely mediators of Campanella and Antonio Persio). Russell draws from an impressive range of scholarship including that on developing notions of genius in early modern visual art, embracing an interdisciplinary approach to invention that produces novel conclusions from well-studied material. The disenchantment of the world described so memorably by Donne (ostensibly on account of Elizabeth Drury's early death) is here turned on its head. While we tend to read *The Anatomy of the World*'s claim that 'The art is lost, and correspondence too' as a nostalgic lament, Russell instead finds in Donne's verse a call to action and a recognition of the powers of poetry and genius to re-enchant. Thus,

> as Campanella sees poetic utterance as having the unique power to pluck at the chords of life and generation, so Donne affirms the generative power of his verses.... What the poet dis-covers, through the inventive activity that produces his poem, is the redemptive creative energy that makes such invention possible. [92]

The move from Campanella to Donne is both unexpected and precisely the kind of thinking that a volume like this can foster at its best.

Unconstrained by the frequent boxing of Ficino into the study of 15th-century Neoplatonism, Donne's relegation to studies of poetics and literary biography, or Campanella's usual assignment to esotericism, Russell draws powerful connections between traditions habitually (if often informally) separated.

Indeed the volume's genuine interdisciplinarity is one of its evident strengths. Both Jacqueline Wernimont's discussion of Descartes' *The World* and Piers Brown's treatment of travel narrative and astronomy carve out welcome space for the porousness of literary, mathematical, and philosophical modes of writing. Perhaps the best example of this admirable interdisciplinarity is the fruitful joining by several contributors of studies of theology with histories of the new philosophy or science. Ryan Netzley's 'Numbering Martyrs' explores Protestant martyrology and investigates what terms like discovery and invention might have meant in a context traditionally far removed from the debates of early science. The links drawn between natural-philosophical invention and John Foxe's conception of immanent events are perhaps best appreciated by scholars already well-versed in English theology. Netzley's overarching claim, however, that the *Actes and Monuments* presents 'a notion of invention that does not react against discovery' and 'is not bound forever to the dialectical interchange between the given and the agentially created' [136] draws on Protestant thought to advance the central intellectual aim of *The Invention of Discovery.* Travis De Cook's lucid 'Unearthing Radical Reform' examines the interplay of discovery with notions of vanity and even blasphemy, representing the most successful détente here between the theological and natural philosophical. Through a close reading of clergyman Thomas Fuller's historical writings, especially his *A Pisgah-Sight of Palestine* (1650), De Cook exposes forces that militated against discovery, revelation, and invention in 17th-century England. In the process, we encounter the surprisingly useful (and prevalent) period rubric of anti-discovery and with it, a new hermeneutic with which to approach the closed dialectic of invention and discovery.

The emphasis on protestant theologies (and especially those of England), which characterizes not only the contributions of De Cook and Netzley but also that of Fleming, sometimes leaves aside pre-reformation traditions of discovery and invention that could have more fully informed these investigations. The Florentine patrician Palla

Strozzi's sincere investment in 're-discovering' the Greek gospels in the libraries of Byzantium might have proved a fascinating counterpoint to the antiquarian endeavors described by these authors. Poggio Bracciolini's almost obsessive drive to unearth ancient texts, recently brought to vivid life in Stephen Greenblatt's *The Swerve* [2011], provides yet another model for how discovery structured experience and knowledge-formation prior to the intersections of Protestant belief and early science.

The printing press, waiting quietly in the wings of many of the volume's essays, takes center stage in Vincent Masse's 'Newness and Discovery in Early-Modern France'. Turning to the material culture of French incunables, Masse proposes an unexpected and ultimately rewarding reevaluation of invention that understands novelty as a powerful rhetorical component of early printed texts. 'Newness' here vouches for, and characterizes, a symbiotic relationship between text and technology that served the interests of printers, book sellers, and authors at a moment of uncertain transition from manuscript to print. Reliant on the landmark works of Elizabeth Eisenstein and Lucien Lefebvre, Masse's essay might have been buttressed by direct engagement with revisionist scholarship on the supposedly essential attributes of print, especially the work of Adrian Johns [1998]. Nonetheless, the author's incisive turn to the rhetorical power of novelty serves as a model for the permeability of the literary, philosophical, and material within historical scholarship.

Indeed, connections between natural philosophy and early modern visual and material culture might have been explored more fully throughout this volume. The place of the visual arts is touched upon principally in Russell's essay and Fleming's introduction; yet 'invention' loomed especially large in the practice and rhetoric of early modern artists, and the conventional emulation of poetic invention in visual form might provide another lens for understanding the thorny dual nature of the volume's dialectic poles. Michael Cole's study of the profound impact of etching's invention not only on printmaking but on the visual arts as a whole, for example, sheds light on notions of invention active not only in Mosse's contribution but in Russell's as well [see Cole 2006]. Likewise, sustained attention to the material cultures of early modern mathematics, alchemy, and industrial design could provide further avenues for evaluating discovery's invention.

Occasionally, in their enthusiasm for the interdisciplinary, contributors overreach in their search for points of discrete contact between diverse traditions of knowledge. Michael Booth's adventurous essay argues that the polymath Thomas Hariott 'blended old world and new...mathematics and linguistics, invention and discovery' [59] in his contributions to algebra and his dictionary of the Algonguin language. The notion of the mutual communication of these starkly divergent enterprises is an interesting one but the congruences marshaled in its favor prove rather vague. Thus, Algonquin 'distributives "express the number of things taken at a time, as each one, two at a time, every third one, four apiece", which seems similar to what algebraic variables and coefficients do' [48]. A strong connection might be present, but this is assumed rather than elaborated and the judgement that distributives and variables 'seem' similar does not reassure a sceptical reader. The author attempts to account for these similarities through the application of 'blend theory', derived from cognitive linguistics. The conceptual framework, however, is not fully articulated here for those without a background in that field. That shared habits of mind informed these apparently separate activities is intriguing. Booth's ambitious study ultimately stops short, however, of fully bringing that congruence to life for the lay reader.

Steven Matthews' 'Francis Bacon and the Divine Hierarchy of Nature' proposes the reliance of Bacon's negative or eliminative induction on the pseudo-Dionysian *via negativa*. The essay expands upon a brief suggestion in Matthews' monographic study [2008] of Bacon's work. The proposed reliance pricks the reader's curiosity, not least on account of the claim that such connections have been largely overlooked. The author quickly acknowledges, however, that one of the principle reasons for the lack of study of pseudo-Dionysius in the Medieval Latin West (and Early Modernity in turn) was the ready assimilation of these concepts into the scholastic tradition at large. There is thus every reason to think that pseudo-Dionysian theology entered Bacon's thought in this rather more round-about fashion. Surely, it is important to understand Bacon in his own theological context (and Matthews' monograph achieves this aim impressively). Here however, it is not clear what we gain by putting too much stock in the direct rather than mediated entry of negative theology into Baconian induction.

Piers Brown's essay on travel narratives in Kepler and Galileo argues for the pivotal role of the scholar's journey in structuring the *Astronomia nova* (1609). Brown begins by distinguishing his notion of the journey from William Eamon's conception of the hunt (*venatio*) as a trope in his *Science and the Secrets of Nature* [1994, 269–300]. The rigid distinction between these models, however, is asserted rather than demonstrated and the evidence provided from Kepler's writings conform as easily to that of *venatio* as to itinerary. Thus, Mars is designated as the quarry and flees from castle to castle with the astronomer in pursuit. Yet, because Kepler writes that 'the route was unknown' between these hiding spots, Brown asserts that we are reading an appeal to itinerary rather than to hunting narrative [15]. Kepler's conceit of the triumphal cart similarly bespeaks mobility but not necessarily 'travel' or 'itinerary' in the sense those are usually employed. Evidence from Galileo is limited here to a single quotation from *Sidereus Nuncius* (1610). Significantly, this focus on a taxonomy of tropes leads Brown to eschew models that effectively blend diverse traditions. The *Astonomicon* of Marcus Manilius (first century AD), for example, was a widely available spur to terrestrial and celestial itineraries alike, inspiring the humanist geography of Francesco Berlinghieri (*ca* 1482) and the astronomical verse of Giovanni Pontano's *Urania* (*ca* 1479, printed 1509). Somewhat inflexible disciplinary distinctions are, hence, drawn here between activities that might have seemed rather more elastic for Galileo, Kepler, and their readers. This focus on classification renders problematic what is otherwise an eye-opening discussion of mobility and process in Kepler's work.

A similar retreat from ambiguity characterizes Jacqueline Wernimott's examination of René Descartes' challenging and enigmatic *The World* (published 1664). Approaching Descartes' model cosmos through the lens of the possible worlds paradigm, Wernimott seeks to free the treatise from its occasionally abusive wrangling into the generic containers of utopian fantasy or proto-science fiction. Instead, the author argues the treatise served as a model by which Descartes' readers were invited to understand mechanistic natural philosophy. The attempt, however, to impose a decisive break between *The World* and works like Lucian's *True History* (second century AD) and Bacon's *New Atlantis* (1624) ultimately imposes the conceit of a 'possible world' as a kind of hermetic seal. Treated as a sort of literary *unicum*, Descartes' treatise becomes detached not only from these forerunners of speculative fiction but more problematically from the environment of resurgent atomism and even Epicureanism within which Catherine Wilson has situated the treatise [2008, 98–101]. Wernimott's conviction that the philosopher's paper world is best understood as a kind of model too could have benefitted from looking outside its literary context to consider the vibrant literature on the role of models in early science, especially the recent work of Matthew Hunter and Roman Frigg [2010].

Not all of the essays fully address the structuring questions and hermeneutic challenge issued by Fleming. Pietro Daniel Omodeo's contribution, for example, provides a succinct and useful description of competing 'Copernicanisms' in the work of Kepler, Giordano Bruno, and Giovanni Battista Benedetti. In teasing apart what are often taken to be facets of a single ideology, Omodeo's essay certainly remains within the spirit of the volume but the author does not address the roles of invention and discovery as explicit components of these paradigms' formation. Likewise, Louise Denmead's 'The Discovery of Blackness in the Early Modern Bed-Trick' is an important contribution to the study of markers of race on the English stage but its link to discovery is a tenuous employment of that term as a cognate for revelation. Here that revelation is of a fraudulent bedmate whose masquerade is unveiled to the audience. The relationship is largely a semantic one here and links to the broader tradition (and the relevance of such revelation to our understanding of early modern discovery) go unexplored. This represents something of a missed opportunity to bring the epistemological concerns of this volume to bear on the recognition and valences of race in early modern England.

As Fleming well realizes, invention and discovery serve throughout this volume not only as period appellations but also as powerful interpretive and epistemological models that swiftly (and often covertly) take their place as master narratives for the historian of science. For this very reason, an invaluable step toward a fully realized reassessment of such paradigms may be the expansion of our vocabulary beyond the confines of these titular classifications. The most promise in this respect is here shown by De Cook's examination of anti-discovery as a structuring trope that both undermined and animated antiquarianism in early modern England. In a similar vein, a kind of rapprochement between invention-discovery and other salient period rubrics, including curiosity and wonder, might complicate the push and pull between apparently fixed alternatives [Evans and Marr 2006]. Among the models deployed by early modern poets, natural philosophers, historians, and painters alike were a host of iterations on 'rediscovery'—a category both related to invention/discovery and profoundly critical of their supposed opposition. For many readers, 'rediscovery' will immediately suggest the early modern fascination with antiquarianism, with Renaissance projects for the revival of pagan antiquity, raising the specter of the once fraught relationship between the supposed empiricism of early science and the strong yoke of classical authorities. Yet, as Christopher Wood and Alexander Nagel [2010], among others, have recently reminded us, the revival of classical antiquity was at least equalled (and sometimes bested) as a model by the potent conception of Christian resurrection as a rubric for novelty in the world. And Leonard Barkan [1999, 60–61] has long pointed to the darker side of such rebirth, calling our attention to antiquarianism's necromantic aspirations, the early modern invention of classical 'undead'. In digesting the lessons of this volume, we are charged with the responsibility of disarticulating binaries like that of invention/discovery, of introducing troubling third terms like resurrection and embracing the jointly hermeneutic and historicist task at hand. Ultimately, *The Invention of Discovery* should prove a spur to precisely this project, challenging the reader's acceptance of traditional and revisionist explanations for early modern science alike.

BIBLIOGRAPHY

Barkan, L. 1999. *Unearthing the Past: Archaeology and Aesthetics in the Making of Renaissance Culture.* New Haven/London.

Cole, M. 2006. ed. *The Early Modern Painter-Etcher.* University Park, PA.

Eamon, W. 1994. *Science and the Secrets of Nature: Books of Secrets in Medieval and Early Modern Culture.* Princeton.

Evans, R. and Marr, A. 2006. edd. *Curiosity and Wonder from the Renaissance to the Enlightenment.* Burlington, VT/Aldershot, UK.

Frigg, R. and Hunter, M. 2010. edd. *Beyond Mimesis and Convention: Representation in Art and Science.* London/New York.

Greenblatt, S. 2011. *The Swerve: How the World Became Modern.* New York.

Johns, A. 1998. *The Nature of the Book: Print and Knowledge in the Making.* Chicago.

Matthews, S. 2008. *Theology and Science in the Thought of Francis Bacon.* Burlington, VT.

Nagel A. and Wood, C. 2010. *Anachronic Renaissance.* Cambridge, MA.

Nauta, L. 2009. *In Defense of Common Sense: Lorenzo Valla's Humanist Critique of Scholastic Philosophy.* Cambridge, MA.

Wilson, C. 2008. *Epicureanism at the Origins of Modernity.* Oxford.

Sonne und Mond, Kalender und Uhr. Studien zur Darstellung und poetischen Reflexion der Zeitordnung in der römischen Literatur by Anja Wolkenhauer

Untersuchungen zur antiken Literatur und Geschichte 103. Berlin/New York: De Gruyter, 2011. Pp. x + 373. ISBN 978–3–11–024712–1. Cloth €102.76

Reviewed by
Karlheinz Schaldach
Schlüchtern, Germany
info@antike-sonnenuhren.de

When Oswald Spengler called sundials an ‘insignificant tool’ of ancient everyday life in his famous *The Decline of the West*, he linked this with the observation that they had not influenced ‘classical life-feeling’ in the smallest degree. Was he right? Where little grows, one may suspect barren ground; and if one takes previous publications as a basis, they seem to give credence to Spengler. Plainly, a monograph devoted only to the sense or understanding of time in antiquity is vainly sought. It is to Anja Wolkenhauer’s credit that she has closed a gap in research with her study of the representation of, and poetic reflection on, time and its order in Roman literature. So far, only a few publications on this exist and they are shorter and scattered.

In her introduction, chapter 1 [1–20], when summarizing the existing articles on the subject, she recognizes a deficiency in the definition of time which shapes philological research: different temporal phenomena are typically placed next to each other indiscriminately [8]. Wolkenhauer, however, distinguishes time itself (*tempus*), the order of time which emanates from the people (*observatio*, *ratio*, *compositio temporum*), and the measurement of time, which does not concern the order as a whole but only its increase. The introduction concludes with an outline of the book and a delimitation of the topic: the period to be examined is about 200–50 BC and includes all the texts of classical Latin literature which show the order of time or use it as a structural element.

The aim of the second chapter [21–66] is to outline the *tempus Romanum* as a specifically Roman concept which is closely connected

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 290–293

to the measurement of time and temporal order. Wolkenhauer shows that the metaphor of the cosmic clock, as it is handed down from Plato's *Timaeus*, can be transferred to the Roman idea of time only partially. The chapter closes with a presentation and examination of the mythical inventors and teachers of the temporal order: Atlas, Prometheus, Palamedes, Romulus, and Numa.

In the third chapter [67–150], which concerns the determination of what was attributed to the concept of time in Rome, the study focuses on a central issue, the order of day and night, which was increasingly dominated by clocks. The centerpiece is Wolkenhauer's analysis of the history of clocks as it is described by Pliny in *Nat. hist.* 7.212–215, which Wolkenhauer calls a 'literarischer Glücksfall' ('literary fortune)' because comparable texts on the history of calendars have been lost [18]. She shows how the Forum in Rome became the center for the measurement of time. According to Pliny, the tradition designated the sundial of L. Papirius Cursor, which was not put in the Forum, as the first known to Rome; but Wolkenhauer argues cogently that this dial was merely a votive gift and not used as a timer [82].

In reference to Vitruvius, she explains that the reason for integrating gnomonics into the body of architecture was to ennoble it. Thus, she understands book 9 and also 10 (on mechanics) not as irritating appendages but as the culminating parts of the presentation and as a demanding field within the art of architecture [96]. In a section on the image of clocks, Wolkenhauer discusses three metaphors in more detail: the sundial as an instrument of force, as a cosmic clock, and as a symbol of human finitude [123–148].

After its very beginnings, when the sundial was cursed and compared to a despotic and violent ruler [124], it became more and more important in everyday life so that by the Augustan period people approached time-measuring devices in a clearly positive way. In later centuries, it even had a literary apotheosis: for Cassiodorus [*Var.* 1.46, 1–2], the clock was a metaphor for good order because without clocks there is no reasonable division of days and so the order of life would become confused [148].

The most extensive chapter of the book is the fourth, which focuses on the order of the year using the calendar [151–270]. Since a rich secondary literature on Roman calendar reform has appeared, Wolkenhauer limits herself to the still little-explored area of the presentation and mediation of this reform in its course and afterwards in Roman literature. Wolkenhauer sees the Julian calendar reform not only as a solution to old inherited problems but also as a trigger for further consideration or as encouragement to use the temporal order for political purposes. Augustus' small calendar correction is re-examined from this perspective. For her, the obelisk on the Campus Martius, which was installed as a gnomon, is a fundamental element of the calendar correction in that it linked the order of time with the person of Augustus. Though Caesar's reform was more important, Augustus succeeded by means of the Sun-pointer in making visible daily his intimate connection with the reform of the calendar as a designer not only of a public space but also of time [248].

A fifth chapter [271–328] deals with eutopic and dystopic schemes of temporal orders. 'Eutopic' stands for 'utopian', whereas 'dystopic' marks the detachment of time from its natural rules, for instance, by the expansion or the reversal of temporal processes.

The final chapter [329–336] reviews the core ideas of the study. Wolkenhauer also asks whether an ancient critique of temporal order can be discerned. Her answer is that the culture of critical scepticism exhibited a quiet tone, an isolated appeal that is only perceived in Ovid and Pliny since all the other Roman authors did not question the *tempus Romanorum* but were interested only in its organization [336]. The book ends with a bibliography [337–363] and an index of the ancient literature used [364–373]. Latin or Greek texts are in most cases not only cited but provided with the author's own congenial translations.

Anja Wolkenhauer has presented a stimulating, challenging, and very well written monograph which illuminates urgently an aspect of antiquity rarely handled. The joy of playing with language and stylistic devices is evident in many details. I will note only that on page 38, the title of the book recurs as a contraction from the beginning and end of two sentences strung together.

Only in a few instances are inaccuracies to be found or are the arguments presented unconvincing to this reviewer—for example, when she writes that the sundial shows a half circular motion of the Sun, which, however, applies only for the equinoxes [27], or that the ninth book of Vitruvius' *De architectura* was integrated not only to ennoble architecture but is also to be understood as the culmination of the presentation. For a climax, I would have expected more profound account from that author, whereas the book is certainly one of the weaker of Vitruvius' work. That he, as an obvious layman in gnomonics, saw the need to integrate it in his work, which was not undertaken by anyone before him, strengthens the argument that his appreciation of gnomonics is based on non-architectural grounds.

With some other topics, there could have been deeper foundations. For instance, when Wolkenhauer discusses temporality in the Roman world in connection with Martial [4.8], who gives the schedule for a workday, she does not mention the Sulpicii Archive of legal documents discovered in Murecine (outside the city of Pompeii) which show the same temporal framework as those found in literary texts [109–114]. Also, when she mentions the relationship between the sundial and death, I missed references to Petronius [*Cena Trim.* 4.71], Posidippus [*Epig.* 52], or the sundials which have been found in cemeteries. Those considerations, however, push the limits of the book because, as Wolkenhauer has indeed pointed out, her work is a literary reflection and not an all-encompassing picture of the ancient temporal order, which would have been a different monograph. My objections should not, therefore, detract from the content in any way. I have missed only a glossary and a decent price.

Ancient Computers: Part I. Rediscovery by Stephen Kent Stephenson

Nook Book. Barnes and Noble, 2011. Pp. 59. epub $3.98

Reviewed by
Duncan J. Melville
St Lawrence University
dmelville@stlawu.edu

In this short monograph—the main text of 40 pages or so is supplemented by a collection of appendices—the author seeks to reconstruct ancient calculation techniques using counting boards. This is a very under-researched and poorly understood area with very little high-quality evidence, either textual or archaeological, so a certain amount of speculation is inevitable.

While a few examples of the small, Roman hand-abacus with constrained beads in columns are known, there are no extant Roman examples of the posited larger counting boards that presumably utilized scratches on boards and pebbles or similar small objects as counters.

In the absence of Roman evidence, the author turns to the Greek remains, specifically, to the Salamis Tablet, an object of considerable speculation since its discovery in 1846. This large marble slab (roughly 1.5m x 0.75m) now housed in Athens has been variously interpreted as a counting board or as a gaming board. The author follows Lang's contention that it was a counting board without addressing alternative theories. In fact, he sees it as an exemplar of the Roman counting board: 'Let us assume the obvious, that the Roman counting board abacus was The Salamis Tablet' [6]. Further, he writes:

> The Romans were borrowers. They borrowed The Salamis Tablet from the Greeks, but the Greeks borrowed it in turn from the Babylonians. [11]

This arc of transmission is adduced in the following way. The author compares the symbols on the slots of the hand-abacus with the line-markings on the Salamis Tablet, finding that 'the mapping is perfect' [7]. However, this mapping forces an 'engineering compromise'

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 294–297

whereby the Roman adaptation of the Greek counting board forces them to use a 'less preferred structure for one of the base-12 digits' [7]. Furthermore, this mapping leaves the 'dashed lines unused', a fact that the author uses to argue for the sexagesimal and, hence, Babylonian origin of the Salamis Tablet. Thus, the Tablet can be used for base-10, base-12 and base-60 calculations, although only the sexagesimal ones use the full power of the counting board.

The author offers a detailed account of his reconstruction of how the Tablet was used as a counting board; and, on the laudable grounds that a visual lesson makes the algorithms much clearer, he has posted a series of accompanying videos on YouTube[1] illustrating the various arithmetic operations. He also makes two points that deserve wider dissemination. The first is that he sees the central dividing line of the Salamis Tablet as allowing an additive side and a subtractive side. A pebble or counter on one side of the 10's column would represent 10, while a counter on the other side of the units column would subtract from the total, or represent -1. Thus $9 = 10 - 1$ could be represented with just two tokens. The concept would be familiar to Romans. The author describes a full-blown additive and subtractive regime and notes that this approach, 'reduces the number of pebbles needed tremendously' [7]. It also makes many calculations easier. One cannot argue with his claims of increased efficiency and this point deserves further investigation. One cannot help think, though, that a single pebble placed on the subtractive side of an empty board should have led to the idea of negative numbers.

The author's second substantive point is that in attempting to understand ancient mathematics, historians need to pay more attention to the available tools, technology, notation, and terminology to see how particular algorithms may have been performed. He gives an example of multiplying two five-digit sexagesimal numbers *via* the formula, showing that this requires 178 separate operations [22]. The author has a video of himself computing the square root of 2 using a set of Salamis Tablets following Heron's method. It takes him 25 minutes. His argument is that using only tables and writing intermediate results on clay would take a lot longer. He issues a challenge:

---

[1] See http://www.youtube.com/view_play_list?p=545ABCC6BA8D6F44.

> The Historians should record their own performance video and post it to YouTube.com so we can compare its length to Stephenson's. [24]

There have been some very careful studies done of calculation techniques but there is plainly more to be done.

By and large, the professional literature is accessible only to professionals. The internet has allowed wide access to information (thus *Aestimatio*) but of varying quality and with unpredictable results. In an aside on the origins of the sexagesimal system and the seven-day week, the author includes this quote, which he footnotes as 'Wilson, 2001':

> The Sumerians had a better reason for their septimalism. They worshipped seven gods whom they could see in the sky. Reverently, they named the days of their week for these seven heavenly bodies.

It turns out that the source of this bizarre claim is a one-page unsigned article in the Christmas Special edition of *The Economist* in 2001. The A. Wilson whom Stephenson cites seems to be the author of the website where he found the link to the *Economist article*. The *Economist* article has no references and I have been unable to trace this further back. However, in the vast echo chamber of the internet, it is widely, and perhaps ineradicably, cited. There is, however, no evidence for a seven-day week in third millennium Mesopotamia. Moving on to the sexagesimal system, Stephenson cites another website where the claim is made that it originates in the fact that 60 is the least common multiple of 12 and 30, the periods of Jupiter and Saturn. The sexagesimal system has its origin in the proto-cuneiform system used to count discrete items in alternate multiplies of 10 and 6. It has nothing to do with astronomy.

Publishing is undergoing a period of extremely rapid change and, in closing, I offer a few comments on accessibility and media. The author has published his monograph in epub format through Barnes and Noble for the Nook Reader (a Kindle edition is also available). Anyone with internet access and a Nook Reader can download a copy and start reading. I do not have a Nook Reader. Barnes and Noble will allow you to download software to read epub books on your computer. But you have to have an account. Given that I had once bought something from them, Barnes and Noble would not allow me to create a new account: nor could they establish an old one. Nor would the Reader recognize a book that I had not downloaded myself (the review copy). None of this is the author's fault, but it does illustrate to those thinking of testing out the brave new world of electronic publishing some of the teething troubles potential readers may have. Eventually I gave up and got a browser plug-in. Elapsed time from preparing to open the book to actually being able to read it: about one hour.

Al-Kindī by Peter Adamson

Great Medieval Thinkers. Oxford University Press, 2007. Pp. xiv + 272. ISBN 978–0–19–518143–2. Paper $40.00

Reviewed by
Cristina Cerami
CNRS, Paris
cristinacerami@hotmail.com

This book is a monographic presentation of the thought and work of the ninth century philosopher al-Kindī. Al-Kindī, whose complete name is Abū Yūsūf Yaʿqū ibn Isḥāq al-Kindī, is usually considered to be the first Arabic philosopher and is known by the great public as the one who drove at its peak the translation of scientific and philosophical works from Greek into Arabic.[1] Nevertheless, the presentation of al-Kindī proposed by Peter Adamson goes far beyond the preconceived image and aims to shape a more comprehensive portrait. Despite the loss of a great part of al-Kindī's literary production, the author manages to provide a complete and unified presentation of an intellectual personality that ranges over almost all scientific and philosophical disciplines. Such a brilliant outcome benefits from the critical studies carried out last century that led to the fundamental edition of al-Kindī's scientific and philosophical works by R. Rashed and J. Jolivet. In actual fact, Adamson's study—unlike several recent presentations that just take into account al-Kindī's metaphysical thought and the Greek Neoplatonic influences on it—has the great merit of investigating and elucidating all the areas and aspects of al-Kindī's philosophical and scientific production.

Al-Kindī was born in Baṣra at about AD 800. He moved to Baghdad early in his life to receive his education and to pursue his intellectual career under the caliphates of al-Ma'mūn and al-Muʿtaṣim. Actually, al-Kindī's family was linked to political power from the outset of the Muslim empire since one of his ancestors was the king of Kinda and a companion of the prophet. Moreover, his proximity to

[1] Actually, the such translation had already begun under the caliph al-Mansūr (reigned AD 754–775) but it reached its peak by the time of al-Kindī.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 298–303

the caliph's family is attested by the numerous letters and treatises dedicated to the caliph or to his son. Several of these essays, as well as many other works by al-Kindī, have not reached us but we are acquainted with them thanks to the 10th-century book merchant Ibn al-Nadīm, who transmitted to us a list of al-Kindī's works. This list shows the astonishing range of his interests since it includes about 300 titles of works on philosophy, mathematics in its various branches, medicine, optics, and astronomy. To this, Ibn al-Nadīm added a series of titles concerning different topics such as astrology, meteorology, zoology, mirrors, jewels, perfumes, and glass.

Adamson assumes, as do most specialists, that despite the great breadth of al-Kindī's learning, there is a common denominator to almost all of his works, namely, the project of promoting and interpreting Greek scientific culture. He makes it clear, though, that al-Kindī's production reveals another mark of his historical position and intellectual personality that should not be underestimated: his eagerness to contribute to all the branches of knowledge of ʻAbāssid culture as well as to the theological doctrines professed by the caliphs. This explains at the same time al-Kindī's keen interest in mathematics, his unceasing dialogue with the representatives of Islamic theology, as well as his competences in the other topics that aroused the interests of the members of caliph's court. Moreover, Adamson assumes that some of al-Kindī's philosophical tenets can be interpreted in the light of this same hypothesis. This is the case of his well known philosophical doctrine according to which God alone is eternal. Actually, Adamson suggests that al-Kindī's insistence on this doctrine reflects, and is in a way influenced by, the theological belief supported by the caliphs that the Koran is not eternal but created.

Concerning al-Kindī's cultural engagement, Adamson assumes that, from al-Kindī's point of view, the translation project aimed chiefly to demonstrate how Greek wisdom and knowledge could be integrated with the Arabic language and the teachings of Islam. The basic assumption is that philosophy was for al-Kindī a tool for proving the central truths of Islamic theological dogma as well as for interpreting the ambiguous or difficult passages of the Koran. This is quite a traditional thesis about al-Kindī's idea of the relationship between Islam and philosophy. However, Adamson's approach has the merit of proving this thesis with regard to the several truths of Islamic revelation concerning divine predication, the creation of the world, the immortality of the soul, and divine providence, by drawing support from a considerable number of texts newly translated from Arabic.

This same knowledge and handling of al-Kindī's corpus allows the author to portray his intellectual stature faithfully. To this end, Adamson deals with the dominant areas of al-Kindī's competence (metaphysics, psychology, ethics, mathematics, astronomy) and devotes a chapter to each one of them. This reconstruction is founded on the hypothesis that al-Kindī's interests moved from philosophical topics to more scientific and technical issues. This hypothesis, though, given what the Adamson himself says, remains pure speculation.

In chapters 3–5, Adamson reconstructs al-Kindī's theory concerning the unity, the ineffability, and, more generally, the nature of God and his causal relationship with creatures. To this end, in chapter 3, he examines in detail the third section of *On First Philosophy* and the philosophical arguments that are found in this treatise. This survey aims to answer one of the most challenging questions of al-Kindī's metaphysical inquiry:

> What is the exact relationship between God and (the) creatures? and Where does multiplicity come from?

The answer was probably in the lost part of the treatise *On First Philosophy* but Adamson tries to reconstruct al-Kindī's theory drawing on other texts that deal with the same topic. Thus, he singles out the different doctrines and authors that influenced al-Kindī on the topics of creation and God's causality. He concludes that al-Kindī's theory is close not only to Neoplatonic tradition but also to Aristotelian physical doctrine. Even if al-Kindī shares most of Philoponus' criticisms of Aristotle's belief in the eternity of the world, he admits some of the major postulates that ground the Aristotelian theory of substantial generation. Concerning Neoplatonic influence, Adamson traces a leading thread between the theory exposed in the *De causis*, which distinguishes proximate and remote causes, and al-Kindī's theory of God's causality on the created world.

Concerning the eternity of the world, the subject of chapter 4, Adamson explains that al-Kindī sides against Philoponus and with Aristotle and Simplicius on the issue of the composition of the heavens, in denying that they have the same nature as the sublunary bodies. The position that al-Kindī finally takes is that the heavens have a finite existence, even if their constitution never changes during the interval of time that God's will appoints for them. Adamson elaborates his analysis of this topic on the assumption that the rebuttal by al-Kindī of the eternity of the world expounded in *On First Philosophy* is based on a mathematical method. Actually, al-Kindī's refutation in this treatise is preceded by a methodological section in which he affirms that, in demonstrating the non-eternity of the world, we have to pursue intellectual 'perception', i.e., a purely conceptual investigation. In order to explain this method, al-Kindī takes as an example the argument leading to the impossibility of conceiving void and, therefore, to the refutation of a spatially and temporally infinite world. Adamson deduces that this type of argument is an intellectual one since it is founded on the definition of void considered as something conceptually impossible and not on (some) empirical premises. He concludes that this kind of investigation could be properly defined as mathematical, even if in another passage of *On First Philosophy* al-Kindī affirms that it is not permissible to use mathematical investigations when studying 'natural things'. In fact, Adamson's analysis leaves open a question concerning the relationship between physics and metaphysics and, more precisely, the problem of their place in the order of the theoretical sciences that make inquiry pertaining to the eternity of the world. The answer to this question is not of minor importance since it entails establishing the horizon of physics and metaphysics, and remains a major desideratum.

Chapter 5 is devoted to the exposition of al-Kindī's psychological doctrine. Through a survey of all the texts dealing with this topic, Adamson connects al-Kindī's theory of the soul to his epistemological teaching. As a matter of fact, the greatest interest of this survey is that it does not take into account just al-Kindī's theory of intellect, as most scholars have done, but covers all the aspects of his theory of knowledge, including the relationship between mathematical knowledge and sensation. In this context, Adamson manages to give a unitary and coherent account of the role that al-Kindī ascribes to the sensible world and to divine action in the process of cognition and prophetic dreams.

In chapter 6, Adamson proposes a survey of al-Kindī's ethical theory. The reconstruction of his ethical corpus, even if much of his works on this topic are lost, confirms the unitary portrait that Adamson sketches. In opposition to some recent interpretations, Adamson shows how al-Kindī's moral theory is linked to or, more precisely, dependent on, his metaphysical theory. According to Adamson's hypothesis, al-Kindī's aim is to draw ethical conclusions from theoretical principles about the immortal soul and the intelligible world.

The last two chapters of this volume are devoted to al-Kindī's scientific works. Though al-Kindī's recognized no firm division between science and philosophy, his scientific production is here studied just to highlight the aspects that are most relevant to his philosophical doctrine. Adamson admits that this corpus is so vast and requires such a technical and wide expertise that is almost impossible to give a thorough survey of it in a single chapter of a book. For this reason, he restricts himself to considering the philosophical questions that arise from al-Kindī's scientific production.

The most important of these questions concerns al-Kindī's methodology, i.e., the kind of procedure by which we can reach scientific primary principles and theories. Adamson insists that al-Kindī attributes an important role to empirical observation in the process of confirming scientific theories, even if the theories themselves are not reached by it. He concludes that al-Kindī's scientific conclusions are most frequently driven by abstract mathematical reasoning and that observation is used chiefly to check the accuracy of the application of its truths.

Then, Adamson organizes the extant material in accordance with al-Kindī's own division of mathematical sciences and presents in this light his doctrine of medical proportion, his theory of vision, and his cosmology. He explains in detail how al-Kindī's medical doctrine is founded on his theory of arithmetic progression. He concludes that, even if in some texts al-Kindī approaches pharmacology from a practical perspective, his theory is in a sense non-empirical in so far as it rests on the assumption that the proportions between chemical properties must be governed by the doubling progression, which is for al-Kindī the 'most natural' of the arithmetical relations.

Next, Adamson shows the role of geometry in the explanation of al-Kindī's theory of vision and colors. Concerning optics, he informs us that al-Kindī's conclusion that light is propagated in all directions along straight lines is considerably influenced Ibn al-Haytham's theory of vision.

Finally, in stressing the importance of Plato's *Timaeus*, Adamson provides evidence concerning al-Kindī's assumption that harmonic theory, viewed as the science of quantity in so far as one quantity is related to another, should be regarded as the science of everything.

The volume ends with a discussion of the study of the heavens that could be considered as the culmination or the synthesis of al-Kindī's philosophical system. Actually, al-Kindī uses his psychology to explain why the heavens move as they do. His physics explains why the sublunary world is affected by the celestial motion. This in turn grounds his use of astronomical observation to predict the future. The entire theory thus provides the basis for al-Kindī's theory of providence that leads, in turn, to grasping God's creative activity.

To conclude, this monographic presentation has at least two virtues: it not only offers valuable information about the formation, organization, and structure of the works of one of the most influential Arabic philosophers, it also affords new insights into his entire philosophical project. For this reason, the results presented in this volume will be extremely helpful to specialists as well to less advanced students who want to break through into the complex and rich history of Arabic philosophy.

Goddu’s Copernicus: An Essay Review of André Goddu’s *Copernicus and the Aristotelian Tradition*★

Peter Barker
University of Oklahoma
barkerp@ou.edu
and
Matjaž Vesel
Research Centre of the
Slovenian Academy of Sciences and Arts
matjaz.vesel@guest.arnes.si

For some 20 years, André Goddu has made important contributions to Copernicus studies as well as to a wide range of scholarship in medieval history. His work is distinguished by a formidable and unusual combination of skills in languages and paleography. These have given him direct access to marginalia and notes in primary sources and to secondary sources in European languages, especially Polish, that have been generally inaccessible to English speakers. This valuable work is extended and to some extent consolidated in his new

★ André Goddu, *Copernicus and the Aristotelian Tradition: Education, Reading and Philosophy in Copernicus's Path to Heliocentrism.* Medieval and Early Modern Science 12. Brill: Leiden/Boston, 2010. Pp. xxviii + 545. ISBN 978–90–04–18107–6. Cloth $191.00.

All Figures in this article are reproduced by permission of the University of Oklahoma History of Science Collections.

Figure 1 [p. 322]: Magini 1589, 58r.
Figure 2 [p. 328]: Copernicus 1543, 67r (detail).
Figure 3(a) [p. 329]: Magini 1589, 3v (detail), repr. from Magini 1608, 11.
Figure 3(b) [p. 329]: Maestlin 1596, 125 (detail).

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 304–336

book, *Copernicus and the Aristotelian Tradition: Education, Reading and Philosophy in Copernicus's Path to Heliocentrism.*

Goddu begins his long book with a helpful summary and guide for readers with special interests [esp. xxii–xxiii]. We will follow a similar plan. In the first part of our review, we will attempt to summarize Goddu's most important results, highlighting things we hope will be of interest to other scholars. In the second part, we will give a critical appraisal of the book; and in the third, we will present a detailed examination of one specific theme, Goddu's general scepticism about Islamic influence in Copernicus.

1. Major themes

Goddu's book divides into three sections. The first two examine sources on Copernicus' education in Poland and in Italy before his permanent return to Poland. The third section considers Copernicus as a philosopher and divides into separate chapters on logic and methodology, natural philosophy, and mathematical cosmology. Two important themes that distinguish this book from other treatments of Copernicus are the framing of his work as an effort at reform rather than revolution, and a careful distinction between the sources and influences that led to the *Commentariolus*, which Goddu dates to the period 1509–1510, and those that led to the more celebrated *De revolutionibus*, which Copernicus composed between *ca* 1526 and 1543.

Goddu's opening chapters make extensive use of Polish material, making much of it accessible to an English-speaking audience for the first time. The main burden of part 1, as the author summarizes it [xxii], is to establish the pervasive indirect influence on Copernicus of John of Glogow (*ca* 1445–1507) in logic and Albert of Brudzewo (*ca* 1445–1495) in astronomy.

Goddu denies any simplistic direct line between what happened to Copernicus in Cracow and heliocentrism. Rather, he argues, in that period, Copernicus received practical training in astronomy and formed intellectual habits in reading, argumentation, and scholarship that would become important more in how he presented his ideas than in how he came to them [16].

A major thesis of the entire book that emerges in this section defines Copernicus' relation to the Aristotelian tradition. According to Goddu, Copernicus should be seen as part of a complex and multifarious Aristotelian tradition—as an internal critic, not as a revolutionary [92]. Goddu draws on new histories of Cracow, which offer a different picture from the earlier literature on Copernicus [13n35]. This together with his own extensive research allows him to present the most detailed account now available in English of the development and structure of the curriculum at Cracow in astronomy and natural philosophy at the time of Copernicus' education. He also gives what is likely to become the definitive reconstruction of Copernicus' studies, and especially of his astronomical studies, at Cracow.

The most influential teacher in the liberal arts curriculum at Cracow was John of Glogow, also known as John of Glogovia or Joannes Glogoviensis [27], and most instructors in Copernicus' day were his students [30]. In addition to the core subjects of logic and natural philosophy, Glogow himself wrote extensively on astronomy including prognostications, almanacs, ephemerides, and astrological calendars, as well as a commentary on the *Sphere of Sacrobosco* [35]. In astronomy, the dominant influence at the time of Copernicus' education was Albert of Brudzewo, who completed his *Commentariolum super theoricas novas planetarum Georgii Purbachi* (*Little Commentary on Georg Peurbach's* Theoricae novae planetarum) in 1482. Most of the teachers in the 1490s, especially those in astronomy, were trained by Glogow or Brudzewo or by both. Brudzewo was the most famous astronomer among this group and his *Commentariolum* would have been used in teaching and circulated among students in manuscript, even when he was not teaching it himself. It is, therefore, likely that Copernicus knew this book from the beginning of his education. Although Brudzewo himself was not giving lectures on astronomy at this time, it is worth noting that Copernicus may have attended his lectures on Aristotle's natural philosophy [32, 37].

It is useful to recall here that in all Latin universities the curriculum in astronomy followed the same plan: a first course based on *Sacrobosco's Sphere* in some form introduced the fundamental celestial circles and phenomena that could be accounted for by means of the daily motion of the heavens around a central Earth. The second course presented models for the motions of the Sun, Moon and planets; considered the origin of eclipses; and previewed the main application of these techniques, the determination of astrological configurations. The main text for the second course was a version of the *Theorica planetarum* and, at progressive universities from the 1480s on, a version of Peurbach's *Theoricae novae planetarum.* That Cracow was indeed progressive is shown by the adoption of Brudzewo's *Commentariolum* as a teaching text. As far as we know, this was the first major commentary to be written on Peurbach's book and the first in a wave of similar works that appeared all over Europe during the next few decades [Barker 2011, 2013]. After the basic courses covering the *Sphere* and the *Theorica*, advanced students would have gone on to study Ptolemy. According to Goddu's reconstruction [32ff.], in summer of 1494, Copernicus would have attended lectures on the *Sphere* by Stanisław Ilkusch and perhaps on astrology by Albert Szamotuli; in winter 1494–1495, these would have been followed by lectures on the *Theorica planetarum* by Ilkusch and on Ptolemy by Szamotuli. Several of these courses would have obliged Copernicus to study Brudzewo's *Commentariolum*, although Goddu minimizes the likelihood of direct reference in the *De revolutionibus* [37n9].

In the teaching of astronomy at Cracow, the influence of Brudzewo is probably more important than John of Glogow. But Glogow is notable as a source for logical doctrines that Goddu finds in Copernicus' mature work. In chapter 3, Goddu reviews Aristotle's works on logic and their development by Boethius and Peter of Spain, before he moves on to consider Cracow. He pays special attention to the logic of conditionals and especially to the paradoxes of material implication, which, in modern terms, are often summarized by the two principles that anything follows from a contradiction and that a logical truth follows from anything. Phrasing the first of these in the form of a conditional might give, for example, 'If Copernicus is and is not a man, then, the Earth has an annual motion around the Sun'. Most people would reject an argument that included this conditional on the grounds that it violated their intuitions about logical consequence, although formally the conditional is acceptable. Building on earlier discussions, Glogow concluded that the principle that anything follows from a contradiction is acceptable in formal, but not in material, contexts. For a conditional to be acceptable in the latter contexts, some relation is required to exist between the antecedent and the consequent of the conditional. This relation is established by topics or topical maxims, for example,

> essential superior to inferior, whole to essential part, integral whole to part, cause and effect, cause of following, correlative, inclusion and containing and contained. [81]

Later in his book, Goddu argues at length that Copernicus follows Glogow in requiring (at least) the relevance of the antecedent to the consequent as an additional stipulation for any conditional statement about natural philosophy or astronomy to be acceptable, and in making particularly prominent use of the topic 'integral whole to a part' in the arguments of *De revolutionibus* 1.

Chapter 4 considers the teaching of natural philosophy and begins to argue Goddu's major thesis [92] that Copernicus should be seen as an internal critic of the Aristotelian tradition, not as a revolutionary who rejected it [133]:

> [What Copernicus] learned above all from his teachers was how to adapt Aristotelian principles to ideas different from those held explicitly by Aristotle. [99]

For Copernicus, the Aristotelian tradition is a long way from Aristotle [93]. According to Goddu, we should not expect to find any defining content, commentary tradition, or school at Cracow [95]. In the early 15th century, the curriculum in natural philosophy followed Buridan [93]. This early dominance was replaced by eclectic teaching that embraced the work of Albertus Magnus, Thomas Aquinas, Giles of Rome, and Duns Scotus. The Cracow *milieu* is portrayed as generally hostile to Plato, at least before Copernicus' generation. Any sympathy that Copernicus shows must, therefore, be attributed to later Humanist influences (some described in the next chapter) and to his time in Italy. Major figures considered in this chapter are Versoris, Albert of Saxony and, again, John of Glogow. In a preview of subsequent discussion of the origins of Copernicus' heliocentrism, Goddu highlights Bernard R. Goldstein's paper [2002] on the distance-period relation as a motive for Copernicus' innovation [124n94].

Chapter 5 examines Humanism at Cracow and its influence on the teaching of astronomy, introducing several other themes that reappear in later chapters. Humanist influences strengthened at Cracow after 1480 [139] and while Copernicus was there as a student. Indicating where his own sympathies lay, Copernicus maintained a correspondence with the Cracow humanist Lawrence Corvinus up to 1509 [141]. Hence, he clearly had some contact with scholars who evaluated Plato positively, which mitigates the hostility to Plato indicated in Goddu's previous chapter. In general, however, Goddu minimizes Copernicus' debt to Plato, in contrast, for example, to the recent work of Anna De Pace [2002, 2009].

In the balance of chapter 5, Goddu presents a number of themes that will be important in Copernicus' later work. Throughout the late medieval and early modern periods, Averroist natural philosophers strongly criticized Ptolemaic astronomy for violating (as they saw it) Aristotelian limitations on the nature of celestial motions. Goddu presents a detailed list of the standard Averroist objections. Key claims concern the impossibility of there being more than a single center of rotation, the ruling out of epicycles and eccentrics, and the denial of any penetration or splitting of the celestial substance, which Averroists believed would be required to accommodate such orbs [143–145]. Goddu goes on to consider the contents of the primary texts in the Ptolemaic tradition, the *Sphere* and the *Theorica*, noting that the 'principal problem' of the latter could be seen as one of reconciling Ptolemy's mathematical models with the concentric cosmology of Aristotle [148]. Goddu also mentions the use of tables and canons as goals of astronomical training and concludes by examining the extent to which Copernicus' education might have included the construction and use of instruments. He concludes that Copernicus was likely to have heard descriptions at least of spherical and plane astrolabes, the parallactic ruler, and a quadrant that used a gnomon.

Next Goddu examines criticisms of Ptolemaic astronomy in this *milieu*, especially in the work of Brudzewo, and presents themes that will reappear later in Copernicus' astronomical work. An interesting connection to be made here perhaps is with one of the most striking features of Copernicus' model for the Moon, the use of a double epicycle. This device was singled out for special praise by contemporaries (e.g., Reinhold and Melanchthon). However, Brudzewo had already introduced a double epicycle in the *theorica* of the Moon in his *Commentariolum*. Such double epicycles also appear in the celebrated Tusi-couple, a mathematical device originating in 13th-century Persia that uses two circular motions to produces a simple harmonic motion along a straight line. Islamic astronomers made extensive use of this device to construct systems of eccentric orbs and epicycles, or sometimes concentric orbs and epicycles, that satisfy the primary condition that they rotate uniformly about their own centers even though these were not all identical to the center of the cosmos.

In the second half of the 20th century, a great deal of attention was paid to Copernicus' use of this device and these models, with many scholars concluding that Copernicus must somehow have gained access to Islamic sources, although the precise route of transmission remains unknown. In the context of his discussion of the origins of the double epicycle found in Brudzewo's work, Goddu voices scepticism about the Islamic derivation of Copernicus' work and suggests a series of European sources from whom Copernicus might have learned about double epicycle models without Islamic intermediaries [155]. He also points out that, strictly, the construction introduced by Brudzewo and his predecessor Sandivogius of Czechel (fl. 1430) to account for the Moon's always showing the same face is not a Tusi-couple: the second epicycle controls the orientation of the spherical body of the Moon, not its motion along a hypothetical straight line or its distance from the observer. Moreover, Goddu suggests other reasons for thinking that Copernicus achieved the same results as Islamic astronomers but from different sources. Thus, another possible source for his Tusi-couple may be the model for Mercury in Brudzewo's *Commentariolum* [Birkenmajer 1900, 110ff.], which produces a straight line motion out of several circular motions. Later Goddu will suggest another completely novel source for the origin of the Tusi-couple in Copernicus—a European tradition starting with Oresme.

From his reconstruction of Copernicus' likely course of study in astronomy at Cracow, Goddu concludes that he would have been introduced to the issue of the realism or fictionalism of astronomical models employing orbs like those found in Peurbach's *Theoricae novae*. He would also have encountered some of the problems with Ptolemy's lunar models, the problem of the equant as a violation of the requirement that celestial motions be uniform about their geometrical centers, disputes about the order of the planets Venus and Mercury, as well as problems of the accuracy of tables, and the problem of calendar reform [161]. The chapter concludes with a summary of Brudzewo's *Commentriolum*.

The thesis that Copernicus derived important aspects of his work from Islamic sources usually locates his contact with this tradition in Italy. Goddu, however, has already expressed scepticism about Islamic influence. In the chapter on Copernicus in Italy, he assigns only a very modest role to Copernicus' studies there. Rather surprisingly, Goddu affirms that there is 'little reason' to think that Copernicus learned any more natural philosophy in Italy [172: cf. 185]. Instead, he presents a picture of a student whose time was exclusively occupied by the study of law and medicine, neither of which, according to Goddu, played any major role in his astronomical innovations.

Copernicus left Italy with a degree in canon law but with no formal qualification in medicine. Although his legal training was clearly relevant for the administrative positions that he held in later life, he actually returned to Warmia as his uncle's personal physician and continued to practise medicine for the rest of his life. However, in contrast to the obvious ongoing interest in astronomy shown by the remains of his library, in later life 'as far as we know...[Copernicus] did not own a single legal text' [180]. Goddu concludes that Copernicus' legal training would have been important as a continuation of his training in dialectic, especially in the use of *loci*, that was begun at Cracow. Copernicus would have encountered very similar doctrines on the status of conditionals during his legal studies, reinforcing the doctrines that he would have encountered in John of Glogow. Similarly, legal training would have given him extensive opportunities to develop skill in dialectical argumentation using topics, that is, in techniques 'aim[ing] to support conclusions that are more probable than alternatives' [182]. More generally, Goddu also proposes that his legal training exposed Copernicus to the idea of intellectual reform, based on new situations and experiences, as a necessary part of the tradition of legal methods and scholarship. In this way, his experience as a law student might have reinforced or rendered more plausible to him the general idea of astronomical reform motivated by the problems of the Ptolemaic tradition that he had encountered in Cracow.

Goddu makes even less of Copernicus' medical education. In both legal and medical studies, we lack the kind of detailed evidence that Goddu has deployed so impressively in the first part of his book; and it is, therefore, not possible to reconstruct Copernicus' course of study in the same detail. Nonetheless, while at Padua, Copernicus only studied medicine for two years—not the three required for a degree by the university statutes. Goddu concludes that he had no intention of getting a formal qualification. Returning to the question of whether Copernicus used his time in Italy to learn more natural philosophy, Goddu establishes firmly that Copernicus was back in Poland by January 1504 at latest. As he arrived in Padua only in 1501 and was both studying medicine and preparing to receive a degree in law, Goddu suggests that he would have had no time for protracted study with the celebrated Averroist natural philosophers at Padua or to learn about the Tusi-couple from them, as proposed by Di Bono [1995]. Goddu also denies that Copernicus owed his knowledge of Greek to his Italian education. Although he began learning Greek in Italy, he really taught himself the language after he returned home through the project of translating Theophylactus' *Letters* [194–195].

Goddu's minimization of the importance of Copernicus' time in Italy is perhaps best understood as a consequence of the limitations in historiographical method that he imposes on himself, although they are not always observed consistently or indeed prudently. We will return to these general considerations in section 2. Even so, Goddu acknowledges that Copernicus' Italian years did include several decisive encounters. First, he lived and worked with the astronomer Domenico de Novara, who may have been important in many ways, but especially for introducing Copernicus to Regiomontanus' *Epitome of the Almagest*, a book that he used constantly in his later research program in astronomy. Second, during his stay in Italy, he became acquainted with the writings of Pico della Mirandola and perhaps with Ficino's translation of Plato's *Parmenides*, which Goddu thinks was especially important in the development of Copernicus' views on method.

In chapter 7, Goddu proceeds chronologically to Copernicus' first statement of his heliocentric ideas. The main concern of the first part of the chapter is to identify and describe the sources that Copernicus assimilated after leaving Italy and on the way to creating his first brief exposition of heliocentrism, the *Commentariolus*. Goddu argues for a date of composition around 1510 and also suggests in passing a novel hypothesis for the appearance in Copernicus' work of the mathematical device now known as a Tusi-couple.

The identification of books owned or used by Copernicus has proceeded primarily by the analysis of notes and marginalia in books that survive at Uppsala. Here Goddu has made a major contribution to scholarship by independently re-evaluating the claims of earlier writers in the light of his careful analysis of Copernicus' handwriting.

In addition to revising a number of earlier attributions, Goddu concludes that, before 1515, Copernicus had access to Regiomontanus' *Epitome of the Almagest* and Bessarion's *In calumniatorem Platonis*, an important source for Neoplatonic ideas. Goddu offers an original argument that Copernicus had also read Ficino's translation of Plato's *Parmenides*, on the basis of the attribution of notes from part 1 of a copy of Ficino's translation of Plato's *Opera omnia* that is preserved at Uppsala. He also makes a case that Copernicus had read Plutarch, pseudo-Plutarch in the translation of Valla [229], and Pliny's *Natural History* [237ff], all of which contain interesting snippets about the cosmologies of Aristotle, the Pythagoreans, and the Stoics. He concludes by considering the possibility that Copernicus had read Achillini, an Averroist critic of Ptolemaic astronomy whose most relevant work appeared while Copernicus was a student at Bologna. This section concludes with an important thesis, that

> *[Q]ualitative* (not technical) mathematical issues *led [Copernicus]* to adopt heliocentrism with its geokinetic consequences. [243: emphasis in original]

The balance of the chapter considers the content of the *Commentariolus*. Goddu's main achievements are the identification of the method that led Copernicus to his postulates and an extended consideration of the date of composition. Goddu concludes that the *Commentariolus* was begun after May 1509 and completed in 1510, thus strengthening the arguments for a date that was already accepted among Copernicus scholars.

Based on the attribution of notes in the Parmenides dialogue that he has just argued for, Goddu proposes that the method which Copernicus uses is dialectical in that it borrows from Plato an approach that examines both the assertion and denial of every relevant hypothesis. As an example, the axiom in astronomy of uniform, circular motion is adopted because its denial is absurd [251]. Non-uniform motion of celestial objects would make them, and the regularity with which they repeat their patterns, incomprehensible. A second important result is that Copernicus rejects Ptolemy's equant device on the grounds that it violates that axiom. However, he accepts that, while the partial orbs of planets have different centers, all the total orbs have a single center. In short, the overall organization of his cosmos follows Ptolemy and Peurbach, although the center of the orbs is relocated. The primary motivation for considering heliocentrism is that if the planetary orbs are ordered around a single center according to a single principle (the distance-period relation), then the Earth cannot be that center [254]. Goddu dismisses Swerdlow's alternative proposal [1973] that Copernicus considered and rejected a Tychonic system, although some of Goddu's reasoning is based on a faulty understanding of the nature of *Theorica* orbs (a matter which we will consider further below). Goddu opts instead for Martianus Capella's system as the inspiration for Copernicus' choice of center.

The chapter ends with an extended presentation of Mario Di Bono's work on Tusi-couples, which had suggested a second possible class of non-Islamic sources for Copernicus' use of the device, the Paduan Averroists. Goddu has, in effect, already dismissed this in his chapter on Copernicus' time in Italy and he has a further original suggestion of his own to make. He does not present it here but in an appendix to the book, where he concludes that rather than encountering techniques for generating straight line motions from circular motions in some Islamic source,

> Copernicus did not invent or discover these solutions independently, but that he adopted and modeled solutions deriving immediately from Brudzewo and Regiomontanus, and indirectly from Oresme and Hesse. [484]

We find that a number of points in this discussion are off-track, and return to the whole topic in detail in section 3 below.

Chapter 8 is titled 'Copernicus as logician'. Goddu published major articles on these issues in 1995 and 1996. Since then, his main new conclusion is that, after his Cracow period, Copernicus would have encountered very similar ideas about the paradoxes of implication and conditionals during his legal education in Italy [275]. Goddu asserts that on matters of philosophy, especially dialectics and what would today be called methodology, 'Copernicus resolved the principal issues well before 1520' [276]. The techniques considered here are, thus, presented as a stable foundation for Copernicus' mature work and especially for the composition of the *De revolutionibus*.

The title of this chapter is perhaps unfortunate, as the main inferences Goddu describes are not deductive and, hence, only logical in an extended sense. Although these arguments can be reconstructed as enthymemes—Goddu does so in an appendix—he portrays Copernicus as primarily using a dialectical method that employs topics as his main persuasive technique in the *De revolutionibus*. A reader using modern logical theory or expecting modern logical arguments may not find Copernicus' arguments satisfying as presented by Goddu, but the important historical question is whether they were satisfying to Copernicus' contemporaries. Goddu shows that such techniques were both common and uncontroversial before and during Copernicus' education; however, he does not go on to address the efficacy of these methods as persuasive techniques beyond a blanket negative appraisal of Copernicus' success.

Goddu adopts a format for presenting topical arguments developed by the 20th-century philosopher Stephen Toulmin [293 and n40]. The arguments are presented in three elements: the claim or conclusion that the argument supports, the grounds offered for the claim, and the warrant or rules that license a conclusion of this type from evidence of the kind offered. As Goddu notes, the schema attracted strong criticism when Toulmin introduced it and, to make matters worse, Goddu employs it in an abbreviated form [293n40] that omits the 'qualifications' or conditions that often have to be specified to establish an evidential link. For all these reasons then, modern readers unfamiliar with this pattern of argument may find it, and the reconstructions of Copernicus' arguments by means of it, difficult to follow and less than persuasive. Although this section of Goddu's book provides a detailed layout of the subjects addressed in *De revolutionibus* 1, the reader leaves the section puzzled by why Copernicus would have chosen these techniques if they raised even more potential objections against his already controversial conclusions. A second consequence of Goddu's almost exclusive attention to topical arguments is that little attention is paid to the theory of demonstration, which was widely acknowledged by Copernicus' contemporaries—and indeed by Copernicus himself—as a higher standard that astronomy ideally ought to meet.

The next chapter is on natural philosophy and expands Goddu's claim that Copernicus modifies rather than rejects Aristotle's principles, in line with the earlier claim that he should be seen as a reformer of Ptolemaic astronomy rather than as a revolutionary intent on its overthrow. Goddu notes that although the 'reformer' reading of Copernicus has recently made headway among scholars, the 'revolutionary' reading is still common when people come to address Copernicus' relation to Aristotle's physics. Goddu argues that 'Copernicus was inclined to revise principles rather than to reject them altogether' [330]. For example, Copernicus modifies Aristotle's principle that an object can have only a single simple natural motion, arguing that a more complex principle is required to accommodate falling bodies on a rotating Earth. The radial component of such a body's motion is clearly natural, in accord with long standing Aristotelian ideas. However, the circular component of a falling body's motion, which is required for it to 'keep pace' with the spot toward which it is falling as the Earth rotates, is also a natural motion [344]. The ultimate application of this new principle is to the Earth itself, which has not one but three natural motions. The chapter proceeds with sections devoted to Copernicus' views on the movement of celestial objects, the movement of terrestrial elements, and the possible infinity of the cosmos.

There is a sustained analysis of the very limited extent to which impetus theory can be attributed to Copernicus: for Goddu, it appears to be confined to situations where a non-natural or violent component was required to explain motion. Throughout, Goddu admits that he may be being more systematic and scrupulous than Copernicus himself, concluding that 'Copernicus' doctrine of motion was undeveloped' [344] and that 'From the brief account that Copernicus provides we are hardly able to construct a coherent physics...' [353]. A further important conclusion is that, at least in the limited area where mathematical astronomy and physical cosmology overlap, priority belongs to the former and not to the latter, as Aristotle and his followers had accepted [337]. As a whole, the reasoning attributed to Copernicus in this chapter further illustrates Goddu's general thesis about the role of dialectical techniques in Copernicus, while also showing their limitations. After all, arguments that show convincingly that one of a series of complex positions is wrong do not establish an alternative demonstratively; at best, they make it more likely. As Goddu notes, Copernicus' efforts to persuade Aristotelians of his position 'have to be counted in the short term among the most miserable failures in the history of philosophy' [359]. So, perhaps the unpersuasive character of the preceding chapter on logic is historically accurate and unavoidable.

Having considered several typical issues in natural philosophy from Copernicus' viewpoint, Goddu proceeds in the penultimate chapter to matters that he calls 'mathematical cosmology'. Here he considers Copernicus' views on the nature of hypotheses and the still controversial question of the extent to which Copernicus endorsed the existence of celestial spheres or orbs as part of his overall cosmic scheme. The chapter has one generally positive feature and one generally negative one. First, Goddu's response to the question of the reality of spheres and orbs in Copernicus converges on the position introduced by Barker and Goldstein [1998] and developed in detail by Barker in more recent papers. Although this is a positive sign, and perhaps an indication of a wider convergence among scholars in the field, Goddu then goes on to discuss a number of points about the nature of *Theorica* orbs in a much less satisfactory way. We will consider these issues in more detail below.

In the conclusion and epilogue, Goddu reiterates his main points about the nature of the Aristotelian tradition in Copernicus' time and his relation to it. The tradition was multifarious. Copernicus saw himself as working within it, not rejecting it. The main positive thesis that Goddu proposes is that Copernicus used topical arguments, particularly the dialectical topic 'from an integral whole', to supply a relevance condition linking the antecedent and consequent of conditional sentences expressing hypotheses. Goddu repeats that, judged historically, Copernicus' innovations were wholly unpersuasive to other Aristotelians.

In the epilogue, he then goes on to review the reception of Copernicus' work by a series of contemporaries and successors beginning with Rheticus and prominently including Tolosani, Offusius, Maestlin, and Kepler. The main sources that he employs are the marginalia and notes recorded in Gingerich's *Census* [2002], and he locates one passage in which Maestlin plausibly can be seen as recognizing and endorsing Copernicus' rhetorical strategy. Goddu proposes an interesting fivefold division of positions about the status of Copernicus' hypotheses:

(1) Tolosani regards them as physical and remains a geocentrist.
(2) The Melanchthon circle and many others take the primary content to be mathematical and retain a geocentric framework.

(3) Maestlin, Goddu suggests, also emphasizes the mathematical content of the hypotheses but shifts to a heliocentric framework.
(4) Rheticus and Kepler represent positions closer to Copernicus himself, both accepting heliocentrism while endorsing the hypotheses as physically significant in different ways. Rheticus is closer to the tradition of natural philosophy and
(5) Kepler innovates in physics.

We look forward to hearing more about Goddu's ideas for understanding the reception of Copernicus, an account that will be based, we hope, on consideration of a larger group of Copernicus' readers and of sources that reach beyond the notes written in the *De revolutionibus*.

Although the main text ends with the epilogue, the book concludes with just over 50 pages of appendices. These include supporting material for some earlier sections—for example, a summary list of dialectical topics from Peter of Spain, extended Latin excerpts from important sources used at Cracow, and the reconstructions of Copernicus' deductively invalid topical arguments as valid enthymemes. Appendix 6 presents an 'Excursus on Transmission' that amplifies Goddu's scepticism about Islamic sources and perhaps belongs in the main text. The same could be said about three paragraphs on Copernicus' understanding of Ptolemy that appear as appendix 9. There is an extensive bibliography, an index of names divided pre- and post-1800, an index of places, and an index of subjects.

In summary, then, Goddu's Copernicus learned a great deal about topical or dialectical reasoning and natural philosophy at Cracow. He also studied astronomy and was introduced to the standard problems of the Ptolemaic tradition, but what he learned had little direct bearing on his later astronomical innovations. He learned little new natural philosophy in Italy, although he strengthened his command of certain techniques in dialectical reasoning. He apparently got onto the track of some important new ideas in astronomy but not from Islamic sources. Exactly when he decided that astronomy needed to be reformed and that the reform entailed abandoning geocentrism for heliocentrism, is not clear, although his legal education may have fostered the idea of intellectual reform. When he returned to Poland, he began a process of self-education that led fairly quickly to a concise statement of his novel heliocentric ideas. The equant problem motivated the reform of Ptolemaic astronomy but the idea that the planets should be ordered around the center of the cosmos according to a single principle, the distance-period relation, led to the postulation of heliocentrism. Copernicus adopted dialectical techniques as the means to establish his new ideas and, having accepted a stable methodology by 1520, he continued to use it for the rest of his life. He worked on the material that would become the *De revolutionibus*, solving technical problems but without adding any major new ideas, and without any real interest in publishing until Rheticus persuaded him to finish the book and get it into print.

This is an oddly unsatisfactory outcome from a long book with many valuable sections. Several big questions go unanswered:

- ‘Where and why did Copernicus begin his research into heliocentrism?’
- ‘Why are there so many similarities between his work and the work of Islamic astronomers?’
- ‘Did he really select a methodology that would itself have been predictably unpersuasive to contemporaries?’
- Although Goddu presents Plato as a key source of Copernicus’ dialectical method, why should Copernicus be seen as working within an Aristotelian tradition and addressing Aristotelians, rather than working within a nascent Platonic tradition and addressing Platonists?

Goddu’s detailed work on the Cracow context and on the textual evidence of various doctrines in Copernicus has lasting value. However, it is difficult to take the picture that he presents as definitive: too many historical factors are excluded or unaccounted for.

2. Critical evaluation

We begin our more critical section of this review by wishing that Goddu’s long book was longer—or at least more comprehensive in its coverage of Goddu’s own work. Goddu repeatedly draws on his earlier papers but also repeatedly fails to include the full range of evidence and argument that he had presented in them. It would have been very valuable for this book to be self-contained: this would aid readers new and old by bringing together in a single place all the work that is now scattered through various and, in some cases, rather inaccessible articles. This is perhaps less the author's fault than a sign of inadequate editorial advice. On occasion, the placement of material also indicates lapses in editing. It makes little sense, for example, to review the contents of the *Sphere* and the *Theorica* in chapter 5, when the reader needs this information to understand the discussion of the curriculum at Cracow and Copernicus' education in astronomy that takes place in chapters 1 and 2.

The book's greatest strength is also its greatest weakness. Its greatest strength is Goddu's meticulous attention to sources, both historical and contemporary. In some cases, he brings a unique spectrum of talents to the re-examination of key historical evidence. The book's greatest weakness is its discomfort with historical evidence or historical conclusions beyond this sort of textual analysis. A case in point is the nature of Copernicus' knowledge of Brudzewo's *Commentariolum*. In the early chapters, Goddu builds what we consider an overwhelming contextual case for Copernicus' having knowledge of this book. But in chapter 10, he concludes:

> In my opinion there is no evidence that Copernicus knew this text directly, but he very likely received instruction on astronomy and astrology from Albert's students. [370n15]

This is bizarre. On Goddu's own account, Brudzewo controlled the curriculum that Copernicus studied, trained Copernicus' teachers and, consequently, Brudzewo's text was used pervasively at Cracow. It is surely unlikely, therefore, that Copernicus was not familiar with the book by the time he left Cracow. How, then, can Goddu say 'there is no evidence that Copernicus knew this text directly'? There is, admittedly, no direct, textual evidence and that is, unfortunately, what Goddu seems to want.

Another blind spot is the Platonic tradition. Goddu documents Copernicus' familiarity with the *Timaeus* and *Laws* [226], in addition to the works of Plutarch and pseudo-Plutarch [229ff.]. But with the exception of his original argument for the influence of Plato's ideal of dialectic from the *Parmenides*, little use is made of the Platonic tradition. Although its influence is acknowledged, Goddu continues to insist that Copernicus should be read as an Aristotelian but without rebutting those who derive more of the structure of Copernicus' work from Plato [e.g., De Pace 2009]. Again the problem seems to be lack of the kind of evidence that Goddu prizes, specifically annotations or direct references.

The question of whether to accept historical evidence and inferences deriving from the context in which Copernicus was working goes to the heart of several contentious issues treated in the book. For example, Goddu rejects Swerdlow's analysis of Copernicus' route to heliocentrism [1973, 1976]. Two important premises of Swerdlow's account are that 16th-century astronomers accepted the real existence of the orbs described in the *Theoricae novae*, and that Copernicus, educated in this context and addressing an audience that shared these values, also accepted the reality of celestial orbs. Both these premises were rejected—inappropriately—by Rosen [1973, 3.123n326, 1975, 1976]. Since the Rosen-Swerdlow controversy, a pall has hung over the whole question of the reality of celestial orbs and the status of the basic texts in astronomy at the time of Copernicus. Rosen's side of the controversy denied the reality of the orbs. He also implicitly denigrated the astronomical texts in use before Copernicus and, with them, the astronomical context in which Copernicus was educated and worked.

Goddu's comments on both the *Sphere* and the *Theoricae novae* seem influenced by the continuing effects of this controversy. For example, he considers the *Sphere*, 'of almost no practical use' [147]. It is not clear what standard of judgment is being invoked here—no use to whom? Practical for what? The *Sphere* was an introductory textbook that served its purpose if it taught students the overall structure of the geocentric cosmos and, particularly, the names and definitions of such basic celestial circles as the tropics, equator, and ecliptic. It is practically useful for learning how to describe the location of a celestial phenomenon (e.g., the rising or setting of an object). It does not really teach astronomical calculations—those come later for Copernicus, along with planetary theory and the motions of the Sun and Moon, in the *Theoricae novae*.

Goddu is equally unsympathetic to the *Theoricae novae*, saying, for example, 'The traditional accounts of orbs never make it clear how the orbs are consistent with the mathematical models' [378 text to n41]. Actually, as Goddu comes close to acknowledging elsewhere, the main purpose of the *Theoricae novae* was precisely that, beginning on its very first page. Any student who had mastered it would have

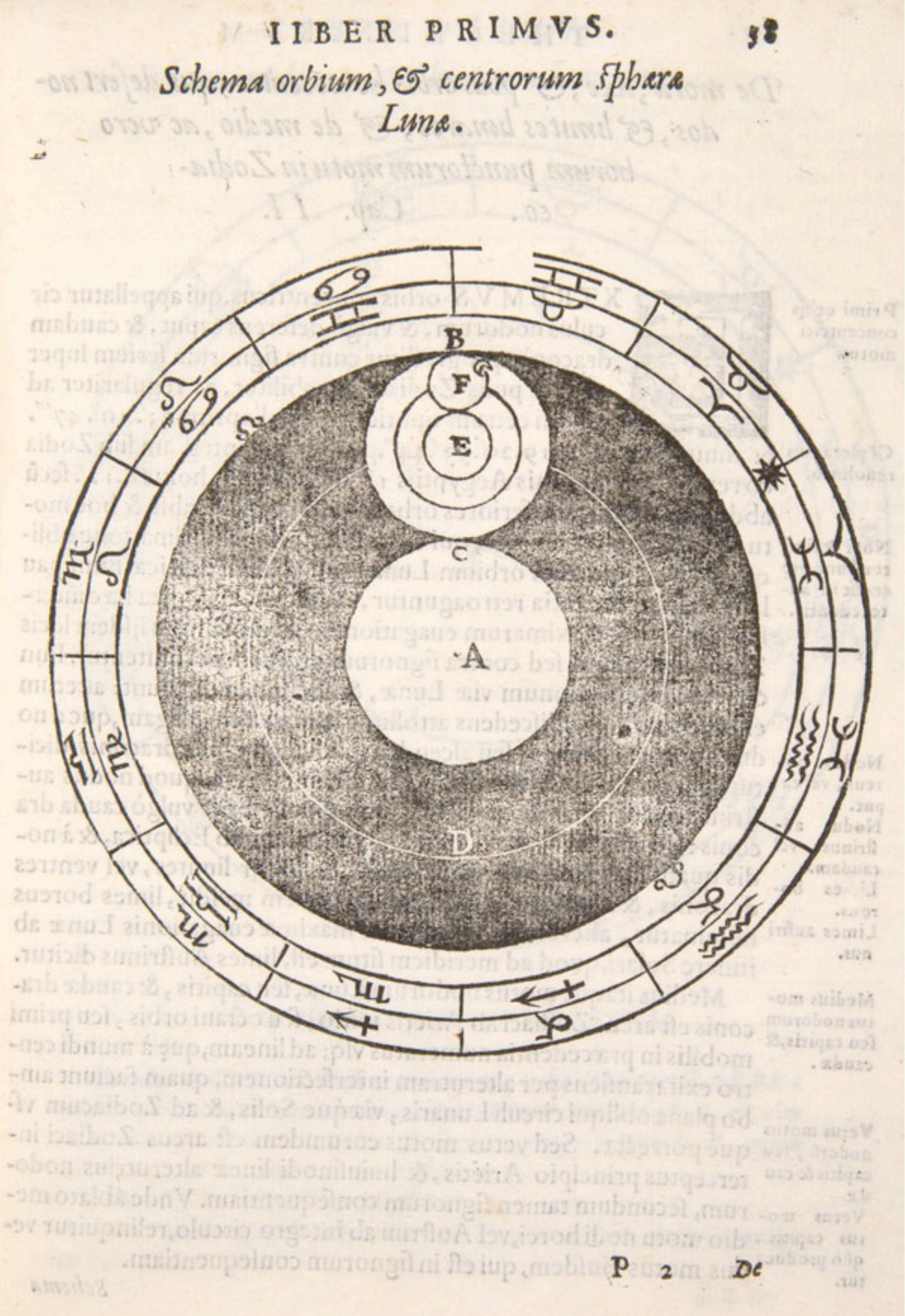


Figure 1. Double epicyclic orb-system for the Moon
[Magini 1589]

been able to pass back and forth between physical orb-models and the two dimensional mathematical representations found in books like Ptolemy's *Almagest* or the old *Theorica*. Perhaps understandably, Goddu is confused about the construction of the *Theoricae novae* orbs and the constraints that they place on astronomical and cosmological theories.

A particularly important instance concerns the double-epicyclic model introduced by Copernicus for the Moon and for one type of Tusi-couple. Goddu mistakenly thinks that orb-models for double epicycles are impossible on the grounds that they require the penetration of orbs:

> If Copernicus thought that the epicycles are also spheres, then it is apparent that the spheres do penetrate one another, for the secondary epicycle penetrates the space occupied by the primary epicycle and the primary epicycle penetrates the space occupied by the deferent sphere. [249]

In fact, orb-systems for double-epicyclic models were not only possible, they were actually published by Copernicus' successors, for example, by Giovanni Antonio Magini in 1589 and 1608 [see Figure 1, p. 322; Swerdlow 1976, 137–141]. To put the matter briefly: in the standard *Theoricae novae* introduced by Peurbach, the eccentric orb carries the epicycle in the form of a small sphere within it 'like a gem-stone in a finger ring' to use a convenient and historically accurate metaphor. The epicycle sphere may rotate freely within its socket and does not penetrate the eccentric orb at all. Similarly the planet is carried within the epicycle sphere as it rotates and again without penetration. Copernicus' models merely add a second epicycle sphere in the place of the planet and embed the planet in this further, smaller sphere. All these objects can now rotate freely within their sockets as they are carried by the orbs in which they are embedded. If the ratio of the radii and speeds of the two epicycle spheres is 2:1, a Tusi-couple and, hence, motion in a straight line on the part of the planet will result. This arrangement of two orbs with one rotating freely within the other was proposed by Tusi himself when he introduced what we now call the Tusi-couple in the *Tadkhira* [see 263 Fig. 1; Ragep 1993, 1.196–199].

A similar misconception occurs when Goddu discusses an aspect of Swerdlow's analysis:

> Some experts[1] speculate that Copernicus anticipated a Tychonic arrangement that he would have rejected because of the interpenetration of the spheres of Mars and the Sun. In fact the spheres of Mercury and Venus, even on the Capellan arrangement, penetrate the sphere of the Sun, yet Copernicus says nothing about it. [254 and nn137, 138]

Copernicus says nothing about it because there is no penetration of spheres: the Sun is carried at the center of the orb of Mercury; the orb of Mercury is carried at the center of the orb of Venus. Both orbs rotate in their sockets without penetrating the orbs inside or outside them. For Tycho, the whole arrangement would be carried by the eccentric orb of the Sun in the same manner as an epicycle orb in the *Theoricae novae*. It is the eccentric orb carrying this entire collection (Sun, Mercury and Venus) that penetrates the (eccentric) orb of Mars and leads Tycho to abandon solid spheres.

Despite these errors and confusions, Goddu concludes, correctly, that it is too simple to classify Copernicus as a realist or a fictionalist in the sense used by modern commentators. In a series of papers beginning with Barker and Goldstein [1988], the general question of the reality of celestial spheres in the 16th century has been addressed in a new way. In addition to new historical evidence, these authors suggested that the issue should be reappraised against the background of the theory of demonstration employed by 16th-century scientists, rather than by means of 20th-century categories like realism and instrumentalism paired with retrojections of 20th-century philosophical conceptions of scientific method. But 16th-century theories of scientific method—in other words, the theory of demonstration—required a three-step process to arrive at a definitive causal explanation, which was taken to correspond to the correct physical constitution of the system considered. The three steps may be labelled 'demonstratio quia', 'negotiatio', and 'demonstratio propter quid'.[2]

---

[1] Swerdlow is cited in the subsequent note; but see especially 1976, 134–136.

[2] These three terms correspond to demonstrations of an effect from possible causes, the reasoning process that eliminates all but the actual cause, and demonstration from the actual or real cause. For example, consider as an effect the shape of the shadow cast on the Moon by the Earth during an eclipse, which according to Aristotle always has a semicircular edge. It is possible to explain a shadow with a semicircular edge by postulating

These issues are treated only briefly by Goddu, who advocates treating Copernicus' method as almost exclusively dialectical. But, given the existence of a clearly articulated standard for scientific knowledge in the form of complimentary arguments *quia* and *propter quid* specified in the theory of demonstration, it is not enough to give a positive account of Copernicus' alternative method of dialectic. It is also necessary to address why Copernicus failed to meet the requirement of demonstration (if that is in fact the case) and how he expected to make headway with an audience that took these standards as the basis for scientific knowledge, without at least himself addressing the divergence between his own methods and the method of demonstration and giving some substantial motivation for doing so.

Barker and Goldstein concluded that the requirement that astronomical theories represent real physical systems was generally accepted by 16th-century astronomers, with the *proviso* that there were special difficulties in meeting this standard in astronomy. Although most people agreed that demonstrations *quia* were possible, the remaining steps in establishing a unique cause were not available. It was not that there was no truth of the matter to discover but rather that terrestrial observers lacked the evidence needed to discover it. Hence, Barker and Goldstein described 16th-century astronomers as 'permanently frustrated realists'. A small number of Copernicans were controversial exceptions.

In subsequent work, Barker has developed these themes and applied them to the specific case of Copernicus. First, he has argued that the introduction of Peurbach's *Theoricae novae* led to the general adoption of partial and total celestial orbs by most mathematical astronomers in the 16th century—the century of Copernicus' career and written work. This did not include natural philosophers and

---

an Earth that is cylindrical, disk shape or spherical, among other options. Each of these explanations would be a *demonstratio quia*. By appealing to the principles of mathematics (a process that is not always possible or successful), we may eliminate all but the last option on the grounds that the sphere is the only solid body that will cast a semicircular shadow regardless of the direction from which it is illuminated. This reasoning constitutes the *negotiatio*. If we now explain the shape of the shadow's edge by appealing to a spherical earth, which we have established is the actual cause, that will constitute a *demonstratio propter quid*.

a few astronomers who supported Averroes' strict reading of Aristotle's physics as it applied to the substance of the heavens, and who accused the astronomers in the Ptolemaic tradition as modified by Peurbach with perpetrating 'fictions'. Copernicus was, therefore, educated in astronomy at a moment when Peurbach's eccentric and epicyclic celestial orbs were becoming a standard feature of courses in astronomy at universities throughout Europe, and this innovation was being opposed by Averroists who insisted that all celestial spheres were concentric to the Earth. In both the *Commentariolus* and *De revolutionibus*, Copernicus was addressing an audience trained in Peurbach's methods and aware of this dispute. There are clear indications that he expected his mathematical models to be understood as collections of partial and total orbs in the former but not in the latter. Copernicus' silence on orbs in his *De revolutionibus* has caused continuing controversy [see Barker 2009, 2011].

Recently Barker has suggested that Copernicus' failure to present orb-models in his *De revolutionibus* has several obvious explanations—some of which are also noted by Goddu. The first, and perhaps least important, is that although Copernicus appears to have taken the *Theorica* as a model for his abbreviated presentation in the *Commentariolus*, he took Ptolemy's *Almagest* as his model for the *De revolutionibus*. Considering orbs would be natural in a *Theorica* but the *Almagest* presents only mathematical models—notably two-dimensional combinations of circles that model motions in longitude. Adding orb-models might also be deemed unnecessary because, in principle, anyone who had read Peurbach could construct orb-models for the new mathematical models that Copernicus was introducing. But there were at least two major obstacles to completing this task. The first obstacle was Copernicus' inability to choose between mathematically equivalent models (for example, eccentric circles *versus* concentrics carrying epicycles) that would lead to quite different orb-models. Copernicus repeatedly presented such alternatives in his solar and planetary models. Although it would be possible to construct orb-models for each one, there was no obvious way of choosing between them. A second major difficulty, and one much more difficult to resolve, was the overall structure of Copernicus' cosmos. In Ptolemy, and in Ptolemy's system as reframed by Peurbach, each set of partial orbs formed the total orb for a single planet. The total orb for one planet fitted perfectly inside the total orb for the next planet out, with the fixed stars forming a boundary to the whole system. There was no empty space between orb-clusters. However, calculating the dimensions of total orbs in Copernicus by the same methods and then applying the fundamental ordering of distances introduced by Copernicus showed that there were substantial gaps between the orbs for Copernicus, and an enormous gap between the outermost surface of the orbs of Saturn and the inner surface of the orb of fixed stars [Barker 2011].

The issue of the reality of celestial orbs and the constraints on their physical construction reappears when Goddu examines the Tusi-couple. He again mistakenly asserts that the corresponding arrangement of orbs would be impossible according to Aristotle's physics on the grounds that there can be no void in the heavens [262 and n150]. But as we have shown above, double-epicyclic models require neither penetration nor voids. Orb-models were fundamental to the application of Tusi-couples in astronomy by Maragha astronomers and their successors, and almost any plausible source for Copernicus' knowledge of Islamic astronomy would have contained diagrams showing such orb-models. So here, Goddu's misapprehensions about the nature of the *Theorica* orbs may have not only misled him on the status of orb-models corresponding to Copernicus' mathematical constructions, but also contributed to his resistance to the possibility of Islamic sources for Copernicus' ideas and especially for the Tusi-couple.

3. Copernicus' debt to Islam

Goddu's treatment of Tusi-couples in Copernicus leans heavily on the work of Di Bono [1990, 1995]. Goddu follows Di Bono in classifying Tusi-couples into three types. The first of these is a 'spherical version with parallel axes and radii in the ratio 1:2' [see 263: Fig. 1]. The second is a 'spherical version with oblique axes and equal radii' [see 265: Fig. 2] and the third is a 'plane version with equal radii' [see 266: Fig. 3]. Only the third or flat version is supposed to appear in the *De revolutionibus* [see Figure 2, p. 328]. However the alleged separation between the first and third forms collapses immediately when we note that a flat version of the Tusi-couple can be generated in the first version by the equatorial circles of the rotating spheres. These are the circles that appear in Copernicus' figure, read by Di

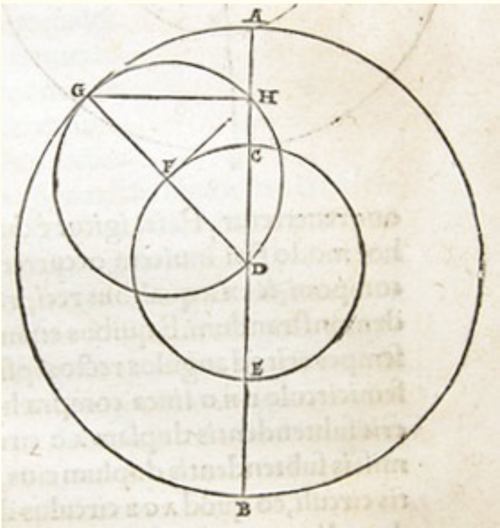
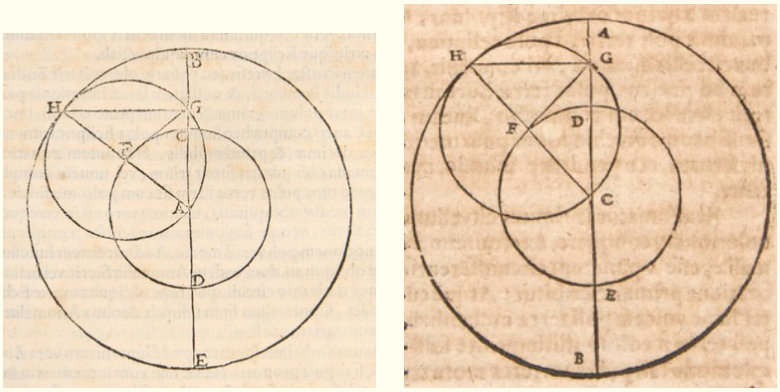


Figure 2. Copernicus' diagram explaining a Tusi-couple [1543]

Bono and Goddu as the unique third version. Goddu goes on to cite Di Bono's denial that the figures representing the third version are the same in Tusi and in Copernicus on the grounds that the first version of the device (ratio 1:2) appears in Tusi's *Tadkhira* but the third version ('equal radii') appears in the *De revolutionibus* [267]. This is simply a mistake. Both figures appear in the *Tadkhira*: the former figure shows the motion of the Tusi-couple spheres as four 'snapshots'; the latter shows the general case and converts to the flat form as indicated above. This latter is the counterpart of the diagram found in Copernicus.[3]

Goddu also quotes Di Bono to refute the key argument that the lettering of the diagrams is identical in Tusi and Copernicus, 'and even where they are, such a coincidence can be explained by mathematical conventions of nomenclature in geometrical figures' [267]. In his original article, Hartner [1973] established that the lettering in Tusi and in Copernicus was suspiciously similar—indeed identical, except for the lettering of the point at the center of the smaller circle.

[3] Hartner 1973, 421 fig. 3, reproducing MS Leleli 2116, fol. 38b–39a. Cf. Ragep 1993, 1.198–199.



(a) Magini 1589

(b) Maestlin 1596

Figure 3. Later versions of Copernicus' diagram

Copernicus' diagram contains one additional auxiliary circle (tracing the locus of the motion of the center of the smaller circle comprising the couple) and one additional line (connecting points G, H). Excluding the points introduced by these amendments, Hartner established that five out of six letters in Tusi's diagram were phonetically transliterated in Copernicus' diagram. George Saliba [2007, 200–201] has since explained the discrepancy at the sixth point. Quite simply, the original Arabic letter at that point would easily be misread by someone not very confident in Arabic as the letter translated in Copernicus. We are left with the historical fact that the diagrams in Tusi and Copernicus are identical, including—inexplicably—the orientation of the radii drawn for the large and small circles and all the lettering.

For further evidence that the correspondence between Tusi's lettering and Copernicus' is not accidental or the result of 'mathematical conventions of nomenclature in geometrical figures', consider the subsequent appearances of the same diagram in Europe in 1589 and 1596 [see Figures 3 (a)–(b), p. 329].[4] The first of these is in Magini's

[4] The earliest copy of Copernicus' diagram that we have found is in the *Hypotyposes orbium coelestium* published at Strasbourg by Rihelius in 1568. The author is sometimes given as Conrad Dasypodius, although this appears to be a work begun by Erasmus Reinhold and completed by Caspar Peucer, whose authorship is acknowledged in a later edition. For the complex publishing history of the work, see Barker 2009. The corresponding diagram

Novae coelestium orbium theoricae, to which we have already referred. The second is Michael Maestlin's new edition of Georg Rheticus' *Narratio prima*, appended to the first edition of Kepler's *Mysterium cosmographicum* and paginated continuously with it. Although both figures reproduce the circles and lines in the same orientation, the lettering differs significantly in both cases.

Taken together the similarities between the diagrams in Tusi and Copernicus are almost inescapable evidence that Copernicus had access to some version of the Arabic astronomical tradition. But Goddu uses other arguments borrowed from Di Bono to cast doubt on this, including the variation in the versions of the Tusi-couples used or implied in the *Commentariolus* and *De revolutionibus*. Di Bono claims that the first and second patterns occur in the former but only the third in the latter, where it accounts for 'variability in precession, variability in the obliquity of the ecliptic, the variations in latitude of all planets, and the variation in longitude for Mercury' [267]. But, according to Swerdlow and Neugebauer, all three versions of the Tusi couple appear in the *De revolutionibus*. Copernicus' famous figure [1543, fol. 67r, v] may be classified as the third type. But Swerdlow and Neugebauer [1984, 1.47: cf. 1.408–409] classify the Tusi-couples used in the precession model, the obliquity of the ecliptic, and the latitude variation mechanism as the second type with oblique axes. As for the longitude model for Mercury, they suggest that this implicitly contains a Tusi-couple of the first type that is carried forward from the *Commentariolus*, where it is explicit, although Copernicus does not explain it in the later book.

Goddu's scepticism about Copernicus' access to Islamic astronomy rests upon a doubtful analysis of the historical evidence and the mathematical interconnections between the versions of the Tusi-couple. To that extent, it is also incomplete. In Copernicus studies, Islamic astronomy is the elephant in the room and the Tusi-couple is only its trunk. Goddu simply never mentions a range of equally important issues beyond Tusi's models. Most significant of these is the mathematical device dependent on 'Urdi's lemma', which is

appears on p. 529 of the Strasbourg edition but is lettered in Greek. The first eight letters of the Greek alphabet have been used. Similarly, Copernicus used the first eight letters of the Roman alphabet. However, the Strasbourg edition letters the points in a different order.

a much larger part of the elephant [Saliba 2007, 151–155]. Used repeatedly by Copernicus, it is not a mathematical variation of the Tusi-device. Copernicus used Tusi-couples of the first type to replace Ptolemy's equant in the longitude models for the outer planets in the *Commentariolus*. But in the *De revolutionibus*, he used a second method by redefining the eccentricity of the main circle producing motion in longitude and transferring one quarter of the eccentricity to the radius of a very small epicycle. This mathematical technique was developed by Mu'ayyad al-Din al-Urdi (d. 1266), a collaborator of Tusi, and used subsequently by al-Shirazi (d. 1311), Ibn al-Shatir (d. 1375), al-Qushji (d. 1474)—who used it in a new Mercury model—and Copernicus [Saliba 2007, 202–205]. Copernicus used it both in the models for the outer planets as well as in a Mercury model which seems to be copied from Ibn al-Shatir. In contrast to the Tusi-couple, then, which appears in the *De revolutionibus* only in the few very limited applications already indicated, the Urdi-construction appears repeatedly and in the main models.

Now it is, perhaps, logically possible that Copernicus could have developed the Tusi-couple himself or from European sources before he wrote the *Commentariolus* and then went on to develop the separate Urdi-device and to apply it in the models for the outer planets, while also developing, among other things, the same Mercury model as Ibn al-Shatir. But for this to have happened, as Ragep has succinctly put it, we are required to believe that

> the 500 years tradition of non-Ptolemaic astronomy in Islam was recapitulated in Europe in scrupulous detail in a 50 year span in the last part of the fifteenth century. [2005, 363]

And this is to consider only the most conspicuous correspondences in mathematical models. Ragep [2007] has recently suggested that a variety of other seeming novelties in Copernicus, including the subordination of physics to mathematical astronomy noted by Goddu and Copernicus' attitude to Aristotle's physics, can equally be located in plausible Islamic sources. Suppose we grant—as Goddu proposes in an ingenious appendix—that there is a European tradition providing access to a device equivalent to the Tusi-couple, perhaps starting with Oresme and perhaps even available to Copernicus. Given the massive collateral evidence, we submit that it is virtually certain that Copernicus had direct access to Islamic materials, quite apart from his access to parallel European traditions, and that what we see in the *De revolutionibus* is a brilliant if imperfect adaptation of them.

4. Two versions of Copernicus

According to Goddu, Copernicus learned a lot in Cracow but very little in Italy. In Cracow, he was interested in logic, natural philosophy, and technical astronomy. Because he was interested in astronomy, he was also interested in everything connected to it, which turns out to be natural philosophy and logic. According to Goddu, in Italy, he added depth in logic but nothing in natural philosophy and was already treating astronomy as a vocation. The knowledge of law and medicine that he acquired had no real bearing on his astronomy (and he made none of the connections a modern reader might conjecture about methods, for example, linking medicine and astrology). So, although we are to believe that his stay in Italy was decisive for his astronomical development, while there he is not supposed to have added to his knowledge in any of the related fields that interested him at Cracow. He also did not have time to study with the Averroist critics of Ptolemaic astronomy at Padua. On his return to Poland, his self-education progressed along with his program of astronomical reform. According to Goddu, he adopted dialectic as a method and developed several new mathematical techniques, including three forms of the Tusi-device, based on hints in earlier Northern European sources which may not themselves have employed the device and which certainly did not apply it to the problems that concerned Copernicus. His methodology and his views on natural philosophy remained unchanged for most of his adult life and the *De revolutionibus* represents the late distillation of several decades' work.

The limitations of this reconstruction of Copernicus are entirely the limitations of Goddu's method which considers textual evidence but little else and gives little consideration to Platonic influences in addition to Aristotle. It may well be true that there is textual evidence to support this 'minimalist' Copernicus. But direct citations, notes, and marginalia are not the only classes of evidence available to the historian.

By contrast, we propose that Copernicus was educated in the latest astronomical ideas at Cracow, including the *Theoricae novae* orb-models, that he heard about the Averroist attacks on them, and became familiar with internal problems of Ptolemaic astronomy such as the equant. He also belonged to a circle of Cracow Humanists that evaluated Plato positively. However, the decisive years for the development of his ideas seem to have been those he spent in Italy. He deepened his understanding of the sources and problems of Ptolemaic astronomy under Domenico de Novara, a direct link to Regiomontanus, and through him to Peurbach and Bessarion. At the same time, and again with the likely mediation of Novara, Copernicus encountered a further set of criticisms of Ptolemaic astronomy in Pico della Mirandola's *Disputationes adversus astrologiam divinatricem*, regarding the order of the planetary spheres and the length of the tropical year [Westman 2011, 84–87]. Also, at Bologna, he very probably encountered the renewed objections to Peurbach's version of Ptolemaic astronomy raised by the Averroist Alessandro Achillini. While in Padua, or perhaps in nearby Venice, Copernicus encountered Arabic astronomical ideas containing techniques from many different authors [Langermann 2007, 295–296; Morrison 2011, 388], including two different methods of circumventing the equant problem. Possibly, this material constituted the common source for the subsequent appearance of Tusi-couples in the works of Amico and Fracastoro. Finally, it was in Italy that Copernicus was exposed to Platonic and Neoplatonic influences that either deepened his already existing Platonist views on the status and tasks of astronomy, or, more likely, incited him to study Plato in depth, which he did with the aid of Bessarion's *In calumniatorem Platonis* and Marsilio Ficino's translation of Plato's works.

On returning to Poland, Copernicus began to work through all the material that he had accumulated in Italy and in the process convinced himself that geocentrism was indefensible. Between the *Commentariolus* in, perhaps, 1510 and the completion of his *De revolutionibus* in the early 1540s, Copernicus continued to work with the material that he had gathered, to read, to make observations, and to add to his fundamental ideas. These included increasing his knowledge of Plato and keeping abreast of the new Averroist astronomical theories. Consequently, when he wrote the preface to the *De revolutionibus*, he claimed to be remedying defects in both Ptolemaic astronomy and its Averroist alternative.

Goddu has written an indispensable book for specialists, and one that has many valuable ideas for those with a less specific interest in

Copernicus the man, his period, or his contribution to the history of science. Where textual evidence is central or an appraisal of textual evidence is required, Goddu's work is nearly irreproachable. However, the wider field of historical evidence is barely touched, and, where this affects matters as important as the influence of Islamic science on the origins of the modern Western scientific tradition, readers are advised to supplement Goddu's approach with a cultural and contextual one.

BIBLIOGRAPHY

Barker, P. 2009. 'The *Hypotyposes orbium coelestium* (Strasbourg, 1568)'. Pp. 85–108 in M. A. Granada and E. Mehl edd. *Nouveau Ciel Nouvelle Terre—La Révolution Copernicienne dans l'Allemagne de la Réforme (1530–1630).* Paris.

________ 2011. 'The Reality of Peurbach's Orbs'. Pp. 7–32 in P. J. Boner ed. *Change and Continuity in Early Modern Cosmology.* Heidelberg/New York.

________ 2013. 'Albert of Brudzewo's *Little Commentary* on George Peurbach's *Theoricae novae planetarum*'. *Journal for the History of Astronomy* 44:1–24.

Barker P. and Goldstein, B. R. 1988. 'The Role of Comets in the Copernican Revolution'. *Studies in History and Philosophy of Science* 19:299–319.

________ 1998. 'Realism and Instrumentalism in Sixteenth Century Astronomy: A Reappraisal'. *Perspectives on Science* 6:232–258.

Birkenmajer, A. 1900. *Commentariolum super Theoricas novas planetarum Georgii Purbachii in Studio Generali Cracoviensi per Mag. Albertum de Brudzewo diligenter corrugatum A.D. MCCCCLXXXII.* Cracow.

Copernicus, N. 1543. *De revolutionibus orbium coelestium.* Nuremberg.

De Pace, A. 2002. 'Copernicus against a Rhetorical Approach to the Beauty of the Universe: The Influence of the *Phaedo* on *De revolutionibus*'. Pp. 77–115 in E. Kessler and I. MacLean edd. *Res et Verba in der Renaissance.* Wiesbaden.

De Pace, A. 2009. *Niccolò Copernico e la fondazione del cosmo eliocentrico con testo, traduzione e commentario del libro I de* Le rivoluzioni celesti. Milan.

Di Bono, M. 1990. *La sfere omocentriche di Giovan Battista Amico nell'astronomia del cinquecento.* Genoa.

———— 1995. 'Copernicus, Amico, Fracastoro and Tusi's Device: Observations on the Use and Transmission of a Model'. *Journal for the History of Astronomy* 26:133–154.

Gingerich, O. 2002. *An Annotated Census of Copernicus'* De Revolutionibus*, Nuremberg 1543 and Basel 1566.* Leiden/Boston.

Goddu, A. 1995. 'Consequences and Conditional Propositions in John of Glogovia's and Michael of Biestrzykowa's Commentaries on Peter of Spain, and Their Possible Influence on Nicholas Copernicus'. *Archives d'Histoire Doctrinale et Littéraire du Moyen Age* 62:137–188.

———— 1996. 'The Logic of Copernicus's Arguments and His Education in Logic at Cracow'. *Early Science and Medicine* 1:28–68.

———— 2006. 'Reflections on the Origin of Copernicus's Cosmology'. *Journal for the History of Astronomy* 37:37–53.

Goldstein, B. R. 2002. 'Copernicus and the Origin of His Heliocentric System'. *Journal for the History of Astronomy* 23:219–235.

Hartner, W. 1973. 'Copernicus, the Man, the Work, and Its History'. *Proceedings of the American Philosophical Society* 117:413–422.

Kepler, J. 1596. *Mysterium cosmographicum.* Tübingen.

Langermann, Y. T. 2007. 'A Compendium of Renaissance Science: *Ta'alumot hokmah* by Moses Galeano'. *Aleph* 7:285–318.

Maestlin, M. 1596. ed. *Georgii Joachimi Rhetici narratio prima.* Pp. 85–160 in Kepler 1596.

Magini, G. A. 1589. *Novae coelestium orbium theoricae, congruentes cum observationibus N. Copernici.* Venice.

———— 1608. *Novae coelestium orbium theoricae, congruentes cum observationibus N. Copernici.* Mainz.

Morrison, R. 2011. 'An Astronomical Treatise by Mūsā Jālīnūs *alias* Moses Galeano'. *Aleph* 11:385–413.

Ragep, F. J. 1993. *Nasir al-Din al-Tusi's* Memoir on Astronomy. 2 vols. New York.

_________ 2005. ''Alī Qushjī and Regiomontanus: Eccentric Transformations and Copernican Revolutions'. *Journal for the History of Astronomy* 36:359–371.

_________ 2007. 'Copernicus and His Islamic Predecessors: Some Historical Remarks'. *History of Science* 45:65–81.

Rosen, E. 1973. *Copernicus: Complete Works*. Warsaw.

_________ 1975. 'Copernicus' Spheres and Epicycles'. *Archives Internationales d'Histoire des Sciences* 25:82–92.

_________ 1976. 'Reply to N. Swerdlow'. *Archives Internationales d'Histoire des Sciences* 26:301–304.

Saliba, G. 2007. *Islamic Science and the Making of the European Renaissance*. Cambridge, MA.

Swerdlow, N. M. 1973. 'The Derivation and First Draft of Copernicus' Planetary Theory'. *Proceedings of the American Philosophical Society* 117:423–512.

_________ 1976. 'Pseudodoxia Copernicana'. *Archives Internationales d'Histoire des Sciences* 26:108–158.

Swerdlow, N. M. and Neugebauer, O. 1984. *Mathematical Astronomy in Copernicus'* De Revolutionibus. 2 vols. New York.

Westman, R. S. 2011. *The Copernican Question: Prognostication, Skepticism and Celestial Order*. Berkeley, CA/London.

The Monochord in Ancient Greek Harmonic Science by David Creese

Cambridge/New York: Cambridge University Press, 2010. Pp. xvi + 409. 126 color illust. ISBN 978–0–521–84324–9. Cloth $110.00

Reviewed by
Stefan Hagel
Austrian Academy of Sciences
stefan.hagel@oeaw.ac.at

The image of Pythagoras at the monochord has become iconic for the beginnings of music theory in terms of the quantification of pitch-relations. The fact that it is in all probability entirely unhistorical has been dawning upon specialists for quite a while. It is to be hoped that David Creese's thoroughgoing study will be received widely enough as to eradicate lastingly the misleading conceptions both of an archaic monochord as a scientific instrument and of a primarily Pythagorean origin of pitch-studies.

But this is by no means Creese's only conclusion and probably not even one of his major concerns. Rather, while tracking the extant evidence related to the tool called monochord or κανών over the centuries, he is interested in the ways in which the material instrument, as well as its more immaterial echoes in writers' minds, interact with arithmetical, geometrical, musical, and physical conceptions in different periods and different authors to produce various flavors of harmonic or 'canonic' science.

The chronological examination is preceded by an important chapter on ancient scientific method, which outlines similarities as well as differences between the κανών and scientific instruments such as the armillary sphere, and shows that the use of the monochord must be understood in the context of mathematical diagrams (and later also in relation to tables). The relation of its origins to a type of arithmetical proposition illustrated by quasi-geometrical diagrams in which lines represent numbers was decisive for the development of its use; although it would have been perfectly straightforward to divide the tone into two equal semitones geometrically,[1] such a truly

[1] Here a reference to Busch 1998, 115–117 would have been in order.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 337–351

geometrical approach was simply never envisaged and the monochord continued to hover (somewhat uncomfortably in my view) between the two mathematical disciplines as a device sporting a continuous line that merely served to illustrate ideas about integers. In a sense, the monochord's 'geometry' remained one-dimensional and I have sometimes wondered if Creese's insistence on a geometrical aspect is not in fact exaggerated, after all. In any case, Creese persuasively argues that this limitation is best understood in historical terms: the establishment of integer arithmetic as a paradigm of mathematical harmonics had predated the monochord.

Creese's historical overview starts by examining and discarding the anecdotal stories related to Pythagoras, blacksmith and all, that pop up many centuries later. Surprisingly, even to a sceptic, 'the invention of the monochord is not attributed to Pythagoras in any extant text before the third century AD [90].' This absence of evidence for its usage before much later inevitably raises the question: 'How, then, were the ratios of the concords discovered?' Apart from the traditional art of tuning panpipes, Creese makes a case to associate this discovery with Hippasus' metal disks, whose pitch would have been proportional to their thickness (while it is inversely proportional to string or pipe lengths). This characteristic would account better for early pitch theories, where higher pitch is associated with greater force or speed.

Creese is always extremely cautious about not taking absence of evidence for evidence of absence, but in the end he settles on the reasonable hypothesis which dates the origin of the monochord in the later fourth century BC, and argues in detail that its existence is not presupposed either in Philolaus or Archytas. When the monochord finally makes its appearance, it is in a perfectly designed (a couple of logical flaws notwithstanding) logico-mathematical argument, the *Sectio canonis*, which owes much to a tradition of public demonstrations (ἐπιδείξεις and ἀποδείξεις) that is fruitfully delineated by Creese. The *Sectio canonis* may be a reaction to Aristoxenus' apodeictic *Elements*, whence perhaps also the preoccupation of grounding its propositions on a physical basis: it contains a very advanced account of sound-transmission, which Creese interprets as inspired by a newly conceived need to integrate the behavior of strings within an acoustical theory of pitch. Although the κανών is not mentioned until the last chapters, one might use it profitably also as a 'diagram-reading instrument' to illustrate some of the preceding propositions.

In later writers, the notion of a 'division of the monochord' becomes more problematic, as this may indicate a merely intellectual (algebraic) enterprise just as well as the real thing (involving at least one sounding string and a ruler), especially whenever big numbers which cannot literally be applied to an instrument are involved. It is unclear, though doubtful, whether this expression may also denote the mere listing of interval sizes. Eratosthenes' figures are plausibly interpreted as a practically successful (albeit mathematically problematic) representation of Aristoxenus' standard shades of the three genera. Along the way, Creese points out that Eratosthenes had developed a root-extracting device which would have made it possible, at least in principle, to construct precisely Aristoxenian sorts of intervals—and that it is very unlikely that he would have applied it to string-division.

Later sources fall within the Roman period, when harmonics had become a subject in the schools and was produced in handbooks in which Aristoxenian and mathematical positions are juxtaposed with different degrees of confusion or mutual integration. In this era, different approaches are typically understood in terms of the roles they attribute respectively to reason and perception, a criterion extensively discussed by Ptolemaïs. Significantly, the term 'canonics' emerges in this context, so that the monochord, after its centuries-long obscurity, finally becomes the hallmark of its discipline.

As a consequence, the sources start discussing different ways to use it as well as some possible complications. Creese elucidates the principles of practical elegance that inform the different approaches that were used to demonstrate the ratios of the concords. For instance, plucking both sides of a string minimizes the number of necessary bridge positions (Panaetius). Here the contrast between a static diagram and the sequential adjustments of the monochord's bridge becomes decisive. However, the octave plus fourth, concordant to the ear but described by a mathematically dissatisfying ratio of 8:3 remains a problem[2] until Ptolemy finally manages to integrate it inconspicuously in his system. Most importantly, in Adrastus

[2] Adrastus, for example, lists it as a consonance but fails to demonstrate it on the κανών.

we find the first evidence of an evolving awareness of fundamental issues regarding the monochord's precision as an instrument. Creese discusses the divisions of Nicomachus' *Harm. man.* (but surprisingly not Boethius' detailed account, which is widely believed to be based on Nicomachus' lost fuller treatment), as well as the accounts of the so-called 'Timaeus Locrus' and of Thrasyllus. Here the major shortcomings of the monochord become finally evident: whenever larger structures are envisaged, the required large numbers can be transposed to a physical ruler only approximately and large ranges lead to ill-sounding small string-lengths.

The final chapter is entirely devoted to Ptolemy, who developed the κανών in regard to its technical practicalities and used it in accordance with hitherto unmatched scientific standards, directly (and largely effectively) addressing issues of precision and reliance as well as reconnecting the study of harmonics with the concert-goer's musical experience. Ptolemy perceived the need of a many-stringed experimental instrument in order to assess properly the validity of musical structures by playing melodies rather than intervals, and introduced new geometrical concepts into its construction. As an astronomer, he was familiar with the demands and the practical issues involved in the production of precise instruments and he made sure that his various new models of many-stringed 'monochords' would obtain interval measurements that satisfied unprecedented scientific standards, pushing potential technical errors beneath the threshold of perceptibility. He also introduced a common 120-units ruler with hexagesimal fractions, perhaps replacing a set of differently marked rulers that were used in former demonstrations: now the positions of notes in different divisions could be compared easily in absolute terms. Even the geometrical construction of the ἑλικών, which used the intercept theorem to construct the most important musical ratios, was made into an ingenuous practical instrument that enabled modulations of key by means of a sliding common bridge. When Ptolemy finally ventures to attach musical meaning to astronomical phenomena, the ecliptic is envisaged as a gigantic curved 'monochord', populated by planets that act as moving bridges.

Creese's lucid and sometimes humorous style makes thorny technical questions accessible—in this, his obvious ease with mathematical problems helps a lot. I have only one serious issue, regarding the fundamental dichotomy between the 'Pythagorean' and the 'Aristoxenian' viewpoints, which I present in the following along with a number of quibbles, if only to pay due tribute to such a committed book.

Like so many writers who focus on the mathematical strand of harmonics, in my opinion Creese does not do perfect justice to Aristoxenus' alternative approach [27–30, 42–46, 163]. Although he gives an exemplarily reasoned and, in principle, accurate account of the questions involved, his conclusion that Aristoxenus talks about the same intervals as the mathematical theorists and is, therefore, ultimately wrong, is not warranted since it is based on the argument that the precise size of the perfect fifth and fourth was uncontested in antiquity.[3] The actual difference in pitch that is in question is smaller than the 100th part of a tone and the difference in consonance was, therefore, not assessable by ancient means: Creese's comparison to the practice of interval-'sweetening' mentioned by Aristoxenus is misleading, since this would have involved intervals as large as an eighth or a tenth of a tone, depending on which modern interpretation one prefers.

Consequently, it is true that Aristoxenus and the mathematical faction talk about 'precisely' the same intervals but only according to the standards of precision available to them; therefore, the Aristoxenian system is nevertheless entirely consistent. It does not involve three different kinds of semitones, albeit of barely perceptible difference (as Creese claims), because it does not start from the same mathematically defined interval sizes for the consonances. Rather it is based on an effectively 'tempered' system, as we would call it, for which Aristoxenus was in the position to prove experimentally that it consisted of incontestable consonances.[4] In doing so, Aristoxenus implicitly denied the *precise* identification of consonances with simple

---

[3] This problem seems eventually to be acknowledged on page 163.

[4] Creese's description of Aristoxenus' experiment [45] implausibly assumes that he worked his way through the circle of fifths only in one direction and then compared the last pitch against the first. Such a procedure is incompatible with the epideictic structure of the argument: for practical reasons, a demonstration (in school or in public) almost certainly demanded a row of 12 strings set up in advance. Their pitches would have formed an interlocking series of 'perfect' fifths and fourths, much as on the modern piano. (It must be borne in mind that the shortcomings of modern tempered tuning are not related to fifths and fourths but to thirds, which do not bear on Aristoxenus' reasoning at all). Cf. Franklin 2005, 19–21.

ratios of whatever; the study of a physical basis for the phenomenon of consonance was alien, anyway, to his project of harmonics.[5] Therefore, when Creese says that Aristoxenus' system was 'founded on the assumption that irrational intervals exist' [163], he interprets it from outside and, thus, unfairly: the Aristoxenian paradigm holds no assumptions on the basis of which such a kind of irrationality could possibly be derived.

The monochord could not mediate between the two positions because the differences in question fall outside its scope: the measured pitch differences could never be smaller than those accessible to the hearing which establishes the measured pitches, a determination governed as well by the precision with which the hand can shift the bridges (and work the tuning pegs, in the case of Ptolemy's many-stringed versions).[6] Therefore, it is hardly possible to demolish the Aristoxenian assertion that there are six tones in an octave by means of the κανών [228]: the construction on the instrument can do no more than reproduce what has been worked out arithmetically, and this only imperfectly.[7] This fact seems misunderstood, when Creese emphasizes, following Ptolemy, that

---

[5] A possible way out of the dilemma why the consonances would happen to seem to close to simple ratios if they are not identical with them is hinted at in *Harm.* 68.10–12: pure consonance might not occur at a precise interval but within a very small range. This view is also much closer to practice, since no physical manipulation of an instrument could ever realize mathematical precision, while the very notion of consonance proves that it can be achieved.

[6] An additional problem in establishing precise intervals on the monochord by ear may arise from the friction between string and bridge, which, when the latter is moved, causes the tension to adjust not smoothly but in small steps. The smaller this effect is, the poorer is the sound quality for a given bridge material.

[7] On a κανών with a free string length of about 90cm, i.e., built as large as possible so as to still play in the range of the cithara (as seems implied by Ptolemy's frequent reference to this instrument), the differences between tempered and perfect fifths and fourths translate into differences in bridge positions ranging from 0.5 mm to 1 mm, depending on the pitch of the bounding notes. When performing a corresponding true perception-based experiment, each position would thus have to be established with a precision significantly below half a millimeter in order to get halfway consistent results.

> the amount by which six tones exceed an octave is constant, as the carefully controlled demonstration proves: the result will be no different if it is repeated.... [327]

This is true, but since that demonstration merely construes on the κανών the results of calculations, as long as arithmetic does not change, the results cannot be different by definition. It is similarly true that the 'apparent aberration...cannot be explained away as observational error', but this is only because observation had never contributed to the setup of the experiment. Contrary to what Creese seems to imply, this 'experiment' highlights that, when it comes to deciding between Aristoxenus' and the canonists' viewpoints, the 'experimental' instrument cannot do anything to confirm the arithmetical prejudices on the basis of which it is set up.

And here come the quibbles. The first is that at some points I would have liked Creese to engage more closely with the Greek texts that he quotes (sometimes extensively—for which I am immensely grateful), instead of relying on existing translations. For instance, he poses the important question 'How did «κανών» come to mean 'monochord'?' [17], but I am not sure his following remarks, which focus on the ruler's straightness rather than its function as a tool of measurement, are meant as an answer. In any case, I think that in Ptolemy's explanation [Düring 1930, 5.12f] «κανονίζειν» should not be translated as 'to straighten' but as 'to measure out' [cf. 228, 260].

Similarly, I do not think that the citation of Ptolemaïs *ap.* (?) Porphyry *In Harm.*

> κανονικὸς δ᾽ ἐστὶ καθόλου ὁ ἁρμονικὸς ὁ περὶ τοῦ ἡρμοσμένου ποιούμενος τοὺς λόγους [Düring 1932, 23.5f]

is correctly translated by

> ...who constructs ratios in connection with attunement. [77f; 217]

The passage abounds with forms of «λόγος» that drift between the various meanings of the word. Here, however, a mathematical meaning would be very awkward, «ποιεῖσθαι τοὺς λόγους» being such a common expression for 'talking'. The definite article and the final position rather suggest that the definition focuses on «περὶ τοῦ ἡρμοσμένου»: so '...who talks about attunement'.[8] Otherwise, the definite article in «τοὺς λόγους» would compel us to construe the implied contrast as somebody 'who constructs the ratios in connection with something else'—not really a viable alternative. As a definition, this is, of course, weak and does not serve to distinguish the κανονικοί from other types of ἁρμονικοί. But the context shows that «καθόλου» must be understood in a strong sense, in contrast to the usual distinctions: 'In a general sense, «κανονικός» denotes the ἁρμονικός, the one who talks about attunement'. This reflects the entirely parallel «καθόλου» in [Düring 1932, 22.25–27], where «κανονική» is introduced as the 'Pythagoreans'' general term for ἁρμονική. And finally, this explains the irritating «καί» (dropped by the ms. of g) in 23.9 «εἰσὶ δὲ καὶ ἑκάτεροι τῷ γένει μουσικοί» ('and *similarly*, both are generically μουσικοί').

Also, I suspect Creese misunderstands Ptolemaïs' evaluation of the Aristoxenian μουσικοί [231: on Düring 1932, 24.5–6]: 'She...regards it as unsurprising that they cannot make intelligent use of the κανών'. As I understand it, Ptolemaïs rather regards it as natural that these people mistrust the monochord:

> κατὰ δὴ τούτους εἰκότως οὐ πανταχῇ αἱ λογικαὶ ὑποθέσεις τοῦ κανόνος σύμφωνοι ταῖς αἰσθήσεσιν
>
> According to these people, to be sure, it is only to be expected that the rational postulates of the κανών are not always concordant with the perceptions. [Barker 1989, 241]

Nicomachus, *Harm. man.* [von Jan 1895, 254.19–21], «διὰ πασῶν εὑρήσει τὸν ἀπὸ τῆς ἡμισείας πρὸς τὸν ἀπὸ τῆς ὅλης ψόφον μείζονα», cannot mean 'you will find that the sound from the half string stands at an octave to the larger sound from the whole' [262, 274], but '...that the sound from the half string is an octave larger than the one from the whole'. This is also demanded by the context, where Nicomachus explains that pitches are inversely related to strings: the higher sound is the 'larger' one.

In Ptolemy, *Harm.* at Düring 1930, 17.20–26 [306f], I find it misleading to translate the participles with present tenses and to

[8] The supposed meaning would call for something like «ὁ ἐν λόγοις σκοπῶν τὸ ἡρμοσμένον».

take them as starting a new sentence ('It [i.e., the string on the κανών] does not acquire its pitch in a random way...'). These do not characterize the monochord in general but specify the conditions (i.e. Ptolemy's innovations) under which it will perform the outlined task: so, '...since it will not acquire its pitch in a random way...'.

Related, finally, is also a slight infelicity on page 259, where a discussion of the algebraic abacus follows a Greek text mentioning the *abacus* (ἄβαξ) in the sense of the geometer's drawing-board.

So much for the translations. Here are some more questions concerning which I should also like to take up in discussion with the author.

Although it is true that Adrastus claims that notes whose pitches stand in no rational proportion are not properly called notes (φθόγγοι) but sounds (ἦχοι) [5], I do not think this view can necessarily be projected to 'the earliest stage of musical thinking in the mathematical tradition' [23]. The claim is either rather sophisticated in allowing the existence of 'notes' only in harmonic relation to each other, i.e., only if more than one is present in the context of a single performance (which Aristoxenus' definition would not necessarily entail [Da Rios 1954, 20.16–19]) or very silly in that it effectively confuses notes and intervals, which would be just typical for Adrastus' arguments.

Although Creese assumes that the monochord was introduced in the later fourth century, he is always at pains to point out that the evidence for its absence earlier is only negative. But, at the least, we might obtain some positive evidence for the monochord's recent introduction from the beginning of *Sect. can.* prop. 19, where the full vibrating length of the string is equated with the βόμβυξ ('entire pipe'), thus introducing the division of strings in relation to the boring of finger-holes in woodwinds [cf. Hagel 2005, 60; 2009a, 333n21]. This is conceivable only if pipes had been the model instrument for similar demonstrations until shortly before.

With regard to Hippasus' disks, Creese states that 'the behavior of pitched sound becomes visibly and directly (not inversely) analogous to the behavior of numbers' [95]. But this presumes a modern view in which higher pitch is conceived of as, well, 'higher', just as higher numbers are 'higher'. The related spatial concept, at least, evolves only in later antiquity.[9] In original Greek thought, high pitch is ὀξύς (sharp), and low pitch βαρύς (heavy) [see 297n29]. From this point of view, the disk experiment runs contrary to expectation: the 'heavier' sound is produced from the lighter disk. Similarly, the material aspects of 'sharp' include especially thin objects such as blades and points [cf. Düring 1930, 7.25–27], while 'sharp' sounds arise from the thicker, blunter disks. Note that even a late author such as Ptolemy [Düring 1930, 8.3–5] may pair the terms, viz. «ἐλάττων τε καὶ ὀξύτερος (ψόφος)» ('smaller and higher-pitched (sound)') [cf. 297]. From an archaeo-technical viewpoint too, the thickness of bronze disks is not a very plausible starting point—would there have been tools for reliably measuring, let alone producing, two disks with a given ratio of thickness?[10] Tuning disks, I presume, involved grinding them down until they rang in the desired harmony. Afterwards, one might have gauged whether the resulting measurements were in accord with known numeric relations rather than detecting these. So, I think that much of the argument for disks breaks down and that pipes may have been a more plausible candidate; note too that lutes had always been lingering at the peripheries, even though Greek iconography remains long silent about such instruments.

Repeatedly, Creese argues that a physical theory of sound in terms of speed or force is 'difficult, if not impossible, to illustrate...with chordophones of any sort' [120]. But pitch is often perceived as τάσις (tension), a concept familiar to a nation of lyre players, where higher pitch is directly connected with exerting greater force in tuning. The cultural awareness of the likeness of bow and lyre string, expressed not only in the figure of Apollo but also in the terms «νεῦρον»/«νευρά» applicable to both, and famously in *Od.* 21.406–411, nicely illustrates how a string of greater tension supplies greater force and speed. All this makes the fact that Archytas [Diels and Kranz 1951, 47 A1] does not mention strings all the more remarkable.

Creese gives a short account of how Archytas may have derived his enharmonic interval ratios from a hypothetical procedure for tuning a lyre [128n149]. There is very little evidence for enharmonic lyres. But even granted their existence, I just do not see why anybody in

[9] See Rocconi 2002: note Nicomachus' idea (quoted below) that 'larger' sounds are higher sounds.

[10] Cf. the technically well-informed remarks at Düring 1930, 17.16–20.

his right mind would tune them 'starting with the upper tetrachord tuned to Archytas' diatonic and the lower tetrachord tuned to the "ditonic" diatonic' (and this by a process which still involves non-concordant adjustments), instead of simply tuning the desired scale.

Creese accepts the unity of the entire document known as the *Sectio canonis*, mainly because of the fact that its title states a project that is not fulfilled until its last chapters [133f; 171]. But even if the title is original (all we know is that it was current in Porphyry's time, almost 600 years later), the original project is plausibly concluded with chapter 19, covering the 'fixed notes' [cf. Hagel 2009b, 247f.]. Notably, its final sentence proudly states 'Thus all notes of the non-modulating scale will have been found on the κανών' ('emended' to 'fixed notes' by modern editors). Chapter 20, which I regard as a later addition, has nothing of that kind.

The term «διαύλων» printed from Plutarch, *Non posse* 1096a–b and translated as 'double-auloi' in the passage [139] is Einarson and Lacy's implausible solution to a textual problem (mss. «δι' αὐλῶν»): it denotes the race course, never that double-pipe called the *aulos*.

I am not sure whether Aristoxenus really 'disallowed the exception of *Posterior Analytics* 1.7' [154], which enabled harmonics to make use of mathematics even though they are different sciences. Aristoxenus still employs arithmetic to add and subtract intervals; he only severs the 'Pythagorean' tie to physics, entirely in line with Aristotle's concept. As a consequence, it is doubtful whether the *Sectio* can be viewed as the more Aristotelian rival project to Aristoxenus' *Elementa* [156].

Creese takes pains to explain why Plato adopted the ditonic diatonic of all possible systems and tends to view the *Sectio* as dependent on his tradition [162]. But since both Philolaus before and the *Sectio* after Plato have the same system, which derives directly from the tuning of heptatonic chordophones,[11] it is Archytas who is the odd one out and Plato's 'choice' demands little explanation [see Franklin 2002].

[11] Cf. the historically doubtless related Near Eastern system, although without ratio mathematics, and the probably independent contemporary Chinese scales.

With his usual salubrious caution, Creese does not assume that the monochord stands behind [Aristotle] *Phys. prob.* 19.12, where by division of the string 'two *nētai* (*e'*) are produced in the *hypátē* (*e*)' [169]. As Creese remarks, the production of harmonics on the lyre is a possible option here. However, the claim that there are *two* of the higher notes would either rest on a remarkable observation of the string's second vibration mode or on an extrapolation from another instrument. The lute, on the other hand, is an unlikely basis, since the higher part of the string is never plucked. On a fretted lute, producing *two* notes at the octave is, anyway, plainly impossible. On a lute without frets, the precise pressing point for the octave is not in the centre of the string; and if the resulting notes are made equal by pressing the middle point, these are sharper than the octave, both because of the length of string occupied by the finger and because the act of pressing increases the tension. All known theories of woodwind pitch presuppose that the sound 'exits' through the first open finger hole, which also precludes the notion of *two* high notes.

Creese wrestles with the question why Eratosthenes kept the traditional ratios of the ditonic diatonic (i.e., what is often called the 'Pythagorean' tuning) instead of producing something closer to Aristoxenus' standard diatonic, and even considers that these ratios are compiled from a different work [208]. However, Aristoxenus is unmistakable that this variant of diatonic results from tuning by consonance—which inevitably yields the ditonic diatonic when described in terms of ratios.

Regarding Adrastus, a crucial question concerns which portion of Theon of Smyrna's text one attributes to him. However, I do not think that one can excise Hiller 1878, 69.12–70.19 but still keep the sequel [254 with n121],[12] since 71.3 refers directly back to 70.16f: note the unusual «ἀεί».

It is agreed that Adrastus' problematizing the spatial extension of a bridge is mistakenly applied to the bisection of the tone; and Creese rightly points out that, if taken seriously, it would tear down the entire edifice of canonic science [256]. However, Creese's restatement of Adrastus' argument in correct terms is practically relevant only if

[12] In this respect, Creese is following Barker [1989, 223n59f], who considers as a possible solution that Theon is summarizing a thought which he did not fully understand.

one uses the parts of the string on both sides of the bridge (which Adrastus does not do): otherwise, nothing would prompt us to place the *center* of the bridge at the desired point instead of its *edge*, which, at least theoretically, makes the lengths directly comparable (the real problem is one of tension, which Creese addresses later on).

In the Greek text of Nicomachus, *Harm.* [von Jan 1895, 260.12–17], the usual comma is printed before «ἕως τοῦ ἑπτακαιεικοσιπλασίου» [263], apparently indicating that the parenthesis about Plato is understood as closed: '...but in the way of Timaeus of Locri (whom Plato also followed): right up to the twenty-seven-fold ratio'. But Creese understands the last part as belonging to the description of what Plato did [267f]. This strikes me as unlikely. Anyway, such a reading would be possible only if following Timaeus 'right up to the twenty-seven-fold ratio' is the point in question (and not a mere side-thought, as which it would make sense only in a limiting meaning, as if Plato could have followed Timaeus *beyond this expanse*, which is of course nonsense). Thus, the nature of the alleged shortcomings of Eratosthenes and Thrasyllus is settled by the text: they did not use the (musically absurd) cosmic 'Pythagorean' range for their divisions of the κανών.

Finally, I think that Creese's criticism of Ptolemy's reasoning regarding a string's distortion by the bridge [313f] is partially flawed: it does not seem to me that the string is 'stretched...also...by an amount equal to the arc' where the string touches the bridge. This arc, which Creese regards as added in some way to the total length, merely reflects (and compensates) the respective arcs on the end-bridges which the string now no longer touches (in Creese's Fig. 6.2, $\Theta M = HK + ON$). So the distortion is fully accounted for by the effect of pushing the string upwards, which, as Creese rightly notes, increases the tension.

All this is not to detract from the fact that David Creese has filled an important gap in the studies of ancient Greek music in a masterful way, in a book that enriches the libraries of everybody interested in this particular field of study, philologist or music historian, as well as in the development of scientific thought in general.

BIBLIOGRAPHY

Barker, A. 1989. *Greek Musical Writings: II. Harmonic and Acoustic Theory.* Cambridge.

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

Busch, O. 1998. *Logos syntheseōs. Die euklidische* Sectio canonis*, Aristoxenos, und die Rolle der Mathematik in der antiken Musiktheorie.* Berlin.

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

Diels, H. Kranz, W. 1951. edd. *Die Fragmente der Vorsokratiker.* 3 vols. 6th edn. Berlin.

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

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

Franklin, J. C. 2002. 'Diatonic Music in Greece: A Reassessment of Its Antiquity'. *Mnemosyne* 55:669–702.

________ 2005. 'Hearing Greek Microtones'. Pp. 9–50 in S. Hagel and C. Harrauer edd. *Ancient Greek Music in Performance.* Vienna.

Hagel, S. 2005. 'Twenty-four in *auloi*: Aristotle, *Met.* 1093b, the Harmony of the Spheres, and the Formation of the Perfect System'. Pp. 51–91 in S. Hagel, C. edd. *Ancient Greek Music in Performance.* Vienna.

________ 2009a. *Ancient Greek Music: A New Technical History.* Cambridge.

________ 2009b. Review of Barker 2007. *Classical Philology* 104: 243–248.

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

Rocconi, E. 2002. 'The Development of Vertical Direction in the Spatial Representation of Sound'. *Studien zur Musikarchäologie 3. Orient-Archäologie* 10:389–392.

von Jan, K. 1895. ed. *Musici scriptores graeci. Aristoteles, Euclides, Nicomachus, Bacchius, Gaudentius, Alypius et melodiarum veterum quidquid exstat.* Leipzig.

The Professors of Secrets: Mystery, Medicine, and Alchemy in Renaissance Italy by William Eamon

Washington, DC: National Geographic, 2010. Pp. 368. ISBN 978–1–4262–0650–4. Cloth $26.00

Reviewed by
Allison Kavey
CUNY John Jay College of Criminal Justice/
CUNY Graduate Center
akavey@jjay.cuny.edu

One of the great joys in an academic's life comes with encountering an elegantly written, intelligently researched, and highly readable monograph. William Eamon's *Professors of Secrets* leaves one feeling in the hands of a master at the pinnacle of his powers, when familiarity with material and competence unite to produce a book meaningful to other specialists and readable by a popular audience interested in the rich intellectual and cultural *milieu* of popular natural philosophy in Renaissance Italy. He provides interesting material to supplement our knowledge of the debates informing what constituted 'legitimate' medical practice and the relationship between experimentation and the acknowledged medical and natural philosophical authorities. He cleverly uses individual stories, which every undergraduate professor will gleefully borrow, to illustrate his argument; and in so doing, he demonstrates the strengths of narrow studies to make important contributions to our understanding of the complex relationship between experience and authority in the construction of medicine and natural philosophy.

Drawing from his earlier work on books of secrets as an important genre within Continental early modern natural philosophy, Eamon shifts his focus in this volume to concentrate on the professors of secrets who proliferated in Renaissance Italy. Drawn into trying to contextualize the biography of Leonardo Fioravanti, Eamon deemed this new approach worthy of his efforts, contending a 'reconstruction of his life can serve as a window into the remarkable world of late Renaissance Italy' [13]. He begins with a speedy review of the history of Bologna in the latter part of the late 15th and early 16th centuries,

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 352–354

when Italy was under siege by political upheaval, violence, and epidemics. He then situates Fioravanti in this complex cultural picture, paying special attention to the medical events, including a brush with the plague and a typhus epidemic that shaped his childhood and young adulthood. He also describes the conflict Fioravanti experienced between anatomical investigations, newly reinvigorated by Andreas Vesalius and embraced as public events by the Italian universities, and Galenic medical tradition. No fan of the new emphasis on anatomy, Fioravanti concluded, 'The only thing the anatomy lesson proved...was that doctors teach and write about things that don't exist' [46]. The chapter describing the often cited but frequently ill represented field of barber-surgeons is especially valuable as a brief and informative review of this group of practitioners.

Eamon's book provides equally valid insights into the multiplicity of arenas that influenced medical practice during the 16th century. Carnival is often discussed as a means of producing and governing cultural chaos, but I have never before seen it explored as a site of medical education and practice. Combined with Fioravanti's explorations into lay medical traditions, pharmacy, alchemy, magic, and warfare, Eamon's investigations provide the most exhaustive description of early modern medical traditions and the Continental scope of this kind of medical career that I have ever read.

Eamon also sheds important light on the patient/healer relationship, which has obviously drawn scholarly attention before [see Pomata 1998] but has a different sense here. Fioravanti was frequently viewed as a charlatan by other practitioners and sometimes by patients but he was also sometimes revered by his patients, some of whom were not accustomed to any regular medical treatment and others of whom had disorders previously deemed incurable by other practitioners. Despite the often painful experiences patients faced in the name of a cure, they were frequently grateful for a successful cure. They also spoke out against the tradition of secrecy that kept these secrets from being more widely practiced, an argument that pushed against the longstanding practice of artisanal secret-keeping which also influenced some surgeons and barber-surgeons. This pressure to make secrets public and, of course to profit from them, was central to the success of the professors of secrets. 'When they were sick, people wanted action, not just an intellectual understanding of the causes of their ailments' [167].

From Bologna to Venice, medical education to urban center mountebanks, Eamon illustrates the meaningful interchanges between cultural events and medical knowledge and practice. Fioravanti's complicated career, ranging from university to printing press, demonstrates the rich intellectual traditions that influenced 16th-century medicine. Magic, alchemy, systems of sympathy, pharmacy, and surgery all played critical roles in shaping the majority of medical practices outside of the university. The challenge that this posed to the authority of traditional medicine, especially when the former was widely available and the latter was expensive and often limited to wealthy patients, was real and fundamentally challenged the ways in which patients and healers understood medicine. It also changed both parties' expectations: Fioravanti himself claimed that 'physicians should care for their patients with compassion and love' [239]. Eamon's book is a welcome addition to the literature on 16th-century popular medicine and its intellectual antecedents. Its readability ensures that its important arguments will be accessible to a broad audience.

BIBLIOGRAPHY

Pomata, G. 1998. *Contracting a Cure: Patients, Healers, and the Law in Early Modern Bologna.* Baltimore/London.

Interpreting Aristotle's Posterior Analytics *in Late Antiquity and Beyond* edited by Frans A. J. de Haas, Mariska Leunissen, and Marije Martijn

Boston/Leiden: Brill, 2010. Philosophia Antiqua 124. Pp. xxiv + 269. ISBN 978–90–04–20127–9. Cloth $153.00

Reviewed by
Sten Ebbesen
University of Copenhagen
se@hum.ku.dk

Recent years have seen the publication of a number of collective volumes studying the fate of particular Aristotelian works through the centuries. The present volume is a welcome addition to the bibliography. Its 10 essays are arranged in three parts:

(1) Concept Formation in *Posterior Analytics* II 19,
(2) Metaphysics as a Science, and
(3) Demonstration, Definition and Causation.

Inevitably, the quality of such a collective work is not even all the way through but on the whole it is very satisfactory, and the concentration on three important topics gives some coherence to the volume. The title's promise of information about the fate of *An. post.* in Late Antiquity is fulfilled by most of the constituent essays, whereas there is precious little about the 'Beyond' except for one essay about Eustratius of Nicaea and one about Roger Bacon plus some that discuss pseudo-Philoponus on *An. post.* 2 (whom the authors wrongly tend to identify with Philoponus himself; see more about this below). The editors' introduction contains some sweeping statements about the way *An. post.* was treated in the Middle Ages. At least as regards the Latin world, it is hardly true that 'either the commentaries had an external aim, primarily the defense of theology as a science, or the commentators selected a fairly limited number of themes useful to the areas of philosophy of their interest', as we read on page xix. Given the considerable number of unpublished and barely studied commentaries from the medieval arts faculties, the claim about narrow interests on the commentators' part is extremely hazardous. And

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 355–366

as regards theology, the main upshot of the debate in the 13th century about whether theology could be an Aristotelian *scientia* was exactly that it cannot, because its axioms are non-evident.

Part 1 starts with a paper by Richard Sorabji, who—lucid as always—first outlines his own, very interesting, interpretation of 2.19 (main point: νοῦς is 'spotting') and then succinctly presents various interpretations of the chapter by Greek commentators, most of whom could not accept Aristotle's rejection of innate rational principles (λόγοι).

There follows a paper by Christoph Helmig about Proclus' objections to Aristotle's theory of concept formation. The critique is found principally in a longish passage in book 4 of Proclus' commentary on the *Parmenides*, in which Aristotle is not mentioned; but, as Helmig makes clear, the main thrust of the passage is to rebut the thesis of *An. post.* 2.19 that concepts have their origin in sense-perception and are arrived at by the inductive process which the Neoplatonists called 'collection'. In the course of his argumentation, Proclus not only inveighs against such latter-born concepts but also introduces a good variant of latter-born concepts and a good variant of collection, in which latter-born concepts come about when the soul collects common features of sensible things guided by the innate λόγοι. Sensible things, thus, are not the origin of such concepts but just the occasion for forming them.

I find Helmig's argumentation persuasive, much more persuasive than Proclus', which, as Helmig repeatedly points out, can only sway someone who has already accepted a number of Neoplatonic principles. I have two small queries. On page 32, Proclus is made to say that 'the universal in the many is of lesser account than every individual'. The sense must be 'the universal in the many is less than each of them' because, as Proclus explains, each singular thing possesses accidental properties over and above its universal nature. Towards the end of the paper, having distinguished between the processes of abstraction and collection, and having claimed that Proclus identified Aristotle's mode of deriving a concept with collection, Helmig nevertheless in the next paragraph [64] speaks as if such an Aristotelian concept was the result of abstraction.

Katerina Ierodiakonou analyzes Eustratius' comments on *An. post.* 2.19. She finds a discrepancy between the commentator's initial five-page paraphrase-cum-excursus and the remaining 10-page more detailed commentary, although the latter repeats much that was already said in the former. The discrepancy is real but her attempt [58] to put the blame on an editor, who, she proposes, may have mixed up two sets of marginal annotation, is farfetched. Apparently, she is thinking of Hayduck, who did the edition in Commentaria in Aristotelem Graeca [Hayduck 1907]. But if anything of the sort happened, it must have happened in late Byzantine times. The first printed edition, by Paulus Manutius, from 1534 had the same text as Hayduck's edition.[1]

More interesting is Ierodiakonou's speculation about what might have motivated Eustratius to defend a couple of views that were not at all, or not clearly, Aristotle's. According to Eustratius, in one passage at least, where he contrasts his own view with both Plato's and Aristotle's, humans do possess full knowledge of the first principles at birth, although this knowledge is obscured by bodily impulses. Ierodiakonou suggests that this may reflect the Christian thought that humans, being made in God's image, are fundamentally perfect. She does not mention original sin; but, if she is right about her main point, the obscuration should probably be attributed to original sin. Eustratius also shows some eagerness to make physics a science in spite of the instability of sublunary physical phenomena. Ierodiakonou suggests that this may have a link to the Christian notion of nature as God's creation and the study of nature as a means to find a way to God. Both points are, she admits, speculative without solid textual support, but they are worth keeping in mind.

The last paper of part 1 is by Pia A. Antolic-Piper, who, after an initial sketch of the introduction of the *Posterior Analytics* in medieval western schools, analyzes how young Roger Bacon in his two sets of questions on the *Metaphysics* (*ca* 1237–1247) understands the acquisition and status of the principles of knowledge/science, and how his discussion of the issue depends on his reading of the *Posterior Analytics*. Two main conclusions are that for Bacon,

[1] This appears from Hayduck's preface, according to which Manutius' edition agrees so much with his own main ms., Ven. Marc. 257, that it must have been based on that ms. Moreover, Andreas Gratiolus' Latin translation from 1542, which is based on Manutius' edition, matches Hayduck's text perfectly. See Gratiolus 2001.

(a) there is no innate knowledge, only innate facilities,
(b) *intellectus* is a state and not an activity or process, the processes leading to it being sensation, induction, and *experimentum*.

Unfortunately, the questions on the *Metaphysics* are not very informative about how Bacon imagined those processes; but it is interesting to notice how small a role is allotted to abstraction as opposed to induction, and how he concentrates on universal propositions rather than universal concepts as the principles of knowledge. He is influenced by Robert Grosseteste's commentary (which comes as no surprise) but does not follow him in all matters.

Antolic-Piper's paper is somewhat difficult to read, in part because the English does not flow naturally. In footnote 9, she mentions some early commentaries on the *An. post.* and among them one by Nicholas of Paris. To the best of my knowledge, there is no such work.

Part 2 of the book starts with a lucid investigation by Maddalena Bonelli of the neck-breaking attempt by Alexander of Aphrodisias to make Aristotelian metaphysics a science in the sense of the *Posterior Analytics*. The attempt involves, among other acrobatics, taking being as a sort of genus of everything. On the whole, I find Bonelli's interpretation of Alexander convincing, including her discussion of how Alexander thought one can use the most general axioms in syllogistic deductions, axioms such as the Euclidian 'Those that are equal to the same are equal to each other'. She fails, however, to point out that for all Alexander's efforts, even he did not manage to produce an Aristotelian categorical syllogism with the axiom of equality as its major premiss.

There follows a brief paper by Angela Longo about Syrianus' use of *An. post.* in his commentary on the *Metaphysics*. Syrianus' unwillingness to harmonize Plato and Aristotle is well known. Longo concentrates on his attempt to show that Aristotle's rejection of hypostatized mathematical objects in the *Metaphysics* is inconsistent with the theory of science of the *An. post.*, with which Syrianus seems to have had no query.

The first essay of part 3 is by Mira Tuominen. She examines Alexander's and Philoponus' comments on *An. prior.* 1.27–30—which together with ch. 31 were traditionally designated «Περὶ εὐπορίας προτάσεων» ('How to get a Good Supply of Premisses')—plus Alexander on *Topics* 1.2, with a view to ferreting out the commentators' views about how to apply Aristotelian syllogistics in scientific practice. Tuominen's explanation of what happens in *An. prior.* 1.27–30 is illuminating but the result of her inquiry is unsurprising: the commentators did not see a problem in the application of syllogistics to the sciences and thought that the teachings of the *Prior Analytics* could be used to construct both dialectical and demonstrative syllogisms. Moreover, Alexander is fairly explicit that the good that dialectic does for science is not to establish scientific premisses but simply to sharpen the mind of its practitioner.

The last three papers are devoted to problems in *An. post.* 2.1–10, 11, and 12, respectively. This is one of the most forbidding parts of the whole *corpus Aristotelicum* and the papers are also very demanding of their readers. As might be expected, their solutions of the severe problems of exegesis lack the quality of obvious correctness but the papers offer good food for thought.

Owen Goldin deals with 2.1–10. He juxtaposes two lines of interpretation, his own, which he tries to show was also Alexander of Aphrodisias', and another followed by pseudo-Philoponus and, he claims, also by Western scholastic exegetes. Alexander's interpretation has to be pieced together from the (more or less certain) fragments of his commentary on *An. post.* 2 and his extant commentary on the *Topics*, and Goldin has to admit that not all the elements of his preferred explanation are actually attested in what can now be glimpsed of Alexander's commentary.

According to Goldin's preferred interpretation, Aristotle is tackling the problem of how to explain anything worth explaining with a tool-box containing just definitional first principles and syllogisms. The commentators who saw this, he says on pp. 155f.,

> took Aristotle's view to be that when we explain a state of affairs, we often understand it as a case in which the nominal definition of an attribute is inherent in some basic subject of the sciences. The inherence of this nominal definition, in turn, can be syllogistically proven on the basis of definitional first principles.

The alternative interpretation takes the text to 'offer an account of how a syllogism can serve to identify conceptually distinct aspects of a single reality' [156], as we may see in pseudo-Philoponus who introduces the distinction between a formal and a material definition.

A slip: in a paraphrase of Alexander, *Top.* 17.3ff. on page 175 we read 'with the supposition (κείμενος) that...'. The paraphrase is passable, but the apparent information that a supposition is called a κείμενος is not. What the text has is a genitive absolute, the subject of which is a nominalized sentence treated as a neuter noun and the predicate «κειμένου»: 'it being posited that...'. On page 178, something has gone terribly wrong with a sentence. I cannot make head nor tail of:

> From this passage, Philoponus(?) learns that there are two different sorts of definitional accounts that the play a role in demonstration of the existence of that kind are definitions of a sort.

An otherwise reasonably transparent part of Goldin's difficult paper suddenly is plunged into obscurity.

A paper by Mariska Leunissen deals with Aristotle's remarks about final causes as middle terms in *An. post.* 2.11. She convincingly shows the untenability of pseudo-Philoponus' interpretation, according to which Aristotle tells his reader to disregard the example that is actually found in the text and construct other syllogisms instead. She also makes a good case for taking a vital «μεταλαμβάνειν» to mean 'substitute'; but I am not at all convinced of the rest of her interpretation, which hinges, in particular, on a distinction between «αἰτίαι» and «αἴτια». Unfortunately, she treats pseudo-Philoponus as if he were John Philoponus and thus puts the text in a wrong historical setting.

The final essay, by Inna Kupreeva, takes up the question raised in *An. post.* 2.12 whether a temporally antecedent cause can necessitate an effect. This leads to a close examination of another relevant text, *De generatione et corruptione* 2.11, and of Alexander of Aphrodisias and Philoponus' interpretations of it. We are also offered a tour of Aristotelian views on time, beginning and ceasing, as well as cyclical causation. All in all, a very stimulating essay.

On page 223, there is an apparent slip. A passage from Alexander in R. Sharples' translation contains the phrase 'does not even come to be the same', 'not even' rendering «μηδέ». But further down on the same page, this is quoted as 'never coming to be the same', as though the text had 'not ever'.

The book contains a moderate amount of misspellings and typing errors. Most of them, though a nuisance, are really innocuous, like ‘than’ for ‘then’ in T16 on page 192 or ‘dealed’ for ‘dealt’ on page 217. Occasionally, sinister forces have been at large and produced nonsense, as in the passage on page 178 quoted above. Remarkably, Greek words usually come out right, whether printed in Greek characters or transliterated.[2]

The original Greek of texts quoted in the articles is sometimes, but not consistently, printed in the footnotes. It would have made the book easier to use if one could always compare the translation with the original.

All but one of the essays are in English. A couple of those written by non-native speakers of the language could have benefited from some more robust editorial intervention, which could also have rectified the claim on page 126 that Kroll, the editor of Syrianus [1902], refers in one place to ‘*An. post.* I 7, 75b15 Ross.’ Ross has nothing to do here. The style of reference is, of course, the standard one to Bekker’s edition of Aristotle [1831].

A note about the Greek commentaries

Both in the introduction and in some of the essays, there is some confusion about the Greek commentaries on Aristotle’s *Analytica posteriora* that are still extant, those that were available in the 12th century, and James of Venice’s Latin translation of a Greek commentary that became known in the West as Alexander’s. Let me try to sketch the situation.

Probably the most influential of all the ancient commentaries was the one by Alexander of Aphrodisias (*ca* AD 200). Paul Moraux in 1979 made a case for its having survived in its entirety (covering both books of *An. post.*) until the early 12th century when, apparently, Eustratius of Nicaea had access to it. Moraux’s argumentation does not, however, suffice to exclude the possibility that what Eustratius really saw were extracts rather than the complete text.

2 Exceptions: page 63 ‘metexein’ for ‘metechein’, page 90 ‘di’ auto’ for ‘di’ hauto’, page 119 «γενέσις» for «γένεσις», and page 147n 27«ὄδος» for «ὁδός».

The oldest surviving Greek companion to the whole of *An. post.* is Themistius' paraphrase (Commentaria in Aristotelem Graeca 5.1) from the fourth century AD. The next may be from the early 13th century and it may have Leo Magentinus for its author, but it is not certain that the commentaries on books 1 and 2 that I tentatively attribute to this author really form a unity [see below]. An unedited commentary by George Pachymeres, which I have not seen, was probably produced in the early 1290s.[3] Generally, we must treat commentaries on books 1 and 2 separately.

The only surviving ancient commentary on *An. post.* 1 is one ascribed to John Philoponus (6th century), the authenticity of which there is no reason to doubt. It was edited by M. Wallies in Commentaria in Aristotelem Graeca 13.3. If Philoponus ever commented on book 2, which he probably did, the work almost certainly did not survive until the renewed interest in the *Organon* in the early 12th century. His commentary on book 1, by contrast, was to become the standard Byzantine commentary on that book and there is no indication that the busy Aristotelians of the early 12th century felt a need to supplant it with a product of their own.

In fact, the earliest Byzantine commentary on *An. post.* 1 seems to be an anonymous one that may be the work of the 13th-century scholar Leo Magentinus. An interpolated version of this work was produced in the late 13th century and is found in several mss. An extract from the interpolated version has been printed in Commentaria in Aristotelem Graeca 21.1:viix–viii.[4]

Another commentary on *An. post.* 1 was produced by John Pediasimus, probably in the 1270s. It remains to be seen, however, to which degree it really deserves the title of 'commentary' rather than 'collection of scholia'. The extant edition of a selection of scholia only contains unsatisfactory information about the constitution of the work.[5]

---

3 See Golitsis 2007. According to Golitsis a commentary on the whole of the *Organon*, hence also on *An. post.* 1–2, is contained in two mss.

4 I intend in a future article to show that all or most of the manuscripts of pseudo-Philoponus on *An. post.* 2 as well as those of the interpolated Leo(?) on *An. post.* 1 derive from ms. Vat. gr. 244, which mainly contains comments by Leo Magentinus, many of them with secondary interpolations.

5 See De Falco 1926 and 1928: cf: Praechter 1927.

We have two eponymous Byzantine commentaries on *An. post.* 2, one by the early 12th-century scholar Eustratius (Commentaria in Aristotelem Graeca 21.1), and another, unedited, by his near-contemporary Theodorus Prodromus [see Cacouros 1992], plus two anonymous ones. In addition, there is an unedited paraphrase by John Chortasmenus from the early 15th century (which I have not seen) [see Cacouros 1994].

The anonymous commentaries on book 2 were both edited by Wallies in Commentaria in Aristotelem Graeca 13.3 together with Philoponus on *An. post.* 1. By far the most interesting of them is the one styled 'Anonymi in analyticorum posteriorum librum alterum commentarium'. It actually does not quite deserve the name of commentary as it fails to comment on parts of the text and misses a proem. Moraux in 1979 showed beyond reasonable doubt that it consists to a high degree, perhaps even exclusively, of excerpts from Alexander of Aphrodisias' lost commentary. Actually, many of its constituent scholia start with «ὅτι», which in Byzantine texts is a standard way of introducing an excerpt (Moraux failed to grasp this point, which only supports his conclusion). There is at present no way to date this collection of excerpts—Moraux argued that the extant collection is even an abbreviated version of an 'original' one.

The other anonymous commentary on *An. post.* 2 is in Commentaria in Aristotelem Graeca 13.3 adorned with John Philoponus' name. This is doubtless due to pressure from the general editor, Hermann Diels, who repeatedly forced Maximilian Wallies, an excellent scholar, to leave untenable attributions found in the Aldine editions untouched. Wallies had fully realized that there is every reason to reject the attribution, which, to the best of his knowledge, was supported by only one late manuscript and the Aldine edition. Few people nowadays read Wallies' Latin preface to the volume and some of those who do so underestimate the force of his argumentation, as does Goldin in the volume under review [156], while Ierodiakonou and Leunissen act as if the attribution to Philoponus were assured. Wallies was no one's fool; and unless you have access to information that he did not have, you had better not challenge his judgement.

On stylistic grounds, I am inclined to date pseudo-Philoponus on *An. post.* 2 to the 13th century. The author is probably Leo Magentinus, at least one of whose mannerisms the text shares. Leo was fond of using «ἤγουν» ('i.e.' to introduce explanations of words or clauses, sometimes stuffing several «ἤγουν»-clauses into the same sentence [see Ebbesen 1981, 1.306–310, 2.285ff]). In Philoponus' commentary on book 1 there are just three examples of «ἤγουν» in 333 pages. In pseudo-Philoponus on book 2, there are about 200 instances on just the first 45 pages! I know of no copy of the text with an attribution to Leo: but in a couple of manuscripts, a mixture of scholia by Pediasimus and pseudo-Philoponus is said to be by Pediasimus and Leo; and in one of them, Cardinal Bessarion has marked the excerpts from pseudo-Philoponus as being by Leo.[6] I believe that Bessarion knew what he was doing. The chances that Leo incorporated material from a lost commentary by the genuine Philoponus are minimal, though the possibility cannot be completely discarded—there just is no reason to believe so.

What the available data suggest is that in the 12th century, apart from Themistius' paraphrase, the only unabridged ancient commentary available in Byzantium was Philoponus' on book 1, while there were also some extracts from Alexander's on both books. Hence, the production of four new ones on book 2 and, as far as we can see, of none on book 1. From various sources the authors could pick up fragments of lost commentaries but the books themselves were no longer to be found. As for James of Venice's translation of a Greek commentary into Latin, there can be no doubt that it existed, although it has been found in no extant manuscript. The evidence is best for book 1, and there several quotations that match Philoponus' phrasing to the word. Some evidence for book 2 is more difficult to interpret and some references, unknown to which book, make no perfect fit with any Greek text available in print, though they do presuppose a Greek source. The Latins generally attribute the work to Alexander but there is little reason to take that attribution seriously, as do the editors of the volume under review, who speak of 'James of Venice's translation of the Aristotelian text and of a commentary, probably Alexander's, or possibly that by Philoponus' [xviii].[7] The transla-

---

[6] See Cacouros 1994–1995, which fails to draw the conclusion that pseudo-Philoponus is really Leo.

[7] The editors seem to depend on Longeway 2005, where one finds:

> The commentary of Alexander of Aphrodisias (or the commentary of Philoponus, which is close to Alexander) was translated by James

tion seems to have been transmitted together with that of Michael of Ephesus' commentary on the *Sophistical Refutations*, which originally bore the correct ascription to Efesius but later on got attributed to Alexander, perhaps through a confusion of Efesius with Afrodisius.

In footnote 42 on page xviii, the editors claim that there is only one fragment of the translated commentary on *An. post.* and refer to a paper of mine from 1977. This paper deals with the only fragment known so far of James of Venice's own commentary on *An. post.* In fact, there are several fragments of the translated Greek commentary, though not a whole lot, as I have shown in several publications and most recently in a revised collection of the fragments from 2008. The work seems, however, to have had a very limited circulation; and as of 2011, there is no basis in actual scholarship for John Longeway's claim [see 364n7] to the effect that while the work itself quickly dropped out of circulation, much of its content was preserved in marginal glosses. Some of its content, yes, but not much of its content.

BIBLIOGRAPHY

Bekker, I. 1831. ed. *Aristoteles graece* 2 vols. Berlin.

Cacouros, M. 1992. *Le Commentaire de Théodore Prodrome au second livre des* Analytiques Postérieurs *d'Aristote. Texte (editio princeps et tradition manuscrite) et étude logique du commentaire de Prodrome.* Thèse de doctorat, Paris IV.

_________ 1994. 'Un commentaire byzantin inédit au deuxième livre des Seconds Analytiques, attribuable à Jean Chortasménos'. *Revue d'Histoire des Textes* 24:150–198.

_________ 1994–1995. 'La tradition du commentaire de Theodore Prodrome au deuxième livre de *Seconds Analytiques* d'Aristote. Quelques étapes dans l'enseignement de la logique á Byzance'. Δίπτυχα 6:329–354.

De Falco, V. 1926. *In Aristotelis analytica scholia selecta.* Naples.

_________ 1928. 'Altri scholii de Giovanni Pediasimo agli Analitici'. *Byzantinische Zeitschrift* 28:251–269.

of Venice. This translation also quickly dropped out of circulation, but much of its content survived in marginal glosses.

Ebbesen, S. 1981. *Commentators and Commentaries on Aristotle's* Sophistici Elenchi. 3 vols. Corpus Latinum Commentariorum in Aristotelem Graecorum 7.1–3. Leiden.

__________ 2008. 'Fragments of Alexander's' Commentaries on *Analytica Posteriora* and *Sophistici Elenchi*'. In *Greek-Latin Philosophical Interaction: Collected Essays of Sten Ebbesen*. vol. 1. Aldershot, UK/Burlington,VT.

Golitsis, P. 2007. 'Georges Pachymère comme didascale'. *Jahrbuch der österreichischen Byzantinistik* 58:53–68.

Gratiolus, A. 2001. ed. *Eustratius Nicaenus. Commentaria in II librum posteriorum analyticorum Aristotelis*. Commentaria in Aristotelem Graeca: Versiones Latinae Temporis Resuscitarum Litterarum 7. Stuttgart-Bad Cannstatt.

Hayduck, M. 1907. ed. *Eustratii in analyticorum posteriorum librum secundum commentarium*. Commentaria in Aristotelem Graeca 21.1. Berlin.

Kroll, W. 1902. ed. *Syriani in metaphysica commentaria*. Commentaria in Aristotelem Graeca 6.1. Berlin.

Longeway, J. 2005. 'Medieval Theories of Demonstration'. *Stanford Encyclopedia of Philosophy*. http://plato.stanford.edu/archives/fall2005/entries/demonstration-medieval/.

Moraux, P. 1979. *Le commentaire d'Alexandre d'Aphrodise aux «Seconds Analytiques» d'Aristote*. Peripatoi 13. Berlin.

Praechter, K. 1927. rev. De Falco 1926. *Byzantinische Zeitschrift* 27:105–113.

Magic, Memory and Natural Philosophy in the Sixteenth and Seventeenth Centuries by Stephen Clucas

Variorum Collected Studies Series 973. Farnham, UK/Burlington, VT: Ashgate, 2011. Pp. xii + 318. ISBN 978–1–4094–1975–4. Cloth £81.00/ $154.95

Reviewed by
Hilary Gatti
University of Rome "La Sapienza"
hilary.gatti@uniroma1.it

Stephen Clucas' collection of richly documented and varied essays is composed of 12 papers originally published between 1993 and 2008. They are neither divided into sections nor ordered according to the chronology of their composition or publication. Rather they are organized around a number of specific themes disposed in a historical sequence that gives the volume a unity of narrative and intent that not all collections of essays possess. These themes have been at the centre of this author's attention for many years. According to the brief preface, they continue to represent his principal interests today. He therefore asks the reader to consider the volume as representing work in progress rather than a fully defined itinerary.

The preface itself is of considerable interest, starting as it does from a letter written to Clucas by the late Hugh Trevor-Roper (Lord Dacre). The letter expresses his dismay at some of the areas into which this author was venturing:

> The intellectual history of the pre-Enlightenment is a fascinating subject: fascinating and, often, deterrent: the disengagement of natural philosophy from its theological integument is so complex and so painful, and accompanied by such desperate attempts to tighten the disintegrating vestment around it. I'm afraid I really despair of understanding those last convulsions.

Although Clucas admits to sharing in both the fascination and the deterrence, the letter (which does him much honor) seems quoted above all to enhance the value of his determined march into this

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 367–373

harsh and at times shadowy territory, which in a last provocative comment he also claims 'we would do well to try to understand if we are to understand the present'. The volume, however, exhausts itself in terms of a rigorously historical intellectual enquiry, furnishing no hints as to how we are to relate its contents to the culture of today.

The first group of three essays deals with the figure of the Elizabethan Magus, John Dee, much discussed in both his own times and ours. Not content with such an intrepid choice, Clucas immediately engages with the most obscure aspect of Dee's remarkably varied intellectual story, squarely facing up to Dee's final phase of angelic conversations and his tortured relationship both with the angels who presented themselves at his visionary sessions and with his skryer (or intermediary) Edward Kelley—by many, both then and now, considered a probable phony with criminal intents. Clucas raps on the knuckles those who take up such preconceived, 'post-enlightenment' attitudes, which he considers 'unhelpful' and improper in a historical enquiry. The attempt made here is to understand in terms of the culture of the time what Dee—in his earlier years one of the foremost intellectual figures of Elizabethan England—was seriously trying to do in his final years, and on the basis of what sources and currents of thought he was doing it. In carrying out this task, Clucas transforms the image of Dee as a Neoplatonic Renaissance Magus (as he appears in the work of Frances Yates and her epigonies), demonstrating with impressive and convincing documentary evidence that the magic dimension at work here had a medieval, pseudo-Salamonic foundation, closely entwined with ancient Christian practices of prayer and invocation. The Dee who emerges can thus later be objectively considered as at least partially acceptable by an Anglican cleric such as Meric Causabon [Essay II], who approved of Dee's devotion although chastising him for his naïve habit of being deceived by evil angels, whom he too often failed to distinguish from those who were good. This devotional and pseudo-Salamonic Dee, more concerned with magic as practice and prayer (and thus with contemplation of magical seals and tables) than with their intellectual foundation (that is, with reading and interpreting them), is what Clucas then rediscovers [Essay III] in Dee's *Liber misteriorum.*

The Dee who emerges from these essays appears much more of a native, northern phenomenon, still deeply rooted in a medieval past, than the Italianate 'Renaissance Magus' of the Yatesian version.

Yet, he is surely too complex a figure to be judged only on the evidence of his final years. A full evaluation of Dee in the light of these essays would need to take account also of his earlier years, of the Dee who taught mathematics to Sir Philip Sydney and his circle, who published an important edition of Euclid, and whose remarkable library contained the text of Copernicus' *De revolutionibus*. The essays here make no attempt to connect up to this earlier phase; nor do they mention the contemporary discussion that Dee's final years gave rise to. For example, Christopher Marlowe's *Dr. Faustus* is a text that surely mirrors the story of Dee in Faustus' sudden decision to throw to the winds all his previous intellectual achievements, in his ardent desire to transform the world through magic, and in his desperate requests to his spirits to enlighten him with total and immediate knowledge, bypassing the need for logical process and reasoning. And does Faustus not mirror Dee too in the paucity of the results he obtains, in the submission with which he is constantly forced to bow down to his angels, good or bad (shades of Mephistopheles)? Marlowe, who was no post-Enlightenment rationalist, presented his dramatic portrait of the magician as a tragedy. Marlowe was a contemporary of Dee's with links to the Sydney circle; so he probably knew (or knew of) Dee personally.

Clucas' essays undoubtedly question the previous Yatesian image of Dee in challenging and (in this reader's opinion, at least) convincing terms, and in doing so they represent a valuable contribution to Dee studies. They also stimulate a series of questions about the wider significance of Dee's story to which these essays themselves, carefully crafted to remain within a very specific and clearly defined framework, furnish no answer.

A second group of essays [IV–VII] deals primarily with Giordano Bruno's art of memory. Clucas had the good fortune to come early into contact at London University with Giovanni Aquilecchia, an important point of reference for his Bruno studies. In this context, Clucas has chosen to underline above all Bruno's memory works, thus keeping alive in the English-speaking world a tradition of studies which other recent English-language scholars of Bruno have tended to consider marginal. This choice means that Clucas has perforce to measure up to Frances Yates' still fundamental volume *The Art of Memory* [1966], where Bruno's memory-art is considered in the light of Ficinian astral magic. A large part of these four essays is dedicated to questioning this Yatesian interpretation, taking as inspiration the work done in Italy by Paolo Rossi and later by Leen Spruit and above all Rita Sturlese, who considers Bruno's art of memory as specifically an 'ars' or a logical-mnemonic technique, which, although founded on metaphysical doctrines specific to the 'Nolan philosophy', remains largely unconcerned with Ficinian astral magic or talismanic influences. A significant contribution to this discussion is provided by Clucas in Essay IV which brings into the picture not only the memory treatises of Alexander Dicson (whose importance had already been underlined by Frances Yates) but above all the pages on memory in manuscripts of Walter Warner which, although already discussed in other contexts in relation to Bruno's thought, had not previously been considered for their memory content. Clucas here underlines the importance of Warner's insistence on 'notation' or 'characterization' to store verbal discourse, demonstrating how he integrated Brunian mnemotechnics with his Ramist training and an Aristotelian psychology. This essay presents a reading of Bruno's art of memory and its successive influence as without any significant magical content and, thus, as essentially different from that put forward by Frances Yates.

This well documented and thought-provoking essay is followed [Essay V] by what seems to be a degree of re-thinking of this thesis. Taking as his main locus Bruno's final work on memory, *De imaginum, signorum et idearum compositione*, Clucas now argues that some kind of magical content to Bruno's memory-art has nevertheless to be recognized. The discussion here is characteristically serious and well founded on a close reading of both Bruno's own works and on recent criticism; but it leaves the reader with an uneasy impression of a subject not completely brought into focus. It is only in the following piece [Essay VI], which, although a previous publication in chronological terms, is centered largely on the *Triginta sigillorum*, that the solution is delineated in the light of Bruno's own claim that Love, Art, Magic, and Mathesis are the four internal rulers of human action. So magic is not the defining characteristic of Bruno's art of memory, as Yates had claimed, but rather one of four cardinal virtues of the mind, one component only of memory, closely connected to the imagination. This conclusion allows Clucas to finish off with a definition of the art of memory conceived of as spiritual exercise designed to regulate the disordered affections of the soul by connecting them to the intelligent order of natural agents. This is a distinguished contribution which succeeds in the difficult task of connecting Bruno's art of memory to his natural philosophy.

Liberated at last from the necessity of measuring himself against Yates, Clucas, in the last of these four contributions [Essay VII], can go off on a quite different tack, concerning himself with the rhetoric of scientific dialogue in both Bruno and Galileo. The rhetoric of scientific discourse has been at the center of much recent attention, and this piece is perhaps less innovative than the other essays. Nevertheless, it is of value in re-proposing a Bruno-Galileo connection that is often ignored, and interesting in its perception that the digressive techniques used by both authors in their pro-Copernican dialogues are essential in so far as the digressions themselves often contain some of their most original and significant observations.

Essays VIII–X are concerned with corpuscular matter theory and particularly with 17th-century English atomism, specifically of the Northumberland and Cavendish Circles, above all in their complex relationships to Aristotelian theories of matter and form. Rather surprisingly, this subject is approached not through Bruno's *De triplici minimo* of 1591 (which actually contained one of the earliest modern attempts to delineate an atomistic theory of matter) or even through Pierre Gassendi's revival of ancient Epicureanism (known in England through Charleton's *Physiologia Epicuro-Gassendo-Charltoniana* of 1654), but rather through the influence of native medieval thinkers such as Grosseteste and Roger Bacon. This is because Clucas here is self-consciously writing in the light of a number of recent claims (by Roy Porter, Mikulás Teich, Ugo Baldini) that history of science needs to be newly approached in local or national terms rather than through the more traditional global narrative of 'the rise of modern science'. The medieval writers mentioned above, with their still Aristotelian concept of substantial forms, are seen to have survived as still lively presences in the culture of early modern England. Walter Warner's concept of *vis* and his discussion of the nature of fire and light, but also Nicholas Hill's more theological concept of corpuscularianism and (perhaps a little too briefly) Francis Bacon's 'natural motion of the atom' as well as the atomism of Margaret Cavendish, are all seen as stages in the negotiation between the new corpuscular philosophy and the survival in 17th-century England of soul-like substantial forms of Aristotelian derivation. Only when Clucas arrives at the more mature and more fully corpuscular philosophy of Robert Boyle does he see a ‘modern’ atomistic theory as developing in England.

One may wish to question the value of national narratives in the context of the very international *milieu* of the European enlightenment or Clucas’ final claim that the only truly ‘revolutionary’ aspect of an emerging ‘modern’ science lay in this complex negotiation between the lengthy survival of an Aristotelian doctrine of substantial forms and the new, more mechanistic philosophy. But to do so here would be unfair. These are remarkably dense and erudite essays, which undoubtedly offer a contribution of value to the historical enquiry into early modern English atomism.

The final two essays (although by no means the most recent in terms of their composition) represent a new departure with respect to the preceding contributions which brings the narrative of the book to its final historical stage in the post-Baconian pre-Enlightenment project of scientific communication developed by Samuel Hartlib. The strength of these essays derives from what was clearly an intense and fruitful season of studies in the Hartlib archive held at the University of Sheffield. Here we find abundant quotations from previously unknown and long buried collections of letters to and from Hartlib, as well as notes and memoranda by Hartlib himself concerning his management of a social network of scientific practitioners. These are essays that offer a rich harvest of new materials (in the first essay, centered on Hartlib’s wide network of scientific correspondents; in the second essay, more specifically on the chemical component of that network). Indeed, at times the sheer abundance of quotations from often obscure and unknown voices from the Hartlib past finishes by overwhelming the reader with an excess of ill-spelt and not always illuminating concerns.

From this at times confused chorus of voices, however, Clucas draws a number of interesting and important conclusions. He shows how not only empirical experiment but also the problem of digesting, indexing, collating, and commenting on the already unmanageable number of ancient and renaissance printed texts became a major concern of Hartlib’s rationalizing project. He also shows—in some concluding pages that link up his discourse in this volume to Dee’s angelic conversations that had opened it—how this Protestant scientific project was far from wishing to limit or contest the sphere of religion. On the contrary, in spite of their post-Baconian credentials, the pursuit of secondary causes is rarely separated by these scientific practitioners from their Protestant faith with its accompanying zeal for social reform. Scientific experiments appear to have been undertaken by Hartlib and his circle as a project designed to celebrate the glory of God, in whose light they hoped to improve the history of the world. This strict dependence of the world of nature on the transcendent sphere of God and absolute truth, as Clucas convincingly demonstrates, ensured the survival of medieval mysticism, of Renaissance Neoplatonism, as well as both medieval and renaissance Hermetic strands of thought, well into this still uncertain prelude to enlightenment rationalism. Over the horizon, the reader catches an occasional glimpse of the figure of Isaac Newton, the giant towards whom the whole volume inevitably tends but with whose imposing shadow Clucas has still to engage.

BIBLIOGRAPHY

Yates, F. A. 1966. *The Art of Memory.* London.

A Tenth of a Second: A History by Jimena Canales

Chicago/London: University of Chicago Press, 2011. Pp. xii + 269. ISBN 978–0–226–09319–2. Paper $25.00, £16.00

Reviewed by
Max Stadler
ETH Zürich
max.stadler@wiss.gess.ethz.ch

'Modernity' is a grand and difficult word—and one all too easily conjuring up, arguably, the somewhat one-dimensional imagery of urbane *flaneurs*, bustling trams, and the arc-lights flickering above them. And yet, there no doubt is a lot to be said about all those narratives of modernity which center on the ever-intensifying, material interminglings of men and machines—of subjectivities and artificial, machine-infused spaces—that indisputably defined this so-called modern age; or which center, if you will, on the ensuing, gradual exposure of that very figure 'human nature' through his (or her) own creations: 'technologically produced stimuli...as the civilizing agents of the psyche', as Schivelbusch's history of *The Railway Journey* once had it [1977, 150–151 (my trans.)]. Indeed, the last three decades or so have seen no small amount of activity in the direction of such civilizing agents on the part, not least, of historians of science, who began charting the various ways in which the devices of modernity impinged on, transformed, made problematic, and helped fabricate conceptions of human physiology, perception, subjectivity, epistemology, and so on. A project which had considerable resonances and correspondences in the history of art, culture. and 'media', the machines of the 19th and early 20th century—from trains, telegraphs, and precision instruments to (more notoriously) gramophones, films, and typewriters—on these accounts produced, exposed, and effected many features of what began to take shape as human nature, *naturalized*.

The bigger picture that has here emerged is one that very productively illuminates the ways in which, say, the physiology of the 'human motor' was deeply enmeshed in the rise of factories, balloons, airplanes, industrializing cities, and alpine mountaineering; and much

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 374–381

the same holds true for a variety of analogous constellations which have come under the purview of historians: the mutual entanglements of laboratory instruments, street-lighting, vision, and attention; of color-blindness, seafaring, and railway safety; of language, voice, radio, and telephony; of wartime cripples, prostheses, and the (nascent) 'cyborg', and a great deal more.[1] The book under review, Jimena Canales' *A Tenth of a Second*, squarely fits into that mold, advancing as it does, an ambitious and complex story of techno-physiological modernity as told through the lens of one such modern man/machine-effect: reaction time. Or, in more dramatic terms, the story it tells revolves around that epistemologically worrisome exposure of the non-instantaneity of cognition, its ineluctable 'temporality' (9). This rather elusive temporality—the 'lag', crudely, between stimulus and response—was something hovering in the range of a 10th of a second; or so it turned out, rather consistently, as the psycho-physiological limitations of the human observer were thrown into relief thanks, largely, to the ever more exacting, intricate, and faster workings of machines. What is more, Canales is making good use of it, somewhat reminiscent of the biography-of-a-scientific-object literature, in order to bring together a range of indisputably crucial scenes and figures in matters of Modernity—some of them familiar, others less so. Covering a period roughly from 1800 into the 1920s, in its six highly readable chapters, *A Tenth of a Second* thus moves elegantly across pertinent developments in the realms of physics, psychology and physiology, weaving, along the way, a number of narrative threads between them—not to mention the multitude of cross-references to the history of photography, cinema, and the philosophy of science; precision instruments (or metrology) naturally loom large in this story of 'micro-temporality', as do such all-time favorites as Hermann von Helmholtz, Étienne-Jules Marey, and Henri Bergson.

Though Canales very well might have capitalized more systematically, and profitably, on the historiographical proximities of her subject matter to the vast range of modern body/machine effects gestured at in the above—her preferences, as we shall see, rest more assuredly on the intellectual history end of things—this is not a 'disciplined' history, then. And it is along these lines that chapter 2

[1] See, among others, Crary 1990, Dierig 2006, Hoffmann 2006, Lenoir 1994, Mills 2011, Otter 2008, and Rabinbach 1992.

(chapter 1 serving as the introduction) sets in with a rereading of the origins of reaction time in the first half of the 19th century. Offered as a revision of the 'standard account'—an account promulgated, we are told, notably by the fledgling science of experimental psychology (which merely treated itself to a story of progress)—the story as painted by Canales emphasizes the rather more practical dimensions of precision measurement in the birth of reaction time; and hence, in such useful sciences as astronomy, which was then quite heavily involved in the business of time and longitude determination. The psycho-physical limitations and idiosyncrasies of human observers—soon circulating as 'personal equations', 'individual differences', or 'observer errors'—first turned problematic within such contexts; and much effort, accordingly, was directed to controlling, effacing, and bypassing them. The very scale of the issues raised is relevant to Canales' retelling of the standard story; the immense spill-over, in other words, of these troublesome revelations beyond the laboratories and into the various 'cultures of reaction' which had been coalescing around the bountiful stimuli delivered by the modern age. By the early 20th century, as Canales recounts, notions of 'reaction time' and its variations were pondered by Taylorist efficiency experts and a new breed of (so-called) psycho-technicians as much as by armchair anthropologists and psychoanalysts, all of whom had some stake in the matter.

Readers hoping to learn more about these broader 'cultures of reaction' will, however, largely be disappointed: the phenomenon in place, chapter 3 shifts gear again, exploring the crystallization of the value $^1\!/_{10}$—and the controversies surrounding it—within the nascent science of experimental psychology. The elaborations of the phenomenon during the decades around 1870, so the story unfolds, involved only few doses of unanimity: a matter of technique, legitimate or no; of what one was inclined to read into the products of one's inscription devices (devices, predictably, not liked by everyone); of accounting for so many sources of contamination—the apparent influence of state of attention, exercise, age, fatigue, sex, and race; and it was, to be sure, an uncomfortable question, smacking of materialism: Is the speed of thought, or of volition, measurable? Adolphe-Moïse Bloch, based at the Muséum d'Histoire Naturelle, here emerges as the principal bad sport, wasting many years and a great deal of energy on finding defective the many attempts to prove that, in fact, there existed a meaningful and measurable entity.

Even so, the opponents of reaction time eventually lost out—no easy victory, as silencing the Blochs required the victors to alter, Canales suggests, the very meaning of experiment: the legitimation of 'experimental systems where the subject under experimentation was an accepted component within a system composed of keys, wires, and automatic inscription devices' [86]. If the consolidation of reaction time thus provided a central moment in the formation of experimental psychology, chapters 4 and 5 return to astronomy, and more specifically, to the run up to, and the events surrounding, the transit of Venus in 1874. Expectations were that this would be an especially delicate and fleeting event, and it made acute once more the problem of 'individual differences' (unless, that was, that rare occurrence be lost to science). The French, we learn, took it seriously enough to come up with an official Transit of Venus Commission. The Commission, geared towards improving the pertinent techniques of measurement and observation, promptly launched a series of pedagogical initiatives devised to come to terms with those vexing individual differences and personal errors. In this connection, it soon transpired that the production of disciplined observers only went so far, however, and the proffered solution increasingly involved getting rid of the human observer altogether. Under the heading 'cinematographic turn', Canales here traces the instrumental role played by the Commission in the sanctioning of the nascent (and contested) enterprise of scientific photography; and most notably in this regard, the role it played in furthering the pivotal doings of the astronomer—and pioneer of chronophotography—Jules Janssen, whose photographic 'revolver' was naturally poised, or so the rhetoric went, to capture objectively that elusive moment of Venus' transit.

We are firmly on the terrain of 'mechanical objectivity', then, or the constructions thereof, Canales pressing the point that, all told, this was a victory by no means uncontested and total.[2] Canales' narrative throughout tends to emphasize, more so than other writings on the subject matter, the observer who was implicated in all this mechanical displacing of natural by artificial eyes, rather than the instruments *per se*. Indeed, the 10th of a second, that essential

[2] On the notion 'mechanical objectivity', see esp. Daston and Galison 1992.

limitation of the human observer, it transpires, turned into a salient entity wherever rapid sequences and elusive, microtemporal events were to be captured (lightnings and electric sparks, for example); whenever the requisite, chronophotographic and similar such protocinematic technologies of moving, animated images were deployed (zoetropes, phenakistoscopes, and so on); and wherever, as chapter 6 narrates, precision and exacting standards emerged as matters of concern. Anxiety about individual differences thus spread still further as the laboratories of that youngish science called experimental physics mushroomed toward the end of the century. Dedicated to rigorous, metrological ideals, the deplorable existence of a physiological unit of time threatened to sabotage even its grandest and most useful endeavors in precision measurement—then launched at places such as the German Physikalisch-Technische Reichsanstalt or its US pendant, the Bureau of Standards. But it had its loftier effects too: as Canales is keen to show, by now, the 10th of a second had long turned into an intellectual specter of sorts, traceable into the influential musings of a Ernst Mach or Pierre Duhem. The final chapter accordingly is devoted to the fundamental divergences which erupted in the early 1920s between two grand thinkers of time indeed: Henri Bergson (no friend, famously, of sliced up time *à la* cinematograph) and Albert Einstein (someone not so inhibited).

If their talking past each other hinged on the smallest moments of time—and their (non)perceptibility—so did, as Canales argues in her conclusion, a great deal of what is called modernity. And sure enough, the story of reaction time, exemplary for that disturbing revelation that 'bodily differences affected knowledge' [10], may very plausibly be read as one crucial ferment in this narrative, forever frustrating those modern dreams of progress, exactitude, and universality (with intellectual repercussions, as Canales suggests, well into the 20th century). Exploring the realms of micro-temporality, as should have become clear, also allows her to draw up an unusually wide and synthetical picture, one which has much to offer to historians of science, photography, and philosophy. Indeed, even as many of the cast are familiar, if not canonized, by zeroing in on the 10th of second Canales still manages to draw together a great many only apparently disparate things in a refreshing and very accessible account.

That said, synthesis tends to come at a price; and most curious perhaps in this regard, the 10th of a second, ostensibly the book's subject matter, remains a strangely unproblematized and under theorized object. As other reviewers have noted, its ontological status is never quite explicated. In itself, this would not be much of a problem, of course—for historians at any rate (who may remain agnostic)—were it not the case that Canales' narrative at times borders on imbuing that elusive fraction of a second with a quasi-causal, historical agency. While one certainly need not worry that *A Tenth of Second* aspires to deal in psycho-history or some such naturalistic sin, it would take little imaginative effort to read it as such (were one so inclined); and even so, the somewhat ill-specified status of Canales' semi-physiological protagonist tends to slightly diminish the force of her overall argument. It is not, for example, always clear what the exact stakes were and who were the various parties involved in the numerous controversies that she examines in the course of her book, nor what ultimately connected them, their convergence on the temporal nature of cognition apart. Similarly, despite the obvious emphasis on instrumentation in her account, the detailed workings and the technological background of the production of such minuscule, exacting intervals of time—'time microscopy', in the words of the great Helmholtz—will largely have to be inferred. (Readers familiar with, say, Rebecca Solnit's story of chronophotography in the 'Technological Wild West' [2003] will find perhaps too much credit given in this regard to the European metropoles). Or again, whence the cutoff, or 'closure', of the story around 1920 is an issue likewise given somewhat short shrift; it certainly is one that may have deserved better justification, however, assuming that human, cognitive 'reactions' and perceptual competences became more rather than less significant as the world gradually turned post-industrial.

The result is the occasional feel of montage. Indeed, no fully consistent, historical explanation emerges as to why 'reaction time' popped up in so many places, whence it seemed so significant to so many actors. There are, to be sure, pointers enough. Above mentioned 'cultures of reaction', for one, may have provided one such line of historical argument, illuminating the import of 'reaction time' from a wider, cultural rather than primarily intellectual perspective. There is mention, for instance, in this connection of the significance of the Franco-Prussian war, and of the less academic dimensions of reacting (quickly), but little is made of it. The same may be said for its appropriations and elaborations by psycho-technicians and other folk taking an interest in the optimal, visual performance not merely of scientists but a much vaster population of laboring men and women. On a related note, one might complain that psycho-optical phenomena such as 'flicker' and 'fusion' meant food for thought not only to aspiring cinematographers but also to those, say, who were engaged in the more mundane (and emphatically modern) tasks of street lighting and factory illumination.

'Reaction time', in other words, may have been a thing with more, and more profound, connections to the real world than what Canales' at times slightly science-centered narrative would seem to suggest. Or a stronger, and even richer, case may have been made by embracing a wider and less aesthetic notion of the modern; by embracing a world, that is, increasingly, and quite generally, infused with signals, symbols, and messages emitted from all manner of machines.

The fact remains that conveying the intricate genealogy of a thing such as 'reaction time' is no trivial task, and these criticisms should not distract from Canales' considerable achievement in this direction. Indeed, in the days of Google-based research, we would all seem to be facing the challenge of narrating a somewhat rhizomatic, sprawling, and non-linear kind of material; and here, when it comes to writing the correspondingly complicated histories, one could do a great deal worse than taking Canales' story as a model case (something still rather hard to come by, certainly as regards the history of science). Whether or not, then, the realms of the microtemporal in fact do allow us 'to rethink 'modernity'', as Canales claims, 'both as a chronologically delimited period and as a conceptually defined category', is a quite secondary matter from this perspective [219]; at the very least, *A Tenth of a Second* is an unusually well crafted and intelligent complication of a *certain* story of modernity: one in which scientists and philosophers set the tone, and one that prominently features that classical *topos*—a profound crisis of perception induced by the irresistible progress of technology.

BIBLIOGRAPHY

Crary, J. 1990. *Techniques of the Observer: On Vision and Modernity in the Nineteenth Century.* Cambridge/MA.

Daston, L. and Galison, P. 1992. 'The Image of Objectivity'. *Representations* 40:81–128.

Dierig, S. 2006. *Wissenschaft in Der Maschinenstadt.* Göttingen.

Hoffmann, C. 2006. *Unter Beobachtung. Naturforschung in Der Zeit Der Sinnesapparate.* Göttingen.

Lenoir, T. 1994. 'Helmholtz and the Materialities of Communication'. *Osiris* 9:184–207.

Mills, M. 2011. 'Deafening: Noise and the Engineering of Communication in the Telephone System'. *Grey Room* 43:118–143.

Otter, C. 2008. *The Victorian Eye: A Political History of Light and Vision in Britain, 1800–1910.* Chicago.

Rabinbach, A. 1992. *The Human Motor: Energy, Fatigue, and the Origins of Modernity.* New York.

Schivelbusch, W. 1977. *Geschichte Der Eisenbahnreise. Zur Industrialisierung Von Raum Und Zeit Im 19. Jahrhundert.* Munich.

Solnit, R. 2003. *River of Shadows: Eadweard Muybridge and the Technological Wild West.* New York.

Darwinism and the Divine: Evolutionary Thought and Natural Theology by Alister E. McGrath

Chichester, UK: Wiley-Blackwell, 2011. Pp. xiv + 298. ISBN 978–1–4443–3344–2. Paper $34.95

Reviewed by
Lily Huang
University of Chicago
lilyx@uchicago.edu

The title of Alister E. McGrath's book, *Darwinism and the Divine: Evolutionary Thought and Natural Theology* suggests a balance scale or two sides of a mirror: it does not contain a thesis but is rather a distillation of McGrath's premise, which is to do some rearranging of colossal, accepted ideas. His clear, well-paced argument demonstrates why Darwinian evolution and natural theology are not necessary antagonists—nor, in fact, historical ones. These are rarefied categories of thought; their actual contents are more eclectic and more mutually permeable than usually supposed. Natural theology is a generic name for a variety of traditions, some of which have opposing ideological commitments. 'Darwinism', too, has stood for widely different accounts of the nature of life, some of which have had the very character of dogma commonly associated with religious faith. McGrath's book, an expansion of the the Hulsean Lectures that he gave at the University of Cambridge in 2009, is a methodical repositioning of these two bodies of thought with respect to each other, starting with properly historicized definitions of the terms. This is followed by a close look at the development of Darwin's ideas in the particular context of English natural theology. The book concludes with the author's vision for a contemporary natural theology that offers answers that the science of evolution cannot.

A telling feature of McGrath's book is that the ideas of natural theology are always presented as belonging to *a* natural theology: there is no single natural theology that Darwinism would come to rival but a number of distinct theological interpretations of nature that included, but were not limited to, the ideas of the English Romantic period. Some of their sources were Cicero, Bonaventura of

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 382–385

Bagnoregio, Augustine of Hippo, Francis Bacon, Sir Thomas Browne, John Ray, Thomas Sprat, and Aubrey Moore. Sprat had an interesting theory about miracles: he believed them to be, as McGrath writes with a certain deliberate mildness, ‘a divine prerogative to be exercised only in situations of exceptional human dullness’, that is, when people were hopelessly incurious about the world and God had to make an effort to be noticed. For McGrath, these different interpretations of the divine in nature are not only important parts of natural theology’s larger historical context but also offer possibilities for a contemporary Christian understanding of the natural world. Saint Augustine’s concept of divine creation, for instance, in which God created the world’s potentialities, is something that McGrath believes could greatly inform a contemporary natural theology which can coexist with a faith in scientific investigation.

McGrath, who was trained in theology and molecular biology, is also versed in science studies and the language of scientific revolutions. This does not give him opportunity to dismiss scientific paradigms as truth alloyed with historical errors; rather, he is so receptive of ideas from the philosophy of science that he imports them to an understanding of religious thought. ‘Every style of “natural theology” is embedded in a social matrix’, he writes, ‘consisting of a series of assumptions.’ He shows English natural theology to be the product of English natural philosophy: its key revelations came not from religious quarters but rather from what we would now call science. Newton’s discovery of mechanical regularities was strong evidence for order in the physical world—namely, God. ‘Physico-theology’ was an active field of serious speculation well before the arrival of William Paley.

Now, if any historical figure emerges from this book a little worse for scrutiny, it is Paley: McGrath—though never accusatory—portrays him as a great popularizer with few original ideas, including the famous analogy of the watch on the heath, which Paley took from the work of the Dutch writer Bernard Nieuwentyt. And if any natural theology really became outmoded after the publication of *The Origin of Species*, it was Paley’s. It was the idea of contrivance, illustrated by the watch analogy, that became Darwin’s foil. McGrath distinguishes this kind of natural theological argument, which he calls ‘an argument from design’, from another kind, ‘an argument to design’. The first is based on the principle that, in McGrath’s paraphrase, ‘Order implies an orderer’; whereas the principle of the second is, ‘There is no purpose without a purposer.' Even before Darwin's writing, Paley's natural theology had been criticized on theological grounds. Here McGrath gives a clear explanation of why Paley's doctrine was deficient in the view of theologians like Cardinal John Henry Newman: it did not touch on the significance of morality and it gave up spirituality in its appreciation of celestial orderliness and regularity. Later, McGrath explains how the idea that 'contrivance proves design' involves a confounding of evidence and inference. Design, if it exists, cannot be observed; it can only be inferred. Altogether, McGrath shows Paley's natural theology—which both later Darwin scholars and history-conscious evolution theorists have tended to take as *the* natural theology—to be both pseudo-scientific and soulless.

Darwinism and the Divine is an account of two sets of ideas with a nuanced, entangled past. In the opening of part 3 of the book, McGrath argues for 'a wider teleology' to make room for design alongside a thoroughly Darwinian view of evolution. The surprise champion that he chooses for this idea is none other than Thomas Henry Huxley, Darwin's most loyal and articulate defender. But what makes the book even more interesting is the wider teleology that McGrath himself has given to these ideas. The story of these ideas also touches on other ideas in the philosophy of science—on the nature of inference, for instance, and the nature of belief—that give a richer texture to the book's argument. The names of some of the book's secondary cast will already give you an idea: they include Charles Peirce, William James, Iris Murdoch, Stanley Fish, and Simone Weil. Two primary characters are, of course, Richard Dawkins and Daniel Dennett, prominent and militant Darwinian atheists. McGrath is admiring even of his adversaries: they are 'distinguished' and 'brilliant'; he calls Dawkins' book *The Selfish Gene* [1976] an 'early masterpiece'. But the concluding sections of the book take Dawkins' and Dennett's doctrinal rejection of all metaphysical speculation to task as both spiritually impoverishing and logically untenable. 'The declaration that "all metaphysical statements are meaningless"', McGrath writes, 'turns out to be self-referential and potentially self-refuting.' His own conclusion recognizes science and natural theology as two enterprises that attempt to answer two manifestly different sets of questions. One interprets evidence for processes; the other offers an interpretation of how humans can relate to a world that works by such processes. This, at least, is McGrath's vision of an enduring natural theology: 'A Christian natural theology', he writes, 'holds that the true meaning of nature is indeed capable of being unlocked; but this requires us to use a hermeneutical key that nature itself cannot provide.'

BIBLIOGRAPHY

Dawkins, R. 1976. *The Selfish Gene.* Oxford/New York.

BOOKS RECEIVED

Aestimatio 9 (2012)

Chris Bennett, *Alexandria and the Moon: An Investigation into the Lunar Macedonian Calendar of Ptolemaic Egypt.* Leuven/Paris: Peeters, 2011.

Mark E. Borrello, *Evolutionary Restraints: The Contentious History of Group Selection.* Chicago/London: University of Chicago Press, 2012.

Kevin Corrigan, John D. Turner, and Peter Wakefield edd., *Religion and Philosophy in the Platonic and Neoplatonic Traditions.* Sankt Augustin: Academia Verlag, 2012.

Andrea Falcon, *Aristotelianism in the First Century BCE: Xenarchus of Seleucia.* Cambridge/New York: Cambridge University Press, 2012.

Allan Gotthelf, *Teleology, First Principles, and Scientific Method in Aristotle's Biology.* Oxford: Oxford University Press, 2012.

James Hannam, *The Genesis of Science: How the Christian Middle Ages Launched the Scientific Revolution.* Washington, DC: Regnery Publishing, 2011.

John Shannon Hendrix, *Robert Grosseteste: Philosophy of Intellect and Vision.* Sankt Augustin: Academia Verlag, 2010.

John Henry, *Religion, Magic, and the Origins of Science in Early Modern England.* Farnham, UK/Burlington, VT: Ashgate, 2012.

Joel E. Mann, *Hippocrates: On the Art of Medicine.* Leiden/Boston: Brill, 2012.

Vanna Maraglino ed. *Scienza antica in età moderna. Teoria e immagini.* Bari: Cacucci Editore, 2012.

Matteo Martelli, *Pseudo-Democrito. Scritti alchemici con il commentario di Sinesio. Edizione critica del testo greco, traduzione e commento.* Paris/Milan: S.É.H.A/Archè, 2011.

Stephanie Moser, *Wondrous Curiosities: Ancient Egypt at the British Museum.* Chicago/London: University of Chicago Press, 2012.

ISSN 1549–4497 (ONLINE) ISSN 1549–4470 (PRINT) ISSN 1549–4489 (CD-ROM)
Aestimatio 9 (2012) 386–387

Vivian Nutton with Gerrit Bos, *Galen: On Problematical Movements.* Cambridge/New York: Cambridge University Press, 2012.

Peter E. Pormann ed. *Epidemics in Context: Greek Commentaries on Hippocrates in the Arabic Tradition.* Berlion/Boston: De Gruyter, 2012.

Marwan Rashed, *Alexandre d'Aphrodise, Commentaire perdu à la* Physique *d'Aristote (Livres IV–VII). Les scholies byzantines. Édition, traduction et commentaire.* Berlin/Boston: De Gruyter, 2011.

Sabine Rommevaux, Maryvonne Spiesser, and Maria Rosa Massa Esteve edd. *Pluralité de l'algèbre à la Renaissance.* Paris: Honoré Champion, 2012.

Livio Rossetti and Massimo Pulpito edd. *Jonathan Barnes et al., Eleatica 2008: Zenone e l'infinito.* Sankt Augustin: Academia Verlag, 2011.

Neil Safier, *Measuring the New World: Enlightenment Science and South America.* Chicago/London: University of Chicago Press, 2012.

S. R. Sarma and Gyula Wojtila edd. *Scientific Literature in Sanskrit.* Delhi: Motilal Banarsidass Publishers, 2011.

Leslie Tomory, *Progressive Enlightenment: The Origins of the Gaslight Industry, 1780–1820.* Cambridge, MA/London: MIT Press, 2012.