# *AESTIMATIO*

Critical Reviews in the History of Science

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Edited by
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# 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.

When we first began publication in 2004, the plan was to make the individual reviews in *Aestimatio* available primarily online as typeset files that could be read on screen in a web browser or downloaded and printed. But recently, we have arranged with Gorgias Press to publish all our annual volumes in print. We are very grateful to George Kiraz of Gorgias Press for his interest in *Aestimatio* and hope that this new mode of publication will enhance the utility of *Aestimatio* to its readers.

Alan C. Bowen

Tracey E. Rihll

*Two Hippocratic Treatises,* On Sight *and* On Anatomy. *Edited and Translated with Introduction and Commentary* by Elizabeth M. Craik

Studies in Ancient Medicine 33. Leiden/Boston: Brill, 2006. Pp. iii + 183. ISBN 978–90–04–15396–7. Cloth € 99.00, $134.00

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Two distinct parts make up this volume: the critical editions (with translation, introduction, and commentary) by Elizabeth M. Craik, now a member of the Northern Centre for the History of Medicine in Newcastle, of two medical treatises conveyed as parts of the Hippocratic Corpus, but neither written by Hippocrates himself. Since *On Anatomy*, a very short fragment, has already been published by Craik in *Classical Quarterly* [1998],[1] I will pay more attention to the first treaty, *On Sight*. Proving to be abrupt and sometimes obscure, this treatise actually needed to be reconsidered and revised critically.

The text of *On Sight*, as the preserved manuscripts show, is very corrupt: Sichel thought it impossible '*de reconstituer un texte irréprochable*'; Joly shared the same idea about '*son caractère parfois sommaire et son état de mutilation*'. The main contribution of this volume does not lie in its reconsideration of the manuscript tradition[2] but in its more in-depth examination of the text as seen within the historical development of medicine. The retrieval of the original text is attempted by resorting to parallel passages in other Hippocratic treatises or in Galen or Celsus—a quite 'hazardous enterprise' [6] because of the uncertainty of the exact position of this text in that scientific tradition, as Craik acknowledges. However, it may also

[1] In an appendix on p. 169, Craik takes account of M.-P. Dumenil's edition of *On Anatomy* [1998], which appeared in the Collection Budé soon after her article in *Classical Quarterly*.

[2] Craik confirms that the only testimony is Marc. gr. 269 [M], while its apographs are unfortunately worthy only for the history, not the constitution, of the text. The *variae lectiones* from Paris. gr. 2142 [H], Paris. gr. 2140 [I] and Vat. gr. 277 [R] are quoted in the apparatus on a regular basis.



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prove a potentially fruitful one, if sustained—and this is the case—through sound judgment regarding language and style. Craik, in fact, states that most of these matches with parallel passages are the result of the 'long currency and inherent conservatism of the physiological theories and surgical procedures concerned' [21].

A brief remark about the reception of this text is in order. Since Sichel, commentators were used to saying that there was no allusion to this treatise in any later medical writing. Craik [7] recalls the Galenic gloss ἄτρακτον and Erotian's gloss φολίδα, which are relevant to different passages of *On Sight*; but she fails to note Galen's *Commentary on Epidemics 2*, which is transmitted in Arabic. Here, in the course of discussing physiognomy, Galen names his teacher (and Hippocratic interpreter) Pelops, along with Numesianus: they cited *On Sight* as asserting that blue eyes denote a warm complexion of organism.[3] Even though this detail is not in our Hippocrates' text, Galen's context guarantees that *On Sight* was included in the Corpus in the second century at least, and that it was read in a much less disfigured state.

The very content of the treatise is questionable: the ὄψις of the title is a word with many meanings, ranging from 'eye' to 'sight' and the editor points out that both abstract and concrete senses are here involved. Foesius, one of the first editors, wondered whether *On Sight* should be counted among surgical or therapeutic tracts. Actually, we cannot be certain about even that, as we are not sure that sight was the exclusive content. For instance, chapter 3 contains information and instructions about the cauterization of the back, giving a hint of the fragmentary nature of these pages, which appear to be no more than a sliver of some wider text about procedures—most likely with a mainly surgical concern—on how to regain health. This somehow justifies the joint publication of both *On Sight* and *On Anatomy*, because the latter too 'may be an abridgement of a fuller and more flowery account' [120], although they are probably of different origin.

The often peremptory and authoritative language, the primitive paratactic syntax, the rough structure with juxtaposed clauses and

[3] Cf. Smith 1979, 152–153. The reference to Galen's *Commentary on Epidemics 2* did not escape Anastassiou-Irmer 1997, 458–459 (among '*nicht identifizierte Testimonien*'). About blue eyes, see also Galen's *Commentary on Epidemics 3* [3.72: cf. Wenkebach 1936, 152].

elliptical expressions reveal that *On Sight* had to be a plain manual of practical medicine, a sort of teaching tool for apprentice surgeons who had to know how to trephine the skull or how to scrape the eyelid. According to Craik's suggestive idea, *On Sight* contains a set of notes supplementary to demonstrations of surgical treatments. The author's concern for prognosis is much stronger than his interest in diagnosis. Because of his familiarity with the practices of surgery, we can suppose that he had some knowledge of human anatomy and pathology, but there is no doubt that he did not know the inside structure of the eye. (He seems to share the common idea that a flux of noxious moisture coming down from the head is the cause of eye disorders, even though he apparently[4] distinguishes between a superficial upper flux from the area above the skull and a deep lower flux from the brain [10].) The most striking peculiarity noted by Craik about language of *On Sight* is, however, the lack of technical terminology: indeed, very ordinary words are used to describe pathological phenomena, such as διαφθείρεσθαι (to be destroyed) [*On Sight* 1.1] to designate the loss of sight.

This is also the main reason why we cannot indisputably identify the nature of the diseases described in this work. Retrospective diagnosis is always a very hard and sometimes precarious exercise, as every reader of Hippocratic treatises knows; and thus it is the subject on which there is most disagreement. One should also remember that in a corrupt text every evaluation of the language can only be provisional. An example is found in chapter 2, where Joly read τὸ ὄμμα and translated '*quant à la vue elle-même, la pupille étant saine, chez les individus jeunes . . . vous ne l'améliorerez par aucun moyen.*' Sichel interpreted it in a similar fashion ('*quant à la vision des yeux*') and, while admitting that '*ce chapitre encore est très obscur,*' conjectured '*une amblyopie amaurotique*'. On the contrary, τὸ ὄμμα (the eye) is not ὄψις (sight), and giving it the meaning of a semantic rarity (according to Sichel, this would be the only case in the Corpus where ὄμμα means 'vision') does not aid the reader. Craik's suggestion is persuasive: she emends ὄμμα to λήμια (the mistake, a *lectio facilior*, is understandable on a paleographical basis as well) and thus interprets the whole chapter as referring to 'sores in the eye', a characteristic symptom of conjunctivitis or blepharitis. Therefore, the action

[4] This supposition rests on a conjectural text, *On Sight* 3.3.

of scraping and cauterizing the eyelid becomes perfectly conceivable as a therapeutic method. Through this amendment it is possible to recognize a probable echo of *On Sight* in Celsus, *De med.* 7.7.3.

Unfortunately, not every problem obtains a definite solution. In the first chapter, for instance, the anonymous author of *On Sight* talks about a disease in which sight is suddenly damaged, the eyes 'become spontaneously lapis-like', and no treatment is successful. Sichel thought of a glaucoma; Ermerins guessed it might be cataract, although showing particular symptoms, while Craik remarks that neither is usually characterized by such a sudden onset and the greenish reflex in glaucoma is noticeable only at an early stage of its development. Therefore, defining the disease is quite difficult and it may be incorrect on a methodological basis to propose a precise diagnosis.

Chapter 4—a rather interesting one—describes procedures in scraping and cauterizing the eyelid in case of trachoma, according to Sichel and others: but Craik notes [76] that in the second part the author prescribes the cutting into the forehead *after* the healing of the wound of the lids, when one would suppose that the process of recovery had already started: this seems odd, to say the least. It is difficult to say whether this sort of *hysteron-proteron* occurs because the text is corrupt (a misplacement or an intrusion is presumed by Craik) or as a consequence of its abridged form, which prevents us from understanding the rational grounds of procedures described.

As the text does not allow for a full understanding, it would be risky to change any reading if one can make no sense of it. Craik's edition reasonably tends to be sometimes more conservative than is Joly's. Furthermore, not only details are liable to misinterpretation, but also the whole outline of the work and thus its origin can give way to misunderstanding. Sichel stressed the similarities between *On Sight* and other texts of the Hippocratic Corpus, such as *Affections* and *Sores*, recalling that the author of *Affections* assured his audience that he would in future write about ocular diseases [*Aff.* 5]. Ermerins and Joly thought of a Cnidian origin and the traditional opinion of a Cnidian preference for cauterizing strengthened this view. However, the very existence of a doctrinal distinction between the fifth/fourth century Cnidian and Coan medical schools has been questioned[5] and

[5] Already before Joly's edition [1978]: cf. Smith 1973; Lonie 1978; Di Benedetto 1986.

a more complex idea of the circulation of medical knowledge has prevailed: 'a free exchange of ideas and techniques between the medical centres' [Langholf 1990, 5]. It is within this interpretation of the mutual interconnections among the treatises of the Hippocratic Corpus that Craik tries to define the nature of this work. According to her, cauterization is nothing more than a widespread practice and is consequently not distinctive. Similarities in language with *Places in Man, Glands, Fractures, Articulations* [see Craik 2005] suggest that its author may have come from Italy or Sicily (Alcmaion of Croton was known to have dissected the eye). Nonetheless, Craik is more inclined to suppose North-African origins, mainly because of the peculiar interest in trachoma, a bacterial—not viral, as stated by Craik [77]—disease which is known, through evidence of existing papyri [cf. Marganne 1994, 3; Luiselli 2004, 52–54] to have much affected Egypt.

We must admit that the foundation for such a hypothesis is not thoroughly compelling, and it cannot be otherwise: still what is really convincing is Craik's choice to consider the problem of the origins of *On Sight* within the context of the problems relating to 'intertextuality' in the Hippocratic Corpus. This issue has been recently addressed by Craik in a paper published in *Mnemosyne* [2006] where she borrows an expression from textual criticism and talks about a 'horizontal tradition' to describe the ancient practice of collecting, copying, and adapting medical material of different kinds, such as 'aphoristic rules for reaching the correct conclusion in the interpretation of key signs and symptoms'. Actually, although *On Sight* cannot be considered as a collection of aphorisms, its structure reveals an analogous pedagogical function to train physicians. We might not be able to say whether this occurred in Egypt or in Greece or wherever, but definitely this work held a wealth of knowledge which could easily be transmitted and adapted for different contexts.

I will conclude by listing the content of the chapters of *On Sight*, according to Craik's interpretation:

1. a. Lapis-like eye: maybe a glaucoma at an early stage
   b. Sea-like eye: cataract
   c. Intermediate kind of eye: cataract or intraocular inflammation
2. Sores
3. Cauterization of the vessels

4. Procedures to scrape and cauterize the eyelid, as a consequence of trachoma
5. *Pterygion* or any kind of palpebro-conjunctival cysts or lesions
6. Preparation of the salve for irritation of the eyelid (blepharitis)
7. Night blindness
8. Trepanation of the skull as a treatment for deteriorating eyesight
9. Seasonal ophthalmia or hay fever

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*Following Pausanias: The Quest for Greek Antiquity* edited by Maria Georgopoulou, Céline Guilmet, Yanis Pikoulas, Konstantinos Staikos, and George Tolias; and translated by Deborah Kazazi

New Castle, DE/Athens: Oak Knoll Press/Kotinos Publications, 2007. Pp. 255. ISBN 978–1–58456–209–2. Cloth $75.00

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In May of 2007, an international conference was held in Athens under the title Στὰ Βήματα του Παυσανίου (literally translated, *In the Footsteps of Pausanias*). The conference was a well-funded affair, with substantial support from the European Union and the Greek Ministry of Culture. Large glossy advertisements for the event were posted on the walls of the Athens Metro and in other high-visibility locations throughout the city. The speeches and paper sessions were accompanied by a poster display in the lobby of the National Hellenic Research Foundation and by a special exhibit of Pausanias-related material from the collection of the Gennadius Library of the American School of Classical Studies. The standing-room-only crowds attending the event were offered mementos to purchase, including a beautiful large-scale map showing the routes of Pausanias, and an attractive companion-volume in Greek bearing the title of the conference. Altogether, a remarkable amount of attention was lavished on an author who is little known outside of academia and who remains, in the eyes of many scholars of antiquity, a dull and uninteresting recorder of interesting information. A short time after the conference, an English translation of the companion-volume emerged with the title transformed to *Following Pausanias*; and it is this edition that is currently under review.

Participants in the conference included philologists and archaeologists from several countries, yet the organizers and a majority of the contributors to both the conference and the current volume were Greek scholars, which is worth noting because little significant scholarship on Pausanias (as distinct from the sites and monuments


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that Pausanias tells us about) has been published in Greek since the richly illustrated edition and commentary by Nikolaos Papahatzis of the 1970s. As we are told in a foreword by Paschalis Kitromilides, Director of the Institute for Neohellenic Research in Athens, the Greeks have always had a special relationship with Pausanias; and the images of ancient Greece that Pausanias uniquely preserves possess great iconic value for modern Hellenes as they negotiate the tortuous dialogue between their own culture and the legacy of their ancestors. If the conference and this volume mark the beginning of a new and fertile period of productivity on the Periegete from the perspectives of contemporary Greek scholars, it will be a welcome development.

As one might expect from the circumstances of its origins, *Following Pausanias* has something of the character of a popularizing coffee-table book (though one with numerous citations of scholarship and a copious bibliography at the end), and serious students of antiquity will want to keep an eye out for the expected publication of papers from the conference. But there are some parts of this volume that break new ground and will be of immediate interest to the scholarly community. It is on these sections of the book that I will concentrate my comments.

The longest and most important part of the text is chapter 3, 'Pausanias in Modern Times (1418–1820)', in which five scholars, Céline Guilmet, Konstantinos Staikos, George Tolias, Alex Malliaris, and Aliki Asvesta, combine to trace the reception of Pausanias' work from the first hint of its existence in Renaissance Italy through to the 19th century. This work takes a solid step toward filling a longstanding lacuna in the *Rezeptionsgeschichte* between, on the one hand, Aubrey Diller's fundamental series of articles on the transmission of the text through the middle ages [Diller 1955, 1956, and 1957], and, on the other, recent studies of the use and abuse of Pausanias by scholars of the late 19th and early 20th century such as Wilamowitz and Farnell.[1] The authors do a good job illustrating the importance of

[1] On Wilamowitz, see Habicht 1998, 165–175; on Farnell, Henderson 2001. On other readers of the 19th and 20th centuries, see Sutton 2001, Wagstaff 2001, Beard 2001. It should be noted that earlier reception is also covered, in complementary rather than redundant fashion, in Maria Pretzler's new book on Pausanias [2007, 118–135], which appeared almost simultaneously with this one.

Pausanias to Renaissance scholars, whom the Ottoman conquest deprived of physical access to Greece soon after the text first appeared in the West. There are times when one wishes that the analysis offered here was more extensive and more precisely documented, but for the type of book this is one cannot complain too much on that score. The authors call attention to a number of interesting original texts; for instance, the extraordinary Latin paraphrase of the *Periegesis* in the form of a dialogue published by Stefano Negri in 1517, only a year after the publication of the *editio princeps*. In his introduction, Negri extols the didactic value the text: the literary journey upon which Pausanias takes the reader can, in Negri's view, go some way toward replicating the illumination from actual travel to antique lands, an experience that was no longer available to the young Philhellenes of Renaissance Europe.

The list of personages who knew and were affected by Pausanias is long and impressive: Lascaris, Chalcondyles, Rabelais, Rubens, Racine, Diderot, among others. Although the authors do not make this connection, scholars pondering the text's ancient reception (or rather, the reception that Pausanias might have expected) would do well to consider the range of responses that one meets in this later pre-modern period. To many readers of the 15th, 16th, and 17th centuries, Pausanias was considered a delight to read rather than the plodding drudge that he seems to many members of his current audience; his intellect was highly regarded and he was thought of as a historian as much as he was as a geographer or topographer. Also of interest is the influence of Pausanias in the early days of the discipline of Art History; here Guilmet's well-illustrated essay on early artistic representations of monuments described by Pausanias is particularly eye-opening.

An important development that one can trace in these pages is the emergence of the common present-day view of Pausanias as a congenial and trusty dimwit, and of his text as a treasure-trove of useful information rather than a work of intellectual merit and literary sophistication. As access to Greece was regained in the 18th and 19th centuries, the utilitarian value of Pausanias *qua* travel guide tended to become the primary focus of readers' interest. For instance, one composer of an abridged edition of Pausanias promises his readers that he will omit 'all the useless digressions [i.e., the historical and

mythical excursuses that enlivened and gave meaning to the landscape for earlier readers] so that the author's route may be better followed'.[2] Another fascinating theme in this chapter is the political dimension to Pausanias' reception. The early interest in Pausanias in the Renaissance was fueled to some extent by anxiety over the Ottoman conquest, and many, including expatriate Greeks like Musurus, who produced the *editio princeps* of Pausanias for the Aldine press in 1516, openly expressed the hope that Pausanias' vivid images from a land now lost would inspire the western powers to action against the Ottomans. The sequel to this part of the story comes in the excellent but tantalizingly brief essay by Tolias on the importance of Pausanias to Greek scholars, and Greeks in general, during the era of the War of Independence. For Hellenes of this time, Pausanias provided not just a record of their past but a blueprint for their future; and the shape of the territory covered by Pausanias even influenced early concepts of what the modern Greek state should comprise geographically.

At times the collaborative nature of this chapter leads to some redundancies and inconsistencies. For instance, when Guilmet states that artistic renditions of stories in Pausanias envisioned by Pontus de Tyard (1585) would have been the first depictions based on excerpts from Pausanias' *Periegesis* [129], one wonders how that statement fits with scholarship cited earlier by Tolias that argues for Pausanias' influence in artworks by Poliziano and Antonio Lombardo nearly a century earlier [97]. But, by and large, the various contributions complement one another effectively. A somewhat more substantial criticism is that the authors of this section, who tend, quite appropriately, to be scholars of things other than ancient literature and civilization, could have benefited at times from more engagement with the classical scholarship on Pausanias. For instance, in discussing 19th-century Greek interest in Pausanias, Tolias mentions the work that the greatest Greek philologist of the time, Adamantios Korais, did on the posthumous edition of Étienne Clavier's text of the *Periegesis* [1814]. One interesting thing about this work that Tolias does not mention is that Korais recommended the rejection of what has turned out to be Clavier's single most influential textual

[2] Le Roy 1758, 2.32, as quoted on p. 127 of the present volume (parenthesis added).

intervention: the insertion of a preposition at 8.27.1, turning Pausanias' apparent reference to the 'misfortune of Roman rule' into a reference to a 'misfortune <during> Roman rule'. The case for and against this emendation has long been a bone of contention among scholars trying to gauge Pausanias' attitudes toward the Roman empire.[3] Whatever philological reasons Korais had for rejecting it (and he specifies none in his notes to Clavier's edition), one suspects that the role he played in detaching the modern Hellenic state from a latter-day imperial power had something to do with it.

Scholars will also be attracted to chapter 4, 'Pausanias Today: an Evaluation', which consists mostly of essays by various scholars on some of the major sites described by Pausanias. The concept of this section holds great promise, since the authors are all prominent archaeologists who are intimately involved in the excavation and/or surface exploration of the sites in question (Leda Costaki for Athens, Eleni Kourinou for Sparta, Xeni Arapogianni for Olympia, Rozina Kolonia for Delphi, Petros Themelis for Messene, and Yanis Pikoulas on 'settlement patterns'). The contributors are thus in an excellent position to provide authoritative and up-to-date analyses of how Pausanias' text matches up with the remains on-site. Unfortunately, this potential remains mostly unfulfilled. Apart from Costaki's detailed and remarkably clear explication of Pausanias' itineraries in Athens, the other offerings are cursory and do little to elucidate the author's aims and methods in describing ancient sites. Arapogianni's treatment of Olympia, for instance, is not really about Pausanias' description at all but about Olympia in the Roman period. Symptomatic is the fact that she discusses the monumental nymphaeum of Herodes Atticus without bothering to tell the reader that Pausanias refrains from making the slightest mention of it. So diverse and inconsistent are the contributions in this section that one suspects that the editors were not clear enough in their instructions to the contributors or did not hold them to their instructions. One point on which the authors are (unfortunately) consistent is that they all seem to assume that Pausanias' text is an immediate record of things the author saw, in the order that he saw them, on a single jaunt through each site; hence, we frequently read things like 'Pausanias entered the city…', 'next

[3] For a review of the controversy, see Pretzler 2007, 28–29.

he saw . . . ', 'he has turned back . . . '. A considerable amount of recent scholarship has demonstrated the danger of such assumptions,[4] and it is difficult to say whether this tendency in the chapter results from ignorance of that scholarship or from an editorial decision to avoid such academic nuances. One benefit that readers will get from this chapter is the generous number of maps and plans provided as illustration. In some cases, these graphics help to compensate for the limitations of the sketchy written reports.

The remaining chapters are largely unobjectionable but also unremarkable from a scholarly point of view: chapter 1 on 'The *Periegetes* Pausanias and his Era' presents little that will be new to readers familiar with, e.g., Habicht's treatment of these issues [1998],[5] and there is little sign that the authors have consulted scholarship more recent than Habicht. Where they innovate, the results are often infelicitous (for instance, the fact that Pausanias mentions no emperor after Marcus Aurelius hardly means that AD 180 'constitutes an indisputable *terminus ante quem* for his life' [38]). Readers (even non-academic readers) seeking an introduction to Pausanias' life and times in English would do better to go straight to Habicht or to Maria Pretzler's new book [2007], which appeared too late for the authors of *Following Pausanias* to take into account. The same could be said for those seeking an introduction to Pausanias' aims and methods as a topographer, something that chapter 4 does not really succeed in delivering. Finally, chapter 2 by Guilmet, surveying the few things we know about the transmission of Pausanias' text 'From Antiquity to the Renaissance', is only a page and a half long and could have easily been worked into chapter 3.

In sum, the chapter on Pausanias' reception is truly important and original, and is highly recommended for those who are seriously interested in Pausanias or in the early-modern reception of antiquity; but there is little else in the text of the volume that any reader will

[4] See, for instance, Akujärvi 2005, 19–20; Hutton 2005, 25–29; Pretzler 2007, 8. For concrete examples of cases where such assumptions have led readers astray, see Habicht 1998, 165–175. See also Williams [Williams and Fisher 1975, 25–29] on the identification of temples in the forum at Corinth that were previously misidentified [Scranton 1951, 3–73] on the basis of just such a fallacious reading of Pausanias.

[5] A paperback edition, largely unrevised, of his publication of 1985.

be much poorer for having missed. In addition to the essays, however, the book is also richly illustrated, with glorious photos of manuscript pages and the like (many in color, and many from the outstanding collection of the Gennadius), and with numerous maps, plans, and diagrams, including a miniaturized version of the poster-sized map of Pausanias' routes mentioned above (produced under the direction of Pikoulas, who is currently unrivaled as an authority on ancient roadways in Greece). For the visual material alone, the book would be a worthwhile addition to anyone's library.

In closing, it should also be mentioned (especially since her name is quite hard to find in the front matter) that Deborah Kazazi's translation from the original Greek is remarkably clear, accurate, and idiomatic. I noticed only the occasional problem: e.g., it should be 'sacred way' at Delphi, rather than 'secret way' [213], and 'beyond the Tholos' rather than 'above the Tholos' at Athens [198].

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*Through a Glass Darkly: Magic, Dreams and Prophecy in Ancient Egypt* edited by Kasia Szpakowska

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When the Apostle Paul wrote his famous ode to selfless love (ἀγάπη) in one of his letters to the Early-Christian community in Corinth (1 Corinthians 13), little did he know that his words would prove to be perfect one-liners applicable to a variety of contexts and situations two millennia later. Whoever attended a wedding must have heard the words 'And now faith, hope, and love abide, these three; and the greatest of these is love' (1 Corinthians 13.13[1]). Similarly, the phrase 'For now we see through a glass, darkly' (1 Corinthians 13.12[2]), a reference to the fact that humans in life can have only imperfect knowledge of the perfect world to come, has a long history of quotation and adaptation in popular culture. It has inspired the title of quite a few novels, books, films, and, record albums. And believe it or not, now there is also a book on magic, dreams, and prophecy in ancient Egypt that carries this title.

The editor of the book, Kasia Szpakowska, does not explain in the introduction why she chose this particular title and how the phrase (or the plethora of intertextual references) is supposed to illuminate the subject under study or to encapsulate the most important conclusions of the book. The volume collects the revised papers presented at an international conference organized by the University of Swansea in 2003. The aim of the conference was to highlight 'current investigations of phenomena related to magic, dreams, and prophecy in ancient Egypt' [ix]. These three concepts, so full of meaning and contention in contemporary scholarship, are not defined in any more detail in the introduction, so that the reader is left with guessing

[1] New Standard Revised Version.
[2] King James' Version.


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how they were related in ancient Egypt, why it is meaningful to study them in conjunction at a conference, and what the book's title has to do with this.

As background for non-Egyptologists, a short note may therefore be helpful. In ancient Egypt, dream interpretation, as one of many forms of divination, was a legitimate and well-developed means of obtaining knowledge about the future. Alternative methods, attested for the pharaonic periods, were the use of books of good and bad days (hemerology) and the interpretation of the shape of oil slick in a bowl of water (lecanomancy). For the later periods, evidence abounds and shows significant diversification in the means and media of divination, which some scholars take as reflecting changes in society and in the perception of the relation between men and the gods. In general, the aim of divination was to foresee misfortune; but it was also used as a legal instrument to expose someone's past misconduct and criminal behavior, or to legitimize political decisions with a label of divine consent. Misfortune was understood in mythological terms and viewed as a deviation from the cosmic and societal order, resulting either from human neglect to observe the proper rituals and social codes or from demonic influence. In the latter case, remedying the precarious situation amounted to interfering in the cosmic cycle of creation and regeneration and mobilizing *heka* or magic, the productive power that the gods used and continue to use to create and maintain the cosmos. To summarize, dream interpretation allowed Egyptians to foresee and anticipate crisis situations, while knowledge of *heka* allowed Egyptians to engage with misfortune actively, either by producing amulets as a means of protection or, in case these proved to be ineffective, by preparing drugs and performing healing rites. And that is why it is so relevant for Egyptologists to study in conjunction magic, dreams, and prophecy—to use the terms of the book.

The 13 articles collected in the volume deal with these phenomena (dream interpretation, divination, and *heka*) in one way or another. The articles are not organized according to these topics, as one would perhaps expect from the title, but follow in the alphabetical order of the author's name. In the introduction, the editor introduces the articles one by one in a different sequence without any apparent principle of classification. This is to be regretted, because, as is so often the case with conference proceedings, the articles form a mixed bag and the reader needs some guidance in order to come

to understand how the articles enrich each other and to discover the comprehensive conclusions of the conference, without which the book cannot be used in a productive way.

For the sake of this review—and as a reflection of my reading experience—I suggest grouping the articles according to the following categories:

- ‘evidence for applied magic in material culture’,
- ‘formularies for divination’, and
- ‘form and function of magic and magicians in literary texts’.

The first group comprises the articles by John Baines (on the restrictive display of amulets in Old Kingdom monumental art), Maria Centrone (on the so-called corn-mummies and rites of the Khoiak festival), Carolyn Graves-Brown (on the meanings attributed to naturally-shaped flint stones with suggestive shapes in the New Kingdom workmen’s community Deir el-Medina), Robert K. Ritner (a survey and classification of serpent wands), and Willeke Wendrich (a discussion of the techniques of binding and knotting in Egyptian magic instigated by an intriguing knotted bracelet found at Tell el-Amarna).

‘Formularies for divination’ are discussed by Joachim F. Quack (a survey of still unpublished handbooks for divination, all dating to the Late and Greco-Roman periods) and Scott B. Noegel (an exploration of the device of punning to guide the interpretation of dreams in ancient Mesopotamia and Egypt).

The topics are addressed from a literary perspective in the articles by Leonard H. Lesko (on the intriguing statements about the end of time in Coffin Texts spell 1130 and Book of the Dead spell 175), Alan B. Lloyd (a survey of the motifs of *heka*, dreams, and predictions in Egyptian literature), Daniel Ogden (on the plot lines and motifs that Lucian’s famous story ‘The Sorcerer’s Apprentice’ shares with Egyptian and Graeco-Egyptian literature of the Hellenistic and Roman periods), R. B. Parkinson (on the meaning and intended effect of the simile of the dream in the variant versions of ‘The Tale of Sinuhe’), and Anthony Spalinger (on reading king Amenhotep II’s dream preceding a day of immolating prisoners of war as a form of pre-traumatic stress syndrome—if I understand the author correctly—instead of manipulative and ideological fiction).

John Ray’s contribution does not fit any of the three categories, but surely deserves mention. Using the dream records preserved

among the famous bilingual and bicultural archive of the *katochos* Ptolemaios and his brother Apollonios as source material, he portrays the social and intellectual milieu at the Serapeum in the mid-second century BC. It is a beautiful case study of how dreams were meaningful in the lives of actual people—even if Ptolemaios was not so ordinary in many respects.

The relevance of the book lies in the fact that it forcefully brings out, though never makes explicit in any of the articles or the introduction, the relative discrepancy between the three categories identified above. There is abundant physical evidence for the performance and application of magical rituals from ancient Egypt, be it for healing, protection, or destruction; but very few of the amulets, execration figurines, amuletic knives and rods, and serpent wands that are on display in museums all over the world have an exact correspondence in the formularies that give instructions and list ingredients for making amulets and healing substances. One wonders where the amulets prescribed in the handbooks are and, *vice versa*, where the instructions are for the objects that we have actually found in the archaeological record. The same holds true for the literary texts. Magic and magicians are common motifs in Egyptian narratives, but for very few of the magical feats performed by these fictional characters can we find instructions in the formularies or any sort of physical evidence in the material record. It is beyond dispute that the three categories share the basic notions about the nature and mechanics of *heka* and the means of divination. Nonetheless, the incongruities are obvious and invite us to reflect on the peculiar nature of our evidence, to outline in sharper relief their commonalities and idiosyncrasies, and to explicate where the gaps in our evidence are. In my opinion, some of the articles extend this invitation to us—and that is very relevant, indeed.

Maria Centrone's article could be read as the paradigmatic article of the collection. In her article 'Corn-Mummies, Amulets of Life' she can hardly hide her frustration with the obvious disparity between, on the one hand, the detailed instructions for manufacturing the corn-mummies of Khenty-Imentit and Sokar, described in detail in the famous compilation of Khoiak texts from Dendera, and, on the other hand, the realities of the archaeological record. The so-called 'grain Osiris figurines', small mummy-form grain packages provided

with the usual Sokar-Osiris trappings and placed in a miniature coffin, are usually taken for the activated products of the Khoiak festival. However, despite obvious similarities, the surviving specimens appear to disagree, on closer inspection, in material, accoutrements, and find locations with the textual instructions. Even if Centrone's alternative explanation, seeing them as the product of yet another ritual (for which there happens to be no textual evidence), fails to convince, her article is important in drawing attention to the discrepancies and putting the burden of proof on future scholars working on the Khoiak rites.

The same holds true for Willeke Wendrich's article on the significance of knotting in Egyptian magic. The article starts with a description of an intricately knotted bracelet found in a refuse dump in the workmen's village at Tell el-Amarna and continues as a useful survey of types of knotting and its usage and meaning in Egyptian ritual. The article relies heavily on the instructions found in the handbooks for healing and protection and concludes, on the basis of the positive and protective value placed on knots and knotting in these formularies, that the knotted bracelet must have been used as an object charged with power, most likely of a protective nature. This is quite convincing, if only for the fact that the bracelet scores high on the 'coefficient of weirdness' scale. Noteworthy, however, is that none of the adduced magic recipes give instructions for anything similar to the knotted bracelet. Yet again, theory and reality are slightly at odds with each other.

John Baines' article addresses the issue from a slightly different perspective. He zeros in on the incongruence between the abundance of amulets excavated in provincial Old Kingdom burials on the one hand and, on the other, the elite's apparent monopoly on the display of amulets in contemporary monumental art. Whereas archaeology teaches us that the use of amulets must have been widespread, the study of statuary and relief leads us to believe that only elite members of society had access to such means of protection. How to solve this apparent paradox? Baines argues that the absence of amulets on depictions of non-elite persons in monumental art demonstrates the social significance attributed to these objects; in the context of elite self-presentation, ritually charged objects were socially exclusive and their depiction submitted to rules of decorum. Only the tomb owner was depicted wearing such items, occasionally also family members,

but certainly not servants, whatever those might have chosen to do in real life. In other words, esthetics of decoration and concerns for social control inform the so-called 'scenes of daily life' in the Old Kingdom mastaba memorial chapels, not the practices of daily life.

Robert Ritner's contribution is a comprehensive publication of nine extant snake wands, the first of its kind. It is fair to say that attention to this object category was long overdue. Ritner must, therefore, be thanked and congratulated for providing scholars with a survey of the snake wands that he was able to identify in museums in the US, Europe, and Egypt. The extant wands are either made of bronze or wood and range in date from the Middle Kingdom (*ca* 2010–1640 BC) through the Late Period (664–332 BC). It remains unclear what their precise function was, but there can be no doubt that they were ritual objects. Depictions of snake wands being held by deities and demons suggest that the wands had an apotropaeic meaning. The article happens to be particularly relevant to the more general issue of the relative discrepancy between material culture, formularies, and literary texts in the study of ancient Egyptian magic. One of the wands was discovered in a Middle Kingdom tomb under the memorial temple of Ramesses II (the Ramesseum). It was lying next to a chest that held a number of formularies for protection and healing. Although none of the recipes prescribes using a snake wand of this sort, the wand and handbooks are archaeologically associated and were probably used—and cherished—by one and the same *hekau* or ritual expert.

Joachim F. Quack's survey of unpublished divination handbooks is important for presenting means of divination that were unknown for ancient Egypt heretofore, such as animal omens that give predictions on the basis of bodily contact between client and animal, and methods that are suggestive of numerology, drawing lots, and throwing dice.[3] It is a salient detail again that none of these methods appear in Alan B. Lloyd's survey of the motifs of *heka*, dreams,

[3] In his article Joachim F. Quack refers to an unpublished icosahedron in the museum of Kharga Oasis. This object has now been published in Minas-Nerpal 2007. At the end of his article, he briefly discusses a Demotic divinatory text featuring Isis posing questions organized according to an intricate numerical system (P. Vienna D 12006). The reader should now also consult Martin Stadler's reply [2006,] to Quack's review of Stadler's publication of the text.

and predictions in Egyptian fictional narratives. Lloyd argues that these motifs were mobilized primarily for comic and entertaining effect, in most cases as instruments to expose wrong-doing and thus to achieve justice in the fictional world, which fits the moral and didactic nature of Egyptian literary discourse well. When summarizing his conclusions, he writes,

> Nevertheless, our analysis of stories, when set against parallel data, yields a clear and convincing picture. ... It follows, therefore, that the references to *heka*, dreams, and prophecy in our stories will reflect in some degree the social reality of their use and function, and that our narratives can be expected to give expression, though in many different ways, to moral, political, religious, or nationalistic issues. [88]

This may very well be the case, but should one not give a bit more weight to the fact that the correspondence between the methods and media of divination and *heka* described in fictional narratives and those found in the formularies and archaeological record is, on closer inspection, not obvious at all? Issues of genre, decorum, and the ideological nature of our source materials should not be left out of our analysis—as is so convincingly demonstrated in Baines' contribution.

To conclude my review, this collection of essays contains a number of valuable contributions to the study of *heka* and divination in ancient Egypt. Several articles raise important methodological issues when read in combination, and present materials that remained unpublished heretofore. It is hoped that the book will entice others to join the ever growing group of scholars studying the manifold ways in which ancient Egyptians tried to bend nature to their will.

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*Tablettes mathématiques de Nippur. 1er partie: Reconstitution du cursus scolaire. 2me partie: Édition des tablettes conservées au Musée Archéologique d'Istanbul, avec la collaboration de Veysel Donbaz et de Asuman Dönmez, translittération des textes lexicaux et littéraires par Antoine Cavigneaux* by Christine Proust

Varia Anatolica 18. Paris: Institut Français d'Études Anatoliennes Georges-Dumezil, 2007. Pp. 356 + 49 plates, CD-ROM. ISBN 978–2–906053–92–9. Paper € 75.00

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The decipherment of cuneiform and the recovery of Mesopotamian mathematics, and especially that of the Old Babylonian period, opened an exciting early chapter in the history of mathematics. The pioneering work of Neugebauer, Thureau-Dangin, and others revealed a complex algorithmic and algebraic body of mathematics involving often ingenious constructions and presenting a sustained interest in quadratic mathematics more than 1000 years before Pythagoras. Inevitably, in this reconstruction, the fundamental problem texts documenting the extent and depth of the mathematical knowledge of the period took center stage. A key characteristic of Mesopotamian mathematics is that the sources are principally from an educational context: the tablets are school tablets produced by students and their teachers, and they derive from the business of teaching and learning mathematics. It is only comparatively recently that scholars have turned their attention from establishing the boundaries of Old Babylonian mathematical knowledge to determining how, and how much of, that mathematics was learned by students of the time. Proust's volume fits squarely into this current trend in understanding ancient pedagogy.

As archaeology and historiography have advanced in the past century, the value of an artifact's archaeological context has increased immeasurably. Regrettably, many of the key tablets whose contents provided the original reconstruction of Mesopotamian mathematics


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were purchased in the antiquities market for large university and museum collections during the 19th and early 20th centuries. Their archeological context is irretrievably lost and their origins may not be known to within 100s of miles and 100s of years. Christine Proust, however, is working with a collection of tablets from a well-defined locale (Nippur's scribal quarter) and time (mid-18th century BC), albeit a collection whose contents were considered sufficiently humble that they languished unread for a century after excavation. Between 1888 and 1900, John Peters and Hermann Hilprecht led the University of Pennsylvania's four Babylonian Expeditions to the city of Nippur, a city on the banks of the Euphrates that in the early second millennium had been famous as a religious and cultural capital renowned for its schools. In what became known as the 'scribal quarter', Hilprecht uncovered some 50,000 cuneiform tablets and fragments, of which some 800–900 had mathematical content. Under the rules of the Ottoman Empire, the finds were divided between the excavator and the state, with the result that about half of the mathematical tablets are in Philadelphia and a third are in Istanbul—the rest went to Jena with Hilprecht subsequently. Hilprecht published some 14 mathematical tablets in his reports on the excavations; the rest had to await later generations of scholars. Recently, Eleanor Robson [2001, 2002] has published the tablets from the University of Pennsylvania. Proust now presents over 300 previously unpublished tablets from the collection in Istanbul and uses the opportunity to give a detailed reassessment of the pedagogical production of mathematics in Old Babylonian Nippur.

The approach typically taken by Assyriologists to publishing collections of tablets is to present them in hand-drawn copies along with transliterations (rendering the cuneiform in modern script), translation, commentary, and a few photographic plates of some of the more important or difficult tablets. This format does not always sit well with publication of mathematical material, especially the kind of numerical tablets that form the bulk of Proust's collection. Proust follows the standard format for the lexical and literary tablets; but, for the rest of the mathematical tablets, she provides (very useful) composite tables in the text along with detailed catalogs, comparison of tablet contents, and excellent photographs of all the tablets on a CD that is included with the book. One must applaud this

appropriate use of technology to deliver the maximum possible information into the hands of the reader while saving the author the labor of drawing hundreds of hand copies of metrological and numerical tablets, although I must note that my CD was damaged and only about half of the files were readable. However, it appears that the tablet images have also been made available through the Cuneiform Digital Library Initiative (CDLI) at http://cdli.ucla.edu/.

Apart from the edition of the Istanbul tablets, the bulk of the volume constitutes an overall analysis of mathematics at Nippur. The entire corpus can now be discussed as a whole for the first time since excavation—Proust also had access to the Jena tablets and incorporates them into the general account.[1] She opens with a chapter on sources that sets the scene, explains the location and importance of Nippur, and details the sequences of expeditions and the tablets excavated. Proust also describes the founding, growth, and development of the Istanbul collection and the place of the Nippur tablets within it. Some of Proust's analysis is statistical and she very carefully confronts the problems of selection bias in the sources. Beyond the accidents of archaeology, there also is the question of the choices made by the original scribes and students. In cases of preserved archives, one might expect that the finest, most important, or most necessary would be selected. The Nippur corpus in some ways presents the opposite picture, since most of the school tablets were either destined for recycling or had already been incorporated into walls and floors of later building-phases of the mud-brick houses. (Such recycling can lead to additional difficulties in reconstructing the original text.)

In chapter 2, 'Scribal Schools', Proust takes on the vexed question of where students learned, that is, of the 'schools' themselves, as well as outlining the overall course of study. We will take these points in reverse order. The Old Babylonian curriculum is reasonably well-understood, following the pioneering work of Veldhuis [1997] and Tinney [1998]. An analysis of catch-lines and different topics appearing on the same exercise tablet has helped establish a general chronology. The core of scribal education was learning to read, write, and speak Sumerian, at a time when this was a dead language and structurally

[1] Proust will publish the Jena tablets in a forthcoming volume with M. Krebernik.

unrelated to spoken Akkadian. This, not entirely practical, education fulfilled a desire for arcane knowledge and status among scribes and was largely accomplished by repeated copying of lists, from simple lists of syllables in the early phases to Sumerian proverbs and poems in the later phases. Mathematics was incorporated into this sequence beginning with repetition of metrological quantities and proceeding to arithmetic lists and culminating in computation. A central issue in the reconstruction of the Old Babylonian curriculum is determining what texts and lists were studied and in which order. There have been several attempts at such a reconstruction, most notably by Veldhuis [1997] and Robson [2001, 2002]. It is clear that there were certain common core texts and many other optional ones, and that the selection varied from place to place. Proust's detailed analysis of the Nippur corpus leads her to make the important and very plausible suggestion that, while there is an overall development from simple to complex texts, within each grouping students may have studied several different lists at one time, and that the search for a linear ordering is misguided.

On the question of the Old Babylonian school itself, the evidence is much less clear. Proust foregrounds the copying of Sumerian poems praising the scribal arts and extolling the importance of scribes, and argues that such a concern for generating an *esprit de corps* is better suited to an institutional framework than through family apprenticeship. On the other hand, the archaeology suggests that the schools were small, with perhaps one to five pupils, thus implying a more family-oriented approach. Andrew George [2005] has recently argued that the scribal 'school-days' literature may look back to a time of larger institutions in the preceding Ur III period.

In chapter 3, Proust turns to (abstract) numbers and metrological units. Old Babylonian metrology was a mixture of ancient systems, such as that for capacity,[2] and newer ones, such as those for weights and volumes. Many metrological units and relationships between them had been altered in the Sargonic and Ur III reforms of the later third millennium in order to create interconnecting systems that

[2] Capacity units measure bulk foods and liquids, as opposed to volume measurements related to length units. Capacity relates to containers of various sizes, as we use cups, gallons and barrels, rather than cubic inches.

allowed easier computation, a bureaucratic convenience. Proust carefully explains the different sets of units for lengths, areas, volumes, capacities, and weights as well as the various notations for multiples and standard fractions of the basic units. Her exposition, together with the tables in the appendix and the detailed treatment of metrological tables in the later chapters is probably the clearest and most comprehensive survey of Old Babylonian metrology yet written.

It is a truism among historians of Mesopotamian mathematics that the abstract sexagesimal place-value number system for which Old Babylonian mathematics is so well known is an artificial construct intended solely for calculation. As a floating-point system, it is hard to use for addition, although good for multiplication. Proust describes the basic notation of the standard system and reiterates her argument [2000] that part of the calculations of mathematical problems took place 'off-tablet'. The additional evidence from the Nippur tablets strengthens her argument. In describing the artificial nature of the sexagesimal system, Proust argues that instead of numbers as such, they can be interpreted more profitably as numerical 'strings', given that multiplication tables are ordered lexicographically by leftmost symbol.

In chapter 4, 'Description of the Tablets', Proust turns to the material culture of her topic, a theme of growing importance. The great mathematical text editions of the early and mid-20th century from Neugebauer to Bruins and Rutten emphasized texts in their titles and treatments: *Mathematische Keilschrifttexte* [Neugebauer 1935–1937], *Textes mathématiques babyloniens* [Thureau-Dangin 1938], *Mathematical Cuneiform Texts* [Neugebauer and Sachs 1945], *Textes mathématiques de Suse* [Bruins and Rutten 1961]. Proust has chosen to differentiate herself from her predecessors by her choice of title: *Tablettes mathématiques de Nippur*. While Neugebauer, especially, was careful to record the physical details of tablets, and introduced elements of a typology for table texts, commentary and analysis were principally text-based. In recent decades, a more detailed typological framework for analyzing scholastic tablets has been developed. Proust takes this standard analytical schema and uses it to extract considerable organizational information on a number of levels. Physically, the tablets are divided into six types. Proust shows how these types correlate with their content as well as how some tablets of a certain type (but not al) never mix categories of content. For example, the multi-column

tablets with long extracts from metrological or mathematical lists or tables are always unified, whereas the daily tablets that feature copying on the obverse and rehearsal of previously learned material on the reverse often do mix categories of content. Proust also notes that the so-called Type II tablets, which are very common at Nippur, are absent from the approximately contemporary corpus from Ur, although whether this is due to difference in pedagogy or accidents of survival and excavation one unfortunately cannot say. In this chapter, Proust also adumbrates the main division of her sources into metrological lists, metrological tables, and numerical tables, a division given detailed treatment in the following chapters. She observes that while these texts do follow clear organizational rules, one must be cautious in generalizing from a given collection as the rules vary from place to place.

Chapters 5 and 6, extending over 120 pages, present Proust's detailed reconstruction of mathematical education at Nippur. Chapter 5, which deals with elementary education, accounts for 90% of the Babylonian Expedition tablets. Mathematics in Old Babylonian Mesopotamia was still very concrete, concerned with computations involving physical objects expressed in series of everyday units. Thus, a pupil's first exposure to mathematics came in the form of memorizing long metrological series. Proust makes a distinction between metrological lists and metrological tables. Metrological lists give the sequence of quantities in a given metrological domain and provide practice in writing; metrological tables have the same list of quantities, but also convert them into sexagesimal multiples and fractions of a base unit, thereby training the student in writing the sexagesimal figures and in computation. Each series proceeds in increasing size from the smallest quantity up to some large unit; and the series were learned in the order of capacity, weight, area, and length. The first three apparently went by the names of grain, silver, and field, reflecting their origins; the length list does not seem to have had a name. Together, the four complete sequences run to some 620 entries, although individual tablets present extracts in a variety of sizes. The majority of exemplars are capacity lists and tables; lists and tables are never found intermingled on a single tablet. The base unit of length is the *nindan*, while the base unit of height is the *kuš* ($12\,kuš = 1\,nindan$). A few of the tables convert metrological lengths

to sexagesimal heights and constitute the last metrological tables in the sequence to be studied.

After learning the written metrological notation and having practiced the conversion of metrological quantities into sexagesimals, the pupil's next exposure to mathematics came in the form of purely numerical tables. The Nippur collection includes reciprocal (or inverse) tables, many multiplication tables, tables of squares, and tables of square and cube roots. In addition to providing much evidence for the standard view that multiplication tables were learned in descending order of principal number, Proust makes some other astute observations. The first concerns the tables of inverses. In Old Babylonian mathematics, division is achieved by 'multiplication by the reciprocal', or, more accurately given the floating point-nature of the sexagesimal system, 'multiplication by the inverse' as Proust prefers to call it. The Nippur inverse tables contain two entries at the beginning: 'of 1, its $\frac{2}{3}$ is 40; its half is 30'. Proust identifies these lines as a two-line table of fractions which she distinguishes from the remaining sets of inverses. Proust notes that this small table is an Old Babylonian innovation which does not appear in the Ur III examples.

Proust gives a very good analysis of the similarities and differences between the Nippur numerical tables, especially the multiplication tables, and those from other locations, thus illustrating the variability of the relatively standardized sources across Mesopotamia. She also argues that the tables of squares belongs with the multiplications as the last in the series. However, she sees the tables of roots not as giving the inverses of tables of squares, but as representing a new mathematical operation and tablet series that was sometimes introduced at the end of a student's study of mathematical tables.

The overall structure of the elementary level of mathematical education as reconstructed by Proust begins with extended practice in writing capacity lists, followed by shorter periods working on the remaining lists. Similarly, the tables begin with an extended period of capacities followed by briefer periods of the other metrological tables. Numerical tables begin with a short period on inverses, followed by a long time working through the multiplication tables and concluding with brief exposure to squares and roots. However, the interval spent working with capacity tables appears to overlap with later phases of metrological lists, and numerical tables make their appearance only slightly later than capacity tables. Proust discusses a

number of possible interpretations of this juxtaposition, but retains a clear sense of the basic difference between lists and tables, suggesting that perhaps different students received different training. Certainly, we should be cautious in ascribing too much homogeneity to scribal education, even education involving few students; and the fact that tables and lists never occur together is very striking. This is an important point of Proust's and deserves to be followed up in the study of other collections.

After the wealth of sources described in the chapter on the early phases of mathematical education, Proust has only some 40 tablets as witnesses to the more advanced stages. Most of these contain calculations of multiples or inverses. Proust describes the different ways of organizing multiplication, the numerical examples and applications which require finding the areas of squares and other quadrilaterals, as well as, on just three tablets, computing volumes. Within this context, Proust well illustrates her thesis of the disjunction between metrology and abstract computation. Among the assemblage is a group of tablets with exercises in computing areas of squares. The statement of the problem and its solution are written in metrological units in sentences in the lower right-hand corner of each of these tablets, while the multiplication involved is written in abstract numbers in the upper-left corner. This is as clear support as one could wish. Most of these area-computation tablets have been published previously and an image of the unpublished example adorns the cover of the volume.

Accompanying the exercises in multiplication is the problem of determining inverses of numbers not in the standard table. Proust notes that all such computations in the Nippur corpus use pairs derived from sequences of doubling and halving of one standard pair. The procedure used to pass from a number to its inverse has been described previously [see, e.g., Sachs 1947]. Proust relates the procedure and adds a very good section on how the physical layout of the numbers on the tablet acts as an aid to computation. Given the power of the method and its practical restriction to one sequence of numbers, Proust sees a tension between original creativity and a conservative pedagogy. As the sequences of doublings progresses, one soon achieves many-place numbers and these often have errors. Proust sees a pattern in these errors indicating that long numbers were divided at the fifth sexagesimal place, which she takes to imply

a limitation on the size of the abacus (or similar 'off-tablet' computational device) used. Given the absence of direct archaeological evidence, I find her inference suggestive but not conclusive.

With respect to the few volume calculations, Proust notes that Old Babylonian metrology presents three different types of 'volume' units—area × height, piles of bricks, and capacity measures—and that abstract volumes occur only in mathematical texts.

Out of the 800 mathematical texts under discussion, only three are problem texts with sequences of solved problems. All three have been published before: one by Hilprecht, the other two by Robson. Proust and Robson have found another fragment that joins one of the other tablets; that fragment is published here for the first time. All three texts contain problems concerning the calculation of volumes or of parameters derived from volumes. Proust re-publishes the tablets in full, discussing previous commentary and noting where her interpretation of these difficult and broken texts diverges from others. Additionally, she stresses the flow of conversion and computation, showing which tables support each step.

After this extensive and detailed survey of the Nippur corpus, Proust summarizes her results with commendable caution. One of her key findings is that education clearly varied from place to place and from pupil to pupil. Naturally, such variability makes generalization problematic. A few children trained as scribes. Students learned how to read and write metrological notation, with the curriculum dominated by the capacity series used (among other things) to measure grain; they learned by practicing with carefully structured lists. Some students learned how to reckon in the abstract sexagesimal system; they learned from structured tables, both metrological and numerical. Some students applied that knowledge in calculating areas or inverses. Abstract numbers were principally for multiplication and finding inverses. There is not much evidence for mathematical problem-solving beyond the computations of areas, for reasons that are unclear.

How far can these results be generalized? While details vary from location to location, the key themes of writing, structured lists, and tables, and the restriction of the abstract numeration system to computation are universal across Old Babylonian Mesopotamia. The final publication of the Istanbul tablets after a century of neglect is a

noteworthy event. By going beyond mere publication of the tablets and providing a synoptic view of mathematical education, Christine Proust has produced an invaluable volume. Her clear and carefully detailed exposition, her concern for both text and tablet, and her extensive statistical analysis (summarized in the text but presented fully in appendices) show that she has mastered modern historiographic techniques. The result is up-to-date and comprehensive.

The fortuitous use of clay as a writing medium means that Mesopotamian scribes have left a legacy unique among ancient cultures whereby historians can reconstruct in detail both the content of education and its pedagogy. There is much in this book for experts; but there is also a great deal for readers from outside the field who have an interest in education, pedagogy, and schooling.

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*A Culture of Improvement: Technology and the Western Millennium* by Robert Friedel

Cambridge, MA/London: MIT Press, 2007. Pp. x + 588. ISBN 07–0–262–06262–6. Cloth $39.95, £24.95

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Robert Friedel's *A Culture of Improvement* is a notable achievement, an engaging, well written, well-grounded distillation and synthesis of academic research in the history of Western technology. Informatively illustrated with new drawings, paintings, and photographs, it should replace widely used earlier texts on the history of technology, such as those by George Basalla, D. S. L. Cardwell, James McClellan, Lewis Mumford, and Arnold Pacey.

His narrative relies on a commitment to the social construction of technology, perhaps most saliently expressed in his chapter 'Networks', where he notes that

> the particular ways in which these new capabilities [media technologies] were developed and exploited were not determined by science at all, but by the complex combination of personal ambitions, social currents, markets, and politics that lay at the foundation of all important technological change. [516]

Friedel's book has two major theses. The first is that over the past 1000 years, the West has developed a 'culture of improvement', which places great value on technological improvement and its sustainability. The second, perhaps more debatable in terms of quantitative measures, is that the rate of technological change has accelerated in the past 500 years because of new, more effective means of making improvement part of a sustained series of changes.

If there is one sure bet that one could make about the American character, it is belief in Progress. My students almost universally use the word in talking about the role of science and technology in society.


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Their view of the recent past is one of an accelerating technological bounty, an explosion unparalleled, transformative of material and social life for the better. We live in the best of all possible worlds, and it's only getting better! Woe betide those who would reign in unfettered creativity and invention.

The principal value of Friedel's account is that he avoids the problematic and debatable idea of 'Progress', and the determination of what desirable goal or goals humanity is progressing toward. Instead he prefers 'improvement', which includes both the gradual evolution of a technology and the unexpected revolutionary transformation of a technology. Improvement tends thus to be localized and particular, although attitudes towards improvement may be diffuse and general. Indeed, Friedel takes care to set technological change in social, economic, and technical context, and seems taken with what he calls 'attitude(s)' toward technical change and innovation.

The focus on improvement has another virtue: it can help explain the contingent and contextual character of technological change, as well as how and why some technological changes catch on while others do not. The 'Culture of Improvement' means simply that 'things could be done better', and what constitutes improvement depends on differing individual and group perspectives. Hence, improvement is both contingent and specific in terms of goals and the times. Not all societies exhibit a belief in or commitment to the value of improvement; and, where such beliefs or commitments exist, they may be limited to particular technologies, not a general feature either of practitioners or society in general.

Such a focus helps make sense of the long periods in which little change in technology seems to have been characteristic. For example, the invention of the water wheel sometime in the first century BC led to its use for grinding grain. Except for a few isolated and temporary cases of other applications, for the next 1000 years or so the water wheel's potential as a general power source for a variety of applications remained unappreciated and unexploited. Not until almost the turn of the first millennium AD was the water wheel applied to a variety of other tasks, such powering bellows and trip hammers in iron forging, felting cloth, sawing wood, crushing olives, tanning bark, powering borers, rolling metals flat, stamping coins, cutting stone, operating pumps, and so forth.

A similar example is illustrated by astronomical instruments, which from their description by Claudius Ptolemy in his *Almagest* (*ca* AD 150) remained essentially unchanged until the Middle Ages, despite shortcomings and opportunities for improvement obvious to the modern eye (and temperament). About 35 years ago, I needed to have models of Ptolemy's instruments made for a lecture; and for several weeks, every day or two, the person in charge of making them, an experimental physicist, would come to suggest modifications to make them 'better'. His attitude toward improvement was irrepressible and totally incongruent with that of those past astronomers who were satisfied with their instruments for more than a 1000 years.

Friedel undertakes to explain how improvement gradually came, from scattered appearances in the European High Middle Ages (1000–1300), sometimes called the 'First Industrial Revolution', to be widely acknowledged, expected, and of great social value in the 18th and early 19th century West. After the middle of the 19th century, widespread technological change became part of Western social expectations, and contributed to the rise of a belief in unending Progress.

It would appear that something significant, perhaps a 'turning point', with regard to establishing a 'culture of improvement' took place in the 16th and 17th centuries. It takes Friedel five chapters to deal with technological change between 1000 and 1500, with relatively little material from the 1400s. Even though they were presumably without *A Culture of Improvement* by 1500, Islam, India, and China were roughly comparable to the West in terms of the level of science and technology. Then, the West began to outstrip the rest of the world in widespread technological change, but just why it did so remains unclear. Friedel characterizes the period from 1500–1700 as one of 'enormous changes in the status and meaning of technology' [154]. On his account, it would appear that the period was a turning point in the diffusion, acceptance, and expectation of technological improvement. The consequences take up a majority of the book, nearly half of which concerns the technological changes of the Industrial Revolution (1700–1900).

The book's chapters, roughly chronologically ordered, but topical within, consist of thematically related developments in science and technology, together with supporting and interwoven social and historical context. Friedel occasionally reserves dealing with an individual or topic until a later chapter, so that what at first might

seem like an omission later turns out to be treated in a different context. For example, in dealing with the rise of civil engineering in the 17th and 18th centuries, and in comparing France with Britain, he devotes several paragraphs to prominent French engineers, but in turning to Britain, does not mention John Smeaton, sometimes termed 'the father of civil engineering'. Smeaton, however, turns up in a later chapter on steam engines. Another example of this concerns the topic of mining and miners, important and valued technology and labor in the medieval period, a topic largely absent until the 16th century publication of *De re metallica* by Agricola.

In order to cover the significant technological, socio-economic, and cultural changes of 1,000 years in 25 chapters, Friedel has by necessity to be selective. Yet his coverage is masterfully comprehensive, with but a few minor lacunae. For example, in discussing Harrison's invention of the marine chronometer, there is no mention of the parallel, less empirical and more theoretical French approach and success. Also absent is another French invention which often figures in weaving and information technology, the Jacquard loom. And there's no mention of one of the key responses to the critiques in the 1970s of technology, the Office of Technology Assessment (1972–1995), which served Congress well until the Office's demise, and served as a model for other countries to emulate.

Friedel considers the costs as well as the benefits of 'improvements', noting that there are both winners and losers as technology changes. One of the greatest costs was the gradual exclusion of women from technology, especially in the 16th and 17th centuries, from traditional crafts, food, and textiles. As the economic importance of cloth-making increased, women's dominance of the trade faded. Women were increasingly seen as outside the domain of technical improvement. If women's participation in the sciences in the late 20th century has in many fields approached parity with men, computer science and engineering still remain overwhelmingly male dominated domains.

There were other costs as well. If in 1700 'improvements were to expand the opportunities for work, not reduce overall labor', by 1800, productive employment 'no longer seemed necessary' as improvement's focus changed from moral and spiritual uplift to individual and

group self-improvement. The costs of the factory system in child labor, disease, pollution, and disemployment were immeasurable, and are still being levied.

In dealing with the military's role in technological change, Friedel notes that 'The improvement of violence became a large-scale enterprise itself', and labels World War I a 'shock to the culture of improvement and moral uplift', with its lesson that technology has no limits. The greatest shock, however, came with the development of eugenics, whose negative form led to 'moral disaster'.

Nonetheless the allure of improvement maintained such power that in the mid-20th century 'the improvement of technologies of all kinds appeared to be an imperative.' Even as the critical ferment of the 1970s over the environment, racism, the Vietnam War, women's rights, and technology left a continuing legacy, the culture of improvement continues dominant to this day, with the rise of networks of power and information, genetic engineering, globalization, and the decline of the local.

At the end of his book, Friedel deals explicitly with the problem of extrapolating from a widespread belief in improvement to its consequent creation of a common underlying culture supporting that belief (sometimes termed technological convergence):

> The culture of improvement... is now a worldwide set of beliefs and expectations. The belief in technological improvement, however, does not and cannot extend to shared beliefs about culture, values, or the best way for humans to live. The dramatic power of technology and the powerful promise of its unending improvement have led to the misperception of more widely shared values in the world. This has, in turn, led to serious errors of judgment, policy, and understanding. [540]

Debates over technological change will continue, because fundamentally they are about who we are, and how we should live. Without a sense of history to clarify and enlighten those debates, we are only too likely to succumb to Langdon Winner's 'technological somnambulism'. For the concerned college student and educated layperson, Robert Friedel's book is an unusually rich and informative historical resource.

*Harmonious Triads: Physicists, Musicians, and Instrument Makers in Nineteenth-Century Germany* by Myles W. Jackson

Cambridge, MA/London: MIT Press, 2006. Pp. x + 395. ISBN 978–40–262–10116–5. Cloth $40.00, £29.95

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*Harmonious Triads* is a scattered book by Myles Jackson about musical practice, physical acoustics, instrument-making, musical pedagogy, and the social and political role of music in the German states and then the unified Germany of the 19th century. One of the themes in a number of its chapters is the impact that natural scientists interested in physical acoustics had on music-making in private homes, churches, and concert halls. Thus, two figures of interest to Jackson are E. F. F. Chladni, the experimental scientist who made a study of the acoustical properties of bells, bowls, and other vessels, and Wilhelm Weber, the more mathematically inclined physicist who, by building on Chladni's study of longitudinal vibrations, was able to work out the physics of reed pipes. At the same time, Jackson wants to call our attention to the parallel contributions made by artisan instrument makers—many unknown to us today—and even industrialists with or without formal technical scientific expertise. Readers thereby discover a group of very different people responding in very different ways to, and even challenging, German ideals of music at this time.

For example, we discover that some straightforwardly scientific results were used to promote the design of new musical instruments and the improvement of already existing ones. Thus, Chladni applied his research to the design of a new instrument that he called the 'euphone', a set of 40 glass rods of different length fixed at their nodal points to a sounding board. A musician could produce notes of different pitch by rubbing different rods with a moistened finger. The tones emitted by this instrument were not unlike that of another new instrument of the time, the glass harmonica; and it apparently


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answered a call heard from different quarters for a new quality of tone in musical expression—one that captured a sought-after spirituality or other-worldliness. On the other hand, Weber's results were explicitly intended to improve the construction and performance of organs. He hoped to fix a metal tongue in an organ pipe so as to play off the transversal vibrations of the former and the longitudinal vibrations of the latter in such a way that the pitch emitted would remain constant even as the volume increased or decreased depending on air flow. The rationale here was that, in the real world, transversally vibrating bodies tend to emit tones slightly higher in pitch as the amplitude of their vibrations diminish, whereas the opposite is true, again in the real world, for bodies that vibrate longitudinally. By engineering a body that produces both kinds of vibrations at once, but in such a way as to use these contrary acoustical tendencies against each other, Weber aimed to help instrument makers design organs capable of crescendos and diminuendos. To that extent, he apparently took himself to be answering a call for a more musically sensitive, expressive, perhaps even spiritual, style of play in contemporary German church music.

Chladni and Weber are interesting because their work was motivated, at least in part, by a desire to use science to meet the perceived needs of music making. But the history of instrument making as craft rather than science as such also provides examples of challenges to certain musical ideals of the time. Thus, Jackson documents a host of 'mechanicians'—some trained as clock-makers, some as organ builders—who produced automated music-making devices: for example, a mechanical trumpet player.[1] We also have a mechanical device that was supposed to imitate a whole orchestra.

These new instruments seem to have multiplied and outdone one another in ambition and weirdness of name: thus, we have a symphonium, a bellonion and even something called the 'chordaulodion'. I am not sure what the motivation for these contraptions could have been at the end of the day, beyond showing that it could be done, with the hope, presumably, of showing that it could be done better even than Jacques de Vaucanson had in the 18th century with his

[1] I was very happy to find some photos of this device which reveal the inner mechanism, though I have to admit that it is still not very clear to me how this thing was supposed to work and what it may have sounded like.

mechanical flute-player, his pipe and drum players, and—of course—the mechanical duck that could allegedly eat, digest and pass duck poop. Vaucanson himself must have been trying to show that he could outdo Hero of Alexandria in the design and construction of automata. If that is right, then the musical automata of the 19th century should probably be understood as an effort to continue and best a tradition of practical mechanics going back to Alexandria of the third century AD. If that in turn is right, I would guess that musical aesthetics as such mattered to these mechanicians not for their own sake, but as a way to set benchmarks for success: you could claim to have bested Vaucanson (and *a fortiori* Hero), if your trumpet-player was more convincing to the ear than his flute-player. In spite of the curiosity that these things sometimes provoked in real musicians (like Beethoven, for example, who wrote 'Wellington's Victory' for an automaton built by Mälzel), it is surprising to learn that they played any role at all in debates about musical aesthetics. At the end of the day, one would expect people to have thought that, however clever some of them might be, they were all pretty gimmicky: I half expect to learn in some future historical study that the merry-go-round has its origin in this technology. But, as Jackson points out, musical automata were an object of interest and even concern to at least one of the German Romantics, namely, E. T. A. Hoffmann, one time music director of the opera in Bamberg and contributor to the *Allgemeine musikalische Zeitung*.

As Jackson notes, Hoffmann—or, rather, some of his characters—are critical of musical automata like those of Vaucanson because they doubt that such devices can ever truly express what is going on in the music: at best they can emit tones in the right order, in the right rhythm, and at the right tempo. Hoffmann's characters rightly doubt whether that is all there is to musical performance. This is at least part of what Hoffmann has Ludwig and Ferdinand say in the short story called '*Die Automaten*'. This is precisely the sort of criticism I think that one would expect: it is the musician's own twist on the dismissive remark that musical automata are just gimmicks. But this is only part of the criticism voiced by Hoffmann's two characters. In a passage that Jackson himself quotes, but does not fully comment on, Ludwig is made to say that the effort to build musical automata is something '*drückendes, unheimliches, ja entsetzliches*'. He goes on to describe it as '*heilloses und gräuliches*'. These are very strong words

with connotations of horror. They might have been intended to express hyperbolic horror at the sound of mechanical music offensive to heightened Romantic sensibility, in something like the presumably mock horror which a teacher of mine at the University of Chicago once (perhaps twice) expressed at the thought that the Philosophy Department would make an offer to a person whom he suspected of high charlatanry: 'I'll die if he comes!', he exclaimed with great passion. But context suggests that Hoffmann's Ludwig means what he says quite literally. This needs explaining.

I do not myself know quite how to give a fully satisfying account of the gimmicky, musical or otherwise, but my sense is that it is usually incompatible with the horrifying. Jackson's explanation, that Hoffmann is out to defend the 'organic' over the 'mechanical' in music, does not seem adequate to me. All things considered, I much prefer organic ducks to mechanical ducks, but the thought of Vaucanson's mechanically digesting duck doesn't alarm me at all: it tickles me. I would have thought that Hoffmann's Ludwig would be tickled too at the the sight and sound of a mechanical flute-player. Instead, he and his friend Ferdinand react as if they had been forced into the presence of the Unholy Undead, something that, by falsely simulating life, is a threat to life itself. Perhaps Jackson might say that the threat is real, namely, that the musical automata will be so perfected as to take over the concert halls and banish living, breathing musical performance as Ludwig knows it. But such an account flies in the face of what the two friends actually say: they agree that even the most pathetic human musician will play more soulfully than any mechanism. This may well have been Hoffmann's own view. There is no reason to think that he would have been at all impressed by the highly favorable review cited by Jackson of the Kaufmanns' presentation of their apparently quite sophisticated musical automata to the British Royals at Buckingham Palace in 1851. The reviewer speculates that such automata might even surpass human organists. Hoffmann would never have taken such a claim seriously: every musical machine, however technically impressive, will always lack something crucial to musical practice, namely, spontaneity, i.e., an ability to sense on the spot what the moment requires and to adjust as best one can. In '*Der Sandmann*', the student Nathaniel falls madly in love with Olimpia, the presumed daughter of his professor, who dances and plays the piano with great technical proficiency. Everyone can tell she is not quite all there:

there is something studied and clockwork in everything she does at the keyboard and on the dance floor. The biggest give-away clue, of course, is that she replies, '*Ach!*' to everything you say. Only Nathaniel cannot see that something is amiss, but that is because his 'vision', i.e., his power of discernment, has somehow been taken from him by the '*Sandmann*'. He is sick, but every sane person knows the truth instinctively long before it comes to light, namely, that Olimpia is just a clever wind-up doll. That is all that any musical automata can ever be, on Hoffmann's view. Whence the puzzle: why fear such gimmicks, as one would a vampire?

I am sure that there is a whole book's worth in the topics that I have mentioned. Moreover, Jackson is surely right to think of using Hoffmann for his distinct perspective on them (and probably wrong to think that Goethe and Hegel are all that relevant however supportive they may have been of the 'organic' over the 'mechanical') because Hoffmann had an investment, both personal and professional, in musical practice. He does not just cause his characters in '*Die Automaten*' to hyperventilate about musical automata; he has them say enough to suggest that he was aware, and likely approved of, Chladni's euphone and other such innovative musical instruments. Moreover, though he died in 1822, shortly before Wilhelm Weber started his work on the physics of reed pipes, he has his characters explicitly call for the application of 'true mechanics', as opposed to the false mechanics practised by Vaucanson and his later German counterparts, to the improvement of musical instruments played by human musicians. (That he would have approved of efforts like that of Weber to improve the expressive play of the organ seems likely, especially in view of the fact that one of the central characters in his masterpiece, *Lebensansichten des Katers Murr*, Master Abraham, is a highly accomplished organ builder.)

But what Jackson gives us is both more and less than the book that I would have liked to read. Less, not just because he does not solve the genuine puzzle about Hoffmann on musical automata. The problem is first of all that the story, as he tells it, raises a host of questions. That is sometimes the mark of a successful book. But if the questions are numerous and natural enough, they raise the suspicion that the book has not yet been completed. One characteristic example will have to suffice.

I still do not understand, even after re-reading the relevant parts of the book, what impact Wilhelm Weber's scientific work on reed pipes had on organ design and construction. On the one hand, Jackson invites us to think that it had a considerable impact just by virtue of the favorable notice it received from Johann Gottlob Töpfer who was a professor of music at the *Schullehrerseminar* in Weimar, an organist and also, it seems, a prominent organ builder. This Töpfer, who comes up twice in the book and probably deserved more focused attention from Jackson gives him, published a number of influential things on organ-building from roughly the 1830s to the 1850s, one of which seems to have been an important textbook. Töpfer discusses Weber in these works, arguing that his research was essential to the construction of fine organs with reed pipes. Given Töpfer's stature as an organ builder and the influence of his textbook, it would seem that Weber's research must have established itself as foundational for organ technology in the German states of the early to mid-19th century. Indeed, that is the conclusion Jackson apparently wants us to draw; but a sentence later he takes it all back. Thus, he writes:

> As Töpfer's texts were the ones most often consulted by organ builders during the mid-19th century, Weber's equation became a part of the organ-building process. Indeed, his acoustical research, though itself without lasting practical influence, signaled a collaboration among German physicists, musicians, and musical-instrument makers that was to last until at least the end of the nineteenth-century. [137]

I am baffled by these two sentences: did Weber, or did he not, have an impact on organ building? (He would remain interesting even if he had had none.) I expected the subsequent paragraphs to clarify this, but they emphasize only that organ design and construction in the German states seem to have lagged behind that of England and France. I then hoped to find clarification elsewhere in the book. Instead, I found myself faced with more unanswered questions.

For example, I learned that the same Friedrich Kaufmann who so impressed Queen Victoria and Prince Albert with his musical automata at Buckingham Palace had devised his own techniques for reed-pipe compensation that allowed for crescendos and diminuendos without changes of pitch—about 10 years before Weber announced his first findings. Together with his father, Kaufmann was involved

in organ-building. Did he not consider using the techniques that he had developed for his automata in the development of more expressive organs—greater expressivity being, as we learn from Jackson, a recognized desideratum among German organ-builders in the 19th-century? Was Weber aware of Kaufmann's innovations? If not, then how far-reaching and fruitful was the collaboration among 'German physicists, musicians and musical-instrument makers' of which Jackson makes such a big issue in the passage that I just quoted and in the book as a whole? Jackson is surely right to make the history of organ building an important part of his story. But that he raises these and many other such questions, apparently without even realizing it, is a sign that he has written less than the book I would have liked to read.

But he has also written more than the book I would have liked to read. He tries not only to cover the topics that I have mentioned, but a host of others as well, e.g., the drive to standardize tuning in orchestras throughout Europe, the invention and marketing of the metronome, and the attempt to use mechanical aides in the instruction of piano technique. At the same time, he also wants to bring cultural history into the mix. This concern leads him to take an interest in the way that German natural scientists and doctors socialized. That they made music, more specifically organized choral concerts, as part of their professional meetings takes on a great significance for Jackson. He claims that this activity was fundamental for defining their identity, both professional and national, and that this fact in turn is somehow illuminating. The problem is that it is very hard to tell what precisely has been illuminated after this fact has been called to one's attention.

At a minimum, Jackson wants to say that it facilitated personal and professional networking among these people. But even that claim seems overstated to me. One of the cases that is supposed to be illustrative of the claim and its significance is that of the relationship between the aforementioned Wilhelm Weber and Carl Friedrich Gauß. They met at the 1828 *Versammlung deutscher Naturforscher und Aertzte* where Weber presented his research on the physics of reed pipes. Gauß attended the lecture and was sufficiently impressed by it and Weber personally to recommend him for the physics chair that became vacant in Göttingen in 1830. There is no doubt that the *Versammlung* as a professional institution facilitated the contact

between these two people, as such meetings are wont to do. But that there was a choral concert at this meeting and that the musical activities at other such meetings were explicitly intended by the organizers to create national and professional bonds among the participants sheds no light on this relationship or—I'll wager—on any other like it. Let me put this in perspective.

In the late summer of 1995, I attended a conference on German Idealism held at Dartmouth College. A fairly elaborate banquet was organized for the last evening, to be followed by dancing. One of the organizers told me that she liked the idea of dancing because it was a way for American and German scholars to 'get down together'. The music was insufferably loud and the forced hilarity oppressive: I fled. Suppose, for the sake of argument, that Gauß was as unreceptive to choral concerts as I was to dancing. Then the *Versammlung* of 1828 can be understood to have facilitated his relationship with Weber just to the extent that it brought the two together professionally: music would have played as much of a role in it as dancing has played in my professional development. But now suppose that Gauß loved choral concerts. What follows? Nothing, so far as I can see. The thing that clearly mattered above all to Gauß, as Jackson himself points out, was whether Weber had the 'right stuff' for Göttingen. Given that he did, it was apparently a bonus that he struck Gauß as *liebenswürdig*. But, again, Gauß could have formed this judgement with or without choral concerts or indeed any efforts to forge some kind of national and professional identity of German doctors and scientists. That choral concerts and *Liedertafeln* were organized for professional meetings of such people is—to my mind—nothing more than a mild curiosity, about as interesting as the fact that dancing sometimes goes on at academic conferences in North America.

In short, there is certainly at least one book's worth in *Harmonious Triads*; but the book, as it stands, badly lacks focus. It covers a vast range of topics without doing enough to synthesize them or show how they fit together. Now it might be said that this is a virtue—indeed, one that comes from the just recognition that historical reality is very messy. But when a book proves to be just as messy as the thing it covers, a reader can be forgiven for feeling frustrated. Historical studies must never falsely simplify reality, but it is not unreasonable for us to expect them to shed light on it. If we cannot expect that, I am not sure why we should be expected to read them.

*History of Science, History of Text* edited by Karine Chemla

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This volume is the publication of a selection of the papers that were presented at a workshop in Berlin in 1995. It explores the relationship between scientific practices and their manifestation in the form of scientific writings. The 10 articles, organized in four sections (plus an introduction and epilogue), cover an unusually broad variety of cultures (Mesopotamia, China, Europe, India) and times (from 1700 BC up to the 19th century AD), and the diversity of topics is just as wide.

Karine Chemla provides the introduction to this volume [vii–xxvii], and she makes clear that it aims to tackle the issues from a global perspective and to abandon as invalid the separation of the content of a text from its physical manifestation or material culture. Each of the respective articles sheds light on one or more specific aspects of the interrelation between the creation, use, and understanding of scientific texts. This collection, therefore, presents a multitude of impressions for a 'professional reader' of any kind of scientific text.

Part 1, 'What is a Text?' consists of one article only: 'Spatial Organization of Ancient Chinese Texts (Preliminary Remarks)' by Vera Dorofeeva-Lichtmann. This article is well chosen to begin this collection, as it questions one fundamental aspect that most people implicitly associate with the notion of a 'text', that is, a linear structure. The examples presented by Dorofeeva-Lichtmann of ancient Chinese texts are those where additional meaning is conveyed through a non-linear structure, i.e., by the positioning of elements of the text. The spatial arrangements can be linked to points of the compass. The evidence from China enables a scholar furthermore to trace the development from the non-linear layout found on bamboo slips, through silk manuscripts serving as a 'blue-print' of the non-linear layout [13: see also Figure 6a on p. 19], to the linear layout found in block-printed


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books. The use of some non-linear texts required the moving (turning) of the text or of its user around it, and can therefore be linked to astronomical instruments such as the *shi* cosmograph (an astronomical instrument used to track the movement of heavenly bodies). The non-linear arrangement of bamboo slips would only have been displayed when the text was in use by a specialized user, and so it served as a way of safeguarding what was considered secret knowledge.

Part 2, 'The Constitution of Scientific Texts: From Draft to *Opera Omnia*', includes three articles: 'Leibniz and the Use of Manuscripts: Text as Process' (Eberhard Knobloch), '*Opera Omnia*: The Production of Cultural Authority' (Michael Cahn), and 'Writing Works: A Reaction to Michael Cahn's Paper' (Hans-Jörg Rheinberger).

Knobloch opposes 'published texts' and the handwritten output of Leibniz's mathematical work (about 50,000 items) in the light of on a quotation by Leibniz himself, 'Those who know me on the basis of my publications, don't know me' [51], and draws on the latter type of text for his argument. He describes how Leibniz used tables, illustrations, and figures to derive conclusions, visualize his ideas, and revise his work. The article thus looks at the role that 'text' (in this case handwritten notes), tables, and figures may play in the actual work of a scientist (mathematician). The available corpus of Leibniz's mathematical manuscripts is detailed enough to follow the evolution of specific ideas, e.g., the distribution of prime numbers [57–63], as well as the process of editing treatises (some of which he ultimately published), e.g., treatises on the arithmetical squaring of the circle, ellipse, and hyperbola [73], a legal-mathematical memoir on the simple rebate [73–75], and a treatise on life annuities [75].

With Cahn's article, we move to the other end of the spectrum, i.e., to the published text in the form of collected works. As he states initially, *opera omnia* have a rather special standing in that they come first in the catalogues of librarians, but last (if at all) in bibliographical research [82]. As Cahn notices towards the end of his contribution,

> collected works standardize all texts of one author into one single format. They cancel the historical singularity of their original modes of publication, and they cancel the differences between the texts which make it up. They murder any text and make them all look exactly the same, all sterilized to

> the same degree, free from the typographical accidents of history, and divorced from contemporary debates and contexts in which these writings were first produced and later re-used. [92]

What then, is the merit of collected works, and why are they so popular? Cahn's analysis reveals that the underlying motive is, above all, political. While the naive reader may think of collected works as a guarantee for a 'reliable, authoritative text' [83], they are also intrinsically linked to politics, status, and power. Various aspects of these underlying motives are studied throughout the article, pointing to the conclusion that collected works 'can alter texts without altering their words' [92] and that such works are worthy of detailed study.

The contribution by Rheinberger takes up a problem indicated by Cahn, that collected works cannot be *written* but must be *edited*. Rheinberger showcases collected works of scientists from the second half of the 18th to the first half of the 20th century. His first example concerns Pierre Louis Moreau de Maupertuis (1698–1759), who edited his own collected works during his lifetime, producing several editions (1744, 1752, 1753 and 1756) which make plain 'the unsolved dilemma of attempting to assemble complete works before the work has been completed' [96]. The central part of Rheinberger's article focuses on Georges Louis Leclerc de Buffon (1707–1788) and his 36 volumes of *Natural History* (1749–1789), which Rheinberger takes as an example of *writing* collected works. Rheinberger examines not only how Buffon himself approached the project and handled its inherent problems, e.g., the change of views over time, but also the reception of the *Natural History* during Buffon's life and after his death:

> 'Buffon' became a synonym for natural history for a major part of the nineteenth century. Ironically, Lanessan's critical edition... also marks the end of its pervasive cultural influence and the transformation of Buffon's writings into an object of purely historical interest. [100]

Part 3, 'How Scientific and Technical Texts Adhere to Local Cultures' takes the reader back to China ('Text, Representation and Technique in Early Modern China' by Craig Clunas), then France ('The Algebraic Art of Discourse: Algebraic *Dispositio*, Invention and Imitation in Sixteenth-Century France' by Giovanna C. Cifoletti) and

India ('Ancient Sanskrit Mathematics: An Oral Tradition and a Written Literature' by Pierre-Sylvain Filliozat).

The essay by Craig Clunas examines the attitudes towards scientific books of the literate elite of Ming China (15th–16th centuries). It begins with an anecdote in which a father's offer to his favored second son to pass on his skills and knowledge of numerology is met by a flat refusal, which then leads the father to order that all his books on the subject are burned. Clunas examines the situation of the family and the social context of numerology at the time to explain this episode. In doing so, he also raises questions about the use and social connotations of technical texts in Ming China.

Giovanna Cifoletti's contribution on algebraic discourse in 16th-century France studies the relations between developments in 16th-century algebra and its contemporary humanist traditions and practices (most notably linguistics). She uses the example of Jacques Peletier du Mans and his *L'Algèbre*, which was published in 1554 and was 'the first printed book on algebra in French and the richest among vernacular books on algebra' [125]. Cifoletti argues that Peletier reshaped algebra according to a humanist model, resulting in a new kind of text.

The final article in part 3 by Pierre-Sylvain Filliozat looks at ancient Sanskrit mathematics and its evolution from an oral tradition to a written literature. After a brief description of the pandits (learned men) whose knowledge included grammar, exegesis, and logic, he begins with a description of the Vedic civilization (*ca* 1300 BC until the beginning of the common era), which apparently transmitted its body of knowledge orally. In this context, text is understood as 'oral text'—no written form existed. Instead, knowledge was preserved using techniques of oral transmission (recitation, memorization, and conservation) [138]. The masterpiece of this tradition is the genre of the *sutra*, 'the mnemonic form par excellence' [140], which is formally characterized according to Filliozat by the

> use of ellipsis extended beyond the tolerance of natural language, multiplication of technical names to avoid descriptive expressions; abridged lists through mention of only the first and the last items, use of markers, and use of variables. [140–141]

*Sulba-sutras* (*Formularies of the Cord*) occur as a section of ritual literature in the *sutra* form. They deal with geometric problems occurring in a ritual context. Again, these texts originated as oral texts. Filliozat uses the example of the construction of a domestic fire-altar to introduce the reader to the style and content of this genre. Following a description of the emergence and use of writing and its contribution to Indian mathematics, he ends his account with an analysis of Sanskrit mathematical texts [148–156] and the role of orality in Sanskrit mathematics which includes sections about the place value numeral system [150–151] according to Aryabhata.

The final part ('Reading Texts') takes us back to antiquity again with the contributions by Reviel Netz ('The Limits of Text in Greek Mathematics') and Jim Ritter ('Reading Strasbourg 368: A Thrice-Told Tale'). They are followed by Karin Chemla's 'What is the Content of This Book? A Plea for Developing History of Science and History of Text Conjointly'. The first two contributions of this section complement each other beautifully—both deal with reading ancient mathematical texts; but, while Netz focuses on the role of text and its limitations using Greek mathematical examples, Ritter chooses to enhance the reading of an example of a Mesopotamian mathematical text by contrasting it with other related texts.

Netz discusses the role of other 'non-textual' elements of Greek mathematical texts. The examples he focuses on are diagrams and orality. He explicitly confines the sense of 'text' to 'verbal written aspect' [161], thus using a much narrower definition than that used by other authors in this book (most notably by Filliozat in his study of the 'oral texts' of Vedic culture). The very manner in which Greek mathematical texts have survived, i.e., mostly without the diagrams that one is so used to looking at when following a proof in editions by Heiberg or Heath, justifies this approach. In dealing with diagrams, Netz distinguishes between a 'global' reading and a 'local' reading:

> In the global way, we read the mathematical proposition from beginning to end, forming a rough impression of what it is trying to say; from the general context, we know the kinds of problems of interest; we have expectations of mathematical relevance; and through the combination of these we gradually may reconstruct a diagram which fits the text and which makes what we see as the 'correct mathematical sense'. [166]

The local reading, in comparison, follows the text step by step while constructing the diagram at the same time. It is through the process of locally reconstructing diagrams that one discovers the under-specifications in the Greek text which occur in a total of 8% of the propositions in book 13 of Euclid's *Elements* and in 37% of the propositions in book 1 of Apollonius' *Conics*. Clearly, the diagrams that we are so used to from our modern editions of the Greek mathematical texts are an integral part of the whole, or, as Netz puts it: 'the text and the diagram present... a cohabitation' [171]. The second observation to be made in this contribution concerns cross-referencing to previously established results. Again, given the practice in modern editions, when a previously proven theorem is used, we are used to a precise, explicit reference to its number. In contrast, as Netz notes for the Greek originals 'usually nothing happens explicitly' [173]. Netz argues that the reason for this lack of precise referencing is to be sought in the oral practice of Greek mathematics. In conclusion, Greek mathematics, which we think we know chiefly from its extant texts, has in fact a 'dual nature, both very oral and very written and... both very visual and very verbal' [175]. Taking these characteristics into account may increase our insight into Greek mathematical practice in the same way that Ritter's method of analyzing Mesopotamian and Egyptian texts has done for those cultures.

Jim Ritter has elsewhere argued that we should respect the formal characteristics of Mesopotamian and Egyptian mathematical texts. In his essay, he reintroduces his method of analyzing Mesopotamian mathematical problems by rewriting them in the form of symbolic algorithms which respect the procedural structure of the texts and provide a way of analyzing these structures and comparing problems more easily. In addition to the comparison with contemporary Old Babylonian mathematical texts, Ritter also contrasts his example of a Mesopotamian mathematical problem [Strasbourg 368] with Egyptian mathematical texts as well as Old Babylonian divinatory, medical, and legal 'practice' texts. Each of these comparisons provides further insights into the specific character of Strasbourg 368 and demonstrates a new method of reading ancient texts—which is usually much less straightforward than the beginning chapters of big overview histories of science make it appear: 'lacunae in the texts, hapaxes, a technical vocabulary for which it is difficult or impossible

to fix the semantic referents' [177] are only some of the obstacles a historian of ancient science has to overcome in reading ancient texts.

The article by Karine Chemla uses two examples to argue for collaboration between history of science and history of text. The example of the 13th century Chinese text *Sea-Mirror of the Circle Measurements* by Li Ye serves to highlight the pitfalls of naively reading a scientific text of another culture and/or age under the assumption that 'science' is universal, i.e., as if

> texts as such, except for the emergence of modern symbolism, had no history, as if they were invariant in time and space, as if they had always required the same operations from their readers, as if the same elements always meant the same things. [202]

Instead, as Chemla demonstrates, 'the kind of reading which would turn them into their modern counterparts might completely miss the way in which they make sense' and 'one would fail to grasp what is at stake mathematically' [216]. The second example from 18th century Europe deals with the structure and notations of a *Mémoire* by Euler published in 1753. The careful analysis of Euler's text leads to the insight that whole sections of it correspond to each other and can be obtained through systematic transformation. That this is not a coincidence becomes apparent when a mistake by Euler made in paragraph 22 is then 'translated' in paragraph 24 [220–222]. Again, it is proven that it is critical to analyze a mathematical phenomenon in a particular source; the type of text that is created to communicate it is not simply a means of conveying mathematical content, but has its own contribution to make in our understanding of the phenomenon.

The volume closes with an epilogue by David R. Olson ('Knowledge and Its Artifacts') who points out the role of text in the representation of knowledge:

> by creating texts in which statements, formula and images serve a representational function, one comes to deal not with the world but with the world as depicted or described. [231]

Olson looks at writing and reading texts in various periods and concludes that changes in our means of writing and our way of reading influence (the representation of) our knowledge deeply, thereby giving a final argument for the interconnection between the history of science and the history of text.

Throughout, the volume contains numerous black and white illustrations of excellent quality. The variety of topics and the quality of the individual contributions renders this book a veritable goldmine for anyone working with texts. It points out various approaches a researcher can take, and indicates the pitfalls of a naive attitude towards texts. While part of the book's appeal lies in its global approach, I hope that it will soon be followed by a series of specialized studies.

*The Science of Harmonics in Classical Greece* by Andrew Barker

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In the vast field of Greco-Roman music—a topic that has recently seen a dramatic increase in publications notwithstanding the difficulties that it offers to those who choose to take it up—the so-called 'science of harmonics' plays a particularly relevant role. It may be regarded as the logical premise of the whole of music theory, its task being to investigate the relations between differently pitched sounds from both a rational (or mathematical) and a physico-perceptional perspective, so that it becomes possible to establish which intervals and scales are acceptable in musical performances and which are not. This makes harmonics an intriguing border territory between philosophy, the history of science (mathematics in particular), and musicology.

It is not surprising, then, that harmonics has been given considerable room in almost every discussion of ancient Greek music, from François-Auguste Gevaert's groundbreaking *Histoire et théorie de la musique de l'Antiquité* [1875–1881] to Martin L. West's *Ancient Greek Music* [1992] and Thomas J. Mathiesen's *Apollo's Lyre* [1999], to mention just the most representative examples.

Andrew Barker himself focused on harmonics in the second volume of his *Greek Musical Writings* [1984–1989]. Though conceived as a handbook, this work is still indispensable for the quality of its translations of the original texts and for its brief introductions and footnotes to these texts, where many an issue was raised that would occupy discourse through the following decades. Almost 20 years later, and after having gained a reputation as one of the most

[1] An Italian version of this review is to be published in *Il saggiatore musicale*.



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renowned scholars in the field, Barker provides a new study of harmonics as a whole, whose achievements and novelties I shall try to discuss in what follows.

To begin, a simple glance at the book's proportions tells us that Greek harmonics has never hitherto been treated so thoroughly as a subject *per se*; moreover, as the very title makes clear, the research focuses almost entirely on the classical period, that is, on the less than two centuries ranging from the Pythagoreanism of Philolaus' time to Theophrastus. Accordingly, the whole discussion gravitates towards the two poles of the theoretic debate in the classical period: the so-called empiricist approach to the problem of the measurement of musical intervals (Part 2: 'Empirical Harmonics'), and the mathematical approach, which can be traced back to the Pythagoreans and reappears in Platonic thought (Part 3: 'Mathematical Harmonics').

Since the book is meant to address not only specialist readers but also classicists and musicologists as well as learned readers in general, there are explanatory sections which unravel abstruse technicalities in a perfect balance of clarity and accuracy. This is the case with the preliminary section which is divided into an introduction [3–18] and a chapter entitled 'Beginnings, and the Problem of Measurement' [19–30], in which Barker elucidates the problem of what musical intervals are and how ancient theorists tried to measure them.

As for the bulk of the volume, at least two points of novelty can be singled out that are quite likely to catch the reader's attention: first is Barker's decision to discuss the empiricist approach *before* the mathematical one; and second, the unexpectedly thorough treatment of the so-called ἁρμονικοί. Many is the time that the empiricists' position has been explained as if it were just a consequence of Aristoxenian thought—as if Aristoxenus was the first to argue for the primacy of perception in assessing intervals—because of the tendency to pigeonhole intervals according to mathematical or numerological categories. Such simplification does not take into account those theorists whose activity has wrongly been overlooked only because it is not very well accounted for in our sources, theorists who are usually referred to as ἁρμονικοί and who are strongly criticized by Plato in

his *Republic*.[2] An accurate reading of the sources allows Barker to go beyond Plato's ironic dismissal of these people, whom he portrays as strongly concerned not only with theoretical research but also with practical music performance and even with the invention of new musical instruments (as was the case with Ion of Chios' hendecachord [98ff.]). Plato's reason for belittling the ἁρμονικοί was, probably, their refusal to abandon empiricism in favor of building 'a theoretically well-organised scheme' in accordance with an epistemological attitude which Barker compares, with reason, to that displayed in the Hippocratic treatise *On Ancient Medicine* [104 and n58].

Barker's remarkably accurate discussion of Aristoxenus is preceded by a useful outline of Aristotelian epistemology ('Interlude on Aristotle's Account of a Science and its Methods' [105–111]) and a clarification of his own view of the unity of Aristoxenus' extant work. Far from considering the so-called first book of the *Elementa harmonica* as a half-baked sketch of what we read now as book 2, Barker sees book 1 as a work separate from books 2–3 and thus detects an evolution in Aristoxenus' thought ('[Aristoxenus'] approach to harmonic science may have shifted during the intervening years' [117]).

The section on Aristoxenus himself leaves the reader with more than one nice surprise, even where—as is the case with such a well-known and thoroughly studied work as the *Elementa harmonica*—one could reasonably not expect any. As for Aristoxenus' distinction between continuous and discrete voice, Barker emphasizes the fact that it is not meant to have an ontological value; rather, it is the perception on the part of the listener that plays an important role in attributing to what is heard the characteristics of the former or the latter [145]. Aristoxenus' alleged empiricism is described in accordance with its philosophical (essentially Peripatetic) background and distinguished from any anachronistic interpretation as a forerunner of modern psychoacoustic theories. His conception of naturalness in the development of melody is effectively linked to Aristotelian biology: a melody can be defined as natural, Aristoxenus says, if it moves in such a way that the role of each note—be it μέση, παρυπάτη, and so on—is made understandable to the listeners' ears [161 ff., 184 ff.].

[2] The relationship between Plato and the ἁρμονικοί has recently been discussed by Angelo Meriani [2003: see my review, 2006]. See also my review [2005, esp. 115n4] of Murray and Wilson's *Music and the Muses* [2004].

The following chapters, dedicated to books 2 and 3 of the *Elementa*, account for such basic concepts as διάνοια, μνήμη, and δύναμις [168 ff.]; nevertheless, there is room left for more general issues related to the cultural environment of Aristoxenus' time (e.g., Barker's remarks on the purposes of scientific activity and his comparison between ancient philosophers and modern scholars [182 ff.]). The so-called theorems that occupy book 3—actually a series of descriptions of those characteristics that make melodies acceptable and melodic—are analyzed with an eye to the difference between what Aristoxenus was trying to do here and other cases of axiomatization in Greek science, such as Euclidean geometry. On the one hand, there is no doubt that Aristoxenus was attempting to give an axiomatic form to harmonic science in order to make it 'something it had never been before, a science whose credentials were as recognisable and legitimate as those of any other' [229: see Brancacci 1984]. On the other hand, his system is not entirely compatible with what could be defined as a 'Euclidean' model, since the general principles were not only expected to be coherent with one another, they also had to comply with data coming from sense perception: accordingly, 'the demonstrations... depend heavily on unstated assumptions... which have not been explicitly integrated into the axiomatic framework of Book III' [203].

The idea that harmonics cannot be reduced to a system of propositions linked to one another by relations of consequentiality brings the most notable results in the important chapter, 'Contents and Purposes of Aristoxenus' Harmonics' [229–259], which ends the second part of the book. It is here, at least in my opinion, that the reader can savour the real novelty of Barker's thought on Aristoxenus. In a new interpretation of the clause ὅτι πειρώμεθα ποιεῖν τῶν μελοποιϊῶν ἑκάστην found in a well-known apologetic passage in book 2 [Da Rios 1954, 40.16–17]), Barker gives the verb ποιεῖν the meaning of 'composing' ('we are trying to make (*poiein*) each of the *melopoiiai*' [231]) and thus depicts Aristoxenus as a composer rather than a pure theorist. He is aware of the problems that this interpretation entails and honestly admits that he does not have answers to all the questions that it may raise [232]. His position should, however, be welcomed in so far as it knocks a healthy nail into the coffin of the *cliché* that the whole of Greek music theory was totally indifferent to music as performed and heard.

A separate and delicate issue is that of the making of musical judgment. It is particularly relevant not only to our understanding of Aristoxenus and his relationship with Plato and Aristotle, but also to our locating Aristoxenus in the mainstream of Western musical thought. The scanty evidence of this theme in the *Elementa harmonica* might lead us to think that Aristoxenus was not concerned with the problem of *ethos* as such, which might make him the first advocate of the autonomy of musical beauty—a Greek Hanslick, as it were. By looking at the evidence coming to us indirectly from our sources, that is, from the Aristoxenian passages in the pseudo-Plutarchan *De musica*, Barker avoids this anachronistic simplification and distinguishes two kinds of judgment in Aristoxenus' thought, the critical and the evaluative one.

As for the former, Aristoxenus' conception of the *ethos* of melody seems to be more mature than Plato's or Aristotle's. For him, the *ethos* comes as a result of the way in which the composer assembles the different elements of which the composition consists—not only the intervals, but also the rhythms and perhaps even timbre—rather than being mechanically determined by some preliminary choices on the part of the composer, such as that of the tetrachordal genre. The role played by the listener in the process of the understanding of music is thus strongly emphasized.

As for the latter kind of judgment, the evaluative one, it obviously implies a moral conception of music within the polarity of good *versus* evil, whereby it is possible to label one melody or musical composition as better than another on the grounds of characteristics that are neither formal nor technical. According to Barker, Aristoxenus is not very inclined towards Damon's and Plato's idea that music can directly affect people's character and behavior; at the most, it can display some of its intrinsic *ethos* through a process of signification that the listener has to decode. The moral evaluation of this meaning falls into the realm of philosophy and has little to do with musical expertise.

The third part of the volume, which is about the mathematical approach, opens in a definitely unexpected way. Instead of an introduction to Pythagoreanism, to which this approach is historically linked, the reader encounters a thought-provoking reading of Philolaus' famous fragment [Diels and Kranz 1951, Fr. 44B6] on the

basic musical intervals [264–288]. *Pace* the commonplace according to which the Pythagoreans were only capable of mathematically measuring the intervals, Barker shows convincingly that the vocabulary used here by Philolaus testifies to an earlier phase of Pythagorean music theory—Philolaus is the first Pythagorean whose dates can be historically determined—in which arithmetical speculations have neither overwhelmed the sensible data nor undone the links between the philosophers' research and the musicians' practical skills. Thus, the fifth is still called συλλαβά, the fourth δι' ὀξειᾶν, and the octave ἁρμονία. The intrinsic nature of Pythagorean tradition, which makes it particularly resistant to diachronic approaches, can often lead scholars to treat it as if it were a coherent and monolithic whole [see Musti 1990]. Such analyses as that carried out here by Barker are a good antidote to this tendency. Barker deals with Archytas' divisions of the tetrachord (as they are described by Claudius Ptolemy) in the same way, by paying sharp attention to the links between mathematical speculation and musical reality. He concludes that Archytas' musical thought is 'a turning-point in the story of Pythagorean harmonics, a shift from a focus on exercises in mathematical cosmology to a direct engagement with the details of musical practice' [295].

After Philolaus and Archytas, Barker turns to Plato [308–327]. A brief introduction to Plato's epistemology [311–315] precedes discussion of the well-known musical passages in Plato's work. In the *Republic*, Barker explains, harmonics is referred to within the broader context of the evaluation on the basis of mathematical criteria of what is good and what is not [315–318]. As for the *Timaeus*, Barker provides a remarkably clear account of the famous scale [318–323] and of the implications of harmonics for psychology [323–326]; but most importantly, he emphasizes the esoteric nature of the *Timaeus*. It is with Plato's Academy, in his view, that there comes into existence a place where intellectuals talk to each other in a jargon inaccessible to outsiders. Since harmonics itself is involved in this process, it ends up, as the last chapter's title reads, 'in the ivory tower'.

The section on Plato, though not particularly innovative—nor could it have been, given the outstanding stature of this philosopher and the enormous relevance of the issues concerning his view of harmonics—is definitely helpful and well thought out as a whole. As for Plato's alleged dismissal of experimental procedures in music

theory, Barker seems to rely vastly on the scholarly mainstream represented, for instance, by Mourelatos' contributions [315n12]. However, it would have been interesting, in my opinion, if Barker had taken into account the issues raised by Andrew Gregory, who has recently and convincingly tried to reduce the extent to which it is possible to speak of anti-empiricism in Plato [see Gregory 2000, 48–60].

The section dedicated to Aristotle opens with a provocative reading of pseudo-Plutarch, *De musica* c. 23, the contents of which—an account of the symmetries within the interval of octave—is attributed directly to the philosopher, including the inconsistencies in the text. According to Barker, Aristotle is himself responsible for these inconsistencies, not the anonymous compile: for Barker, though Aristotle knew the Pythagoreans' musical doctrine, he did not understand it thoroughly.

Barker writes here an important chapter in the study of the sources of the *De musica* ('An Aristotelian Fragment on Pythagorean Harmonics' [329–338]). He takes chapter 3 of the *De sensu*, in which the making of colors is accounted for in a way that is closely reminiscent of musical ratios, as evidence for the fact that at the end of the 4th century the system of consonant intervals began to include the so-called compound intervals (that is, those greater than an octave). As for the problem of a unit of measurement for the intervals, Barker shows that the search for it is incompatible with the basic assumptions of Pythagorean thought, whereby many an interval cannot be divided into equal parts. Barker's conclusion could hardly be more straightforward: 'There is no point in beating about the bush; so far as this aspect of the subject is concerned, Aristotle did not understand what he was talking about' [353]. The major merit of this section lies, in my view, in its showing that no later than the 4th century BC the relationship between the two main approaches to harmonics was quite far from being dogmatically polarized; on the contrary, in spite of a declared belligerency, 'a great deal of diplomatic activity was going on behind the scenes' [362].

The first treatise in harmonics, properly speaking, is the so-called *Sectio canonis*, which Barker, after a detailed account of the *status quaestionis* on the work's chronological placement, authorship, and unity of composition, dates to about 300 BC. As usual, Barker's way of approaching the text is quite unconventional: although the

*Sectio* is often treated as an example of geometric thought applied to harmonics, Barker approaches the text from an arithmetic point of view rather than from a geometric one. As he sees it, some propositions, namely, those implying the insertion of new notes within a given interval, seem to originate from mathematical reasoning and to have been integrated into a geometric-oriented context only at a later stage. Moreover, some incoherencies in the work's axiomatic structure are accounted for by Barker as a result of the compiler's activity. Most importantly, Barker opportunely points out that the author of the *Sectio* does not manage to escape the theoretical *cul-de-sac* intrinsic to every mathematical approach to harmonics, that is, the impossibility of establishing any relation of undeniable necessity between the quality of the consonance granted to some intervals by perception and some formal characteristics belonging to the ratios corresponding to those intervals.[3] Such a relation is denied by the fact that there are intervals which are not consonant albeit their ratios are multiple, and that there are consonances whose intervals are neither multiple nor epimoric. However, the historical value of the *Sectio* lies, in Barker's opinion, in the attempt by its anonymous compiler to establish the fundamentals of harmonics according to an axiomatic procedure, thus enabling the 'mathematicians' to counter the analogous activity that Aristoxenus was carrying out in enemy quarters, as it were, more or less in the same years.

The last part of the volume is devoted to Theophrastus' position against quantitative inquiries in harmonics [411–436], a topic Barker has dealt with in the past [1977]. Our only source for Theophrastus' musical thought is a lengthy fragment quoted by Porphyry in his *Commentary on Ptolemy's Harmonics* [Fortenbaugh 1992, Fr. 176]. He seems to think that the soul can generate the melody through a kind of activity which he refers to as a κίνημα (movement), where this movement cannot be evaluated in terms of quantity and so cannot correspond to a rato of whole numbers (as the Pythagoreans require). According to Barker, the target of Theophrastus' theory of the κίνημα might be Archytas himself. As for the hypothesis that Theophrastus is polemicizing in this passage against Aristoxenus, Barker is particularly convincing in ruling it out and in suggesting that the target could be Plato's ἁρμονικοί.

[3] E.g., its epimoric form, $(n+1)/n$, or its multiple form, $pn/n$ with $p \geq 2$.

Barker's comparison of Theophrastus and Aristoxenus reveals several similarities: they are both interested in the nature of what is melodic (τὸ ἡρμοσμένον), which is thought to have its own foundation in itself rather than in some mathematical principle. While Aristoxenus' conception of the ἡρμοσμένον seems to be an immanent one, Theophrastus pictures it as if it mirrored the inner movement of the soul, thus making the melody, somehow platonically, a reflected image of some other reality. Barker provides here the most thorough analysis Theophrastus' fragment has ever received, so far as this reviewer knows, and his interpretative skills make even more regrettable the loss of Theophrastus' *Harmonica* and of the light this work would have shed on both harmonics and acoustics in the fourth century.

With Theophrastus the span of time referred to in the volume's title is exhausted; nevertheless, the brief 'Postscript: The Later Centuries' [437–449] is quite useful especially for the non-specialist reader, as it helps to place harmonics in a wider chronological and cultural context. Once again, Porphyry turns out to be an indispensable resource for filling the gap between the 4th century BC and the treatises from the Roman and Imperial ages. It is in his *Commentary* that we find the fragments of Ptolemaïs and Didymus, who probably date back to the first century AD. In particular, the latter might be the initiator of the traditional opposition between Pythagoreans and Aristoxenians—at least if it is true that Ptolemy derived from him the terms in which he describes this opposition in his *Harmonics*. Barker takes this polarity as a guideline for his rapid outline of the following centuries, up to Porphyry himself.

This *coda* exhaustively illustrates the increasing divide between mathematical speculation *per se* and harmonics as a science of sounds. With Porphyry, a pupil of Plotinus, such a separation reached quite an advanced stage. His *Commentary* on Ptolemy covers only the first chapters of the treatise, which deal with the mathematical grounds of the discipline, while the commentator seems to quickly lose interest in the text as soon as it addresses specifically musical technicalities. Harmonics tends to became a speculation on a music that is not audible any more, rather than a means to bring order, problematic as the task may be, into the realm of concrete sounds; and the beautiful verses from *The Tale of Orpheus and Euridice his Quene* by the Scottish *makar* Robert Henryson (15th century), with their visionary

depiction of musical intervals causing 'the moving of the hevin', sound an effective conclusion to the skyward path of this science.

The bibliography, though admittedly selective, is definitely up-to-date and in accordance with the book's scope. The indexes are accurately compiled and easy to use.

All things considered, what we have here is a *summa*. Works like this leave no doubt that the study of harmonics has left what only a few decades ago seemed to be an academic limbo and has reached its full maturity; and it is easy to predict that further inquiries will hardly achieve any serious accomplishments without making the most of this volume.

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*Mechanics from Aristotle to Einstein* by Michael J. Crowe

Santa Fe, NM: Green Lion Press, 2007. Pp. xxii + 332. ISBN 978–1–888009–32–3. Paper $17.95

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This book is designed as a text for undergraduate courses, though the preface tells us that it has also been used for a graduate course. As a text, it is a good standard text dealing mostly with early modern physics (mechanics), with some background on Aristotle and an end section on Einstein's special and general relativity. The main part of the book covers Galileo to Newton, and, in fact, is concerned mostly with Sir Isaac himself. There are ample selections from the writings of the famous authors treated, which makes it useful for giving students some sense of primary sources. Then, there is Crowe's commentary, which is clear and for the most part admirable. There are also exercises or problems directed towards students that will help them think through the issues raised by the texts and commentary.

Crowe advances the thesis that 'a number of the main advances that occurred in mechanics were directly linked to an enriched understanding of the relativity of motion' [26]. One could quibble with certain of the interpretative claims, such as 'the law of inertia provided the Copernicans with an explanation of why objects on a moving earth were not left behind' [21] or the treatment of Newton's *Principia* as a hypothetico-deductive work [209ff.]. But with such an admirably clear commentary, these would be mere cavils. Besides, instructors need to find some faults with a text in order to make the course their own.

I do have one more serious complaint however. I called it a 'standard' text above, because like most treatments of mechanics, it gives short shrift to the 18th century. Maybe it is time for someone to write a more inclusive history, and spell out, for example, why Euler and Lagrange thought they were revolutionizing physics, and why they did not really consider themselves Newtonians.


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Finally, it is of note that the book is excellently produced. Green Lion Press, run by William H. Donahue and Dana Densmore, continue to publish books of outstanding quality.

*Before Darwin: Reconciling God and Nature* by Keith Thomson
New Haven/London: Yale University Press, 2005. Pp. xiv + 314, 8 plates, line drawings. ISBN 0–300–10793–5. Cloth $25.00

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Keith Thomson begins his book with the observation that the young Charles Darwin occupied the rooms in Christ's College, Cambridge which had once been home to William Paley. He seeks to explore the irony, as he puts it, that as a student Darwin would have read Paley's works of natural theology, and to understand the arguments of natural theologians in the century and a half before 1831, when Darwin embarked on his epoch-making voyage on HMS *Beagle*. At times this delivers a purely historical enquiry, for example when Thomson considers how, in the light of contemporary natural theology, Darwin struggled to make sense of the evidence of geological change that confronted him on the coast of South America. Yet the book also seeks to intervene in contemporary debates about the status of evolutionary theory, claiming that it is essential for the modern reader to choose between science and religion (as Thomson suggests that Darwin did) in a manner that was not required of early modern natural theologians. The thrust of Thomson's argument is that, once the theory of evolution had been advanced and defended, natural theology ceased to be an arena for scientific debate and argument, and became simply another way to assert faith in a deity. Thomson is thus writing both a history of early modern (English) natural theology and seeking to use that history to determine what is and what is not legitimate in contemporary debate about science and religion, in particular concerning the theory of evolution.

Thomson is a retired professor of natural history and his treatment of modern ideas about evolution is usually clear and assured. He is careful to argue that science is about a search for secondary causes and that scientific knowledge remains provisional. Reasonably enough, however, he wants to persuade others to share his convictions about the importance of the theory of evolution. Above


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all, he is eager to debunk common-sense claims that complexity cannot have arisen through random mutation and to counter other ill-informed attacks on Darwinism. Discussion of the argument from design mounted by Paley and his predecessors, therefore, becomes a means to demonstrate the superiority of Darwin's analysis of nature and to suggest that modern appropriations of natural theology represent an anachronistic form of scientific argument.

It is not clear what the readership of this book is supposed to be. As a piece of apologetic for Darwinism, it seems unlikely to be particularly successful (despite having been welcomed by the popular scientific press). Those who doubt the validity of evolutionary explanations of the natural world are unlikely to change their minds on the basis that Darwin's science was more up-to-date than Paley's. To provide an accurate historical account of the development of theories of natural selection requires going beyond Darwin (in particular through consideration of genetics, mentioned briefly by Thomson) and demands sensitivity to the ways in which the science of evolution has modified and even rejected aspects of Darwin's own theories. It is misleading to claim that debate over Darwinism was settled immediately after the publication of the *Origin of Species* [1859], as the final chapter appears to do, or, indeed, to argue that late 19th-century evolutionary theories were always faithful to Darwin. Thomson is complacent about the difficulties that Darwinism posed even for its sympathizers. Nevertheless, his conclusion appears to be that scientific knowledge is necessarily incomplete but that this incompleteness is not by itself an argument in favour of a role for some kind of deity in nature. This will not be enough for those who wish to reconcile science and religion, and it will seem coy to those who simply do not believe in religious arguments of any kind.

For readers in search of historical knowledge rather than apologetics, *Before Darwin* is terribly flawed. Thomson makes the elementary error of judging his characters on the basis of an evaluation of the similarity of their ideas to those of modern science, trying, for example, to assess 'how close some of these early geologists got' to the theory of plate tectonics [188]. He appears to assume that natural theology was about saving a role for God in an increasingly scientific world and that, to do this, it was necessary for natural theologians to make nature accord with a literal reading of the text of Genesis. Yet many of the natural theologians (including both John Ray and

William Paley) whom Thomson discusses were as concerned with classical, philosophical arguments against a deity, or with contemporary, moral attacks on Christianity and the reliability of the Bible, as they were with the consequences of new scientific ideas. They did not see those ideas as the product of a rational spirit of enquiry that was inimical to religion; instead they believed that both God and nature could only be understood through the suitable exercise of reason.

Thomson recognizes some of this [141] but his understanding is undermined by an inability to accept that early modern theologians were more sophisticated in their reading of the Bible than present-day creationists. Thomas Burnet, for example, was not seeking 'to bring together in one unifying theory the Biblical account of creation and the new sciences of the earth' [142]. On the contrary, Burnet recognized that there were problems with a literal reading of Genesis. He wished to read the Old Testament theologically through the light of the New Testament (in particular, 2 Peter), and wanted to advance a theory about the creation, dissolution, reformation, and future destruction of the Earth that was compatible both with classical mythology and with contemporary Cartesian physics. A Christian tale of redemption provides the narrative for Burnet's history of the Earth, but the point of the theory is to tackle problems in classical philosophy and ideas of nature rather than to make modern science agree with the Bible. Thomson advances a number of possible targets for John Ray's natural theology, which he describes as being that of the 'founding father' of the school he is studying. Unfortunately, every one of these (Locke, Hume, Toland, even Boyle) wrote long after the arguments that were eventually embodied in Ray's *Wisdom of God* [1691] had originally been advanced in sermons at Cambridge in the late 1650s and early 1660s. Ray, indeed, later considered Boyle to be an ally in his attacks on Descartes, whose crime was not to be in some vague sense an 'atomic-deist' [31], but, quite specifically, to have cast doubt on ideas of the immortality of the soul and to have denied the existence of final causes in nature.

Time and again, Thomson assumes that early modern writers were constrained by a need to be faithful to the literal sense of the Bible at all times. This misconception appears to arise from a misunderstanding of the nature of Galileo's crime, which was not simply to offer an interpretation of nature apparently at odds with the literal meaning of some passages of scripture, but to disobey a prohibition

on teaching a hypothetical interpretation of nature as if it were absolutely true. Thomson fails to appreciate the relative freedom provided by techniques of accommodation or by figurative readings of biblical terms. More seriously, he misunderstands the way in which a prevailing culture of literal interpretation encouraged readers to view the Bible as a historical narrative, which gave context to natural phenomena (such as fossils) and for which they might in turn provide factual evidence. Thus, the problem for early modern readers, as distinct from their 19th-century counterparts, was not so much that fossils demonstrated that there had been mass extinctions or that the Earth had a long history as that they might be proof of the spontaneous creativity of nature. Thomson tends to project the theological difficulties faced by mid-19th-century apologists back onto earlier writers, in the process ignoring or playing down the threats that were most pressing to them.

Thomson constructs a familiar narrative in which the argument from design is advanced and to some extent refined in the late 17th century and then subjected to successive evidentiary challenges that come to undermine it by the mid-19th century. There are problems at every stage of this narrative. Thomson's tendency to confuse the largely theist argument from design with deism obscures one of the principal motives of early proponents of natural theology. His work fails to convey the sense in which the argument from design represented a relatively ecumenical response to a variety of intellectual challenges to orthodox Christianity. Those challenges came from freethinkers, from a growing awareness of the atheism of much classical thought, and from advocates of individual spiritual inspiration. The design argument hinged on the claim that human reason, which linked mankind, however fallen, with God, could trace the signs and purposes of divine activity in nature. Over the course of the 17th century, strict adherence to the concept of the lawfulness of nature directed natural theologians towards the argument from design and away from more wide-ranging celebrations of divine involvement in natural wonders and marvels. In the process, the involvement of divine superintendence with secondary causes in nature was widely accepted. Writers such as Thomas Burnet, for whom the secondary causes that shaped the contemporary, fallen world were distinct from the original physical intentions of God, or other Neoplatonic authors (even, at times, John Ray), for whom nature retained a creative role,

did not, in this respect, represent the mainstream of thinking about the argument from design. Instead, the contrivance that was so important for Paley in demonstrating divine involvement at every stage in the regular working of creation was a direct statement of God's power over the natural world. Thomson does not always appear to understand that the God of the design argument was a God who was in control of secondary causes, rather than a deistic first cause [cf. 107–108] or a God of the gaps.

This conception of God explains both why natural theology remained so adaptable throughout the 18th century, even when understandings of the working of secondary causes altered, and why Hume's critique of natural theology was, at the same time, so powerful and so relatively ineffective. Hume argued that it was not possible to identify the power that lay behind effects that were visible in nature through a chain of consequences and that it was human imagination, rather than reason, that presumed that a person might be always and fully in control of natural actions and reactions. The relationship between secondary causes and divine power posited by natural theologians was, for Hume, a denial of the true nature of God as supreme creator, which made God responsible for the trivial, contradictory, and mundane. Clear though Hume's reasoning seems to us, it ran up against the overwhelming success of Newtonian physics and theology in demonstrating that the universe was orderly and lawful, and in claiming that it therefore must respond to divine direction. Thomson says little about the appropriation of Newton for the design argument, a process that began before Newton himself endorsed it publicly through the General Scholium to the second edition of the *Principia* [1713]. Indeed, he appears quite confused about Newton's role in natural theology more generally, claiming, for example, that the Neoplatonist, Ralph Cudworth, who believed in the activity of a plastic, creative force in nature, was 'quasi-Newtonian' [205].

This sort of schoolboy error plagues Thomson's book on a scale that is remarkable for a work published by a reputable university press and that would be shocking even in the most hurried of trade publications. Almost every word in a foreign language is spelled incorrectly, including names ('des Cartes' [29]; 'Stenson or Steenson' [113]; 'Leewuenhoek' [81]; 'Leibnitz' [46]; 'd'Holback' [61], for Descartes, Stensen, Leeuwenhoek, Leibniz, and d'Holbach) or titles of books (Burnet's *Telluris theoria sacra* becomes *Telluria Theoria Sacra*).

Technical theological terms are garbled: 'Non-Jurant' is thus put for nonjuring and 'Armenians' mysteriously appear from the Caucasus to do battle with the theology of John Calvin (in place of followers of Arminius of Leiden) [52]. Attempts to provide a little historical context are hilarious in their ineptitude: the Plymouth Brethren, a sect whose development would usually be placed in the 1830s, are made to stand for the trials of nonconformity after 1662 [52]; with reference to almost the only year of relative peace in over two decades of conflict between Britain and post-revolutionary France, we are told sententiously that 'we must also remember that in 1802, Britain was at war with France' [54]. The loyal churchman, John Ray, is labelled, in a convoluted expression that betrays multiple misunderstandings, 'not just a dissenter' but 'a Dissenter... although he retained a lay membership of the Anglican Church' [64–66]. Titles are regularly mistaken: the Cambridge Hebraist, John Lightfoot, receives a knighthood [115], whereas James Ussher, Archbishop of Armagh, is downgraded to a bishopric [114]. Thomas Burnet loses his doctorate [141], while the homosexual libertine, Dr. John Woodward, is received into the priesthood of the Church of England ('the Reverend Doctors... John Woodward' [142]). Robert Hooke is correctly described as being eight years younger than Robert Boyle [33], but incorrectly assigned the year 1627 for his birth (which was, however, when Boyle was born). Strangest of all, perhaps, is the appearance of a drawing of Hooke [34], one of several vignettes executed by Thomson's wife, Linda Price Thomson. Most of these, though smudgy, are recognizable for being copied from well-known images that might have been reproduced more accurately at little cost to the author or publisher: what is, of course, remarkable (although not mentioned) about Hooke's portrait is that it must be the pure invention of Mrs Thomson, since no known likeness of Robert Hooke survives. Unless, that is, the portrait labelled 'Hooke' is meant to be of someone else. The book has been published with Yale's usual, frustrating system of endnotes: these, however, are more frustrating than most, since the author has not bothered to provide page references for any of them (even those that reference quotations).

Although misunderstandings about how change took place bedevil Thomson's account, in one respect his work offers the reader a more interesting perspective than the existing, standard accounts of the rise and demise of the design argument. Despite his frequently

teleological language and focus, Thomson remains alert to the fact which initially surprised him, that is, that Darwin himself was trained in the school of Paley. His best chapters consider this relationship more fully and show how the questions and solutions raised by Paley occupied Darwin on the *Beagle* voyage. It is a pity that Thomson did not devote more time and effort to his study of Paley himself. His account of the relationship between Paley and Malthus, for example, is pretty slight. The focus of much of the book on seeing Paley as the end of a tradition detracts from the more interesting possibilities that Thomson raises for tracing the continuity and development of the design argument in the early 19th century. In part, this is a consequence of the intellectualism of Thomson's account of change, in which arguments that are less wrong (from Thomson's perspective) inevitably win out over ones that are even worse in the march to improvement of science. Thomson does not consider the possibility that intellectual (let alone social or political) factors extrinsic to the narrow debate over the argument from design might have played a role in altering perceptions of the value of natural theology. As Thomson acknowledges, Paley himself was involved in the development of the concept of utility, which, in the hands of Malthus, Bentham, and, particularly, Mill, shifted the terms in which the argument from design might be understood. If utility implied the greatest good for the greatest number, then the utilitarianism of much natural theology, in which the activity of an all-powerful God was held responsible in a piecemeal fashion for what were often fairly limited or subjective benefits, seemed pretty unsatisfactory. Voltaire or Hume, in their very different ways, had exposed some of the logical shortcomings of the claims to utility made by natural theologians. Their criticisms of principle, however, may well have been less damning than the growing awareness of the shoddiness of argument practised by natural theologians in a society increasingly used to complex, statistical assessments of loss and gain.

Nevertheless, it seems unfair both to Paley and to the young Darwin to argue, as Thomson does, that 'Paley... contributed to the promotion of atheism in the form of the evolutionary theory of Darwin' [259]. Instead, it might have been safer to suggest that Darwin substantially reworked the utilitarian calculus that was present in most early modern natural theology, but which Paley had brought

to the fore. The consequences of that reworking for religious apologetics were complex and, in the initial uncertainty, Darwin's own faith was one among many that were shaken. Yet, for a God who works through natural causes, the theory of natural selection was not necessarily fatal. Only from a position of hindsight (and, to some extent, with the assistance of Darwin's later writings) does it become apparent that the theory of natural selection advanced in the *Origin of Species* cannot properly be understood in a form that allows straightforwardly for divine direction of its processes. The importance of random mutation in theories of sexual selection and, eventually, genetics posed and poses genuine problems for the idea of divine purpose in natural causes. Such randomness, however, remained deeply controversial for scientists as well as theologians, even during the 20th century. It is not to Thomson's credit that, in his principal description of natural selection [217–221], this critical aspect of the debate is left obscure. In this sense, if Thomson's purpose was to use history to clear up a contemporary disagreement between science and religion, he has simply chosen the wrong subject or at least the wrong period to write about.

Despite its clarity and the engaging insight that frames its narrative, this remains therefore a deeply unsatisfactory book. That there still exists no good, general treatment of natural theology that covers the entirety of its hey-day, from the 1650s until the 1870s, makes that all the more frustrating. Dissatisfaction with Thomson's argument is turned into annoyance by the shoddy presentation of his book. The grotesque sequence of factual and orthographical errors (most of them repeated in the index) indicate that nobody involved in the production of Thomson's book paid much attention to it once it had been drafted. If neither the author, nor his agent, nor the publisher's editor, nor a proof-reader, nor any of the distinguished names who are thanked for reading the manuscript, or who provide comments quoted by the publisher, could take the trouble to read the book carefully enough to correct such howlers, one must ask the question why should anyone else bother?

*The Symbol at Your Door: Number and Geometry in Religious Architecture of the Greek and Latin Middle Ages* by Nigel Hiscock

Burlington, VT/Aldershot, UK: Ashgate, 2007. Pp. xx + 421. ISBN 978–0–7546–6300–3. Cloth $99.95

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If indeed all of western philosophy is to be taken as a commentary on Plato, Nigel Hiscock would include the religious architecture of the European Middle Ages in the same extended footnote. He explains that his recent *The Symbol at Your Door* is meant to serve as a companion volume to his *The Wise Master Builder* [2000]; but where the earlier work explored the implicit use of Platonic geometry in medieval building, this new one, he says, is explicit in showing 'the occurrence of number and geometry. . . <to be> beyond doubt' [9]. Moreover, Hiscock is forceful in arguing that this application of Pythagorean/Platonic mathematics to religious architecture was not merely a question of arbitrary pattern-making or of blind lodge practice, but rich with deliberate symbolism in affirming, through number and geometry, the concurrence of cosmic order and Christian doctrine.

In this, the author says, he is taking issue with the modernist denial of meaning that made architecture a self-referential question of structure and function; and he allies his own project to post-modernism and the retrieval of meaning. The post-modern critique of modernism is not new, nor for that matter is the argument for the importance of number and symbolism in medieval architecture. Besides his own previous work [2000], Hiscock mentions the work of Conant [1963], Frankl [2000], Krautheimer [1965], Panofsky [1953[, and Otto von Simson [1956], among others. He might also have mentioned the work of Joseph Rykwert [1988, 1996]. Most recent among books demonstrating the connection between Platonism and Gothic architecture is Philip Ball's 'biography' of Chartres cathedral, *Universe of Stone* [2008].


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Hiscock's new book does, however, appear singular in the range of its ambition, in the tenacity of its author's determination to demonstrate the significance of virtually all combinations of geometric forms and numbers in the plans, elevations, and volumes of the medieval religious buildings analyzed, and in his virtually unwavering ascription of it, ultimately, to the Platonic fountainhead. The number of cases explored is, to say the least, impressive, as indeed is the scope of textual evidence brought to bear: from Plato, Pythagoras, and Vitruvius, through the Old Testament, the Gospels, St Augustine and Boethius, to Alcuin, Robert Grosseteste, Hugh of St Victor, Alberti, and beyond. In an investigation where, as Hiscock puts it, the thematic is meant to take precedence over the chronological, 'the ideas exemplified are relatively timeless' [11]. Even the popular number song known as 'Green Grow the Rushes-O' which begins with 12 (the 12 apostles) and ends with one (God), is shown to be rife with number mysticism, with 5 standing for the pentagram as apotropaic symbol—'the symbol at your door' the song calls it, and the source for Hiscock's title [47].

The *Symbol at Your Door* consists of a prologue, six chapters of varying length, and an epilogue. The prologue provides a historical introduction, most of it, Hiscock acknowledges, a digest of material already presented in *The Wise Master Builder* [2000]. It is a useful review for those not already familiar with this well-known background, but reveals nothing new—certainly not to someone who has recently covered much of the same territory. The key ancient source is, of course, the *Timaeus*, the most Pythagorean of Plato's dialogues, which was well-known throughout the Middle Ages. With its divine craftsman who fashions the universe in keeping with a mathematical model using 4, especially, as the number of cosmic order, and with its account of the regular, 'Platonic', solids, each with its own elemental referent, the *Timaeus* was (as it remains, for many) the ultimate touchstone for reflections on the cosmic dimension of architecture.

Two of these solids, the sphere and the cube (the heavens and Earth, in the *Timaeus*' repertory), supply the theme for Hiscock's first chapter, 'Heaven and Earth', which deals with the symbolism of domed churches in the Byzantine world. Chapter 2, 'Temple and Body', is a brief, 20-page account of the relation between macrocosm and the human microcosm, and of the temple as their architectural point of intersection. Chapters 3, 4, and 5 deal, respectively, with

the numbers 3, 4, and 5 and the deployment of their corresponding geometrical figures (triangle, square, pentagon) in building. Chapter 6, 'The Whole Frame of the Universe', ranges widely, devoting over a 100 pages to the symbolism of circular and centralized structures including apses, wheel imagery (in rose windows, e.g.), and tracery. The epilogue reviews the material covered and ventures a short excursus into survivals of symbolism in Romance literature and music.

The principal and most difficult question Hiscock appears to be addressing in this rather unwieldy book is the following. Abundant textual evidence from the Middle Ages leaves no doubt that the teachings of Plato and Pythagoras survived in Christian Platonism and that, because of their symbolic resonance with Christianity, Platonic/Pythagorean numbers became an integral part not only of Christian teaching but also of how churchmen responsible for initiating building projects imagined their buildings. The evidence presented also seems to leave no doubt that the relevant numbers and geometrical figures are in fact present, to an overwhelming degree, in the fabric of medieval religious buildings. To put it simply, the question then becomes how and why the numbers moved from the metaphysical speculations of architectural patrons, through to the building yards of master builders and their teams of masons who then transferred them into plans, and chiseled them into shapes of trefoils, quatrefoils, pentagrams, and the like. What is the connection between the learned, textual evidence and the silent evidence, so to speak, on the ground? What, in other words, was the connection between medieval theory and practice? Hiscock infers that the transfer of metaphysics to solid matter occurred not just through training in practical geometry but that it was also entirely self-conscious, the result of teaching in monastery schools of the liberal arts (the trivium and quadrivium) which were the main vehicle for the Christian Platonist world view. To teach this world view to the illiterate was, according to Hiscock, the purpose of deploying number symbolism in religious architecture.

The simplistic exercise of such a linear approach is bound to falter, and often does, with declamation frequently standing in for

genuine scholarship,[1] and much recourse to phrases like 'would have been', 'conceivably', and 'it may be safely assumed that'. The assumption underlying this linearity is that theory directs practice just as the king directs the architect the 13th-century manuscript drawing which Hiscock reproduces on page 39. It might have been helpful to entertain alternative scenarios, especially in view of how faint the traces of transmission actually are.

The book is more convincing when it comes to dealing with specifics. In his first chapter, 'Heaven and Earth', the discussion of Hagia Sophia, which Hiscock says supplied the fundamental pair of Platonic solids—sphere and cube—to the schematic design of Byzantine churches, is quite compelling. So too, in the final chapter, is his well-documented argument that the geometrics of tracery carried a weightier narrative than that of the stained glass it framed. The suggestion raised in the epilogue that, in the Renaissance, Alberti and others were as much perpetuating a medieval tradition as reviving an ancient one also raises interesting questions.

In general, however, Hiscock's book is overly long with far too many quotations, clumsily written, and redundant. It is flawed by weak scholarship, displaying no apparent awareness, for instance, of basic references such as Karl Lehman's *The Dome of Heaven* [1945], Carol Krinsky's bibliography of the Vitruvius manuscripts [1967], or J. E. Raven's important 'Polyclitus and Pythagoreanism' [1951], not to mention recent work on Vitruvius [see, e.g., McEwen 2003]. Translations cited—Leoni for Alberti, Granger for Vitruvius—are often no longer acceptable. Nor does the arguable claim that 'the ideas exemplified are relatively timeless' [11] excuse a tendency to anachronism.

The *Symbol at your Door* is of potential value for its detailed case studies but, as an attempted synthesis, falls disappointingly short of its avowed aims.

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[1] See, e.g., on page 50: ' Is it likely that an architect would have built a church devoid of symbolic content, divorced from his patron's religious predilections, and confounding literary and documentary evidence to the contrary?'

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*La scuola medica Salernitana. Gli autori e i testi* edited by Danielle Jacquart and Agostino Paravicini Bagliani

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When Benjamin of Tudela visited Salerno in the 1160s, he reported that it was a place 'where Christians have a school of medicine'. The late 16th-century translator of Benjamin's travel book, the Seville theologian Benedictus Arias Montanus (d. 1598), rendered the sentence slightly differently to assert that Benjamin went to Salerno, a city famed for its medical schools (*urbem medicorum scholis illustrem*). Was there a single, distinct, formal institutionalized medical school in 12th-century Salerno? Or does Montanus' corrupt translation, stressing a multiplicity of schools, reflect more accurately a contemporary sense of fluidity regarding the institutional reality of the medical school in Salerno?

It is now hardly debated that the medical school in 11th- and 12th-century Salerno was not an institutionalized system of education but an 'open' school of thought defined by its members' adherence to a specific philosophy of medicine and style of representation. But who were the driving forces behind such a school? Was it bishop Alphanus of Salerno [*fl.* 1058–1085], who translated Nemesius' *On the Nature of Man* from the Greek, thereby providing the Latin West with the terminology necessary for the reception of Islamic medicine? Was it the community of monks at Monte Cassino where Alphanus was a monk before answering the call to the archbishopric?[1] Or was it the enduring Greek presence in southern Italy in and around Salerno? Or was it perhaps Islamic medicine itself? Once translated into Latin, this superior scientific knowledge which

[1] Constantinus Africanus translated there the *Pantegni* and much more in the 1080s.



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used the natural part of Aristotelian philosophy as a basis for medical practice and introduced the *quaestiones* as a key tool of thought and presentation could have inspired Western thinkers to acquire and then disseminate it [Jacquart and Micheau 1996, 87–129]. A comprehensive history of medicine in 11th- and 12th-century Salerno would have to be based on a balanced account of all these variables.

In early 12th-century Salerno, historians tell us, a historic shift took place when Salernitan masters, confronted with newly translated Arabic works grounded in a theoretical framework of elements, humors, qualities, and complexions, became aware of the need to establish a firmer philosophical foundation for their discipline and consequently transformed the Latin medical discourse. In Salerno, the commentary tradition began. And in Salerno, basic texts on *materia medica*,[2] diagnosis,[3] and medical ethics were composed and became standard manuals for decades and even centuries to come. This thesis is supported by the close association of the earliest surviving copies of the core collection of medical works, henceforth constituting the standard curriculum of medicine, with medical education in southern Italy, more specifically in early 12th-century Salerno [see Kristeller 1986; Jordan 1990; García-Ballester 1994: 13–29; Skinner 1997: 127–36; O'Boyle 1998, 95–102]. This core collection of five or six texts heavily loaded with theory—Joannitius' *Isagoge* (normally appearing first), Hippocrates' *Aphorismi* and *Prognostica*, Theophilus Protospatharius' *De urinis*, Philaretus' *De pulsibus*, and, from the last quarter of the 12th century, Galen's *Tegni*—was sometimes called *Ars medicinae* or *Ars medica*. Renaissance editors named it *Articella*. Some of these manuscripts attest to an intensive production of commentaries on these core texts, based on sources translated or taught at Salerno and attributed to Salernitan medical practitioners such as Matthaeus Platearius, 'Archimatthaeus', Bartholomaeus of Salerno, and Maurus of Salerno.

All these texts suggest the emergence of a rift between pre-12th-century Salernitan physicians who showed not the slightest interest in the natural part of philosophy and rational methods of analysis,

[2] To name just the two most famous of the pharmaceutical treatises: *Circa instans* and the *Antidotary of Nicholaus*.

[3] Giles of Corbeil's treatise on urine, for example.

and who remained indifferent to or ignorant of the work of Constantinus Africanus, and the Salernitan physicians of the second or third decade of the 12th century who dramatically changed their attitude and transformed their scientific discourse. But is this account correct at all? What is the connection between such 11th-century Salernitan physicians as Gariopontus or Petrocellus and their 12th-century successors who produced the Salernitan commentaries? And if the above change did happen, who were its carriers? Practicing physicians? Members of the ecclesiastical *milieu*? The mid-12th-century physicians who produced new-style commentaries on the *Ars medica*? And what role did the social and political conditions in southern Italy play in allowing the initiation and the further development of such an intellectual process?

The phenomenon of the medical school in Salerno has so far remained a hypothesis, and historians have consistently qualified their words with conditional clauses and quotes. The nature of the change which took place in 12th-century Latin medicine is relatively clear. The school's prominence is plainly attested by a series of thinkers who made a special effort in the 12th century to visit Salerno and to export all over Europe ideas learned there as well as practices, theoretical terms, and teaching methods.[4] But how exactly, when, and where this change took place is less clear. The lack of evidence that there was any institutional structure in Salerno for the teaching of the *Ars medicinae*, and the wide gap between these works with their commentaries and all we know about medical practice and teaching in 11th-century Salerno, have consistently fed doubts. The Salernitan texts, whose exact chronology is far from certain, are all too often transmitted in several versions, which makes it difficult to determine their original form and renders all available printed editions practically unreliable. And not only are the texts volatile; the identity of many of their authors is shrouded in heavy mist. This applies to prominent Salernitan figures like the famous Trota, and to less known figures who are unjustly understudied. For example, Raphaela Veit draws our attention here to the *Liber aureus*, an understudied medical manual composed by Afflacius, Constantinus Africanus' disciple and heir of his books. Veit's preliminary investigation suggests that

[4] To name just a few: Adelard of Bath, Herman of Carinthia, Giles of Corbeil, William of Conches, Alexander Neckham.

the book is among the sources for the two essential versions of the *Practica pantegni* attributed to Constantinus Africanus.

On top of all these doubts and uncertainties, Piero Morpurgo [1990, 1993] has more recently claimed that many of the exegetical innovations attributed to Salerno were the product of the schools of northern France (Paris, Tours, and Chartres) and not of Salerno. According to Morpurgo, the northern schools were not only centers for the diffusion of Salernitan medicine, they also trained the Salernitan medical masters in using the new tools of logic before these masters then returned to southern Italy in their search for medical works newly translated from Greek and Arabic. So, although southern Italy played a crucial role in the recovery of medical texts, it was in northern France that they were first subjected to the new pedagogical techniques of glosses and commentaries (largely borrowed from theological and philosophical discourses at the schools) and then taught within the framework of Aristotelian natural philosophy. Thus, a unique cultural encounter between the medical sources circulating in southern Italy in and around Salerno and physicians trained at the French schools, is the key to understanding the Salernitan innovation.

The phenomenon of Salerno requires, therefore, not only the philological analysis of Salernitan texts and manuscripts. For it is also the story of a multicultural society, a center of international commercial activity, and part of a network of centers of learning to the north (in Italy and beyond). It is also the story of a successful attempt to claim a place for medicine in the schools. For this to happen, medicine needed to be considered a *scientia* capable of being conveyed by *doctrina*. This depended on establishing medicine on authoritative texts from antiquity, hence the creation of the *Articella* (an anthology of medical semiotics based on Greek material and introduced by a theory-oriented text with a Greek title, *Isagoge*) by the Salernitan masters. And it is also the story of some harsh criticism leveled against the folly and perversity of the Salernitans whose innovations were not always well received. These Salernitans were occasionally suspected of introducing a material approach to the soul governed by elemental influences and manipulated, for example, by dietary regulations. John of Salisbury even denounced vehemently in his *Metalogicon* 1.4 the vanity, folly, and avarice of physicians who

return from studying medicine in Salerno and Montpellier, cite Hippocrates and Galen, throw in aphorisms, and market themselves as omnipotent against all diseases.

The story of Salerno as an intellectual center derives from the undeniable wealth of medical texts emanating from that region from at least the 10th century. But the systematic study of these texts and manuscripts has relied heavily on the monumental 19th-century compilation by Salvatore de Renzi, who in five volumes published many of these texts in his *Collectio salernitana*. Though very helpful, the very nature of such an uncritical compilation prevented it from becoming a reliable research tool for those wishing to reconstruct the intellectual reality in 11th and 12th-century Salerno. The Salernitan connection of some texts published by de Renzi is doubtful (e.g., the versed *Speculum hominis*, discussed here by Paul Gerhard Schmidt).

*La Scuola Medica Salernitana. Gli autori e i testi*, a collection of 18 articles written in four different languages, come with an elegant and thoughtful introduction by Danielle Jacquart and a panoramic concluding paper by Giovanni Vitolo laying out the view of the updated Salernitan story arising from these papers and the after-history of Salerno in the 13th-century under Frederick II. It treats the school of Salerno as a real intellectual phenomenon and does not set the term in scare quotes. But, given the volume's title, its editors clearly believe that studying the authors and the texts associated with Salerno is still the main key to understanding the phenomenon of Salerno, which was more a center of book production and diffusion than a formal school. These editors convened a similarly entitled conference in 2004 at the University of Salerno on the occasion of the acquisition of a mid-13th-century manuscript containing the *Curae magistri Platearii* or his *Practica brevis* by the Biblioteca Provinciale in Salerno (discussed here by Maria Galante). That conference launched a new project aiming at a critical republication of Salernitan texts to provide the basis for an updated study of Salerno within the broader context of the history of Western culture, science, and thought in the Middle Ages. This volume, which is based on papers read in this conference, promises to be the first in a rich series that in the coming years should provide us with a whole range of Salernitan texts published for the first time in critical editions—a major contribution to the history of medicine and science in the Middle Ages.

We may hope that it will render De Renzi's magisterial enterprise at least partially redundant.[5]

This explains the nature of most papers in this collection: they are based on philological and codicological discussions, and normally target a single text or author that should be edited. Many of the articles include appendices that expose for the first time partial editions and sometimes translations of the discussed text, thus providing the expert reader with a unique tool. Taken together, they point to two main conclusions. First is that there is an absolute need to produce scientific editions of key Salernitan texts because of the unreliability of the Renaissance editions as well as De Renzi's. Second is that we must be constantly aware that the phenomenon of 12th-century Salerno should not be described as a sudden leap but as the continuation of a long-term development which owes much to Monte Cassino, as well as to the heritage of continuous Latin-Greek exchanges in southern Italy.

Among the Salernitan texts and authors which should be studied anew, free of blind reliance on De Renzi's editions, are those attributed to Trota of Salerno. Monica Green reconstructs the complex picture of the medical works by (and attributed to) this perhaps most famous of all Salernitan heroes, who ignited international attention and represented the phenomenon of the *mulieres salernitanae*. Green's starting point is the great *De aegritudinum curatione*, a compilation by a 12th-century Salernitan editor who synthesized bits and pieces of the *Practicae* by seven of Salerno's greatest physicians. Included in this compilation are major portions of Trota's work. This shows her lasting reputation as a contributor to Salernitan practical medicine. Green, who is presently engaged in editing various original versions of Salernitan writings on women's medicine (including the *Practica secundum Trotam*), shows that despite the enigmatic fact that no Salernitan writer cites her by name, Trota was in dialogue with her fellow male practitioners in Salerno (Copho, for example). Her texts are highly practical, far removed from the philosophical speculations that her male peers were developing in Salerno, and evince unrestricted access to the bodies of her female patients. The

[5] At the time of my writing this review, the first text had already been published: see García González 2008.

dating of her texts (possibly within the first half of the 12th century) remains, alas, a subject for speculation.

Laurence Moulinier draws our attention to the unedited *Sinthomata magistri Mauri*, a practical manual on the semiology of urine attributed with great probability to Maurus of Salerno (*ca* 1130–1214), nicknamed *Galienus salernitanus* because among other things of the rich body of commentaries on the *Articella* that he left behind. This text is not included in De Renzi's compilation, which does include Maurus' other clinical treatise dedicated to urine and entitled *Regulae urinarum*. It goes beyond the conventional nosology based on an analysis of urine colors and teaches how the symptoms must be taken into account in an efficient uroscopy. Uroscopy played a major role in 12th-century Salerno. Isaac Israeli's *Liber urinarum* was translated from the Arabic by Constantinus Africanus, and Theophilus' *De urinis* from sixth- or seventh-century Byzantium was possibly translated in Salerno. Maurus' role as a theoretician and practitioner of Salernitan uroscopy is attested by his pivotal position in the Salernitan *quaestiones* dealing with urine (11 of 13 such *quaestiones* cite him, not Isaac) and eternalized for future generation in the verses of his disciple Giles of Corbeil. A detailed codicological examination of the entire body of Maurus' treatises on urine is thus necessary to reassess Salerno's role in disseminating theories and practices of uroscopy.

Marilyn Nicoud lays the foundation for a critical edition of the versed, emblematic *Regimen sanitatis salernitanum* and a study of its presumed Salernitan origins. This treatise, the authorship of which is still debated, was widely diffused—there are over 100 manuscripts, transmitting manifold versions—and thus is historically significant.

Bruno Laurioux introduces the *Summula de preparatione ciborum et potuum infirmorum*, a unique treatise linking medicine and cookery, food and therapy, and attributed by several of the 15 manuscripts preserving it to the Salernitan Petrus Musandinus, active in the late 12th century. Here too the unsatisfactory edition of a version of the treatise in De Renzi's compilation is a severe obstacle to a proper historical appreciation of this important text.

A substantial number of the articles are devoted to Salernitan treatises dealing with *pharmacopoeia*. This is hardly a coincidence, given that this field served as a continuous source for Salernitan fame throughout the Middle Ages. Mireille Ausécache discusses the

authorship and the contents of the 12th-century *Liber iste* (a compilation of medicines and their recipes extracted from previous books of *pharmacopoeia*) attributed to Matthaeus Platearius. She presents two versions of the text, determines which is probably the original and thus the proper basis for an edition, and reopens the debate on its author's identity. In doing so she lays the foundations for the production of a critical edition of this treatise, which is preserved in some 33 manuscripts. Similarly, Corinna Bottiglieri prepares the ground for an edition of the *Liber* or *Opus pandectarum medicinae* compiled by Matteo Silvatico, who practiced and taught medicine in Salerno in the first half of the 14th-century. The *Pandectae* mark the fruition of intensive Salernitan interest in *pharmacopoeia* over the preceding two centuries. This immense dictionary of *materia medica* was preserved in at least 14 manuscripts. From mid 14th-century Montpellier comes the anonymous *Summa medicinae*[6] which is studied here by M. Jesús Pérez and Cristina de la Rosa. They show convincingly that among the sources of the text one can detect long and accurate citations from Salernitan texts. Specifically, sections from the *Anatomia porci* by the Salernitan Copho and from the *Alphita* are cited verbatim in the author's chapters on anatomy and simples. Thus, Salernitan texts remained useful 200 years after their composition, and their later life after the 11th- and 12th-centuries and in various academic contexts demands further study. Such later treatises may shed intriguing light on the form of the original Salernitan texts, and highlight their diffusion and impact over time.

Iolanda Ventura studies the readership, the later diffusion, and consequent reception of *Circa instans*, a famous pharmacological compilation by an anonymous Salernitan author of the third quarter of the 12th century, which encompasses both theoretical and practical pharmacological data. The text, deriving most of its substances from Dioscorides and the Constantinian textual tradition available in Salerno during the first half of the century, was organized according to some 250 alphabetically arranged headings describing the therapeutic property of simples emerging from plants, animal bodies, or minerals. It enjoyed fast and copious diffusion beyond the Salernitan *milieu* in France and Germany. Ventura uncovers the complex

[6] This treatise has been attributed to Aranu de Vilanova, since it relies heavily on his *Speculum medicinae*.

codicological tradition of the text, and its two fundamental versions (the earlier shorter one, and the longer one which emerged towards the end of the 13th century and ranges through some 480 headings), and lists guidelines for an edition of such a text.

Another group of papers highlights the Greek-Latin nexus in the story of Salerno. Charles Burnett discusses the possibility of a concomitant 11th-century Salernitan translation from Greek and Arabic of Nemesius' *On the Nature of Man* as well as of a 12th-century Salernitan translation from the Greek of Hippocrates' *On the Nature of Man.* He studies the treatise entitled *Epistola Ypocratis de elementis* and identified as the chapter on the elements in Nemesius' *On the Nature of Man.* Then, he traces the rich Salernitan tradition starting from the late 11th century of citing from or referring to Hippocrates' elemental approach in *On the Nature of Man.* Attached as an appendix to the article is an edition of the chapter on elements from Constantinus Africanus' *Pantegni* that quotes from the Hippocratic text even more profusely and became the most important source of Hippocratic elemental theory for Western thinkers in the 12th century. From this paper, a *Civitas Hippocratica* emerges, with a vibrant, continuous, and ever-growing interest in Hippocratic ideas and direct access to Hippocratic texts from the late 11th-century on.

Anna Maria Ieraci Bio shows the infiltration of Salernitan gynecological knowledge, terms, and modes of expression into Byzantine medical treatises and discourse (more specifically, the *Dynameron* of Nicola Mirepso and an unedited *quaestio* linking coitus and leprosy). This suggests close links between the three different cultures (Greek, Arab, Latin) in southern Italy and the Salernitan school, and a greater need to explore them and to include them in the story of the school of Salerno.

M. Cruz Herrero Ingelmo and Enrique Montero Cartelle show how in translating medical treatises from Greek, Salernitan users created new Latin terms based on the original Greek term which was rendered intelligible by assimilating it etymologically to a Latin word with a similar sound.

Irene Caiazzo and Faith Wallis attempt to remove some of the obscurity which surrounds the origin, the stages of the formation, and the motives underlying the creation and dissemination of the *Ars medica* in 12th-century Salerno. Caiazzo introduces a hitherto unedited

commentary on Joannitius' *Isagoge* (in Paris BnF, MS lat. 554) whose provenance is Saint-Martial in Limoges and which is clearly linked to the Salernitan tradition, but in a most surprising way. She cautiously suggests that the commentaries in the famous Chartres and Digby manuscripts (regarded as the earliest products of 12th-century Salernitan exegesis on the *Articella*) seem to follow and elaborate this shorter commentary, and not the reverse. If confirmed, this finding will transform the narrative of the exegetical output of 12th-century Salerno which has hitherto relied on the chronological priority of the 'Chartres' and 'Digby' commentaries. Furthermore, it will add more substance to the debate on the role of northern Europe in initiating 'Salernitan' ideas and approaches, not just disseminating them. The paper thus highlights the urgent need to prepare critical editions of the key Salernitan exegetic output.

Faith Wallis studies the *Articella* commentaries by Bartholomaeus of Salerno, who belongs to the first generation of identifiable masters teaching the *Articella* and under whose name appeared around 1175 the first full set of commentaries on the six-book compilation. By placing the *Tegni* after the *Isagoge*, Bartholomaeus may have tried (unsuccessfully) to reorganize the *Articella* to make the *Isagoge* an introduction to Galen. These commentaries acquired great acclaim in northern Europe, to judge by the number and provenance of the manuscripts which contain them. To reassess Bartholomaeus' common image as a principal agent of the shift in Salernitan medicine to its reinvention as *physica* or natural philosophy, Wallis focuses on Bartholomaeus' commentary on Hippocrates' *Aphorismi* and compares it with the 'Digby' gloss. Bartholomaeus appears as an innovator in reinventing medical practice as an locus of disinterested benevolence or common utility worthy of a philosopher, and in furthering the theoretical turn in medicine. He presents it as an academic discipline to be studied in a disinterested way as an end in itself, hence free of market and profit considerations.

Finally, two papers tackle the wider cultural and political contexts of Salerno. Piero Morpurgo describes the broader setting of 12th-century Salerno as a major meeting place in a European network of traveling men of science, political agents, and clerics. The Salerno

phenomenon is not only about medical men interacting among themselves to produce a new medicine. It must be studied in the larger political and institutional setting of southern Italy (the relationship between the Norman kingdom of Sicily and the powers to its north, the papacy, France, and England) as well as in the more extensive bookish culture of the Byzantine world, southern Italy, and northern Europe. Situated on the land-route that connected northern Europe and the kingdom of Sicily, Salerno was a conventional stopover for kings and pontiffs, whose entourages provided a natural setting for cultural, not only political, encounters. Courts and *curia* made Salerno a hub, and disseminated the medical ideas and terms it created well beyond the medical and scientific *milieu*. It is, thus, necessary to check the infiltration of Salernitan knowledge and terms into non-medical texts, namely, literature, poetry, chronicles, and encyclopedias.

Agostino Paravicini Bagliani shows the substantial impact of Salernitan ideas, texts, theories, and individual physicians on the way people associated with the papal *curia* around 1200 discoursed about the human body and took care of it. It is significant that Giovanni Castellomata, the first person to hold the title *medicus papae*, thus launching a long tradition of the new office of papal physician, was evidently a member of a distinguished Salernitan family. He served Innocent III, and was later associated with the first treatise dedicated to the delay of old age. Gregorio da Montesacro (d. 1239) was the author of an encyclopedic poem entitled *De hominum deificatione*, which drew on the 12th-century Salernitan Dioscorides for its botanical part (rather than the old sixth-century Latin translation of Dioscorides, which was still in circulation) and also relied heavily on Salernitan sources for its medical part. Salerno thus opened the gates to new fields of interest and activity, namely, the *cura corporis* and *prolongatio vitae* which became central in papal circles from the pontificate of Innocent III onward.

This fine and rich collection of essays by great experts in the history of medicine and science and the history of medieval southern Italy shows how far we are from a real understanding of the phenomenon of Salerno, and how much hard work is needed to construct an accurate picture of the texts and people responsible for the flourishing of medicine there. The amount of primary work still to be done is overwhelming indeed. But, at the same time, it creates real hope that the impetus to produce working editions and the paths charted

by many of the articles in this collection will open the way to a new, and more accurate story of the school of Salerno and its impact on medieval medicine and science. Any expert interested in the story of the medical school at Salerno will act wisely if he or she first delves into this book before leaping into his or her own specific topic. One may now look forward to the forthcoming books in this series with great anticipation.

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*The Oxford Handbook of Engineering and Technology in the Classical World* edited by J. P. Oleson

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This book was designed 'to survey the role of technology in the Greek and Roman cultures and their respective technological accomplishments, from approximately the eighth century BC through the fifth century AD' [3]. More specifically, contributors were asked to provide critical summaries of what the ancients achieved in particular areas, to chart the development of their technology in these cultures over the period in question, to consider the historiography of important issues in their area, and 'to help put an end to the myth of a 'technological blockage' in the classical cultures' [6]. The book is not meant to be a compendium of all technological procedures, devices, and machines in the classical world [6]; but the title and size raise the expectation that it will serve as an introduction to the field as a whole, suitable both for undergraduate students and academics new to it. Generally that expectation is realized, though, as usual with this sort of volume, the level at which chapters are pitched varies considerably and some contributors stick to their brief better than others.

To what extent does this volume achieve its three principal aims—to summarize what the ancients achieved, to explain how their technologies changed through antiquity, and to explode the myth of technological blockage? The answer turns out to be bound up with how far contributors have engaged with those aims. With 33 chapters over nearly 900 pages, this review would become inordinately long were every contribution to be discussed equally and therefore it, like the volume itself, will be selective in its coverage, though every contribution is acknowledged to some extent. My selection is based on the apparent relevance of the material to *Aestimatio*'s principal audience, historians of science.


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The contributions

The brief introduction offers a general outline of the book and its *raison d'être*, an overview of Greek and Roman attitudes to manufacturing, and some pointers for future work. Contributions originally submitted in German, Italian, and French have wisely been translated into English, thereby making this fraction of continental scholarship more readily accessible to the book's intended audience.

Serafina Cuomo leads the volume with a characteristically assured introduction to the literary sources, to produce one of the best chapters in the book. She proceeds chronologically (in keeping with the developmental aim), which means, concretely, beginning with the inscriptions that survive on the building of the Parthenon (completed in 432 BC). The next sources to receive attention are a pair of plays from later in the fifth century, Sophocles' *Antigone* and Aeschylus' *Prometheus Bound*, which provide insight into public discussion of technology at the time. Papyri provide the material for the next example, Cleon the ἀρχιτέκτων (whose life and work were the focus of Lewis 1986, ch. 2)—the accuracy of his title in Greek, literally, *leader* of builders, becomes apparent here. The first technical handbook that we meet with is next, Philo's *Belopoiica*, which Cuomo astutely characterizes as a

> text [that] provides respectability, both for the discipline of mechanics in general and catapult-building in particular, to which it gives a history and epistemological and mathematical ratification, and also for its author, to whom it offers a platform for his claims and his designs. [23]

The lack of a similar text by his contemporary Archimedes, despite ancient reports that his catapults were 'versatile and impressive', and that he built not just catapults but burning-mirrors, massive ships, and astronomical globes, is rued. Vitruvius' handbook on architecture takes us into the Roman empire, where we also meet Hero, Frontinus, and the *Corpus agrimensorum romanorum*. We are introduced to two more kinds of inscriptional evidence for technology, quasi-legal and commemorative respectively, before Cuomo turns to consider the late antique period, when official edicts required those in possession of certain types of technical knowledge—mechanicians, geometers, and architects—to spend half their time teaching, thus ensuring the future of these skills, in return for tax breaks [29].

Of many fine points made in the course of this chapter, possibly her strongest arises with regard to the myth-debunking aim: *à propos* the *Babylonian Talmud Shabbath* 33b, she says:

> The Roman reaction to dissenting rabbis is appropriate to their degree of technological appreciation: Judah is praised by the authorities, whereas Simeon is condemned to death. Evidently, criticism of the Roman infrastructures is perceived as criticism of the empire. The identification of Rome with its forums, baths, and bridges appears complete, and puts a dent in interpretations that see technological achievements, ancient or modern, as 'objective' or 'neutral'. Roman technological achievements were arguably meant, and arguably perceived, as politically charged. [27]

One is reminded of possibly the funniest episode (and sharpest observation) in Monty Python's *Life of Brian*: the range and length of the insurgents' answer to the question 'What have the Romans done for us?'

Roger Ulrich gives an overview of the range and scope of pictorial representations of technology on a variety of ancient media; and tabulates the results, achieving the aim of a critical summary splendidly. This chapter is a careful, comprehensive, and sensible discussion of the issues. The problems for anyone using this sort of evidence may be illustrated by the fact that a second century AD bronze statuette from Trier here identified as a ploughman (Fig. 2.13, and cited again in chapter 7 on agriculture) bears a striking resemblance to a bronze statuette from Athens that is some 500 years older and there identified as a Hermes figurine [acq. no. 13219: see my Figure 1]. Incidentally, we may have, represented in the principal difference between these statuettes, a Germanic innovation in clothing: the Trier figure wears what appears to be a *hooded* leather cape—the caracalla after which the emperor who adopted it was nicknamed?—whilst the Athens figure has a hat.

Kevin Greene provides an excellent survey of the historiography of ancient technology studies in chapter 3—which this reviewer found especially interesting on the 18th century—before he goes on to discuss some current trends in technology studies and classic works based on more recent periods, e.g., Bijker, Hughes and Pinch on

Figure 1

*The Social Construction of Technology* [1987], and Edgerton's eclectic theses and technologies-in-use concept [1999]. This is one of the few places in the book where one finds reference to, or awareness of, technology studies as a discipline in its own right from which classicists and classical archaeologists could learn much. Add now Edgerton's insights on the importance of old and creole technologies, put forward in *The Shock of the Old* [2006].

Paul Craddock opens the section on extractive industries with a clear and concise high-level description of the sources of ancient ores, mining technology, and mine organization, and also of ore dressing, smelting, and refining. Differences in treatment required by different metals are indicated, as are the methods used to reconstruct ancient practice and the areas of current debate. To fit all mining and metals processing into one chapter is very demanding and requires a degree of compression and generalization that is generally excellent but occasionally has unfortunate consequences. The description of the cupellation of silver [104], for example, is seriously misleading. Note too that the reference is wrong—it should be Pliny *Hist. nat.* 33.95 not 34.159.[1]

Craddock gives a very clear and concise explanation of ancient ideas about metals [106–107] that is relevant to anyone with an interest in alchemy: the ancients thought that metals grew in the ground; that the properties of metals [like plants] therefore varied with the environment in which they grew; and thus, for example, that gold from one place was not exactly the same as gold from another. Moreover, a variety of golds were all considered as gold *simpliciter* rather than as alloyed gold. They did not have the modern notion of elements with precise and invariant properties. But the invention and use of coinage generated a need for standards of purity, and refining and then other metallurgical procedures were viewed as rapid reproduction of natural processes. A striking example of the chronological lag that can occur between archaeological and literary sources arises with respect to brass. As Craddock explains, brass rapidly gained in popularity and became the regular material for certain types of artifact in the first century BC [110]. Yet the earliest surviving literary account of its manufacture (al-Hamdānī's) is from the 10th century AD [111].

[1] For a correct explanation of the Pliny passage, see Craddock 1995, 223 or Rihll 2001, 122–124.

Örjan Wikander deals with energy sources and power supply, covering direct solar energy, chemical energy, animal power, water power, and wind power. Solar energy was widely used for heating and drying. Chemical energy was the principal type employed in manufacturing as fuel for ovens, kilns and furnaces; although charcoal was the principal fuel in antiquity, coal was also used, especially in Britain, especially in the second century [139]. Animal power was largely confined to agriculture and water lifting [140]; overland haulage should be included. Wikander emphasizes the particularly provisional nature of current hypotheses on water power since they are so dependent on archaeological evidence, which is constantly growing: more finds of pre-medieval water mills have been published since the 1980s than the total number that were known to exist before 1980 [141]. Moreover, water power was exploited for other industrial uses too, such as sawing stone. As Wikander notes, we may not yet understand the 'true economic importance of water power in antiquity' [152]. Two genuine turbine water mills excavated in a late third/early fourth century context in Tunisia demonstrate a level of sophistication in milling technology that was not reached again till the 16th century [145]. Consequently, Bloch's hypothesis [1935] that the breakthrough in water mill technology occurred in the early Middle Ages, which was based only on 'the first *documentation* of a situation already established in the Roman Empire' (emphasis added), is demonstrably wrong [149]; it occurred in the first century AD or slightly earlier. Wind power was widely exploited for sailing and threshing, and is mentioned by Hero [*Pneum.* 1.43] in connection with driving an organ, but remains a 'dark horse in the field of ancient energy exploitation' [153].

In one of the longest and most densely informative chapters in the book [175–222], Geoffrey Kron emphasizes the positive transformation of modern views about ancient animal husbandry over the last generation, largely as a result of archaeo-zoological research in tandem with the classical literary sources. For example, domesticated animal bones reveal that they were bred and fed to be consistently larger in classical times than in other periods before or after, un-

til the 19th century AD.[2] It is noteworthy that ancient agronomists' advice differs little from contemporary organic practice, and that most of the veterinary surgical procedures and instruments being employed in the mid-20th century were known to, and used by, the ancients [175, 185]. The scale and sophistication of commercial fish and game farming would not be matched again till the late 20th century [176, 192, 204, 205, 212–213], so too sheepfolds on the Crau and pigpens [183]. Intensification methods, like getting two litters from sows per year [181] and battery farming of poultry, were almost modern [177], while Roman hens were typically about 25% heavier than those found on Celtic sites [180]. Pigeon coops for 1,000 breeding pairs [192 and Fig.8.3] point to areas where the Romans still have no equal. More controversially, Kron suggests that, where the terrain allowed it, the ancients employed convertible husbandry, the most intensive and dominant form of mixed farming today [181–182].

Part 3 opens with Frederick Cooper's argument that the genius of Greek architecture lies in the engineering rather than the appearance, and that a deep appreciation of the properties of various building materials and a theory of construction which could cope with earthquakes were more important than proportion and the other things with which students of Greek architecture typically concern themselves [226–227]. The focus is on temples. He suggests that Theophrastus *Inquiry into Plants* 'contains all the earmarks of a modern-day handbook on wood construction' [226], a remark which I find bizarre. He makes a number of assertions that need references but lack them, such as that the buildings in the worst shape now were demolished in the late antique period to recover their metal clamps and dowels [230]; and he assigns intention without apparently considering accident or coincidence, for example, when discussing the aseismic properties of a mat foundation [230].

With regard to the editor's aims, I found this to be one of the least satisfying chapters of the volume. As a research paper it also failed to persuade me: the case for 'a body of scientific theory... behind the applications of scientific technology to building' [226] requires substantiation way beyond Theophrastus' appreciation

[2] This is true of both Greek and Roman domesticated animals [176]. Typically, the animals were up to 20% taller at the withers than Iron Age or medieval specimens, for example [180].

of the properties and proper treatment of various woods (on which see Ulrich's discussion, 448-450) and three inscriptions, whose interpretation by Cooper is very significantly more far-reaching than the surviving texts [250]. There are two issues here: practice *versus* theory and Theophrastus' working methods. The first requires proper analysis [cf. Rihll and Tucker 2002] and the second needs to be seen in the context of data gathering at the time: that Theophrastus learned about woods by talking to people who worked with wood (there was no other source of such knowledge then) does not demonstrate a scientific approach to construction in wood any more than Aristotle's talking to beekeepers demonstrates a scientific approach to honey production [cf. Beavis 1998]. Nor is it relevant that recent American practice lacked awareness possessed by the ancients [249]. Cooper ignores the crucial difference between modern construction handbooks and Theophrastus' account: mathematics and measurement, which is conspicuous by its absence in ancient handbooks and by its presence in modern ones. Moreover, as the next chapter demonstrates very clearly, the putative aseismic design principles and practice that supposedly explain Greek column construction by drums were ignored by the Romans, who preferred monolithic columns and developed the technology to cut, move, and lift these massive shafts.[3]

This takes us to the chapter on Roman engineering and construction by Lynne Lancaster, which offers an excellent overview of the topic and can be confidently recommended to students, unlike that on Greek engineering and construction. Lancaster has a very different view of ancient construction theory and practice, most of which cannot be attributed to differences between Greek and Roman practice; and it is a pity that these two chapters are not explicitly cross-referenced and the disagreements explored. Lancaster offers an excellent example of a technological development which was an improvement in one respect (speed of completion) whilst a decline in others (less stable, less durable), and which was recognized as such at the time [262]. This technology (*opus reticulatum*—building a wall using stones cut to the same size and shape) can also be seen now to have saved the wall builder the time that would hitherto have been

[3] Compare also Roman foundations and wall compartmentalization [259, 265f].

spent choosing a suitable stone for each space as he built, and separating the job of shaping the stone from laying the wall, thus allowing the former to continue off site and round the clock and speeding up production as well. The cost of this development was the liability that the wall would crack along the diagonal and its reduced durability in comparison with the 'crazy paving' type walling that preceded it (*opus incertum*). Thus, Lancaster's account is sensitive to the compromising nature of most technological developments and to the role of organization as well as of materials and tools. This chapter is also particularly good on the variety that existed in the detail of solutions to particular problems, and on the diffusion of materials and techniques (such as those for vaulting) from periphery to core and thence or directly to other peripheries [266–278].

Andrew Wilson supplies a comprehensive, reliable, and up-to-date survey of hydraulic engineering, covering wells, cisterns, aqueducts, urban distribution and uses, irrigation, and waste water management, to which it is essential to add only Smith's explanation [2007] of the routing, *via* high points within the depression being crossed, of some of the so-called inverted siphons. (Smith shows that this was probably done not in order to reduce the length run at maximum pressure [*pace* 297] but to facilitate filling and maintenance: air-locks can be a real problem in this sort of system, and relative high points facilitate bleeding.) To Wilson's list of possible factors for the proliferation of aqueduct technology from Augustus' time onward (increased prosperity, spread of the bathing habit, and export of the Roman urban model *via* the foundation of veteran colonies in the provinces [298]) we should add, emphatically, 'peace'. This chapter is sprinkled with numbers that really help the reader grasp the scale and variety of the enterprise: falls on aqueducts vary between 0.07 m and 16.4 m per kilometer, roofed cisterns had a capacity up to 50,000 $m^3$ (that is about four times larger than the Piscina Mirabile in Bacoli, for those who know that remarkable structure), and there were 591 street fountains in Rome of the first century AD, for example.

Wilson's second of three contributions is on Greek and Roman machines. Although this opens with a clear and accurate definition of a machine and a listing of simple machines, he subconsciously equates machine with complex machine when he says that 'the use of machines in manufacture was relatively limited', citing the loom and water-powered devices as exceptions [337–338]. Since most tools are,

strictly speaking, simple machines, they are actually everywhere in ancient manufacturing; thus, for example, potters use wheels, wood- and stone- and leather-workers use wedges galore in a variety of chisels and blades, metal-workers use levers (tongs), and so on. It is all too easy to overlook the fact that many basic and not-so-basic tools (machines) such as the carpenter's plane [446] were apparently invented and certainly developed by the Greeks and Romans. Unfortunately it has been overlooked here, which does not help the book's aim to help put an end to the myth of technological blockage. This is all the more surprising given that Wilson's section on simple machines [339–342] is excellent, and that during the course of the chapter he notes the earliest evidence for a variety of devices—e.g., the compound pulley, the winch, the gear, the rack-and-pinion, the worm gear, and so forth—thus implicitly or explicitly recognizing the probable Greek origin of a host of simple gizmos which formed the basis of all tool kits since, and which transformed people's ability to apply power to things and to harness natural forces like gravity, wind, and water. The Greeks also combined them to make complex machines of even greater power. For example, two of these simple devices, the pulley and the winch, were combined to produce a very important Greek invention, the crane; and this prompted the invention of the (anachronistically named) Lewis bolt. It is not clear on what basis the selection of complex machines has been made; it certainly illustrates range and diversity. There is discussion of cranes, traction devices (for reducing fractures and other medical applications), and engines of war—it is in this chapter, rather than the chapters on warfare, that we find the most detailed discussion of catapults [346–350]—water-lifting devices, water-powered mills and other applications, unusual types of transport such as hodometers and paddlewheel boats, presses, and machines to entertain. Here the most important statement for the volume's aim to debunk the myth of technological blockage is that 'the archaeological (as opposed to documentary) evidence for water-mills and millstones also appears scarcely less abundant for the Roman than for the high medieval period' [362].

Robert Curtis' chapter on food processing and preparation concentrates heavily on the Mediterranean triad of cereals, grapes, and olives. This would have benefited from excision of mechanical material (water mills and various presses) that was already done well in chapters 6, 11, and 13. Some of the space thus saved could have been

used for an account of food processing—such as ways of cooking other than baking in a large oven—preservation, and storage techniques here omitted or for a fuller discussion of the other fruits, vegetables, and nuts eaten [384] or even for a discussion of fast food in antiquity. His observation that modern butchers use almost identical tools as did the Greeks and Romans [385] renders his use of precisely this trade as an example of the ancients' 'persistent conservatism' [388] rather bizarre. How conservative does that make modern butchers?! This is how the myth of technological blockage lives on, even in a volume that aims to explode it, and in spite of the evidence against it. The explanation for butchers' conservatism from ancient times to the present is rather that technologies have peaks and that once reached they cannot be appreciably exceeded except by a new technology (what Lienhard 2006 calls 'completed'). The cannon is not a catapult; the car is not a chariot. The tools in a modern butcher's shop are similar to their classical counterparts because the technology of hand butchery peaked early. Modern society has developed a new technology, the *abattoir*, which co-exists with the butcher's shop now, and which retains some ancient hand butchery tools but also includes devices for which there is no ancient version.[4]

We then have a third chapter by Wilson, this time on large-scale manufacturing, standardization, and trade. He emphasizes the interrelationships with the economy writ large, with real growth in productivity, and with mechanization. The discussion focuses on the mass- or large-scale production of pottery, bricks, and foodstuffs, the standardization of the marble trade, and the division of labour in a large bakery and in an imperial marble workshop—all of the Roman period. It is a pity that mining and metallurgy are omitted, since interesting things could have been said on all these themes and the reader could have been introduced to some Greek material too by discussing the Laurion silver mines (large-scale production), for example, or the production of bronze statues (standardization, see Mattusch, next chapter, esp. 426–431). Wilson writes with characteristic clarity and sprinkles revealing numbers throughout: he mentions, for example, the 12 potters and 30,000+ vessels attested in a single kiln firing [398], the 6.9 million bricks used in the baths of Caracalla

[4] The same or similar continuity is visible with many other ancient technologies; see, e.g., woodworking on 440, 446, 460.

[402], and the 466 $m^3$ of space devoted to gutting and salting fish at Plomarc'h in Brittany [411]. The throw-away suggestion that some *pueri* at La Graufesenque were slaves 'or perhaps apprentices' [398] is unfortunate, and appears to be innocent of the evidence for apprenticeships in the ancient world: an apprentice potter would be unique even in imperial Roman Egypt, which is the only place where apprenticeships are yet attested, and most of them concern weavers (see below). Indeed, this sort of anachronistic assumption about the organization of large-scale manufacturing, which also underlies Peacock's typology of manufacturing establishments (described by Wilson in his introduction [396]), creates difficulties for the interpretation of the Roman mass-production pottery facilities in France and Italy. Wilson astutely observes that the documentary evidence from these places (lists of vessels for firing, potters stamps, and so on) indicates that we are not dealing with employees. Rather, the landowners on whose properties these impressive facilities were developed may have either engaged the potters to produce a given number of vessels or rented space to them [400]. In either case, the potters are independent craftsmen, not employees in big ceramic production units. This is important.

John Wild writes authoritatively and concisely on textiles, covering Greek and Roman production with equal facility. He proceeds systematically from types of fiber exploited to dyeing. A sharp difference between ancient and later practice is observed with regard to the production of cloth: except for sailcloth, the ancients did not produce bolts of cloth but individual pieces that required little or no cutting and sewing [470–471]. Regional diversity, such as cut-loop pile in eastern Roman textiles, is noted [472–474]. One of Wild's last observations has the power to shock and ought to prompt students to more sophisticated thinking about hand- versus machine-made production: 'only an expert can spot the difference between a Greek or Roman textile and its modern equivalent' [477].

In a densely informative chapter on glass production, Marianne Stern suggests that the ancient Greek philosophers' association of glass with metals arose as a result of their familiarity with glass working only, not glass making, so that they did not see this manufacturing process as the true transmutation of materials that it is [521]. In light of that and of ancient notions about material composition (see above on Craddock's chapter), the issue of 'counterfeiting'

would bear re-evaluation [527–528]. (Glass making and glass working were two separate crafts, and glass making was undertaken in only a very few places [520].) Colorless glass features in a variety of ancient devices and experiments, such as Aristophanes' burning glass and Ptolemy's experiments on refraction [528–529]. The notorious ancient anecdote about unbreakable glass is explained—a vessel being blown was perhaps dropped on the floor, where it might 'dent' but it would not shatter; and it could be picked up, reheated, and restored to its former shape [535]. There is a particularly interesting case of technology transfer between crafts in the ancient glassworkers' employment of wheels (like potters' wheels), a technique since lost [532–535: see also 540]. The remainder of the chapter is divided into sections covering primary and secondary workshops, glassmaking, the working properties of glass, colored glasses, colorless glass, glassworking in classical Greece, glass pottery, glassblowing, other decorative techniques; Stern concludes with a section on the scale of glassblower's establishments and outputs.

Part 5 concerns transportation and the relevant infrastructure, first land, then sea. Lorenzo Quilici deals with (Roman and Italian) roads and bridges, while Georges Raepsaet deals with what moved on them in a more theoretical as well as a historically more wide-ranging way. Technology transfer is raised again with respect to viaducts and aqueducts, gates and arches [569, 570]. But this time it seems inappropriate because, although these engineering projects may be 'very different types' by modern standards, they were not by ancient standards, and because these projects were carried out by the same personnel then. Raepsaet has to deal with one of the landmark publications in the 'technological blockage' thesis, Lefébvre des Noëttes 1931, and he does it well. The historiography is briefly given and the author's own position clearly stated—land transport technologies were neither insignificant nor marginal [580–581, 590–591]; and the rest of the chapter substantiates his position through sections on the mechanics of forces and potential energy, on general categories of portage and harnessed transport, and on customs, context, and cost. The relative strengths and weaknesses, physical and economic, of a variety of draft animals in a variety of roles are compared. Indeed, Raepsaet constantly emphasizes the existence of that variety: see, e.g.,

> This stability [of sources of energy available until the 19th century] did not stand in the way of either a great diversity of vehicles and harnesses or multiple forms of progress, innovations, and adaptations to the needs encountered in each type of society or preindustrial environment. [589]
>
> Both the fixed and the turning axle coexisted, their contemporaneity more a question of quality of workmanship than of chronological evolution. [598]

This surety of touch does not unfortunately extend to economic issues [601], and the comments about distribution of goods should be tempered with Parker's account [2008, esp. 178–183]. In view of the importance of Noëttes' ideas about ancient traction to the myth-debunking aim of the book, it is not surprising that most of the chapter is focussed on vehicles and harnessing. But that leaves little room for porterage and packsaddle, the fundamental importance of which is stressed [589–590] if not much discussed and barely illustrated. See my Figure 2, which shows the sort of structures employed, in this case in ancient Greece, where to make a child's toy of it the burden-bearing ox is fitted with four wheels!

Seàn McGrail cautiously discusses the methodological issues attending the study of ancient ship design, construction, and use. Methodology is a live issue because nautical archaeology is a relatively young subject. It is young because ancient written sources on the subject are almost non-existent, and because excavation or even study of known ancient wrecks generally requires the sort of equipment that has become available only recently (especially in the last 20 years or so). For example, sponge divers could bring up much of the contents of the Antikythera wreck *ca* 1900 AD, but study of the remains of the ship itself on the seabed was not begun until 1953 by J.-Y. Cousteau [see Moity, Rudel, and Wurst 2003, 127]. McGrail emphasizes that reconstructions and replicas are sometimes constructed on a rather small and uncertain evidential base using unexpressed assumptions, and that once built they can represent an impediment to understanding instead of an aid [612–613].[5] This is all very sound, and anyone with experience of reconstructions of any type of ancient machine or

[5] There are good color photos of what actually remains of some larger wrecks and of the practice of underwater excavation in Moity, Rudel, and Wurst 2003.

Figure 2


device will know how, despite the best intentions and with everyone in a multi-disciplinary team trying to make a genuine reconstruction using their combined talents, when ancient evidence and modern mechanics clash, the ancient evidence is usually ignored, with the result that the supposed reconstruction is neither what was built in the past nor what would be built today. McGrail's real interest is in hulls (the discussion of sails is very thin and omits topsails, depicted in Fig. 25.1) and in NW Europe rather than Greece and Rome. This chapter is highly technical; the glossary is essential and unfortunately

incomplete—the reader insufficiently familiar with ship bits might require some additions (e.g., What is a hogging hawser? A stringer?). A couple of well labelled diagrams would have been helpful as well.

Part 6, entitled 'Technologies of Death', meaning warfare, consists of just two short chapters, despite warfare's being 'the most innovative and pervasive human technology from at least the Early Bronze Age through the present day', and despite 'the importance of the subject and the richness of the literary and archaeological evidence' [7]. The editor excuses this brevity—this section is only 38 pages long; compare 75 on sources, 103 on technologies of the mind, 119 on transport, 129 on primary technologies, 141 on engineering, and 178 on manufacturing—by referring to the extensive existing literature on warfare. But that is a weak excuse, for it does not explain the prominence given in the volume to mining and metallurgy, Roman engineering, and hydraulic engineering, for example, all of which also have extensive literatures. The truth is rather that, although warfare and fortification do have an extensive literature, relatively little of it produced in the last 30 years has focused on the technology and engineering involved, a fact reflected also in the contemporary clutch of *Companions* to ancient warfare. Here perhaps is the most striking demonstration that the recent historiography of a topic does not just form part of a chapter's content (consideration of which was one of the volume's aims) but to large extent *determines* its content. Here too the reviewer should declare an interest, having published in 2007 a 400-page monograph on the history of the catapult, the first in English for 30 years and a topic chosen precisely because of its technological significance as well as for its rich literary and archaeological estate.

The Greek chapter is a routine overview of Greek armor, weapons, and fighting style that tells the reader very little about technological matters. We are told, for example, that this or that group favored this or that type of bronze, iron, linen, or leather armor; but we are not told about the properties or performance of these materials in this role, which could (would) have contributed to an explanation of the choices. Similarly, we are told about changes in fortifications 'in response to improved assault techniques' [685], and that the advantage moved from defense to attack in the latter part of the fourth century BC [684]. But next to nothing is said about those techniques or how this remarkable change was achieved. Bizarrely,

Figure 3


the catapult, which was invented in Syracuse around 400 BC, is mentioned first in passing, in a paragraph on infantry training in the third century BC [682], and next as a component of 'effective siege trains, with... numerous powerful, stone-throwing torsion catapults' [685]. The one paragraph focussed on catapults [688] is inconsequential and wrong on the earliest evidence for torsion catapults [see Rihll 2007, 78–80]. The beginnings of mechanized warfare are thus skipped over; and the most complex mechanical technology in routine use across the length and breadth of the ancient world is not even described. The aims of the book are frustrated badly here, apparently because there was confusion about who was to deal with military technology and in what chapter (see below). Even the choice of photos is unfortunate: Eleutherai's defensive strength at the principal pass into Athens from the north [686–687] is better shown by the approach from the would-be invader's side [see my Figure 3] than by a photo shooting along the wall to show the plains of north Attica south of it [Fig. 26.4].

Some of these deficiencies are made good in Gwyn Davies' chapter on Roman warfare, e.g., in his discussion of the pros and cons of various sorts of body armor [701]. He understood that 'siege engines and related technical aids' [702] were going to be considered elsewhere, specifically, in chapter 13 (Wilson on machines); Greene meanwhile thought that military technology would be considered in these two warfare chapters [810]. Thankfully, Davies gives a brief overview of the origin and development of the catapult anyway [698–699] but then concentrates on fixed structures (fortifications, earthworks, and the like). However, apparently unbeknown to him, the only 'engine of war' considered in chapter 13 is the catapult [346–350], where it appears between 'surgical traction' and 'water-lifting devices'. Siege towers, ram-tortoises, borers, *sambucas*, pontoon bridges, and all the other ancient war technologies have thus fallen between stools. I note in passing that all the most spectacular, and for that reason famous, bridges in antiquity were constructed to facilitate or support military invasions: Xerxes' bridge over the Bosporus, Caesar's bridge over the Rhine, Trajan's (Apollodorus') bridge over the Danube. Given the importance of the military in making and breaking the most famous Greek states (Spartan army, Athenian navy), the kingdom of Macedon (Philip II, Alexander the Great), and the Roman Republic and Empire (rise and fall), and the fact that the military possessed and trained most of the Romans' engineers (e.g., Vitruvius), one would have thought that exploration of the technological capabilities of these armies would be central to this book's project. Instead it seems to be a Cinderella section.

Part 7 takes us into fresh territory, 'Technologies of the Mind', which opens with Willy Clarysse and Katelijn Vandorpe's chapter on writing, book production, and the role of literacy. There are sections on writing, writing materials, roll and codex, book production and the book trade, libraries, record-keeping, and literacy. Here we find an excellent example of how one technology (parchment codex) supersedes another in the same domain (papyrus roll), how traces of the old may survive in the new, and the perseverance of habits even when their *raison d'être* has long gone [719–724]. Papyrus began to be used as a writing material in Old Kingdom Egypt, and the standard papyrus roll was about three and a half meters long—long enough for a single Greek tragedy, for example. Sticking many rolls together produced an unwieldy object (up to 20 m!); so large

works were typically divided across multiple rolls ('books'). The original codex, which goes back to the Assyrians, was a book of wooden sheets. Parchment was invented in the city from which it derives its name, Pergamon in Anatolia, sometime in the Hellenistic period when access to Egyptian papyrus was denied during the wars between Alexander's successors. The book as we know it combined the codex format with the parchment material but retained some of the habits of writing on papyrus scrolls, such as the multiple narrow column format on the page, so that the open book looked like the open scroll. In fact, what we now call 'front matter' was at the end because it was left to the next reader to rewind the scroll; thus, the end was inevitably the bit that they saw first. (Some nations, e.g., the French, still put the title at the end of the book.) This wonderful discussion also makes some important points relevant to notions of scholarship and plagiarism: the codex allowed for easier and more precise referencing than did the papyrus roll, and pagination in codices is more common than numbered columns in rolls; but, since every book prior to printing was an individual handcopy, pagination was not a reliable means for referencing [724]. The result was referencing by numbered paragraphs (as in religious, legal, and other texts) or lines (as in poetic works), which worked whatever the medium or handwriting in use.

Robert Hannah's chapter on timekeeping provides a clear and concise guide to the topic and is particularly relevant for students of ancient astronomy in that it describes and contextualizes the known technology associated with daily and seasonal observation and timekeeping. There are sections on *parapegmata*, which he glosses as almanacs rather than calendars [742], the Antikythera mechanism, sundials, hours, portable dials, and waterclocks. The discussion of the Antikythera mechanism [744–746, with a photo of the Wright reconstruction], was unfortunately outdated on publication, thanks to dramatic recent discoveries and developments concerning it (see below). Hannah himself will no doubt be fully conscious of this, and one would hope that material can be added before the book appears in paperback. He concludes with a caution against the interpretation of ancient instruments without the fundamental understanding that comes from careful study of the objects themselves [754].

The next chapter, entitled 'Technologies of Calculation', is another that one would expect to be of special relevance to historians of science. This chapter is in three parts: Charlotte Wikander writes

on weights and measures, Andrew Meadows writes on coinage, and Karin Tybjerg writes on practical mathematics. It is noted that the precision of weights was not good in ordinary contexts: finds both at Athens (in the agora) and Olympia (in a ritual context) suggest that there was variation of up to 20% [765]—though there are huge methodological problems attending analysis of ancient weights and these should not be considered acceptable tolerances. The significance of measurement for ancient cultural achievements is indicated [768], but this section could serve equally well as an ordinary *Companion* entry, as there is little attention either to the technological aspects or to the less ordinary acts of measurement in antiquity—how, for example, Archimedes measured the weight (or volume) of the wreath that Hieron commissioned or how finely and accurately the beam of a typical Roman unequal arm balance (steelyard) was calibrated. More interaction between Wikander and Tybjerg would have paid dividends because, at the end of the day, much practical mathematics was concerned with measurement. Finding ways to measure—i.e., attach numbers to—natural and manmade phenomena has been a key task in many scientific stories,[6] and it would have been good to have some discussion of this, even if only to note its apparent absence in most areas. That some of what now appears to be 'pure math' had a practical application or even origin is emphasized [e.g., on 782]; but Tybjerg does not venture into the more controversial areas such as the relationship, if any, between Archimedes' *Quadrature of the Parabola* and his involvement in the design and construction of the largest ship that the world had then seen, or delve into the connections between his *On Floating Bodies* and allegations about fraudulent goldsmithing in Syracuse of the third century BC.

Örjan Wikander, however, is not afraid to go there when he returns for the chapter on 'gadgets' and scientific instruments, pointing out that Archimedes' *Dimension of the Circle* contains something essential for Archimedes' hodometer, namely, a tolerably accurate value of $\pi$ [796]. This chapter is characteristically concise and solid, and there is a fair amount of debunking of modern myths on ancient automata. On the down side, 'gadget' is not defined and there is some overlap of material with chapter 13. After some background,

[6] Such as that of the measurement of temperature and pressure.

historical and technological, there are sections on automata, water-clocks, astronomical instruments, hodometers, and 'gadgets' in the Roman Empire, which last section draws the reader's attention to some only lightly attested but nevertheless significant examples of Roman high-tech such as the rotating ceiling in Nero's Domus Aurea [797], though a rotating ceiling is definitely not the sort of thing that most people envisage as a 'gadget'. The same contributor briefly discussed the most famous 'gadget' of antiquity, Hero's *aeolipile* ('steam turbine'), in his earlier chapter on sources of power and energy [154], so its omission here is explicable, if unfortunate for readers of this chapter and not that.

My quibbles:

- There is no evidence that any ancient scientist or engineer was employed to work at or in the Museum [786, 787, 790]:[7] this is another modern myth, anachronistic in concept and fact [see Rihll 2009].
- Athenaeus should be credited for citing his sources rather than castigated as 'a notorious name-dropper' [786–787].
- A modern mantra denies Aristotle authorship of the *Mechanical Problems* attributed to him in antiquity, which treatise was handed to some anonymous presumed pupil [787]. The reasons for this view and the chronology need to be re-examined.
- The 'armchair invention' [789] is the last refuge of the stumped scholar: something is only called an armchair invention until someone works out how it worked, or better, builds a reconstruction—Archimedes' hodometer, for example [795].
- I do not understand why devices that entertain are not considered practical [789]. The practical is not confined to mere survival. A very significant chunk of the modern economy is wrapped around the computer games industry, to say nothing of the wider leisure sector.
- While repeating another orthodoxy, Wikander rightly asks, 'If the goal [of certain automata] was educational, why was there so much emphasis on the manifestation of marvels?' [790]. Exactly. These machines are carefully designed to *conceal*, not to *reveal*, their workings [so Greene, 802]. The scholarly idea that they are 'educational aids' transforms a mechanical attention-grabber

[7] The same claim is made by Greene on 805–807.

into a respectable piece of laboratory apparatus—which is most interesting historiographically.

Kevin Greene appears again, now to write on inventors, invention, and attitudes towards technology and innovation. This is obviously a key chapter given the book's aims. It has sections on optimism, pessimism, human ingenuity, ancient perceptions of machines, ingenuity and the status of work, inventions, inventors (five are identified), as well as on stability, continuous development, and stepwise change in antiquity. It is dense with data, contains some excellent ancient sources in translation to demonstrate attitudes, and includes what are often the only mentions anywhere in the book of a variety of technologies, e.g., of musical instruments [812]. But, as in this case, discussion of such items is mostly descriptive and tantalizingly brief. One gets a real impression of the vast and multi-colored mosaic that is ancient manufacturing, but the discussion is untidy (e.g., the concept of technology-in-use is explained and referenced on page 813 although already used in context on page 812).[8] Nor are the components properly marshaled to support an argument. One senses that Greene does not yet have an overarching answer to questions about invention, innovation and change in antiquity [see esp. 815], but that he is still gathering the materials to form an answer; and, given the scale of the enterprise, this is not a failing. I have myself spent almost 20 years accumulating knowledge about ancient technology. Most classicists do not know of the existence of the wood, let alone what number and variety of trees are contained within it. Yet exposure to the trees makes one cautious about generalizing about the wood. Ancient technology and engineering is a young topic, and like most pioneering works, every chapter in this book is destined to be superseded, most sooner rather than later. As Wikander puts it, 'the presentation that follows here may be better founded than its forerunners, but it, too, should be taken for what it is: a working hypothesis' [141].

The last Part, mistitled 'Ancient Technologies in the Modern World', consists of just one chapter. Michael Schiffer closes the volume with a contribution that sits uneasily with the rest and would

[8] This concept has been used by Greene and others earlier in the volume, but that's an editing issue. Still, this is the only chapter in the volume without typographical errors.

be more comfortable in a collection on theoretical archaeology. He offers a manifesto for what he calls an 'expanded ethnoarchaeology' that uses historical sources as well as ethnology to model, i.e., to theorize generally, about artifacts and their use in technological processes. As noticed by the editor [8], this 'simply makes explicit' what many of us do already. The example by which he illustrates his vision is electrical technologies from the recent past—the typical sort of topic and period one finds in the technology studies literature. The applicability, for the book's intended audience, of the methods discussed is recognized as only potential [823 *et pass.* esp. 832] and testing of them is explicitly postponed to the future [826]. A single worked example from before the 18th century would have sold the model more effectively; citing one example from a paper published 21 years ago [830] is no substitute. Some reference to the SCOT (Social Construction of Technology) school, launched by Pinch and Bijker in 1984, and to the classic statement in Bijker, Hughes, and Pinch 1987, would also have been appropriate in the discussion of deliberate non-adoption of a new technology [827], for example, especially since Hughes' work is acknowledged as the catalyst for Schiffer's own [830]. Readers interested in that topic should consult Oudshoorn and Pinch 2003. Some methods explained along the way (life-histories, performance characteristics) that are said to be in use in archaeology look rather positivist by the standards of recent technology studies: see, e.g., Bijker 1995, Bijker and Law 1992, and Edgerton 2006. I venture to suggest that the transfer of the technologies of technology studies between academic disciplines over the decades 1980–2010 would be an interesting historiographical project for someone!

Other chapters of less obvious relevance to readers of *Aestimatio* are interspersed between those discussed above. Clayton Fant writes authoritatively on quarrying and stone-working, paying particular attention to innovations even in this technologically relatively static industry. Evi Margaritis and Martin Jones survey agricultural practices, emphasizing the differences between those followed in the Mediterranean littoral and those followed north of the Alps where soils were typically wetter, heavier, and richer. They draw attention to developments provoked by the organization and management techniques employed by the Romans to extract surplus from imperial territories that had hitherto been populated by more self-contained communities less well connected to trade networks.

Klaus Grewe's chapter on tunnels includes translation and discussion of Nonius Datus' famous inscription and tunnel at Saldae, and the observation that methods of construction were not very different in the 19th century [333], which is relevant to the myth-debunking aim. An explanation of how a tunnel to drain a lake was dug from both ends would have been useful [325–326], particularly of how it was dug at the lake end—by means of a coffer dam? Moreover, we should now include the extraordinary aqueduct tunnel at Gadara [Schulz 2009].[9]

Carol Mattusch concentrates on bronze statue production, on the ground that most metal-working techniques are found here. But it naturally slants her discussion towards art-historical issues. Thus, for example, it is implied that it was 'the usual practice' to construct ancient foundries for a single large commission and then to close them down [434]. Surely this was not the case for those making everyday items such as nails, hobnails, knives, keys, tools, brooches and other accessories, or furniture knobs, handles, and feet? One gets very little sense from this chapter of the range and scope of ancient metal-working, or that 'miscellaneous metalwork', most of it unidentified, fills a significant amount of storage space for most classical excavations.

Ulrich appears for the second time in chapter 17, now writing to great effect on woodworking. He has a section on specialized woodworking tools, and is especially good on the non-obvious but fundamental uses of timber in construction (e.g., in piles and caissons) and on the continued use of old technologies alongside later developed ones.

Self-referencing reaches rarely plumbed depths with Carol van Driel-Murray's contribution, where her own work constitutes fully a third of all references: one could get the impression from the opening paragraph [483] that no one else has or does work on the topic of leather in antiquity.[10] That unattractive feature apart, van Driel-Murray's is a very concise and competent overview of leather produc-

[9] This recently discovered aqueduct has three tunnels of length 1, 11 and an amazing 94 km—which beats the tunnel at Bologna by 75 km.

[10] Schiffer's contribution is even worse in this regard: his self-references amount to almost a half of all his references, though almost half of his papers are co-authored and he does not always appear first in the name list.

tion, and one which could confidently be added to student reading lists.

Kevin Greene appears for a third time, now with Mark Jackson, to write on ceramic production in chapter 20. This is concise and readable, after a long front end focused on the modern reception of ancient pots which, rightly or wrongly, many students of ancient technology will deem irrelevant. The technical discussion is sprinkled with figures attesting to the scale, firstly of the ceramic industry, and secondly of the economies where they were produced and where their contents were consumed: Greene mentions, for example, the estimated 53 million amphorae, most of them made in Spain and shipped to Rome, that went into making Monte Testaccio in Rome [508].

Blackman's chapter on harbor development is the most up-to-date in the volume; indeed, it contains many 'forthcomings', which can be problematic when details have changed by the time an item appears.[11] That a variety of methods were in use simultaneously is apparent again, even within the same project this time, the Claudian harbor at Portus [645].

## The volume itself

There is no attempt to conclude the volume or synthesize the analyses offered by the various contributors, either *in toto* or by section; the volume simply ends disappointingly with Schiffer's superfluous chapter. Cross references are few and slight, and are sometimes lacking even when easy to supply: there is, for example, no effort to let the reader know that a frieze discussed in some detail on 408 (Eurysaces' bakery) is illustrated in part on page 38, or that the sundials mentioned on page 814 are discussed (and illustrated) in a section devoted to them on pages 746–749. On the other hand, when there are cross-references, they can leave the reader confused rather than better informed. For example, the editor should have asked the relevant contributors to address their disagreements about the date of the introduction of the truss or at least to lay out rather better

[11] This has happened with at least one item: see page 668 and my List of Typographical Errors.

the arguments for their own views [cf. 228, 266, 457–459]. Misleading comments (and outdated references) regarding the Antikythera mechanism could have been clarified easily by reading the relevant part of chapter 29 instead of just referring the reader to it.[12] A similar problem arises regarding the codex: compare what's said on page 813 with the discussion on pages 721–724. It is a pity that more effort was not made to make the volume greater than the sum of its parts by providing contributors with copies of relevant other chapters or sections (which is very easy to do and very quick using email) and insisting on greater consistency in terms of what is offered to the reader. As it is, we have a fuzzy assemblage which seems unduly dependent on the initiative of the contributors. Glossaries would be helpful throughout, not just in chapter 24, as would a gazetteer of sites mentioned in the entire volume, not just in chapter 25. The benefits accruing from having all these chapters in the one very large volume (even a reader fascinated by the topic is likely to be flagging by page 500) are thus less than they might have been.

Despite its size, I note with regret that there is no chapter on training or education in engineering or technology in antiquity, which would have been particularly relevant to both the developmental and the myth-debunking aims. For example, some discussion of the development of apprenticeships in Imperial Roman Egypt, about which there seems to be little knowledge even amongst these experts, would have been welcome: 42 διδασκαλικαί contracts are currently known, of 1–8 years' duration, mostly for training free boys to weave; some are apprenticeships proper, some are for paid tuition instead [Bergamasco 1995]. There are no chapters on the production of bone and ivory (boars' tusk as well as elephant); on colors (dyes are treated *very* briefly at the end of the chapter on textiles, paints nowhere at all); on fuels (barring Wikander's theoretically-orientated section [138–139]); on glue and other binders; on hand tools (barring Ulrich's section on carpenters [444–447]; Mattusch's chapter is mistitled); on jewelry and intaglios (the later especially important

[12] On 628: the 'certain stars' are the wandering stars, better known as those planets that are visible with the naked eye; and the gears do not just 'appear' to have been capable of modeling the motion of the celestial bodies, they really were capable of it. See also 792–793.

in their role as signatures in antiquity); on *materia medica* and cosmetics; on mosaics; on ovens, kilns, and furnaces (required, as Pliny the Elder noticed, for most important production processes in antiquity, e.g., bread, ceramics, metals and glass); or on medical or musical instruments—even the famous, popular, and technically sophisticated water organ gets only one paragraph in the volume [360].

It is to me inexplicable how little mention is made of *the* most complex surviving technology from antiquity, the Antikythera mechanism, which is not really (actually, not even) a time-keeping device; and that no-one involved in the production of the book seems to have noticed or thought worth reporting on the scores of new fragments found and announced to the world in a conference on ancient technology in Athens in November 2005 (there are now 82 fragments). Granted, Hannah, who has the longest discussion of it [744–745], does know Wright's article [2006] from the conference proceedings but not the papers by Andreopoulou-Magkou [2006] and Zafeiropoulou [2006] from the same, where the new fragments are announced.

This touches on a more general issue. Any enterprise of this scale and with this number of contributors must be rather long in the making, but most bibliographies (there is no consolidated bibliography) terminate around 2004/2005, which seems to correspond to their composition date. Only Curtis seems aware of Lawton 2004, though it is of relevance to many parts, e.g., to the discussions of simple machines, power generation, mills, transport, ships, attitudes to manufacturing, agriculture, mining, metalworking, textiles, and warfare. Lucas 2006 is also missed except by Wikander; but since Lucas' article appeared after most contributions appear to have been finalized, this is more understandable. The delay in production is, thus, very regrettable, not only for the Antikythera mechanism, but in a number of areas where the quantity or quality of research being done makes them dynamic. To keep interested parties informed on this particular fast-moving topic, the Antikythera Mechanism Research Project (AMRP) has its own website (www.antikythera-mechanism.gr). Latest news (August 2008) is that the month names are derived from the Corinthian calendar—a fact which to my mind certainly does not (contra the website) indicate probable production in a Corinthian colony in the Western Mediterranean (rather than somewhere in the eastern Mediterranean, as hitherto thought), firstly because human mobility was high in the first century BC when the device was made,

so a maker with origins in Corinth or a Corinthian-colony could be working in Alexandria, for example; and secondly because if the device was bespoke (as is likely), it would be the client's preferences, rather than the maker's, that are shown.

There is very little explicit awareness (Greene and Cuomo are exceptions) of the history of technology as a discipline in its own right, with its own theories, insights, and agendas, so that questions relevant to the book's aims which could have been asked on issues such as the deliberate rejection of advanced technology (e.g., by the Roman army of most Hellenistic Greek military high-tech), are not even raised. There are many good contributions and most can be recommended to students as first ports of call to provide overviews of the topics covered; but other contributions wander excessively from the required frame of reference to summarize critically ancient technological achievements and to narrate their development through antiquity.

Taking the book as a whole, one does get a real sense of the scale, range, and scope of the ancient economy; but there are some important omissions on the one hand and some repetitions on the other, and almost all chapters were several years out of date on publication. There is also the issue of bulk without bond. Even production quality is not up to the standards usual for this press (and that one has a right to expect at this price). Production quality is acceptable, but the proofreading was far from meticulous, the claim to the contrary on page vii notwithstanding: there is, for example, a recurrent problem with miniscule 'f' where there should be majuscule 'F' throughout the first half of the book [see my List of Typographical Errors below]. However, once issued in a paperback that is affordable (as is planned) and, one hopes, corrected (at least on the easily fixed slips and oversights), enough chapters offer good introductions to their areas to justify setting it as a course text for undergraduates on ancient technology courses, and as a companion volume to Humphrey, Oleson, and Sherwood's excellent and pedagogically indispensable sourcebook on ancient technology [1998]. It will surely stimulate more interest and new work in this young and exciting topic.

Vital statistics

- 33 chapters, in 8 parts, plus front matter and introduction
- list of contributors

- abbreviations and a note on spelling norms
- a glossary of nautical and navigational terms used in chapter 24 appears on 630–632.
- a select bibliographical gazetteer of sites mentioned in chapter 25 appears on 664–665.

There are no notes; references are in brackets in text. The bibliography for each chapter follows that chapter; there is no consolidated bibliography. All of this is convenient for anyone photocopying individual chapters.

There are a significant number of figures, but there is no list of them. Likewise there is no list of tables. As a service to readers of *Aestimatio*, I supply both after the Bibliography along with a list of typographical errors.

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LIST OF FIGURES

LIST OF TABLES

## LIST OF TYPOGRAPHICAL ERRORS

Errors are located by page number/paragraph number/line number/word number with a few self-explanatory exceptions.

| | |
|---|---|
| 9.1.13.2 | read ‘provided’ for ‘providing’ |
| 18.2.7.5 | read Ictinus for Icinus |
| 83.3.9.13 | read 2002 for 2003 |
| 97.4.8.4 | delete ‘used’ |
| 103.1.10–11.13–1 | read ‘allow’ for ‘allowed’ |
| 106.5.5.13 | delete ‘other’ |
| 121.1.9.6–7 | delete ‘of operations’ |
| 153.1.9.7 | read ‘be’ for ‘he’ |
| 219.line 4 up.3 | insert “ before ‘The’ |
| 228.2.fin | insert full stop at end of line |
| 244.line 3 up.3 | read ‘due’ for ‘dure’ |
| 307.1.3 up.3 | read ‘Frontinus’ for ‘frontinus’ |
| 310.4.4.1 | read ‘Frontinus’ for ‘frontinus’ |
| 320.Table12.1,col.7,line2 | delete comma |
| 320.Table12.1,col.7,line3 | read ‘Eupalinos’ for ‘Eupalincs’ |
| 320.Table12.1,col.1,line9 | read ‘Crypta’ for ‘Cripta’ |
| 320.Table12.1,col.1, 4 up | move this entry up six places (to follow the Claudius tunnel, unless the date is wrong and this should be mid 2nd c. AD) |
| 322.2.9.8 | read, for„ |
| 324.3.6.2 | move full stop to after close bracket |
| 325.1.5.11 | move full stop to after close bracket |
| 325.1.7.13 | move full stop to after close bracket |
| 335.2.11.2 | read ‘France’ for ‘france’ |
| 339.1.3.3–4 | insert space |
| 362.4.5.7–8 | insert space |
| 372.2.2 up.5 | read ‘1980s’ for ‘1908s’ |
| 373.1.9.3 | read ‘Finley’ for ‘finley’ |
| 373.3.2.6 | read ‘Foxhall’ for ‘foxhall’ |
| 376.2.8.13 | read ‘fitted’ for ‘fit’ |
| 381.2.10.11 | read ‘Foxhall’ for ‘foxhall’ |
| 381.3.1.fin | insert ‘the’ |
| 382.1.6 up.4 | read ‘Frankel’ for ‘frankel’ |
| 383.2.4 up.2 | read ‘Frankel’ for ‘frankel’ |

| | |
|---|---|
| 390.Lawton | this book has two distinct titles that are confused in the publication itself; the one here given appears on the title page and the publication data inside the book, but on the cover and the recto of the series title page it is called *Various and Ingenious Machines* with '*The Early History of Mechanical Engineering*' as a subtitle |
| 397.2.3 up.5 | read 'Fülle' for 'fülle' |
| 399.2.2 up.7 | delete full stop after 'Veyre' |
| 419.2.3 up.12 | read 'in' for 'on' |
| 432.1.1.6 | read 'Formigli' for 'formigli' |
| 435.1.1.2 | insert 'are' after 'bronzes' |
| 441.4.6.5–6 | read 'the most' for 'most the' |
| 444.last line | Matthäus is not in the Bibliography (except with Gaitzsche) |
| 448.2.3.9–10 | insert space |
| 450.1.5.3 | 'twenty' cannot be right if the Comacchio wreck is first century BC (444) |
| 451.2.fin | move '(figure 17.5)' to the end of para 1 |
| 470.1.2 up.3 | delete 'the' |
| 512 figure caption | read 'photograph' for 'photogarph' |
| 516. Greene 2007 | volume and page numbers missing |
| 524.1.5.5 | delete space, delete close bracket |
| 537.1.4.1 | read 'reasons' for 'reason' |
| 541.4.2.3 | read 'contemporary' for 'comtemporary' |
| 552.2.10.4 | read 'Syene' for 'Siene' |
| 557.2.3.last | read 'retaining' for 'retailing' |
| 562.2.3 up.7 | read 'the animals' for 'te animal' |
| 564.1.2.3 | after 'walls' insert 'of' |
| 564.1.2.last | delete full stop before '(figure' |
| 566.1.7.14 | read 'M.' for 'M.'' |
| 568.2.8.8 | delete 'with' |
| 580.1.2.3 | insert 'and' |
| 580.1.3.4 | insert 'the' |
| 581.1.17.1 | read 'produced' for 'producing' |
| 590.1.4.7 | read 'figure 23.2' for 'figure 23.3' |
| 592.2.last.1 | move '(figure 23.5)' up three lines and insert before full stop |
| 602.2.16.9 | read 'growth' for 'grown' |
| 604 | read 'Lefébvre' for 'Lefebvre' |

| | |
|---|---|
| 605. Whitehead entry | read '*Athenaeus Mechanicus*:' for '*Athenaeus: Mechanicus*,' |
| 609.3.1.2 | insert 'archaeologically known' before 'early' |
| 620.3 lines up. 4 | delete full stop after 2005 |
| 634.Lewis/Linder entries | insert line break after 'Press.' to separate the entries |
| 640.3 lines up.7 | insert 'was' after 'latter' |
| 648.1.14.13 | delete ')' after 'side' |
| 658.6 lines up.3 | read 'Citium' for 'Citiium' |
| 660.2.4.9 | insert 'maximum' after 'approximate' |
| 660.last line.6 | read 'capstan' for 'windlass' |
| 661.1.2.7–8 | read 'a ship of 10,000 talents burden, with wooden towers and bulwarks' for 'small boats' |
| 661.1.8.9 | read 'any' for 'most' (the ship couldn't dock at Alexandria either; it was drawn up on the beach and never sailed again) |
| 661.2.6.1–2 | insert comma after 'Alexandria' and delete open bracket |
| 661.2.11.7 | delete 'harbor' |
| 668.Keay/Millett entry | the paper in Hohlfelder is now published, pp. 97–104, has a third co-author, K. Strutt, and a slightly different title, 'Recent Archaeological Survey at Portus' |
| 684.2.7.end | insert close bracket before full stop |
| 685.picture caption | I guess that 'D' is a typo for 'P' in the photographer's name |
| 686.2.12.6 | read 'formerly' for 'modern' |
| 703.2.7 | swap the text in the two brackets |
| 704.fin | insert full stop |
| 717,3.8.7 | delete full stop after 'forth' |
| 718.2.15.7 | 'wordsearch' is a more appropriate analogue than 'crossword' |
| 721.2.2 | something has gone wrong with the references for the quotations (short quotes do not come from five/four pages) |
| 760.2.fin | delete colon and page numbers |
| 766.4.12.fin | delete full stop |
| 768 Ioppolo entry | the page numbers do not match with the citation on 769, so one or both is wrong |
| 773.1.14.1 | read 'way' for 'ways' |
| 780.4.9.fin | read '$\frac{1}{12}$' for '$1\frac{1}{2}$' |
| 785.1.5.6 | read full stop for comma |

| | |
|---|---|
| 793.3.11.7 | read ‘Vienna’ for ‘Vienne’ |
| 824.3.9.6 | read ‘Differential’ for ‘Ddifferential’ |
| 824.3.9.7 | insert close double quotation marks after ‘adoption’ |
| 833.Arnold entry | insert line break after the page numbers to separate next entry (Barlow) |

*The Catapult: A History* by Tracey E. Rihll

Yardley, PA: Westholme, 2007. Pp. xxv + 381. ISBN 978–1–59416–035–6. Cloth $29.95

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Ancient catapults would appear to be an immensely popular topic. A quick Google search reveals the existence of sites that sell catapult-making kits, and of the alarmingly-named 'The Hurl' ('a worldwide community of catapult enthusiasts pursuing the art, history, science and engineering of hurling'!).[1] In contrast, there is relatively little scholarly literature on the subject, and the best recent studies have come out in German or Italian.[2] Thus, for years, the main point of reference for English speakers has been the work of E. W. Marsden [1969, 1971]. Now the publication of Tracey Rihll's book has finally provided an update that is both authoritative and widely accessible.

Rihll's account is organized chronologically. Unlike most accounts that focus on the bow as an obvious precursor, she starts the pre-history of the catapult by describing in her first chapter the bow *and* sling—the rationale for this will emerge later. Chapter 1 sets the tone for the rest of the book in more than one way: Rihll focuses on the older, more 'primitive' weapons, because she will argue throughout that newer technologies did not displace older ones. Moreover, the way in which she describes the sling and bow, by paying attention both to the materials used and to actual deployment and effects in a military setting (what could one actually do with a sling? How accurately could one hit a target, and with how much force? How would slingers fit in with their differently-equipped co-fighters?), mirrors her description of catapults later. Rihll's attention to the likely circumstances of production and use of military technology is one of the strong points of this book.

[1] http://www.thehurl.org (accessed 26 Mar 2009).

[2] I am thinking especially of D. Baatz's and F. Russo's works, abundantly cited in Rihll's bibliography.



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The catapult is introduced in chapter 2, in the form of a 'bow catapult that shot sharps' [46], or *gastraphetes*. Rihll accepts Diodorus of Sicily's account, according to which in 399 BC 'the catapult was invented in Syracuse by an artisan responding to the encouragement and incentives offered by Dionysios I' [35]. Despite some (to me unconvincing) speculations to the effect that the artisan in question may have been a slave [36], Rihll makes as strong a case for Diodorus' credibility as perhaps could be made. Whether that is enough to settle the question of discovery once and for all, I am not sure. Rihll herself is prepared to be sceptical elsewhere. For instance, at a later point she remarks of crossbows:

> It is far more likely that, even though we have no record of it, this type of weapon—a personal compound bow with mechanical locking device and trigger—was being reproduced and developed over the centuries, than that it was lost and then reinvented in a slightly different form. [74]

The operative words here are 'even though we have no record of it'. The documentary record for catapults, or for specific types of catapult such as those later described by Rihll in chapter 5, is very patchy—even authors who could, in principle, talk about them do not, or do so in such terms that a lot of ambiguity remains [60, 82, 83, 134, 183]. The silence, or muttering, of many sources is in itself an interesting issue, which deserves further exploration. But could we not, conveniently, also invoke the sources' silence about a possible version of the weapon developed in the Eastern Mediterranean at an earlier stage? Be that as it may, more interesting is, in my view, Rihll's reconstruction of the early third-century-BC political and military environment which led to the invention or re-discovery by the Greeks of the hurling device.

On cue, chapter 3 discusses the development and diffusion of tension (otherwise known as non-torsion) catapults. Rihll's choice of term is motivated by her desire not to see everything in terms of the torsion catapult, for reasons that we shall see later. She is clear from the beginning about the 'scattered and fragmentary' [47] nature of the evidence from the period: this includes fortifications, notoriously difficult to date, and inscriptions, notoriously difficult to interpret with accuracy. Rihll does a good job of both showing the intricacies of reading the sources, and getting what she can out

of them. The tension catapults of the title would have been larger versions of the original *gastraphetes*, shooting sharps. She also argues for the invention around this time of the 'torsion one-armed stone-thrower', possibly at the hands of the Phocians or the Thessalians [62]. While the tension two-armed sharp-caster was 'a mechanization of the hand bow' [62], the *monagkon* (one-arm) is to be seen as 'a mechanization of the staff sling' [62].

Bow and sling: the choice of subject of the first chapter is now brought to bear on Rihll's argument in chapter 4 that

> [t]he torsion catapult has two very different antecedents and predecessors .... It makes sense for it to have emerged as a union of two machines, a mechanized bow, the *gastraphetes*, and a mechanized sling, the *monagkon*. [77]

Chapter 5 is another strong chapter, on small, hand-held, one-man-operated catapults, whose existence and role in ancient fighting Rihll draws out of obscurity. In both chapters, her detailed analysis of the evidence is hard to summarize here and it involves some leaps, but I found both cases generally persuasive.

In chapter 6, Rihll moves on to what we could call the golden age of catapults, the time of Demetrius Polyorcetes (the Besieger) and, later, Archimedes. It is at this stage, she claims, that the technology really came into its own:

> The first catapults had not made a decisive difference to the outcome of battles in which their users were engaged. By 320 there had been perhaps one to three decades of unspectacular development of two-armed torsion machines during which time they had moved from being a new technology with potential to a hallmark technology.... The real breakthrough came in the penultimate decade of the fourth century. This breakthrough was, I suggest, the discovery of the formula and scaling law, so that people understood why some catapults worked well and others did not; they realized that good experimental models could be scaled up to produce good weapons, and good weapons could be replicated again and again.... The random element that had plagued catapult technology to this point ... was squeezed out by the introduction of mathematics. [110–111]

This 'great leap forward' [110] begs more questions: Why was there no name attached to the discovery of the formula? How did the involvement of mathematics change the skills and knowledge required of people who built and operated catapults? Later, Rihll draws a neat picture of the relationship between theory and practice in ancient catapult-building [154, 172–175], where she rightly emphasizes the approximation and informality of practice—how does that square with the prominent role she gives to mathematics here? I do not think that these questions are problems for Rihll's argument, but I do think that answering them would enrich and complicate it.

Chapter 7 concentrates on the main technical treatises. Rihll and I disagree on the nature of one of them (of which more later). The chapter valuably weaves archaeology into the discussion of some of the texts, which helps in several cases to make sense of them. On the other hand, it does not give much of an idea of the variety of voices involved, and of what may have been the individual contexts for each (of which also more later).

The last three chapters focus on Roman catapults. Chapter 8 covers the Roman Republic, during which nothing much new happened, at least not in the world of catapult technology. Rihll goes through the evidence from this period, mostly showing that it conforms to the picture she has put together so far. Chapter 9 moves on to the Empire and to different types of catapult developed then, boasting a metal frame and, in some cases, in-swinging arms. Rihll's discussion of the, here, mainly archaeological evidence is on the whole persuasive even when, or especially because, she sensibly leaves some questions of definitive interpretation open, as in the case of the Hatra ballista [226–227]. On the other hand, I had some trouble with chapter 10, which seems informed by a declinist view of late antiquity. Decline is apparently back in fashion; but still, one would expect a remark to the effect that those were years of 'ignorance, arrogance, suspicion, and rampant superstition' [234] to be post-modernly tongue-in-cheek, rather than (apparently) to be taken at face value as the background to a stifling of the 'the natural tendency to diversity in technology' [234]. It is almost surprising that in this barren landscape Rihll finds enough material for a whole chapter. Interesting material it is too, leading rather seamlessly into the early Middle Ages and the end of the book.

There are two appendices: one on the calibration formulae and elements of the catapult, and one a useful list of the known remains. The bibliography includes an again very useful list of relevant inscriptions, with a summary description of their contents.

One of the great merits of Rihll's book is that she moves effortlessly between archaeological and literary evidence and reflects on the fit, or lack thereof, between the two. Sometimes the standard specifications that we have from the texts allow useful speculation on the type of weapon that the material remains are remains of [132–133]. Other times Rihll offers sensible reflections on the mismatch between artifact and text:

> T]he [Azaila] counterplate came from a catapult that did not follow Philon's formula exactly, for the frame at least. . . . We might be tempted to suppose that this counterplate is so badly made. . . that it was discarded, but the eight surviving nails that once fixed it to a frame speak loudly against that idea. Perhaps what we have here is one of those catapults that was not even trying to be a formulaic scorpion or a ballista, but was rather one of the many other types of catapult to which the historical sources keep referring but about which we have no details. [188]

Her willingness to leave some questions of interpretation open (I have mentioned the Hatra material [226–227]), rather than force an explanation on the material, is definitely to be praised.

Rihll also has a good grasp of the physics and engineering involved, both in terms of dynamics and of material science. All combined, this allows her to talk competently about both what the ancient designers of catapults might have been saying in their works, and about what the objects themselves might have been like. It is unfortunate that we often do not have a full context for the archaeological remains—whenever Rihll tells the reader more about where the pieces of a torsion spring where found, as in the case of Ephyra [130–134], for instance, it is always a fascinating story.

Above all, Rihll makes a very valuable contribution on a more general level, as will be made clearer by providing a summary of the (by-and-large still dominant) orthodoxy, namely, Marsden's work. In addition to publishing (collected in one place) the ancient technical treatises with an English translation, Marsden provided a simple

model for the development over time of catapult technology. Simplifying (and note that the orthodoxy that I refer to is already a simplified version of Marsden's arguments), he proposed what we could call a linear model of development: it all started from the bow (not the bow and sling). The first catapult was a sort of big bow, the belly-bow or *gastraphetes*, a non-torsion weapon invented at Syracuse in 399 BC. The drive to build bigger and stronger catapults then led, around the time of Philip II of Macedonia, to the invention of torsion catapults, which were powerful enough to shoot large stones. Torsion catapults more or less made non-torsion catapults obsolete, to the point that, chronologically, evidence for a non-torsion catapult would point to an earlier date than evidence for a torsion catapult. Marsden, like many historians of technology of his generation, assumed that, generally speaking, new, 'better' technology displaces an older one.

Successful as Marsden's work has been, it also had its weak spots. For instance, his translations often stand in need of revision. The whole view of technical literature has changed, and there is a tendency now to situate it more and more within the background of the literary production of its time, rather than seeing it almost as the product of a separate subculture. This means emphasizing rhetorical strategies, for instance, or harboring more doubts about the precise meaning of technical terms. Above all, Marsden's developmental model has become problematic in the light of new approaches to the history of technology, where notions such as 'progress' or 'effectiveness' are increasingly seen as culturally constructed rather than absolute.

Rihll makes two substantial revisions to Marsden's arguments: she argues that the torsion catapult derived from a merging of the non-torsion, or tension, catapult and the one-armed torsion catapult. Secondly, she argues that, while large catapults existed and attracted much attention, small, hand-held catapults were much more common than previously recognized.

Her first claim[3] introduces the one-armed catapult as a main player on the scene, and indicates that she is much more comfortable with the idea of tension and torsion weapons co-existing than

[3] A similar claim is made independently Russo 2004.

Marsden was. Her shift of emphasis explains why she terms some catapults 'tension' rather than 'non-torsion'. Rihll also envisages a more gradual development of the two-armed torsion catapult than Marsden did. Her second claim serves as a corrective to the 'noticeable desire by moderns to emphasize size' [137] and again contributes to a picture where old technologies, including bow and sling, are not replaced by new ones, where small weapons can be just as important in battle as big ones, and the machines given pride of place in the extant technical treatises need not be the only ones in existence.

Thus, Rihll's two claims significantly change the catapult's development story. Instead of an allegedly 'natural' progression towards stronger, bigger and higher, smaller catapults co-existed alongside large ones, and more 'primitive' weapons alongside more 'advanced' technical products. She puts to rest the 'widespread but false assumption that since bow catapults preceded torsion catapults, any bow catapult described by someone living in the torsion catapult age must be an old catapult, if not an antique' [169]. Political and economical circumstances played an obvious part: 'there were simultaneously, for essentially the same design of catapult and the same problems, different solutions that would have suited different clients with different budgets' [146].

The notion that technology does not follow universal rules of efficiency, simplicity or 'progress to the best artefact', is still being absorbed by specialists working on the pre-modern period. It is, however, common currency in other periods of the history of technology, and there is plenty of literature on the topic. Despite her declared intention to provide historiographical discussion at the end of each chapter, I found Rihll a bit disappointing here. She inclines towards a sort of evolutionist view of technology [19, 111, 234], which one may not completely agree with, but which gives the reader a handle on several of the issues she discusses. Other than that, however, her discussion of big questions such as technological innovation, or the relationship between 'theory' and 'practice', are impressionistic rather than fully and cogently articulated. Admittedly, the space at her disposal does not allow for deeper delving, but she might have helped her case by choosing more substantial and more up-to-date literature from the general field of the history of technology. Hardly

any of the works familiar to an STS student from either side of the Atlantic are mentioned here.[4]

The other aspect of Rihll's book that I found a bit disappointing was its emphasis on the thing itself, more than on the people making or designing or operating it. Granted, she does talk about some of them and lists their names [318n38] but the focus is squarely on the catapult—whose history this is, after all. The problem of training is dealt with by assuming, rather hurriedly, that no great skill was needed to operate the new devices [189, 195]. Then why did some Greek cities introduce catapult shooting as part of the ephebes' training [64]? The motivations of catapult writers, a very motley bunch in my view, are not as thoroughly discussed as they could have been. What function did these texts serve, exactly—who were they for? At one point, we find the implication that technology could be transferred through books [195]—but surely not in the absence of people to supply the background, tacit knowledge? Again, what relationship was there between catapult construction and other branches of knowledge, in the cases, such as Philo of Byzantium, where we know that an author wrote about other things as well? On the authorship of Ctesibius/Hero's treatise [142 and chapter 7 *passim*], Rihll and I disagree; and I am afraid I did not see any arguments here to make me change my mind. It would have been interesting for the reader, however, and would have partially filled the gap that I described above, if Rihll had given more thought to the question of why someone like Hero of Alexandria, given his other works and what else we know about him, should decide to 'edit' a centuries-old catapult treatise at a time of alleged *pax Romana*, especially when such an undertaking (an 'edition' of a technical treatise, without the author's at least occasional explicit intervention into the text) is virtually without parallel in the ancient literature?

But these are relatively minor quibbles (as is my dislike of 'authentic' spellings—'Arkhimedes'??—when 'wrong' spellings are so commonly established). Indeed, more than quibbles, they are opportunities to open and stimulate further discussion. On balance, *The Catapult: A History* is a must-have for anyone interested in the

[4] Cf. for quick references the material contained in popular textbooks such as MacKenzie and Wajcman 1985 or Collins and Pinch 1998. See also the more recent Edgerton 2007.

subject, a pleasant and instructive read for novices to the field, and the best systematic attempt so far to return what has become a rather specialized topic to its wider context, and thus to mainstream ancient history.

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*Unexpected Links between Egyptian and Babylonian Mathematics* by Jöran Friberg

Hackensack, NJ/Singapore: World Scientific Publishing, 2005. Pp. xiv+ 294. ISBN 978–981–256–328–6. Cloth $64.00, £35.00

*Amazing Traces of a Babylonian Origin in Greek Mathematics* by Jöran Friberg

Hackensack, NJ/Singapore: World Scientific Publishing, 2007. Pp. xx+ 476. ISBN 978–981–270–452–8. Cloth $118.00, £56.00

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In these two books, Jöran Friberg, an expert on the mathematical texts of ancient Mesopotamia, revisits old questions concerning the transmission of mathematical ideas and methods between the various ancient cultures situated in the Middle East and around the Mediterranean. Although the overall structure of the two books is different, the comparative approach is rather similar. The basic strategy is to examine some Egyptian or Greek mathematical text which has been translated and interpreted mathematically by methods developed or adopted by Friberg, and then to follow this by some selection of Mesopotamian texts that are similar in various ways. There has been much development in our understanding of the mathematical texts of the ancient Mesopotamian cultures in recent years and a comparative study of these texts with those of other ancient cultures is most welcome.

Except for some fairly brief remarks, however, there is little to guide the non-specialist reader through the argument, and there is almost no discussion of the social or intellectual context in which these texts were produced. Indeed, some chapters simply consist in translations of the texts followed by Friberg's mathematical interpretation, seemingly implying that mathematics speaks for itself. This is a dubious assumption under the best of circumstances; but in the


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case of mathematical cultures so far removed from our own, it is particularly precarious. Furthermore, because Friberg's stated aim is to find similarities between the texts he examines, he often overlooks key differences or transforms the mathematical presentation in the text into his own idiom, which serves to highlight the mathematical, or structural, similarity, but often at the expense of ignoring the historical, or practical, differences.

*Unexpected Links* explores similarities in the structure and content of the mathematical papyri from Egypt and mathematical cuneiform tablets. While in certain specific cases, we may question the historical significance, or doubt the relevance, of particular similarities, on the whole this book does indeed demonstrate the usefulness of the comparative approach for generating new interpretations of these sources, especially the Egyptian papyri, of which we have so few.

According to Friberg, the opening chapter gathers together and examines the texts that formed his personal point of departure in comparing the mathematics of ancient Egypt and Mesopotamia, and is, as a subtitle suggests, somewhat 'fanciful' [2]. It treats a number of texts that discuss ascending and descending geometric series and their sums, which are mathematically, and sometimes thematically, related to the nursery rhyme:

> As I was going to St. Ives,
> I met a man with seven wives.
> Each wife had seven sacs,
> each sack had seven cats,
> each cat had seven kits.
> Kits, cats, sacks and wives,
> how many were going to St. Ives? [14]

This chapter presents a rather striking example of the phenomena of closely related problems cropping up in different mathematical cultures, which Høyrup [1989] has called 'sub-scientific mathematics'. Whereas Høyrup, however, generally believes that these sorts of problems circulated, and were transmitted, through oral traditions, Friberg, on the other hand, seems to believe that the transmission took place through a 'supposed chain of related texts' [23].

After a brief introductory chapter, the book is divided into three sections organized by texts written in very different periods of Egypt's history and in three languages. The first chapter treats Hieratic texts, of which there are two larger papyri, P. Rhind (P. BM 10057/8) and P. Moscow E 4676, and some fragments. Friberg shows that there are a fair number of similarities between these Egyptian papyri and certain Babylonian tablets, both in terms of the overall structure and in terms of the types of problems addressed. Using the comparison of a few key examples, he argues against the opinion that, in the early part of the second millennium BC, Egyptian mathematics was much inferior to Babylonian mathematics. Nevertheless, despite Friberg's high opinion of Hieratic Egyptian mathematics, this approach, because it largely ignores the social and intellectual contexts, still involves a supposed ability to rate the mathematics of one culture against that of another. If such a rating is to be carried out fruitfully, however, the scale upon which this rating is done must be made fully explicit.

The second chapter compares texts that were written much later, and in Demotic Egyptian, with Babylonian sources. The core argument of this section centers on a papyrus of the third century BC, P. Cairo J. E. 89127–30, 89137–43 (*verso*). Friberg convincingly argues that there is a marked similarity between the types of mathematics found in P. Cairo and those found in late Babylonian texts. He shows, for example, that many of the problems of P. Cairo can be fruitfully explained by the style of Babylonian mathematics that scholars have recently dubbed metrical algebra; and that the method of solving certain problems, such as calculating the area of a circle, is the same in the Demotic Egyptian and Babylonian mathematical texts. Although Friberg is not the first to have argued for the transmission of Babylonian mathematics into Demotic sources [Parker 1972, 5–6; Høyrup 2002, 405–406], he brings a wide array of evidence to bear on the issue. In this regard, Friberg claims that these texts show that in Egypt during the time of Euclid, or slightly thereafter, there were individuals familiar with solving problems using Babylonian metrical algebra [191]. The influence of this assumed familiarity forms the main topic of *Amazing Traces*.

The third chapter of *Unexpected Links* discusses Greek mathematical papyri of Egyptian provenance. Friberg's main findings are

that this Greek material is essentially similar to the Demotic material and, hence, likewise shows evidence of influence from late Babylonian sources. These texts span a long period but many of them are contemporary with Greek astronomical papyri containing methods which have been shown to originate in Babylonian sources [Jones 1999]. This comparison with the astronomical sources, however, may be taken as a cautionary tale. In the case of astronomy, it is now clear that the theoretical tradition as represented by works such as the *Almagest* and the practical tradition as represented in the papyri co-existed for long periods of time, despite being based on a different set of theoretical assumptions, employing different mathematical methods, and being practiced by individuals from different cultural groups. So we should be wary of assuming that all the mathematical texts written in Hellenistic or Imperial Egypt were of interest to all who were practicing mathematics in that region.

Whereas the final chapter of *Unexpected Links* discusses Greek papyri that were written in what we may call the practical tradition, *Amazing Traces* investigates selections of texts from the more theoretical traditions that we generally think of as constituting the core of Greek mathematics. In fact, over half of the book is devoted to comparisons of Euclidean texts with Babylonian texts. This is followed by comparisons of Babylonian texts with other Greek authors, either directly or indirectly reported, such as Heron, Diophantus, Hippocrates, or Theodorus. Unlike *Unexpected Links*, which largely proceeds chronologically and is divided linguistically, *Amazing Traces* is organized into many small chapters treating specific mathematical topics, such as '*Elements* X and Babylonian Metrical Algebra,' 'Hippocrates' Lunes and Babylonian Figures with Curved Boundaries' or 'Theodorus of Cyrene's Irrationality Proof and Descending Infinite Chains of Birectangles'.

As a collection of Babylonian texts that are mathematically related to Greek texts, *Amazing Traces* will be a valuable resource for historians of Greek mathematics; but as a reading of the Greek texts themselves, this work is beset with a number of difficulties. The orientation of the scholarship is much more mathematical than historical and Friberg often allows similarities that can be extracted through mathematical analysis of the text to guide his views, with much less regard for the historical circumstances. A few examples may serve to make this point.

In order to compare the *demonstrations* in *Elements* 2 with the *calculations* in Babylonian tablets, Friberg is compelled to address the difference in presentation between these two types of text. He does this by imagining what would happen to the Greek lettered diagrams of *Elements* 2 'if the letters are removed and instead lengths and areas with their numerical values are explicitly indicated in the Babylonian style' [4–5]. This supposedly simple transformation, however, completely changes the underlying nature of *Elements* 2 from drawing diagrams and making arguments about them to laying the theoretical foundations for the transformations of certain equations, which although geometric in some sense are meant to represent numeric values.

Following this mode of interpretation, Friberg reads *Elem.* 2.5–6 as demonstrations that certain Babylonian style *rectangular-linear systems of equations* have certain solutions [12–13]. There is still, however, no evidence that Greek geometers working in the Euclidean tradition were concerned with solving such equations. There is, on the other hand, considerable evidence that they were interested in using the geometric theorems provided by *Elem.* 2.5–6 to solve problems that arose in their geometrical investigations, that is, in the course of drawing diagrams and making arguments about them. Saito [1985] has argued for a purely geometric reading of *Elem.* 2.5–6 on the basis of the role of these theorems in Greek conic theory. A similar argument for the fundamentally geometric nature of these theorems could be based on Apollonius' *Cutting off a Ratio*, a text which shows at great length how to draw a line through a given point, falling upon two given lines and cutting from them a given ratio, and which makes extensive use of *Elem.* 2.5–6.

The fact that, as *Unexpected Links* makes clear, there were individuals in Egypt roughly contemporaneous with Euclid and Apollonius who were using Babylonian style metrical algebra to solve equations and make computations only serves to highlight the differences between these two traditions. It is in exploring such differences that it would be useful to consider the cultural contexts of these different mathematical traditions and social positions of the practitioners.

By focusing on the similarities between Greek geometry and Babylonian sources, Friberg often interprets Greek mathematical methods as being essentially similar to our own or to those of the

Babylonians and offers readings that are fairly far from a straightforward geometrical reading of the text. For example, he reads a number of theorems of Euclid's *Data* as providing justifications for 'the steps of an algorithmic computation' whereas there is no indication in the *Data* that computations are at issue [232]. In fact, in authors such as Heron and Ptolemy, *Data* style arguments are certainly used to give generalized expressions of algorisms; but it remains to be shown that this practice goes back to Euclid and certainly there are early authors, such as Apollonius and Archimedes, who use the theorems of the *Data* in purely geometric ways.

Because he often does not provide any discussion of the texts beyond a mathematical analysis, it is sometimes not clear what link Friberg sees between the Greek and Babylonian sources. Thus, chapter 6, '*Elements* IV and Old Babylonian Figures within Figures,' gives a brief discussion of the construction of a regular pentagon from *Elem.* 4 and then a list of problems that involve the calculation of the properties of regular figures in Babylonian sources. Since the Euclidian text has no interest in calculation, however, and the Babylonian texts have no interest in construction, the only connection is the appearance of regular figures.

Despite these reservations about Friberg's approach, historians of mathematics will be thankful that he has brought together such a large number of sources and thus laid the groundwork for other comparisons of these different traditions of ancient mathematics.

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*The Astrolabe* by James E. Morrison

Rehoboth Beach, DE: Janus, 2007. Pp. xviii+438. ISBN 978–0–939320–30–1. Paper $60.00

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The astrolabe is probably the archetypal astronomical instrument of the pre-telescopic age.[1] It combines a simple observational tool for measuring angles (typically, a star's elevation above the horizon) with both fixed and moveable planispheric projections of the heavens. Combining these features in a hand-held device proved to be a remarkably felicitous idea. The astrolabe allowed one to tell time, day or night; to determine the altitude and azimuth, as seen from a specified place at a specified time, of a star or an ecliptic location; and in general to convert freely between equatorial (celestial) and altazimuth (local) coordinate systems. When used with planetary tables, the astrolabe provided the simplest way to draw up an astrological chart. In Europe, the astrolabe dropped out of favor in the 17th century due to its limited precision; but in the Islamic world, ingenious enhancements for determining the circumstances of daily prayer extended the astrolabe's useful life through the 19th century.

Real astrolabes are intricate, valuable, and often beautiful instruments, usually made of brass. Some remain in private hands, but most by now have been snapped up for museum collections. Replica astrolabes are used in university courses to teach the history of astronomical practice. And forged astrolabes are still commonly offered to tourists in the bazaars of North Africa and the Middle East.

This large new book is about the astrolabe as an instrument. Most fanciers of astrolabes will have no use for it at all, I am afraid.

[1] I should disclose at the start that the dozen or so photographs of actual astrolabes decorating this book were provided (free of charge) by Chicago's Adler Planetarium, my employer. Morrison's use of these images has not influenced my evaluation of his book.



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It is above all a book for people interested in the theory and practice of the device.

My first observation is that if you want to design or actually to make an astrolabe or one of the many related instruments, you would be extremely foolish to proceed without having this book in front of you. Jim Morrison has made many astrolabes, of many types, and in so doing has worked out technical issues that simply do not arise in more superficial study. In this book he makes what he has learned available to all of us. He shows how to calculate the scales that appear on an astrolabe, and how to use them, taking care to point out subtle distinctions that one would likely not think of until it was time actually to produce an instrument. Two appendices supply the stellar positions and solar coordinates that the ideal readers of this book will use in laying out their astrolabes.

Second, if you want to know how to use an astrolabe or even really to understand how astrolabists in the past used it, Morrison's book provides perhaps the most thorough discussion (which is not to say the easiest) available anywhere today. His longstanding obsession —that is not too strong a word—with the astrolabe has caused him to think about the significance of all the little details that other, less dedicated students have usually decided to put off for another day. He makes explicit, in equations and diagrams, the mathematics underlying almost every scale that has ever appeared on an astrolabe.

Morrison's broad scope adds greatly to the book's appeal. The mariner's astrolabe, a weighted sighting device, is omitted; but variant astrolabes of any mathematical interest receive concise and fully detailed analysis. These include, beyond the standard planispheric astrolabe, ingenious so-called universal astrolabes useable at any latitude, such as the Saphea Arzachelis, the Mathematical Jewel of John Blagrave, the Rojas and the de la Hire astrolabes, and the astrolabe quadrants associated with the names of Prophatius, Gunter, and Sutton. It is safe to say that if a handheld instrument involves a projection of the sky, Morrison has noticed it, figured out exactly how it works, and provided a careful explanation in this book.

If your only interest is in physical astrolabes, astrolabes as objects, you should go elsewhere. This is not a book about actual astrolabes, but a book about the astrolabe as an idealized scientific instrument. Works that describe and picture individual instruments—

collection or auction catalogues and the like—offer the specific details (and the visual pleasure) that Morrison foregoes. R. Gunther more or less established this genre in *Astrolabes of the World* [1932]. Notable examples include the catalogue by S. Gibbs and G. Saliba of astrolabes at the National Museum of American History [1984]; A. Turner's catalogue of astrolabes in the former Time Museum collection [1985]; K. van Cleempoel's *catalogue raisonée* of instruments from the Flemish Louvain school [2002], as well as his catalogue with F. Charette and others of astrolabes in the National Maritime Museum collection at Greenwich [2005]; and the catalogues by R. and M. Webster [1998] and D. Pingree [2009] of Western and Eastern astrolabes in the Adler Planetarium collection.

Complementing these descriptive works is a generous shelf of technical literature that delves more or less deeply into astrolabe mathematics, while remaining primarily devoted to the analysis of actual instruments. This book does not fit at all on that shelf. Its photos of instruments are purely decorative; its explanations are abstract and mathematical. It is not that Morrison ignores specific details and the quirks of real instruments; far from it. But historical nuance is not of primary importance to him. His historical assertions are well-informed and for the most part correct, so far as they go, but tend to be blunt and oversimplified. That is because they are not the point of his book.

Morrison writes for people interested in the construction and function of ideal astrolabes. Some of the questions he addresses seem strangely irrelevant to this historian: for example, how precise could a measurement of solar time be, using an astrolabe laid out and fabricated as accurately as 21st-century technology allows? To me, the question simply does not arise. To Morrison it is of very great interest.

The author's scholarly research extends to sources available in English or French, but not in Latin or Arabic. Since most of the sources that he requires are available in English translation, this limitation is less problematic than it would be in a historical monograph or a descriptive catalogue of actual instruments

Despite its ahistorical approach, the book has very considerable merit for a historian. Morrison's broad and precise coverage of astrolabe variants condenses a great amount of analysis that historians

will not need to repeat—indeed, analysis that few historians are capable of repeating. Trigonometric expressions for astrolabe scale values capture the ideal mathematics of an astrolabe. Numerous example calculations ensure that readers who care how an astrolabe was used can feel confident that they really understand the procedures captured in Morrison's mathematics.

If you merely want to understand the astrolabe in general terms, this book is much more than you need. If you basically understand the theory and use of the astrolabe, but have not worked through all the esoteric details—and fear that some of them might ensnare you if you ever were to need them—you will cherish the book. It is paperbound to save cost, but a generous margin will allow heavy-duty readers (of whom I foresee quite a few) to punch the pages for a three-ring binder.

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*Fibonacci's* De practica geometrie edited and translated by Barnabas Hughes

Sources and Studies in the History of Mathematics and Physical Sciences. New York: Springer, 2008. Pp. xxxvi + 408. ISBN 978–0–387–72930–5. Cloth $129.00

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Après les traductions anglaises par Laurence Sigler du *Liber quadratorum* et du *Liber abacci*, Barnabas Hughes propose dans cet ouvrage la traduction anglaise du troisième ouvrage majeur de Leonardo da Pisa, ou Fibonacci, le *De practica geometrie* (1220)[1]. Plus exactement, comme Hughes le précise dans son introduction: « The translation offers a reconstruction of *De practica geometrie* as I judge that Fibonacci wrote it » [xvii].

Le *De practica geometrie* de Fibonacci s'inscrit dans une longue série de textes des traditions grecque, byzantine, arabe, puis latine portant sur la mesure et le découpage des aires. Le traité de Fibonacci est divisé en huit chapitres. Le premier porte sur la mesure des aires des champs rectangulaires. Le deuxième présente l'extraction des racines carrées. Le troisième chapitre porte sur la mesure des aires de figures géométriques autres que le rectangle comme les triangles, les quadrilatères, les polygones, les cercles. Le quatrième chapitre traite de la division des aires. L'extraction des racines cubiques est l'objet du cinquième chapitre. Le sixième chapitre aborde la géométrie des solides, dont il s'agit de déterminer les dimensions. Le septième chapitre porte sur la mesure des hauteurs. Hughes place dans ce chapitre la table des cordes et des arcs qui se trouve au chapitre 3 dans l'édition du texte latin par Boncompagni. Dans le huitième et dernier chapitre, on trouve une série de problèmes sur la mesure des côtés des pentagones et des décagones, ainsi que des diamètres de cercles inscrits et circonscrits, résolus par des méthodes algébriques

[1] Dans l'avant-propos, Frank Swetz date le *De practica geometrie* de 1223 [vi]. Hughes donne la date de 1220 [xxxiv].



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et l'usage des proportions. Dans ce chapitre, Hughes insère à la suite du problème 20, une série de 13 problèmes géométriques (qui dans le texte latin de Boncompagni se trouvent à la fin du traité). Ces problèmes sont d'attribution douteuse, mais ils forment un tout avec le problème 20.

Chacun des chapitres du traité de Fibonacci est précédé d'un commentaire de Hughes dans lequel il présente rapidement le contenu du chapitre et fait la liste des textes cités par Fibonacci ou de ses sources probables. Par ailleurs, l'ouvrage débute par une introduction [xvii–xxxv] dans laquelle Hughes s'interroge sur la connaissance que Fibonacci pouvait avoir de l'arabe et par conséquent sur son usage direct de textes arabes sans le passage par les traductions latines disponibles à son époque. Hughes s'interroge aussi rapidement sur le type d'éducation que Fibonacci a pu recevoir, notamment à Bougie, et par conséquent sur les textes arabes qui auraient pu être à la base de son éducation. Hughes présente ensuite les sources principales du traité de Fibonacci puis les sources probables. (Nous reviendrons sur cette question des sources qui est très problématique, mais je souligne déjà ici la difficulté qu'il y a à trouver les informations concernant les sources de Fibonacci, qui se trouvent distribuées en différents endroits.) Vient enfin la présentation du matériel utilisé par Hughes pour sa traduction. Il manque dans cette introduction une présentation de la vie et des ouvrages de Fibonacci, permettant de situer le *De practica geometrie* dans l'œuvre du pisan.

Pour la traduction-reconstruction, Hughes prend comme texte de base la transcription d'un manuscrit du Vatican (Urbano, lat. 292) faite par Boncompagni au XIXe siècle, qu'il compare à plusieurs manuscrits pour les passages qui lui semblent problématiques ou fautifs. (On peut regretter que Hughes n'ait pas profité de l'occasion pour proposer une édition critique du texte latin à partir des manuscrits qu'il a consultés.) Il utilise aussi des versions italiennes du traité que l'on trouve dans des manuscrits des XVe et XVIe siècles. À partir de ces témoins, Hughes reconstruit un texte qu'il juge devoir être le texte offert par Fibonacci à son ami Dominique, sans doute un clerc espagnol qui l'aurait introduit auprès de l'Empereur Frédérique II [1]. Ainsi Hughes écrit, à propos de la table qu'il a déplacée du chapitre 3 au chapitre 7:

> Inasmuch as I think it inexplicable as well as unacceptable that Fibonacci would foist an incomplete chapter on his good

> friend Dominic from whom he probably expected a few favors, I hypothesize that when Fibonacci wrote Chapter 7, he placed therein the information about sines and versed sines together with the well-developed and exemplified *Table of Chords and Arcs*, as I show after [9]. [344]

Il explique ensuite qu'un utilisateur du *De practica geometrie* aurait déplacé cette partie dans le chapitre 3 « where they are found in many manuscripts ». Cette dernière remarque est incomplète: l'utilisation de l'expression « in many manuscripts » semble suggérer que Hughes aurait trouvé au moins un manuscrit dans lequel la table n'est pas placée au chapitre 3, mais on n'en sait pas plus et rien n'est moins sûr étant donné que tous les manuscrits consultés par Hughes appartiennent à la même famille que celui transcrit par Boncompagni. De manière générale, on ne trouve aucune remarque sur les variantes des manuscrits dans les commentaires qui se trouvent au début de chaque chapitre, ni dans l'introduction générale, ni dans les notes qui émaillent la traduction, de sorte qu'on ne peut juger de l'ampleur de la reconstruction faite par Hughes. Et on ne peut pas savoir si les manuscrits permettent de justifier certains choix faits par Hughes.

Je ne reviendrai pas ici sur la question de la connaissance de l'arabe que pouvait avoir Fibonacci, ni sur la connaissance directe qu'il pouvait avoir de traités arabes par ailleurs disponibles en latin à son époque. C'est une question difficile, qui fait encore objet de débats. Notons que Hughes n'apporte pas d'éléments nouveaux à ce sujet, mais il soutient que Fibonacci a utilisé directement des textes arabes.

J'en viens maintenant à la question des sources dont le traitement par Hughes pose de nombreux problèmes et j'en donnerai quelques exemples. Dans les commentaires, Hughes ne donne pas la référence des traités dont il parle, à quelques exceptions près. On trouve de telles références, parmi les sources secondaires, dans la bibliographie qui se trouve à la fin de l'ouvrage, mais pas pour tous les traités évoqués. Ainsi Hughes parle du traité sur le rapport et la proportion d'Ahmad ibnYusuf sans que l'on trouve dans la bibliographie la référence à l'édition du texte latin par D. Schrader.

Certains des textes que Hughes identifie comme étant des sources arabes du traité de Fibonacci sont problématiques. Ainsi, Hughes

affirme que Fibonacci aurait utilisé la traduction arabe du livre 1 des *Eléments* d'Euclide produite par al-Ḥajjāj:

> Of the fourteen statements regarding construction in [3] below and allowing for a measure of variation in translation, twelve statements represent word-for-word statement from Euclid's *Elements*, Book I, as translated by al-Hajjaj ». [4]

Je me demande quel texte a utilisé Hughes pour faire cette comparaison mot à mot. En effet, nous ne connaissons à ce jour aucun manuscrit arabe de cette traduction (tous les textes arabes que nous avons conservés portent la traduction de Isḥāq ibn Ḥunayn révisée par Thābit ibn Qurra, même si on peut trouver des traces de la traduction hajjajienne dans certains manuscrits). De même, il est bien connu aujourd'hui que le texte publié par Heiberg sous le titre *Euclidis elementa ex interpretatione al'Hadschdschaschii cum commentariis al'Nayrizi* n'est pas une version latine commentée de la traduction d'al-Ḥajjāj mais celle d'un texte mêlant la traduction d'al-Ḥajjāj à la version Isḥāq/Thābit. Ainsi, nous n'avons à ce jour aucun texte comportant le livre 1 des *Eléments* dans la traduction d'al-Ḥajjāj auquel nous pourrions comparer le texte de Fibonacci.

Les renvois sont parfois imprécis, voire erronés. Ainsi, Hughes signale dans sa partie introductive générale que le livre 5 des *Eléments* d'Euclide est utilisé dans le chapitre 1. Mais on ne trouve aucune référence à une proposition de ce livre dans les notes de ce chapitre 1. Le renvoi à la proposition 6.13 dans la note 31 page 32 est une erreur. De même, l'énoncé du problème 32 [27] n'est pas la proposition 2.2, même si c'en est une généralisation. Par ailleurs, dans le chapitre 3, Hughes renvoie au livre 5 des *Eléments* d'Euclide pour la composition des rapports [99n81]. Or il n'est pas question de composition des rapports dans le livre 5. Je n'ai pas relevé toutes les erreurs de ce type, mais il convient de vérifier les références données par Hughes en notes.

Par ailleurs, je n'ai pas compris pourquoi, pour certaines propositions d'Euclide, Hughes renvoie à un article de Folkerts [2006] dans lequel ce dernier s'interroge sur la version des *Eléments* qui a servi de base à Fibonacci (en note ces propositions sont référencées comme **Elements*). En effet, certaines propositions citées par Folkerts ne sont pas marquées par Hughes. Et toujours à propos de ces références à Euclide, il aurait été bon que Hughes signale à quelle édition renvoient les numéros de propositions. Je suppose que c'est à

l'édition de Heiberg. Ce détail a toute son importance, lorsque l'on travaille sur les mathématiques médiévales, puisque l'on sait que dans les traductions arabes, puis arabo-latines, l'ordre des propositions est souvent bouleversé.

À propos du chapitre 4, Hughes mentionne, dans son introduction générale [xxiv], le traité sur la mesure d'Abū Bakr. Marc Moyon [2009], qui a édité et étudié ce traité dans sa thèse, m'a signalé qu'il ne pouvait pas être la source de Fibonacci à cet endroit. D'ailleurs, Hughes ne signale plus ce texte dans son commentaire introductif au chapitre 4, ni dans les notes.

Voici quelques exemples de problèmes soulevés par les commentaires de Hughes. En conclusion, si la traduction de Hughes a le mérite de permettre aux non-latinistes d'avoir accès à un texte fondamental des mathématiques médiévales, ses commentaires sont à utiliser avec la plus grande prudence.

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*Naturwissenschaften im Kulturvergleich. Europa–Islam–China* by Karl Wulff

Frankfurt am Main: Verlag Harri Deutsch, 2006. Pp. iv + 408. ISBN 978–3–8171–1782–6. Paper € 36.00

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This book is a poor attempt to contribute to an old debate: the origins of modern science. It attempts in particular to explain why modern natural science originated in Western Europe as late as the 17th century, and why only there and not in China or the Arab world. The question has become well known as the Needham puzzle [see Needham 1969, Graham 1973]. The book's object of inquiry and title are thus suspiciously close to Toby Huff's slightly earlier work *The Rise of Early Modern Science: Islam, China and the West* [1993]. The same holds for the book's conclusion: it was the neutral institutional space provided by universities and the 'scientific popular masses' [72] which these institutions produced as well as the factors (free inquiry, reason, legal theories, religion, the separation of state and church and Greek philosophy of nature) that led to their establishment that allowed the development of modern science in Europe or the West. Wulff demonstrates that all of these elements were absent in China, and most of them likewise in the Arab world.

From the preface, the reader might expect this book to contain some profound discussions of the nature of science. As the author, a natural scientist in the field of physics, chemistry, and biology, claims:

> I am of the opinion, that only on such a basis, one should write about natural science. [i]

It is, thus, all the more surprising that the book contains only very little mathematics and astronomy, close to nothing about natural science or their precursors, but mostly information on the cultural context of ancient Greece and more than 100 pages concerning the history of Chinese civilization and philosophy, of which the author


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only has a truncated knowledge deriving from recently completed undergraduate studies in Sinology.

The first section of the book, 'The Old Greeks', intends to show the significance of Greek geometry for 'Occidental thinking' [34]. It introduces us to 'the development of rational thinking', 'Euclid and the science of geometry', and the Aristotelean basis of 'our medieval world view'. In this collection of loosely related facts, one also learns about the three basic patterns of creational myths [10], today's conception of the solar system [26–27], the *Timaeus* by Plato, and the work of other Greek philosophers who pursued science more as a 'personal hobby' [71].

Then, the author turns to 'the parallel world', China, his personal hobby. Drawing on the main German language undergraduate manuals in Sinology, Wolff recalls the basics of the language, history, and philosophy of China. He presents China as a culture which differs starkly from 'ours' in everything but ethics. He finally turns to science in China. Although oriented towards 'practical problems', 'since there are singular occurrences of astonishing achievements', he says, 'the Chinese accomplishments in the field [of arithmetic] are indeed worthy of being considered'.[1] Wulff thus devotes altogether four pages to the history of Chinese mathematics and astronomy before turning to speculations about early cultural contacts between Europe and China. A final section on 'Euclid in China' serves as transition to the third part of the book in which the author claims to 'reflect upon the fundamental reasons that hindered the understanding of Euclid in China' [174].

This third part of the book, 'Where is the Difference ?', is constructed around the basic assumption of antithetical attributes, namely, that Greece and China differ in the structures of their societies,

[1] Die Chinesen befassten sich, wie die Babylonier und Ägypter, mit praktischen Problemen. Der Schwerpunkt ihres Interesses lag zudem im Bereich der Arithmetik. Hier waren sie weiter als die Babylonier. Es ist daher durchaus lohnend, sich mit den chinesischen Errungenschaften auf diesem Gebiet zu befassen, da sie hier in Einzelfällen Erstaunliches geleistet haben. Doch auch hier finden wir nur Einzelbeispiele, aber keine allgemeine Struktur eines systematischen Lehrgebäudes. Es gibt viele schöne Steine, aber kein Haus. [155]

their geographies and climates, their religious ideas, their historical developments, their linguistic structures, and their modes of logical thinking. The question of how all this exactly is related to the complex cultural history of the reception of Euclid's *Elements* in China is not answered. Instead, Wulff, argues that scientific genius emerges out of a fusion of mysticism and rational thinking. Thomas Mann's *Doktor Faustus* serves him as an illustration, while the Chinese third century alchemist Ge Hong (葛洪) serves to exemplify another deficiency in China, since he was only a mystic but otherwise 'not interested in nature' [224].

Where one might now expect a separate chapter on science in the Arab speaking world, one finds instead some remarks relating this vast field to European developments within the framework of a conflict between 'Reason' and 'Revelation' and the transmission of Arabic learning to the West. Next comes what is central to Wulff's argument in the book, a discussion of the professionalization of scientists during the development of medieval universities. In these institutions, Wulff argues, Europe could 'produce the human masses educated in science', which then fostered the Scientific Revolution.

The final part of the book, 'Conclusion and Outlook', first reviews the changes in traditional astronomical views which the Scientific Revolution brought about, and then comes back to the initially formulated question, why modern natural science only emerged in Europe. Wulff underlines once more 'the lack of all positive conditions in this [China's] culture that could have allowed such developments' [348]: they had neither Euclidean geometry, nor Ptolemaic astronomy, and there was no broad audience that might have understood what the Jesuits had presented to them. Not only was the rational thinking of the Greeks exotic in Chinese eyes, the Chinese, Wulff believes, had no reason at all to change their traditional modes of thinking.[2] In contrast, Wulff states, the Arabs found absorbing a foreign scientific culture much easier, since 'they did not have an equivalent proper highly developed culture at hand' [34]. The book's epilogue concludes on a moral tone, warning us of the dangers that our modern scientific rational world view is exposed to:

We should preserve and protect this cultural artifact...

[2] That such is certainly not the case in astronomy has been shown in various publications that Wulff has entirely overlooked: see, e.g., Hashimoto 1988.

> the modern scientific world view, based on reason, which is one of the most precious cultural artifacts that Europe—and only Europe—has brought about. [357]

I assume it has become clear from the tone of this review that a reader of Wulff's book cannot but feel uneasy with his approach. On the one hand criticizing, Needham for taking a Eurocentric approach to Chinese science, Wulff systematically falls into the pitfalls of dealing with non-European scientific cultures. Instead of studying the Chinese scientific tradition for its own sake, Wulff takes modern science in Europe as the yardstick by which he measures scientific development in China. He then finds neither an equivalent concept of proof[3] nor a comparable degree of formalization or interest in generality[4] in China.

Wulff's comparative approach, based as it is on such externalism, therefore focuses on the prerequisites necessary for the production of scientific knowledge and technological progress as observed during the Scientific Revolution in Europe, and on the absence of these prerequisites in China. Sivin's critique [1982, 93–94] of Needham's above mentioned puzzle was precisely to show that the absence of a certain development cannot be described with the tools of the historian. The merit of Needham's puzzle though is that he additionally asked why from the first until the 15th-century Chinese civilization was more successful than Europe in exploiting human knowledge about nature for practical needs. He emphatically underlined the Chinese scientific and technological achievements, which are surveyed in his

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[3] Eine vergleichbare Unterscheidung zwischen der Wahrheit einer Aussage und der Schlüssigkeit eines Beweises hatten die Chinesen nicht. Vor allem fehlte ihnen das Konzept eines *Beweises*. [217]

[4] Allerdings waren auch Argumente in Form eines aristotelischen Syllogismus den alten Chinesen nicht fremd. Sie verwendeten sie aber nur ‚unbewusst' in konkreten Fällen. Ihnen gelang nicht der Schritt hin zur Formalisierung und Systematisierung eines Argumentationsschemas. [218]

> Die Griechen unterschieden streng zwischen der Meinung (*doxa*) und dem gesicherten Wissen (*episteme*). Diesen Unterschied kannten die Chinesen nicht. Die alten Chinesen hatten auch keinen Sinn für ein Anhäufen von Wissen um der reinen Erkenntnis willen. [219]

multivolume encyclopedic project on Chinese science and civilization [1954–2004].

The very concepts of 'Europe', 'Islam' and 'modern science' have already been subject to a fundamental critique by such historians of science as George Saliba in his discussion of Toby Huff's approach [http://baheyeldin. com/history/george-saliba-1.html] or Roger Hart [1999] more generally concerning the historiography of Chinese science. Although Wulff devotes a short chapter to the 'Greek Islamic heritage', he does not question his own use of these categories.

Finally, a word on the bibliography. The author has not read any primary sources but relies entirely on secondary research in Western languages. This is not an *a priori* deficiency, but it does become problematic especially since he ignores most of the recent scholarship with respect to the history of Arabic or Chinese science. The latter is a field that has seen a tremendous expansion and historiographic reorientation during the last two decades. Wulff ignores this and cites only his own six page article in the *Bulletin of the German China Association* [1999] as the major 'extensive presentation' of the image of China in Europe nourished by the Jesuits in France and Germany during the Enlightenment. But numerous recent publications give new insights into the many facets of the cultural history of science analyzed from within China. As for the mathematical writings in China for example, the specific theoretical aspects of algorithms and the role of commentary have been analyzed in detail [see Chemla and Guo 2004, Bréard 1999]. And the work of Catherine Jami and others[5] has contributed largely to our understanding of the role of the Jesuit missionaries in the scientific exchanges since the late 16th century, not to mention the many Chinese and Japanese researchers who have published important monographic studies and research papers in their own language. Not including their work and views in an ambitious comparative project such as Wulff's cannot but result in an unduly Eurocentric vision of the history of 'modern science'.

[5] See Jami 1990; Jami and Delahaye 1993; Jami, Engelfriet, and Blue 2001; and Dold-Samplonius, Dauben, Folkerts, and van Dalen 2002.

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# A Response to McEwen on Hiscock, *The Symbol at Your Door*

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The publication of Indra McEwen's Review of my book, *The Symbol at Your Door* [2008] is to be welcomed for several reasons. That an exploration of medieval architectural design should be reviewed in a journal of classical philosophy and science suggests an encouraging breadth of interest in the subject and recognizes the interdisciplinary intent of the book. The Journal's policy to invite authors to respond to reviews acknowledges that critics should be as accountable as the authors and publishers of the work reviewed, a reciprocation which is much needed and long overdue. Of obvious value is the opportunity to discuss matters raised or omitted by critics and the critical methodology used. All this is most welcome.

Three-quarters of McEwen's review is devoted to a full summary of the book, which should be helpful to readers, and is generally well understood and objectively written. McEwen rightly identifies one of the fundamental questions addressed by the book as being the nature of the connection between medieval theory and practice in architectural design, for which there is believed to be little hard evidence [but see Hiscock 2009]. Arising from this apparent lacuna, she is clearly uneasy with the speculation necessary for exploring this connection, seeing it as a substitute for 'genuine scholarship'. Surprising though this may be, coming from a background of classical philosophy, it is to be understood rather by the quest for certainty which is the imperative of much modern scholarship. This often results in propositions which cannot be proved being ruled out, even for discussion. Yet there has to be a place in scholarly argument for distinctions being made between the possible, the probable, and the definite, and for examining what the evidence may permit when not amounting to proof.

A greater problem with this review arises from the penultimate paragraph which, in only 10 lines, surprisingly pans the book. It accomplishes this brevity by leaving all but one of the criticisms


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completely unsubstantiated, raising serious questions about critical rigor. Some of the claims, that the book is 'unwieldy' and 'overly long with far too many quotations', relate to a perceived need for further editing. To be credible, this needs to be demonstrated by example. Where is there superfluity? Which quotations could be cut? Other claims that the book is anachronistic and redundant are left entirely unexplained. What examples are there to support this? How can a book be redundant and, at least in part, 'impressive' in its evidence, 'convincing', 'compelling', 'well-documented', while it 'raises interesting questions', and is 'of potential value' for its case studies? Equally baffling is the accusation that the book is 'clumsily written', given the quantity of writing successfully published by the present author. As it stands, this suggests a liberty being felt to write anything without constraint. Yet a basic requirement of criticism is no different from that of the work being reviewed. The critic needs to make a case, demonstrate it, and leave the reader to judge on the basis of the evidence provided; otherwise the piece will be little more than an exercise in mudslinging.

Finally, the book is accused of displaying 'weak scholarship' on two counts, both of which beg important questions. The first count relates to the absence of certain works from the bibliography, and is based on a *non sequitur*. It is a feature of much current scholarship that footnotes and bibliographies are packed with references that are mainly there to reassure readers that the author has read, or is aware of, writings pertinent to the work in question. This practice may be required of a review article or a historiography but it is not the purpose of citation in an academic thesis. Here reference should only be made to works that are directly cited in an argument. It does not follow that a work that is not cited has not been read. If any such omission is to be challenged, the critic must demonstrate its relevance, for example, of 'Polyclitus and Pythagoreanism' [Raven 1951] or the Augustan background to Vitruvius [McEwen 2003], to the design of medieval churches, although to complain about the omission of one's own work almost inevitably risks compromising the appearance of impartiality. The second count relates to the use of translations held to be outdated; but later translations are not always better, or even wholly better. It is up to an author to be discriminating in choosing which translation to use, and a critic to

avoid an indiscriminate blanket approach to reception and to show where and how a particular quotation falls short.

To conclude, just as it is important that critics should be as accountable as authors and publishers, so the grounds for criticism should be, to at least an equal degree, as substantiated as the work being reviewed. In both cases, this leaves the reader with the means to gauge the respective merits of the work and its review. Mere assertion is not enough.

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*The* Cosmographia *of Sebastian Münster: Describing the World in the Reformation* by Matthew McLean

St Andrews Studies in Reformation History. Aldershot, UK/Burlington, VT: Ashgate, 2007. Pp. 378. ISBN 978–0–7546–5843–6. Cloth $99.95

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Sebastian Münster's *Cosmographia* was one of the most popular books of the 16th century. First published in 1544, by its 1628 edition over 50,000 German copies of the book had been printed. And yet, to modern readers, Münster's book remains almost unknown. Even historians who refer to it have seldom taken the time to read it from cover to cover and are often content merely to dip in, especially for the more sensational descriptions of Plinian peoples or exotic others. As Matthew McLean indicates so aptly, this is an incorrect reading of Münster and we lose an understanding of the book, the author, and the era if we think Münster's interests were confined to (or indeed, particularly attracted to) these peripheral places and interests.

McLean seeks in this book to introduce us to the full richness of Münster's *Cosmographia*. He argues that Münster set out to rectify the errors of the ancients with regards to Germania and, through a huge network of correspondents and contributors, produced an encyclopedic work that provided a bridge between mathematical and human geography, between Protestant and Catholic thought and communities, between old and new knowledge of the world. In this task, Münster's irenic convictions, his home in Basel, and his work as a scholar of Hebrew were all important contributors to his conception of this mammoth undertaking.

The major contribution of this monograph is that it provides us with a close reading of Münster's work. McLean demonstrates that the organization of the *Cosmographia* is by *periegesis* (that is, by following geographical travels). After describing the section on Alsace to give us the flavor of this layout, he examines how Münster


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describes the geography, the history, the ethnography, the flora and fauna, and the prodigies of nature of all the parts of the world. In terms of geography, Münster has a mini-Atlas at the beginning and a first chapter explaining geographical terms and surveying techniques. McLean does not seem to know that Münster here employs a standard geographical structure, perhaps borrowed from Peter Apian. History is a major preoccupation of the *Cosmographia* and McLean does a fine job of discussing Münster's use of ancient and more modern authors. In assessing Münster's descriptions of peoples of various regions, McLean points out that Münster produced measured and temperate descriptions, only descending to national stereotypes on occasion. McLean further points out that Münster's descriptions of so-called prodigies of nature—odd races of humans, fabulous plants, volcanoes, and so forth, constitute a very small portion of the book, despite the fact that these are the sections most commonly cited by modern commentators.

McLean also reveals Münster's web of correspondents. He points out that Münster composed the *Cosmographia* by forging a huge network of contributors and supporters throughout Europe. Münster did this intentionally and systematically, which is why the book expanded from the 1544 to 1550 editions so much and also (perhaps) why it was so popular. While the section on Germany was at the heart of the book, contributions from other parts of Europe ensured an impressive coverage.

Münster was a deeply religious man, beginning his career as a Franciscan monk and ending it as a Protestant scholar in Basel. It is perhaps not surprising, then, that one of the strong underlying messages of the *Cosmographia* was that of the importance of providence. McLean argues that the *Cosmographia* was in fact designed to show the power of God's providence to raise up and lower empires or to make the land fertile or infertile, for example. We must be mindful of this power, Münster tells us, since poor behavior can lead to a withdrawal of God's favour, as we see in historical examples around the world. Interestingly, the *Cosmographia* itself says little about the Bible *per se*, largely, argues McLean, because it was to be a companion piece to the Bible, as well as being aimed at an ecumenical audience.

Unfortunately, there are some major problems with McLean's book. While he does a wonderful job of reading Münster's text

and explicating it for us, he is much less able at situating it within its intellectual context. The chapter on the sources of the *Cosmographia*, especially, is very weak. This chapter is essentially a potted history of geography and cosmography from Antiquity to the 16th century. Most of it is accurate, although it cites a lot of quite dated material and is not informed by some recent debates. Each chronological section is unconnected to the next, so there is no flow of narrative or argument from one time period/section to the next. McLean starts with Antiquity, arguing that there are two cosmographical traditions—mathematical (from Ptolemy's approach) and anthropocentric (from Strabo's). While I think McLean is mistaken in calling the former experimental, the basic distinction works. He does not try to put any of this into social context, however, or even to show the relationships of thinkers to each other. We then move to the Middle Ages, where McLean narrates a sort of modified 'dark ages' story about the loss of ancient knowledge. Although he tries to recover from this by arguing that the world of the *mappae mundi* was coherent and complete, he still presents us with a very old fashioned view of the Middle Ages. We then jump to the late 15th and 16th centuries, the recovery of Ptolemy, and the explosion of cosmographical writings. This section jumps back and forth chronologically and from country to country. It is rather confused, with only a naïve argument about the huge increase in this cosmographical genre. There are lots of details about individual scholars, but there is no clear overarching argument. His discussion of what constitutes cosmography (as opposed to geography) is simplistic and is not informed by recent scholarship.

It would have been better if McLean had concentrated on the more recent context of the 15th and 16th centuries. For example, how does Münster's geographical work compare with Apian's or Frisius'? How does his historiography compare with other renaissance historians? While the chapter does provide a broad sweep from the Greeks to the 16th century (it is almost one third of the book), Münster's work is not situated historically, culturally, or intellectually.

McLean's book, therefore, is a flawed piece of scholarship. While he makes an important contribution through bringing Münster's masterpiece to a new generation of scholars and by giving us wonderful detail of this book, its organization and contents, the lack of engagement with modern scholarship and debate make it of more limited

value for historians. The book is largely unattached to recent debates about, for example, the changing nature of cosmography in this period, the place of geography within intellectual change in the early modern period, and interpretations of the scientific revolution and geography's role therein. This is unfortunate, since a clearer understanding of Sebastian Münster is very important to our interpretation of these issues.

*A Platonic Pythagoras: Platonism and Pythagoreanism in the Imperial Age* edited by Mauro Bonazzi, Carlos Lévy, and Carlos Steel

Turnhout: Brepols, 2007. Pp. 249. ISBN 978–2–503–51915–9. Paper € 45.37

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This volume collects the papers presented at a colloquium of the same title held at Gargagno on Lake Garda in April 2005, and becomes now the second in a new series of publications of colloquia in ancient philosophy, *Diatribai*, edited by the above distinguished trio. It is as such warmly to be welcomed. There are nine papers, ranging chronologically from Philo of Alexandria to Proclus, and having as an overall theme the various modes of appropriation of 'Pythagorean' themes in the Platonism of the Imperial period.

The papers are as follows:

Carlos Lévy, '*La question de la dyade chez Philon d'Alexandrie*'
Francesca Calabi, '*Filone di Alessandria e Ecfanto. Un confronto possibile*'
Daniel Babut, '*L'unité de l'Académie selon Plutarque. Notes en marge d'un débat ancien et toujours actuel*'
Pierluigi Donini, '*Tra Academia e pitagorismo. Il platonismo nel* De genio Socratis *di Plutarco*'
Christoph Helmig, 'The Relationship between Forms and Numbers in Nicomachus' *Introduction to Arithmetic*'
Dominic O'Meara, 'Hearing the Harmony of the Spheres in Late Antiquity'
Elena Gritti, '*Insegnamento pitagorico e metodo dialettico in Proclo*'
Alessandro Linguiti, '*Prospettiva pitagorica e prospettiva platonica nella filosofia della natura di Proclo*'
Carlos Steel, 'Proclus on Divine Figures: An Essay on Pythagorean-Platonic Theology'.


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The volume is completed with an *index locorum* but not, sadly, of subjects. I will deal with the papers, briefly, in turn.

Carlos Lévy's paper deals with a significant problem in Philo's thought—what to do about the Pythagorean-Platonist second principle, the Indefinite Dyad. Necessarily, Philo has to recognize that something corresponding to a material principle is necessary for the creation of a world at all, but he is wary of postulating anything that would be at all independent of God. A solution is to recognize God's Wisdom, or Sophia, which is an entity sufficiently subordinated to God not to challenge his uniqueness or omnipotence. He notes the interesting passage at *Opif.* 8, where Philo declines to describe the passive element in the universe as a 'cause' (αἴτιον).

Francesca Calabi, in a useful contribution (now appearing in English [2008]) confronts Philo with the pseudo-Pythagorean tradition of political treatises, especially that by Ecphantus, and discerns substantial similarities. These treatises have been given very varying dates over the years, but I see no great difficulty in situating them around the latter part of the second century BC, giving them time to acquire a patina of authenticity by the time of such figures as Nigidius Figulus, Eudorus, and Philo.

The third contribution, that of Daniel Babut, though very sound and interesting, comes oddly, perhaps, in such a collection, since his main concern is to explore the nature of Plutarch's acceptance of the New Academic tradition within Platonism, and his rather 'Academic' rejection of the excesses of Pythagorean dogmatism and credulity. His paper involves extended studies of such works as *De primo frigido* and *De genio Socratis*; and I think he proves his point.

Pier-Luigi Donini (who receives much praise from Babut, despite certain disagreements of emphasis) pursues much the same topic, with, once again, special concentration on the *De genio*. It is indeed remarkable how this dialogue seems to combine Socratic/New Academic and Pythagorean strands in Plutarch's thought. As Donini sees it, the unifying figure here is Epaminondas, who combines Pythagorean training with an admirably Academic streak of scepticism. This in turn he relates to the biographical detail that we glean from the *De E* 387f, where Plutarch speaks of himself as learning 'Academic' moderation after a spate of youthful fascination with Pythagorean number-mysticism. I find his arguments most persuasive.

We turn next to the figure of Nicomachus of Gerasa, and a most useful study of his position on Forms and numbers by Christoph Helmig. I agree with him that the balance of probability points to the conclusion that for Nicomachus, Forms are numbers—though there are also, of course, Forms of numbers—and that Nicomachus distinguishes between 'Form-numbers' and scientific numbers, which are the proper subject of the *Introductio arithmetica*. Nicomachus is thus more of a Pythagorean than an 'orthodox' Platonist. This position is distorted by later Neoplatonic commentators, such as Philoponus and Asclepius—though not by Iamblichus.

Dominic O'Meara next contributes a most insightful study of the Pythagorean doctrine of the music of the spheres, also drawing on Nicomachus, though the evidence in respect of him is indirect, relayed through Porphyry, Iamblichus, and Proclus (doubtless drawing on his lost *Life of Pythagoras*). O'Meara takes us through, first, the evidence for Pythagoras' somehow cognizing the harmony of the sphere (his pneumatic vehicle was in much better shape than that of most of us), and then for views on the utility of this achievement (*scil.* by transmuting this into therapeutic music for the emotionally disturbed).

We turn next to Proclus himself, with an extended account by Elena Gritti of Pythagorean-influenced theories of the nature of arithmetic and geometry as an influence on Proclus' dialectic, particularly in the *Commentary on the Parmenides* and the *Platonic Theology*. This becomes something of a detailed account of Proclus' own procedure; but the connection with Pythagoreanism is preserved by emphasizing the iconic role of numbers, the Pythagoreans having been identified by Proclus [*Theol. Plat.* 1.2, 1.4] as pursuing theology δι' εἰκόνων.

Alessandro Linguiti, in a much briefer paper, focusses on Pythagorean elements in Proclus' philosophy of Nature. These involve, as it turns out, the expressing of phenomena of the natural world in arithmetico-geometrical terms, and in emphasizing vertical, rather than horizontal causation, in both cases at the expense of an Aristotelian perspective. Inevitably there is some overlap with Gritti, but this is a sound and useful paper.

Lastly, Carlos Steel provides a masterful overview of Proclus' interpretation of 'figure' (σχῆμα) in the divine realm, which, it must

be said, strays pretty far in its elaboration from anything that any Pythagorean, or even pseudo-Pythagorean, can have conceived; but yet Steel can show that it has its roots in a document of pseudo-Philolaus about the dedication of different angles and figures to different gods. As Steel well shows, the doctrine derives from close exegesis of passages both of the *Parmenides* and of the *Phaedrus*, ultimately arising in the fertile brain of Syrianus.

All in all, a most stimulating collection of papers.

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*Astronomy, Weather, and Calendars in the Ancient World:* Parapegmata *and Related Texts in Classical and Near Eastern Societies* by Daryn Lehoux

Cambridge/New York: Cambridge University Press, 2007. Pp. iv+566. ISBN 978–0–521–85181–7. Cloth $125.00

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In *Astronomy, Weather, and Calendars in the Ancient World*, Daryn Lehoux offers a comprehensive look at the interplay between *parapegmata* and the often eclectic astrometeorological and calendric traditions of the Classical and Ancient Near Eastern worlds. The primary objective is to show how these traditions developed over time and explore the sources that they drew upon.

The book is largely addressed to a general audience with little technical experience in astronomy or the *parapegma* tradition. It includes a balanced amount of introductory material (explaining, for example, what is meant by the rising and setting of fixed stars) and extensive footnotes pointing the interested reader in the direction of both more specialist treatments and alternative arguments. The book is divided into two sections. Part 1, '*Parapegmata* and Astrometeorology' consists of seven chapters that constitute the body of the discussion, while part 2 'Sources', making up roughly two-thirds of the book's total size, presents the text and translation of extant *parapegmata*, as well as a number of handy cross-referenced tables.

The somewhat oblique chapter headings in part 1 ('The Rain in Attica Falls Mainly under Sagitta', 'Spelt and Spica') are supported by useful and descriptive subheadings—highlighting the strength of the book as a reference work. Chapter 1 outlines the astronomical origins of weather prediction and introduces us to both literary and inscriptional *parapegmata*. Chapter 2 explores the important relationship between fixed-star astronomy and agriculture, and how the need for the accurate timing of seasonal events spurred the development of increasingly accurate solar calendars. The presence of


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regulated solar calendars led, in turn, to a change of emphasis in later Roman *parapegmata*. Chapter 3 is concerned with the use of fixed-star phases as 'signs', and whether these were actually observed on a regular basis after the initial ordering and calibration of *parapegmata* had taken place. Lehoux contends that regular observation was, in fact, unlikely. Chapter 4 addresses various calendrical cycles and how they relate to the development of *parapegmata*. Chapters 5 and 6 look at Babylonian and Egyptian material respectively, in search of possible Near Eastern origins or influence. Chapter 7 draws together the various discussions and concludes that 'ancient systems of astronomical weather prediction relate to calendrical systems in fairly complex and diverse ways' [143]. That there exist seemingly related elements of the *parapegma* tradition in the apparently independent traditions of Egypt, Mesopotamia, and Greece should come as no surprise owing to the universal need to regulate agricultural, navigational, and political events across cultures.

The sources presented in part 2 provide an invaluable resource for those interested in ancient calendars, time-keeping, and *parapegmata*. The first section catalogues the *parapegmata* under types (astrometeorological, astrological, astronomical, and so forth) and provides a brief description of each. The second section provides a translation of all extant *parapegmata* (excluding the farming manuals of Varro and Pliny, for example).[1] The first of two appendices documents authorities cited in *parapegmata*, while the second provides a handy table of correspondence between Lehoux's catalogue and those of Rehm and Degrassi. As well as a comprehensive general index, there is also an astrometeorological index which allows the interested reader to locate references to such diverse subjects as 'heat', 'Meton', and 'Hyades' in the extant *parapegmata*.

Many interesting points are addressed in this book, and the author pays special attention to the varied use of predictive texts within different cultural contexts. An analysis of the Greek and Roman construction and use of *parapegmata* shows clearly that the universal goal of prediction is adopted and adapted to changing needs and technologies. Lehoux asserts that although in many cases the Greek

[1] Greek or Latin is included in many cases unless, as the author notes, 'a specific text will be on the bookshelf of the average classicist or of a moderately good university library' [217n1].

astrometeorological material is 'preserved, reworked, and augmented' in the Roman tradition, there remain fundamental differences [28]. Where the Greek literary *parapegmata* tend to exist as entities in their own right—whether as a stand-alone text (Ptolemy) or as a dedicated section in a longer work (Geminus)—Roman authors tended to intersperse this information throughout works dedicated to much else besides—usually with an agricultural theme (Pliny, Columella). Lehoux sees in this a peculiarly Roman curiosity in not just tracking the various calendric, astrometeorological, astrological, and cultic cycles, but in understanding the fundamental relationship *between* these cycles. The Roman agricultural writers were committed to what Lehoux calls an *intercyclical approach* to periodic phenomena.

In the astrometeorological *parapegmata*, the stars act as signs for weather prediction. The extent to which these signs relate to actual observation, however, is an issue that Lehoux puts under serious scrutiny. He believes that despite the fact that ancient authors explicitly emphasize observation,[2] *parapegmata* 'became a locus of authority that *canonized* the timing and sequence of the stellar phases and weather' [55]. Observation is thereby made redundant as the *sign* moves from the observed phenomena to the text, table, or instrument—what Lehoux calls the 'sign-in practice'. Indeed, Lehoux maintains that the day-to-day observation of stellar phases is entirely impractical in an agricultural context as most of the meteorological phenomena (e.g., rain, storms) clearly make it impossible to view the stars! A certain degree of interpolation based on observation under clear skies would be necessary. Lehoux asserts, however, that provided the peg was moved every day (or the calendar date known) even this observation is unlikely. He does concede that observation could be used to calibrate the *parapegmata* but seriously downplays this point to the extent that Ptolemy and Sextus, despite their insistence on observation, are seen as *deliberately suppressing* the shift from observable stellar signs to observing markers in the *parapegma*-tradition [69].

Overall, the author has presented a lucid and well-researched survey of the various astronomical, calendrical, and meteorological traditions from which the *parapegmata* and related texts develop. The detailed footnotes make it ideal for those with a casual interest in

[2] At least of the 'foundational' variety.

the subject, while the catalogue of extant *parapegmata* make it an essential one-stop reference for the specialist.

*Ancient Greek Cosmogony* by Andrew Gregory

London: Duckworth, 2007. Pp. xii + 314. ISBN 978–0–7156–3477–6. Cloth £80.00

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This work is a study of theoretical treatments of how the world came to be, from the Presocratics to late antiquity. It takes the theories as quasi-scientific proposals, based at least in part on scientific criteria. 'A key argument of this book', explains Gregory, 'will be that there are perennial philosophical and scientific problems relating to cosmogony' [2]. Recognizing that most interpreters of ancient Greek philosophy view cosmogony 'as a single issue', and see it as ancillary to philosophical questions, the author wishes to show that cosmogony can be a complex subject that motivates debates among thinkers [1].

In the first chapter, Gregory distinguishes between mythological accounts of creation and philosophical cosmogony. He finds four issues 'which separate myths from philosophy' [13]. First, philosophical theories are 'parsimonious'. Second, philosophical theories appeal to 'invariance'. This he regards as more general than the often-invoked feature of 'depersonification'. It is not just that philosophers replace personified deities with things, but that, even if they allow for some personification (think of Empedocles, Parmenides, Plato), they take the beings in question to 'act in a regular and predictable manner' [14]. Third, philosophical theories are 'non-contradictory', a feature that might seem trivially true. But Gregory contrasts this situation with mythology, in which competing and incompatible myths are often accepted without demur. Finally, philosophical theories reject the supernatural—a feature that may follow from the others, but which he is content to treat as a distinct one. This list of features provides a wider range of considerations than are often used, and offers a useful set of criteria. The author presents it rather more briefly than the subject warrants. For instance, more needs to be said about what he means by 'parsimony' (in what way, for instance, is Thales'


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theory parsimonious that Hesiod's is not?), and it seems hasty to say that philosophical theories *reject* the supernatural. Ancient theories often ascribe to the ruling power of the universe what they understand to be supernatural powers (immortality, omniscience), but, in accordance with the third feature, take it to act in a rational and consistent way. Indeed, this harmonizes with the author's treatment of animism, anthropomorphism, and hylozoism, which ancient philosophers may allow but restrict to regular operation [15–18]. Gregory prefers the term 'panpsychism' to describe the numinous features of matter in early theories [28, 30].

In his second chapter, on the Milesians, Gregory confronts the question of whether the philosophers of Miletus employed a vortex motion in their respective cosmologies. He argues that they recognized a 'steering principle' immanent in matter (an instance of panpsychism) which directs the κόσμος [31–32]. This obviates the need for a vortex motion. It also obviates the need for multiple κόσμοι, which, as becomes clear in later chapters, are posited by those who see a κόσμος as being generated out of chance events. What emerges from this and subsequent discussions is a typology of theories: those with steering principles make do with a single κόσμος, those with chance encounters require plural κόσμοι. Furthermore, those which posit successive κόσμοι do so for certain determinate reasons.

This typology is attractive. Yet it is difficult to argue for it in light of doxographical statements to the contrary, for instance, claims that Anaximander had multiple κόσμοι and a perishable present κόσμος [39]. Gregory rejects these statements as misinformed, after giving five arguments for a unique world [33–36]. Unfortunately, several of these arguments tend to beg the question by assuming correlations that are in dispute. Gregory does show that there is no convincing evidence for a vortex motion in Thales and Anaximander, and argues this for Anaximenes. He ignores, however, one piece of evidence which might testify to a vortex motion in the last philosopher.[1] Epicurus argues against those who ring the earth with walls to protect

[1] Epicurus *ap.* PHerc. 1042.8.vi [Arrighetti 1973, I′A′ [33] with Perilli 1992].

against the vortex, an apparent reference to Anaximenes' high mountains. The passage is not necessarily decisive—Epicurus may be assimilating Anaximenes to later theorists—but it provides *prima facie* support for the vortex reading.

Gregory takes Heraclitus as not offering a cosmogony (*versus* those who attribute a doctrine of periodic ἐκπύρωσις or conflagration to him). Heraclitus sees the κόσμος not as 'a state of good order' like the Milesians but as 'a well-ordered process' [62]. Fire is primary as the cause of cyclical changes and as maintaining the κόσμος through its steering function. It may or may not have temporal and ontological primacy. Parmenides presents a new problem for cosmogony with the 'sufficient reason problem' [71]. In a homogeneous initial state, why should a cosmogony begin at any given time? Or at any given place? Furthermore, what-is is one and not many. Parmenides goes on to produce his own cosmogony, which is meant as 'a demonstration of some of the problems which face mortal cosmogony' [74]. Parmenides argues against creation *ex nihilo*, a view that no one seems actually to have held before he criticized it [77]. (Does this suggest a failure in the standard interpretation of the Presocratics?)

Empedocles develops a cyclical cosmogony that is driven by Love and Strife, which are to be understood as principles of association and dissociation, respectively, rather than as forces. Chance plays a large role in the cycle, producing κόσμοι that are not identical from one cycle to another (except sometimes accidentally). Gregory gives reasons why teleology would be a difficult concept to connect with Empedocles' cycle. Empedocles is in part reacting to Parmenides, but he is also appealing to conflicting principles to account for cosmogony, inspired in part by Heraclitus' criticisms of the Milesians. He is the first philosopher to posit a cyclical cosmogony with successive κόσμοι. Anaxagoras has a κόσμος initiated by νοῦς. He envisions a single κόσμος (*pace* some readings of B4) and first appeals to an anthropic principle (the universe must have properties which allow life to develop) to account for the world.

The atomists, Leucippus and Democritus, appeal to an indifference argument to argue for an unlimited number of atoms with an unlimited number of shapes. The conditions conducive to cosmogony occur by chance in infinite space, so there are multiple κόσμοι. The

atomists are the first philosophers to advocate a plurality of coexisting worlds, according to Gregory. They reject the notion of a governing principle and also avoid Parmenides' sufficient reason problem. Our world is not a unique product of special circumstances but part of a recurring process that takes place wherever conditions are right. Gregory briefly reviews other Presocratic figures such as Xenophanes (who has no full-blown cosmogony) and Diogenes of Apollonia, who, Gregory thinks, had only a single κόσμος.

In a detailed chapter on Plato, Gregory stresses the teleological character of his cosmogony. He defends a literal reading of cosmogony against those who would take it metaphorically. He is more cautious about Plato's account of chaos, which presents special problems. Overall, however, Plato provides another example of a cosmogony in which a steering principle, in this case personified in the demiurge, produces a single world. In contrast, Aristotle provides a cosmology without a cosmogony: the world has no beginning and no end. Thus, Aristotelian theory offers a sophisticated counterpoint to ancient theories of cosmogony.

Epicurus adopts atomic theory but he rejects indifference arguments as a basis for cosmogony. Rather, he claims that 'κόσμοι form wherever there is a seed for them to form' [175]. He rejects providence as directing the universe. The fall of the atoms and the swerve are best taken as 'permanent features of . . . physics' rather than as the first causes of random motion in the universe [181–182]. By contrast, the Stoics believe in providential direction of the world and in a succession of κόσμοι interrupted by episodes of ἐκπύρωσις. In all other theories that allow multiple κόσμοι, each world is significantly different from the others. But for the Stoics, the successive worlds are identical or very similar. According to Gregory, 'the key question for the Stoics is how to couple together a degenerating κόσμος with the idea of a providential god' [195].

The Christians make an important innovation in theory by arguing that the world is created *ex nihilo*. This view was rejected much earlier by Parmenides and goes beyond anything explicit in the Bible. The Christians see God as creating not only matter but space also. God is completely omnipotent and able to create miraculously all things out of nothing. Gregory goes on to examine cosmogony in later Platonism. He finds that 'Philo believed in an origin for matter,

space, time and the κόσμος. These were all generated by god at the same instant' [223]. This seems to clash with his view about the early Christian fathers as the originators of the idea of creation *ex nihilo*, since they are writing after, and in several cases drawing on, Philo [215–216]. Platonists debated each other, Aristotelian theory, and Christian theology, which itself became increasingly Platonic.

In conclusion, Gregory identifies four main approaches to cosmogony in antiquity:

1. A unique κόσμος governed by design principles.
2. Multiple co-existent κόσμοι generated by chance.
3. Cyclical κόσμοι, of the Empedoclean or Stoic types.
4. Anti-cosmogony, of the Heraclitean, Parmenidean or Aristotelian types. [240–241]

A major shift in thinking occurs when the Christians conceive of God as creating matter and space as well as the world. One thing missing from all these accounts is anything like a theory of gravity, which is crucial to the modern understanding of cosmogony.

Gregory reviews essays in cosmogony, and even anti-cosmogony, from the sixth century BC to roughly the sixth century AD. He provides a coherent and attractive account of the issues, some of which continue throughout the period, and the developments in the field. He makes a plausible case of the importance of cosmogonical principles in shaping debates among thinkers and influencing the course of philosophical and scientific discussion. In most cases, I found myself in agreement with the author's specific interpretations of philosophers, some of which remain quite controversial.

Above I expressed doubt about whether Gregory had demonstrated that the Milesians did not posit plural worlds. Let me offer one interpretation that might help his case. Hippolytus claims that in Xenophanes extinction and regeneration of man occurs with a flooding and drying out, and that this change happens 'in all the κόσμοι' [*Ref.* 1.14.6]. This is typically taken as an expression of plural worlds; but in this case, the story makes sense only in a cycle of recurring wet and dry periods in the same world. Could it be that κόσμος means not 'world' or 'world-order' but 'phase' of the world?

If that is so, it is possible that later writers, reading their own quasi-technical sense of κόσμος into non-technical occurrences may have misconstrued statements of Xenophanes and writers of his period.[2]

In this work, Andrew Gregory gives us a study of cosmogonical theories from the whole of antiquity. He provides careful explication and thoughtful analysis of the theories studied. He often makes illuminating comparisons between ancient philosophical and modern scientific theories of cosmogony. There is no other work which deals with the subject of ancient cosmogony as a topic in its own right. One recent study, however, overlaps with Gregory's and complements it, namely, David Sedley's *Creationism and Its Critics in Antiquity* [2007], which focuses on teleological aspects of world-making. In these works, students of cosmogony can compare two provocative and lively treatments of a subject that has stirred little systematic interest until now. What Gregory's work offers is a comprehensive survey of cosmogony in antiquity, illuminated by an awareness of philosophical and scientific debates continuing to the present, and grounded in a solid study of the ancient evidence. Future studies of cosmogony should surely begin with *Ancient Greek Cosmogony* and will profit from its careful examination of the field.

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[2] See also the treatment of Anaximander at Hippolytus, *Ref.* 1.6.1.

*Andreas Libavius and the Transformation of Alchemy: Separating Chemical Cultures with Polemical Fire* by Bruce T. Moran

Sagamore Beach, MA: Science History Publications/USA, 2007. Pp. viii + 344. ISBN 978–0–88135–395–2. Cloth $49.95

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Andreas Libavius (or Liebau, *ca* 1550–1616) was an enormously learned, prolific and, in his day, respected writer whose supposedly pivotal role in the history of chymistry has been asserted a good deal more often than it has been analyzed.[1] Having studied philosophy, history, and medicine at Wittenberg and Jena, Libavius became a city physician and school inspector at Rotenburg ob der Tauber for a time, and gained something of a reputation for his Latin poetry. Most of his career, however, was spent as teacher or headmaster at assorted secondary schools, inculcating logic and rhetoric into teenage boys. The interests that he pursued in his spare time were encyclopedic, encompassing theology, philosophy, literature, logic, and medicine; but his primary concern and the subject of by far the greatest number of his published works—works dryly described by Hugh Trevor-Roper as being 'of Teutonic length, depth and weight' [2006, 86]—was the tantalizingly ill-defined topic that Libavius himself generally referred to as 'alchemy'.

Progressivist historians of the last century routinely cited Libavius as one of the first to distinguish, or at least to begin to distinguish, between superstitious, fanciful 'alchemy' and rational, experimental 'chemistry'.[2] It is now, however, becoming increasingly accepted that

[1] Newman and Principe [2001] argue persuasively for the resurrection of the early modern term 'chymistry' to refer to any study of the nature of matter in that the period, without distinguishing anachronistically between 'chemistry' and 'alchemy'.

[2] See for instance the key role ascribed to Libavius in chapter 13, 'From Alchemy to Chemistry', of Taylor 1949, and the remarks of Buntz 1970, 194. I do not mean to denigrate either author, merely to illustrate the intellectual climate of the time.



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if at least some writers of the period did draw semantic distinctions between the terms '*chymia*' and '*alchymia*' and their respective cognates, those distinctions have little if any relation to the modern one between chemistry and alchemy. Bruce Moran [5] is not (and does not pretend to be) the first to point out the irony that the works in which Libavius supposedly helped to differentiate these terms typically bore titles such as *Alchemia* [1597] and *Alchymia triumphans* [1607].

In fact, as Moran makes plain in this study, Libavius was a stout defender of many of the supposedly 'superstitious' beliefs of the 'alchemists', including that in the transmutation of metals (which he like many others, including Isaac Newton a century later, saw as a natural process analogous to the transmutation of a caterpillar into a butterfly [61]), the efficacy of viper wine (in which the venom of poisonous snakes was purportedly transformed into a medicine or cordial) [263], and the propensity of murdered bodies to bleed spontaneously in the presence of the murderer due to the action of rather speculatively defined 'occult forces' [272]. Libavius became a darling of progressivist historians not so much for what he believed as for what he rejected, and in particular for his vituperative denunciations of Paracelsus and his disciples.

Already by the 18th century, Paracelsus (1493–1541) had come to be seen by many Enlightenment thinkers as the archetypal alchemical charlatan, with his advocacy of folk medicine, his pretentious neologisms, his contempt for traditional learning, and his guilt by association with radical mystic Protestantism. If Libavius hated Paracelsus, the reasoning seems to have been, he must have been on the side of reason, truth, and light. The reclamation of Paracelsus towards the end of the 19th century as a 'symbol of the German *Urgeist*' [298] served if anything to endorse the view of Libavius as (for better or worse) a proto-rationalist.[3]

But as a number of recent studies have argued, an over-emphasis on the individual role and influence of Paracelsus has long had an invidious effect on the history of chymistry. As Moran pertinently asks, 'if we were not looking for signs of Paracelsian life in texts deemed to have been written by "Paracelsians", what might we otherwise see?' [293]. While it would be absurd to deny that Paracelsus

[3] See pages 296–298 for a very interesting account of the sea change in Paracelsus' reputation between the late 19th and mid-20th century.

(or the works published in his name, a great number of which were spurious) had an enormous impact on 16th- and 17th-century chymistry, it is important to bear in mind that many other traditions co-existed with the Paracelsian, and that there was no simplistic dichotomy between pro- and anti-Paracelsian camps in the minds of most early modern practitioners. That perceived dichotomy is very much a product of 19th- and 20th-century historiography; and, as the work of William Newman and Lawrence Principe in particular has shown, it has had the particularly unfortunate side-effect of encouraging scholars to view even pre-Paracelsian chymistry through Paracelsus-tinted glasses, looking in medieval Arabic and European chymistry for supposed foreshadowings of Paracelsian mysticism and religious radicalism.[4]

One enormous merit of Moran's book is that rather than focus (as almost all previous commentators have) on Libavius' best-known work, *Alchemia*, Moran has rather heroically taken it upon himself to read and summarize the rest of his subject's dauntingly copious output too—together with the even more profuse jungle of contemporary chymical literature that spurred Libavius into print, responded against him, or (not infrequently) did both those things at once. This enables Moran to show that Libavius was by no means as consistently or unequivocally anti-Paracelsian as he is usually painted. When he was at full anti-Paracelsian throttle, Libavius spared no jibes or insults to drive his point home. But like most pugnacious polemicists of his or any other time, he was apt to shift his ideological ground in the course of squaring up to a given opponent. Defending the Paracelsian-inclined French chymist Joseph Duschesne (Quercetanus) against the censures of the Paris Medical Faculty—who had rashly, and without consulting him, cited Libavius as a champion of their (Galenic, Aristotelian, anti-Paracelsian) camp—Libavius affirmed that 'one had to recognize that Paracelsus sometimes spoke the truth and that Hippocrates had propounded not a few things that were false' [192].[5]

Later in the 20th century, as the pioneering studies of F. S. Taylor [1949], Walter Pagel [1958], and Charles Webster [1982] began

[4] See especially pages 293, 296; and Newman and Principe 2001.
[5] Paraphrasing Libavius 1607, 12–13.

to rehabilitate Paracelsus and the 'spagyrists'[6] once again, this time as genuine if sometimes misguided precursors of modern chemistry, a new false dichotomy arose, this time between 'traditional' Aristotelians and Galenists and 'modern' spagyrists and chymists, the latter becoming the vanguard of the 'scientific revolution'. A figure such as Francis Bacon, who was as dismissive of Aristotle (or at least of the stranglehold of self-styled Aristotelians on the academic life of his day) as he was of Paracelsus, could be deemed 'progressive' by either analysis. But a figure such as Libavius, who revered Aristotle even more than he disliked Paracelsus, yet also vigorously upheld the validity of many 'spagyric' doctrines, illustrates how misguided it is to attempt to reduce the thought of any period into self-contained and mutually exclusive camps.

What really worried Libavius about the rise of the Paracelsians was, arguably, not so much their theories as their promotion of practical expertise above book-learning, the suggestion that someone with little or no training in language or logic could become, merely by dint of a certain practical or technical proficiency, a better chymist than the likes of Libavius himself. That said, it would be misleading to portray him as an intellectual snob: he had a real appreciation of the contributions made to chymistry and medicine by apothecaries, surgeons, and other practitioners from the lower echelons of society. When repudiating the claims of Georg am Wald to personal, quasi-religious chymical revelation, for instance, he stressed the importance of practical laboratory experience and empirical testing of such claims [129]. The question of Libavius' own practical laboratory skill and experience is, as Moran frankly admits, vexed and probably unanswerable. Though some of his writings seem to imply that his chymical cogitations were based on personal empirical practice [129, 237–238], it would be rash to take them at face value. As Moran puts it, 'Libavius himself may have proclaimed these duties more than he may have performed them' [301].

[6] 'Spagyria' is a term, possibly coined by Paracelsus himself, meaning (debatably) 'the art of separating the pure from the impure'. Moran [201, 204 and 295] offers a lively account of contemporary debate about the precise meaning (and spelling) of the word. Various chymical practitioners—not all of them Paracelsians—described themselves as spagyrists.

The 'transformation of alchemy' with which Moran associates Libavius was perhaps less a transformation of the subject itself than a transformation of its perceived status. Libavius sought to give chymistry credibility as an academic discipline. By this means he hoped to rid it of the taint of anti-Aristotelian subversiveness, while at the same time excluding the genuinely anti-Aristotelian subversives and rude mechanicals who had hijacked it in an attempt to disguise their ignorance of ancient learning as revolutionary championship of the new.

And it was, in fact, during Libavius' lifetime that the world's first university chair of chymistry (or, more precisely, *chymiatria*, that is, chymical medicine) was established—at Marburg in 1609, by appointment of Landgrave Moritz of Hesse-Kassel. The first incumbent, however, was not Libavius (who would surely have relished the post) but Johann Hartman, a promoter of Paracelsus and of Paracelsus' Danish disciple Petrus Severinus. Moran suggests that the date of Hartman's inauguration may have been 'one of the worst days on Libavius's intellectual calendar' [225]. Though the two had earlier been on friendly terms, Libavius would subsequently inform Hartman (in print) that 'yours is a mental darkness stitched together from falsehoods . . . new and old wisdom alike are a disgrace to you because they will not be gulped down with your Paracelsian muck' [233].[7]

Steeped as he was in the tradition of academic disputation, Libavius seems to have relished debate for its own sake more than he cared which side of any argument was objectively right. As Moran remarks in one of the engagingly colloquial asides that periodically lighten the tone of his dense study, Libavius' uncompromising and often *ad hominem* polemical style is now apt to make him seem 'more like an off-putting sour-puss than a compelling or attractive historical figure' [292–293]. This ability to argue either side of a case was precisely what gained Libavius such credit as an academic virtuoso in his own day. In ours, it is what makes him so hard to pin down.

He was, it seems to me, a figure who did not so much effect change as reflect it. I remain unconvinced that Libavius himself actually had much to do with a transformation of alchemy, however one defines 'alchemy' and whether one sees that transformation as

[7] Translating Libavius 1613–1615b, 93–95.

being from a purely speculative subject into a scientific one, from an artisanal discipline into an academic one, or from a practical study into a primarily textual one. William Newman has affirmed that 'in regard to the art-nature debate... most of [Libavius'] points had already been made by the alchemists of the thirteenth and fourteenth century' [2004, 112]. That does not in itself make Libavius any less interesting a character, but it does rather undermine the apparent premise of this study.

What Moran does demonstrate, repeatedly and persuasively, is that for Libavius himself the issue in question was first and foremost a textual one: 'what was relevant for Libavius were texts'—texts read by a

> Lutheran, male [community] educated in the logic of Aristotle and Ramus, trained in disputation, and, above all, accomplished in the reading and comparison of the written word. [83]

This is not, of course, to suggest that texts were not important to hard-line Paracelsians and dyed-in-the-wool Galenists too. But for devotees of both those camps, texts were a means to an end: for Libavius, they were ends in themselves. It becomes abundantly clear that for Libavius, whatever he may sometimes have claimed to the contrary, a clever pun, a well-turned rhetorical figure or a learned Classical allusion counted for more than any amount of experimental data when it came to lending credibility to a discourse. And if he spotted a flaw in someone's Latin grammar, their testimony could immediately be ruled out of court, irrespective of any mere vulgar facts that might be adduced in their favour [18].

Indeed, this seems to me the point most usefully illustrated by this study. As Moran himself observes, Libavius was in many respects a traditionalist, a humanist polymath of the old school who 'might well have been represented in the notebooks of his students as a *Schulfuchs*' (literally 'school-fox', i.e., an old-fashioned scholastic stick-in-the-mud) [13]. Yet the chief objects of his traditional scholarly analyses were the most up-to-date and controversial texts on the rapidly evolving discipline of *chymia*. Surely, what this demonstrates is not that Libavius himself was a paradoxical or transitional figure, but rather that modern historiography is still overly inclined to cram early modern thinkers into rough-hewn pigeonholes labelled

'traditional' and 'progressive', 'Aristotelian', 'Paracelsian', and the like—pigeonholes that reveal more about the 21st-century analysis of early modern thought than about early modern thought itself.

Moran's commendable stress on language, semantics, and textuality, however, makes it seem particularly perverse that he has—or his editors have—chosen to present the copious source quotes only in English translation, except in a few cases where Moran evidently feels that his source is so punning or allusive that he needs to justify his translation with a bracketed (and usually partial) source quote. It would presumably be argued that to include the full original versions (generally Latin, sometimes German) of all the source quotes given would at least double the length of the already extensive endnotes. Yet when language itself is so central to the theme and argument of an academic study, the extra cost and labour would surely have been worthwhile.

The English translations and paraphrases seem in general to run smoothly and to convey the sense persuasively. Moran's own command of Latin and German is not in question. But neutral and objectively 'correct' translation of any natural-language discourse is simply not possible, especially not in the case of puns, allusions, deliberate ambiguities, and passages where the whole point at issue is the precise meaning of a given word in a given language. There is, for instance, a very interesting account on p. 170 of Israel Harvet's discussion of various definitions of 'alchemy'—but since Harvet's arguments are presented only as English paraphrases of a Latin original, it is impossible (without consulting the original) to be certain exactly which word Harvet was arguing about the definition of. I am not suggesting that such passages should not be translated at all, but there will be many points at which readers competent in Latin and/or German might wish to draw their own conclusions about the intended sense. There are also a few, admittedly rare, instances of translational infelicities where it really is almost impossible to discern the intended meaning without a source text for guidance: for instance (Moran is here paraphrasing Libavius),

> Some use the word *tingere* ... when a virtue is passed from one thing to another or where an effective medicine is pre-

> pared in a way that the whole nature is changed and altered. [264–265][8]

My guess is that this should read '... in *such* a way that ...', but it would be very reassuring to have the source to hand for confirmation.

The inquiry also suffers from a failure to define its own terms. It contains much interesting discussion of contemporary semantic distinctions, but nowhere does Moran fully explain how he himself distinguishes between the terms 'chymistry', '*alchymia*', 'chemistry' and 'alchemy' (though he uses all four), let alone what exactly he means by statements such as 'chymistry followed the procedures of traditional alchemy' [43].

This is a valuable and well-written summary of Libavius' life, work, and thought; but at several points it conveys a sense of dutiful plodding, rather reminiscent of the quasi-encyclopedic studies of Lynn Thorndike [1923–1958] and J. R. Partington [1961–1970]—works that demonstrated their authors' ability to read huge numbers of arcane chymical texts in various languages and distill their content into English summaries, but offered little in the way of synthesis. The concluding chapter [291–301] makes a brave attempt at tying together the many loose ends of the preceding narrative, and argues cogently for the importance of figures such as Libavius to a contextualized understanding of early modern thought. It also features some wittily barbed summaries-cum-parodies of the sort of 20th-century historiography that sidelined such figures:

> Alchemy was interesting when Isaac Newton did it [... but] by itself, alchemy still bore the reputation given it by the Enlightenment. It stank of superstition. [298]

However, while Moran is very good at pinpointing the things Libavius should not be dismissed as, he provides little clear formulation of what he was.

That said, this is a work of solid and useful scholarship that throws up many interesting and challenging ideas. It is also, by a considerable margin, the fullest account to date in any language of

[8] Translating Libavius 1613–1615a, 10.

Libavius' personal history and broader influence. 'Off-putting sourpuss' or not, Libavius was undeniably a major player in the intellectual world of his day; and this study is an important step towards a more detailed and nuanced assessment of his significance.

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*The Chrysokamino Metallurgy Workshop and Its Territory* by Philip P. Betancourt

Hesperia Supplement 36. Athens: American School of Classical Studies at Athens, 2006. Pp. xxii + 462. ISBN 978–0–87661–536–2. Paper $65.00, £40.00

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This is an exhaustive study of a small Minoan copper working site, and farmhouse in the vicinity, on an isolated and windswept headland on Crete, excavated between 1995 and 1997, to which 32 scholars, scientists, and students from a variety of different disciplines have contributed. 22 chapters and 14 appendices, over 432 pages, cover all aspects. Production quality is exceptionally high. There is some repetition and disconnection, and the more important matter is difficult to excavate from the background of a detailed description of everything. Betancourt's overviews of the workshop [179–189] and the survey [257–278] are helpful; an executive summary of the results that mapped the project aims (stated on 18) would also have been useful.

Study of the natural environment of the site reveals that there is no trace of copper ore in the rocks here, and available evidence suggests there probably never was. Rather (as with other copper smelting sites in other times and places), foreign ore, perhaps from Laurion and Kythnos (note the caution on 145) and probably already prepared for smelting [144], was brought in by ship [41–42], perhaps as ballast [180]. This site was probably chosen for smelting because of the presence here of one or more other things needed to make metal from ore: of the possible things suggested, fuel (perhaps including olive press-cake), flux, and naturally directed wind (providing a draft for the furnace and removing toxic fumes) are the most convincing [142–145, 186]. It is established that the bowl furnaces used at Chrysokamino were probably small ($\approx$ 44 cm. max. diameter), that output was correspondingly small and perhaps seasonal, and that there was great chemical and temperature variability (up


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to 1200° C) between different firings [183–189], but that knowledge of how to mix local materials to make refractory clays just for smelting purposes already existed [112–113]. Muhly's chapter on the history of early metallurgy [155–177] puts the site into a wider context and highlights its significance: Chrysokamino reveals the use of an experimental smelting technology that points towards shaft furnaces and pot bellows, and therefore stands at the threshold between the Early and the Middle Bronze Age.

*Alchimie et paracelsisme en France à la fin de la Renaissance (1567–1625)* by Didier Kahn

Cahiers d'Humanisme et Renaissance 80. Genève: Librairie Droz, 2007. Pp. x + 806. ISBN 978–2–600–00688–0. Paper $98.28

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Voilà un ouvrage volumineux, une somme, pourrait-on dire; une étude minutieuse, pointilleuse même, de 800 pages, qui ne représente pourtant que le premier volume d'une oeuvre qui doit en compter trois. *Alchimie et Paracelsisme en France (1567–1625)* devrait en effet être suivi, chez le même éditeur, par *Cercles alchimiques et mécénat princier en France au temps des guerres de religion*, puis *Science, religion et littérature dans la France alchimique de la fin de la Renaissance*. L'ensemble représente une version remaniée et augmentée de la thèse de doctorat de Didier Kahn, soutenue en 1998 à Paris IV Sorbonne. L'ambition de l'auteur est, dans ce premier volume, d'établir une chronologie rigoureuse des livres et des idées, avant de se consacrer, dans les autres, aux hommes dans leur milieu, puis à une analyse plus thématique de la question. Son étude doit montrer que l'alchimie et le paracelsisme sont étroitement liés aux préoccupations de leur temps et ont participé à façonner l'esprit de toute une époque. Il s'agit de reconstituer tout un pan d'histoire de la culture de la France de la fin de la Renaissance sans lequel celle-ci ne peut être complètement comprise. La méthode adoptée par l'auteur pour remplir cet objectif est la philologie qui est, selon lui, dans le domaine de l'alchimie, « seule garante de la validité et de la pertinence du discours critique » [7]. Aussi les approches philosophique et épistémologique des doctrines exposées sont-elles négligées au profit de l'objet livre, saisi dans le cours de ses différentes éditions et polémiques.

Didier Kahn remet légitimement en cause l'idée d'une alchimie immuable du 3e au 17e siècle, que l'on ne saurait confondre avec la magie et la sorcellerie, qu'il définit, non pas comme la science chimique de l'époque considérée, mais plutôt comme une pratique chimique sous-tendue par une théorie transmutatoire et médicale (on


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s'étonnera d'ailleurs de trouver une allusion la liant à l'« alchimie » du 20e siècle d'Eugène Canseliet [37]). Il souligne en outre que l'alchimie de la seconde moitié du 16e siècle ne saurait être réduite au paracelsisme au sens étroit du terme, c'est-à-dire à une symbiose de la tradition médiévale et des conceptions propres à Paracelse. Le paracelsisme a surtout été le lieu de cristallisation de nouvelles influences de la Renaissance (kabbale chrétienne, doctrine ficinienne du *spiritus mundi*, courant mytho-hermétique), et a donc offert une grande diversité de visages. Ce que l'auteur nomme le « renouveau paracelsien » — mesuré en nombre de publications des oeuvres de Paracelse (une douzaine durant les dix-huit années qui ont suivi la mort de Paracelse, contre environ 180 de 1560 à 1589) — correspond à l'influence déterminante de l'essor du paracelsisme sur l'extension spectaculaire de l'intérêt qui se manifeste alors en Europe pour l'alchimie. Quant aux dates servant de bornes chronologiques à ce travail, elles se justifient: 1567 correspond à la première diffusion dans les publications françaises des doctrines paracelsiennes, et 1625 à l'année de la censure par la Sorbonne de l'*Amphitheatrum sapientiae aeternae* de Heinrich Khunrath, qui illustre la crise que traverse alors l'alchimie en France.

L'ouvrage est composé, à l'exception d'une introduction générale, d'une très riche bibliographie et d'un index très utile, de quatre grandes parties accompagnées chacune de nombreuses annexes. L'auteur déplore l'absence d'un vaste répertoire chronologique de la production imprimée, pour ne rien dire de la production manuscrite. Aussi la première partie se propose-t-elle de dresser un état des lieux extrêmement précis de l'édition des livres alchimiques en France et en Europe avant le renouveau paracelsien, dont la publication du *Rosarium philosophorum* de 1550 à Francfort peut être considérée, pour Didier Kahn, comme la première manifestation.

La seconde partie de son livre complète la première, de manière tout aussi détaillée, en ce qui concerne la période de renouveau paracelsien jusqu'en 1567, qui se distingue cette année-là par la parution en Europe de treize ouvrages concernant l'alchimie ou le paracelsisme; en particulier celle, à Paris, du *Compendium* de Jacques Gohory, qui suit de quelques mois la première traduction à Anvers d'un texte de Paracelse (sa *Grosse Wundartzney*), et dont l'objectif était à la fois d'intégrer étroitement Paracelse à la République des lettres humanistes et de l'associer pleinement aux maîtres les plus savants des secrets de la nature, tels que Arnaud de Villeneuve, Trithème et

d'autres alchimistes et philosophes. Cette période est caractérisée par des tentatives de légitimation de l'alchimie, allant de paires avec les premières réactions anti-paracelsiennes, dont la plus éclatante étant la fameuse « querelle de l'antimoine » qui débute en 1566 entre Loys de Launay et Jacques Grévin.

La troisième partie est, suivant son titre, une « chronique de la réception de l'alchimie et du paracelsisme en France et en Europe (1568–1594) ». Cette partie s'ouvre sur la présentation de l'oeuvre de Gérard Dorn qui a participé à répandre les idées de Paracelse à travers toute l'Europe. Paracelsisme et alchimie montent en puissance. C'est une époque où apparaissent les premières tentatives de conciliation entre la médecine paracelsienne et la médecine ancienne, et aussi de synthèse de la pensée de Paracelse — avec surtout la parution de l'ouvrage majeur de Petrus Severinus, *Idea medicinae philosophicae*. Des dictionnaires de termes paracelsiens sont édités en plusieurs langues vernaculaires, dont celui de Toxites qui a pour but de rendre les étudiants débutant dans l'art hermétique plus aptes à lire Paracelse, de peur que, découragés par ses obscurités, ils n'abandonnent finalement l'art lui-même. Mais c'est aussi l'époque de réactions anti-paracelsiennes, de la querelle entre Joseph Du Chesne et Jacques Aubert, et du procès de Roch Le Baillif. Il est toutefois dommage que les écrits de Blaise de Vigenère et de Palissy (tout comme le *Grand miroir du monde* de Du Chesne) ne soient pas davantage discutés dans cette chronique.

La quatrième partie présente, quant à elle, les trente dernières années de cette histoire de manière moins exhaustive; Didier Kahn ne saisit la réception de l'alchimie et du paracelsisme de 1597 à 1625 qu'à travers les grandes controverses, l'affaire des placards parisiens de la Rose-Croix (présentée comme une mystification dont l'instigateur serait un certain Etienne Chaume, futur étudiant de la faculté de médecine de Paris, et non John Dee suivant l'interprétation de Frances Yates), le scandale des thèses de Villon et de de Clave, et la censure de l'ouvrage de Khunrath; mais le traitement de chacun de ces points reste très détaillé. L'histoire se termine au moment où l'alchimie, dans les années 1620, certes traverse une crise, envahit désormais la vie publique et intellectuelle.

On retiendra de cet ouvrage, en plus des innombrables informations d'ordre philologique, que l'alchimie de la fin de la Renaissance,

fortement nourrie de la pensée pseudo-lullienne et de la doctrine de Jean Trithème — comme le souligne avec raison l'auteur — s'introduit sur le marché du livre au 16e siècle grâce à l'initiative de quelques éditeurs qui en font une de leurs spécialités, tout en se voyant étroitement liée à la médecine. C'est ce contexte qui explique une partie du succès de Paracelse qui, en retour, va faire connaître à l'édition d'ouvrages alchimiques un essor spectaculaire; les 2000 pages du célèbre recueil de textes alchimiques et paracelsiens de Lazare Zetzner, *Theatrum chemicum*, de 1602 en sont un exemple très significatif. Toutefois, le renouveau paracelsien, à partir de 1550, va s'appuyer sur un contresens majeur, faisant de Paracelse un complet alchimiste qui a su obtenir la pierre philosophale grâce à laquelle il a pu accomplir des guérisons miraculeuses.

Pour Didier Kahn, écrire l'histoire de l'alchimie, c'est « à la fois dresser une bibliographie chronologique et se faire le biographe des auteurs les plus importants de la période » [33]. Cette approche savante, engagée dans une lutte contre les « mythes historiographiques » (ce qui ne va pas sans donner de temps à autre le sentiment que l'auteur endosse le rôle de redresseur de torts), à la fois impressionne par son souci du détail, par son flot d'informations (l'étude de la question se faisant parfois mois par mois, voire semaine par semaine), mais laisse également sur sa fin: on aurait aimé à certains moments une analyse conceptuelle plus approfondie des textes présentés. Il n'en reste pas moins que ce travail, hautement documenté, qui parvient parfois à redonner quasiment vie aux éditeurs et alchimistes dont il traite à travers leurs querelles, leur recherche de manuscrits, leur mission de faire connaître le paracelsisme, est d'une très grande valeur scientifique, et nous attendons impatiemment la suite.

*Archimedes and the Roman Imagination* by Mary Jaeger

Ann Arbor: University of Michigan Press, 2008. Pp. xiv + 230. ISBN 978–0–472–11630–0. Cloth $65.00

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Jaeger's recent book, *Archimedes and the Roman Imagination*, is not so much about Archimedes himself as about the Roman Archimedeses as they emerge from the works of Polybius, Cicero, Vitruvius, Plutarch, and others. Jaeger explores the most famous stories about Archimedes, like the ones about his '*Eureka!*' and the planetarium, puts them in their context, and draws new conclusions.

The book is divided into three parts: the first is assigned three chapters; the second, two; and the third, one. The first part deals with the creation of Archimedes as a figure embodying ideas of invention and transmission. In chapter 1, Jaeger examines the most famous story about Archimedes in which the great mathematician leaps from his bath crying 'Εὕρηκα!' Hieron II, ruler of Syracuse, had commissioned a gold crown as an offering to the gods, but was suspicious of the material that the craftsman had used to make it: was it only gold, as expected, or gold and silver? Hieron turned the problem over to Archimedes and the latter uncovered the fraud while immersing himself in a bath. He had discovered something akin to the principle of specific gravity. Jaeger brings to light details of this story that have been forgotten and makes interesting remarks about the meaning for Roman authors of Archimedes' naked body and his running in public while crying 'I have found [it]! I have found [it]!'. For Jaeger, the 'Archimedes' that emerges from this analysis is a *topos* for discovery, a figure of an intellectual athlete as well as a mocked slave.

Chapter 2 deals mainly with Cicero's *Tusculan Disputations*. On a first level, Jaeger explores the place of Archimedes in this dialogue as a figure that brings together two cultures. On a second level, she


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goes into the role of Cicero in Archimedes' story. Cicero, by rediscovering Archimedes' tomb and by writing about it, becomes part of Archimedes' story and the tomb becomes emblematic of Cicero's appropriation of Greek learning for Rome. Jaeger highlights, then, the central role of Cicero in this story as an *exemplum* and comments on issues of invention, discovery, memory, death, and the immortality of the soul.

Chapter 3 examines the passage in Cicero's *De republica* where we learn that after sacking Syracuse, Marcellus took with him two spheres: a solid sphere, an old invention reproduced by Archimedes, and a mechanical sphere, Archimedes' own invention. Jaeger argues that the two spheres act as an extended metaphor for the transfer of Greek cultural capital to Rome and for the 'Roman appropriation of Greek cultural capital as both inheritance and rediscovery' [68].

The coda of the book's first part is devoted to the reception and rereading of Cicero's account of the spheres in the *Mathesis*, a fourth-century astrological treatise by Julius Firmicus Maternus. In this treatise, Firmicus argues first that the spheres serve the same role as in Cicero's dialogue and, second, that later writers in general imitate Cicero's manner of using the spheres rather than the description of the spheres themselves.

In the second part of the volume, Jaeger explores the figure of Archimedes as it relates to that of Marcellus, starting in chapter 4 with the various accounts of Archimedes' death. After sacking Syracuse, Marcellus ordered the troops not to kill Archimedes; but a Roman soldier, who did not recognize the scientist, killed him while Archimedes was drawing some diagrams. By focusing in particular on Archimedes' killer, Jaeger shows how Roman authors tried to mitigate the political problem for Marcellus posed by Archimedes' murder by taking the emphasis off Archimedes' death and bringing forth Marcellus' grief. Thus, Cicero omits to mention the killer; Livy and Valerius say that he did not know who Archimedes was and blame Archimedes' own character for his death; Pliny blames the soldier's thoughtlessness; and Plutarch says that Marcellus later shunned him as if he were polluted. No one mentions the Roman soldier by name or describes the death either completely or directly. In effect, the anonymity of the Roman soldier, according to Jaeger, keeps Marcellus from being directly responsible for the killing and so Archimedes'

death comes to symbolize the end of old Syracuse and the beginning of a city that belongs to Rome.

Jaeger continues her analysis in chapter 5 by examining the siege of Syracuse. She argues that Plutarch uses this story and Archimedes' role in it to delineate Marcellus's character as philhellenic and to mark the limits of Roman Hellenization. Jaeger also deals with Archimedes' inventions for Syracuse's defense like the Big Ship and the Hand, highlighting the element of humor and surprise that these machines brought to those who came across them.

The same approach is followed when Jaeger analyses in the coda of the second part of her book Claudian's short poem from late antiquity on Archimedes' sphere. 'The sphere and the hand', she says,

> have in common the fact that they both record or anticipate the responses of those who see them in action, viewers who marvel at the movements of the machines and the genius of their maker. [123–124]

Chapter 6, the only chapter of the third part of the volume, deals with Petrarch's rediscovery of Archimedes in his works *De viris illustribus* and *Rerum memorandarum libri*. Jaeger shows how the anecdotes that she has discussed in previous chapters take new shape in these two prose works by Petrarch. Petrarch chooses to draw attention to different things and leave others in the background. He also questions and criticizes the ancient sources, showing how well he knows them. In this way, he presents himself as a figure between the distant past and posterity; as a collector, collator, and judge of texts who presents the positive results of his own historical and biographical research; and as a researcher working at the limits of the knowable. The themes of loss and recovery of intellectual tradition are once more in evidence.

Another theme that traverses the whole book is the idea that Archimedes was important to Cicero because he was important to Marcellus, and that he was important to Petrarch because he was important to Cicero. In general, there is an underlying argument according to which the figure of Archimedes was important to many later writers because he was important to Cicero, and his appearance in Cicero shapes the manner in which they use it.

Jaeger's analysis brings to light very promising details concerning Archimedes' 'after life' and concludes with very useful remarks.

She offers, for example, very interesting insights regarding the way in which biographers present their subject matter, emphasizing the importance of politics and ideology in writing biographies. She also pays attention to the emergence of a Roman cultural identity, which constitutes an important underlying theme of the book. She argues in particular that

> Cicero incorporates Archimedes' technology into his own program of creating an aristocracy of Romans linked not by noble ancestors but by intellectual achievement. [151]

In line with that, it would have been interesting to explore more works from the Roman period so as to have a more complete picture of what intellectual achievement meant for these people and how it was used for different purposes and with different outcomes. One also feels the need to go further regarding the audience of each work that Jaeger deals with and to say more about Archimedes' own work at his time as well as his specific agenda.

Another issue that could be looked into in more detail is that of patronage, when, for example, Jaeger examines the '*Eureka!*' story and Archimedes' revealing the fraud to Hieron. Although she notices that this story 'may reflect the tension of the early part of Hieron's reign, when he was securing his hold on the city and was perhaps more vulnerable to insult than he would have been later' [17], she does not take this remark any further. I think an analysis of the context and type of Hieron's rule along with an examination in depth of his relationship to Archimedes could bring to light interesting results. The need to explore this relationship of patronage becomes stronger when Jaeger categorizes Hieron and Archimedes as master and slave, accordingly arguing that Archimedes 'does not fit the image of the Greek intellectual' and his 'public nudity might have appeared memorably scandalous to Roman eyes' [7]. The scandalous and laughable picture of Archimedes as a running slave is not in my opinion convincing. Jaeger does not explore what the label 'Greek intellectual' means and does not deal with the relationship between the ruler and the scientist. As her account stands, the idea that Archimedes was of low status is hardy compelling. His identity needs to be explored further and used as a starting point for an account of what being an ancient scientist actually meant in those times.

The scientific identity of Archimedes comes up many times in the book, but Jaeger prefers not to give it too much attention. She states in the introduction that

> this book is not about math or the history of math; nor does it attempt to ascertain the historicity of the traditions about Archimedes or the nature of some of his inventions. [7]

Instead, she claims, it is a book about the way in which Romans used and reused Archimedes' story. However, the way in which the Romans used his story is solidly connected with Archimedes' status as a scientist and with the impact of his inventions. Archimedes was part of a scientific community and was discussed as such by later authors. Jaeger states that she wants to differentiate her approach from the ones that put the life of Archimedes in the larger context of the history of science. She says that

> when we examine this 'Life of Archimedes' with an eye directed less toward its science and more toward its rhetoric, we can perceive that we see the life of Archimedes only through the eyes of others, first Hieron and then Marcellus. [105]

My objection here is that Hieron himself was treating Archimedes as a scientist. It is hard to imagine otherwise, especially since Archimedes' after life is so much entwined with his achievements and his fame as a man of knowledge. Archimedes was useful to the king exactly because he was a scientist, a man of knowledge. It is because of this knowledge that Hieron needed and trusted Archimedes and that Marcellus gave an order not to kill him. Archimedes' scientific identity as well as issues of power and knowledge come out, then, as of great importance since rhetoric and science seem to go together.

*Archimedes and the Roman Imagination* is the first book to explore the after life of Archimedes and, although there is room for further analysis, it is a very useful work on the numerous Archimedeses that have come down to us. Both classicists and historians of science will find this book very interesting and helpful, and I am confident that the stories on Archimedes will stimulate their imagination as they did for his Roman descendants.

*Astrologia. Opere a stampa (1472–1900)* by Leandro Cantamessa

Biblioteca di bibliografia italiana 187. Florence: Leo S. Olschki, 2007. Pp. xxx + 1108. ISBN 978–88–222–5670–6. Paper €120.00

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Ce catalogue est l'une des premières bibliographies consacrées à l'astrologie et à ses oeuvres. La toute première bibliographie moderne exclusivement consacrée à l'astrologie, imprimée à Londres en 1911, a été élaborée par Frederick Leigh Gardner (1857–1930), un franc-maçon proche des cercles occultistes. Elle comprenait environ 1300 oeuvres dotées d'un descriptif minimal (sans mention des libraires et imprimeurs).

La bibliographie de Cantamessa couvre un peu plus de quatre siècles, du début de l'imprimerie jusqu'à la fin du 19e siècle, et contient 5049 notices pour environ 2500 auteurs. Près de 99% des ouvrages mentionnés sont en latin (26%), italien (21%), anglais (21%), français (14%), allemand (11%) et espagnol (6%). Quelques ouvrages en néerlandais, dans les langues scandinaves, et en portugais complètent la collection.

L'auteur s'est servi des ouvrages de ses prédécesseurs, bibliographes généralistes [Brunet 1810, Graesse 1859–1869, Houzeau et Lancaster 1887–1889, Caillet 1912, Palau 1923–1927, etc.) et historiens de l'astrologie [Boll et Bezold 1917, Thorndike 1923–1958, Carmody 1956, Capp 1979, par exemple], mais inclut dans ses références bibliographiques des ouvrages de second plan (anglais et italiens notamment) et ignore nombre d'études spécialisées qui contiennent d'importantes bibliographies, parmi lesquelles les ouvrages de Gustav Hellmann au début du siècle précédent, la *Geschichte und Bibliographie der astronomischen Literatur* d'Ernst Zinner [1941], *Tamburlaine's Malady* de Johnstone Parr [1953], *The Scientific Revolution in Astrology* de Mary Ellen Bowden [1974], *Le signe zodiacal du Scorpion* de Luigi Aurigemma [1976], ou encore *Le recueil des*


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*plus célèbres astrologues de Simon de Phares* de Jean-Patrice Boudet [1990].

L'auteur a inclu dans son corpus un certain nombre d'almanachs, notamment du 16e siècle, des éphémérides (qui constituent un outil de consultation privilégié des astrologues), ainsi que des critiques de l'astrologie: à raison car certains « anti-astrologues » sont souvent les auteurs d'exposés compréhensifs des théories de leurs adversaires. L'argumentation de Pic de la Mirandole a permis au plus grand astrologue italien du 15e siècle, Giovanni Pontano (1426–1503), d'affiner son point de vue.

Leandro Cantamessa est un collectionneur plus qu'un historien ou un bibliographe, et explique dans son introduction bilingue (italien-anglais) que sa passion lui est venue de sa mère qui lisait dans les années 60 des ouvrages d'astrologues français contemporains.

Pour chaque ouvrage référencé, sont indiqués les éventuelles rééditions, la localisation dans les bibliothèques publiques, ainsi qu'un commentaire plus ou moins étendu selon les cas. Les oeuvres en langue italienne sont généralement mieux renseignées.

L'auteur, qui s'est servi du CATAF (Catalogue Alphabétique des Textes Astrologiques Français) que j'ai édité en février 2001, et dont les entrées totalisent plus de la moitié des textes français qui ont été retenus, n'a pas éprouvé le besoin de mentionner l'adresse internet adéquate, ni même le nom du CURA.

Cantamessa précise qu'il n'a pas inclus les éditions des *Prophéties* de Nostradamus dans sa bibliographie, mais il a retenu en revanche, avec une certaine inconséquence, des ouvrages non astrologiques, d'auteurs qui ont interprété les quatrains des prophéties: Chavigny 1594, Chavigny 1603, « Jaubert » 1656, Jant 1673, Guynaud 1693, Le Roux 1710, etc.

En outre la plupart des almanachs et pronostications de Nostradamus n'apparaissent pas dans son recensement, alors qu'y figurent ceux de ses imitateurs tardifs: pas moins de 9 opuscules de Giovanni Maria Coloni, 3 de Marc Coloni, 21 d'Antoine Crespin, etc. !

Les ouvrages et opuscules concernant la littérature afférente à Nostradamus sont dans l'ensemble assez mal renseignés. La transcription des titres, les notes et les attributions sont extrêmement confuses: par exemple,

- Francesco Barozzi, commentateur d'un faux almanach de Nostradamus, et auteur d'un *Opusculum* sur la certitude des « sciences mathématiques » paru à Padova en 1560, n'est pas né en 1550, mais en 1537 [n. 348 et 349].
- Antoine Couillard [n. 1116]: Cantamessa confond ses pseudo *Propheties* (1556) avec ses *Contredicts* (1560): il s'agit de deux ouvrages différents !
- L'auteur, Laurent Videl, de la *Declarations* [*sic*] des abus ignorance [*sic*] et seditions de Michel Nostradamus [n. 1233], n'est pas identifié.
- Florent de Crox (n. 2356): Cantamessa qui ne signale que l'*Almanach* pour l'an 1586, se trompe sur le nom de l'auteur comme sur celui de l'imprimeur: « Florente de La Crox » (*sic*), et Antoine « Howic » [*sic*].
- Un opuscule concocté par un fraudeur (Mi. de Nostradamus le Jeune) est attribué à Nostradamus (n. 3183), ainsi qu'un opuscule de Crespin [n. 3187].

Il aurait été préférable que Cantamessa s'abstienne finalement d'aborder ce domaine spécifique de la bibliographie nostradamienne, ou appuie son recensement sur de meilleures sources que le CATAF (par exemple, Ruzo 1975, Chomarat et Laroche 1989 ou Benazra 1990 ignorés, ou encore la *Bibliographie du Corpus Nostradamus*, CURA 2006–).

Malgré ces insuffisances, l'ouvrage reste utile, bien que fermé aux nouvelles sources de documentation. En effet la numérisation des ouvrages anciens a connu ces dernières années un développement considérable (notamment en Allemagne et en Italie), si bien que l'on peut estimer à près d'un millier le nombre d'ouvrages et d'opuscules déjà numérisés à ce jour, soit près de 20% des titres présentés dans la bibliographie. Et bien évidemment la ressource en ligne remplace avantageusement pour le lecteur toute une série de localisations devenues accessoires. Ce projet est l'objet du DIAL (Digital International Astrology Library, http://cura.free.fr/DIAL.html), initié par moi-même en septembre 2008.

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*Philosophy and Exegesis in Simplicius: The Methodology of a Commentator* by Han Baltussen

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In *Philosophy and Exegesis in Simplicius*,[1] a preparatory study for a history of the ancient philosophical commentary [224nn10, 13], Han Baltussen addresses the 'methodology' of pagan antiquity's last major Platonist and its greatest philosophical scholar, Simplicius of Cilicia (AD *ca* 480–*ca* 540). What 'methodology' means can be best appreciated if the book's general conclusions are first summarized.

By laying 'special emphasis on the philological and historical features of a commentator who is often viewed as a mere mediator of earlier thinkers' [2], Baltussen finds in his voluminous works of Aristotelian exegesis a 'multi-layered, inter-textual, extravaganza' [90], 'a cornucopia of sources' [169], consisting of scholarship that went well beyond orthodox explications and analyses of texts to create a learned artifact reflecting ancient Platonism in its maturity. The whole exercise, 'incredible as this may seem' served 'a higher purpose, the preparation of the human soul to ascend to god' [169], being 'geared towards revealing an ancient spiritual wisdom by rational means ... a theology with philosophical underpinnings mixed with spiritual insights and religious rituals' [90], a 'pagan "gospel"' [209] sung by a 'great pagan choir of voices' [207] in a unison created by an 'extreme harmonization' (συμφωνία) of ideas from disparate

[1] Disclaimer: I am thanked on p. xi for 'advice or support'. I did not, however, see any part of the manuscript prior to publication except for a contents table, and my input was limited to the provision of some factual information [see 237n37] and one photocopied item, Hoffmann 2006.



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sources, all designed to match its Christian counterpart [86–87, 207].[2] Thus fortified intellectually, Simplicius, and a like-minded elite [181], undertook a 'rearguard action' [87] in a grossly mismatched battle with 'the ever-growing presence and impending victory of Christianity' [209]. Such, then, is Simplicius' almost tragic narrative, and when dramatized in evocative metaphors it reveals a commentator whose personality so often seems buried beneath his ancillary role [23, 133], much as his Christian contemporary, John Philoponus, has come to life over the past two decades, though on the basis of rather different evidence.

In this account, 'methodology' defines the nexus between scholarship (primarily the assimilation of sources and authorities) and a religious goal, not the procedures governing exegetical explorations of substantive philosophical issues.[3] We are indeed warned that to ignore the religious dimension of his program is to risk

> turning [Simplicius and other late ancient Platonists] into secular (analytical) philosophers,[4] whose philosophical *nous*

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[2] Golitsis [2008] now offers a well documented account of Simplicius' deployment of harmonizing strategies. Baltussen's appendix 3 [218–220] lists instances of the term συμφωνία in Simplicius, while his discussions in the main body of the book are sporadic and descriptive. One case in the appendix is not relevant: *In Phys.* 341.27 [218] refers to a consensus of views regarding the existence of τύχη, which is quite different from the harmonization of ideas involved elsewhere, when there is a *reconciliation* of often seemingly incompatible views.

[3] Baltussen [73] warns us that his discussion of Simplicius' attack on Philoponus for adopting Christian creationism will emphasize 'the religious nature of the motivation for this debate' rather than 'the interesting *philosophical* details' (Baltussen's italics). More generally, Baltussen [196] notes that 'Simplicius has been judged on his intentions rather than the results he offers in what he takes Aristotle to be saying' and defines his general purpose as being to show how 'exegetical strategies served [Simplicius'] philosophical outlook' [201]. *Philosophy and Exegesis* also contains nothing for the historian of science; the two references for 'astronomy' and 'science' in the subject index are insignificant.

[4] See p. 205 for similar language used to describe the bulk of recent scholarship on Platonic commentators, where allegedly they have been subject to '(analytical) philosophical investigations'. But that would not make the subject of such an investigations an '(analytical) philosopher'. Baltussen does not explain the brackets on 'analytical'.

> would make them seem interested only in analyzing the world through language and logic. [149]

But even so, religious values are not central to the text of his works, whereas his scholarly method (the 'philological and historical features' that Baltussen is targeting) is omnipresent. This contrast turns *Philosophy and Exegesis* in effect into two overlapping books—a painstaking and detailed analysis of Simplicius' method of handling his inherited materials blended with a straightforward assertion of the commentator's wider religious purpose. But the pains that have to be taken to complete the first of these are quite considerable when the author's chosen data base is over 3,000 pages of the Commentaria in Aristotelem Graeca containing Simplicius' commentaries on Aristotle's *Categories*, *De caelo* and *Physics*, even if the latter receives 'particular emphasis' [8].

*Philosophy and Exegesis* begins with an introduction and opening chapter that includes basic bio- and bibliographical information [12–14; resumed at 48–51],[5] Simplicius' early career, and no mention of the possibly spurious commentary on the *De anima* attributed to him.[6] After a selective review of the literature stressing the negative or one-sided attitudes towards this commentator that Baltussen thinks still prevail [2–8; see my Additional Note 3, p. 220 below], there is a preparatory survey of exegetical goals [33–38] and of scholarly techniques (the use of manuscripts, textual criticism, the deployment of quotations) [38–48].

For a book ostensibly concerned with the 'intellectual framework' [14] of its author's writings, Simplicius is introduced in surprisingly generic terms on the opening page as 'one among a group of late

[5] The second of these goes unnecessarily deeply into the drawn-out debate over where Simplicius returned after his departure from Athens *ca* 531–532. Baltussen is open-minded about the actual location but inclines to its being Athens [204].

[6] The abbreviation '*in DA*' is included in the list on p. viii, and the bibliography includes some items that address its authenticity, yet this work is not cited in the *index locorum*. This should be collected with other complaints about the indexing.

Platonists who lived and worked in Alexandria and Athens' [1], with no mention of Philoponus here or when Ammonius (who taught them both) is first introduced [12] and then later profiled [163]. Philoponus appears on page 43 without a word of introduction (and with no dates given until page 176).[7] Yet Simplician methodology could have been profitably compared from the outset with Philoponus', particularly in his commentary on the *Physics*.[8]

Chapters 2–5 are the core of the book. They focus on the players in Simplicius' exegetical extravaganza (for some of whom he has famously become our only source) and explore his methods of citing and using them. These chapters, any one of which could easily be enlarged into a monograph [see 55], proceed chronologically:

2 the Presocratics (principally Parmenides, Empedocles, and Anaxagoras)
3 the early Peripatetics (principally Theophrastus and Eudemus)
4 Simplicius' 'prototype' [121], 'benchmark in commentary composition' and 'beacon in navigating the Aristotelian text' [135], Alexander of Aphrodisias, the epitome of Peripatetic orthodoxy
5 the Platonists of the centuries between Plotinus and Simplicius' teacher at Athens, Damascius

Chapters 3 and 4 reflect the evolution of the Peripatetic school in antiquity, but no attempt is made to explain why Alexander is 'a died-in-the-wool (*sic*) Peripatetic' [107] in contrast with less dogmatic earlier members of that school [105–106], except for the unexplored suggestion that 'a shift in the first century BCE' [106] produced 'the notion of a canon as established doctrine'. More should

[7] *Philosophy and Exegesis* could have benefited from a prosopographical appendix: Ammonius is assigned dates on his 12th appearance, Iamblichus on his 10th.

[8] Golitsis 2008 adopts such an approach. On the general contrast between Simplicius and Philoponus, see Wildberg 1999, 120–121 and my forthcoming review of Golitsis [Todd 2009].

be said here.[9] By contrast, the evolution of Platonism between Plotinus and Damascius is handled in some detail, albeit in a derivative [137] survey [147–164].

The book concludes with a final chapter on the rhetorical aspects of Simplicius' polemic against Philoponus on the eternity of the world (really a side-issue in a study of basic methodology, and just a way of re-emphasizing Simplicius' paganism) and an epilogue. There is no separate chapter on Plato, whose works are cited only 18 times (though the role that the *Timaeus* plays in Simplicius' reconfiguration of the *Physics* and *De caelo* could be the subject of a monograph)[10] nor on Aristotle (19 references),[11] despite Simplicius' interaction with him being the basis of his methodology.

Baltussen's own methodology, background surveys aside,[12] is to list examples illustrating various Simplician procedures in relation to the wide range of authorities and sources that he addresses. Sampling is inevitably selective and texts are often rather briefly treated. The whole process leads, as Baltussen repeatedly acknowledges [e.g., x, 107, 108, 134, 137, 165, 170] only to tentative or preliminary conclusions, or just to daunting statistics [64, 109, 118, 128–129, 154, 199, 255n10] inviting further research, or in one striking case to an unanalyzed bar graph [217] of the distribution of references to Alexander

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[9] Baltussen does not, for example, consider the evolution of the philosophical commentary as part of the return to authority that some scholars have recently seen as occurring in the first century BC: see Falcon 2008, 7–10 for a useful orientation to the literature on this issue. Baltussen's brief references [26, 88, 106] to the creation of a Peripatetic canon lack any precise historical focus.

[10] See Guldentops 2005 (not cited by Baltussen) for a study of its role in Simplicius' critique of Alexander in his commentary on the *De caelo*. Gavray 2007 is a recent study of Simplicius' use of another Platonic dialogue, the *Sophist*.

[11] One of these, 279b17–2 [191], should be to the *De caelo*. The *index locorum* misses this and two additional references: *Phys.* 189a32 [119] and 251b15f. [219]. See 218n24 below.

[12] See pp. 24–27 on the evolution of the commentary, pp. 173–182 on the history of philosophical polemics, especially between Christians and pagans, and pp. 140–158 on Platonism between Plotinus and Proclus.

in the *Physics* commentary by units of 25 pages rather than by the content of the Aristotelian and Simplician treatises.[13] Sometimes we are offered the minute detail usually found in formal commentaries;[14] at other times, lists of references.

The evidence canvassed is more accessible when consisting of 'programmatic statements' [16] than where specific loci are used to illustrate methodology. This is because by being invariably detached from their exegetical context they are difficult to assess[15] and open to misinterpretation.[16] Aristotelian *lemmata* are, as far as I can see, pro-

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13 The fewest references (less than five) are at *In phys.* 625–650, all but five pages of which are taken up with the *Corollarium de loco*—which would exclude Alexander because this digression was not exegetical in nature. The next section [650–675] covering *Phys.* 4.6–8 on the void has no references to Alexander at all, perhaps because Simplicius had cited Alexander fully on this subject in his earlier commentary on the *De caelo* (on its date see Golitsis 2008, 18n38) at 285.2–286.27, an important item in Simplicius' Alexandrian material, which Baltussen overlooks in his ch. 4. The most references (over 30) are on pp. 700–725, which address *Phys.* 4.10–11 [218a31–219b33] on time, a subject in which Alexander had a special interest [see Sharples 1982]. I offer the preceding as a specimen of the work that Baltussen's minimal bar graph has left to his readers.

14 See, for example, the analysis of the quotation on p. 165.

15 On p. 118 there is a reference to Simplicius' report of Themistius' disagreement with Alexander on the issue of instantaneous change in *Phys.* 6.4; on this see my translation of Themistius' paraphrase of this chapter at Todd 2008, 50–51 with nn309–310. Baltussen's failure to identify the relevant Aristotelian context renders his comments almost incomprehensible. See also p. 191 where the Aristotelian context is defined but as 'part of the concluding section discussing time in *Phys.* 4', which is little help except to patient readers with a text of the *Physics* to hand. The same goes for the vague references to '*Phys.* A7' (actually 189a11–14) at p. 119 (where there is also no reference for the Simplician passage [192.14–21] translated except for an interpolated '192.20') and that to '*Phys.* 6' on p. 164.

16 For example, I doubt that *In Phys.* 193,16–19 has anything at all to do with 'the late technical rephrasing of Platonic doctrine in a new framework' [161]. It would seem only to record a disagreement with Syrianus about what the 'contrariety' (ἐναντίωσις) said to be 'in every γένος' (i.e., in every category) involves. Syrianus had argued [at 129.29–32] that there was a single contrariety based on excess and defect for every category; and in the passage that Baltussen cites, Simplicius is responding to this by saying that 'contrariety is proprietary to quantity alone' if we take 'excess' and

vided only at pages 77, 81, and 151; and their absence raises not just a trivial issue of documentation but one of principle, since the effect of Baltussen's discussion of these passages is to treat the commentator's discussions in isolation from their basic exegetical purpose [see also my Additional Note 4, p. 220]. Baltussen realizes the need to take account of the context in which Simplicius quotes and cites authorities [e.g., 15, 55] but equally important is the broader exegetical context that is the basis for this activity.[17] Fewer samples more closely scrutinized would have made such contextualization possible and also allowed Baltussen to explore rather than skirt philosophical issues excluded by his restrictive notion of methodology. The structure and character of the three commentaries utilized is also not well defined.[18] Probably just Simplicius' most important commentary, that on the *Physics*, should have been Baltussen's main focus in a study of this length; certainly the survey of it at pages 34–38 is, for example, too brief.

Baltussen is at his best in playing to his pre-established strengths in dealing with Simplicius' reception of source material, especially where direct quotations are involved. The most famous of these are from the Presocratics in the commentary on *Physics 1* (of which unfortunately no English translation is currently available), and pertinent criticisms are offered of the principles governing Diels' collection

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'defect' in a strict sense, because they exist derivatively in other categories due to quantity. Baltussen's translation, 'the appropriate antithesis would belong to quantity alone', obscures this reasoning. 'Appropriate' (οἰκεῖος) is predicative here, and is used in the sense of unique (equivalent to ἴδιος). Also, the presence of the terms 'excess' and 'defect' is due to their presence in the Aristotelian text at *Phys.* 1.4, 187a16–17, which is recalled in 1.6 at 189b10–11.

17 See Todd and Bowen 2009, 167–175 for three passages from Simplicius' commentary on the *De caelo* translated along with their Aristotelian *lemmata* to contextualize reports of Heraclides of Pontus' theory of the rotation of the Earth. Baltussen might have offered similar examples to show the full range of interaction between Simplician methodology and its exegetical context.

18 The 'headings' (κεφάλαια), notably the 'goal' (σκοπός), by which the *Physics* is analyzed at the outset of the commentary are dealt with piecemeal at pp. 37, 42, and 116–117, with some historical background at 145, and the σκοπός of the *In de caelo* discussed at p. 160; contrast Golitsis 2008, ch. 2, where these propaideutic classifications are handled systematically for the *Physics* commentary.

of evidence [63–44, 72].[19] The treatment of the Simplician reports of Theophrastus and Eudemus, on which Baltussen also has a proven track record, is also effective. But the practical problem, as already indicated, is that it is difficult to engage there or elsewhere with Baltussen's detailed discussions without determining the relevant Aristotelian *lemmata* and in many cases checking ancillary texts. In fact, something like Simplicius' library, which Baltussen tries to reconstruct in his first appendix [211–215],[20] is required. Quotations are certainly too few and too brief (I counted 56, most shorter than 10 lines), often in borrowed translations[21] or questionable ones;[22] and there is no complementary appendix of annotated translations, as is standard in studies of a less familiar author like Simplicius [see Gavray 2007 and Golitsis 2008].

*Philosophy and Exegesis*, then, is a challenging book to use and also not an easy one to read, too often wordy, sometimes repetitious,

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19 Lewis 2000, 10–12 should have been cited for his attempt to identify a new 'B' fragment of Anaxagoras at Simplicius, *In phys.* 164.20–22.

20 Baltussen includes references to several works that are inherently implausible candidates for the Simplician bookshelf. Also, titles are given mostly in English, but some in Latin and Greek, and one (Alexander, *De mixtu* rather than *De mixtione*) in an unorthodox form. Themistius' paraphrase of the *Categories* (cited at Simplicius, *In cat.* 1.1. and 1.9) is omitted, as, more pardonably, is a hidden reference to Ptolemy, *Almagest* 1.7, 24.7–10 at *In de caelo* 445.1–2 in a partial quotation [see Todd and Bowen 2009, 175]. Baltussen does mention on p. 36 that 'Ptolemaeus' (*sic*) is cited at *In de caelo* 9.29, and so it is surprising that he is omitted from the 'library'.

21 This can be risky [see also 219n26 below]. Thus, on p. 157 Baltussen cites *In phys.* 611.25–26, a reference to Proclus, as 'he expounded his opinion clearly and expertly'. But this is the late J. O. Urmson's incorrect translation [1992, 32] of the second adverb, συνῃρημένως, which means 'in comprehensive terms'. It is συνηρτημένως that means 'expertly' and since it is just four items down the page at *LSJ* 1716 col. 1, the translator's eye may have fallen on it mistakenly.

22 On p. 108, for example, δι' ἐνδόξων at Alexander, *In top.* 27.10 is oddly translated 'through what is approved' (rather than, say, 'through reputable opinions'); and on p. 73 the contrast between things that are φυσικά and those that are ὑπὲρ φύσιν at Simplicius, *In phys.* 21.17 is blunted by translating the latter as 'those above nature' and the former as 'physical things' rather than 'natural things', or 'the realm of nature'. Finally, on p. 218 τύχη is unusually translated as 'fate'.

with too many overloaded paragraphs and too much untranslated transliterated Greek, and defects that cannot be overlooked in its bibliography[23] and indices.[24] It will not be the widely 'accessible' work that Baltussen initially hoped to produce [ix], and determined specialists may want to consult it selectively. Its vision of Simplicius as a religiously engaged scholarly exegete was well worth displaying but perhaps not in the context of a book that needs to go in so many other directions.

Finally, there are five general topics on which I take issue with Baltussen's treatment and now append comments.

[23] It cites but omits some commentaries in the Ancient Commentators on Aristotle series ('Gaskin 2000' at p. 99 and 'Mueller 2004' at p. 131 have no entries); A. Graeser is morphed into 'A. Gaiser'; Gersh (1992) is cited in abbreviated form on p. 3 but not included; Sharples 1990 is not 'The School of Alexander of Aphrodisias' but 'The School of Alexander?'; and the two editions of 'Ong, W. J.' are confusingly cited separately. The system of multiple entries for a given year breaks down: 'Luna 2001' [50] should be '2001c' and the review by R. Netz of 'Mansfeld 1999' [273] should be of 'Mansfeld 1999a'.

[24] On p. 287, the last three references from the *In de caelo* are to the *In physica*. The index of names is highly selective for modern names, omits Plato, and gives only two references for Aristotle. The index of subjects and terms omits οἶμαι [129, 199] (part of an important discussion of Simplicius' modes of self-expression) and ὑπόνοια (discussed as part of the background to the evolution of exegesis on p. 25), and also has nothing under 'Christianity', 'religion', and 'rhetoric', yet manages to include 'anonymous commentator' when this is in fact the *Anonymous Commentary on the Theaetetus* listed in the *index locorum*. See also 214n11.

ADDITIONAL NOTES

1. *Orality*

Baltussen [47–48: cf. 86] considers rationalizing Simplicius' penchant for quoting the original words of a source as an analogical application of Plato's preference for the spoken over the written word: as he says,

> Could Simplicius' emphasis on the original words perhaps be inspired by the thought that teaching by the living voice (*viva voce*) was superior to writing, as was suggested in a programmatic way by Plato (*Phaedrus* 267–268: cf. *Seventh Letter* 342d–444d)?

A resounding 'no', surely, if Baltussen wants to use that capacious term 'inter-textuality' [1] to describe Simplicius' use of inherited material as the creation of 'books about books'. In a literary culture, quotation can hardly be rationalized as quasi-oral. Baltussen's later speculation [53] that Simplicius' quotations were 'based on an acute awareness of his scholarly responsibility for future generations' seems preferable, since it at least reflects the actual result of his efforts.

2. *Target audience*

This is speculatively identified as, among other possibilities,[25] 'future teachers', for whom the commentaries are intended as 'almost the equivalent of an elaborate textbook' [22: cf. 201, 206]. But Simplicius himself *explicitly refers* to an intended audience, in language that Baltussen (unlike Golitsis [2008, 18]) misses, as 'readers' or 'future readers' (οἱ ἐντυγχάνοντες, οἱ ἐντευξόμενοι).[26] This usage allows the commentaries to be identified securely as literary works

[25] On p. 51 Baltussen speculates that Simplicius' isolation in the 530s means that 'he could have been writing for an imaginary student body'. The subject index has no entries under 'education' or 'teaching'.

[26] See *LSJ* ἐντυγχάνω III for this use of the verb. At pp. 130 and 199, Baltussen relies on translations that take this verb in the generic sense of 'encounter', though on p. 43 he cites a translation that does render it 'read'. He himself uses 'those who encounter' at p. 192 in a translation of the (unreferenced) *In de caelo* 298,21–22, thereby missing the significant *future* participle, οἱ ἐντευξόμενοι.

directed to an informed audience of readers, whatever professional identity unfounded speculation may suggest that they had.

3. *The negative image*

- The image against which this book is reacting is not usefully constructed even partially from Galileo's Aristotelian Simplicio in his *Dialogue Concerning the Two Chief World-Systems* (1632) [see 3–4: cf. 209] since in the preceding century Simplicius was a respected authority in Aristotelianism.[27]
- Baltussen is not, as he often seems to imply, the first scholar to protest Simplicius' being represented as merely learned. The late Henry (not 'Henri', as on page 4) Blumenthal in an article that Baltussen does not cite argued that

  > it is necessary to take account of the ideas and purpose of these [i.e., Neoplatonic, but particularly Simplicius'] commentaries if one is to make any serious critical use of their work [1976, 64]

  and added that this could not be done 'if one merely dips into their voluminous works in the hope of occasional enlightenment'. Here is the essential rationale for Baltussen's project articulated in 1976, though probably an invitation to more 'serious critical use' of Simplicius than uncovering his religiously motivated scholarly method.
- Baltussen is surely correct in saying that Simplicius has received more attention as the study of later Platonism has expanded during the 20th century; but his brief sketch of the revival of Neoplatonic studies on pages 4–5 is selective and superficial, and neglects recent relevant secondary literature [see Hankey 2005, 2007; and Todd 2005].

4. *Exegesis and paraphrase*

The term 'paraphrase' [27, 164] used to describe Simplicius' treatment of Aristotelian texts is perhaps best confined to exercises,

[27] See the still authoritative study by Nardi 1958. Recently Mueller [2006, 200n71] has identified eight editions of Simplicius' commentary on the *Physics* for the period 1526–1587. There is, as Mueller notes, no study of this commentator's *fortuna* in the Renaissance. I believe that an entry for the Catalogus Translationum et Commentariorum has been long delayed.

like Themistius' in the fourth century,[28] in which the commentator restates the *whole text*, almost entirely in the persona of Aristotle. This process is significantly different from that by which Simplicius, like Alexander before him, guides the reader to an understanding of a *finite text* (a *lemma*) and its relationship to associated texts in the given work as well as to other Aristotelian works.[29] *En route* Aristotle's words may well be restated but they are not invariably or systematically paraphrased or epitomized, and the structure of passages necessarily cannot be adjusted in this format as they can in paraphrases proper. Baltussen calls Simplicius' attempts to 'clarify' [21, 90] the Aristotelian text 'his immediate objective' [85: cf. 90], the bedrock perhaps of the multiple layers of Simplician exegesis; but as such it is just as crucial to his methodology as are quotations and citations of other authors. To say that it consists of 'expansive but straightforward paraphrases' [162, 164] oversimplifies matters and highlights the problem created by pretty much excluding Aristotle from a book on an Aristotelian commentator's methodology.

5. *The 'religious dimension'*

If this is indeed central to an understanding of Simplicius' exegetical procedures (and what prevents him from being taken for an '(analytical) philosopher'), then the moving and eloquent prayer to the Demiurge at the end of the commentary on the *De caelo* [731.25–29][30] should surely have been cited. It is a much better

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[28] Simplicius' references to this commentator are briefly discussed at 166–167; but Baltussen does not acknowledge the numerous cases of the tacit incorporation of material from him, which are easily traced in the Commentaria in Aristotelem Graeca edition of Themistius' *Physics* paraphrase. Also, in summarizing Simplicius, *In phys.* 1051.9–13, Baltussen [41] misses its evidence that Simplicius may have had two copies of Themistius' paraphrase of the *Physics*, an important indicator of his philological method [see Golitsis 2008, 69 with n12].

[29] For some careful work on the role of paraphrase in the context of lemmatized exegesis in Alexander, see Abbamonte 1995 and 2004. The relation between paraphrasing and lemmatized exegesis was already acutely analyzed *ca* 1300 by the Byzantine monk Sophonias: see the proem to his paraphrase of the *De anima* 1.4–3.9 (Commentaria in Aristotelem Graeca 23.1).

[30] This is my translation:

example than the alleged prayer from the *Physics* commentary discussed at pages 182–183.[31] Here Baltussen's quotation of *In phys.* 5.17–26 is selective and omits a key sentence [5.20–21] in which natural philosophy is said to merit practice because of the awe felt for nature as a result of knowledge of its workings, with the verb for 'practice', ἀσκεῖν, identifying exegesis as a spiritual exercise rather than prayer. This passage is also not identified as part of Simplicius' preparatory study of the utility (τὸ χρησιμόν) of the *Physics*; contrast Golitsis [2008, 53–55] who is fully mindful of this context. Religion is also linked with exegesis in the conclusion of this passage [*In phys.* 5.23–26] when Simplicius quotes part of the opening sentence of the *Physics* [184a12–14] to extract religious implications from Aristotle's advocacy of knowledge of the first principles of nature before subsequently explicating the same text [11.32–12.14] by citing Alexander and Plato. Baltussen's method of atomized sampling militates against identifying this kind of instructive ramification.

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*LSJ* = Liddell, Scott, and Jones 1940

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O master of the whole cosmos and demiurge of the elements in it, I convey praise on these things for you and for the things that have come into being through you, being zealous to behold (ἐποπτεῦσαι) the magnitude of your deeds and to reveal (ἐκφῆναι) them to those who are worthy, so that by reckoning nothing petty or human about you we may worship you in accordance with the eminence that you possess in relation to everything created by you.

Baltussen makes some general references to Simplician exegesis as the revelation of mysteries [e.g., 198, 208]; here is language to support them.

[31] Further, in the area of religion, Simplicius, *In phys.*1360,24–25 is said to involve 'reverence and worship' [183]; but what Simplicius says there, on the basis of passages in *Meta.* Λ [see McKirahan 2001, 151 with nn565–567] is that *Aristotle* praises 'the prime mover as mind, eternity and god'. Baltussen says that the verb used for 'praise' is ὑμνεῖ (it is in fact ἀνυμνεῖ, which is not readily determined thanks to the absence of any reference for this passage), but the use of this verb in this context to describe *Aristotelian* language does not in itself tells us anything about Simplicius' attitudes.

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*Seeing the Face, Seeing the Soul: Polemon's Physiognomy from Classical Antiquity to Medieval Islam* edited by Simon Swain

Oxford: Oxford University Press, 2007. Pp. x + 699. ISBN 978–0–19–929153–3. Cloth £95.00, $199.00

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This is undoubtedly the most important volume produced on the subject of physiognomy—the science of assessing personal character through the observation of external physical features—since Foerster's *Scriptores physiognomici graeci et latini* [1893]. Two influential ancient Greek treatises on the subject are extant: one attributed falsely to Aristotle and the other written by the scholar and politician Polemon (*ca* AD 88–144). Polemon's work is lost in its original form, but survives in abridgments in different languages.

*Seeing the Face, Seeing the Soul* deals particularly with Polemon and his treatise. After Swain's helpful orientation to the sources and earlier scholarship in the introduction [ch. 1], the volume presents several detailed studies that situate physiognomy in the contexts of ancient Greek and Roman philosophy, society, and visual culture, and in its important Arabic reception. The essays range far and wide enough to make physiognomy a relevant matter for many areas of intellectual and cultural history in which it is often not normally taken it into account. They show that the extant treatises on physiognomy are important sources of information about the customs and manners of the societies in which they were produced and reproduced. Together these historical-contextual essays constitute two of the three sections of the work: 'Antiquity' and 'Islam'.

The third section, 'Texts and Translations', filling about half the volume, presents the most important primary sources for Polemon's work in Greek, Latin, and Arabic, along with facing English translations. The Greek and Latin texts are basically reprints of Foerster's texts with some new notes added, but none of them have been translated into English before. Although the more important


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of the two Arabic texts has also been printed before in the edition of Georg Hoffmann [in Foerster 1893, 1.93–294], Robert Hoyland's new edition here, based on a fresh consultation of the unique manuscript, is clearly superior for reasons to be explained below. All in all, this is a large amount of material to digest, with new contributions by six authors.

In the 'Antiquity' section there are three essays. The first [ch. 2], by George Boys-Stones, is a monograph-length search for the roots of physiognomy in the ancient philosophers' views on the relationship between the form of the soul and the form of the body. Boys-Stones' approach is rigorously philosophical and he aims his presentation at scholarly readers who know already about the texts which he discusses. The conclusion is not that physiognomy 'influenced' philosophers, but rather that physiognomy in certain instances was invoked to support a theory about the soul. Thus, the investigation into the ancient views on the relationship between bodily and psychic forms has less bearing on physiognomy than one may have guessed, but demonstrates the sort of discussions that formed a context for the genesis of physiognomy in the first place. As an essay, this chapter will be independently of interest to historians of ancient philosophy.

In chapter 3, Swain introduces Polemon and his *Physiognomy* by focusing on Polemon's historical context, the second-century Mediterranean society in which he lived and wrote. We find Polemon as one of a set of smart gentlemen seeking the patronage and largesse of Roman emperors, men who have to know just what to say at the right opportunity in order to acquire and maintain status and privilege. These men were subjected to intense personal scrutiny while delivering their orations, and Polemon's system of physiognomy makes sense when understood as an instrument of that sort of social scrutiny. Swain covers a lot of territory: patronage, the culture of politeness that characterized the elite society of the 'second sophistic', sex, gender, and family norms in the Roman Empire, and thinking about 'ethnic purity' among Greeks in this time. He contextualizes not only Polemon but also authors such as Fronto, Plutarch, and Bryson. Swain is to be congratulated as one of the few classical scholars to do something useful with Bryson, an author whose first-century work, surviving only in Arabic, was made available decades ago, but which classical scholars have largely overlooked. (Swain alerts the reader to another collaborative project of his, in preparation, focusing on

Bryson.) In the end, one takes the impression that Polemon's *Physiognomy* was the work of a nervous backbiter keen to use his claimed scientific expertise in the analysis of character types to make allies and to shame enemies.

Jaś Elsner's contribution [ch. 4] is the shortest. It asks whether physiognomical concerns influenced the visual representation of men and women in the Roman Empire, primarily in statuary but also in painting. The question is interesting, but unfortunately it proves to be too difficult to find definite correlations between the physiognomical treatises that survive and the material representations under consideration. The main problem is that the representation of persons in sculpture aimed to flatter, praise, and magnify, whereas physiognomical analysis tended to find fault. These different purposes entailed different sorts of attention to the human face. But Elsner does find a bit of evidence that Polemon wrote his physiognomy while having in mind certain examples of famous portraits, such as that of Alexander of Macedon. Also included is a brief discussion of Polemon's terminology for eye colors.

The first of two contributions by Robert Hoyland is 'The Islamic Background to Polemon's Treatise' [ch. 5]. This begins with a general introduction to the reception of Greek and other ancient works in Arabic translation, following Dimitri Gutas' standard monograph, *Greek Thought, Arabic Culture* [1998], very closely. Then Hoyland turns to Polemon in Arabic tradition. Given the testimonies of Arabic authors, the text must have been translated into Arabic by the middle of the ninth century. The name of Polemon was relatively little known and, when known, it was from his work in translation. The Arabic word used for physiognomy is *firāsa*, but Hoyland does not make a special inquiry into the meaning of the term—which refers fundamentally to discrimination through scrutiny—leaving the reader to infer the reason why this word in particular was adapted as the special name of the technique taught by Polemon. There is plenty of material here from which to make the inference: information about Polemon's physiognomy in Arabic tradition is relatively scanty (covered in three pages) when compared with the abundant collection of references to and excerpts about *firāsa* that Hoyland has assembled from Arabic texts (filling 70 pages). All this raises some important questions that are not asked or answered. To what degree is *firāsa* in Arabic really a continuation of ancient Greek physiognomy

as exemplified in Polemon's treatise? Or is it rather that Polemon's work was absorbed into a preexisting set of established practices, already called *firāsa*, which were thereby elevated to the rank of an ancient science? Where and when was Polemon's physiognomy identified with the proverbial divine *firāsa* of prophets and mystics, from whose authority *firāsa* became an 'Islamic science'? The rich survey of materials, obviously the result of very wide research, is, however, presented according to themes, and not chronologically, thereby obscuring the history of *firāsa* as such and hindering attempts to answer these questions. Above all it is the choice to see physiognomy in Arabic as a part of an implicitly uniform 'Islamic civilization', to which testimonies from hundreds of years and thousands of miles apart are all equally relevant, that prevents the potential of the data from being realized. This chapter, as a wide-ranging collection of thematically organized source information, will form the basis for a future historical study of *firāsa*.

Antonella Ghersetti's first contribution to the volume, 'The Semiotic Paradigm: Physiognomy and Medicine in Islamic Culture' [ch. 6], focuses on physiognomy and its relationship to medicine as it was understood in Arabic. We are informed that the majority of authors of Arabic treatises on physiognomy were physicians. Ghersetti points out that Avicenna (d. 1037), in his classification of the sciences, puts physiognomy together with medicine in the 'second rank' of natural sciences as he classified them. This proved to be influential with later Arabic authors who likewise saw medicine and physiognomy as sharing the same 'semiotic paradigm and inferential procedure' [286]—that is, the observation of signs in the body—therefore putting the two types of knowledge in the same class. The connection between medicine and physiognomy began much earlier than Avicenna. The *Physiognomy of Pseudo-Aristotle*, in its Arabic translation by Ḥunayn ibn Isḥāq (d. 873), appears to have been more influential here than Polemon. Rhazes (d. 925 or 935) and Fakhr al-Dīn a-Rāzī (d. 1209), among others, are given as examples of authors who discussed the underlying theoretical bases of physiognomy and treated it as a part of medicine. Like the preceding chapter, this one presents a wealth of information about the occurrence of physiognomy in learned treatises of several types.

We return to Polemon in 'Polemon's Physiognomy in the Arabic Tradition' [ch. 7], also by Ghersetti, with a comparative chart

and further notes added by Swain. The body of the chapter is a very useful inventory of the manuscripts of the Arabic Polemon, of pseudo-Polemon, and of earlier printed editions. The manuscripts and texts are described in sufficient detail, leading to the conclusion that there are two types of the text surviving in Arabic, both based on the lost original Arabic translation of Polemon: that found uniquely in Leiden MS Or. 198, which appears to represent the Greek original with relatively high fidelity but not without alterations, and the highly adapted and renovated 'TK' tradition (TK for the Topkapı Sarayı, where two of the manuscripts of this type are found; it is also called 'the Istanbul Polemon' in this volume). Quotations of the text in the TK type were made by al-Dimashqī (d. 1327), providing a *terminus ante quem* for the reworking. Pseudo-Polemon is the name chosen here to refer to manuscripts bearing Polemon's name but dealing with other material, which prove thus to be irrelevant for the rest of the study. As for the two recensions of Polemon in Arabic, the comparative charts of their contents here provide an essential tool for dealing with both of them and for relating them to the surviving Greek abridgment.

The third part of the volume provides the texts derived from Polemon in all these languages, along with English versions. A brief orientation is in order. Again, Polemon's work does not survive in its original Greek form. In Greek, we have only an abridgment of the work by the probably third-century Adamantius (as well as a later abridgment of that abridgment). There is an anonymous Latin work of physiognomy based explicitly on the earlier works of Loxus, (pseudo-)Aristotle, and Polemon, but apparently using Polemon the most. Then, there are the two Arabic recensions of the lost Arabic translation of Polemon's work, mentioned above. All of these Greek, Latin, and Arabic texts are witnesses to Polemon's original work; where they are in harmony, we can be fairly sure that we have arrived at true Polemonic material. The volume under review presents all of these texts individually with facing English translations.

Chapter 8 ('The Leiden Polemon'), that is, the text of Polemon as found in the Leiden manuscript, is edited by Hoyland with facing English translation. The previous edition by Georg Hoffman treated the text with a heavy hand, 'correcting' it, sometimes bizarrely, to conform with the known Greek and Latin abridgments, on the assumption that they were truer to Polemon's original. Hoyland clearly

shows that this assumption was often false (as Hoffman's readings in general sometimes were), and in doing so he provides dozens of improvements to the text. The Leiden text must be accepted as it is as a witness to Polemon's original work; and, in the end, it appears that this 'Leiden Polemon' is the single most important witness to the contents of Polemon's treatise. Hoyland accordingly reproduces the text of the manuscript with a minimum of editorial interference. In this he goes perhaps to another, albeit preferable, extreme, in retaining even non-classical, 'incorrect', forms in the Arabic text, contrary to normal editorial practice, and unnecessarily including trivial data about the manuscript pointing. Hoyland thoughtfully provides cross-references to each section of the Greek of Adamantius and to the corresponding folio and line numbers in the TK manuscripts, as well as to the excerpts of TK in al-Dimashqī. The latter cross-references would be more useful if we had an edition of TK in its entirety [see below], but the future editor of TK will appreciate the notes. Hoyland does break from his strict adherence to the Leiden MS when he interpolates a paragraph from TK where Leiden lacks it [340–341], because of evidence that this paragraph was in the original Arabic translation of Polemon's Greek; the insertion is clearly marked as extraneous to the manuscript.

Chapter 9 ('The Istanbul Polemon' or 'TK recension') is edited by Ghersetti only in part because the contributors 'did not think it worthwhile' to include the text in its entirety given that 'the extent of the rewriting... takes us away from Polemon'; instead we have only the lengthy introduction of TK 'as evidence of the importance of Polemon' in the Arabic tradition [5]. As Ghersetti explains [465], even this partial text is 'not a critical edition' because this recension has such a large number of textual variants between witnesses as to make such an edition impractical. What we have is a publication of the introduction of the text in MS Topkapı Ahmet 3.3207 as collated by Ghersetti with Topkapı Ahmet 3.3245. The text includes a legendary account of Polemon's encounter with, and physiognomical assessment of, Hippocrates. (This story is based on an ancient account originating in Phaedo of Elis' lost dialogue *Zopyrus*, where it is Zopyrus who physiognomizes Socrates [23, 282–285].) It also includes remarks on the theoretical basis of physiognomy, exemplifying Ghersetti's argument in chapter 6.

Chapter 10, Adamantius' Greek abridgment, is presented in Foerster's edition with the new English translation provided by Ian Repath on facing pages. In combination with the Arabic of the Leiden recension, this text brings us apparently quite close to Polemon's original, but it excludes much of Polemon's anecdotal material that is found in the Arabic. For what it is worth, the Arabic manuscript of Leiden, copied in Damascus in 1356, is slightly older than all the Greek manuscripts of Adamantius, which were copied in the 15th and 16th centuries.

In chapter 11, the Anonymous Latin *Physiognomia*, written perhaps near the end of the fourth century and based in part on Polemon's treatise, is presented also in Foerster's edition with the facing English translation of Ian Repath. Repath provides cross-references to Adamantius.

Finally, the volume includes an appendix presenting the Greek of pseudo-Aristotle's *Physiognomy*, again reprinted from Foerster's edition, with a facing English translation reprinted from Jonathan Barnes, [1984, 1.1237–50] (both text and translation with comments and modifications by Swain). The Arabic translation of this Greek text has been previously published by Ghersetti [1999].

Only the Arabic texts here are truly new in the sense that they are not reprintings of earlier editions and are based entirely on the fresh inspection of manuscripts. Lacking new critical editions of the Greek and Latin texts, it is nevertheless useful to reprint Foerster's editions for those without access to the Teubner texts. The English translations of the Greek and Latin texts are good overall and will serve further research into this subject.

The collection of materials in this volume could easily facilitate further studies on Polemon's text. For example, there is room now for a more detailed attempt to reconstruct what can be known about the contents of Polemon's lost treatise from the main witnesses, based on Swain's chart on pages 322–325, which correlates the sections of Adamantius' abridgment with the two Arabic recensions. Comparison of Bar Hebraeus' Syriac excerpts of the lost Syriac translation of Polemon with the Greek and Arabic texts may also shed a little more light on Polemon's original wording. Similarly, a study of the Arabic renderings of Greek vocabulary, where the Arabic of the Leiden manuscript correlates closely with Adamantius' Greek, would be useful

for the ongoing efforts in Arabic lexicography; Swain acknowledges this but explains simply that such studies were not the aim of the volume [7].

I have only one quibble with the work as a whole, which I raise only because of the real possibility that this volume, in view of its successful execution, will become a model for future studies of texts and their histories in both Greek and Arabic, and for further collaborations between scholars in the two fields. Indeed the book poses itself as such a model on page 1. The problem is not with the collaboration, which is very much to be encouraged; it is rather with the use of 'Islam' as a blanket term for many societies, countries, and times, and as a category that can be set in parallel with 'Antiquity'. These mismatched terms are implicitly based on ill-founded and misleading but nevertheless all too common theories of determined civilizations. Even a division of the sources by language (Greek and Latin, Arabic) would be sounder, and certainly no more simplistic.

With any volume of this size and complexity certain typographical lapses are to be expected. Many of these problems clearly derive from the typesetters of Oxford University Press, who leave something to be desired when it comes to diacritics and non-Roman fonts. No fewer than four different Arabic typefaces are found, and even certain Greek letters are used inconsistently. These are only a few examples of the irregularities. Although such flaws in production are a disservice to the authors as well as the readers, fortunately they do not hide or diminish the quality of the scholarship presented.

All in all, this volume is bound to be one of the most important references on ancient Greek, Latin, and medieval Arabic physiognomy and its place in the societies that practiced it. One cannot summarize briefly the many areas of investigation that will benefit from this volume. In particular, though, it will be useful to Arabists for the presentation of the Arabic texts and the important collection of data on *firāsa*, and it should also bring the importance of the Arabic reception of Polemon, and other ancient Greek authors such as Bryson, more fully to the attention of classical scholars. Polemon is clearly an important source now to be taken fully into account in studies of 'the second sophistic'.

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*Ekphrasis, Imagination and Persuasion in Ancient Rhetorical Theory and Practice* by Ruth Webb

Farnham, UK/Burlington, VT: Ashgate, 2009. Pp. xiv + 238. ISBN 978–0–7546–6125–2. Cloth $99.95

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'*Ecphrasis*', defined by James Heffernan [1993, 3–4] as a 'verbal representation of a visual representation', established itself as the buzzword of comparative literature and visual culture departments at the end of the 20th century. Championed by the likes of James Heffernan [1993], John Hollander [1995], and W. J. T. Mitchell [1994] in particular, *ecphrasis* has constituted the dominant model for theorizing the paragonal relationship between what can be seen and what can be said—the ways in which visual and textual media work at once collaboratively and competitively with each other.[1] Ever keen to associate the cutting-edge with ancient precedent, classicists have very much followed suit, tying modern thinking about *ecphrasis* to its supposed archaeology in the Graeco-Roman world.[2] An unsuspecting Greek word, barely known or used before the 1960s, has subsequently been thrust to the fore of the humanities: it has generated a whole industry of 'intermedial' or 'iconotextual' criticism, among a broad range of different disciplinary specializations.[3]

[1] In addition to the work of Leo Spitzer (discussed on pages 33–35 of the book under review), earlier foundational studies include Krieger 1967, Bergmann-Loizeaux 1979, and Dubois 1982. For my own overview of this scholarly history, see Squire 2009, 138–146.

[2] For two surveys of scholarship, see Elsner 2002, Bartsch and Elsner 2007. For a more detailed (but by no means exhaustive) bibliography, see Squire 2009, 141–142 and n197.

[3] On 'iconotexts', see Wagner 1995; on the 'intermediality' of *ecphrasis*, see the excellent discussion in Klarer 1999 [esp. p. 2]. For one of many recent championings of *ecphrasis* as a transhistorical concept—as a 'literarische Tradition der Grossdichtung in Antike, Mittelalter und früher Neuzeit'—see Ratkowitsch 2006.


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But what exactly do *ancient* concepts of *ecphrasis* have to do with more *modern* appropriations of the term in the late 20th and early 21st centuries? That is the question which Ruth Webb explores in this book—an at once abridged and expanded version of her much cited 1992 doctoral dissertation at the Warburg Institute. Webb focuses almost exclusively on the Greek rhetorical handbooks, or *Progymnasmata*, of the second through sixth centuries AD—above all, on the work of Theon, (pseudo-)Hermogenes, Aphthonius, and Nicolaus. Webb consequently situates ancient Greek theories of *ecphrasis* within the specific demands of ancient Greek rhetorical practice: exploring 'the range of meaning of the term as it was used in antiquity' [1], together with its subsequent influence on Greek literary criticism and historiography, she demonstrates the cultural remove of ancient understandings of *ecphrasis* from those that predominate in the modern academy. Where modern critics are said to have defined *ecphrasis* around its *artistic* subject matter, ancient writers are shown to have used it within a specific and culturally contingent 'set of ideas about language and its impact on the listener' [1]. Webb's interest in the ancient 'oral conception of language' [98], in turn touching upon notions of visual imagination, memory, emotion and reader response, therefore lends the project an interdisciplinary relevance that transcends the *Progymnasmata* alone: 'this is almost as much a study of ancient psychology as of rhetoric', as Webb puts it [5];

> the study of ecphrasis and *enargeia* provides important information about ancient habits of reading and deeply rooted attitudes towards texts, which are seen as inviting imaginative and emotional involvement. [195]

The book is structured in seven discrete chapters, topped and tailed by an introduction and conclusion. Webb begins by defining her subject against modern definitions of *ecphrasis*: while 'there was indisputably a strong tradition of describing real or imaginary works of art in oratory, historiography, epigram, epic and other poetry', she writes, 'there is no evidence that these were considered to form a single genre, or that the genre had a name, still less that the name would have been "ekphrasis"' [1–2]. To understand 'what ekphrasis was, how it functioned and what its purpose was', Webb instead 'mines the rhetorical handbooks of the first centuries CE', concentrating on 'the rhetorical theory and practice of ekphrasis for the

simple reason that it is in the rhetoricians' schools that ekphrasis was defined, taught and practised' [3].[4]

Webb advances her argument—or so it seemed to me—in three overlapping sections. The book's first three chapters contextualize the term '*ecphrasis*' itself: chapter 1 ('The contexts of ekphrasis') situates the phenomenon within the broader framework of ancient reader response, surveying its subsequent reception and historiography; the second chapter ('Learning ekphrasis: The *Progymnasmata*') proceeds to explore the specific terminology in which the rhetoricians discuss *ecphrasis*; the third chapter ('The subjects of ekphrasis') summarizes the range of subject matter prescribed for *ecphrasis*—regardless of its referent, *ecphrasis* is presented as 'part of an intimate communication between speaker and addressee which has an impact on the recipient which is always imaginative, and often emotional' [85]. The second part of the book homes in on Quintilian's *Institutio oratoria*. Examining the particular rhetorical uses of *ecphrasis*, at least according to Quintilian, Webb explores its relation to Greek ideas about first *enargeia* or 'visualization' ('*Enargeia*: Making absent things present'), and second *phantasia* or 'imagination' ('*Phantasia*: Memory, imagination and the gallery of the mind').[5] The third and final part of the book examines surviving examples of *ecphrasis* in a range of literary texts. The sixth chapter looks to Sopatrus the Rhetor, pseudo-Dionysius, and Menander Rhetor, comparing the respective uses of *ecphrasis* in declamation and epideictic ('Ekphrasis and the art of persuasion'). Chapter 7, by contrast, widens the book's perspective to include brief mention of Achilles Tatius, Lucian's *De domo*, Heliodorus, and Philostratus' *Imagines* ('The poetics of ekphrasis: Fiction, illusion and meta-ekphrasis'). A brief conclusion restates the centrality of *enargeia* to Greek rhetorical definitions of *ecphrasis*—'the vividness that makes absent things seems [*sic*] present by its appeal to the imagination' [193].

[4] For the argument, which is at the crux of Webb's 1992 doctoral dissertation, see also Webb 1999. Throughout, Webb places particular emphasis on the *Progymnasmata* of Theon, although she rather downplays controversies about a supposed fifth-century date [cf. 14n3 on Heath 2002–2003].

[5] It is unfortunate that Webb was unable to consult Hagemeier 2008.

Webb is remarkably adept at moving from the micro- to the macro-scale. She demonstrates an exemplary sensitivity to the intricacies of Greek language and terminology, yet proves no less comfortable with her Genette [7–9], Tompkins [23–24], and Hamon [105–106]. Mindful of her mixed audience [cf. xiii], she provides an excellent appendix containing all the key texts discussed together with careful translations and some interpretative notes. As a scholar based in both Paris and London, Webb forges numerous intellectual bridges between British and French perspectives on the 'Second Sophistic' (especially Francophone work on ancient systems of memory, rhetoric, and emotion). In addition to offering a masterfully polyglot survey of the field, Webb cites extensive chunks of the most important French texts in the footnotes, accompanying these with her own helpful translations.[6]

This is without doubt the most important monograph on the *Progymnasmata* to have been published. It situates the explicit discussion of a standardized rhetorical trope within much larger ancient traditions of theorizing seeing: the result will be essential reading for anyone interested not only in the *Progymnasmata* but also in the pre-modern epistemology of vision. Webb's grounding of *ecphrasis* within ancient theories of rhetoric proves a timely antidote to those critics who have raided the *Progymnasmata* for apposite definitions and labels, and who have not paid due attention to the specific ideologies, functions, and readerships that lay behind their production.

But—at least to my mind—some problems inevitably remain. Let me focus on just three. My first difficulty with the book is structural—about the coherence of its chapters, which add up to less than the sum of their parts. This no doubt reflects the checkered archaeology of the project—the fact, as Webb confesses, that the book 'has undergone several permutations over the years' [xiii]. But the multiple layering of perspectives sometimes obscures the clarity of Webb's thesis. The argument about the 'modernity of the modern definition' of *ecphrasis*, for example, is treated in the introduction

[6] My only complaint is that German criticism is conspicuously underplayed: it is puzzling that there should be no mention of Boeder 1996, and Graf 1995 is incorrectly cited. And why no engagement with the important and highly relevant work of Irmgard Männlein-Robert?

[5–7] and becomes a *leitmotif* throughout the book. And yet it frequently distracts—not least in the first chapter [28–37], in a section originating in her doctoral thesis but which is rather unhelpful here—effectively turning the chapter into a second introduction ('this study aims. . . ' [36]). The organization of chapters and subsections exasperates these difficulties. The second chapter's excellent introduction to the *Progymnasmata* seemed to me to belong in the first, for example, and the third chapter's discussion of the 'subjects of ekphrasis' is likewise pre-empted by the second [esp. 54–55]. Given Webb's argument that *enargeia* and *phantasia* form part of the same discourse, the decision to split the two themes across the fourth and fifth chapters seemed to me slightly misguided; I subsequently got lost in the miniature subsections of the sixth chapter. Although Quintilian is nicely introduced in these chapters, other authors and texts are left hanging—not least Longinus, to whose discussion of *enargeia* Webb frequently returns but without any introductory contextualization. These difficulties are testimony to the creativeness of the book: the project seems to have grown too intellectually ambitious for the structure artificially imposed upon it. Still, they may make the volume an unfriendly introduction for undergraduates and the uninitiated.

My second reservation has to do with the actual *content* of the book: I remain unconvinced by Webb's decision to treat the *Progymnasmata* in isolation from the much larger literary history of *ecphrasis*—both in antiquity and, indeed, beyond. As I see it, the strange, puzzling, and contradictory claims of the rhetoricians about *ecphrasis* can only be understood against (and as part of) the broader Graeco-Roman interrogation of the nature of vision. When the *Progymnasmata* define *ecphrasis* as a 'speech that brings the subject matter vividly before the eyes', they are resonating against theories about visual-verbal relations on the one hand, and about visibility and invisibility on the other. Webb is quite right to insist on the Greek term's breadth of meaning. But as she admits, *ecphrasis* was always understood to interrogate the nature of sight and insight, regardless of particular subject: 'any ekphrasis rivals the visual arts in that it seeks to imitate their visual impact'; so it is, as Webb continues, that 'any ekphrasis is haunted by the *idea* of the work of art' [83–4: cf. 194]. In this sense, I would argue that ancient concepts of *ecphrasis* very much foreshadow the critical projects of intermedial

criticism in the later 20th century.[7] More fundamentally, I cannot see why the *Progymnasmata*'s rhetorical discussions of *ecphrasis* should be read in isolation from the production of literary texts.[8] Webb admits that 'many of the poetic descriptions of works of art... do fulfil the basic requirement of "placing before the eyes" and seem to rival the visual arts, as ekphrasis should' [3]. And yet, on the previous page, Webb had insisted 'that there is no evidence that these were considered to form a single genre' [1–2]. I'm confused and I suspect that the author is too.

Part of the problem here lies in our quest for neat classification: like the authors of the *Progymnasmata* themselves, classicists are sticklers for discrete rhetorical categories. But while Webb correctly argues for a much more complex definition of rhetorical *ecphrasis* than scholars have been wont to assume, her reluctance to treat the phenomenon from a perspective *beyond* the *Progymnasmata* seems unduly reductionist in scope. One thing demonstrated by the publication in 2001 of Posidippus' third-century poetry book, for example, is its organization of epigrams according to discrete artistic subject matter. Now, it has been said that these are not '*ecphrastic*' because

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[7] I would therefore maintain that 'modern' definitions of *ecphrasis* as 'a verbal representation of a visual representation' [Heffernan 1993, 3–4] very much align with 'ancient' thinking. The evolution of Webb's thinking over the last 20 or so years lead her at times to agree—as when she discusses the 'ultimate closeness' [37] of 'ancient' to 'modern' concepts of *ecphrasis*.

[8] Recent work suggests that I am not alone: compare, e.g., Francis 2009 on the earliest *ecphraseis* of Homer, *Iliad* 18 and Hesiod, *Theog.* 570–615 and *Op.* 60–109:

> The relationship between word and image in ancient ecphrasis is, from its beginning, complex and interdependent, presenting sophisticated reflection on the conception and process of both verbal and visual representation. [3]

Still more important is Chinn 2007 on Pliny, *Epist.* 5.6.42–4: in response to Webb's earlier work, Chinn talks 'in Pliny's time of a conception of ekphrasis that is more "modern" than we might have expected' [2007 265]. As I have argued elsewhere, the difficulty lies in isolationist approaches to the *Progymnasmata*, understanding them as 'purely' rhetorical texts removed from other forms of literary (or indeed artistic) production and criticism.

they are not sufficiently 'vivid' or 'descriptive'.[9] But as recent scholarship has shown, such poems self-consciously play with the boundaries between physical image and mental impression as well as between visualized image and verbalized text; they toy with a 'trialectic' of what Irmgard Männlein-Robert calls 'voice, writing and image'.[10] All this, I think, suggests that the rhetorical discussions of *ecphrasis* in the *Progymnasmata* are very much attempts to rationalize, categorize, and order a much older and more extensive literary phenomenon; and to reorient that phenomenon, moreover, for specific rhetorical ends.[11]

Of course, the objection might come that the *Progymnasmata* were simply not that sophisticated or engaged.[12] In part, I would have to agree. But the *Progymnasmata* certainly were aware—or so it seems to me—of their paradoxical claim: exactly how it is that words *could* bring about vision? Webb is spot on in suggesting that 'rhetoricians tend to place emphasis on the *ability* of words to create presence, rather than the problematic nature of that presence' [105]. But there can be no doubting that the rhetoricians knew that they were speaking in metaphors: hence, for example, Theon's and Hermogenes' qualifier of 'almost' (σχεδόν) bringing about seeing through hearing, or Hermogenes' acknowledgment of the formulaic derivation

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[9] See Zanker 2003, 61, 62: 'These poems were very rarely intended to give a vivid description... They were poems *about* statues, paintings and gems' (cited by Webb in apparent agreement on 2n2). For my own response to this debate, see Squire 2010a and 2010b.

[10] See especially Männlein-Robert 2007; compare also Prioux 2007 and 2008 as well as Tueller 2008 (also absent from the bibliography).

[11] I should once again come clean about my own interest here developed in part of a forthcoming book on the *Tabulae Iliacae* or 'Iliac tablets' [Squire 2011, esp. ch. 7].

[12] Cf. Bartsch 1989, 7–14:

> The approach these handbooks take proves to be relatively dry and matter-of-fact; they provide guidelines for content and procedure rather than provide suggestions on function in a literary context, and their theory, if it deserves the name, strays within bounds too narrow to reveal how such passages might be manipulated for broader aims. [9]

As Bartsch demonstrates, the *Progymnasmata* are therefore at their most revealing when set against other texts—which is why, I think, Webb's seventh chapter is her most successful.

of the simile ('as they say'—ὥς φασιν). Webb states that the book 'is leaving aside the question of what the mental experience expressed by the claims to "see" actually might have been' [24n34]. This strikes me as a problematic intellectual maneuver: for the ancient dialectic between sight and insight—what Richard Wollheim [1980, 205–229] calls 'seeing as' and 'seeing in'—intersects on a much larger level with ancient dialectics about theorizing sight and insight on the one hand, and images and texts on the other. When Longinus complains of *enargeia* that it not only persuades the listener but also *enslaves* [δουλοῦται] him [98], or when pseudo-Dionysius laments the problematic *fictiveness* of *ecphraseis* [154], they are harking back to longstanding Greek debates about what vision is, and therefore about the extent to which words can capture visual experience.[13]

This brings me to a third and related issue: chronology. As her title suggests, Webb uses the *Progymnasmata* to reconstruct an 'ancient' phenomenon in 'theory and practice'. But it seems worth asking at least the question of whether these texts reflect a specific cultural moment in the development of late antique thought. As the book proceeds, 'ancient' becomes an ever looser category: we move backwards and forwards from Longinus and Quintilian in the first century AD, through late antiquity, even into the writings of Augustine and the still later Byzantine world; by the end of the book, the 'ancient' is bracketed together with the 'medieval' [195] in stark contrast to the 'modern'.[14] Of course, this sort of 'big picture' approach has many advantages. But it also runs the risk of collapsing historical difference. In particular, I wonder what Webb makes of John Onians' claim [1980] that the late antique championing of the imagination as found in the *Progymnasmata* reflects a peculiar cultural historical moment—that the rhetorical championing of the imagination went

[13] In this capacity, Webb has lots to say about Aristotle but much less about Plato. As I have argued in Squire 2010b [cf., e.g., Rouveret 1989, 14–15; Goldhill 1998, 207–210; Steiner 2001, 33–35], the crucial text here is Xenophon, *Mem.* 3.10.

[14] See p. 27, on how 'ancient "criticism" [is] a very different phenomenon from modern literary criticism'.

hand in hand with the increasing abstraction of the contemporary visual arts.[15] As Webb hints [190–191], this cultural history is somehow bound up with the rise of Christianity whereby *ecphrasis* served 'to bring out the spiritual qualities of a monument or work of art' [190]. Despite (or rather because of) Webb's overarching argument about 'modernity' and 'antiquity', I left the book feeling rather puzzled about the terms in which these debates had been framed.

These three qualms should not detract from the overwhelming merits, value, and importance of the book.[16] The sheer number of observations along the way—about Pausanias' *Periegesis* as a self-consciously *ecphrastic* text [54—particularly revealing in the light of Webb's analysis on 157], the analysis of Thucydides' ancient reception as the master of *ecphrastic* razzle-dazzle [19–20, 69–71, 195], or Webb's superb overview of Philostratus' (meta-)meta-*ecphrastic* project in the *Imagines* [187–190]—will launch this monograph onto all manner of different reading-lists. And rightly so. Still, the book's chief virtue seems to me to lie in its analysis of 'persuasion in ancient theory and practice' rather than that of '*ekphrasis*' and 'imagination'. Those fields, I think, still remain wide open; and initial indications suggest as many continuities as discontinuities between 'antiquity' and 'modernity'.

A final eulogy of the book's presentation: Ashgate have done an almost faultless job in producing the book. It would have been useful to have a separate *index locorum*, and it is frustrating too not to have more subentries in the general index. But I found no glaring mistakes

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[15] See also Onians 1999, 217–278:

> Not only was it inherent in this visual imagination that it did not need to be limited by the reality of what was presented to the eyes, it was actually desirable for one to be able to imagine the exaggerated and the false... As art becomes less and less descriptive, the accounts of art become more so. [261]

[16] Errors are otherwise few and slight. I list them here in the hope of a future (and more affordable?) paperback edition: p. 34 (three lines from bottom) misquotation—missing definite article; p. 35 (eight lines from bottom) 'classical and archaeology'; p. 40 (second paragraph, second line) mistaken symbol; p. 55 (second paragraph, five lines down) mistaken typeface; p. 73n35 mistaken symbol; p. 117n37 (penultimate line) omitted verb. Occasionally, textual references are omitted—as with the passage of Plutarch that is cited on p. 20.

in the Latin or Greek. The wonderful choice of dust-jacket should also not go unmentioned: where we would usually expect to see a picture, Ashgate gives us Aphthonius' verbal definition of *ecphrasis*—as visualized in a 1591 manuscript. 'Ancient' writers would have very much appreciated the wit.

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*The Occult Sciences in Byzantium* edited by Paul Magdalino and Maria Mavroudi

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Few fields of European intellectual history are as richly documented, yet as little known, as the history of Byzantine science and philosophy. This pioneering collection of essays, based on a symposium hosted at Dumbarton Oaks in Washington, DC in November of 2003, seeks to reduce this gap in our knowledge by examining 'occult science as a distinct category of Byzantine intellectual culture' [11]. In the category of the 'occult sciences', the editors, both well-established Byzantine historians, include astrology, alchemy, dream interpretation, and a variety of other divinatory traditions that fall somewhere between the poles of 'science' and 'magic'. The problem with the label 'magic', they argue, is that it collapses any distinction between, on the one hand, the much-maligned practitioners of magic at the poorest and least educated levels of society and, on the other hand, those 'sophisticated masters of occult knowledge', who sometimes held, in Byzantium, the highest offices of church and state. As a prime example of the latter group, the editors point to the career of Michael Psellus, the 11th-century polymath and court philosopher, who composed, among other things, a treatise on alchemy at the request of the patriarch Michael Cerularius (1043–1058). Psellus' writings even provide, in the editors' view, 'a coherent Byzantine definition of occult science as a discrete epistemological category' [20].

The category 'occult science' deserves a more robust and systematic explication than it receives in the book's introduction [11–37]. Magdalino and Mavroudi contend that the Byzantines possessed 'a clear notion of the occult sciences as distinct from, but consistently associated with, other types of learning, both practical and theoretical' [27]. But the assertion of this distinction by Michael Psellus and


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other Byzantine writers only underscores how fluid such definitions could be in practice. The editors themselves emphasize the variability of the manuscript tradition, in which one encounters a bewildering mixture of treatises on alchemy, astronomy and astrology, botany, dream interpretation, geomancy, medicine, magic, numerology, and Christian apocrypha [21–25]. Fortunately, the book's value does not hinge on its ability to demonstrate a unified Byzantine definition of the 'occult sciences'. What it does offer is a learned introduction to a set of closely related themes in the history of Byzantine science, philosophy, and magic.

The collection's first essay, Maria Mavroudi's 'Occult Science and Society in Byzantium: Considerations for Future Research' [39–96], reviews the modern historiography of Greek science that underlies the entire volume. This historiography includes landmark achievements, such as the 12 volumes of the *Catalogus Codicum Astrologorum Graecorum* [Brussels, 1898–1953], and the nine volumes of the *Catalogue des manuscrits alchimiques grecs* [Brussels, 1924–1932]. No less noteworthy, though, are its gaping holes. Despite the creation of appropriate series in the early 1980s, fewer than a dozen Byzantine astrological and alchemical texts have been published so far in proper editions [45]. Mavroudi contrasts this neglect with the relatively abundant evidence for divinatory practices throughout Byzantine history, not only at the court, but even among the ranks of the clergy [81]. The prestige of Byzantine 'occult science' also cut across linguistic and political frontiers. Few readers will dispute Mavroudi's call for new studies in the circulation and reception of Byzantine science in the Islamic world and the Latin West. I would add only that scholars of Syriac and Armenian literature, notably absent from this volume, could have much to contribute to this dialogue.

In her contribution, 'The Greek Concept of *Sympatheia* and its Byzantine Appropriation in Michael Psellos' [97–117], Katerina Ierodiakonou explores how one (admittedly idiosyncratic) Byzantine intellectual remolded the ancient philosophical concept of cosmic sympathy (συμπάθεια) to accord with Christian doctrine. Psellus agreed with the Neoplatonists that all the parts of the world were bound together by an 'ineffable (ἄρρητος) sympathy' analogous to the unity of a living organism [106], but rejected the Neoplatonists' belief in the ability of human beings to manipulate these bonds. The duty of man was rather to observe and study the signs embedded in this world.

For Psellus, the mysteries of God's creation could be discerned in signs as intimate as the motions of an icon or the letters of the Greek alphabet. Psellus promotes observation as a viable strategy for intellectual inquiry since direct knowledge of the Divine is impossible. As Psellus reminds his audience in his commentary on the letters of the Greek alphabet, since 'we cannot experience God's light in all its glory, it is at least possible to see its reflection in water' [116].

Paul Magdalino's essay, 'Occult Science and Imperial Power in Byzantine History and Historiography (9th–12th Centuries)' [119–161] explores the 'close but tense relationship' between experts in divination and Byzantine rulers. The major Byzantine historians relate numerous anecdotes attesting to the prevalence of astrology, statue magic, and other forms of divination at the court of Constantinople. According to these narratives, divinatory practices peaked during the reigns of the iconoclast emperors and patriarchs. The mid-10th century history known as *Theophanes Continuatus*, for instance, presents a savage invective against the iconoclast patriarch John the Grammarian (*ca* 837–843), who allegedly kept a stable of good-looking nuns to assist him in the dark arts of divination. Byzantine historians generally paint a more ambivalent picture of astrology, accepting the 'interpretation of celestial phenomena as a legitimate *technē*' in principle, but condemning its use in practice [137–138]. Magdalino documents this strain of ambivalence among nearly all the major historians of the Middle Byzantine period. *Theophanes Continuatus*, the same chronicle that skewers John the Grammarian for his addiction to divination, preserves an admiring portrait of the astrological expertise of John's cousin, Leo the Mathematician, 'an account that has been enormously influential in creating modern perceptions of the "first Byzantine humanism"' [124]. Magdalino's study thus underscores the risk of citing individual episodes from the histories without sufficient attention to the larger narrative patterns in which these episodes are embedded.

The difficulty of segregating the 'occult sciences' from other forms of philosophical inquiry is well illustrated by Maria Papathanassiou's essay, 'Stephanos of Alexandria: A Famous Byzantine Scholar, Alchemist, and Astrologer' [163–203]. Building on the work of the Polish Byzantinist Wanda Wolska-Conus, Papathanassiou argues that Stephanus, a distinguished teacher of medicine and philosophy in early seventh-century Alexandria, was also deeply engaged in both

astrology and alchemy. Furthermore, Stephanus combined this expertise with explicit Christian piety; his treatise *On the Great and Sacred Art of Making Gold* begins and ends 'with prayers greatly influenced by the works of the early Christian fathers' [192].[1] Later Greek and Arabic tradition attributed to a certain 'Stephanus the astrologer' an astrological treatise known as the *Apotelesmatike Pragmateia*, which includes a famous horoscope of Islam. Papathanassiou supports this attribution by identifying the astrological content in Stephanus' alchemical lectures, where she finds evidence for astral observations made on 1 September AD 621. If she is right, scholars will need to give more credence to the 10th-century reports linking Stephanus to the court of the emperor Heraclius (reg. 610–644).

The larger history of alchemy in Byzantium is addressed in Michèle Mertens' contribution, 'Greco-Egyptian Alchemy in Byzantium' [205–229]. Mertens considers, in particular, the formation of the Byzantine alchemical corpus. References to alchemy, sparse in Byzantium before *ca* AD 500, surge during the reign of Heraclius; but the situation in subsequent centuries remains obscure. The surviving corpus, Mertens argues, was probably formed in the ninth or 10th century in parallel with the formation of other encyclopedic compendia such as the *Geoponica*, the Hippocratic corpus, and the *Palatine Anthology*. Passing references by writers such as Photius, George the Monk, and the *Suda*, indicate that interest in Zosimus of Panopolis (writing *ca* 300 AD) and other esoteric writers extended 'widely beyond strictly alchemical circles' during the Middle Byzantine period [229]. Such fundamental questions of dating and citation will need to be answered before a proper intellectual history of Byzantine alchemy can be written.

Other essays in the volume address the circulation and translation of texts between Byzantium and its neighbors. In his contribution, 'Late Antique and Medieval Latin Translations of Greek Texts on Astrology and Magic' [329–359], Charles Burnett provides a brief bibliographic introduction to an intriguing array of anonymous and pseudonymous Latin texts. The influence of these translations, he contends, has frequently been underestimated by scholars focusing

[1] Hopefully, Papathanassiou will document which fathers, in particular, Stephanus draws upon and how he uses them in her forthcoming edition of the treatise.

on the Arabic sources for Latin astrology and magic. As *exempla*, Burnett chooses two texts from the 'large and murky field' of prognostication texts. Patterns of diction, vocalization, and syntax suggest that these texts, including, for example, the *De luna secundam Aristotilem*, derive from Greek prototypes. An appendix to the article presents a new edition of the *De luna* and two other short texts based on manuscripts that were previously unknown or unavailable to Burnett.

In the hands of a master like David Pingree (d. 2005), to whose memory the editors dedicate this volume, patterns in the circulation of texts can reveal broader patterns of cultural interaction across the medieval world. In his article, 'The Byzantine Translations of Māshā'allāh on Interrogational Astrology' [231–243], Pingree explains why the works of this Abbasid court astrologer (a Persian Jew from Basra) were highly influential in the West but largely ignored in Byzantium. Māshā'allāh's treatises, composed between the 760s and *ca* 810, contain a sophisticated fusion of Indian, Persian, and Greek astrology; but his work became 'antiquated' by the ninth century, as Islamic astrologers 'revised and systematized Māshā'allāh's inept and unintegrated borrowings from both the Greek and Indo-Persian traditions' [242]. Translators in the Latin West, which had inherited only one major work of ancient astrology, the fourth-century *Mathesis* of Firmicus Maternus, found Māshā'allāh's treatises both accessible and exciting. Byzantine translators, by contrast, turned directly to the more advanced treatises of the ninth-century astrologers Sahl ibn Bishr and Abū Ma'shar.

Debates over the legitimacy of astrology in Byzantium intensified during the reign of Manuel Comnenus (reg. 1143–1180), the bold, Western-influenced emperor whose own devotion to astrology is well documented. In his essay, 'Did the Biblical Patriarchs Practice Astrology? Michael Glycas and Manuel Comnenus I on Seth and Abraham' [245–263], William Adler carefully dissects the 12th-century debate over the legitimacy of astrology, in which both sides appealed to the authority of the patriarchal tradition. The emperor Manuel and other proponents of astrology claimed that Adam's son Seth had learned the practice of astrology from an angel, and that the patriarch Abraham, a Chaldaean by birth, had practiced a sanctioned form of astral observation. Manuel's contemporary, the monk Michael Glycas, countered with his own reading of the patriarchal models. As

proof that Abraham had rejected the astrology of his youth, Michael pointed to Abraham's victory over the magicians of Egypt as described in the ninth-century *Chronicle* of George the Monk [261]. Adler rightly emphasizes here the delicate crux in Michael's argument, which required separating astrology from its legitimate cousin, astronomy. As a chronicler himself, Michael was sensitive to the power of small details. Departing from previous tradition, Michael Glycas asserts that God had sent the angel Ouriel to reveal to Seth the science of astronomy.

Byzantine intellectuals of the Palaeologan period continued to debate the propriety of predictions based on astral observation. In her essay, 'Astrological Promenade in Byzantium in the Early Palaiologan Period' [265–289], Anne Tihon surveys the extensive data on astronomy and astrology in the works of six major Byzantine scholars of the 13th and 14th centuries. Vocal opponents of astrology, such as George Pachymeres [1242–1307], rejected the legitimacy of casting any individual's horoscope since such horoscopes negated the significance of free will. This standard Christian objection to astrology, articulated already in the fourth century by the Cappadocian fathers, still carried weight in the 13th century. By the end of the century, though, the patronage of the emperors of Trebizond encouraged the importation of new astronomical data and methods from Iran. This Persian material was soon thoroughly mixed with other forms of Byzantine science. One Greek manuscript from the Vatican, copied during the reign of Andronicus II (reg. 1282–1328), juxtaposes treatises by Euclid, Aristarchus, Ptolemy, and John Philoponus (among others) with astrological texts and tables of Persian astronomy [276]. As Tihon observes, a 'more precise inventory' of these manuscripts could clarify the volume and nature of this scientific exchange between Byzantium and Persia.

Jewish intellectuals in Byzantine South Italy also became embroiled in debates over the legitimacy of astrology. In his essay, 'Hebrew Astrology in Byzantine Southern Italy' [291–323], Joshua Holo closely examines the presentation of astrology in two Hebrew texts from the region: the *Chronicle of Ahimaaz* composed in Capua in AD 1054 and Shabbetai Donnolo's *Sefer hakhmoni*, a late 10th-century treatise commenting on a late antique mystical cosmogony. Both works 'unambiguously embrace' the use of astrology, but they

adopt very different strategies to do so [293]. The *Chronicle*, for example, assiduously distinguishes astrology from astronomy, presenting the latter as more neutral and, therefore, less consequential. In one telling episode, an unnamed Christian archbishop of South Italy proves more adept at calculating the appearance of the new Moon than his rival, Rabbi Hananel. The rabbi's astronomical error, however, causes no harm since God intervenes to match the position of the stars to Hananel's prediction [309]. The author of the *Chronicle* thus separates the issue of astronomical precision from the question of the righteousness of the practitioner. The same *Chronicle* emphasizes the benefits accrued by pious astrologers: in a later section, Hananel's great-grandson Paltiel earns the favor of the future Fatimid caliph al-Mu'izz by the accuracy of his astrological predictions. Holo argues that the endorsement of astrology in this and other Hebrew texts from Byzantine South Italy belongs to the tradition of *aggadah*, in which 'ambivalence and theological daring can flourish without encroaching on the fundaments of Jewish doctrine and law' [320]. This openness to astrology among prominent Jewish intellectuals of Byzantium contrasts with the sharp opposition to astrology articulated in the following century by Maimonides (1135–1204).

In the final essay of the volume, 'Revisiting the Astronomical Contacts Between the World of Islam and the Renaissance Europe: The Byzantine Connection' [361–373], George Saliba scrutinizes a well-known problem in the study of Copernicus (d. 1543), namely, how much did Copernicus' concept of linear motion as the product of two combined circular motions owe to the advances of much earlier Muslim astronomers? How, in particular, could he have become familiar with the crucial theorem by the great Muslim astronomer, Naṣīr al-Dīn al-Ṭūsī, director of the Marāgha observatory in northwestern Iran founded in AD 1259? Byzantine astronomers of the early 14th century were well versed in the latest developments in Islamic astronomy, but there is no direct evidence that any of them copied al-Ṭūsī's theorem. Building on an insight of the historian of science Willy Hartner, Saliba argues that Copernicus learned the theorem directly from an Arabic manuscript. Saliba identifies the cities of Padua, Bologna, or Ferrara in North Italy as the most plausible setting in which Copernicus could have collaborated with a translator who possessed the necessary fluency in written Arabic.

The book concludes with a 60-page bibliography and an index that could have been made more useful by the addition of subheadings for key entries (for instance, planets, predictions, and stars). A separate 2-page index of manuscripts highlights how much of the raw material for these studies remains unpublished. In sum, the essays in this volume provide stimulating insights into the evolution of astrology, alchemy, and other 'occult sciences' that flourished in the medieval world. While portions of some essays are dense with technical detail and several would have benefited from tighter organization, the collection as a whole admirably achieves its goal. Brought together in a single affordable volume, the essays mark a significant advance in the study of a vital, yet often neglected, component of Byzantine culture and society.

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