AESTIMATIO

Critical Reviews in the History of Science

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Preface

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

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

Alan C. Bowen

Tracey E. Rihll

The Mathematics of the Heavens and the Earth: The Early History of Trigonometry by Glen van Brummelen

Princeton/Oxford: Princeton University Press, 2009. Pp. xx + 329. ISBN 978–0–691–12973–0. Cloth $39.50

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An etymological transformation perhaps unrivaled in the history of mathematics is that of the evolution of the lexical term for sine. Often recounted,[1] the linguistic passage of this term begins in India (*jyā/jīva*), is subject to the methodical magic of the translator's pen as it traverses the Islamic Near East (*jaib*) and ends up in the Latin west (*sinus*) as we recognize it today. This passage reveals a mathematical concept that is diachronic and richly multicultural. Indeed, as its etymology reveals, any adequate account of the field of trigonometry of which sine is just a part must too follow this trajectory. And for the first time, this has been achieved in a single work. *The Mathematics of the Heavens and the Earth: The Early History of Trigonometry* by Glen van Brummelen follows the history of one of the most familiar areas of mathematics—trigonometry—and van Brummelen is acutely aware of the heritage of this discipline,

> a subject that is so pervasive that almost all of us see some of it during our high school education, one whose story goes back well into ancient times. This subject crosses most major cultures and places; indeed, it is not easy to identify societies that contributed significantly to science without using it in some way. [xi]

[1] See, for example, Plofker 2009, 257. Van Brummelen partially follows this passage [138].

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Accordingly, van Brummelen's considerations begin in the ancient Near East, Egypt, and ancient Greece; they continue with its emergence and development in India and the Islamic Near East, and its transmission to the European Middle Ages and the Renaissance.

Trigonometry is a term well familiar to students and scholars alike. It features in both pure and applied mathematics and is as commonplace to beginners as it is to experts. Thus, van Brummelen's book is a welcome addition to this high profile topic. No other scholarly work provides historical coverage, mathematical analysis, and reflective commentary devoted to this subject. His analysis offers translations of key primary source texts and thorough but accessible accounts of their mathematical content.

Van Brummelen's style is warm and inviting. The book is well set out, diagrams are carefully rendered, and primary source extracts are integrated seamlessly into the main text. He endeavors to provide numerous footnotes for the express purpose of supplying readers who want to delve more deeply into the mathematical history than the text allows with appropriate resources. The bibliography has over 600 entries.

This book fills a conspicuous gap in the field. Even recently, Eli Maor, in his preface, stated that his book is 'neither a textbook of trigonometry—of which there are many—nor a comprehensive history of the subject, of which there is almost none' [1998, xi]. With the publication of van Brummelen's latest contribution, scholars now have access to the first half of such a comprehensive history. Until now, texts like Maor's and strong entries on the history of trigonometry in popular mathematics history[2] have been the only sources to provide researchers with significant scholarship that traces the roots and development of trigonometry. As for his inspiration, van Brummelen acknowledges a clear debt to Anton Von Braunmühl [1900–1903], but it is clear that this work surpasses this classic in many ways.

From the outset, indeed, even in his title, van Brummelen conveys that this science was inspired by celestial musings as well as measuring and reckoning in the terrestrial realm. Because of these practical orientations, he includes a short introductory chapter with details on the essential and basic concepts of spherical astronomy.

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[2] For example, survey texts such as Katz 2008.

Here, he defines a variety of terms (set-off nicely in bold italics) and provides several diagrams depicting features needed for solar time-keeping, oblique angle of ascension, and calculating rising times.

Van Brummelen begins by carefully defining trigonometry. He provides two necessary conditions:

(1) a standard quantitative measure of the inclination of one line to another; and
(2) the capacity for, and interest in, calculating lengths of line segments.

In establishing these necessary conditions for considering trigonometry as a science, van Brummelen provides examples in which one or the other condition fails to exist. This gives justification for those sources that he has included and those that he has left out. For example, he describes why Plimpton 322 will not be included in the discussion—unlike Maor, for instance, who did take the position that Plimpton 322 was the first trigonometric table. Later in the work, he discusses this distinction when considering the *analemma* [172], which, according to the definition, does not satisfy the necessary conditions either though its importance to those involved in trigonometric activities is vital.[3] He considers Egyptian Pyramid slope calculations and Babylonian astronomical computations as precursors, providing evidence of early interest in angle measurement. He focuses on the ancients' contributions to measuring length and angles and locates the first glimmers of trigonometry proper in the third century BC with Aristarchus and his consideration of the relative distances of the Sun, Moon, and Earth, and soon after with the work of Archimedes. Of particular interest in the chapter is the treatment of Archimedes' Theorem of the Broken Chord [31] and its influence on the work of al-Bīrūnī.

Next, van Brummelen discusses the contributions of Alexandrian Greece, with an emphasis on Hipparchus (e.g., his chord table), Ptolemy, Archimedes, Menelaus, and the emergence of spherical trigonometry. In turn, he considers how Greek and Babylonian astronomy influenced the development of Indian astronomy. In this

[3] Van Brummelen states regarding the *analemma* that 'it seems fair not to consider it not as part of trigonometry as such, but rather as a mentoring older sibling.'

chapter, van Brummelen describes the most significant Indian contributions to trigonometry and his coverage in this area is noteworthy in scope and detail. Here we are provided with an impressive range of authors and contrasting techniques which gives us a good sense of how trigonometry flourished at this time. Two important themes are well developed in the chapter: the development and improvement of sine tables and the establishment of trigonometric identities. Additionally, he considers the work by Indian astronomers to improve upon methods of spherical trigonometry, largely in the service of astronomy. Mathematical highlights include Nīlakaṇṭha's ability to accurately handle 'all ten cases of the astronomical triangle in one place' [129], the computation of the sine of 18° in the 12th century [105], Bhāskara II's ingenious relation of $\sin(A+B)$ [106–107], Brahmagupta's second-order interpolation scheme for approximating sines [111–112], as well as the Taylor series for trigonometric functions in Mādhava's Kerala school [113ff]. Van Brummelen makes the compelling comment 'Indian scientists wrote much more on their results than on their methods' [124]. This observation is indeed true; but the reasons for this circumstance are complex and fascinating, and deeper than he lets on. Calling it later Indian 'reticence' [105], van Brummelen remains silent on the broader ambient social and intellectual traditions that were responsible for this feature. Similarly, he deals with issues of intellectual transmission sensitively and soundly; but his statement that 'the main distinction between Greece and India is not in what they chose to study, but in what they chose to write' [95] again does not capture the richness of the Indian intellectual circumstances. Because of the oral tradition and other predominant aspects of society and culture, the transmission and subsequent reception of foreign ideas into India is nuanced and multidimensional.

Some further reflections about applications may have further enriched his coverage. Van Brummelen observes that these authors never left the astronomical content for more general mathematical discourse [132]. In fact, as Plofker [2009, 210ff] notes, trigonometry *per se* was a special application in astronomy of geometry and, as the texts themselves reveal, was not considered part of more abstract mathematics at all. On the subject of application, Indian practitioners discriminated between those results that were 'practical' and those that were 'accurate'. Van Brummelen notes cases where practitioners 'improved' the rate of the convergence of series approximations by the addition of correctional terms. It has been argued that the new improved and ever increasingly accurate procedures were in fact not used by astronomers in their computations—their field could not take account of the level of precision that these iterative procedures offered them. It seems, then, that they were developed for their intrinsic mathematical interest, a supra-utilitarian motivation.

Following the contribution by Indian scholars, van Brummelen expertly describes the major achievements of Islamic Near Eastern mathematicians in the field of trigonometry—both planar and spherical. This is by far the most substantial chapter, and his presentation of sources otherwise not available and lucid mathematical analysis are impressive. The chapter begins with ibn Yunus' improvement upon Ptolemy's work to 'build a better sine table' [140]. It travels through the development of early spherical astronomy (e.g., an emphasis of graphical methods and *analemmas*) and the influence and use of Menelaus' Theorem in Islam. As van Brummelen identifies them, the threads of transmission are a real tangle in the Islamic Near East and for a significant period of time the '*Almagest* was in the strange position of competing with the theories of its predecessor' [137].

Mathematical highlights include ibn Yunus' [141] and al-Kāshī's computation of the sine of 1° [148], al-Ṭūsī's work on spherical trigonometry [191], attempts to establish the direction of the *qibla* [195], and the application of trigonometry to astronomical instruments [209], to name a few. Van Brummelen establishes three main approaches to the study of spherical trigonometry in medieval Islam: the Greek approach, emblematized by Menelaus, the Indian approach with plane triangles on the sphere, and the tradition of the *analemma*. Van Brummelen argues here [167] and elsewhere that its use in India was overstated; however, it is certain that as more of the Islamic sources become better known, this relationship will become clearer.[4] Van Brummelen is conscious of the extent of the Islamic empire and considers the regional variation of mathematical activity

[4] For example, in an as yet unpublished manuscript of al-Khāzini, a 13th century astronomer who wrote the *zīj al-Sanjari*, refers to an analemma-like construction as 'the Indian circle'.

in al-Andalus (Muslim Spain), which is an area rising in prominence in studies of the exact sciences of this area [217ff].

The book closes with a thorough treatment of the transmission of this study from the Arab world to the West. Van Brummelen explores subsequent activity until 1550 with the work of Rheticus, a fitting concluding point, as Rheticus' work, the *Canon doctrinae triangulorum* (1551), is significant in two ways: it relates trigonometric functions directly to angles (and not to circular arcs) in keeping with contemporary practice and it tabulates all six of the trigonometric functions that we recognize today. Navigation gave impetus to further developments; and the long and lengthy computations required in this practical application, as well as in astronomy and geodesy, encouraged exploiting the various relations between trigonometric functions to ease the burden of computation.[5]

The book finishes with a glimpse of the future. While van Brummelen notes the decline of one branch of inquiry, spherical trigonometry, the growing importance of trigonometry is established in different ways: the solutions of differential equations representing harmonic oscillations, hyperbolic trigonometric functions, Fourier series, and infinite series, to name a few. Furthermore, trigonometry directed mathematicians' attention towards fundamental, more general notions in mathematics, such as continuity, functions, series, and limiting concepts. These topics are promised in a sequel to this volume.

Just as van Brummelen intended, this book will have wide appeal, for students, researchers, and teachers of history and/or trigonometry. The excerpts selected are balanced and their significances well articulated. As well as giving many vital details that have shaped this discipline, he has made some important observations about the transmission of mathematical ideas. It is a book written by an expert after many years of exposure to individual sources and in this way van Brummelen uniquely advances the field. This book will no doubt become a necessary addition to the libraries of mathematicians and historians alike. We look forward to the sequel with great anticipation.

[5] Note, for example, the technique of *prosthaphaeresis* [264].

BIBLIOGRAPHY

Katz, V. 2008. *The History of Mathematics.* 3rd edn. New York.

Maor, E. 1998. *Trigonometric Delights.* Princeton.

Plofker, K. 2009. *Mathematics in India.* Princeton.

Von Braunmühl, A. 1900–1903. *Vorlesungen über Geschichte der Trigonometrie.* 2 vols. Leipzig.

Eastern Astrolabes by David Pingree

Historic Scientific Instruments of the Adler Planetarium and Astronomy Museum 2. Chicago: Adler Planetarium and Astronomy Museum, 2009. Pp. xxii + 268. ISBN 1–891220–02–0. Cloth $75.00

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I was present at the conception of this book 25 years ago. I recall the moment when Roderick and Marjorie Webster, trustees of the Adler Planetarium and Astronomy Museum, formally invited David Pingree (1933–2005) to catalogue the eastern astrolabes and related Islamic instruments in the Planetarium's collection. I had just arrived in Chicago to take up the position of Curator of the history-of-astronomy collection at the Adler Planetarium, and one of my first initiatives was to secure funding for an interpretive catalogue of the scientific instruments.[1] There was no debate on who should document the Adler's world renowned collection of astrolabes. Rod and Madge would prepare the catalogue of the western astrolabes with my help; and David Pingree, Professor in Brown University's Department of the History of Mathematics, would do the eastern ones.

To understand why Pingree was an outstanding choice, readers will need a brief history of the planispheric astrolabe.

The astrolabe is arguably the most sophisticated and elegant of early astronomical instruments. Fashioned of brass and mathematically complex, the astrolabe was both an observational tool and an analogue computer that could be used to solve astronomical, astrological, mathematical, and geographic problems.

The principal elements of the astrolabe include:

[1] National Endowment for the Humanities Planning Grant, Program of Museums and Historical Organizations (Sara Schechner Genuth, Principal Investigator/Project Director for an interpretive catalogue of scientific instruments at the Adler Planetarium), 1984–1989.

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- a pierced stereographic map of the stars, which rotates;
- a stack of plates, each engraved with the stereographic projection of the local coordinates for a particular latitude;
- a sighting- and angle-measuring device; and
- a suspension shackle.

Invented in the Greco-Roman world (perhaps in Alexandria) perhaps as late as the fourth century AD, the astrolabe incorporated mathematics from the time of Hipparchus and parts of earlier instruments—e.g., the Greek surveyor's *dioptra*, the anaphoric clocks of Vitruvius, second-century portable sundials with stereographic projections, and Ptolemy's observing armillary and horoscopic instruments. The earliest known treatise was written in Greek by Hypatia's father, Theon of Alexandria, in the late fourth century AD. By the seventh century, we have treatises in Syriac followed by others in Arabic in the eighth. Early production was centered on Ḥarrān, an ancient pagan city in northeastern Syria, where people worshipped the stars and scholars shared their interests in Greek philosophy, astronomy, and the astrolabe with Syrian neighbors who were Christians. Under ᶜAbbāsid rule (established in AD 750), Muslim astrolabists flourished in Syria. Sometime before the 10th century, knowledge of the astrolabe spread eastward from the Syro-Egyptian region to Iraq and Persia. Like those in Ḥarrān, the early workshops were predominantly located between the Tigris and Euphrates rivers. About a dozen Islamic astrolabes survive from this period; the earliest dated example is by Basṭūlus in AD 927/8.[2] Continuing eastward, the travels of Muslim scholars such as al-Bīrūnī may have brought the astrolabe to southern India in the 11th century, although the earliest known Sanskrit text on the astrolabe dates from about 1370. It was completed by a Jain scholar under the sponsorship of the Tughluk Emperor, Fīrūz Shāh, who also promoted the fabrication of astrolabes in India. No Indian instruments survive from this period, however. In the mid 16th century, the astrolabe was introduced to Mughal India from Persia, and Lahore became a center of the production of Indo-Persian astrolabes.

Moving westward along the southern Mediterranean, Muslim scholars also spread knowledge of the astrolabe to North Africa and

[2] The oldest in the Adler Planetarium's collection was made by Badr ibn ᶜAbdallāh in Baghdad in 1130/1 for the Saljuq Sulṭān, Mughīth al-Dīn Maḥmūd II.

Muslim Spain (Andalusia) by the 10th century, and from there to the Latin West as Christian and Jewish scholars traveled to Spain and returned with astrolabes and Arabic texts translated into Latin and Hebrew. Knowledge of the astrolabe may also have come directly to Europe from the Byzantine Empire and Greek sources. One Byzantine example dated 1026 survives and was clearly patterned after Islamic instruments.

The Adler Planetarium's collection of eastern astrolabes and related instruments is representative of this diverse history. Made in Spain and western North Africa, in West Asia, the Middle East, and South Asia, the instruments are engraved with inscriptions in Arabic, French, Hebrew, Latin, Persian, Sanskrit, and Turkish. Few scholars have the skills to analyze these instruments, and David Pingree was among them.

Pingree was Otto Neugebauer's successor at Brown, and renowned for his scholarship in the history of the exact sciences (notably astronomy and mathematics), magic, and astrology in ancient Mesopotamia, classical Greece, Byzantium, India, Latin Europe, North Africa, the Islamic world, as well as for his work in the linguistic and intellectual cultures that linked these regions. Pingree had come to know the Adler Planetarium's collection of astrolabes in the mid 1960s when working as a research associate at the Oriental Institute of the University of Chicago on a project with E. S. Kennedy. Pingree was studying the geographical treatises that astrolabe makers had used as their sources for producing the gazetteers inscribed on the instruments for mosque astronomers and other users to determine the *qibla* (the direction of Mecca) and the times of prayer. The Websters made the Adler's collection available to Pingree in an extraordinary way. Every Monday, they delivered an astrolabe to Pingree at the Oriental Institute and on Friday of each week they retrieved it. This went on until each Islamic astrolabe had been examined. Since this was 20 years before the catalogue project, the Websters and I welcomed Professor Pingree back to Chicago to re-examine the instruments. I well remember fetching the instruments for him and watching him pore closely over them with eyes weakened from diabetes. His intensity was as noteworthy as his generosity in sharing his knowledge with a young scholar like myself.

The catalogue that Pingree produced includes full descriptions of 49 eastern astrolabes. These are divided into those from eastern

Islam (the Mashriq), those from western Islam (the Maghrib), and Sanskrit Indian astrolabes in order to accentuate their differences. Each instrument is photographed in its entirety—both assembled and dismantled—in order to show details of the principal parts. The parts include:

- the *mater* (body), which is inscribed with circles of degrees, calendrical scales, horary quadrants, cotangent scales, gazetteers, and more;
- the *tympans* (plates) engraved with stereographic projections of the altitudes and azimuths of the sphere at given latitudes;
- the *rete* (star map), whose rotation on top of a *tympan* simulates the apparent rotation of the stars around the celestial north pole;
- the *alidade* (or diopter, an older term preferred by Pingree), which is used as a sight and sometimes also as a rule with mathematical scales; and lastly,
- the bolt and 'horse' (an equine-shaped pin) that secure the parts together.

Each catalogue entry fully documents these components, and Pingree is at pains to point out any unusual features. Inscribed words in Arabic, Persian, Turkish, or Sanskrit are transliterated in the entries. The catalogue entries, moreover, include tables of the stars named on the *retes*; cities and geographical parameters given on gazetteers; and the latitudes and longest daylights of the *tympans*. This is a noteworthy feature of the Adler catalogues, and something not typically done in the catalogues of other museums.

In addition, the catalogue also documents 27 other related Arabic, Islamic, or Sanskrit instruments in the Adler Planetarium's collection. These include astrolabe and horary quadrants, *qibla* indicators, sundials, dialing instruments, levels, artillery levels, celestial globes (described by Emilie Savage-Smith), and magic bowls. These entries are sequenced by alphabet letters rather than numerals because these instruments will eventually be included in other volumes of the Adler catalogue devoted to time-finding, surveying, and cartography. The idea of including them here in this volume was that an individual interested in all the Arabic or Islamic or eastern instruments in the collection could thus access them in one handy volume.

Pingree has written a terse historical introduction to the catalogue and a section devoted to biographies of the makers. Useful appendices include lists of Arabic and Sanskrit star names that appear on the instruments, which will enable scholars to see how naming conventions change over time and place. Other appended tables are devoted to astrological information on the instruments such as the planetary lords of the *decans*, terms and triplicities, and the lunar mansions. Bruce Stephenson, a curator presently at the Planetarium, includes a report on the metallurgy of the astrolabes as analyzed by high-energy X-rays at Argonne National Laboratory. Other back matter includes a concordance of catalogue numbers with Adler accession numbers, a bibliography, and an index.

It should be noted that *Eastern Astrolabes* is volume 2 in the series Historic Scientific Instruments of the Adler Planetarium and Astronomy Museum, and that it is intended as a companion to *Western Astrolabes* [see Webster and Webster 1998]. Readers unfamiliar with planispheric astrolabes will wish to consult volume 1's technical introduction and historical essay on the astrolabe and its uses cross-culturally. In volume 2, Pingree presumes that the reader is fully versed in astrolabe arcana, astronomy, astrology, and Muslim practices. By itself, this is a book written by one scholar for other scholars. But since the Adler Planetarium has one of the world's great astrolabe collections—on par with those at the British Museum, the National Maritime Museum in Greenwich, and the Istituto e Museo di Storia della Scienza in Florence, and second only to the Museum of the History of Science in Oxford—this is a volume well worth perusing.

This has been a book long in the making but the outcome is a significant contribution to the field of Islamic scientific instruments and the history of astronomy. It is regrettable that David Pingree did not live to see the book in print, but he would be very pleased to see his 'baby' recognized for the exceptional scholarship that it is.

BIBLIOGRAPHY

Webster, R. and Webster, M. 1998. *Western Astrolabes*. vol. 1. Chicago.

La Géographie *de Ptolémée en occident (IVe–XVIe siècle)* by Patrick Gautier Dalché

Terrarum orbis 9. Turnhout, Belgium: Brepols, 2009. Pp. 443 with 30 illus. ISBN 978–2–503–53164–9. Cloth € 95.00

Reviewed by
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The reviewed work, a piece of meticulous scholarship by Patrick Gautier Dalché, deals with the reception history of the *Γεωγραφικὴ ὑφήγησις* by Claudius Ptolemy, written shortly after AD 150 in Alexandria. Dalché covers the work's influence from late antiquity down to the first third of the 16th century. The *Geography*, consisting of eight volumes, was the Alexandrian scholar's second major work beside the better known *Σύνταξις μαθηματική* or *Almagest*. Until its rediscovery by the Byzantine scholar Maximus Planudes around 1295, the *Geography* had largely fallen into oblivion, although the work received mention every now and then in Arabic sources. In the West, it became available only through its translation into Latin by Jacopo Angeli in 1406. Afterwards, however, the distribution of the *Cosmographia*—thus the Latin title—underwent an explosive rise and significantly influenced the cartographical outlook of the West far into the 16th century.

After a short general introduction [ch. 1], Gautier Dalché offers a detailed overview of the transmission history of the *Geography* in late Antiquity and in the Byzantine East until its rediscovery by Maximus Planudes [ch. 2]. Thereafter, he considers the knowledge of the work in the Latin West before Jacopo Angeli's translation [ch. 3]. Presenting many testimonies, Gautier Dalché modifies the widely accepted doctrine that the Ptolemaic *Geography* was not known in the West before its 'rediscovery'. In the realms of astronomy, astrology, and geography, there are numerous indications that the Ptolemaic work was known in outline. It is undisputed that Ptolemy's overall *oeuvre*—especially the *Almagest* and the *Tetrabiblos*—exercised a significant influence on the geographical world view of the Late

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Middle Ages. This can also be gathered from the field of cartography. Given this background, the 'rediscovery' of the *Geography* and its translation into Latin at the hands of Jacopo Angeli must not in fact be overrated. The acceptance of Ptolemy's view of the world forms rather part of a process that took place in the framework of the intellectual *milieu* of the time (humanists, astrologers, astronomers, and physicians). Yet there seem to be no sources that indicate direct access to the original text of Ptolemy's *Geography* in the Latin West before 1406—that is, neither to its theoretical parts nor to the catalogue of places and the maps.

In the following three sections [chs 4–6], Gautier Dalché recounts extensively the reception of Ptolemy's *Geography* and its role as a 'model' for the history of science in the 15th and early 16th centuries. The author bases his account on a great number of testimonies. In the first half of the 15th century, humanists increasingly begin to engage with the ancient philosophy of nature, especially its cosmological models. This was also done with a view to doing research on areas of the world unknown to Ptolemy. In a first stage that lasts until the second half of the 15th century, the Ptolemaic maps are 'brought up to date'—for example, by redrawing the regions of Christian northern Europe—but their value is not put into question in any fundamental way. Only in a second stage, which begins with the editions of Nicolaus Germanus around 1460, are so-called *tabulae modernae* attached to the Ptolemaic maps. Nevertheless, the model of the Ptolemaic *Geography* remains dominant. Gautier Dalché refers to two obstacles to a quicker realization of the new view of the world: the poor translation of Jacopo Angeli, which renders the theoretical parts of the *Geography* insufficiently comprehensible, and the humanists' habit of using the *Geography* primarily to find out about ancient topography.

According to Gautier Dalché, the true scientific value of the *Geography* in comparison with the descriptive works of Mela, Pliny, or Strabo is only recognized in the works of Johannes Regiomontanus and his successors in southern Germany. These men increasingly directed their attention towards the theoretical parts that constituted the essential goal of Ptolemy's *Geography*, namely, an adequate cartographical representation of the Earth. This concern became only the more pressing the less the Ptolemaic view of the Earth could be brought in line with the discoveries made at the threshold of the

16th century. As a consequence, the 'modernization' of the maps at the beginning of the 16th century triggers a break with the traditional Ptolemaic view of the world. This trend manifests itself in two ways: on the one hand, in an ever more rigorous separation of the Ptolemaic maps from the *tabulae modernae*; on the other hand, in a more thorough critical engagement with the original Greek text of the *Geography*, resulting in the Greek *editio princeps* produced by Erasmus of Rotterdam in Basel in 1533.

Patrick Gautier Dalché's book offers an excellent *histoire intellectuelle et culturelle* that ranges from the 13th to the 16th century. Its strengths are grounded in its meticulous analysis of the sources. In the epilogue, Gautier Dalché rightly pleads for a more sustained attempt to make the documents accessible. However, the standards of quality that will be imposed on future projects will be quite high in view of the virtues of this book. An extensive catalogue of the sources and bibliography as well as an index and a section of tables round off the work; it is to be highly recommended in every regard.

The Mirror, the Window, and the Telescope: How Renaissance Linear Perspective Changed Our Vision of the Universe by Samuel Y. Edgerton

Ithaca, NY: Cornell University Press, 2009. Pp. xviii + 199. ISBN 978–0–8014–7480–4. Paper $19.95, £10.95

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In this book, Samuel Edgerton Jr aligns three objects—the mirror, window, and telescope—with three stages in the history of perspective, each having distinct implications for ways of seeing the natural world. The stakes are high. Like Panofsky, Gombrich, and others, Edgerton is convinced that perspective is bound up with the origins of modernity and modern science. His argument centers on a careful reconstruction of the use of linear perspective by the Florentine architect Filippo Brunelleschi and the polymath Leon Battista Alberti. He closes with an argument for Galileo's dependence on the perspectival tradition.

This version of Edgerton's story builds on a career in the history of art and optics. Three and a half decades ago, Edgerton began his *Renaissance Rediscovery of Linear Perspective* [1975] with a chapter titled 'The Western Window'. There, he identified Brunelleschi's lost 1425 painting of the Florence Baptistery as the first example of true linear perspective, raising the inevitable question of why linear perspective painting should have arisen in 15th-century Florence. For an answer, Edgerton pointed to late medieval Franciscan spiritual art and architecture in Florence, which seemed to provide artisanal parallels to how medieval philosophers such as Roger Bacon and Thomas Bradwardine valued optics for its theological insight. At about the same time (around 1400), Ptolemy's *Geographia* was rediscovered in the West, which provided three different examples of geometrical projection for mapping, mathematical techniques similar to that used in linear perspective. Optical imagery was 'in the air'. In laying out the strands of this cumulative argument, Edgerton

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meticulously reconstructed the mechanics of linear perspective from Alberti's descriptions, explaining the three technical requirements of linear perspective: vanishing point, distance point, and horizon line isocephaly (whereby the horizon line is presented at the same level as the viewer's eyes). This accumulation of events, texts, techniques, and people, argues Edgerton, is evidence for a strong connection between perspective and the rise of modern science. Perspective entailed an 'objective' way of seeing, on this argument, because it created the expectation that a picture be like a window faithfully presenting the reality beyond.

Edgerton was fully aware that such a claim for Florentine exceptionalism entailed a bolder claim for Western exceptionalism. In 1991, he published *The Heritage of Giotto's Geometry: Art and Science on the Eve of the Scientific Revolution*. This volume mixed, *inter alia*, Joseph Needham's analysis of European scientific singularity with Edgerton's own elegant analysis of the art and geometry of artisanal practice. For example, a chapter on the 'Geometrization of the Supernatural' detailed the apparently pervasive medieval desire to 'see' how God sees geometrically—a desire which some Franciscans, Brunelleschi, and Alberti thought could be actualized by means of perspective. Here Edgerton expanded on comments he had made earlier about a 'centralizing tendency' which can be seen—for one instance—on the walls of the Basilica of San Francesco, Assissi. There angles of painted modillions and dentils converge to a vertical axis, hinting at the vanishing point in linear perspective. After a suggestive argument about Galileo's indebtedness to the perspectival tradition in recognizing the three dimensions of Moon 'spots', Edgerton compared Western and Chinese knowledge of perspective. Jesuits took the geometry of perspective eastward with them, and it seems that Chinese manuals only begin employing perspectival images after that point.

In *The Mirror, the Window, and the Telescope*, Edgerton both recapitulates and adds to the argument developed in his previous books. Again, he starts with how Western Renaissance art begins to look very different from medieval art. His 15 short, crisp chapters can be roughly divided into four groups organized around three figures significant to historians of science and art. In the first group [chs 1–5], Edgerton sets Florentine thinking about geometry and optics against the background of late medieval religious values. Considering

‘T-O *mappae mundi*’ (shaped like a ‘T’ within an ‘O’), he suggests that their centering on Jerusalem stimulated Roger Bacon and others worried about Christendom’s global fortunes to think hard about the technological benefits promised by optics, such as burning mirrors. In parallel, the Franciscan mandate to preach and convert was focused on attempts at realistic representation of devotional scenes. Such visual preaching matched an understanding of optics as insight into God’s own way of seeing, as Robert Grosseteste and Meister Eckhart held. Having painted these various levels of optical meaning, Edgerton considers Fra Antonino, the Archbishop of Florence. His *Summa theologia*, Edgerton tells us, was a condensation of ideas that he had already aired to the Florentine public in popular sermons. So when Archbishop Antonino describes intellective power in the technical terms of optics, Edgerton wants the reader to consider the impact:

> What effect might Antonino’s preaching have had on fifteenth-century Florentines, especially artists, who were also beginning to think of their pictures as mirrors reflecting the grandeur of God’s Creation? [36]

Having compared Edgerton’s hypothesis of what was in Antonino’s sermons to my own reading of Antonino’s *Summa*, I remain unconvinced that the sources support such a strong causal inference. But, causal arrow aside, Edgerton’s worrying of the Florentine context is evocative. By closing the fifth chapter of the book with the supported claim that optics and mirrors employed by artisans like Brunelleschi were invested with intertwined spiritual, intellectual, and practical meanings, Edgerton avoids reductive dichotomies of theory and practice.

The second group of chapters [chs 6–9] is dedicated to a vindication of Edgerton’s earlier reconstruction of how Brunelleschi first painted the Baptistery of Florence, as recounted by Alberti. Using computer modeling and his own photographs of the Baptistery, Edgerton argues (against other reconstructions, such as that by Richard Krautheimer and David Summers) that Brunelleschi must have drawn the first image by transferring it from a mirror, with his back to the Baptistery. Contemporary evidence from artisan Antonio Manetti (Filarete) indicates that other artisans were aware of the foreshortening effects of mirror images. Manetti’s description of

Brunelleschi's work explicitly mentions Brunelleschi's use of a mirror [69 (quotation)]. The pictorial illusion of perspective, for Edgerton's Brunelleschi, is seeing in a mirror—it is seeing truly. Chapters 10 and 11 connect Edgerton's account of early 15th-century perspective to Florentine religious paintings, suggesting that by flouting rules of perspective (or mirroring), artists were making statements about how sacred subjects might, or might not, be seen with fleshly eyes [cf. 116].

Edgerton devotes a third group of chapters to Leon Battista Alberti's rules for constructing perspective pictures. Though the account is more nuanced than I represent here, this section is driven by how Alberti employed the metaphor of a latticed 'window' to direct the creation of a perspective painting. This construct, Edgerton argues, encouraged thinking about the painter as replicating events, objects, and people on a realistic background. Rather than simply mirroring reality, a perspective painting faithfully organizes nature. Edgerton pursues this epistemic implication of Alberti's method through the religious art of Raphael and Titian, suggesting that such geometrical organization was too concrete to convey abstract dogmas without becoming absurd. Windows are open to nature but do not peer into heaven.

The last chapter stands as its own group, connecting the optics of perspective to Galileo's telescope or 'perspective tube'. In 1609, Thomas Harriot also used a telescope to observe and even draw the Moon. Galileo alone, however, noticed that the Moon's 'spottedness' was due to three-dimensional mountains and valleys on the lunar surface. Edgerton argues that this insight, and the paintings that Galileo made from his observations, are as artistic as they are scientific. As the postscript makes clear, Edgerton sees the language of art and the language of science merging in perspective, encompassing both disciplinary domains, much as do modern computer-generated images of distant galaxies.

This is a telling note on which to end the book. Not only does art appropriate the methods of optics to become more objective but the representations of science are also art. I doubt that Edgerton makes this point to introduce subjectivity into his definition of science. As he says in the preface, Edgerton sees himself as an apologist for linear perspective in art history which 'no longer considers it [perspective] a positive idea' but instead sees it as 'merely a brief sidetrack in the evolution of world art' [xiv]. Of course, being aimed at 'the general reader' [xv], the book cannot address every minor controversy. But not all controversies are minor. Some readers may be uncomfortable with the implicit dichotomy between 'the persistence of religious belief' and the subjective representation of dogma in medieval non-perspectival art [144–147], on the one side; and, on the other side, secularized perspective, objectivity, and science [see especially preface and epilogue].

Moreover, the correlation of three ways of seeing or knowing with three objects is a conceit bearing an air of inevitable scientific progress through objectivity. Edgerton, after quoting from Galileo's description of the Moon in the *Sidereus nuncius*, exclaims:

> Did ever a Baroque painter express the new secular spirit of landscape art better than this? ... Moreover, after thus having marveled at the picturesque lunar terrain, Galileo quickly reverted to his scientific self .... [163]

Subjective artistic experience is something separable from the secularizing objectivity of the scientific self, apparently. Those lacking confidence in this dichotomy might have wanted, for example, Edgerton to provide some critical interaction with James Elkins' *Poetics of Perspective* [1995], which influentially explored how the notion and practice of 'perspective' developed into a metaphor for subjectivity during the same period covered by *The Mirror, the Window, and the Telescope*. (Elkins is listed in Edgerton's bibliography, and thanked in the preface.) But the only difference Edgerton notes between them is that Elkins believes Brunelleschi's insight to be less sudden than does Edgerton—which seems to miss the deeper point of disagreement [90]. Neither does Edgerton address Stuart Clark's massively documented *Vanities of the Eye* [2007], which powerfully shows how dubious was the epistemic status in which early moderns held vision, mirrors, and other phenomena related to optics. By skirting such debates, Edgerton seems to repeat older definitions of art and science, not to provide new arguments.

Despite these caveats, the appeal of Edgerton's book lies first in the elegance, refined over many years, with which he presents the basics of Renaissance linear perspective. That elegance is found in the same simplicity that I have questioned. And that simplicity will make this an excellent undergraduate text. Historians of science who have read Edgerton's other work may not find a new interpretation of the history of optics and perspective—but they will find fresh insight into the concrete interactions that Edgerton finds between the mechanics of perspective and Renaissance art in several Florentine contexts.

BIBLIOGRAPHY

Clark, S. 2007. *Vanities of the Eye: Vision in Early Modern European Culture.* Oxford.

Edgerton, S. Y. 1975. *The Renaissance Rediscovery of Linear Perspective.* New York.

________ 1991. *The Heritage of Giotto's Geometry: Art and Science on the Eve of the Scientific Revolution.* Ithaca, NY.

Elkins, J. 1995. *The Poetics of Perspective.* Ithaca, NY.

In the Age of al-Fārābī: Arabic Philosophy in the Fourth/Tenth Century edited by Peter Adamson

Warburg Institute Colloquia 12. London/Turin: The Warburg Institute/Nino Aragno Editore, 2008. Pp. xii + 302. ISBN 978–0–85481–147–2. Paper £40.00

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As Peter Adamson writes in his preface, this book includes the proceedings of a conference held at the Warburg Institute in 2006. It is the second publication of a series that aims to explore topics and currents in the philosophy of the Arabic-Islamic world.

The 17 articles in this volume cast light on some of the most relevant figures, trends, and themes of Arab-Islamic thought in and around the 10 century (the fourth century of the Islamic calendar); and they offer analysis of different intellectual traditions and comparative investigations of particular topics and arguments. They draw a structured picture of this complex and vivid period, which was surely formative in shaping the subjects and the doctrinal contents of philosophy in the Islamic world.

Abū Naṣr al-Fārābī (870–*ca* 950), the 'second master', is probably the most significant thinker of this period, whose writings have been published and translated by modern scholars. His influence on later so-called Aristotelian philosophers has been documented not only within the Islamic tradition (e.g., ibn Bājja and Averroes), but also in the Jewish one. Maimonides, for example, wrote that in order to learn logic al-Fārābī's logical treaties should be studied and that all that he wrote is full of wisdom. As a particularly representative figure of 10th-century thought, al-Fārābī's views and arguments are referred to, directly or indirectly, as a term of comparison in a number of the articles in this book that seek to point out influences, differences, or parallels between different authors and tendencies.

The vast movement of translation of scientific and philosophical works from Greek into Syriac and thence into Arabic, as well as

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directly from Greek into Arabic, cannot be separated from the rise and the development of Arabic-Islamic philosophy. This translation movement began in the first decades of the ninth century under the Abbasid caliphate—the first evidence of translation activity actually goes back to the end of the eighth century—and continued through different phases until the first half of the 10th century. In nearly 150 years, there came into existence a corpus of writing in Arabic which was based essentially on texts of the philosophical curriculum of Neoplatonic schools in late antiquity and, in particular, on the Alexandrian model. These translated texts became the starting point for the specific system of thought that was *falsafa*, with its different traditions and the variety of its developments.

The last phase of this process of acquiring Greek learning is connected to the 'Aristotelian school of Baghdad' and related to the revival of Aristotelian studies in the capital of the Abbasid Empire in the 10th century. The school of Baghdad is characterized, among other things, by philosophical education, the interpretation of Aristotle, and the continued translation of further works by Aristotle and his Alexandrian commentators or the renewed translations of works that had already been translated (notably by Abū Bishr Mattā ibn Yūnus, d. 940, and some of his disciples). In this context, one paper in the volume (by E. Giannakis) is devoted to the study of the views of the philosopher Anaxagoras (fifth century BC) as they are reported in the Arabic commentaries of Aristotle's *Physics*, which are based on Alexandrian commentaries.

The two most representative figures of this circle are al-Fārābī and the Christian Jacobite translator, theologian, and philosopher Yaḥyha ibn ᶜAdī (d. 974). C. Ehrig-Eggert considers the question of the existence of general notions (universals) according to ibn ᶜAdī. This leads to an examination of the central theological problems of divine knowledge and the knowledge of particulars, which will be crucial also in later *kalām* and philosophy.[1] Ehrig-Eggert also analyzes the positions of ibn ᶜAdī's contemporary (and teacher), al-Fārābī, on these matters and points out the two authors' common sources in order to identify specific Christian elements and goals in ibn ᶜAdī's argument.

[1] Recall, for example, al-Ghazālī's criticism of the philosophical positions on these points.

The related question of the nature and the possibility of knowledge, and consequently of the use of Greek sources on that matter, is the subject of one contribution (by D. L. Black) dealing with Meno's paradox in al-Fārābī's writings.

Another group of papers deals with thinkers whose education and philosophical arguments can be ascribed to a second intellectual tradition that characterizes philosophy in the Arabic-Islamic world, Neoplatonism.

The group of translators who were gathered around Abū Yūsuf Ya'aqūb ibn Isḥaq al-Kindī (d. *ca* 870) and known as the 'circle of al-Kindī' produced Arabic translations of fundamental Neoplatonic texts,[2] whose importance would be crucial for the development of *falsafa*. Kindī exerted influence *via* his teaching, his disciples' teaching, and their written transmission of his works. In the West (North Africa and Andalusia), al-Kindī's teachings became direct sources for such Neoplatonic Jewish thinkers as Isaac Israeli (*ca* 850–950), whose writings in Arabic show the author's familiarity with al-Kindī's treatises. In the oriental part of the Empire, Abū al-Ḥasan al-Āmirī (d. 992) was one of the major disciples of al-Kindī: his teacher, ibn Balḫī, was al-Kindī's immediate disciple. Only a few of al-Āmirī's works are still extant, but he was probably well known in his time and his teaching influenced two other 'Kindian' thinkers of the late 10th-century in Baghdad's intellectual circles. As E. Wakelnig notes, there are quotations and non-literal references to al-Āmirī in the works of al-Tawḥīdī (d. 1023) and Miskawayh (940–1030) as well as in anonymous sources, citations from which Wakelnig derives information about al-Āmirī's biography, aspects of his philosophical thought, and a lost work. The encyclopedist ibn Farīghūn (second half of the 10th century) was ibn Balḫī's disciple too. H. H. Biesterfeldt's paper presents ibn Farīghūn's unique work on the classification of sciences, its structure, and its doctrinal and literary contexts. D. C. Reisman's paper on Abū Ḥāmid Aḥmad ibn Abī Isḥaq al-Isfizārī (first or second half of the 10th century) discusses a very little known thinker. This contribution gives an accurate account of his biography, his known

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[2] Notably, Proclus' *Elements of Theology*, the *Theology of Aristotle*—a paraphrase of the Arabic translation of Plotinus' *Enneads* 4–6—and the *Book of Aristotle's Explanation of the Pure Good* known as *Liber de causis*.

and extant works, some aspects of his doctrine, his intellectual tradition and, especially, the variety of his classical and Alexandrian philosophical sources.

This Neoplatonic tradition, which was transmitted by the circle of al-Kindī, is a crucial source also of Ismaʿīlī thought, even if this doctrine is primarily a religious theme within Shi'a. Two contributions (by A. Straface and D. De Smet) consider aspects of this intellectual tradition. The first gives a detailed account of Neoplatonic elements and concepts related to esoteric and symbolic Ismaʿīlī thought; the second analyzes the influence of al-Fārābī on al-Kirmānī's through a comparative study of their doctrine of Intellects.

The review of the intellectual developments in this period would not be complete without discussing the Iḫwān al-Ṣafā' (Brethren of Purity), authors of the earliest encyclopedia of sciences of the Islamic world, whose compilation has been chronologically placed between 961 and 980 [see Marquet 2010]. The questions of the religious affiliation of the authors and their intellectual orientation, as well as the classification of their *Epistles* from a doctrinal point of view, have been the subject of many studies. The Shiite, and specifically Ismāʿīlī, theological background, the variety of philosophical sources (Plato, Aristotle, Plotinus, Galen, for instance), and the diversity of themes treated by them are discussed in three articles in the book. The first by (C. Baffioni) deals with aspects of the Brethren's cosmology and epistemology; the second (by G. de Callataÿ) addresses their teachings on science; and the third (by P. L. Heck), their positions in political theory, epistemology, and ethics.

Although the Aristotelian and Neoplatonic traditions are two of the major trends of thought in the Islamic world (of course, boundaries between them are not geometrically rigorous), the role of Qur'anic sciences and, in particular, of theology (*kalām*) and theological problems must be taken into account for a comprehensive overview of this period. It should be remembered that even for theologians and thinkers who rejected philosophy, the Greek scientific and philosophical heritage provided methodological bases and concepts for their reflection. By the same token, the development of philosophy cannot be separated from that of theology.

An analysis of al-Fārābī's *Principles of the Opinions of the Inhabitants of the Virtuous City* (by U. Rudolph) takes into account the connection and interaction between different trends within Arabic-Islamic thought (in particular, between theology and philosophy) by analyzing some elements of the title of this treatise, its structure, and some of the themes discussed in it. The article seeks to determine the purpose of the book with reference to its historical context, and to show how its structure and arguments relate to theology and to challenges that faced theological treatises of the same period.

M. Rashed's paper offers a clear and detailed reconstruction of a dispute on the specific theological topic of the inimitability of the Qur'ān: this dispute involved a number of figures directly and indirectly; and Islamic-Christian controversy, polemical Islamic texts and Muᶜtazilite discourse form the historical and theoretical context in which it took place.

The variety of trends, matters, influences, and developments that characterize Islamic thought of the 10th-century extends also to the status of medicine in the hierarchy of sciences and its link with philosophical speculation. This is the subject of L. Richter-Bernburg's contribution. It offers a comparative analysis of the attitude towards medicine as a discipline in the writings of al-Fārābī and Abū Bakr al-Rāzī (864–925), a renowned physician and a controversial philosopher.[3] Some aspects of al-Rāzī's medical thought are taken into account also by P. E. Portman, whose article deals with the methodology of medicine and its practice. The philosophical thought of al-Rāzī is taken into account by P. Adamson, who examines his ethical ideas, in particular, those concerning pleasures: Adamson analyzes the statements expressed in al-Rāzī's Greek sources, his use of them, and his position relative to them and to some contemporary arguments. D. Urvoy, finally, explores eventual intellectual and historical links between al-Rāzī and Yaḥyha ibn ᶜAdī: taking his cue from an obscure note of the historian al-Masᶜūdī (*ca* 896–956) which puts these two thinkers together, Urvoy aims to explain the purpose of al-Masᶜūdī's statement through a meticulous historical and doctrinal analysis.

Some of the investigations in this volume draw on manuscripts and other unpublished sources, of which unfortunately no index is

[3] Because of his 'unorthodox' philosophical and theological positions, Urvoy [1996] and Stroumsa [1999] count him among the 'free thinkers' of Islam.

provided. This is notably the case with the research on the biography and philosophical teaching of Abū l-Ḥasan al-ᶜĀmirī [Wakelnig, 215 ff.], and on the obscure and very little known philosophers Abū Ḥāmid Aḥmad ibn Abī Isḥaq al-Isfizārī [Reisman, 239ff.] and ibn Farīghūn [Biesterfeldt, 265 ff.]. I, therefore, thought it useful to list those unpublished sources here. The list below follows the order of the table of contents and indicates the pages of the book where the manuscripts are mentioned.

Article 3: Manuscripts containing a 10th-century philosophical correspondence dealing with Anaxagoras' theory of homeomeries and the so-called 'Baghdad Physics' respectively

London, British Museum, Or. 8096:	35
Leiden, University Library, Warner Or. 583:	36

Article 14: Quotations and fragments of al-ᶜAmīrī's philosophical writings

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Istanbul, Aya Sofya 4130:	220
Tehran, Kitābkhāna-yi Millī-yi Malik, 4694:	220
Hyderabad, Osmania Univ. Library, Acq. 1411:	220–221
Istanbul, Servili 179:	221
Istanbul, Esad Efendi 1933:	221
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Article 15: Identified manuscripts of al-Isfizārī's extant works

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Damascus, Zāhiriyya 4871:	242, 244
Kitābkhānah-yi āqā-yi Duktur Aṣghar Mahdavī 596–597:	243

Article 16: Extant manuscripts of ibn Farīghūn's work

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Article 17: Manuscripts attesting a passage of an epistle of Qustā ibn Lūqā and two sources of *kalām* authors

London, British Library, Or. 8613:	280, 282
Leiden, Or. 2949:	280
Ṣan'ā, ᶜilm al-kalām 189:	284

See contents and updated bibliography in Adamson and Thaylor 2005, D'Ancona Costa 2005 and 2007, and Nasr and Leaman 1996.

BIBLIOGRAPHY

Adamson, P. and Thaylor, R. 2005. edd. *The Cambridge Companion to Arabic Philosophy.* Cambridge.

D'Ancona Costa, C. 2005. ed. *Storia della filosofia nell'Islam medievale.* Torino.

________ 2007. ed. *The Libraries of the Neoplatonists.* Leiden/Boston.

Marquet, Y. 2010. 'Ikhwān al-Ṣafā''. Volume 3.1071–1076 in P. Bearman, T. Bianquis, C. E. Bosworth, E. van Donzel, and W. P. Heinrichs edd. *Encyclopaedia of Islam.* 2nd edn. Brill Online. Leiden.

Nasr, S. H. and Leaman, O. 1996. edd. *History of Islamic Philosophy.* London.

Stroumsa, S. 1999. *Freethinkers of Medieval Islam: ibn al-Rāwandī, Abū Bakr al-Rāzī, and Their Impact on Islamic Thought.* Leiden.

Urvoy, D. 1996. *Les penseurs libres dans l'Islam classique. L'interrogation sur la religion dans l'Islam classique.* Paris.

Apollonius de Perge, Coniques. *Tome 2.2: Livre IV. Commentaire historique et mathématique, édition et traduction du texte arabe* by Roshdi Rashed

Berlin/New York: Walter de Gruyter, 2009. Pp. xii + 319. ISBN 978–3–11–019938–3. Cloth € 99.95, $140.00

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This book is part of a bigger, and important, project (*Apollonius de Perge, Coniques. Texte grec et arabe établi, traduit et commenté*) involving a commented edition and French translation of Apollonius Arabicus, that is, the seven extant books of Apollonius' *Conica* (the last three of which are preserved only in Arabic), and a new edition and French translation of the Greek text. It is the work of a team of scholars under the leadership of Roshdi Rashed,[1] who, for the first time to my knowledge, studies systematically the 'elementary books', 1–4, in their Arabic guise and compares them to the Greek, Eutocian text, making them also available in a Western language. The book appears in the series Scientia Graeco-Arabica edited by Marwan Rashed, the son of the book's chief editor and a well-known scholar of ancient philosophy and specialist in Alexander of Aphrodisias.[2]

The multi-volume project comprises four volumes in seven:

Volume 1: 1.1: *Livre 1. Commentaire historique et mathématique, édition et traduction du texte arabe* and 1.2: *Livre 1. Édition et traduction du texte grec* respectively by Rashed and by his two partners, Descorps-Foulquier and Federspiel [2008]

[1] The others are Micheline Descorps-Foulquier and Michel Federspiel.

[2] The editor's wife, Françoise Rashed, is another family member participating in the project (*on reste en famille...*), being responsible for the diagrams, which, by the way, are not always easy to disentangle due to their size. There also seems to have been no attempt at their collation.

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Volume 2.1: *Livres 2 et 3. Commentaire historique et mathématique, édition et traduction du texte arabe* by Rashed (forthcoming)

Volume 2.2: *Livre 4. Commentaire historique et mathématique, édition et traduction du texte arabe* by Rashed [2009a]

Volume 2.3: *Livres 2–4. Édition et traduction du texte grec* by Descorps-Foulquier and Federspiel (forthcoming)

Volume 3: *Livre 5. Commentaire historique et mathématique, édition et traduction du texte arabe* by Rashed [2008].

Volume 4: *Livres 6 et 7. Commentaire historique et mathématique, édition et traduction du texte arabe* by Rashed [2009b].

It is still a work in progress, to be finished during 2010; and it promises to fulfill a longstanding *desideratum*, that of a reliable edition and translation of the complete Arabic *Conics*, supplementing Toomer's still fundamental two-volume edition and English translation of books 5–7 in Banū Mūsā's version [1990].

Book 4 of the *Conics*, the object of this review, belongs, together with the first three books, to the 'elementary' part of the treatise. It deals with the greatest number of points at which conic sections, including the double section, can meet one another and the circumference of a circle. On this the Greek and Arabic texts agree, though, as shown by Rashed, there are otherwise extensive differences between the two. Neither the Greek nor the Arabic text is fully systematic in its exposition (Rashed 'corrects' this in his analysis), though the latter comes closer to that goal.

The text established by Rashed is based on the collation of four manuscripts out of the nine discussed in chapter 3 of volume 1 of the edition ('Histoire des textes').[3] These manuscripts were copied in the 11th and 13th centuries and include one, the earliest, copied by ibn al-Haytham in 1024. Rashed has established a *stemma* in volume 1 on the basis of 'the study of the manuscripts, their history, [and] the accidents of transcription—omissions, additions, language

[3] This contradicts the claim made in the same place: 'Il nous est en effet parvenu sept manuscrits de la traduction' [218]. It is also not clear what the 27 mss listed under 'sigla' in the volume under review exactly are [xi].

faults, mathematical errors, geometrical diagrams' [2008, 232]. This vacuous generality, however, applies generally to all collations worthy of the name. In its disarming vagueness and the lack of any specific procedural details it is, to say the least, utterly disappointing.

Apollonius speaks explicitly, and generally, of book 4 of the *Conics* in two places, the letter to Eudemus accompanying the dispatch of the first book and the letter to Attalus introducing book 4 itself. The two statements are basically in agreement:

> The fourth book shows in how many ways the sections of a cone intersect with each other and with the circumference of a circle, and contains other things in addition none of which has been written up by our predecessors, that is in how many points the section of a cone or the circumference of a circle and the opposite branches meet the opposite branches. [Taliaferro 1952, 603]

and

> This book treats of the greatest number of points at which sections of a cone can meet one another or meet a circumference of a circle, assuming that these do not completely coincide, and, moreover, the greatest number of points at which a section of a cone or circumference of a circle can meet the opposite sections. Besides these questions, there are more than a few others of a similar character. [Fried 2002, 1][4]

Now, here is the first and shortest of Rashed's many descriptions of the book, in which he improves on Apollonius:

> Dans le quatrième livre des *Coniques*, Apollonius traite du nombre des points communs à une droite variable et à une conique, ainsi que du nombre des points communs à deux coniques quelconques. [v]

Apollonius, of course, does not mention explicitly variable lines and their intersections with given conics. Being a Greek, he could not. Strictly speaking, he never spoke of the intersection of curves with

[4] Rashed does not seem to be aware of this book. The statement appearing in the Arabic edition established by Rashed [116, 117] is essentially the same.

variable lines. This inclination to over-interpretation, sprinkled, however, with numerous sane statements, is not a mere oversight on Rashed's part, as we shall see.

The Arabic text of the *Conics*, which is extant in a number of manuscripts and in at least two translations (to say nothing of the rich tradition of commentaries, abridgments, completions, paraphrases, epitomies,and so forth, which it engendered) by Thābit ibn Qurra and the team of Hilāl ibn Abī Hilāl al-Himṣī and Isḥāq ibn Hunayn, is not just more complete than the Greek text preserved by Eutocius in containing the last extant three books, 5–7; it is also as a rule more reliable at least (but not only) with respect to book 4.[5] If one grants Rashed his editorial *modus operandi* (and this is not as simple as it may sound), then he has shown convincingly the superiority of the Arabic manuscript tradition over the Greek. Still, the question remains: Is this an untainted *manuscript* tradition of the *Conics*?

In his pathbreaking edition and translation of books 5–7 of the *Conica* in 1990, Toomer has shown that in

> almost every instance where *H* [Rashed's main ms.] presents a text different from [that of the other mss used], the reading of *H* makes better sense mathematically. The reason is surely that in these cases ibn al-Haytham changed what he found in his exemplar in order to present a mathematically 'correct' text. *H*, then, represents that bugbear of the textual critic, the 'intelligent scribe'. There can be no doubt that in almost every case where *H* presents a reading 'superior' to that of [the other used mss], the 'inferior' reading is that of the archetype.... But, since *H* is certainly descended from that archetype... there are a few places where it is at least possible that *H*'s text is more faithful to the original. [1990, 1.lxxxix–xc]

As a result of this troublesome state of affairs, Toomer's wise editorial principles dictated that he

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[5] There is, however, a serious problem here because of the weight Rashed gives in his edition to *A* (*H* in Toomer's edition [1990]), the manuscript of 1024, a transcription by ibn al-Haytham which is not an innocent transcription but contains heavy recensional elements that improve the mathematics of the archetype which it allegedly transcribes.

> deliberately [keep] a number of mathematical errors which, in [his] judgment, are to be laid at the door of the Banū Mūsā, the Arabic translator, or possibly the imperfect Greek exemplar from which he was working. Hence, in most cases where *H* offers a mathematically superior reading, [he has] preferred the 'faulty' reading of [the other mss], since, as remarked above, almost all such differences are due to deliberate correction by ibn al-Haytham, and have little weight as textual evidence. [1990, xc]

What all this means, of course, is that somebody like Rashed who relies heavily on *A* (*H* in Toomer) in establishing a critical *manuscript* text is in deep water.[6]

While it is possible that the imperfections in the Greek text of book 4 stem from a corrupt source, the Arabic text, the source of which seems to be a better archetype than that relied upon by Eutocius, seems to be less defective and more systematic in its presentation. This conclusion is, however, marred by Rashed's excessive and uncritical reliance on the manuscript by ibn al-Haytham.

Thus, Apollonius scholars, who are now required to take into account the Arabic manuscript tradition of the seven extant books of the *Conics,* should always keep handy, near Rashed's text, Toomer's sober edition as a salutary corrective.

We now come to Rashed's historical analysis of the text. It is acutely, distressingly wanting. It is blatantly, and consciously, anachronistic, using concepts and mathematical procedures foreign to Greek mathematics and to the *Conics*; and it does this proudly, stridently, demonstratively, in full awareness of the discrepancy between text and commentary, in the wrong belief that this is the best way to understand the Apollonian text. This is already clear in the introduction to the first volume of the project:[7] 'Dire que les *Coniques* sont un livre de géométrie, c'est enfoncer une porte ouverte' [Rashed et *alii* 2008, vii]. How nice! Thus he writes:

[6] This has also negative bearings on Rashed's much praised superiority of the Arabic *manuscript* tradition over the Greek.

[7] To limit the length of this review, my examples shall be exemplary, not exhaustive.

> It is enough to glance at this treatise to realize the full absence of any equations of plane curves and of any algebraic concept whatsoever. One could easily verify, for example, something well established long ago, that the concept of *symptoma* is not at all equivalent to that of an equation. [2008, vii]

Eminent historians and mathematicians—Heath and Bourbaki foremost among them—all knew this and yet did not hesitate to read the *Conics* algebraically [2008, vii]. Rashed will follow their glorious example. Since it is abundantly clear what the *Conics* is, geometry, we may as well elucidate it by means of what it is not, algebra. This is precisely Rashed's reasoning. Thus, he says:

> The appeal to the terminology of algebraic geometry [*sic*] runs the risk of displeasing some.... [Apollonius'] is a geometrical theory of conic sections: no algebraic, projective, or differential geometry. And yet, we took the liberty of appealing in our commentaries to algebraic geometry,[8] incurring thus, in full awareness, the reproach of anachronism from the guardians of the temple. [2008, vii]

Why proceed this way? Answer: Because the proper way of reconstructing the past is not only by starting from the present but by keeping it always in sight. *Ipse dixit. Q.E.D.*

This is how Heath and Zeuthen proceeded when appealing to geometric algebra in their elucidation of the *Conics* and this is also the 'historical' methodology of Bourbaki.[9] There is no inconsistency in such an approach, since it represents

> the deliberate choice of a style of writing history, by retrograde elucidation, as practiced by Bourbaki: starting with the present to restitute the past; it is also a matter of didactic concern: addressing one's contemporaries in their mathematical language. [2008, viii]

Still, Rashed's reasons for calling on 'algebraic geometry' (*sic*) as his main historical interpretive tool are different [2008, viii], one being instrumental and the other historiographic.

[8] Rashed appears occasionally to speak indiscriminately of 'algebraic geometry' and 'geometric algebra'.

[9] Surprisingly, and inconsistently, it seems to me, Rashed rejects the legitimacy of geometric algebra.

Once the historian has established the ancient mathematical text on solid grounds, it is incumbent upon him to use all tools at his disposal to plumb its richness, uncover its underlying structures, verify its results, and check the limits of its internal logic. It is only 'in this manner that what made of this work an inexhaustible source for later mathematics is bound to become manifest' [2008, ix] and explain its great appeal throughout the centuries. Keeping faith with the text, its *mathesis*, its mathematical procedures and concepts, on the other hand, is limiting and runs the risk of issuing into a mere paraphrase. And here comes the unbelievable statement, quoted in the original for its pregnancy and offensive outspokenness:

> Pour lire une oeuvre mathématique ancienne, il nous a donc semblé nécessaire de solliciter l'aide d'une autre mathématique, à laquelle on emprunte les instruments qui pouront en restituer l'essence. Un modèle construit dans une autre langue mathématique permet en effet d'aller plus loin dans l'intelligence du texte, particulièrement lorsque cette langue est celle d'une mathématique plus puissante, mais qui trouve dans l'oeuvre commentée l'une de ses sources historiques. Pour les *Coniques*, c'est la géométrie algébrique élémentaire qui fournit ce modèle. [2008, ix]

It simply could not be said better! Still, as if this were not enough, it is followed by the conceptually self-contradictory statement:

> In short, if the instrumental use of another kind of mathematics seems to us indispensable for commenting an ancient work, it is only because of the diffuse relation of identity and difference which unites the one to the other. That the instrument, the model, is not the object is a truism. They simply do not concern the same *mathesis*. [2008, ix]

So far the instrumental reason for opening widely the welcoming door to anachronistic history.

Now, what is Rashed's 'historiographic' reason for writing the kind of history that he does? It is, in a nutshell, the need to unveil the historical *fortuna* of the text or texts studied, the attempt to see in it or them what its or their successors found in those texts, how they used them, and what they inspired them to achieve. Again, Rashed says it best:

> Starting with the IXth century, one discerns in the study of the *Conics* an extension of some of its chapters, as well as their application to the most diverse domains, and their essential contribution to the creation of elementary algebraic geometry. To convince oneself that this is indeed the case, it suffices to read the *Algebra* of al Khayyām, *The Equations* of Sharaf al-Din al Tūsi, the *Geometry* of Descartes, the *Tripartite Dissertation* of Fermat. Neglecting the context of the successors leads inevitably to the mutilation of the studied work's history. *Even when they transform its meaning, the successors allow the historian, in effect, to grasp the work with increased clarity and profundity. This endeavor has indeed been ours.* [2008, x (my emphasis)]

The real challenge of the historian consists in using all the means at his disposal, philological, historical, mathematical

> to bring to the further progress of historical research, pushing it a little farther than the achievements of his eminent predecessors (especially E. Halley, I. H. Heiberg, P. Ver Eecke). [2008, x]

So, we have it now from the horse's mouth: proper historical study of past mathematics comes from illuminating it with the blinding light of latter-day results somehow stemming from it.

These views, needless to say, color also the book under review, in which Rashed establishes an authoritative Arabic text[10] and a faithful French translation, a lasting contribution to Apollonius studies, to which, alas, he adds numerous mathematico-historical commentaries, practically all of them contaminated by anachronism. His text differs from the Eutocian Greek text in both trivial and substantive matters. As I already said, with the publication of this book, any student of book 4 of the *Conics* has at his disposal a welcome and necessary addition to the preserved Greek text, ultimately stemming from another, and better, manuscript tradition than that available to Eutocius. Sadly, this is served in the framework of an unacceptable historical approach.

[10] But, remember A, the problematic al-Haytham ms. and the heavy role that it plays in Rashed's edition.

To grasp Apollonius' approach, Rashed's commentary is often couched

> in algebraic language, occasionally appealing [even] to projective concepts. These concepts are, of course, foreign to Apollonius, even though they find in the *Conics* one of their historical roots. One does not, therefore, leave the historical ground, when he distances himself deliberately from the geometrical language of Apollonius, in order to see a little farther and more profoundly. [vii]

This is a *non sequitur*, since not even Rashed can have it both ways, though he tries very hard. Thus, his brand of eating the cake and keeping it too, enables him to reach the conclusion that Apollonius, with his methods, managed to deal only with less than half of all possible cases of intersecting conics, something established by means of 'another mathematics than that of Apollonius' [viii]. This is by choice the model provided by projective geometry, a model permitting the unification of the study of conics and the considerable simplification of the analytical approach in order to save the complicated calculations required by the latter, though it too could have been used, were it not (unlike the projective model!?) too distant from 'the spirit of the fourth book' [viii].

> By introducing the points at infinity, one can interpret the parabola as the limit case between ellipse and hyperbola, its center and second focus being thrown to infinity. The asymptotic directions of the parabola and the hyperbola are those of chords passing through a point at infinity of the conic, and the asymptotes of the hyperbola are its tangents at infinity. [viii]

What, pray tell, has all this to do with Apollonius? *Nothing.* Strangely, and incomprehensibly, but in character, Rashed agrees:

> These concepts and the structure of the ontology underlying them are surely different than those of Apollonius. For him, in effect, as for all his followers until Desargues, the three conics were distinct and each was approached by its proper methods; parallel lines never meet and there are no points at infinity. Still, it is nevertheless the case that propositions XXX to XL of the third book and their converses in the

> fourth book are one of the historical origins of the writings of Desargues, Pascal, and de la Hire. [viii]

So what? And, by the way, what exactly is, for Rashed, the difference between 'geometrical algebra', which he rejects—

> Heath n'a pas hésité à lire les *Coniques* à la lumière de la géométrie algébrique [!]. Plus encore, il a justifié cette lecture par la fameuse doctrine de « l'algèbre géométrique des Grecs », déjà défendue par Zeuthen et Tannery, *et selon nous historiquement insoutenable* [Rashed *et alii* 2008, viii (my emphasis)]

—and 'algebraic geometry', which he embraces, though, at times, as in the just quoted passage, he seems to conflate and confuse them?

Now, book 4, part of the 'elementary' introduction to the *Conics* comprising books 1–4,[11] is, as we saw, about the relative positions and meetings of two conic sections with one another and with a circumference of a circle and about their common points, be they points of intersection or of tangency. Neither the Greek nor the Arabic text is systematic, though the latter is more so than the former. In his detailed description of the text that he has established and in his analysis, Rashed provides the missing systematization of the 53 propositions (57 in Greek), classifies them logically and mathematically, and analyzes them with all the means at his disposal, including, alas, mathematical concepts and techniques unavailable to Apollonius and his contemporaries. Thus, he speaks of poles and polars, sub-tangents, harmonic divisions and conjugate points, projective and affine transformations, and the like; and uses powerful analytical techniques 'to illuminate the structure' [61] of the Apollonian text,[12] as well as modern algebraic symbolism and techniques to unravel the subtext of the *Conics*. A superficial browsing through the pages of the book should convince any potential sceptic of the accuracy of this assessment.

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[11] Rashed argues convincingly, as far as it goes, for the 'elementariness' of book 4. However, his reasons should be supplemented by Fried's more sensitive and detailed discussion in the second part of his translation of the Greek text [2002, xxi–xxvii].

[12] In this case, the use of a fourth degree equation to study the intersection of two conics in general [61–62].

It is, of course, impossible (and not really necessary) to go with a fine comb through all the offensive mathematical analyses and historically unacceptable statements copiously adorning the book without writing another little book. I shall, therefore, limit myself as I approach the end of this review to a few typical examples drawn from both the book under review and Rashed *et alii* 2008 which introduces the whole enterprise.[13]

In his mathematical analysis of the propositions of the *Conics*, Rashed uses indiscriminately anachronistic concepts and does not hesitate to reformulate the genuine enunciations to fit his discussion [see 19, 25, 49, *et passim*]. Thus, speaking of drawing a tangent to a conic from an external point, he writes the necessary and sufficient condition algebraically and adds:

> The division (A, B, Δ, H) is harmonic. For the parabola, one has a limit case of the harmonic division, since the conjugate of the vertex A in relation to Δ and H is thrown on the diameter to infinite. [10]

His discussion of proposition 4.1, stretching over more than six pages [25–31] is purely analytic and ends in the following statement:

> This analytic commentary—foreign to Apollonius' mathematics—has the advantage of making comprehensible the choice of sections in this proposition. . . . [31]

The trouble is that Apollonius could not have benefitted from this so-called advantage! And yet, many of Rashed's discussions involve such analyses. Another case in point is his *Commentaire analytique des propositions 3 à 7* [38–44]. There are also occasions when Rashed contradicts himself. Here is an example:

> Tout indique dans cette proposition [IV.23] qu'Apollonius, sans avoir la notion du point double, compte le point de contact pour deux points d'intersection. [76]

How, pray tell, is this possible? And:

[13] Since this is, after all, a review of book 4, I shall not deal in detail with the many errors, some mere errors of fact, concerning Rashed's description of book 1 [2008, 49–56] *et passim.*

> On comprend qu'Apollonius... ne considérait pas encore [when proving proposition 4.32] le point de contact comme un point double. [92]

Clearly, then, the dramatic reverse change happened in the interval between the two propositions 4.23 and 4.32, when Apollonius shifted from counting the point of contact as two points (without ever having the concept of a double point!) to not yet considering it a double point! Miracles do happen, after all.

A few more gems: '... propositions 3.18 and 3.19 put into play the power of a point with respect to conics' [85].

> Apollonius' proof [of proposition 4.51] involves eight particular cases. It is, however possible to give a general demonstration by means of projective concepts. [98–99]

Indeed it is. The proof is given in the appendix entitled 'Théorie projective' [237–252]. There is also another appendix entitled 'Théorie affine' [252–294]. Both of these elegant appendices, incomprehensible to Apollonius, are the work of Christian Houzel. Finally:

> Two parabolas cannot, therefore, be tangent in two points, only in one. In such a case, they can be tangent at a point at infinity, and the line joining the two points is a common diameter of the two parabolas. [108]

No comment.

As intimated above, Rashed has shown, to my satisfaction, that the Arabic manuscript tradition of book 4, as defined by him, is more satisfactory than the Eutocian text preserved *inter alia* in Vaticanus graecus 206. Still, in his ardent desire to emphasize the superiority of the Arabic tradition over the Greek, he occasionally goes overboard, making inaccurate assertions. A few instances should suffice.

Speaking of book 4 in his general introduction to the whole project, while comparing the main Greek ms. of the *Conics*, Vaticanus graecus 206 (V), and one of the Arabic mss that he uses in his edition, Teheran, Milli 3597 (M), Rashed finds fault with the proof of 4.7 in V, which, according to him, unlike M lacks a crucial assumption, namely, 'that the secant be parallel to an asymptote' [2008,14]. This is wrong because 4.6, the assumptions of which are identical to those of 4.7, contains in its protasis the required hypothesis of parallelism.[14] It follows, then, that the next statement about the Greek *manuscript* tradition, based, as it is, on the cited wrong statement, is also wrong.

Comparing 4.20 in M to its corresponding Greek proposition in V, 4.19, Rashed asserts that, unlike the rigorous proof in Arabic, the Greek proof is faulty since it gives the conclusions 'sans avancer les justifications requises' [2008, 5]. Again, this is, strictly speaking, wrong. In this case too, the comparison between V and M is inaccurate. There is, *pace* Rashed, no harmonic division in V, and the assessment of 4.19 in V is not only anachronistic but also inexact. The proofs in question (4.6, 4.7, 4.19, 4.20) in V are real proofs, though less prolix than the corresponding proofs in Arabic. True, they are elliptical, occasionally only alluding to the reasons for the facts without spelling those reasons out explicitly; but, when read in context, they do precisely the job they are supposed to do.[15]

In sum, the great merit of this book, as of the project, of which it is a part, in its entirety, is the scholarly edition and translation of an Arabic text of the *Conics*. This is an important achievement.

In the *avant-propos* to the whole enterprise [2008], x], Rashed enumerates the goals that he set himself in bringing the project to fruition:

- the production of the *editio princeps* of books 1–4 in the Arabic version;
- a new edition of the Greek, Eutocian, version;
- a new edition of books 5–7;
- a French translation of all the books comprising the project; and
- finally, a historical/mathematical commentary on the whole.

[14] See the translation at Fried 2002, 8–9.

[15] See the analysis in Fried 2002, 15. Rashed himself remarks that 'il arrive souvent que l'on ait dans V une demonstration abrégée; c'est ce qu'on observe dans les propositions 4, 5, 8 et 20, entre bien d'autres' [19]. Rashed considers this uncharacteristic of Apollonius. I am not entirely sure. It seems to me rather that Apollonius abbreviated only simple proofs, giving the others in full; and that many of the propositions in book 4 belong to the 'simple' category. That is all.

Judging from the book reviewed here, and with all the reservations stated above, he has accomplished, though not perfectly, most but not all of of his goals. It seems to me that the historical commentary and its accompanying mathematics, as well as the basic assumptions under which they were conceived, are, well, *pardonnez l'expression*, deplorably egregious. For me and my cohorts, the 'guardians of the temple' as he refers to us disparagingly (and we are not as few as Rashed seems to think), this conclusion is, alas, unavoidable.[16]

BIBLIOGRAPHY

Fried, M. N. 2002. *Apollonius of Perga:* Conics *Book IV.* Santa Fe, NM.

Rashed, R. 2008. ed. *Apollonius de Perge,* Coniques. *Livre V. Commentaire historique et mathématique, édition et traduction du texte arabe.* Berlin/New York.

________ 2009a. *Apollonius de Perge,* Coniques. *Livre IV. Commentaire historique et mathématique, édition et traduction du texte arabe.* Berlin/New York.

________ 2009b. *Apollonius de Perge,* Coniques. *Livres VI et VII. Commentaire historique et mathématique, édition et traduction du texte arabe.* Berlin/New York.

Rashed, R.; Descorps-Foulquier, M.; and Federspiel, M. 2008. edd. *Apollonius de Perge,* Coniques. *Livre I. Commentaire historique et mathématique, édition et traduction du texte arabe* and 1.2: *Livre 1. Édition et traduction du texte grec.* Berlin/New York.

Taliaferro, R. C. 1952. *Apollonius of Perga: On Conic Sections.* Great Books of the Western World 11. Chicago.

Toomer, G. 1990. ed. *Apollonius,* Conics *Books V to VII: The Arabic Translation of the Lost Greek Original in the Version of the Banū Mūsā.* Heidelberg/Berlin/New York.

[16] I thank Fabio Acerbi, Alain Bernard, and others who extended to me generously their assistance.

Alexander Aphrodisiensis, De anima libri mantissa*: A New Edition of the Greek Text with Introduction and Commentary* by Robert W. Sharples

Peripatoi 21. Berlin/New York: Walter de Gruyter, 2008. Pp. viii + 269. ISBN 978–3–11–019644–3. Cloth €82.24, $136.00

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Alexander of Aphrodisias (*flor. ca* AD 200) was the last ancient commentator on Aristotle's works to write as an Aristotelian rather than as a Platonist. An index of his later stature is his being referred to by his successors in this line of philosophical business simply as 'The Commentator'. In addition to his extensive commentaries, we still posses a considerable number of other treatises. The mss containing his *On the Soul* present as its second book what are really a series of 25 short and loosely related pieces on psychological, physiological, and moral topics. The title *Mantissa*—literally 'Makeweight'—or *Supplement* is due to Freudenthal and was adopted by Ivo Bruns for his 1887 edition as part of the monumental Berlin edition of the Commentaria in Aristotelem Graeca.

Broadly speaking, these short tracts divide into lists of arguments against theses held by rival schools such as the Stoics on the one hand and a rather heterogeneous group of discussions of topics familiar from other treatises by Alexander, on the other. An example is provided by the second tract or section, the *On the Intellect*, which develops Alexander's most influential theory (identifying Aristotle's active intellect with God), and the treatment of the same subject in *On the Soul* [Bruns 1887, 80–92]. There are certain differences between the two accounts which are perhaps best explained—as argued persuasively by Paolo Accattino [2001]—by seeing *Mantissa* 2 as an earlier tract. In other cases, however, the relation seems to be the reverse, that is to say, what we have in the *Mantissa* represents a reworking of discussions on the same topics offered by Alexander elsewhere. It is not even certain that everything assembled in the

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Mantissa is by Alexander himself. In any case, the collection may be a reflection of the teaching of Alexander's school about which, in spite of his later renown, we know almost nothing. Still, the text offers valuable glimpses of the philosophical 'scene' of Alexander's day with its discussions between Peripatetics, Platonists, and Stoics.

This edition by the late R. W. Sharples is based on a comprehensive and meticulous study of the manuscript traditions (including the Arabic one) and represents a considerable improvement over that of Bruns, from which it diverges in 132 places. Bruns, moreover, had to work in a hurry and, as Sharples shows, his apparatus is riddled with mistakes. Indeed, Sharples' edition is an impressive work of scholarship of a kind that has become rare. The introduction and excellent comments included in this volume are based on those accompanying Sharples' translation [2004] in the well-known Aristotelian commentators series published by Duckworth. Readings followed in Sharples 2004 have here been changed in 15 places (listed in 28n67).

For the historian of science, the *Mantissa* is perhaps most interesting for its relatively extensive dealings with the physiology of vision [§§9–16]. In addition to Aristotelian mind-body theorizing (hylomorphism) there are also discussions of fundamental physical concepts such as body and the elements.

BIBLIOGRAPHY

Accattino, P. 2001. *Alessandro di Afrodisia.* De intellectu. Turin.

Bruns, I. 1887. ed. *Alexandri Aphrodisiensis praeter commentaria scripta minora: De anima liber cum mantissa.* Commentaria in Aristotelem Graeca 2.1. Berlin.

Sharples, R. W. 2004. *Alexander of Aphrodisias: Supplement to* On the Soul. London.

Mathematics in India by Kim Plofker

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Foreign interest in Indian mathematics has a long history, but it has often been accompanied by puzzlement and frustration. The reaction of the 11th century Muslim astronomer al-Bīrūnī contains many themes which have been echoed by later writers:

> ...even the so-called scientific theorems of the Hindus are in a state of utter confusion, devoid of any logical order, and in the last instance always mixed up with silly notions of the crowd, e.g., immense numbers, enormous spaces of time, and all kinds of religious dogmas.... Therefore it is a prevailing practice among the Hindus *jurare in verba magistri* [to appeal to the word of the master, i.e., to argue from authority]; and I can only compare their mathematical and astronomical literature, as far as I know it, to a mixture of pearl shells and sour dates, or of pearls and dung, or of costly crystals and common pebbles. Both kinds of things are equal in their eyes, since they cannot raise themselves to the methods of a strictly scientific deduction. [Sachau 1992, quoted by Plofker on page 262].

Costly crystals, once recognized as such, were quickly appropriated. Thus, Arabic mathematicians, and then Europeans, adopted the Indian decimal place-value system (the greatest achievement of the Hindus, according to Cajori's *A History of Mathematics* [1919, 88]) and their trigonometric tables (improvements of Ptolemy's chord tables). Once Europeans made direct contact with India, other crystals were found, including evidence that the Hindus knew the binomial theorem 'much better than Pascal' [283]. This last fact came to light too late to influence European mathematics; but it was further evidence of a sophisticated Indian mathematical culture in former times,

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and curiosity about the extent of this culture led to the translation of whole texts from Sanskrit into European languages.[1]

However, by the start of the 20th century, the opinions of western historians of mathematics differed little from those of al-Bīrūnī, 700 years earlier. Smith wrote that

> in the works of all these writers there is such a mixture of the brilliant and the commonplace as to make a judgement of their qualities depend largely upon the personal sympathies of the student. [1923, 152]

Cajori thought that the Indians had climbed to a great height in mathematics (although their actual route was no longer traceable). For example, as well as their decimal system, their algebra too was far advanced of anything that the Greeks had [1919, 83]. (Of course, the Greeks were the standard for what an ancient mathematical culture ought to be like.) Furthermore, the Indians had invented general methods for indeterminate analysis, where Diophantus had used only *ad hoc* methods [1919, 94–95]. But Indian geometry had no definitions, no postulates, no axioms, and no logical chain of reasoning [1919, 86]. In other words, it was not Euclid. Cajori also thought it unfortunate that Indian mathematics had always remained a servant of astronomy, as opposed to its apparently independent existence for the Greeks. Although Cajori appreciated that the Indian habit of expressing their mathematics in verse could aid the memory of someone who already understood the subject, he thought that such verse could only make mathematics obscure and unintelligible to everyone else [1919, 83]. Finally, he claimed [1919, 85] that after Bhāskara II in the 12th century, Indian 'scientific intelligence decreases continually', a sentiment echoed by Smith:

> Mathematics was already stagnant, and the European influence gave it no stimulus. India has always been content to take her time. [1923, 435]

Fifty years later, Boyer presents a similar picture, contrasting the Hindu's 'intuitive' approach with the 'stern rationalism of Greek geometry' [1968, 238]. Indeed, for Boyer, Āryabhaṭa has 'no feeling for logic or deductive methodology' and Brahmagupta, treating

[1] Colebrooke 1817 is an early example.

irrational roots as numbers, displays logical innocence rather than mathematical insight [1968, 232, 242].[2]

It is only quite recently that western writers have tried to understand Indian mathematics on its own terms, to appreciate the context which produced those costly crystals, and to provide the connecting narrative which turns episodes and highlights into a coherent story.

As far as context is concerned, writers such as Cajori and Smith were, to an extent, echoing the mathematical attitudes of their time. Since the mid 19th century, people like Dedekind and Cantor had worked to set mathematics on a firm foundation, independent of physical considerations. Indeed, their aim was to make arithmetic independent of geometry, and separate branches of mathematics soon came to prize their independence of one another. So a culture in which mathematics was so closely entwined with astronomy as Indian mathematics was must have seemed quite backward. Furthermore, a generation or two of mathematicians grew up, many of whom knew nothing of spherical trigonometry or astronomy; and so ancient mathematics embedded in such contexts became difficult to appreciate or even to recognize.

In more recent times, though, scholars have mined new sources, finding new mathematical pearls in non-mathematical rubbish dumps (not just astronomical texts, but also texts dealing with sacred ritual, astrology, or metrical rules for verse). In addition, historians generally have turned away from writing history as a triumphal victor's narrative and have become more open to presenting other participants' points of view. Thus, recent general histories such as Katz' *A History of Mathematics* [1993], have shown more interest both in the mathematics of other cultures and in the problems and contexts which gave rise to mathematics. Like Boyer [1968], Katz devotes a single chapter to the mathematics of India and China; but his text also gives some of the astronomical background needed to appreciate not just Indian, but also Greek and Islamic, trigonometry. Moreover, he makes space for another, more recently discovered, pearl: Mādhava's 14th century discovery of infinite series for the sine, cosine, and arctangent functions, over 200 years before Gregory and

[2] Presumably because Brahmagupta failed to observe the distinction between number and magnitude which, according to Cajori [1919, 93], had retarded the progress of Greek mathematics for 100s of years.

Newton—a pearl which calls into question Cajori and Smith's judgement of a stagnant or declining Indian mathematics after the time of Bhāskara II.

Viewing Indian mathematics on its own terms and providing some narrative structure is probably outside the scope of general texts such as Katz 1993. Their own overall narrative, how global mathematics got to where it is today, needs to concentrate on the main stream of history—Katz devotes three whole chapters to aspects of Greek mathematics, for example—and this probably precludes spending too much time on the smaller streams of other narratives. So these tasks have fallen to other writers.

The fine detail of Indian mathematics continues to be presented through the publication of primary sources and commentaries. One recent example is Keller's *Expounding the Mathematical Seed* [2006], which includes a translation of both Āryabhaṭa's chapter on mathematics and the commentary on this by Bhāskara I.[3] Another is Plofker's own chapter in Katz' sourcebook [2007], which contains excerpts from Indian texts spanning perhaps 2000 years along with brief historical comments and even briefer mathematical explanations. But it is still hard to find an up-to-date, coherent narrative for the history of Indian mathematics; and it is this gap which Plofker tries to fill with the book under review.

Chapter 1 is a short introduction explaining the book's aims, giving a brief history of the Indian subcontinent, and describing the role of Sanskrit, the language in which most of India's mathematical texts are written.

Chapter 2 examines mathematical thought in the earliest Sanskrit texts, the Vedas. These texts are thought to have reached canonical status by about the middle of the first millennium BC. Although the content of the texts is essentially religious, consisting of prayers and descriptions of ritual, they refer to what we now think of as mathematical ideas such as a decimal system of numbering (although not yet a place value system) and factorizing integers. Ritual geometry described in the *Śulba-sūtras* used cords or ropes to solve problems associated with altar shapes and orientation, and included ways of constructing right angles or of transforming rectangles into

[3] [Ed.] See the review in *Aestimatio* by S. R. Sarma [2006].

squares or circles. Plofker discusses attempts to find quantitative ideas in Vedic references to astronomical phenomena. This is one of many controversial topics covered in the book and in each case Plofker gives a brief account of what she calls ‘the mainstream narrative’, admitting where direct supporting evidence might be lacking (very few documents are more than 400 years old) and mentioning alternative theories which are not quite so mainstream. She is invariably polite towards opposing theories and gives references for those seeking to explore those theories.

Chapter 3 looks for traces of mathematical thinking during the Early Classical Period, which extends from about the middle of the first millennium BC through to the first few centuries AD. It seems that the decimal place value system was adopted during this period, but its origins are obscure. Less obscure, perhaps, are the origins of Indian trigonometry. The incursions of Alexander the Great brought at least northern India into contact with Greek culture, so it may not be too surprising to find Sanskrit verses from this period listing properties of what we now call the sine function. Even here, though, Plofker points out [52] that there is no hard evidence of transmission; and so we can say only that Indian astronomers appear to have been the first to use sines rather than Ptolemy’s chords. Mathematical ideas pop up in surprising places, and Plofker shows in section 3.3 how an analysis of metrical structure in poetry can lead to a variation of binary representations.

As already mentioned, astronomy and mathematics are closely interlinked in Sanskrit texts. Chapter 4 provides the necessary background for appreciating these links. Here Plofker explains the basics of geocentric astronomy. With the help of a dozen or so diagrams, she elucidates a series of Sanskrit verses describing how sines can be used to calculate various astronomical parameters. Of particular interest to mathematicians here is the way in which Indians used interpolation techniques to calculate sine values between the standard values. Ptolemy tabulated his chord values at steps of $\frac{1}{2}^\circ$ (360 values between 0° and 180°) but Indian mathematicians recorded just 24 values in steps of 3.75°. This meant that key values could be memorized in verse form. Intermediate values could be then calculated using interpolation techniques which were also remembered in verse form. Of somewhat wider interest perhaps are the uncertain relationships between observations, numerical parameters, and geometric models in medieval Indian mathematical astronomy [120]. As Plofker says, there is still much work to be done here, but the apparent lack of commitment in Indian texts to a particular geometric model for astronomical phenomena seems to place them more in the Babylonian tradition than the Greek. Perhaps this is another situation where the Indians' 'logical innocence' allowed them to experiment in ways which would not have occurred to their European counterparts.

Chapters 5 and 6 deal with the medieval period and the writings of (among others) Āryabhaṭa, Bhāskara I, Mahāvīra, and Bhāskara II. This means that these chapters have a substantial overlap with Plofker's chapter in Katz 2007.[4] Plofker acknowledges this and says that the two accounts are meant to complement one another. As the title of Katz' book suggests, its main purpose is to provide readers with original sources translated into English. There is just enough history and commentary to help readers make sense of these sources. On the other hand, the present book's focus is on building a coherent narrative; so there is significantly more historical background and more commentary, not just on the mathematical meaning of the texts but also on their place in the grand narrative. Knowing that a good proportion of the sources were available in another book, Plofker often simply summarizes the content of a group of verses; and in these cases, I found that it helped to have both books open at the same time, so that the combined texts provided a broad selection of source material and a reasonably full commentary. Space constraints mean that there are still many omissions, but Plofker always indicates where the reader can find a fuller treatment of individual works.

The content of Chapters 5 and 6 is a fascinating portrayal of many aspects of medieval Indian mathematics. We see the emergence of mathematics, if not as a separate discipline, at least as separate chapters on calculation [123] and what we might call algebra [140]. The content of the earliest text devoted solely to mathematics, Mahāvīra's ninth century *Gaṇita-sāra-saṅgraha*, is surprisingly close to medieval European texts such as Fibonacci's *Liber abbaci*, although it also includes topics of particular interest in Sanskrit culture such as the number of poetic meters with a fixed number of syllables [168]. A theme recurring in these chapters is the development of ideas which we might see as being related to calculus, starting

[4] [Ed.] See the review of this book in *Aestimatio* by Clemency Montelle [2007].

from an interest in division by zero [151, 163] that may be useful in astronomy [185, 197], and culminating in Bhāskara's calculation of the area of a sphere [199] by dividing it into regions rather like the segments of an orange (although his actual comparison is with an Indian gooseberry).

The contrasting roles of text and commentary have only recently attracted attention in western mathematics [see Netz 2004],[5] but the verse text and prose commentary format of Indian mathematics formalized this distinction at an early stage. Plofker discusses several examples, including situations where the commentator is the same person as the author and even refers to himself in the third person [190]! Apart from elucidating the mathematical text, commentators also offer higher level views on topics such as, why there are so many rules [190], how it feels to have a clear demonstration, and what makes a good mathematician [198]. The essentially oral culture of dense verse is also fascinating. Are the verses deliberately obscure to test the student's competence [308] or are they sources of fruitful ambiguity [142, 214]? Plofker offers one example [183] of what might be called playful ambiguity from Bhāskara's *Līlāvatī*:

> Those who keep in their throats the *Līlāvatī* having entirely accurate [arithmetic] procedures, elegant sentences, [whose] sections are adorned with excellent [rules for] reduction of fractions and multiplication and squaring [etc.] ...
>
> (Alternative translation:) Those who clasp to their necks the beautiful one completely perfect in behavior, enticing through the delight of [her] beautiful speech, [whose] limbs are adorned by the host of good qualities [associated with] good birth ... attain ever-increasing happiness and success.

Chapter 7 looks at the work of the school of Mādhava in Kerala from about the 14th to the 17th centuries. This focuses mainly on the series expansions mentioned earlier, with a careful discussion both of what the verses attributed to Mādhava actually say, and of the associated commentaries produced by this same school. Explanations and rationales were highly valued in this school, to the extent that some were even rendered in verse [247].

[5] [Ed.] See the review article in *Aestimatio* by Fabio Acerbi [2005].

In Chapter 8, Plofker discusses Indian interactions with the Islamic world and the struggles that both cultures had in understanding one another, as illustrated in the quotation of al-Bīrūnī at the start of this review [45]. Of particular interest is the question of why Indian mathematics adopted some ideas—for example, after the 12th century detailed tables came to be preferred to the shorter Sanskrit tables which had been memorized in verse form [274]—but not others such as axiomatic deductive geometry [277]. In this connection, I was a bit surprised that there was only limited discussion anywhere in Plofker's book of links with Chinese mathematics, especially as there was mention of Chinese pilgrims returning with Sanskrit texts [181]; but this may be another topic where there is no documentary evidence.

Chapter 9 concludes the main body of the book with a discussion of developments in the modern period, including further interest in clear demonstrations [293] and an account of direct relations with European culture, once again characterized by both interest and mutual misunderstanding.

Two useful appendices introduce the reader to relevant features of Sanskrit language and literature, and list biographical data on 40 or so Indian mathematicians. There is a comprehensive bibliography (over 20 pages) which, along with Plofker's helpful footnotes, should enable the interested reader to look into Indian mathematics in more breadth or depth.

The book is well written and easy to read. There is a good balance of commentary and technical detail, so that a scientifically literate reader can appreciate the overall picture and yet the mathematical reader can still confirm the steps of a representative sample of Indian calculations or explanations. The overall theme of seeing Indian mathematics develop in its own context is well handled, with good discussions of how Indian society, culture, or astronomy are relevant to each mathematical development. I spotted only a couple of minor misprints; and the only irritating feature was the unusual system of bibliographic references which is based on abbreviated names and dates, and occasionally puts names out of strict alphabetic order (because the author's initial took precedence over the next letter of their surname).

There is still much work to be done: there are manuscripts still unread, and paths of development and routes of transmission not understood. But Plofker's book finally offers us, at least in outline, an up-to-date and coherent narrative for the history of mathematics in India.

BIBLIOGRAPHY

Acerbi, F. 2005. 'Archimedes and the Angel: Paths from Problems to Equations'. *Aestimatio* 2:168–225.

Boyer, C. B. 1968. *A History of Mathematics.* New York.

Cajori, F. 1919. *A History of Mathematics.* 2nd edn. New York.

Colebrooke, H. T. 1817. *Algebra, with Arithmetic and Mensuration, from the Sanscrit of Brahmegupta and Bhascara.* London.

Katz, V. J. 1993. *A History of Mathematics.* New York.

——— 2007. *The Mathematics of Egypt, Mesopotamia, China, India and Islam: A Sourcebook.* Princeton.

Keller, A. 2006. *Expounding the Mathematical Seed.* 2 vols. Basel.

Montelle, C. 2007. rev. Katz 2007. *Aestimatio* 4:179–191.

Netz, R. 2004. *The Transformation of Mathematics in the Early Mediterranean World: From Problems to Equations.* Cambridge.

Sachau, E. 1992. *Alberuni's India.* 2 vols. New Delhi.

Sarma, S. R. 2006. rev. Keller 2006. *Aestimatio* 3:143–147.

Smith, D. E. 1923. *History of Mathematics: 1. General Survey of the History of Elementary Mathematics.* New York.

Omar al-Khayyam, Algebra wa al-Muqabala*: An Essay by the Uniquely Wise ᶜAbel Fath Omar bin al-Khayyam on Algebra and Equations* translated by Roshdi Khalil

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The volume under review consists of an English translation of Omar Khayyam's 12th-century classic, *Algebra*, which is devoted to the enumeration of all types of cubic equations and the solution of such types as have a positive root. Despite what one might think from Omar's title, 'Algebra', his methods depend heavily on three classic geometric works: the *Elements* and the *Data* of Euclid (the latter a treatise on given magnitudes) and the *Conics* of Apollonius. (The latter two, especially, are not easy going for even mathematically trained readers, ancient or modern.) Accordingly, Omar's solutions to cubic equations are expressed as line segments determined by the intersection of conic sections. Although Omar explicitly states that he tried to find numeric solutions for such equations (like the ones he knew for the roots of quadratic equations), he admits frankly that he was unable to do so and expresses the hope that a later mathematician will succeed where he has failed. (Jerome Cardan realized this hope with the publication of his *Ars Magna* in 1545.)

Despite the importance of its contents, Omar's *Algebra* was not one of the many Arabic works that contributed so importantly to the European Renaissance. Indeed, it was only in 1742, with Gerard Meerman's *Specimen calculi fluxionalis*, that the attention of Western scholars was drawn to a copy of Omar's treatise in the Warner collection in Leiden. The eminent historian of mathematics F. Woepcke first published the Arabic text with a French translation of the *Algebra* in 1851; but it was only in 1931 that Dr. Daoud S. Kasir published an English translation, one based on an Arabic manuscript of the work in the possession of Professor D. E. Smith of Columbia

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University. Kasir made considerable use of previous scholarly studies relevant to the topic, especially of Woepcke's French translation and the valuable mathematical and historical notes that Woepcke included in his work. Kasir does not present an Arabic text but remarks [1931, 9] that the text of the manuscript which he used is 'substantially identical' to that of MS 14 in the Warner collection in Leiden. In 1950, H. J. J. Winter and W. ᶜArafat published another English translation of Omar's work and a Russian translation was published in Moscow in 1961.

In 1981, there appeared an edition of the text based on all known manuscripts of Khayyam's work with a French translation by A. Djebbar and R. Rashed, which was republished in *Al-Khayyam mathématicien* in 1999. More recently, an English version of this has appeared [see Rashed and Vahabzadeh 2003].[1]

We now have, therefore, four English translations of Omar Khayyam's *Algebra*. The work under review, the one of these four most recently published, is difficult to relate to previous publications since Khalil says only that he translated a copy of the book that 'is in Aleppo'. Djebbar and Rashed [1999] make no reference to a copy in Aleppo; so one assumes that Khalil got a microfilm of a manuscript copy of the book from the archives of the Institute for the History of Arabic Science in Aleppo.

I shall now compare a few passages of the version under review with those in Kasir's book. (I have used Djebbar and Rashed's Arabic text as a check on both.) First, from the beginning of the work:

Khalil

> One of the educational notions needed in the branch of philosophy known as mathematics is the art of algebra and equations, invented to determine unknown numbers and areas. [1]

Djebbar and Rashed[2]

> One of the mathematical notions that one needs in the part of knowledge known as mathematics is the art of algebra and *al-muqābala*, intended to determine numerical and geometrical unknowns. [1999, 11]

---

[1] I have not seen the editions of 1999 or 2003, and have relied on the edition of 1981.

[2] In quoting Djebbar and Rashed [1999], I have translated their French.

Kasir

> One of the branches of knowledge needed in that division of philosophy known as mathematics is the science of completion and reduction, which aims at the determination of numerical and geometrical unknowns. [1931, 43]

Khalil has taken the modern usage of *ta^{c}limiyya*, namely, 'educational'; but the sense of that word in medieval mathematical texts was, as Kasir renders it, 'mathematical'. On the other hand, Khalil's translation of the last part reflects the Arabic text more closely, since the Arabic text plainly says'unknowns relating to areas', though Omar probably intended to include other types of unknown geometrical magnitudes such as lines and volumes.

From the solution of the first species of trinomial cubic equations ('cube plus some sides are equal to a number'), I have underlined some of the main differences between Kasir's and Khalil's translations [1931, 77–78, and 18, respectively].

Kasir begins, 'Let the line AB be the side of a square equal to the given number of roots'. Khalil begins, 'We set AB to be the side of a square <u>whose length</u> equals the given number of the roots'. Kasir explains in a footnote that it is the area of the square on AB that is equal to the given number of the roots, whereas Khalil's addition of the words 'whose length' misleads the reader into thinking that it is the length of AB, not its square, that is equal to the number of roots. (Khalil has also dropped the 'a' from the diagram.)

Kasir continues,

> Construct a solid whose base is equal to the square on AB, equal in volume to the given number. The construction has been shown previously. Let BC be the height of the solid.

Khalil's version renders this as

> We construct a <u>parallelepiped</u> with a square base whose side is ab, and its height is bc, which we assume is equal to the given number. The construction is similar to what we have done before. We make bc perpendicular to ab.

Kasir brings in a reference to 'volume' and Khalil brings in one to 'parallelepiped', both of which are doubtless helpful to the modern reader, though each is an addition to the Arabic text which simply says that the solid is to be equal to the given number.

In their translations of Khayyam's solution of his 'first type' of cubic equation, Kasir and Khalil more or less agree on their translations of Omar's explanation of the phrase 'solid number', although Khalil's decision to call it 'numerical parallelepiped' loses the clear reference of Khayyam's terminology to that of book 6 of Euclid's *Elements*.

Kasir then translates Khayyam's construction of a circle and a parabola by

> Produce AB to Z and construct a parabola whose vertex is the point B, axis BZ, and parameter AB. Describe on BC a semicircle. It necessarily intersects the conic. Let the point of intersection be D.

Khalil renders the same passage as

> We extend *ab* to *z*, then construct the parabola *mbd*, with vertex *b*, axis *bz*, and its perpendicular side *ab*, so the parabola *mbd* is known, as we have shown previously, and it is tangent to the line *bc*. We construct a semicircle on *bc* which must intersect the (conic) section, say, at *d*.

Khalil's translation correctly reflects the medieval terminology 'perpendicular side' for the modern term 'parameter' (though the modern reader might appreciate an explanation), as well as Khayyam's reference to intersecting 'the section' (not the 'conic') and his decision not to name the parabola until he has constructed it.

Khayyam concludes his construction by dropping a perpendicular DZ from the point D onto the axis of the parabola at Z. Kasir refers to DZ as an 'ordinate', whereas Khalil's literal translation of the Arabic as 'one of the lines of order' may well leave some of his readers puzzled.

A number of nuances in the Arabic text are lost in this translation. For example, in the introductory part of his work [2], Khalil translates an admittedly difficult passage as:

> By quantities we mean continuous quantities, and they are of four types: line, surface, solid and time.... Some (researchers) consider place to be a continuous quantity of the same type as surface. This is not the case, as one can verify. The truth is: space is a surface with conditions, whose verification is not part of our goal in this book.

One may compare this with the rendering of the same passage in Djebbar and Rashed:

> By magnitudes I mean continuous quantity, and they are four: line, surface, body and time.... Some people think that place is a species subdividing surface under the genus of the continuous, but exact acquaintance overthrows this opinion. We will thus correct: Place is a surface in a certain state, whose exact knowledge does not stem from the subject occupying us here. [1999, 2]

The word 'magnitudes' is the standard translation of the Arabic plural 'maqādīr', and using the same term, 'quantities', for conceptually different words blurs an important distinction between the broader term 'quantity' (which includes the discrete quantities, numbers) and 'magnitude' (which is limited to continuous quantity). Then, near the end of this passage, Khalil translates

> Euclid proved certain equations to find the required rational measurable quantities in chapter five of his book (the *Elements*)... [3]

The Arabic of this passage is, admittedly, somewhat loose; but one acquainted with the history of ancient mathematics will recognize immediately that Omar is simply referring to the fact that Euclid proved certain propositions relating to proportions of magnitudes in his fifth book. It has nothing to do with equations or rational measurable quantities.

A welcome feature of this edition is its inclusion [44–57] of a short treatise by Omar on solving a problem of dividing the arc of a quadrant of a circle, AB, with center H and radius HB, into two parts at a point Z so that when a perpendicular is dropped from Z onto the radius HB the result is that $BH{:}ZM :: HM{:}MB$. That this short treatise, highly relevant to Khayyam's work on cubics, is not in Kasir's edition, which is widely available in college and university libraries, is unfortunate; so its inclusion in the book under review was a good decision. Unfortunately, there are places here, too, where the translation is either loose or inaccurate. For example, Khalil writes of dividing the arc into 'two halves' (though the text clearly says 'two parts') and he refers to the circle's 'diagonal' BD, rather than the text's 'diameter'BD. Later in the demonstration Khalil refers to ‘reflecting figure eight of the second article of the book of sections’. An accurate translation (such as that of Djebbar and Rashed) would be ‘the converse of theorem eight of the second book of the Conics <of Apollonius>’. Another unfortunate mistake in translation is ‘. . . this triangle cannot be an equilateral triangle’ [47], where the text reads ‘this triangle cannot be isosceles’.

On the whole, however, the translation is competent and the book serves the useful purpose of making available to English readers the algebraic work of one of the great figures in the history of mathematics in a short and inexpensive version. If certain nuances are lost in the translation, it is still the case that one reading the book will understand that Omar solved a difficult problem and will come away with a good sense of how, in terms of the mathematics of his own time, he did it.

BIBLIOGRAPHY

Djebbar, A. and Rashed, R. 1999. *Al-Khayyam mathématicien.* Paris.

Kasir, D. S. 1931. *The Algebra of Omar Khayyam.* New York.

Rashed, R. and Vahabzadeh, B. 2003. *Omar Khayyam, the Mathematician.* Persian Heritage Series 40. New York.

Winter, H. J. J. and ^{c}Arafat, W. 1950. ‘The Algebra of ^{c}Umar Khayyam’. *Journal of the Royal Asiatic Society of Bengal* 16:27–70.

Woepcke, F. 1851. *L’algebre d’Omar Khayyam.* Paris.

Reenacting Galileo's Experiments: Rediscovering the Techniques of Seventeenth-Century Science by Paolo Palmieri

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Before I start with my review proper, it is worth mentioning a particular feature of the book under review. Paolo Palmieri's *Reenacting Galileo's Experiments* is more than a monograph on Galileo's science (*scienza*) of motion: in addition to the text of this book, readers are invited to consult the corresponding website of the Experimental History and Philosophy of Science Research Unit at the University of Pittsburgh (www.exphps.org). This website contains a series of videos illustrating some recently performed reconstructions of Galileo's experiments and a 68 page-long report of the team's reenactment of them.[1] These moving images have the potential to be worth more than a thousand printed illustrations.

The structure of the book is straightforward. After a short introduction (3 pages) and the three main chapters—to wit:

Chapter 1. Galileo and Experiment (16 pages),
2. The Puzzle-Box (78 pages), and
3. New Science (92 pages)

—a general conclusion (4 pages) follows. There are three important appendices to the book:

[1] In his review of this book in *Isis*, Joseph C. Pitt [2009] does not mention this unique feature of Palmieri's monograph. Nor does he mention the characteristic 'robustness' [see below] involved in Palmieri's reenactments. The report, which contains the links to some 30 videos, can be downloaded from www.exphps.org/pdfs/projects/Galileo's%20pendulum%20experiments.pdf.

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Appendix 1. A discussion of the computer models that Palmieri used in his investigations (25 pages),

2. The reconstructions of Galileo's experiments (35 pages), and
3. Palmieri's translations of some crucial Galilean fragments on pendulums which are based on the original texts in Antonio Favaro's *Edizione Nazionale* (13 pages).

Chapters 2 and 3 contain virtual Galilean dialogues which are based on the writings of Galileo and his contemporaries.[2] The first dialogue, occurring in chapter 2, is between Galileo and his collaborator Benedetto Castelli; the second and third, in chapter 3, between Galileo and his pupil Vincenzo Viviani and between Viviani and Evangelista Torricelli, respectively.

The aim of this monograph, which becomes clearer as one works through it, is to study 'Galileo's innovative methodology', that is, his 'experimental philosophy' [1]. Additionally, Palmieri wishes to show that 'there is much to learn from reenacting the experimental practices of scientists (typically of a past period)' [3: cf. 193–194] and that

> [w]hile obviously fundamental, textual hermeneutics need not ... be exclusive, especially when experimentation is invoked in scientific texts of the past. [3]

In what follows, I shall not survey Palmieri's monograph *a capite ad calcem*; rather I shall highlight what I consider to be the merits and possible shortcomings of the book under review.

Chapter 1 serves as a general stage-setting for the problem of Galilean experimentation. Central to *Reenacting Galileo's Experiments* is the so-called 'matching problem', that is, whether Galileo's published accounts are in agreement with his actual experiments. For instance, Galileo's claim about the isochronism of the pendulum has puzzled modern interpreters. Hitherto, Palmieri writes, solutions to the matching problem 'rest on arbitrary, anachronistic assumptions about what constitutes good or bad empirical evidence for a theoretical claim'; and, furthermore, they are understood solely from Galileo's published accounts [8]. In order to remedy this situation,

[2] A genre which Stillman Drake has tried before [1981].

Palmieri has reenacted Galileo's experiments using computer simulations, which are more robust with respect to the repeatability and consistency of outcome over a wider range of parameters that control the experiments. 'Since Galileo does not tell us much about the setup of his experiments', Palmieri notes,

> we face formidable indeterminacies, which may affect our interpretation of the texts to a point that we risk failing to see what Galileo might have seen, and vice versa. To resolve the indeterminacies we need to make the experiments robust over as wide a range of parameters as possible. [9: cf. 195–196, 238]

This I conceive as a major advance in comparison to previous attempts at reenacting Galileo's experiments.

From 1602 and onwards, Galileo claimed—erroneously, as we now know—that the motions of a simple pendulum were isochronous, although he admitted that he had no mechanical proof in support of it.[3] While Ronald Naylor has speculated that Galileo must have relied on 'a wider range of evidence than he indicated in the *Discorsi*' [1974, 23], others have claimed that Galileo 'published some things [i.e., the isochronisms of the circular pendulum] which he knew to be false' [Hill 1994, 513] and that Galileo's claim about the isochronism of the pendulum was 'based more on mathematical deduction than on experimental observation' [MacLachlan 1976, 173]. In appendix 2, Palmieri shows that light pendulums set to swing from modest angles can indeed be isochronous; however, by using heavier pendulums or greater angles, the isochronism of the simple pendulum breaks down—a phenomenon, Palmieri says, Galileo could not have failed to notice himself [37ff]. Galileo's epistemic rule that experience does not teach the causes of things neutralized the problem of discrepancy from isochrony [244].

Palmieri distinguishes between three important stages in the development of Galileo's experimental philosophy, which are fleshed out

[3] See Galileo's letter to Guidobaldo del Monte (Guido Ubaldo dal Monte) on 29 November 1602 [Favaro 1890–1909, 10.97–100; translated on pages 258–260]. See pages 101–122 for Palmieri's discussion of corresponding material from the *Discorsi*.

and contextualized in the next two chapters. According to Palmieri [10–17], these stages are:

Stage 1. Galileo stuck to the epistemic rule that the causes of phenomena are not taught by experience and that they can only be established *via* some form of reasoning.

2. Here Galileo emphasized that the causes of phenomena may be investigated on the basis of patterns of phenomena generated by the variation of an artifact's control parameters—at this stage, causal inference is still guided by reason.
3. Finally, Galileo bracketed, but did not fundamentally reject, the search for causes. Correspondingly, he came to distinguish between causality and inference so that the 'inferentially engageability' of mathematics was separated from causal knowledge.[4]

In chapter 2, Palmieri provides adequate contextualization of the conceptual difficulties that Galileo had to overcome, by surveying the work of Girolamo Borri and Giacomo Zabarella and, more particularly, their attempts at reconciling internal and external causes of motion [24–43]. Thereafter, it is shown convincingly that Galileo's early work on (the causes of) local motion resulted from a generalization of Archimedes' study of floating bodies, that is, by conceiving all local motions as acting along the lines of a balance [43–62]. At the same time, Galileo conceived of mechanical causes as acting in accordance with, rather than in opposition to, nature. Although his early work on local motion was not without tensions [see 52, 62, 79], mathematical deduction and causal inference fitted hand in glove in Galileo's early conception of the study of local motion.

Chapter 3 deals with Galileo's *Discorsi* project, in which he 'bracketed' the search for causes, specifically, the cause of acceleration [125, 140]. Correspondingly, Palmieri shows that the 'Second Day' in Galileo's *Discorsi*, which addresses the resistance to fracture, is based on an empirical rather than a causal principle, that is, the principle of the equilibrium of the balance of different arms [139–150]. Its principles are, therefore, on a par with the non-causal principles introduced in 'Day Three' and 'Day Four', which address local motion and the motions of projectiles respectively.

[4] Palmieri warns that these stages are not distinguishable chronologically with precision [11].

Surely, the most intriguing and exciting material surveyed in this monograph is Palmieri's reenactment of several of Galileo's experiments. I recommend that readers study this material in conjunction with the material provided on the website of Pittsburgh's Experimental History and Philosophy of Science Research Unit. In the remainder of this review I shall, however, point to some possible worries for Palmieri's assessment of Galileo's experimental philosophy. More precisely, in what follows, I seek to evaluate his claims about Galilean causation. The idea that Galileo increasingly played down the significance of causal explanation in his later work has been suggested before by Stillman Drake [1981, xxviii] and Pietro Redondi [1998, 185], for example. I shall divide my discussion between two topics: Palmieri's assessment of the role of causation in Galileo's *scienza* and his claims regarding demonstrative *regressus*. In doing so, I allow myself the freedom to refer to some of my own work.

I am sympathetic to Palmieri's approach to Galilean causation. He starts from the premise that instead of focusing on the past traditions from which Galileo's terminology seems to be derived, we should pay more attention to the notion of causation as embedded in Galileo's scientific practice.[5] In this context, Palmieri points to the significance of using artifacts that allow him to vary parameters in a more controlled way [10, 87]. This seems to be related to what I have labeled Galileo's interventionist notion of causation [Ducheyne 2006, 443–444, 452, 458], which first emerged explicitly in his *Discourse on Floating Bodies* (1612). The defining characteristic of causal interventionism is that in order to establish whether A is a cause of B, we need to establish whether deliberate hands-on variations in A result in changes in B. Unfortunately, Palmieri does not go into the details of Galileo's causal interventionism in the *Discourse on Floating Bodies*, which nevertheless contains vital clues on the matter [see Drake 1981, xxvii, 26, 74; Favaro 1890–1909, 4.27, 4.64, 4.89]. Although it is certainly correct that in some parts of the *Discorsi* (1638) Galileo set aside the search for a causal explanation of acceleration, this does not imply that he dispensed with causal explanations entirely. In 'Day Three' of the *Discorsi*, Galileo's spokesman, Salviati, states that

[5] Compare with my own view on the matter in Ducheyne 2006, 448.

> at present it is the purpose of our Author merely to investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be). [Crew and de Salvo 1954, 167; Favaro 1890–1909, 8.202: cf. Drake 2001, 272; Favaro 1890–1909, 7.260–261]

However, even in the *Discorsi*, his most a-causal work, Galileo introduced and speculated on the causes of certain phenomena. For instance, in 'Day One' of the *Discorsi*, Salviati notes:

> I know for a certainty, that it [i.e., the cause of the cohesion of water] is not owing to any internal tenacity acting between the particles of water; whence it must follow that the cause of this effect is external [*onde resta necessario che la cagione di cotal effetto risegga fuori*]. [Crew and de Salvo 1954, 70; Favaro 1890–1909, 8.115]

Similarly, Salviati says that

> the variation of speed observed in bodies of different specific gravities is not caused by the difference of specific gravity but depends upon external circumstances [*non ne sia altramente causa la diversi gravità, ma che ciò dependa de accidenti esteriori*] and, in particular, upon the resistance of the medium, so that if this is removed all bodies would fall with the same velocity; and this result I deduce mainly from the fact you have just admitted and which is very true, namely, that, in the case of bodies which differ widely in weight, their velocities differ more and more as the spaces traversed increase, something which would not occur if the effect depended upon differences of specific gravity. [Crew and de Salvo 1954, 73; Favaro 1890–1909, 8.118]

Moreover, in 'Day Four' of Galileo's *Dialogo* (1632) causal explanations play a pivotal role [see Ducheyne 2006, 453–459]. The tides were to Galileo's mind a physical proof that the Earth moved. Salviati stresses that in dealing with questions like these, 'a knowledge of the effects is what leads to an investigation and discovery of the causes' [Drake 2001, 484]. Such an investigation may lead to the true, primary, and universal causes of the effects we observe [Drake 2001, 485, 533; Favaro 1890–1909, 7.444, 7.485]. Galileo constructed a mechanical model—alas, the details have been lost—on the basis of which he sought to demonstrate that the tides are caused by a combination of the Earth's annual motion from west to east and its diurnal motion from west to east. The resulting mixed motion is 'the most fundamental and effective cause of the tides, without which they would not take place' [Drake 2001, 497; Favaro 1890–1909, 7.454]. In renouncing competing explanations of the tides, Salviati formulates a positive criterion for a true cause (*vera causa*) of the tides, namely, artificial reproduction:

> But I believe that you have not any stronger indication that the true cause of the tides is one of those incomprehensibles than the mere fact that among all things so far adduced as *verae causae* there is not one which we can duplicate for ourselves by means of appropriate artificial device. For neither by the light of the moon or sun, nor by temperate heat, nor by differences of depth can we ever make water contained in a motionless vessel run to and fro, or rise and fall in but a single place. But if, by simply setting the vessel in motion, I can represent for you without any artifice at all precisely those changes which are perceived in the waters of the sea, why should you reject this cause and take refuge in miracles? [Drake 2001, 489; Favaro 1890–1909, 7.447]

Galileo later adds that

> if it is true that one effect can have only one basic cause, and if between the cause and the effect there is a fixed and constant connection, then whenever a fixed and constant alteration is seen in the effect, there must be a fixed and constant variation in the cause.
>
> che se è vero che di un effetto una sola sia la cagion primaria, e che tra la causa e l'effetto sia una ferma e costante connessione, necessaria cosa è che qualunque volta si vegga alterazione ferma e costante nell'effetto, ferma e costante alterazioni sia nella causa. [Drake 2001, 517; Favaro 1890–1909, 7.471]

The material briefly surveyed in this passage seems to suggest, *pace* Palmieri, that, in his later period, Galileo did not exclusively endorse a-causal principles. What this reveals, according to my own judgment, is that the late Galileo relied on causal as well as a-causal principles, depending on the specifics of the context at hand.

When discussing the young Galileo's decision to use the *methodus resolutiva* in order to establish the true cause of acceleration, Palmieri notes:

> The resolutive method has nothing to do with the real process of discovery of the cause of acceleration. So why does Galileo says that he is going to use the method, here and now in the notes, in order to investigate the true cause, not the *unknown* cause? Because this is the first time he has gotten round to putting ideas in writing. This is especially true because the resolutive method starts from the 'given', the objective of inquiry. The objective of inquiry is assumed to be 'given', to be known. We assume that we can grasp it. Take the example of Greek mathematics. If Greek mathematicians eventually publish an analysis, it is because the resolutive method creates suspense in the reader, the illusion that discovery unfolds before the reader's eyes. [72; italics in original]

This quotation is worth giving in full because it brings some of Palmieri's assumptions to the fore. Palmieri assumes that the natural/philosophical analysis or resolution starts from the given, in this case, a cause, just as the mathematical analysis or resolution does. Indeed, the mathematical analysis proceeds from *what is sought*—as if it has been achieved—and by working backwards one arrives at what is *proved or known previously*. However, the natural-philosophical analysis consists in reasoning from *what is known*, an effect, to *what is sought*, its cause. In other words, there is an important asymmetry between mathematical and natural-philosophical analysis [cf. Ducheyne 2005, 219]. As a consequence, Palmieri's criticism is directed at the mathematical analysis, but not at the natural/ philosophical analysis, his true object of criticism. In the accompanying footnote 93 on the same page, Palmieri adds:

> I am at variance with the myth of a logic of discovery, a resolutive method, or analysis, or *regressus*, either in philosophy or mathematics or natural science. I think that some recent historiography (cf. Wallace 1992) has labored under the delusion that such a method, in whatever form, existed, and that it was applied by early modern natural philosophers. None of the scholars embracing this historiography has ever produced a reconstruction of such a method based on the documented

> praxis of early modern natural philosophers. This historiography starts from the prejudice that accounts of methods to be found in the logical literature of the time reflect the praxis of early modern philosophers, and then coerces the historical data into the straightjacket of those accounts....

While I am in agreement with Palmieri that some of the claims on demonstrative *regressus* have been blown out of proportion in the past, I do not think that it follows from this observation that demonstrative *regressus* was of no importance at all to understanding Galileo's scientific work. It can be argued that demonstrative *regressus*, although it will not tell us much about the characteristic innovative aspects of Galileo's *scienza* or about the specific inferences as provided in his scientific practice, was nevertheless important to understanding some general features of Galileo's scientific thinking. That is, it can be argued that, although Galileo surely innovated with respect to the specific procedures by which causes are inferred from their effects, demonstrative *regressus* is still relevant to understanding Galileo's science in so far as he thought that the science proceeds from effects to causes and in so far as he used its terminology.[6] In other words, while the semantics of Galileo's causal talk was definitively innovative, the syntax remained traditional.[7]

I conclude this review with some general remarks. It would have been useful if the material covered in the appendices were incorporated into the main text. Parts of Palmieri's work would have been more precise if more secondary literature had been taken into account, especially when Galileo's intellectual trajectory is concerned. In this way, the reader could have gotten a better sense of the specifics of how Palmieri's account differs from and improves upon previous work. Earlier, I pointed out that the aim of the book becomes clearer whilst working through it. During that process, I came to realize that it is not a monograph on Galileo's experimental philosophy in general—consider the fact that little or nothing is said about the role of abstractions and idealizations in Galileo's experimental *scienza*—but a more specific study of the Galilean matching problem. Despite the reservations listed in the preceding paragraphs, I think that, in the

[6] This is what, according to my understanding, has been accomplished in Wallace 1992.

[7] I have made this case for Newton in Ducheyne 2005.

end, Palmieri has written a fascinating work, which no one seriously interested in Galileo's *scienza* should overlook. This is an exciting book, which, in combination with the corresponding website, offers insight into some of Galileo's experiments and on that account it is to be valued.

BIBLIOGRAPHY

Crew, H. and de Salvo, A. 1954 [1914]. *Dialogues Concerning Two New Sciences.* New York.

Drake, S. 1981. *Cause, Experiment and Science, A Galilean Dialogue Incorporating a New English Translation of Galileo's 'Bodies That Stay atop Water, or Move in It'.* Chicago.

________ 2001 [1953]. *Dialogue Concerning the Two Chief World Systems.* New York.

Ducheyne S. 2005. 'Newton's Training in the Aristotelian Textbook Tradition: From Effects to Causes and Back'. *History of Science* 43:217–237.

________ 2006. 'Galileo's Interventionist Notion of "Cause"'. *Journal of the History of Ideas* 67:443–464.

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

Hill, D. 1994. 'Pendulums and Planes: What Galileo Didn't Publish'. *Nuncius* 9:499–515.

MacLachlan, J. 1976. 'Galileo's Experiments with Pendulums: Real and Imaginary'. *Annals of Science* 33:173–185.

Naylor, R. 1974. 'Galileo's Simple Pendulum'. *Physis* 16:23–46.

Pitt, J. C. 2009. 'Review of Reenacting Galileo's Experiment, Rediscovering the Techniques of Seventeenth-Century Science'. *Isis* 100:661–662.

Redondi, P. 1998. 'From Galileo to Augustine'. Pp. 175–210 in P. K. Machamer ed. *The Cambridge Companion to Galileo.* Cambridge.

Wallace, W. A. 1992. *Galileo's Logic of Discovery and Proof, The Background, Content, and Use of His Appropriated Treatises on Aristotle's* Posterior Analytics. Dordrecht.

Science Translated: Latin and Vernacular Translations of Scientific Treatises in Medieval Europe edited by Michèle Goyens, Pieter De Leemans, and An Smets

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Science Translated is the product of an international conference on historical translation sponsored by the Institute for Medieval Studies at the University of Leuven in 2004. The 23 essays in it are organized into two sections focused on translations from Greek, Arabic, and Hebrew into Latin and translations into French, Italian, and Dutch vernaculars. Prefacing these more specific treatments of translation is a general essay by José Lambert on translation studies, in which he opens with two suggestive, if not surprising, quotations from Umberto Eco and Peter Burke, the former noting that translation is more fundamentally a shift between two cultures rather than two languages, the latter observing that history deserves a large role in the field of translation studies, and conversely that translation studies deserve a prominent place in historical work. Translation, by its very nature, signals a transmission from one person, place, time, or condition to another as well as a transformation, alteration, and renovation in the process of transmission, since the initial and final loci or cultures are rarely, if ever, the same. And sometimes the translation occurs within the same individual, place, time, and culture: in the very process of reading and understanding the object, the individual transfers meaning, sometimes literally, at other times metaphorically. Clearly, translation studies can never be a simple matter of finding isomorphisms between languages.

While the editors' approach has been to segregate the essays along linguistic grounds (Latin translations and vernacular translations), the foregoing would suggest that there may be multiple ways

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of viewing the content of this volume. It would be impossible to undertake a comprehensive attempt to do so or even to summarize all the essays contained in the volume; but three rubrics may suggest other ways to view the results of this conference. First, in keeping with Peter Burke's observation about the relationship between history and translation, there are methodological problems of translations as sources. Charles Burnett notes that many medieval translations were produced as a succession of revisions, making it difficult for the modern editor to determine which was prior and posterior, who was responsible for the various stages of the text, and, most fundamentally, how an edition of a text that lacks a base can be presented on firm scholarly grounds that follow traditional editorial procedures. William of Moerbeke's various texts, for example, were series of recensions, not strictly speaking translations: as Jef Brams suggested, it is likely that Moerbeke would not have regarded any of the successive versions as definitive. Instead, we have 'snapshots' of texts that have become fossilized by the chance survival of particular manuscripts of the recensions. Although it adds complexity to the historian's task, Burnett's conclusion is probably necessary:

> Every text, therefore, makes its own demands, and no rules can be universally applied when faced with the choice of editing an 'original' translation, or one of its revisions. [20]

Joëlle Ducos presents other elements of complexity. As translators proceeded to convert texts from various base languages into French during the 13th through 15th centuries, the language itself was evolving. In the face of apparent differences that resulted from linguistic shifts, scholars have nevertheless attempted to create a typology that transcends language. While some have suggested the presence or absence of a prologue as a marker for such a typology, as Ducos observes, the prologue by itself does not always determine the nature of the translation, since among Oresme's three translations, two have prologues but are not significantly different from the third that does not. Moreover, some translations tend to insert commentary elements—e.g., changing Aristotle's first person nominative to third person in the translation—and, hence, do not follow the *de verbo ad verbum* tendency of others. And if this were not sufficiently complicated, Ducos also notes the existence of a large corpus of incomplete translations, fragments of works that either were never completed or have suffered from the ephemeral tendency of manuscript transmission.

Laurence Moulinier-Brogi, while focusing on late medieval texts in uroscopy, provides additional examples of considerable bilingualism in medicine, yet notes that Latin was still the technical language of both physicians and apothecaries. This continued prominence of Latin resulted in back-translations of vernacular texts into Latin, as for example when a German translation of Maurus' *De urinis* was itself the object of Latin reverse translations [234]. Apparently by translating texts into the vernacular, the potential geographical circulation was limited, and so the translation back into Latin ensured a wider readership. But, of course, now the modern historian has two Latin textual traditions, one flowing from the original author, the other mediated through a German translation, thereby complicating the situation described by Burnett and Ducos.

Finally, Erwin Huizenga observes that vernacular translations were themselves products of an evolutionary rather than monolithic development. If his investigations of Middle Dutch translations of surgical works can be extrapolated to other vernacular communities and genres, it would point to articulated stages of the vernacularization movement. As Huizenga notes, from the early 13th century, short marginal vernacular notes appear in blank spaces within manuscripts of the so-called *artes*-literature. After 1250, and continuing into the 14th century, whole texts were translated to inform laymen who had no formal education in Latin, partly in response to the movement of the surgical center of Europe from Italy to the North at the end of the 13th century. And finally, around 1300, there seems to have been two categories of surgical professionals, one with feet in both the Latin and the vernacular worlds, the other whose linguistic abilities were limited to the vernacular. Surgeon translators like Jan Yperman catered to this new community, which moreover preferred shorter, abbreviated versions of the grand encyclopedic texts of the previous two centuries. When placed next to Moulinier-Brogi's conclusions about vernacular texts and bilingualism, we can see that translation efforts did not conform to a single trajectory, either linguistically, or nationally, or disciplinarily.

A second rubric, not surprisingly, concerns linguistic problems in scientific translation. While Burnett had focused on the issue of the evolutionary development of translations from Arabic into Latin, Carla Di Martino observes that part of the problem of translating from Arabic to Latin was the very different syntax of Arabic and occidental languages. Already in the 12th century, translators were aware of the prolixity of Arabic, and some at least considered this helpful in expanding the sometimes terse and confusing Greek original. To illustrate this, Di Martino compares the Arabic original of Averroes' *Talkhīs Kitāb al-Ḥiss wa-l-Maḥsūs* (*Epitome of De sensu et sensato*), the Hebrew translation, and two Latin versions. In some instances, the Latin translator attempted to provide a faithful rendition of the Arabic concept, either by using a grammatical similarity or by providing a paraphrase that expanded the term. But in other instances, he did not. For example, in a section on happiness and intellectual faculties in book 2 chapter 3, the Latin translator sometimes omitted or added—for example, he added the idea of the difference between dreams (caused by angels), divinations (caused by demons) and prophecy (caused by God). In Averroes, by contrast, the issue is the distinction between veridical and non-veridical dreams. Both, according to Averroes, are the result of the imaginative faculty; so both have human causes. This is an instance of doctrinal corruption of the text.

Joëlle Ducos observes that translators into French also remarked on the difficulty in finding an accurate equivalence: to them French did not have as rich a scientific vocabulary as Latin. The practice of borrowing and creating neologisms varied more or less successfully with the discipline; astronomy found it easy to coin technical terms from Latin, while in meteorology it was restricted to certain areas of the text. And, of course, the act of borrowing itself contributed to the development of the language.

In his essay on Renaissance translations of *Meteorologica* 4 and the commentary tradition, Craig Martin argues that book 4 is important because of the large number of technical terms it contains, terms that translators found difficult to render accurately in the object language. Moerbeke's translation was an improvement on earlier medieval ones; and despite humanist criticisms, Renaissance commentators frequently continued to use it. Beginning with Palmieri's translation in the 1460s, there were several new versions; and particularly within the humanist tradition represented by Leonardo Bruni and Theodore Gaza, emphasis was frequently placed on the scientific vocabulary that avoided medieval use of graecisms. Adoption of a new vocabulary, the humanists believed, would produce more elegant Latin versions and make medieval translations obsolete. While several humanists rejected Gaza's goals for creating an Aristotelian 'tabula rasa', contemporary translators did avoid transliterations of terms. Martin gives as an example the term πέψις (often rendered by modern scholars as 'concoction') and related terms. Aristotle's problem here and elsewhere was the creation of a technical term out of ordinary language, but the byproduct of this agenda was imprecision or (as many critics have charged) obscurity. Although early modern commentators on *Meteorologica* 4 slowly adopted humanist terminology, only four (Francesco Vimercati, Francisco Vallés, Johannes Hawenreuter, and Christoval Nuñez) used Renaissance translations as the basis for their commentaries. In many cases, the medieval text was emended with Renaissance terminology; and by the mid-16th century, a new type of commentary formed, dedicated to patterns of translation, with appendices explaining terminology. Even this did not satisfy every reader: beyond the disagreements over the particular choice of words used to translate technical Greek terms, commentators also criticized translators for having disregarded the sense of the passage.

In a similar vein, Pieter Beullens focuses on Aristotle's nomenclature of fish, which medieval and Renaissance readers found problematic in part because Aristotle believed that once fixed, the names would not change, and because he provided little descriptive information about the organism. Consequently, beyond the limitations of natural habitats, it was difficult for medieval and Renaissance scholars to determine which animal Aristotle was naming. In the absence of other evidence, medieval and Renaissance translators—Beullens examines the approaches of William of Moerbeke, George of Trebizond, and Theodore Gaza—fell back on surveys of names in previous works (like Pliny's *Natural Histories*) or transliteration of Greek terms. The success of Gaza's reformulation of fish nomenclature can be seen in its use by (among others) Conrad Gesner and Linnaeus and the almost complete obscuration of earlier translations.

Géraldine Veysseyre argues that Jean Corbechon's translation of Bartholomaeus Anglicus' *Liber de proprietatibus rerum* was a 'service translation': there is no reorganization of content, and the chapters and headings are all retained as in the original. This makes comparison of terms very easy. In some cases, where the French term is not identical in meaning with the Latin one, Corbechon inserts a paraphrase; for example, 'animal' is explained as 'beste et personne' because the French term 'animal' was not generally applied to beasts and humans. The same technique is employed when Corbechon used neologisms. When the Latin text employs concise syntactical forms (e.g., *econtra*, *e converso*), Corbechon does not translate the phrase but instead employs a different syntax to express the same idea, and this seems to have been done consciously to preserve clarity for his French readers. In particularly difficult passages, Corbechon inserts a brief gloss that explains the untranslatable material. When there is no single word that translates a Latin term, especially verbs, Corbechon's habit is to substitute either *faire*+adjective or *estre*+adjective for an action verb. This makes the vernacular text less creative linguistically. Nor does he like the frequent Latin tendency to make double verbs joined with a copulative; instead, he reduces this to one verb that preserves the general sense but alters the cadence of the phrase. The same is true for substantives: 'venas et arterias' is rendered 'vaines'. While the goal of the encyclopedist is universal knowledge, Corbechon takes this one step further: he attempts to make the vernacular version wholly self-sufficient, so that the reader need not know the allusion in the text or look up a quoted or paraphrased text, even if it is from the bible. Although Corbechon attempted to remain true to the Latin text, he also realized that he was addressing a different audience, the royal court. It is interesting to note that the majority of the surviving copies of Corbechon's translation, in contrast to the Latin base, are *de luxe* copies, illuminated and with fewer abbreviations than contained in the Latin text.

Other linguistic issues may be dealt with more concisely. Sara Marruncheddu, for example, observes that the French translation of the falconry treatise *Moamin* by Daniel de Lau (about whom little is known) uses more North Italian words than any other non-French terms, making the translation an example of Franco-Italian literature. With the discovery of a second manuscript (Bruxelles, BR IV.1208) of the French text, it is possible to analyze the lexical structure of the translation more completely. Among other things, Daniel de Lau adopts words from a variety of French dialects because they are living representations of the language. The Franco-Italian version is also rich in Latinisms and Arabisms, as well as a few Greek derivations. Alessandro Vitale-Brovarone assesses this more theoretically. In an ideal situation, the act of translation sets up two languages and two texts mediated by the act of translation. But this ideal situation is never perfect: the two linguistic communities may not be completely separate and, in addition to texts, there may also be oral interaction that affects translation. The translator as the medium between communities sets up several senses: the bilingual, the borrower, the diplomatic exchange. Indicative of the complexity of translation, Vitale-Brovarone describes the etymologies of four 'mots sans mémoire'—words that have a common use, reflect multiple linguistic origins, and whose developments are poorly understood or recollected. Overlaid on this, individual translation techniques demonstrate that the common assumptions about direct translations do not apply universally. And finally, we cannot ignore the social context of translation: one cannot limit the phenomenon of translation to a formal act of moving from one written text to another. Rather, translation is a relationship between two different groups of peoples involving a dialogue between the translator, the source, and the destination.

Vitale-Brovarone's reference to the social context of translation brings us to the third rubric, the cultural domain of scientific translation. While this cuts across most if not all the essays in the collection, two are especially illustrative of the relationship between culture and translation. Focusing on Latin translations of the Pseudo-Aristotelian *Problemata* and their readers, Iolanda Ventura observes that translation of scientific works into vernaculars involved more than just transferring a text from one language to another. Because the recipients of the translated text were largely excluded from the cultural networks of the original language, the translator had to provide in addition the information derived from glosses and commentaries. Both Latin and vernacular translators faced the problem of enlarging the native vocabulary with technical scientific terms that did not exist prior to the translation. The *Problemata* was particularly problematic (an unintended pun) for several reasons: the existence of two sets of translations, that is, medieval and Renaissance versions, each provided different translation strategies and goals; the structure and content of the work allowed translators and commentators different ways of approaching the text; and finally, the intrinsic difficulty of the text required specific strategies to access the text.

Some indication of the difficulty of the work can be seen in the fact that while the 13th-century translation by Bartholomew of Messina is extant in some 70 manuscripts, only 14 non-anonymous commentaries have been identified. While medieval commentators (especially Pietro d'Abano) were interested in correct and precise terminology in the text, subsequent translators were even more scrupulous in this regard. In addition to retranslating the Aristotelian corpus in the Renaissance, humanist translators also discussed new theories of translation. In particular, newer translators favored more nuanced translations than *de verbo ad verbum*, aimed at more expert grammar, syntax, and vocabulary; and they attempted to contextualize the texts they were translating. Once again, Theodore Gaza is illustrative: in his translation of the *Problemata*, he gave emphasis to the form, even to the point of sometimes sacrificing the exactness of the content. The criticisms articulated by many of these Renaissance translators may derive from several sources, but one (according to Ventura) was a changed culture, in which the privilege that Latin once held was now giving way to the reality that scholars more and more could consult the original Greek text. Moreover, the emerging patronage system of the Renaissance supported these translation efforts, especially in Italy.

Marianne Elsakkers' examination of early medieval Latin and vernacular terms for abortion and embryology provides a very interesting and nuanced example of cultural influences on translation. The early Middle Ages produced two sets of embryological treatises, one descriptive, the other normative, the latter generally restricted to two stages of development (corresponding to murder of the foetus or some lesser infraction), while the former employed finer and more numerous stages of embryonic development. At the same time, while normative treatises gravitated toward a bifurcated fetal development, they also created multiple synonyms for the criteria distinguishing early- and late-term abortions, including formation, movement, sensation, vivification, and the most elusive of all, ensoulment. Moreover, normative legal treatises can be found in both civil and canon law traditions, making abortion in the early Middle Ages a more complicated phenomenon than the distinction between secular and sacred. Within this confusing framework, because embryological terminology was largely restricted to normative discussions, the richness of descriptive terminology increasingly came to focus on the issue of abortion.

From an ambiguous passage in Augustine, the idea of ensoulment probably arose to explain what earlier descriptive embryologists referred to as formation. As Europe became more Christianized, the use of 'anima' became more ambiguous: earlier it may have referred to animation or movement, but gradually it came to be synonymous with ensoulment. The use of 40 days as the moment of ensoulment had both textual and theological foundations, the latter rooted in the fasts of Christ or the period of Lent. Although one might assume that authors of normative texts would consult embryological descriptive texts, there are very few early evidences of that. In the end, the normative texts—undoubtedly written by men—depended on the testimony of women to determine the particular stage of development of the embryo. And, thus, it was unlikely that women would incriminate themselves or other women as murderesses.

As a whole, *Science Translated* is a sophisticated and far-reaching examination of an extraordinarily complex and extensive field. I would simply offer two criticisms of the volume, both focused on tools of entry into the book and the field. First, while the editors have included two indexes—one of the manuscripts cited in the essays, the other of proper names of medieval and Renaissance authors and anonymous works—there is no subject index. Given the broad array of topics covered by *Science Translated*, such an index would help readers to see the connections among the individual essays. Second, while José Lambert's essay focuses on preliminary considerations of medieval translations and translation studies, it does not really constitute an introduction to the volume, and it raises more questions than it answers—even provisionally. It would have been very helpful had the editors themselves expanded the prefatory remarks beyond the fairly evident observation that the volume examines Latin and vernacular scientific translation. Nevertheless, this is a richly rewarding collection of clear and precise studies that will be cited both for their contributions to translation studies and their conclusions in more specific disciplinary investigations.

Averroes' Physics: A Turning Point in Medieval Natural Philosophy by Ruth Glasner

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The Muslim philosopher Averroes played a major role in the reception of Aristotle's philosophy in the Latin West. In referring to him as to the 'Commentator', the Scholastics themselves recognized Averroes' authority as an interpreter of Aristotle, the 'Philosopher'. It is indeed on Averroes' extensive word-by-word commentaries, translated into Latin in the first quarter of the 13th century, that the Scholastics relied in trying to understand the obscure and very compressed works of Aristotle (even more obscure in their Latin translation than in the original Greek). Averroes not only provided a literal explanation of Aristotle's texts but was also very alert to the exegetical and doctrinal problems raised by them, often comparing different solutions presented by other commentators and then expressing his own view. Although Averroes' main concern was to offer his solutions as those which capture the genuine intention of Aristotle, it was already clear to the Scholastics that on many controversial issues, far from being a faithful interpreter of Aristotle, Averroes went well beyond what Aristotle actually said and could have intended. What is not at all clear, and in fact very hard to assess, is whether Averroes had his own philosophical agenda, distinct from that of Aristotle, which he somehow followed in his interpretation of Aristotle. Did Averroes modify Aristotle's philosophy and in what direction?

In her book Ruth Glasner addresses this difficult question, focusing on the case of natural philosophy, and gives a positive answer. While for Aristotle the basic structure of the physical world was continuity (bodies, motions, space, and time are all continua),

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Averroes supported an atomistic view of bodies and motions, relegating continuity to the abstract realm of geometry. Glasner's expression for Averroes' new physics is 'Aristotelian Atomism', where the adjective 'Aristotelian' indicates that the atomism advocated by Averroes originated from internal tensions and ambiguities in the Aristotelian corpus and that, unlike the atomism of Epicureans and that of Muslim theologians, it was not in conflict with other fundamental aspects of Aristotle's thought, e.g., on causality. That the 13th-century Scholastics developed an Aristotelian atomistic theory of natural bodies—the theory of *minima naturalia*—is well known to historians of science, who have pointed out its significance for the early modern thought on matter and motion. In Glasner's view, however, they have neglected the contribution of Muslim philosophy and in particular of Averroes, assuming that the immediate origin of the theory of *minima naturalia* is to be found in some remarks by Aristotle himself. On the contrary, Averroes did give a fundamental contribution and in fact developed this atomistic theory farther than many Scholastics philosophers.

In addition to arguing for the atomistic character of Averroes' new physics, Glasner also advances a much more general and fascinating conjecture, namely, that 'the motive force behind Averroes' "Aristotelian atomism" was his aspiration to find a sound scientific foundation for indeterminism' [173]. As Glasner presents this issue, while Averroes was keen to support the indeterminist stance of Muslim theologians as opposed to the determinist one of Muslim philosophers (e.g., Avicenna), he was not happy with the lack of scientific foundation of both Greek (Epicurean) and Muslim indeterminism, which resulted from their denial of causality. Both Greek and Muslim indeterminists assumed that the physical world had an atomic structure and that only an atomic structure and not also a continuous one is compatible with indeterminism; they also assume, however, that this structure was not subject to causal laws. It is with this latter assumption that Averroes was deeply dissatisfied. As a good Aristotelian, he was convinced that a scientific account of reality cannot be achieved without causality. This is why in his view atomism had to be provided with a solid basis in the Aristotelian theory of causality.

Investigation into Averroes' new physics is made very difficult by the nature of his writings. He did not devote a specific treatise to the presentation of his own ideas in natural philosophy. His innovative views have to be retrieved from his commentaries on Aristotle's works, especially his three commentaries on Aristotle's *Physics*—the short, the middle, and the long commentary—where the explicit task of Averroes was to elucidate Aristotle's thought. Accordingly, Averroes' program is, in Glasner's words, an 'innovation by way of exegesis' [3]. It is not only the exegetical component as such that complicates the task of discovering the innovations introduced by Averroes. There is also the fact that the three *Physics* commentaries are extremely complex writings. As Glasner herself and other scholars have established, Averroes revised all three of them and more than once, so that they exist in different versions. The complexity of the textual tradition of Averroes' *Physics* commentaries cannot be neglected in the retrieval of his new physics. It is only through a comparative study of the extant versions of all three *Physics* commentaries that Glasner was able to unearth Averroes' new physics. Accordingly, before passing to the presentation of Averroes' new physics, in the first part of her book Glasner gives a detailed overview of the textual tradition of these writings. Although, as Glasner indicates, this part of her study may not be of immediate interest to historians of science, I think that it is of great methodological relevance for the historians of science too. It shows that accurate and deep textual studies are in some cases indispensable to discovering and assessing philosophical ideas.

The most salient sections of the first part of the book are those devoted to the different versions of the three *Physics* commentaries. For the short commentary (dated before 1159, and the only one extant in the original Arabic, and also extant in a Hebrew translation dated around 1250), there is direct evidence provided by the manuscripts of an early version (version A, written before 1159) and a late revision (version B, written after 1186) for the beginning of the first chapter of book 8. The Hebrew translation suggests that Averroes had modified version A too, possibly more than once, before writing the final version B. For the middle commentary (dated 1170, and extant in two Hebrew translations dated 1284 and 1316 respectively, and in the 16th-century Latin translation from the Hebrew), there is evidence of two versions of book 8, chapter 2. The two versions A and B of the middle commentary correspond to the two versions A and B of the short commentary, and are found in the 1284 and 1316 Hebrew translations respectively. The long commentary (commonly dated 1186, and extant in the 13th-century Latin translation by Michael Scotus and in a 14th-century Hebrew translation) was the one most heavily revised, as the significant differences between the Latin and the Hebrew translations show. Glasner distinguishes two patterns of revision:

- editing, that is, brief additions and modifications, and
- rewriting, that is, more substantial revisions like the replacement of whole paragraphs by new ones and the addition of long passages.

The case of the long commentary is complicated by the fact that the two versions of which these revisions are witnesses do not correspond precisely to the distinction between the Latin and the Hebrew translations: editing and rewriting are present in both translations, although cases of editing are more numerous in the Hebrew, while long additions are more frequent in the Latin. The fact that no complete manuscripts of the original Arabic text have survived makes it impossible to attempt a precise reconstruction of how the two versions were transmitted in the Latin and Hebrew translations. Glasner's conjecture is that the two versions derive from one single manuscript of the Arabic text, transmitting the revisions of the new version in the margins and leaving copyists (and perhaps translators) to decide which of these marginal insertions to incorporate into the main text. This seems a sound hypothesis and Glasner gives some illuminating examples in its support. The significant extension of the revisions makes it possible, however, to individuate some distinctive features of the late stratum of the long commentary. According to Glasner, these are:

- the formal introduction to the commentary (present only in the Hebrew translation), a stylistic element which was adopted especially in the school of Alexandria in the fifth and sixth centuries;
- more extensive application of logic to natural science and in particular of syllogisms to formalize Aristotle's arguments (more frequent in the Hebrew translation); and
- significant use of the *Physics* commentary by Alexander of Aphrodisias.

It is this last feature that for Glasner is the more illuminating one. She suggests that what inspired Averroes in revising his long commentary was exactly his reading of Alexander's commentary.

What textual evidence does this complex system of revisions of the three *Physics* commentaries provide for Averroes' new physics?

In dealing with this question, Glasner focuses on Averroes' discussion of three arguments concerning motion: the 'succession argument' (*Phys.* 8.1); the 'divisibility argument' (*Phys.* 6.4); the 'moving-agent argument' (*Phys.* 7.1). The sections corresponding to these three arguments were heavily revised in all three *Physics* commentaries. Also, these sections show a similar exegetical pattern, the 'turning point pattern', as Glasner labels it. Averroes first points out that earlier commentators found difficulties in Aristotle's argument. He declares that he himself had initially followed the commentators and been puzzled by the argument. After a period of hesitation and intensive study the turning point occurred to him: he came to realize that the difficulties raised by the commentators did not reflect genuine problems; rather, they derived from a misunderstanding of Aristotle's intended meaning of the argument. Accordingly, he proposes a new interpretation which in his view avoids the difficulties found by the commentators and at the same time reflects the true meaning of Aristotle. These three turning points and the three new interpretations associated with them are the textual evidence for Averroes's new physics that Glasner presents. The second part of her study mainly consists of three chapters devoted to the three turning points respectively.

The first turning point, which is about the succession argument, is the most fundamental one, since in Glasner's view the innovation that Averroes intends to introduce with it is the 'breakdown of determinism'. The succession argument is presented by Aristotle in the opening chapter of book 8 in establishing the thesis of the eternity of motion. The point of the argument is that before any change there must have been a previous change. The argument seems to apply to temporally finite changes, that is, to changes in the sublunar world, and thus shows that sublunar changes are chained. Glasner argues that the relevant question for the issue of determinism is whether Aristotle means that the sublunar changes are essentially chained or only accidentally chained. If sublunar changes are essentially chained, then every change in the chain is determined by the changes preceding it; whereas there is no such determination in an accidental chain. There is not a clear-cut answer to this question in Aristotle's presentation of the argument. In *Physics* 5.2, however, he seems to deny an essential links between changes, making the explicit statement that change of change is possible only accidentally. The Greek commentator Philoponus points out that Aristotle's view in *Physics* 5.2 is in contrast with the succession argument of *Physics* 8.1, thus implying that the succession argument shows that sublunar changes are essentially chained. Averroes at first defended the succession argument against Philoponus' objection. It is this defense that characterizes the early version (version A) of all three *Physics* commentaries. At a certain stage, however, Averroes re-examines the succession argument and gives a radically new interpretation of it (henceforth, interpretation B, following Glasner). The outcome of interpretation B is that the succession argument applies not to sublunar changes but to the first celestial motion, and proves that this motion is eternal. It is an argument *per impossibile*: the first celestial motion must be eternal because otherwise it would have been preceded by another motion, and this contradicts the assumption that the celestial motion is the first motion.

The most authoritative Scholastic commentator, Thomas Aquinas, dismissed interpretation B of Averroes as completely false (*omnino falsum*) because it contravenes both the actual words of Aristotle in *Phys.* 8.1 and the whole plan of *Physics* 8, given that Aristotle explicitly addresses the question of the eternity of the first motion later in that book. Aquinas seems to be right: it is hard to see how interpretation B can capture Aristotle's intention, despite Averroes' claim to the contrary. It is a departure from Aristotle's intention and not a faithful exegesis. It is a great merit of Glasner's approach to try to reconstruct the assumptions behind interpretation B and make sense of it. A crucial assumption is that sublunar changes are only accidentally and not essentially chained, though their succession is necessarily eternal, i.e., not interrupted. This latter condition implies that the sublunar changes are contiguous one to another. Contiguity, however, cannot be guaranteed by the accidental nature of a sublunar chain. It is guaranteed by the continuity of the celestial motion on which sublunar processes ultimately depend. As Glasner herself admits, the idea of a vertical order according to which the persistence of sublunar processes depends on the eternity of the celestial motion is not at all new. It is already suggested by Aristotle and commonly repeated throughout the Aristotelian tradition. She points out, however, that Averroes uses this idea to make a very innovative *negative* point, namely, that the persistence of the sublunar world cannot be derived from considerations of the horizon, that is, from the causal structure of the chain of sublunar changes: it is not the case that the existence of a sublunar change is determined by the changes preceding it, as the succession argument seems to suggest. It is with this negative point that Averroes wants to rule out a deterministic reading of Aristotle's argument. In support of her reconstruction of interpretation B, Glasner adduces the suggestion that on this issue Averroes closely follows Alexander, who had used the idea of a vertical order against the determinism of the Stoics. Glasner also maintains that Aristotle's distinction between contiguity and continuity is very relevant to Averroes' indeterministic campaign. In her view, Averroes associates continuity to necessity and contiguity to possibility/contingency, and then posits that true continuity and, hence, necessity is possible only in the celestial region; whereas ordered sublunar changes are simply contiguous and not also continuous, failing in this way to have a deterministic structure. As has been pointed out earlier, this association is the crucial ingredient of Glasner's conjecture about the link between the three new ideas she ascribes to Averroes. However, it is not supported by adequate textual evidence and is not in itself very convincing. In particular, note that according to Aristotle any chain or collection of changes, just in virtue of the fact that it consists of numerically distinct changes, is not continuous; but the position of numerically distinct changes seems to leave the question of whether they are deterministically connected or not totally open.

The second turning point, which concerns the divisibility argument of *Physics* 6.4, introduces Averroes' innovation about the structure of motion, the 'breakdown of motion', in Glasner's words, that is, the breakdown of Aristotle's view that motion is continuous and its replacement with the view that motion is contiguous. In a first approximation, motion conceived as continuous is a homogeneous interval-like entity while motion conceived as contiguous is a heterogeneous entity such that the structures of the whole and of its parts are different. The divisibility argument belongs to Aristotle's discussion of the continuity of motion and establishes the divisibility of the body subject to motion (the mobile). This conclusion is inferred from the premise that during a change the mobile is partly in the initial state of the change (the *terminus a quo*) and partly in the final state of the change (the *terminus ad quem*), which implies that the mobile has parts and, hence, that it is divisible. This argument has puzzled Aristotelian commentators of all eras because it does not seem to be valid in the case of instantaneous changes, typically generation and corruption, and some qualitative changes such as the illumination of a house (one of Averroes' examples). The body subject to an instantaneous change is indeed divisible but the premise of the divisibility argument only applies to temporal changes. Averroes reports the solutions attempted by Alexander, Themistius, and Avempace. He declares that for a long time he had followed the solution of Avempace but he has now come to abandon it. The general idea of his new solution is that instantaneous changes are not proper counterexamples to the divisibility argument because they are not proper (*per se*) changes but rather accidental changes. Only temporal changes are *per se* changes, whereas instantaneous changes are accidental changes because they are ontologically dependent on temporal changes: they occur as end points of temporal changes. For example, the illumination of a house is the end point of the temporal motion of a candle, and the substantial change from water to ice is the end point of the qualitative temporal change of cooling water. Averroes further describes a temporal change followed by an instantaneous change as a change such that its end point is of a different genus from that of the temporal change itself (e.g., the motion of a candle is a local motion, while the illumination of a house is an alteration). In Glasner's view, this description is the most compelling evidence offered by Averroes' new solution to the divisibility argument for the turning point from the continuous/homogeneous view to the contiguous/heterogeneous view of motion: a change followed by a change of a different genus is not a homogeneous entity but a heterogeneous one.

Glasner is aware that this evidence is not conclusive. One obvious problem is that nothing in Averroes' text suggests that all changes are heterogeneous in the way defined. On the contrary, Averroes explicitly distinguishes two kinds of *per se* change:

(1) those whose end points are of the same genus and
(2) those whose end points are of a different genus.

Glasner, however, maintains that for Averroes every change should be conceived as a heterogeneous entity and relies on other sections of Averroes' discussion of motion to substantiate this claim and also to arrive at a more precise understanding of the structure of motion as contiguous entity. A major ingredient of Glasner's reconstruction is Averroes' position on the ontological status of motion in *Physics* 3—a position very well known to historians of Scholastic natural philosophy for its centrality in the Scholastic debate. Averroes introduces a distinction between a reductionist view and a realist view of motion. In the reductionist view, motion is not a thing in itself totally distinct from the formal determinations successively acquired by the mobile body during a change, whereas in the realist view motion is such a distinct thing. These two views were often referred to by the Scholastics as the flowing form (*forma fluens*) and the flow of a form (*fluxus formae*) views respectively. Averroes sides with the *forma fluens* view, that is, the reductionist view. He claims that the *forma fluens* view is the true one, whereas the *fluxus formae* view, although it is suggested by Aristotle in the *Categories*, does not reflect Aristotle's genuine thought. Glasner finds this distinction between two ontologies of motion very relevant to her project because she believes that while motion conceived as *fluxus formae* is basically a homogeneous/continuous entity, motion conceived as *forma fluens* is a heterogeneous entity. Indeed, the association between *fluxus formae* view and continuity is explicitly made by Averroes. Also, it is not immediately clear how the *forma fluens* view can be translated into a continuity theory of motion. Can then the *forma fluens* view be associated with the alternative theory considered by Glasner, that of the contiguity of motion? Glasner tries to argue for a positive answer. It is puzzling, however, that she does not take into account a serious obstacle to the association of the *forma fluens* view with the contiguity theory of motion. The description of motion as a contiguous/heterogeneous entity in the turning point of *Physics* 6 is in contradiction with the kind of reductionism that Averroes explicitly advocates in his presentation of the *forma fluens* view in *Physics* 3: the heterogeneous change of *Physics* 6 is such that its end is of a different genus from the change itself, whereas motion as *forma fluens* is an entity of the same genus as the form that is its end point (with an example of Averroes *ire ad calorem est calor quoquomodo*), that is, an entity homogeneous to its final form. Accordingly, the *forma fluens* view is echoed in the turning point of *Physics* 6, but it is associated with the other class of *per se* changes distinguished by Averroes, namely, (1) those whose end points are of the same genus as that of the change.

The third turning point, which is about the moving agent argument of *Phys.* 7.1, introduces the 'breakdown of physical body', that is, the breakdown of Aristotle's view that natural bodies are continuous, i.e., divisible *ad infinitum*, and its replacement with an atomistic view according to which natural bodies are composed of minimal parts (*minima naturalia*). The moving agent argument in which Averroes' innovation most explicitly appears does not belong to Aristotle's theory of continuity but to the causal account of motion; and it establishes the conclusion that everything which is moved is moved by something else, a fundamental step in Aristotle's proof of the existence of an immobile mover. The relevant part of the argument is the premise that a body moved essentially (*per se*) is such that its motion comes to an end if the motion of one of its parts comes to an end. The idea underlying this premise is that a body essentially moved has such a strong unity that it can only move as a whole. What is the physical entity to which this strong unity belongs and to which essential motion can be attributed? This is the controversial question for Averroes.

As Glasner argues, the main source of Averroes in this controversy is Alexander's *Refutation of Galen's Treatise on the Theory of Motion*, which was available to him in Arabic translation. Alexander argued that the physical entity to which essential motion is to be ascribed is the simple body, which he regards as a true homoeomer, that is, a body such that its parts are of the same nature of the whole and, hence, not other than the whole. Galen criticized Alexander's conclusion that in simple bodies parts are not other than the whole, pointing out that also in these bodies there is a distinction between a whole of parts and only a part, and provided a more careful analysis of what essential motion is. While Alexander did not provide a definite answer to the question about the physical subject of essential motion, Averroes does provide it and, in Glasner's view, by doing so, he pursues Alexander's ideas.

Averroes formulates his answer in terms of the first moved entity, and maintains that the first moved entity in a natural body is the minimal part of it. For example, in the case of water, the first moved entity is the minimal part of water, that is, a part of water so small that no smaller part can take on the form of water. In Averroes' view, these minimal parts do exist in fact in a natural body: they are actual particles, so to speak, and not simply theoretical limits to division.

Averroes is aware that positing minimal parts of natural bodies is in conflict with Aristotle's view that natural bodies are continuous, that is, infinitely divisible so that any given part can be further divided. He tries to resolve this contrast by distinguishing between a natural body considered *qua* natural and a natural body considered *qua* continuum/quantity: considered *qua* natural a natural body contains minimal parts, but considered *qua* continuum is infinitely divisible. This exegetical strategy is also common among Scholastic supporters of the theory of *minima naturalia*. As Glasner rightly emphasizes in her assessment of this strategy, in Averroes' reading Aristotle's theory of the continuum turns out to be valid only for the abstract realm of geometry and not also for the physical world. On the other hand, the atomistic structure of the physical world proposed by Averroes is still deeply Aristotelian in that the minimal units are essential units composed of matter and form and subject to natural motion.

Of fundamental importance for tracing Averroes' intellectual biography is to establish when exactly the three turning points occurred. Glasner carefully investigates this difficult issue. Especially in the case of the first and second turning points, the middle commentary and the long commentary provide conflicting evidence and give rise to two possible accounts: the turning points occurred either

(1) when Averroes was writing the middle commentary, that is, around 1170 or
(2) when he was writing the long commentary in the 1180s.

Glasner's very well argued conclusion is that (2) is more plausible.

Glasner's book is an ambitious attempt to establish the innovative character of Averroes' natural philosophy, but I think that it is only partially successful. It does show that Averroes, like many 13th-century commentators after him, rejected the Aristotelian assumption that natural bodies are continuous and replaced it with an atomistic theory. It fails to show convincingly, however, that Averroes had an analogous atomistic view of motion and that his atomism was inspired by a concern to find a scientific basis for indeterminism. Despite these shortcomings, Glasner's investigation has the great methodological merit of being based on an extensive and detailed study of the very intricate textual tradition of Averroes' commentaries on Aristotle's *Physics*.

Le parole delle Muse. La formazione del lessico tecnico musicale nella Grecia antica by Eleonora Rocconi

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It is well known that in ancient Greece the term μουσική (*scil.* τέχνη) was used to designate the art of tightly interweaving into each other two or three different activities in a single communicative event, namely, the performance of (nearly all) the poetic texts in singing or recitation, the sound of musical instruments (winds, strings, percussions), and the rhythmic movements of the performers' bodies (i.e., dance). For several reasons, only a small group of scores and fragments of scores of ancient Greek music has come down to us: the evidence is too scanty to show us exactly how ancient Greek music sounded—and this is the case for many other activities of human life in Antiquity. Moreover, the sound itself was only one of three elements of which the μουσική consisted. Still, a large number of ancient literary texts and pictorial images on pottery testify that μουσική permeated the daily life of the Greeks. And since musicologists should seek to examine and understand the ways through which music, in its entirety, appears and develops within specific historical contexts, for ancient Greece we are forced to study carefully, in addition to (and perhaps more than) the few surviving scores and fragments of scores, the images that reflect the diverse and complex manifestations and practices of μουσική, along with literary texts that explicitly deal with or allude to music at different levels (acoustics, the psychology of auditory perception, music theory, the influences of music on the human soul and behavior, and so forth). This is why, in recent decades, studies of μουσική have notably proliferated: the topic continues to generate interest among increasingly large and diverse

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categories of scholars—philologists, archaeologists, ancient historians, musicologists, and ethnomusicologists.[1]

It follows that, if the study of μουσική can give us a privileged perspective on Greek culture and civilization as a whole, one of the most effective keys for unlocking the world of μουσική would be to know the technical terminology of this art, to say nothing of the fact that the technical vocabulary of music in most modern languages is strongly indebted to the ancient Greek one. In fact, such technical terms as 'symphony', 'diapason', 'harmony', 'melody', 'rhythm' were also technical terms in ancient Greek, though in coming into the technical vocabularies of modern languages they changed their original meanings, sometimes alot. Only a few scholars have addressed this important topic. Maarit Kaimio, in her lexicological study on the verbal characterizations of sound in literary texts before 400 BC, gives us a 'short survey of the characterization given to sound in such contexts where the sound itself is the object of research' [1977, 218] on the basis of a limited selection of texts, and without any programmatic intention to pinpoint the connections between the characterizations of sound in non-technical literature and the technical terminology known to us from the ancient Greek treatises on musical theory. Solon Michaelides' book [1978] serves both as an encyclopedia and as lexicographical resource: as a reference or dictionary-like work, it gives profiles of musicians, theoreticians, and philosophers who deal with music, along with explanations of a wide if not complete range of technical terms alphabetically arranged. Otto Christoph Steinmayer's doctoral dissertation [1985] gives a lexicological contribution to the story of selected technical terms, a useful but limited picture. None of these works aims to investigate systematically the *formation* of the technical vocabulary of music, as Eleonora Rocconi does in this very welcome book which results from a rewriting of her own doctoral dissertation. The book is targeted primarily at an audience of scholars (classicists, musicologists, and linguists interested in the formation of technical vocabularies), but advanced students in these subjects may also read it with profit.

The topic addressed is vast and difficult, and Rocconi has identified a number of pathways along which technical musical vocabulary has been formed (we shall illustrate them further in this review). In

[1] See, for example, Raffa 2005 on Murray and Wilson 2004.

so doing, Rocconi has given scholars a new, clear, and reliable starting point. Another reason why every classicist should be grateful to her is that she has collected and discussed a considerable range of material on the topic. As M. L. West has recognized [2005], one of the unquestionable strengths of the book in comparison to previous works and in proportion to its small size, is the enormous wealth of material brought to the attention of scholars. In 98 pages of text and footnotes, Rocconi quotes or cites an impressive number of texts, spanning the chronological range from Homer to Manuel Bryennius (14th century AD)—the index of passages mentioned occupies 17 pages. All texts quoted by Rocconi are translated into Italian, but only for some of them is the original Greek given as well. Though this will be a welcome aid to students without Greek, it may disappoint some classicists.

The central idea of the book is the belief, previously expressed by Rocconi in an article [1999, 93–94], that the musical vocabulary of ancient Greece was formed mainly when musical practice became the object of theoretical reflection, that is, around the same time that specialized musicological treatises began to be written. In the field of Greek music theory, the earliest author of whose works a substantial amount survives is Aristoxenus of Tarentum (fourth century BC). However, not only from Aristoxenus himself, but also from a number of other ancient authors (Plato, Aristotle, Theophrastus, Ptolemy, Porphyry) we learn that music theorists of different theoretical orientations did exist before him. Thus, Rocconi turns her attention to the age preceding Aristoxenus when the process must have started. The most important authors here were Lasus of Hermione (sixth century BC), who, as we are told, was the first to write a real treatise on music; Archytas of Tarentum; Philolaus of Croton; and Damon of Oa (fifth century BC). The remains of their writings are so meagre, however, that we cannot fully evaluate their contributions to the formation of musical vocabulary at this stage of its development.[2] That is why Rocconi has extended her study to the texts of different literary genres (prose and poetry) where we find several references to sound and music which, while not technical in the strict sense, give

[2] On Philolaus and Archytas, see Huffman 1993 and 2005; on ancient Greek music theory, see now Barker 2007; on other aspects of ancient Greek musical terminology, see also Rocconi 2003a and 2004.

us very important pointers to the processes by which some words were given an increasingly technical meaning.

In the introduction [1–10], Rocconi clearly outlines the general plan of her work. She states that the vocabulary of music was heavily influenced by such other disciplines as philosophy and rhetoric, and that the process of its development must have occurred in three ways:

1. the meanings of some words used in the jargon of stringed instrumentalists were extended to embrace technical concepts, musical events, and phenomena in a broader sense;
2. a number of words (mostly adjectives) originally used in common parlance or in poetic language to describe sounds and often derived from other sensory spheres became technical terms in μουσική through metaphor and 'synaesthetic' association;
3. a few onomatopoeic words originating in the representations of sounds made by animals were eventually adopted as technical musical terms.

Consequently, the book is divided into three chapters, each of them dealing with one of the three processes described.[3] A large bibliography [99–107], a detailed index of passages [109–125] and a very useful glossary [127–147] conclude the book.

In discussing the ancient texts, Rocconi does not always follow chronological order and sometimes, even when it would have been possible, fails to identify precisely the moment in the history of language at which a particular word of the everyday language became a technical term in a strict sense. Moreover, many important texts, which could have been usefully discussed in detail, are only mentioned in the footnotes, thus leaving the reader on occasion to struggle to follow the thread of the argument and to integrate several steps that are not immediately evident.

The task Rocconi has set herself involves some specific difficulties and she seems to have made a few general assumptions which I

[3] That is:

I. *La lingua degli strumenti: il lessico tecnico dei cordofoni* [11–51]

II. *Percezione acustica e descrizione metaforica del suono presso i Greci* [53–80]

III. *Suoni animali e suoni musicali: gli epiteti onomatopeici e la formazione del lessico tecnico* [81–98].

should like to explain here briefly. The first concerns what counts as 'technical vocabulary'. If we represented the vocabulary of a given language by means of the set of its words, a technical vocabulary of that language would be a subset within it, a subset built up from words referring to a particular sphere of human activity of a specialized character. In this way, there would be technical vocabularies for medicine, nautical matters, cooking, music, and so forth. But when exactly should we say that a given word is an item in that technical subset of words? That is to say, what exactly is a technical term? While words in everyday language usually have more than one meaning (polysemy), a word of technical vocabulary should have only one meaning or very few, and this meaning must be defined as precisely as possible so as to avoid, or at least to minimize, misunderstanding and misinterpretation. Such a word should do no more than designate a specific referent (object, action, phenomenon) or a very small number of such referents that fall within the domain of a definite activity. That is to say that technical terminology has one linguistic function only, the cognitive-denotative one. A word of this kind is what we define as a 'technical term' (both 'technical vocabulary' and 'technical term' being of course technical terms in linguistics!).

Although every technical vocabulary consists of words falling outside common language, it also includes words that belong to it; these last, when used in a technical sense, take a different, specific meaning. Generally, there are three main ways by which any technical vocabulary is formed, each leading to a group of technical terms:

- the use of loanwords from other languages ('external' route);
- the development of neologisms using existing word material and following the normal processes of word formation (composition, prefixation, suffixation) ('internal' route); and
- the assignment of new meanings to words that already exist in the common language (or within a technical vocabulary or jargon already established): such words are applied in the new technical area by extension (metonymy) or semantic transfer (metaphor) (another 'internal' route).

We can be sure that a common word has become a technical term in a given area when its functional capacity is reduced and its referential field has been restricted so that its technical meaning is unrecognizable in the semantic sphere to which it originally belonged.

Now, it is quite clear that the vocabulary set of a language no longer spoken, like ancient Greek, is virtually a finite set, and that, within that set, each technical vocabulary subset will also be finite. Quantitatively speaking, then, ancient Greek technical languages include a very limited number of terms when compared to their counterparts among the languages still spoken. This fact, however, regarding the technical vocabulary of ancient Greek music, does not always help us. Despite the limited number of entries to consider, the evidence that we have—a number of texts heterogeneous in content, form, and destination, which range over a very broad time span, thus making them sometimes very difficult to interpret—does not always allow us to trace the history of all the words related to sound and music, and to follow all the steps of formation of each technical term. In many cases, even when the evidence is extensive, it is not sufficient to confirm hypotheses or even to warrant proposing them. Hence, it is not always possible to pinpoint when a word referring to a sound is actually a technical term in the strict sense. Furthermore, we must be aware that a technical musical term derived from common language may continue to be applied to sound events in quite a generic and non-technical sense long after its has become technical, and that this may also occur in technical literature in the strict sense or in contexts that we could call technical. Lionel Pearson drew attention to the difficulties that can arise sometimes when we try 'to distinguish the special or technical use of a word from its general meaning' in such a technical writer as Aristoxenus of Tarentum [1990, xxxiv n20]. Yet, in spite of these difficulties, it is still important to try to restrict the boundaries of our uncertainties whenever this is possible.

But there is a deeper difficulty. In the vast range of the perceptional experiences that human beings are capable of, sound and music are perhaps the most difficult ones to force through the needle's eye of language. As a result, every language—ancient Greek is no exception—has almost no words, if it has any, which are primarily used to describe sound or are specifically related to the sphere of auditory sensations, both sound and the perceptions of it being of course the raw materials of music. If merely studying the processes of the verbalization of sound in ancient Greek (even without taking the next step, namely, the study of the formation of a specific technical vocabulary of music) requires thorough knowledge in linguistics (including semantics and the history of language) and musicology (including the conceptions that the ancients had of sound (acoustics) and of its perception (psycho-acoustics)), these two sets of skills are not always coupled to the same extent in the person of one scholar. Still, studying the vocabulary of sound and music and paying attention to its strictly technical aspects can help us to expand our knowledge of the ways in which the ancient Greeks conceived sound and music.

Rocconi [6] rightly points out that, within the technical vocabulary of ancient Greek music, the group of loanwords (type 1) is limited to names of musical instruments (except πηκτίς), a subject which Rocconi decides not to address. Many terms are formed by composition, suffixation, prefixation (type 2). But the majority of musical technical terms derive from common language through sometimes very complex processes of metaphor and metonymy (type 3) that bear witness to the evolution of meanings and the ways in which Greek culture conceived music and represented it in language. Moreover, some of the most important semantemes (e.g., the -τονος and -χορδος terminations) employed for the creation of compounds (e.g., ὀξύτονος, τετράχορδος) were formed precisely within this terminological framework. Rocconi's study is dedicated to this group of terms. Consequently, the terms considered, being formed by composition, suffixation, prefixation, are not isolated in accordance with purely morphological criteria but are instead analyzed and examined within her general discussion. Almost all terms are listed in the final glossary [127–147],[4] which includes references to passages of the book in which each term is discussed and thus also serves the functions of an index.

The book is very stimulating and every page deserves attention. An analytical discussion of all material supplied by Rocconi would, however, go far beyond the tasks of a review. Accordingly, I will just follow her argument, adding some of my personal observations and occasionally registering disagreement with her interpretations.[5]

In the first chapter, Rocconi shows that an important part of the technical vocabulary of ancient Greek music consists mainly of nouns

4 As far as I can see, a very few items are missing: ἀνάδοσις [15], βραχύς [7n424], ἔντονος [18], στενάχω [55], and ἀκαριαῖος [69].

5 Unfortunately, the book also contains a large number of typographical errors. See page 125 below for a list of those that are most obstructive and likely to lead to misunderstandings.

and verbs originally belonging to the jargon used by musicians to describe the gestures made in their work, especially in playing stringed instruments (which traditionally enjoyed greater cultural status than wind or percussion instruments) [11]. Moreover, it was in this 'pragmatic' context that musical theory incorporated the names of the musical notes into its technical vocabulary; every musical treatise employed these names to identify musical sounds regardless of how they were produced. Indeed, the names of the musical notes originally designated the strings on stringed instruments: almost all of them (νήτη, παρανήτη, τρίτη, παραμέση, μέση, παρυπάτη, ὑπάτη) originate from the position of the strings on the instrument that produced them [11–12]; only the intermediate note between the μέση and παρυπάτη was designated by the term λίχανος/λιχανός (forefinger), because of course it was originally produced by the string plucked with that finger. Within this group of terms, Rocconi introduces a very important distinction between those recruited into the vocabulary of musical theory and those that remained in the jargon of instrumentalists (string players) to describe precise technical gestures intended to produce special effects of sound.[6]

Rocconi rightly says in the introduction [2] that the oldest lemma (and also the richest in meaning) to have developed in this area is most certainly the word ἁρμονία, whose original meaning, 'conjunction' or 'seam' between different parts, pertains to the sphere of carpentry and comes in music to designate the 'connection (*scil.* of sounds)' or the 'tuning (*scil.* of an instrument)'. The long history of the term is sketched briefly but very clearly [2–3].[7] In particular, Rocconi shows that notions related to μέλος, ῥυθμός, and ἦθος live together with the original musical meaning of ἁρμονία, for the term was used to indicate traditional systems of musical sounds characterized by a set of rhythmic and melodic features that gave them a peculiar ethical influence. Moreover, it was precisely the development of theoretical speculation that caused the word's broad sense to become obsolete: the theoreticians needed to distinguish clearly between the many elements forming the ancient concept of ἁρμονία,

6 See, e.g., διάληψις [6] (the practice of placing a finger on the central part of the string and then lifting it off as soon as the plectrum made it vibrate so as to produce the harmonic the next octave up) and κατάληψις [3] (the technique used to suddenly dampen the vibrations of the string being struck).

7 To the bibliography quoted by Rocconi in 213n8, add Meyer 1932.

which was too rich in different meanings, and to find a single word for each of them. Other terms were introduced into the technical vocabulary of music from the fourth century BC onwards to designate each of the various meanings inherent in the concept of ἁρμονία:

- σύστημα, which covers the 'disposition of sounds within the octave'; this term appears for the first time, as far as we know, in Plato, *Philebus* 17d and reflects the idea of a 'spatial' organization of musical sounds elaborated by the theorists preceding Aristoxenus [76n461];
- γένος, a clearly Aristotelian term which became the technical denomination for particular dispositions of sounds within specific tetrachordal frameworks (in expressions like, for example, γένος χρωματικόν); and
- τάσις, a word from the pragmatic jargon of instrumentalists that came to indicate the pitch of a sound or of a scale.

From Rocconi's account it emerges that the term ἁρμονία never disappeared completely from theoretical literature but acquired new specialized meanings to indicate referents other than the original ones. In Pythagorean parlance, for example, it came to indicate the octave (for which the expression τὸ διὰ πασῶν was also used), while in Aristoxenus' writings it indicates the enharmonic γένος [see 2n8, 3n13]. On the other hand, Platonic and Pythagorean philosophical literature extended the semantic value of ἁρμονία with the result that its technical musical meaning faded.

Rocconi divides the chapter into four sections:

I. 'Il lessico della *tensione* e dell'*allentamento*: ἐπιτείνω/ἀνίημι' [13–21],
II. 'Il suono come *risultato* della tensione: τόνος e τάσις' [21–26],
III. 'Il *pizzicamento* delle corde con le dita: ψάλλω e i suoi derivati' [26–32], and
IV. 'La *percussione* delle corde con il plettro: il campo semantico di κρούω' [32–51].

The reasons for this division derive from data which are quite obvious to the specialist reader. But since Rocconi takes them for granted, it will be useful to provide here an explanation for the nonspecialist. On stringed instruments, sound is produced by the vibration of strings under tension: at a given length and thickness, the higher the tension applied to the strings, the higher the pitch of the sounds that they produce, and *vice versa*—the lower the tension, the lower the pitch of the sounds. It is also important to consider that in order to produce sound on a stringed instrument, the strings may be set to vibrate either by plucking them with fingers or by striking them with a plectrum. Indeed, several technical terms (e.g., κροῦμα and ψαλμός) derive from the percussing and plucking of strings. Of course, it is also true that at a given thickness and tension, the shorter the string, the higher the pitch; and that, conversely, the longer the string, the lower the pitch of the sound produced by it. Nonetheless, while there are many words referring to the tightening and slackening of strings that became technical terms defining the pitch of all instrumental and vocal sounds, there are none referring to their length. The reasons are probably to be found in the fact that instruments with strings of equal length (e.g., the φόρμιγξ, λύρα, κιθάρα, and βάρβιτος) were, apparently, far more common than those with strings of unequal length (e.g., the πηκτίς and τρίγονον). Furthermore, the designation of stringed instruments as ἐντατά or κατατεινόμενα ὄργανα shows clearly that tension was the important factor.[8]

It is by semantic extension (metonymy) that words from the 'pragmatic' area are employed within the technical vocabulary. Thus, words originally designating specific actions (e.g., the tightening and slackening, percussing, and plucking of strings), came to designate, first, the consequences that those actions have on the sound produced by those instruments and, second, specific facts and technical phenomena within the broad spectrum of musical practice (vocal and instrumental) which have traits in common with those to which they originally relate but are no longer linked to specific referents of that area. This latter would include raising or lowering the pitch of *all* sounds (not just those produced by stringed instruments) or the production of sound by wind instruments or even by the voice. In other words, since at a given length and thickness *a string* producing a high pitched sound is 'tauter' than that producing a low pitched one, and *a string* producing a low pitched sound is 'slacker' than that producing

[8] See, for example, Aristoxenus fr. 95 in Wehrli 1967, 34; Aristides Qunitilianus, *De mus.* 2.16 [Winnington-Ingram 1963, 85.8]. In general, on stringed instruments in ancient Greece, see West 1992, 48–80 and Maas and Snyder 1989.

a high pitched one, it was said quite naturally perhaps that the relevant sounds are 'taut' and 'slack' respectively. In contrast, it would be less natural, if at all, to say that a sound produced by *a wind instrument* or by *human voice* is 'taut' or 'slack' in itself, meaning that it is 'high pitched' or 'low pitched'. The extended meanings of these words were common at least from Aristoxenus on, but the pathway that led them to be employed in such a way must have had different steps, to judge from some terminological distinctions that we find in Aristoxenus himself [*Harm. elem.* 1.10.24–11.1 ~Da Rios 1954, 15.14–21]. In any case, Rocconi conjectures plausibly that, at a first stage, the spontaneous and, to some extent, rough employment of a purely 'pragmatic' vocabulary might have generated some conceptual inaccuracies within the technical literature itself: in the passage referred to above, Aristoxenus, in opposition to (or in polemic against) many people (οἱ πολλοί) who believed that ἐπίτασις and ὀξύτης were the same thing and likewise that ἄνεσις and βαρύτης were so too, applies an 'Aristotelian' distinction between causes and effects in specifying that ἐπίτασις *produces* ὀξύτης, and ἄνεσις *produces* βαρύτης. So, we should assume that he had in mind his predecessors (or contemporaries) who were engaging in the same field of musical theory, and were using such a terminology a little bit incorrectly—in fact, we can recognize traces of this kind of technical development in Plato.

Indeed, Rocconi [15–16, 23] correctly notes that in some cases [e.g., ps.-Aristotle, *De aud.* 802a5 ff., 803a23 ff.] words denoting tightening and slackening are applied to sounds without any clear reference to their pitch, but to volume or duration in time or other parameters too. Moreover, in ps.-Aristotle *Physiogn.* 806b26 [15], the participles ἐπιτεινομένη and ἀνειμένη do not in any way refer to high and low pitch respectively, but to higher and lower intensity of sound. It is very interesting to consider the series of passages alluding to the 'tones' of the voice, meaning the *volume* of the sounds uttered or the utterer's *emotional intention* as well as the sounds' *pitch* [23nn55–57]. In this regard, if I understand Rocconi's point, I would not be so sure as she is that the musical meanings of verbs like ἐπιτείνω and ἀνίημι are to be connected to the 'natura musicale dell' accento greco' [15]. Rocconi also quotes a series of texts spanning a chronological range from Aeschylus to Plutarch, in which words referring to 'tightening' are applied to the *duration* of sounds in time. I would try to explain this phenomenon by recalling, in addition to what Rocconi says, that the root ταννυ-, from which the semantic sphere of τείνω derives, originally contains the idea of 'extension' or 'prolongation', an idea that could be, of course, applied also to duration in time.[9] Given the texts adduced by Rocconi, the reader might note that occurrences of this kind always refer to sounds uttered by voice or produced by wind instruments such as the σαλπίγξ [Plutarch, *Sull.* 7.6], never to those produced by stringed instruments. It seems to me that this is easily explained: sounds produced by stringed instruments (by means of plucking or percussing the strings) can in no way be sustained; whereas, in contrast, sounds produced by a wind instrument can be sustained and even increased in volume—which makes their duration in time still more evident.

Obviously, in all these cases of evident polysemy, we should not speak of technical terminology strictly but rather of a particular influence of the ways in which ancient Greeks conceived and linguistically represented a physical phenomenon like sound. According to the written evidence that we have of the earliest phases of the history of Greek language, it seems that no clear lexical distinction between different characters of sound was made. At the same time, no clear distinction was made between the sound itself and the perception of it.

According to Rocconi [14–15], these technical terms never lose their link to the semantic sphere of provenance; yet, she provides a number of texts [30–32] where verbs like κρούω and the synonym κρέκω ('strike') or ψάλλω 'pluck', which both refer originally to two different ways of producing sound on stringed instruments, pass into the vocabulary of both the αὐλός (a wind instrument) and the singing voice respectively. Now, I would take these cases as evidence that the link, if not broken, has faded. As far as we know, it is difficult to pinpoint the moment in the history of Greek language when exactly the link with the technical jargon breaks, and in many cases we only are able to record statements where the link has already broken. In this respect, the same passage of Aristoxenus [*Harm. elem.* 1.10.24–11.1 ~Da Rios 1954, 15.14–21] that Rocconi quotes for other purposes should be considered an important piece of evidence in this regard. In this passage, we are told that

> tension (ἐπίτασις) is the continuous movement of the voice from a lower position to a higher (κίνησις τῆς φωνῆς συνεχὴς

9 See Chantraine 1999, *s.v.* τάνυμαι.

> ἐκ βαρυτέρου τόπου εἰς ὀξύτερον), relaxation (ἄνεσις) that from a higher to a lower (ἐξ ὀξυτέρου τόπου εἰς βαρύτερον). Height of pitch (ὀξύτης) is the result of tension, depth (βαρύτης) is the result of relaxation.

What is remarkable here is that tension and relaxation are referred to, in a strictly technical sense, as movements of the *voice* (φωνή) and not as actions exerted on *vibrating strings*. Moreover, the notions of 'movement (κίνησις)' and of 'position (τόπος)' clearly imply the idealization and visualization of a 'sound space'; and it is obvious that the adjectives ὀξύς and βάρυς, referring to high and low pitch of sound respectively, already has a precise technical value. Thus, Rocconi should perhaps have noted that the technical development of these originally 'pragmatic' words implies in turn the pre-existence of a special vocabulary related to qualifications of *pitch*. Anyway, even after a word of common language or jargon has become a technical term of music, it is always possible to find occurrences of its common meaning still in reference to sound—and this may occur even in technical literature, as, for example, in both passages of the *De audibilibus* [802a5 ff., 803a23 ff.] mentioned above. Such ambiguous usage is one of the many difficulties encountered in ancient Greek musical lexicology.

Rocconi draws attention to the fact that this lexical sphere is used also by Pythagorean theoreticians who studied acoustic phenomena without considering the *tension* of the strings producing sounds as the relevant factor for variations in pitch, but taking into account their *length* only. In this sense, the *Sectio canonis*, a treatise attributed to Euclid and dating to around 300 BC [see Barker 1989, 190], has a special importance (even though its author is not a Pythagorean). For, although the author is particularly concerned with the study of ratios between the different pitches of the sounds and the different lengths of the vibrating strings producing them, the vocabulary applied throughout to designate any variations in a sound's pitch consists of terms originally related to *tightening* and *slackening*. However, in this case too, it seems that Rocconi is inclined to see the persistence of some 'active' link between these terms and their original semantic field [14], and to believe that certain evidence that this link has definitively disappeared does not come until the authors of the *Anonyma Bellermanniana* (first few centuries AD) or even Manuel Bryennius (14th century AD) [15]. However, in my view, the fact that, in order to refer to variations of pitch—a phenomenon which they consider dependent on the strings' *length*—Pythagorean theoreticians regularly employed terms originally intended to define the *tension* of strings without any actual reference to particular actions made upon strings of musical instruments or the like, does not mean that they really felt a sort of 'active' link between those terms and the semantic field which they came from. Rather, it means in all likelihood only that they appropriated technical terms which had already come into use as such, without any awareness at all of their semantic origin. Moreover, on the basis of the evidence provided by Rocconi herself [see 14n52], it is easy to see how this technical development is a *fait accompli* in later authors like Cleonides (probably second/third century AD), and of course Nicomachus of Gerasa (first century AD) as well as Claudius Ptolemaeus (second century AD), in whose works verbs like ἀνίημι, ἐκλύω and ἐντείνω appear in theoretical contexts to designate the lowering of pitch without any reference to the instrument that produces the sound. Nonetheless, in these cases too, Rocconi believes that those verbs, in these very contexts, imply a link to their pragmatic origin [14].[10] My reading of these texts is different from Rocconi's: as it seems to me, they do not testify to the persistence of that link but to the fact that those verbs have developed their meaning in a strictly technical sense.

Rocconi observes that this pragmatic section of musical terminology is also employed in the fields of ethics and political theory [19n69, 70]; thus, we find it in a corpus of texts defining what Abert [1899] called 'Ethoslehre'. Here, as Rocconi notes [16ff.], the employment of these same terms is clearly based on observation of the influences exerted by different kinds of music on the soul or behavior of the listeners, and on the ancient assumption that music could affect the human soul in a recognizable way. In particular, since it was believed that music acts at a physical level, it was supposed to cause tension or relaxation on the tendons and nerves of the human body and, thus, that these physiological conditions could determine at the psychological level corresponding emotional conditions and specific forms of behavior [4]. What is especially remarkable is that those forms of behavior were classified and referred to using exactly the

[10] For the meaning of ἔκλυσις, see De Simone 2004.

same categories and the same words that were technically applied to their proximate causes, the ἁρμονίαι, as well as to the musical tension and the slackening of the strings of musical instruments enabling the production of these ἁρμονίαι. In this connection, Rocconi states [17] that the capacity possessed by music of a particular type to make a listener 'tense' or 'relaxed' could be explained by taking into account physical rather than linguistic (metaphorical) factors, and shows that in Plato's hands this metaphorical terminology serves as a tool to develop 'principi metafisici ben più significativi' [20]. Indeed, we should still say that we are in the presence of *metaphors* by means of which physical and musical meanings are transferred to areas so far apart as the physiological, the psychological, and the behavioral. Indeed, it is precisely this extraordinary extension of meaning that impresses modern readers. For example, it is interesting to follow the semantic development of adjectives like χαλαρός, μαλακός and σύντονος and of the participle ἀνειμένος which in different contexts, from Plato on, designate the ethical powers of the ancient ἁρμονίαι and the behavior determined in those who were accustomed to listen to them, given that they originally refer to the slackening and tightening of strings producing this or that sound of those ἁρμονίαι [3–4, 16–21, 59]. Rocconi identifies traces of such lexical usage in Pratinas, a poet who was active in the early Classical Age [18].

The discussion of the different meanings of τόνος ('il derivato di τείνω che più ha avuto fortuna in lingua greca quale termine tecnico-musicale') is very well documented [21–25] and achieves good results, illustrating how in this case too the contribution of the theoretical literature to the systematization of technical terminology is fundamental.[11] As a guide for her account, Rocconi wisely chooses a passage from Cleonides, *Isagoge*:

> The term τόνος may have four different meanings: note, interval, vocal range, and pitch.
>
> Τόνος δὲ λέγεται τετραχῶς· καὶ γὰρ ὡς φθόγγος καὶ ὡς διάστημα καὶ ὡς τόπος φωνῆς καὶ ὡς τάσις. [von Jan 1895, 202.6–8]

[11] Rocconi's account should be integrated with the penetrating observations of Steinmayer 1985, 176–179.

This text is very valuable because it offers us a veritable catalogue of the different possible meanings of a technical musical term, and testifies that, in Cleonides' time (second/third century AD), there was the need to contain and systematize, to some extent, a polysemy which clearly existed before. It is also valuable because, to explain the first of the four meanings, Cleonides quotes two very important poetic fragments, one by Terpander [fr. 4 in Gostoli 1990, 51–52], and the other by Ion of Chios [fr. 5 in Gentili and Prato 1985, 67]. On the basis of the texts presented by Rocconi, I would add some of my personal observations. In both Terpander and Ion, we find the compound adjective ἑπτάτονος referring to a stringed instrument: the φόρμιγξ in Terpander, and the λύρα in Ion. Now, if a stringed instrument is qualified as ἑπτάτονος, this can only mean that it has seven strings (χορδαί)—which may confirm that the synonymy τόνος-φθόγγος should be also extended to χορδή, so that at least three different terms could be used to indicate the concept of 'musical note'. I add to the rich documentation provided by Rocconi, a gloss by Hesychius [ε 5558: ἑπτάτονος· ἑπτάχορδος in Latte 1966, 182], and, above all, the text by Strabo, who, in quoting the fragment of Terpander, speaks of a λύρα τετράχορδος which was commonly used before Terpander, and a λύρα ἑπτάχορδος which was introduced by Terpander himself, who designated it by means of the adjective ἑπτάτονος:

> Τέρπανδρον δὲ... γεγονέναι φασὶ... τὸν πρῶτον ἀντὶ τῆς τετραχόρδου λύρας ἑπταχόρδῳ χρησάμενον, καθάπερ καὶ ἐν τοῖς ἀναφερομένοις ἔπεσιν εἰς αὐτὸν λέγεται· σοὶ δ' ἡμεῖς τετράγηρυν ἀποστρέψαντες ἀοιδὴν ἑπτατόνῳ φόρμιγγι νέους κελαδήσομεν ὕμνους. [Strabo, *Geog.* 13.2.4]

To qualify the two types of instrument, Strabo uses two compound adjectives, τετράχορδος and ἑπτάχορδος, whose second parts (-χορδος) are to be connected to the noun χορδή. Now, if ἑπτάχορδος is to be considered as a synonym of ἑπτάτονος, this must mean that the second parts of both compound adjectives (namely, -χορδος and -τονος) are also synonyms. If not the synonymy τόνος-φθόγγος-χορδή, which is confirmed by the texts quoted by Rocconi [21–22nn87–90], all dating to the fifth century BC, at least the synonymy τόνος-χορδή is as old as Terpander (sixth century BC). It may also be observed that when the neutral substantivized adjective τὸ τετράχορδον is used in theoretical literature to designate a scalar unit formed by four contiguous notes spanning an interval of a perfect fourth, it is clear that the second part of the compound (-χορδος) has lost any link to its semantic provenance because it no longer refers to an instrument's strings but to the musical notes without regard for the instrument or voice producing them. It is a different situation from that of the compound adjective τετράχορδος, -ον, which refers to a stringed instrument, and whose second part still has a pragmatic value because it refers to the strings and not to the sounds.

It is not very easy to pinpoint the moment when each of the technical meanings of τόνος, as documented by Cleonides, began to be stabilized as such. As for one of them, namely 'interval of a tone', I agree with Rocconi's reasoning, except for the conclusion (probably affected by an awkward misprint): according to Rocconi, the meaning is implied by the term διάτονος = 'going on by tones', which appears for the first time, as far as we know, in the text preserved by P. Hibeh 13 and dated with some certainty to fifth/fourth century BC [see Avezzù 1994; Lapini 1994]. If this is so, the term διάτονος of the papyrus is the *terminus ante quem* (not *post quem* as Rocconi states [24]) for the meaning 'interval of a tone'.

According to Rocconi [22], the first occurrence of τόνος in a strictly musical sense, i.e., 'sound with a definite pitch', would be in Aristophanes, *Equites* 530 ff.,[12] where the term would have the same meaning that it is going to take in later times. Rocconi quotes Plato, *Resp.* 617b and Aristotle, *De an.* 424a 30ff as evidence for these developments. In my opinion, however, the meaning of τόνος is not the same in all the three passages, and I think it worth making some clarification.

The passage from Aristophanes has troubled interpreters both ancient and modern.[13] The poet, alluding to the poetic activity of Cratinus, presents it as a stringed instrument that is going into pieces. If this is correct the image seems to contain three very interesting details of a musical sort:

[12] This comedy was first staged in 424 BC.

[13] For the former, see the scholium to Aristophanes, *Eq.* 532a–c, 533a. The different opinions of the latter are explained in Imperio 2004, 203–207.

- ◦ the pegs are falling out—taking for granted, of course, that the term ἤλεκτρος in the expression ἐκπιπτουσῶν τῶν ἠλέκτρων (here unusually declined in the feminine) has to be given the meaning 'peg', which is controversial in that some scholars think it refers to other parts of the instrument;
- ◦ the instrument no longer has any τόνος (τοῦ τόνου οὐκέτ' ἐνόντος); and
- ◦ the joints of the instrument will not hold any more, or alternatively, the attunements (tunings) are totally impaired (τῶν ἁρμονιῶν διαχασκουσῶν).

Now, if the passage were about pegs, their fall from the instrument would make it impossible to produce any sound at all because the strings would not be under tension any more, a situation where tuning is irrelevant. It is clear, then, that in this passage the term τόνος cannot indicate, as Rocconi states, a particular sound with a certain pitch but must refer to the basic mechanical condition—the tension of the strings—which would make it possible to produce all the sounds of the instrument but which has now failed because the pegs have fallen out. In fact, τόνος does not appear to be a 'technical term' in the strict sense, or at least in the direction indicated by Rocconi. Rather, given this image of the pegs' falling out, we must think that Aristophanes wanted to communicate that Cratinus' poetry is completely ineffective.

In Plato, *Resp.* 617b, the second passage quoted by Rocconi, Socrates tells Glaucon the famous account that he heard from Er about the structure of the entire universe. He says that, according to Er, the universe is made up of eight concentric spheres revolving around the Ananke's spindle, that on the outside of each sphere a Siren, driven by circular motion, produces φωνὴν μίαν and ἕνα τόνον, and that from all eight Sirens there was the concord of a single ἁρμονία (ἐκ πασῶν δὲ ὀκτὼ οὐσῶν μίαν ἁρμονίαν συμφωνεῖν). The interpretation of the myth of Er is not easy, and this is not the place to discuss it in full.[14] Still, it is clear that there is an identity between φωνὴν μίαν and ἕνα τόνον, and that the term τόνος is employed to clarify, from a technical musical point of view, the meaning of φθόγγος. Now, to say that each single sound (presumably vocal: φωνή) produced by each of the Sirens *is* a single τόνος implies that the term is a synonym of

[14] See, for instance, Proclus, *In Plat. rem pub.* [Kroll 1899–1901, 2.237].

φθόγγος, i.e., ‘a sound with a definite pitch’ (or, in modern parlance, ‘a note’)—the same meaning that φθόγγος has in another passage by Cleonides, in which it is said that

> φθόγγος is the melodic incidence of musical sound on one pitch
>
> φθόγγος μὲν οὖν ἐστι φωνῆς πτῶσις ἐπιμελῆς ἐπὶ μίαν τάσιν. [von Jan 1895, 179.9–10]

In *Resp.* 617b, then, τόνος clearly has the meaning noted by Rocconi. Once again, the ‘proof’ that we are in the presence of a technical term is precisely the fact that it is not applied to the tension of strings but to the pitch of a sound produced by a different source—the Sirens’ voice (φωνή).

I do, however, have doubts about the meaning that Rocconi assigns to τόνος in the last passage that she quotes, Aristotle, *De an.* 424a30 ff. The text is concerned mainly with the limits of our sense organs’ capacities for perception. Aristotle’s general assumption is that when the power or intensity of the objects of sense-perception are excessive, they destroy the sensory organs [424a29–30 τῶν αἰσθητῶν αἱ ὑπερβολαὶ φθείρουσι τὰ αἰσθητήρια], that is, such excesses damage our perceptual capacities. Aristotle explains this as follows:

> ἐὰν γὰρ ᾖ ἰσχυροτέρα τοῦ αἰσθητηρίου ἡ κίνησις, λύεται ὁ λόγος—τοῦτο δ’ ἦν ἡ αἴσθησις.

In fact, the ἰσχυροτέρα κίνησις is here a practical manifestation of what Aristotle called earlier τῶν αἰσθητῶν αἱ ὑπερβολαί (the excesses of the objects of sense). Thus, we can say that, when the movement set up by an object is too strong for the organ, i.e., when the perceptual stimulus exceeds the organ’s capacity to perceive it, λόγος—that is to say, as Aristotle explains, perception itself—fails.

The example that follows illustrates this rule within the domain of auditory perception: ὥσπερ (*scil.* λύεται) καὶ ἡ συμφωνία καὶ ὁ τόνος κρουομένων σφόδρα τῶν χορδῶν. That is, when the strings of an instrument are struck strongly, συμφωνία and τόνος are no longer perceived. This example is not very easy to understand exactly (which should perhaps have persuaded Rocconi not to present it as a context in which τόνος would obviously be meant as a synonym of φθόγγος). But we know that in music the term συμφωνία always designates the concord between different sounds, and this implies that Aristotle had in mind the production and, thus, the perception of *more than one* *sound*. When two consonant sounds are produced by striking the two relevant strings too strongly, the perception of concord between them is impaired because an essential factor of both, namely, the tuning, fails. It is possible to observe this phenomenon on modern musical instruments; and modern acoustic physicists do allow that the perceived pitch is altered by the intensity of sound production. In other words, under these conditions, there is an interference between two factors of sound, the intensity (volume) and pitch [see, e.g., Frova 1999, 121–165]. Consequently, it seems to me that the two terms used by Aristotle do not refer to the *single sounds produced*, but to *two different factors of the auditory perception*, the concord *between* the sounds (συμφωνία) and the intonation of each of them, each being considered in its own right (τόνος). In fact, it is scarcely to be believed that if a string is struck too strongly, *the single sound produced by it* fails (λύεται), while it is much more plausible to think that the perception *of that sound's exact intonation* (the τόνος) would be altered. It is clear, then, that in this case too the meaning of τόνος is not 'sound of definite intonation' (the same, in Cleonides' terminology, as φθόγγος), as intended by Rocconi. Rather, its meaning is 'intonation' or 'pitch', namely the factor of sound that, employing again Cleonides' terminology, we should call τάσις. In Aristotle's example, the τόνος is the precise and specific pitch that the sound would have if it were produced without excessive force.

In sum, of the three passages quoted by Rocconi as examples of τόνος meaning φθόγγος, I think that the only one that is really relevant is *Resp.* 617b and that, given the evidence that she presents, we are not entitled to conclude that τόνος got this musical technical meaning before Plato.[15]

Rocconi [22] develops interesting observations on two compound adjectives in -τονος, namely, ὑπέρτονος and ὀξύτονος. In light of the passages that she cites from Aeschylus, Sophocles, and Aristophanes, it turns out clearly that in ὑπέρτονος the second element has none of the meanings indicated by Cleonides. Rather, it refers to the *volume* of the sound. As for ὀξύτονος, it seems that, according at least to the occurrences quoted by Rocconi in which the adjective qualifies the funeral song (θρῆνος) or lament (γόος), -τόνος might refer in some

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15 Steinmayer [1985, 176–179] is inclined to dating at least the technical meaning of τόνος = 'interval of a whole tone' sometime in the fifth century BC.

way to the (high) pitch of the voice. Nevertheless, it should be noted that in Greek of this time, the linguistic qualifications of sound refer to the subjective factors of perception rather than to the objective factors in its production. Thus, it does not seem feasible to consider them strictly as technical terms. In this regard, Rocconi's remarks are usefully supplemented by what she argues in chapter 2. I would offer here only a few observations.

Although ὀξύς may sometimes qualify the quickness of objects in motion, the meaning of the compound adjective ὀξύτονος, referring to the air (or wind) in Sophocles, *Phil.* 1093, is not unambiguous: it might mean 'quick', as maintained by Rocconi [22], or 'piercing', as explained by Liddell, Scott, and Jones 1996, *s.v.* Unlike Rocconi [22], I think that in Xenophon, *Cyn.* 6, 20 a clear distinction is made between two characters of sound: the *intensity or volume*, for which Xenophon employs the adjectives μέγας and μικρός, and the *pitch*, for which he employs ὀξύς and βαρύς. A clearer distinction of this same sort is made by Aristotle [see 22n96]. In the passage from Xenophon, the meaning of τόνος in τόνους τῆς φωνῆς seems to be 'sound' rather than 'intonation', as Rocconi [23] seems to understand. Again, I am not completely sure that the meaning of τόνος in Aeschines, *Ctes.* 209 refers to the sound's 'intensità o volume', as Rocconi assumes [23]. Rather, it should, I suspect, be referred to the voice's emotional character: Aeschines is in fact talking about Demosthenes' tears (δάκρυα) and τόνος τῆς φωνῆς, when he asks the Athenians, 'Where can I take refuge?' (ποῖ φύγω;), adding 'You have blocked all the roads, and there is no place where I can take refuge' (περιγράψατέ με· οὐκ ἔστιν ὅποι ἀναπτήσομαι). In this context, it seems more probable to read an allusion to the character (the *tone*) of Demosthenes' pleading voice than to its volume. In short, as I see it, the passages from Xenophon and Aeschines contain references to sound that are non-technical.

As for τάσις, Rocconi [25] very properly remarks that the term's purely musical meaning seems not to have been codified before Aristoxenus [*Harm. elem.* 12.1–4 ~Da Rios 1954, 17.2–4], who defines τάσις as μονή τις καὶ στάσις τῆς φωνῆς. It is obvious that this cannot mean that the concept itself of intonation did not exist before its terminological codification. Moreover, the passage of Cleonides quoted above shows that in the second/third century AD there did exist a synonymy between τόνος and τάσις, which implies that the concept of τάσις could also be expressed by the term τόνος. I would add that perhaps this synonymy could be traced back to the fourth century BC, when Plato used the term ὁμότονον (the neuter substantive formed from the compound adjective ὁμότονος) to designate the sameness of the pitch (*scil.* of two sounds) [see Plato, *Phil.* 17c4].

Also, within the semantic sphere of the *plucking* of strings with the fingers (ψάλλω and its cognates), it is not always easy to decide whether a given word is used in a technical or a non-technical sense. Nor is it always easy to pinpoint when a given word got a technical meaning and whether this meaning overshadows or even obliterates the common one. Despite these difficulties, the lexical analyses developed by Rocconi [26–32] are sensible and very useful in helping us to understand several technical details of musical performance, and to develop further hypotheses about some possible settings of the real practice of playing stringed instruments in Antiquity. As Rocconi reports [26], the verb ψάλλω originally defines the action of plucking a string of whatever kind (even, for example, that of a bow) and making it vibrate; the employment of the verb in musical contexts, namely, in reference to stringed instruments of the harp type, is documented from the sixth century BC on [see, e.g., Anacreon frr. 93, 96 in Gentili 1958, 65, 67]. In fact, the verb continues to be employed with its original meaning in literature of the late fifth century, as, for example, in Euripides, *Bacchae* 783–784, where it defines the action of plucking the bow's string. Further, there is a hint of a semantic development in Euripides' use of the term ψαλμός at *Ion* 173 [27n126], which again refers to a bow but in this instance to the *sound* produced by the vibration of its string in contrast to the sound produced by Apollo's φόρμιγξ.

According to the evidence we have, it seems that we may confidently conclude that, in reference to stringed instruments, the verb ψάλλω always indicates the action of plucking a string with the fingers and never of striking it by means of the πλῆκτρον. The same could be said for the original meanings of all the technical terms derived from the root ψαλ- [147]. More specifically, on the instruments of the harp family, it is absolutely certain that the sound was only produced by plucking the strings [27]. Nevertheless, as Rocconi persuasively argues, it is by no means certain that on the instruments of the lyre family the sound was only produced by striking the strings with the πλῆκτρον: the strings of these instruments were either struck or plucked and, on occasion, both the techniques were performed at the same time. For the Classical Age, plucking is documented in a series of texts [28];[16] and Rocconi is right to say that 'la circoscrizione di ψάλλειν alle sole arpe sembra comunque un fenomeno linguistico abbastanza recente e non univoco', and to suppose that the opposition within the group of the stringed instruments between being plucked (ἐπιψαλλόμενα) and being struck (κρουόμενα) probably appears no earlier than the Hellenistic Age. In this context are to be interpreted some interesting pieces of epigraphical evidence [27n126], related to two different musical specialities of the educational program in the middle of the Hellenistic Age in which young students competed: the κιθαρισμός that required use of the πλῆκτρον, and the ψαλμός that required plucking with the fingers. I would add that, since it is not known that a different instrument was used for each of the two specialities, it could well have been a single instrument on which both were allowed. Moreover, there are a number of occurrences of ψαλμός in the sense of 'sound produced by a stringed instrument', without any clear and technical reference to a particular instrument and/or a particular way of producing the sound [27n126]. Considering that all the texts adduced by Rocconi date to the second century AD [Plutarch, *Alex.* 67.5, *Pomp.* 24.5; Aretaeus, *De cur. acut. morb.* 1.1.5], I would note that the technical distinction between ἐπιψαλλόμενα and κρουόμενα already at work in theoretical texts of that period did not rule out the non-technical use of the word.

Rocconi discusses a series of texts by Plutarch in which the verb ψάλλειν defines the musical activity that takes place within sympotic contexts.[17] On the basis of her reasoning, she presents a sensible and

[16] Ion fr. 5 in Gentili and Prato 1985, 67: Ion employs the verb ψάλλω in reference to the λύρα. See also Herodotus, *Hist.* 1.155.4 and Plato, *Lys.* 209b, along with the scholium *ad loc.* [Greene 1938 458], where we are told of two different ways of performing on the λύρα. Rocconi's quotation of Dionysus of Halicarnassus *De comp. verb.* 25, which concerns the ability to play the cithara (οἱ κιθαρίζειν τε καὶ αὐλεῖν ἄκρως εἴδοτες) [8n133], does not seem relevant.

[17] Plutarch, *Per.* 1.6; *Pomp.* 36.4; *Arat.* 6.4; *An seni resp. ger.* 785f. [see 28–29]. In another series of Plutarchan texts [29]—*Quom. adul.* 67f, *De Alex. fort.* 1.334c, *Quaest. conv.* 2.634d, and *Reg. et imp. apophth.* 179b—we find a remarkable use of the term ψάλτης, which, according to Rocconi, designates 'the typical instrumentalist' engaged in sympotic contexts. Among the pas-

plausible hypothesis.[18] She argues that in such semi-private contexts, a very strong sound was unnecessary; thus, the accompaniment to song was performed by plucking the instrument's strings[19] and not by striking them with the plectrum. In addition, Rocconi notes that the occurrences of the verb ψάλλειν in sympotic contexts are as old as some texts of Anacreon [fr. 93 in Gentili 1958, 65] and Pindar [fr. 25 in Maehler 1989, 111: see 26, 27n123; Steinmayer 1985, 210–211], and that in the *symposia* from the Classical Age on there often appear female players of stringed instruments called ψάλτριαι [29–30]. A more detailed scrutiny of the iconographic evidence would, I expect, bring further confirmation of Rocconi's hypothesis.

From another point of view, we might suggest that ψάλλειν and its derivatives do not have very specific technical meanings but refer

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sages cited, the setting in a symposium is explicitly mentioned in *Reg. et imp. apophth.* 179b and *Quaest. conv.* 2.634d only; but it should of course be understood also in the other two, considering that in all four there is an account of the same episode in different argumentative contexts—Philip of Macedonia is elegantly silenced by a musician with whom he had tried to discuss technical questions.

18 Regarding the passages that she cites [28–29: for references, see n17 above], Rocconi states that 'when the verb ψάλλειν refers to the lyres, the context in which it is preferably employed is the symposium' [28]. But none of the sympotic texts that she cites mentions any musical instrument explicitly. Indeed, evidence that in sympotic contexts the stringed instruments which were prevalently used were those of the lyre family (λύρα, otherwise known as χέλυς, and βάρβιτος/βάρβιτον) comes from other literary sources and from copious iconography. It might, therefore, have been helpful if Rocconi had noted that in the Plutarchan passages the reference to instruments of that type is no more than implicit, even though it is probable.

19 Rocconi [29] recalls a part of a text which, in its entirety, seems problematic. In Plutarch, *Apophth. Lac.* 33.233f., we are told of a fine imposed by the Spartans on a musician who played his stringed instrument with his fingers: ψάλτης ἐπιδημήσαντα ἐζημίωσαν, ὅτι δακτύλοις κιθαρίζει. It is clear that the word ψάλτης here cannot be meant in a technical sense to designate a player of a stringed instrument of the harp family, whose strings were usually plucked. After all, why would he be fined for playing the instrument with his fingers, that is to say, by playing it exactly in the way it should be played? But if, as seems quite likely, the verb κιθαρίζειν means here 'to play the κιθάρα', then ψάλτης designates the player of that instrument (or else the singer who uses it to accompany his own song) who in this instance was fined because he played it in an unusual way, namely, without the πλῆκτρον.

generally to the action of playing stringed instruments (very likely, given the sympotic context, the lyre) almost always in accompaniment to the song but without any reference to a particular method of sound production. Moreover, the word ψάλτης is not always used as a technical term referring to a player of a stringed instrument whose strings were usually plucked; it also serves to define in general a stringed instrument player *tout court*, without any reference either to the instrument itself or to a specific way of sound production [see, e.g., P. Hibeh 1.13.col. I 7; col. 2.7–8]. In this sense, Rocconi offers a very useful contribution in recalling some interesting semantic developments, namely, ψαλμός = 'sound' [27],[20] which may be compared with κροῦμα = 'sound' [40], ψάλλειν = 'to sing', and ψάλτης = 'singer' [30–32]. In these cases too, the process from concrete to abstract is evidence that the words involved became real technical terms. The texts cited by Rocconi allow us to see how, from the Classical Age on, these words were not only connected to the sphere of instrumental sounds but also to that of the human voice.[21]

There was, however, a decisive semantic shift of ψάλλειν from the sphere of the instrumental sound to that of singing within the Christian tradition, a shift surely influenced by the *Septuagint* (third century BC), which uses ψαλμός to translate the Hebraic 'mizmor',

[20] Rocconi maintains [27n123] that in Pindar [fr. 125 in Maehler 1989, 111] the term πακτίς designates the βάρβιτος. But this seems incorrect: in Pindar's text, it is said that Terpander invented (εὗρεν πρῶτον) the βάρβιτος while listening to the sound (ψαλμὸν... ἀκούων) of the πακτίς. The passage is problematic in other details as well [see West 1997, 48]; but it is clear that each of the two terms indicates a different instrument and that the meaning of the term ψαλμός is specifically referred to the sound produced by plucking the strings of the πακτίς.

[21] See Ion fr. 22 in Snell and Kannicht 1971, 102; Aeschylus fr. 57.7 in Radt 1985, 179; pseudo-Euripides, *Rhes.* 360 ff. All these texts are recalled by Rocconi on pages 29 and 31. It is curious that, within a few pages, Rocconi provides two different interpretations of Herodotus' κιθαρίζειν τε καὶ ψάλλειν [*Hist.* 1.155.4]. In one instance, she interprets the phrase as as a hendiadys designating the act of playing the cithara *and* singing [31]; in another, as a linguistic evidence of two *different* ways of playing the instruments of the lyre family—κιθαρίζειν involving use of the πλῆκτρον and ψάλλειν involving the plucking of strings with the fingers [28].

which designates a hymn sung to the accompaniment of a stringed instrument whose strings were usually plucked [31n149]. From this moment on, within the Christian tradition, the term ψαλμός indicates specifically the chant even without instrumental accompaniment.[22] Yet, verbs like ἐπιψάλλειν and a noun like ἐπιψαλμός still remain confined to the sphere of instrumental sound [32].[23]

Pages 32–51 should be considered as the most complete account on the semantic sphere of κρου- in musical contexts. Words connected to κρου- were originally and prevalently employed in relation to stringed instruments; due to their semantic extension, we also find them used of wind instruments, and, in a very limited number of occurrences, of vocal sounds. Among the derivatives of κρούω, Rocconi dwells on ἀγκρούομαι [48–49], which means, technically, 'to play an instrumental prelude to the song'. But the verb appears to have more general meanings as well, such as 'to play, to perform (vocal or instrumental) music', or 'to begin (a musical piece)'. Furthermore, it should also be noted that, in Plutarch *Cleom.* 16.6 (a passage that Rocconi does not take into account), the verb has the different meaning 'to retune, to bring again to a proper pitch'.

In chapter 2, 'Percezione acustica e descrizione *metaforica* del suono presso i Greci' [53–80], Rocconi shows that an important part of the technical vocabulary of music originates from the vocabulary of acoustic perception. All the available evidence of the relevant ancient Greek theories is found in texts later than the archaic period: for earlier periods, we only have literary documents in which words refer to the perception of acoustic phenomena in quite a general way. Since ancient Greek, like all other languages, as we have seen, has no words specifically related to the sphere of auditory sensations, the vocabulary of this domain was developed by analogy, metaphor, or synaesthesia—what Rocconi rightly calls 'aggettivazione primordiale

[22] Note that in modern Greek ψάλτης means the singer who takes part in the liturgical services of the Orthodox Church.

[23] The occurrences of the verb ἐπιψάλλειν have either the general sense 'to play a stringed instrument' [Philo Judaeus, *Quod Deus sit immutabilis* 25 and perhaps also Sophocles fr. 60 in Radt 1977, 136: see 32n158] or the more specifically technical sense 'to accompany the song with a stringed instrument' [Plutarch, *Quaest. conv.* 713b; Philo Judaeus, *Somn.* 37]. The noun ἐπιψαλμός occurs in Ptolemy [Düring 1930, 67.7 ff.] and designates a specific instrumental technique.

squisitamente *soggettiva* o *psicologica*' [53]—using language originally employed to qualify other perceptions. Thus, the chapter is divided into four sections depending on the perceptual sphere involved:

- *Termini della sfera tattile* [54–69]
- *Termini della sfera visiva* [69–77]
- *Termini della sfera gustativa* and
- *Termini della sfera olfattiva* [79–80].

As usual, the discussion is very stimulating and rich in references: where not discussed in full, a number of texts are cited in footnotes.

In the earlier stages of the history of ancient Greek, acoustic perceptions were identified without making rigorous distinctions between the different features of sound: each of these features—pitch, volume, timbre, duration in time—were isolated and studied separately from one another only much later (in modern physics, of course). Thus, these features had no special denominations in ancient Greek for a long time. Numerous words belonging to the vocabulary of perceptions, words which would eventually become technical terms in this or that sense, were applied to sound in a very general and global way, each of them defining sometimes more than one feature at at a time.[24]

Rocconi notes that most archaic adjectives describing sounds treat them as '"corpi" fisici (ὥσπερ καὶ τὰ σώματα) o "grandezze" materiali (μεγέθη)' [54]. In this regard, among the texts that she cites [54n306], Philolaus fr. 6 [Diels and Kranz 1951, 409.10] seems to me irrelevant. In it the expression ἁρμονίας μέγεθος refers to the width of the interval of an octave, not to the 'dimension' of a single sound. Still, Rocconi wisely observes that a number of words primarily pertaining to the tactile sphere were employed in musical technical vocabulary to indicate specific qualities of sound in either of two possible ways, giving life to two different groups of terms:

[24] In fact, the clearest expression of the distinction in Antiquity between the pitch and intensity of sound is found, as far as we know, in Aristotle, *De gen. an.* 787a2ff (ἀλλ᾽ ἐπειδή ἐστιν ἕτερον τὸ βαρὺ καὶ τὸ ὀξὺ ἐν φωνῇ μεγαλοφωνίας καὶ μικροφωνίας). Granted, such a distinction in implied in Xenophon, *Cyn.* 6.20, but precise distinctions seem to occur only within strictly technical literature.

- pairs of antonyms used in common language were transposed as such into technical musical vocabulary while preserving their antonymic value, and
- pairs of words that were not antonyms in common language became antonyms in technical musical vocabulary.

The first group is certainly the largest; it includes such pairs as:

μέγας/μικρός	big/small
σκληρός/μαλακός	hard/soft
λεπτός/παχύς	thin/thick
ἀραιός/πυκνός	loose/compact and
τραχύς/λεῖος	harsh/smooth.

The second, very much smaller group includes the very important opposition ὀξύς/βαρύς (piercing/heavy), which served within the technical vocabulary of music to qualify sounds that are high/low in pitch. In its original sense, the antonym of ὀξύς is not βαρύς, but ἀμβλύς; and the antonym of βαρύς is κοῦφος. In fact, it is hard to imagine how any music theory, however primitive, could have come into being without the concepts of high and low pitch [Steinmayer 1985, 35–36] and, of course, without the relevant terms for them.

A number of these terms retain some polysemy in acoustic or musical contexts. Consider, for example, the meaning of μαλακός [59–61], an adjective used in a strictly technical sense only to qualify a variety of the diatonic genus (γένος διατονικὸν μαλακόν), while in some texts it qualifies either low pitched sounds (as a synonym of ἀνειμένος, in opposition to σύντονος), sounds of low intensity, or the ethically debauched character of some ἁρμονίαι that lead the listeners to types of behavior considered unethical. In this regard, Rocconi [61] rightly speaks of fluctuation in the meaning of μαλακός from the pragmatic to the perceptual spheres.[25] Furthermore, the antonym σκληρός seems to designate the timbre of sounds primarily. For, although the pragmatic sense of μαλακός points to the slackening of strings of an instrument as the reason for the low pitch, this is not the case for σκληρός: it does not point to any reason for high pitch

[25] In the qualifications of the ἁρμονίαι in Plato, *Resp.* 398e, it seems that μαλακός has a rather general than a strictly technical meaning, while χαλαρός is technical jargon: see Barker 2005, 25–27.

[62]. In addition, the pair ἰσχυρός/ἀσθενής (strong/weak), although it would seem appropriate only for referring to the intensity of sound, appears together with other adjectives that qualify timbre and pitch [61n356]. As for λεπτός, it is not always easy, even when the adjective occurs in technical texts, to identify precisely which character of sound, if any, it qualifies (pitch, intensity or timbre) or to decide when it simply refers to the sound's pleasantness in general.

Rocconi discusses a series of passages from poetic texts dating from the Homeric poems to the fifth century BC in which ὀξύς and/or βαρύς qualify sound in a quite general way. It is important to note that these very general meanings were the starting point for the development of the technical ones, which were intended to qualify with increasing precision the pitch of the sounds [56–57]. On the basis of the textual materials discussed by Rocconi, it would be appropriate to reflect that in Greek the adjective ὀξύς derives from the root **ak-* (which includes the notions of sharpness and hitting) and properly qualifies objects such as points capable of pricking or blades capable of cutting.[26] By extension, analogy, or synaesthesia, the adjective gained a number of other usages, e.g., to qualify the speed of objects in motion, a person's mental acuity, the impulsiveness or hastiness of actions or behavior, and especially one's subjective impressions and sensations (*via* sight, taste, smell, hearing) or the things that cause them.[27] If we observe the different occurrences in which ὀξύς refers to sound, we see that this adjective does not necessarily qualify only one of its features, namely, its pitch. Indeed, it may also refer to the capacity that the sound has to induce auditory sensations in the percipient subject similar to the tactile ones induced by sharp objects. A sound thus qualified as ὀξύς is perceived as affecting the hearing in the same way as a sharp object (for example, the tip of an arrow or needle) affects touch (analogy). From such usage, we see that sound is in this instance conceived as a body. Now, in my view, to be certain that, in a given context, such a qualification has a strictly technical musical value, we should also be sure that it exclusively (or at least prevalently) refers to the pitch of a sound: and this certainty

[26] See Chantraine 1999, *s.v.* ὀξύς, and words such as ἀκίς, ἄκρος, ὠκύς, ἀκμήν, *acer*, *acus*, *acies*, and so on.

[27] See Liddell, Scott, and Jones 1996, s.v. ὀξύς; Steinmayer 1985, 142–144.

is not always easy to get, especially because a high pitched sound is almost always penetrating in timbre as well.[28]

Steinmayer sketches the pathway to the technical development of ὀξύς in this way:

> The sounds called ὀξύς are of higher pitch relative to others, and... from constant use to describe higher-pitched sounds, the adjective developed a technical sense of 'high-pitched' which dropped the sense of sharpness... As in the case of βαρύς, it would be difficult to admit, in spite of the lack of attestations, that this technical sense did not exist in the fifth century, for it already exists in Plato, and must (or some such word serving the purpose of distinguishing high and low pitch) have been required by even the earliest musical theorists. [Steinmayer 1985, 143]

Indeed, ὀξύς and βαρύς appear as antonyms referring to the pitch of sounds, that is to say, as technical terms in some of Plato's dialogues which, even though they were written in the fourth century BC, were set in the fifth;[29] and the first occurrences of ὀξύς qualifying technically high pitched sounds are in two fragments of the Pythagorean philosophers Philolaus (*ca* 470–390 BC) and Archytas (*fl.* between 400 and *ca* 350 BC).[30] Moreover, according to Aristotle, it was Heraclitus

[28] Such a qualification occurs in modern languages too: e.g., in Italian, 'acuto', 'penetrante'; in English, 'sharp', 'piercing'; in French, 'aigu'; in German, 'scharf'; in Spanish, 'agudo'. Moreover, in Italian, the opposition 'acuto-grave' operates in exactly the same way as the opposition ὀξύς-βαρύς does in ancient Greek and has a strictly technical musical value, serving exclusively (or at least prevalently) to indicate the pitch of a sound.

[29] Rocconi [56n314] recalls Plato, *Symp.* 187a–b, *Phaedr.* 268d, *Phil.* 17c, *Crat.* 399b, *Tim.* 80a, as well as Xenophon, *Cyn.* 6.20. (In my view, the reference to *Cratylus* is not connected to the matter at hand, because in that Platonic context the couple ὀξύς/βαρύς does not concern musical sounds but the accent of the words.) Perhaps we should recall also Plato, *Leg.* 812d, a passage dealing with the ὀξύτης and βαρύτης of the sounds in a clearly technical sense with reference to their pitch, which Rocconi quotes in a different context [65].

[30] Archytas fr. 1 [Diels and Kranz 1951, 431–435] which mentions the utterance of strong and high-pitched vocal sounds, and Philolaus fr. 6 [Diels and Kranz 1951, 408–410], in which the expression δι' ὀξειᾶν appears. Rocconi cites the first [56n319] but not the second passage.

(*fl. ca* 500 BC) who developed observations about ὀξύ and βαρύ in reference to ἁρμονία,[31] and in this case too we ought to imagine that the words in question had technical meanings.

From Rocconi's argument [55], it seems to emerge that, as she sees it, unlike ὀξύς which was employed to qualify a sound that is perceived by the listener, βαρύς referred to the emotion felt *by those who produce the sound, and not by those who perceive it.* In the formulaic expression βαρὺ στενάχων (literally, 'groaning heavily') found in a number of Homeric poems [see 55n311], the adverbial neuter βαρύ would qualify

> la *pesantezza* del dolore (e del conseguente lamento) da un punto di vista soggettivo. Il gemito è 'grave' nel senso che opprime l'animo come un peso.[32]

The same argument is made in reference to Aeschylus, *Pers.* 571 (στένε καὶ δακνάζου, βαρὺ δ' ἀμβόασον). Yet again, it seems clear to me that in both cases the verbs στενάχω, ἀμβοάω) indicate two different ways of *producing* the sound, and that the adverbial neuter points to the way of *perceiving* the sound produced.

The difference in the meaning of the two verbs that Aeschylus uses ('groan' and 'cry') may, I expect, be of some importance from an expressive point of view; but Rocconi seems to understand both verbs as denoting the same action as that of uttering a 'lament' (γόος), an action referred to in Sophocles, *Elect.* 243 (ὀξυτόνων γόων) and in Euripides, *Phoen.* 883 (πικροὺς γόους). In fact, however, that βαρύς does not qualify the emotion felt by someone who consequently utters a sound but qualifies the sound produced itself is easily be seen in Homer, *Od.* 8.95 and 534 (both passages cited by Rocconi). In these passages, the finite verb (ἄκουσεν) in the formulaic expression

[31] Aristotle, *Eth. Eud.* 1235a25, a passage not cited by Rocconi.

[32] On the same line, according to Kaimio [1977, 40], in *Il.* 18.70–71 βαρὺ στεναχόντι, as opposed to ὀξὺ κωκύσασα, 'does not refer to a proper quality of sound at all but to the heaviness of Achilles' sorrow'. But I think that the verb στενάχω involves the *production* of a sound, and that βαρύ modifies the *sound produced.* Moreover, Kaimio interprets ὀξύς in such a way that excludes its qualifying the pitch only: granted, it is Thetis who cries (the verb used here is κωκύω) and her feminine voice is certainly higher in pitch than her son's; nevertheless, in this context it is not a matter of high pitched sounds but of loud ones.

βαρὺ δὲ στενάχοντος ἄκουσεν indicates someone's (Alcinous') listening to the lament uttered by someone else (Odysseus). This means that a 'heavy' sound, even if it is prompted by 'heavy' emotions, is in any case still a sound *produced* by someone; and syntactically speaking, βαρύς cannot, of course, qualify anything other than this sound. Moreover, in this sense, Aristotle, *De an.* 420a29 ff. [see 55n23] says that what is heavy (τὸ βαρύ), like what is sharp (τὸ ὀξύ), 'moves' (κινεῖ) the senses.

The discussion of the terms borrowed from the visual sphere (the only one that contains both adjectives and nouns) is the most convincing, perhaps because we have a clearer documentation. Rocconi [69] divides the topic into two different groups of terms: those pertaining to the sphere of color and light and closely connected to the description of sound as a body in a physical sense (with its qualities of form and color), and those that mostly indicate a surface or τόπος (*scil.* τῆς φωνῆς), διάστημα/σύστημα, ὅρος, πέρας, ἄκρος, χώρα, εἶδος, σχῆμα, διάγραμμα, ἀκαριαῖος, and so on. The terms that become properly technical are χρῶμα ('color'), which provides the root for the denomination of the γένος χρωματικόν, one of the three γένη of musical theory, and χρόα, which refers to the different varieties (literally 'shades') of the γένος χρωματικόν and of the γένος διατονικόν. It is remarkable that some of these terms (for example, λαμπρός) were also employed to define the incisiveness in the articulation of sound [71].

In her fascinating third chapter, 'Suoni animali e suoni musicali: gli epiteti *omomatopeici* e la formazione del lessico tecnico' [81–98], Rocconi provides a detailed examination of the very few words originating from onomatopoeia that were applied to sounds and music in Greek. She distinguishes [81] between words imitating the sound of a musical instrument[33] and words originally born as onomatopoeic representations of the sounds of nature and eventually transferred by metaphor into the vocabulary of music. The words in this second group derive from the verb τερετίζω and originally designate the swallow's shrieking or the cicada's chirping. Rocconi shows clearly that the only word which eventually becomes a real technical term is τερετισμός, word used in a number of cases as a synonym of αὔλημα but also applied to sound produced by the human singing voice, as

[33] Words such as τήνελλα created by Archilochus or θρεττανελό and τοφλαττόθρατ which appear in Aristophanes [see 81nn497–499].

well as by stringed instruments. The long history of this interesting technical development is traced convincingly, and the different technical meanings are usefully outlined in the glossary [144].

In conclusion, Eleonora Rocconi has produced a very useful tool: scholars who wish to make further inquiries in the lexicological field of ancient Greek music ought to start from her work and to take it into serious consideration.

BIBLIOGRAPHY

Abert, H. 1899. *Die Lehre vom Ethos in der griechischen Musik*. Leipzig.

Avezzù, G. 1994. 'Papyrus Hibeh I, 13: Anonymi fragmentum "De musica"'. *Musica e Storia* 2:109–137.

Barker, A. 1989. *Greek Musical Writings: II. Harmonic and Acoustic Theory*. Cambridge.

Barker, A. 2002. 'Word for Sounds'. Pp. 22–35 in C. J. Tuplin and T. E. Rihll edd. *Science and Mathematics in Ancient Greek Culture*. Oxford/New York.

________ 2005. *Psicomusicologia nella Grecia antica*. A. Meriani ed. Naples.

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

Chantraine, P. 1999. *Dictionnaire étimologique de la langue grecque. Histoire des mots*. Paris.

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

De Simone, M. 2004. 'Nota a Pherecr. fr. 155, 25 K.-A.'. Pp. 119–137 in S. M. Medaglia ed. *Miscellanea in ricordo di Raffaele Sodano*. Naples.

Diels, H. and Kranz, W. 1951. *Die Fragmente der Vorsokratiker*. 6th edn. vol. 1. Berlin.

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

Frova, A. 1999. *Fisica nella musica*. Bologna.

Gentili, B. 1958. *Anacreon*. Rome.

Gentili, B. and Prato, C. 1985. *Poetae elegiaci graeci.* vol. 2. Leipzig.

Gostoli, A. 1990. *Terpander.* Rome.

Greene, W. G. 1938. *Scholia platonica.* Haverford.

Huffman, C. A. 1993. *Philolaus of Croton.* Cambridge.

_________ 2005. *Archytas of Tarentum: Pythagorean, Philosopher and Mathematician King.* Cambridge.

Imperio, O. 2004. *Parabasi di Aristofane. Acarnesi, Cavalieri, Vespe, Uccelli.* Bari.

Kroll, W. 1899–1901. ed. *Procli Diadochi in Platonis rem publicam commentarii.* Leipzig.

Latte, K. 1966. *Hesychii Alexandrini lexicon.* vol. 2. Copenhagen.

Lapini, W. 1994. 'Ancora su Papyrus Hibeh I, 12'. *Musica e Storia* 2:139–170.

Kaimio, M. 1977. *Characterization of Sound in Early Greek Literature.* Helsinki.

Liddell, H. G.; Scott, R.; and Jones, H. S. 1996. *A Greek-English Lexicon with a Revised Supplement.* Oxford.

Maas, M. and Snyder, J. M. 1989. *Stringed Instruments of Ancient Greece.* New Haven/London.

Maehler, H. 1989. *Pindari carmina cum fragmentis.* vol. 2. Leipzig.

Meyer, B. 1932. *Ἁρμονία. Bedeutungsgeschichte des Wortes von Homer bis Aristoteles.* Zurich.

Michaelides, S. 1978. *Ancient Greek Music: An Encyclopaedia.* London.

Murray, P. and Wilson, P. 2004. edd. *Music and the Muses: The Culture of Mousike in the Classical Athenian City.* Oxford.

Pearson, L. 1990. *Aristoxenus,* Elementa Rhythmica*: The Fragment of Book II and the Additional Evidence for Aristoxenean Rhythmic Theory.* Oxford.

Radt, S. 1977. *Tragicorum graecorum fragmenta: 4. Sophocles.* Göttingen.

_________ 1985. *Tragicorum graecorum fragmenta: 3. Aeschylus.* Göttingen.

Raffa, M. 2005. rev. Murray and Wilson 2004. *Aestimatio* 2:108–118.

Rocconi, E. 1999. 'Terminologia dello spazio sonoro negli *Elementa Harmonica* di Aristosseno di Taranto'. *Quaderni Urbinati di Cultura Classica* ns 61(90):93–103.

________ 2003a. 'The Development of Vertical Orientation in the Spatial Representation of Sound'. Pp. 389–392 in E. Hickmann, A. D. Kilmer, and R. Eichmann edd. *The Archaeology of Early Sound: Origin and Organization.* Rahden/Westfalen.

________ 2003b. *Le parole delle Muse. La formazione del lessico tecnico musicale nella Grecia antica.* Rome.

________ 2004. 'Colours in Music: Metaphoric Musical Language in Greek Antiquity'. Pp. 29–34 in E. Hickmann and R. Eichmann edd. *Music-Archaeological Sources: Excavated Finds, Oral Transmission, Written Evidence.* Rahden/Westfalen.

Snell, B. and Kannicht, R. 1971. *Tragicorum graecorum fragmenta.* vol. 1. Göttingen.

Steinmayer, O. C. 1985. *A Glossary of Terms Referring to Music in Greek Literature before 400 B.C.* PhD Dissertation, Yale University.

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

Wehrli, F. 1967. *Die Schule des Aristoteles: 2. Aristoxenos.* 2nd edn. Basel/Stuttgart.

West, M. L. 1992. *Ancient Greek Music.* Oxford.

________ 1997. 'When is a Harp a Panpipe? The Meaning of πηκτίς'. *Classical Quarterly* 47:48–55.

________ 2005. rev. Rocconi 2003b. *Classical Review* 55:487–488.

Winnington-Ingram, R. P. 1963. *Aristidis Quintiliani de musica libri tres.* Leipzig.

Corrigenda

12n42 line 1	'destra', not 'sinistra'
17n61 lines 4–5	ἐπιτεινομένω, not ἐπιτεινομένῳ, and ἀνιεμένω, not ἀνιεμένῳ
23 line 2 (from bottom)	ὀξειᾶν, not ὀξεῖαν
24n103 (end)	ὀξειᾶν, not ὀξεῖαν
27n120 line 1	'una', not 'un'
27n121 line 2	'capaci', not 'capace'
27fn122 line 2	'Trendall', not 'Trenddell'
28 penultimate line	'36.4', not '36.3'
28 last line	ψήλασα, not ψήλαντα
29 lines 9–10 (from bottom)	'pizzicata', not 'pizzicato'
29n137 line 3	ἔκρουε, not ἔκρου
31 line 10	'una', not 'un'
32 line 17	ἐπιψάλλωνται, not ἐπιψάλλονται
35n185 line 3	ἐκρέκεσ', not ἔκρεκεσ'
37 line 11	Ἕλληνας, not Ἑλλήνας
38n201 line 1	'1132f', not '1132e'
41 lines 4–5	'vengono', not 'vengano'
41 line 5	τόνος, not τονός
47, second paragraph line 2	'sostantivo', not 'aggettivo'
48 line 1	'sostantivo', not 'aggettivo'
56n316 line 2	διαφορὰς, not διαφορᾶς
60 line 15	λεπτὰς, not ληπτὰς
63 line 5	λεπτή, not ληπτή
83 line 10	'una', not 'un'
87n540 last line	'uno', not 'una'
89 line 8	'Analytica', not 'Analitica'
90n553 line 1	'Neubecker', not 'Nenbecker'
90n554 line 1	'Filosofi', not 'Sofisti'
91n562 line 5	'preposizione', not 'proposizione'
92n567 line 2	ᾄδῃ, not ἄδη
93n569 line 6	'Analytica', not 'Analitica'; 'Wallis', not 'Wallies'
94 line 6 (from bottom)	'preposizione', not 'proposizione'
99 line 6 (from bottom)	'Benitz', not 'Benitez'
99 line 7 (from bottom)	'48, 1998', not '47, 1997'
100 lines 9 and 26	'Möllendorff', not 'Mollendorff'
100 line 10 (from bottom)	'Synaulia', not 'Synanlia'
100 line 3 (from bottom)	'Ciancaglini', not 'Ciancaglimi'

101 line 23	‘interpretatione’, not ‘interpretazione’
102 line 14 (from bottom)	‘Fernández’, not ‘Fernàndez’
105 line 7	‘schema’, not ‘shema’
105 line 11	‘traduzione’, not ‘tradizione’
106 line 18	‘Trendall’, not ‘Trenddell’

Anubio Reconsidered

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The Greek poet Anubio, who lived probably in the first century AD, was hitherto a rather shadowy figure in the history of ancient astrology. His poem was one of many ancient texts dealing with the alleged influences of the heavenly bodies on Earth, a product of that widely spread ancient view according to which astrology and astronomy were two indiscernible halves of the one and only astral science. There was no clear terminological distinction between these two parts,[1] and what we call 'astrology' was by many considered to be the practical application of the more theoretical sister science ('astronomy').[2] Important discoveries have now been made, and new insights gained, concerning one of these astrological manuals.

Obbink's new Teubner edition[3] of the fragments of the astrological poet Anubio grew out of his earlier edition [1999] of five papyri from Oxyrhynchus, namely, P. Oxy. 66.4503–4507.[4] These new fragments[5] substantially deepened our knowledge of the poem of Anubio and called for a collection of all its fragments. It is praiseworthy that the editor, an expert in papyrology but not in astrology, agreed to undertake this difficult task and to make his collection of all relevant

[1] See Hübner 1989. I owe some observations in this review to personal communications from W. Hübner. My borrowings from his review of the same work [2008] will be acknowledged in the notes.

[2] See, e.g., Ptolemy, *Tetr.* 1.1.1.

[3] Dirk Obbink. ed. *Anubio. Carmen astrologicum elegiacum.* Bibliotheca Scriptorum Graecorum et Romanorum Teubneriana. Munich/Leipzig: K. G. Saur, 2006. ISBN 978–3–598–71228–9. Pp. x + 79 (with 4 plates). €64.95, $91.00.

[4] For a detailed discussion of Anubio's life and times, his poem, its structure, its relation to Firmicus' *Mathesis*, its content, and its meter and versification, see Obbink 1999, 57–66. I agree on most, yet not all, detail of that otherwise very useful and informative discussion. The account of Gundel and Gundel 1966, 155–157, is largely obsolete and should be used with extreme caution.

[5] In Obbink 2006, they are F1 [4503 *recto*], F3 [4504], F4 [4503 *verso*], F5 [4505], and—among the *fragmenta incerta*—F19 [4506], F20 [4507].

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texts available within a few years after the first publication of the new papyri.[6] I have rarely found it so exciting to work through a new book. Despite various shortcomings that will be addressed in the following, this book has the potential to stimulate much subsequent research, as the length of the present review article indicates.

Obbink's edition is based on all relevant texts except for one important, recently published fragment [P. Gen. IV 157].[7] It contains nine testimonia and 14 fragments with a total of about 100 original verses. In addition, Obbink presents eight uncertain fragments [F15–F22]. Obbink 2006 is, therefore, much more than a simple reproduction of Obbink 1999. Its value is further increased by the facts that Obbink 1999 is no longer available in print, that the papyri are now presented in a double page layout[8]—the diplomatic transcript (left) facing the edited text (right), and that some details have been corrected or updated.[9] The volume is illustrated with four plates [F1, F3, F4, F5]. As usual in the Teubner series, the texts are presented without translations or commentaries. In the case of the new fragments from Oxyrhynchus, English translations and commentaries are available in the previous publication [Obbink 1999]. However, many of the texts collected in Obbink 2006 were never translated into any modern language. The expected readership is, then, experts in classical philology and/or in the history of the astral sciences in antiquity.[10] Therefore, detailed comments will be given below in the second part of this review article, regarding each single *testimonium*/fragment.

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6 Various other scholars—but no historian of astrology—made contributions: see the acknowledgements in the *praefatio* and in the *apparatus criticus*.

7 See Schubert 2009a and 2009b as well as Appendix 3, p. 178. In a few cases Obbink did not use all relevant passages of a text [e.g., F21]; more on this below.

8 Except for F19–F20, which are too badly preserved as to deserve such a layout.

9 There are, however, new typographical errors in Obbink 2006 which were absent in the original publication.

10 Note, however, that the astronomical and calendrical computations in the fragments are not numerous and of an elementary character [see esp. F2 and F16.1–7].

1. Anubio's place in the history of Greco-Roman astrology

First, however, I will offer a general survey in order to give the reader an idea of the philological methods that made this collection of more than 20 fragments possible despite the fact that only three of them bear explicit attributions to Anubio [F2, F7, F9].[11] This survey will lead to new insights concerning both the sources and the reception of Anubio.

It was W. Kroll who observed around 1900, while working with O. Skutsch on the second volume of their edition of the *Mathesis* of Firmicus Maternus,[12] that two Greek prose paraphrases, one explicitly derived from Anubio, one without attribution, both matched the content of *Math.* 6.3–27 so closely as to leave no doubt that all three texts went back to a common source, which Kroll identified with Anubio.[13] Soon after (this was overlooked by many, including Obbink) J. Heeg [1910a] argued convincingly that the paraphrase without attribution does not go back to Anubio but to Dorotheus of Sidon, author of a lost astrological poem in dactylic hexameters of which scattered Greek fragments and a complete (rather free) translation in Arabic are preserved.[14] Since these paraphrases will be mentioned frequently in the following, I shall avoid confusion by calling them consistently 'Par. Anub'. and 'Par. <Dor.>'.[15]

An important new step towards the edition that is here under review was the publication in 1950 of the astrological papyrus P. Schubart 15 (P. Berol. inv. 9587), since this publication led to S. Weinstock's discovery [1952, 211] that its elegiacs distichs 'are almost verbally translated by Firmicus Maternus, 6, 31, 78–85'. Chapters 6.29–31 of Firmicus' *Mathesis* contain a large collection of examples:

11 On F13, see p. 157.

12 Vol. 1 (1897) and vol. 2 (1913): repr. with addenda by K. Ziegler [see Kroll, Skutsch, and Ziegler 1968].

13 See Kroll's analysis in 1900, 159–160.

14 See Heeg 1910a. Kroll acknowledged the correctness of Heeg's argumentation in 1913 [see Kroll, Skutsch, and Ziegler 1968, 2.71]. Dorotheus was edited by Pingree [1976].

15 For full references to the available editions of these texts, see the bibliography below. As will be shown in the following, *Par. Anub.* is—despite its explicit attribution to Anubio—mostly derived from Dorotheus. Its short title will, therefore, be expanded later to 'Par. Anub. <et Dor.>'.

more precisely they contain typical alignments which were probably derived, at least partially, from the analysis of the charts of historical individuals and serve to illustrate and deepen the theoretical instruction concerning the effects of astrological aspects in the previous chapters 6.3–27. Since

- *Math.* 6.3–27 has a complete Greek equivalent in *Par. Anub.* and
- 6.29–31 has a partial Greek equivalent in the elegiac distichs of P. Schubart 15 and
- Anubio is the only known astrological poet to have written in elegiac distichs,[16]

it is reasonable to infer that all of *Math.* 6.3–31 goes back to Anubio. This assumption was further substantiated by Obbink's discovery that the new elegiac fragments F3–F5 from Oxyrhynchus almost verbally correspond to sections in *Math.* 6.29–31,[17] thereby forming a group with P. Schubart 15 [= F6].

This brilliant philological reconstruction done by several generations of scholars leaves no reasonable doubt that all Greek astrological texts in elegiac distichs that correspond with passages in *Math.* 6.3–31 derive from the lost poem of Anubio. Other astrological texts in elegiac distichs, which have no equivalent in *Math.* 6.3–31, are very likely to be of Anubio, too. Yet, these cases are not certain and need, therefore, to be listed as *fragmenta incerta*. This is the basic, convincing rationale that underlies Obbink's selection and arrangement of the fragments. In some cases, however, Obbink did not apply his own criteria rigorously enough or there are special circumstances that need to be taken into consideration. These cases, which will be discussed below, suggest a partial rearrangement of both the *testimonia* and the fragments.

Before we embark upon the discussion of single *testimonia* and fragments, one question of fundamental importance remains to be addressed: What is the actual source that Firmicus drew on in *Math.* 6.3–31? Is it

[16] Authors from late antiquity such as Hephaestio as well as authors from the Byzantine period speak of Anubio in a way that shows that he was the only elegiac astrological poet whom they knew of.

[17] F3 = 6.29.23–30.5; F4 = 6.30.6–7; F5 = 6.30.20–22.

(a) Anubio's original poem, or
(b) the preserved paraphrase *Par. Anub.*, or
(c) the poem of Dorotheus of Sidon?[18]

While all scholars so far either take one of these various possibilities or hesitate between (a) and (b), I do not find their arguments compelling. I wish to propose instead a hitherto unexplored alternative, namely, that all these authors (Anubio, Dorotheus, Firmicus, and also pseudo-Manetho) drew, independently from each other, on a common source, one that was authoritative enough to influence numerous successors. I will now outline briefly the main arguments for this view.

Firmicus never mentions the poet Anubio by name[19] and there is no evidence that he knew the elegiac poem at all. As Obbink and others have rightly observed, Firmicus treats his astrological topics in much more detail than the preserved corresponding passages of Anubio's poem do. This is usually explained as the result of textual expansions and changes either by Firmicus himself or by the author of *Par. Anub.* (if Firmicus drew on that) or by both of them.[20] But a close inspection of the material gives rise to serious doubts. For example, F4 b 7–9 says exactly the opposite of *Math.* 6.30.6.[21]

Let us take a closer look at F3. The whole hexameter F3 ii 4 has no equivalent in the corresponding passage *Math.* 6.29.23, while *Math.* 6.30.1 *et Sol sit in MC., Luna et horoscopo in Cancro constitutis* has no counterpart in F3 ii 15–16. The immediately preceding condition regarding Mars is less clearly defined in Anubio [F3 ii 14] than in Firmicus, and the following condition regarding Saturn's aspect to the Moon bears in each of the two texts a specification that cannot be found in the other one (μοῦνος, *pariter*). Interestingly, both these conditions are fulfilled perfectly in the chart of Oedipus, which forms the last part of *Math.* 6.30.1, so as to suggest that both Anubio and Firmicus drew in a selective manner on a common prose source which

[18] This is the view of Heeg [1910a] and Stegemann [1943].
[19] I agree on this with Boll [1909, 2371]. On T3, which must be rejected as a *testimonium* see p. 140.
[20] *Math.* 6.30.2, for example, has no counterpart in Anub. F3. The preceding paragraph [*Math.* 6.30.1] can be paralleled with F3 ii 10–18 and the following paragraph [*Math.* 6.30.3], with F3 ii 19–24.
[21] See Obbink 1999, 80 for an attempt to explain this.

already contained that horoscope as an example. Note that these idealized horoscopes at 6.30.1 (Oedipus), 6.30.11–12 (Paris), 6.30.22–26 (Demosthenes, Homer, Plato, Pindar, Archilochus, Archimedes), and 6.31.37 (Thersites) were absent from Anubio's poem, as F3 ii 10–18 [~ *Math.* 6.30.1] and F5 b [~ *Math.* 6.30.22] show, where Firmicus' final remarks that these were the horoscopes of Oedipus and Demosthenes, respectively, are missing. Moreover, it is very unlikely that Firmicus himself made them up (except, maybe, that of Archimedes, the most recent historical individual and the only one from Sicily, Firmicus' homeland). These ideal horoscopes look quite archaic in their simplicity, and it is noteworthy that the core of the *Corpus Manethonianum*, i.e., pseudo-Manetho 2/3/6,[22] which can be dated to the early second century thanks to the author's autobiographical horoscope [pseudo-Manetho 6[3].738–750],[23] also contains in the same book the horoscope of Oedipus [pseudo-Manetho 6[3].160–169]. If one examines the details, one finds that both authors, pseudo-Manetho as well as Firmicus, seem to have derived this horoscope from a common source, independently from each other.[24] This strongly indicates that Firmicus' ideal horoscopes in 6.30–31 are from the first century AD or even earlier. In order to conclude this part of the argument with regard to Anubio, it is important to keep in mind that Firmicus seems to have drawn not on Anubio, nor on paraphrases derived from Anubio, but on the same source as Anubio. Whoever prefers to stick

[22] These are books 1, 2, and 3 in the restored order in Koechly 1858.

[23] The alignment can be dated to AD 80 May 27/28.

[24] This is all the more obvious because also the context in both texts reveals striking parallels which, however, cannot be explained on the hypothesis that Firmicus used pseudo-Manetho. Compare, for example, the following passages that precede the horoscope of Oedipus in both texts:

pseudo-Manetho	Firmicus, *Math.*
6[3].151–153	6.29.20
6[3].154–159	6.29.22
6[3].180–184	6.29.24

and so forth. It would go beyond the scope of this article to compare both books systematically, but there is no doubt that pseudo-Manetho and Firmicus drew their examples from the same source.

to the commonly accepted view that Firmicus drew his material in book 6 from Anubio must, then,

- resort to the unlikely hypothesis that Firmicus regularly checked Anubio against Anubio's source (the 'common source'), because otherwise Firmicus would not have found the references to Oedipus, Demosthenes, and others, and
- deny the validity of the arguments that will be adduced later with regard to *Par. Anub.* [p. 134].

It is now time to take a closer look at Dorotheus. As has long been observed, the Arabic translation of Dorotheus (hereafter, Dor. Arab.) contains a long section [2.14–33] that corresponds so obviously with *Par. Anub.*(!) as to make Pingree [1976, 344–367] include *Par. Anub.* in his edition of the fragments of Dorotheus. Pingree [1976, 344] assumed that Anubio used Dorotheus and that the text of Anubio was then translated into Latin by Firmicus. But why should a poet find it attractive to rephrase in a closely related meter (elegiac distichs) astrological material that had already been versified in dactylic hexameters by Dorotheus? An additional, more compelling argument against Pingree's view is the following: as the new fragments F3, F4, F5, combined with P. Schubart 15 [F6], show, Anubio did the same as Firmicus, namely, after his exposition of general rules concerning the effects of the aspects [= *Math.* 6.3–27], he continued with the presentation of *specific* examples [= *Math.* 6.29–31].[25] Since these examples were (as the Arabic version shows) completely absent from Dorotheus' poem, Anubio cannot have drawn this material from Dorotheus. And since the general rules and the specific

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[25] Compare Firmicus' explicit remarks in the transitional chapter 6.28 which begins thus:

> [6.28.1] completis his omnibus [i.e., 6.3–27], antequam sermo noster ad horoscoporum transferatur exempla [i.e., 6.29–31], illud prudentiam tuam breviter admonemus etc.

and ends thus [6.28.2]:

> ut quicquid generali explicatione monstravimus [i.e., 6.3–27], specialiter rursus iunctis sententiis explicemus.

> [6.28.1] Now that we have finished all these discussions and before our work turns to the examples concerning the ascendant, we must briefly call to your attention that... [6.28.2] so that whatever we have described in general we shall show again in detail.

examples form a unit whose two parts logically follow upon each other, it is reasonable to assume that already in Anubio's and Firmicus' common source they formed a unit. Dorotheus arranged the material differently. After the exposition of general rules for aspects, he decided to fill the remaining part of his second book with other material from the common source, namely, the effects of the planets in the centers [2.21–27] and in each other's houses and terms [2.28–33]: this is material that Firmicus had already treated earlier, in his fifth book, and Anubio must also have treated it, as F22 shows.[26] Table 1 illustrates the correspondences, including also the core poem of the *Corpus Manethonianum*, i.e., pseudo-Manetho 2/3/6 [1/2/3]. The table is based on the order of the material in Firmicus, which must have been that of the common source because it logically proceeds from the isolated effects of single planets in certain places to the combined effects of two or more planets aspecting each other.

While Pingree wrongly thought that Anubio used Dorotheus, he wisely included *Par. Anub.* in his edition of the fragments of Dorotheus (this is the last important clarification to make here). For despite the explicit attribution to Anubio in the heading of the first chapter, the anonymous excerptor obviously also had at his disposal a copy of Dorotheus, whose name he mentions twice explicitly.[27] Analysis of this paraphrase shows that the scribe very soon after the start switched from Anubio to Dorotheus, and one gets the impression that he kept following Dorotheus until the end. Note, however, that the manuscript attribution of this paraphrase's chapters on aspects to Anubio is not just a scribal mistake or guesswork of a later copyist: in the same manuscript, the immediately preceding chapter contains literal quotations of elegiac distichs from Anubio [= F8]. Apparently the scribe really started the paraphrase [T8] from Anubio and switched, then, to Dorotheus.

This insight is important because it makes Table 1 more easily understandable and has the consequence that not only F10 (from *Par. <Dor.>*) but also F9 (from *Par. Anub.* [= T8], which will in the following be more appropriately called *Par. Anub. <et Dor.>*) must

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[26] On F22, see p. 169.

[27] T8.342 φησὶ γὰρ ὁ Δωρόθεος κτλ. = Pingree 1976, 355.6 and—beyond the section that Obbink included in his edition—361.19–20 φησὶ γὰρ καὶ Δωρόθεος κτλ.

Dor. Arab.	*Par. Anub.*	Anubio	pseudo-Manetho[a]	Firmicus, *Math.*	Subject[b]
2.20–27	(*)[c]	F22.3–4	3 [2] 8–226	Lost in the *lacuna* before 5.5	The seven 'planets' in the four centers (κεντροθεσίαι)
2.28–33	T8.411–54	F22.6–7 F22.14–15 F22.11–12	2 [1] 141–396	5.5–6[d]	The seven 'planets' in each other's houses and terms (τοπικαὶ διακρίσεις)
					Effects of:
2.14	T8.1–75	(F10.2)[e]	3 [2] 227–362	6.3–8	trine aspects
2.15	T8.76–207	——	3 [2] 227–362	6.9–14	square aspects
2.16	T8.208–302 + 305–307[f]	(F9),[g] F10.5[h]	3 [2] 227–362	6.15–20	oppositions
2.17	T8.302–304	——	——	6.21	sextile aspects
2.18–19	T8.308–410	(F10.1),[h] F14	2 [1] 397–485	6.22–27	conjunctions
——	——	F3–F6	6[3]	6.29–31	Typical charts

[a] Book numbers are given according to the mss tradition, followed by Koechly's restored order in parentheses [1858].

[b] The Greek key words in parentheses derive from Dorotheus: see Pingree 1976, 367.21–23 (quoted in full below).

[c] This is the final section in Pingree 1976, 361–367. Obbink rightly omits it as irrelevant to Anubio.

[d] Most of this long chapter is lost in the *lacuna* before 5.5; only the final section is preserved. See Kroll, Skutsch, and Ziegler 1968, 2.58 *app. crit.*

[e] *Re vera* from Dorotheus! See p. 154 on F10.

[f] T8.305–307 is an addendum (after the concluding remarks on sextile aspects) on the opposition of the luminaries. It equals Firmicus, *Math.* 6.18, but is missing in Dor. Arab. 2.16. Thus, the scribe was following Dorotheus here.

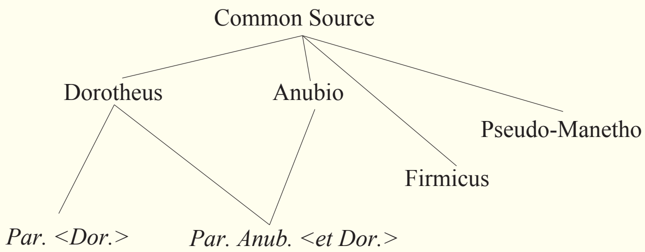
[g] *Re vera* from Dorotheus! See p. 153 on F9.

[h] *Re vera* from Dorotheus! See p. 154 on F10.

Table 1

be eliminated from the list of fragments of Anubio. This crucial point will be substantiated with detailed argument in Appendix 2 [p. 173].

Altogether, then, it is clear that this paraphrase, despite its initial attribution to Anubio, is almost entirely derived from Dorotheus. It seems most plausible to assume the following relationships between the authors in question:



Can the 'Common Source' be identified? Firmicus provides two clues for an answer. After his quotation from the chapters on κεντροθεσίαι and τοπικαὶ διακρίσεις, he assures Mavortius that he left out absolutely nothing of what 'the divine men of old' had put forth:

> haec tibi sunt omnia Mavorti decus nostrum specialiter intimata, nec a nobis aliquid est praetermissum, quod divini veteres et istius interpretes disciplinae prudentis sollertiae et docti sermonis studio protulerunt. [Firmicus, *Math.* 5.7.1]
>
> These matters have now all been explained to you in detail, my dear Mavortius, and nothing has been left out by me of what the divine men of old and the expounders of this discipline produced in their eagerness for prognostic expertise and learned discourse. [my trans. with borrowings from Bram 1975, 180]

He is probably referring to Nechepso and Petosiris, the major authorities of Hellenistic astrology.[28] The second clue is from the presence

[28] See also Firmicus, *Math.* 5.prooem. 6: *animus* [*scil. noster*] *divina inspiratione formatus totum conatus est quod didicerat explicare, ut quidquid divini veteres ex Aegyptiis adytis protulerunt, ad Tarpeiae rupis templa perferret.* Boll [1909, 2371] interprets this as 'einen deutlichen Hinweis auf die Ägypter, d.h. Nechepso-Petosiris'. See also *Math.* 8.5.1 *divini illi viri et sanctissimae religionis antistites, Petosiris et Nechepso.*

of that large collection of more than 100 typical charts preserved in Firmicus, *Math.* 6.29–31. The only prose collection of such examples from the time before Valens that we know of are the (now lost) παραδειγματικαὶ γενέσεις of 'the Egyptian authors' that Ptolemy mentions in *Tetr.* 1.21.18.[29] Ptolemy probably means Nechepso and Petosiris. Both clues hint, then, at the same source.[30] Even if certainty is impossible, it is very likely that all three poets, Anubio, Dorotheus, and pseudo-Manetho, versified extensive prose sections from the famous, authoritative manual of Nechepso and Petosiris, and that Firmicus translated them in books 5 and 6.[31] That would also explain why almost nothing of that 'bible of astrology'[32] is preserved in the original.

If Obbink and earlier scholars, starting with Riess,[33] are right with their dating of Anubio to the reign of Nero, which is the time of Dorotheus, both poets may have versified their common source more or less contemporaneously, independently from each other, in a period when astrology was especially *en vogue*, so much so that it gave rise both to versifications by poets wishing to satisfy the high demand of practitioners for summaries that could easily be learned by heart, and to such derisory texts by critics as the epigram of the

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29 Ptolemy mentions these exemplary horoscopes in the context of the Egyptian system of terms. In Firmicus' Latin adaptation, references to the astrological terms are admittedly rare: see, e.g., *Math.* 6.30.2 *in finibus Mercurii* and 6.30.6 *in finibus Veneris*.

30 Note that Firmicus moves on from *Math.* 5.7.1, where he mentions the *divini veteres*, to the immediately following sixth book without indicating a change of source.

31 Already Boll [1909, 2371] thought that the ultimate source of *Math.* 6.3–27 on aspects was the manual of Nechepso and Petosiris, and still earlier Kroll [1906, 62] had expressed his opinion that Valens' long chapter on aspects [*Anthol.* 2.17] went back to Nechepso and Petosiris: *ad Nechepsonem et Petosiridem haec redire haud dissimile est veri*. To my knowledge, however, no comprehensive view of Firmicus and the three astrological poets, like the one proposed here, has been put forth so far. Note that besides Valens, *Anthol.* 2.17, there is another prose treatise on aspects in papyrus PSI 158 [see Boll 1914, 5–10] whose internal order is, like that of *Anthol.* 2.17, confused; and it is unclear which relationship they have to the texts that are included in the stemma above.

32 Boll 1908, 106 = Boll 1950, 4 (*die Astrologenbibel*).

33 See Riess 1894, col. 2322, and Riess 1895, 186n1.

Neronian poet Lucillius (think also of the zodiacal dish in Petronius' *Cena Trimalchionis*).[34] As for the concise, poetical versions of authoritative yet endless manuals like that of Nechepso and Petosiris, Anubio's choice of the elegiac meter seems particularly happy because it combines the mnemotechnical advantage of an alternating meter with the somewhat more modest stylistic level of elegiac distichs which may seem more suitable to such versifications than the epic grandeur of stichic hexameters.[35]

2. Remarks on individual *testimonia* and fragments

The dating problem brings us to the second part of this review article, comments and observations on single *testimonia* and fragments of Obbink's edition.[36]

T1, T2, T9, F14 These all come from a collection now called the *pseudo-Clementines*, both the *Homilies* and the *Recognitions*. Within the *testimonia*, Obbink rightly separated T9, which deals with a specific astrological tenet, from T1 and T2, which are of general interest for the identity of Anubio. Pingree [1978, 2.422] saw no reason to identify the Anubio mentioned on numerous occasions in the *Pseudo-Clementines*[37] with the poet of the preserved astrological fragments, but that seems overly cautious to me. Several characters in the *Pseudo-Clementines* are based on such historical individuals as the apostle Peter, his (indirect) successor Clement of Rome, Simo Magus, and the Alexandrian scholar Apion against whom Josephus wrote his defense of Judaism, *Contra Apionem*. Why should the unknown author of the *Pseudo-Clementines* not have been inspired by the astrological work of Anubio to include the figure of a homonymous astrologer in his novel? This latter Anubio, whom Clement's

[34] *Anth. Pal.* 11.164 [= Riess 1891–1893, Test. 3] and Petronius, *Cena* 35.

[35] An additional reason for the choice of elegiac distichs may have been the existence of literary and funerary epigrams of astrological content that inspired Anubio to compose a larger poem in the same meter. See also Obbink 1999, 63–64.

[36] Note that it is not my intention to give a list of the numerous typos in the preface, in the *apparatus*, and in the quotations from Firmicus in this edition. Only typographical errors in the Greek main text will be mentioned.

[37] For a complete list, see Strecker 1989, 480.

father accepts as an authority, provides the Christian author with important opportunities to discuss and refute deterministic pagan beliefs that are irreconcilable with the Christian faith. As long as one duly emphasizes our lack of certainty, as Obbink [2006, iv] does, the inclusion of T1, T2, T9 and F14 in an edition of the astrological poet Anubio is justified.

Since the Anubio of the novel is introduced as a contemporary of the apostle Peter, Obbink follows a conjecture that was, to my knowledge, first published by Riess [1894] and followed by others, namely that the astrological poet lived under Emperor Nero [Obbink 1999, 60–62 and 2006, iv]. This is possible but not certain, and one can only hope that the authors of future encyclopedic articles will not simply present this narrow chronological frame as a matter of fact. It would be interesting to know when exactly the *Pseudo-Clementines* originated, and how well their author was informed about the poet Anubio. Interestingly, T9 [Rufinus, *Rec.* 10.9.4–7], which includes F14 [= *Rec.* 10.9.5],[38] is part of an important discussion between the protagonist Clement and his father on the value and truth of astrology, and a long part of this discussion [10.9.7–10.13.1] is preserved not only in the late Latin translation of Rufinus but also in a quotation by Origen (*ca* AD 185–253/4) from the lost Greek original.[39] This indicates that the whole passage from which T9 and F14 are derived originated no later than *ca* AD 200, right in the middle of those two centuries (the second and third) from which almost all the papyri in Obbink's edition are preserved. In this period, the poem of Anubio must have been quite successful and well known. This may explain the introduction of a certain Anubio as spokesman of astrology in the *Pseudo-Clementines*, and it is hard to believe that the Christian novelist openly distorted commonly known chronological and biographical data of the poet Anubio, if any such data were commonly known. They may of course have been fictitious data that the poet Anubio revealed about himself in his poem. Be this as it may, the reference to Anubio's provenance from Diospolis [T1.8–9 Ἀνουβίωνα τὸν Διοσπολίτην τινὰ ἀστρολόγον] must have been acceptable to those readers of the Greek original of the *Pseudo-Clementines* who

[38] On T9, see p. 144.

[39] Origen, *Philocal.* 23.21–22 (from Origen, *Comm. III in Gen.*). See the synoptic edition of Rehm 1965, 330–334.

were familiar with the poem of Anubio, and so it deserves our attention.[40] As to Anubio's date, the combined evidence of the papyri and the *Pseudo-Clementines* points to the second half of the first century AD or, at the latest, to the early second century AD.

T1 Correct T1.4 κατελήφει to κατειλήφει and T1.8 πρός μοι to πρὸς πατρός μοι.[41]

T2 Correct T2.3 'nuber' to 'nuper' and T2.5 'fortassis' to 'fortassis autem'.

T3 The inclusion of Firmicus, *Math.* 3.prooem.4–3.1.2 among the *testimonia* implies a problem that Obbink is aware of, as his circumspect discussion in 1999, 61–62 [cf. 2006, iii and n1] shows. Yet he does not draw the necessary consequences. The problem is: Does the name 'Hanubius' at T3.8 refer to the Egyptian god Anubis or to Anubio, author of our astrological poem? And in the latter case, is Anubio the real name of a historical individual (other such Anubios are attested with certainty) or a pseudonym referring to the god Anubis? T3 says that Nechepso and Petosiris (second/first century BC) followed the doctrine of Aesculapius and Hanubius regarding the horoscope of the world (*thema mundi*), which Hermes Trismegistus had revealed to them. Therefore, Aesculapius and Hanubius denote, strictly speaking, the gods Asclepius and Anubis from which the author(s) who wrote under the pseudonym of Nechepso and Petosiris claimed to have learned the secrets of the horoscope of the world. The only way to identify this Hanubius with our elegiac poet is to postulate that a very early astrological poet, whose real name may or may not have been Anubio, chose to write under the theophoric name Anubio as if he were the god Anubis, and that the author(s) who wrote under the pseudonym of Nechepso and Petosiris actually used that earlier poem as a source.

This hypothesis must be rejected for various reasons: from all that we know about the history of ancient astrological literature, it is unthinkable that our elegiac poem originated at such an early date.

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[40] According to Obbink [1999, 60], the city in question is Diospolis Magna, capital of the Theban nome in Upper Egypt, not Diospolis Parva in the Delta.

[41] I owe these observations to W. Hübner.

Instead, it must have been written at least one, probably two (or even three) centuries later than the manual attributed to Nechepso and Petosiris.[42] In addition, Obbink himself rightly points out that all references to Anubio in later sources [T1–2, T4–6] 'betray a view of him as a didactic technician, rather than a mythical bearer of revealed knowledge' [1999, 62].

And what about Aesculapius? We know of an early (lost) book *Myriogenesis* (not *Moirogenesis*) that circulated under the name of the god Asclepius [see below on T3.16], but are we to think that it contained the horoscope of the world just as the hypothetical early 'Anubio' did, and that it was used together with this early 'Anubio' as a source by Nechepso and Petosiris? Certainly not. The passage in Firmicus is much easier to explain on the assumption that the author hidden behind the pseudonym of Nechepso and Petosiris let his human protagonists, the King Nechepso and the Priest Petosiris, make a standard claim to revelation through divine authorities (in this case, Asclepius and Anubis) without actually drawing on any real texts under those names. Altogether, then, the Hanubius mentioned by Firmicus cannot be our astrological poet,[43] and T3 must be eliminated from the list of *testimonia*.

T3.16 Μοιρογένεσις is a conjecture of Claude Saumaise (1588–1653). I prefer to stick to the manuscript reading Μυριογένεσις. For a detailed discussion, see the commentary on Antigonus of Nicaea, F5 §§68–70 in Heilen 2011.

T5 In this quotation from Tzetzes, read (T5.3) Ῥητόριος instead of Ἑκτόριος. Between Πρωταγόρας (last word on page 3) and ἀποφαίνεσθαι (first word on page 4) two lines of text are missing. Supply

> Νικαεὺς Δωρόθεος καὶ λοιποί, ὧν τά τε ὀνόματα καὶ τὰς χρήσεις ἐπέφερον ἄν, εἰ μὴ φορτικός τε καὶ ἀλαζὼν καὶ μακρός τισιν ἔμελλον.
>
> . . . from Nikaia, Dorotheus and the remaining ones whose names and practices I would adduce, if I were not likely to be tiresome and boastful and tedious to some.

[42] Obbink basically agrees with this chronological relation, as his dating of Anubio to the time of Nero shows.

[43] Boll [1902, 141] and Heeg [1910a, 315–316] came to the same conclusion.

	Olivieri 1900a	Cumont 1921	Rhetorius 5.82[a]
F7	190.15–21 (also mentioned by Obbink: this is Rhetorius, *Epit.* 4.27.2[b])	208.2–8 (Obbink quotes from this source)	5.82.2 (unknown to Obbink; 'Anubio' is corrupted to σάρει)
T7	190.32–191.1 (Obbink quotes from this source; it is Rhet. *Epit.* 4.27.8–9[b])	208.18–24 (not mentioned by Obbink)	5.82.6–7 (unknown to Obbink; 'Anubio' is here suppressed: 5.82.6 φησὶ δέ τις τῶν σοφῶν)

[a] I am currently preparing the late David Pingree's edition of this compendium for publication.

[b] In Pingree 1977.

Table 2

In both cases, the entries in the *apparatus* call for correction too because the emendations Ῥητόριος and Νικαεύς are attributed to the codex Lipsiensis of Tzetzes (which actually reads Ἑκτόριος and Νικήρατος) rather than to the modern philologists Koechly and Pingree.

T6 The source indication should read 'Hephaestio ... 2.2.11'.

T7 This text is from a chapter Περὶ πράξεως καὶ ἐπιτηδεύματος ('On Profession and Business') attributed to Rhetorius of Egypt (early 7th century AD). It is quoted from one of the two preserved epitomes of this chapter (the original is lost). Correct T7.2 τία to τίνα and T7.5 ἐπιτροπον to ἐπίτροπον. Note that F7 is from the same chapter, but—as far as Obbink's quotation is concerned— not from the same branch of transmission. One of them, which is Rhetorius, *Epit.* 4.27 in the count of Pingree 1977, was edited by Olivieri [1900a] from codd. Marc. gr. 335 and Paris. gr. 2506; the other one is chapter 5.82 of the version of Rhetorius' compendium that is preserved in cod. Paris. 2425 [= Rhetorius, *Epit.* 3.82]. The two versions preserve the same chapter in slightly different wording. A conflated version of it, which never existed as such in the manuscript tradition, was edited by Cumont [1921] on the basis of all three mss [see Table 2, p. 142]. It is possible that the few lines between T7 and F7, which Obbink omitted, go back to Anubio as well.

T8 This anonymous prose paraphrase is by far the longest *testimonium* [Obbink 2006, 4–19]. It has been mentioned above [see p. 134]; and it will be proven in Appendix 2 [p. 173] that this paraphrase is, despite the explicit attribution to Anubio in the first chapter heading, mostly derived from Dorotheus. Nevertheless this text deserves inclusion in this edition as an indirect *testimonium* because both Anubio and Dorotheus drew on the same source [see the stemma on p. 136]. The metrical traces that this paraphrase contains are from Dorotheus and will be included in the collection of hitherto overlooked fragments of Dorotheus in Appendix 1 [p. 173].

This text allows for an interesting observation of how scribal habits can distort grammar and syntax. See, for example, T8.16–17

> ὁ Κρόνος τριγωνίζων Ἄρην, εἰ καὶ Ζεὺς μὴ ὁρᾷ μήτε ὁ Ἑρμῆς, εὔποροι γίνονται κτλ.
>
> if Saturn casts a trine aspect on Mars, even if Jupiter does not watch nor Mercury, then [the natives] become ingenious etc.

Correct Greek grammar would require a genitive absolute at the beginning, τοῦ Κρόνου τριγωνίζοντος Ἄρην. The reason for this and many similar odd constructions in the following is probably that the lost exemplar from which our preserved manuscripts (C and H) stem used symbols instead of full words for those stereotypical lists of conditions in the opening of each prediction (in the above example: ♄△♂).[44]

T8.53 Ἄρης Δία τριγωνίζων κτλ. is not a duplicate or variant of the discussion of trine aspects between Mars and Jupiter, which was given *suo loco* [T8.36–40], but about a trine aspect between Mars, Sun, and Jupiter, as the parallel passages in Firmicus *Math.* 6.5.2, Dor. Arab. 2.14.17 and *Par. <Dor.>* 383.28–30 clearly show. Hence, correction to Ἄρης <Ἥλιον καὶ> Δία τριγωνίζων (or the like) is needed, and the preceding line break must be deleted.

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44 The various planetary aspects are discussed in a clear order that goes back to the common source (Nechepso and Petosiris): first trine aspects, then squares, then oppositions, then conjunctions. Each section of this text is arranged according to the usual astrological sequence of the planets (Saturn, Jupiter, Mars, Sun, Venus, Mercury, Moon) and comprises 21 predictions (6+5+4+3+2+1): Saturn trine with Jupiter, Saturn trine with Mars, etc.; then: Jupiter trine with Mars, etc.; lastly, Mercury trine with the Moon.

In T8.169 εἰ μάλιστα ἢ ἀμφότεροί εἰσιν ὑπὲρ γῆν ἢ ὅμως ὁ Ἄρης looks suspicious:[45] one might expect ἢ μόνος (‘or alone’) instead of ἢ ὅμως (‘or at least’). While there seem to be no parallels for ἢ ὅμως in Greek literature, many can be adduced for the type ἀμφότεροι... ἢ μόνος.[46] The corresponding passages in Firmicus, *Math.* 6.11.8 (at the end) and Dor. Arab. 2.15.27 do not contain the specification in question. Therefore, it was probably absent from Dorotheus’ original and ἢ ὅμως may be a clumsy, contracted expression for ἤ, εἰ μὴ ἀμφότεροι, ὅμως κτλ. (‘or, if not both, at least ... ’).

T9 The reader does not learn on which grounds the passage from Rufinus [*Rec.* 10.9.4–7], which includes F14 [*Rec.* 10.9.5] is relevant to Anubio. The context as quoted in T9 does not mention Anubio’s name, nor does the wider context in the immediately surrounding chapters of the *Recognitions.* Nevertheless Obbink is probably right in drawing the reader’s attention to this passage. It would have been useful if he had started his quotation a bit earlier, from the important paragraph

> quia ergo cum eo mihi sermo est, qui in astrologiae disciplina eruditus est, secundum ipsam tecum agam, ut de his quae tibi in usu sunt accipiens rationem, citius adquiescas. [*Rec.* 10.9.1]

Clemens, the protagonist, is here talking to his father. Clemens announces that he plans to convince his father, who is knowledgeable in astrology, by following the rationale of that very discipline so that the father may acquiesce more promptly when presented with arguments drawn from those texts or tenets that he is familiar with. Clemens moves on to quote specific astrological tenets from ‘you’ (plural), the astrologers.[47] Who are these authorities with whom Clemens associates his father, who is not to be thought of as an author in his

45 In Obbink 2006 as well as in its source [see Pingree 1976, 349.32] and in the first edition by Olivieri [1900c, 208.27]. The respective *apparatus critici* do not mention the problem.

46 Cf., e.g., in the works of Galen: Kühn 1821–1833, 3.63.14–15 ὅταν μέγαν ὄγκον σώματος ἢ ἀμφοτέραις ὁμοῦ ταῖς χερσὶν ἢ μόνῃ τῇ ἑτέρᾳ περιλαμβάνωμεν, 12.848.8–9 ἐπ’ ἀμφοτέροις ἢ θατέρῳ μόνῳ συμβαίνῃ τις ὀδύνη, 15.602.8–9 καὶ γίνεται τοῦτο ποτὲ μὲν ἀμφοτέρων τεινομένων σπασμωδῶς ἢ τῆς ἑτέρας μόνης, and so on.

47 See, e.g., Rufinus, *Rec.* 10.9.2 *secundum vos*, 10.9.4 *dicitis*, 10.9.5 *ponitis... pronuntiatis*, 10.9.6 *dicitis.*

own right but as one of their followers? Since the father is in other passages characterized as a close friend and admirer of the astrologer Anubio [see esp. Rufinus, *Rec.* 10.52.3 = T2.4–6], Anubio is the only candidate to think of.

This may, at first sight, seem to be an over-interpretation of a generic reference to widely spread astrological tenets. But there is an additional argument in favor of the view that the Christian author is here referring specifically to the poet Anubio. There are two significant parallels (overlooked by Obbink)[48] in the sixth book of Firmicus, a book which is so important for the analysis of Anubio's fragments: Rufinus, *Rec.* 10.9.5 [= F14] corresponds to Firmicus, *Math.* 6.23.5 combined with 6.24.2. It would have been illuminating if Obbink had printed both Latin passages in the margin of F14 [compare the layout of F1, F3–6, and F16].

Since a main criterion for the order of Anubio's fragments in Obbink 2006 is the order of the corresponding passages in Firmicus, *Math.* 6.3–31, F14 should not be listed last of the *fragmenta loci incerti*, but between F2 and F3. That is, if Rufinus, *Rec.* 10.9.5 really were to be classified as a fragment. But since we are dealing with the Latin translation of a lost Greek novel, whose author, in his turn, seems to have drawn on original Greek verses of Anubio, the whole of Rufinus, *Rec.* 10.9.4–7 [T9], including 10.9.5 [F14], is a *testimonium*, not a fragment. It needs to be treated in the same way as T7 and T8 which equally report specific astrological tenets of Anubio in the form of prose paraphrases. The extraction of a fragment from the surrounding *testimonium* would be justified only if we had a real Greek verse, as is the case with T8.277 = F9.

This brings us to Obbink's modest presentation (in a smaller font) of his skillful attempt at restoring two Greek distichs from Rufinus' Latin translation. In the absence of any preserved word of the equivalent passage of the Greek original on which Rufinus drew, this restoration remains purely hypothetical. It does not justify the treatment of Rufinus, *Rec.* 10.9.5 as a fragment.

F1–F2 I should rather assign these fragments to the first book than to the third. For detailed discussion of this problem, see below on F5.

[48] For two similar cases, see pp. 153–154 on F9 and F10.

F1 The attribution of this text [P. Oxy. 66.4503 *recto*] to Anubio is secured, apart from the inconclusive arguments from elegiac meter and parallels with the second book of Firmicus [*Math.* 2.1.1, 2.4.1, 2.4.4–6], by the fact that on the back of the same papyrus is F4, which equals Firmicus, *Math.* 6.30.6–7 and falls, therefore, in the significant section *Math.* 6.3–31. It is extremely unlikely that astrological distichs on the two sides of one and the same papyrus be of different authors. While I agree with Obbink on the inclusion of F1 among the certain fragments, I cannot follow him regarding the book number: F1 must have been from the first book of Anubio, not from the third [see p. 148 on F5].

F1 is precious because it provides us with a much earlier attestation of a special doctrine that was hitherto known from Firmicus alone, the subdivision of the 36 *decani* into 108 *liturgi*. Probably both Anubio and Firmicus drew this basic information from the same source, which is likely to be again the 'common source' discussed earlier, Nechepso and Petosiris.

Note that in F1 ii 11–12 οὗτοι was removed from the position where it belongs and where the papyrus has it, at the end of the hexameter, to the beginning of the following pentameter. This mistake in Obbink 2006, 24–25 goes back to Obbink 1999, 70/73.

F2 This text concerns the determination of the ascendant at birth when the hour is not known.[49] In the fifth elegiac couplet [F2.9–10],

> χρὴ δὲ Σεληναίης προτέρης ἀνελέσθαι ἀριθμόν
> ὥρην νυκτερινὴν σκεπτόμενον θέματος.

> When examining the nocturnal ascendant of a chart, one must first take the number (of degrees) of the Moon.

I prefer the reading νυκτερινοῦ [cod. **P**] to νυκτερινήν [cod. **A**], which has been adopted by the editors so far [Cumont 1929a, 147.20; Pingree 1973, 90]. The methodological distinction in this passage is between the ascendant of either a day chart [F2.3 ἡμερινῇ γενέσει] or a night chart [F2.10 νυκτερινοῦ θέματος],[50] not between either the day ascendant or the night ascendant of a chart. The reading of cod. **P** creates a poetically preferable hyperbaton (which may have given rise to the

[49] See Bouché-Leclercq 1899, 389 and Feraboli 1981, 159.
[50] The terms γένεσις and θέμα are synonymous.

lectio facilior νυκτερινήν) and is supported by a poetical parallel in pseudo-Manetho 1[5].277–278:

> ἡνίκα δ᾽ ἡ Κερόεσσα μέσον πόλον ἀμφιβεβῶσα
> νυκτερινοῦ θέματος κατὰ μοῖραν ἰοῦσα φαανθῇ.[51]
>
> When the Moon, reaching the middle pole of a nocturnal horoscope, appears to go to the actual degree (of midheaven). [trans. Lopilato 1998, 197].

There is no parallel for the reading of cod. **A** in the required sense 'ascendant of a night chart'.

F3 This text makes the correct interpretation of a debated passage in Firmicus easier: the critical view of women's mysteries adopted in Firmicus, *Math.* 6.29.24 [*in nocturnis sacrorum vigiliis etc.*]

> provides no ground (as is sometimes alleged) for connecting the Firmicus Maternus of the *Mathesis* with the one who wrote *De errore profanarum religionum*, in part a Christian attack on the pagan mysteries. [Obbink 1999, 89]

because the same thought is already present in the corresponding passage, F3 ii 5 θιάσοις παννυχίσιν τ᾽ ὀλέσει.

F3 ii 7 κείμενος ὥσπερ ἔφην seems to confirm the correctness of a scholarly conjecture in Firmicus, *Math.* 6.29.24 * * * * * *ante collocatus*, where Kroll, Skutsch, and Ziegler [1968, 2.139.10] tentatively filled the lacuna with the words *effectus, et sit etiam ipse sic ut diximus.*

After F3 ii 20, the interpunction must be changed to a comma because F3 ii 21 is a relative clause referring to F3 ii 20 μοῖραν... τήνδε.

In F3 ii 23, Obbink reads ἢ δυτικῷ στείχωσι Κρόνος Κυθέρεια τ᾽ ἄποικοι. But the corresponding passage in Firmicus, *Math.* 6.30.3 *si... Venus uero et Saturnus in Capricorno uel Aquario pariter constituti et eundem partium numerum possidentes* makes it clear that Saturn is envisaged as being in one of his own houses with Venus at his side. Therefore, the last word, which in the diplomatic transcript [Obbink 2006, 26] reads ạποικο ̣ ('away from home'), was probably not the plural ἄποικοι but the singular ἄποικος referring to Venus

[51] Besides, there is one prose parallel in Olympiodorus: see Boer 1962, 49.9 εἰ μέντοι νυκτερινὸν ἦν τὸ θέμα.

alone. That also suits the prediction better: sterility (as opposed to Venus' proper domain, fertility). The opposite scenario is envisaged in F4.7–8: Venus together with Saturn in her own places, i.e., Saturn being away from home.

F5 In F5 b 4 after Ἠέλιος insert δ'.[52] The missing end of line scans – ˘ ˘ – –, not – ˘ ˘ – ˘ ˘ – –. The following lines F5 b 11–13 contain a numeral (Γ = 3, a book number) followed by two lines of text:

] Π̣ερὶ τοῦ δεσπότου̣
τοῦ τρί]τ̣ου θέματος

According to Obbink [1999, 101], this is the colophon to book 3 of Anubio's poem. The fact that the preserved lower margin of the papyrus [P. Oxy. 4505] follows right after θέματος seems to support this interpretation.[53] Note, however, that the preserved text of F5 (as well as the whole of Firmicus, *Math.* 6.3–31 with which F3–F6 present correspondences) contains nothing to which the words of F5 b 12–13 can be applied as a title.[54] I am not a papyrologist, but I do wonder if the words F5 b 12–13 (maybe also the numeral in F5 b 11) were not meant to be *prospective* but rather *retrospective*. Note that the line ends of this preserved column are missing. Therefore, we do not know if more columns of text followed and, if so, what their content was.[55] Another possibility that comes to mind is that the numeral in F5 b 12–13 denotes the book that is ending, as Obbink assumes, while F5 b 12–13 may be a catch-word referring to the next book in sequence or, more precisely, to the title on the parchment label attached to the outside of the next papyrus roll.[56]

[52] This letter is clearly visible on the photograph at the end of Obbink 2006 and correctly noted in both the *apparatus criticus* and in the diplomatic transcription.

[53] See the photograph at the end of Obbink 2006.

[54] Obbink himself saw this [1999, 101 on δεσπότου], although his explanation of δεσπότου as the 'ruling sign' is astrologically impossible.

[55] An additional, admittedly weak argument in favor of F5 b 12–13's being a book heading and not a colophon may be found in the presence of two indisputable prose headings that precede groups of elegiac distichs in F5 a 2 and F5 a 7.

[56] Obbink himself remarks [1999, 101] that 'often the book number follows the title in colophons, rather than preceding as here'.

Be this as it may, the editor's tentative restoration of F5 b 12–13[57] is inadmissible. I rather tend to assume that the missing part of both lines was blank and read:

] Π̣ερὶ τοῦ δεσπότου̣
] τ̣οῦ θέματος

This would mean 'On the ruler (i.e., the ruling planet) of the chart' and be equivalent to the more usual phrasing Περὶ τοῦ οἰκοδεσπότου τῆς γενέσεως [see, e.g., Firmicus, *Math.* 4.9 *De domino geniturae*]. Although there is no Greek parallel for ὁ δεσπότης τοῦ θέματος, this unusual terminology is easy to explain: οἰκοδεσπότης does not suit dactylic meter, nor does γενέσεως, unless one resorts to synizesis as Dorotheus did in writing καὶ γενέσεως τὰ ἕκαστα διίξομεν, ὄφρα δαείης [Dorotheus in Hephaestio, *Apotelesm.* 2.18.20 = Pingree 1976, 339.3]. Therefore, it is probable that in the lost lines of his poem to which F5 b 12–13 refer, Anubio spoke of the δεσπόζων θέματος. Both terms occur in other passages of Anubio's preserved fragments, e.g., F2.4 οἴκου δεσπόζων and F3 ii 2 δεσπόζοντα γάμου. The scribe who inserted F5 b 12–13 probably followed the terminology of the poem. For a similarly indented heading whose second line begins right below the first letter of the first line, see F15 i 25–26 [Obbink 2006, 47].

A thorough discussion of this problem also requires a closer study of the corresponding chapters in the *Mathesis* of Firmicus. F5 equals Firmicus, *Math.* 6.30.20–22. In his preface to book 6, Firmicus says that he plans to discuss the effects of the astrological aspects, which he actually does in the following chapters 6.3–27.[58] So far, there is nothing in book 6 that would justify the assumption that Firmicus' source, which was also Anubio's source, mentioned a 'ruler of the chart' (δεσπότης τοῦ θέματος). But this changes in the remaining part of book 6, which is devoted to a second large topic: time rulership. Framed by a brief transition [6.32] and concluding remarks [6.40], the discussion of the *dominus temporum* comprises

[57] In his *apparatus criticus*, Obbink]2006, 33] writes, 'τοῦ τρί]τ̣ου *vel* καθ' ἑκάσ̣τ̣ου, e.g., *supplevi*'. See also the English translation in Obbink 1999, 99: 'On the Ruling Sign of the Third (?) (i.e., type of?) Horoscope'.

[58] The intervening chapter 6.2 about the bright fixed stars is but a brief excursus meant to adorn the beginning of book 6. See Firmicus, *Math.* 6.1.10 *ut huius libri principia augustarum stellarum explicationibus adornentur.*

6.33–39. It is again based on some Greek source, as not otherwise to be expected from Firmicus and confirmed by the initial information that the Greek technical term for *dominus temporum* is χρονοκράτωρ [6.33.1]. Firmicus' decision to include this second part into book 6 accounts for a surprisingly long book (by far the longest in the *Mathesis*)[59] and may be seen as an indication that in writing 6.33–39 he kept following the same source as in 6.3–31, i.e., probably Nechepso and Petosiris. Note also the close structural resemblance between the two parts and their stereotypical underlying patterns. One may wonder if it is this 'time ruler' of the chart which the δεσπότης τοῦ θέματος announces. Since the term χρονοκράτωρ does not suit dactylic hexameters or elegiac distichs,[60] a poet could theoretically resort to a metrical expression such as δεσπόζοντα χρόνων θέματος, thus giving rise to the prose expression preserved in F5 b 12–13.[61]

In conclusion, the interpretation of lines F5 b 11–13 is uncertain and requires further discussion, especially with regard to the question whether F5 b 12–13 may be interpreted as a catchword.

Another point, however, is certain: Obbink is wrong in assigning F1–F5 *en bloc* to Anubio's third book [2006, 22 'Liber III']. It is just unthinkable that F1 belongs to any book but the first. Obbink rightly points out that there are clear correspondences between F1 and the second book of Firmicus. But a second book is still not a third; and, what is more important, even in Firmicus' case book 2 is, in a way, the true beginning of the *Mathesis* because the first book is just a hypertrophic introduction to the seven books of the compendium proper (seven in analogy with the number of planets known in antiquity).[62] Anubio wrote in a much more succinct style than Firmicus, as the preserved fragments of his poem show and the mnemonic purpose of versified astrological manuals demands. It is

59 This is the length of each of the eight books of the *Mathesis* in the edition of Kroll-Skutsch-Ziegler 1968: 1 (39 pp.), 2 (50 pp.), 3 (105 pp.), 4 (84 pp.), 5 (66 pp. with a very long *lacuna* in the mss tradition: see Kroll, Skutsch, and Ziegler 1968, 2.58 *ad loc.*), 6 (141 pp.!), 7 (73 pp.), 8 (81 pp.)

60 Only the oblique forms can theoretically be used by an astrological poet, but there is no preserved evidence of such practice.

61 There would be enough space left for χρόνων in the missing first half of line 13, but it is also possible that the scribe limited the expression somewhat vaguely to the δεσπότης (without χρόνων).

62 See Firmicus, *Math.* 8.33.1 and Hübner 1984, 143.

unthinkable that he filled an entire book (or even two) before coming to the elementary information that the number of the zodiacal signs is 12 [F1 a i 2]. F2 on the determination of the ascendant belongs probably to the same first book of Anubio.

Among the following books of his poem, F3–F6 very likely belonged to one and the same book because they form a unit, having their obvious equivalents in Firmicus, *Math.* 6.29–31. Thus, I disagree with Obbink who assigns F6 to a later book than F3–F5. His reason for doing so is the book end indicated in F5 b 11–13; but it is possible that F5, which preserves less than 10 of the original distichs, derives not from a complete copy of Anubio's poem but from a series of excerpts. The question remains whether F3–F6 are from Anubio's third book (which is, apart from F6, Obbink's view) or from the second.

If one takes into account the comparable poems of Dorotheus and pseudo-Manetho [see Table 1, p. 135], one finds that the latter presents the material that equals Firmicus, *Math.* 6.29–31 in what was originally the third book (now book 6 of the enlarged *Corpus Manethonianum*). This may be taken as an argument in favor of the assignment of F3–F6 to the third book of Anubio, and of the correctness of Obbink's interpretation of the numeral in F5 b 11. However, the evidence is inconclusive because Dorotheus managed to treat the same material with which pseudo-Manetho filled his first two books in the second half of his second book [cf. Dor. Arab. 2.14–33].[63]

In conclusion, F3–F6 must *en bloc* have been from either the second or, more likely, the third book of Anubio.

F6 This was probably part of the same book as F3–F5, not of a later book as Obbink assumes. For details, see pp. 148–150 on F5.

In F6 ii 32 Obbink's intention was apparently to print -ετερείη [cf. *apparatus criticus* '-ετερείη *scripsi*']; but in the text he actually kept οετει εἰ ἤ, the reading of Schubart [1950, 33]. In F6 ii 35b add another *breve* after μαινόμενος ⏑ ⏑ – ⏑.[64] The long quotation from Firmicus, *Math.* 6.31.78–85 is obscured by numerous typographical errors, omissions of words, and the inexplicable transposition of *constituti in occasu fuerint inuenti, et his tertius* from 6.31.83 to 6.31.82

[63] Dorotheus has no equivalent to Firmicus, *Math.* 6.29–31.

[64] I owe this observation to W. Hübner [see 127n1].

[Obbink 2006, 37]. One *locus similis* from Firmicus is missing: F6 ii 55–59 ~Firmicus, *Math.* 6.31.86. This is important because it shows that in the hexameter ὡρονό[μ]ον δ᾽ ὀλο[ὸς κατέχῃ Φαίνων Πυρόεις τε [F6 ii 56], whose second half was tentatively restored by Weinstock [1952, 214], κατέχῃ is probably wrong: Firmicus has ‘horoscopum vero Saturnus et Mars diversa radiatione respiciant’, which makes me rather think of κατίδῃ.

F7 This is the first among the *fragmenta loci incerti.* It is from the same chapter of Rhetorius as T7. The source from which Obbink quotes [Cumont 1921, 8.4.208.2–8] presents a version that was conflated by the editor and never existed as such in the manuscript tradition. However, in view of the complicated editorial problems connected to the compendium of Rhetorius [see above on T7, p. 142], Obbink’s choice is acceptable for the purpose of his edition. Note that τὸν before πρῶτον (F7.5) must be deleted. At the end of line 6 read ‘;’ (Greek question mark).

F8 The attribution of these anonymous excerpts to Anubio is very likely, not only because of the elegiac meter but also, as Obbink rightly emphasizes [1999, 57], because what follows right after F8 in the manuscript is the paraphrase T8, whose attribution to Anubio in the first chapter heading has been discussed above [see p. 134].

In F8b correct the unmetrical τὰ <πάντα> to <πάν>τα. Obbink apparently intended to adopt this emendation which was first proposed by Ludwich [1904, 119]. Ludwich’s τὰ [πάντα?] μέγιστα διδοῖ gave rise to a lapse.

Obbink commendably gives in a smaller font the prose context of F8d and F8e but he omits the context of F8a–c. Supply:

> F8a ὁ Κρόνος εἰς Ἀφροδίτην (*scil.* ἐπεμβὰς)...
> F8b ὁ Κρόνος εἰς Ἑρμῆν (*scil.* ἐπεμβὰς) ἢ νόσον ἢ θάνατον σημαίνει, ἀπὸ δ᾽...
> F8c ὁμοίως καὶ ἡ Ἀφροδίτη εἰς Ἄρην (*scil.* ἐπεμβᾶσα βλάπτει) πλὴν ἥττων ἡ βλάβη·....

Apart from the metrical elements of this text that Obbink included into F8, there are two more (admittedly, very small ones) which Olivieri, the first editor, printed in expanded font to draw attention to their metrical character: see Olivieri 1900b, 203.18 καὶ μάλα χαίρει, 203.19 οὐ πάνυ χαίρει.

F9 This is part of *Par. Anub. <et Dor.>*, i.e., T8 in Obbink 2006, and preserves two metrical fragments that are, as was shown above [p. 134], actually from Dorotheus. Nevertheless, they deserve some comment here.

F9.1 [= T8.264] βίος ἄρκιος ἔσ<σε>ται αὐτῷ: Ludwich's conjecture ἔσσεται for the mss reading ἔσται is certainly right. Compare, in the same source, T8.113 ἔσσεται, the only instance in T8 where the correct epic form has survived.

F9.4 [= T8.277] is a complete hexameter: ἤθεσιν ὁρμητήν τε καὶ οὐκ εἴκοντά περ ἄλλῳ.[65] *Par. <Dor.>* 382.1–2 contains the same passage in a prose version (ἤθεσι δ᾽ ὁρμητὴς καὶ ἄλλῳ τινὶ οὐκ εἴκων) which must go back to the metrical original that is preserved in F9.4. Compare also Dor. Arab. 2.16.20 'he will be one of those who relies on himself and will not obey another' [trans. Pingree 1976, 220].

Obbink does not mention that the two hexametrical fragments in F9 have parallels in Firmicus, *Math.* 6.3–31 [= F11]. F9.1 corresponds to Firmicus, *Math.* 6.16.5

> Habebunt tamen in quibusdam maxima felicitatis augmenta.
>
> Nevertheless, the natives will have a very big increase in good fortune in some cases.

and F9.4 corresponds to Firmicus, *Math.* 6.16.8

> Sed et omnia potentiae ornamenta decernit, et facit talem qui nunquam possit alienis potestatibus subiacere, et qui semper virtutis gratia et animi constantia alienis confidenter resistat potestatibus.
>
> But he [Jupiter] also attributes all the adornments of power and produces such a person that can never be subject to the power of others and that always with courage and steadfast character confidently resist other powers. [my trans. with borrowings from Bram 1975, 195]

Maybe Obbink omitted this information because his intention is not to adduce all parallels but only the most important ones as he states

[65] In the context [F9.2–3], change αὐξιφωτοῦσα to αὐξιφωτεῖ [= T8.275]. The discrepancy is due to the fact that in T8 Obbink quotes from Pingree's edition [1976] and in F9, from Olivieri's edition [1900c, 211].

[2006, 41 on F11 = Firmicus, *Math.* 6.3–31], 'ex quibus et aliis locis praecipue comparanda excerpsi et addidi iuxta fragmenta F3, F4, F5, F6, F16'.[66] However, it would, I think, be more consistent to indicate all correspondences between Firmicus, *Math.* 6.3–31 and the Greek fragments. This would also secure methodological consistency: while F9 and F10 are now listed among the *fragmenta loci incerti*, they would (if they were from Anubio) have, thanks to their equivalents in Firmicus, *Math.* 6.3–31, the same right as F3 and F4 to be among the *fragmenta* along with F5.

F10 This is from *Par.* <*Dor.*> and derives, therefore, from Dorotheus, not from Anubio [see above 129n14]. Nevertheless F10 deserves extensive comments here which will make the establishment of a supplement to Pingree's edition of the fragments of Dorotheus possible [see Appendix 1, p. 173].

W. Kroll [1900], the first editor of this paraphrase, noticed that the three metrical elements in F10 had parallels in the second half of Firmicus' *Mathesis* which was not yet critically edited at that time. These parallels are now, in vol. 2 of Kroll's and Skutsch's edition of the *Mathesis* [1968], Firmicus, *Math.* 6.23.7 *omnem fortunae substantiam cum maxima deiectione debilitat semper et minuit* [~F10.1], 6.4.4–5 *alios faciunt caelestium siderum secreta cognoscere* [~F10.2], and 6.17.4 *religiosa fidei commercia polluentes* [F10.5].[67]

Kroll further noticed that the same paraphrase contained several more elements that were, in his judgement, beyond doubt of poetic origin.[68] He had these elements printed in expanded character spacing. I shall present and discuss them in the order of the paraphrase, which is different from the order of the corresponding passages in Firmicus, *Math.* 6.3–27.

- Pingree 1986, 370.28 (on Saturn in conjunction with Mars): εἰ μὴ ἄρ' Αἰγίοχος δαμάσει σθένος ὀλοὸν αὐτῶν. This is obviously a dactylic hexameter, even if minimal changes are needed to restore the original.[69] Since the whole paragraph about Saturn in conjunc-

66 Add: F1.

67 On F10.5, see 191n b.

68 Kroll [1900, 159–160] says, 'hexametri apparent dictionisque epicae frustula manifestissima quae diductis litteris distinguenda curavi ita ut certa tantum respicerem.'

69 Note in the *apparatus criticus*: 'δαμάσῃ et οὐλοὸν fuit in versu'.

tion with Mars [Pingree 1986, 370.17–28] equals Firmicus, *Math.* 6.22.4–8, there can be no doubt that the Greek words quoted above have their Latin equivalent in *Math.* 6.22.8 *nisi Iuppiter... omnia malorum discrimina mitigarit.* A decade after Kroll had first published the Greek paraphrase [1900] in the erroneous belief that its source was Anubio, Heeg discovered that the verse in question here is a fragment from Dorotheus: in a Vatican codex edited by Heeg [1910b, 125.11], the verse is quoted as εἰ μὴ ἄρ Αἰγίοχος δαμάσει σθένος οὐλοὸν (*sic*) αὐτῶν with explicit attribution to Dorotheus. The importance of this hexametrical fragment for the attribution of the whole paraphrase in Kroll 1900, 159–180 [= Pingree 1986, 369–389], to Dorotheus has rightly been emphasized by Heeg [1910a]. Pingree [1976] included the verse in question at 369.6 of his authoritative edition of Dorotheus.

- Pingree 1986, 371.13 (on Saturn in conjunction with the Sun): βαρυδαίμονες ὄντες ~*Math.* 6.22.11 *erunt sane hi ipsi tristitia semper obscuri.*
- Pingree 1986, 371.20–21 (on Saturn in conjunction with Venus): ἀνάξια λέκτρα γυναικῶν δίδωσι ~*Math.* 6.22.12 *indignarum mulierum nuptias decernit.* The words ἀνάξια λέκτρα γυναικῶν seem to be the end of a dactylic hexameter.
- Pingree 1986, 374.4 (on Saturn opposite Mars): ἐκ μόχθων μόχθους ~*Math.* 6.15.5 *ex laboribus labores* and Dor. Arab. 2.16.3 'misery on top of misery'.
- Pingree 1986, 375.21–22 (on Saturn in square aspect with Mercury): αὐτοὺς δ' ἑτέροις προσώποις ὑποτεταγμένους... σημαίνει ~*Math.* 6.9.13 *facit etiam alienis semper potestatibus subiacere.* In the poetic original, the first words were probably αὐτοὺς δ' ἑτέροισι προσώποις.
- Pingree 1986, 380.29–30 (on Jupiter opposite Venus): ἕτερα μὲν λέγοντες ἕτερα δὲ βυσσοδομεύοντες ~*Math.* 6.16.4 *aliud malitiosa cogitatione tractantes et aliud ficta sermonis bonitate dicentes.* The singular (!) βυσσοδομεύων is a frequent hexameter ending in Homer and Hesiod.
- Pingree 1986, 382.1–2 ἤθεσι δ' ὁρμητὴς καὶ ἄλλῳ τινὶ οὐκ εἴκων is a prose version of F9.4 [see p. 153].
- Pingree 1986, 383.12 (on Mars in conjunction with Mercury): ψεύστας μέν, συνετοὺς δὲ καὶ πολλῶν ἴδριας κατ' ἐξοχήν ~*Math.* 6.24.5 *cordatos quidem et maximarum disciplinarum studiis eruditos, sed*

mendaces. The original ending of the hexameter may have contained the word πολυπείρους, as the corresponding passage in *Par. Anub. <et Dor.>* suggests: ψεύστας μέν, συνετοὺς δὲ καὶ πολυπείρους [T8.373 = Pingree 1976, 356.4].[70] In that case, more syllables between καὶ and πολυπείρους are lost (– ⏑ ⏑ –).

- Pingree 1986, 383.21 (on Mars in conjunction with the Moon): θερμόν τε καὶ οὐ δύστευκτον ἔθηκεν ~*Math.* 6.24.9 *faciet ista coniunctio homines calidos, et quos in omnibus prospere frequenter sequatur eventus.*
- Pingree 1986, 383.33–384.1 (on the Sun in square aspect with Mars): πταίσματα γὰρ πάμπολλα φέρει ~*Math.* 6.11.2 *infortuniorum cumulus <in>ponitur.*
- Pingree 1986, 384.6–8: see p. 170.
- Pingree 1986, 387.9 (on Venus in square aspect with Mercury): ἀστείους τέχνης εἰδήμονας ~*Math.* 6.13.1 *praeclara enim et amabilis cuiusdam artis officia.*
- Pingree 1986, 388.29–30 (on Mercury in conjunction with the Moon): μηχανικῆς πολύπειρος ~ *Math.* 6.27.2 *mendaces.*
- Pingree 1986, 389.7 (on Mercury in opposition to the Moon): αὐτοὺς δὲ δειλοὺς εἶναί φασι τῷ λόγῳ καὶ ἀθαρσεῖς ~*Math.* 6.20 *sed et animo et verbis eorum deiectam trepidationem timoris indicunt*, but it is unclear why Kroll highlighted these words as traces of a metrical original by using expanded character spacing.

F11 Firmicus, *Math.* 6.3–31 is not a fragment of the original poem but an indirect Latin *testimonium* that goes back to the same source that Anubio used. It would be appropriate to place F11 either before or after T8.

F12 and F13 The sources ought to be quoted as Hephaestio, *Epit.* 4.23.4 (lunar prognostication on which one of the parents will die first) and 4.21.4–7 (calculation of the ascendant sign). I do not understand why Obbink inverted Hephaestio's sequence of these passages, which goes back to Ptolemy (*Tetr.* 3.2 Περὶ σπορᾶς καὶ ἐκτροπῆς and 3.5 Περὶ γονέων) and implies a natural progression from consideration of the native *per se* to consideration of him/her within his/her

[70] For another occurrence of the adjective πολύπειρος in Dorotheus, see below on 179.13.

closest familiar environment. Besides, these texts, being prose paraphrases of original Greek distichs, ought to be placed among the *testimonia* just as the prose paraphrase T8 is (rightly) placed in that category.

F12 This fragment reports Anubio's predictions concerning the effects of the Moon in Pisces on which of the native's parents will die first. The critical parameters are the phases of the Moon and the astrological gender of the zodiacal signs. If Firmicus' long section on the effects of the planets in the various signs, which begins in *Math.* 5.3.1, were preserved in its entirety (it actually breaks off early at 5.4.25 with Jupiter in Capricorn), it would be worth checking his prediction for the Moon in Pisces in order to find out if the 'common source' contained yet another large chapter on which both Firmicus and Anubio drew. It is, however, more likely that Anubio was here following a chapter by an earlier authority that was based not on the order of the zodiacal signs but on the familiar relationships of the native, a chapter *On Which of the Parents Will Die First* like Firmicus, *Math.* 7.9 or Hephaestio, *Apotelesm.* 2.5. The latter chapter preserves an original verse of Dorotheus' discussion of the same topic, which was based on a different astrological method than the one recommended by Anubio and located in the first book of Dorotheus.[71] Based on this meager evidence, I tentatively assign F12 an early position in the list of *testimonia*, right after F13, which precedes F12 both at the level of content and in the order of the material in Hephaestio, *Epit.* 4.

F13 It has escaped Obbink's attention that this is a prose paraphrase of the distichs in F2:[72] Hephaestio, *Apotelesm.* 2.2.11–15 [= F2] ~Hephaestio, *Epit.* 4.21.4–5 [= F13.1–6]. The remainder of F13, i.e., Hephaestio, *Epit.* 4.21.6–7 [= F13.6–12] ~*Apotelesm.* 2.2.16–17 is not included by Obbink in his edition.[73] Note that the author of the fourth epitome wrongly speaks throughout his whole chapter 21 and especially in the section on Anubio [4.21.4–5] of the ascendant at conception, while Anubio and Hephaestio actually meant the ascendant

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71 Hephaestio, *Apotelesm.* 2.5.3 καὶ γενέτην ὀλέκουσι παροίτερον ἠὲ τεκοῦσαν. Cf. Pingree 1976, 332–333 and Dor. Arab. 1.15.

72 This editorial mistake has been observed independently, and earlier, by W. Hübner [see 127n1].

73 See the concordance in Pingree 1973–1974, 2.352.

at birth. On the epitomizer's motive for doing so, see Feraboli 1981, 160.

F14 See above on T9, p. 144.

F15 This is P. Oxy. 3.464, the first among the *fragmenta incerta*. Obbink's criterion for this group is the presence of elegiac distichs of astrological content that bear no attribution to Anubio nor have a parallel in Firmicus, *Math.* 6.3–31. Apart from one case [F22], I agree with Obbink on which fragments ought to be included in this group.

F15 contains mixed predictions (mostly about children, childbirth, number of children, and their chances to survive) that are each preceded by a short prose heading. One gets the impression that in the process of excerpting tenets that he found interesting, the author of P. Oxy. 3.464 did not always respect the original wording of his source. This is evident in the case of F15 i 5–6:

> ε]ἰ δὲ Κρόν[ος ἴδοι μ]ῄνην κ̣α̣ὶ [ὕ]ψ[οθεν ἑστώς,
> ἐ]κ δ̣ο̣υ̣λ̣ων δούλους τούσδε νοεῖ ξυ̣[νέσει.
>
> If, however, Saturn aspects the Moon, positioned above, know with your intelligence that these [natives] are slaves and from slaves. [my trans. based on Lopilato 1998, 199]

This distich is independently preserved in pseudo-Manetho 1[5].344–345 [= F21.85–86]:

> καὶ ταύτην τετράγωνος ἴδοι Κρόνος ὑψόθεν ἑστώς,
> ἐκ δούλων δούλους τῇδε νόει ξυνέσει.
>
> [If ...] and Saturn aspects it [Venus] from quartile, positioned above, know with your intelligence that these [natives] are slaves and from slaves. [trans. Lopilato 1998, 199][74]

Deplorably, there are no cross references between these two passages in Obbink 2006, neither in the *apparatus* nor in the *subsidia interpretationis* [2006, 67]. The version in F15 i 5–6 is meant to be complete, as is clear from its being preceded by an indented, almost entirely lost prose heading [F15 i 4 Ομ[...] and immediately followed by another

[74] Lopilato follows the manuscript reading τούσδε ('these [natives]'), not—as Obbink [2006, 63] does—Axt's and Rigler's conjecture τῇδε ('this [intelligence]').

such heading [F15 i 7]. However, F21.85–86 shows that the original source (probably Anubio) presented a more complex syntactical structure that comprised not one but two or more distichs: only the last of these was excerpted by the author of F15 who resorted to clumsy adjustments in order to make the distich syntactically independent. This accounts for the fact that the hexameter is so strangely fluffed in the papyrus [F15 i 5]. It is tempting to conjecture κατίδοι for the unmetrical ἴδοι,[75] but the lacuna is too short for that. Instead, ἴδοι fits perfectly. Apparently, the scribe of P. Oxy. 3.464 kept the simplex of the original [F21.85 ἴδοι] unchanged. He further omitted the original information on the kind of astrological aspect (square, τετράγωνος), replaced the pronoun ταύτην with the noun referred to (μήνην, the Moon), and connected the finite verb ἴδοι with the following participle ἑστώς[76] by means of a very inelegant (but metrically needed) καὶ. This is enough to get an idea of how poetically unskilled the scribe of P. Oxy. 3.464 was, and how freely he treated the original text. Nevertheless his testimony is precious in so far as it helps to determine with certainty to which planetary deity the pronoun ταύτην in pseudo-Manetho 1[5].344 = F21.86 refers (the Moon, not the other female deity, Venus) and to confirm that the manuscript reading τούσδε in the *codex unicus* (Laurentianus graecus 28.27) is correct. Koechly, who edited the Manethonian corpus long before the publication of P. Oxy. 3.464, wrongly adopted the conjecture τῇδε of Axt and Rigler. In the present edition, it would have been good to return to the manuscript reading τούσδε in F21.86 [Obbink 2006, 63], as Lopilato [1998, 36] actually does.

F16 The first editor Franz Boll [1914] interpreted this papyrus [PSI 3.157] as containing new fragments of the astrological poem of Manetho.[77] He also saw that three verses (3, 27, 39) are pentameters. This justifies their inclusion in Obbink's edition of Anubio (where verse

75 Cf. e.g., pseudo-Manetho 5[6].173–174: ἣν δὲ Σεληναίη ὕψωμ' ἀνιοῦσα σὺν Ἑρμῇ | αὐξιφαὴς κατίδοι κλυτὸν Ἥλιον κτλ.

76 ὕψοθεν ἑστώς, which Housman brilliantly restored in the papyrus from the only preserved letter (ψ) by way of comparison with pseudo-Manetho 1 [5].344, refers to the astrological concept of καθυπερτέρησις. Cf. the very similar prose expression in T8.111 ὁ Κρόνος Σελήνην τετραγωνίζων, τοῦ Κρόνου καθυπερτεροῦντος, κτλ. In Obbink's *apparatus criticus* [2006, 44], Housman's restoration is inadvertently recorded twice.

77 Boll 1914, 1 [No. 157]: 'Carminis astrologici Manethoniani fragmenta nova'.

27 needs to be indented). Boll also directed the reader's attention to parallel passages in the *Mathesis* of Firmicus. Obbink quotes these passages, which are not part of Firmicus, *Math.* 6.3–31 (hence the commendable inclusion of F16 among the uncertain fragments), *in margine*. Deplorably, there is no clear indication of which lines of the Greek text are their respective equivalents. This is unfortunate because the Latin quotations are generally printed several lines above the positions where they actually belong. Note that Firmicus, *Math.* 4.6.1 goes with F16.8–13, Firmicus, *Math.* 3.6.29 with F16.18–21,[78] and Firmicus, *Math.* 3.5.30 with F16.35–37. A fourth parallel is missing *suo loco* [51] but mentioned among the *subsidia interpretationis* [67]: F16.22–27 ~Firmicus, *Math.* 3.4.23.[79] This is the only case where one of the three Greek pentameters of F16 falls into one of the four parallel passages of Firmicus. In the Greek text of Obbink 2006, 51 and 53, correct verse 8 β̣[α]σιλήιδα to β̣[α]σιληΐδα,[80] verse 10 ὁρ[ί]ο[ς

[78] After 'semper' add the missing words 'Venus cum', and note that from 'quae fortiora' onwards the source is Firmicus, *Math.* 3.6.31.

[79] This entry is *s.v.* 'F17' (read 'F16'). The whole reference for verses 22–27 to Firmicus is a rather sloppy quotation from Boll 1914, 3 (without acknowledgement). The lines quoted as 'Firm. Mat. I 121,19' are part of the paragraph Firmicus, *Math.* 3.4.23. Instead of 'Venus et Iouis' read 'Venus aut Iouis' (this lapse is Boll's); instead of 'pereant' read 'depereant' (this lapse is Obbink's). The following words 'igitur Iouis testimonio sors eorum paulo melior fit' are not a continuation of Firmicus' text but Boll's comment on it. Therefore, they should be formatted in italics or put into quotation marks. My attention was drawn to this last sentence by W. Hübner, who acutely noticed that it is not likely to be a continuation of the text of Firmicus because ancient authors mostly use *igitur* in postposition, due to its origin from enclitic *agitur*. In this context it deserves to be mentioned that throughout Obbink 2006 the *apparatus criticus* below the Latin quotations from Firmicus would be more easily comprehensible if Obbink's own words were (as is customary in Latin editions) systematically italicized and thus clearly distinguished form the ancient Latin author's words. This kind of distinction is applied only to F4 [2006, 31]. Besides, the *lemmata* of the *apparatus* ought always to be preceded by the number of the paragraph to which they refer, as on page 24 (proper indication is missing on page 26 and elsewhere).

[80] Correct also the index in Obbink 2006, 70.

to ὀρ[ί]ο[ις, and verse 34 καταχεύει ('pours down') to κατατεύχει ('makes, renders').[81]

F17 P. Rylands 3.488 contains one badly damaged column of text. No more than roughly eight letters from the second half of each line are preserved; most line ends are broken off. The meter is probably elegiac[82] and the content astrological, but neither of these features is certain. Therefore, the most that can be admitted is inclusion among the *fragmenta incerta*.[83]

F18 In P. Schubart 16 (P. Berol. inv. 7508), one damaged column of astrological poetry is preserved. Line 11 is the only clearly discernible hexametrical line end. Lines 8, 12, 15, 19, and 21 can only be pentameters. Inclusion among the *fragmenta incerta* is plausible. Note the poet's personal remark in F18.16 ἐγὼ ὁδὸν ἡγεμον[εύω (or ἡγεμον[εύσω), to which Schubart [1950, 37] first drew attention.

F19–F20 P. Oxy. 66.4506–4507 contain traces of elegiac distichs in the preserved line-ends of F19 a, F19 b, and F20 b 2–3. F19 and F20 both contain traces of astrological terminology. Inclusion among the *fragmenta incerta* is plausible.

F21 This fragment is from the first book of the *Corpus Manethonianum*.[84] To discuss this fragment comprehensibly requires some preliminary information. The six books of dactylic hexameters attributed to 'Manetho' are composed of various elements taken from different sources and composed at different times. They fall into three groups that are usually quoted with the book number in the *codex unicus* first, followed in square brackets by the restored order of Koechly 1858.[85] The earliest element, which was also called the 'core' earlier in this review, comprises books 2[1], 3[2], and 6[3]; book

81 These are lapses. Obbink did not mean to change the text as established by Boll 1914.

82 See esp. line 9, ending in -τυχίη (with a blank line following): this seems to be a pentameter, as was correctly noted by the first editor Roberts [1938, 102].

83 The line number '5' ought to be printed one line below its current position.

84 The numerals '84–99' in the source indication 'Manetho, Apotelesm. A [E], 84–99 (Koechly)' [Obbink 2006, 61 and 66] refer to the page numbers in Koechly 1858.

85 Koechly's rearrangement of the book sequence was criticized by many.

4 is by a later author, and books 1[5] and 5[6] form still another unit of uncertain date. Hence, F21 is *not* from the core poem by pseudo-Manetho that was included in the stemma on p. 136. The whole corpus was re-edited by Lopilato [1998] in a doctoral thesis directed by the late David Pingree. It is deplorable that this edition, which also provides a full English translation and commentary, remains unpublished and is available only on UMI Microform 9830484. (In any case, this edition has escaped Obbink's attention).[86]

It has been observed more than a century ago that some 20 elegiac distichs are interspersed in the dactylic hexameters of the first (fifth) book, and that they are likely to derive from Anubio because he is the only ancient author known to have written elegiac distichs of astrological content [see Kroll 1898, 131–132; Usener 1900, 335–337]. Obbink rightly included these verses in his edition among the *fragmenta incerta*. His method, however, is unclear. He starts quoting the first 57 lines from book 1[5] in their entirety (in a small font), although in this portion only lines 37–38 (an elegiac couplet, printed in the larger, regular font) are relevant to Anubio. After line 57, which is an arbitrary dividing line, Obbink stops quoting the context and presents the reader only with the elegiac couplets to be found in the remaining part of the same book. For various reasons, he should have done this from the beginning: lines 1–57 do not contain a unit of content but a proem [1–15] followed abruptly by a series of short, poetically as well as astrologically unconnected prognostications. Some of them are of such a low quality as to deserve (in Koechly's opinion) cruces at the beginning of each line (verses 16–17 and 38–41), a peculiar use of this diacritical sign that is normally used to denote textual corruptions.[87] The reader who is interested in Anubio would not miss anything if the long quotation from pseudo-Manetho 1[5].1–57 were reduced to 1[5].37–38. And Obbink ought to have made it clear that the first of these two lines, the dactylic hexameter, is a conjecture by Koechly that cannot be found in the manuscript tradition. Therefore, Koechly prints it in a smaller font and does not

[86] For book 1[5], see Lopilato 1998, 263–275 (Engl. trans.), 394–425 (comm.).

[87] Obbink follows Koechly's special use of these cruces without explanation. See Koechly 1858, vii 'praefixis crucibus ineptissima quaeque notavi'.

include it in his line count.[88] This seems to have escaped Obbink's attention. As a consequence, Koechly's line count in parenthesis on the right side of Obbink 2006, 62 is, from '(40)' to '(55)', indicated one line above the position where it actually belongs.

In F21.20, Obbink, who generally follows the text of pseudo-Manetho as established by Koechly 1858, returns here to the reading ἠδὲ λαφύροις [Koechly 1851] instead of ἤθεα φαύλαις [Koechly 1858]. Note that Lopilato [1998] prints ἧστε λάφυρα.

In F21.42, Obbink prints μοῖραν δ' οὐκ ἐκφεύγουσι, attributing this in the *apparatus criticus* to Koechly: I assume that he means the edition of 1851 (which I have not seen) because the revised *editio minor* [Koechly 1858] reads μόρον αἰνὸν ὑπ' ἐμφαίνουσι. Note that Lopilato [1998, 25] conjectures μόρον αἰνὸν <δ>' οὐκ φεύγουσι.

F21.61–62 are verses 89 and 91 (not 90–91) in Koechly 1858.

In F21.63/67/69, the small font is a faithful reproduction of Koechly's layout; it means that each of these three lines is based on conjecture and is not to be found in the manuscript tradition. In Obbink 2006 it is not made clear that the use of a small font for these three dactylic hexameters is different from the one in F21.1–58 where it was reserved to providing authentic hexametrical context without giving it too much prominence. The potential confusion grows still wider when Obbink uses the small font for a hexametrical line [F21.91] which is neither a conjecture of Koechly nor clearly identifiable as part of a stichic hexametrical context.

F21.79 δεκτεῖρα κακῶν would mean 'receiver of evil' (the Moon), a sense opposite to what the context demands ('evildoer'). Correct the unattested noun δεκτεῖρα to ῥεκτεῖρα, the reading of the *codex unicus* [ms **M**], Koechly, and Lopilato. Apparently δεκτεῖρα is a lapse due to the similar shapes of δ and ῥ.

In F21.83 δούλους ποιήσει καὶ γονέων στερέσει, although καὶ (second hand in **M**) is preferable to ἢ (first hand in **M**) for metrical reasons, Lopilato [1998, 36] is probably right in assuming hiatus and printing ἢ. The question is complicated by the fact that Byzantine scribes frequently confuse ἢ ('or') and καὶ ('and'). Note, however,

[88] See Koechly 1858, vii 'quae a me probabili coniectura suppleta videbantur minoribus literis exprimenda curavi'.

that apart from being the original reading and yielding better sense, ἢ is supported by the disjunctive syntax of the parallel in Firmicus, *Math.* 6.29.3 *aut . . . aut* (this has hitherto been overlooked). For more details, see the synoptic Table 3 [p. 167].

In the *index verborum*, the final word στερέσει is listed under στέρεσις. However, instead of being dative singular of the noun στέρεσις, στερέσει must be the third person singular future indicative of the verb στερέω. Admittedly, the regular form ought to be στερήσει, and I do not know of any parallel for the future tense of στερέω without the obligatory lengthening from -ε- to -η-; but the context here (esp. ποιήσει) leaves no doubt about the grammatical interpretation. Besides, the noun στέρεσις is in itself a rare variant of the regular form στέρησις. I assume that the poet took the freedom of coining an analogous variant for the future tense of the verb, one that suited his metrical needs.[89] Lopilato [1998, 199] interprets this line correctly: 'will make them slaves or deprive them of parents'.

The distich F21.85–86 made its way from the original source (probably Anubio) into both pseudo-Manetho 1[5].344–345 and P. Oxy. 3.464 [F15 i 5–6]. In F21.86 change τῇδε to τούσδε. For a detailed discussion, see pp. 158–159 on F15.

F21.90 is line 349 in Koechly's edition, not 351.

Obbink is probably right in rejecting Usener's attempt to restore a pentameter from pseudo-Manetho 1[5].335 [Obbink 2006, 66 *s.v. Spuria*]. But there are, in addition to the elegiac couplets accepted by Obbink in F21, some further traces of pentameters that might have been worth inclusion in Obbink's new edition. One such verse seems to be hidden in pseudo-Manetho 1[5].168–169 (about Mars in the midheaven of day-born children):

> πρῶτον μὲν γονέων βίον ὤλεσε, καὶ λέχος αὐτῶν
> χωρίζει θανάτῳ κακῷ ἠὲ διχοστασίῃσιν.
>
> First, it destroys the life of parents, and it separates them from the marital couch by evil death or dissension. [trans. Lopilato 1998, 193]

[89] This phenomenon is not limited to poetry. Compare the grammarian Phrynichus Arabius (2nd c. AD), *Atticistes ecloge* n° 420 [Fischer 1974, 108] who reminds us that the correct spelling of εὕρημα is with -η-, not with -ε-.

Koechly (and Obbink) did not know that Hephaestio of Thebes quotes these lines with explicit attribution to Manetho [Hephaestio, *Apotelesm.* 2.4.27], reading the final words as χωρίζει θανάτῳ ἢ καὶ διχοστασίῃ. Both Pingree [1973, 102] and Lopilato [1998, 316] saw that this may originally have been a pentameter. Neither of them, however, tried to restore it to impeccable Greek meter. Yet, it can be restored by changing ἢ καὶ to ἠὲ, the reading of the *codex unicus* **M** of the direct transmission of pseudo-Manetho. On the assumption that the original couplet was inserted into the text of pseudo-Manetho, the surrounding hexametrical context may have led to the change from pentameter to hexameter. The restored elegiac distich to be included among the *fragmenta incerta* of Anubio would then be:

> πρῶτον μὲν γονέων βίον ὤλεσε, καὶ λέχος αὐτῶν
> χωρίζει θανάτῳ ἠὲ διχοστασίῃ.

> First, it destroys the life of parents, and it splits their marital union by death or dissension.

Moreover, pseudo-Manetho 1[5].336 deserves attention. Koechly presents it, with substantial changes, as καὶ Πυρόεις, μήτηρ προτέρη πατρὸς οἴχετ᾽ ἐς Ἅιδην, while the manuscript transmission (followed by Lopilato [1998, 36]) reads a pentameter: ἡ μήτηρ προτέρη οἴχεται εἰς ᾿Αΐδην.

While it is generally believed that only book 1[5] contains scattered elegiac fragments, two more of them may be contained in book 5[6]. These two books are closely related to each other and form together what Koechly considered to be the youngest part of the pseudo-Manethonian corpus.[90] Lopilato interprets the somewhat damaged verse 5[6].292 φαινόμενον πάλιν καὶ μακαριζόμενοι as a pentameter and prints τιόμενοι πᾶσιν καὶ μακαριζόμενοι [cf. Koechly 1858, xxviii 'quasi pentameter esset']. Lopilato further suspects [1998, 408] that beneath the corrupt hexameter verse 5[6].55 another original pentameter may be hidden, which he tentatively restores thus: ψυχρὸς γάρ τε πέλει, τῇ δὲ Κρόνοιο βολή ('For you see, Saturn is cold, and so, too, is its ray.')[91]

[90] Therefore they come last, as books 5 and 6, in his rearrangement.

[91] Lopilato's translation does not convince me.

It remains to ask if there are, apart from the elegiac meter, textual correspondences with the common source of Anubio and Firmicus (as indirectly attested in books 5–6 of Firmicus) which support the suspicion that the elegiac distichs in pseudo-Manetho go back to Anubio. Some of these distichs are preserved in a too fragmentary form as to allow for comparisons, especially when the whole astronomical protasis is missing [e.g., F21.71]. But some other distichs yield interesting results, even if these are not as striking as the parallels that Weinstock and Obbink detected between F3, F4, F5, F6 and Firmicus, *Math.* 6.29–31. I shall present two cases where the apodoses [A] are virtually identical, while the protases [P] are slightly different, yet not so different as to obscure the fact that there must be some relationship between the Greek and the Latin versions [see Table 3].[92]

More difficult to judge are cases like pseudo-Manetho 1[5].89/91 [= F21.61–62]:

> Ἑρμείας διάμετρον ἔχων Κρόνον ἠδὲ Σελήνην
> ἐμμανέας τεύχει τ᾽ ἠδὲ φρενοβλαβέας.

The passages to compare are

- Firmicus, *Math.* 6.15.16–17 esp. *linguam sic positi tardo sono vocis inpediunt, ut in ipsis faucibus tardis conatibus inpedita verba deficiant, aut verba linguae obligatione confundunt*
- Dorotheus [Pingree 1976, 351.30–352.4] esp. δυσγλώττους ἢ τραυλοὺς σημαίνει... βλαβήσεται ἡ λαλιά
- *Par. <Dor.>* 375.25–376.2 esp. βραδυγλώσσους καὶ δυσέκφορον τὴν λαλιὰν ἔχοντας ἢ τραυλούς, and
- Dorotheus Arabus 2.6.12–13 esp. 'it indicates a stammer of the tongue and few words, or he will be a lisper'.

This time the astronomical protases are all identical (Mercury in opposition with Saturn, while the Moon is in conjunction with one of them), but the astrological apodoses are different: while the *loci similes* quoted above unanimously predict a speaking disability, the pseudo-Manethonian passage insists on a mental disorder. But there is more to be observed. Koechly's rearrangements easily make one overlook that the manuscript tradition has another hexameter between lines 89 and 91. Lopilato prints the passage without comment:

[92] Complex astronomical protases are more likely to be corrupted than the rather simple astrological apodoses.

pseudo-Manetho	Firmicus, *Math.*
1[5].122, 124, 124b, 128 [= F21.67–70]	6.30.5
[P] Ἄρης δ᾽ ἣν τετράγωνον ἴδοι καλὴν Ἀφροδίτην, | μαρτυρίην τούτῳ καὶ Κρόνος ἀμφιβάλοι,	[P] *Si Mars et Luna diametra sibi fuerint radiatione contrarii, et easdem ambo in diametro constituti partes accipiant, Venus vero in dextro eorum quadrato fuerit constituta, et Venerem de diametro Saturnus respiciens per sinistrum quadratum Lunam Martemque pulsaverit, ut Venerem quidem de diametro, Lunam vero et Martem de quadrato respiciat, et Mercurius MC. possederit,*
[A] εὐνούχους στείρους, ὁτὲ δ᾽ ἑρμαφρόδιτον ἔτευξαν, | δισσάς, ἀχρήστους εἰς ἓν ἔχοντα φύσεις.	[A] *ex hac stellarum mixtura aut steriles aut hermaphroditi aut certe generantur eunuchi.*
1[5].341–345 [= F21.82–86]	6.29.3–4
[P] Ἠελίῳ τετράγωνος Ἄρης, Μήνῃ δέ τε Φαίνων,	[P] *Si Lunam*[a] *de diametro Mars et Saturnus pariter aspexerint, et nulla benivolarum stellarum vel Lunam vel illos qui sunt in diametro constituti salutari radiatione convenerit,*
[A] δούλους ποιήσει ἢ γονέων στερέσει.	[A] *aut servos efficiet ista coniunctio aut privatos parentum praesidio misero faciet orbitatis onere praegravari.*
[P] ἢν δ᾽ ἔτι καὶ Παφίη κατεναντίον Ἄρεος ἔλθῃ, | καὶ ταύτην τετράγωνος ἴδοι Κρόνος ὑψόθεν ἑστώς,	[P] (4) *Si Venerem et Lunam in diversis locis constitutas Saturnus et Mars quadrata vel diametra radiatione respexerint, et his omnibus Iovis oportunum testimonium denegetur,*
[A] ἐκ δούλων δούλους τούσδε[b] νόει ξυνέσει.	[A] *a servis parentibus natos ista coniunctio perpetuo faciet servitutis onere praegravari.*

[a] One is tempted to conjecture ‘Si <Solem et> Lunam’.

[b] For τούσδε and not τῆσδε, see the comments on F5, p. 148.

Table 3

> Ἑρμείας διάμετρον ἔχων Κρόνον ἠδὲ Σελήνην
> κεντρωθεὶς δ᾽ αὐτὸς[93] κατ᾽ ἐναντίον ὡρονόμοιο,
> ἐμμανέας τεύχει τ᾽ ἠδὲ φρενοβλαβέας.

> Mercury having Saturn and the Moon in opposition, and being encardined opposite the ascendant, makes [people] who are mad and deranged. [trans. Lopilato 1998, 80]

I wonder if one pentameter has dropped out after the first line, a pentameter in which the speaking disability was mentioned, maybe thus:

> Ἑρμείας διάμετρον ἔχων Κρόνον ἠδὲ Σελήνην
> <δυσγλώττους τεύχει, τραυλοὺς τὴν λαλιάν,>[94]
> κεντρωθεὶς δ᾽ αὐτὸς κατ᾽ ἐναντίον ὡρονόμοιο,
> ἐμμανέας τεύχει τ᾽ ἠδὲ φρενοβλαβέας.

This would imply a progression from a moderate disability to a severe one, both belonging to the astrological domain of Mercury (speaking, writing, reading, communication, sciences, mental skills), the latter one occurring only under particularly disadvantageous circumstances, when Mercury is setting. The context of Firmicus, *Math.* 6.15 contains other references to the centers and the places of the *dodecatropos*, for example 6.15.3 and 6.15.10. Compare especially 6.15.2–3 where a similar progression from simple opposition (Saturn-Jupiter) to the additional requirement that Saturn be rising is found. Therefore, pseudo-Manetho 1[5].89–91 may well go back to the same common source on which Firmicus, Dorotheus, and also Anubio drew [see the stemma on p. 136]. However, the absence of the reference to the setting point in all the *loci similes* that have been adduced above suggests that Anubio, if he really is the author of the two distichs quoted in the pseudo-Manethonian corpus, added the latter distich either *Marte suo* or drew (or inferred) it from the section of the common source that dealt with κεντροθεσίαι, especially from the chapter

93 ἀυτὸς is the reading of the *Liber Halensis*, αὐτοῖς that of the *codex Laurentianus* (followed by Koechly).

94 With *spondiazon* and intentionally onomatopoeic accumulation of the dentals -δ- and -τ-? My tentative restoration of the pentameter means 'creates [people] with a speaking disability, lisping in their talk'.

on Mercury in the centers.[95] That Anubio was familiar with the section on κεντροθεσίαι is clear from F22.3-4 [see below].

Maybe a close examination of the remaining elegiac elements in the Manethonian corpus will reveal some more correspondences with Firmicus and the other texts that go back to the common source, especially if one keeps in mind that many of these elegiac elements are mutilated and entire lines are missing, which makes the comparison awkward. Such an endeavor would, however, go beyond the scope of the present article. Suffice it to have pointed out what remains to be done.

F22 This fragment is transmitted in the commentary on *Job* by Julian the Arian whom Usener [1900, 335–336], who first drew scholars' attention to this fragment, mistakenly identified with the sixth century bishop Julian of Halicarnassus. Hagedorn [1973, lvi], the modern editor of this work, was able to show that it was written much earlier, between AD 357 and 365. The commentary on *Job* 38.7 ὅτε ἐγεννήθη ἄστρα ᾔνεσάν με φωνῇ μεγάλῃ πάντες ἄγγελοί μου preserves five separate fragments of elegiac astrological poetry (four distichs and one pentameter). Julian addresses the astrological poet by way of apostrophe in the second person singular (καταψεύδῃ, συνᾴδεις, φῄς, λέγεις), yet without mentioning his name. That seems to be the reason why Obbink placed F22 among the *fragmenta incerta*, together with other fragments in elegiac distichs that (a) bear no explicit attribution and (b) have no equivalent in Firmicus, *Math.* 6.3–31. In the present case, however, it has been overlooked that condition (b) is not fulfilled. See the introductory words of Julian: τί δ' ἄρα τῶν ἄστρων καταψεύδῃ λέγων, ὅτι ἂν τριγωνίσῃ Ἄρης τὴν Ἀφροδίτην, μοιχοὺς ποιεῖ; [F22.1–2]. This reference to the effect of Mars in trine aspect with Venus corresponds to Firmicus, *Math.* 6.5.3.[96] Therefore F22.1–2 would belong among the *fragmenta*, if it were original metrical text. However, it is a prose

[95] The relevant passages of the preserved texts are in Pingree 1976, 366.24–367.20; Dor. Arab. 2.27; pseudo-Manetho 3[2].90–105.

[96] Firmicus, however, envisages only the positive effects of this astrological aspect: *quottidiana lucra ex assidua quaestuum continuatione decernunt, et prosperi matrimonii nuptias . . . perficiunt.*

paraphrase. Therefore, it belongs among the *testimonia*, with a reference to the following original verses that are to be listed among the *fragmenta*.[97]

Dorotheus treated the same aspect, as is clear from *Par. <Dor.>* 384.6–8:

> πρὸς δὲ τὴν Ἀφροδίτην τρίγωνος ὢν ὁ Ἄρης εὐπορίαν καὶ λέχος εὔνυμφον δίδωσιν· φιλοσκόσμους ποιεῖ καὶ μεγαλόφρονας καὶ πολλῶν γυναικῶν λέχη θηρῶντας

and from *Par. Anub. <et Dor.>* 346.22–24 [= T8.58–60]:

> ὁ Ἄρης Ἀφροδίτην τριγωνίζων ἐμπόρους, εὐγάμους, φιλοκόσμους καὶ μεγαλόφρονας ποιεῖ, οἱ τοιοῦτοι δὲ πολλῶν γυναικῶν λέχη θηρῶσιν ἤτοι μοιχοὶ γίνονται.

The similar wording (note also the hunting metaphor in both versions) shows that both paraphrases drew on the same source, i.e., Dorotheus. While the version in *Par. <Dor.>* seems to preserve a poetical expression of the original (λέχος εὔνυμφον), it may need emendation of εὐπορίαν to ἐμπορίαν (maybe εὐπορίαν originated under the influence of the following εὔνυμφον?).

Now back to Julian. Note that the first elegiac distich quoted by him [F22.3–4] is about the luminaries together in a center, while the second and third distichs quoted by him [F22.6–7 and F22.11–12] are about the effects of Mars in a 'house' of Jupiter (i.e., in Sagittarius or Pisces) and of Saturn in a 'house' of Venus (i.e., in Taurus or Libra). These predictions belong to the κεντροθεσίαι and τοπικαὶ διακρίσεις which were discussed in the same order in the common source (probably Nechepso and Petosiris) that has been analyzed in the first part of this review article. While Firmicus translated this material into Latin, Dorotheus and pseudo-Manetho versified it.[98] Apparently Anubio did the same, and it is almost certain that he did so *before* embarking upon the discussion of the aspects. Within that earlier section, the κεντροθεσίαι must have preceded the τοπικαὶ διακρίσεις, as Julian's words καὶ μετὰ βραχέα [F22.5] prove. Julian also clarifies the relative order of all other elements in F22, except for

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[97] Compare Obbink's analogous treatment of T6/F2 and T8/F9. See also T7/F7 which, however, do not immediately cohere in the source.

[98] For details, see Table 1, p. 135.

the transition between the two halves F22.1–9 and F22.10–15. Although F22.10–15 comes later in Julian's text, its metrical elements must have preceded those of F22.6–7 in Anubio's original not only because Saturn precedes Mars in the typical descending order of the planets but also because we have specific evidence to this effect from the order of the corresponding passages on τοπικαὶ διακρίσεις in Dorotheus.[99]

Altogether, then, Julian's remarks and the preserved astrological treatments of κεντροθεσίαι and τοπικαὶ διακρίσεις show beyond reasonable doubt that Anubio followed the order of the material as he found it in the common source. As a consequence, F22 ought to be placed between F2 and F3, and the various metrical elements of F22, which probably belonged to the same book of the original, ought to succeed each other in the following order as distinct fragments:[100] F22.3–4, F22.11–15, F22.6–9.[101]

Some final remarks on F22.

- Julian's quotations require more emendations than this badly preserved text has hitherto received. For example, F22.6 εἰ δ᾽ Ἄρην ἐσίδοις εἰς τὸν Διὸς ἀγλαὸν οἶκον is certainly not an authentic hexameter of Anubio but its distortion by a Byzantine scribe. Its second half must have been ἐν τῷ Διὸς ἀγλαῷ οἴκῳ in the original [cf. F22.11 ἐν Κύπριδος οἴκῳ]. In addition, F22.6 ἐσίδοις and F22.11 ἐσίδῃς look suspicious (originally κατ-?), and so does F22.11 γεραρόν [see *app. crit.*; I prefer Usener's conjecture παρέοντ᾽].
- F22.3 κεντρογραφηθείσης ('placed in a center of the drawing') is the only attestation of the verb κεντρογραφέω[102] and commendably highlighted as such (with an asterisk) in the *index verborum*

[99] F22.11–12 ~ Pingree 1976, 357.19–23 [= T8.421–425 Obbink] ~ Dor. Arab. 2.28.3. F22.6–7 ~ Pingree 1976, 358.17–18 [= T8.448–449] ~ Dor. Arab. 2.30.2.

[100] Compare Obbink's commendable distinction between T4 and T5, both from the same work of Tzetzes.

[101] F22.11–15 came before F22.6–9 because Saturn precedes Mars in the typical descending order of the planets.

[102] Note, however, that there is also one attested case of the compound συνκεντρογραφέω in Greek: see Cumont 1929b, 174.3 συγκεντρογραφηθῇ.

[Obbink 2006, 69–79].[103] Interestingly, this verb describes astronomical positions not only with reference to the observer's horizon, but also with reference to the chart drawn up by the astrologer to illustrate the heavenly alignment.

- In F22.3–4 Dorotheus did not discuss the conjunction of the luminaries in a center, as the relevant chapters in Dor. Arab. 2.21–22; Pingree 1976, 361.16–362.16; and *Par. <Dor.>* show.[104] Hence, we have yet another argument against Anubio's dependence on Dorotheus.
- In F22.12 γάλλους ἢ μοιχοὺς ἔννεπε τὴν γένεσιν, the person born with Saturn in a house of Venus [F22.11] is called, by way of a frequent astrological metonymy, 'the birth' (ἡ γένεσις for ὁ γεννηθείς). The grammatical congruence between direct object (singular) and predicative nouns (plural) is awkward but somewhat mitigated by the astrological concept of typical alignments under which several 'copies' of the same type of human being can be born. For this concept, compare, e.g., Firmicus, *Math.* 6.30.25 where the same planetary alignment is said to have caused the births of two famous lyric poets, Archilochus and Pindar.

3. Rearrangement of the preserved *testimonia* and fragments

In light of the first two parts of this article, I suggest rearranging the preserved *testimonia* and fragments of Anubio as follows [see Table 4a–e on pp. 185–189]. I use a single asterisk (*) to indicate that the passage in question was placed in another category[105] by Obbink. Some elements of the mixed elegiac predictions in F21 deserve to be mentioned among the certain fragments, but only in the form of references preceding and following F6, in a smaller font, and without being assigned a number of their own, because they are too uncertain to justify their definitive excision from F21.

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[103] In the same index, correct ἄποικοις to ἄποικος, ἄφραστοs to ἄφραστος, βασιλήιδα to βασιληΐς, γεραρόν to γεραρός, ἤθεσιν to ἦθος, μειρόμαι to μείρομαι, ὀλίγας to ὀλίγος, and στέρεσις to στερέω.

[104] As to the omission in *Par. <Dor.>*, see Kroll, Skutsch, and Ziegler 1968, 2.128.

[105] *Fragmenta / Fragmenta loci incerti / Fragmenta incerta.*

APPENDIX 1
DOROTHEUS ON ASPECTS

Addenda to Pingree's Collection [1976] of the Fragments of Dorotheus of Sidon

Pingree included only *Par. Anub. <et Dor.>* in his collection, not *Par. <Dor.>*. Since the latter paraphrase contains a considerable number of obvious metrical fragments, and the former paraphrase contains three of which only one was highlighted as such by Pingree,[106] it will be useful to give a list of all fragments of the Greek original text of Dorotheus from the section on aspects that corresponds to Dor. Arab. 2.14–19. Any uncertain elements are underlined. See Table 5 on pp. 190–192.

APPENDIX 2
THE SOURCE OF THE PARAPHRASE T8

This appendix serves to substantiate the claim made above on p. 134 that the paraphrase T8 is, despite its explicit attribution to Anubio in the heading of the first chapter, mostly derived from Dorotheus and has therefore, in this review, rightly been labeled 'Par. Anub. <et Dor.>'.

The metrical fragments in this paraphrase that Obbink considered relevant to Anubio, F9.1 [T8.264] and F9.4 [T8.277], are from the three page chapter that deals with oppositions [T8.208–307]. Already in the previous chapter on square aspects [T8.76–207], the scribe must have switched from Anubio to Dorotheus, as the section on Mars in square aspect with Mercury shows [T8.170–173]:

> εἰ δὲ τὸν Ἄρην ὁ Ἑρμῆς ἐπιδεκατεύει, δεινοὺς ἐξετέλεσεν, πανούργους, ἀλλοτρίων ἅρπαγας· οἱ τοιοῦτοι γὰρ ἀπὸ ἄλλου εἰς ἄλλον μετέρχονται ὅπως κακόν τι αὐτοῖς προστριψάμενοι προδώσουσιν αὐτοὺς καὶ τῶν χρημάτων γυμνώσουσιν.

106 By way of centered formatting and blank lines preceding and following the hexameter; see Pingree 1976, 353.6. This is item 11, F9.4, in Table 5b, p. 191.

The beginning of the apodosis seems to go back to a metrical original like δεινοὺς ἐξετέλεσσε, πανούργους – ˘ ˘ – ⏓.[107] The Latin equivalent is Firmicus, *Math.* 6.11.9:

> malos malignos malitiososque perficiet [~δεινοὺς ἐξετέλεσε], pessima ac pestifera semper cupiditate mentis armatos, omnia circumscriptionum exercentes officia [~πανούργους], rapaces et qui de rebus alienis varia mentis cupiditate pascantur [~ἀλλοτρίων ἅρπαγας].

There is no equivalent in *Par. <Dor.>*. A fortunate coincidence has it that Rhetorius adapted the same metrical original, on which the scribe of *Par. Anub. <et Dor.>* [= T8] drew, in his discussion of the nativity of the grammarian Pamprepius of Panopolis [AD 440–484], which is Rhetorius 5.113–117 or, more precisely, in 5.115, the chapter that discusses why Pamprepius was a traitor. This chapter reads, in Pingree's forthcoming attempt to emend the badly corrupted *codex unicus* Paris. gr. 2425 (dactylic hexameters are indented):

Ὅρα τὸν Ἑρμῆν καθυπερτεροῦντα τὸν Ἄρην κατὰ τετράγωνον. φησὶ γάρ τις τῶν σοφῶν·[108]

    εἰ δέ νυ τετράπλευρος ἐὼν τὸν ἀνώτερον ἴσχει
    Ἑρμείας, βαιὸν δὲ τόπον φ<α>υλώτατος Ἄρης,
    δεινῶς ἐξετέλεσ<σ>ε πανούργος ἤτε μέλοντας
ἁρπαγὰς καὶ ἀλλοτρίων στερήσεις <ποιεῖν>,
    εἰς ἕτερον δ' ἑτέρου μεταν<άστ>ασιν ἀνέρος ἄνδρα.
ἄλ<λ>οτ' εὕρομεν καὶ τὸ λοιπόν·
                                        ἐνισκήψουσι <πρ>οδόντες
    <σ>φὶ<ν> κακομηχανίῃ, κτεάνων δ' <ἀπο>γυμνώσουσιν.

This is not the place to discuss Rhetorius 5.115 in detail. For previous attempts to restore this passage and for the indispensable *apparatus criticus*, see Pingree 1976, 368.[109] Suffice it to say that the reading

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107 Cf., e.g., Homer, *Od.* 2.110 = 24.146 and (in an astrological context) pseudo-Manetho 3[2].169 with ἐξετέλεσσε in the same position.

108 The names of the sources quoted are systematically suppressed in this branch of the transmission of Rhetorius [cod. Paris. 2425]. In the lost original, Rhetorius must have mentioned Dorotheus.

109 See further Stegemann 1943, 122–125, who provides a synoptic table that includes also his German translation of fol. 4 of the Arabic excerpt which was omitted by Pingree [see note a in Table 5a [p. 190].

δεινοὺς of the paraphrase [T8.171] is preferable to Cumont's conjecture δεινῶς for the manuscript reading δυνὸς in Rhetorius 5.115.2, which Pingree accepted; and, more importantly, that the source of both passages [T8.170–173, Rhetorius 5.115] was undoubtedly written in stichic dactylic hexameters. In other words, the scribe of the paraphrase cannot have followed the elegiac distichs of Anubio when writing T8.170–173.

In the following chapter on oppositions [T8.208-307], which contains the two elements that Obbink assigned to Anubio [T8.264 = F9.1 and T8.277 = F9.4], the scribe kept following Dorotheus, as arguments drawn from the beginning and from the end of this chapter indicate. Regarding the beginning, compare the paragraph about Saturn in opposition to Mars in the paraphrase's version [T8.211–226] with Dor. Arab. 2.16.3–9 and *Par. <Dor.>* 374.1–14.[110] As for the end, note that the opposition of the luminaries is missing *suo loco* in the paraphrase,[111] as it is missing in the Arabic translation of Dorotheus. Probably Dorotheus himself omitted it. But it was present in the common source, as Firmicus, *Math.* 6.18 shows, who has this paragraph where one would expect it. Interestingly, the paraphrase adds the missing paragraph at the end of the chapter on oppositions [T8.305–307: see note f in Table 1, p. 135], certainly not from Dorotheus, because we would then expect to find an equivalent right after Dor. Arab. 2.17, where nothing of the sort is to be found. In all likelihood, the scribe of the paraphrase made the addendum based on his second source, Anubio, which he compared after completing his chapter on oppositions. But altogether he was following Dorotheus, and therefore F9 Obbink [T8.264 and T8.277], which falls into this chapter on oppositions, is to be excluded from the edition of Anubio. This is confirmed by the fact that the other paraphrase, which Heeg [1910a] proved to be from Dorotheus, contains the words ἤθεσι δ' ὁρμητὴς καὶ ἄλλῳ τινὶ οὐκ εἴκων [*Par. <Dor.>* 382.1–2], which are undeniably a prose version of what F9.4 [= T8.277] preserves in the metrical original, i.e., ἤθεσιν ὁρμητήν τε καὶ οὐκ εἴκοντά περ ἄλλῳ.

[110] The equivalent in Firmicus is *Math.* 6.15.4–11.
[111] One would expect it after T8.295.

Hence, both paraphrases must here be drawing from the same source, namely, Dorotheus.[112]

In the next chapter, which is about conjunctions, the paraphrase that started with that misleading attribution to Anubio quotes again from Dorotheus, first implicitly, and then explicitly. The implicit instance occurs in T8.310–317

> ὁ Κρόνος σὺν Ἄρει τοῖς ἤθεσι πραεῖς ποιεῖ καὶ ἀργοὺς ἐν ταῖς πράξεσι καὶ ἐν πολλοῖς ἀποτυγχάνοντας, νοσερούς τε καὶ ὑπὸ μελαίνης χολῆς βλαπτομένους,. . . εἰ μὴ Ζεύς ποθεν ἐπιμαρτυρήσῃ, ὑπομονητικοὶ δὲ οἱ τοιοῦτοι καὶ βαρύθυμοι.

This goes back to Dorotheus, as an excerpt from his poem in the important manuscript Vat. gr. 1056, fol. 156, shows. The scribe quotes the following lines with explicit attribution to Dorotheus:[113]

> ἢν δ᾽ ἄρ᾽ Ἐνυαλίῳ συνέῃ Κρόνος, ἤθεα τεῦξε
> πρήεα· δὴ γὰρ Θοῦρος ἀεὶ σφοδρός τε καὶ ὠκύς
> εἰς ὁρμὰς ἄσκεπτον ἀεὶ τάχος ἠδ᾽ ἀλόγιστον
> θερμὸς ἐὼν ἤνεγκεν, ὁ δὲ βραδύς, ἀμφοτέρων δὲ
> κιρναμένων μέσσος κείνων βροτὸς ἔσσετ᾽ ἄριστος.

εἶτα προστίθησιν ὅτι κωλύσεις ἔργων καὶ χολῆς μελαίνης κίνησιν ποιεῖ,

> εἰ μὴ ἄρ᾽ Αἰγίοχος δαμάσει σθένος οὐλοὸν αὐτῶν.

The second instance occurs in T8.342–353, and it is here that the author of our paraphrase quotes for the first time explicitly from Dorotheus. This quotation combines two paragraphs from the chapter Περὶ τοπικῶν διακρίσεων [T8.411–541], after which Obbink's quotation in T8 breaks off, and has obvious equivalents in the Arabic translation of Dorotheus:

> T8.342–347 ~T8.432–437 ~Dor. Arab. 2.29.2
> T8.347–353 ~T8.448–451 ~Dor. Arab. 2.30.2

It is clear that the chapter Περὶ τοπικῶν διακρίσεων [T8.411-541] is from Dorotheus, who had this chapter (plus the one on κεντροθεσίαι) in the same position, *after* the discussion of the various aspects,

[112] Compare also Dor. Arab. 2.16.20 'he will be one of those who relies on himself and will not obey another' [trans. Pingree 1976, 220].

[113] See Pingree 1976, 368.25–369.6. This text was first published by Heeg [1910a, 125]. See also the discussion in Stegemann 1943, 116–119.

as the Arabic translation shows [Dor. Arab. 2.28–33], while Anubio and Firmicus followed the common source in placing the same two chapters *before* the discussion of the aspects, and in presenting *after* the aspects a collection of typical alignments [see Table 1, p. 135].

At this point the anonymous author of our paraphrase reached the end of the second book of Dorotheus and decided to add, before finishing his work, the one chapter that he had for some reason (lack of interest?) left out previously, that is, the chapter on κεντροθεσίαι, which concerns the planets and the luminaries in the four centers [see Pingree 1976, 361–367 ~Dor. Arab. 2.21–27]. It actually made sense to recover this previously skipped chapter because its content is closely related to the τοπικαὶ διακρίσεις [T8.411–541 ~Dor. Arab. 2.28–33]. Within this last section on κεντροθεσίαι [Pingree 1976, 361–367], Dorotheus is once more mentioned explicitly as the author of two consecutive dactylic hexameters, in which a hitherto overlooked emendation is needed [Pingree 1976, 361.19–22].[114] The paraphrase ends with a remark on the usefulness of all three topics that have been discussed:

> Ἰστέον δὲ ὅτι ταῦτα πάντα τὰ εἰρημένα, αἱ τοπικαὶ διακρίσεις τῶν ἀστέρων καὶ αἱ κεντροθεσίαι καὶ οἱ πρὸς ἀλλήλους σχηματισμοὶ χρειώδεις εἰσὶν ἐν ταῖς καταρχαῖς κτλ. [Pingree 1976, 367.21–23]

Altogether, it is clear that the scribe had two sources at his disposal, Anubio and Dorotheus. In their poems, they had both versified (among other things) three sections of their common source that dealt with τοπικαὶ διακρίσεις, κεντροθεσίαι, and σχηματισμοί. The scribe started from Anubio but very soon switched to Dorotheus, from whose second book he drew most of the following material. Only at the end of each chapter does he seem to have checked the corresponding passages in Anubio and made rare addenda.[115]

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[114] These verses in Pingree's edition read: ἢν Ζεὺς μὴ λεύσσῃ μιν ἢ αὐτὴ πότνια θεία | ἢ δόμον ἢ ὕψος τύχῃ λελαχυῖα Σελήνη. Instead of the unmetrical mss reading ὕψος, the original must have read ὕψωμα, a frequent astrological term that is once attested with certainty in the fragments of Dorotheus [see Pingree 1976, 324.5 αἱ δὲ ταπεινώσεις ὑψώματα ἐν διαμέτρῳ]. Besides these verses, see also Pingree 1976, 365.26 with another (somewhat mutilated) hexameter bearing no explicit attribution to Dorotheus.

[115] See p. 175 on T8.305–307.

APPENDIX 3
THE NEW GENEVA PAPYRUS

P. Gen. IV 157 was recently edited by Paul Schubert [2009a, 2009b]. It is F9 in my rearrangement of the fragments of Anubio.[116] This find increases the total of preserved verses of this poet by roughly 25%, adding substantially to our knowledge of his vocabulary. The Geneva fragment provides further arguments in favor of the views expressed in the first part of the present review article. An observation that neatly ties in with what has been said about F3 on p. 131 above can be made with regard to P. Gen. IV 157 ii 10–24. These lines correspond to Firmicus, *Math.* 6.31.53–54. However, while lines 14–16 and 21–24 of Anubio's version have no counterpart at all in the Latin text, Firmicus, *Math.* 6.31.54 gives more details than Anubio in lines 19–20. This may again be explained with the assumption that both authors drew on a common source [see Table 1, p. 135].

With regard to my conjecture [see 132] that Firmicus' ideal horoscopes in 6.30–31 are from the first century AD or even earlier, it deserves attention that the description of an imperial horoscope (*decretum potentissimi imperatoris*) in Firmicus, *Math.* 6.31.55 [cf. P. Gen. IV 157 ii 25–30] is unusually detailed, providing a complete set of astronomical data for the luminaries and the five planets. Maybe this is not just a fictitious alignment but the birth chart of a historical individual, comparable to indisputable cases such as the anonymously transmitted chart of Emperor Nero in Vettius Valens, *Anthologiae* 5.7.20–35. The only date within centuries that astronomically matches the positions given by Firmicus is 27 (or 28) Sept. 96 BC, *ca* 4 AM (Alexandria).[117]

[116] See Table 4c, p. 187. I am grateful to Paul Schubert for directing my attention to this new Anubio fragment and for sharing his (at that time still) forthcoming publications with me.

[117] I realized only after establishing this date that already Holden [1996, 74] had the same idea. However, his tentative identification with Ptolemy XI, Auletes must be rejected on chronological grounds as pointed out by Hübner [2005, 15n13]. As for the astronomical data, 96 BC suits the zodiacal positions perfectly if one takes into account that sidereal longitudes computed by ancient astronomers for the early first century BC would be roughly 7° higher than tropical longitudes obtained with modern computer software for the same period. The date in 96 BC is unsatisfactory only with regard to the additional condition that all five planets be in their own boundaries (*et*

P. Gen. IV 157 ii 1–2 corresponds to Firmicus, *Math.* 6.31.51 with the difference that Anubio speaks of Venus (Κύπρις) symbolizing the ἄλοχος (lit. 'partner of one's bed', i.e., either wife or concubine), while Firmicus speaks of the Moon (*Luna*) symbolizing the *uxor* (legitimate wife). If Firmicus had translated Anubio, one would expect 'Venus' instead of 'Luna'. Schubert [2009b, 423] in his commentary refers to Bouché-Leclercq's remark [1899, 449–450] that 'la planète Vénus, qui laisse à la Lune le premier rôle quand il s'agit du mariage légitime, le reprend quand il s'agit des passions de l'amour.' If, as argued above, both authors drew on a common source, this may have spoken of 'either Venus or the Moon', with Anubio quoting only the former deity and Firmicus only the latter. But on closer inspection another explanation seems preferable: the German branch of the MSS tradition of Firmicus omits the name of the planet in question, which suggests that *Luna* in the other (Italian) branch may be nothing more than a failed attempt to restore a name (or an astrological symbol) which had been lost in the course of transmission. Despite Bouché-Leclercq's correct observation above, it would not be surprising if the common source had spoken of Venus symbolizing the legitimate wife. This is clear from Obbink's F6 ii 30–33—a fragment belonging to the same roll as the Geneva papyrus[118]—where Venus (Κυθέρεια) indisputably symbolizes the legitimate wife (ἀλόχου) as opposed to a prostitute (πόρνης). The corresponding passage in Firmicus [*Math.* 6.31.82] speaks of *Venus* and *matrimonium* as opposed to *meretrices publicas*. See also Obbink's F4 b 12 where Venus (Κυθέρεια) symbolizes the ἄλοχος (probably again = 'wife'), while Firmicus in his corresponding passage [*Math.* 6.30.6] speaks of *Venus* and *uxor*.

omnes in suis sint finibus constituti). This detail may have been stylized in an otherwise historical alignment in which, as Holden [1996, 74] has rightly observed already, only Mars would, taking the 7°-shift into account, be in his own boundaries. Note that there is reason to suspect another historical horoscope behind a closely related passage, namely Firmicus, *Math.* 6.31.1 which Hübner [2005] tentatively dates to 23 May 139 BC, and identifies with Sulla. The date, but not the identification, was already ascertained in Holden 1996, 73.

118 See Schubert 2009a, 73; 2009b, 406.

BIBLIOGRAPHY

CCAG. *Catalogus codicum astrologorum graecorum.*

Dorotheus Arabus. See Pingree 1976, 159–322.

Par. Anub. <et Dor.>. *Paraphrasis Anubionis <et Dorothei>*: Anonymous. *Περὶ τῶν πρὸς ἀλλήλους σχηματισμῶν τῶν ἀστέρων, ἐκ τῶν ᾿Ανουβίωνος.* See Pingree 1976, 344–361.[119]

Par. < Dor.>. *Paraphrasis <Dorothei>*: Anonymous. *Περὶ κράσεως καὶ φύσεως τῶν ἀστέρων καὶ τῶν ἀποτελουμένων καὶ σημαινομένων ἐκ τῆς συμπαρουσίας καὶ τοῦ σχηματισμοῦ αὐτῶν.* See Pingree 1986, 369–389 (first edited in Kroll 1900).[120]

pseudo-Manetho. *᾿Αποτελεσματικά.* See Lopilato 1998.

PSI. *Papiri della Società Italiana.*

Boer, E. 1962. ed. *Heliodori ut dicitur in Paulum Alexandrinum commentarium. Interpretationes astronomicas addiderunt O. Neugebauer et D. Pingree.* Leipzig.

Boll, F. 1902. Review of *CCAG* 2–3 (1900–1901). *Byzantinische Zeitschrift* 11:139–143. Brussels.

________ 1908. *Die Erforschung der antiken Astrologie.* Neue Jahrbücher für das klassische Altertum, Geschichte und deutsche Litteratur 11:103–126. Repr. in Boll 1950, 1–28.

________ 1909. *Firmicus.* Cols. 2365–2379 in *Paulys Real-Encyclopädie* 6.2. Stuttgart.

________ 1914. ed. *PSI III 157. Carminis astrologici Manethoniani fragmenta nova.* Pp. 1–4 in *Papiri Greci e Latini.* vol. 3. n.i 157–279. Pubblicazioni della Società Italiana per la ricerca dei papiri greci e latini in Egitto. Florence.

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119 This paraphrase is mostly derived from Dorotheus [see Appendix 2, p. 173] and presents the same material as Firmicus, *Math.* 6.3–27 in the same order, i.e., by aspects. Pages 345.1–354.3 (= T8.1–307 in Obbink 2006) were first edited by A. Olivieri [1900c, 204–212]. Pages 345.1–361.14 were reprinted as T8 in Obbink 2006 and contain F9.

120 This paraphrase is basically derived from Dorotheus (with other elements from Valens and Ptolemy) and contains F10 in Obbink 2006. Its order (by planets) differs substantially from Firmicus, *Math.* 6.3–27.

Boll, F. 1950. *Kleine Schriften zur Sternkunde des Altertums.* V. Stegemann ed. Leipzig.

Bouché-Leclercq, A. 1899. *L'astrologie grecque.* Paris. Repr. Brussels 1963, Aalen 1979.

Bram, J. R. 1975. *Firmicus Maternus. Ancient Astrology: Theory and Practice. Matheseos libri VIII.* Park Ridge, NJ.

Cumont, F. 1921. *Rhetorii Aegyptii capitula selecta (excerpta ex codice Parisino 2425).* Pp. 115–225 in *CCAG* 8.4. Brussels.

________ 1929a. *Hephaestionis Thebani capitula selecta ex codice Paris. 2417.* Pp. 141–159 in *CCAG* 8.1. Brussels.

________ 1929b. *Hermetis Trismegisti methodus mystica excerpta ex codice Paris. 2419.* Pp. 172–177 in *CCAG* 8.1. Brussels.

Feraboli, S. 1981. *Sulla μοῖρα ὡροσκοποῦσα (Heph. Theb. II 2). Quaderni Urbinati di Cultura Classica* 37:157–160.

Fischer, E. 1974. *Die Ekloge des Phrynichos.* Sammlung griechischer und lateinischer Grammatiker 1. Berlin/New York.

Gundel, W. and Gundel, H. G. 1966. *Astrologumena. Die astrologische Literatur in der Antike und ihre Geschichte.* Sudhoffs Archiv 6. Wiesbaden.

Hagedorn, D. 1973. *Der Hiobkommentar des Arianers Julian.* Patristische Studien und Texte 14. Berlin/New York.

Heeg, J. 1910a. 'Dorotheus von Sidon und Firmicus Maternus *Math.* VI'. *Hermes* 45:315–319.

________ 1910b. *Excerpta ex codice Vaticano 1056.* Pp. 76–132 in *CCAG* 5.3. Brussels.

Heilen, S. 2011. *Hadriani genitura. Die astrologischen Fragmente des Antigonos von Nikaia. Edition, Übersetzung und Kommentar.* (forthcoming).

Hermann, G. 1812. ed. *Draconis Stratonicensis liber de metris poeticis. Ioannis Tzetzae exegesis in Homeri Iliadem.* Leipzig.

Holden, J. H. 1996. *A History of Horoscopic Astrology: From the Babylonian Period to the Modern Age.* Tempe.

Hübner, W. 1984. *Manilius als Astrologe und Dichter.* Pp. 126–320 in H. Temporini and W. Haase edd. *Aufstieg und Niedergang der*

Römischen Welt. Geschichte und Kultur Roms im Spiegel der neueren Forschung sect. 2: vol. 32.1. Berlin/New York.

Hübner, W. 1989. *Die Begriffe „Astrologie“ und „Astronomie“ in der Antike. Wortgeschichte und Wissenschaftssystematik mit einer Hypothese zum Terminus „Quadrivium“*. Wiesbaden.

__________ 2005. 'Sulla's Horoscope? (Firm. *math.* 6,31,1)'. Pp. 13–35 in G. Oestmann, D. Rutkin, and K. von Stuckrad edd. *Horoscopes and Public Spheres: Essays on the History of Astrology.* Religion and Society 42. Berlin/New York.

__________ 2008. rev. Obbink 2006. *Gnomon* 80:682–685.

Koechly, H. 1851. ed. *Arati phaenomena et prognostica. Pseudo-Manethonis et Maximi carmina astrologica, cum fragmentis Dorothei et Anubionis.* Pp. 41–101 in *Poetae bucolici et didactici. Graece et Latine*, vol. 2. Paris. Repr. 1931.

__________ 1858. ed. *Manethonis apotelesmaticorum qui feruntur libri VI. Accedunt Dorothei et Annubionis fragmenta astrologica.* Corpus poetarum epicorum graecorum 7. Leipzig.

Kroll, W. 1898. 'Astrologisches'. *Philologus* ns. 11:123–133.

__________ 1900. *Anonymi de planetis.* Pp. 159–180 in *CCAG* 2. Brussels.

__________ 1906. *Excerpta ex Vetti Valentis florilegio (ex codice Vaticano 191).* Pp. 27–129 in *CCAG* 5.2. Brussels.

Kroll, W.; Skutsch, F.; and Ziegler, K. 1968. *Firmici Materni Matheseos libri VIII.* 2 vols. Leipzig. Repr. with addenda of the 1st edn 1897–1913.

Kühn, K. G. 1821–1833. ed. *Claudii Galeni opera omnia.* 20 vols. Leipzig.

Lopilato, R. 1998. ed. *The* Apotelesmatika *of Manetho.* PhD Thesis, Brown University. UMI Microform 9830484.

Ludwich, A. 1904. 'Das elegische Lehrgedicht des Astrologen Anubion und die Manethoniana'. *Philologus* ns. 17:116–134.

Obbink, D. 1999. 'Anoubion, Elegiacs'. Pp. 57–109 (with plates 11–14) in N. Gonis *et alii* edd. *The Oxyrhynchus Papyri 66 [nos. 4494–4544].* Graeco-Roman Memoirs 86. London.

Obbink, D. 2006. ed. *Anubio. Carmen astrologicum elegiacum.* Munich/Leipzig.

Olivieri, A. 1900a. *Περὶ πράξεως καὶ ἐπιτηδεύματος καὶ τεχνῶν.* Pp. 190–191 in *CCAG* 2. Brussels.

________ 1900b. *Ἔτι περὶ ἐπεμβάσεων <Anubionis>.* Pp. 202–203 in *CCAG* 2. Brussels.

________ 1900c. *Περὶ τῶν πρὸς ἀλλήλους σχηματισμῶν τῶν ἀστέρων, ἐκ τῶν Ἀνουβίωνος.* Pp. 204–212 in *CCAG* 2. Brussels.

Pingree, D. 1973–1974. ed. *Hephaestionis Thebani apotelesmaticorum libri tres.* 2 vols. Leipzig.

________ 1976. ed. *Dorothei Sidonii carmen astrologicum.* Leipzig. [Edition of the Arabic translation of the Greek original; English translation of the Arabic translation; edition of the Greek and Latin fragments].

________ 1977. 'Antiochus and Rhetorius'. *Classical Philology* 72:203–223.

________ 1978. ed., trans., and comm. *The Yavanajātaka of Sphujidhvaja.* 2 vols. Harvard Oriental Series 48. Cambridge, MA/London.

________ 1986. ed. *Vettii Valentis Antiocheni anthologiarum libri novem.* Leipzig.

Rehm, B. 1965. ed. *Die Pseudoklementinen. II: Rekognitionen in Rufins Übersetzung.* Die griechischen christlichen Schriftsteller der ersten Jahrhunderte 51. Berlin.

Riess, E. 1891–1893. 'Nechepsonis et Petosiridis fragmenta magica'. *Philologus* Suppl. 6:325–394.

________ 1894. 'Annubion'. Cols. 2321–2322 in *Paulys Real-Encyclopädie* 1.2. Stuttgart.

________ 1895. 'Epikritisches zur Astrologie des Nechepsos und Petosiris'. *Philologus* ns. 8:185–189.

Roberts, C. H. 1938. ed. *Catalogue of the Greek and Latin Papyri in the John Rylands Library Manchester. III: Theological and Literary Texts (Nos. 457–551).* Manchester.

Schubart, W. 1950. *Griechische Literarische Papyri.* Berichte über die Verhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig. Philologisch-historische Klasse 97.5. Berlin.

Schubert, P. 2009a. ed. 'P. Gen. IV 157. Anoubion de Diospolis, poème astrologique en distiques élégiaques'. Pp. 73–76 in S. Gaffino Moeri, S. Gällnö, N. Poget, and P. Schubert edd. *Les papyrus de Genève. Nos 147–205. Textes littéraires, semilittéraires et documentaires.* Les papyrus de Genève 4. Geneva.

________ 2009b. ed. 'Le papyrus de Genève inv. 268. Un nouveau fragment du poème astrologique d'Anoubion, précurseur de Firmicus Maternus'. Pp. 399–432 in Comptes Rendus de l'Académie des Inscriptions et Belles-Lettres 153.

Stegemann, V. 1943. 'Dorotheos von Sidon und Firmicus Maternus. Ein Beitrag zur Bewertung der Quellenexcerpte in den Matheseis'. *Hermes* 78:113–131.

Strecker, G. 1989. *Die Pseudoklementinen. III: Konkordanz zu den Pseudoklementinen.* Zweiter Teil. Die griechischen christlichen Schriftsteller der ersten Jahrhunderte 60. Berlin.

Usener, H. 1900. 'Aus Julian von Halikarnass'. *Rheinisches Museum* ns. 55:321–340.

Weinstock, S. 1952. 'A New Anubio Fragment'. *Chronique d'Égypte* 27:210–217.

TABLE 4

REARRANGEMENT OF THE PRESERVED CITATIONS OF ANUBIO

[see p. 172]

General *testimonia* on Anubio and his poem

H[a]	O[b]	Source	AM[c]	IT[d]	Notes
T1	T1	pseud.-Clem., *Hom.* 4.6	•		
T2	T2	Rufinus, *Rec.* 10.52.2–3	•		
	T3	Firm *Math.* 3.pr. 4–3.1.2			Refers to Anubis (the god) not Anubio[e]
T3	T4	Hermann 1812, 33.15–18	•		
T4	T5	Hermann 1812, 53.26–54.8	•		
T5	T6	Heph., *Apotelesm.* 2.2.11	•		Introduces F2

[a] H = Heilen. [b] O = Obbink. [c] AM = Anubio mentioned.

[d] IT = Indirect *testimonium*, that is, a *testimonium* in which the author draws not on Anubio but on Anubio's source.

[e] See p. 140.

Table 4a

Specific *testimonia* on the topics treated by Anubio

H[a]	O[b]	Source	AM[c]	IT[d]	Notes
T6	F13*	Heph., *Epit.* 4.21.4–7	•		How to determine the ascendent when the hour is unknown (paraphrases F2)
T7	F12*	Heph., *Epit.* 4.23.4	•		On which of the parents will die first
T8	T8	*Par. Anub. <et Dor.>*	•[e]	•	On the various aspects, and the seven planets when in each other's houses and terms
T9	F11*	Firm., *Math.* 6.3–31		•	On the various aspects, plus a collection of typical charts
T10	F22.1–2*	Hagedorn 1973, 255.3–4		•	Mars in trine aspect with Venus [= Firmicus, *Math.* 6.5.3]
T11	T9 + F14*	Rufinus, *Rec.* 10.9.4–7			Venus in conjunction with Jupiter *vs* Venus in conjunction with Mars [= Firmicus, *Math.* 6.23.5 + 6.24.2]
T12	T7	Rhetorius, 5.82.6–7/*Epit.* 4.27.8–9	•		On the profession and business [cf. Ptolemy, *Tetr.* 4.4]

[a] H = Heilen. [b] O = Obbink. [c] AM = Anubio mentioned.

[d] IT = Indirect *testimonium*, that is, a *testimonium* in which the author draws not on Anubio but on Anubio's source.

[e] Mostly derived from Dorotheus, despite the initial attribution to Anubio. See p. 134.

Table 4b

Fragmenta

H[a]	O[b]	Source	Attribution A[c]	B[d]	C[e]	D[f]	Firmicus, *Math.*	Topic
F1	F1	P. Oxy. 66.4503[r]		•		•	2.1.1 2.4.1 2.4.4–6	12 zodiacal signs, 36 decans, 108 subordinate deities (λειτουργοί, *liturgi*)
F2	F2	Heph. *Apotelesm.* 2.2.11–15	•	•				determining the ascendent at birth
F3	F22.3–4*	Hagedorn 1973, 255.5–6		•		•[g]		luminaries (and planets?) at the centers (κεντροθεσίαι)
F4	F22.11-15*	Hagedorn 1973, 260.2–6		•		•		planets in each other's houses and terms (τοπικαὶ διακρίσεις)
F5	F22.6–9*	Hagedorn 1973, 255.8–11		•		•		
	F21.61–62*	ps.-Manetho 1[5].89–91[h]		(•)	(•)		(6.15.16–17)	on aspects (esp. oppositions)
	F21.82–86*	ps.-Manetho 1[5].341–345[i]		(•)	(•)		(6.29.3-4)	
F6	F3	P. Oxy. 66.4504		•	•		6.29.23–30.5	
	F21.67–70*	ps.-Manetho 1[5].122, 124, 124b, 128[j]		(•)	(•)		(6.30.5)	typical charts
F7	F4	P. Oxy. 66.4503[v]		•	•		6.30.6–7	
F8	F5	P. Oxy. 66.4505		•	•		6.30.20–22	
F9		P. Gen. IV 157		•	•		6.31.51–55	
F10	F6	P. Schub. 15		•	•		6.31.78–86	

[a] H = Heilen. [b] O = Obbink. [c] Explicit attribution to Anubio in context. [d] Astrological content in elegaic meter. [e] Parallels in Firmicus, *Math.* 6.3–31. [f] Other reasons. [g] On F22, see p. 169. [h] On F21.61–62*, see p. 189. [i] On F21.82–86*, see p. 189. [j] On F21.67–70*, see p. 189.

Table 4c

Fragmenta loci incerti

H[a]	O[b]	Source	Attribution A[c]	B[d]	C[e]	D[f]	Firmicus, *Math.*	Topic
F11	F7	Rhetorius 5.82.2, *Epit.* 4.27.2	•	•				on the profession, business (περὶ πράξεως καὶ ἐπιτηδεύματος)
F12	F8	Olivieri 1900a, 203.3–36		•		•[g]		on arrival in places (περὶ ἐπεμβάσεων, *de revolutionibus nativitatum*)
	F9 + F10[h]							
	F11 [= *T9*]							
	F12 [= *T7*]							
	F13 [= *T6*]							
	F14 [= *T11*]							

[a] H = Heilen. [b] O = Obbink. [c] Explicit attribution to Anubio in context.
[d] Astrological content in elegaic meter.
[e] Parallels in Firmicus, *Math.* 6.3–31.
[f] Other reasons. [g] On F8, see p. 152.
[h] From Dorotheus, to be omitted. On F9 and F10, see pp. 153–156.

Table 4d

Fragmenta incerta

H[a]	O[b]	Source	Attribution				Firmicus,	Topic
			A[c]	B[d]	C[e]	D[f]	*Math.*	
F13	F15	P Oxy. 3.464		•				mixed predictions concerning children
F14	F16	PSI 3.157		•			3.4.23[g]	on Mars in the eighth place of the dodecatropos
F15	F17	P. Ryl. 3.488		•				(unclear)
F16	F18	P. Schub. 16		•				(unclear)
F17	F19	P. Oxy. 66.4506		•				(unclear)
F18	F20	P. Oxy. 66.4507		•				(unclear)
F19	F21	verses from ps.-Manetho 1[5][h]		•				(various)
F20		verses from ps.-Manetho[i] 1[5].168–169, 336; 5[6].292		•				(various)
	F22 [= *T10* + *F3–F5*]							

[a] H = Heilen. [b] O = Obbink. [c] Explicit attribution to Anubio in context.

[d] Astrological content in elegaic meter.

[e] Parallels in Firmicus, *Math.* 6.3–31.

[f] Other reasons.

[g] Other passages in PSI 3.157 equal Firmicus, *Math.* 3.5.30, 3.6.29, and 4.6.1; but they are composed in stichic hexameters, not in elegaic distichs.

[h] For F21.61–62, F21.67–70, and F21.82–86, compare the entires before and after Obblink's F3 in Table 4c.

[i] See comments on F21, p. 164

Table 4e

TABLE 5

ADDITONAL FRAGMENTS OF DOROTHEUS OF SIDON

[see Appendix 1, p. 173]

		Source			Parallels	
	Text	*Par.* <*Dor.*>	*Par. Anub.* <*et Dor.*>	Other Sources	Dor. Arab.	Firm. *Math.*
Trine aspects						
1	ἄλλοι δ’ αἰθερίων ἄστρων ἐπι-ΐστορές εἰσιν [F10.2]	381.5			2.14.12[a]	6.4.4–5
2	λέχος εὔνυμφον	384.6–7			2.14.18	6.5.3
Square aspects						
3	αὐτοὺς δ’ ἑτέροισι προσώποις	375.21			2.15.10	6.9.13
4	ἔσσεται		348.12		2.15.12	6.9.15
5	πταίσματα γὰρ πάμ-πολλα φέρει	383.33–384.1			2.15.23	6.11.2
6	quoted on p. 174		cf. 349.33–350.3	Rhetorius 5.115	2.15.28	6.11.9
7	ἀστείους τέχνης εἰδήμονας	387.9			2.15.33	6.13.1

[a] See further Stegemann 1943, 126–127, which provides a synoptic table that includes also a German translation of an Arabic excerpt (a different Arabic prose version of Dorotheus’ chapters on aspects which was omitted by Pingree) from MS Leiden or. 891, fol. 1–27: at fol. 2: ‘Und zu ihnen gehört der, der die Wissenschaft von der Berechnung der Gestirne unterstützt’.

Table 5a

		Source			Parallels	
	Text	*Par. <Dor.>*	*Par. Anub. <et Dor.>*	Other Sources	Dor. Arab.	Firm. *Math.*
Oppositions						
8	ἐκ μόχθων μόχθους	374.4			2.16.3	6.15.5
9	βυσσοδομεύων	380.30			*lacuna*	6.16.4
10	βίος ἄρκιος ἔσ<σε>ται αὐτῷ [F9.1]		352.28–29		*lacuna*	6.16.5
11	ἤθεσιν ὁρμητήν τε καὶ οὐκ εἴκοντά περ ἄλλῳ [F9.4]		353.6		2.16.20	6.16.8
12	πίστιν ἀποστέργουσι δικαίων[b] [F10.5]	384.26–27	cf. 353.17		2.16.25	6.17.4

[b] 'They reject/betray the trust that just men put into them'. Note that instead of δικαίων, *Par. Anub. <et Dor.>* reads δικαίαν [T8.288 = Pingree 1976, 353.17]. Cf., e.g., pseudo-Clement, *Hom.* 9.21.3 (and later authors) τὴν δικαίαν πίστιν. The non-Greek parallels of our fragment are Firmicus, *Math.* 6.17.4 *religiosa fidei commercia polluentes* and Dor. Arab. 2.16.25 'he will run away from the discharge of [his] trust' [trans. Pingree 1976, 220].

Table 5b

	Text	*Par. <Dor.>*	**Source** *Par. Anub. <et Dor.>*	Other Sources	**Parallels** Dor. Arab.	Firm. *Math.*
	Conjunctions					
13	quoted on p. 176	370.28[c]	cf. 354.6–12	Dorotheus [Pingree 1976, 368.25–369.6]	2.18.2–3	6.22.4–5, 22.8
14	βαρυδαίμονες ὄντες	371.13			2.18.5	6.22.11
15	ἀνάξια λέκτρα γυναικῶν	371.21			2.18.7	6.22.12
16	καί κεν ἀμαυρώσειε τύχην καὶ μείονα θείῃ [F10.1]	379.25			2.19.11	6.23.7
17	ψεύστας μέν, συνετοὺς δὲ καὶ –⏑ ⏑ – <u>πολυπείρους</u>	383.12			2.19.16	6.24.5
18	ʽθερμόν τε καὶ οὐ δύστευκτον ἔθηκε	383.21			(2.19.23)[d]	6.24.9
19	μηχανικῆς πολύπειρος	388.29–30			2.19.30	6.27.2

[c] These lines preserve only the last hexameter. [d] The relevant detail is omitted.

Table 5c

In memoriam
Ian Mueller
(1938–2010)[1]

Ian Mueller died on 6 August 2010, in Hyde Park, the University of Chicago neighborhood where he had spent the last more than 40 years of his career. He had been struck down by a mysterious illness, apparently a massive viral infection, only two days before; he had been enjoying a healthy, energetic, and very productive retirement. His wife and colleague Janel Mueller, his constant companion since their first month in graduate school 51 years before, was with him to the end. He is survived by Janel, their daughters Maria and Monica, and two grandchildren. His death is a heavy blow to his past students and to the whole scholarly community in Greek philosophy and Greek mathematics. (We had a very bad year: we had already lost Steven Strange, Vianney Décarie, David Furley, Jacques Brunschwig, and Pierre Hadot in the previous 12 months; and Bob Sharples died a few days after Ian.)

Ian first made his name with contributions in Greek logic, on the logical structure of Greek mathematical texts, and on Greek philosophy of mathematics.[2] But for many years much of his interest had been on how Greek thinkers, especially in late antiquity, interpreted earlier philosophers (and mathematicians, and so on). Some topics which are now fashionable, concerning, for instance, doxography and heresiography or late Neoplatonic strategies of reading Aristotle and the *Timaeus*, were not at all fashionable when Ian got into them; he often worked in isolation at the beginning, and I think and hope that it was a source of satisfaction to him when the scholarly community belatedly realized that these topics were interesting, and realized that Ian had been there first. Ian played an important role in the revival of the serious study of Neoplatonism in the English-speaking world and especially in the project led by Richard Sorabji of translating the Greek commentators on Aristotle into English: he translated 10

[1] I would like to thank for their comments and conversations about Ian: Alan C. Bowen, Eric Brown, Zena Hitz, Rachana Kamtekar, Alison Laywine, Henry Mendell, Richard Sorabji, Bill Tait, James Wilberding, and especially Janel Mueller.

[2] For a list of Ian's publications, see pp. 222–228 below.

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and a half volumes in the series, and also made generous and very useful critical comments on other translators' drafts.

But Ian was also important in the study of Greek philosophy more broadly, outside these particular specializations. He was what Diogenes Laertius calls a sporadic, being self-educated in ancient philosophy and a follower of no individual or school; and certainly he neither founded a school himself nor imposed any orthodoxy on his students. This was in itself unusual in a field dominated by charismatic teachers who generally produced students in their own image: Gregory Vlastos, G. E. L. Owen, Harold Cherniss, Joseph Owens, Michael Frede, Terry Penner, not to mention Leo Strauss and his students, and the Tübingen esotericists. Ian never bought into the programs of Owen and Vlastos in particular, which for decades dominated English-language ancient philosophy outside of sectarian enclaves. He was nonetheless tolerated and respected by the establishment, perhaps mainly because he was so much better at the mathematics and logic than they were. (Many of his papers were written for conferences on some Greek philosophical text or issue where they needed someone to explain the mathematical background.) He shared Owen's and Vlastos' goal of logically precise reconstruction of ancient philosophers' theses and arguments, but was deeply suspicious of their tendency to impose modern concerns, and often specific then-fashionable modern theories, on the ancient texts. He had too much awareness of the multiple possibilities of reception and interpretation ever to believe with Vlastos that Plato's early dialogues give a transparent window onto the historical Socrates. He rejected the view of Ryle, Owen, and Vlastos that Plato's late dialogues pursue issues of philosophical logic while abstaining from, or outright rejecting, any otherworldly metaphysics of Forms. Ian kept doing his own thing; and by his independence, courage, and even stubbornness, he showed his students and other admirers that we too could do something different, while at the same time he held us to standards of rigor as strict as, and stricter than, the 'analytic' school. He was also very aware, and kept us aware, both of older traditions of interpreting ancient philosophy and of contemporary non-Anglophone traditions. And he lived to see the old orthodoxy collapse.

Ian was an undergraduate at Princeton (where he studied with the young Hilary Putnam, and also took a class with the visiting William Faulkner), graduating in 1959, and then did his graduate work at Harvard, at the time the dominant philosophy department in the US. I am not sure how much he studied Greek philosophy, if at all; he did not learn Greek. His dissertation was on 'The Relationship of the Generalized Continuum Hypothesis and the Axiom of Choice to the von Neumann-Bernays-Gödel Axioms for Set Theory'; he took his Ph.D. in 1964. (A generous traveling fellowship from Harvard allowed him to spend some time in Zürich with the already retired Paul Bernays.) In theory, his first advisor was Burton Dreben; but Dreben did nothing and, in fact, Ian worked with Hao Wang. (There is a good picture of the Harvard department around this time, and of the often amazing inattention of dissertation supervisors toward their students, in Robert Paul Wolff's memoirs, available on his website.[3]) Ian was, I think, rather traumatized by Dreben's behavior, and certainly his own sense of responsibility toward his graduate students was very different. Also traumatic were Paul Cohen's articles 'The Independence of the Continuum Hypothesis' [1963] and 'The Independence of the Continuum Hypothesis, II' [1964], later developed in his book *Set Theory and the Continuum Hypothesis* [1966].

When Ian did most of his dissertation research, it was known that the axiom of choice and the generalized continuum hypothesis are relatively consistent, i.e., that if set theory (in the Zermelo-Fraenkel or some similar axiomatization) is consistent, then set theory together with the axiom of choice and the generalized continuum hypothesis is also consistent. But it was not yet known that these axioms are also independent of set theory, i.e., that if set theory is consistent, then set theory together with the negation of the axiom of choice is also consistent, and set theory together with the axiom of choice and the negation of the generalized continuum hypothesis is also consistent. When Cohen proved these results, by a very technical and completely unexpected method, Ian felt, first, that he would never be able to understand the proof; then, when he did master the proof, that he would never himself be able to come up with anything like that (most of us would not). Ian said (in an autobiographical talk that he gave to the undergraduate philosophy society at the University of Chicago, which I heard probably in the late 80's) that he was

[3] Wolff's memoirs can be found at http://robertpaulwolff.blogspot.com/, in postings in June 2009 and April-August 2010. For the dissertation supervisors, see http://people.umass.edu/rwolff/memoirchapterfour.pdf.

so discouraged that he almost gave up philosophy, and might have if he had not had a family to support. Instead he gave up working on the philosophy of contemporary mathematics. (Ian would never have adopted the solution of many philosophers, of continuing to philosophize about mathematics without understanding the technical results.) Ian felt that he had an analytic method to apply but now no subject matter to apply it to. Then, he discovered Greek philosophy and Greek mathematics. As Ian told the story—and I suppose it is true, although it could scarcely happen nowadays—when he was appointed at the University of Illinois at Urbana-Champaign, the philosophy department announced that they needed someone to teach Greek philosophy, and Ian volunteered to do it on condition that they give him a year off to learn Greek. They did and he never looked back.

After teaching as an Instructor at Harvard from 1963 to 1965, Ian was Assistant Professor at Urbana-Champaign from 1965 to 1967, and then moved to the University of Chicago, where he was promoted to tenure in 1970, and to full Professor in 1979. He retired in 1999, but remained for a while heavily involved in the university's Master of Arts Program in the Humanities at the special request of the dean.

The dean had particular persuasive power with Ian because she was his wife. Janel was hired at Chicago at the same time Ian was, but to a non-tenure-track position; and Ian was bluntly told that, while she was well qualified, a woman would not get a tenure-track slot. But Janel prevailed; she became a distinguished scholar of 16th- and 17th-century English literature, chair of the English department, holder of a named chair, and dean. Ian later credited Janel's experiences with awakening in him an awareness of, and horror at, all forms of discrimination and exclusion. Ian and Janel designed, and for many years jointly taught, a humanities core course on Greek thought and literature; Ian's handout translations and notes on the Presocratics and sophists were, at the time, hard to match and very useful.

Several of Ian's early publications came out of an invitation to an American Philosophical Association symposium on Stoic logic in spring 1968. They are characteristic of his work in two ways. First, they combine control over the fragmentary source-material with technical logical and mathematical skill—'On the Completeness of Stoic Propositional Logic' uses the Gentzen sequent-calculus to prove a completeness theorem for one particular modern reconstruction of

Stoic logic. But, second, they show a deep scepticism about the evidence for any such modern reconstruction, and an awareness that the Stoics are unlikely to have been interested in anything like completeness in a modern technical sense (since, for instance, they reject the inference 'the first, therefore the first'). Given Ian's sceptical approach, it is not surprising that Stoic logic never became a major research direction for him, any more than the Presocratics. But Ian was turning, in the late 60's and the 70's, to areas which would remain central to his work: the argument-structure of Euclid's *Elements*, and also of Greek mathematical treatises on astronomy, harmonics, and optics; the role of mathematics in Plato's philosophical program; Aristotle's understanding of mathematical epistemology and of mathematical objects; and the Greek commentators, especially the later (post-Iamblichus) Neoplatonists and their interpretations of earlier philosophy and mathematics.

Ian's work on Euclid, culminating in his *Philosophy of Mathematics and Deductive Structure in Euclid's* Elements [1981], was guided mainly by careful attention to the logical structure of Euclid's arguments both in individual propositions and in whole books. So far as he had a grand interpretive thesis, it is what might seem an obvious one: that Euclid very often proves some proposition—either proving a theorem or showing how to construct a solution to some problem—because he is going to use it in proving something else later in the *Elements*, so that the significance of the individual proposition will emerge from seeing its place in the larger deductive structure, not only what it rests on but what it will be used for. Again, this may seem obvious, at least as a general program. But it led Ian to argue against what were then two very widespread tendencies in the scholarship on Euclid. One was the tendency to modernize Euclid, and in particular what Ian called the 'algebraic interpretation' of Euclid, going back to Zeuthen and famously exemplified by B. L. van der Waerden, according to which notably *Elements* 2 and the 'application of areas' constructions in 6.26–30 were interpreted as exercises in manipulating and solving quadratic equations. The other was the amazingly broad willingness to treat Euclid as a 'blundering schoolmaster' (as Ian put it in the title of one article), whose *Elements* was a compilation like Diodorus Siculus' *Library of History*, which modern scholars could exploit to reconstruct the work of lost geniuses like Eudoxus. Any merits would be attributed to the lost source;

any faults, to Euclid; and the present context of the propositions in the larger structure of the *Elements* would be used only to look for incongruities which could give a clue to the original context. Against this, Ian wanted to interpret Euclid out of Euclid. Thus, *Elements* 2 was for him not an independent 'geometrical algebra' but a means of securing what is needed for later geometrical constructions, notably the construction of the regular pentagon: here, as with the 'Pythagorean theorem' and squaring the rectangle (and thus squaring any rectilineal figure), Euclid wants to show how much can be done without using proportion theory, just as in *Elements* 1 he wants to determine how much can and cannot be done without using the fifth postulate. Again, in *Elements* 6, elliptic and hyperbolic application of areas are not ways of solving quadratic equations but arise from the proportion-theoretic analysis of the regular pentagon, with Euclid stating the construction in as general a form as he can. Likewise, in Euclid's arithmetical books, Ian stressed their service to the theory of irrationals in *Elements* 10.[4] And *Elements* 10 itself, clever in technique but degenerating into a long boring catalogue of kinds of irrational lines not redeemed by any overall theory, makes sense as an attempt to locate the edge-length of the icosahedron in *Elements* 13 and to distinguish it from more readily constructed kinds of irrational lines. Ian was of course also interested in the logical structure of Euclid's proportion theory (or his two proportion theories in *Elements* 5 and 7) and the method of exhaustion, as well as in the status of the postulates and of construction. He argued against Oscar Becker's attempts to assimilate Euclid (or a hypothetical smarter predecessor) to modern intuitionism/constructivism: a construction postulate is a license to perform (or to be agreed to have performed) a certain activity, and we cannot identify it, as Becker wanted, with an existential (or $\forall\exists$) proposition. I will return below to some more surprising things that Ian said about Euclid's postulates.

Ian was always interested in the relationship between the understanding of mathematics that emerges from mathematical writers themselves and the understanding that we find in the philosophers, starting with Plato and Aristotle. He did not try to harmonize them.

[4] One might also look at their service to mathematical harmonics, and Ian of course recognized that they also contain independent things such as the theory of perfect numbers.

The mathematics that Plato and Aristotle were talking about may be importantly different from the mathematics that Euclid was doing perhaps a century later, and the programmatic descriptions that Plato and Aristotle give of mathematics may not map well onto any kind of real mathematics. Ian treated Plato as an enthusiast for mathematics among the philosophers, encouraging the philosophers to study mathematics and to imitate the mathematicians' methods (*Meno*, *Phaedo*) or even to surpass them (*Republic*); and perhaps later ancient sources are right that Plato gave problems as challenges for the mathematicians to solve. Ian did not assume that Plato himself had any great technical mastery of mathematics (in fact, he thought that the less enthusiastic Aristotle probably knew more math), or that there was a way to make coherent sense of everything Plato says about mathematics and its significance for philosophy: rather, as he saw it, Plato gave a series of tantalizing incomplete and probably incompletable programs.[5] He thought that Aristotle was probably right that Plato held mathematics to be about special 'mathematicals', e.g., mathematical squares, which would be like the Form of square and unlike sensible squares in being *perfectly* square, but like sensible squares and unlike the Form of square in that there would be many of them: for the Pythagorean theorem to be precisely true, so the argument goes, it must be precisely true *about something*, and it cannot just be making an assertion about the unique Form of square, since it mentions three squares. Aristotle argues that the same reasoning should lead Plato, absurdly, to admit intermediate astronomicals, harmonicals, and opticals. In his 'Ascending to Problems: Astronomy and Harmonics in *Republic* VII' [1991b], Ian bit the bullet and tried to make sense of this 'absurd' result: by making use not only of *Republic* 7 but also of texts like Autolycus' *On a Moving Sphere* and *On Risings and Settings*, he showed how someone might treat 'pure' and 'mixed' mathematical disciplines equally as idealizing, proving theorems about hypothesized rather than observed objects.

[5] See particularly Ian's papers 'Mathematics and Education: Notes on the Platonist Program' [1991], 'Mathematical Method and Philosophical Truth' [1992a], and 'Mathematics and the Divine in Plato' [2005], besides others discussed below.

Ian thought that Aristotle shared Plato's realist assumption that if mathematical statements are precisely true, there must be something that they are precisely true of. But, as Aristotle argues notably at *Metaphysics* B.2 997b34–998a6, they are not precisely true of sensible things (except perhaps in the heavens—but not even there, if, as in Autolycus, astronomy assumes that stars are points); and yet Aristotle is unwilling to accept the Platonic positing of a separate mathematical realm.[6]

In one of his earliest and most famous articles, 'Aristotle on Geometrical Objects' [1970], Ian argued against the standard view that for Aristotle solid geometry (say) treats natural substances but not *qua* natural substances, by abstracting from their matter, weight, natural powers, and so on, and considering only their geometrical attributes. In the first place, Aristotle is clear that geometrical objects do have matter, although a special kind of matter, 'intelligible matter': Aristotle does speak of mathematical objects as arising from 'abstraction' without properly explaining what that means; but this must be abstracting from natural attributes, not abstracting from matter so as to yield a universal. (In fact, Aristotle never speaks of 'abstraction' of universals, only of mathematicals; it was Alexander of Aphrodisias who combined universals and mathematicals into a single theory of the agent intellect's operation in abstracting from *phantasmata*.) Mathematics is about universals only in the sense in which physics is also about universals: for Aristotle, as for Plato, since the Pythagorean theorem says that one square is equal to two others, it must be an assertion about three squares, not the single *universal* square but three things that fall under it. Furthermore, abstracting from natural attributes will not be enough to turn natural substances into geometrical objects: no natural substance is, say, a perfect tetrahedron; and abstracting from its weight and color

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[6] Myles Burnyeat [1987, 222 and n24] said that Ian was failing to see the Platonist character of Aristotle's argument at 997b34–998a6. Ian asked me what I thought about that, and I said,

> Well, I thought, 'If Ian didn't see that it was Platonist, then Ian was being pretty foolish'; but then Myles seemed to take that to mean 'Platonist *and not also Aristotelian*', and that's something else again.

Ian smiled and nodded.

will not turn it into one. If we turn it into a perfect tetrahedron by 'abstracting' from its bumps and cavities, that is not abstracting anymore: the tetrahedron would not be this substance under any description, but would rather be most but not all of this substance together with some parts of neighboring substances.

For these reasons Ian proposed, not that geometrical *objects* are natural substances with their natural attributes disregarded, but that geometrical *matter* is natural matter with its natural attributes disregarded, so that all that is left is three-dimensional extension; geometrical objects arise when particular shapes are 'imposed' on this geometrical matter. This seems to me to be clearly right as an interpretation of Aristotle; and it is puzzling that, while Ian's paper is constantly cited, the lesson does not really seem to have sunk in. The least satisfactory part of the article is the talk of 'imposing' shapes on matter: it is not clear how this is supposed to happen, but it sounds as if the imposition were purely mental, which seems in tension with the realism that Ian attributes to Aristotle. But I think the right answer to the difficulty—and I *think* that this was Ian's view, but am no longer sure—turns on what Aristotle says at *Metaphysics* M.3 1078a28–31, that geometers are talking about real beings 'because being is twofold, [what exists] in actuality and [what exists] materially.' This must mean that geometrical objects do not *actually* exist, but exist *potentially* within geometrical matter because the matter *can* be divided along, say, the face-planes of a perfect tetrahedron. Aristotle in general thinks that when some whole body actually exists, the various internal surfaces on which it could be divided potentially exist, and so do the various part-bodies into which these surfaces would divide it. Even if the actual bounding surfaces of bodies are never perfect planes or spheres and the actual bodies are never perfect geometrical solids, it seems Aristotelian to say that they have a potentiality for being divided along perfect planes and spheres into perfect geometrical solids: like the potentialities for infinity and the void, discussed in *Metaphysics* Θ.6 1048b9–17, this potentiality is never entirely actualized, but can come progressively closer and closer to being entirely actualized. So the geometers' theorems are not about what *actually* exists in sensible things, but about what *could* exist, what could be carved out of the matter of sensible things; and this is enough to make the theorems true and scientific.

Ian compared Plato and Aristotle with Euclid on mathematics, on demonstrative method rather than on ontology, in his early paper ‘Greek Mathematics and Greek Logic’ [1974]; and then, building on that, in his later paper ‘On the Notion of a Mathematical Starting Point in Plato, Aristotle and Euclid’ [1991b], which drew, or at least conjectured, some strong and surprising conclusions. The main claims of the earlier paper were that Greek mathematics was not detectably influenced by either Aristotelian or Stoic logic, and conversely that neither Aristotle nor Chrysippus were seriously influenced by examples of mathematical argument in formulating their syllogistics. Obviously, Aristotle gives mathematical examples, especially in the *Posterior Analytics*; but if he had ever tried regimenting geometry in any systematic way according to his syllogistic, he would have seen that it would not work: individual arguments might be shoe-horned in but not whole chains of arguments. Later Greek logicians do try harder to give an account of actual mathematical arguments: post-Chrysippan Stoics speak of ‘unsystematically concluding arguments’, e.g., from the transitivity of equality; and Galen redescribes at least some such arguments as ‘relational syllogisms’. The Epicurean Zeno of Sidon had attacked arguments in Euclid, and Posidonius had tried in response to patch up Euclid’s arguments by supplying the missing premisses, such as the transitivity of equality. Ian suggests that the discussion of ‘unsystematically concluding arguments’ and ‘relational syllogisms’ arises from Posidonius’ reply to Zeno, and that some of the dubious ‘common notions’ found in manuscripts of Euclid also arise from this later ancient attempt to plug logical gaps. But, as usual, Ian also intended a negative lesson, that this later ancient logical discussion was a series of patches with no systematic theory, and that Galen’s talk of the inadequacy of Aristotelian and Stoic syllogistic to the geometers’ practice should not fool us into thinking that his own theory of ‘relational syllogism’ was anything remotely like the modern predicate calculus.

‘On the Notion of a Mathematical Starting Point in Plato, Aristotle and Euclid’ [1991b] continues the work of pulling Euclid’s practice apart from (especially) Aristotle’s theory of science. According to the *Posterior Analytics*, a science has three kinds of starting points:

(1) hypotheses, by which Aristotle means especially the hypothesis of the existence of some domain of objects which the science will study;
(2) definitions, both of simple things like points (which are on a standard modern theory undefinable) and of complex things like triangles; and
(3) axioms, by which Aristotle means topic-neutral generalizations such as the law of non-contradiction and, apparently, 'equals added to equals are equal' and the like.

Euclid's *Elements* 1 also gives us three kinds of starting points: definitions, postulates, and common notions (further definitions are added in later books of the *Elements*, but no further postulates or common notions). It is tempting to try to match the two lists of three: it seems clear enough that Euclid's definitions correspond to Aristotle's definitions, and Euclid's common notions (such as 'equals added to equals are equal') to Aristotle's axioms; so by process of elimination, Euclid's postulates should correspond to Aristotle's hypotheses. Most but not all of Euclid's postulates postulate some activity, e.g., 'from any point to any point to draw a straight line'. If postulates like this were current in the geometry of Aristotle's time, and if Aristotle is trying to reflect them in his class of 'hypotheses', he must have deliberately disregarded their constructional aspect. He would, then, be analyzing their scientific contribution as equivalent to a $\forall\exists$ proposition, 'between any two points there is a straight line'—or rather, since he gives no sign of recognizing the logical difference between a $\forall\exists$ proposition and a purely existential proposition—just as 'there is a straight line between any two points', or even 'there are [enough] straight lines'. Aristotle would thus be trying to analyze what is accomplished in a geometer's constructions as well as in his arguments, but trying to analyze it purely in terms of argument, without mentioning anything distinctive that could be accomplished only by a construction.

Ian, however, thought that this kind of harmonization of Aristotle and Euclid was all a mistake. He noted that, in the *Elements* beyond book 1, all the explicitly posited starting points are definitions. We might think that this is because the common notions listed at the beginning of *Elements* 1 are supposed to hold of all types of quantity, and thus to be starting points for all of mathematics: Euclid might have the program of reducing his starting points to definitions, topic-neutral theoretical propositions (the common notions), and topic-specific *practical* propositions, construction postulates, which would occur only in geometry because constructions occur only in geometry. Ian rejected this, pointing out that the fourth postulate ('for all right angles to be equal') is a theoretical proposition, and that Euclid's postulates are not in fact sufficient for domains beyond plane geometry (e.g., for constructing a plane through three points, or even for adding two numbers). He proposed instead that writers before Euclid made definitions (and perhaps common notions) their only explicit starting points, that explicit postulates are Euclid's innovation, and that he did not carry out his project of making the postulates explicit systematically, but only for book 1. Furthermore, if earlier writers explicitly laid down definitions as starting points, they may well have done so, not to use them as premisses for demonstrations, but (as *Phaedrus* 237b7–d3 seems to recommend) to fix the references of terms, to ensure that speaker and hearers are thinking of the same object. Of course, mathematicians would sometimes lay down a hypothesis on which something can be proved or constructed (Plato testifies that they did); but this would be a hypothesis assumed for a particular proposition, not something laid down before the exposition of a whole mathematical discipline.

Ian also insisted on the difference between construction postulates and $\forall\exists$ propositions: a construction postulate is a license to construct something, as an inference rule is a license to infer something, and we can no more replace all construction postulates with $\forall\exists$ propositions than we can replace all inference rules with axioms. We might still think that Aristotle disregarded this difference, that for purposes of his analysis of the logical structure of geometry he treated construction postulates as equivalent to $\forall\exists$ or just existential propositions. But Ian thought, on the contrary, that Aristotle thought of construction as lying outside of the logical structure of geometry, that he intended his analysis of demonstration to apply only to the demonstration-in-the-narrow-sense of a geometrical proposition—to the argument that takes place after the construction is completed. If this is what Aristotle was trying to analyze, then he might reasonably think that the only premisses used in the demonstration would be common notions ('things equal to the same thing are equal', 'equals added to equals are equal', and the like). Ian thought this was in fact Aristotle's view—that only common notions are basic premisses in mathematics, that definitions function just to fix the meanings of terms and existence-hypotheses just to ensure that the terms do indeed refer. There are obvious objections to this interpretation (for instance, Aristotle says that we prove the existence of triangles, but 'triangle' cannot be in the conclusion of a valid argument if it is not in one of the premisses, and 'triangle' is not in the common notions or existence-hypotheses, so it seems that it must be in a definition that is taken as a premiss), and in the end I think that something like the Euclid-Aristotle harmonization that Ian was attacking is more likely to be right. Ian did not claim to have proved that it was impossible. But he wanted to force those who maintained it to acknowledge that it is a historical construction, not something explicit in the texts or forced on us by the texts, but a choice that we must take responsibility for, conscious of our fallibility as interpreters. And something like this was the goal of many of his papers.

A striking feature of 'Aristotle on Geometrical Objects' is that it is constantly in dialogue with the Greek commentators, Alexander of Aphrodisias but also the Neoplatonists, as much as with modern scholars. Ian was introduced to the Greek commentators when (as one of the few competent readers who could be found) he was asked to referee Glenn Morrow's translation of Proclus' commentary on Euclid's *Elements* 1, published in 1970 by Princeton University Press. Morrow found Ian's comments so helpful that (as he explained in the preface) he quoted many of them in his footnotes with the initials 'I.M.' attached [1970, xxxv]. Some 20 years later, when the Press reprinted the translation after Morrow's death, they would ask Ian to write a new foreword, which remains an excellent way into Proclus on mathematics. Morrow had been almost alone in America, along with L. G. Westerink, in his interest in the Greek commentators. (E. R. Dodds was for many years almost as isolated in England; the situation was better in France.) But from this time on, thus for 40 years, Ian's work on Plato and Aristotle, as well as on Euclid, was regularly in dialogue with late ancient commentators. He did not value them chiefly as sources of historical information that might be traced back to the days of Plato and Aristotle (undeniably Proclus' commentary on Euclid contains much information that goes back to the *History of Geometry* of Aristotle's student Eudemus—but Ian enjoyed poking holes in this 'information'), but rather for their engagement as interpreters of the primary texts. Sometimes he found them preferable to modern interpreters: certainly they knew the classical texts better than any of us do, had deeply internalized the question of how Plato or Aristotle would respond to any challenge, and were very sensitive to all the places where one text of Plato or Aristotle was in tension with another, or a text of Plato with a text of Aristotle; although, more than one of us would, they saw such tensions as problems to be solved by better interpretation. But he appreciated them especially because they asked different questions and approached the texts with different presuppositions, than we do; from across the centuries, their presuppositions are pretty obvious, and they help us to become aware of what we ourselves are often unconsciously presupposing and where our assumptions might be questionable. And he found the act of interpreting, of trying to make systematic sense of a text, to extract from it answers to our questions, intrinsically interesting and worth studying.

For these reasons, when Richard Sorabji began the enormous project of publishing The Ancient Commentators on Aristotle, and began trying to badger a crew of scholars (mostly experts on Aristotle and not on late ancient philosophy) into contributing a translation, Ian got increasingly involved: he made an outsized contribution to the effort, as translator (he translated more than any other contributor) and as vetter and improver of others' translations. He started with Alexander's attempts to interpret Aristotle's modal syllogistic: both Aristotle's and Alexander's texts are technically demanding enough that most other scholars would shy away from such a translation-assignment, but probably a particular source of interest for Ian was that Alexander was attempting the impossible, since Aristotle's modal syllogistic simply cannot be coherently interpreted *in toto*. But Ian's biggest contribution to the project was on Simplicius' commentary on the *De caelo*. Perhaps Ian initially seemed a plausible person to ask to help translate the *De caelo* commentary because of the technical astronomical and cosmological material (e.g., the history of measurements of the circumference of the earth) in Simplicius' commentary on *De caelo* 2. But Ian was also interested in the larger issues, about creation in time or from eternity, about the status of the heavens and of the meteorological domain, about the relation of a providential god with the world; and also issues about the relation between physics and mathematics, raised especially for

Simplicius by Aristotle's criticism of the *Timaeus*' reduction of the physical 'elements' to polyhedra and ultimately to triangles. And while Sorabji's translation project was limited to the commentaries on Aristotle (a few texts of other kinds got slipped in later), Ian was interested in the whole late Neoplatonic project of making sense of earlier philosophy and mathematics, not separating commentaries on Aristotle from commentaries on Plato or Euclid or Ptolemy.

Simplicius' commentary on the *De caelo* was called forth by Proclus' commentary on the *Timaeus*, which defended Plato against Aristotle's criticisms, in part by arguing that Plato did not hold the 'extremist' Platonist views which Aristotle attributed to him and which some later Platonists did indeed hold (e.g., that the world was created in time or that the heavens are made of the same kind of fire that exists in the sublunar realm), and in part by defending 'moderate' Platonist views against Aristotle's arguments. Once Plato has been 'saved' in this way, there is an obvious question whether Aristotle too can be saved: does he hold the 'extremist' Aristotelian views held by later Peripatetics, e.g., that God causes only motion and not being to the world, or that God is only a final and not an efficient cause, or does he hold only 'moderate' Aristotelian views that can be reconciled with moderate Platonism, and are his apparent criticisms of Plato themselves savable as criticisms only of Plato's extremist followers? These issues were especially urgent for Simplicius because John Philoponus, for Christian reasons, had recently attacked Aristotle and defended 'extremist' Platonist theses, and Simplicius wants to defend a united front of moderate Platonism and moderate Aristotelianism, in part to defend a united pagan philosophical heritage against the Christians. While Simplicius' project can be described as a 'harmonization' of Plato and Aristotle, Ian was very cautious about attributing to the late Neoplatonists in general a thesis of the harmony of Plato and Aristotle, and especially critical of attributing to them the simple solution that Plato is the authority on the intelligible world and Aristotle is the authority on the sensible world. On the contrary, Ian was very interested, especially in the last years of his life, in Proclus' and Simplicius' attempts to defend what he called the 'mathematical chemistry' of the *Timaeus* against Aristotle's objections.

Ian did not, in general, go into the study of late ancient interpretations with the expectation that they would be right as interpretations. He and Catherine Osborne got interested at about the same time in Hippolytus' *Refutation of all Heresies*, an important source for the Presocratics and various other thinkers, where Hippolytus tries to discredit each Christian heresy by showing that it has taken its ideas not from divine revelation but from some Greek philosopher. Both Ian and Osborne wanted to study Hippolytus' interpretations of those Greek philosophers, not just as sources for earlier thinkers, but as interpretations. But, as Ian said [1989a, 237] in his essay review of Osborne's *Rethinking Early Greek Philosophy: Hippolytus of Rome and the Presocratics*, Osborne sometimes seemed to speak as if we could not hope to interpret the Presocratics better than Hippolytus did, or as if all interpretations were equally valid. By contrast, Ian pointed out that when Hippolytus argued that the Naassenes, who worshiped the snake from the Garden of Eden and apparently associated it with life-giving moisture, had taken their ideas from Thales, it is just possible that Hippolytus' interpretive comparison might help us understand the Naassenes, but extremely unlikely that it will give any new insight into Thales.[7] But Ian could be very sympathetic to late ancient interpreters. He wrote at the end of his foreword to the second edition of Morrow's translation of Proclus' commentary on Euclid:

> To understand a philosophical or scientific text is to make sense of it, and what makes sense is relative to an outlook. Proclus' own outlook and the understanding of Plato on which it is based are not ours. So naturally his understanding of Euclid is not always ours. But his attempt to read Euclid in the light of his own philosophical outlook is not importantly different from a modern philosopher/teacher reading an ancient text in terms of his or her own philosophical perspective. Nor are Proclus' methods of teaching the text of Euclid fundamentally different from the methods we use: he pursues a general line of interpretation, a reading, while presenting a great deal of material about the history of his subject and of interpretations of his text and related matters. ... Proclus taught as a preserver of a noble intellectual

[7] For Ian's own approach to Hippolytus see also his 'Heterodoxy and Doxography in Hippolytus' *Refutation of All Heresies*' [1992b], 'Hippolytus, Aristotle, Basilides' [1994], and the apparently still not published 'The Author of the Refutation of All Heresies and His Writings'.

> heritage in a society increasingly indifferent and even hostile to that heritage. Many members of today's academy see themselves in a similar position. It is unlikely that this similarity of structure has no reflection in content. About eight hundred years separate Proclus from Socrates, Plato and Aristotle; only about two hundred years separate our 'postmodern' world from the Enlightenment. Proclus is not a postmodernist, but reflection on his ways of thinking and their relation to his time may shed light on the intellectual turmoil of our own. [1992c, xxx–xxxi]

Ian also wrote with evident sympathy that Proclus in this commentary was trying to persuade sometimes resistant philosophy students that it really is important for a philosopher to study at least elementary mathematics.

A particular fruit of Ian's study of the Neoplatonists was his paper 'Aristotle's Doctrine of Abstraction in the Commentators' [1990], in the collection edited by Richard Sorabji, *Aristotle Transformed*. This built on 'Aristotle on Geometrical Objects' [1970] and explored further some of its themes: the difference between abstracting from matter and abstracting from irrelevant predicates, the status of mathematical matter, the way shapes are imposed on mathematical matter, how far mathematical objects are mind-dependent. But Ian was not expecting the ancient commentators to agree with his own interpretation of Aristotle: both Alexander and the Neoplatonic commentators, in different ways, make mathematical objects more mind-dependent than any of the most likely modern contenders do. Alexander takes mathematicals, like universals, to exist only in the soul as a result of the agent intellect's act of abstraction: in both cases, the way in which we understand the things does not match the way in which they exist outside the soul; but this does not involve falsehood, since we are not adding to the things anything that is not there but only abstracting, i.e., taking away from the things something that is there. As Ian shows, Alexander's account is taken up by Neoplatonists including Porphyry and Ammonius but is rejected by more radical Platonists beginning with Syrianus: all Neoplatonists think that mathematics serves as a bridge leading us up from the sensible to the intelligible world; but if the abstractionist account is correct, how can it do so? This worry leads Syrianus to work out the alternative account which Ian calls 'projectionism': mathematicals exist, not outside the soul in a world intermediate between sensibles and Forms, but only in the soul's imagination. But rather than coming up from sensation by the imagination's recombining images taken from sensible things, they come down from the rational soul by the soul's 'projecting' some concept, creating an illustrative image of it in the imagination. This is the only way in which mathematical objects can, for example, be precisely tetrahedral when sensible objects are not (if the soul can correct the imperfections of what it takes in from the senses, it must be looking at an intelligible paradigm and must be able to reproduce this paradigm in imagination).

Projectionism allows Syrianus, and Proclus following him, to reinterpret both Aristotle's reports of Plato on intermediate mathematicals (they are 'intermediate' because soul is intermediate between the intelligible and sensible worlds), and also what Plato says about mathematical thought in the Divided Line: the mathematician might not be dependent on external diagrams (as a straightforward reading of the *Republic* would suggest) but he is still dependent on 'diagrams' in the imagination in order to set out his propositions in an individual instance and thus to demonstrate them. Although Ian does not work out all the historical connections here, he knew that, in rediscovering and clarifying projectionism, he had found something with a historical influence far beyond the philosophy of mathematics. Projectionism must somehow have arisen from Plotinus' description of the creative activity of the lower world-soul or nature at *Enn.* 3.8.4 (nature is represented as saying that its contemplation produces bodies as a kind of diagram, 'as the geometers draw when they contemplate, except that I do not draw, but only contemplate, and the outlines of bodies are spontaneously produced'), which Coleridge [1817, 254] was to cite and to try to syncretize with post-Kantian idealism. And projectionism must also somehow be the source of ideas in Avicenna and Ibn ᶜArabī about a 'world of images', generated by the soul in accordance with its character and midway between the sensible world and the separate intelligences (or the divine attributes), in which the Qur'ānic events of the Last Day take place. Ian thought that Syrianus was probably using the projectionist account of mathematical things only to interpret Pythagorean 'symbolic' statements about numbers rather than real mathematics, but that Proclus turned it to good use as a philosophy of geometry. Here as elsewhere Ian shows deep respect for Proclus as someone who valued and tried to make sense of the real discipline of mathematics, while too many other philosophers just tried to exploit the prestige of mathematics without interest in its content.[8]

I want finally to talk about two further highly reflective papers of Ian's, devoted to analyzing the current impasses of Plato scholarship and assaying the prospects for emerging from them: 'Joan Kung's Reading of Plato's *Timaeus*' [1989b] and 'The Esoteric Plato and the Analytic Tradition' [1993]. Both papers should be read much more widely than they have been.[9]

The Joan Kung paper arose from a sad personal circumstance. Joan taught Greek philosophy at Marquette University in Wisconsin, and was an enthusiastic participant in Chicago events in Greek philosophy and a friend of Ian's and of many others in Chicago; she fell mysteriously ill in late fall 1986, was diagnosed with liver cancer, and died only six weeks after her diagnosis, aged 48, leaving an unfinished book-manuscript, 'Nature, Knowledge and Virtue in Plato's *Timaeus*.' Her friends held a memorial conference on her work and the different papers were published as a special number of *Apeiron* with almost the same title as Joan's manuscript, *Nature, Knowledge, and Virtue* [Penner and Kraut 1989]. The organizers gave Ian Joan's computer and told him to figure out what she was trying to do with the *Timaeus*. Joan's manuscript was not as far along as had been hoped and Ian could not fully reconstruct an argument that Joan had not yet finished making. But he took the occasion to reflect on the challenges that Joan was trying to overcome in her reading of the *Timaeus*; and this led him to reflect more broadly on the deadlock over the *Timaeus* (represented in the exchange between Owen and Cherniss), and more broadly still on the problems of interpreting Plato in the second half of the 20th century.

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8 Ian's conclusions about the contrast between Proclus and the Iamblichan tradition were close to those drawn more or less simultaneously by Dominic O'Meara [1989]. See also Ian's 'Iamblichus and Proclus' Euclid Commentary' [1987a], besides his foreword [1992c] to the second edition of Morrow's translation and his 'Mathematics and Philosophy in Proclus' Euclid Commentary' [1987b].

9 The 'Esoteric Plato' paper was published in *Méthexis* in Buenos Aires: searches on Google Scholar and Google Book suggest that it has been cited only twice in English, more often in other languages.

Ian saw the problems as arising fundamentally from the breakdown of an older commonplace interpretation of the theory of Forms as a theory of concepts or meanings motivated by the conviction that there is no satisfactory referent in the sensible world for the terms that Socrates was trying to define. That older interpretation has trouble making sense of, for instance, the *Phaedo* on Forms as causes, the *Republic* on the Form of the Good as the source of being and intelligibility, or the *Symposium* on the Form of Beauty as the highest object of desire. As Ian put it,

> such views can be and have been accommodated to the interpretation of the Theory of Forms as a theory of meaning by arguing that, for example, Plato is given to hyperbole and uses terms like 'cause' and 'being' in ways broader than we do; but such moves do not completely allay one's misgivings. [1989b, 6]

Scholars might allow Plato to find such heavy metaphysical implications in his solution to the problem of meaning

> as long as [they] were willing to be fairly easy-going in their expectations concerning the reasonableness and intelligibility (to us) of a philosopher of antiquity, [1989b, 6–7]

but the development of analytic philosophy raised the standards, and the old solutions were no longer convincing. The most popular solution was to hold that the full metaphysical theory of Forms was an excess of Plato's middle period, from which he had recovered by the time of what Owen called 'the profoundly important late dialogues'. Unfortunately, this is untenable if the *Timaeus* is a dialogue of Plato's last period—which it is. Since at the time of Ian's paper many Plato scholars in the analytic tradition still believed, or tried to believe, that Owen had won the argument against Cherniss or at least that he had held him off to a standstill, Ian added a long digression on the evidence for dating, which involved Ian in an enormous amount of technical work, and which remains the best available broad introduction to the uses of stylometry in dating Plato's dialogues [1989b, 8–20]. While Owen had, of course, mainly content-based reasons for putting the *Timaeus* in the middle period, he also tried to show that the stylometric evidence supported this dating or that, at a minimum, it pointed both ways and allowed us a choice. Ian completely exploded these claims and exposed Owen's manipulations of the evidence. Then, he got back to his and Joan's problem: how do we make sense of the Forms, the receptacle, the mathematically described human and cosmic souls, and the polyhedra associated with the physical elements, which we find alongside the Forms in the *Timaeus*?

Joan's basic thought, which Ian endorsed, was that Plato was positing the Forms, and these other entities, not as *meanings* but as *causes*, as part of a would-be reductionist theory of the world and of human beings. That is, it would be reductionist in trying to reduce the phenomenal entities to posited abstract entities (what we call fire is just lots of little tetrahedra), not in trying to ground phenomenal *laws*, since any phenomenal laws that we can formulate are probably just misleading approximations.[10] Joan thought Plato's positings of abstract entities and his reductionist project were aiming at a unified theory not just of the physical world but also of the soul (the cause of motion and order in the physical world), including both its cognitions and its virtues—hence her title 'Nature, Knowledge and Virtue in Plato's *Timaeus*.' Ian agreed with all this, but unlike Joan he stressed the failure of Plato's explanatory and unifying projects.[11] Ian thought that Plato's approach to mathematical science was reactionary even for his own time—geometers had moved on from Plato's almost-Pythagorean obsession with numbers (i.e., integers)—and that what Plato was laying out was not, as Joan thought, a scientific theory, but a poetic amateur sketch of what a worldview based on science might look like.

The deadlocks about the theory of Forms, and about the *Timaeus*, are connected with the even deeper deadlock in the scholarship

[10] On Joan's interpretation, the Forms are 'real properties of things', causally explanatory properties, which may be quite different from the phenomenal properties captured by our language. Joan, influenced by Quine, contrasted Plato's approach with Aristotelian essentialism; but David Charles' interpretation of Aristotle's essences [2000] as causes rather than meanings brings Aristotle closer to Joan's Plato. Ian developed his own thought about Forms as causes in 'Platonism and the Study of Nature' [Mueller 1998].

[11] As Ian wrote elsewhere,

> subsequent history has shown that Plato was in a certain sense uncannily right about the scientific power of number. It has not, alas, confirmed his view of the connection between scientific and moral understanding. [1991b, 104]

about Plato's 'unwritten teachings', which Ian analyzed in 'The Esoteric Plato and the Analytic Tradition'. The analytic Plato-scholars of the time tried their best never to mention the topic. Sometimes they said that Cherniss had shown that Aristotle's reports of Plato's teaching arose from projecting Aristotle's own concepts back onto the dialogues (although, for the theories of numbers and their principles, Cherniss was supposed to show this in the unwritten, and unwritable, volume 2 of *Aristotle's Criticism of Plato and the Academy*). Sometimes they tried to show that the subject was not worth studying (so Vlastos and Burnyeat, in passages Ian cites at the beginning of his paper). But the impasse was worse than that: Ian cited not just analytic scholars' contemptuous dismissals of the Tübingen school, but each school's contemptuous dismissals of the others (including Krämer's quite amazing denunciation of all his opponents, and Gadamer's comparison of the Tübingers' doctrinal results to 18th-century school-metaphysics), and he asks what is to be done. As Ian says,

> the problems here are not simply intellectual or 'scientific'. Enormous personal commitments are involved, commitments which are reinforced by institutions of historical scholarship based on distinct schools of interpretation each of which pushes its 'line' as far as it can be pushed. [1993, 116]

The Platonic data simply underdetermine interpretation, and Ian saw no alternative to 'personal commitments' guiding our interpretation; but he thought that, if we were conscious of our own and others' presuppositions, we could secure agreement on some issues and at least understand other scholars' reasons for disagreeing with us on disputed points. Ian thought the discussion had led, or should have led, to the agreed results that 'Plato placed a higher value on oral than on written communication'; that 'the *agrapha dogmata* to which Aristotle refers at *Physics* 209b14–15 are ideas which Plato expressed orally', including an account of first principles, lying behind many of Aristotle's (correct or incorrect) extended descriptions of Plato's views; and, furthermore, that although there were unwritten teachings there were no *secret* teachings [1993, 119].

The importance of the unwritten teachings for the larger interpretation of Plato remains, of course, very much in dispute. The different schools' justifications of their positions on this tend, perhaps surprisingly, to turn on chronology, as in the case of the *Timaeus*. The standard view seems to be that Plato worked out (or tried out) the unwritten doctrines only late in life; and this seems to make them irrelevant to the interpretation at least of most of the dialogues. Krämer tried to find allusions to the unwritten teachings even in early dialogues and concluded that they were an unvarying underpinning of all the dialogues; while several leading analytic scholars, connecting the Lecture on the Good with *Republic* 6–7 on mathematics and the Good itself, argued that the unwritten teachings were part of the excesses of Plato's middle period, which he later abandoned—and so they would be irrelevant to the interpretation of 'the profoundly important late dialogues'. Ian argued [1993, 121–122], building on what he had done in the Joan Kung paper, that the *Timaeus* has 'clear references to an unstated theory of principles' in 48b3–d1 and 53d4–7 and, therefore, that this whole attempt at chronological damage-limitation collapses if the *Timaeus* is a late dialogue, which, of course, it is. But if the unwritten teachings and at least the middle-through-late dialogues are going on at the same time, how are they related? The analytic school and the Tübingen school should be able to agree that the dialogues present partial and tentative results from an ongoing series of live dialectical discussions, and that this incompleteness means that the interpreter has to 'come to the aid' of the written statements (the phrase is from *Phaedrus* 278c4–6). But how? For the Tübingen esotericist, by showing how they flow from the unwritten teachings. For the analytic scholar, the reason that Plato has not said anything clear in the dialogues about the theory of principles is that he has not worked it out to his satisfaction and has decided to make his arguments without it; and the interpreter too should 'come to the aid' of the proposals in the dialogues by filling in arguments from plausible premisses that do not depend on grand metaphysical hypotheses.

The esotericists, at their best, do not think of the unwritten teachings as a set of formulae immune to dialectical debate which would explain the dialogues and not be explained by them. Gaiser is clear in 'Plato's Enigmatic Lecture on the Good' [1980], probably the most sympathetic introduction to the Tübingen approach for non-sympathizers, that while the unwritten teachings could be expressed in a few short formulae, those formulae would be uninteresting and meaningless when detached from any ongoing dialectical investigation: Plato refuses to put them in writing, not because he is keeping something valuable from us, but because we can find value in them only if we reach them starting from the dialogues. Nonetheless, as Ian saw it [1993, 128], the goal of interpreting the dialogues remains for Gaiser 'an all-encompassing theoretical vision which cannot in any real sense be articulated', resulting from lifelong dialectical investigation and at least symbolically represented by the unwritten teachings: this belief in an intellectual intuition as the Platonic goal fundamentally differentiates the Tübingen school from the analytic tradition and even from Gadamer. Ian thought Gaiser was probably right that Plato was aiming at some such vision, and that this fact is important in interpreting the dialogues. But, as in the Joan Kung paper, Ian stressed that the project is a failure. Gaiser was surprisingly credulous about the scientific character of the *Timaeus* as filled out by the unwritten teachings (citing, e.g., Heisenberg's warm words about the *Timaeus*). But the 'reductions' of the soul and the physical elements to mathematical principles, which both Kung and Gaiser laid great hopes on, cannot be turned into anything like science, not even fourth-century BC science: Plato 'was at best a naïve enthusiast for science', and not only the 'scientific' details but also the general 'scientific' picture that they are supposed to illustrate are, Ian says, ultimately empty.

> Although reference to the *dogmata* gives us a proper historical perspective on Plato, it does not deepen our philosophical understanding of his physics or metaphysics. On the contrary, it enables us to see that we were probably wrong to be looking for a deep understanding of at least his treatment of the simple bodies. ... That may be an unwelcome result, but gains in historical understanding need not always be pleasant. [Mueller1993, 131]

I think Ian's article is an excellent example of the progress that can be made by sympathetically understanding the work of radically different scholarly traditions and forcing them into discussion with each other. But it also raises the question why he cared so much—why devote so much effort to interpreting Plato, if what Ian says about him is true? Ian clearly had a deep lifelong love for Plato and for some aspects of Neoplatonism in a way that he did not for Aristotle or Euclid despite all his contributions to understanding them. Friends of his whom I have talked to have said that they too thought Ian was somehow a natural Platonist. But Ian thought that we moderns were, or at least that he personally was, barred from simply appropriating the language of soul and God, or the conflation of mathematical and value-language, as describing objective features of reality. His unpublished paper 'From "Know Thyself" to "I Think, Therefore I Am": Self-Knowledge and Self-Consciousness' shows that he thought the Platonists were in some way existentially sensitive to depths of the self that were flattened out by Descartes' theories, and apparently also by the Stoic theories that the Neoplatonists attacked.[12] But he also showed his Platonism by holding all formulations of these 'depths' to high standards of precision, finding them all wanting, and concluding in *aporia*.[13]

This was also Ian's teaching method. His student Eric Schliesser wrote on the memorial blog set up by the University of Chicago philosophy department,

> His graduate teaching style can be best described as follows: you take a canonical text. You go through it line by line with your students, eliciting from them the now standard/canonical (often very dull) reading (sometimes you assign that, too). You then carefully show with them how it cannot possibly be right. Then you draw attention to an exciting, non-standard reading. Just before the end of class you show it, too, has fatal objections. Class ends (like a Platonic dialogue) in *aporia*. Repeat exercise at next class.[14]

This teaching style was not good at telling students who needed to be told what Plato or Aristotle were about, nor at motivating

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[12] I tried to get him to insert the Stoics into his story of philosophers on self-knowledge, but he would not. He told another of his students, 'Epictetus is not a philosopher with whom I conjure'.

[13] Eric Brown and Zena Hitz recall Ian reading out in class, with evident identification, a passage from Maimonides' *Guide of the Perplexed* 2.24:

> The extreme predilection that I have for investigating the truth is evidenced by the fact that I have explicitly stated and reported my perplexity regarding these matters as well as by the fact that I have not heard nor do I know a demonstration as to anything concerning them. [Pines 1963, 327]

[14] To read the blog, go to http://lucian.uchicago.edu/blogs/mueller/2010/08/24/guest-book/#comment-5.

students who came in needing to be motivated—there were several students who left in disillusion. But it was very good for those of us who came in full of enthusiasm and certainty about what the texts were about, and who needed to be shown the difficulties that any interpretation must confront. If he was convinced that we understood the responsibilities, he was respectful of our 'personal commitments' in interpretation (as in his 'Esoteric Plato'), even when he could not share them: he did not try to shape us either into his own model or into the model of the analytic school, although he warned us that when we got out into the wider world we would need to deal with it.[15]

Students who worked with Ian on their dissertation (not necessarily as first reader) included Michael Wedin, Deborah Modrak, Stephen Menn, Rachana Kamtekar, Eric Brown, Wes Sandel, David Rehm, Scott Schreiber, Eric Schliesser, Erik Curiel, James Wilberding, Brian Johnson, and Zena Hitz (who finished her PhD at Princeton University but remained close to Ian); I am sure I am missing other names. Many of us came back to Chicago to speak at a lovely conference for Ian on the occasion of his retirement in 2002. Some more senior figures were also there: Myles Burnyeat gave his paper 'Eikōs Muthos' [2005], a remarkable change from the old analytic dismissal of the *Timaeus*. It was certainly easy enough to pick up a tone of pessimism from Ian. But he had a career of accomplishments in research and teaching that he could be justifiably proud of, he had helped to transform the profession of ancient philosophy, and he seemed deeply gratified by the conference. He took his teaching and supervisory responsibilities very seriously, and we must have caused him much annoyance and anxiety. He was also not happy with the direction that the Chicago philosophy department was going in. But after he retired, he seemed to all of us to have become a much happier person. He kept working long hours in his little

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15 I remember that when I asked him what literature to look at for one paper I was writing, he told me to write it first, look at the literature later, and stick in footnotes if necessary. And when I gave him a draft of what became my first published paper, he sent back several pages of comments, with some comments marked 'IM', others marked 'OX', and others marked 'OX, IM.' I figured that 'IM' were his initials, but had to ask him what 'OX' meant; he said, 'oh, I figured that's what they'd say at Oxford.' The comments marked 'OX, IM' were things that they would say at Oxford which he agreed with too.

study in Regenstein library with his computer and the *Commentaria in Aristotelem Graeca*, as before; and he and Janel were happy together, as before. He threw himself with amazing productivity into his work for Richard Sorabji's translation series which, without the anxieties of writing monographs, allowed him to make excellent use of his erudition, his familiarity with the language and thought of the commentators, his knowledge of the permanent difficulties of the texts they were commenting on, and his constant effort for conceptual and linguistic exactness. He was also able to travel, for scholarly and other purposes; he and Janel had been just about to start splitting their time regularly between Chicago and London. He should have had more years for all this, but it was a happy ending.

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BIBLIOGRAPHY

Burnyeat, M. 1987. 'Platonism and Mathematics: A Prelude to Discussion'. Pp. 213–240 in A. Graeser ed. *Mathematics and Metaphysics in Aristotle/Mathematik und Metaphysik bei Aristoteles*. Bern/Stuttgart.

________ 2005. 'Eikōs Muthos'. *Rhizai* 2:143–165. Repr. pp. 167–186 in C. Partenie ed. *Plato's Myths*. Cambridge, 2008.

Charles, D. 2000. *Aristotle on Meaning and Essence*. Oxford.

Cohen, P. 1963. 'The Independence of the Continuum Hypothesis'. *Proceedings of the National Academy of Sciences of the United States of America* 50.6:1143–1148.

________ 1964. 'The Independence of the Continuum Hypothesis, II'. *Proceedings of the National Academy of Sciences of the United States of America* 51.1:105–110.

Cohen, P. 1966. *Set Theory and the Continuum Hypothesis*. New York.

Coleridge, S. T. 1817. *Biographia Literaria*. vol. 1. London.

Gaiser, K. 1980. 'Plato's Enigmatic Lecture on the Good'. *Phronesis* 25:5–37.

Morrow, G. 1970. *Proclus: A Commentary on the First Book of Euclid's* Elements*, Translated with Introduction and Notes.* Princeton. Repr. with a Foreward by Ian Mueller. Princeton, 1992.

Mueller, I. 1970. 'Aristotle on Geometric Objects'. *Archiv für Geschichte der Philosophie.* 52:156–171. Repr. in

- pp. 96–107 in J. Barnes *et al.* edd. 1979. *Articles on Aristotle.* vol. 3. London.
- T. Irwin ed. 1995. *Classical Philosophy, Collected Papers.* vol. 7. Hamden, CT.

________ 1974. 'Greek Mathematics and Greek Logic'. Pp. 35–70 in J. Corcoran ed. *Ancient Logic and its Modern Interpretations.* Dordrecht.

________ 1981. *Philosophy of Mathematics and Deductive Structure in Euclid's* Elements. Cambridge, MA.

________ 1987a. 'Iamblichus and Proclus' Euclid Commentary'. *Hermes* 115:334–348.

________ 1987b. 'Mathematics and Philosophy in Proclus' Euclid Commentary'. Pp. 305–318 in J. Pépin and H. D. Saffrey edd. *Proclus. Lecteur et interprète des Anciens.* Paris.

________ 1989a. 'Hippolytus *Retractatus*'. *Oxford Studies in Ancient Philosophy* 7:233–251. Repr. in T. Irwin ed. 1995. *Classical Philosophy, Collected Papers.* vol. 1. Hamden, CT.

________ 1989b. 'Joan Kung's Reading of Plato's *Timaeus*'. Pp. 1–27 in T. Penner and R. Kraut, edd. *Nature, Knowledge, and Virtue. Apeiron* 22.4.

________ 1990. 'Aristotle's Doctrine of Abstraction in the Commentators'. Pp. 463–480 in R. Sorabji ed. *Aristotle Transformed: The Ancient Commentators and Their Influence.* London/Ithaca, NY.

________ 1991a. 'Mathematics and Education: Notes on the Platonist Program'. Pp. 85–104 in I. Mueller ed. *ΠΕΡΙ ΤΩΝ ΜΑΘΗΜΑΤΩΝ: Essays on Greek Mathematics and Its Later Development. Apeiron* 24.4.

________ 1991b. 'On the Notion of a Mathematical Starting Point in Plato, Aristotle, and Euclid'. Pp. 59–97 in A. C. Bowen ed. *Science and Philosophy in Classical Greece.* London/New York.

Mueller, I. 1992a. 'Mathematical Method and Philosophical Truth'. Pp. 170–199 in R. Kraut ed. *Cambridge Companion to Plato*. Cambridge.

———— 1992b. 'Heterodoxy and Doxography in Hippolytus' *Refutation of All Heresies*'. Pp. 4309–4374 in *Aufstieg und Niedergang der römischen Welt*. Pt. II. vol. 36.6. Berlin/New York.

———— 1992c. 'Foreword'. Pp. ix–xxxi in G. R. Morrow. *Proclus: A Commentary on the First Book of Euclid's* Elements. Princeton.

———— 1993. 'The Esoteric Plato and the Analytic Tradition'. *Méthexis* 6:115–134. Trans. 'El Platón esotérico y la tradición analítica'. *Méthexis* Suplemento para países de habla hispana 6:111–130.

———— 1994. 'Hippolytus, Aristotle, Basilides'. Pp. 143–157 in L. P. Shrenk ed. *Aristotle in Late Antiquity. Studies in Philosophy and the History of Philosophy* 27.

———— 1998. 'Platonism and the Study of Nature (*Phaedo* 95e ff.)'. Pp. 67–89 in J. Gentzler ed. *Method in Ancient Philosophy*. Oxford.

———— 2005. 'Mathematics and the Divine in Plato'. Pp. 99–121 in T. Koetsier and L. Bergmans edd. *Mathematics and the Divine*. Amsterdam.

O'Meara, D. 1989. *Pythagoras Revived: Mathematics and Philosophy in Late Antiquity*. Oxford.

Penner, T. and Kraut, R. 1989. *Nature, Knowledge, and Virtue. Apeiron* 24.4. Edmonton, AB.

Pines, S. 1963. trans. *Moses Maimonides:* The Guide of the Perplexed*, with an Introductory Essay by Leo Strauss*. Chicago.

BIBLIOGRAPHY OF IAN MUELLER'S PUBLICATIONS[16]

Monograph

1. Mueller, I. 1981. *Philosophy of Mathematics and Deductive Structure in Euclid's* Elements. Cambridge, MA.

Translations with notes and commentary

2. 1999. *Alexander of Aphrodisias: On Aristotle's* Prior Analytics *1.8–13.* London/Ithaca, NY. (with J. Gould)
3. 1999. *Alexander of Aphrodisias: On Aristotle's* Prior Analytics *1.14–22.* London/Ithaca, NY. (with J. Gould)
4. 2004. *Simplicius: On Aristotle's* On the Heavens *2.1–9.* London/ Ithaca, NY.
5. 2005. *Simplicius: On Aristotle's* On the Heavens *2.10–14.* London/ Ithaca, NY.
6. 2006. *Alexander of Aphrodisias: On Aristotle's* Prior Analytics *1.23–31.* London/ Ithaca, NY.
7. 2006. *Alexander of Aphrodisias: On Aristotle's* Prior Analytics *1.32–46.* London/ Ithaca, NY.
8. 2009. *Simplicius: On Aristotle's* On the Heavens *3.1–7.* London/ Ithaca, NY.
9. 2009. *Simplicius: On Aristotle's* On the Heavens *3.7–4.6.* London/ Ithaca, NY.

Forthcoming

10. *Simplicius: On Aristotle's* On the Heavens *1.2–3.*
11. *Simplicius: On Aristotle's* On the Heavens *1.3–4.*
12. *Simplicius: On Aristotle's* Physics *1.7–9*, half-volume.

[16] This list is based on a *Curriculum vitae* of Ian's and should be fairly complete through about 2006. I have supplemented the list from various sources and added items that were published later or are still forthcoming; but I may well be missing some recent publications, and perhaps especially book reviews (where Ian had listed only one piece after 2000). I would appreciate hearing about anything that I have missed.

Edited Collection

13. 1991. *ΠΕΡΙ ΤΩΝ ΜΑΘΗΜΑΤΩΝ*: *Essays on Greek Mathematics and Its Later Development. Apeiron* 24.4. South Edmonton.

Articles

14. 1969. 'Zeno's Paradoxes and Continuity'. *Mind* 77:129–131.
15. 1969. 'Stoic and Peripatetic Logic'. *Archiv für Geschichte der Philosophie.* 51:173–187. Repr. in T. Irwin ed. 1995. *Classical Philosophy, Collected Papers.* vol. 8. Hamden, CT.
16. 1969. 'Euclid's Elements and the Axiomatic Method'. *British Journal for the Philosophy of Science* 20:289–309.
17. 1970. 'Aristotle on Geometric Objects'. *Archiv für Geschichte der Philosophie.* 52:156–171. Repr. in
    - pp. 96–107 in J. Barnes *et al.* edd. 1979. *Articles on Aristotle.* vol. 3. London.
    - T. Irwin ed. 1995. *Classical Philosophy, Collected Papers.* vol. 7. Hamden, CT.
18. 1970. 'Homogeneity in Eudoxus' Theory of Proportion'. *Archive for History of Exact Sciences* 7:1–6.
19. 1974. 'Greek Mathematics and Greek Logic'. Pp. 35–70 in J. Corcoran ed. *Ancient Logic and its Modern Interpretations.* Dordrecht.
20. 1978. 'An Introduction to Stoic Logic'. Pp. 1–26 in J. M. Rist ed. *Stoic Philosophy.* Berkeley.
21. 1979. 'The Completeness of Stoic Logic'. *Notre Dame Journal of Formal Logic* 20:201–215.
22. 1980. 'Ascending to Problems: Astronomy and Harmonics in *Republic* VII'. Pp. 103–122 in J. Anton ed. *Science and the Sciences in Plato.* Albany.
23. 1980. 'Coping with Mathematics: the Greek Way'. Morris Fishbein Center for the History of Science and Medicine Publications 2. Chicago.
24. 1981. 'Aristotle and Simplicius on Mathematical Infinity'. Pp. 179–182 in *Proceedings of the World Congress on Aristotle.* Athens.
25. 1982. 'Aristotle and the Quadrature of the Circle'. Pp. 179–182 in N. Kretzmann ed. *Infinity and Continuity in Ancient and Medieval Thought.* Ithaca, NY.
26. 1982. 'Scepticism and Greek Geometry'. Pp. 69–95 in J. Barnes *et al.* edd. *Science and Speculation.* Cambridge.

27. 1983. 'Parmenides 133a–134e: Some Suggestions'. *Ancient Philosophy* 3:3–7.
28. 1986. 'On Some Academic Theories of Mathematical Objects'. *Journal of Hellenic Studies* 106:111–120.
29. 1987. 'Aristotle's Approach to the Problem of Principles in *Metaphysics* M and N'. Pp. 241–259 in A. Graeser ed. *Mathematics and Metaphysics in Aristotle/Mathematik und Metaphysik bei Aristoteles.* Bern/Stuttgart.
30. 1987. 'Hypatia'. Pp. 74–79 in L. S. Grinstein and P. J. Campbell edd. *Women of Mathematics: A Biobibliographic Sourcebook.* New York.
31. 1987. 'Iamblichus and Proclus' Euclid Commentary'. *Hermes* 115:334–348.
32. 1987. 'Mathematics and Philosophy in Proclus' Euclid Commentary'. Pp. 305–318 in J. Pépin and H. D. Saffrey edd. *Proclus. Lecteur et interprète des Anciens.* Paris.
33. 1989. 'Hippolytus *Retractatus*'. *Oxford Studies in Ancient Philosophy* 7:233–251. Repr. in T. Irwin ed. 1995. *Classical Philosophy, Collected Papers.* vol. 1. Hamden, CT.
34. 1989. 'Joan Kung's Reading of Plato's *Timaeus*'. Pp. 1–27 in T. Penner and R. Kraut, edd. *Nature, Knowledge, and Virtue. Apeiron* 22.4.
35. 1990. 'Aristotle's Doctrine of Abstraction in the Commentators'. Pp. 463–480 in R. Sorabji ed. *Aristotle Transformed: The Ancient Commentators and Their Influence.* London/Ithaca, NY.
36. 1991. 'Sur les principes des mathématiques chez Aristote et Euclide'. Pp. 101–113 in R. Rashed ed. *Mathématiques et philosophie de l'antiquité à l'age classique. Hommage à Jules Vuillemin.* Paris.
37. 1991. 'On the Notion of a Mathematical Starting Point in Plato, Aristotle, and Euclid'. Pp. 59–97 in A. C. Bowen ed. *Science and Philosophy in Classical Greece.* London/New York.
38. 1991. 'Mathematics and Education: Notes on the Platonist Program'. Pp. 85–104 in I. Mueller ed. *ΠΕΡΙ ΤΩΝ ΜΑΘΗΜΑΤΩΝ: Essays on Greek Mathematics and Its Later Development. Apeiron* 24.4.
39. 1992. 'Heterodoxy and Doxography in Hippolytus' *Refutation of All Heresies*'. Pp. 4309–4374 in *Aufstieg und Niedergang der römischen Welt.* Pt. II. vol. 36.6. Berlin/New York.
40. 1992. 'Foreword'. Pp. ix–xxxi in G. R. Morrow. *Proclus: A Commentary on the First Book of Euclid's* Elements. Princeton.

41. 1992. 'Mathematical Method and Philosophical Truth'. Pp. 170–199 in R. Kraut ed. *Cambridge Companion to Plato.* Cambridge.
42. 1993. 'The Esoteric Plato and the Analytic Tradition'. *Méthexis* 6:115–134. Trans. 'El Platón esotérico y la tradición analítica'. *Méthexis* Suplemento para países de habla hispana 6:111–130.
43. 1994. 'Hippolytus, Aristotle, Basilides'. Pp. 143–157 in L. P. Shrenk ed. *Aristotle in Late Antiquity. Studies in Philosophy and the History of Philosophy* 27.
44. 1995. Articles: 'Celsus', 'Doxographers', 'Hypatia', and 'Simplicius'. In R. Audi ed. *The Cambridge Dictionary of Philosophy.* Cambridge.
45. 1996. 'Euclid as Blundering Schoolmaster: A Problem of Proportion'. Pp. 145–152 in T. Berggren ed. *Proceedings of the Third International Conference on Ancient Mathematics.* Delphi.
46. 1996. 'The Triangles in Plato's *Timaeus*'. *Mathesis* 12:286–333 (even pages). Trans. 'Los triángulos en el Timeo de Platón'. *Mathesis* 12:286–333 (odd pages).
47. 1997. 'Greek Arithmetic, Geometry, and Harmonics: Thales to Plato'. Pp. 271–322 in C. C. W. Taylor ed. *The Routledge History of Philosophy.* vol. 1. London/New York.
48. 1997. 'Plato and Mathematics'. *Mathematische Semesterberichte* 44:1–17. (with B. Artmann).
49. 1997. Articles: 'Bryson', 'Clement of Alexandria', 'Early Greek Mathematics', 'Euclid', 'Hippolytus', 'Nemesius of Emesa', and 'Theon of Smyrna'. In D. J. Zeyl ed. *The Encyclopedia of Classical Philosophy.* Westport, CT.
50. 1998. 'Platonism and the Study of Nature (*Phaedo* 95e ff.)'. Pp. 67–89 in J. Gentzler ed. *Method in Ancient Philosophy.* Oxford.
51. 2000. 'Syrianus and the Concept of Mathematical Number'. Pp. 71–83 in G. Bechtle and D. J. O'Meara edd. *La philosophie des mathématiques de l'antiquité tardive. Actes du colloque international. Fribourg, Suisse (24–26 septembre 1998).* Fribourg, Switzerland.
52. 2000. 'Plato's Mathematical Chemistry and Its Exegesis in Antiquity'. Pp. 159–176 in P. Suppes, J. M. Moravcsik, and H. Mendell edd. *Ancient and Medieval Traditions in the Exact Sciences.* Stanford.
53. 2003. 'Remarks on Euclid's *Elements* I.32 and the Parallel Postulate'. Pp. 287–297 in R. Netz, ed. *New Questions and New Approaches in the History of Mathematics: I. Ancient Mathematics.*

54. 2004. 'Ideas and Numbers'. Pp. 109–132 in A. L. Pierris ed. *Aristotle on Plato: The Metaphysical Question.* Patras.
55. 2004. Some Translations in R. Sorabji ed. *The Philosophy of the Commentators.* London.
56. 2004. 'Remarks on Physics and Mathematical Astronomy and Optics in Epicurus, Sextus Empiricus, and Some Stoics'. Pp. 57–87 in P. Lang ed. *Re-inventions: Essays on Hellenistic and Early Roman Science. Apeiron* 37.4.
57. 2005. 'Mathematics and the Divine in Plato'. Pp. 99–121 in T. Koetsier and L. Bergmans edd. *Mathematics and the Divine.* Amsterdam.
58. 2005. 'Remarques sur les cinq mathèmata chez Platon'. Pp. 105–124 in M. Dixsaut and F. Teisserenc edd. *Études sur la* République *de Platon 2. De la science, du bien et des mythes.* Paris.
59. 2006. 'Physics and Astronomy: Aristotle's *Physics* II.2.193b22–194a12'. *Arabic Sciences and Philosophy* 16:175–206.
60. 2006. 'Greek Mathematics to the Time of Euclid'. Pp. 686–718 in M. L. Gill and P. Pellegrin edd. *A Companion to Ancient Philosophy.* Malden/Oxford/Carlton.
61. 2007. 'La physique et la philosophie. Le tournant conceptuel du IVe siècle'. Pp. 39–56 in J. Gayon and R. M. Burian edd. *Conceptions de la science. Hier, aujourd'hui, demain. Hommage à Marjorie Grene.* Bruxelles.
62. 2007. 'Physis Anthropeia.' Pp. 209–260 in A. L. Pierris ed. *Physis and Nomos.* Patras.
63. 2009. 'Aporia 12 (and 12 *bis*)'. Pp. 189–209 in M. Crubellier and A. Laks edd. *Aristotle's* Metaphysics Beta. Oxford.
64. 2010. 'What's the Matter? Some Neo-Platonist Answers'. Pp. 151–164 in R. D. Mohr and B. M. Sattler edd. *One Book, the Whole Universe: Plato's* Timaeus *Today.* Las Vegas.

Forthcoming

65. 'Generalizing about Polygonal Numbers in Ancient Greek Mathematics'. In K. Chemla ed. *History of Mathematical Proof in Ancient Traditions.* Cambridge.
66. 'The Author of the *Refutation of all Heresies* and His Writings'. *Aufstieg und Niedergang der römischen Welt.* Pt. II. vol. 27:4.

67. ‘Aristotelian Objections and Post-Aristotelian Responses to Plato’s Elemental Theory’. J. Wilberding and C. Horn edd. *Neoplatonic Natural Philosophy.* Oxford.

Reviews

68. 1969. N. Rescher, *Galen and the Syllogism. Archiv für Geschichte der Philosophie.* 51:209–212.
69. 1970. E. Maziarz and T. Greenwood, *Greek Mathematical Philosophy. Philosophical Review* 79:427–428.
70. 1972. D. R. Dicks, *Early Greek Astronomy to Aristotle. Classical Philology* 67:217–219.
71. 1974. A. Mourelatos, *The Route of Parmenides. Classical Philology* 69:145–147.
72. 1974. K. von Fritz, *Grundprobleme der Geschichte der Antiken Wissenschaft. Isis* 65:112–114.
73. 1976. J. Hintikka and U. Remes, *The Method of Analysis. Journal of Philosophy* 73:158–162.
74. 1977. M. Frede, *Die Stoische Logik. Philosophical Review* 86:226–229.
75. 1978. A. Preus, *Science and Philosophy in Aristotle’s Biological Works. Archiv für Geschichte der Philosophie* 60:65–68.
76. 1978. J. Annas, *Aristotle’s Metaphysics M and N. Philosophical Review* 87:479–485.
77. 1980. H. J. Waschkies, *Von Eudoxos zu Aristoteles. Historia Mathematica* 7:200–210.
78. 1981. K. von Fritz, *Schriften zur Griechischen Logik. Archiv für Geschichte der Philosophie* 62:41–46.
79. 1982. J. Lear, *Aristotle and Logical Theory. Philosophical Review* 91:625–628.
80. 1982. A. Preus, *Aristotle and Michael of Ephesus on the Movement and Progression of Animals. Isis* 73:591–592.
81. 1984. D. J. O’Brien, *Theories of Weight in the Ancient World*, vol. 1. *Classical Philology* 79:240–243.
82. 1986. S. Waterlow, *Nature, Change, and Agency. Archiv für Geschichte der Philosophie* 67:193–195.
83. 1990. G. E. R. Lloyd, *The Revolutions of Wisdom. Classical Philology* 85:155–159.
84. 1991. A. C. Lloyd, *The Anatomy of Neoplatonism. The Philosophical Quarterly* 41:510–512.

85. 1992. A. Grafton, *Forgers and Critics. Modern Philology* 90:407–409.
86. 1992. D. O'Meara, *Pythagoras Revived. Ancient Philosophy* 12:528–531.
87. 1993. B. Vitrac, *Euclide d'Alexandrie. Les* Eléments*, Livres I–IV. Ancient Philosophy* 13:456–460.
88. 1996. B. Vitrac, *Euclide d'Alexandrie. Les* Eléments*, Livres I–IV, V–IX. Historia Mathematica* 23:85–87.
89. 1996. H. Tarrant, *Thrasyllan Platonism. Ancient Philosophy* 16:246–255.
90. 1996. B. Vitrac, *Euclide d'Alexandrie. Les* Eléments*, Livres V–IX. Ancient Philosophy* 16:266–270.
91. 1997. P. Pritchard, *Plato's Philosophy of Mathematics. Ancient Philosophy* 17:458–461.
92. 1997. K. J. Flannery, *Ways into the Logic of Alexander of Aphrodisias. Physis* 34:695–699.
93. 1999. J. Mansfeld and D. T. Runia, *Aëtiana: The Method and Intellectual Context of a Doxographer. Classical Philology* 94:111–114.
94. 1998. L. Siorvanes, *Proclus: Neoplatonic Philosophy and Science. Philosophical Review* 107:600–603.
95. 2000. H. Thesleff, *Studies in Plato's Two-Level Model. Journal of the History of Philosophy* 38:272–273.
96. 2000. B. Vitrac, *Euclide d'Alexandrie. Les* Eléments*, Livre X. Ancient Philosophy* 20:243–247.
97. 2000. R. J. Hankinson, *Galen: On Antecedent Causes. Ancient Philosophy* 20:247–250.
98. 2000. 'Working with Trivial Authors'.[17] *Apeiron* 23:247–258.
99. 2005. F. De Haas and J. Mansfeld, edd. *Aristotle: On Generation and Corruption, Book I. Notre Dame Philosophical Reviews* 2005.06.17. http://ndpr.nd.edu/review.cfm?id=2961.
100. 2005. R. J. Hankinson, *Simplicius on Aristotle's* On the Heavens *1.5–9. Aestimatio* 2:119–126. http://www.ircps.org/publications/aestimatio/pdf/Volume2/2005-11-01_Mueller.pdf.
101. 2009. H. Baltussen, *Philosophy and Exegesis in Simplicius: The Methodology of a Commentator. Bryn Mawr Classical Reviews* 2009.05.19. http://bmcr.brynmawr.edu/2009/2009-05-19.html.

[17] Reflections on S. Cuomo, *Pappus of Alexandria and the Mathematics of Late Antiquity.*

A Response to Trifogli on Glasner, *Averroes' Physics*

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Professor Trifogli offers an informative and useful overview of Ruth Glasner's book *Averroes' Physics: A Turning Point in Medieval Natural Philosophy.*[1] It is far beyond my competence to comment on her disagreements with the book, except, I hope, for a single, but essential, methodological point. Before formulating it, in the interest of disclosure, let me state that Professor Glasner has been a close friend and an esteemed colleague of mine for many years; I do not think however that this biographical fact in any way interferes with what I am about to say.

Professor Trifogli writes as if Glasner started with a given body of textual evidence, and set out to 'reconstruct' Averroes' late physics. Weighing what she takes to be the 'evidence' against the reconstruction of Averroes' thought process as proposed by Glasner, she finds that the evidence is insufficient:

> What textual evidence does this complex system of revisions of the three *Physics* commentaries provide for Averroes' new physics? [82]

she asks before concluding, 'However, it is not supported by adequate textual evidence and is not in itself very convincing' [84].

A word on the state of the 'textual evidence' is in order. It is not the case that we have in hand two or more versions of each of Averroes' three commentaries on the *Physics* and then try to reconstruct the development of his thinking. The situation is much more complex. When one compares the manuscripts of the commentaries, some available in Arabic, some in Hebrew, some in Latin, one faces chaos: while a large basic text is (more or less) common to

[1] *Aestimatio* 7 (2010), 78–88. http://www.ircps.org/publications/aestimatio/pdf/Volume7/2010-12_Trifogli.pdf.

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the manuscripts of a given commentary, there are also various small textual units (a sentence or a few sentences) that are not part of this shared text and are found in some but not in all the manuscripts of a given text. Thus, we are confronted with a very large set of small unconnected textual units haphazardly (as it seems) accompanying the shared text in diverse manuscripts. A first question facing the researcher is, Can they all be assumed to go back to Averroes himself? Certainly not at the beginning of the research: at the outset one has rather to assume that some of these isolated textual units could be glosses by scribes or readers. Only when the global picture begins to emerge will one feel confident to decide which textual units are Averroean and which not.

Assume now that a selection has been made and that the inauthentic textual units have been eliminated. For each commentary, one then has a set of unsystematic variations between the manuscripts, which one takes to go back to Averroes. It seems natural to conjecture that they were penned at different moments and reflect different states of Averroes' thought. But they still form a chaotic gathering because almost each manuscript has its own text and textual variations. How was this chaos formed? Glasner (plausibly) assumes that over many years Averroes revised and added marginal glosses to a 'master copy' that was repeatedly copied by various scribes at different moments. Each such copy thus reflected a different state of advancement of the 'master copy'. Farther down the road, copyists and translators were confronted with manuscripts carrying differing texts and marginalia, and made decisions as to what should be copied and what not. The result is the observed chaos where two manuscripts of a given text are rarely identical.

One of Glasner's major achievements is this: by working through the thicket of the unshared textual units, she has introduced some intelligible order. Put differently: she has found a hypothesis that accounts for the evolution of Averroes' thought and allows her to assign each textual unit to a stage in this development, thereby arranging the textual units in chronological order. Recall that the textual units are not dated and that you cannot know to which chronological 'layer' any given textual unit belongs. Without knowing the pattern of the jigsaw puzzle—how can one even try to put the pieces in order? There is only one possible way (as far as I can see): to try to imagine different evolutionary patterns of Averroes' thought and to see if they fit the bill, i.e., if they allow a coherent, intellectually plausible ordering of the texts. To arrive from a chaos of unordered texts to a likely reconstruction of the evolution of Averroes' thought demands a considerable measure of imagination and intuition in addition to real philological competence in their languages, not to mention infinite patience.

This, then, is Glasner's major accomplishment: to have had the uncommonly penetrating insight that allowed her to transport herself into the mindset of Averroes (to rephrase Dilthey) and to envision a reconstruction of his thought, given a disordered body of textual units. This she did through a process of conjectures and refutations: she framed and rejected successive hypotheses before arriving at the one which she presented in her book and which in her judgment best accounts for the evidence that she had amassed.

When Professor Trifogli writes, 'it is not supported by adequate textual evidence', she writes as if the evidence was out there, independent of the gathering process that had constituted it. She overlooks that the 'evidence' itself is a constructed set of textual units that became 'evidence' through the long process of trial and error in which it was assembled. Constituting the body of 'evidence' and hypothesis-formation went hand in hand. It is, therefore, a bit misleading to write as if we had a body of evidence on the one hand and a hypothesis on the other. More important, the philosophy of science has long taught us that any body of evidence can be explained in a great many (in theory, infinitely many) different ways. The present case is no exception and conceivably Averroes' thought can be reconstructed in different ways than that proposed by Glasner. In such a situation, the only sound methodology of criticism is to show that an alternative reconstruction of Averroes' thought exists that does better justice to all the available texts. It is facile, and unfair, to content oneself with voicing the subjective feeling that Glasner's hypothesis 'is not in itself very convincing': one really must indicate a more convincing alternative. On this, however, Professor Trifogli does not say a word. The challenge is at her door.

The Alchemy of Glass: Counterfeit, Imitation, and Transmutation in Ancient Glassmaking by Marco Beretta

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In this book, Beretta sets out to demonstrate the role of glass in ancient alchemy, especially the role of glassmaking and glassworking in ancient alchemical theory [xi]. The extended argument of the book is that glass was seen as metallic, that glassblowing was achieved only after and because furnace-makers achieved sufficient temperatures, and that the rise of alchemy and of glassblowing were nearly contemporaneous and causally linked: I return below to these theses. Beretta emphasizes the complex and multicultural origins of alchemy [xi] and builds upon an earlier essay [2004, xiii], in which he raised several of the points developed in this book. As there, so here, Beretta deploys a wide range of sources. The slim volume is beautifully produced on high-quality paper in a sturdy binding, and is enriched with over five dozen well-reproduced high-resolution images, most in color, many of which depict objects rarely or never seen in print. There are five chapters:

(1) ‘Artificial and Natural Glass in Mesopotamia and Egypt’,
(2) ‘The Greek Philosophers: Between Crystal and Glass’,
(3) ‘A Technical Revolution: The Introduction and Cultural Impact of Glassblowing’,
(4) ‘Glass and Alchemy’, and
(5) ‘From Byzantine Glass to Early Modern Alchemy’

plus an epilogue. The relevance of glassmaking to alchemy is clear, though noting it is hardly novel [see, e.g., Keyser 1990]; and a modern work of synthesis on ancient alchemy is welcome. The emphasis throughout [e.g., 3–4, 37, 47, 84, 95n21] on the slippery border between artificial and natural stuffs is valuable, as is the collection of images of glass vessels of possible or certain alchemical function.

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The work should be on the shelf of every scholar working in ancient science or technology.

Alas, the book is a flawed gem and demands caution. Errors small and large pervade the text, and many of the arguments deployed are muddled or mistaken. Despite its beauty and value, readers must read with care. Six categories of defect could be fixed in a second edition and are the sort that might be found in any book in our imperfect world—I am sure there are some in this review—though rarely in such numbers. But the logical muddles and invalid conclusions seriously undermine the main thesis of the work.

Let me first describe the remediable defects:

(1) translation troubles,
(2) typographical or spelling errors,
(3) citation muddles,
(4) missing or garbled references,
(5) chronological confusions, and
(6) the use of outdated authorities.

Beretta has been ill-served by his editor(s), since all of these defects should have been caught before the work was printed. It is not their presence that is noteworthy and disappointing—every book has some—it is their total number.

Translation troubles might confuse the reader, as when 'vile' is used for base (metal) [x, 22, 109] and the 'asteroid' of Theophrastus must surely be a meteorite [49]; others are minor. Similar are the typographical errors, such as 'sardonic' for sardonyx [92–93] or 'breath' for breadth [132]. Some names are garbled, both ancient[2] and modern.[3] And some words from Latin or Greek are garbled.[4]

[2] 'Dami[n]geron' [53], 'Eut[h]oc[h]ius' [128, 195], 'Ira[e]naeus' [95n20], 'R<h>etorius' [54, 197], 'T<h>rasyllus' [102–103, 198], or even 'Trimalchus' for 'Trimalchio' [110].

[3] 'Dercahin' for Derchain [17] and Scar[a]borough [129, 188, 197].

[4] Thus, 'lap<is> lazuli' [17], 'hyalocides' for 'hyaloeides' [47], 'megnes' for 'magnes' [59], 'hyalöides hyton' for 'hyaloeides khiton' [70], 'artifici[fici]osum' [87n6], 'cheriokmeta' for 'cheirokmeta' [103n49], 'ungu[n]entarii' [112, Figures 7–8], and 'rython' for 'rhyton' [114–115].

Moreover, Beretta does not always make it easy to check his ancient sources, sometimes giving only page numbers in a translation (which might not be available to every reader); in addition, some references are incomplete or wrong.[5] For example, Anaximenes, Empedocles, Heraclitus, and Philolaus are cited [24–28] from the very reliable translation and commentary of Kirk, Raven, and Schofield 1983—but for readers lacking that book the fragment numbers in Diels and Kranz 1951 should be given at each occurrence rather than only once [24n4]. Moreover, the citations that *are* given are somewhat muddled.[6] Similarly, Beretta cites Theophrastus, *De lapidibus* by page numbers in the edition and commentary of Caley and Richards 1956 without always providing section numbers,[7] although Beretta's citations of Plato, Aristotle, Lucretius, Vitruvius, Strabo, and Pliny are all in order.

Modern citations too are sometimes garbled, especially at 87n7, where Beretta cites the *Gospel of Philip* in the translation by 'Wesley

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[5] See also:

- 111n77, where the missing citation of Pliny is 36.195, and add a reference to Stern 1999, 441–442 on the whole episode;
- 29 where the reference back is to page 26, citing *De igne* 73.

[6] Thus,

- 24n3: Heraclitus fr. 219 in Kirk, Raven, and Schofield 1983 = Diels and Kranz 1951, 22B90.
- 24n4: on Empedocles' theory, I would cite Inwood 2001 rather than Kingsley 1995; and the fragment cited, A37, does not correspond to the fragment quoted, which is Diels and Kranz 1951, 31B6.
- 25 and n6: correct 'love-lived' to 'long-lived' and 'offering' should be plural; and Empedocles fr. 356 in Kirk, Raven, and Schofield 1983 = Diels and Kranz 1951, 31B23.
- 27n13: the Empedocles fragment is from Aetius [Diels 1879, 2.11.2, not 2.11.1] = Diels and Kranz 1951, 31A51a.
- 27n15: Anaximenes fr. 154 in Kirk, Raven, and Schofield 1983 = Diels and Kranz 1951, 13A14, (from Aetius [Diels 1879, 2.14.3–4]).
- 28n17: the Philolaus fragment is Diels and Kranz 1951, 44A19 (from Aetius [Diels 1879, 2.20.12, not 2.25.11]); and a citation of Huffman 1993 266–270 would be good here.

[7] Some prefer the more recent edition and commentary of Eichholz 1965. See:

- 47n64: *De lap.* §30;
- 48n67: *De lap.* §§48–49; and
- 49n69: *De lap.* §24, where Beretta's page reference is in fact to the commentary section of Caley and Richards 1956.

Wisenberg'. Although Beretta does not give a source citation, this is from the website http://www.theologywebsite.com and the translator is Wesley W. Isenberg. But why not use the widely available and reliable translation in Robinson 1988?

Beretta's quotation [42] of the *Periplus maris erythraei* §6 from the magisterial edition of Casson [1989] omits the section number; and Beretta claims that the passage refers to India when in fact the items listed, including 'glass stones', are for export to Adulis, a port on the southern Red Sea near 15° N, 40° E [Casson 1989, 109–112]; the glass exported to India was unworked (ἀργή) [*Periplus* §§49, 56].

More serious, though still reparable, are the confusions over dates. Hecataeus of Abdera is placed in late Antiquity (the fourth century AD) rather than in the fourth century BC [14, possibly by a typo: <B>CE]. Diodorus of Sicily is once placed in the first century AD [30], perhaps another typo, since his correct date (first century BC) is given later [89]. Following the unreliable *Souda*, Beretta tentatively assigns Philostratus to the 'first century CE' [52]; but one of the works of Philostratus referred to, the *Life of Apollonios of Tyana*, concerns a man who died *ca* AD 97, and was written in the third century AD [see Anderson 1986]. Moreover, it was Philostratus the father of the author of the *Life of Apollonios of Tyana*, who wrote the other work referred to, the lost *Lithognomikon* [see Keyser and Irby-Massie 2008, 660].

The *Revelation* attributed to John is a mysterious book, but its date is pretty securely late first century AD [Mounce 1998, 15–21: cf. 11–15]. Thus, Beretta's dating of the work to the 'end of the second century CE' [89: cf. 27n14] is unexplained and strictly impossible, since Irenaeus of Lyon (*ca* AD 180) records it as a long-known book [*Adversus haereses* 5.30.3]. Beretta also twice quotes the book in Latin although the Greek is extant and widely available.

Often a date is given vaguely and wrongly. Strabo is dated to 'about a century' after Cicero's *Pro Rabirio* [42]—'75 years' actually—and said to be first century AD [80n4], but later Strabo is said to be 'some decades' after the Flavian writer Josephus [58]: Strabo's *Geography* was composed around AD 20 [see Keyser and Irby-Massie 2008, 763–764]. Varro is dated correctly, albeit vaguely, to the first century BC [25n5], but then [94] is said to have written 'some decades before Pliny' (who was over a century later), which is at least misleading.

Beretta argues from Athenaeus, *Deip.* 11 [784c][8] that, despite the lack of archaeological evidence, glass was worked in Alexandria in the third century AD [43]. He tries to strengthen that argument from the *Historia Augusta*, which he dates to the late third century AD [85–86]—but all parts of that work are very likely by one author writing at the end of the fourth century AD.

The last category of remediable errors concerns Beretta's use of outdated modern authorities. For example, when discussing faience (the vitreous coating baked onto sand cores by Egyptians and others, and often colored blue), Beretta cites the expert Harden 1956 in 1–2n3, and the magisterial Forbes 1966 in 9–10n19. Those were fine works in their day and are still worth consulting, but why not cite the more recent and reliable work in his bibliography, Shortland *et alii* 2001? Other recent works on faience that ought to have been cited by Beretta are Moorey 1994, 166–186 and Nicholson and Peltenburg 2000 [cf. Lucas 1962, 156–167]. On Egyptian natron, Beretta [6n11] cites a work from 1877 (not in his bibliography), and his own work on the medieval German writer Georgius Agricola: better would have been Shortland *et alii* 2006b. On the rise of Phoenician trade, Beretta [7n13] cites Partington 1935—a fine old book, but hardly relevant; better would have been Negbi 1992 or Aubet 2001 [esp. 97–143, 159–193]. Important for Beretta's argument is that the Egyptians were focused on colors and color-transformations: he is surely correct, but in 22n63 and 98n28 he cites Hopkins 1927 when more relevant would be Baines 1985.

Far more serious than such readily remediable defects are the flaws in Beretta's arguments. The thesis of his book is that glass was crucial to the development of ancient alchemy. More precisely, Beretta wishes to argue that glass and metals were long treated alike, and that when at last furnace-makers achieved a temperature high enough to allow glassblowing, the new properties and wide use of glass encouraged the growth of alchemy—and that the expansion of glassblowing was nearly contemporaneous with that of alchemy. A few minor auxiliary arguments are raised to support that case, to which I first turn.

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[8] This passage occurs in the lacuna filled out by the epitome, but reads like an inserted scholium; it is found in the alphabetical section on the names of vessels, *s.v.* Βαυκαλίς.

Beretta wishes to show that that scholars typically regard the scope of alchemy as being merely 'gold-making' [ix–xi, 88, 96, 106], which underwrites his contention that the role of glass in alchemy is under-appreciated. There may be some surveys or studies of alchemy that adopt such a perspective—Beretta cites one modern work [x n6]—but given the overall outlook of scholars, this is a straw man. Recent works cited by Beretta, such as Lindsay 1970, Hershbell 1987, and Letrouit 1995, certainly do not adopt that view; nor did my own survey [1990].

Beretta briefly treats the Greek *kyanos* [20, 37], rightly connecting it with 'Egyptian blue'—as in Theophrastus *De lap.* §58 and Vitruvius, *De arch.* 7.11.[9] However, as Trowbridge [1930, 11–19] has shown, the substance was known to Homer. Moreover, the Mycenaean tablets record *ku-wa-no* (which may derive from Hittite *kuwanna*) and the *ku-wa-no-wo-ko*, arguably the *kyanourgos*; and there is archaeological reason to believe that *kyanos* was produced in Mycenaean Greece [see Goetze 1947, Nightingale 1998]. Thus, *kyanos* was not 'exclusively Egyptian' [22] and glassmaking was not wholly foreign to Greek culture.

Beretta twice falls into the error of referring to 'glass paste' [49, 64] which is an effectively meaningless designation [see Lucas 1962, 193–194; Forbes 1966, 112–114]. Moreover, he confuses the issue in Theophrastus, *De lap.* §49, and follows J. M. Stillman [1924, 21] in interpreting the passage as a reference to 'the coloring property of copper once it is combined and melted together with glass paste' [49]. But Theophrastus is recording an unusual 'earth' which, when mixed with copper during its smelting, produces a 'beautiful color'. So Theophrastus, like Aristotle [*De gen. et corr.* 1.10 328b13–14], is speaking of the production of brass or bronze by adding something to copper ore, where Aristotle specifies that it is κασσίτερος, almost certainly tin or its ore. Theophrastus, then, is not referring to glass or *kyanos* here [see Caley and Richards 1956, 162–167].

Let me now turn to the fundamental errors in Beretta's attempt to connect the rise of alchemy with the expansion of glassblowing.

First, Beretta often draws a close connection between valuable metal and glass in order to connect glassmaking with the alchemical

[9] Beretta [12] cites 'VII, 2'.

goal of producing valuable metallic materials [see 3–4, 16, 31–32, 36–37, 51–52, 89, 131]. There were indeed deep connections between glass and metal in that the processes of their creation or extraction were similar and were perceived as similar, especially in so far as they were both produced by fire from substances of very different properties and were susceptible of melting; and in that, for much of the ancient period, some glass and some metal were both regarded as valuable.

But the sense in which, for ancient alchemists or glassworkers or metalworkers, glass was 'like' metal was never, so far as our evidence goes, such that we can say that they saw glass as being the same as metal or a kind of metal. It is not even clear that there was an ancient concept of 'metal' in our sense of a material that is fusible, malleable, opaque, and specular, miscible or susceptible of alloying with others of its kind. (Crucial and always implicit in our concept is that a metal be electrically and thermally conductive, concepts utterly out of view in antiquity.) There was no agreed term that maps exactly to our 'metal': Halleux [1974, 19–60] shows that μεταλλ- usually refers to things 'mined' and that our 'metal' is perhaps only in view in Isidore, *Etym.* 16.17.2, as *metalla*, where he lists precisely seven species: *aurum*, *argentum*, *aes*, *electrum*, *stagnum*, *plumbum*, and *ferrum.* Aristotle appears to indicate metals in *Meteor.* 3.6 378a19–28 where he distinguishes ὀρυκτά (e.g., realgar, ochre, ruddle, sulfur, and cinnabar) from μεταλλευτά (e.g., iron, gold, and copper) and in *De sensu* 5 443a15–21 which, while discussing their smells, lists those three plus silver and tin as μεταλλεύοντα. However, *Meteor.* 4.10 388a10–13 lists λίθος among the μεταλλευόμενα.[10] For example, we commonly think of mercury as a metal; but there is no evidence that it was seen in antiquity as a member of the same category as gold, silver, copper, tin, lead, and their alloys [Halleux 1974, 108, 179–188]. On the other hand, although iron was usually listed with gold, silver, copper, and so on (thus, implicitly a 'metal'; explicitly a metal in Pliny, *Nat. hist.* 34.142–143), almost no Greco-Roman text

[10] On Theophrastus' lost *On Metals*, see Halleux 1974, 171–174 and Sharples 1998, 169; there are very few fragments, and we do not really know how Theophrastus conceived the category. Theophrastus, *De lap.* §1 indicates that the μεταλλευόμενα were created from water, in contrast to other substances created from earth.

states clearly that it was fusible.[11] So we ought not to phrase the question as 'Was glass considered as a metal, or not?' but rather as 'In what way was glass considered and to what other substances, if any, was it considered similar, and how?'

The primary connection is simply that which Plato already drew in the *Timaeus*, that what we call metals, plus some stones as well as glass, all shared the mysterious property that they could be melted like ice and then cooled and solidified again. That is what likely lies behind *Timaeus* 59b–c and the passages quoted by Beretta: Aristotle's *Meteor.* 4.10 389a7–9, which includes glass and stones with some metals (gold, silver, copper, tin, lead) as fusible [36–37]; and Galen's *Simpl. med.* 9.1.4 on earths from which are produced silver or gold or iron or glass [51–52].[12] Moreover, for much of its history, glass of certain colors was not simply a 'fake' gem but a gem artificially produced, so that such glass was received as a valuable product; that seems to be the sense of *Timaeus* 61b [31–32] and is likely what the Egyptians meant [16].

A few texts refer to glass having the 'look' of gold [3, 89, 131]. I suggest that here we have to do with the scintillating sheen of well melted and cast glass, which although not as specular as polished metal is nevertheless remarkable and evidently was desirable [cf. pseudo-Aristotle, *De coloribus* 3 793a13–19]. In any case, the comparison cannot refer simply to the color, nor to the value.

Second, Beretta claims that the making of transparent glass and glassblowing both require high-temperature furnaces that were not developed until the fourth or first centuries BC, respectively. With respect to transparent glass, Beretta claims that

> the possibility of producing perfectly transparent glass crucially relied upon the availability of furnaces capable of producing temperatures of 1000°C;

[11] Aristotle, *Meteor.* 3.6 378a27–28 gives iron as one example of substances that are either fusible or malleable (ἢ χυτὰ ἢ ἐλατά: the two other examples, gold and copper, are both); and *Meteor.* 4.6 383a30–b5 seems to describe (some stage of the smelting of) iron as melting, but he means 'grow soft' like horn (as he says there and at 4.9 385b6–12 and 4.10 388b30–33): cf. the parallel (or paraphrase?) in Pliny, *Nat. hist.* 34.146. See the discussion in Halleux 1974, 189–198.

[12] Quoted without the citation from Halleux 1974, 136.

and concludes that these were attained by the fourth century BC in Greece [37], and that 'new kinds of furnaces that could reach higher temperatures' enabled the production of transparent glass [98]. With respect to glassblowing, Beretta writes [11–12n26],

> The highest temperature reached in glassmaking during antiquity reached 1000–1100°<C> only around the first century BC,

and cites Neuburger 1919. Beretta also claims that the

> construction of furnaces which reached high temperatures (above 1000°<C>) and which made raw glass liquid

was one of the crucial factors that enabled glassblowing [64].

Indeed glassblowing can only be done above a certain minimum temperature, which depends upon the composition of the glass; and for typical Greco-Roman glass that temperature was perhaps around 1050°C.[13] But that temperature was regularly attained in kilns and furnaces many millennia before glassblowing was invented. The casting of copper requires temperatures of 1000 to 1100°C, and is attested from *ca* 5000 [*sic*] BC [Radivojević *et alii* 2010]. Moorey [1994, 150–151] records kiln temperatures of 1050–1150°C by 4500 [*sic*] BC in Mesopotamia. Nicholson and Jackson [2000] report easily achieving 1000°C with reproductions of Egyptian furnaces (of *ca* 1350 BC); and with some work temperatures of 1100–1150°C were attained. Shortland [2000, 22–23] computed the temperatures at which various vitrified materials found at Amarna would have vitrified and determined that they had been subjected to temperatures of 1050–1200°C. He also performed experimental refirings at 1100–1250°C which confirmed those calculations [2000, 35–42]. Rehder [2000, 40] reports examinations of furnaces at Hagia Triada, Crete, from the 14th century BC, showing that they had attained temperatures of 1250°C. Stern [1999, 446; 2008, 522–526] has argued that a new form of glass furnace was developed about a century after glassblowing was invented, which allowed greater control of the temperature; moreover, the actual temperatures attained in such Roman imperial

[13] See Stern 1999, 451; but note Fischer 2008, 78 which reports 950°C, apparently confirmed by Stern's discussion [1999, 452–454] of blowing that starts with a chunk, at *ca* 900–950°C.

furnaces have been shown to be only slightly higher than prior furnaces [see Taylor and Hill 1999]. In sum, there is no evidence of any new development in furnace technology that allowed or encouraged the discovery of glassblowing.

Likewise, transparent glass can only be produced if the materials are heated sufficiently to allow gas bubbles to escape (and if no opacifier, such as tin or antimony or others, is added); transparent glass was made in many colors, including colorless. But here also, the ability to produce transparent glass long predates Greek philosophy or alchemy. Transparent glass bowls are known from Gordion *ca* 700 BC [von Salden 1959: cf. Stern 2008, 528–529]; and as early as *ca* 1450 BC, some Egyptian glass is transparent [Shortland and Eremin 2006, 584–588, 591]. The marvels of transparent glass or rock crystal (clear quartz) referred to by Philolaus [Diels and Kranz 1951, 44A19], Herodotus [*Hist.* 3.24], and Aristophanes [*Nubes* 768], depend upon no novelty in the manufacture of glass [cf. 27–31]. On the other hand, the references in Anaximenes [Diels and Kranz 1951, 13A14] and Empedocles [Diels and Kranz 1951, 31A51a] to the *krustalloeides* are references to 'ice-like' solids, contrary to what Beretta maintains on pages 26–27.

Third, Beretta bases his case for the connection between glassblowing and alchemy in part upon alleged coincidences of date. Such arguments are unsound even when the dates are secure, which they are not either for the invention of glassblowing itself (for which we still have only an archaeological *terminus ante quem*) or for the alchemical texts (almost none of which are dated precisely). However, Beretta misuses even that set of evidence and most of his mistakes about dates tend towards forcing them into synchronization with the expansion of glassblowing—recall the dates of Diodorus of Sicily, Strabo, Philostratus, and Varro, discussed above.

The date of Heron of Alexandria was long disputed, but Beretta [80n41, 117] seems to accept a date in the first century AD.[14] However, Beretta also claims that Heron wrote 'about the same time that glassblowing was introduced' [80]. No, he wrote at least one century later, so that he provides no evidence of a connection between alchemy

[14] Strictly, the modern consensus is for the mid-first century AD: cf. Keyser and Irby-Massie 2008, 384–387.

and the invention of glassworking. Beretta is quite right to emphasize that the use of transparent glass for scientific apparatus, such as the experiments of Heron or indeed of the alchemist Maria, for example, does allow tests and procedures that would otherwise be difficult or impossible. Beretta [110] claims that 'as early as the first century CE, the appreciation of glass' chemical neutrality was extremely common', citing Dioscorides on storing mercury. Beretta also provides six valuable images of glass apparatus [see Fig. 15 on p. 81 and Figs. 5, 10, 12, 14, 15 on pp. 109–123]. However, many pharmacists before Dioscorides stored compounds in glass, presumably due to its inert character—the earliest attested is Mnesitheus of Cyzicus (*ca* 180 BC);[15] in at least one case, a glass container is used for its transparency by the pharmacist Krates (*ca* 10 BC) [Keyser and Irby-Massie 2008, 489–490]. None of the other evidence cited by Beretta supports his *terminus* of the first century AD, and of the five alchemical authors whom Beretta cites for the use of glass apparatus [113–120], likely the earliest is Maria, whose dates are famously uncertain. (The others are Iulius Africanus, Olympiodorus, Synesius, and Zosimus.)

Already in 2004, 258–269, Beretta himself argued that at (or by) the time of Celsus (*ca* AD 15–35) and Rufus of Ephesos (*ca* AD 70–100), the nomenclature of the parts of the eye began to refer to 'glassy' humors and tunics, due to the recent development of glassblowing. He now reprises that argument [69–74]. First, the nomenclature has to do with transparency not glassblowing, and transparent glass had long existed, as I have already shown. Moreover, a 'linguistic reform' [71] in the nomenclature of the eye that is first attested in Celsus is very likely due to the work of the influential Herophilean oculist Demosthenes 'Philalethes' (*ca* 50 BC to AD 25) [Keyser and Irby-Massie 2008, 239–240]. Further, the introduction into medicine of the concept of a 'glassy' (*hyaloeides*) material dates back to the doctor Praxagoras of Kos (*ca* 300 BC) and his student Phylotimos, who have the 'glassy humor' as one of their chief constructs [Keyser

15 Apuleius Celsus of Centuripae (*ca* AD 30), Cornelius (*ca* 100 BC), Mnesitheus of Cyzicus (*ca* 180 BC), Spendousa (*ca* 10 BC), and Truphon of Gortun (*ca* AD 5): for these dates, see Keyser and Irby-Massie 2008, 119, 216, 561, 756, 817 respectively.

and Irby-Massie 2008, 694–695]; presumably they were indeed referring to the transparency of glass, well-known at that time.

Beretta [86–88] quotes Pliny's encomium on fire [*Nat. hist.* 36.68], to which he compares the 'relatively new Stoic notion of *pur technikon*', citing von Arnim 1905–1924, 1.44. More apropos to Pliny on fire might have been Theophrastus, *De igne* 1–3. But in any case, Beretta should explain that the citation of Zeno fr. 171 in von Arnim 1905–1924 derives from Cicero, *De nat. deor.* 2.57, and Diogenes Laertius, *Vitae* 7.156. There is nothing 'new' about the doctrine, not at the time of Pliny nor even of Cicero. Moreover, Beretta wrongly relates the passage to recent developments in alchemy. The powerful transmuting effects of fire surely did play a role in the development of theories of material change, as can already be seen in Plato's *Timaeus*; and surely the making of glass was one (of many) such effects considered. But that inspiration long antedates glassblowing [cf. Keyser 1990].

Beretta [89–97] argues, from several passages each in Diodorus of Sicily and Pliny (plus one fragment of Varro and a passage in Irenaeus of Lyon), that treatises on the imitation of gemstones began to be produced at around the same time as, and because, glassblowing was invented; and that those treatises influenced the expansion of alchemy. The imitation of gems is a well known part of the alchemical literature, and two of the earliest such works are usually placed before 100 BC [see Keyser and Irby-Massie 2008, *s.vv.* Bolos, Petosiris]. Beretta [98–107] adds to his argument regarding imitation of gemstones the evidence provided by the fragments of pseudo-Democritus, which include material on gemstones. Now this mass of material is certainly an important part of the alchemical corpus; but it is likely due to multiple authors, composing a wide variety of works (on stones, on alchemy, on pharmacy, on medicine, and even on agriculture), variously dated between 250 BC and AD 200 [see Keyser and Irby-Massie 2008, 236–239]. This material thus provides no basis for an argument that any particular pseudo-Democritus wrote in response to the development of glassblowing. Glassblowing would not affect the means of production of imitation gems, and there is no basis for dating all such works after the invention of glassblowing.

Beretta claims, as another part of his case that glass production came to Greece from Egypt, that in contrast to Egyptians the

Greeks rarely dealt with gemstones, at least up through the time of Theophrastus, *De lapidibus*; and that only with the poetry of the Alexandrian writer Poseidippos did gemstones become widely known to Greeks [45–46: cf. 96 on imitating gems]. The facts, however, are that Greeks made extensive use of some gemstones, especially as signets, from the archaic period onwards. Aside from the slipperiness of the category 'gemstone', the difficulty is that the referents of most words (in Greek or whatever language) for gemstones shifted over the course of centuries, and most of them never at any time referred to what we would call a single mineral species. For example, Theodoros of Samos (*ca* 550 BC) carved a σμάραγδος as a signet [Herdotus, *Hist.* 3.41], which must have been some hard (green) gemstone, if not our emerald (green beryllium-aluminum silicate). Pythagoras' father, Mnesarchus of Samos, was also a gemcutter [Diogenes Laertius, *Vitae* 8.1], and others are known. Plantzos [1999, 13] notes the ring set with ὄνυξ preserved in the Parthenon treasury in the late fifth century BC [*Inscriptiones graecae* I^3, 1.351.23–24], though no-one can say whether that stone was the same kind of stuff as the ὀνύχιον of Theophrastus, *De lap.* §31, itself likely our crypto-crystalline banded quartz. Boardman and Wilkins [1970, 374–379] note that preserved archaic signets were typically carved from various kinds of crypto-crystalline quartz (agate, chalcedony, cornelian, jasper, onyx, and sard) plus the softer rarer lapis lazuli, which last is the σάπφειρος of Theophrastus, *De lap.* §23. Boardman [1968] includes two signets carved from amethyst [Nos. 32 and 70 on pp. 27 and 45], and probably the ἀμέθυσον of Theophrastus, *De lap.* §30 is the same kind of stone. Plato [*Phaedo* 110d] lists as example gems the ἴασπις, the σάρδιον, and the σμάραγδος; but what they were besides valuable, he does not say.[16] Aristotle, *Meteor.* 4.9 387b17–18 knows a valuable red stone ἄνθραξ, known also to Theophrastus *De lap.* §18, that is immune to fire. Of course, amber (ἤλεκτρον) was known and used from very early times.

Some gemstones did arrive in the Greek world in the Hellenistic period or later, but that says nothing about earlier Greek confection of glass stones. The diamond arrives with the rise of Indian trade [cf, *Periplus* §56], whereas the ἀδάμας of Plato, *Timaeus* 59b and *Politicus* 303e, of Theophrastus, *De lap.* §19, and Heron, *Pneum.* 1

[16] In Theophrastus, *De lap.* §27 ἴασπις is green, and in §30 σάρδιον is red.

[Schmidt 1899, 6.11–28] likely refers to the dark, dense, and hard osmiridium grains found with placer gold [cf. Meeks and Tite 1980]. Earlier, ἀδάμας may have referred to steel or iron [cf. Halleux 1974, 90–91, esp. n20]. Stones such as the green βήρυλλος, the green τόπαζον/ς or τοπάζιον, and the golden(?) χρυσόλιθος are attested first in the *Septuagint* and are not attested in Poseidippus. The blue ὑάκινθος and the yellow-green(?) χρυσόπρασος are not apparently attested before the *Revelation to John* 21.20, late first century AD.

In summary, Beretta's book is composed of very fine materials—a wide range of sources, beautiful images, a rich topic, and a good imagination. But the elements have not fused well, so that the compound is a missed opportunity. Glass in antiquity was certainly one of the chief artificial pyrotechnical products, along with the smelting of ores, the creation of dyes and pigments, and even the brewing of beer and wine, that ultimately led to the development of alchemical theory. Beretta's account of this, however, is muddled and flawed, and offers little novelty. Glassblowing was a surprising discovery that led to novel and beautiful forms, and a much wider use of glass, and the availability of glass vessels and instruments, exploited by a variety of ancient scientists (workers in alchemy, in optics, in pneumatics, and in pharmacy). Here Beretta's account offers greater novelty but less conviction. Scholars and students should surely read the book but we should read with caution.

BIBLIOGRAPHY

Anderson, G. 1986. *Philostratus: Biography and Belles Lettres in the Third Century A.D.* London/Dover, NH.

Aubet, M. E. 2001. *The Phoenicians and the West: Politics, Colonies and Trade.* 2nd edn. Cambridge.

Baines, J. 1985. 'Color Terminology and Color Classification: Ancient Egyptian Color Terminology and Polychromy'. *American Anthropologist* ns 87.2:282–297.

Beretta, M. 2004. ed. *When Glass Matters: Studies in the History of Science and Art from Graeco-Roman Antiquity to Early Modern Era.* Florence.

Boardman, J. 1968. *Archaic Greek Gems: Schools and Artists in the Sixth and Early Fifth Centuries B.C.* Evanston, IL.

Boardman, J. and Wilkins, R. L. 1970. *Greek Gems and Finger Rings: Early Bronze Age to Late Classical.* New York. Repr. 2001.

Caley, E. R. and Richards, J. F. C. 1956. edd. *Theophrastus on Stones.* Columbus, Ohio.

Casson, L. 1989. *The* Periplus maris erythraei. Princeton.

Diels, H. 1879. *Doxographi Graeci.* 4th edn. Berlin.

Diels, H. and Kranz, W. 1951. *Die Fragmente der Vorsokratiker.* 3 vols. 6th edn. Zurich.

Eichholz, D. E. 1965. *Theophrastus:* De Lapidibus. Oxford.

Fischer, A. 2008. *Hot Pursuit: Integrating Anthropology in Search of Ancient Glassblowers.* Lanham, MD.

Forbes, R. J. 1966. *Studies in Ancient Technology.* vol. 5.110–231. Leiden.

Goetze, A. 1947. 'Contributions to Hittite Lexicography'. *Journal of Cuneiform Studies* 1.4:307–320.

Halleux, R. 1974. *Le problème des métaux dans la science antique.* Paris.

Harden, D. B. 1956. 'Glass and Glazes'. Pp. 311–346 in C. Singer, E. J. Holmyard, A. R. Hall, and T. I. Williams edd. *A History of Technology.* vol. 2. New York/London.

Hershbell, J. 1987. 'Democritus and the Beginnings of Greek Alchemy'. *Ambix* 34:5–20.

Hopkins, J. A. 1927. 'Transmutation by Color: A Study of Earliest Alchemy'. Pp. 9–14 in J. Ruska ed. *Studien zur Geschichte der Chemie. Festgabe E. O. Von Lippmann.* Berlin.

Huffman, C. 1993. *Philolaos of Croton: Pythagorean and Presocratic. A Commentary on the Fragments and Testimonia with Interpretive Essays.* Cambridge.

Inwood, B. 2001. *The Poem of Empedocles: A Text and Translation with an Introduction.* 2nd edn. Toronto.

Keyser, P. T. 1990. 'Alchemy in the Ancient World: From Science to Magic'. *Illinois Classical Studies* 15:353–378.

Keyser, P. T. and Irby-Massie, G. L. 2008. edd. *Encyclopedia of Early Natural Scientists: The Greek Tradition and Its Many Heirs.* London/New York.

Kingsley, 1995. *Ancient Philosophy, Mystery and Magic: Empedocles and the Pythagorean Tradition.* Oxford.

Kirk, G. S.; Raven, J. E.; and Schofield, M. 1983. edd. *The Pre-Socratic Philosophers.* 2nd edn. Cambridge.

Lindsay, J. 1970. *The Origin of Alchemy in Greco-Roman Egypt.* London.

Letrouit, J. 1995. 'Chronologie des alchimistes grecs'. Pp. 11–93 in D. Kahn and S. Matton edd. *Alchimie, art, histoire et mythes.* Paris.

Lucas, A. 1962; *Ancient Egyptian Materials and Industries.* 4th edn. Revised by J. R. Harris. London/Mineola, NY. Repr. 1999.

Meeks, N. D. and Tite, M. S. 1980. 'The Analysis of Platinum-Group Element Inclusions in Gold Antiquities'. *Journal of Archaeological Science* 7:267–275.

Moorey, R. S. 1994. *Ancient Mesopotamian Materials and Industries.* Oxford.

Mounce, R. H. 1998. *Book of Revelation.* rev. edn. Grand Rapids, MI.

Negbi, O. 1992. 'Early Phoenician Presence in the Mediterranean Islands: A Reappraisal'. *American Journal of Archaeology* 96:599–615.

Neuburger, A. 1919. *Die Technik des Altertums.* Leipzig. 2nd edn 1921, 3rd edn 1922.

Nicholson, T. and Jackson, C. M. 2000. ' "Kind of Blue": Glass of the Amarna Period Replicated'. Pp. 105–120 in Nicholson and Shaw 2000.

Nicholson, T. and Peltenburg, E. 2000. 'Egyptian Faience'. Pp. 177–194 in Nicholson and Shaw 2000.

Nicholson, T. and Shaw, I. 2000. *Ancient Egyptian Materials and Technology.* Cambridge/New York.

Nightingale, G. 1998. 'Glass and the Mycenaean Palaces of the Aegean'. Pp. 205–226 in P. McCray ed. *The Prehistory and History of Glassmaking Technology.* Columbus, OH.

Partington, J. R. 1935. *Origins and Development of Applied Chemistry.* London.

Plantzos, D. 1999. *Hellenistic Engraved Gems.* Oxford/New York.

Radivojević, M.; Rehren, T.; Pernicka, E.; Sljivar, D.; Brauns, M.; and Borić, D. 2010. 'On the Origins of Extractive Metallurgy: New Evidence from Europe'. *Journal of Archaeological Science* 37:2775–2787.

Rehder, J. E. 2000. *The Mastery and Uses of Fire in Antiquity.* Montreal/Ithaca, NY.

Robinson, J. M. 1988. *The Nag Hammadi Library in English.* 3rd edn. San Francisco.

Schmidt, W. 1899. *Heronis Alexandrini opera quae supersunt omnia.* vol. 1. Leipzig.

Sharples, R. W. 1998. *Theophrastus of Eresus: Sources for His Life, Writings, Thought, and Influence: 3.1: Sources on Physics (Texts 137–223).* Philosophia Antiqua 79. Leiden.

Shortland, A. J. 2000. *Vitreous Materials at Amarna: The Production of Glass and Faience in 18th Dynasty Egypt.* British Archaeological Reports: International Series 827. Oxford.

Shortland, A. J.; Nicholson, T.; and Jackson, C. 2001. 'Glass and Faience at in Amarna'. Pp. 147–160 in A. J. Shortland ed. *The Social Context of Technological Change.* Oxford.

Shortland, A. J. and Eremin, K. 2006. 'The Analysis of Second Millennium Glass from Egypt and Mesopotamia: Part I. New WDS Analyses'. *Archaeometry* 48.4:581–603.

Shortland, A. J.; Schachner, L.; Freestone, I.; and Tite, M. 2006. 'Natron as a Flux in the Early Vitreous Materials Industry: Sources, Beginnings and Reasons for Decline'. *Journal of Archaeological Science* 33:521–530.

Stern, E. M. 1999. 'Roman Glassblowing in a Cultural Context'. *American Journal of Archaeology* 103.3:441–484.

———— 2008. 'Glass Production'. Pp. 520–546 in J. Oleson ed. *Oxford Handbook of Engineering and Technology in the Classical World.* Oxford/New York.

Stillman, J. M. 1924. *The Story of Early Chemistry.* New York/London. Repr. New York, 1960.

Taylor, M. and Hill, D. 1999. 'Experiments on the Reconstruction of Roman Wood-Fired Glassworking Furnaces'. *Journal of Glass Studies* 50:249–270.

Trowbridge, M. L. 1930. *Philological Studies in Ancient Glass.* Urbana, IL.

von Arnim, J. 1905–1924. ed. *Stoicorum veterum fragmenta.* 4 vols. Leipzig.

von Salden, A. 1959. 'Glass Finds at Gordion'. *Journal of Glass Studies* 1:22–49.

In memoriam
John Phillips Britton
(6 December 1939 – 8 June 2010)

John Phillips Britton, renowned historian of science and scholar of Babylonian astronomy, died at his home in Wilson, Wyoming on 8 June 2010 of cardiac arrest. He was 71 years old.

The rich mixture of talents with which John Britton was gifted clearly shows up in his career. After obtaining Bachelor of Arts degrees in History and Physics (1961) and a Philosophical Doctorate in the History of Science (1966), both from Yale University, he entered the investment management business and eventually founded his own asset management firm. But in the 1980s, his scientific side started itching and—now being a man of independent means—he decided to follow his heart and to go back to the passion of his youth: history of science. And in a manner typical of the intensity and drive with which he did things, he was successful again. He went back to Yale, took classes in Akkadian and Sumerian, the languages of ancient Mesopotamia written in cuneiform script on clay tablets, and over the next two decades developed into one of the world's experts in Babylonian Astronomy and its transmission to the Hellenistic world.

In his doctoral thesis, submitted to Yale University in September 1966 and carried out under the supervision of Asger Aaboe, John analyzed the way in which the famous ancient Greek astronomer Claudius Ptolemy (second century AD) arrived at the parameters of his solar and lunar theories from observations.[1] After obtaining his degree he left the field, but an adapted version of the first chapter of his thesis was published three years later as a paper [1969] in the Festschrift at the occasion of the 70th anniversary of Otto Neugebauer, one of his teachers and one of the examiners at his thesis defense. It may be considered not only a sign of the quality of Ptolemy's but also of John's work that a somewhat updated version of his thesis was published as a monograph 25 years later [1992]. In the meantime, a heated debate had raged in the literature triggered by the publication of R. R. Newton's book *The Crime of Claudius Ptolemy* in 1977. Contrary to the accusations made by Newton, and

[1] There is a list of John Britton's publications on pages 255–257.

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160 years earlier by J. B. Delambre in his famous *Histoire de l'astronomie ancienne*, John [1992] came to the scholarly and balanced judgment that while

> it does not seem reasonable to accept Ptolemy's solar observations as the results of careful, independent observations

nevertheless

> the *Almagest* should be seen as a great, if not the first, scientific treatise.

This awe for the intellectual achievements of the ancients, both the Babylonians and the Greeks, is a recurrent theme in many of his papers.

The second paper [1987] that John wrote, and the first one after having returned to the history of science, was on column Φ, the first column in Babylonian lunar ephemerides of system A and most probably one of the oldest elements of Babylonian lunar theory. This paper also appeared in a Festschrift, this time at the occasion of Aaboe's 70th anniversary, John's greatly admired teacher and intellectual father figure.

Function Φ and lunar theory more generally would remain central themes in John's research. Much of his early work is based on digesting and further elaborating previous discoveries by Aaboe. This holds for his extensive study [1989] of lunar nodal motion based on Text S, which treats an early variant of system A eclipse theory, and also for his paper [1990] on the possible relation between the 19-year solar calendar cycle and function Φ based on Text E. The research published in these papers also assisted in the formation of his ideas about the gradual development of Babylonian lunar theory, the topic of his review [1993a] presented in 1991 at a symposium held in Graz, Austria. In this review, he also included results from two forthcoming publications [1991b, 1994] on the Saros cycle (the lunar eclipse cycle of 223 synodic months discovered by the Babylonians).

In the early 1990s, John also published two interesting papers on texts from the fourth century BC, one [1991a] on an early model of the planet Venus (with C. F. B. Walker) and the other [1993b] on a mathematical text containing a list of fourth powers of regular numbers (products of powers of 2, 3 and 5). These are the first papers in which he actually got involved with transcribing, translating, and interpreting cuneiform texts himself. The analysis of the Venus text further shows the impressive grasp of Babylonian astronomy that John had acquired in the decade since returning to the field. This also clearly shows up in his paper [2000] (with A. Jones) on a first century AD papyrus from Oxyrhynchus in Greco-Roman Egypt containing a Babylonian model of the planet Jupiter, in the popularizing chapter [1996] on Babylonian astronomy and astrology that he wrote with C. B. F. Walker in the British Museum publication *Astronomy before the Telescope*, and in his critical review [1998] of *The Babylonian Theory of the Planets* by N. M. Swerdlow.

Around the turn of the century, the term of his apprenticeship was over and the phase of his master craftsmanship could begin. This is very much noticeable in his review papers, where he addresses the same themes as before but now put in broader and deeper perspective. The emphasis on the historic context and the broad picture, his superb command of the English language and his fluent elegant style of writing make his papers quite stimulating reading. Still, some of the arithmetical detail both in his writing and in his oral presentations, originating in his conviction that the Babylonian mind was first and foremost a mathematical one, could be somewhat overwhelming at times. In his papers, the use of spreadsheets is a common feature consistent with his remark that 'the spreadsheet was a Babylonian invention'.

Starting with his review papers, 'Lunar Anomaly in Babylonian Astronomy' [1999] and 'Treatments of Annual Phenomena in Cuneiform Sources' [2002a], John embarked on a program to unveil and understand in detail the road followed by Babylonian scholars in the fifth and fourth centuries BC when Babylonian lunar theory was developed step by step into the sophisticated systems A and B that we know from the lunar ephemerides of the Seleucid period. Many of the basic ideas on which this reconstruction is based derive from Aaboe's fundamental contributions, further extended and worked out by John in several of his papers in the 1980s and 1990s. Preceded by a paper on corrections for solar anomaly in Babylonian lunar theories [2004a], this eventually led to a series of papers entitled 'Studies in Babylonian Lunar Theory', of which parts 1–3 [2007a, 2007f, 2010] were published or in press at the time of his death. It is fascinating to follow him on this intellectual journey which shows his great knowledge of the intricacies of Babylonian lunar theory and which illustrates his conviction that clever mathematical manipulation of combinations of lunar and solar periods forms the foundation on which the theories are built.

In addition to this systematic study of Babylonian lunar theory, John managed to publish a number of interesting papers over the last 10 years on a variety of other topics: a late theoretical Venus text [2001a], an early observational Mars text [2004b], two early 'lunar-six' texts [2007b, 2007d] (one co-authored with P. J. Huber), a late lunar procedure text [2007c] (with W. Horowitz and J. M. Steele), and an interesting review [2007e] of the gradual improvement of the calendar in Mesopotamia, with special emphasis on the progress in the estimate of the year-length, paralleling the increase of astronomical knowledge in Babylon during the last seven centuries BC. The last paper [2011] in his bibliography (with C. Proust and S. Shnider) on the famous mathematical tablet Plimpton 322 is a prime example of John's erudite scholarship, of his desire to understand the Babylonian mind, and of his ambition to put the subject matter of a text in the proper historical and cultural context.

John Britton was an independent scholar not permanently affiliated to any university or academic department, but during his career as a historian of science he held several visiting positions at institutions of higher learning: the history of science departments of Yale University (1984–1991) and Harvard University (1994–1995), the Dibner Institute at M.I.T (2003–2004) and the Institute for the Study of the Ancient World at N.Y.U. (2008–2010).

The fact that more than one quarter of his papers are contributions to Festschrifts of colleagues and friends is very much in line with the fact that John was a very personable man: he did not get acquainted, he entered into relationships. This was partly due to the delightful mixture of cordial joviality and New England reserve that was one of his trademarks. He was also very generous in sharing his views and ideas with students and colleagues, stimulating them in their own research, even sometimes materially supporting their endeavors. He could openly admire the work of others; but he could also be quite critical, in particular when his own views were at stake, however always remaining polite and respectful, gentleman as he was. His open mind and his keen sense of humor were essential elements of his natural charm. One of his characteristic jokes to friends who expressed admiration for his work was that he could just be making it all up and there was nobody alive who would know the difference.

During his lifetime, John developed into the world's expert in Babylonian lunar theory. Here he made his most seminal contributions. He greatly admired the arithmetical skills of the ancient Babylonian scholars and their impressive achievement of having successfully modeled the motions of the Sun, Moon and planets. In a colloquium talk entitled 'Babylonian Lunar Theory and the Invention of Science' that he gave at the Dibner Institute in Cambridge, Massachusetts on 30 September 2003, he ended his presentation as follows:

> In closing, I would hope to leave with you two thoughts. The first is that this was no trivial development or merely a clever manipulation of simple numbers as sometimes asserted, but rather a persistent and profoundly disciplined exercise in theoretical and practical analysis. The second is that its author, whoever he was, possessed an intellect of uncommon power, deserving perhaps to be ranked among the best.

Maybe the last phrase also applies to John Britton. For his intellectual power and generosity, but above all for his warm personality, he will be greatly missed by all his Babylonian friends and colleagues.

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BIBLIOGRAPHY OF JOHN PHILLIPS BRITTON'S PUBLICATIONS

1. 1966. *On the Quality of Solar and Lunar Observations and Parameters in Ptolemy's* Almagest. PhD dissertation, Yale University.
2. 1969. 'Ptolemy's Determination of the Obliquity of the Ecliptic'. *Centaurus* 14:29–41. [Festschrift O. Neugebauer].
3. 1987. *The Structure and Parameters of Column* Φ. Pp. 23–26 in J. L. Berggren and B. R. Goldstein edd. *From Ancient Omens to Statistical Mechanics.* Copenhagen. [Festschrift A. Aaboe].
4. 1989. 'An Early Function for Eclipse Magnitudes in Babylonian Astronomy'. *Centaurus* 32:1–52.
5. 1990. 'A Tale of Two Cycles: Remarks on Column Φ'. *Centaurus* 33:57–69.
6. 1991a. 'A Fourth Century Babylonian Model for Venus: BM 33552'. *Centaurus* 34: 97–118 (with C. B. F. Walker).
7. 1991b. 'Saros Cycle Dates and Related Babylonian Astronomical Texts'. *Transactions of the American Philosophical Society* 81.6 (with A. Aaboe, J. A. Henderson, O. Neugebauer, and A. J. Sachs).
8. 1992. *Models and Precision: The Quality of Ptolemy's Observations and Parameters.* New York/London.
9. 1993a. 'Scientific Astronomy in Pre-Seleucid Babylon'. Pp. 61–76 in H. D. Galter ed. *Die Rolle der Astronomie in den Kulturen Mesopotamiens.* Grazer Morgenländische Studien 3. Graz.
10. 1993b. 'A Table of Fourth Powers and Related Texts from Seleucid Babylon'. *Journal of Cuneiform Studies* 43–45:71–88.
11. 1994. 'Rituals for an Eclipse Possibility in the 8th Year of Cyrus'. *Journal of Cuneiform Studies* 46:73–86 (with P.-A. Beaulieu).
12. 1996. 'Astronomy and Astrology in Ancient Mesopotamia'. Pp. 42–67 in C. B. F. Walker ed. *Astronomy before the Telescope.* London (with C. B. F. Walker).
13. 1998. 'Planetary Theory in Babylon'. *Journal for the History of Astronomy* 29:381–385. review of N. M. Swerdlow, *The Babylonian Theory of the Planets.* Princeton: Princeton University Press, 1998.
14. 1999. 'Lunar Anomaly in Babylonian Astronomy'. Pp. 187–254 in N. M. Swerdlow ed. *Ancient Astronomy and Celestial Divination.* Cambridge/London.
15. 2000. 'A New Babylonian Planetary Model in a Greek Source'. *Archive for the History of Exact Sciences* 55:525–554 (with A. Jones).

16. 2001a. ‘Remarks on a System A Text for Venus: *ACT* 1050’. *Archive for the History of Exact Sciences* 54:349–373.
17. 2001b. ‘Light on the East’. *Journal for the History of Astronomy* 32:169–170. A review of H. Hunger and D. Pingree, *Astral Sciences in Mesopotamia.* Handbuch der Orientalistik, Abteilung 1, Band 44. Leiden/Boston: Brill, 1999.
18. 2002a. ‘Treatments of Annual Phenomena in Cuneiform Sources’. Pp. 21–78 in J. M. Steele and A. Imhausen edd. *Under One Sky: Astronomy and Mathematics in the Ancient Near East.* Alter Orient und Altes Testament 297. Münster.
19. 2002b. ‘Remarks on BM 37361’. Pp. 35–36 in C. Wunsch ed. *Mining the Archives.* Dresden. [Festschrift C. B. F. Walker].
20. 2004a. ‘On Corrections for Solar Anomaly in Babylonian Lunar Theories’. *Centaurus* 45:46–58. [Festschrift B. R. Goldstein].
21. 2004b. ‘An Early Observation Text for Mars: HSM 1899.2.112 (= HSM 1490)’. Pp. 33–55 in C. Burnett, J. P. Hogendijk, K. Plofker, and M. Yano edd. *Studies in the History of the Exact Sciences in Honour of David Pingree.* Leiden/Boston.
22. 2005. ‘Babylonian Eclipse Records’ *Journal for the History of Astronomy* 36:235–237. A review of P. J. Huber and S. de Meis, *Babylonian Eclipse Observations from 750 BC to 1 BC.* Milan: IsIAO-Mimesis, 2004.
23. 2007a. ‘Studies in Babylonian Lunar Theory: Part I. Empirical Elements for Modeling Lunar and Solar Anomalies’. *Archive for the History of Exact Sciences* 61:83–145.
24. 2007b. ‘Remarks on Strassmaier Cambyses 400’. Pp. 8–34 in M. Ross ed. *From the Banks of the Euphrates: Studies in Honor of Alice Louise Slotsky.* Winona Lake.
25. 2007c. ‘BCM A1845–1982.2’. *Wiener Zeitschrift für die Kunde des Morgenlandes* 97:47–54 (with W. Horowitz and J. M. Steele). [Festschrift H. Hunger].
26. 2007d. ‘A Lunar Six Text from 591 BC’. *Wiener Zeitschrift für die Kunde des Morgenlandes* 97:213–217 (with P. J. Huber). [Festschrift H. Hunger].
27. 2007e. ‘Calendars, Intercalations and Year-Lengths’. Pp. 115–132 in J. M. Steele ed. *Calendars and Years: Astronomy and Time in the Ancient Near East.* Oxford.

28. 2007f. 'Studies in Babylonian Lunar Theory: Part II. Treatments of Lunar Anomaly'. *Archive for the History of Exact Sciences* 63:357–431.
29. 2010. 'Studies in Babylonian Lunar Theory: Part III. The Introduction of the Uniform Zodiac'. *Archive for the History of Exact Sciences* 64:617–663.
30. 2011. 'Plimpton 322: A Review and a Different Perspective'. *Archive for the History of Exact Sciences* (with C. Proust and S. Shnider, in press).

BOOKS RECEIVED

Aestimatio 7 (2010)

Amar Annus ed. *Divination and Interpretation of Signs in the Ancient World.* Chicago, IL: The Oriental Institute of the University of Chicago, 2010.

Avner Ben-Zaken, *Cross-Cultural Scientific Exchanges in the Eastern Mediterranean, 1560–1660.* Baltimore: Johns Hopkins University Press, 2010.

Sylvia Berryman, *The Mechanical Hypothesis in Ancient Greek Natural Philosophy.* Cambridge/New York: Cambridge University Press, 2009.

Alan C. Bowen and Christian Wildberg edd. *New Perspectives on Aristotle's* De caelo. Philosophia Antiqua 117. Leiden/Boston: Brill, 2009.

Riccardo Chiaradonna and Franco Trabattoni edd. *Physics and Philosophy of Nature in Greek Neoplatonism: Proceedings of the European Science Foundation Exploratory Workshop (Il Ciocco, Castelvecchio Pascoli, June 22–24, 2006).* Philosophia Antiqua 115. Leiden/Boston: Brill, 2009.

David Creese, *The Monochord in Ancient Greek Harmonic Science.* Cambridge/New York: Cambridge University Press, 2010.

Andrew Cunningham and Sachiko Kusukawa, *Natural Philosophy Epitomised: A Translation of Books 8–11 of Gregor Reisch's* Philosophical Pearl *(1503).* Farnham, UK/Burlington, VT: Ashgate, 2010.

Richard DeWitt, *Worldviews: An Introduction to the History and Philosophy of Science.* Chichester, UK: Wiley-Blackwell, 2010.

Robinson Ellis ed. and trans. *Aetna, with a New Introduction by Katharina Volk.* Exeter, UK: Bristol Phoenix Press, 2008.

Christopher D. S. Field and Benjamin Wardhaugh edd. *John Birchensha: Writings on Music.* Farnham, UK/Burlington, VT: Ashgate, 2010.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 7 (2010) 258–260

Markham J. Geller, *Ancient Babylonian Medicine: Theory and Practice.* Malden, MA/Oxford, UK: Wiley-Blackwell, 2010.

Christopher Gill, Tim Whitmarsh, and John Wilkins edd. *Galen and the World of Knowledge.* Cambridge/New York: Cambridge University Press, 2009.

Stefan Hagel, *Ancient Greek Music: A Technical History.* Cambridge/New York: Cambridge University Press, 2009.

Robert Hahn, *Archaeology and the Origins of Philosophy.* Albany, NY: State University of New York Press, 2010.

Brooke Holmes, *The Symptom and the Subject: The Emergence of the Physical Body in Ancient Greece.* Princeton/Oxford: Princeton University Press, 2010.

Helge Kragh and Kurt Møller Pederson, *The Moon that Wasn't: The Saga of Venus' Spurious Satellite.* Basel/Boston/Berlin: Birkhäuser, 2008.

J. H. Lesher ed. *From Inquiry to Demonstrative Knowledge: New Essays on Aristotle's* Posterior Analytics. Kelowna, BC, Canada: Academic Printing and Publishing, 2010.

Flora R. Levin, *Greek Reflections on the Nature of Music.* Cambridge/New York: Cambridge University Press, 2009.

Carolina López-Ruiz, *When the Gods Were Born: Greek Cosmogonies and the Near East.* Cambridge, MA/London, UK: Harvard University Press, 2010.

Alexander Marr ed. *The Worlds of Oronce Fine: Mathematics, Instruments and Print in Renaissance France.* Donington, UK: Shaun Tyas, 2009.

Maria Chiara Martinelli ed. (in collaboration with Francesco Pelosi and Carlo Peringotti) *La musa dimenticata. Aspetti dell'esperienza musicale greca in età ellenistica.* Pisa: Edizione della Normale, 2009.

Brice Maucolin, *Untersuchungen zur hippokratischen Schrift „Über die alte Heilkunst".* Berlin/New York: Walter de Gruyter, 2009.

Alan Musgrave, *Secular Sermons: Essays on Science and Philosophy.* Dunedin, New Zealand: Otago University Press, 2009.

Carlo Natali ed. *Alessandro d'Afrodisia (Tito Aurelio Alessandro),* Il Destino. *Trattato sul destino e su ciò che dipende da noi. Dedicato agli imperatori.* Sankt Augustin: Academia Verlag, 2009.

Tim O'Keefe, *Epicureanism.* Berkeley/Los Angeles: University of California Press, 2010.

Aaron Poochigian, *Aratus:* Phaenomena*, Translated with an Introduction and Notes.* Baltimore, MD: Johns Hopkins University Press, 2010.

Kurt A. Raaflaub and Richard J. A. Talbert edd. *Geography and Ethnography: Perceptions of the World in Pre-Modern Societies.* Malden, MA/Oxford, UK: Wiley-Blackwell, 2010.

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Francesca Rochberg, *In the Path of the Moon: Babylonian Celestial Divination and Its Legacy.* Leiden/Boston: Brill, 2010.

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Elisa Rubino ed. *Aristoteles Latinus,* Meteorologica*. Liber quartus. Translatio Henrici Aristippi.* Brussels: Brepols, 2010.

Peter S. Rudman, *The Babylonian Theorem: The Mathematical Journey to Pythagoras and Euclid.* Amherst, NY: Prometheus Books, 2010.

Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture,and Society, and Struggling for Credibility and Authority.* Baltimore, MD: The Johns Hopkins

Michael Squire, *Image and Text in Greco-Roman Antiquity.* Cambridge/New York: Cambridge University Press, 2009.

Gerd Van Riel and Pierre Destrée edd. *Ancient Perspectives on Aristotle's* De Anima. Leuven: Leuven University Press, 2009.

Gudrun Vuillemin-Diem, *Aristoteles Latinus:* Meteorologica*. Translatio Guillelmi de Moerbeka.* Brussels: Brepols, 2008.

Benjamin Wardhaugh, *How to Read Historical Mathematics.* Princeton/Oxford: Princeton University Press, 2010.