AESTIMATIO

Critical Reviews in the History of Science

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Edited by
Alan C. Bowen and Tracey E. Rihll

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

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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.

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

Alan C. Bowen

Tracey E. Rihll

The Archaeology Coursebook: An Introduction to Study Skills, Topics, and Methods by J. Grant, S. Gorin, and N. Fleming

London/New York: Routledge, 2005. Pp. xl+346. ISBN 0–415–36077–3. Paper $33.95

Reviewed by
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The practice of archaeology has been undertaken in some fashion for centuries. As early as the 12th and 13th centuries, Italian farmers living around Pisa found Roman statues and sarcophagi in the course of plowing, and eventually even started to look for them to collect. Explorations in the regions around Vesuvius began in the 18th century and are usually considered to mark the beginning of modern archaeology. Yet it was not until the 19th century, principally with Heinrich Schliemann, that archaeological methodology finally started becoming somewhat scientific, and included record-keeping and systematic exploration. Nonetheless, for most of the 20th century, archaeologists were still trained as classicists or historians, that is, as specialists in particular cultures or geographical areas, and archaeological methodology was something that was taught on site and through practice rather than in the classroom. Not until the last few decades has archaeology come to be considered a discipline worthy of study in its own right. Archaeology, with its own specific theory and methodology, is now considered to be a discipline separate from all others and is no longer connected to the study of any particular region or culture. Techniques and practice are learned in the classroom and in the field, and can then be adapted to the specific sites and cultures to be explored. In the United States, it is typically a discipline offered at the post-secondary level; but in the United Kingdom, students can begin the study while still in high school, and indeed it is a subject available for A level exams.

Textbooks designed for courses in archaeology are still relatively new. A few introductory level textbooks are available, but the choices are quite limited. Grant, Gorin, and Fleming's *The Archaeology*

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Coursebook enlarges this small group. The intended audience is mostly secondary school students and teachers, and the authors are in fact secondary school teachers themselves. The book contains specific sections on study skills and projects that are designed to correspond with secondary school classes in England and Wales. The authors also note that they intend the textbook to be appropriate for first year undergraduates as well [xxvi–xxvii].

Unfortunately, even with the significant revisions present in its current second edition, the book still compares unfavorably in most ways to another introductory text, *Archaeology: Theories, Methods, and Practice*, by Colin Renfrew and Paul Bahn (Thames and Hudson), now in its 4th edition. The most important advantage of Grant, Gorin, and Fleming's text is cost. Their book retails for only $34.95, while Renfrew and Bahn's is priced at $71.00. Nonetheless, the difference in the quality of the two books is so striking that even at twice the price, Renfrew and Bahn's text will still no doubt prove a superior choice in most situations.

The differences are immediately apparent on visual inspection. Despite the fact that the two books have similar dimensions in height and width, and similar size font and type, *The Archaeology Coursebook* contains only 346 pages, compared to *Archaeology*'s 656; so it is immediately clear that *The Archaeology Coursebook* lacks the depth of *Archaeology*. The quality of the paper and binding is also significantly different. *The Archaeology Coursebook*'s pages are thin, matte-finished, and nearly see-through, while *Archaeology*'s are thicker and glossier. The quality of the paper matters especially with the reproduction of the illustrations. Both books are heavily illustrated, and contain diagrams, charts, maps, architectural drawings, and side-boxes in addition to many black and white photographs. Neither book uses color photos. But in *Archaeology*, the sheen on the paper helps to enliven all the illustrations and make them more appealing, while in *The Archaeology Coursebook* they appear dull and drab. In addition to black, white, and gray, *Archaeology* also uses various shades of blue throughout the book in places such as the background in the side-boxes, as well as for highlighted text, emphasis on the maps, and so forth, which adds another level of visual interest, while *The Archaeology Coursebook* uses only black, white, and gray.

But even more significant than the appearance of the books, the text in *The Archaeology Coursebook* compares unfavorably to *Archaeology*. For one thing, *The Archaeology Coursebook* is clearly aimed at a British audience. Most examples of sites and finds are drawn from those in the United Kingdom, and the legal and bureaucratic institutions and procedures discussed are all based on how things are done in the UK.[1] The authors quite obviously do not expect to find much of an audience elsewhere. And with such a UK-centered perspective, they will not get one.

The text is very straightforward and matter-of-fact, and it delivers information about archaeological methods and practice in a clear and organized manner. The intention is to prepare students to succeed in an introductory level course, to develop study skills, and to manage class assignments and exams; and this drives the authors to ensure that the material is presented in a way that is clear and easily accessible. But the tone is so even that it fails to convey a sense of excitement or enthusiasm about the subject. The sentences are short and follow only a few simple construction schemes, so that the writing itself lacks punch or interest. In terms of content, the emphasis is almost entirely on how and where to practice archaeology, but, with a few notable exceptions (such as the section on social archaeology) rarely on why. The authors do a good job of explaining procedures, giving background and examples, highlighting key sites and key terms, suggesting tasks for practice, and inviting further study by providing information about websites that relate to the topics under consideration, but they have not written an interesting book that will engage many students or encourage them to develop an interest in the subject that goes beyond the classroom. In other words, *The Archaeology Coursebook* is an adequate text that does not present misinformation or otherwise mislead, but lacks a dynamic approach.

The text is divided into three major sections. The first, 'Understanding Archaeological Resources', is further subdivided into parts

[1] See, for example, pages 5–9 on researching maps and historical documents: the examples of types of files, offices where they are kept, and procedures to use them are all British.

that outline basic methodology and practices in archaeological exploration, explain various means of analyzing findings (including a separate section on dating issues), discuss interpretation of the findings, examine ways to protect and preserve sites and objects, and finally, discuss ways to present the findings. The emphasis throughout this section is always on the beginning student, so that terminology is carefully defined, technical skills are explained, basic exercises are incorporated in side-boxes throughout, and lots of illustrations and examples are presented. All of it is solidly grounded in the experience of the authors and all of it is presented in clear and understandable fashion. Section 2, 'Studying Themes in Archaeology' has a grander ambition in that it focuses on human experience and includes subsections on religion, settlement patterns, trade and economy, and social issues such as political organization, power, gender, and ethnicity, and even provides a few pages [285–290] on social change and conflict. This section is where the authors largely confine their thoughts about how human beings lived in the past and how archaeology helps to illuminate aspects that text cannot. Finally, the last section, 'Examination, Success and Beyond', is filled with practical applications relating to the classroom, and includes examples of projects and exercises that can be completed for a grade, advice and practical suggestions for studying for exams, and includes information about places to study archaeology both below university level and at universities in the UK. This section is quite obviously principally designed for high school students in the UK; and while some of it can be adapted to other levels and locations, it limits the usefulness of the book.

Archaeology, on the other hand, is far superior, both at speaking to a global audience, and at presenting an introduction to a discipline that the authors themselves clearly find fascinating. While it too delivers the requisite material, it does so in a way that is lively and interesting, asking lots of questions (indeed, every chapter title is a question), highlighting controversial issues, making discussion of ethics central to practice, and examining current political and cultural thought that shapes the way archaeology is practiced and funded in various places throughout the world. It takes a much more universal perspective and draws on a much more global range of examples. It does not shy away from difficult but important archaeological issues such as the excavations carried on at the site of the World Trade Center after September 11, 2001, the consequences

of the war in Iraq, or the manipulation for political or nationalistic reasons of how we present and interpret the past. Thus, *Archaeology* is not only interesting, it presents archaeology as a vitally important discipline, relevant to the present. *The Archaeology Coursebook* really cannot compare in this regard.

In conclusion then, an engaging, enthusiastic high school teacher in the UK could probably overcome the dullness of *The Archaeology Coursebook* with an exciting approach to the subject in the classroom, and might therefore choose *The Archaeology Coursebook* for its clarity, brevity, and low cost. But there is little reason why anyone else would want to use this book when a much better, more expensive but still affordable alternative exists.

L'alchimie et ses racines philosophiques. La tradition grecque et la tradition arabe edited by Cristina Viano

Paris: Libraire philosophiqe J. Vrin, 2005. Pp. 242. ISBN 2–7116–1754–8. Paper € 28.00

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This book is a rather delayed publication of a series of seminars organized by two sections of the Centre national de recherche scientifique between 1996 and 1998. Six of the 11 contributions have already been published in the alchemical periodical *Chrysopoeia* 8 (2000–2003). The introduction by Cristina Viano, however, gives more recent bibliography, and most of the articles themselves have been brought up to date. The book provides a valuable overview of the interface between theories of matter and alchemy. It begins with three articles by established historians of Greek philosophy concerning, respectively, matter in the *Timaeus* of Plato and Aristotle's criticism of Plato's theory (Luc Brisson; accompanied by useful diagrams of Plato's regular solids), the Stoic theory of matter and its relationship with the Timaean account (Jean-Baptiste Gourinat), and matter and emanation in Plotinus' *Enneads* (Denis O'Brien). This is followed by sections on Greek and Arabic alchemy respectively. The Platonic aspects of alchemy are particularly emphasized in Viano's own article on Greek alchemists and in Maria Papathanassiou's article on the alchemical work of Stephanus of Alexandria, where Plato's *Timaeus* is particularly prominent. The 'Plato' of the *Liber quartorum* (an Arabic text also extant in a Latin version, discussed by Pierre Thillet), however, has little to do with the genuine Plato.

The article by Thillet is the first study to be devoted to the *Liber quartorum*. This work purports to be a text of Plato, commented on by an otherwise unknown Abu'l-ᶜAbbas Ahmad ibn al-Husayn ibn Juhar Bukhtar on the request of a 'Thabit', who has been presumed to be Thabit ibn Qurra (Thillet follows the common view that this

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attribution should be held in doubt, but does not discount a 'Harranian' origin for the text). Thillet wonders whether the text and commentary were forged together (as has been suggested by Richard Lemay for the *Centiloquium* attributed to Ptolemy and its commentary by Ahmad ibn Yusuf, who incidentally appears here [205] as the forger of 'Greek Testaments' based on the *Republic* of Plato). The interruption of longer lemmata would seem to argue against this. There follows the more general question of the extant and nature of pseudo-Platonic literature, which has not received as much attention as pseudo-Aristotelian literature, though we can now add to Thillet's study a chapter on the subject by Dag Nikolaus Hasse [2002]. While many of these pseudo-Platonica have been listed, and some edited, by Abdu'l-Rahman Badawi, the only alchemical writing we know is a brief treatise entitled *fi'l-kimiya* in MS Tehran, Majlis-i Sura-i Milli, 6160, pp. 342–345, though some of the recipes in the magical *Liber vaccae*, which concentrates on organic concoctions, could be described as alchemical. This work, by the way, is mentioned twice by Thillet as an Arabic fragment taken from the *Book of Laws (al-nawamis) of Plato*, in Paris, BNF, ar. 2577 (pp. 205–206), without notice of the fact that the full text appears in Latin, with the title *Liber vaccae*, in several manuscripts. The title of the *Liber quartorum* recalls the fact that Plato's dialogues were arranged in groups of four ('tetralogies'). The 'third part' is said to have been translated by 'Astuminas' who appears as the dedicatee of Balinas' *Great Book of Talismans*[1] in Arabic and corresponds to Soustomos Thalassos in the Greek version of the same text. Another 'Harranian' work cited is Hermes' *Kitab al-Ustutas*. Most of the article is taken up with the discussion of titles in the Arabic text, the possibility that 'Iklidis' is not Euclid but a transcription of the Greek word for 'keys' and that the pupil of Plato, 'Umanitis', may simply be ὁ μαθητής (a 'pupil'). The book ends with useful summaries in English of all the articles.

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[1] Balinas = Apollonius of Tyana.

A Response to McGee on Ferreiro, *Ships and Science*

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David McGee's critique of my book *Ships and Science: The Birth of Naval Architecture in the Scientific Revolution 1600–1800* [2006] begins with a flawed premise; and from that he derives a series of equally flawed analyses of the text, which along with several outright falsehoods, paints an altogether erroneous picture of my work. My purpose here is to spotlight the flaws in David's analysis and to correct the unfavorable light he casts upon the book.

David begins his review with a 'full disclosure' expressing surprise at the title of my book *Ships and Science* and noting the original use of the title in his unpublished thesis, which he had sent me seven years ago. It is necessary that I respond in kind. The publisher of my book correctly felt that my original title was rather unwieldy, so my editor and I kicked around a few ideas until we arrived at '*Ships and Science*'. I attempted, as a courtesy, to inform David, but his place of employment (Dibner Institute) was closing down, my emails bounced back, and no one could provide me his forwarding information. However, I must point out that in my book I acknowledge his many contributions and that I remain grateful to him for the assistance that he has provided over the years.

David's flawed premise is that 'naval architecture' has *nothing* to do with engineering and science, and he fabricates his entire critique from that point. He does not, however, define the term himself, but merely quotes a dictionary definition that it is 'the design of ships and the superintendence of their construction'. As I carefully explain in my preface, such a dictionary definition is far too expansive to allow any serious study of the subject, as it would involve all aspects of conceptualization, design, and fabrication, and would cover the range from log rafts to ocean liners. I further explain that the term 'naval architecture' quickly evolved from its first usage, which originally

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meant 'an architecture of the sea', to encompass elements of geometry, mathematics, engineering, and science. A cursory examination of any naval architecture text today would show that the term refers to the *prediction* of a ship's characteristics and performance *before it is built*, and it is the evolution of this capability that forms the thesis of my work. It is simply wrongheaded to assume, as David does, that constructors in the past had no interest in prediction but were simply content to sketch, draw, or lay out a ship without any forethought as to how it might behave once it was built.

David completely misrepresents my words when he claims that I 'admit that 18th-century theories relating to the behavior of ships were. . . rarely, if ever applied to actual ship design'. In fact, my entire book is specifically devoted to showing how ship theory *was* extensively applied to actual ship designs during that century in many different navies (in France, Spain, Sweden, Denmark, Venice, for example), and supplies numerous instances of the actual calculations performed by naval constructors during the design process. It is impossible to understand how David came to exactly the opposite conclusion.

David continues to fires damp squibs into my work by claiming that I include extraneous material on the mathematical and scientific concepts underlying the relevant theories of ship resistance, stability, and so forth; but later he contradicts himself by stating that 'too little attention is paid to the underlying concepts'. He then provides a series of essentially meaningless summaries of the different chapters in my book that willfully ignore the basic themes and simply state what *he* thinks the book should contain. The important point which he misses, and which I am afraid the readers of his review will also miss if they do not read other book reviews, is that each chapter provides not just a summary of the major developments but also the *context* in which they were developed. This was essential as I intend this book to be read by historians as well as by practicing naval architects. I quite deliberately sought at every turn to explain history to engineers and engineering to historians, without sacrificing accuracy or clarity in either case. I will, therefore, correct David's long series of mistakes and outright falsehoods by briefly describing the chapters.

The first chapter, 'Mere Carpenters', serves as an overall introduction, establishing the underlying thesis that naval architecture

was developed and implemented in response to a bureaucratic need by naval administrations for greater control over their constructors, rather than as a means of optimizing the engineering of ships. It then describes the changing naval and maritime situation in Europe and explains how it provided the catalyst for the development and acceptance of naval architecture as part of ship design.

The next three chapters describe, in roughly chronological order, the evolution of the major lines of research into the theory of ships. Chapter 2 shows how the theories of maneuvering and sailing were debated and evolved in the context of published journals and professional bodies such as the French Academy of Sciences; thus, the chapter begins with a description of these structures during the Scientific Revolution. Chapter 3, 'A Shock to the System', demonstrates how the evolution of the theory of ship resistance became a small but strategically vital part of the development of rational mechanics. I carefully explain how Newton's 'shock' theory of resistance evolved into the notion of streamlines and pressure, through changing mathematical analyses as well as experimentation. At the core of this research were the great names of Huygens, Euler, D'Alembert, and the Bernoullis, all of whom contributed immensely to the understanding of ship theory. The navies of the era— principally France— supported such research with the obvious goal of making their ships go faster, so I critically examine historical data using modern analysis to determine whether these theoretical efforts paid off in faster ships. (Plot spoiler: French ships *were* faster than British ships, but not due to their constructors' use of ship theory.) Chapter 4 is a detailed explanation of how stability theory came to be developed. Once again, it was necessary to put this development in context, by carefully explaining that actual 'stability accidents' such as that experienced by the Swedish warship *Vasa* were quite rare, and were *not* the impetus for examining the science of ship stability (by contrast, navigational accidents, very common at the time, *did* spawn major state-sponsored research and improvements in astronomy and navigation science). Thus, I carefully lay out how stability theory evolved in discrete, comprehensible steps starting with Archimedes; and illustrate how the final synthesis occurred as a 'multiple' (Robert Merton's term) of three men working exactly simultaneously, but completely separately, to arrive at nearly identical solutions.

The final two chapters tie the work together. Chapter 5 describes how the elements of naval architecture were assembled into the great works of synthesis that laid the foundations of the profession and became the touchstones for further work. Chapter 6 explains how the development of ship theory occurred hand in hand with the growing professionalization of ship constructors (including the first engineering schools, professional corps, and learned societies), and describes how improved knowledge of ship construction quickly passed from one country to another through an almost continuous exchange of people and technologies. The chapter winds down by setting the stage for the passage of naval architecture from the age of wood and sail to the dawning age of iron and steam.

David wraps up his review with yet another series of misunderstandings and outright fabrications. He clearly does not understand stability theory, stating that ship stability is due to 'the movements of the centers of gravity and center of buoyancy', when in fact any basic text on the subject will show that it involves factors such as the distribution of waterplane area. He wrongly claims that 'naval science... would not even begin to be applied in a meaningful way until 1870', even though I provide specific examples of the use of resistance theory in the works of Robert Fulton, Isambard Kingdom Brunel, and John Scott Russell, dating from as early as 1809. Finally, he continues to make the discredited positivist assumption that ship theory *must* have been developed in order to solve problems with ship designs, when in fact (as stated above) these theories, e.g., ship stability, were *not* developed to solve otherwise insurmountable problems, but primarily in response to a bureaucratic need by naval administrations to gain greater control over the processes of designing and building ships.

David McGee's review of *Ships and Science*, in summary, is distorted, riddled with falsehoods, and completely misrepresents my work to the readers of *Aestimatio*. I encourage those readers to view the many other critiques available in professional publications.[1]

[1] For example, the American Library Association's *Choice: Current Reviews for Academic Libraries* [June 2007 Vol. 44 no. 10] rates the book as 'Highly recommended'. The influential maritime history journal *Mariner's Mirror* [August 2007 Vol. 93 no. 1] says 'This is a superb volume... to be regarded in coming years as [a] starting point for the study of applied science and en-

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gineering'. The Society of Naval Architects and Marine Engineer's *Marine Technology* [July 2007, Vol. 44 no. 3] says 'This volume should be required reading for all students of naval architecture'. Updated citations from reviews may be found on the book's website, or the MIT Press website.

Principles and Practices in Ancient Greek and Chinese Science by G. E. R. Lloyd

Aldershot, UK/Burlington, VT: Ashgate, 2006. Pp. xvi + 302. ISBN 0–86078–993–4. Cloth $114.95

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Since the early 1960's, G. E. R. Lloyd has been a leading figure in advancing the study and understanding of the history of Greek science, in large part effecting the metamorphosis of the discipline from the fragmented and narrow focus on modern categories of 'science' to a holistic investigation of inquiry into the natural world within the cultural, social, economic, and political *milieu*: compare Cohen and Drabkin [1948], Sarton [1952], and Clagett [1957] who excluded fertile but 'non-rational' fields of study, including astrology—based, in Mediterranean antiquity, on rigorous rules of mathematical astronomy—and alchemy, whence the modern discipline of chemistry. In recent years, Lloyd, casting his intellectual net even more broadly, has undertaken comparative studies of scientific approaches, particularly Greek and Chinese, in various foci of intellectual inquiry, especially mathematics and medicine [see Lloyd 2004].

For *Principles and Practices in Ancient Greek and Chinese Science*, Lloyd undertook the daunting task of choosing 15 of the most important or influential articles from the approximately 90 articles which he has penned over the last 20 years. These 15 articles (dating from 1987 to 2003) are now conveniently collected and easily accessible to students of Greco-Roman antiquity and the history of science. *Principles and Practices* falls into three parts. Part 1 includes five articles exploring the interpretation of Greek medicine. Part 2 includes six articles exploring technical questions in Greek science and philosophy. In Part 3 (four articles), Lloyd uses comparativist approaches to inquire into issues that may not occur to the specialist in a single area [ix]. Lloyd's comparativist studies succeed not only in emphasizing the themes developed in the first two sections of the

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volume (and throughout his publishing career) but also in revealing new questions. Several of the articles were originally delivered orally [I, XI, XIII, XV], and this collection generally exudes a readable and intimate conversational quality. The articles are presented here as originally published (including the original pagination); a dagger (†) appears in the margin where Lloyd has altered or augmented his text. The author provides a supplementary bibliography of recent publications at the end of each section, *per se* a valuable tool. Each article is numbered sequentially [I–XV], with that number reproduced on each page. Citations to the volume in this review include this Roman article number and an Arabic page number.

Principles and Practices is underpinned by three major themes [ix–x]:

- a continuing preoccupation with disciplinary boundaries and how that fixation hinders rather than furthers exploration, interpretation, and understanding [XV.197];
- the polemical nature of Hellenic society which prejudiced Greek investigations into the natural world and shaped the interrelationships between teacher and student as well as between competing schools; and
- the open-endedness and unpredictability of scientific research.

Complex intellectual and institutional factors contributed to the development of rational interpretations of the natural world. Close textual readings show that divergent approaches and conflicting results occurred not only between cultures (Chinese and Greek), but also within cultures[1] and even within philosophical frameworks.[2] There is nothing predictable or inevitable about how an institution or discipline develops, and full appreciation can be gained only through combining social, cultural, political and abstract considerations [I.118, XI.1, XII–XV]. It is from this holistic approach to the philosophy and history of science that the collected articles derive their thematic cohesion. Interpretations based on modern values and expectations reveal only the biases of the interpreter and lead to no true

[1] For example, competing Greek explanations of the physical nature of the cosmos such as the Stoic *plenum* theory *v.* Epicurean 'atomic' theory.

[2] Disagreements about causes and different interpretations are noted *within* both the Hippocratic [I.117] and the Herophilean medical traditions [von Staden 1989].

understanding. Lloyd's approach to such an ambitious program is far-reaching, but his method is always cautious, detailed, and meticulous; and he is the first to admit to conjecture [e.g., VII.178, VIII.171]. He draws extensively from the sources, both ancient and modern, and his arguments never fail to be axiomatic, deductive, and rational, with Euclidean precision.

In the first article, 'The Transformations of Ancient Medicine', Lloyd explores the changes in our perception of ancient medicine and shows how the shifting post-antique reception of Greek medicine in fact parallels ancient approaches: even in antiquity there was a selection and distortion of medical texts. Lloyd traces the history of the modern reception of ancient Greek medicine from the persistence of the canonical authority of Hippocrates,[3] Aretaus, Galen, and Celsus well after challenges to their expertise in anatomy, physiology, and pathology had been successfully mounted [I.114], to the debunking of that authority, and, finally, to a shift in recent inquiry regarding what cultural insights can be gleaned from the study of Greek medicine with an aim to restoring 'the wholeness and complexity of Greek medicine' [I.130]. Drawing from a wide range of medical and philosophical evidence, Lloyd illustrates the heterogeneous scope and aims of Greek 'scientific' (e.g., non-magical, non-ritualized) medicine. Medicine in antiquity was not institutionalized and medical 'schools' in the modern sense were lacking. There was no unified approach, theory, or understanding of the fundamentals or of the details within the medical sects, much less between them: consider the conflicting Hippocratic theories of humors [I.126] and the Herophilean articulations of pulse theory [von Staden 1989]. Rational disciplines in Hellenic Greece—and medicine was no exception—were influenced by the paradigm of rhetoric and the ideal of citizen participation in government and politics. Plato had the sophist Gorgias [*Gorgias* 456b ff; Lloyd 1979, 254; I.128] pride himself on his rhetorical skills which rendered him more persuasive on medical issues than even his brother, a trained and practicing physician. Even Hippocratic treatises, including *Precepts*, *Decorum*, and *On Diseases*, advise practitioners on how to debate with a patient, family member, or fellow-physician

[3] Littré [1839–1861] was motivated by his own conviction of the contemporary value of the Hippocratic corpus [I.114].

[I.128]. A physician's success was measured by his ability to convince patients and peers of his authority and of his claim to the right answers *via* debate and dialogue [I.129–131].

Lloyd, who has long been interested in articulating the scope of science in general and natural philosophy in particular [XV.195: cf. Lloyd 2004, 33], takes up an ancient Greek debate on the precise parameters of 'medicine', the investigation of which can do much to elucidate how the original thinkers perceived and understood the issues. The polemical nature of Greek intellectualism and the plurality of medical traditions gave rise to debates over the definition of medicine, whether medicine constituted a rational discipline (skill or τέχνη as opposed to luck, experience, or knack [II.259]), and who had the authority to practice the medical τέχνη. These questions are explored in the second article, 'Definition, Status, and Methods of the Medical *Techne* in the Fifth and Fourth Centuries'. It was the answers to these questions which distinguished physicians (ἰατροί) from imposters, laymen, and midwives, who nonetheless served important functions.[4] Lloyd's scrutiny of three Hippocratic treatises (*On the Art*, *On Regimen in Acute Diseases*, and *On Ancient Medicine*) reveals wide-ranging and conflicting views of the definition, aims, scope, methodology, and limits of medicine and medical practitioners. The author of *On the Art* expansively defines medicine as the complete removal of suffering from illness and the 'alleviation of violences of diseases', but nonetheless allows the physician to refuse to treat a patient 'where the disease has already won the mastery' [I.253].[5] To the author of *On the Art*, medical τέχνη involves *doing* or *not doing* something to effect a cure, even if a patient cures himself without consulting an expert. The author of *On Regimen in Acute Diseases* suggests that the debate is waged because of the incompetence of some practitioners and disagreement among others [II.254–245].[6] *On Ancient Medicine* attests the conservative and traditional nature of

[4] Although male physicians did oversee normal and abnormal births [[Hippocrates], *De nat. mul.* 1.34, *De nat. puer.* 30], a midwife was more likely than a ἰατρός to attend at a birth [Dean-Jones 1994, 34–35, 212–213: cf. Euripides *Hipp.* 293–296].

[5] τέχνη cannot in all cases be expected to secure a cure; success proves the power of the τέχνη, but failure does not belie its authority.

[6] Interestingly, Lloyd points out the frustration noted even in the primary sources over Greek medical pluralism.

the discipline. Its author attacks those who tried to apply 'the new-fangled method of postulates (ὑποθέσεις)' to medical diagnosis as if they were undermining the status of medicine as a τέχνη 'since there is no criterion to which one should refer to obtain clear knowledge' [II.256], thereby oversimplifying the questions [IX.263] and reducing the art to chance.

In article III, 'Scholarship, Authority, and Argument in Galen's *Quod Animi Mores*', Lloyd further explores the primary themes of the first two articles: the effect of rhetoric in medicine and the aims and definition of the art. In *Quod animi mores*, Galen inverted Gorgias' argument to claim that a physician is in a better position than a philosopher to make patients better and more intelligent: since the soul follows the body, the physician can suggest changes in diet and regimen to improve a patient's character and intelligence—'the best doctor is also a philosopher' [IX.260: Kühn 1821, 1.53–63].[7] Galen drew extensively, but selectively, from medical, philosophical, and poetic sources. He simplified Plato's arguments in support of his own thesis on, for example, the nature of the soul and immortality [III.19]; and he also misinterpreted or misapplied Aristotle on, e.g., the correlation between physical features and character or intelligence [III.25–26]. Galen's purpose in *Quod animi mores*, as was the aim of the protreptic and apologetic Hippocratic treatises evaluated in article II, was to advocate the importance and prestige of the medical art as a τέχνη. Lloyd shows how Galen's judicious and sometimes distorted use of sources attests to the perceived and real connections between the medical τέχνη, rhetoric, and philosophy.

Philosophy, modern as well as ancient, shares with medicine a plurality of approaches and interests as well as a lack of consensus on its definition, scope, and aims: definitions and understandings of terminology were far from concrete [IV.262, IX.261, X.1]. Philosophers were admired by some, but reviled by others, e.g., the author of *On Ancient Medicine* [X.3]. Medicine and philosophy, with their

[7] See also van der Eijk 2005 for the circumvolutions of philosophy and medicine.

own theoretical and practical agendas, developed sometimes conflicting, sometimes harmonious, epistemological models. Lloyd scrutinizes the interrelations and rivalries between medicine and philosophy more broadly in articles IX ('Philosophy and Medicine in Ancient Greece') and X ('Pluralism of Intellectual Life Before Plato'). As Galen relied upon the authority of Hippocrates of Cos, so too did Plato have Socrates enlist Hippocrates as an ally [*Phaedrus* 270c] on the connection between ethics, morality, and the study of nature and the human body. Lloyd rightly points out the ambiguity in Plato's text: What precisely is meant by the study of 'the whole nature' [IX.258]? Like Lloyd, ancient thinkers pursued wide-ranging interests. Plato drew analogies between justice and health in opposition to disorder and disease [*Gorgias* 504b–d: cf. *Republic* 564b–c], as Aristotle sketched analogies between health and morality [IX.259]; and Galen argued that the 'philosopher-doctor' was in the best position to help his patients physically, intellectually, and morally. Medical texts are replete with discussions of language, nature, convention, cosmology, and elemental theories: the author of *Airs, Waters, Places* even includes an ethnographic survey [X.5–6: cf. Herodotus, *Hist.* 3.106; Strabo, *Geog.* 6.4.1]. Among the Greeks, specialists were rare, excepting Euclid, Euctemon probably, and Meton; and epistemological approaches and explanations in all rational disciplines were pluralistic and heterogeneous [XV.199].[8] Lloyd, while sceptically attempting to define what constitutes philosophy before Plato, emphasizes that Presocratic philosophers cannot be made to fit into artificial modern categories, that those thinkers whom we thus categorize hardly shared a methodology or an interest in all the points which might constitute philosophy (e.g., cosmology, the relationship between language and reality) [X.10], and that insight can be gained only through caution and an appreciation for the complexity of their polymathic interests [X.12].

The criticism of the application of ὑποθέσεις to medicine meted by the author of the Hippocratic *On Ancient Medicine* [II.256] points to an early connection between medicine and mathematics, and Lloyd scrutinizes this link specifically in two articles chosen for *Principles and Practices*. Galen prided himself on his mathematical training

[8] For the extensive and flexible scope of Greek 'scientific' writers, see Rihll 1999, esp. 7, 89–90.

and applied its principals to his medical philosophy [IV–V]. In article IV, 'Theories and Practices of Demonstration in Galen', Lloyd explores Galen's application of axiomatic-deductive reasoning to demonstrations in medicine. The strengths of such an approach are self-evident: it is orderly, systematic, and methodical; a clear logical structure is demanded; and refutation must be guided by the application of logical analysis. This approach, however, is overly idealistic [IV.264] and inappropriate to medicine [IV.276], which is hardly an exact science. Galen certainly made no claim to conclusive knowledge, yet he repeatedly claimed that parts of medicine could be made the subject of rigorous geometrical-style demonstrations [IV.274]. Galen recast empirical observations in a geometrical mold 'with deductions stemming from starting points for which axiomatic indemonstrable status is claimed' [IV.277]. Galen's application of axiomatic-deductive reasoning was contentious and rhetorical; by appealing to the prestige and incontrovertibility of his method, Galen sought to establish his own authority [IV.273–274].

In articles V ('Mathematics as a Model of Method in Galen') and VII ('The *Meno* and the Mysteries of Mathematics'), Lloyd considers the status of mathematics, its connections to other rational fields of inquiry, and how Galen [V] and Plato [VII] invoked mathematics to establish their *éclat* and persuade the audience of the truth of their opinions, theories, and conclusions. Despite the difficulty and unpopularity of mathematics amongst physicians,[9] Galen valued the prestige and exactitude afforded by mathematically linked examples[10] and demonstrations, and so he used mathematics to persuade his readers of his own expertise and incontestability. Like Galen, Plato may have used mathematics for its prestige without fully understanding its complex nuances in *Meno* 86e–87b [VII].[11] As the use of mathematics in Galen may have served a non-mathematical purpose (to establish irrefutability), so Plato's aim may have been one of investiture. Plato under-described his hypothesis regarding whether

[9] According to Galen, his readership considered mathematical explanations obscure and unduly lengthy [V.113].

[10] E.g., the geometrically informed explanation of the cone of vision: see *On the Usefulness of the Parts* 10.12: V.124–125.

[11] Gregory 2000 does not address this passage and assesses Plato's geometry only in the context of his astronomy, atomism, and ideals of philosophical education.

an 'area can be stretched out as a triangle in this circle', thus making it unnecessarily obscure [VII.177–178] by withholding essential information [VII.180], in antithesis with orthodox Greek mathematical practice wherein the teacher would logically and methodically lead the student step-by-step through a proof until arriving at the conclusion by whose indisputability the student would be overwhelmed [XIV.136]. Lloyd suggests that Plato's geometrical obliquity may have served a non-mathematical purpose, that of initiation: a guide (Plato's Socrates) attempts to facilitate a student's (Meno's) exploration of various possibilities of the problem's resolution [VII.180]. Lloyd guardedly suggests that the obscurities of the mathematical problem point to 'the need of initiation—not, of course, into the method but in the fields to which it is applied' [VII.181]. The deliberate obscurity and Lloyd's cautious suggestion of a context of initiation bring to mind Pythagoreanism which has been connected to Orphism, which was itself characterized by mystery and initiation rites [Kerényi 1950; Kahn 2001, 19–22; Assmann 2002]. De Zwarte [2004] argues intriguingly that in the Heraeum at Paestum, a temple possibly created by the Pythagorean community and wherein there seems to be embedded much mathematical knowledge, the placement of at least three columns of the *naos* may have been selected for the sake of (perhaps secret) mathematical wisdom, to which only members of the community would have had easy access. That is, the columns may have been so placed for reasons other than aesthetics. Worshippers of Hera at Paestum may have understood the mathematical implications of the temple under guidance similar to that which Socrates attempts to proffer to the intractable Meno. Plato's partially presented mathematical problem further bears some interesting resemblance to Chinese educational dynamic (as well as Druidical, if Caesar is to be believed: see *Bell. gall.* 6.14), wherein the student is expected to memorize and internalize texts (often marked by rituals of initiation in Chinese culture [XIV.136]) before coming to understand them, and then to conserve and transmit the corpus [XIII.162].

In article VI, 'The Alleged Fallacy of Hippocrates of Chios', Lloyd evaluates an ancient debate about whether Hippocrates of Chios engaged in paralogism in his quadrature of lunes. The debate underscores one of Lloyd's underpinning themes: the desire to establish incontrovertibility and to challenge the credibility of a rival. As in article I [117–118], the question remains, 'Who has the right to

speak with authority about "science"?' Hippocrates' original text is long lost, and the argument and proof have been mediated through Aristotle, Themistius, Eutocius, Eudemus, Alexander of Aphrodisias, and Simplicius. Simplicius, Lloyd suggests, used the issue to undermine the credibility of his rival Alexander of Aphrodisias by representing him as having attributed a fallacy to the esteemed Hippocrates [VI.12]. Lloyd's methodically crafted examination of the language in the extant evidence shows that Hippocrates must have made some unreconstructable rhetorical remark about his quadrature of the lunes which subsequently misled Aristotle. Lloyd asks an important question on VI.118: To what extent does Simplicius offer 'verbatim quotations from Eudemus'? This query raises a fundamental issue regarding the survival of scientific texts, here hinted at but not fully explored. In essence, Lloyd asks, How reliably has Simplicius represented Eudemus' original ideas? Although some scientific treatises have been transmitted intact, many are distilled only through later commentators and encyclopedists (e.g., Iamblichus and Simplicius). The modern scholar is handicapped in working with later interpretations of scientific ideas rather than with the original formulations of those ideas.[12] Simplicius' presentation, eclectically compiled from

[12] The problem arose early in the transmission of the texts: Plato and Aristotle (both valuable sources for extracts and *testimonia* of earlier philosophers) engaged in the exegesis of earlier natural philosophers, and Galen's compendious corpus is an invaluable but selective and biased source of earlier medical theory and practice. Galen, for example, levels vicious attacks against the Methodists, claiming that he had succeeded where they had failed, taking over their cases and exposing their murderous ignorance [*De meth. med.* 10.31–38, 162–173, 204–205, 316–357, 390–391]. Galen, furthermore, had only secondhand access to many texts through Andromachus, Asclepiades, Criton, and others. The fragmentary and piecemeal nature of scientific texts brings to the fore even deeper problems , as Lloyd palpably demonstrates in his cautious attempts at reconstructing the argument. Lack of textual context and continuity (just as a lack of cultural and social context) lead to tentative resolutions of philosophical problems, especially as regards the particularly fragmentary survival of Presocratic texts whose disputed details stimulate debate rather than engender resolution: this issue is particularly brought to bear on Lloyd's discussion of Empedocles' 'theory of evolution' [XI.6–7]. Interestingly, though hardly surprisingly, the same problems are noted in other traditions, including Chinese. In his investigation into the cosmological and heterogeneously compiled *Huainanzi* [XXX.148–149, 153],

remote sources, drew upon the authority of one source (Eudemus) to undercut the influence of another (Alexander). Likewise, Galen selectively relied upon the authority of Hippocrates of Cos, Plato, and Aristotle, although passages he cited from these authors and others 'for support of his general thesis [did] not do so to the full extent he requires nor in quite the way he represents' [III.12, 18–31].

In Article VIII, 'Plato and Archytas in the Seventh Letter', Lloyd delves into the debate of the authorship of that letter. Of the letters attributed to Plato, the seventh is the most substantial and most philosophical [VIII.159]; and, although inconsistencies and un-Platonic statements are evident [VIII.60], Platonic authorship should not be discounted [VIII.171]. Lloyd conjecturally posits that a careful reading of the seventh letter reveals subtle criticisms of Archytas as a mediator or interpreter of Plato's philosophy to Dionysius II. A close reading of the letter also undercuts the king's claim to understand 'the most important things' (τὰ μέγιστα) [VIII.164, 168–169]. Despite (or perhaps because of) his professed enthusiasm for mathematics, Pythagoras, and Archytas, Plato may have viewed Archytas as a rival, and so, eschewing the heavy-handed tactics observed in Lloyd's assessment of the use of the rhetorical arts by the medical writers, he (if Plato is the author of *Epist.* 7) may have applied subtle but persuasive language to secure 'the independence and originality' of his philosophy, thereby elevating his own status and weakening a potential rival [VIII.172].

The need to understand ancient texts within their cultural context has remained one of the core themes in Lloyd's considerable scholarly corpus, and he commends, on the side of Hellenic studies, recently fashionable scholarship on attitudes to and beliefs about the human body (Greek assumptions are not the same as our own): 'attitudes to the body often provide a key to much else in the belief systems, the values, the cosmology, of the society in question' [I.118]. Aristotle's explanation of the existence and function of females—that females are failed males because of their inability to concoct blood [I.124: Lloyd 1983, 94–105, esp. 95n139]—brings to the fore Aristotelean (and Hellenic) gender biases. The cultural *milieu* is especially

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Lloyd raises precisely the same problems noted in his discussion of the 'Presocratic' philosophers and the transmission of Hipparchus' claim regarding the quadrature of lunes.

important in the question of Greek theories of evolution, the topic of article XI, 'The Evolution of Evolution: Greco-Roman Antiquity and the Origin of Species'. Lloyd sets the debate securely in the ancient world, surveying the documentary evidence within the larger philosophical dialogue about, e.g., coming-to-be and passing-away,[13] teleology,[14] and popular and traditional beliefs such as that in the Minotaur. Xenophanes recognized substantial changes in the configuration of land and sea [Kirk, Raven, and Schofield 1983, §184], but the extant fragments of his writings suggest no theory of the extinction of species or changes ('evolution') in species. Aristotle, in fact, rejected the notion of the evolution of species [XI.11]. Investigations into the natural world of animals and animal species were fueled not exclusively by zoological interest but also by philosophical and moral interests (especially in cases of Epicurus and Lucretius). The ancient discussion was 'not in anticipation of modern ideas' [XI.1] but was far different in scope, purpose, and resonance. The Greeks adhered to no cosmological dogma (in contrast with the modern cultural backdrop of continuing debates on the merits of Darwinism) but looked to the animal world for ethical and moral *exempla*. Lacking in Greek antiquity were explosive population growth, the dogmatic theory of creationism, and new information about the dissimilarities between species and varieties which informed Darwin's work [XI.14]. As tempting as it may be to interpret the charming tales in Xenophanes and Empedocles through a 21st-century filter, the conclusions reached would reveal more about the intellectual biases of the modern scholar than the philosophy of species developed by ancient thinkers.

Over the span of the past two decades, Lloyd has devoted much of his substantial intellectual energy to questioning the comparisons between scientific traditions, recognizing that no cross-cultural universals (or even scientific universals) exist. In the three comparative articles [XII–XIV] chosen for the Ashgate collection, Lloyd applies a multidisciplinary approach to explore the nexus between science, philosophy, tradition, and mythology, advancing his earlier researches

[13] Even among proponents of an eternal *versus* a created cosmos there was a wide acceptance of large scale cyclical change [XI.3].

[14] Advanced by Aristotle, but denied by Epicurus as producing animals useless and hostile to humans [XI.8–12].

[Lloyd 1979, 1983, 1987]. He delves into variant aspects of intellectual approaches to unravel and note important cultural diversity in the expectations about the nature of wisdom, the purpose of scientific texts, how cultures thought about science and philosophy and used various categories, and the very purpose of the act of philosophizing within distinct cultures.

Lloyd explores how social and political constructs shape intellectual approaches in article XII, 'Appearance *vs* Reality: Greek and Chinese Comparisons and Contrasts'. All cultures recognize hidden realities, but for the Greeks the question of seeming *v.* being played a crucial role in the development of Greek philosophical thought [XII.306–310]. An essential component of the Greek discussion was the debate regarding change, that is, whether change—coming-to-be or passing-away—is possible. Divergent conclusions of competing schools and thinkers, working within the framework of the debate defined by the dichotomy between the intelligible and the perceptible, were often agonistically motivated in the competition for prestige and in attempts to persuade peers and students. Such pressures did not exist within Chinese intellectual society, where the role of the philosopher was to transmit (not alter) the canon and the goal was to advise benign rulers who provided the primary source of employment and patronage [XII.314, XIV.133], which was antithetical to the independent and self-sustaining existence of the Hellenic Greek philosopher. Within this social framework, Chinese philosophers, since their work was addressed not to peers or potential students but rather to the benign patron-ruler, felt no pressure to engage in polemic; and, although they questioned the difference between appearance and reality and acknowledged hidden forces [XII.311], Chinese scholars recognized no fundamental ontological dichotomy and posited no ultimate truth that was acquired only through reason. The Chinese explained physical, social, and moral change [eventually, XII.312], through the workings of *yin yang* and they accounted for the five phases (*wu xing*) as governing cycles of mutual change not dependent upon the sharp contrast between reality and appearance [XII.315].

Likewise, as Lloyd shows in article XIII, 'Mythology: Reflections from a Chinese Perspective', the same cultural and political phenomena—the Greek love of rhetoric and the competition for prestige, the Chinese favor of consensus and disdain for debate—explain the absence of the dichotomy between mythic and rational accounts

(μύθος *v.* λόγος), so prevalent in Greek rational thought, in the Chinese understanding of cosmology. The cosmological *Huainanzi* includes etiological, empirical, mythical, and rational elements—'an extraordinary variety of material handled in a striking diversity of ways' [XIII.151–152] yielding a text which cannot be demarcated into μύθος and λόγος but instead 'quite simply presents itself as a seamless whole' [XIII.153]. Lloyd analyzes the Greek and Chinese use of language and rhetoric to conclude that diverse technologies of communication and linguistic idiosyncrasies themselves help to explain discrete intellectual approaches [XIV.133]. Similarly, cultural and political factors (expectations of citizen participation), combined with variant modes of writing and literacy (the Greek alphabetic script), in part account for disparate mathematical and medical approaches. The axiomatic-deductive paradigm of Greek mathematics begins with an indemonstrable premise, alphabetically building up a diagram, as the student is led through a sequence of incontrovertible steps, ending with an irrefutable conclusion [XIV.136]. The social and political factors, as they affect Chinese intellectual institutions, lack the dichotomy between perceptible and intelligible as well as between μύθος and λόγος, further explaining the Chinese approach to mathematics, which eschewed the axiomatic-deductive paradigm in favor of consent, unity, and loyalty within the intellectual lineage [XIV.136–138]. In China, 'the negative models' of 'rhetorical debates', so prevalent in Greek culture, were simply lacking [XIV.136].

In the last article of the collection (XV), 'Is there a Future for Ancient Science?"' (his valedictory lecture marking his retirement from teaching), Lloyd reflects on facets of both ancient and modern rational inquiry, including how the study of ancient Greek science has changed and how advances in modern science have been brought to bear on the history and philosophy of science. E. R. Dodds' seminal *The Greeks and the Irrational* [1951] shifted the emphasis in classical cultural studies away from 'the triumph of Greek rationality' and narrow inquiry, thereby encouraging the analysis of the whole culture. Lloyd emphasizes the importance of examining both the successes and failures of Greek science, philosophy, and culture as a means of restoring the 'the wholeness and complexity' of Greek scientific pursuit [I.130, XV.197]. Comparativist studies reveal universal questions raised by human thinkers (interest in the heavens, the nature and health of the human body), but endemic cultural investigations into

these same questions are inspired by assorted motives and yield contrastive explanations. The comparativist approach poses significant demands, including the mastering of many (non-related) languages and cultures [XV.208]; but, Lloyd argues, such an approach will be most fruitful, as indicated by recent scholarship at Cambridge (and elsewhere) in ancient science, which, though 'not explicitly comparativist, had been animated by an awareness of alternative traditions' [XV.208]. As Lloyd has rightly stated elsewhere [1991, 353], he reiterates the importance of inquiring into ancient science, the pursuit of which far exceeds the technical details of mathematical, medical, and philosophical explanations of the world: 'For science in antiquity, read the ancient understanding of the world—and that takes you straight to the heart of the values of the culture in question' [XV.207].

The articles which Lloyd selected for *Principles and Practices* focus tightly on ancient texts and issues, but his interest in modern relevance [especially Lloyd 2004] does peep through on occasion. Specifically, he suggests that the modern physician can learn much from the Hippocratic dialectical approach, which results in an open ended discussion between physician and patient about health and disease [I.32]. Epistemologically, Lloyd emphasizes that cognitive models vary within and between cultures [IX.271–272], and that the pluralism of intellectual approaches reflects nothing less than the diversity of human experience, as illustrated, for example, by anthropological studies into how human cultures classify animals, which habitat stratum they occupy (air, forest canopy, and so forth), whether they are domesticable, whether they are edible, and so on. [IX.266–268]. These queries have far-reaching implications for the philosophy of science as a whole in underscoring that there are no absolute answers [IX.268–269].

It is clear that differences between Hellenic and Chinese culture resulted in divergent rational explanations of the natural world. The same criteria of geographical location (e.g., Attica, and Ionia) and variant political systems (e.g., democracy and aristocracy) could perhaps be applied in scrutinizing differing Greek approaches to philosophy, mathematics, medicine, and other rational disciplines. Like Greek medicine, Greek cultural identity was hardly homogenous. The language, of course, was a unifying factor (although dialect and vocabulary varied regionally), and individual πόλεις worshipped many of the same gods and celebrated many of the same

festivals (with local modifications). Nonetheless, there was no pan-Hellenic political or philosophical concord, not even after Alexander. It is agreed that certain social, cultural, and political factors were at work to spark the scientific revolution in Ionia[15] Welcome would be a study of geographical trends in Greek medicine and/or mathematics to ascertain whether peculiar local factors (e.g., political, economic, intellectual) affected the trends in the development of rational fields of inquiry.

Principles and Practices in Ancient Greek and Chinese Science is not a monograph, and this fact results in some minor inconsistencies. While most are carefully and thoroughly annotated, article XI lacks citations (easily supplied by the advanced or talented intermediate student), and there are inevitable overlaps of evidence and examples: Gorgias' persuasiveness [I.128, II.252–253, X.7, XII.08, XIV.131]; Plato's attempt to educate Dionysius II [VIII.159–170, 172–173; XII.314; XIV.127]. Nonetheless, the volume is an invaluable collection, adhering to the mission of the Variorum Collected Studies Series to bring together a 'selection of articles by a leading authority on a particular subject' and to 'make available research that is scattered, even inaccessible in all but the largest and most specialized libraries'. The brief introduction is most helpful in establishing the cohesiveness of the collection, and each essay follows from its predecessor. Lloyd's treatment of manifold questions always engages the reader, and each article rewards with insight and elucidation. The anthology stands not only as a useful synopsis of recent trends and discoveries in the history of Greek (and Chinese) science but also as a testament to the continuing guidance and contributions to the history of science and ideas by the indefatigable Geoffrey Lloyd.

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[15] Graham 2006 is the most recent to affirm this.

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Die antiken Sonnenuhren Griechenlands. Festland und Peloponnes mit CD-ROM by Karlheinz Schaldach

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This is a carefully documented book on ancient sundials in mainland Greece and the Peloponnesus, beautifully illustrated, covering almost all the extant evidence. Numerous excellent photographs supported by a CD-ROM and generous, carefully laid-out diagrams provide a vivid visual impression and enhance the presentation of the measurement data. The measurements themselves were carried out with meticulous care, and the mathematical evaluation is presented in a way that makes it accessible to a general reader. The book is very useful as a reference tool. When supplemented by the already existing catalog of Roman sundials in the West (by the same author) and the planned catalog of Greek and Roman sundials on the islands, it will indeed replace the now current standard reference work by Sharon Gibbs [1976]. For it does constitute a genuine advance: more sundials are taken into account, more details and data are included, and improved measurements are provided. There are more illustrations and images, and criteria for a rough dating of sundials are developed. A new method of analysis is proposed with a simplified mathematical apparatus, in geometrical representation; it yields more specific, and arguably more descriptive, parameters for evaluating sundials. The author's main objective, which is to provide sufficient (if preliminary) documentation for further work towards a 'bottom-up' account of ancient sundialing that takes full account of the obtainable data, is duly met. In addition, as I have said, this book has the advantage of being accessible to a general reader, though it could be used as a source for academic study as well.

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The book falls into three parts:

1. A history of sundialing from the ancient Orient to Byzantium
2. A catalog of extant sundials on mainland Greece and on the Peloponnesus (this is the heart and core of the book), and
3. Mathematical analyses.

An appendix provides maps, diagrams on latitudes and *climata*, tables, a bibliography, and indices. Especially noteworthy are the beautifully illustrated catalog [part 2] and within it, the chapter on the Tower of the Winds [2.1]. The chapters on the history of Greek and Roman sundialing [1.2] and the methodological remarks [3.1, *passim*] are also well worth considering in more detail.

1. History
   1.1 Pre-Greek (Babylonian and Egyptian) sundials
   1.2 Greek and Roman sundials
   1.3 Byzantine sundials
2. Catalog (descriptions with photos, rough dating, evaluation)
   2.1 The Tower of the Winds and its sundials, treated separately
   2.2 General remarks on the cataloguing of the other finds
   2.3 Catalogue of the remaining finds on the Greek mainland and the Peloponnesus
3. Analysis
   3.1 Methodological remarks on the mathematical tools for analysis, and on their status
   3.2 Documentation of the results of the mathematical analysis of the measurements, comparison with other evaluations

Introduction

According to Schaldach, Sharon Gibbs' book on Greek and Roman sundials [1976] is still the state of scholarship. He builds on her account everywhere, even the set-up and organization of his work parallels hers. Even so, he argues that a fresh look at the evidence is needed, and suggests a shift of emphasis in how one interprets the finds. A historical approach has so far tended, in Schaldach's opinion, to override, even sideline, the material evidence, and to overemphasize a purely mathematical interpretation of the lines on ancient sundials. As a result, the emerging picture is unnecessarily poor in detail and unclear as far as the actual objects are concerned.

Data collection and measurements on the actual objects do not take full advantage of what is available in the material. In addition, there are other interesting questions that are obviously relevant for a better understanding of the sundials in their cultural context but not often addressed in historical studies. They include: Who used the dials and for what purpose? Why were they erected in societies that otherwise showed scant interest in measuring time? By what criteria should their accuracy be judged? Who made them and how? Who paid for them? What was their cost? What was the source of the materials used? What do they imply about the societies that financed and erected them? Schaldach proposes an approach to the ancient sundials of Greece that would allow the pursuit of such questions by providing an analysis that places more weight on, and makes more extensive use of, the data that can be directly observed in an object as opposed to a few data that correspond, more or less (mostly less) to an ideal line distribution derived from some theory for an ideal sundial-shape.

1.1 Early sundials

Schaldach's historical overview begins with a survey of pre-Greek sundialing in Egypt and Babylonia, with the intention of conveying to the reader what the Greeks took over from the Orient and where their specific innovations lie. Among other things, the types of instruments for measuring time and the key item, the *gnomon*, are discussed. In my view, this chapter is not the strongest in the book. In particular, I am not sure that it achieves its declared goal because Babylonian and Egyptian sundialing does not become clear enough in outline so as to compare it with the specific Greek achievement. The main conclusions of the chapter are that there were Babylonian and Egyptian sundials; that the Babylonian approach to time measurement was predominantly arithmetical; and that time measurements in both cultures have a religious context. Moreover, Schaldach maintains that all pre-Greek sundials are flat, not hollow; and that the closest parallel to early Greek sundials is a flat semicircular disk attested in 13th century (BC) Egypt. This, according to him, is the type that was transmitted into Greece.

1.2 Greek and Roman sundials

This chapter is very interesting and stimulating. Schaldach labels it 'preliminary' because a fuller evaluation should, in his opinion, be based on the complete material evidence, including the sundials on the islands. Even so, a clearly argued, well-reflected general outline for the history of Greek and Roman sundials in their cultural context emerges.

Contrary to an established standard opinion, Schaldach maintains that the earliest sundials were not spherical. Eudoxus (fourth century BC) worked as a gnomonician. His device, the *arachne*, was a flat sundial with vertical gnomon. Sundials were taken over from the Orient; and in classical Athens, once scientists were given Eudoxus' new geometrical model of the cosmos, they explained the lines on the dials in light of that model. The science of gnomonics was thus created. The dials constructed by the practitioners of gnomonics had at the outset no public role or place: they were of purely theoretical and scientific interest. Scholars, not engineers, were the ones who developed the sundials further, in the context of applied astronomy for its own sake. '*Sie waren—was im weiteren Text noch deutlich wird—vorrangig am* Modell dargestellte Theorie *und wurden damit zu Boten* neuer wissenschaftlicher Erkenntnisse' [32].

This was not the case throughout antiquity, however. Schaldach makes a convincing case for a developmental story. Ὥρα, meaning 'hour' (not 'season'), was introduced into society in Hellenistic times. Over time, the sundial changed from being a scientific 'diagram' or instrument to a commonly used clock. This development is reflected in the shapes, and in the presentation and function of the sundials.

With reference to the shapes for Hellenistic sundials, Schaldach differentiates three stages:

- equatorial,
- hollow cone, hollow sphere, and
- all other types.

In his account, spherical and conical sundials were a Hellenistic development. Alexandria and Rhodes, not Athens, were centers of gnomonics in the Hellenistic era. Even so, the most impressive achievement of Hellenistic gnomonics is the Tower of the Winds in Athens.

It was built *ca* 100 BC by Andronicus. No further development of theoretical dialing is attested in Greece after Andronicus.

Towards the end of the Hellenistic era, changes in attitude towards time and time measurements made themselves felt. They were reflected in the further development of the construction of such dials, but also in the place which they actually took in communities. While all other earlier dials come from sanctuaries, Andronicus' Tower stands in what soon became a public space (the Roman Agora in Athens). According to Schaldach, this is indicative of a changed perspective on sundials as objects and instruments. The transition to the public/secular sphere seems to be a takeover from Rome, according to Schaldach.

Most Roman sundials in Greece are in public spaces; there were presented either freestanding on a column or on walls so as to be publicly visible. Despite a general decrease in population, the number of sundials actually increased during this period. Schaldach argues that this means that time and timekeeping became more important as a factor in public life. At the same time, as the function of the dials changed, so did their nature. They were no longer seen as diagrams in connection with a scientific theory, or as implementations of a scientific world picture, or even as precise scientific instruments. The dials became rough and ready indicators of the hours of the day—their accuracy was tolerable—and the datelines on them (a feature that makes the dials useful as calendars) deteriorated until they become purely decorative. From the standpoint of accuracy, these dials are of much poorer quality. It appears as though the masons gradually lost the knowledge and the expertise needed to produce dials that implement gnomonic theory in detail. Nevertheless, as Schaldach correctly insists, one should not interpret these dials in the light of gnomonic theory alone because that is not what went into them and it does not take account of what they were for. The main reason for the 'decline' in gnomonic quality is a change in the purpose and function of the sundials, and we should try to account for the dials in a more contextualized way. We do more justice to the dials, and to what they meant to the people who produced and used them, if we accept their inaccuracy as scientific instruments for measurement, and explain them as the deliberate result of a less sophisticated design.

The third and fourth centuries AD represent a last stage in ancient sundialing. In concurrence with the general cultural trend, science, and generally rationalistic explanations of the physical world as a meaningful whole, fell out of favor. The sundials from this period show purely decorative date lines. The inexactitude even of hour lines increases drastically. In the cultural context, though, this makes perfect sense. The inexactitude was well within the range of tolerance for the user. These dials should no longer be viewed as tokens for gnomonics at all. The only valid criterion for their production was purely pragmatic and solely concerned their use as simple devices for indicating the hour. This is manifested in the material evidence in other ways too, apart from the inaccuracy of hour lines and absence of date lines. We find dials of irregular hollow shapes: some are spherical, some are cylindrical or conical, and others are mixed in shape. Often the dominant shape cannot even be determined. Yet this should not be interpreted as a failed attempt to produce a regular shape, one of the 'canonical' shapes in traditional gnomonics. The masons apparently just did not care about those aspects any more. Instead, the shapes are due to the fact that the masons strove to give their dials a more or less traditional look. In the end, we observe a reversion to the original simple, flat sundial with functional hour lines only. '*Es ist die alte Idee aus dem Orient, die hier wiederkehrt und—weil sie so einfach umzusetzen war—bis ins Mittelalter Bestand haben sollte*' [36].

The above developmental line that traces a shift in emphasis and function leads to a new criterion for dating the extant sundials. Granted that later dials were used for hours only, a 'sloppier' dial is, in general, going to be later. Schaldach's rough dating proposal in terms of periods—Classical, Hellenistic, Roman, and Late antiquity—seems sound.

Schaldach's developmental story is convincing on the whole. Specifically, his insistence on taking the purpose, the function, and the intended user into account when evaluating ancient dials seems convincing, though I should have liked to hear more about ancient astronomical theory and gnomonics for the Hellenistic sundials, since the scientific context was relevant for them, as Schaldach himself maintains. The chapter on the Tower of the Winds only partially fills this gap. Is it possible that we could push a little deeper with our

understanding of those ‘‘scientific’ sundials by taking written documents about gnomonics into account, even if they are fragmentary and indirect?

1.3 Byzantium

The survey of sundials from the Byzantine era gives an interesting, rather general outline. It seems quite convincing to state that the role of time and time measurement in Byzantine culture led to a lack of interest in dials as instruments for telling the actual secular time. The dials, where they are found, became quasi-icons for true, absolute Time, and were subjected to a rather determinate ‘iconography’. They are found in the context of churches in monasteries, not in the cultural centers. Because of their distinct, canonical elements in design, Byzantine dials can be easily recognized as tokens of a single relatively stable type attested from AD 800 to the 17th century, that is, a flat dial of palmetto shape, directed south, with semicircles as well as partitions with 10, 11, and 13 lines (with preference for partitions with 11 lines) and idiosyncratic number signs. The dials are neither useful nor intended for telling the actual time. The ‘iconographic’ criteria serve to identify a Byzantine dial and differentiate it from ancient dials.

2.1 Tower of the Winds

As an extraordinary and superb monument of Hellenistic gnomonics, the Tower of the Winds receives a separate, more extensive, treatment within the catalog part of Schaldach’s book. Schaldach discusses not only the nine sundials that are integrated into the Tower, but also questions of its dating, purpose, historical significance, and ‘reception’. This chapter can be read on its own, and it provides a very compelling and attractive interpretation of the monument. The interpretation, though not the first one of its kind, is very well worth reading.[1] The exposition is accompanied by numerous suitable illustrations and diagrams. Of the nine sundials on the Tower, several are unique in their kind. In effect, the whole building is a kind of

[1] One might want to consult in addition the literature given in Schaldach’s references, especially the works by Gibbs, Hüttig, and Kienast.

cosmic clock, symbolizing the power of the Sun and of the mathematical theory that here captures the very regularities and working of nature. The sundials, indeed the whole Tower, were certainly constructed with recourse to gnomonics. The dials have a very high degree of precision. The complete design is also esthetically convincing and pleasing as a whole. Schaldach proposes a date of 100 to 90 BC for its construction. In his opinion, Andronicus himself commissioned the Tower. In this assessment, Schaldach agrees with some other scholars. At any rate, Andronicus certainly was the architect of the Tower as well as the designer for the dials. After discussing the 'reception' and interpretations given by observers of the Tower over time, Schaldach concedes that it is possible, though at present not verifiable, that the Tower once housed a mechanism for projecting the zodiacal signs in circulation over the year (in the interior, using the waterclock, of which remnants are still visible). He sees the main purpose, however, in the fact that the Tower, as a whole, is a kind of cosmic clock.

> *In der Symmetrie des Baus zeigt sich die Ordnung der Natur, in der Dominanz der N-S Richtung die massgebende Kraft der Sonne. Die Betrachtung des wandernden Schattens fordert dazu auf, die Sonne als den Motor der Natur und als ihren Regulator zu erkennen. Sie regiert sowohl die Winde als auch die Zeit. Sie ist das Zentralgestirn, das die regelmäßige Wiederkehr von Tag und Nacht, Sommer und Winter, Wachstum und Zerfall verantwortet und dem Menschen seine Bestimmung in den fortwährenden Kreisläufen der Natur zu-weist. ... Mathematik wird so zu einem allseits sichtbaren Werkzeug, das die Vorgänge der Natur nachzubilden und sogar vorweg zu nehmen vermag. ... Die Sonnenuhren erhalten so eine metaphysische Bedeutung als Widerspiegelung einer naturwissenschaftlichen Weltordnung.* [68]

Given that the dials on the Tower are clearly designed in accordance with gnomonic calculation, the question arises as to whether they were actually constructed beforehand or later introduced by way of measurements and adjustments on the finished building. In answering this, Schaldach proposes that in addition to the mathematical tools available one should also consider the planning and proportions of the stone heights in the walls as well as the position

of the edges of the stones. His account of how the date and hour lines came to be in their present positions is altogether quite compelling.

2.2 Catalog of the remainder of the finds

The catalog, together with 2.1, is the centerpiece of Schaldach's book. It is very nicely done and extremely user friendly. The photographs provided for each individual sundial are of generous size and good quality. A detailed description and characterization of each piece is given. In addition, one finds an evaluation, a rough dating, and references to other literature where the dials are published or discussed. Because of its generous, visually satisfying presentation of the material and its thoroughness in documentation, the catalog is very useful as a reference tool with visual material. The accompanying CD-ROM with its visual material enhances the value of the catalog. The catalog will, in my view, prove very helpful for readers who find the book's language (German) to be an obstacle.

3.1. Methodological considerations for a mathematical analysis of ancient sundials

Schaldach names three major problems or obstacles for an unrestricted and exclusive use of mathematical apparatus in the interpretation of our evidence on ancient sundials. In principle there is, of course, always a problem involved in the relation of mathematics to reality when one evaluates objects that implement a mathematical theory for practical use. With regard to the ancient sundials, one has the additional problem that the theory in question is no longer accessible to us in detail, and that the extant dials themselves are often in fragmentary state and come from vastly different time periods, thus raising such questions as Which theory and what amount of theory, anyway? On the very basic level, the state of the evidence causes problems for actual measurements: there is a high degree of inaccuracy, not all of which is always due to the poor quality of the object under investigation. As an illustration of this fact, one might consider that different measurements in Gibbs' account of one and the same ancient sundial yield different categorizations for that same sundial. Finally, when theoretical concepts are imported into data, one has to be aware that they may not fully capture, and may not even concur with, the ideas that went into the production, so that

the explanation in terms of the theory simply misses the point. In light of such observations, Schaldach proposes a new methodological approach, one that allows for more flexibility and is more cautious, even quasi-minimalistic, in its theoretical apparatus, while resting on a larger amount of observed details and more detailed measurements as well. Schaldach draws part of his justification for his new catalog from improved and more numerous measurement data, and a more flexible and accessible theoretical apparatus for evaluation.

3.2 Documentation and analysis of the measurements

This chapter provides detailed documentation of the measurements, with introductory explanations, generous diagrams, description of mathematically relevant idiosyncrasies, and comparison with other existing measurements and results. It must be read in connection with 3.1 (and will be a more profitable reading, if taken in connection with 2).

Schaldach gives a coherent and comprehensive account of the evidence concerning ancient sundials in Greece and on the Peloponnesus following the plan of Gibbs 1976. The chapters of the book can be read individually or selectively. Some weak points, in my view, include the following. The author appears to be a bit ungenerous towards Gibbs, on whose work he constantly builds. Also, the chapters on Babylonian and Egyptian sundials are not as successful as the others. A somewhat more detailed discussion of gnomonics in the Hellenistic era would have been desirable. These minor drawbacks do not, however, diminish the value of this book.

There are many very interesting general points made in the book, which include the following:

- The cultural relativity of time and time measurements becomes visible and accessible in Schaldach's account of ancient sundialing. Changes of interests and perspectives with regard to measurement of time can be detected, and play out, of course, in the design and placement of dials. Shifts in the view on science and scientific explanation are thus reflected or materialized in the sundials.
- Schaldach's methodological remarks have more general implications for the history of science and technology. Though formulated for the evaluation of ancient sundials and in opposition to taking

sundials merely as tokens for an astronomical theory, they argue for a more extensive consideration by historians of material evidence and its actual use in practice. His new evaluation of ancient sundials may serve as an exemplary point of departure in discussions that aim to include so far underrepresented perspectives in the history of technology. The science of gnomonics, though relevant in attempts to take account of ancient sundials, was not necessarily the governing *leitmotif* for all ancient sundials in themselves, especially and increasingly so in the period after the Hellenistic era. Changes in purpose, cultural function, and practical use were reflected in the dials, and should find their way into any account of them as technological instruments. It seems to me that Schaldach makes a good case for de-emphasizing the role of astronomical theory in a comprehensive understanding of the sundials within ancient culture(s). Maybe something like this should be the case for other items in the history of technology as well, since any technological instrument is, as such, not merely an application of a scientific theory.

To sum up: this is a very accessible and rich source book on ancient sundials, as they are found in Greece and on the Peloponnesus. It is carefully crafted, beautifully illustrated, very informative, and stimulating. It will reward both the general reader and the specialist.

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Karnak: Evolution of a Temple by Elizabeth Blyth

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Karnak is probably the most famous of the ancient Egyptian religious complexes. Visited every year by thousands of tourists, it has been for decades the focus of careful and painstaking archaeological work that continues to reveal new details on the long evolution of this important sacred site. Because of the vastness of the remains, of the necessarily dispersed and detailed nature of the archaeological records, and of many ancient pharaohs who unscrupulously demolished to the foundations earlier buildings that were in the way of their new plans, following the historical evolution of this temple is not an easy task. At least, not until Elisabeth Blyth wrote this extremely useful book.

In a single, compact volume, the author summarizes in a comprehensive yet detailed way the history of Karnak from its uncertain origins through its period of splendor until its decline towards the end of the Roman era. The structure of the book is strictly chronological (1. the early temple, 2. the New Kingdom, 3. the Late Period) and the result is particularly effective: obviously, the New Kingdom (the period to which most of the standing remains date) occupies a large portion of the book, but the earlier and later periods (the evidence of which is, in many cases, scantier and less visible) have been given equal attention by the author. As a result, the reader can truly follow the evolution of Karnak through over 30 centuries of history. Although mainstream Egyptology is an obvious target of this book, as we shall see below other contiguous areas of research may certainly benefit from this historical and architectural reconstruction.

The importance of Karnak resided in its being the contact point between the god Amun, the supreme ruler of the universe, and the

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pharaoh, the supreme ruler on Earth who represented all the Egyptian people [Introduction]. Thus, especially from the New Kingdom onwards, every king who wished to be remembered forever was virtually compelled to contribute to the splendor of this most important temple [33 ff.]. In this respect, Karnak is a faithful mirror of the historical events that happened from the Middle Kingdom onwards: the complex political and religious changes that took place during and immediately after the so-called Amarna period, for instance, are clearly reflected by the equally tormented building phases of the temple.

It may be worth analyzing this example in detail by comparing historical events and building activities. Amenhotep IV, son of Amenhotep III, a few years after his accession abandoned the traditional polytheism (that gave power and wealth to a large number of priests) and opted for a cult centered on the Aten, the Sun-disk (that, very conveniently, communicated only with the king). He changed his name into Akhenaten and founded a new capital in Middle Egypt (at Amarna) called Akhetaten, 'The Horizon of the Sun-Disk', where he built new temples dedicated to Aten, characterized by a deeply innovative design. After his death, his designated successor reigned only a couple of years and was soon followed by the young Tut-ankhaten, closely watched by the older Ay and Horemheb. The young king soon abandoned Akhetaten, moved back to Thebes, changed his name into Tut-ankh-amun, and fully restored the old religious cult, no doubt heavily influenced by his older mentors. A radical *damnatio memoriae* fell over Akhenaten, his religion and his achievements. After the premature death of Tutankhamun, the power was taken for a short time by Ay, who must have been already old, and then by Horemheb. The latter reigned for about 30 years, but some inscriptions suggest that at some point he started to count the years of his reign from the death of Amenhotep III, thus attributing to himself all the regnal years of Amenhotep IV-Akhenaten, Tutankhamun, and Ay: in this way, the 'problematic' period (including the lack of linearity in the succession to the throne, since Horemheb was not of royal blood) was completely swallowed by the newly-restored, old-style traditional system.

The temple of Karnak fully reflects all these events. In the earliest years of his reign, before becoming Akhenaten, Amenhotep IV duly completed two monuments that his father had started within the sacred area and added some of his own. His taste for unconventional

forms can be already detected from the extremely scant remains of these buildings, which shared the same *damnatio memoriae* that befell their founder. They were so thoroughly destroyed—their stones were re-used as filling of later monuments—that we only have a faint and incomplete idea of their original position, outline, and size [118–126]. After the Amarna interlude, the young Tutankhamun set up a large stele at Karnak proclaiming the restoration of the ancient cult, and spent energy and wealth to restore the sacred complex [126–32]. Most of his achievements, however, were later usurped by Horemheb, who also flattened Akhenaten's buildings and erected three monumental pylons in full traditional style [133–142]. It is clear that, at Karnak, history and architecture proceeded in parallel.

Interestingly enough, it is suspected that Horemheb conceived the entire plan of the large Hypostyle Hall (perhaps the most famous part of the temple) that fills the space between Second and Third Pylon. Even if Horemheb was not directly involved, the beginning of this major construction must have taken place within a few years anyway: Horemheb was followed by Ramses I, who reigned less than two years and left the throne to his son Seti I. Most of the Hypostyle Hall was certainly built under the latter king, who managed to complete the decoration of half of it; the rest was the work of his son and successor Ramses II [36, 143–157].

This means that Karnak must have rarely been a quiet place: all these events, from Akhenaten's accession to the throne to the completion of the decoration of the Hypostyle Hall, took place in less than a century. Considering that erecting a stone building implied amassing large quantities of mudbricks and rubble to build construction ramps (and, of course, later dismantling them); that huge stone blocks had to be quarried, transported and lifted to their final position; that wooden scaffoldings had to be erected to decorate walls and columns; that disgraced buildings were not savagely pulled down but carefully dismantled in order to re-use their blocks; and that all these operations took place several times in the space of a century, one may conclude that, paradoxically, Karnak must have been qui-

eter in periods when kings did not have enough power and wealth to embark on the construction of their own additions to the sacred site.[1]

During the most splendid periods, instead, clouds of dust, mountains of mud, forests of scaffoldings, cries of encouragement and the rhythmic noise of the stonecutters must have provided scenery and soundtrack to this most important Egyptian temple. Karnak was not simply a monument, it was a living symbol of the royal connection with the most important god and, as a consequence, of the royal power as a whole. As such, in practice it was also the busiest and most important working site of ancient Egypt, and may still offer new insights into the ancient practical and administrative organization. A study of how the successive buildings sites were organized within the older monuments, for instance, would find in this book a useful and reliable starting point. A complement to the archaeological excavations that must necessarily focus on the careful and extremely detailed study of small areas, this overview of the entire temple, beside being interesting and important on its own, may also provide the basis for future studies on its general layout, distribution, and internal organization.

[1] Compare, for instance, the difference between the five centuries corresponding to the New Kingdom and those corresponding to the Third Intermediate Period, chapters 4 to 13 *vs* chapters 14 and 15.

Hellenic Philosophy: Origin and Character by Christos C. Evangeliou

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The book contains six chapters that were published separately in different periodicals. They all aim at clarifying the character of ancient Greek philosophy, its Mediterranean—most prominently its Egyptian—sources, and the influence which it has exerted on European thought.

The first essay is about the Egyptian origins of Greek philosophy. We read of a contrast between the opinion of ancient writers and the views held by modern Europeans, exemplified by W. Jaeger and W. K. C. Guthrie. According to Evangeliou, the ancients were free of the prejudices that govern conventional modern historiography. As he sees it, they were ready to acknowledge their debt to the great civilizations of the Euphrates and the Nile. The reports about statesmen and scholars visiting Egypt prove their interest in that civilization. Facing a common foe, the Persian empire, the Greeks and their Egyptian allies developed close cultural bonds and relations [14]. Evangeliou stresses that certain philosophical doctrines (Pythagorean 'number theory', the Socratic 'care of the soul' and Plato's ideal state) are rooted there [9]. Furthermore, he claims, the references in Plato's *Theaetetus*, *Phaedrus*, and *Laws* show an awareness of this debt. Among others, Evangeliou cites also Isocrates [*Bus.* 13–20] for the Egyptian origin of the principle of specialization that we find in the *Republic*. By contrast, modern European scholarship, in Evangeliou's view, insists on the indigenous nature of Greek philosophy. Jaeger is quoted to show that his statements concerning this issue reveal 'a Teutonic attitude towards other nations and races' [23]. Similarly, Guthrie also is also taken to deny any formative influence by Egyptian science and philosophical doctrines on Greek philosophy. In sum, this 'Northern European approach is shown to have

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been unfair to the Egyptians and insulting to the Hellenes' [27]. In conclusion, Evangeliou draws a sharp dividing line between the tolerant, pluralistic *milieu* of Hellas and the monotheistic intolerance and theocratic despotism in Northern and Western Europe [29], and calls for shaping 'a new millennium in the renewed spirit of philosophic diversity, tolerance and democratic freedom for the common good of humanity and its fragile sanity' [35]. This is the argument of the whole book as well.

There are some points to be disputed in this account. Evangeliou takes Plato to say that philosophy originated in Egypt [16–17]. The passages on which the claim is based are *Theaet.* 155c–156a and *Phaedr.* 274c–275a. The first connects the origin of philosophy to Thaumas the father of Iris; the second talks about the Egyptian king Thamus to whom the god Theuth revealed the arts of geometry, astronomy, and writing. Evangeliou equates Thamus with Thaumas, and thus concludes that on Plato's view philosophy comes from Egypt [17]. But Plato never equates Thamus and Thaumas explicitly. Moreover, the equation seems to be ill-founded since the two words have different roots and different etymologies.[1] Evangeliou also states that

- Plato had an intimate knowledge about the educational system in Egypt since
- he spent three years there, in Heliopolis [26 and n96].

The first claim may be true, but it has yet to be proven by drawing on what we know of the Egyptian educational system from other, preferably non-Greek sources. There is no such proof offered in this chapter. The only hint that the case may not have been exactly as described by Plato is given in a reference to Caminos 1954 [n95]. If Plato's picture is an idealized one, however, then the question is what did he take over from Egypt. The second claim is based on Strabo's report in *Geog.* 8.17.1.29. The problem with this report is not just that Strabo is not considered a very reliable author in general, but also that the *Seventh Letter* does not mention such a sojourn at all, although this is the text where Plato (if he is the author) talks about his travels. Why does he keep quiet about this important period of his life? To put it otherwise, if the author is a member

[1] See Frisk 1970, *ad* θαῦμα, related to words signifying seeing: cf. Chantraine 1968.

of the Academy, why did Plato's followers fail to mention that the scholarch had spent such a considerable amount of time in Egypt? At a more general level, it seems to me that what Jaeger emphasized in his *Paideia* is the unique nature of the classical Greek culture. This is not to say that the Greeks did not rely on other cultures—for instance, the Anatolian origin of many elements in their mythology has been well researched by Walter Burkert [e.g., 1984]. Even if Jaeger's definition of culture was formulated in a way to fit the Greek *milieu*, which is to concede that it was overly narrow, we should allow that his aim was to emphasize those complex features in Greek culture which were without precedents. For Jaeger, the political and cultural environment in fifth century Athens (the paradigmatic case in his work) was without precedents in other Mediterranean cultures. Of course, this does not contradict the statement—which he admits—that certain elements were around in other places as well. But, for Jaeger, these elements did not constitute the essence of what we see in Athens in that period. Still, there are other approaches to clarify the unique nature of ancient Greek culture that might have been taken into account.[2]

The picture of a pluralistic, tolerant and civilized Egyptian society [23] may also be somewhat idealistic. At certain periods at least, it does not seem to have been the case. Herodotus [*Hist.* 2.91: cf. 2.35, 49, 79] mentions that the Egyptians avoided adopting other people's customs (Greek customs included), which fits the picture the Bible gives of them [*Gen.* 43.32]. Their negative attitude towards foreigners manifests itself in their pantheon as well, with Seth becoming the god of what is foreign [see Brunner 1983]. As regards Egyptian perceptions of the Greeks, cases of Hellenophobia can also be cited.[3] In Hellenistic times, the strained relations between the Hellenized cities in Egypt and the χώρα are also well known. Finally, I think we have to make a distinction between how Egypt (or certain elements therein) was perceived by Greek authors and how it actually was. Evangeliou also mentions that these two things did not always match. In the light of this discrepancy, then, we have to ask which features were taken over, and which ones were projected. Furthermore, to mention just two samples, the charges of ἀσεβεία at

[2] Assmann [1992, ch. 7], e.g., stresses the role of the Greek alphabet.
[3] For references, Assmann 2005, 37–49.

Athens in the classical period and the somewhat gloomy picture we find in the *Laws* may also modify what we say about religious and intellectual tolerance in that period.

The second essay deals with the fate of Plato's teachings in Europe. Again, we find a marked distinction between the true Plato and the way in which his teachings were distorted in the Frankish West. Thus, Evangeliou thinks that we have to modify Whitehead's dictum that European philosophy consists of footnotes to Plato. He argues that European 'philosophy' acquired certain bad habits, and that first of them all was a docile servitude to alien authorities to dogmatic theology and theocracy, which appears to have been transferred to modern technology and Marxist political ideology. For this reason, philosophy has become something very different from what it was in Hellenic times [59]. After a brief survey of the intellectual conditions in late Antiquity, with an admittedly 'synoptic, speculative and oversimplified' [87n20] account of Rome's fall, two case studies are offered—one about the relation between Porphyry and Augustine; and the other, considerably shorter, about Gemistus Pletho. The first focuses on *The City of God* and aims to show that Augustine's critique of Porphyry marked a gap between the believer in a Christian god and the philosopher who can only be persuaded by rational arguments. Augustine thought that he had found *the* way to salvation and aimed to show that a combination of selective doctrines from Plato and Porphyry would yield essentially the same truth. Porphyry, however, looked at the matter from a different angle. His contact with Plotinus and the study of Platonism helped him, as Evangeliou claims, 'to rise above the common superstitions of his time in search of the philosophical way which does not exclude other ways for other schools, but tolerates them by giving each "its due"' [73]. Next Evangeliou surveys the history of European philosophy from the Renaissance up to Whitehead, and shows that the doctrine in *Process and Reality* fails to include the two versions of Platonism, the Christian and the Hellenic. Because of Caesar's (and the Pope's) domination of Christianity and of Christianity's domination of the European mind and ethos, the so-called 'European philosophy' cannot be characterized simply as 'a series of footnotes to Plato' without serious equivocation [83].

Just a few points to raise. Evangeliou talks about a split within the revived Platonism in late antiquity and maintains that there existed one group the members of which were in favour of cultural

diversity and tolerance towards new ideas, trends, cults, and another group which accepted the new Christian faith and demanded radical change in every aspect of civil tradition [62]. As Porphyry's criticism shows, Christianity was not by any means something to be tolerated—and this was well before Christianity became a received (not to mention the ruling) religion of the empire. Porphyry was not alone in his criticism. Among other Platonists, Celsus accused Christianity of καινοτομία. For him, it was a new-fangled divergence from the Jewish tradition which was in turn a divergence from the Egyptian wisdom [*ap.* Origen, *Contra Cels.* 3.5]. The theme turns up in other Platonists as well.[4] This fits with the Platonist trend of rejecting what they thought to be derivative cultural traditions. Moreover, despite the tolerance stressed by Evangeliou, Neoplatonic texts contain much sharp critique of the views of other philosophical schools. Plotinus offers good evidence for that, to mention but one example. On the other hand, monotheistic tendencies are evident in the pagan culture of the late Roman Empire as well.[5] One should also note that Augustine's relation to Porphyry and his notion of *fides* were more complex than the picture Evangeliou presents. His debt to Porphyry is documented in his early works. More importantly, his notion of *fides* and use of the verb *credere* show that the contrast between his acceptance of the revealed grace of God and the rational arguments employed by Porphyry [72–73] is overstated by Evangeliou. Augustine adopted Stoic and Sceptical theories both in his early works [e.g., *Contra acad.*] and in his later works [*De praed. sanct.* 2.5, *Ench. ad Laur.* 5–7] to show among other things that faith is a rational act. In *Divers. quaest.* 54, the contrast is between the content of *credere*, which can be either false or true, and the content of *intellegere*, which can only be true and forms a part of the true content of *credere*. In other works, Augustine connected belief, which does not involve full knowledge, to the concept of the *reasonable* [*Conf.* 6: cf. Menn 1998, 185–194]. All this amounts to saying that Augustine's theory of knowledge was more complex than the simple

[4] See Theophilus, *Ad Autolycum* 3.4; Porphyry, *Contra christ.* fr. 69.7–8, 25–27 (Harnack). For pagan views of early Christianity, see the collection of *testimonia* in den Boer 1965.

[5] For ample references and insightful studies, see Athanassiadi and Frede 1999.

contrast of *credere* and *intellegere* would suggest.[6] Finally, one may also say that the relation in *Phaedrus* 279b between Pan and the gods is not that between 'God and other gods' [83 and n94]: the dialogue does not ascribe to Pan such an eminent role—in the myth, it is Zeus, a pure intellect, who leads.

The third essay discusses the role of Aristotle in Western thought. In order to show that Aristotle's philosophy is not responsible for what he considers as the two European vices, *ratio* (the rule of the calculating human reason) and *imperium* (imperialistic power of the colonialist type), Evangeliou examines the concept of λόγος (discursive reason) and νοῦς (intuitive mind). Thus, Aristotle turns out for him to be someone who is more than a mere representative of European and 'Western rationality' [99]. On Evangeliou's view, Aristotle claims that intuitive mind is prior to discursive reason also in the sense that discursive reason must be surpassed in order that we attain our final goal. For Evangeliou, 'discursive reason must yield to intuitive and superior power of energized human intellect' [100]. The intellect suddenly grasps, as in a flash of self-awareness, the truth that the human being is essentially the same as the divine intellect.[7] Nonetheless, Aristotle was presented in the West equally narrowly,

> either as the scholastic logician and rationalist thinker in the service of dogmatic medieval theology, or as the empirical and analytic thinker in the service of technocratic modern science. [100]

Next, Evangeliou examines the meaning of the terms 'reason' and 'rationality' in Aristotle. I am not sure that he says it at the end, but he claims rightly that Aristotle cannot be classified a rationalist or an empiricist, which is not surprising, to my mind, since these categories are inventions of modern historiography to describe the philosophical currents of the early modern age. By drawing on passages in the *Metaphysics* [981b25–982a6, 1069a18–34, 1072b14–29], *De anima* [402a1–8 with short sections from books 2 and 3] and *Nicomachean Ethics* [1094a1–4,[8] 1177a13–18], Evangeliou states that Aristotle recognized

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[6] On the historical background of his theory, see more recently Fuhrer 1999, 191–213.

[7] The reference is to *Eth. Nic.* 10.1177b–1178a.

[8] Which is not about 'bringing together two divinities'.

the kind of life of which man is optimally capable, as well as the communal and political arrangements which would make possible the flourishing of such a life for the best qualified citizens. They are not arbitrary recommendations of some divinely inspired and dogmatic prophet, rather they form the fulfillment of an *entelechy* which is present in the human soul and human nature *qua* human. The distinction between *ontology* and *ousiology* aims at showing that Aristotle moved dialectically from the former to the latter. It means that the theory of being *qua* being was transformed into the theory of substance, the most primary substance being God [109]. The noetic powers of human soul are the best of psychic powers, and shared with other divine intellects. Nevertheless, surprisingly enough, Evangeliou concludes that the 'end of man' is the well-ordered πόλις [110]. At the end of the chapter, there is a list of five possible post-modern objections to Aristotle's political theory. They concern natural slavery, the lack of women in legislation, the division between Hellenes and Barbarians, the very limited number of those who can be virtuous, the identification of human goal with virtuous activity of he citizens, and the connection of the supreme good for humans with the noetic activity of the Divine.

One problem for Evangeliou's account is that the emphasis on intuitive intellection is not alien to medieval thinking at all. The idea of *visio dei* (or *Gottesschau* in Meister Eckhart), a direct, unmediated vision of God, runs through the whole epoch. It may suffice to mention the names of Eriugena, Bonaventure (in the *Itinerarium mentis in Deum*, for instance) and Cusanus [e.g., *De docta ignorantia* 1.26]. If this is the case, however, it is going to be difficult to maintain the view that the role of Aristotle's νοῦς was suppressed or downplayed by medieval theologians.[9] In the same vein, it is also going be difficult to say that medieval thinking is responsible for what Evangeliou calls one of the two vices of European thinking, the emphasis on *ratio*. Moreover, it is a well attested tradition in medieval philosophy that the human soul carries a trace or spark of the divine. This *scintilla animae/rationis* connects our souls to God and inclines us always towards the good [e.g., Bonaventure, *In II Sent.* 2.7b (Quaracchi)]. The

[9] Evangeliou once [84n2] refers the reader to a volume edited by W. Beierwaltes. This same scholar wrote extensively on, say, the *visio beatifica* and related issues in medieval Platonism.

use of this motif starts in late antiquity and flourishes in the 14th century [see Tardieu 1975]. In sum, the relation of Christian Platonism to its pagan counterpart raises important questions that cannot be treated in such a cursory manner.[10] Furthermore, one needs further clarification about the goal of man. If, according to *Eth. Nic.* 1177b–1178a, the best way of life is characterized by the activity of νοῦς, and this theoretical activity has determinate objects, then what allows us to say, with Evangeliou [100], that it is the well-ordered πόλις where the 'end of man' is located? What the well-ordered πόλις can provide at best is just a necessary means to this end.

The fourth, short, essay deals with Aristotle's critique [*Pol.* 2.1–5] of Plato's political theory in the *Republic*. For Aristotle, Plato's ideal state is based on the community of women and children, and on the community of property for the guardians. Evangeliou stresses that Aristotle's criticism is based on a commonsense view of human nature, and disregards the substantial role of Platonic education in the formation of a guardian or philosopher in the ideal state. The guardians were a new type of men, transformed by proper education. Aristotle assumes, however, that human nature is very much constant, that humans would behave and feel in very much the same way in Plato's ideal state as they do elsewhere. Evangeliou suggests that as a way out of this difficulty,

> Plato would have to argue that his proposal of total communism was not an innovation. For it had been in practice in the very distant past not only among primitive African peoples, but also among the Athenians and even the Atlantans. [146]

This is an interesting suggestion and should have been supported by more textual evidence. But note that the *Critias* does not speak about total communism, that is a community of property in the whole society. It says at 110c–d only that in Atlantis soldiers—and not everybody—had everything in common. So far as ancient Athens is concerned, the *Critias* just reports that soldiers made no use of silver and gold [112b–d]; it does not speak about the community of everything. And no mention is made of primitive African people.

The fifth chapter is about Pletho's criticism of Aristotle's innovations. His criticism of Aristotle and Averroes greatly contributed

[10] See most recently Steel, Vella, and Iozzia 2006.

to the revival of Platonism [153]. Pletho's objections to Aristotle are rooted in his conviction that Western Scholasticism depended on valorizing Aristotle's doctrines at the expense of Plato. Pletho's critique accomplished three important tasks:

- it revived the debate about the respective merit of Platonism and Aristotelianism,
- it injected the Renaissance movement with a strong dose of Platonism (though Ficino's version was to prevail), and
- it initiated the process of liberating Aristotle from the embrace of Christian and Islamic scholasticism.

Evangeliou concentrates on the critique of Aristotle's concept of the homonymy of being, the failure to apply the notion of immortal intellect in ethics, and his theory of art and cause. Pletho's critique of the homonymy of being seems to rely on Platonic principles. He supposes that if the multiplicity of beings derives from a single source, they have to have something in common, which is being. But if being is homonymously predicated of them, it cannot stand for their essential commonality [158]. The main problem with Aristotle's ethics is that it considers ethical virtue as a mean between two extremes, and identifies the supreme good with pleasure. Pletho also criticizes Aristotle's objections to the theory of ideas. As for his critique of the Aristotelian 'third man' argument, it would have been interesting to read about Pletho's view of the argument in the *Parmenides*. As Evangeliou concludes, Pletho's arguments made clear that Aristotle's theory is in fact incompatible with what the medieval theologians said about him.

To start with this last point, the preceding remarks about Pletho seem to show rather that Aristotle's critique of Plato failed, not that his doctrines were different from the Averroist and Thomist pictures of Aristotle. Evangeliou mentions that on Pletho's view Aristotle is silent about the creative function of God. It would follow, then, that Aristotle's views are incompatible with the Biblical or Koranic creation stories [155]. This raises the question of whether Pletho has any knowledge of the late antique commentators in the school of Ammonius in Alexandria who interpreted the Aristotelian God as having a creative role in the universe.[11] Ammonius wrote a whole

[11] The claim was based on Aristotle's *Physics*.

book on the efficient causality of the Aristotelian God [*ap.* Simplicius, *In phys.* 1363.4–12]. It is ironic (at least if we think that Pletho's aim was to liberate Aristotle from the embrace of Western theologians) that Thomas Aquinas had reservations on this interpretation [*In phys. VIII lectio* 2, 986–987; *Summa theol.* I q.46, a.1 resp.]. One might also note that Pletho was not quite correct in attributing to Averroes the view that the soul is mortal [154]. It was not his view at all. The problem with Averroes' psychology was that on his view the individual human soul became part of the universal intellect, thus losing its individual characteristics, which makes reward and punishment in the afterlife difficult, if not impossible. Pletho's views on Aristotle's notion of virtue of character as a mean seem also a bit strained. First of all, we have three components (one good and two bad), not two. Evangeliou is right to note that the Aristotelian concept of virtuous character is not free of difficulties (though they are perhaps of a kind different from the one Pletho was noting), but it is a pity that the only literature he is able to refer the reader to is his M.A. thesis of 1976 [168n38]. Even if it was an exhaustive treatment of the subject, quite a lot has been published on this topic since then. One may also note that the revival of Platonism in Italy had many sources [see also 74], from Petrarch to Bruni's translations of some of Plato's works in the early 15th century. One should also study the extent to which Pletho was cited by those Florentine writers (e.g., Ficino) in doctrinal matters and how they received his criticism of Aristotle.

The last essay reiterates the main points about the character of Hellenic philosophy. It has a clear political agenda, which is not my business to discuss. To put Evangeliou's main thesis simply, true Hellenic philosophy has nothing to do with Western 'philosophy'; and because Westerners were alone responsible for colonization and all the horror that has happened since then, Hellenic wisdom is immaculate and is waiting to be appreciated by other nations accordingly.

In general, Evangeliou's agenda is very clear. But when it comes to carrying out its primary task, which is to prove the case on the basis of an analysis of the available textual evidence, the whole argument seems ill-founded. Even if one of the central claims concerns the nature of medieval philosophy in the West, no medieval texts were examined, except for a few passages in the *De civitate Dei*. There is only one scholastic writer who is mentioned, Thomas Aquinas, and this without textual analysis or regard for the fact that Thomas is

not an authority to be taken to characterize the whole current of medieval philosophy. There are also some interesting historical remarks made in the course of Evangeliou's argument. To take one example:

> Practicing monotheistic intolerance and theocratic despotism, particularly in Western and Northern Europe, the Popes managed to dominate European culture in the last two millennia. [29]

This is not true even of the pre-Reformation Papacy; and one really should make a clear distinction between a Christian culture with its many facets and a papal cultural dominance, the latter being subjected to various royal interests among other things. Evangeliou also seems to forget about the Age of Enlightenment.

The book is graced with a glossary, bibliography and a detailed subject index. There are some typos: e.g., in 38n23, we should read 'Burkert' for 'Burckert'; in 44n48, 'Patrology' for 'Partology' (repeated in the bibliography in the entry for B. Altaner). In 46n63, the reference is to the *Laws*, not to the *Timaeus*; in 46n64, the Greek accents are partly missing; in 131n61, they are wrong. In the bibliography, the editor of Porphyry's *Sententiae* is Lamberz, not Lambart or Lambert [168n42],[12] and the author of *The Meaning of Aristotle's Ontology* is not a certain Werner, M. (repeated in the index), but W. Marx [1954].

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[12] And no mention has been made of Smith's edition [1993] of Porphyry's fragments.

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Studies in Medieval Astronomy and Optics by José Luis Mancha

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Over the last 15 years, J. L. Mancha has published a series of studies, mostly connected in one way or another with the work of Levi ben Gerson, in which he brings to bear his considerable skills in Romance philology and mathematics. Eleven of these have been collected in the volume under review, to which Mancha has added a preface and indices. Mancha was led to study Levi by way of his dissertation in which he wrote on Henry of Hesse's *De reprobatione eccentricorum et epiciclorum*. Hesse's criticisms of eccentrics and epicycles resonate in the work of Regiomontanus and Copernicus. The search for Hesse's own sources led Mancha to Levi, who, as noted, has become the chief focus of his work. Only one of the essays reprinted here, 'Ibn al-Haytham's Homocentric Epicycles in Latin Astronomical Texts of the XIVth and XVth Centuries', (to which I shall return below) is related to Hesse. By contrast, nearly all of the others, and certainly the longest and most important, are related directly or indirectly to Levi.

Research into Levi, in turn, led Mancha to other projects. The first two items in the volume are studies of Latin texts on pinhole images. Levi employed the pinhole camera for his measurement of the solar eccentricity; a study of the Latin version of the relevant chapter from Levi's astronomy is appended to the second of the two essays on optics, 'Pinhole Images in William of Saint-Cloud's *Almanach*'. The last study in this volume, 'Al-Biṭrūjī's Theory of the Motion of the Fixed Stars', confirms the suggestion of E. S. Kennedy, against the view of B. R. Goldstein, that al-Biṭrūjī did employ a pair of homocentric spheres (dubbed by Mancha a 'Eudoxan couple') in his reformed astronomy. The clue to this new interpretation, as Mancha tells us in his third footnote, was the discovery that Levi had used homocentric

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spheres in dealing with the hippopede, a topic which Mancha investigates in full in the seventh item in the volume, 'Right Ascensions and Hippopedes: Homocentric Models in Levi ben Gerson's *Astronomy* I. First Anomaly'.

In the following I shall report upon some selected essays from this collection, in particular, those to which I wish to draw attention or to which I feel that I may have something to add. I shall, however, jump directly to my conclusion now, and state that this a welcome addition to the Variorum series and a most useful work for historians of medieval science.

Let me begin with a remark on the hippopede. Mancha observes that the chief source for the technical features of the model is Simplicius' commentary to Aristotle's *De caelo*. Although that text is not known to have been translated into Arabic, somehow the procedures became known, and even became 'standard'. In this connection, I should draw attention to the very interesting Hebrew text, *Meyashsher Aqov*, which is likely to have been written by Abner of Burgos (early 14th century), and which is a repository of some rich Hellenistic mathematics not known from other medieval sources as well as other materials whose transmission history is unclear. Abner was clearly drawing upon texts that may well have been at the disposal of al-Biṭrūjī and others.[1]

'Heuristic Reasoning: Approximation Procedures in Levi ben Gerson's *Astronomy*', is the most thorough study I have seen of iterative methods in pre-modern science. An iteration is a process for solving algebraic (or transcendental) equations in which one first makes an educated guess as to the solution, then 'plugs' this solution into an algorithm which will yield a better approximation. This second solution is then 'plugged' into the same algorithm, yielding an even better solution. After a number of successive iterations, the differences between the results become insignificant, and one can be satisfied with the result.

Iterative procedures were employed not only by Ptolemy (as Mancha notes) but elsewhere in Hellenistic mathematics and in medieval Hebrew mathematics as well.[2] However, as Mancha points out,

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[1] I report much of this in Langermann 1996.

[2] See Hogendijk 1994, Langermann 1994.

astronomers and mathematicians did not always explain exactly what they were doing, nor did they attempt to justify the method on mathematical or logical grounds. Levi, however, did both at the beginning of chapter 49 of the Latin version of his *Astronomy*. After translating and analyzing the relevant text, Mancha goes on to display six examples of its application in a variety of astronomical problems.

By far the longest study (nearly 100 pages) is 'The Provençal Version of Levi ben Gerson's Tables for Eclipses'. The text, whose discovery is an important achievement in itself on Mancha's part, sheds new and stronger light on Levi's relationships with Christian scholars. In particular, Mancha demonstrates that the Provençal text, which is earlier than the corresponding Hebrew version and contains materials not found there, is due to Levi himself, who may have dictated it to a Christian scholar, very likely Petrus of Alexandria, who is known to have been associated with Levi in various capacities. Having now the opportunity to study closely Levi's work in all of the three languages in which they circulated (Hebrew, Provençal, and Latin), Mancha is led to the important conclusion that one ought not to speak of 'originals' and 'translations'. Instead, each version, whatever the language, presents Levi's original research at whatever stage the text was issued. Moreover, the Provençal and Latin versions are not mere translations, but rather form part of a program undertaken in collaboration with Levi himself for making available the results of his astronomical research to the aforementioned Christian circle.

'Levi ben Gerson's Astronomical Work: Chronology and Christian Context', deals as well with Levi's relations with Christians. It is, however, a departure from Mancha's usual work in that neither mathematical astronomy nor Romance philology play a major role here. It is, nevertheless, one of the most interesting and revealing contributions to Levi's biography that I have seen. Making use in particular of the many dated observations in Levi's writings, Mancha points to peaks in astronomical research that correspond to requests from Christians, as well as to low points that came about when Levi was stymied. Levi's writings in other fields, especially biblical exegesis, fit in nicely to those years when astronomical activity is not attested.

'Ibn al-Haytham's Homocentric Epicycles in Latin Astronomical Texts of the XIVth and XVth Centuries' tries to draw a connection between a physical model proposed by Ibn al-Haytham to account

for the complex motions in latitude of the planets, and some models that are found in Latin texts. Ptolemaic astronomy analyzed motion in latitude as the product of an oscillatory motion on the part of one, or, in the case of the inferior planets, two diameters of the epicycle. Yet how is one to explain this physically in accordance with the principle that only one motion can be attributed any single body in the heavens? Ibn al-Haytham (965–*ca* 1040) devised a system of concentric spheres rotating on different poles to account for it. Mancha has found in works by Henry of Hesse, a certain magister Julman, and Albert of Brudzewo (author of a gloss to Peurbach's *Theorica*), 'physical arrangements of two concentric spheres enclosing the epicycle (or the eccentric) identical to that of Ibn al-Haytham. . . although without parameters'.

So far so good. The problem—and Mancha is certainly aware of it (see his first note)—is that the particular monograph in question, *Treatise on the Movement of Iltifāf*, is not extant even in Arabic; and there is no reason at all to suppose that it, or the reply of Ibn al-Haytham to a critic of his solution, which does survive, would have been available to readers in the West. Ibn al-Haytham's *On the Configuration of the World*, by contrast, was widely read and translated more than once into Latin. In that book, however, Ibn al-Haytham does not offer a solution to the problem at hand, though the oscillations are mentioned, and the Latin terms (*reflectio, inclinatio*) are the same as those that appear in the texts studied in this article. Mancha observes that the authors whom he studies did not themselves design the system, since they speak of it as the theory of others. However, the models are not complex, and involve little more than the assumption of spheres that will be responsible for the execution of the motions that are described in *On the Configuration* and other writings.

I have some criticisms concerning Mancha's translations and transcriptions. Mancha's command of English is excellent, and his expositions and translations read well. However, he has a tendency to translate some terms much too literally. For example, in the first study, 'Egidius of Baisiu's Theory of Pinhole Images', he consistently renders '*pyramis*' by 'pyramid', instead of 'cone', the correct and

precise translation.[3] Mancha chooses 'diaphaneity' for '*diaphanitas*', instead of the perfectly good 'transparency' [passage 13]. He uses 'experience' too often, inconsistently, and not always correctly. At the beginning of passage 19, '*Etiam est contra sensum*' is rendered 'It is also contradicts (!) experience'; and at the beginning of passage 20, '*Ad quod experimentum quidam falso nituntur respondere*' is given in English as 'This is an experience against which some try in vain to argue'. '*Sensum*' should be rendered 'senses', or, if one insists, 'sense experience'; and '*experimentum*' in this context really means 'observation', or, if that seems inappropriate, 'empirical datum'. (The same applies to Mancha's use of 'experience' on page 15 of the fifth item in the collection).

Passage 18 in the first study presents a more difficult challenge. The Latin has '*virtus curvaretus et fortificaretur*', which Mancha translates, 'their virtuality would curve and increase in strength'. 'Virtuality' is of course out of place here; '*virtus*' must mean here 'power, strength', and so one would accordingly translate '*fortificaretur*' by something like 'would intensify'. I admit, though, that the full intent of the passage is beyond me. Finally, a word on transcriptions from the Greek. In the eighth study, Mancha consistently writes '*egklisis*'. This may be a precise letter for letter transcription; but gamma before kappa is a palatal nasal stop, and so the correct and standard transcription is '*enclisis*'.

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Charles Darwin, Geologist by Sandra Herbert

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While the title of Sandra Herbert's scientific biography of Charles Darwin will generate no surprise among scholars of 19th century science, the audience for whom this volume is clearly intended, it will for those who still think of Darwin's primary achievement as 'biological'. It is worth noting, therefore, the opening lines of Charles Lyell's *Principles of Geology*, perhaps the single most important influence on Darwin during his formative, Beagle, years:

> Geology is the science which investigates the successive changes that have taken place in the organic and inorganic kingdoms of nature; it enquires into the causes of these changes. [Lyell, 1830–1833, 2.1]

That would help the surprised reader to see that the work for which Darwin is best known, *On the Origin of Species*—an investigation into the causes of the successive changes in organic nature—was a contribution to geology, at least as Lyell understood it. But those who contribute to, or follow, the 'Darwin Industry' learned that lesson long ago, thanks to the work of people like Jonathan Hodge, David Kohn, and Sandra Herbert in her earlier work.

The current book calls for a much greater reorientation and will do much to restore balance to Darwin studies. For what Herbert shows, with the loving attention to detail of a gifted scholar, is that Darwin would have stood shoulder to shoulder with Lyell as one of the giants of geology had his species project never seen the light of day. It is not until the last two chapters, after nearly 300 pages tracing Darwin's life as a geologist, that Herbert turns to the topic that is the center piece of virtually every other book on Charles Darwin, the 'Species Question'. Herbert thus quietly and gently makes the case

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that to see his geological work through the lens of his work on the species question inevitably distorts our image of both. Indeed, her conclusion is that Darwin's reputation as a geologist has been tainted by the success of a project that he and Lyell would have viewed as geological! History plays cruel tricks on us.

The title of her first chapter, which consists of the last three words of her concluding chapter, 'I a geologist', is lifted from an autobiographical note which Darwin penned in 1839 contrasting his form of imagination with that of others. The entire volume is fairly scattered with such quotations—a constant reminder that this study comes from the hand of one of the foremost editors of Darwin's heretofore unpublished notebooks, a gift to Darwin scholars that keeps on giving. One imagines that Herbert has much of this notebook material ordered in a mental filing system, ready to hand when it is needed. Even if you have studied the edited notebooks carefully and worked through Janet Browne's definitive two volume scientific biography of Darwin, this chapter will give you a feel for Darwin's day to day geological toils during the Beagle voyage that makes earlier studies pale in comparison. If recent work by Philip Sloan and others have urged upon us the importance of invertebrate zoology before, during, and after the Beagle years, Herbert reminds us that the geological mission was primary. This chapter and the next intertwine discussion of Darwin's fieldwork and theorizing in geology with a rich discussion of the context for his work.

The great strength of these chapters lies in the richness of the description of both theoretical geology and its empirical methods during the 1820s and '30s as Darwin would have experienced them. I found gratuitous references to Latour [e.g., 8, 31] off-putting, and thought that insufficient emphasis had been put on the *philosophical* impact on Darwin of Sir John Herschel's *Preliminary Discourse on the Study of Natural Philosophy* (a deficiency somewhat ameliorated in chapter 7) and volume 2 of Lyell's *Principles of Geology* (though again, Herbert has more to say on the topic in chapter 9). That being said, I left these first chapters with a better sense of the working life of Darwin the geologist—and of geology—in these years.

Chapter 3 recounts in detail the developing methods for specimen collection, measurement, labeling, describing, recording, and storing during the 19th century; and provides an extremely 'thick'

description of Darwin's mastering and applying those methods in his field work. While obviously relying on a painstaking study of Darwin's specimen notebooks and the remaining objects to be found in various museums, the narrative is not overwhelmed by the detail. There is a valuable lesson here for historians and philosophers of science. Darwin was a theoretician, and a brilliant one. But a notable feature of that brilliance lay in his powers of *integration*, of seeing the unifying thread in what might otherwise be an overwhelming mass of data. This chapter is a clarifying reminder of the painstaking work Darwin did collecting, ordering, describing, and studying the concrete objects about which he theorized. It becomes clear why he is inclined to call abstract generalizations 'large classes of fact'.

In his college years, Darwin was attracted to the writings of Alexander von Humboldt; and we are reminded of that in a too brief fourth chapter in which Herbert provides a lovely literary analysis of Darwin's published journal from the Beagle voyage. As she notes and illustrates, 'Darwin was artful in constructing narrative that emphasized the dramatic elements in landscape,' perhaps reflected in, or influenced by, his fondness for Wordsworth.

The next four chapters take us through the years during which Darwin was focused on writing up and publishing his increasingly systematic geological ideas. Once again, Herbert takes us into the details of how Darwin moved from the raw data of his field and specimen notes through a variety of stages, before beginning to prepare a manuscript for publication. Once again we are reminded of the amazing breadth and depth of Darwin's empirical base. In fact, I know of no other study of Darwin that provides such a rich picture of this side of his work. But Herbert also reminds us that he was willing to do what his teacher Sedgwick was not—interleave hypothetical explanations and theoretical interpretations in his field notes. Though the balance between theorizing and concrete description is reversed, the 'species notebooks' also reveal this aspect of Darwin's thinking, a desire to move back and forth between the concrete and abstract.

Chapter 6, 'Negotiating Genesis and Geology', is less groundbreaking, though certainly necessary for the narrative Herbert develops; and I take it the ground it covers will be obvious from the title, Darwin being one of the negotiators, of course. Chapters 7 and 8, however, are a fascinating study of the perils of theorizing, that drive

to find the grand unifying hypothesis which so easily leads an integrator like Darwin astray. Here Herbert notes a common theme in Darwin's two favorite college authors, Humboldt and Herschel, their stress on theoretical simplicity as an ideal. A careful analysis of his work on coral reefs and volcanic islands suggests that Darwin recognizes the dangers of overlooking problems in the search for that 'one great cause' which will explain a wide range of phenomena. The next chapter, 'Simplicity Challenged', gradually focuses in on the mistake which early on in his career taught Darwin that lesson in a most painful way, his faulty explanation for 'the parallel roads of Glen Roy', an explanation which in his family autobiography he called 'a great failure' that 'has been a good lesson to me never to trust in science to the principle of exclusion' (quoted by Herbert on page 285). She uses some tantalizing quotations from his later correspondence to suggest that this failure affected his attitude even to his beloved natural selection. In the second chapter, Herbert quoted a note from Darwin's soon-to-be bride Emma Wedgwood saying she hoped he would 'manage to finish Glen Roy now & get shut with it' [78]. This suggests a more prosaic reason for Darwin's rushing a poorly tested theory into print.

Only by comparison with the new insights that fill the early chapters is it true to say that the 'Species' chapters are less rewarding. Even here however, Herbert's stress on the importance of Darwin's strictly geological work to his developing theory of descent with modification by natural selection enriches our understanding of that development. Chapter 9, 'Geology and Species', closes, appropriately, by recounting the correspondence between Lyell and Herschel in early 1836 regarding 'that mystery of mysteries', the 'origination of fresh species' (Herschel's words). Four months after Herschel sent these words from Cape Town, South Africa, in praise of Lyell's willingness to put the problem on the geologist's table [in Lyell 1830–1833, vol. 2], H.M.S. Beagle arrived in Cape Town on its way back to England with a young geologist on board newly converted to Lyell's vision of the subject. We know that Herschel and Darwin met more than once during this stopover. Did Herschel show Darwin a copy of his letter? A notebook entry two years later suggests not, though entries in the *Red Notebook*, edited for publication by Herbert herself, suggests otherwise. Did they discuss Lyell's *Principles*? Almost certainly. Did they discuss the question of species origination opened up by Lyell?

Neither of them ever say. Herbert says pretty much everything the evidence allows. These encounters remain a tantalizing enigma.

The last chapter is a substantive study of the development and presentation of the argument in *On the Origin of Species*. Here again we are on more familiar territory, but here again Herbert asks us to look more carefully at the role played by geology in the process of developing and presenting the argument. This provides her an opportunity, as one example, to focus on Darwin's disagreement with certain of his allies over whether evidence from geology should trump that from biogeography and classification in evaluating theories. Some of his allies were willing to essentially invent land bridges for which there was no geological evidence, in order to account for certain relationships among species on different continents. Darwin ('I a geologist') consistently sides with the geological evidence (or lack thereof).

Charles Darwin, Geologist is, besides being a enormously valuable addition to Darwin scholarship, a beautifully produced volume in which both the author and publishers should take great pride. One feature of the production in particular should be praised and emulated. There are a generous number of figures and plates throughout the volume. Rather than have them sequestered in bundles, as is so often the case, the editors have chosen to place the images precisely where they are being discussed, saving the reader the task of searching around for the appropriate illustration. The notes and bibliography are as rich in detail as the volume itself.

There is a lesson from this study that I doubt was intended by the author, a lesson that emerges when we place this study of Darwin in the context of the scholarship of the last 20 years or so. Darwin's achievements are too great (in both the descriptive and normative senses) to capture on one canvas. Herbert notes that Darwin essentially gives up field work in geology after his marriage and move to Down; and, thus, her volume focuses the vast majority of its pages on Darwin's early years. Yet those are the same years that have led Hodge, Kohn and Sloan to paint Darwin as a 'generation theorist', driven by a passion for understanding the multitude of ways in which the continuity in 'the coral of life' is reproduced. Neither of these images of Darwin takes into account the fact that after the *Origin*, during the last two decades of his life, Darwin became one of the world's leading botanists, displaying in volume after volume a subtle talent

for experimentation that few studies of Darwin emphasize. His modest, even self-deprecating, manner should not mislead us: he was a multifaceted genius to whom no one book can do full justice. *Charles Darwin, Geologist* illuminates in rich detail one of those facets.

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Animals, Gods and Humans: Changing Attitudes to Animals in Greek, Roman, and Early Christian Ideas by Ingvild Sælid Gilhus

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Anyone who would offer an extended study of animals in classical antiquity must essay an oft choppy voyage between two scholarly perils. On the one side lies a Scylla of the sheer bulk of evidence and the scholarship involved in interpreting it. Articles in the standard classicist encyclopedias offer treatment both exhaustive and exhausting; they recount, as they inevitably must, the enormous quantity of ancient evidence while remaining cognizant of the equally enormous quantity of scholarship. On the other side lies a Charybdis of narrowing the evidence by means of chronology, theme, and interpretational guidelines. Most would, with Odysseus, favor a middle course between those two perils, since no one mortal can command all the evidence, all the scholarship, and all the possible interpretations. But Scylla and Charybdis make for rough sailing; the scholar sailing that middle course will be buffeted by important, often intractable, issues of genre and use of literary evidence, to say nothing how to yoke this all to the extensive archaeological evidence, especially from vase painting. Even after the voyage has led to the safe haven of a finished monograph, the scholar still risks storms on land. Despite a monograph's justifiably reduced purview, some readers will carp about what they consider to be a 'crucial' missing bit of evidence or its interpretation. They will be wrong. They should judge according to the integrity of the voyage, and the quality of the evidence offered in its support.

Gilhus focused her study on animals in Greco-Roman religion during 'a limited period in human history, the first to the fourth century CE' [3]. The period may be limited, but the issues of Greco-Roman religion in that period are enormous; the interaction with

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Christianity which Gilhus unhelpfully stylizes as 'the transition from *paganism* to Christianity' [3 (my italics)] redoubles the issues. Actually, what seems to interest her most is the transition from blood sacrifice to bloodless sacrifice, a theme evident throughout the book.

There are 12 chapters and a conclusion, all helpfully subdivided. The first four chapters cover the Greco-Roman material in roughly chronological progression, with the first devoted to key concepts or issues such as divination, sacrifice, and diet. Within that progression appear various themes such as soul and reason [ch. 2], vegetarianism and physiognomics [ch. 3], and metamorphosis into animals [ch. 4]. Throughout these chapters, Gilhus must inevitably range beyond the bounds of strict chronology in the interests of presenting the necessary complementary evidence and interpretation. The next three chapters are transitional: they examine both Greco-Roman and Judaeo-Christian material salient to the religious value of animals [ch. 5], to their sacrifice [ch. 6], and to critiques of sacrifice [ch. 7]. A wise move this, since it juxtaposes critics from the two contrasting religious traditions, often to good effect. The next four chapters consider the *New Testament*'s Lamb of God [ch. 8], as well as beasts and demons [ch. 9–11]. These chapters are particularly important because Christianity rejected the actuality, concept, and practice of animal sacrifice. The concluding chapter soars, considering, as it does, the θεῖος ἀνήρ (divine man) and talking animals, both important issues given that animals were no longer sacrificed in Christian ritual and, hence, had an entirely different religious role.

Reader or reviewer must wonder 'Animals or sacrifices—which is it?' First, if sacrifices, Gilhus has said too little: nowhere does she provide an overview of Roman, let alone Greek, animal sacrifices; nor does she ever indicate the lack of a single canonical ritual and, thus, the extreme variability of the various possible components of the ritual. Worse, she seems to view the bloodless sacrifices of Christianity as an innovation. But what of the very old and important role, albeit enigmatic, of non-animal sacrifices in Greco-Roman religion? It has often been held that bloodless offerings characterized the worship of chthonic powers. For example, one Roman festival of the ancestral spirits (*manes*), the Lemura (May 9–11–13), utilized offerings of black beans in the dead of night [Ovid, *Fasti* 5.419–492]. But contrast Odysseus' use of blood to reanimate and consult the

spirits of the dead at the gates of the underworld [Homer, *Od.* 11.20–50, 97–99].

Second, if animals, Gilhus has said too little. It would be absurd to carp that she does not seem to know about simians in classical antiquity [McDermott 1938], since their relation to religion remains tangential at best. It would be less absurd, but still unreasonable, to complain that despite her obvious interests in the *Natural History* of Pliny the Elder, she does not utilize his famous passage that 'the Magi consider no animal to be fuller of religion than the mole' [30.19: *nullum capacius religionum ... animal*]. But her discussion of bees [73–74] misses the mark. She knows of Vergil's Fourth *Georgic* but apparently only in passing. The connection of the bees with the tale of Aristaeus, Orpheus and Eurydice [4.315–558] entails issues of mystery religions and Orphism, surely germane to Gilhus' thesis. Moreover, the idea of the bee's arising spontaneously from an animal carcass (*bugoneion*) involves the issue of spontaneous generation and a diminished divine role, considerations which Aristotle thought significant enough to discuss at length [*De gen. an.* 759a8–761a11]. Such omissions, too numerous to list, occur throughout the book and greatly weaken its argument.

It was not so good an idea, I think, to focus in chapters 2–3 on the transition between traditional Greek and Roman religion to Christianity. In order to prosecute that agenda successfully, one needs a sure touch for, and deep knowledge of, Greco-Roman religion in its socio-historical context, both of Gilhus' period and of its antecedents. One needs the skills of a practicing classicist, although one does not need to be one, because many of these material are accessible only in the original languages. But that is not Gilhus, as the examples I have just given, and will give, sadly indicate.

Although Greek and Latin literature of Gilhus' chosen period obviously relies heavily on its antecedent traditions, her book does not. For example, the index gives but one unhelpful reference each to Homer and Hesiod, thus raising the question of the textual foundation of her already compromised interpretative edifice.[1] Overall, the index is not good. It is incomplete for the animals it does cover, and omits many animals mentioned in the text: in short, the index makes the book toilsome to use and is itself misleading.

[1] I note that her bibliography of texts contains no reference at all to Homer.

As for matters of evidence, Gilhus does not appear up to her task. It is hard to have confidence in her views of the Greco-Roman non-Christian authors, since she relies almost exclusively on translations, and not the best translations in any case [287–293]. That is, too often she relies on the Loeb Classical Library.[2] Some volumes in that series, especially the more recent ones, are of very high quality indeed.[3] But there is real danger in using so many provably dated and inaccurate translations. The upshot is that where a translation is misleading, so is Gilhus.[4] Matters are rather better for the Christian authors. The texts from the Judaeo-Christian tradition tend to the reliable; *Sources chretiennes* is mentioned, and various other recognized editions of authors. But even there, the translations are not always the best—for example, the translations from the Ante-Nicene Fathers which appear throughout.

But there arise issues larger than those of evidence *per se*. Which Greco-Roman religions and whose Greco-Roman religions is she considering—and what was 'traditional'? Ancient Roman religious specialists thought traditional religion came from the period of the monarchy (usually given as 753–509 BC), especially and anachronistically from the reign of their second alleged king, Numa Pompilius. Likewise, much of the ancient Greek educational system was based on the paradigmatic vision of divine machinery in the Homeric epics. Reality differs. There were in fact multitudinous cults and wide variations of worship and theology even inside one cult—should one be surprised when several cities in the Greek East each claimed to be the birthplace of Aphrodite? In other words, although everyone knows that Greek and Roman religions were not 'religions of the book', Gilhus has not grasped the implications that this has for her account.

Which Romans and which Greeks? The socio-economic elites? They produced virtually all of the literary evidence, and yet they

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[2] Gilhus fails to consult some fundamental collections of fragments that have not been translated: for example, given her interest in physiognomics [74–176], the absence of any reference to Förster 1892–1893 is striking.

[3] For example, Martin West's *Hesiod* volumes [1966, 1978]; George Goold's *Manilius*; Roland Kent's *Varro: De Lingua Latina*.

[4] Curiously, she cites the older translations by date of reprint, so there inevitably arises the surely unintended misperception of very many recent translations when, in reality, many of those translations are a century or more old.

constituted a tiny minority of the total Mediterranean population in any era. The majority of the lower socio-economic orders? There the issue lies with merely getting the evidence. The elite's authors satirized the lower orders regularly; since the lower orders left virtually no literature, one is obliged to reinterpret the elite's evidence for them and to cull scraps of papyrus, stray inscriptions, and archaeology for clues. I offer such considerations as questions, because nowhere does Gilhus attend to them as she implicitly presents Greek and Roman religion as monolithic entities in monolithic societies, all the while privileging literary evidence over any other kind of evidence, while even in that literary evidence she demonstrates a serious lack of acquaintance with the authors, their traditions, meanings, and implications. Gilhus' views of Christianity verge on the antediluvian. Which Christianity and whose Christianity? Only in the fourth century AD did a canon of the *New Testament* books begin to appear. Despite that, and despite the same century's Council of Nicaea (AD 325), there simply did not exist, and never had existed, one 'orthodox' Christianity. Rather, there were competing varieties of Christianity, characterized by the ultimate contest-winners (Nicenes) as 'heresies'. It scarcely needs elaboration that animals will serve very differently in, say, Nicene Christianity, Carpocratian Christianity, and Arian Christianity. Gilhus is aware of the alleged 'heresies', but apart from a few pages [238–242 with notes] her Christianity seems of the older unjustifiable scholarly view which posited one original and unchanging 'orthodox' religion; if she has considered the work of Walter Bauer [1971] and John Gager [1975], there is no sign of it here. She considers Gnosticism a hybrid of Christianity and Greco-Roman religion [108], contrary to recent thinking on the subject [see King 2003]. Gilhus' lack of clarity on these very basic issues of evidence and scope inevitably casts doubt on her deployment of evidence and on her interpretations both small and large-scale.

While I have written above that Gilhus rightly will not be bound by strict chronology in the Greco-Roman chapters, she has unfortunately not avoided an oft willy-nilly oscillation between topics. Consider the second chapter, 'United by Soul or Divided by Reason' whose chronological and thematic leaps have no apparent rationale. Thus, there is a wild ride from Plutarch to Philo to Homer [44–51], the last appearing in a riddling reference [see below]. Then, she goes on to a woefully brief treatment of Philostratus on Apollonius

of Tyana [53–56], and concludes with the obviously here misplaced discussion of Origen and Celsus. When such seemingly gratuitous, free associations of genre, geography and author appear, they do no good since they level important differences of genre and intellectual *milieu*. Indeed, the chapter's conclusion [61–63] might be summarized as 'There was an ancient philosophical debate on animals. There were unsolved problems. Let us move on to vegetarianism.' Vegetarianism, the subject of the next (third) chapter rightly opens with Pythagoras. But how seriously can one take a treatment which shows no awareness of Walter Burkert's fundamental study [1972]? This is not the traditional reviewer's carping on the omission of a favorite (to her/him) piece of bibliography. Even casual use of Burkert would have saved Gilhus' discussion. I note in passing that Burkert does not appear at all in her bibliography; although I have been critical in print of some of Burkert's more recent work [see Phillips 1998, 2000b], he and Martin Nilsson indisputably constitute the two 20th-century scholars whose works are fundamental for the study of Greek religion. Unhappily, the absence of Burkert and Nilsson from Gilhus' bibliography does not surprise. Unhappily again, such a lack of logic, scholarship, and evidence characterizes all the chapters of Gilhus' book.

I turn now to a brief selection of missed opportunities, and supply either some clarification of points Gilhus has made enigmatically, or introduce some requisite considerations of which she does not seem cognizant. In both cases, I organize them thematically the more readily to demonstrate their relevance for her undertaking. This brief enumeration is not meant imply that she should have presented precisely these items of detail since, as asserted above, that would be unfair. Nevertheless, it remains passing strange that she gives no hint of anything of their ilk.

Varieties of Myth and Ritual The metamorphosis of humans into animals constitutes a major aspect of animals in Greco-Roman religion; Gilhus rightly devotes her fourth chapter to it. But her perspective and, hence, most of her observations on same start from an almost fatal flaw. She takes Ovid's *Metamorphoses* as canonical. This will not do. Certainly, Ovid offers important evidence for the Roman appropriation of Hellenic mythological traditions at the start of her chosen period, but he scarcely initiated it. For example, Livius Andronicus' third-century BC Latin translation of Homer's *Odyssey*

must surely have included Circe's transformation of Odysseus' men [*Od.* 10.233–243]. While Gilhus observes that Actaeon still retains his human mind in Ovid's version [80–81], what of Io, to whom she gives but cursory discussion? In the first century BC, Calvus did a mini-epic (epyllion) about Io's transformation and unhappy wanderings; at least two of the poem's surviving fragments strongly imply she retained her human mind as well [see Courtney 1993, 205 #s 9, 10]. In short, the fact that Ovid's work represents the first fully preserved account of transformations does not make his the first Roman account ever.[5]

Further, were these even Roman 'mythologies' at all, or literary tropes? Here lies a major issue of Roman religion—whether the Romans had mythologies before their contact with the Greeks and what constituted those mythologies. There is an ever larger issue of what constituted a mythology. Was there one core mythology which remained relatively stable across time despite various tangential accoutrements added to it? Or did the very mythology and, hence, its range of meanings shift across time as a kind of theological amoeba? Discussion of these last issues has long been, and unfortunately still is, a scholarly blood sport; nevertheless, it behooves any scholar dealing with mythologies to stake out a position. Returning to the particular, compare Gilhus' brief summary of Ovid's version of the Io mythology [79] with the enormous number of variations which existed in the Hellenic tradition [Gantz 1993, 198–204]. Again, on the issue of humans retaining consciousness while in animal form, there is the famous literary example of Homer's appearance to the Roman poet Ennius (early second century BC) in which the Greek bard recalls becoming a peacock [Skutsch 1985, 71, ix]. This raises the still larger theme of transmigration of souls, a belief which was certainly well known by the time of Herodotus [*Hist.* 2.123] and earlier with the Presocratics.[6] Largest of all is the myth-ritual issue. Such theoretical concerns have long exercised not just classicists but also anthropologists and historians of religion. Wherever one comes down on the myth-ritual issue, and one really must alight somewhere, there then arises the question

[5] Gilhus' footnote [78n1] is utterly baffling, as if to retail names she has gotten from a handbook without pondering their relevance.

[6] The clearest brief introduction to this enormous topic appears in Lloyd 1993, 59–60.

of how to apply it to Gilhus' period. On the Roman side, matters are bedeviled by the issue of what Roman mythology 'was'. On the Greek side, there are continuities with the earlier traditions, but continuity does not imply unvaried transmission [see Dowden 1992, 102–118].

Mechanics of Roman Ritual How can a monograph which claims sacrificial ritual as major concern avoid a clear explication of animal sacrifice and its issues [22–26]?[7] Few without competence in Roman law will understand Gilhus' claims about the legal liability (noxal liability) for injuries caused by animals, and just as important yet totally absent in her presentation is the consideration of what constitutes a 'tame' or 'wild' animal.[8] Then, there are various ritual details small and large. Of the former, consider the Roman instauration, the repetition of a ritual otherwise marred by a flaw such as an animal's being understandably averse to offering its life for the ritual and running away. How often did this happen, and how often was a full instauration practiced [see Cohee 1994, Nock 1939]? Of the large issues, consider the *ver sacrum*, the dedication of the agricultural fruits to the gods. An extremely old and widely practiced Italic ritual, this was a kind of mass sacrifice by an entire population [cf. Phillips 2002c]. But for Gilhus, the difficult fact remains that there was no one canonical sacrificial ritual common to the polytheistic religions of her period. There were common features, of course, to all sacrificial rituals. But those were outweighed by the cult-specific components. In short, the sacrificial rituals in Greco-Roman polytheism were even more polymorphous than one might plausibly expect from religions not 'of the book' [cf. Phillips 2002a].

The Nature of Animals and Divination Three pages [26–28] on this animals and divination seem rather brief, briefer still when only half of them are devoted to the Greco-Roman material. In such a compressed format, an enormous amount of basic information is inevitably suppressed, consequently skewing the further use that Gilhus makes of it. All agreed that signs of the gods' intentions could and did appear; the issue became what constituted a sign. In a way, the famous line from Homer where Eurymachus criticizes a prophecy from birds is crucial: 'while many birds fly in the sky, not

[7] On the Romans, see Phillips 2000a.

[8] Gilhus wrongly downplays noxal liability [22–23]; as for wild and tame animals, see Frier 1982–1983.

all of them have meaning' [*Od.* 2.181–182]. The very complex and detailed Roman system had the advantage of avoiding this, although it should be noted that much of the Roman system came from the Etruscans (who are totally missing in Gilhus' account). Thus, there were the haruspices, the augurs, and the information to be drawn from a sacrifice, to give but three examples. Gilhus' treatment is so general as to confuse anyone except the specialist. She knows of Jerzy Linderski's work, but not his fundamental article on the regulations for the augurs [1986]. Her discussion of Cicero's well-known passage on the absence of a liver from a sacrificial victim [*De div.* 1.118–119, 2.36–37] seems totally misguided since she takes it as satire, when in reality it presents two very real, contradictory interpretations. She treats omens, augury, and haruspices as aspects of the same thing, despite their different methodologies, organizations, and the different people who could practice each method. That is, Gilhus has leveled so much here that the enormous complexity and sophistication of the Roman system vanishes.

There is also another way of looking at signs from the gods, and it consists in focusing on how the ancients conceptualized them. Gilhus' use of 'prodigy' to fit all signs will not do. Anything out of the ordinary, as the ancients variously considered 'the ordinary', could be a θαυμασιόν or *mirum* (wonder). If a religious specialist deemed such an appearance significant, that meant that the sign offered probative information of divine will, for him and for those to whom he possessed credibility. But simultaneously, anyone could proffer such an interpretation and, once again, his claim of probative information of divine will would be credible for him and for those to whom he possessed credibility. That is, there was a twofold system: religious experts and non-experts both interpreted omens, and sometimes even the same omens. There also existed a category which was not merely θαυμάσιον or *mirum* but called a τέρας in Greek or *monstrum* in Latin. Each could be a token of divine will, but with a quality of the dire or dangerous. For example, a seriously deformed human or animal birth constituted a τέρας/*monstrum*. It is unclear how the Greeks handled such an appearance, but the Romans typically would either kill it or expel it [see McBain 1982]. Finally, such words were not limited to signs: the terms θαυμάσιον/*mirum* and τέρας/*monstrum* are applied to various mythological entities in a dizzying number of permutations as a function of author, author's

ideology, and genre. Finally, Aristotle uses τέρας to describe deformities without the least hint of any inherent divine communication [*De gen. an.* 767b4–14, 769b1–10, 29–30].

Material Evidence Here Gilhus offers little. I give one example, not to imply she should have used this particular one, but to demonstrate what material evidence can offer. Since Gilhus' sole interest in Greco-Roman mythological animals devolves on the metamorphoses of humans into them, it does not surprise that beings such as Cerberus, Chimaera, and Typhon are passed over. I focus on Cerberus the canine who traditionally guarded the entrance to the underworld. He is simply 'the dog' in Homer [*Il.* 8.366–369]. Hesiod gives him 50 heads [*Theog.* 311–3412], while Pindar seems to have assigned him 100 heads [Scholiast to *Il.* 8.368 = Pindar frg. 249b Snell]. Later literary evidence makes him three-headed [Sophocles, *Trach.* 1098; Pausanias, *Graeciae desc.* 3.25.6], sometimes of three bodies [Euripides, *Herc.* 24]. There are frequent elaborations on the nature of the heads. As for quantity, one to three heads appear to be the norm for Greco-Roman material evidence. Why not more? One could, of course, argue a case for mere stylistic and technical limitations—how does a vase painter depict more than, say, four heads? All of this raises important questions of the relation between artistic and literary traditions while, writ large, there looms the question of why there was no canonical version of Cerberus' anatomy and what that says about how the ancient conceived animals in the mythico-religious realm.[9]

Greek Traditions No one should expect Gilhus to trace the development of animals and religion *in extensis* previous to the first century AD. But everyone should expect some use of those earlier periods to cast light on her chosen period. One simply cannot understand and interpret the evidence from one period fully without regard for the earlier traditions which fed that evidence. But she demonstrates minimal awareness, at best, of the earlier periods. For example, the index gives one reference each to the two authors with whom Greco-Roman literature begins, Homer and Hesiod. The former [75] appears in her section on physiognomics, where she asserts that comparison between humans and animals is common. In one sentence, she has

[9] West 1966 on *Theog.* 312, 769–773, Woodford and Spier 1992 both provide full details, the latter especially on the material evidence.

unburdened herself on the famous Homeric simile and missed absolutely everything about its implications for her study [see Reizler 1936]. The passage from Hesiod [23] involves the issue of νόμος, which she mistranslates as 'law' rather than 'way of life' for animals coming from Zeus [Hesiod, *Op.* 276–280: see West 1978, *ad loc.*]. That is, she has missed the point of the passage she does cite, and has not utilized Hesiod's extraordinarily rich information on animals and religion for the light it can cast on the texts of authors of her chosen period. Put simply, she does not take account of the earlier Hellenic traditions, a serious failure because these traditions were appropriated and incorporated into the later Roman traditions, and because they continued to be observed in the Hellenic regions of Roman empire. Cults continued, Hellenic rituals continued, Greek authors continued. By neglecting the earlier Hellenic traditions, then, Gilhus offers a partial and imperfect view of the contemporary evidence for animals in her period.

I would repeat what I have tried explicitly to state at several points above. There is simply too much material for Gilhus to have taken account of all of it. There are serious issues with the evidence she does use. Put differently, her period's literary evidence would go some ways to sufficing for her topic if, and only if, she had attended to its implications and underlying traditions. (By 'underlying traditions', I mean both the earlier literary evidence and also the material evidence.) But Gilhus seems to have snatched some evidence, grabbed the first scholarship to hand, and soldiered on. The result is unsatisfying not only for her analysis of the Greco-Roman traditions but as they provide a point of comparison with her (somewhat surer) discussion of the Judaeo-Christian tradition. If one will work outside one's field, one should at the least become tolerably conversant with the evidence of that field, its scholarship, its issues, and how it operates. This Gilhus has not done.

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Explaining the Cosmos: The Ionian Tradition of Scientific Philosophy by Daniel W. Graham

Princeton: Princeton University Press, 2006. Pp. xviii + 344. ISBN 0–691–12540–6. Cloth $45.00

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Daniel Graham's splendid monograph, *Explaining the Cosmos*, takes on the task of presenting a new reading of Presocratic philosophy under a single, unifying theme: the development of a scientific paradigm —or rather, two—for explaining the natural world. This is an ambitious undertaking, and Graham's lucid and thoughtful account offers much of value to interpreters of early natural philosophy. The book undertakes two projects, intertwined but nonetheless distinct: one is to present an account unifying the main themes in Presocratic thought; and the other, to present that thought as scientific in a modern sense of the term. While I think there are unanswered questions about the second project, these should not undermine the strength of the book's contributions to the first.

Graham challenges what he calls the 'Standard Interpretation' of Presocratic thought, an interpretation going back to Aristotle, in which the monistic materialism of the Ionians is rejected—in response to Parmenides' criticisms—in favour of various pluralist systems. Unlike one recent interpretation, his critique of this narrative does not amount to a rejection of the idea of a unifying story [see Osborne 2004]. Quite the contrary: Graham tries to save what he presents as the scientific character of Presocratic thought by reformulating our understanding of the connecting narrative. While Graham is acutely aware of the lack of historical evidence linking some of the figures—in many cases we are speculating who, if anyone, they are responding to—he reformulates the standard story of thesis-and-response, retaining the ambition to make sense of the complexities of Presocratic thought through a single, connecting narrative. The result deserves serious consideration.

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Graham highlights the importance of Ionian philosophy, by which he means, principally, Anaximander and Anaximenes, with the likely addition of Xenophanes. Graham adapts and develops a proposal by Cherniss and Stokes that the Ionians were not, as commonly thought, monistic materialists. Rather, they thought that a single generating substance gives rise to all else through transformation. In Graham's revised interpretation, Anaximenes is no minor figure but the culmination of Ionian scientific philosophy: his provision of a 'mechanism' by which the primary substance can transform (by condensation and rarefaction) sets this Ionian Generating Substance Theory (GST) on a solid scientific footing. Graham questions Barnes' argument that an account based on condensation and rarefaction is evidence of an attempt to view other substances as made out of something else. However familiar such an approach is to us, Graham argues, there were no criteria available at the time to make sense of the claim that one substance is really another. He suggests that consumption is the natural way to conceive of transformations, and that the idea that apparent transformation could occur in a monistic materialism is a later idea, hard won.

Although GST belies Aristotle's reports on the Ionians, Graham notes the scarcity of textual evidence that the Ionians were monists: he thinks that Aristotle was mislead by the popularity of a later natural philosopher who was a material monist, Diogenes of Apollonia. Against the background of the fifth century, when the distinctions between appearance and reality, nature and essence were well established, he argues, monistic materialism became a reasonable option; and Aristotle could easily have been read Diogenes' view back onto earlier thinkers.

Graham sees the Generating Substance Theory of the Ionians, moreover, as providing a better foil for the reactions of Heraclitus and Parmenides than the Standard Interpretation. On his reading, Heraclitus is no proponent of contradiction, but is merely educing the consequences of Ionian philosophy. Heraclitus articulates the principle that constant change of substance is compatible with stability of the higher-order structures that supervene on them. Flux becomes law-like. Graham even suggests that this reciprocity would allow for laws of conservation of proportions, an idea that Heraclitus does not develop. Heraclitus' response to the Ionian theory of transformation was to develop a philosophy that focused on the process, not on the

material. In doing so, however, he identified a difficulty implicit in Ionian philosophy: that there is really no argument for taking one substance over others as primary.

Parmenides, Graham suggests, is reacting to another difficulty implicit in the Ionian program, the problem of accounting for change if transformation of substance happens without remainder. Had the Ionians already proposed monistic materialism, he argues, they would already have solved this problem. Parmenides' criteria for Being are, he suggests, directed to 'mortals' who 'think that to be and not to be are the same and not the same' [159], i.e., to Heraclitus' recognition that Ionian philosophy implies the simultaneously ever-changing and unchanging nature of reality. Parmenides' four criteria for Being—it is ungenerated, all alike, motionless, and complete—could be given a minimal or maximal interpretation: either these are properties anything must have to count as a being, or they could be taken to be sufficient to delimit the only viable account of reality. Only on the maximal interpretation is Parmenides a monist; the former implies a critique of Ionian philosophy but does not reject cosmology out of hand.

Graham gives a prominent place to Parmenides' *Doxa* in his narrative; he stresses that this account is not presented as falsehood but as mere opinion. Although later Eleatics like Melissus became the filter for subsequent understandings of Parmenides, Graham takes Parmenides to be offering a serious attempt at a cosmology that surpasses the Ionian accounts and that he regards as the best, albeit flawed, way to make sense of the world [171]. It is unlike Ionian cosmologies in beginning from the idea that equal and permanent opposites account for everything. Graham suggests that the *Doxa* is the basis for the new theories of the pluralists—especially Empedocles and Anaxagoras—who, contrary to the Standard Interpretation, are not explicitly critical of Parmenides. The view which they and the atomist develop constitutes a new paradigm, the Elemental Substance Theory (EST), replacing the earlier, Ionian GST.

This is an innovative and philosophically rich reading of Presocratic thought. It contains some gems aside from the central narrative itself: the analysis of textual parallels linking Heraclitus and Parmenides, the readings of the indifference argument in Democritus, the serious attention paid to Xenophanes and Diogenes, and the clear

and economical presentation of major trends in scholarly interpretation. But its strength is the attempt to provide a textually supported, philosophically coherent, and historically plausible narrative that motivates the reactions and responses of different figures. This is a fresh and innovative thesis, one that should be taken seriously.

But it would help to clinch the deal if we had more of a story on the background to the questions addressed by the Ionians in particular. Graham argues that the Ionian cosmologies should be regarded as a 'scientific paradigm', an attempt to produce a unifying account of the natural world. In a context where there is no methodological or epistemological tradition to draw on, he understands this to be achieved simply by offering a concrete example of a complete cosmology, allowing for development or rejection. While he acknowledges the absence of other features like a professional community, a program for empirical testing, or an institutional setting, Graham takes the very act of offering a complete, material world picture which eschews the superstition of mythological accounts, as establishing a 'research program'.

What is interesting here is surely the suggestion that a particular way of explaining the world became dominant, was challenged, and was superseded by another. It is a little disappointing, then, that there is not more to be said on the motivation for articulating such an account or why it might have seemed compelling to others. He argues later than Ionian philosophy provided the unifying framework within which other fields such as history, medicine, mathematics, technology, and rhetoric were able to make substantive contributions to knowledge and denies that these fields helped spur the development of philosophy [302–305]. He also declines to suppose that the motivation for the Ionian program was a deliberate attack on mythology [104]. He occasionally allows for a technological or political inspiration for particular ideas, but does not seem to think we can really *explain* the emergence of such a powerful world picture: he describes the physicalism of the Ionian cosmologies as 'miraculous' [98]. But this seems too quick.

Graham follows a usage common with much of 20th century scholarship—despite cautionary notes by Balme [1941], Lonie [1981], Furley [1987], and Hirsch [1990]—and writes of 'mechanical explanation' in the Ionians. Graham generally uses the term to signify

the rejection of intentional or teleological accounts, but the claim is surely not that this rejection is motivated by a machine analogy or an appeal to the workings of mechanics. He in fact denies that Ionian natural philosophy was driven by experience with technology [305]. In a work that aims to illuminate the early history of scientific thought, one might want to hear more about the reasons for this rejection. What commitments constrained the speculation of the Ionians; what heuristics or exemplars might have guided their accounts? One further reason for stressing the scientific character of Ionian thought seems to be that, at least implicitly, the transformation of substances is law-governed. Here again, one might like to hear more about the conception of law at work and also its relationship to the idea that nothing comes from nothing: Graham scarcely mentions, except in passing [125], a commitment that others have seen at the heart of Ionian cosmological speculation.

Graham defends a belief in scientific progress against Kuhn's reservations [299]. Although he makes a case for the cumulative effect of some unprogrammatic ideas like the borrowing of the Moon's light from the Sun, others may be sceptical of the idea that 'conceptual progress' can be identified by noting anticipations or forerunners of modern theories [300]. Indeed, Graham's scholarly reserve sometimes prevents him from assuming that cumulative progress is the norm: why else would he hesitate to ascribe to Diogenes the view that differences of heat go along with differences of density [284], an association well established in the Ionian philosophy?

But these reservations should not be taken to detract from the worth of Graham's proposal. There are many interesting and valuable insights in this book, which has much to contribute to Presocratic interpretation. Graham's fine prose and uncluttered style is exemplary; his scholarship, sound and accessible to non-specialists. He helps the reader keep track of an alphabet soup of theories admirably, and uses formal precision without letting it dominate his account. This book leaves many questions unanswered in its attempt to portray the Ionians as the founders of western science, but its ambition in offering a bold new unifying narrative is to be applauded.

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Archytas of Tarentum: Pythagorean, Philosopher and Mathematician King by Carl A. Huffman

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The volume offers a full discussion of all the genuine fragments and *testimonia* ascribed to or concerning Archytas, the Tarentine mathematician and philosopher who is currently (and correctly) taken to have been one of the first thinkers who applied mathematical procedures to the investigation of natural phenomena. The book is divided into three parts. The first part [3–100] presents a number of introductory essays, organized in two broad sections—one about Archytas' life, his writings and the reception of his work; another about Archytas' philosophy—and concluding with a particularly valuable discussion of the authenticity of the received texts and *testimonia*. Huffman accepts as genuine the four fragments that scholarship has commonly ascribed to Archytas at least since Diels' and Kranz' collection [1951–1952, 1.47]. He devotes the second part to the discussion of these and related texts [103–252]. The third part presents the genuine *testimonia*, arranged into seven broad sections: life, writings and reception, moral philosophy and character, geometry, music, metaphysics, physics, and miscellaneous [255–594]. Two appendices contain a rather substantial discussion of the spurious writings and *testimonia* (actually a non-negligible portion of the writings ascribed to Archytas), and a short investigation about Archytas' name. A bibliography, a select index of Greek words and phrases, an *index locorum*, and a general index complete the volume. A rather appealing feature of Huffman's exposition, already tried in his preceding volume on Philolaus, is the absence of footnotes, except for the first part.

A typical discussion of a fragment and of some of the main *testimonia* has the following structure. The Greek text of the fragment or *testimonium* is presented with a critical apparatus; it is followed by

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a translation and possibly by related passages, for which the Greek text (usually without apparatus) and translation are also provided. What then follows is articulated in sections concerning authenticity, context, the Archytan work from which the fragment could possibly have been extracted, and a number of items whose argument depends on the contents of the fragment or *testimonium* at issue. Under the heading 'context' one finds also very valuable textual discussions such as, for example, the one concerning the entangled question of the two versions of the text of Fragment 1.[1] The genuine fragments and some of the *testimonia* receive also a final, line-by-line commentary, where important textual issues are discussed in great detail and very well.[2] Huffman has checked the readings of the main manuscripts only for Fragments 1 and 2, namely, those coming from Porphyry's commentary on Ptolemy's *Harmonica*. As it turns out, Düring's standard edition of this work is in fact quite unreliable. In these cases, Huffman provides a text that is greatly improved with respect to that found in Diels' and Kranz' edition; in all other cases Huffman has relied on the best editions. As is clear from the extent of the volume, the discussion aims to be, and in fact is, exhaustive: this work will be a landmark of careful and serious scholarship, not only of Pythagorean scholarship, for many years to come.

I shall offer a few critical remarks focusing on the main *testimonium* of the geometrical achievements of Archytas, specifically, his striking method for finding two mean proportionals between given straight lines as reported in Eutocius' commentary on Archimedes' *De sphaera et cylindro* 2.1. Huffman reports this proof and supplements it, as a further *testimonium*, with Eratosthenes' account of the Delian problem, coming from the same source. Some of the remarks below, however, will possibly apply also to other fragments or *testimonia*.

There are several problems in the presentation of the Greek text of Archytas' method. One concerns the *sigla* adopted in the apparatus: they are in fact the ones employed in the standard edition of the Archimedean texts and Eutocian commentaries thereon, by J. L.

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[1] The first is reported by Porphyry and the second, which is less extensive, by Nicomachus.

[2] The discussion of the use of Doric forms is a case in point.

Heiberg in the second decade of the 20th century. However, Huffman provides no key to these *sigla*, and so the reader is not told to what they correspond. In the case of the Eutocian passage containing Archytas' duplication of the cube, this may cause some trouble. First, *siglum* **A** does not denote an extant manuscript, but a sub-archetype. Granted, the history of this manuscript can be reconstructed fairly well from its possible first surfacing as a model of a part of Moerbecke's translation all the way to its last resurrection in the middle of the 16th century; but it remains that this *siglum* stands in fact for the consensus (or the majority) of four extant manuscripts copied from **A**.[3] True, Heiberg is straying form standard practice in employing a Latin capital letter as a *siglum* for a (sub)archetype, but he explains his choice in his *praefatio*. In any case, it is regrettable that there is no clarification of this to be found in Huffman's book.

Second, *siglum* **B** is not a Greek manuscript but the Latin translation contained in the ms. *Ottobonianus Latinus* 1850, an autograph of William of Moerbecke. The first place where this feature of the *testimonium* can be surmised is at 343.27; but unfortunately the variant readings here come from a second hand (in fact an owner of the manuscript, the early 16th century scholar Andreas Coner). The reader must wait until 361.22, where the text of Eratosthenes' account is presented, to realize on his own that **B** is in fact written in Latin. As a consequence of all this, a variant reading made by a Greek syntagma followed by the *sigla* **AB**, such as, for example, in the apparatus at 342.12, is misleading unless some explanation is offered. In sum, the attentive reader who does not know the textual history of the Archimedean text will be at a loss in trying to interpret the rather surprising and contradictory indications contained in Huffman's apparatus.

Third, the apparatus of Heiberg's edition has not been reported in its entirety: some variant readings have been skipped without any

[3] These variant readings marked by *siglum* **A** are printed in Heiberg's and accordingly in Huffman's apparatus without accents or breathings. (There are two typos in Huffman's apparatus at 361.22 and 362.27, where a breathing and an accent have been marked.) A scribal note (transcribed in full at Heiberg 1910–1915, 3.x–xi) to an apograph of **A**, namely, *Parisinus graecus* 2360, justifies this in that it ends by asserting that the model was almost completely deprived of prosodical marks.

clear rationale that I can detect. Frankly speaking, these drawbacks make the usefulness of Huffman's apparatus accompanying the Eutocian text rather doubtful. I have not checked the other apparatuses in detail, but the reader should take their indications with some care, excepting those set up by Huffman himself after a personal inspection of the manuscripts.

A fourth problem concerns the ascription to E. S. Stamatis of some emendations to the text. The reference should definitely be to Heiberg, as these emendations are singled out by a *scripsi* in his edition: the name of Stamatis is unduly attached to the photostatic reprint of Heiberg's edition [1972] and for reasons that escape me. In fact, the reader will search in vain even for the *corrigenda* that in the title page of the reprint are said to have been added by Stamatis (the *corrigenda* on 3.vii–viii are by Heiberg). Stamatis did not modify the apparatus—except perhaps by inserting the correction indicated by Heiberg at 1.445 (at least, the Greek fonts employed appear to be different from those regularly used in the rest of the edition)—nor any other feature of the reprinted text. To ascribe to Stamatis even the smallest crumb of Heiberg's magisterial, scholarly work is a slip that could and should have been avoided.

The translation offered of Archytas' solution is correct and well done; but it does include some idiosyncrasies suggesting that Huffman did not rely on well-established conventions in the art of translating Greek mathematical texts. Cases in point are:

- the use of 'to connect' for the standard, and more adequate from the etymological point of view, 'to join',
- a rectangle is said to be 'formed' (instead of the correct 'contained') by two lines, and
- a square is rather oddly said to be 'formed by' (instead of the correct 'described on') a single line.

Moreover, consistency is not always maintained, as, for example, when

- different forms of πίπτειν are translated with forms of 'to fall' or 'to be dropped' (the use of the passive is misleading, since the Greek has an active form);
- a rather crucial particle such as δή is frequently left untranslated [see, e.g., 342.8, 12, 14, and 20], though it should be, since it has a resultative value that makes the deductive chain tighter;

- the only occurrence of οὖν, another resultative particle, is left untranslated;
- ὑποκείμενον ἐπίπεδον is translated by the 'plane that lies under them', i.e., two semicircles (there is in fact no pronoun corresponding to 'them' in the Greek text), rather than as the 'plane laid down', that is, the reference plane;
- κύκλος at 342.3 is translated by 'the circle' rather than 'a circle': it is the first occurrence of that mathematical object in the proof, and therefore it is indefinite; accordingly, the noun does not have the article in the Greek text; and
- the last clause is likewise rendered by 'Therefore of the two given lines [...]' rather than by 'Therefore, of two given lines [...]'. The clause is that kind of 'instantiated general conclusion' by which a geometrical problem typically ends: it is a general statement and hence an indefinite one. An even better version would take the genitive as absolute and translate accordingly, viz., 'Therefore, given two lines...'.

The overall plan of the commentary on the geometrical passages is explained in Huffman's assertion that his

> goal is to present an account of the solution which will be intelligible to classicists and historians of philosophy and which can serve as a basis for discussion of the basic mathematical and philosophical issues raised by the proof. [349]

Huffman refers to well-known discussions in the secondary literature for the more technical aspects of the proof, which are completely absent in his own discussion. Yet, I wonder whether such a dismissive attitude towards discussing technical aspects is a mild form of the well-known 'obsession of the intended readership' (a widespread disease affecting the editorial offices of most scholarly publishers), or simply a consequence of the even more widespread belief that mathematical technicalities are irrelevant to the history of ancient thought. At any rate, the lack of any serious analysis of the more technical features of the proof (e.g., its connection with the very advanced domain of the loci on surfaces) greatly diminishes the value of Huffman's presentation. The analysis of the solution is simply a lengthy [351–355] and, at times, quite roundabout[4] restatement of Archytas'

[4] See, e.g., the paragraph at 354–355.

procedure. A short discussion [357–360] follows of the 'elements of geometry' possibly at work in the solution.

Huffman warns the reader that 'most of the language [of the proof] does not go back to Archytas' [349]. But much more care than this is needed in dealing with the Eutocian testimony. In truth, the most likely hypothesis is that the proof has benefited from a robust Eutocian (or pre-Eutocian) rewriting that aimed to put it in accordance with the canonical style of geometrical proofs. This rewriting has very likely affected large-scale syntactical structures in the proof, and not only lexical points.[5] But given this, it is, then, pointless to inquire about what results underlying the proof can ascribed to Archytas in the form we have. A comparison with the Arabo-Latin version preserved in the *Verba filiorum* corroborates this, since the differences between them can well be ascribed to the (double) process of translation. Both this and Eutocius' version come from the same source, but this source should by no means be identified with the Eudemian account. The ancient commentators of the Neoplatonic school consistently worked on epitomes and by epitomes, and we should take this as our main working hypothesis unless contrary evidence is adduced, when dealing with mathematical fragments reported by such commentators as Eutocius. In any event, it is poor policy to dismiss such caution as a 'hypercritical' [346].

In fact, the whole segment of Eutocius' commentary reporting the methods for finding two mean proportionals is likely to have been lifted by Eutocius from some previous collection, be it Sporus' *Keria* as Tannery suggested or not. A comparison with the fairly different mathematical style and language displayed in the passage on Hippocrates' quadrature of lunules reported by Simplicius on Eudemus' authority shows that this must have happened. (Note that Simplicius' institutional career is rather complex, and it is likely that he had access to mathematical sources unavailable to his colleagues in Alexandria: Simplicius appears at times to be proud of presenting hard-to-find texts.) All of this entails taking into account a further, pre-Eutocian rewriting of the solutions to the problem of finding two mean proportionals: it is, for instance, clear that the two proofs

[5] Thus, it comes as no surprise that so many passages can be found in the proof that fit more or less exactly the elementary results or formulaic phrases found in the *Elements*.

ascribed to Menaechmus are entirely rewritten, while the Dioclean solution is modified on crucial points. Eutocius' indication that the Archytan proof is 'as Eudemus reports it' would then simply have been contained in his source.

Huffman asserts that he has

> included references to relevant parts of Euclid's *Elements* to aid in understanding of the proof, but does not intend these references to suggest anything about what elements, if any, Archytas had access to. [351]

Yet he employs the identification of such references as a basis for a rather extended discussion of the starting points assumed by Archytas and of the nature of the 'geometrical elements' accessible to him. However, singling out such references tendentiously skews the ensuing discussion: the implicit reference to 'elements' is not only given prominence by the rewriting that the proof has been subjected to; but is also taken explicitly for granted by Huffman, who loads the solution with an interpretative structure that can be properly assigned only to the author of the text we read, not to Archytas. As a consequence, Huffman is lead to see 'elements' where we are not entitled to see them: we cannot assume that Archytas was 'thinking by elements' when devising and writing down his solution, simply because we have no idea of the way in which Archytas' proof was originally formulated. The only evidence on which the whole discussion rests are Proclus' testimony (itself based obviously on a chain of epitomes) about the existence of pre-Euclidean collections of 'elements' ascribed to Hippocrates, Leon, and Theudius [Friedlein 1873, 66.7–8, 66.20–21 and 67.14–15] and the parallel with allegedly analogous features of Hippocrates' proofs. The latter reduces in fact to a single sentence of Eudemus/Simplicius where it is said that Hippocrates took

> as a starting point and assumed as first among the [results] useful for them [*scil.* the quadratures] that similar segments of circles have the same ratio to one another as their bases in power have (and this he proved by proving that the diameters have the same ratio in power as the circles). [Diels 1882, 61.5–9]

Such an emphasis on starting points in a sentence deriving from a pupil of Aristotle is grounds for scepticism: it is entirely possible that it is simply a product of Eudemus' reading of the Hippocratean

achievement, and that nothing should be inferred from it about Hippocrates' starting points, if he assumed any, in his quadratures.

In short, the whole discussion of the 'elements' in Archytas' text is a historiographical artifact whose real motivation appears to lie in the mere fact that there is some secondary literature perceived as authoritative discussing it. The same must be said of the final page of the section dealing specifically with Archytas' proof. This is a discussion of a natural but totally conjectural connection between the discussion about the doubling of the square in Plato's *Meno* and Archytas' solution of the problem of doubling the cube. Of course, Huffman rejects such connection as resting on no evidence, but employing even one single page to discuss such pointless lucubrations is a way to perpetuate them and to give to such minor products of scholarly romance a prominence that they by no means should have. An important scholarly achievement such as this edition of the Archytan remains should have made itself less dependent on other works of secondary literature, even when technical features are at issue.

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Miracles in Enlightenment England by Jane Shaw

New Haven/London: Yale University Press, 2006. Pp. x + 244. ISBN 0–300–11272–6. Cloth $45.00, £25.00

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In this lucid and engaging book, Jane Shaw argues that during the war of ideas in revolutionary and post-revolutionary England the debate concerning miracles was not the result of abstract, intellectual theorizing. Rather, in her narrative, she privileges the large number of religious sects that appeared during the 1640s and 1650s which emphasized the ability of certain individuals to have unmediated contact with God; this new phenomenon then produced a proliferation of miracles, especially miraculous healing. This social reading of events she calls the study of 'lived religion' rather than 'popular religion'; 'lived religion' is an anthropological term that she has borrowed from historians of American religion such as David Hall [see 1997] and Robert Orsi [see 1985], and it is defined by Shaw as beginning 'with practice in the dynamic sense' and then proceeding 'to draw out the theological (and other) meanings of that practice within a specific context' [10]. By focusing on the reality of religious practices as far as is possible, she then demonstrates that doctrine both emerges from and informs practice. Contested practices were debated by participants and their audiences, both of which included intellectuals. Shaw's thesis is validated by her thorough use of case studies and a cogent discussion of the flaws within many of the secondary sources.

This is an ambitious book because scholars of intellectual history have tended both to ignore lived religion and to take the Protestant doctrine of the cessation of miracles at face value. Shaw's privileging of lived religion means that she is able to explain the importance of religion to all strata of society on an everyday basis. Within this context, it then becomes apparent that early modern miracles had a far wider audience than might have hitherto been thought. At the centre of this debate were radicals and Independents interested

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in miracles as a validation of their sects; Latitudinarians interested in the boundary between reason and revelation, the natural and the supernatural; and those such as the Presbyterians who disavowed miracles on theological grounds. Beyond this centre was an outer ring of participants that operated within the public sphere, people who read and discussed pamphlets and visited miracle sites. The wider the audience, she argues, the wider the spectrum of opinions was concerning miracles; and thus she demonstrates convincingly that there was an intra-Protestant debate concerning whether the age of miracles had passed. Whilst the official Anglican view maintained that the age of miracles had passed, this debate was largely unresolved, she argues, and was partly the result of an anxiety concerning the Popish nature of miracles. She therefore cautions against confusing prescription with practice.

Her book is divided into eight chapters. The first sets the context for her study. The second, 'Protestantism and Miracles', discusses the debate concerning the cessation of miracles and the revival of healing practices according to Biblical injunction. She describes how the Baptists, by rejecting Roman and Anglican customs, created new liturgical healing practices: ritual was rigorously based on scripture (James 5); the site was the home or the place where a small congregation gathered, and not a church or shrine; and the object was either the holy oil or the Word in the material form of the Bible. Thus were miracles recast in a Protestant sectarian image.

The third chapter, 'Miracle Workers and Healers' discusses the Quakers and the royal touch ceremony whereby the English sovereign healed scrofula (known as the 'King's Evil') by the ritualistic laying on of hands. Quakers are contrasted with Baptists, and seen to be less organized and not following scripture with equal rigor; they are mystical and claim power directly from God; and so were criticized for undermining the resurrection and for appearing Popish. The analysis of the royal touch relies on earlier treatments by Raymond Crawfurd [see 1911] and Marc Bloch [see 1973], and like many scholars Shaw emphasizes the political value of the ceremony to the restored Charles II. Her account of the decline of the ceremony focuses on James II's return to a more Roman ritual, and the undermining of divine right ideology that occurred in 1688 and 1714, when the direct line of succession was broken. And yet politics depends upon a belief system, and so Charles II could only use the ceremony to his

advantage if enough people believed in the possibility of his curing their scrofula. Furthermore, it would be interesting to know whether any of those who supported the new regimes in 1688 and 1714 argued that providence was working through Parliament, which would then suggest in turn that the monarch could remain thaumaturgical. The popularity of the healing ceremony during the reign of Queen Anne also undermines Shaw's thesis with regard to 1688.

In her fourth chapter, 'Valentine Greatrakes and the New Philosophy', Shaw explains that Greatrakes, the Irish stroker and healer, was not motivated by sectarian imperatives but rather by altruism. However, he did address the issue of why 'God should now cure diseases in an extraordinary manner' [83] and gave three reasons: to prove His existence in an age of atheism, to be merciful to humankind, and to contrast true Protestant miracles with Popish shams. Greatrakes was a successful, high profile healer, and his work was investigated by the newly formed Royal Society, a process that took the debate beyond doctrine into natural philosophy. Shaw describes the disagreement between Henry Stubbe and Robert Boyle, and explains that the natural philosophers investigated Greatrakes *via* a process of scrupulous observation; and so the miraculous was treated as the physical. Boyle was present when Greatrakes healed a tinker, and even put on the Irishman's glove and stroked the tinker. The natural philosophers advocated the recruiting of honest, reliable witnesses as well as the recording of events in objective language as a method for collecting dependable evidence that proved the truth of the healings.

This is a stimulating chapter, but it is a shame that Shaw did not discuss the issues within the context of changing attitudes to magic. Boyle was presumably interested in the glove because Greatrakes wore it during healings, and because, as part of his empirical investigation, the scientist wanted to determine where the healing powers resided. She could also have said more about why the Royal Society prioritized the natural world over the supernatural, despite Joseph Glanvill's scientific investigations into the spirit world as recorded in his *Sadducismus Triumphatus* [1681]. Although some of its members were interested in the supernatural, the Society's prioritizing of the natural world was probably due to its understanding that magic was capricious, whereas nature obeyed rules and so was more discernible through observation. However, Shaw concludes that the corollary of this scientific debate was the discussion of the nature of God.

Chapter 5, 'Fasting Women', discusses female prophets, particularly the case of Martha Taylor, who continued to fast after the Restoration when radical sects were persecuted. Taylor was observed fasting in bed between 1667–1669 by various teams of women in order to ascertain whether this was a genuine case or a fraud, and Shaw connects her analysis to Laura Gowing's work [2003] on the body. Writing about pregnancy, Gowing argues that uncertainties concerning women's bodies led to the production of stories and claims that were an attempt to make sense of the mysterious, and Shaw demonstrates convincingly that this applied to the prodigiously abstinent female body too. Taylor did not claim to be performing a miracle; and aside from one comment from Hobbes, who wrote that it was for the church to decide whether the matter was or was not miraculous, the feat of endurance seems to have been understood as preternatural rather than supernatural. In other words, it was her remarkable body—literally a body of evidence —that seemed to fascinate visitors and observers, and not her piety.

'Perfectly Protestant Miracles', Shaw's sixth chapter, is a discussion of the cases that did claim to be miraculous, and of the importance of faith and reading the Bible as pre-requisites for the miraculous. Protestant miracles did not require intermediary figures, external trappings, or petitionary prayer; and were usually sited within the home. As such, they were earthbound miracles: that is, the power of God visited temporarily the everyday lives of Protestants as a result of their being the elect; and so this contrasted with Catholic miracles, which relied on trappings and intermediaries in order that earthbound issues could come to God's notice in heaven. Again, these remarkable events precipitated a discussion of the nature of God: James Welwood MD, discussing a healing, wrote that,

> if it be said why should God work such a miracle, if it be any?... I must own... that if I do not know all the secrets of Nature, I do much less know the secrets of the Author of Nature. [132]

The four case studies that Shaw discusses suggest, she argues, that there was a heightened awareness of the morality of those involved in healings, and hence of the nation too; and a profound interest in the relationship between reason and revelation.

Chapter 7, 'Miracles and the Philosophers', contains a discussion of the French Prophets, the sceptical reading of miracles as exemplified by Hume, and the apologists' response. Shaw accounts for the unpopularity of the French Prophets by referring to the emergence of polite society which viewed their extremism as vulgar. It is axiomatic that the 18th century experienced the emergence of polite society and so Shaw seems to understand magic and politeness as existing in a negative correlation. Yet she does not define or map politeness or describe its limitations, but seems instead to accept it as an absolute. Nor has she addressed the problem of politeness and Methodism, which saw belief in magic flourish in a sectarian Whig ghetto, and so undermines the politeness hypothesis.[1]

In her conclusion, Shaw describes how scepticism travelled from the domain of deists and free thinkers to a wider elite culture, but cautions in a healthy manner against understanding this as an orderly cultural division. Reactions to the Lisbon Earthquake in 1755 demonstrated that ideas regarding providence were very much alive. Key to Shaw's book is the argument that a range of attitudes towards miracles existed in mid 18th-century England, and that Hume's work appeared at a late stage in the debate. Arguing against a process of teleological secularization, she borrows from the sociologist David Martin and suggests a pattern of successive Christianizations and recoils.

This book is important because it makes a valuable contribution to the current reassessment of the Enlightenment. It provides great insight into the place of spiritual healing within English society at this time, and hence too into the discourse concerning faith and authority. Whilst it will appeal to those interested in ideas, science, and philosophy, Shaw is at her best when analyzing her case studies in lived religion. It is to be regretted that she did not use visual sources to enhance her work, and there is a factual error on page 147: *Sadducismus Triumphatus* was written by Joseph Glanvill, not Henry More. But this book engages with a fascinating subject, miracles in the Age of Reason; and it is to be hoped that it will inspire further work within this field.

[1] For her brief discussion of Methodism, see 178–179.

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Materials in Eighteenth-Century Science: A Historical Ontology by Ursula Klein and Wolfgang Lefèvre

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Perhaps the first thing to be said about this book is that it is not advisable to try to read it in a library, or indeed to read any other copy than one's own. Librarians (and friends) tend to frown on readers who scrawl 'YES' or large tick marks and arrows in the margin, or who disturb reading rooms with cries of enthusiasm. This book elicited all these reactions from me, so consider yourselves warned. The book sits firmly in the canon of scholarly works on the history of chemistry, as might indeed have been expected from these authors; and it should form part of the library of all who consider themselves historians of chemistry of whatever period. It offers an example of a new and exciting way of 'doing' history of chemistry, judiciously mixing sound historical study with what I would like to call analytical history and a good dose of philosophy of science. The methodology will be partly familiar to anyone who knows Ursula Klein's previous work, as indeed will some of the book's themes, but the drawing together of ideas stemming from over 10 years of research into this book length study extends their scope tenfold.

Broadly, the book offers a historical study of the changing ontological status of materials (substances) in the chemistry of the 18th and 19th centuries. But beyond this, it also explores the very nature of the chemical substance: where such entities originate, how they are constituted and, once individuated and identified, how they are classified. The notion of substance has been insufficiently studied in the past by historians, and philosophers of science have tended to approach the question from a somewhat metaphysical point of view. This work instead examines the science of materials, chemistry, in the 18th and 19th centuries when a concept of substance that we might

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recognize was initially formed. Rather than taking that somewhat overused route into the study of any science, the study of its sociological networks, this work is clearly intended to be about the science itself. Although it is concerned to emphasize the artisanal origins and medical/pharmaceutical roles of the substances which form the work's focus, all these are delineated through the practices and concepts that chemists adopted in order to understand such substances. This is a book about science in history, not scientists in history; and it is all the better for it. Similarly, the authors have chosen not to adopt the usual present-centered focus on systems of ultimate elements or particles, but instead to look at 'the most significant scientific objects of classical chemistry—chemical substances' [9]. The book thus examines a level of theorization that differs from more metaphysical philosophical systems, instead dealing with questions relating to the material, perceptible world: how substances interact and how they can be ordered and related to each other. This kind of theory has recently begun to be recognized and explored by historians of chemistry and, in particular, by those who have examined the doctrine of affinity that was so prevalent in 18th century chemistry [see, e.g., Kim 2003]. Nevertheless, the epistemological status of this level of theory has not so far been explored thoroughly. Klein and Lefèvre argue that this kind of theorization originated in the early years of the 18th century with the new conceptualization of the combination of pure chemical substances to form compounds and their corresponding analysis. As the authors show here too, classification systems also encapsulated ideas about the materials studied by chemists and thus offer access to this level of theorization about material substances.

The book is divided into three parts with 16 chapters bookended by an introduction and conclusion. It is extremely densely written—there is a huge amount of information contained in every line; nay in every phrase. Those who have read Ursula Klein's previous publications will know that her work is difficult to paraphrase simply because it is so thickly textured. This is not a book for merely dipping into; it must be read from end to end. Although each of the three parts can stand alone, each chapter is carefully structured so as to build on the work done by the previous one, and the journey taken by the reader as he follows the authors' arguments through each part is not one that could be taken by short cut.

The first part of the work is occupied with a discussion of the historical and philosophical status of the materials and substances studied by chemists and characterizes their various approaches to the study of these bodies. It also sets the work itself in its own historiographical and philosophical context. The latter is important, as the work as a whole, and particularly part 1, is deeply informed by philosophy of science as well as by history, referencing (and often disagreeing with) philosophers such as Foucault, Hacking, Bachelard, Rheinberg, Pickstone, Putnam, and, of course, the omnipresent Kuhn. What becomes clear is that although philosophers have explored a variety of scientific objects through studies of taxonomies and theories of reference as well as through scientific approaches to the notion of substance, they have rarely, if ever, approached either topic from a chemical point of view. Klein and Lefèvre seek in this work to rectify this omission by focusing their attention on the objects of chemical enquiry, the substances themselves, as 'multidimensional objects of enquiry'. They show that, beginning in the 17th century, 'academic chemists' studied materials in a variety of different ways: as applicable, useful materials; as perceptible objects with perceptible properties; and as carriers of imperceptible features.

The authors have seized here on a whole domain of practices that have so far been relatively unexplored. They seek both to define this domain—an act for which I think future historians of chemistry will have cause to be grateful—and to explore the activities and practices that characterize it. The domain they describe lay between (and often overlapped with) the two often denominated 'natural history' and 'experimental philosophy'. The first of these tended to involve the observation, description, and ordering of natural objects and phenomena, while the second entailed the experimental investigation of the imperceptible entities that underlay the visible world with the aid of a variety of philosophical instruments. The third domain described by Klein and Lefèvre (which they call, after Bacon, 'experimental history') was concerned with the collecting and ordering of facts relating to 'the perceptible dimension' of phenomena obtained by the intervention in nature. This Boyleian style of investigation was deliberately free from speculation and connected closely with artisanal activities and practices. This, together with technological improvement and experimental philosophy, describes the three

different styles of experimentation and observation adopted by 18th century chemists in their studies of materials.

Klein and Lefèvre are also concerned to expose the strong material connection between early modern chemistry and the practices of artisans and craftsmen. As they point out, historians of chemistry have not previously shown much interest in the provenance of the substances that appear in chemists' laboratories. For the authors this is an important omission. Substances did not arrive in the laboratory with a clean slate; they had a past and chemists' approaches to them were in part colored by that past. Most of the substances studied by 18th century chemists were commodities, products of artisanal processes; many were imported from foreign lands and used in manufacture of a variety of goods. These substances, they argue, were boundary objects, linking academic chemists with apothecaries, metallurgists, mineralogists, assayers, and so forth; but they were also multidimensional objects of inquiry with different aspects that were studied in different ways. However, they also argue that as chemists studied and investigated substances, so these substances began to be changed:

> Materials were... transformed when they became objects of inquiry for academic chemists. Chemists invested them with new meaning, and sometimes even transformed their boundaries by splitting them into different kinds of substances. New individuations and identifications of substances—such as the division of air into different kinds of air—went hand in hand with material transformations. [19]

This book sets out to explore the patterns of such changes from the 17th into the 19th centuries; but rather than exploring these 'shifting ontologies' through a focus on specific substances or materials and the changes in their ontological status, the authors have chosen to take a broader, more general view. Classification systems, they say, reflect chemists' understandings of the materials with which they work, providing access to

> what kinds of objects were handled in scientific practices of the past, how historical actors conceived of these objects, and how they selected and highlighted those of their manifold features they considered significant. [9]

Classification systems also govern the division of the historical part of the book into two separate parts, the first [part 2] covering the predominantly mineral substances that Klein and Lefèvre denominate 'pure chemical substances', while the second [part 3] examines the taxonomies applied to vegetable, later 'organic' substances. The notion of 'pure chemical substances' will be familiar to those who have read Klein's early works on affinity tables [1994, 1995, 1996] and here she builds on those studies very effectively. The authors point to a new concept that emerged at the beginning of the 18th century that established a 'conceptual network' linking 'concepts of chemical compounds, separation or analysis, recomposition or synthesis, and affinity in new ways' [48]. This new network of concepts underpinned the tacit demarcation of 'pure chemical substances' as those substances which could be combined together to form compound substances and then reliably be recovered from such combinations (by the informed manipulation of the affinities between substances) from those substances (like the majority of plant and animal substances) which, once decomposed, were not resynthesizable. The authors' examination of chemists' classification of this relatively small group of substances shows that it was in this class of substances that what can (perhaps a little whiggishly) be recognized as a modern concept of analysis became evident in chemical practice. From the first few decades of the 18th century, these substances were identified, individuated, ordered, and classed on the basis of their composition; and as Klein and Lefèvre show, both the pre-revolutionary affinity tables and the revolutionary *Tableau de nomenclature chimique* adopt the same taxonomical structure based on composition.

This comparison of pre- and post-revolutionary taxonomies has obvious implications for the historiography of the chemical revolution. Conventionally seen as a rupture, in Kuhnian terms a revolution, they show that although the new chemistry did indeed instigate nomenclatural reforms, these were built on a presupposed classification that had been tacitly used for these 'pure chemical substances' since the early years of the century. Taking Kuhn's own pointer as a guide to incommensurability, they perform an astonishingly deep analysis of the assumptions and logic that underpinned the *Tableau de nomenclature chimique* as it appeared in the 1787 *Méthode de nomenclature chimique*, a work that many have argued was the manifesto of the new chemistry. They compare the taxonomic structure

evinced by the *Tableau* with that of the tables of affinity that had proliferated since the middle of the 18th century, showing that 'the kinds of the one system are directly translatable into the kinds of the other' [185]. Where other historians have sought to map particular substances pre- and post- chemical revolution, and have encountered problems of reference and translation, Klein and Lefèvre seek instead to map taxonomical categories represented in the tables of affinity onto the categories represented in the *Tableau*. They therefore offer a new approach to the vexed question of whether phlogiston can be mapped onto a single substantial entity of the anti-phlogistonist taxonomy in a consistent and coherent way. In this regard they claim that phlogiston was *the phlogistic counterpart* [180] of oxygen and *calorique*. This claim seems to be based on the operations and processes in which phlogiston was combined with or separated from other pure chemical substances—the resulting classes of compound substances are shown to be a mirror image of those formed by the addition or removal of oxygen or caloric. This is why the practices associated with blowpipe analysis remained essentially unchanged before and after the chemical revolution; the addition or removal of phlogiston from mineral substances that formed the basis of this kind of analysis was achieved by the same practices that added or removed oxygen, albeit in reverse. The classes of phenomena produced were also the same in pre- and post revolutionary blowpipe analysis.[1] The authors characterize the classification change that took place during the chemical revolution as 'not the result of a change of the mode of classification but rather a change in the existing mode of classification' [67]. There was, therefore, 'no ontological rupture'. Thus, they succeed in putting flesh on the bones of the nagging doubt (to mix metaphors somewhat) which must be familiar to most historians of 18th century chemistry that pre- and post-revolutionary chemistry were not, in taxonomic terms at least, incommensurable. The significant changes that they pick out occurred instead in the early years of the 18th century as the new network of concepts of compound, analysis, and affinity emerged and in the 1830s, when a compositional or constitutional approach to the classification of what by now were called 'organic' substances became feasible.

[1] The author gave a paper exploring this point at the Annual Conference of the British Society for the History of Science at Manchester in 2007.

The work demonstrates clearly how much historians can gain from a close, even a micro-reading of certain texts. Tables are logical structures, built on complexes of assumptions and encapsulating a network of theories and ideas. The kind of analysis carried out by Klein and Lefèvre in part 2 offers an example of how such a structure can be logically analyzed to glean valuable information about the compilers' understanding of the bodies being ordered. This is almost a combination of history and science in action, and it is an example that historians can learn a great deal from. Historians of science have always wrestled with the problem of how to gain access to the most fundamental assumptions that underpinned the practice of science. These assumptions and beliefs are, for the most part, so basic that they remain unarticulated. The analysis of logical structures like tables can, as Klein and Lefèvre show in this work, offer a way to gain information on the kinds of tacitly held beliefs that is only rarely perceptible from more conventional texts.[2]

Much of part 3 will be largely familiar to readers who have encountered Klein's recent papers on the classification of plant substances [2005a, 2005b]. This part of the work focuses on chemists' attempts at ordering, individuating, and classifying plant substances, most of which were excluded from the class of 'pure chemical substances'. These substances could not be reliably manipulated, and as the authors show, they were ordered and classified throughout the 18th and well into the 19th centuries on the traditional basis of perceptible properties. As they also make clear though, this is not to say either that analysis was not carried out on these kinds of substances, nor that the taxonomy of these materials was static. Their study indicates that analysis was indeed carried out on plant substances but that this was a different kind of analysis in terms of its objective, methods, and meaning. Prior to the middle of the 18th century, the term 'analysis' was only rarely used with regard to plant substances; and in the few cases when it was mentioned, it was used to indicate the acquisition of knowledge in a theoretical rather than experimental context. From the mid-18th century though, the authors point to the increased emphasis on, and study of, the relatively compound, 'proximate principles' of plants which, they argue, was driven in part

[2] A similar kind of logical analysis can be found of the assumptions that underlay affinity tables in Taylor 2006, ch. 5.

by the focus on composition prevailing in the study of the pure chemical substances. These proximate principles became the substances that were identified, ordered, and classified as plant substances, although the mode of their ordering remained the traditional one of perceptible properties. It was only in the 1830s, as the authors very briefly show, with the work of Dumas and Boullay that a similar mode of classification on the basis of a binary composition could be instituted for what were now known as 'organic' substances. Where part 2 demonstrated that the ontology of pure chemical substances remained surprisingly unchanged in a deep sense throughout the 18th and into the 19th century, part 3 shows that although the mode of classification of plant substances remained similarly static throughout the period under consideration, the broader demarcation of what were originally denominated plant substances but later became organized bodies and eventually organic substances, shifted regularly. Throughout the period, what was considered as one class or species of plant substance changed, and substances were included or excluded as the ontologies shifted. These shifts cannot be described as revolutionary or as ruptures, but they did reflect deep changes in the conceptualization of substances emanating from the vegetable realm.

This part in particular emphasizes a number of subtly distinct processes that were necessary (but not sufficient) conditions of the creation of a chemical classification. One such process is the demarcation of the substances to be classified from those that, for example, were of the wrong ontological order or had originated in the wrong place or had been submitted to the wrong processes or were groups rather than individuals. Part 2 explored the demarcation of the 'pure chemical substances' from other bodies, but part 3 points to a number of shifts in the groups of substances that were classed as 'plant substances' or 'proximate principles of plants' or finally as 'organic substances'. Beyond this there is the difficulty of individuation. This is a problem rarely considered by historians or philosophers of science, but which lies at the heart of many of the ontological shifts described by the authors. How did the chemist decide that the substance with which he was dealing was just one homogeneous substance? Once again, in part 2 the authors showed how this problem was dealt with in regard to pure chemical substances, where the homogeneity of each substance was built into the initial classification system. The contrast with the messy and often incoherent individuation of

plant substances is clear. Further beyond still is the more familiar philosophical problem of identification; connected to the problem of individuation, this was again a key concern for chemists of both the 18th and 19th centuries. As plant materials could not be decomposed and resynthesized in the same way as the pure chemical substances could, all these classification processes were much more problematic when applied to vegetable substances.

This book is important for the history of chemistry in so many ways. It sets a historiographical, methodological, and philosophical example for historians of science in general as well as historians of chemistry. It demonstrates the advantages that can accrue to both history and philosophy of science by adopting an approach that is becoming known as 'integrated history and philosophy of science'.[3] The historical case studies which make up the heart of this work are thoroughly informed by philosophy of science, and indeed many of the questions being asked by the authors originate in that discipline. At the same time, the historical study and, in particular, the analysis of the taxonomical structures of chemistry are seen to offer answers to these questions which in turn must color our acceptance of a number of philosophical generalizations. History of chemistry has to date been somewhat under-utilized for historical case studies; and, as the authors show, such historical studies have much to offer to philosophers of science. The authors use their own historical investigations to show that, so far as chemistry of the 18th and 19th centuries is concerned, neither Foucault's epistemes, Bachelard's rupture, nor Kuhn's revolution provide appropriate models.

Their study also demonstrates how the history of chemistry can be brought into the 21st century, while leaving most of the present-centeredness of our century behind. Debunking a number of myths that have been too long propagated by historians of chemistry in thrall to the atom- and element-obsessed present, this book emphasizes the materiality of chemistry, and the role of tangible, sensible, physically manifest substances, the objects of study of chemists of the 18th and early 19th century. The authors avowedly state in their introduction that their intention is not to follow the conventional route of histories of chemistry, focusing on particles, Newtonian forces, and eventually atoms. Nor indeed do they spend much time on systems

[3] See Chang 2004, particularly the introduction and chapter 6.

of elements or principles, at least no more time than did their protagonists. This is a new direction for the history of chemistry (although Klein's earlier work has already led us a little way down this path) and it is to be wholeheartedly welcomed. Their work fulfills a long outstanding desideratum by focusing instead on the materials, the substances, with their tangible and not so tangible properties such as color, taste, smell, acidity, alkalinity, medicinal value, solubility, inflammability, and so on. This was, of course, how most chemists looked on the substances with which they worked, and for too long historians have chosen to ignore this fact in order to concentrate on speculative hypotheses of particles, atoms, and primitive principles. Such hypotheses were present throughout the 18th and into the 19th century, of course; but, as Klein and Lefèvre show, from the middle of the 18th century, in terms of what the majority of chemists actually did, they were far less relevant than the classificatory structures that chemists adopted. Even though most chemical textbooks paid lip service to one or other such elementary system, this seems to have been dictated more by convention than by actual chemical practice. Klein and Lefèvre emphasize that in spite of the elementary rhetoric, most chemists were in fact more concerned with the proximate principles or pure chemical substances that they could get by analysis (whether in the modern sense or in the older, less familiar sense), and with which they could make new substances. Accordingly, it is with the endeavors to identify and classify those substances that were subject to this order of ontological decomposition and recompounding that they concern themselves in this book. Klein and Lefèvre's work offers an example to today's historians of chemistry of how their discipline can be enhanced by adopting not only their actors' categories, but also their chemical concerns.

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The Secret History of Hermes Trismegistus: Hermeticism from Ancient to Modern Times by Florian Ebeling. Foreword by Jan Assmann. Translated from German by David Lorton

Ithaca/London: Cornell University Press, 2007. Pp. xiii + 158. ISBN 978–0–8014–4546–0. Cloth $29.95, £15.95

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The work under review here is the first survey of 'Hermeticism' from antiquity until the present. No one has previously attempted such a comprehensive summary of this subject in the form of a book. The present review deals solely with the English version, translated from a German original that I have not seen [Ebeling 2005].[1]

After a foreword by Jan Assmann, the book is organized by chapters along an increasingly specific chronological framework: early origins and ancient *Hermetica*, followed by chapters on the *Hermetica* in the Middle Ages, in the Renaissance, in the 17th century, in the 18th and 19th centuries, and in the 20th century. The treatment is most convincing, and contributes the most, in the half of the book comprising the third, fourth, and fifth chapters: Hermeticism from the Renaissance to the Enlightenment is clearly the author's strength.

The subject is difficult because, as Ebeling acknowledges, historians have not succeeded in defining Hermeticism decisively. The ancient Greek texts attributed to the Egyptian sage Hermes Trismegistus had influence in several different times and places, but the nature of that influence varied widely according to the conditions and needs of those receiving the texts and their interpretations of them. Therefore, 'the goal is to offer an impression of the multiplicity of conceptual worlds handed down to us under the rubric of Hermeticism'

[1] I cannot see why the *Geheimnis* of the original title was rendered as 'Secret History' and not as 'Mystery', a word that has importance in Ebeling's treatment [104–107]. There is nothing secret about this history.

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without getting lost 'in exhaustive detail' [1]. These words at the beginning of the book not only alert the reader to the highly variable nature of this thing called 'Hermeticism', but also cast doubt on the coherence of the category forming the basis of the whole narrative. Ebeling states that his discussion will be based on works ascribed to Hermes or that rely on Hermes' authority, a commendable approach. What follows is, appropriately, in large part a history of reception, interpretation, and innovation. This initial definition of the project gives it validity.

However, a problem immediately arises in the application of these plans to the whole history. Ebeling has already begun with the assumption of a category of 'Hermeticism', as an 'ism', for all the periods and places treated, instead of following the lead of the texts. It is legitimate to speak of Hermeticism for 17th-century Europe, when philosophers, like Benedictus Figulus in 1608, could write of 'this Hermetic philosophy of ours ..., which includes true astronomy, alchemy, and magic, and also Cabala' [76]. In western Europe of this period, authors explicitly regarded their program as Hermetic as such. However, it is unsound for historians to borrow such terms from the 16th and 17th centuries and to apply them uncritically to earlier periods and other places. The results are confusing.

Chapter 1 presupposes that Hermeticism as such existed in ancient times. Ebeling asks 'What was ancient Hermeticism?' and 'What was the essence of Hermeticism?', and then goes on to explain how vaguely defined it must have been. Newcomers to the subject will perhaps benefit from the summaries of the contents of influential ancient *Hermetica* provided in this chapter; but, in asking such questions about 'Hermeticism', we have already lost sight of the cautious approach stated at the outset. Without much regard for chronology beyond the designation 'ancient', this chapter tries to paint a picture of who Hermes Trismegistus was thought to be and what the basis of 'ancient Hermeticism' was, and then finds it a difficult matter to discover consistency behind them. This difficulty signals problems in the categories and approaches employed.

The generalizations offered here about the *Hermetica* in antiquity are sometimes incorrect. For example, Ebeling proposes that 'in antiquity it was not important whether Hermes was a historical figure' [8]. In fact, Hermes Trismegistus appears in several Christian histories and chronicles as a historical figure, and his alleged

historicity and, in particular, his antiquity were an important part of Hermes' authority. Ebeling describes the supposed eclecticism of the Greek *Hermetica* by drawing from several (sometimes conflicting) theories held by previous scholars. Thus, the *Hermetica* present 'a conglomeration of Aristotelian, Platonic, Stoic, and Pythagorean doctrines, interspersed with motifs from Egyptian mythology and themes of Jewish and Iranian origin' [31]. This conclusion is obviously unhelpful, particularly for the beginner. Similarly problematic is the assertion that the intellectual climate of late antiquity can be 'characterized by an attitude of "anything goes"' [9].

Seeing the *Hermetica* as part of a vague, 'broad literary field' [35] and simultaneously as the basis of an ancient 'Hermeticism' distracts from the effort to locate the meaning of the ancient *Hermetica* either individually or according to a specific social context. Ebeling could have made better use here of Garth Fowden's standard work [1993] on the early *Hermetica* in their Egyptian *milieu* (cited in the bibliography). Emphasizing the amorphous and ungraspable character of ancient 'Hermeticism' only demonstrates the inapplicability of the early modern category to antiquity.

The treatment in chapter 2 of 'Hermeticism' in the Middle Ages is similarly problematic. We begin with an outdated and Eurocentric notion that the 'ancient world' came to an end in the sixth century and that 'after Clovis converted to Christianity, the geopolitical center of gravity shifted north of the Alps' (!) [37–38]. 'With the end of the ancient tradition, the survival of Hermeticism was endangered' [38]. After having been told in chapter 1 that ancient Hermeticism was such an ill-defined thing, what exactly can the reader think was endangered?

The description of medieval theologians' treatment of the *Hermetica*, based on those of the church fathers, is quite clear. Christian apologists such as Lactantius regarded the *Hermetica* as affirming Christianity, while other Christian writers treated the doctrines of *Hermetica* such as the *Asclepius* as potentially dangerous. These views were formative for the later reception of the Greek *Hermetica* in the Italian Renaissance.

The treatment of Arabic 'Hermeticism' (though there is no such word or concept in premodern Arabic) is a valiant effort, given how little of the relevant material has been published. Fortunately, Ebeling

has excellent German scholarship, such as that of Manfred Ullmann, for a guide. In these eight pages we catch a glimpse of a few important Arabic *Hermetica*. Ebeling briefly describes the widespread Arabic myth of three separate ancient sages named Hermes. Unfortunately, there is no sense of the meaning of these texts to their Arabic-speaking audiences across North Africa and western Asia. They are not treated as a part of any social or historical context. There is no reference to the important recent studies of the Graeco-Arabic translations of the eighth to 10th centuries, the background against which the appearance of Arabic *Hermetica* must be understood. By contrast, the medieval Latin *Hermetica* under discussion are situated in a chronological and intellectual context.

There are, in fact, many more works of Hermes in Arabic manuscripts than those discussed in this survey. The real problem here is not, however, the lack of information, for which a specialist in European languages might not be held accountable, but rather the narrative treatment of the Arabic *Hermetica* as just a 'medieval' phenomenon, important only in so far as they came to the attention of later Europeans, an incidental step in the transition from the ancient to the Renaissance *Hermetica*. Ebeling thinks that 'few of these [Arabic] texts were of lasting effect and enduring significance for western Hermeticism' [49]. Scholars have not yet proven this to be the case. Moreover, the tradition of Hermetic texts outside of Europe, parallel to and contemporary with the Hermetic movement in later Europe, awaits further research. But, in so far as this is an introductory survey that is heavily reliant on earlier studies similarly focused on western Europe, it is not the source of the oversight, though it demonstrates the imbalance in the scholarship.

Once we get to Italy in the 1460s [chapter 3], the book is on much firmer ground and becomes an excellent treatment of its European subject. The coverage of Ficino's translation and interpretation of the Greek *Hermetica* is clear. Ficino understood them in the context of the pre-existing medieval Latin interpretations of Hermes. The influence of the Greek *Hermetica* on such philosophers as Pico, Bruno, and Patrizi comes under discussion. Yet here Ebeling is, with good reason, less willing to describe automatically anyone who read the *Hermetica* as a 'Hermetist'.

In Germany, during the 16th and 17th centuries, the alchemical movement of Paracelsus developed, drawing on the authority of Hermes for its legitimation. These alchemists called their practice the Hermetic Art, working mostly independently of the reception of the *Hermetica* in Italy.

> Notwithstanding some connections, neither the discourse of the *Ars Hermetica* nor its origin and theological and natural philosophical legitimation can be understood as deriving from Renaissance humanism. [70]

Here the Emerald Tablet, a short text translated from Arabic, became an important common point of reference. The followers of Paracelsus (d. 1541) saw him as a new Hermes, reviving the pristine natural philosophy. This movement reacted against the Aristotelianism of the schools and wanted to promote their true philosophy as more ancient and Egyptian in origin. Here at last we have a movement that is self-consciously Hermetic, a true Hermeticism. Ebeling hints at further Arabic sources for an important work of this movement, the *Liber Apokalypsis Hermetis* [81], but the connections between Arabic alchemy and German Hermeticism evidently will have to await future studies. The discussion of these German alchemists side-by-side with the Italian philosophers demonstrates two different Hermetic currents moving simultaneously in Europe. The northern current has not hitherto received much attention.

Ebeling also discusses the role of the *Hermetica* in Christian religion of this period. Specifically, several authors, such as Sebastian Franck (d. 1543), saw the *Hermetica* as a valid revelation on par with the Bible. Christianity could, therefore, be explained as a religion of nature in harmony with the teachings of Hermes, Zoroaster, and other sages. Ebeling sees this as part of an argument in favor of religious tolerance. How this could be so in the writing of Philippe de Mornay (d. 1623), one of Ebeling's main examples, is hard to say, given the title of his book *Treatise on the Truth of the Christian Religion, against the Atheists, Epicureans, Pagans, Jews, Mohammedans, and Other Unbelievers*. Nevertheless, Ebeling claims that Mornay was not 'concerned with distinction or exclusion' and 'promoted tolerance' thanks to Hermeticism [85–86].

Chapter 4, on Hermeticism in 17th-century Europe, deals primarily with the important scholarly critiques of the Hermetic movements of that time. Casaubon (wr. 1614) showed that the Greek *Hermetica* were dated to the early Roman period, not to the antiquity of the patriarchs. In 1648, Hermann Conring published a work attacking the claimed connection of the Paracelsian philosophers with Hermes. Criticisms like these attenuated the appeal of Hermes and his works. Meanwhile, as Ebeling shows, the 'Hermetic' alchemy remained, in the eyes of many, a basis for true science at harmony with their Christian beliefs. Theologians like Colberg (d. 1698) nevertheless attacked what they saw as a heretical 'Platonic-Hermetic Christianity'.

Chapter 5 deals with the 18th and 19th centuries, during which Hermeticism remained strongly identified with alchemy. At the same time, the society of the Freemasons, through the agency of figures such as Ignaz Edler von Born, adopted some of the imagery, language, and mythology of the *Hermetica*, including the Hermetic alchemy, as part of their own invented ancient heritage. Scholars of the 19th century included discussions of the *Hermetica* in an attempt to comprehend a universal, idealistic philosophy.

Chapter 6, the last and shortest, describes how, in the 20th century, the alchemical Hermeticism and its rich symbolism provided material for philosophers and literary critics to discuss affectedly symbolic or deliberately incomprehensible works of art and literature. The word 'Hermetic' comes to have its modern significances: it refers to alchemy, to veils of symbols behind which are mysteries or perhaps nothing at all, and to a supposed counter-current of 'irrational' philosophy. Critics like Umberto Eco adopt the term 'hermetic' as a part of their own technical vocabulary having little relationship to earlier applications of the word.

There is no conclusion, just a timeline to recapitulate the main points (assigning Abū Ma'shar, d. 886, to the eighth century). A few problems in the book have already been discussed. One might add a small quibble about unexplained jargon perhaps not appropriate for an introductory text, such as 'philosophemes' and 'theologoumena' (not in the glossary at the end). Other faults of the work are not the author's. The translator has clearly distorted some proper names. More serious is the decision of Cornell University Press not to require a fuller scholarly apparatus, including fuller documentation, or

a more extensive guide for further reading of the kind appropriate to an introductory scholarly work. The select, general bibliography lists only 21 items, mostly in German, though some more references for specific points can be found in the footnotes to the text. This is a symptom of the current general trend of American university presses toward popularizing their work at the expense of learned content. Ebeling's own extensive research does not get the credit that it deserves.

There are two main contributions of this book, both praiseworthy. The first is its attempt at a comprehensive survey of literature associated with Hermes. Despite the shortcomings entailed in its realization in the first part, there definitely is value in looking at the entire history of these texts. In this regard it provides something otherwise unavailable. The second, and more important, contribution is the discussion of German Hermeticism and the incorporation of the Paracelsian, alchemical current into the overall narrative. Both students and scholars will benefit from this aspect of the work. I would readily use chapters 3–5 as readings in an introductory course on the subject. The fresh collection of information on European Hermeticism may spur new research. It is hoped that Ebeling will provide another, and much more detailed, study of early modern German Hermeticism.

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Philosophy in the Roman Empire: Ethics, Politics and Society by Michael Trapp

Aldershot, UK/Burlington, VT: Ashgate, 2007. Pp. xiv + 285. ISBN 978–0–7546–1618–6. Cloth $99.95, £55.00

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Philosophy in the Roman Empire addresses philosophy (*philosophia*, as Trapp likes to refer to it, to remind us of its immigrant status in Rome) as an aspect of Imperial culture [x]. Its principal focus on 'ethics, politics and society' makes for a slightly artificial sense of the range of philosophical activity at this period (especially striking is the absence of cosmology and metaphysics, not least because it effectively marginalizes the Platonist revival, which is arguably the most distinctive and influential product of Imperial philosophy). Nevertheless, it also allows Trapp to focus on writers unduly neglected by many philosophical histories—e.g., Dio Chrysostom, Maximus of Tyre, and the Neopythagoreans. Furthermore, it establishes limits within which Trapp is able to develop a narrative that keeps philosophical doctrine and social context in close dialogue with each other—something he does to great effect.

The topics covered in the book reflect Trapp's conviction that it is the Stoics who set the agenda for philosophy in the Roman period [cf. esp. 144]: ethics, emotions and their control, selfhood, interpersonal relations, and political theory. In each case (not to make things sound too formulaic) the prevailing pattern is that Trapp sets out the issue, breaks it down as necessary, and explores under distinct headings how it was treated by the various schools and individuals of the time. The influence of scholars such as Miriam Griffin (on Seneca), A. A. Long (on Epictetus) and R. K. Sorabji is palpable, but so is Trapp's own familiarity with his wide-ranging material, which he surveys in a very assured and elegant manner. Trapp's partiality for the Stoics, although expressed as a dispassionate historical thesis,

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is in the end not furnished with any evidence independent of his Stoicizing selection of topics; and the fact that other schools, especially and strikingly the Epicureans, end up with so little to say, and that of such little value, suggests that the story could have been told very differently. In one respect, too, it seems to me that Trapp does not give full credit even to the Stoics, and that is in ascribing to them a decidedly dualistic anthropology which lands them in some philosophical embarrassment. In developing this dualism, Trapp reflects well enough the fact that our texts talk about the superior value of the soul over the body. But if one thinks of the soul as something like the life that the body has, this need not mean more than that *what we do with the body* is more important than (mere) corporeal integrity. It might, to this extent, be misleading to think that the Stoics ask us to value one entity (the soul) above another (the body). The difficulty that Trapp's position leads to lies in the Stoics' claim that altruism has the same basis as our natural instinct to self-preservation. Trapp is inclined to see here an attempt to reconcile the irreconcilable: roughly, the demands of soul and body, respectively. It is (he thinks) not to the credit of the Hellenistic Stoics that they overlooked the point; and the adherence to their position by Seneca and Epictetus can be no less than 'willful blindness' [141]. But perhaps, after all, the Stoics had a perfectly coherent way of saying that one cannot feel at ease with oneself if one's behavior is at odds with the world (including the social world) of which one is a part? The inclination to self-preservation for a rational creature should, in this case, be an instinct to preserve oneself as a creature that behaves in a certain way—a way which crucially includes treating other people as no less intrinsically valuable than oneself.

Trapp is, however, surely right to think that Seneca and Epictetus, blindly or not, are in close conformity with the Hellenistic school; and in general that 'Roman' Stoicism (and Epicureanism, and Cynicism) differs hardly at all in philosophical substance from the schools of the second century BC—or even the fourth [e.g. 63, 96]. It is to his credit that Trapp does not fight shy of the conclusion, even if it leads him on occasion to the rather desperate expedient of soliciting our interest precisely in the *static* nature of the debates [74–75]. But not all is stasis, as we discover in the last chapter, which deals with the place of *philosophia* itself in society. Here Trapp discerns an interesting failure in alignment, peculiar to his period, between the

language and values of *philosophia* and the norms of the audience it wishes to educate. Useful though the doxography of the earlier chapters will be found, his remarks here will surely constitute the book's most valuable contribution to scholarship—for they address in very lucid terms one of the most serious challenges to our understanding of philosophy at this period.

Trapp considers the dissonance that he identifies in narrowly social terms as an attempt on the part of philosophers to establish a 'detached vantage point' whose defamiliarizing language enabled its adherents to develop 'a principled mistrust of the ordinary' [233]. What might perhaps deserve more emphasis is the way in which this vantage-point is achieved precisely by the conservatism of the debates, the philosophical stasis, described in the earlier chapters of the book. It is, after all, the failure of philosophical texts to keep up, as it were, with an evolving social context which leads to the friction in which Trapp is here interested. Not only does it help to elucidate the two principal phenomena addressed by Trapp's discussion to make this connection, it also suggests a way of relating them to a characteristic obsession of Imperial philosophy with its own history. The reasons for this obsession are reasonably well understood [cf. Hadot 1987, Sedley 1989]: it is to do with the fact that philosophers in the Empire were operating without the benefit of the living Hellenistic institutions which for centuries were the reference-points for philosophical identity and orthodoxy. A post-Hellenistic philosopher who wished to establish his credentials as a Stoic, for example, or an Epicurean, had only one way to look, and that was backwards. This dynamic fits very well with the idea that their texts turn out to be uncomfortable and defamiliarizing because of their anachronism: they comment on their own society precisely by tracing the distance between themselves and the past by which they too are validated. An interesting case-study in the kind of dialogue that results may be found in the surviving *Epitome of Greek Theological Traditions* by the first-century Stoic Cornutus. Cornutus in this work is addressing himself to the education of a *child* through the study of *ancient* religious traditions. (His very first words make a programmatic juxtaposition of ancient material and a youthful recipient—who is not just παῖς but emphatically παιδίον.) Much of the work involves the recognition that the philosophical roots of these traditions in distant

antiquity have been obscured by every kind of corruption and accretion in their subsequent transmission. But the conclusion is that it is precisely by becoming conscious of our own distance from the purer theology in which the tradition originally took its rise that we can benefit from it [Lang 1881, 76.9–16]. Of course, in most philosophical writing, the defamiliarizing historical gap is a structural feature, not something thematized in this way. But it is a structural feature of Cornutus' work *as well*. There is the usual conservatism of doctrine; there is also, in this case, the fact that the work is dominated by traditional Greek material, although one might imagine that its ostensible recipient was a Roman child (Cornutus worked at Rome, after all). If this were not enough, Cornutus explicitly represents his work as a summary account of work pursued more fully in earlier (*scil.* Hellenistic) studies [Lang 1881, 76.7–9].

In general, then, there is reason to think that the gap Trapp identifies between the Hellenistic continuations and their social environment is not *just* a gap (as the conservatism of doctrine by which it is created is not *just* conservatism): it is not any old defamiliarization that it offers. It is a gap that calls attention specifically to the cultural past of the readers of this material. This, quite specifically, may be what gives them a means of standing outside of their own society in order to understand it the better.

A final remark on this point. The tools for the kind of engagement envisaged may be as much literary as philosophical, just in so far as it involves the interplay and reception of earlier texts. One thing worth emphasizing, then, is the astonishingly rich overlap of literary and philosophical activity we find in the Roman period. The 'Second Sophistic', with which Dio Chrysostom and Maximus of Tyre are often associated, constituted a major renaissance of literary philosophy; the literary output of Plutarch and Seneca hardly needs comment; Cornutus, to whom I have just appealed as a philosopher, was also a grammarian, commentator on Virgil, and tutor to Persius, one of the major poets of his age. One could go on. These connections are important enough if one is interested in the cultural and educational context of philosophy (it is relevant that Seneca and Cornutus were active in a lively intellectual scene at the court of Nero). But beyond this, the Imperial period above all shows, if it needed showing, how the tools of literature can subserve rigorous philosophical argument and debate. Without them, as Trapp goes

some way to demonstrating, the texts of this period will seem more naive, philosophically, than a principle of charity can bear.

The Imperial period is a difficult and multifaceted area which remains very much work in progress for historians of philosophy. Trapp's study of ethical thought in the continuations of the Hellenistic schools and derivative contexts provides us with a reference work of lasting value—and much food for further thought.

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The Development of Mathematics in Medieval Europe by Menso Folkerts

Variorum Collected Studies Series CS811. Aldershot, UK: Ashgate, 2006. Pp. xii + 340. ISBN 0–86078–957–8. Cloth.

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This is Menso Folkerts' second Variorum volume. The first was published in 2003 [see Høyrup 2007b for a review]; it contained papers dealing with the properly Latin tradition in European mathematics, that is, the kind of mathematics which developed (mainly on the basis of *agrimensor* mathematics and the surviving fragments of Boethius' translation of the *Elements*) before the 12th-century Arabo-Latin and Greco-Latin translations. This second volume deals with aspects of the development which took place after this decisive divide, from *ca* 1100 to *ca* 1500.

Few scholars, if any, know more than Folkerts about medieval Latin mathematical manuscripts. It is, therefore, natural that the perspective on mathematics applied in the papers of this volume is on *mathematics as a body of knowledge*, in particular, as it is transmitted in and between manuscripts. To the extent that *mathematics as an activity* is an independent topic, it mostly remains peripheral, being dealt with through references to the existing literature—exceptions are the investigations of what Regiomontanus and Pacioli *do* with their Euclid [in articles VII and XI]—or it is undocumented, as when it is said that Jordanus de Nemore's *De numeris datis* was 'probably used as a university textbook for algebra' [VIII.413]. There should be no need to argue, however, that familiarity with the body of mathematical knowledge is fundamental for the study of mathematics from any perspective: whoever is interested in medieval Latin mathematics can therefore learn from this book.

It is more questionable that Folkerts tends to describe the mathematics which he refers to through their modern interpretation. To say, for instance, that the *Liber augmentis et diminutionis* shows

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'how linear equations with one unknown or systems of linear equations with two unknowns may be solved with the help of the rule of double false position' [I.5] does not help the reader who is not already familiar with the kind of problems to which this rule was applied to understand that the treatise contains no equations but problems which modern scholars often explain in terms of linear equations.[1]

Since many of the articles are surveys, they touch by necessity on topics outside Folkerts' own research interest. In such cases, Folkerts tends to mention existing disagreements or hypotheses instead of arguing for a decision (even in cases where one may suspect that he has an opinion of his own).[2] This is certainly a wise strategy, given the restricted space for each topic; but the reader should be aware that this caution does not imply that existing sources do not allow elaboration or decision.

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[1] For instance,

> Somebody traded with a quantity of money, and this quantity was doubled for him. From this he gave away two dragmas, and traded with the rest, and it was doubled for him. From this he gave away four dragmas, after which he traded with the rest, and it was doubled. But from this he gave away six dragmas, and nothing remained for him. [Libri 1838–1841, 1.326]

Seeing this simply as 'an equation' also misses the point that it may just as legitimately be seen (for example) as a system of three equations with three unknowns (the successive amounts traded with).

Actually, the treatise solves this problem (and many others) not only through application of a double false position but also by reverse calculation and by means of its *regula* (which Fibonacci calls the *regula recta*, first-degree *res*-algebra).

[2] In I.n13, it is said that the author of a reworking of al-Khwārizmī's algebra could be Guglielmo de Lunis. This hypothesis is quite widespread. It is not mentioned that the only two independent sources which inform us about a translation of this work (whether Latin or Italian) made by Guglielmo (Benedetto da Firenze and Raffaello Canacci, Lionardo Ghaligai depending on Benedetto), both quote it in a way which appears to exclude the identification of Guglielmo de Lunis as its author. I guess Folkerts knows both sources.

With one exception, all articles in the volume turn around the tradition and impact of the *Elements*, and/or the figure of Regiomontanus. Unlike many Variorum volumes, several articles are not published in their original form but have been rewritten so as to encompass recent results. In total, 12 articles are included.

I. 'Arabic Mathematics in the West'

This revised translation of a paper originally published in German in 1993 deals with the arithmetic of Hindu numerals, algebra, Euclidean geometry (*Elements*, *Data*, *Division of Figures*), spherics, and other geometrical topics (Archimedean works on the circle and the sphere, conics, practical mensuration). Given its brevity (16 pp.), this is obviously little more than a (very useful) bibliographic survey.

II. 'Early Texts on Hindu-Arabic Calculation'

This article (26 pp.), which was first published in 2001, falls into two parts. The first part (6 pp.) is a general survey covering the Indian introduction of the decimal place value system and its diffusion into the Arabic world, some of the major Arabic texts describing the system, the early Latin redactions of *Dixit algorizmi*, and the most important Latin algorism texts from the 13th and 14th centuries. The second part (17 pp.) is a detailed description of *Dixit algorizmi*, the earliest Latin reworking of the translation of al-Khwārizmī's treatise on the topic. Of this reworking, two manuscripts exist; the second one was discovered by Folkerts, who also published a critical edition [Folkerts 1997].

III. 'Euclid in Medieval Europe'

This is a completely revised version (64 pp.) of a paper first published in 1989. The first half of the article describes all known medieval European translations and redactions from Boethius until the mid-16th century; it also includes a brief discussion of the Arabic versions. The second half is a 'list of all known Latin and vernacular manuscripts up to the beginning of the 16th century that contain the text of Euclid's *Elements* or reworkings, commentaries, and related material'.

IV. 'Probleme der Euklidinterpretation und ihre Bedeutung für die Entwicklung der Mathematik'

This article (32 pp.) was originally published in 1980. An initial section covers the same ground as the first part of article III, but with more emphasis on the character of the various versions of the *Elements*. Sections 2 and 3 look at how late ancient as well as Arabic and Latin commentators and mathematicians concentrated on specific aspects of the *Elements*: proportion theory, the parallel postulate, the theory of irrationals.

V. 'Die mathematischen Studien Regiomontans in seiner Wiener Zeit'

This paper (36 pp.) was originally published in 1980. It deals with a phase in Regiomontanus' mathematical development of which little had been known. In Folkerts' words, it shows that

> laborious work on details may still allow one to find many mosaic cubes which, admittedly, do not change the picture of Regiomontanus the mathematician completely, but still allows making it much more distinct. [V.175–176]

At first, Folkerts analyzes Regiomontanus' *Wiener Rechenbuch*, a manuscript from Regiomontanus' hand written between 1454 and *ca* 1462 (Codex Wien 5203), containing original work as well as borrowed texts (at times, however, apparently rewritten in Regiomontanus' own words). Next, Folkerts traces which treatises on *Visierkunst* (the practical mensuration of wine casks) Regiomontanus must have possessed or known, using the posthumous catalogues of Regiomontanus' library and those parts of the codex Plimpton 188 which once belonged to Regiomontanus. Finally, Folkerts digs out from the same Plimpton codex evidence that the algebraic knowledge which Regiomontanus displays in his correspondence with Bianchini and others was already his in 1456 (including matters which are now known to have been current in Italian 14th-century *abbaco* algebra but not found in the *Liber abbaci* nor in al-Khwārizmī). Even the symbolism that Regiomontanus uses after 1462 turns up in the Plimpton codex, both in passages that stem from Regiomontanus' hand and in others for which he is probably not responsible.

VI. 'Regiomontanus' Role in the Transmission and Transformation of Greek Mathematics'

This article (26 pp.) was originally published in 1996. After some biographical information, it presents Regiomontanus' 'programme', that is, the leaflet listing the works which Regiomontanus intended to print on his own press (plans that were never realized because of his sudden death). Beyond some of Regiomontanus' own writings, it includes in particular the *Elements*, Archimedes' works, Menelaus' and Theodosius' spherics, Apollonius' *Conics*, Jordanus de Nemore's *Elements of Arithmetic* and *On Given Numbers*, Jean de Murs' *Quadripartitum numerorum* and his *Algorismus demonstratus*. The 'programme' is supplemented by Regiomontanus' Padua lecture from 1464, which refers to many of the same works and also to Diophantus. Next, Folkerts uses manuscripts which were demonstrably in Regiomontanus' possession, his annotations, and so forth, to determine how much Regiomontanus actually knew about the authors and works he mentions—which was indeed much. Only in the case of the *Conics* is it not certain that he was familiar with more than the beginning of the work as translated by Gherardo da Cremona.

The final pages of this article present various numeric, geometric, as well as determinate and indeterminate algebraic problems not coming from Greek sources but present in: the *Wiener Rechenbuch*, a problem collection in the Plimpton manuscript (in Regiomontanus' hand and apparently from 1456), the manuscript *De triangulis*, and the letters exchanged with Giovanni Bianchini, Jacob von Speyer, and Christian Roder. Some of the geometric problem solutions make use of algebraic techniques.

The discussion of approximations to the square root of a number $n = a^2 + r$ on VI.109 invites comment. The *Rechenbuch* as well as the Plimpton collection offer the usual first approximation $\sqrt{n} \approx n_1 = a + r/2a$. The Plimpton collection then gives a second, supposedly better, approximation

$$n_2 = a + \frac{4a^2 + 2r - 1}{(4a^2 + 2r) \cdot 2a}$$

about which Folkerts says that it is not clear where it comes from. Actually, the formula is wrong—it reduces to

$$a + \frac{4a^2 - 1}{8a^3}$$

when $r = 0$, not to a. However, iteration of the procedure which yields n_1 gives

$$\tilde{n}_2 = a + \frac{(4a^2 + 2r) \cdot r - r^2}{(4a^2 + 2r) \cdot 2a}$$

which coincides with the Plimpton second approximation for $r = 1$. In the present context, one might have expected that Regiomontanus dealt only with an example where $r = 1$, and that the general formula as such is a reconstruction due to Folkerts. However, in VIII.422, Regiomontanus is quoted for the observation that the second approximation cannot be applied to all numbers, which is obviously not true for the approximation $\tilde{n}_2$. Regiomontanus must, therefore, be presumed to be at least co-responsible for the mistake.

Folkerts quotes the *Rechenbuch* for a different second approximation, *viz*

$$n_2 = \frac{n}{n_1} : 2.$$

This is obviously a misprint for

$$n_2 = \left(n_1 + \frac{n}{n_1} \right) : 2.$$

By the way, a bit of calculation shows that this n_2 and what was called $\tilde{n}_2$ above are algebraically equivalent.

VII. 'Regiomontanus' Approach to Euclid'

This paper (16 pp.) is a completely revised translation of an article first published in German in 1974. Its first half elaborates in greater depth the Euclidean aspect of the previous article and the presentation of the posthumous catalogues of Regiomontanus' *Nachlaß* from article V. The second half analyses Regiomontanus' endeavor 'to establish a correct text of Euclid' which was mainly based on mathematical critique of the Campanus version but also drew on 'Version II' (formerly known as 'Adelard II'). As summed up by Folkerts [VII.10], Regiomontanus' aim was 'to establish a mathematically correct text

(not to be understood in modern text-critical sense of a reconstruction of the original text)', as was indeed 'typical for Regiomontanus'.

VIII. 'Regiomontanus' Role in the Transmission of Mathematical Problems'

This article (18 pp.) was first published in 2002. It broadens the range of problem types with respect to those discussed in the end of article VII, and says more about the way in which the problems are solved. The sources are the Plimpton problem collection, the correspondences, and the *Wiener Rechenbuch*. In particular, a number of problems going back to the Italian *abbaco* tradition are presented.

Several of these problems turn up again in the following decades in mathematical writings from southern Germany, first in a manuscript copied by Fridericus Amann in 1461—at times with the same numerical parameters. Folkerts concludes that 'Fridericus Amann must have learned something of the contents of MS Plimpton 188 soon after it was finished' [VIII.414], and that 'Regiomontanus played a crucial role in transmitting mathematical knowledge from Italy to Central Europe in the 15th century.' Given that even the problems in the Plimpton manuscripts are copied from an earlier source, this seems to me to be a daring conclusion to say the least.[3]

Some observations should be made. First, on VIII.418 it is stated that nos. 16–32 of the Plimpton collection ask for a number and serve as examples for al-Khwārizmī's six problem types. This seems to be a typographical mistake (for 16–21?).[4] Next, the erroneous second-order approximation to a square root from the Plimpton collection is repeated on VIII.422, whereas the one from the *Rechenbuch* is correct this time. Finally, on VIII.419, something is wrong in the presentation of a 'special arithmetical problem'—probably already in the original.[5]

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[3] See 138n17 below, and preceding text.

[4] According to Folkerts, no. 22 deals with compound interest (but illustrates al-Khwārizmī's fourth type), and nos. 27 and 30 are, respectively, of the types 'purchase of a horse' and 'give and take.'

[5] The problem from the Plimpton collection states that 'somebody wants to go as many miles as he has dinars. After every mile the dinars he possesses are doubled, but he loses 4 dinars. At the end he has 10 dinars.' Folkerts solves

IX. 'Leonardo Fibonacci's Knowledge of Euclid's *Elements* and of Other Mathematical Texts'

This article (25 pp.) was still to appear when the present volume was prepared (it was eventually published in the Fall of 2005). Going through the *Liber abbaci*, the *Pratica geometrie*, the *Flos*, the letter to Master Theodorus, and the *Liber quadratorum*, Folkerts traces the mathematical works that are used with 'due reference' as well as those which are used without recognition of the borrowing. Euclid is quoted very often; Archimedes, Ptolemy, Menelaus, Theodosius, and the *agrimensores*, occasionally; but Arabic authors are not cited at all (with the sole exception of *Ametus filius*, i.e., Amad Ibn Yūsuf).[6]

The last part of the article raises the question 'Which version of Euclid did Leonardo use?' Often Fibonacci seems to quote from memory—the same proposition may be formulated in different words in the *Liber abbaci* and the *Pratica*, none of the formulations agreeing with any known Latin or Arabic version. Elsewhere, it is clear that Fibonacci uses the Latin translation from the Greek.

X. 'Piero della Francesca and Euclid'

This article (22 pp.) was first published in 1996. It starts by sketching the story of the Arabo-Latin *Elements* (with emphasis on Campanus) and by giving a brief general description of Piero's mathematical

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this without making use of the magnitude of the remainder (the algebra involved cannot correspond to anything Regiomontanus would do), finding that the man starts with 4 dinars—but in that case he will be left with 4 dinars after each doubling and subtraction, never with 10. Regiomontanus has a marginal note that the problem has to be solved 'in a reversed order', which Folkerts suggests might mean by 'trial and error'. This is not likely: stepwise backward calculation was a standard method for such 'nested-box' problems. Going backwards from 10 dinars, we get the successive remainders 7, 5½, 4¾, 4⅜, ... The data of the problem are thus inconsistent (if rendered correctly), which Regiomontanus does not seem to have noticed.

6 Since Fibonacci asserts regularly that his methods are of Arabic origin, this *could* mean that he made his apparent borrowings from Abū Kāmil, al-Karajī, and others indirectly. However, his obvious verbatim copying from Gherardo da Cremona's translations of al-Khwārizmī [Miura 1981] and Abū Bakr [Høyrup 1996, 55] weakens the argument—at times, Fibonacci clearly did not want to reveal his sources.

works based on Davis 1977. Turning then to the use of Euclid, Folkerts shows that even Piero is fond of citing Euclid (mostly the *Elements*, but in *De prospective pingendi* the *Optics* as well). There is no doubt that Piero used the Campanus version—he cites Campanus twice and uses some of his additional propositions. However, Piero's words and terminology often differ from those of Campanus in a way which reflects Piero's background in the *abbaco* tradition—both in the *Libellus de quinque corporibus regularibus*, which was originally written in Italian but is only extant in Latin translation, and in the *Trattato d'abaco*. Folkerts supposes this to reflect lack of familiarity 'with the style used in scientific mathematical works' [X.302] and not the use of a non-Campanus version. He points out that Piero's numbering of certain propositions from book 15 show that the manuscript he used is not among those known today.

Article X concludes by examining the citations of Vitruvius, Ptolemy, Archimedes, and Theodosius in Piero's mathematical writings as well as the possible sources for his treatment of semiregular solids—for which Jean de Murs' *De arte mensurandi* might be one but not the only source.

XI. 'Luca Pacioli and Euclid'

This article (13 pp.) was originally published in 1998. Within the framework of a short biography concentrating on Pacioli's interaction with Euclid, it discusses the traces of his translation of Euclid into the vernacular, the excerpts from the *Elements* in the *Summa de arithmetica geometria proportioni et proportionalita* from 1494 (drawn from the Campanus tradition), and his Latin edition of a purportedly restored Campanus text in 1509.

The vernacular translation turns out to have probably been made before the first part of the *Divina proportione*, i.e., before 1497. The arithmetical part of the *Summa* contains excerpts from *Elements* 5;[7] the geometrical part excerpts from books 1–3, 6 and 11. The material is transformed in a way which was presumably suited for a public with practical but only modest theoretical interests: the Euclidean

[7] These excerpts, dealt with previously by Margherita Bartolozzi and Raffaella Franci [1990], are not discussed further by Folkerts.

material is brought in the beginning of sections—thus serving as 'theoretical' underpinning for what follows—but there is no clear separation between definitions and enunciations, and proofs are mostly replaced by explanations with reference to diagrams.

The definitions from book 1, as well as all excerpts from book 11, are rendered rather freely. The rest of the excerpts from book 1 as well as those from books 2–3 and 6 are very close to the Campanus text. They cannot have been taken over from Pacioli's vernacular translation, since they agree rather precisely with passages in the manuscript BN Florence, Palatino 577, probably from *ca* 1460.[8]

[8] This agreement appears from the presentation to have been established/checked by Folkerts himself. For the statement that the 'geometrical section of Pacioli's *Summa* agrees in the other parts, too, with that Florence manuscript' [XI.226], Folkerts refers to Picutti 1989.

Because of the widespread, unconditional acceptance of the thesis of this paper, which is meant to convince readers that Pacioli, *in claimed contrast to other abacus writers*, was a vile plagiarist, the reviewer would like to make some observations. Picutti's paper is written in a strong and explicitly anticlerical key, which may be quite understandable in an Italian context, but is in itself no argument for its reliability—nor of course for the opposite. (Compare Libri's wonderfully and similarly engaged *Histoire des sciences mathématiques en Italie* [1838–1841], which is still valuable after more than 150 years). So, without further evidence, one should probably not follow an author who claims that Pacioli divides his text into chapters instead of 'distinctions' [Picutti 1989, 76]. Actually, the chapters are subdivisions of the distinctions, the distinctions are indicated in the titles, and the actual distinction as well as the chapter are indicated in the running head of all pages, in the 1494 edition of the *Summa* as well as the second edition from 1523. Picutti seems not to have examined any of them seriously. (Without endorsing peer-review hysteria, the reviewer also asks himself why Picutti only published in the Italian edition of *Scientific American* and never substantiated his assertions in a professional journal.)

On the other hand, it is obvious from a reproduced passage that Pacioli sometimes used either Palatino 577 or a precursor manuscript. Since Pacioli has diagrams which are omitted in the Palatino manuscript (as admitted by Picutti), it is plain that Pacioli either used this manuscript creatively or that he borrowed from a precursor where the diagrams were present (the one shown in the reproduction is not in Fibonacci's *Pratica*, at least not in Boncompagni's edition [1862]). Elsewhere in the *Summa*, however, misprints in the lettering of the diagrams can be corrected by means of the Boncompagni edition of the *Pratica*. Pacioli evidently felt free to copy without acknowledg-

Folkerts' comparison of Pacioli's edition of the Campanus text with the *editio princeps* from 1482 shows that the proper corrections are minor, and that the main difference consists in the addition of comments introduced by the word *castigator* (which suggests that they were meant to be understood as corrections). In total, Folkerts counts 136 additions, 42 of which are more than 10 lines long. For the most part, 'Pacioli confines himself to explaining terms or individual steps within a proof or construction' [XI.228]; at times, he 'makes remarks that are not immediately necessary for the understanding of the theorem, but are suggested by it' [XI.229]. So, we may assume 'that the edition of Euclid contained elements from Pacioli's mathematical lectures' [XI.230].

XII. 'Algebra in Germany in the Fifteenth Century'

This article (18 pp.) has not been published before. Its theme was already touched on in articles V, VI, and VIII; but here the perspective is broadened. Some of the essential sources for the arguments have been published but much material remains in unpublished manuscripts, and a survey like the present one is certainly needed, if only to create a context for further research.

The article starts by presenting the background in Italian *abbaco* algebra. This account, as explained, is built on Franci and Toti Rigatelli 1985, which must now be considered partially outdated.[9] The claim [XII.3] that Piero della Francesca 'contributed not only to perspective but also to algebra', and that therefore and for other reasons Luca Pacioli 'has enjoyed unmerited fame, for his algebra

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ing his sources explicitly, while stating in the initial unfoliated *Sommario* that most of his volume has been taken from Euclid, Boethius, Fibonacci, Jordanus, Blasius of Parma, Sacrobosco, and Prosdocimo de' Beldomandi. Fibonacci, Piero, and many other writers in the *abbaco* traditions borrowed as freely and gave neither specific nor general reference when the name of the source carried no prestige. Only renewed scrutiny of the Palatino manuscript will reveal whether Pacioli also copied directly from Fibonacci's *Pratica* or *only* indirectly.

[9] Its aim was 'to shed light on the algebraic achievements of the Italian algebraists of the Middle Ages, rather than to investigate their sources and internal links' [Franci 2002, 82n2]; it even precedes a paper [Franci and Toti Rigatelli 1988] which the authors characterize as a 'first summary'.

contains nothing new of any value' is unwarranted. After all, Piero—truly impressive as he is as a geometer—repeated without distinction traditional nonsense along with valuable material in his algebra: he obviously copied texts without checking or making calculations. Pacioli reflected on the algebraic material that he borrowed, exactly as he reflected on his Euclidean borrowings.[10]

The treatment of Germany begins with a presentation of Regiomontanus' contributions, with particular emphasis on his symbolism. Its thesis is that Regiomontanus 'was central for the transmission of Italian ideas about algebra to Central Europe' [XII.3].

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[10] Piero repeats those false rules for higher-degree equations which had circulated at least since Paolo Gherardi (1328). See, for instance Arrighi 1970, 13 on solving the problem 'cubes equal to things and number' (in modern symbols, $\alpha x^3 = \beta x + n$) as if it had been '*censi* equal to things and number' ($\alpha x^2 = \beta x + n$). Rules which hold in specific cases only (as pointed out by Dardi da Pisa in 1344) are stated by Piero as universally valid—see, for example, Arrighi 1970 146. Piero also copies a long sequence of rules for quotients between algebraic powers, in which 'roots' take the place of negative powers, the first negative power being identified with 'number' (the rules appear to go back to a treatise written by Giovanni di Davizzo in 1339) [cf. Høyrup 2007c and Giusti 1993, 205]. See also Enrico Giusti's characterization of the algebraic Piero as

> a copyist who does not even notice—witness the very high number of repetitions of cases that were already treated (13 out of a total of 61)—that what he was writing had already been copied one or two pages before,

and as 'an author... who did little more than to collect whatever cases he might find in the various authors at his disposition, without submitting them to accurate examination' [Giusti 1991, 64 (trans. JH)].

Pacioli points out explicitly [1494, 1.150r] that no generally valid rule had so far been found for cases where the three algebraic powers are not separated by 'equal intervals'. (He was not the first to point it out: a similar observation is made in the *Latin algebra* [Wappler 1887, 11]—see 137n16 below and pertinent text). Pacioli also stays aloof of the confusion between negative powers and roots. He does include [1494, 1.67v, 143r–v] a terminology where 'nth root' stands for the $(n-1)$th (positive) power of the *cosa*. But, since this system identifies the 'first root' with the *cosa*, it is likely to be an outgrowth of the al-Khwārizmīan use of root (namely the square root of the *māl/census*) for the first power—an outgrowth of which Pacioli is *not* the inventor, since he describes the system for completeness' sake.

According to Folkerts, Regiomontanus uses the following symbols or abbreviations:[11]

- a superscript r or R provided with a curl to indicate an abbreviation for *res* or *radix*, following after the coefficient,
- a superscript c also provided with a curl and following the coefficient, for 'census',
- a long horizontal stroke connecting the two sides of the equation (which may thus be read as an equality sign in the function of equation sign),
- a sign for minus that has been interpreted as $\bar{i}$ (that is, *in*) followed by the curl meaning *us*, $\iota\overline{\mathrm{q}}$.[12]

However, the shapes shown in a photo in Cajori 1928–1929, 1.96 from the calculations made for a letter to Bianchini—*viz* $\widehat{\mathfrak{m}}$, at times becoming $\widehat{\mathfrak{m}}$—look more like pen variants of the traditional Italian shape $\widehat{\mathfrak{m}}$,[13] while a page from the Plimpton manuscript[14] uses the shape $\widehat{\mathfrak{m}}$ twice but the shape $\mathfrak{mi}$ (meaning *mī(us)*) four times. The same page shows the abbreviation for *res* superscripted once but more often on the line (and even more often with the full word *cosa*). All in all, Regiomontanus symbols (mostly used as mere abbreviations) are much less fixed than Folkerts' description would have us believe.

In his Vienna period, as pointed out, Regiomontanus copied al-Khwārizmī's algebra (in Gherardo's translation) and Jean de Murs' *Quadripartitum numerorum*, and annotated both carefully. As concerns the algebraic problems contained in the Plimpton collection, *De triangulis*, and the correspondences, Folkerts restricts himself *grosso modo* to a cross-reference to articles V, VI, and VIII.

Afterwards, a number of other 15th-century German writings are presented or mentioned briefly:

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[11] These are only described in words by Folkerts, but see the depictions in Curtze 1895, 232ff., 278–280; Cajori 1928–1929, 1.95ff.; and Tropfke, Vogel, *et alii* 1980, 281.

[12] Thus not only Folkerts but also the re-drawings in Tropfke, Vogel, *et alii* 1980, 206 and Vogel 1954, Tafel VI.

[13] This shape is found, e.g., in Vatican Library, Chigiana, M.VIII.170, written in Venice in *ca* 1395. A reduction of the equally classical shape $\widehat{\mathfrak{m}}$ is definitely less likely.

[14] Reproduced in high resolution on the webpage: http://columbia.edu/cgi-bin/dlo?obj=ds.Columbia-NY.NNC-RBML.6662&size=large].

- the (mostly non-algebraic) problems added to the *Algorismus ratisbonensis* by Fridericus Amann and the algebra written by Amann in 1461 (both Bayerische Staatsbibliothek, Clm 14908);[15]
- from Dresden, C 80, a 'Latin algebra' as well as a 'German algebra' from 1481 which 'seems to depend on the "Latin algebra" ' [XII.9];[16]
- marginal notes in the same manuscript made by Johannes Widmann, and the same author's *Behende und hubsche Rechenung auff allen kauffmanschafft* from 1489;
- the writings of Andreas Alexander (b. *ca* 1470), a pupil of a certain Aquinas (an otherwise obscure Dominican friar from whom Regiomontanus says that he has learned);
- the *Initius algebras* which *may* have been written by Alexander or by Adam Ries;
- Ries' (non-algebraic) *Rechenbuch* as well as the two editions of his *Coss* [1524, 1543+];

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[15] The problems were published in Vogel 1954; the algebra, in Curtze 1895, 49–73.

[16] The former was published in Wappler 1887; the latter, in Vogel 1981. The codex was in the possession of Widman, and the *Latin algebra* was used by him. Since the *German algebra* makes abundant use both of a fraction-like notation for monomials known from Italian writings [see below, text around 139n 21] and of the phrases 'mach mir die rechnung'/'Und moch des gleichen rechnung alzo' corresponding to the Italian 'fammi questa ragione'/'così fa le simiglianti', none of which are found in the *Latin algebra*, the *German algebra* must either draw on several sources of inspiration, or it must share a precursor with the *Latin algebra* rather than depend on it (or both).

That it must depend on several sources was indeed already observed by Vogel [1981, 10]. To Vogel's observations can now be added not only that the fraction-like notation for monomials is of Italian origin but also that the strange term and abbreviation for the fourth power (*wurczell von der worczell*/'root of the root') looks like a crossbreed between Piero's negative powers and Pacioli's alternative notation [see 135n10, above]. The idea to provide the fifth case (the one with a double solution) with three examples also corresponds to what can be found in Italy (Jacopo da Firenze as well as Dardi)—the original point being that one case requires the additive solution, one the subtractive solution, and one is satisfied by both.

The use of 'root of root' in passages of the German algebra that are parallel to passages where the *Latin algebra* has the regular repeated *zensus*-abbreviation ℨℨ suggests that these parallels are due to the sharing of a common source rather than to direct translation.

- Rudolff's *Coss* [1525] and Stifel's *Arithmetica integra* [1544]; and
- the Cistercian Conrad Landvogt (*ca* 1450 to 1500+), whom Folkerts himself has brought to light.

Folkerts bases his claim regarding Regiomontanus' central role in the transmission on various pieces of evidence. First, the algebraic problems in the Plimpton collection have the heading *Regule de cosa et censo sex sunt capitula, per que omnis computatio solet calculari*; whereas Amann gives the title *Regule dela cose secundum 6 capitola*. The similarity is not striking. Moreover, if Amann *had* copied Regiomontanus, he would have had no reason whatever to restore Italian grammar (*dela cose* instead of *de cosa*). A close common source, however, is very likely.[17]

Second, Regiomontanus is supposed to have invented his own symbolism; and Amann, to have borrowed it. For, given that Amann appears to have visited Vienna in 1456, Folkerts thinks that 'there are good reasons to assume that he met Regiomontanus there and at this meeting... learnt of his symbols' [XII.8]. (Regiomontanus was 20 years old by then, while Amann must have been close to 50). Amann's symbols for *res/cosa* and *zensus* are indeed fairly similar to those of Regiomontanus. However, in V.201ff., Folkerts indicates that parts of the Plimpton manuscript which appear *not* to be written by Regiomontanus also use symbols and that one section uses exactly the same symbols as Regiomontanus. There Folkerts points out that this might represent a precursor to Regiomontanus' symbolism. In that part of the Plimpton text, it is true, the symbols are not superscript, but even this is hardly an innovation due to Regiomontanus (nor is it, as we have observed, a constant habit of his): superscript symbols following the coefficient (the square meaning *censo* sometimes above, but *co* for *cosa* always following) were also used by Pacioli in

[17] Indeed, the two examples from Regiomontanus' text which are reproduced on the web [see 136n14, above] coincide substantially with those of Amann—much more so, indeed, than they would have done if Amann had reproduced from memory what he had discussed with Regiomontanus (see imminently), but much less than if he had translated from the Plimpton manuscript. One difference is informative. In Regiomontanus' text, there is a reference to the principle that when equals are added to equals, equals result. This Euclidean argument for the traditional *restoration* operation is absent from Amann's text, and thus likely to be Regiomontanus' own contribution—and an early manifestation of his characteristic approach.

a manuscript finished in 1478 (Vatican, Vat. Lat. 3129), which also (for example, on fol. 67v) uses the horizontal stroke as an equation sign (but $\overset{\frown}{m}$ for minus).[18] Since superscript □ and *co* (and sometimes *cen* for *censo*) written above the coefficient are also used in the Italian manuscript Vat. Lat. 10488 of 1424, for instance, on foll. 36v, 38v, 92r–v (original foliation), it is clear that Pacioli did not take his inspiration from Regiomontanus.[19] Ultimately, this notation is likely to be a borrowing from Maghreb algebra.[20]

A different, fraction-like notation was used by Dardi of Pisa,[21] and also in the draft manuscript *Trattato di tutta l'arte dell'abacho* from *ca* 1334: $\frac{12}{c}$ stands for 12 *cose*, $\frac{4}{c}$ for 4 *censi*. The same notation is used in the *German algebra* in C 80.[22] All in all, it is possible though not certain that some later cossists learned their symbolism (or part of it) from Regiomontanus. It is certain, however, that not all of them did, and equally certain that Regiomontanus did not invent it.

Third, it is said on XII.9 that the

> order of the [equation] types, which is elsewhere varied, is the same in the 'German algebra' in MS C 80 and in the Regiomontanus text in MS Plimpton 188. This cannot be a coincidence.

Evaluation of this statement is difficult since Folkerts gives no exact information about the presentation of the cases in MS Plimpton 188. However, in VIII.418, it is stated that

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18 For a discussion of the stroke as equation sign in Pacioli's *Summa*, see Cajori 1928–1929, 1.110ff.

19 Vat. Lat. 10488 sometimes uses $\overset{\frown}{m}$, sometimes $\overset{\frown}{me}$ for minus.

20 Cf., e.g., Tropfke, Vogel, *et alii* 1980, 376.

21 Høyrup [2007a, 170] argues that this symbolism, found in the two earliest manuscripts, was already used in Dardi's original from 1344.

22 With a set of symbols for the algebraic powers which is neither identical with what can be found in Italian treatises nor with those of Regiomontanus, Amann, or the *Latin algebra*; see the facsimiles in Vogel 1981, Tafel 1–3, and the comparison in Vogel 1981, 11 (where it should be observed that the symbolic notation ascribed to Robert of Chester and the year 1150 refers to marginal notes in C 80 and to an appendix to Robert's translation found in 15th-century manuscripts from the South-German area).

In the very last problem of the *German algebra* [Vogel 1981, 43], a different (but equally Italian) notation is used: a superscript *c* (for *cosa*), above or following the coefficient.

> the Latin text in the Plimpton manuscript, which describes the six forms of equations, agrees word-for-word with the German translation that Fridericus Amann wrote five years later.

But this simply means that the order for these six fundamental cases is the standard order of Italian *abbaco* algebra—which certainly differs from the order of al-Khwārizmī, Abū Kāmil, and Fibonacci [see Curtze 1895, 50]. The same order is found in the *Latin algebra* as well as in the *German algebra* from C 80. Such agreement concerning the fundamental cases thus only indicates common roots in the *abbaco* tradition and nothing more.

Then, there are 18 more cases, which are either homogeneous or reducible to the second degree. These cases are found in the *Latin algebra* [Wappler 1887, 12ff.] as well as in the *German algebra* [Vogel 1981, 22] from C 80.[23] These cases share not only their order (which is unusual and may perhaps be of Italian origin) but also the numerical parameters. *This* is certainly not be a coincidence, even though the cases themselves were all familiar in *abbaco* algebra since the early 14th century. Regiomontanus also has 18 more cases, and most of them coincide with those of the two algebras from C 80 and follow the same order. But, if Folkerts' transcription in modern symbols in V.n150 is reliable, two cases are different:

- no. 12 is $ax^4 + bx^2 = cx^3 + dx^2$, while agreement with the algebras in C 80 would require $ax^4 = cx^3 + dx^2$;
- no. 14 is $ax^2 = \sqrt{b}$, whereas agreement would demand $ax^2 = \sqrt{bx^2}$.

The latter deviation *might* be a miswriting due to Folkerts or his typographer, but the former is not. So, once more, the evidence suggests shared inspiration rather than copying from Regiomontanus.

Summing up, Folkerts' description of 15th-century German algebra is certainly indispensable for any further discussion of the topic in that it lists all known important and several (though not all) minor manuscript sources and points to many of the parameters that have to be taken into account. Thus, it was only through the use of Folkerts' text that I was able to grasp and sift the material well

[23] The *Latin algebra* has one more case, which is corrupt and lacks an illustrating example), and which its compiler claims he 'found elsewhere' (*alibi inveni*) [Wappler 1887, 12].

enough to formulate my objections. In my view, Folkerts' conclusion is premature and sometimes contradicted by precise inspection of the sources. In consequence, I believe it to be mistaken: Italian *abbaco* algebra appears to have inspired and spurred the German development not through a single but through multiple channels.[24] However, no definite conclusions should be drawn before manuscripts are gauged against the essential parameters both on the Italian and the German side. Unfortunately, few of the printed editions of Italian *abbaco* manuscripts that have been published during the last 50 years have bothered much about symbolism-like abbreviations and non-geometric marginal diagrams. It is to be hoped, then, that Folkerts' overview may contribute to changing this state of affairs!

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[24] Further evidence for this, beyond the parameters discussed by Folkerts and the observations made in 137n16 above, comes from the German standard spellings *coss*, *zensus* and *unze*. They point to inspiration from northern Italy [cf. Rohlfs 1966–1969, I.201f., 284, 388], where *cossa/chossa*, *zenso* and *onzia* are common, say, to Genoa rather than to Venice. Regiomontanus, in MS Plimpton 188, writes *cosa*; the *German algebra* has *cossa*. The *Latin algebra*, as mentioned in 137n16 above, uses an abbreviation for the second power (ȝ) which is derived from 'zensus'.

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Astronomical Diaries and Related Texts from Babylonia: Volume VI. Goal Year Texts by Hermann Hunger, including Materials by Abraham J. Sachs

Vienna: Österreichische Akademie der Wissenschaften, 2006. Pp. xviii + 471 (with 73 plates). ISBN 978–3–7001–3727–6. Cloth € 95.20

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The Astronomical Diaries and Related Texts from Babylonia, a series of editions and translations of Late Babylonian astronomical tablets by Hermann Hunger and the late Abraham Sachs, has been one of the most significant contributions to the study of ancient astronomy over the past two decades. Volumes 1–3 (published in 1988, 1989, and 1996) contain editions of all known datable Astronomical Diaries: texts that record the night-by-night observations made by Babylonian astronomers, and the primary source for all other types of Late Babylonian non-mathematical astronomical texts. Volume 5 (published in 2001) contains editions of tablets that report lunar and planetary observations and predictions. By and large, the astronomical data in the lunar and planetary texts was, we believe, abstracted from the Astronomical Diaries. The volume under review, volume 6 (published in 2006), contains all known datable and undatable Goal-Year Texts. Planned future volumes will include the undated Astronomical Diary fragments (volume 4) and the Normal Star Almanacs and Almanacs (volume 7). When complete, this series will contain editions of more than two thousand Late Babylonian astronomical texts, more than half of the known corpus of cuneiform texts concerning astronomy.

The Babylonian Goal-Year Texts contain lunar and planetary data taken from the Astronomical Diaries that was to be used in making predictions for a coming 'Goal' year. The principle behind these predictions is that after a certain number of years, individual phenomena for each planet repeat on about the same day in the Babylonian calendar and at about the same location in the sky. For

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example, first visibilities of Saturn happen roughly every 13 months, separated in longitude by about ⅓ of a zodiacal sign in longitude. However, after 59 years, the first visibility of Saturn will once again occur on roughly the same day in the Babylonian calendar (allowing for the possibility of a one month correction to take into account intercalation) and at about the same celestial longitude. Therefore, by simply going back 59 years from the Goal Year for which predictions are sought, and copying the Saturn data for that year, it was possible to predict the Saturn phenomena in the Goal Year. By going back different numbers of years for the different planets, all planetary phenomena for a coming year could be predicted.

On pages ix–xiii of the book under review, Hunger provides a short but clear account of the contents and purpose of the Goal-Year Texts. Goal-Year Texts are divided into 10 sections:

1. Greek Letter Phenomena (first visibilities, first stationary points, acronychal risings, second stationary points and last visibilities) of Jupiter from 71 years before the Goal Year,
2. Passages of the Normal Stars by Jupiter from 83 years before the Goal Year,
3. Greek Letter Phenomena and passages of Normal Stars by Venus from 8 years before the Goal Year,
4. Greek Letter Phenomena and passages of Normal Stars by Mercury from 46 years before the Goal Year,
5. Greek Letter Phenomena and passages of Normal Stars by Saturn from 59 years before the Goal Year,
6. Greek Letter Phenomena of Mars from 79 years before the Goal Year,
7. Passages of Normal Stars by Mars from 47 years before the Goal Year,
8. The sums of the lunar six intervals[1] ŠÚ+*na* and ME+GE$_6$ for the second half of the year 19 years before the Goal Year,
9. Reports of observed and predicted eclipses of the Sun and Moon from 18 years before the Goal Year and
10. Lunar six data from 18 years before the Goal Year.

[1] On six occasions during a month, the Babylonians measured the time interval between the Sun's crossing the horizon and the Moon's crossing the horizon. Each such series of measurements constitutes what is now called a lunar six.

Generally sections 1–6 are on the obverse and sections 7 to 10 are found on the reverse. Sections 8–10 are always given in three columns, to be read from left to right.

For each of Venus, Mercury, and Saturn, only one section was given, as the individual Goal-Year periods work well for both synodic phenomena (Greek Letter phenomena) and sidereal phenomena (passages by Normal Stars). However, for Jupiter and Mars, different Goal-Year periods were used in each case for synodic and sidereal phenomena in an attempt to make more accurate prediction. Evidence from procedure texts, and from analysis of Almanacs and Normal Star Almanacs, which are believed to contain the results of Goal-Year predictions, indicate that small corrections of a few days were applied when using the Goal-Year material to make predictions.

Predicting lunar phenomena using the Goal-Year Texts was somewhat more involved than for the planetary data. The lunar six data was predicted using values of the same lunar six interval from 18 years earlier plus a correction of either plus or minus $\frac{1}{3}$ of the sum of two of the lunar six from either 18 years or 18 years + 6 months earlier [Brack-Bernsen and Hunger 2002]. This explains the presence of the sums ŠÚ+*na* for the second half of the year 19 years before the Goal Year.

Lunar and solar eclipses were predicted using a scheme based upon the Saros cycle [Steele 2000]. The lunar and solar eclipse data recorded in the Goal-Year Texts provided the data necessary for predicting the time and likely visibility of the predicted eclipses.[2]

Hunger has identified and edited 178 Goal-Year Texts in this volume. Of these, 95 have been dated either from preserved dates in the text or astronomically by Hunger. The remaining 83 are largely small fragments, generally containing only lunar six data. Until recently, no techniques have been available for dating lunar six data. However, Huber has developed a statistical method which has proved effective in dating many lunar six tablets [Huber and Britton 2007, Huber and Steele 2007]. It is to be hoped that application of Huber's method to some of the undated Goal-Year Texts may prove fruitful.

[2] For two possible methods for predicting the time, see Brack-Bernsen and Steele 2005.

Hunger's editions are a model of accuracy and his translations uniformly clear and consistent. I have come across only two trivial typographical errors: in Obv. 11' of No. 16 [65], the star MÚL *ár šá* ALLA *šá* ULU is wrongly translated as δ Scorpii instead of δ Cancri; and at Rev. 19 of No. 73, 'Month XI' should read 'Month IX' and 'Month IX' should read 'Month XI'. Both of these mistakes are easily corrected by the reader from either the transliteration or the context.

The publication of the Goal-Year Texts opens up for study an important aspect of Babylonian astronomy, the prediction of planetary and lunar phenomena using Goal-Year periods. Important work has been done on Goal-Year methods for predicting lunar phenomena in the past decade, but little has been published on the Goal-Year techniques for the planets since Kugler's pioneering works in the early part of the 20th century. The publication of these texts is already stimulating new research in this area.

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Aristotle on Teleology by Monte Ransome Johnson

Oxford Aristotle Studies. Oxford: Clarendon Press, 2005. Pp. xi + 339.
ISBN 0–19–928530–6. Cloth $99.00

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Aristotle's teleology is probably the most pervasive and the most celebrated doctrine in the corpus. It serves in multiple capacities and appears under many guises: as the core of his outlook on the natural world (nature does nothing in vain), as a principle of ethics (every action and pursuit... aims at some end), and even as the starting point for a philosophical anthropology (all human beings by nature desire to know). It is for many readers, expert and general alike, what one means when one speaks of Aristotelianism.

Yet, both despite and because of its centrality, teleology in Aristotle has been subject to a vast range of interpretations and criticisms over many centuries. Is his teleology an all-embracing cosmic orientation toward a single end? Have the ends of nature been designed and imposed by a presiding divine intellect? Are human beings the chief beneficiaries of nature's teleological orientation? The range of reactions and responses to such questions is very wide, revealing a great deal about how Aristotle's ideas have been appropriated and used by various readers.

Monte Ransome Johnson's *Aristotle on Teleology* is an ambitious attempt to come to terms with the central doctrine of teleology in Aristotle. It ranges over the history of its reception, the theoretical terms in which it is articulated, and the subjects to which it has been applied. Its scope makes the book part history of philosophy and science, part sustained philosophical analysis, and even part exhortation. The result is a work that deserves careful study and will undoubtedly be consulted by anyone interested in its issues.

The book is in two parts and 10 chapters. The first part, made up of four chapters, considers the explanatory framework provided

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by Aristotle's teleology; the second part and final six chapters turn to teleological explanation in natural science and elsewhere (Johnson considers teleology's role in Aristotle's ethics and politics as well as in his 'theological treatise', by which Johnson means *Metaphysics* 12).

Johnson's chief aim is, in his words, 'to reopen a line of Aristotelian interpretation that originated in the early twentieth century' [3]. This is an approach which he traces back to Zeller, Gomperz, and Ross, who take Aristotle's teleology as immanent in natural things, not as a transcendent guiding or creative force. The goal of each species is simply to be that species and to perpetuate its kind and not, as Johnson takes pains to emphasize, to serve the needs of some 'higher' entity, be it human beings or god.

The core of Johnson's interpretation comes in a striking and intentionally surprising formulation: for Aristotle, animal parts and their behaviors are adapted to their environment rather than the environment being adapted to them. In speaking this way, Johnson deliberately echoes a key notion in evolutionary biology. His point is not, however, to imply that Aristotle was an evolutionist. Rather, it is to signal his own rejection of a comprehensive or cosmic reading of Aristotelian teleology. In cosmic teleologies, not just a creature's immediate surroundings, but literally the whole universe is adapted to serve some overarching end. In Aristotle's teleology, Johnson argues, the primary beneficiary of natural ends is always some specific kind of thing.

The anthropic principle is a good example of the sort of approach that Johnson rejects: because even a slight change in any of several basic universal constants would have rendered intelligent life impossible, advocates of the anthropic principle argue that the universe must have been designed to promote such life. The ancient world had those who maintained similar doctrines: Stoic teleology was explicitly anthropomorphic, with the physical universe crafted to serve human ends. But Johnson rejects this as a reading of Aristotle's teleology. He argues instead that one must distinguish between an end as cause in the sense of the aim of the process on the one hand, and an end as beneficiary of the process on the other. Each species has been suited by nature to its environment so as to promote its own welfare. Whether in terms of the means of locomotion, habits of breeding, or preferred habitat, the fit between animal and environment—the

adaptation—is as close as it is for the benefit of that animal itself, not to serve the advantage of some other kind of animal. This includes both human beings and god as the putative ultimate beneficiary.

The book takes as its point of departure the history of interpretations of the teleological doctrine by thinkers in radically different traditions (a separate chapter is devoted to what he calls Aristotle's 'dialectical interrogation' of his predecessors). Johnson does not, in other words, present his position in a relative vacuum occupied by only the most recent studies of a particular approach. He locates his interpretation in relation to, and develops it out of, a broad range of sources, including Aristotle's predecessors, his medieval inheritors, and his modern interpreters. In several brief sections, Johnson considers reactions to, and versions of, teleological explanation by Peripatetics and Neoplatonists, medieval Arabic philosophers, Aquinas, Ockham, Descartes, Wolff, and Kant. Though space allows him room for only a fairly cursory summary of this history, Johnson argues that the tendency of those receiving Aristotle's teleology has been to shape it to their own purposes, whether broadly sympathetic to a teleological outlook or antagonistic.

An interesting but ultimately disappointing aspect of this survey is Johnson's use of Aristotle's colleague Theophrastus at the survey's conclusion, after Johnson considers various medieval and modern reactions and appropriations. In spirit, it is a commendable move that gives Theophrastus more credit as an interpreter of Aristotle than is usual. Johnson uses Theophrastus to reinforce his reading of Aristotle as working to articulate standards or limits for teleological explanation, against what Johnson describes as the excesses displayed in quasi-teleological explanations in predecessors such as Xenophon and Plato. According to Johnson, the *Metaphysics* of Theophrastus is an aporetic challenge to unbridled attempts to seek a teleological explanation for all phenomena; and as such it is largely in sympathy with Aristotle and not, as is often said, critical of the Master's approach. Unfortunately, Johnson's brief discussion is able to furnish little more than a flavor of this interesting, neglected work and so gives at most a suggestive plausibility to the idea that it can furnish insights into Aristotle's complex intentions.

An unexpectedly rich discussion of a vexed portion of the *Posterior Analytics* turns up when Johnson sets forth Aristotle's concepts

of cause and explanation. Johnson's overall aim (in the book's second chapter) is to discuss the four causes generally, before turning to a detailed exploration of the teleology of the final cause. But in what turns out to be an extended treatment of *An. post.* 2.11 and issues arising from it, Johnson explicitly draws a version of the four causes under the ambit of Aristotle's theory of scientific demonstration. As Johnson points out, *An. post.* 2.11 has been neglected and even dismissed by commentators, giving the impression that explanation in terms of the four causes was not a main concern at the time of the *Analytics*. Johnson effectively rebuts this assumption through a careful analysis of the roles of the various types of cause as middle terms in an explanatory demonstration, and of how one should understand the temporal sequence of cause and effect in Aristotelian proof. His analysis provides a touchstone for later parts of the book, giving a sense of continuity between Aristotle's theoretical remarks and the application of his theory in various treatises.

Given the very comprehensiveness of Aristotle's teleology, it is only to be expected that it should succeed better in some areas than in others. In the book's central chapters, Johnson argues that it works best at the level of the individual organism—indeed, that it was derived primarily from a study of living things as organisms—but less well both below and above that level. It is not a coincidence that Aristotle's scientific ideas have lost nearly all their plausibility with regard to the elements on the one hand and the living bodies of the stars on the other. Teleological explanation had to be left behind in these areas if science was to move forward. The situation is very different in contemporary biology. There teleological notions continue to seem not just useful but indispensable in ways that Johnson specifies.

Somewhat unusually for a book in ancient philosophy, Johnson intends *Aristotle on Teleology* to make a difference in contemporary attitudes. In the book's conclusion, he argues that Aristotle's teleology can change the way readers relate to nature. Such a claim certainly cannot be dismissed out of hand; indeed, it has considerable plausibility. One need only recall how Aristotle's doctrines about virtue and character have become central to recent work in ethics to be reminded that the study of ancient philosophy carries considerable promise for modern readers.

In his final chapter, Johnson draws ethical implications from his interpretation of Aristotle's teleology as immanent in things rather

than as transcending them. He argues that the species-specific ends of plants and animals are intrinsically valuable. Though other natural things can be made into instruments of human intentions—all artifacts are essentially the product of human ends that have been superimposed on materials naturally predisposed toward another end—Johnson claims that human techniques have a natural limit derived from what is necessary for our survival and successful functioning. To exceed that limit is to act in a way that is contrary to nature. In this way, Aristotle's distinction between κατὰ φύσιν and παρὰ φύσιν has ethical consequences.

Grasping the ethical consequences of the ends of nature, Johnson argues, is part of what constitutes theoretical wisdom. By beholding natural ends, we come to comprehend our place in a larger whole. It is thus part of the task of contemplation to recognize the limits implicit in naturally appointed ends of a well-ordered cosmos. For the philosopher, these ends constitute a proper and worthy item for contemplation and so become a part of the ultimate end of human life (though Johnson is careful not to turn the benefit of doing so into an anthropomorphic justification of other ends after all). Just as the wise person realizes that practical wisdom is not the highest wisdom, so too, Johnson claims, he or she understands that human ends as carried out through technology cannot trump other natural ends beyond naturally imposed limits.

A 'green' Aristotle is an interesting and even attractive notion, and the conviction that human intentions are incidental to the natural ends of organisms may indeed have profound consequences. But the argument goes well beyond anything in the corpus. When in the *Nicomachean Ethics* Aristotle praises the relatively few needs of the contemplative life against those of the life of political involvement, he does not invoke the intrinsic value of the resources that the moderate philosopher will not be expending. Though the highest wisdom provides, I am sure, protection against over-reaching, Aristotle's Greek cultural context suggests that one who forgets human limits is tempted to presume godlike importance, not to become a despoiler of the environment. In a world centuries before the machine age and exploding populations, when human survival was made tenuous by the constant threat of disease and poor harvest, it is hard to see how any amount of consumption of natural things (animal, plant, or mineral) could have been seen as exceeding a natural limit.

As John Locke thought 2000 years later, nature's abundance had no limit. Environmentally conscious readers of ancient philosophy might sense a kindred spirit in a philosopher who spent years wading in Mediterranean tidal pools gazing in wonder at shellfish. But their cause needs more direct support.

Perhaps inevitably for a book of this scope, there is sometimes a feeling of a survey. The range of topics covered in the table of contents is truly impressive; in practice, the pages devoted to some of them can be quite few. One may also feel surprise at how certain material is announced. Aristotle's biology offers the most extensive application of teleological principles in the corpus. Yet Johnson does not give the core of this material, *De partibus animalium* 2–4, a systematic reading. Rather, passages from these books are selected and treated topically, with relatively few passages used to illustrate key points of his interpretation. And despite Johnson's striking take on an animal's being adapted to its environment, I was disappointed by how little is said about *Historia animalium* 8-9, which is filled with careful observations of animal ecology. A few passages are quoted and others are referred to. But there is no sustained exploration of the details of the information Aristotle gathered so carefully. Even if (as Johnson says) the *Hist. an.* is a preliminary collection of data that does not include teleological explanations, evidence pertaining to adaptation is plentiful in these books; and I for one feel the lack of a fuller discussion of it.

This may just mean that Johnson's book is not the book I would have written (the unspoken lesson of many a review!). It is not, to put it impersonally, a study of teleology in Aristotle's biology. What it is, is a careful study of teleology as it permeates Aristotle's philosophy, and as such, one that at least touches all the bases—and then some. Like any good work of scholarship, it invites further efforts along the directions it has laid out.

While it is notable that Johnson traces the origin of interpreting Aristotle's teleology as immanent and not transcendent to commentators from as much as a century ago, it is both a bit disingenuous and ultimately unnecessary for him to claim to 'reopen' that approach: as he readily admits, this view has had many advocates since. True, none of the interpretation's more recent proponents have devoted a book-length study to teleology, a fact Johnson cites as justification for

his own effort. But as it stands, Johnson offers a comprehensive examination of the doctrine as it appears throughout the corpus and as it bears on more general philosophical ideas in Aristotle and beyond. The degree of success achieved in meeting these goals is, it seems to me, justification enough for this stimulating, far-ranging work.

Stöffler's Elucidatio: The Construction and Use of the Astrolabe edited and translated by Alessandro Gunella and John Lamprey

Cheyenne, WY: John Lamprey, 2006. Pp. xviii + 249 (with 73 plates).
ISBN 978–1–4243–3502–2. Paper $50.00[1]

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How does one review a classic? Johannes Stöffler's treatise on the astrolabe, *Elucidatio fabricae ususque astrolabii* (*Explanation of the Construction and Use of the Astrolabe*), while not the most innovative treatise ever written, was certainly the most influential in the Renaissance. It was reprinted 16 times after its original publication in 1513, and virtually every treatise on the astrolabe since has referenced it. In fact, it was common to refer to the normal planispheric astrolabe as a 'Stöffler astrolabe' in Renaissance literature.

Johannes Stöffler (1452–1531) was the first to hold the chair in mathematics at the University of Tübingen (1507).[1] In addition to his treatise on the astrolabe, Stöffler also published books of astronomical tables and wrote on sundials and astrological instruments. He operated an atelier producing instruments and globes. The first edition of his treatise on the astrolabe was published in Oppenheim in 1513, with later editions from Mainz, Frankfurt (in German), Paris (10 editions), and Cologne. The edition translated here is from 1553 and was published in Paris by Guillaume Cavellat.

Clearly, this success stems from the fact that the treatise is clear, concise, and complete for its time; and that it requires only a modest background to understand. It contains detailed instructions on how to lay out the components of a planispheric astrolabe and how to use this astrolabe for common problems. Then or now, any interested person with moderate drawing skills could make a perfectly useable

[1] This book is available only from John Lamprey at lamprey@frii.com.Please include the word 'book' or 'Elucidatio' in the subject line of any inquiries.

[1] Johannes Kepler attended Tübingen in the next century.

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astrolabe with nothing more than this book, drawing tools, and a few sources giving current star positions, a modern calendar, and latitudes for places of interest.

The history of treatises on the astrolabe is rich. The first known treatise devoted strictly to the astrolabe was by Theon of Alexandria (Hypatia's father) in about 375. The treatise itself has been lost except for the table of contents which is included in a later work. The first treatise describing actual instruments is by John Philoponus of Alexandria (Johannes Grammaticus) in the sixth century (*ca* 530).

The earliest treatises concentrated on how to draw the astrolabe plate and how to use it to solve common problems. Islamic astronomers added a solid theoretical foundation. Al-Farghānī was the first to establish the mathematical theory of the astrolabe. In the mid-ninth century, al-Khwārizmī applied analytic methods to the astrolabe's design. However, most medieval Islamic astrolabes were designed using tables prepared for that purpose rather than from first principles.

Later, such notable Persian scholars as al-Bīrūnī (973–*ca* 1048 [363–440 AH]) and Nasīr al-Dīn al-Tūsī (1201–1274 [598–673 AH]) wrote detailed treatises on the astrolabe. In 986–987 [376 AH], 'Abd al-Rahmān ibn 'Umar al-Sūfī wrote an amazing treatise of 386 chapters presenting 1,000 uses for the astrolabe.

The astrolabe followed the expansion of Islam into Moslem Spain (al-Andalus, Andalusia). Knowledgeable treatises from Spain date from around 1025, but clearly the astrolabe was known earlier in Western Islam. A treatise on the use of the astrolabe by ibn al-Saffār (1026 [417 AH])[2] became very influential in Europe in a Latin translation made by John of Seville during the middle of the 12th century. This translation, which incorporated both an account of the astrolabe's construction and instructions for its use, was re-edited, copied, and expanded many times, eventually becoming the most widely used text on the astrolabe. All the early treatises on the astrolabe were based in some way on earlier Western Islamic treatises and contributed to the adoption of Arabic names for stars and other astronomical elements.

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[2] This treatise is often falsely attributed to Māshā'allāh or, occasionally, to Maslama ibn Ahmad al-Majrītī, al-Saffār's teacher.

Transmission to Christian Europe of Islamic scientific knowledge in general and of the principles of the astrolabe in particular was aided by Christian monasteries on the border with Andalusia. Notable was Santa Maria de Ripoll, a Benedictine monastery near the Pyrenees whose monks translated many Arabic documents for their own use in the 10th and 11th centuries. One manuscript includes at least 11 sections concerning astrolabes. The rapid movement of this knowledge is demonstrated by the fact that Hermann Contractus (Hermannus, Hermann the Lame) (1012–1054), a student at the Reichenau monastery school in Germany, wrote a treatise on the astrolabe based on a Latin translation of the Ripoll manuscripts by Llobet of Barcelona.

The earliest Latin astrolabe treatises were based on Arabic translation; they were not very well organized and often contained meaningful errors (such as incorrect instructions on how to divide the ecliptic). Adelard of Bath (*ca* 1080–*ca* 1160), who traveled extensively in the Middle East, where he learned Arabic and the basics of Islamic science and astronomy, dedicated a treatise on the astrolabe to Henry Plantagenet (Henry II) in 1147. Newer and better treatises evolved in the 13th century as more experience was gained. The most widely used treatise was compiled from several texts, mainly the translation of ibn al-Saffār mentioned above. This translation became the standard text for the astrolabe's construction and use, and is referenced often by Stöffler. One notable, entirely European contribution was the *De plana spera* in the early 13th century by Jordanus de Nemore, which presented the theoretical foundation for the stereographic projection.

The first European treatise in the vernacular on the use of the astrolabe was written in French by Pèlerin de Prusse in 1362 at the request of the Dauphin Charles, later Charles V (reigned 1363–1380). In about 1390, no less of a literary figure than Geoffrey Chaucer wrote a treatise on the astrolabe in vernacular English for his 10-year-old son, Lewis, which a later scribe with a sense of humor apparently subtitled *Bread and Milk for Children.* This work, which is hard going for an informed adult much less a child, demonstrates a high level of astronomical knowledge and, as a vernacular work, received fairly wide circulation.

Meanwhile, back in the Islamic world, treatises on astronomical instruments continued to develop and, perhaps, reached their peak in

the 13th century. Texts and tables covered the entire range of practical Islamic astronomy and led to a rich literature in instrumentation that included astrolabes, sundials, and quadrants. For example, a 14th century Mamluk treatise by Najm al-Dīn al-Misrī includes detailed illustrated descriptions of over 100 variants of the astrolabe, sundial, and quadrant.

Given this long history of treatises on the astrolabe, what sets Stöffler's apart? The answer seems simple: Stöffler's treatise was a printed book, whereas the older treatises existed only as handwritten copies. As a printed book, it could enjoy wide distribution at a reasonable price. That is not to say it is not a very good book, because it is; but it did hit the market at exactly the right time with exactly the right information as the popularity of the astrolabe was nearing its peak in Europe. The practice of astrology was almost universal in 16th century Europe and the astrolabe was a convenient astrological tool for constructing horoscopes. The popularity of the astrolabe was directly related to the cultural importance of astrology. For example, the conjunction of the Moon and all the planets in Pisces in February 1524 was considered an omen of terrible catastrophes and prompted tracts by no fewer than 56 different authors, including Stöffler.

The treatise has two parts: 'Construction' and 'Use'. The part concerning construction contains very clear, illustrated instructions on how to lay-out the front and back of a standard planispheric astrolabe. The instructions can be used today if the reference tables are supplemented with modern values.

The part on usage begins with instructions for such basic uses of the astrolabe as finding the time from the altitude of the Sun or a star and describes several methods of timekeeping. It also provides some instruction on astrological topics such as house systems, planetary influences, ascensions, and revolutions. This part finishes with some very interesting material on using the astrolabe to solve surveying problems, which I particularly enjoyed.

There are two ways to contextualize Stöffler's treatise. It can be understood in relation to other old treatises with English translations and in relation to modern treatises. It seems reasonable to restrict the present discussion to European treatises, since including Islamic treatises, which were more complete and sophisticated, would enlarge the subject to unmanageable proportions.

There are two other old European treatises on the astrolabe available in English.[3] Both were originally written in the vernacular and have recently been made widely available in modern English. The first is the *Practique de l'astrolabe* (1362) written by Pèlerin de Prusse in French: this treatise has been published in translation by Laird and Fisher [1995]. The other is Geoffrey Chaucer's treatise which has been published, transliterated and documented in a number of books. The most widely quoted commentaries are Skeat 1872 and North 1988. There are many, possibly hundreds, of commentaries on Chaucer's treatise. Several transliterations into modern idiomatic English are available on the web.

These two earlier treatises only describe the astrolabe and its uses in brief. Stöffler's treatise supplants both with its detailed description of how to lay out an astrolabe and and its detailed figures illustrating both the instrument and its uses. All three provide insight into astrological thinking of their eras.

Gunella and Lamprey's translation can also be understood by comparing Stöffler's treatise to modern works of the same genre.[4] The modern treatises on the astrolabe written during the last half-century in order of date of publication are:

- Michel, Henri. *Traite de l'astrolabe.* Paris: Librarie Alain Brieux, 1976.[5]
- Saunders, Harold N. *All the Astrolabes* Oxford: Senecio, 1984.
- D'Hollander, Raymond. *L'Astrolabe. Histoire, thèorie et pratique.* Paris: Institut océanographique, 1999.
- Tardy, Jean-Noël. *Astrolabes. Cartes du ceil.* Aix-en-Provence: Édisud, 1999.
- Morrison, J. E. *The Astrolabe.* Rehoboth Beach, DE: Janus, 2007.

I am using the term 'treatise' in a very narrow sense in this regard. There are many other books that contain basic information about

[3] There is also Georg Hartmann's treatise which was written in 1527; but this was never published until the translation by Lamprey [2002]. This treatise shows a high level of technical sophistication.

[4] I should advise the reader that I have known John Lamprey for many years, that I did some proofreading of his manuscript, and that I will be comparing Stöffler's treatise to my own [2007] *inter alia.*

[5] A PDF file of an English translation by James E. Morrison may be obtained by contacting him at janus.astrolabe@verizon.net.

the astrolabe's design and use and many have lovely pictures of old instruments. However, for present purposes, I will restrict this discussion to complete books devoted to the technical aspects of the design and use of the astrolabe.

A brief perusal of any of these texts immediately shows the difference in how the design of these instruments is described today *versus* how Stöffler approached the same subject.

- All the modern treatises devoted to the astrolabe rely on mathematical constructions based on relatively simple trigonometry and analytic geometry to describe the arcs and circles on the astrolabe plate. Stöffler's treatment is purely geometric, with no supporting theory presented. D'Hollander [1999] and Morrison [2007] both have detailed derivations of the underlying mathematics. Saunders [1984] and, to some extent, Tardy [1999] contain some of the mathematical background, but both are so poorly organized that it is difficult to dig it out. Michel [1976] contains the basic math, but lacks detail in its application.
- Stöffler has very detailed instructions on how to draw the astrolabe's components with only a straight-edge, compass, and protractor. This approach has undeniable appeal to those whose talents are more artistic than technical. Only Morrison [2007] presents both geometric and analytical methods.
- D'Hollander [1999] and Morrison [2007] include extensive historical background. Michel [1976] has a very brief historical overview; Tardy [1999] and Saunders [1984] have very little. Stöffler has none, unless you consider the book itself to be living history.
- All the modern treatises on the astrolabe cover universal astrolabes and other instruments related to the astrolabe, while Stöffler's is concerned only with the planispheric astrolabe. This is not a criticism: universal astrolabes were not well known in Europe until after Stöffler's treatise was published, although they were known to specialists and academics.
- Finally, D'Hollander [1999] and Tardy [1999] are available only in French. The English translation of Michel [see 159n5, above] has limited availability to date. Saunders 1984 has been out of print for many years. This leaves Morrison 2007, this translation of Stöffler's *Elucidatio*, Lamprey's translation of Hartmann's work [2002], and the older treatises mentioned above as the only astrolabe treatises available in English.

I am reluctant to comment in any detail on the fidelity of the translation itself since I do not read Latin. Still, I can say that the translation is clear, well-presented and illustrated, with relevant footnotes as required to clarify obscure references in the text. In some places, the translation seems a bit strained as idiomatic English, which is probably unavoidable since it was done from Latin to English *via* Italian. I would quarrel with tiny details on the selection of certain words, but the meaning is never less than clear. Many translations of classical material include a commentary. The subject matter in this instance is simple enough that additional annotation or explanation is not really needed, although some commentary on the accuracy of Stöffler's calculations would have been welcome.

In the final analysis, this translation is a major contribution to the literature in English on astrolabes and should be in the library of every student of the history of science. I am hopeful that the publication of this translation and Faith Wallis' translation of Bede's *De temporum ratione* [1999] inspire others to undertake projects in the history of astronomy of similar value.

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Atoms and Alchemy: Chymistry and the Experimental Origins of the Scientific Revolution by William R. Newman

Chicago/London: University of Chicago Press, 2006. Pp. xiv + 250. ISBN 0–226–57697–3. Paper $30

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Over the past 20 years, William Newman has produced a substantial number of articles and books aimed at repositioning alchemy in medieval and early modern science. He has insisted on the experimental program which, he claims, was an integral part of alchemy. The interpretation of alchemy as a spiritual discipline was, in his view, popularized by the occultists of the 19th century and then adopted by Carl Gustav Jung and Mircea Eliade. Newman has even proposed to replace the term alchemy with 'chymistry', thereby stressing the continuity of alchemy with modern chemistry. In the present book (bearing the term 'alchemy' in its very title), Newman has a very ambitious aim that goes beyond reassessing the role of alchemy. As he puts it,

> my hope is that the present book, by revealing the violent rupture that alchemy helped to precipitate in traditional scholastic matter theory and by outlining the role of this discipline in the formation of the experimental version of the mechanical philosophy, will give cause for reconsideration of the 'grand narrative' of the Scientific Revolution. [19]

For Newman, the Western alchemical tradition was both experimental and corpuscular:

> The alchemists of the High Middle Ages established an experimentally based corpuscular theory that would develop over the course of several centuries and eventually supply important components to the mechanical philosophy of the Scientific Revolution. [26]

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As he sees it, the source of modern corpuscular theory of matter was not ancient atomism, but Aristotle's *Meteorologica*.

The dismissal of ancient atomism as not influential is surprising, given the well-known interest of Renaissance and early modern natural philosophers in Epicurus and Lucretius. But, according to Newman, ancient atomism was metaphysics and had little or nothing to do with modern science. He spells out his position as follows:

> The metaphysical origins of Democritean atomism are clear enough, even if the details of his system are lost in the haze of historical amnesia. His revivers Epicurus and Lucretius, who came at opposite ends of Hellenistic period, made important additions to the Democritean system, but they too were strangers to the laboratory. [25]

Having dismissed the ancient atomists as 'strangers to the laboratory', Newman then maintains that the source of early modern corpuscular philosophy was Aristotle. In his view,

> it is well known that *Meteorology* IV lays out a detailed corpuscular description of matter expressed in terms of *poroi* (pores) and the *onkoi* (corpuscles) that can fill them. [66]

Newman takes for granted that Aristotle's ὄγκοι were corpuscles and that his πόροι were void spaces filled by corpuscles—as in modern mechanical philosophy. But it is not so easy to make sense of the apparent contradiction between Aristotle's unambiguous rejection of atomism in his works (including the criticism of the doctrine of πόροι as empty spaces to be found in *De gen. et corr.* 326b) and the interpretation of *Meteor.* 4 as containing a corpuscular theory of matter. In 1915 [35–36, 189–199], Hammer-Jensen claimed that in *Meteor.* 4 there is an atomistic theory; and for this reason, she concluded that this book is spurious and should be attributed instead to Strato of Lampsacus. More recently, Carmela Baffioni [1981, 35–36] has maintained that the πόροι and ὄγκοι in *Meteor.* 4 are not to be seen as evidence of Aristotle's adherence to atomism. She claims that the atomists employed the term κενόν for void, and that the term ὄγκοι can hardly be translated by 'atoms'. While in *Meteor.* 4 πόρος is only once identified with κενόν, Strato did not differentiate πόρος and κενόν. But this was Strato, not Aristotle. In short, the existence of an Aristotelian corpuscular theory of matter remains at least debatable.

As Newman has pointed out on many occasions, a key text in the history of alchemy was pseudo-Geber's *Summa Perfectionis*, which 'contains a comprehensive theory of mineral formation, chrysopoeia, and artisanal laboratory operations expressed in terms of particles and pores' [13]. The *Summa Perfectionis*, according to Newman, 'developed a corpuscular side of Aristotelian matter theory that is present in book 4 of the Stagirite's *Meteorology*...' [13]. After the pseudo-Geber, Newman takes into account Erastus and Libavius, both outspoken opponents of Paracelsus. Whereas Erastus rejected alchemy, Libavius aimed at reforming chrysopoeia and defended medieval alchemists against Paracelsus. Newman traces the lineage of corpuscular matter theory as it starts with Aristotle *Meteorology*, proceeds *via* pseudo-Geber and Libavius, and finally reaches Sennert and Boyle in the 17th century. He describes this descent of modern atomism as follows:

> The corpuscular theory of the *Meteorology* merged with the tradition of Geberian alchemy—now seen in the light of Paracelsian *spagyria*—to yield a widely held physical theory based on the experimental analysis and synthesis of substances believed to consist of minute particles, a position that would reach its consummate expression in the work of Daniel Sennert. [67]

Sennert wanted to combine Aristotelianism and atomism, and resorted to chemical experiments to prove the existence of atoms, notably by means of the so-called reduction to the pristine state.[1] For Newman,

> Sennert followed the lead of Libavius in making Democritus into a sort of Aristotelian and Aristotle into a sort of quasi-Democritus.... [94]

Newman's discussion of Sennert, which focuses on his *De Chymicorum* (1619), is insightful and takes into account the relationships of Sennert to late Aristotelians such as Scaliger and Zabarella.

The third part of *Atoms and Alchemy* deals with Boyle's matter theory. Newman restates the views held by Kuhn and Marie Boas, among others, that Boyle's matter theory was strictly mechanical.

[1] Meinel 1988 documents this well.

He distances himself from both Kuhn and Boas by assuming that no opposition existed between corpuscularianism and chemistry, and that alchemy played a central role in the establishment of mechanical philosophy. The evident tension between Boyle's mechanism and the notion of active principles (i.e., seminal principles) which some scholars have stressed in the past two decades, receives only passing reference in a footnote.

Overall the volume, which is beautifully illustrated, is an important contribution to intellectual history, notably to the ongoing debate about early modern theories of matter.

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Galileo's Instruments of Credit: Telescopes, Images, Secrecy by Mario Biagioli

Chicago/London: University of Chicago Press, 2007. Pp. xii + 302. ISBN 0–226–04562–5. Paper $20.00

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More than a decade has passed since Mario Biagioli's *Galileo Courtier: The Practice of Science in the Culture of Absolutism* provided historians of science with a novel and convincing structure for understanding the rise and rise of Galileo Galilei. The Renaissance court, both in ideal and actual forms, was depicted and deployed as the central instrument in enabling Galileo's spectacular career path. Rather than merely providing a limiting social context for science or a consuming market for its products, court life and courtiership were shown to provide powerful models for producing natural philosophical knowledge, especially the virtuoso display and courtly debate, at which Galileo excelled. Patronage, clientism, gift-giving, and etiquette swiftly became central categories for understanding the very practices constituting early modern science.

Biagioli's welcome return to the field, *Galileo's Instruments of Credit: Telescopes, Images, Secrecy*, revisits the period analyzed most successfully in *Galileo Courtier*, 1609 to 1616. In these years, using a modified version of the recently invented spyglass, Galileo made a series of spectacular discoveries: lunar mountains comparable to those on Earth, four moons orbiting Jupiter, and strange spots on the face of the Sun, to name only the most famous. These celestial phenomena were used, *Galileo Courtier* argued, to work two crucial transformations: Galileo himself left his university job as a mathematics professor at Padua to become a court philosopher and mathematician for the Medici in Florence; and he used his astronomical observations to threaten the constitution of the dominant Ptolemaic cosmology. *Galileo Courtier* insisted on a strong relationship between these social and epistemological transformations.

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The very success of Biagioli's model for charting and explaining Galileo's astronomical apotheosis also raised some unresolved historiographical problems. There was a danger that court society might seem as deterministic as other discarded models, a simple wind-up mechanism with which to explain away one of the most important series of discoveries of the Scientific Revolution. And the model itself, while working perhaps too well as a tool to understand Galileo's rise, seemed to strain to near breaking point when applied to the trial of 1633. The idea that Galileo was condemned in part because court favorites have a general tendency to fall after they rise ignored the complexity of actors' categories and motives. Biagioli's own account was finely nuanced, but his readers have sometimes been less subtle.

Galileo's Instruments of Credit both builds on *Galileo Courtier* and replaces it. Biagioli invokes the logic of Derrida's supplement at several points; his own book embodies it. The structures of the older book seem too rigid when read against the more supple methodologies of the new work. Between the two is a historiographical shift from structuralism to post-structuralism. Rather than positing a dialogic dynamic between the social and epistemological, Biagioli here provides us with a series of tactical interventions that well display the flexibility and range of both his, and Galileo's, thought. The certainties of omniscient actors playing clearly defined roles in fixed structures are gone. Now we are witness to bricolage in action: 'Product, producer, and market were shaped simultaneously' [3].

The book is arranged chronologically in four chapters. Several are adapted from previously published versions, and an introduction and epilogue show the relationship between them. Whereas *Galileo Courtier* played variations on a central theme as it moved through Galileo's career, *Galileo's Instruments of Credit* uses the implicit chronological narrative as a series of points of departure into virtuosic analyses both of specific debates and the methodologies developed to understand them. It has been claimed that historians, like dog owners, resemble their subjects. If *Galileo Courtier* were a brilliant manifesto about a brilliant manifesto, *Galileo's Instruments of Credit* not only secures Biagioli's reputation, but displays the admirable adaptability and responsiveness of his work. The *ad hoc* nature of Galileo's strategies is identified and remobilized by Biagioli to spectacular effect.

The introduction and four chapters lead us through four very different disputes. The substantial introduction, 'From Brass Instruments to Textual Supplements', depicts the artisanal economy constructed and inhabited by Galileo in Padua. As was normal at the time, the university professor kept his fingers in several pies: he ran a boarding house for students, gave them private lessons, and sold them both mathematical instruments and manuscript instruction booklets. Biagioli reconstructs these overlapping and overlooked economies well, largely in order to contrast them with the very different economy of the Florentine court to which Galileo would soon return. If Galileo's Paduan economy was based on manual and oral labour, with knowledge produced in close proximity to its consumption, Florence promised a more cerebral existence of leisurely writing books for print that was free from the mess of teaching and students. While the image of two separate Galilean economies—the utilitarian Venetian Republic and the courtly Tuscan Grand Duchy—is certainly appealing, it is unclear to what extent Galileo shifted from one to the other in 1610. His Medici courtiership predated his telescopic discoveries; and his Paduan alumni and friends offered an international network for the rest of his life and functioned, even in the years before his transfer out of the Veneto, as a link to court life. It is unclear whether Galileo was quite as unknown before the telescope brought him fame as is generally assumed: his portrait by Domenico Robusti, now in the National Maritime Museum, Greenwich, seems to portray him around 1607. He was a central figure in both Paduan and Venetian intellectual circles, and certainly planned a series of publications before the *Sidereus Nuncius* happily disturbed him. On the other hand, a 1608 payment authorization from the Medici mistook his given name as 'Giulio' implying a lack of familiarity even amongst Florentine bureaucrats.[1]

Biagioli regards Galileo's only noteworthy intervention in the world of print in the pre-telescopic period to be an edition of his instruction manual for the geometric compass, with its modest print run of 60 copies produced out of fear that his neat local monopoly

[1] Archivio di Stato di Firenze, Mediceo del Principato 300, fol. 136r-v. (Medici Archive Project, http://www.medici.org/, entry 13860 in the Documentary Sources database).

on his version of the instrument might be threatened. This fits Biagioli's analysis well, but there are other texts to consider: Galileo may well have had a hand in the pro-Copernican satirical dialogue in Paduan dialect, published in 1604, which Biagioli relegates to a footnote. Contemporaries thought Galileo was the author; and even if he were not, the example of pseudonymous publication adds a nice twist to Biagioli's story of the construction of 'aura' and the indeterminacy of context. While *Galileo's Instruments of Credit* is not a biography of Galileo, any reconstruction of the economy of his household in these years should surely mention Antonio Poppi's important discovery of 1993 that Galileo's servant and amanuensis Silvestro Pagnoni had testified against him to the Inquisition in 1604. Edward Muir has recently provided a provocative analysis of Paduan intellectual and political life in this fraught decade, and Venetian protection of Cremonini and Galileo would certainly work as a nice backdrop to Biagioli's analysis of the later lack of protection leading to the injunction of 1616 against Galileo (and, indirectly, to the trial of 1633).

Chapter 1, 'Financing the Aura: Distance and the Construction of Scientific Authority', argues that distance, usually seen as a hindrance in the production of scientific knowledge, should be reconsidered as a crucial tool. The episode under consideration, one of the best known in the history of science, is the publication of the *Sidereus Nuncius*. Biagioli's analysis recasts the familiar terms through which it is usually understood, bringing what previously seemed extraneous or disruptive into play as central and formative. By exploiting the distance between himself and his potential patrons, Galileo managed not only to buy himself time but also to construct a self-authenticating process in which all participants acted on partial knowledge and blind trust. This is a far more nuanced and sensitive reading of the way in which the Medicean moons, for example, were negotiated into existence than the usual center/periphery model of discovery. Biagioli steps out of Galileo's world for the second half of the chapter to show the crucial relevance of his model even to the production of matters of fact in the early Royal Society. Shapin's Boyle is his target here, not from the usual direction of anti-constructionists, but from a more radical position that starts to make the entire Society feel real only, or mainly, in so far as it is virtual. Henry Oldenburg's correspondence is often studied as a web spun and sensed by the bloated spider; Biagioli sees it instead as a

dense rhizome produced by multiple actors, with its center defined by its limits. The results of this experiment are striking: partial information and distance no longer seem to prevent facts from being forged but rather enable them. Credit and credibility rely on rhetorical inflation, of course, but this happens only at a distance. An even more extreme version would be Lana Terzi's contemporary Brescian academy, which existed only virtually.

There are, however, some problems here. Distance, in itself, explains nothing in these examples; and Biagioli uses it as shorthand for something more difficult to quantify. What is needed is a map of each actor's perception of power, time, and distance rather like mobile versions of Braudel's isochronic maps with two extra dimensions added. Galileo exploited Giuliano de' Medici in Prague, and the distance between the two was part of the story of the production of the Medicean moons; but similar effects could be produced by mis-timing posts or using slow routes. Distance has to be understood historically, as Carlo Ginzburg showed in *Wooden Eyes*, for it to become an actor or a factor in history. One of the great unspoken ironies of this chapter is that the validation of the telescopic instrument that promised to manipulate distance and collapse time took place through such old-fashioned technologies as postal systems and was, at the same time, compared to an ideal, angelic, dematerialized message.

There is another issue at play that also deserves further thought: one of the things distance does produce in the early modern period is archives. The existence of the correspondence that we use is to a large extent the product of distance. One tended not to write letters to neighbors unless a visible paper trail were needed, though some conversations, such as trials, were also written down. So when Biagioli makes an argument about the effect of distance in the production of knowledge, there is a hidden issue about the existence or non-existence of sources. Galileo was no Descartes, withdrawing from the world and engaging with it through writing; he forged his reputation through distance, as Biagioli shows, but also through proximity, though the sources may be harder to locate for these actions. Thus, Biagioli's counterintuitive concluding aphorism, 'local knowledge is an oxymoron' [74], may itself only be true for the locale in question. His analysis certainly provides new and exciting models to understand and exorcise the Derridean specter of the 'metaphysics of presence' [74] that haunts the sociology of scientific knowledge, but

the model is more an essential supplement to the sociology of scientific knowledge and Biagioli's own earlier work than a replacement system. Some knowledge is produced through the flux of virtual negotiation, and the discoveries of 1609–1610 are excellent examples. But this does not invalidate other models of knowledge production or render the concept of local knowledge redundant. As the other chapters in Biagioli's book show, many ways of producing knowledge can compete and coexist.

Chapter 2 'Replication of Monopoly? The Medicean Stars between Invention and Discovery', offers a different analysis of the same period, supplementing the account based on distance with one based on the relationship between credit and disclosure. Biagioli identifies a peculiar tactic adopted by Galileo during the crucial period after his initial astronomical observations. Galileo, Biagioli argues, was caught between two competing demands: in order to secure the status of his discoveries, he needed others to replicate them; in order to secure his current and future priority in the field, he needed others not to replicate them. Biagioli asks a fundamental question to chart Galileo's response to this bind: to whom did he send decent telescopes and to whom did he send copies of the *Sidereus Nuncius*? Historians have generally presumed that these objects travelled together or that astronomers immediately got hold of telescopes and the book, while a less expert audience received only the book. Biagioli's findings are surprising: Galileo sent telescopes to patrons who could not use them, and sent his book to astronomers who could not verify its observations without telescopes. Most interestingly, Biagioli makes a strong case for the mutability of the objects themselves as they passed through these different economies:

> [Galileo's] tactics (as well as those of his competitors and critics) were not unnecessary obstacles on the path to truth, but constitutive elements of the production of the objects he called 'Medicean stars'. [135]

Biagioli roots Galileo's tactics and the telescope in the local culture of invention and charts their nonlinear transition to a culture of discovery. As William Eamon has shown, secrecy was not just a tactic deployed by inventors but an epistemology of nature itself: one of the jobs of the natural philosopher was to force nature to yield her secrets. The process of translation from one economy to another is still poorly understood.

New readings are also offered for the very 'objects' that Galileo discovered: these are usually depicted as stable physical objects, but Biagioli shows how, instead, they both came into being and were presented as processes rather than objects. It was only through the establishment of periodicity that the Jovian moons became discrete objects. There is one bibliographical slip in this analysis, though. The illustrations by which this information is conveyed in the *Sidereus Nuncius* were originally intended to be woodcut strips, with the relative position of the planet and its moons initially cut into a table, then each observation sawn off and surrounded by text. This idea was rejected, probably by the printer, who substituted the woodcuts (which, like all the images of stars in the book, were meant to be printed white on black, not black on white) with typographical characters. This detail does not affect Biagioli's argument in any way, but offers an interesting example of proto-digital technology, with the relative positions of two signs '*' and '0' conveying all Galileo's data. This was probably done to save money, not to start the digital age. Biagioli himself occasionally lapses into powerful yet inaccurate anachronism by referring to the sequences as 'movies'. They seem so now to us, but surely there existed a rich contemporary artistic and rhetorical vocabulary to describe the depiction of stages in a story? Painted narrative cycles were especially strong in 16th-century Venetian *scuole*; going to the movies might be a distraction to understanding this.

Visual evidence is also central to chapter 3, 'Between Risk and Credit: Picturing Objects in the Making', where the analysis of satellite periodicity is extended to Galileo's debate with Christoph Scheiner over marks on the face of the Sun. Here the depiction of sequence is contextualized, though the reader remains unclear about how innovative Galileo's image-making agenda was and to what extent it was strictly his and not that of the printer or patron. The sunspot debate was complex and is hard to reconstruct—an edition by Albert Van Helden and Eileen Reeves, to which Biagioli was a one-time contributor, is due soon from Chicago—and Biagioli does a good job of both conveying what happened and granting access to the actors' viewpoints. He crucially recasts the two protagonists' differing modes of depicting sunspots not as a competition of realism, but as appealing to, and constructing, different audiences. Galileo's images, committed to representing a process unfurling in order to posit the

existence of a troublesome object, ended up, Biagioli argues, by doing much more: they convinced him to adopt a dangerous 'ontology of change' [217].

The final chapter, 'The Supplemental Economy of Galileo's Book of Nature', addresses the series of manuscript letters and tracts that culminated in the famous 'Letter to the Grand Duchess Christina' in 1615. This text is generally considered to offer a separation of natural philosophy from theology, a kind of disciplinary truce. Biagioli elegantly shows how Galileo's transformation of the trope of the Book of Nature forced him to base the model and authority of natural philosophical knowledge on the theological model. Far from declaring the discipline autonomous, this move rendered it even more reliant on its authenticating source. Biagioli's argument posits a logocentrism at the basis of Galileo's bibliocentrism; but, given the power convincingly attributed to diagrams and illustrations in the preceding chapters, I do not see why this should necessarily be the case. When Galileo uses words like 'language' to describe the content of the Book of Nature, he may well be redeploying his central argument within the 'Letter to the Grand Duchess Christina' that even in Scripture not all words are literal. That he should use a metaphor to make himself understood does not detract from the truth behind the metaphor. In this sense, the printed sunspots, with their suggestive technological effacement of human intervention in their process of production, come close to providing a glimpse of Galileo's actual Book of Nature. The mathematical language in which it is expressed is not another form of representation for Galileo, but a laying bare of the only true qualities humans may know of objects. It is not so much that 'Galileo's book of nature stretched the metaphor of the book so far that it started to fall apart at its many seams' [242], but that the book was itself an extraordinary act of mental experimentation, a single sheet coexistent with the universe imprinted, one assumes, with animate diagrams containing the past, present, and future of the universe. In the same period, several attempts were made to rethink the prison of language—Campanella's pedagogical icons in *The City of the Sun* are only the best known semiotic experiments in an extraordinary century. But this is an aside. The main point of the chapter is to make strange some of the self-evident tools of analysis: terms such as 'obstacle' and 'resource' are given the same kind of shakeup that Galileo himself proposes to overburdened Aristotelian language in

Sunspots. Biagioli suggests that our language itself is metaphysically loaded, and that we should rethink these terms as 'the conditions of possibility for the articulation of Galileo's discourse' [259] rather than as simple explanations of causes.

This is a brave, timely, and welcome historiographical experiment; and it remains consciously open-ended, though far from inconclusive. The debates over comets leading to *The Assayer* in 1623 and the trial documents of 1633 especially, now demand fresh readings. Latent positivism lurks in even the most groundbreaking histories of science, with their tendency to chart the closure of debates by recourse to fully informed actors in control of their resources. Biagioli, by contrast, presents a more disturbing and liberating vision, a nomadic and hybridizing historiography that takes its aporias and differences as it finds them, reconfigures its field as it moves, producing and embracing a Galilean ontology of change.

The Telescope, Its History, Technology, and Future by Geoff Andersen

Princeton/Oxford: Princeton University Press, 2006. Pp. 248. ISBN 978–0–691–12979–2. Cloth $29.95, £18.95

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The Telescope is a book of about 240 pages that was written for the general public rather than the historian of science. If this is the reader's first visit to the wonderful world of the telescope, this book is highly recommended as a good place to start, for one gains an appreciation of the importance of the telescope in the development of scientific ideas as well as of its diverse uses that include surveillance, mapping, and laser weaponry. The author takes the reader on a rapid journey from the time of the invention and application of the telescope in the early 1600s, to the time of Hubble. and up to the present with a discussion of future telescopes. The style is relaxed with digressions along the way that bring the human element to the subject. There is a lot of ground to cover in 240 pages; so some readers may find that their favorite subject within the 400 year old story, from the patent of the telescope in 1608 to the present time, does not receive enough attention or is not even mentioned. Hence, there are gaps in the story of telescope as told by the author; but if one is to keep the book to a manageable size, this is necessary. This book should whet one's appetite for further reading into the fascinating story of the telescope.

The author begins with a short chapter titled 'The Naked-Eye Universe' to lay the background for the introduction of the telescope. He ends the chapter with the understatement that the Dutch optician, Hans Lippershey, 'had invented a device which would dramatically increase the pace of inquiry into the physical universe and usher in the Enlightenment'. This point could have been given more emphasis. The author ends the main text of the book with the statement that the discoveries made with the aid of the telescope 'completely altered the way we view our universe and our place in it' [219].

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Chapter 2 describes the development of the telescope from its humble beginning, when Hans Lippershey, by chance, looked through two lenses separated by several inches and discovered that distant objects appeared larger. When Galileo heard about this new instrument, he quickly constructed one himself; and when he turned it to the heavens, he saw things that no one had seen before. He saw the moons of Jupiter, the phases of the planet Venus, and craters on the Moon— observations that were in various ways not compatible with the accepted doctrines of the day. There now was no turning back, for these findings truly altered how we view the universe and our place in it. Most of the chapter describes Galileo's work with an equal amount of space devoted to further developments up to the early 1800s. The contributions of giants such as Newton, Descartes, and Huygens are briefly discussed in the second half of the chapter.

The contributions of the many makers of telescopes (scientists, craftsmen, and opticians) from Galileo's time to the early 1800s are not even mentioned. There is no sense of the interplay and feedback between the many interacting, scientific, technical, and social forces associated with the telescope's development. There is the making of the telescope by the instrument makers, there are the many uses put to the telescope, and there are the results of that use. These three interrelated stories are important because they continue to be told today. There is no mention of developments in the 19th century.

In chapters 3, 4 and 5, the author describes how an ideal telescope works, the limitations of the telescope imposed by physical laws governing the interaction of light and matter, the wave nature of light, and the nature of the telescope design itself. For example, in a simple lens the focusing properties depend upon the color or wave length of the light. This is not a problem when the telescope uses mirrors instead of lenses. Additional external conditions, such as atmospheric turbulence, reduce the ideal design properties of the telescope and are discussed with sufficient detail that the reader will gain a real appreciation of why the Hubble telescope was developed and why most telescopes built today are on the tops of mountains. All of these requirements led to the current condition that virtually all telescopes made today for professional astronomical observations use mirrors for the main light collector rather than lenses.

Chapters 6 and 7 cover the subject of the measurement of the properties of the light that are collected by the telescope, as well as

the analysis and interpretation of these measurements. 'The next big thing for telescopes', according to the author, is combining the light waves of two or more telescopes in a coherent manner so that interference patterns are produced. One possible result of such a measurement technique would be the observation of objects very close to a star. Stay tuned.

Chapter 8 covers the many issues that must be considered when building a telescope observatory. The engineering process, along with the large number of issues that must be considered, would cause even a genius like Galileo, with his cardboard tubes, a difficult time with it all. Chapter 9 tells the intriguing story of the Hubble telescope, where things went wrong and how they were made right. The author also discusses some of the results obtained from the Hubble telescope, noting the tremendous growth of knowledge that has been added to our understanding of the universe. The Hubble dramatically reaffirms the impact of the discoveries of Galileo on our understanding of humankind's place in the expanding universe.

The book shines in the discussion of advanced telescope techniques in chapter 10 (and in chapter 15 which deals with future telescopes). Active optics, segmented primaries, adaptive optics, laser guide stars, terms we all read about but really do not completely understand, are subjects that the author covers with just the right amount of detail at just the right level. If you add the advances of detector technology and computers into the process, you have the necessary components for a 'renaissance' in telescope building.

Chapters 11 and 12 discuss applications of telescopes, such as surveillance and laser communication, areas one does not normally associate with telescopes. The word telescope has taken on a larger meaning than when it was introduced in 1611. The question may be asked, When is a telescope a camera, or a camera a telescope? One could argue that even the surveillance camera on the street corner is a telescope. If so, the telescope has become ubiquitous in our daily activities though we may not be aware of it.

This leads us to chapter 13 where some non-traditional observatories are described, principally to detect and measure energetic particles, light, and gravitational waves arriving from outer space. Clearly if one is to learn as much as possible about the universe, one must measure the emission of the various forms of energy from the

dynamical processes that occur in space. The author does a good job in discussing these issues.

The title of chapter 14 should be 'Recent Key Discoveries', for it will be hard to top the impact of the initial discoveries of Galileo. That said, the amount of new information we are obtaining about the universe is overwhelming and the pace will only quicken as new and bigger telescopes become operational.

The book ends with a very good discussion of future telescopes. Aperture fever sets in as we desire to see more distant objects with higher and higher resolution. As this reviewer pointed out earlier, the 17th-century story of the desire for bigger and better telescopes is repeated on a larger and more expensive scale. We now understand the physical laws that govern light behavior, which was of concern in the 17th century. Today it becomes an epic effort in engineering with the goal of understanding better the forces that mold our universe, a universe that almost defies comprehension because it appears to be beyond our everyday experiences and imagination.

The Mathematics of Egypt, Mesopotamia, China, India, and Islam: A Sourcebook edited by Victor J. Katz

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It is no understatement that the arrival of this unique reference has been eagerly anticipated by the community of historians of mathematics. Never before has a single work delivered to scholars such a rich and comprehensive guide to the history of non-western mathematics. Victor Katz was the perfect candidate to initiate and oversee this project and, as the editor, his vision of a single sourcebook in which each cultural area was prepared by a renowned specialist in their field has been fully accomplished. The resulting product is a thorough and insightful coverage of five key centers in non-western mathematics: Egypt, Mesopotamia, China, India, and Islam, each of which is allotted a single chapter with its own reference section and bibliography. Indeed, the selected authors epitomize the new trends in the history of mathematics: in effect, they show that to produce well-rounded, critical, and perceptive accounts of mathematics past, you must be fluent in the requisite languages and that you must have familiarity with the primary sources, awareness of the broader issues in historiography, as well as mathematical facility. As expected, the authors show historical and mathematical sympathy, always with a notable respect for preceding generations of scholarship when they disagree; and they display an impressive (even daunting) knowledge of other intellectual fields, including anthropology, archaeology, linguistics, material culture, paleography, philology, philosophy, and sociology, to name but a few, which they draw upon where appropriate to deepen and enrich their accounts. Far from being disjointed, as can be the case with a multi-author collection, this work is highly cohesive. In fact, one highly valuable, possibly unanticipated, consequence of this book is the presentation of five distinct methodologies by top professionals who each tackle the history of mathematics

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differently—sometimes subtly, sometimes substantially—but always coherently, in a way that you would never find in a monograph.

The work opens with a modest introduction: Katz politely and quite properly lets the authors speak for their own fields. Each chapter is self-contained and its subject is introduced and illustrated by means of primary sources, translations, and mathematical and historical commentaries. Excerpts are carefully selected to give an overview of the breadth of the field and, where appropriate, pictures, diagrams, metrological tables, and transcription guides clarify the main body of the text. All accounts give the reader a reassuring taste of a much bigger field.

What is immediately distinctive, particularly in the first three chapters, is the self-conscious, revisionary tone in the scholarship and the identification of the inadequacies of earlier accounts. As the authors observe, practices such as casting past mathematics directly into its 'modern' equivalent, or comparing and evaluating these non-western traditions with respect to their 'western' counterpart, do little to help modern audiences appreciate these mathematical cultures. Annette Imhausen stresses the need to keep mathematical algorithms in their original layout and format, and not to decontextualize them by translating them directly with modern notation. Eleanor Robson laments the lack of attention to vitally relevant details such as provenance and chronology, in previous scholarship 'when [cuneiform] tablets were considered not as archaeological artifacts but rather as bearers of text' [92]. Joseph Dauben questions the assessment of the Chinese mathematical tradition as 'authoritarian' as potentially trivializing, and suggests that it needlessly polarizes it with the Greek tradition. Furthermore, there are many anachronisms and much idle speculation circulating in histories of mathematics, particularly, it seems, in those bearing on non-western mathematics, where our knowledge is sketchy and many details are still to be filled in. These are immediately and firmly dispelled at appropriate times throughout the book. For example, Kim Plofker tactfully questions the notion of the 'ritual origins of geometry' [387] as well as claims which associate Vedic mathematics with various modern-day computational algorithms.

One delightful feature of the various accounts is the inclusion of exchanges between members within these early mathematical communities. Imhausen opens her section with details of a competitive

squabble between two Egyptian scribes, each trying to outdo the other in mathematical prowess [10ff]. Robson illustrates mathematical pedagogy by a humorous dialogue [80] between a supervisor issuing to a younger trainee scribe advice that had been passed down to him. Plofker gives us a glimpse into the divisions and disagreements in the Indian mathematical community by including an outline of one of the most famous rivalries recorded, that between Āryabhaṭa and his successor Brahmagupta [419]—despite the fact we only have occasional references to this rivalry in texts, they suffice to show that the relationship was far from collegial! Berggren includes a debate between two medieval Islamic scientists who argue over optimal solutions to various problems and dispute the validity of approximation used in mathematics [568ff]. They draw not only from their own mathematical tradition but invoke mathematical precedents set by others such as Archimedes, Aristotle, Galen, Hipparchus, and Ptolemy. These excerpts remind the reader that such historical texts have immediate human appeal, and also give a sense of the sociology of the individuals who were responsible for them and of the ways in which those individuals interacted professionally.

One important feature of the history of mathematics is the transmission of ideas from culture to culture, particularly in the ancient and medieval periods. Reflecting upon the importation of new ideas into a pre-existing culture can lead to valuable insights. An idea, technique, or concept may be adopted; but it may be also be changed, misunderstood, or rejected. The authors have each included aspects of transmission as they are able where appropriate, and the references stir the reader's curiosity for seeing it covered more systematically.

Imhausen was given the task of covering the Egyptian mathematical tradition from roughly the Archaic period (*ca* 3000 BC) to the Graeco-Roman Period (ending *ca* AD 395). At the outset, she gives a personal insight into the highs and lows experienced by any historian of mathematics in this field. Among the particular frustrations for an Egyptologist in this area is the lack of sources. Nonetheless, she expertly selects a number of wide-ranging texts which reveal the mathematical sophistication of that culture at various times. Imhausen not only details the mathematics, but also highlights linguistic and grammatical features and offers comments of a more anthropological, archaeological, and paleographical nature as well. She introduces the

reader to some simple features of Egyptian hieroglyphs and the various transcriptional conventions used by modern Egyptologists. Texts of a more technical nature are always given with their hieroglyphic transcription and mathematical interpretation, and, on occasion, a photo image of the particular papyrus. Given this careful tutelage and the inclusion of the appropriate sources right there, the reader feels as though they could actually read the original themselves! Accompanying every example is a thorough commentary which consciously attempts to stay as 'literal' as possible. Lack of sources compels historians to be more versatile and resourceful, and indeed Imhausen draws from the progress within the wider field of Egyptology to deepen her analyses.

Imhausen notes the striking similarity between the solutions of similar sorts of problems and suggests that there may have been some general algorithmic-type approach that was understood but never explicitly expressed. She hints at a more general typology of features within individual texts, although notes that there was no standardized practice—the format of each text was to a large degree a product of the tastes and predilections of the individual who was writing it. She introduces three useful typologies—rhetorical, numeric, and algorithmic [24]—which highlight other various aspects in the texts. She offers a fascinating insight into the state of technical terminology in Egyptian mathematics [25]: Egyptian scholars, unlike those in Mesopotamia, seem to have used different but closely related technical terms to express mathematical nuance and the exact significance of this is still to be determined. She shows that different stages of a mathematical problem were consistently presented in accordance with various grammatical markers [25]; for example, the title was expressed in the infinitive construction, the working in second person, the results in the third person (*sḏm.ḥr.f*), and so on. She makes brief mention of the role of diagrams and their uses within the Egyptian tradition. Among some of the mathematical features likely to be of interest are the technique of false position [28], rules for the area of a circle [29] and the object referred to as a *nb.t* [31], bread-and-beer problems [38ff], and various ratio problems from the Graeco-Roman period [48–50].

Robson, in her usual dynamic and definitive approach, reminds the reader that Mesopotamian mathematics is so much more than the nine-times table and the 'Pythagorean triples' of Plimpton 322.

Her account revolves around the following three themes: first, the ways in which Western views of Mesopotamian mathematics have changed over the last two millennia; second, the who, why, and how of this mathematical tradition; and third, the rationale behind the selection and production of her translations [58]. She gives an overview of the scholarly tradition and shows the need to revise it, citing such deficiencies in earlier approaches as the lack of any sustained questioning of authorship, context, and function [60]. She definitively distances Mesopotamian mathematics from the so-called 'infancy of the western tradition', revealing a picture far more complex and rich than ever previously described. She upholds that doing a proper job as a historian of mathematics is more than just reading the numbers and shows how much more we can know about the mathematical aspects when we expand our lines of inquiry beyond the texts' contents alone. She highlights the interconnections between mathematics and other aspects of social and culture enterprises, and describes features hitherto overlooked such as social context and financing.

She carefully outlines the 'multistage' operation for the preparation and publication of cuneiform tablets [66]. She notes translation worries, establishing her preference in the conformal *versus* modernizing [67] debate,[1] and raises issues concerning the translation of technical terminology. She informs the reader of editorial conventions and not only standardizes best practice, but epitomizes it herself throughout the chapter. The reader is treated to photos of tablets, expertly executed transcriptions, thoughtful translations, mathematical commentaries, and even reflections on the physical state of the antiquities themselves when appropriate.

Robson conveys to us the nature of mathematics as a human enterprise, reminding us that no scribe was simply a mathematician. She outlines scribal education, carefully details scribal errors, and describes the situations and circumstances of particular scribal families such as the Shangû-Ninurta family from the late fifth century BC [161] and the Sîn-leqi-unninni family which flourished around 200 BC

[1] That is, whether to translate the text as literally as possible so that the English is made to 'conform' to the original Akkadian as far as the translator is able (following Friberg and Høyrup for example), or to 'modernize' it by rendering it in language and symbolism that is instantly recognizable to modern readers (following Neugebauer and others).

in Uruk [174]. She concludes from the frequency of occurrence of particular mathematical examples certain trends about the reception, the audience, and the popularity of particular areas in mathematics. Importantly, for future generations, she highlights the challenges of unprovenanced tablets and she draws tentative conclusions about regional differences in mathematical practice, something that she can do because of her interest in archaeological provenance.

She selects a breadth of mathematical material from the nuances of sexagesimal arithmetic to 'geometrical algebra', arithmetic progressions, geometry, and various practical applications of mathematics in their multifarious formats (which include problem texts, trainee scribes' rough-working, and reference lists). She presents previously published as well as unpublished tablets; the famous tablet concerning the square root of 2 is highlighted with its previously unpublished reverse. She illustrates some of the difficulties that the mathematical Assyriologist [130ff] encounters by including a challenging tablet (BM85194) made difficult by its numerical errors, rare words, and accidental omissions and additions, and by detailing the various attempts to make sense of it.

Yet further east, the three millennia or so that span the Chinese mathematical tradition are covered by Dauben. In his opening words, he invokes the 'standard' view of Chinese mathematics as 'utilitarian, authoritarian, and basically conservative' [187] and challenges this characterization; his selections thereafter are very much made with this sentiment in mind as he presents both techniques and problems that are especially typical of Chinese mathematics in addition to those that are distinctly innovative. He superbly conveys the difficulties of working with the Chinese language and the perils of translation particular to it by his illustration of the ways in which scholars have disagreed quite significantly about how to translate even a title. For example, the classic Chinese mathematical text *Jiu zhang suan shu* [227ff] has been translated as 'Arithmetic in Nine Sections', 'Nine Chapters on the Mathematical Art', 'Computational Prescriptions in Nine Chapters' and 'Nine Categories of Mathematical Methods', among others. Indeed, Dauben refers to it simply as 'Nine Chapters'! Other philological delights are littered throughout his chapter, notably the astonishing fact that there was no word for triangle within the Chinese tradition [232].

Dauben incorporates archaeological finds from as late as two decades ago and draws from a variety of media to highlight features of Chinese mathematical industry. He includes illustrations from a stone relief from a Han dynasty tomb, a drawing on silk from 350 BC, and a bronze standard measure, as well as a mathematical text written on bamboo strips. He gives the actual Chinese characters where appropriate and uses a hand-rendered font to demonstrate the various workings of the 'counting rods'—effectively demonstrating how this system for depicting numerals lent itself readily to efficient arithmetic algorithms [194ff]. He carefully describes the ingenious procedure known as the 'out-in' principle [199ff], a technique invoked usually in the context of geometrical proofs for demonstrating equivalencies. He also highlights the innovative use of colors in proofs [251, and elsewhere].

He notes that Chinese mathematical texts served two primary purposes, one research-directed and the other educational [193]. He gives insight into the motivations of mathematicians, quoting the Chinese author Zhao Shuang who stated 'my sincere hope was to demolish the high walls and reveal the mysteries of the halls and chambers within' [194]. This sentiment is perhaps atypical in the history of mathematics—it is sometimes speculated, for example, that Sanskrit Paṇḍits deliberately obscured their material for the specific purpose of keeping it esoteric and esteemed!

Dauben reveals his command of the broader Chinese intellectual tradition by noting methodological similarities with Chinese philosophy [213]. He shows that Chinese mathematics was not just a practical offshoot from the various needs of the empire, but also firmly an 'art' in its own right [213]. He gives glimpses into the various challenges for a practicing mathematician, including a quote from Liu Hui who admits that the complete solution of a problem is beyond his abilities [249]; and he outlines the broader interests of mathematicians in philosophical or metaphysical issues [301] as well as details of their position in the social hierarchies [303]. He reminds the reader of the fact that the status of mathematics in any society is not assured: he shows how mathematics fell in and out of favour [308] at various times throughout Chinese history and notes the arrival of the Jesuits and the impact that this had on Chinese mathematics [366]. He describes the reception of Euclid by Chinese scholars, who, paradoxically enough, were more interested in the results than his axiomatic

method. He notes that Chinese scholars remained puzzled by the reception and status that this work had in other cultures, since they found it repetitive and needlessly complicated, giving them no new mathematical detail than already existed in their own tradition!

Importantly, Dauben covers the *goug-gu* (better known as the Pythagorean) theorem [215] and the more controversial threads of scholarship surrounding it.[2] As is well documented in this book, there are other early instances of this numerical relationship: Plofker notes [387–390] its first appearance in Sanskrit sources in about 800 BC and an indirect appreciation of it can be found can be found in Egyptian [49–50] and Mesopotamian sources [140–141]. Most scholars are now firmly of the conviction that instances of particularly useful mathematical facts can appear independently in different cultures, without the need for far-flung speculation about intellectual appropriation.

Among some of the other interesting mathematical aspects of Dauben's account are square- and cube-root algorithms, calculation of volumes [259], the double difference method [288], the representation of big numbers [297] and links therein to Archimedes, conceptions of infinity and the endless cycle of numbers [301], the Chinese remainder theorem as well as Chinese 'algebra' [324, 345], the binomial coefficients [330]—which very nicely illustrated by a reprint from the actual manuscript—and various applications in mathematical astronomy and time-keeping, for example [213ff]. Furthermore, Dauben gives us insights into counting boards [447] and the ways in which they can keep track of the coefficients of various combinations of unknowns of arbitrary power, so that elimination becomes a mechanical process.

Oft quoted is al-Bīrūnī's assessment of Indian mathematics as being 'a mixture of costly crystals and common pebbles' [435]. As has been shown by Plofker through the excerpts that she presents, this metaphor is completely inappropriate. More importantly though, she shows us how al-Bīrūnī misunderstood the circumstances in which Indian mathematics was practiced. As she points out, Indian mathematics, like most other intellectual disciplines in India, were carried out for the most part in an oral environment, which meant that mathematicians had quite different pressures on them as they engaged in

[2] E.g., 'Was Pythagoras Chinese?' Indeed, compare Dauben's reference to Liu Hui as the Chinese Euclid!

mathematical activity. Plofker documents mathematical highlights drawn from a staggering time period—from the emergence of literate intellectual cultures until it was 'westernized'—and details its assimilation into modern global mathematics with excerpts from just half a century ago.

In this chronological span, she illustrates the various manifestations of mathematical activity, be it mathematics proper, its various applications, instrumentation, mathematics education, or the various early 'ethnomathematical' expressions of mathematical knowledge [386]. She speculates on the reasons for the commissioning and copying of mathematical texts in their thousands and she covers the standard favorite authors, including excerpts from the Śulbasūtras and the Bakhshālī manuscript, Āryabhaṭa, Bhāskara I, Lalla, Mahāvīra and so on; much is presented here for the first time. She includes many excerpts which until recently have escaped the notice of historians of mathematics because they are not directly in mathematical sources but appear in other intellectual traditions. For example, an interesting technique for the computation of 2^n is found in an early work on prosody—not only obscure in location but laconic in expression—the mathematical content of which she teases out expertly and seemingly without effort.

Plofker emphasizes the importance of the relationships between mathematicians and their successive generations, an aspect critical to a discipline carried out in an oral environment. She ironically notes [400] that in fact the more detailed texts and explanatory diagrams were reserved for the 'dull-witted', and that the brilliant student was one who could untangle the terse abbreviated metrical verses to make both a linguistically and mathematically consistent interpretation. She observes the challenges in this by citing the example of one of India's most gifted mathematicians, Bhāskara, as he struggled to make sense of a particular rule given by Brahmagupta concerning cyclic quadrilaterals [462]. It would seem that he simply misunderstood the 'cyclic' prerequisite of the rule—one can hardly blame him, for it was never originally mentioned by Brahmagupta in the first place!

Plofker quickly reveals her versatility and breadth as a scholar by frequently noting details of transmission. India has been called the 'recipient and remodeler of foreign traditions' [Pingree 1978] and she knowledgeably and frequently comments on issues concerning the

transmission of ideas into and out of the Indian mathematical tradition. Perhaps because of the reputation that Sanskrit has for being notoriously difficult, she has been careful not to dwell on technical terminology. Plofker gives insight into the role of the commentary [400ff]; and, slipping naturally into the role of commentator herself, she provides a hyper-commentary to an excerpt from Bhāskara, who is himself commenting on a work by Āryabhaṭa. She relates the importance of colophons [441] and the development of different schools of thought. She details the various social structures in India, situating the audience of these texts and defining the typical status of the mathematician. She remarks on the hereditary nature of mathematics education as well as the role of women within the mathematical context. She also makes the remarkable observation that many mathematical techniques which are used and described in mathematical applications are never seen in general mathematical works. She wraps up her coverage of this area by describing encounters with modern western mathematics [507], the details of mathematical education in British India, and the struggle between indigenous knowledge and the implementation of the modern European curriculum.

Excerpts of particular mathematical interest are abundant and include her description of the number systems and numerals [395–398], the 'circulature' of the square [392], the *karanīs* [407], the computation of sines [408–409, and elsewhere], the 'pulverizer' [416], cyclic quadrilaterals [424–425], computation with seeds or 'algebra' [467], infinite series—which include the Mādhava-Newton series and various manifestations and approximations of π [481ff]—sequences, combinatorics, and magic squares [493ff], as well as various applications in mathematical astronomy.

Berggren's assignment concerns a geographical region previously covered in this book, but far removed in time, circumstances, population, language, and society. Robson's account of mathematics in the Ancient Near East ended in the first century AD; Berggren picks up some 700 years later with the emergence of Islam. He notes at the outset that the designation 'Islamic' refers to those regions of the world where Islam was the dominant religious and cultural tradition, but that this term can be overly exclusive since many of the notable practitioners belonged to other religious traditions and cultural groups. The other appellation commonly invoked in this field is 'Arabic', to convey the dominant language in which these texts were composed;

but this designation too has its drawbacks. Berggren himself prefers the former designation.

Berggren deliberately chooses to arrange his material differently than the other contributors. His organization is primarily thematic, though the material within a given theme is then arranged chronologically—a wise decision that enables him to manage the overwhelming number of sources available. He includes a satisfying explanation of Arabic names [520], a topic often quite daunting to new readers. He refrains from too much detail on the Arabic language and paleography, perhaps because the script and orientation is so different to what the majority of his readers are comfortable with. At one point, he directs the reader to 'spotting' features in an original manuscript [533] but with little assistance for those unfamiliar with Arabic paleography. A small table outlining number systems and numerals might have been useful at this point, particularly as they are the origin of our present notation.

Berggren perfectly characterizes Islamic mathematics as *heuristic*, that is, as an enterprise of solving geometrical problems; and he identifies it as being inspired from three traditions principally—Greek mathematics and geometry, the numerical solutions of indeterminate problems from Diophantus, and the practical manuals of Heron. He illustrates a tight relationship between mathematical theory and practice [519, and elsewhere], and the excerpts that he selects reveal the various audiences that these texts might have been composed for—audiences that include practitioners and artisans, students, and other colleagues [585]. Berggren pays particular attention to the details of the translations that he provides, warning against the allure of 'false-friends'[3] [519] in mathematical texts and gives considered accounts of some difficult terms.[4] He has done an excellent job of covering and describing some quite thorny passages and unraveling excerpts densely packed with sophisticated mathematical ideas.

Of particular delight to those interested in mathematical details will be the various construction problems, the extraction of a fifth root [538], 'algebra' [542ff], stereographic projection [573], the description and function of the rusty compass [577ff] and the perfect

[3] That is, when the name of a mathematical object or concept in that context is distinctly different from what we associate it with in modern mathematics.

[4] See, e.g., 594n102 and his description of *manshūr*.

compass [595], volumes of revolutions [587ff], trigonometry and non-linear interpolations of sine tables [621ff, esp. 626–627], determination of the directions (the *qibla*) to Mecca [635], combinatorics [658], and especially that theory as applied to eclipse possibilities [659].

This reference work will not only be of constant use to the professional researcher, but also the interested amateur and the teacher of both secondary and tertiary mathematics. It will prove ideal not only for the purpose of injecting history into the regular mathematics curriculum, but also for teaching the history of mathematics. As this book will serve as a reference guide for consultation on particular topics and themes, there is a need for a comprehensive and exhaustive index. The index at present provides a basic coverage of the content, but could be greatly expanded. For example, some old favorites do not appear as such in the index ('Plimpton 322', the 'Rhind Mathematical Papyrus', 'Rusty compass', to name a few). Some less obvious entries are indexed but more prominent ones are left out; for example, particular Mesopotamian scribes are indexed but the scribal families on which Robson spends several pages[5] are left out. Many other entries could be amplified as not all instances are listed: for example, 'False Position' could have added to it pages 148 and 550; 'Zero' and 'Errors in Calculation', to name a few, could be similarly expanded. Furthermore, this book is unique because it contains so many excerpts from primary sources, many of which are definitively translated and published here for the first time. To aid the reader, it would have been beneficial to compile an *index locorum* cataloging the passages translated by author for quick reference. This would allow both professional and amateur quick access to relevant passages by author as well as by theme or chronological period.

All in all, this sourcebook does something tremendously important for the field. By means of carefully selected examples and adequate guidance, the authors have consciously given the reader a chance to work with and interpret the primary sources themselves. Thanks to their groundwork, explanations, and reference tables, readers get to feel something of the exhilaration and empowering experience of penetrating aspects of these texts. It is through such an

[5] That is, the Shangû-Ninurta family [161ff] and the Sîn-leqi-unninni family [174ff].

accessible exposition that the next generation of historians of mathematics will be inspired and motivated to continue the tradition as it should be practiced.

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The Stoics on Determinism and Compatibilism by Ricardo Salles

Aldershot, UK/Burlington, VT: Ashgate, 2005. Pp. xxii + 132. ISBN 0–7546–3976–2. Cloth $79.95

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Those who find determinism unappealing are typically motivated by worries over the freedom of action and, relatedly, attributions of moral responsibility. It may be, however, that our intuitions concerning freedom and responsibility can be respected without abandoning the equally attractive assumption that all events have antecedent causes. Chrysippus and other ancient Stoics espoused a version of universal causal determinism which, they argued, has ample room for a satisfying theory of action and moral responsibility. Theirs is, then, a *compatibilist* system. Ricardo Salles' brief but carefully argued monograph scrutinizes several Stoic compatibilist claims, seeking to understand how the historically attested arguments fit together into a tight and convincing demonstration.

Two initial chapters are devoted to the foundations of Stoic determinism itself. These are, first, that all states and events, including human actions, are caused; and second, that causes necessitate their effects—if a certain thing or set of circumstances, C, is the cause of effect E, it can never be the case that C obtains but E does not. Both points, Salles argues, are deeply entrenched in Stoic physics. The first is established from the principle of bivalence as applied to future events: if it were not the case that all states and events have antecedent causes, then future-tensed propositions could not be either true or false, but in fact *all* propositions are either true or false. The second, Salles suggests, follows from the Stoic doctrine of eternal recurrence, that in cyclical time the new universe that arises after each periodic conflagration must be identical, down to the smallest detail, with the one that existed before. These recurring universes could not be truly identical if it were not the case that causes necessitate their effects—and so they do.

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Chapters 3–5 explore three theorems which are attested for the ancient Stoics—at least two, perhaps all three, for Chrysippus himself—and which play different roles in the defense of Stoic compatibilism. Following the order of logical priority established by Salles, these may be summarized as follows:

T_1 Stoic determinism is consistent with the view that our actions are determined at least in part by internal factors.

T_2 Stoic determinism is consistent with the view that we have the capacity to act otherwise than we do.

T_3 It is sufficient for moral responsibility if the agent acts from a fully rational impulse. Thus, the attribution of moral responsibility does not *depend* on our having the capacity mentioned in T_2.

The emphasis falls, rightly, on T_1. The opponent of Stoicism in Cicero, *De fato* 40 holds reasonably enough that praise and blame are not justified if the causes of our actions are not internal to us. (Salles agrees with Pamela Huby [1970] that this opponent is Epicurus.) Chrysippus replies with an analogy: just as a cylinder rolls not only because it is pushed but also, and more importantly, because it is of the right shape to roll, so an action may have an external cause and yet have its principal cause in the character of the agent. To this one may of course reply that one's character might itself be the product of external factors. Salles here suggests on behalf of Chrysippus that just as the cylindrical shape cannot be imposed on the cylinder from without (it can only be imposed on the lump of wood that existed before), so the peculiar quality (ἴδιος ποιότης) that individuates one as an agent is temporally coextensive with that agent. Since the external factors that make me the agent I am did not act upon *me*, it does not make sense to say that I was made by them to act as I do.

T_2 concerns the capacity to act otherwise than one actually acts, a capacity which has sometimes been considered indispensable to attributions of responsibility. The extent to which T_2 belongs to discussions of moral responsibility is unclear; Salles labels it a theorem in metaphysics. Nonetheless, it is highly relevant in that it clarifies certain important modal notions. While an impulse, like any other event, has causes and is necessitated by those causes, this is a different sort of necessity than is involved in certain necessary truths

such as 'virtue is beneficial' or 'fire burns'. Even if it cannot but happen that I sit down at time t, my doing so is not necessary in the way that fire is necessarily hot. I retain the capacity to stand by virtue of the sort of being I am, for as long as I remain that sort of being, whether or not I exercise that capacity. This at least bolsters Chrysippus' case against those indeterminists who argue that the capacity to act otherwise, which *they* consider to be required for moral responsibility, is ruled out by determinism.

However, Stoic compatibilism does not rely on the capacity to act otherwise in order to justify ascriptions of praise and blame. According to Alexander of Aphrodisias in *De fato* 13 and Nemesius of Emesa in *De natura hominis* 35, it is sufficient for moral responsibility if the agent acts upon a fully rational impulse. This is T_3. Salles accepts Nemesius' attribution of this theorem to Chrysippus himself, and explicates 'fully rational impulse' as an impulse arising from reflection on the all-things-considered appropriateness of the contemplated course of action. At this point, he draws an interesting comparison to a modern compatibilist argument by Harry Frankfurt. For Frankfurt, a person who makes a choice after careful consideration of options is responsible for that choice even if (by thought experiment) an electrode secretly implanted in his brain would have prevented him from behaving differently. Similarly for Chrysippus the fact that an action proceeds from a rational impulse is sufficient for responsibility; responsibility does not depend on alternative possibilities.

Salles recognizes that actions arising from this sort of reflection are only a subset of what we do. In his final chapter, he considers how Stoics can justify ascriptions of responsibility for unreflective or precipitate actions. At this point, he departs from Chrysippus to take up the perspective of the later Stoic Epictetus: even when we do not reflect on the all-things-considered appropriateness of what we are about to do, we are still responsible for that precipitate action because we both can and should deliberate fully before acting. Because the capacity for reflection belongs to our nature as rational beings, we are ethically required to use it. We are, like Aristotle's drunkards, liable to blame for what we do in thoughtless moments that we might have guarded against. To be sure, not everyone has access to the kinds of therapeutic exercises Epictetus recommends to develop reflectiveness to its fullest potential. Still, it seems, some

sufficient moral teaching is available to nearly everyone, and our rational nature ought to motivate us to seek it out.

Salles' book is carefully researched and clearly presented, with a pleasing economy of style. His well-structured arguments move swiftly to the heart of the matter and will be appreciated by those who desire a speedier introduction to the subject than Susanne Bobzien's *Determinism and Freedom in Stoic Philosophy* [1998]. Her more comprehensive work remains indispensable, however, for those who are concerned to understand these Stoic arguments in depth or to trace their historical development. For instance, one should suspend judgment on Salles' attribution of the doctrine in Nemesius, *De natura hominis* 35, to Chrysippus until one has studied Bobzien's extensive arguments for much later authorship in her chapter 8. (It may still be the case that Chrysippus insisted on the sufficiency of impulse for responsibility, which is the feature of T_3 that is of greatest philosophical significance for Salles' discussion; further on this below.)

Only two portions of Salles' analysis seem to me to require specific comment here. The first of these concerns the relation between Chrysippus' principle of causal regularity (like causes produce like effects) and the cosmogonical doctrine of eternal recurrence. Attempting to derive the former from the latter, Salles is driven to some lengths to provide a Stoic-style argument that can in turn ground recurrence. Following Jaap Mansfeld [1979], he finds that grounding in the beneficent nature of the creator god, which guarantees that he must already have created the best possible world and so will do so again. But identity in goodness might not imply complete indiscernibility down to the most minute events, and so Salles pursues a complicated line of reasoning meant to support the derivation of complete indiscernibility from Zeus' rational beneficence. The depth of the water here must be indicative of something. He would have done better to conclude that eternal recurrence is not the argumentative basis for causal regularity, but rather a consequence of it. Given that Zeus himself persists (as technical fire, as seminal principles) after every conflagration—indeed in a real sense he *is* the conflagration—the recurrence of an identical universe follows by causal regularity. But causal regularity itself does not need to be derived from any sort of cosmogonical argument; it is just part of what Stoics mean by the word 'cause', a matter of definition rather than a theorem requiring justification.

A second and (for this work) more fundamental difficulty becomes evident at the point where Salles turns from Chrysippus' theory, put forward in the course of the third century BC, to that of Epictetus some four centuries later. The combination of Chrysippus' T_1, T_2, and T_3 establishes that generating a rational impulse to act is a sufficient condition for moral responsibility. But Salles' interpretation of the phrase 'rational impulse' is quite narrow: it covers only those actions that are preceded by full-scale deliberation on whether the action in question is in accordance with providence. One actually has to ask oneself, 'Given the present circumstances, is the action choiceworthy? Should I assent to the impression?' All other actions, including emotion-driven actions and surely a large proportion of all our misdeeds, fall into the category of precipitate actions. For these, it seems, Chrysippus had no argument to justify any ascription of responsibility, since it is the 'distinctive contribution' of Epictetus to supply one [91]. Medea is a case in point. Although her infanticide does proceed from practical reasoning (she regards infanticide as a means to revenge on Jason, and revenge as more advantageous to her than the life of her children), it does not meet the criterion of *all-things-considered* reflection and so it is only Epictetus' argument concerning precipitancy that renders her responsible.

Something is amiss. We surely cannot think that Chrysippus, who is known to have studied the example of Medea, had nothing to say about how considerations of responsibility apply in her case. More generally, we cannot think that he and many subsequent generations of Stoic thinkers employed a criterion for moral responsibility which failed to cover most of the domain of human action. Salles' understanding of rational impulse must therefore be a great deal too restrictive. We should instead believe that in Chrysippan compatibilism, it is sufficient for moral responsibility if an action is performed in the way that the actions of adult human beings characteristically are performed; that is, through assent to impulsory impressions, assent being determined by the prior contents and internal dynamics of one's belief-set. That is, the kind of practical reasoning we engage in all the time, as long as we are of age and neither sleepwalking nor insane, suffices to make our actions an expression of our moral character. This too is rationality, though the impulses so generated will not always be rational in the fullest (normative) sense of the word.

One further observation I make purely in the interests of ease of reading. Salles' manner of exposition is in general extraordinarily lucid, making it easy to comprehend the structure of his argument even where the material itself is difficult. He is sensitive, too, to the limited patience of readers when it comes to matters of source criticism and scholarly controversy; and so far as possible is careful to relegate the inevitably dense apparatus of primary and secondary citations to the bottom of the page, out of the way of readers grappling with his argument. One could wish, then, that he had shown similar consideration when laying out theorems T_1, T_2, and T_3. On pages xx–xxi, where the three are first presented, the order is T_1, T_3, T_2; in the recapitulation on page 69, the order is T_3, T_1, T_2; and the actual order of exposition is also T_3, T_1, T_2. Only on page 89 is the logic behind this seemingly capricious numbering system explained. At that point, all becomes clear; but it would have done no harm to offer this helpful bit of explanation much earlier, or even better to present the theses in the same *logical* order in which they are numbered.

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The Cambridge History of Science: Vol. 4. Eighteenth-Century Science edited by Roy Porter

Cambridge: Cambridge University Press, 2003. Pp. xxx + 912. ISBN 0–521–57243–6. Cloth $140.00

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This volume edited by the late Roy Porter, a renowned historian of medicine, offers to general and specialist readers alike a complete survey of the development of science in the 18th century. This volume explores the implications of the 'scientific revolution' of the 17th century and the major new growth-points of the 18th century, particularly in the experimental sciences. This is the first comprehensive history of 18th-century science in more than 30 years. It is, bar none, the fullest and most complete work of its kind.

The volume is broken into five distinct parts:

I. Science in Society
II. Disciplines
III. Special Themes
IV. Non-Western Traditions
V. Ramifications and Impacts

Primary attention is paid to western science, though space is also given to science in traditional cultures and colonial science. The coverage within the volume strikes a balance between analysis of the cognitive dimension of science itself and interpretation of its wider social, economic, and cultural orientation. The contributors, all world leaders in their respective specialties, engage with current historiographical and methodological controversies and strike out on directions of their own.

In the remainder of this review, I shall highlight some of what I think are the more notable contributions. (My selection of chapters for comment is not intended to imply that the other chapters will not reward the reader's attention.) In the introduction, Porter notes that

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whereas Enlightenment thinkers of the 18th century have received much (inordinate?) attention from the academy, 18th-century science, in contrast, has typically been portrayed in a subdued manner, and that the period has generally been perceived to lack the 'heroic quality' of the century preceding it [1]. He remarks, however, that even in well-plowed fields of inquiry such as natural history, remarkable changes in thinking can be seen. Indeed, scientific inquiry in the 18th century did not stall here. Linnaeus, for example, developed his the enduring taxonomic system of plants and the first evolutionary theories were advanced at this time. As a matter of fact, the forerunners of Darwin found the static and hierarchical chain of being no longer to possess the greatest explanatory power, and felt the need to re-conceptualize living biota in a more dynamic framework and an extended timescale. Porter consistently reminds readers that in order to understand 18th-century science properly, one must place it in its proper context. He asserts that the central problem of attempting to comprehend 18th-century science is the question as to the species of knowledge that it was supposed to constitute [14].

In chapter 2, 'The Legacy of the "Scientific Revolution": Science and the Enlightenment', by Peter Hans Reill, one finds a characterization of the Enlightenment as a movement that 'adopted, extended, and completed the intellectual and social project usually characterized' as the 'Scientific Revolution' [23]. Reill notes that mechanical natural philosophy was dominant in the period of roughly the late 1680s to the early 1740s. In this period, matter's essence was extremely simplified and defined as merely a homogeneous 'heap of things' that are extended, hard, impenetrable, and inert [25]. However, in the late 18th century, there was a revolution by Enlightenment vitalists who viewed living matter as containing an immanent principle of self-movement whose source lies within an active power inherent within it. Teleology was, thereby, effectively reborn.

In the fourth chapter, 'Scientific Institutions and the Organization of Science', by James McClellan III, it is argued that out of the intellectual revolution of the 16th and 17th centuries grew an organizational revolution in the scientific enterprise during the 18th century. McClellan notes that science was drastically reorganized in the 18th century after the government moved to support science in part by developing new academies, various new observatories, botanical gardens, and new forms of publications. In chapter 10, 'Classifying the

Sciences', by Richard Yeo, it is asserted that in the 18th century there were significant changes in the social and cultural conditions related to the classifications of knowledge. Yeo concludes that with the collapse of the categories of natural philosophy and natural theology, classification schemes no longer sought to show how the various scientific subjects related to one another.

In one of the most important chapters within the volume in my view, 'Ideas of Nature: Natural Philosophy', John Gascoigne shows that the 18th century inherited a long tradition derived from Greek antiquity which maintained that nature could be understood by the employment of reason. He contends, moreover, that, although natural philosophy remained at the beginning of the 18th century a branch of philosophy (along with metaphysics, logic, and moral philosophy), by the end of the 18th century it saw increasing independence from its philosophical origins. He further contends that by the end of the 18th century, natural philosophy grew in scale and complexity to the extent that it began to give birth to separate disciplines.

Shirley A. Roe, in a chapter entitled 'The Life Sciences', notes that for much of the 18th century (what today is known as) the biological world was seen as a highly ordered and somewhat static place. This notion, however, was forcefully challenged by the middle of the century. Roe highlights the relationship between matter and activity as one of the burning issues of the 18th-century advancement of the life sciences. She focuses upon two principal areas in which questions of mechanism, vitalism, and materialism arose: physiology and the theory of generation.

All in all, this volume is designed to be read as both a narrative and an interpretation, and also used as a work of reference. It will be an excellent reference for historians and professionals in the history of science. According to Porter, his aim in producing this volume was to provide critical syntheses of the best modern thinking regarding scientific developments in the 18th century. He more than exceeded his expressed intentions.

The Birthday Book: Censorinus translated by Holt N. Parker

Chicago/London: University of Chicago Press, 2007. Pp. xiv + 102.
ISBN 978–0–226–09974–3. Cloth $25.00

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The little *Birthday Book* (*De die natali liber*) by the third century AD grammarian, Censorinus, was originally presented as a birthday gift to his friend Quintus Caerellius in AD 238—the date is derived from the text itself, where it is expressed allusively and eruditely as the year 986 of the era of Nabonassar [*De die nat.* 21.9]. The *Birthday Book* uses the idea of the birthday as the starting point for a brief but virtuosic survey of the measurement of time itself.

What the book says about time is not particularly original, but it is useful from a cultural perspective inasmuch as Censorinus demonstrates a breadth of learning that was typical of his class and time. From an antiquarian point of view, the essay is especially valuable for what it reports from earlier authors whose works have not survived, notably the early Imperial polymath Varro. In what has survived of the *Birthday Book* itself, the first half [cc. 1–13] takes the idea of the birthday as the starting point in an analysis of the development of human life from conception to death. The second half [cc. 16–24] then discusses the various measures of time from eternity down to the hour. It may be, as Parker suggests [56–57], that the work was meant to be balanced around the encomium to Caerellius in chapter 15, and to finish with a further five chapters to provide a coda that returned to the honorand's own birthday, perhaps with his horoscope: this would also nicely draw together some of the preceding themes.

The book is a compilation-piece, then, but one in which Censorinus demonstrates his own remarkably wide knowledge and his easy ability in passing it on to his reader(s). One may reasonably judge that he knew a little about a lot and had skill in knitting it all together, however disparate the items may look at first glance. Underlying this knowedge lies the basic curriculum of ancient Classical

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education, revolving around the four disciplines of geometry, arithmetic, astronomy and music—Platonic in origin, if not earlier, and to be found a couple of centuries after Censorinus bound together as four of the seven liberal arts in the work of Martianus Capella. Characteristic of the ancient world also is Censorinus' starting point of religious observance, specifically to the Genius of the birthday. Religion provides an appropriately cosmic context for the study of time.

Something of the discursive mode adopted by Censorinus may be gleaned from a simple analysis of the first half of the *Birthday Book*. The initial task is to honour the Genius of one's birthday. But who is this Genius? Censorinus defines him as 'our companion from the moment we are taken from our mother's womb' [3.5]—we might think of the Christian guardian angel as a close relative—a definition that becomes the impetus for a discussion of how mankind came to be, and indeed how we got into our mother's womb in the first place. Once there, so to speak, Censorinus uses a mixture of (in our terms) mythology, early natural philosophy, and astrology to explore the growth and maturation of the child in the womb, down to the right month for its birth. The introduction of astrology into the discussion becomes the cause for a description of some elements of the art, notably the 'aspects' of the signs one to another around the zodiac. Censorinus returns to the issue of the lengths of pregnancies, and highlights—from Pythagorean philosophy—the seven-month and the 10-month pregnancies, each of which has its own internal ratios of numbers of days for development, from seed to milky humor to blood to flesh to the full formation of the body. These ratios are 'harmonies'—a digression briefly explains musical harmonies so as to assist in defining the developmental ratios. Harmony in the microcosm of the human body is matched by harmony in the macrocosm of the universe, with the planets set at distances from each other that correspond to musical intervals.

This summary takes us to half-way through the surviving text (to chapter 13). The second half, driven as it is by its emphasis on gradually diminishing units of time (eternity, ages, centuries, the Great Year, the year, months, days, hours), is generally more logically constructed and less digressive to our modern mind (although there are still excursions into the calendar and the history of the world), but one needs the first half to appreciate the allusive mode of thinking so characteristic of ancient philosophy.

As anyone who has worked on matters calendrical in the Greek and Roman worlds will know, the *Birthday Book* is a mine of information. One just has to glance at A. E. Samuel's still valuable *Greek and Roman Chronology* [1972], to see how often he refers to it. I did the same in my *Greek and Roman Calendars* [2005], but in translation. Well-regarded in Late Antiquity, the text was known through many manuscripts in the medieval period, and was one of the earliest books to be printed [first edition, 1497]. Better known authors have fared much worse from the vagaries of fortune. It is therefore remarkable, on the one hand, that the book has become the preserve mainly of scholars engrossed in the niceties of the Greek and Roman calendars and, on the other, that this translation by Parker is the first into English. (A German one by Sallmann [1988] exists, and it is Sallmann's Teubner text [1983] that forms the basis of Parker's translation.) It is a nice touch, but perhaps also indicative of the modern 'boutique' nature of the work, that this translation was prepared and presented as a birthday present from Parker to a significant other. Yet more people should certainly find Censorinus valuable and this excellent translation will assist in the wider dissemination of the text.

Translating Censorinus is, I think, a relatively straightforward task—teachers of Latin take note: this text would provide an excellent resource for beginners' courses, along with a pleasant introduction to ancient culture—and Parker handles the job well. The pleasing quality of his translation may be judged from a comparison between my own literalist translation and his of a passage that demonstrates also something of the quality of Censorinus' Latin and the detail of his information on the Roman calendar.

> *adeo aberratum est, ut C. Caesar pontifex maximus suo III et M. Aemilii Lepidi consulatu, quo retro delictum corrigeret, duos menses intercalarios dierum LXVII in mensem Novembrem et Decembrem interponeret, cum iam mense Februario dies III et XX intercalasset, faceretque eum annum dierum CCCCXLV, simul providens in futurum, ne iterum erraretur: nam intercalario mense sublato annum civilem ad solis cursum formavit. Itaque diebus CCCLV addidit decem, quos per septem menses, qui dies undetricenos habebant, ita discriberet, ut Januario et Sextili et Decembri bini accederent,*

> *ceteris singuli; eosque dies extremis partibus mensium adposuit, ne scilicet religiones sui cuiusque mensis a loco summoverentur. Quapropter nunc cum in septem mensibus dies singuli et triceni sint, quattuor tamen illi ita primitus instituti eo dinoscuntur, quod nonas habent septimanas, ceteri tres omnes alii reliqui quintanas.* [*De die nat.* 20.8–10]

Things had deviated so much that Gaius Caesar, as *pontifex maximus* in his third consulship and that of M. Aemilius Lepidus, in order to correct the past mistake, inserted between the months of November and December two intercalary months of 67 days, since he had already intercalated 23 days in the month of February, and made that a year of 445 days, at the same time taking care that the mistake would not be repeated in future; for with the intercalary month done away with, he shaped the civil year to the course of the sun. And so to the 355 days he added 10, which he distributed through the seven months which had 29 days as follows: two days were added to January, Sextilis, and December, and one to the others; and he placed these days at the ends of the months, evidently so that the religious ceremonies of each month might not be moved from their place. Therefore now, although there are 31 days in seven months, nevertheless four are distinguished by this feature of the original tradition, that they have the Nones on the seventh day,

Things got so bad that Julius Caesar, when he was *pontifex maximus*, during his third consulship, which he shared with M. Aemilius Lepidus, in order to correct the accumlated errors, had to insert two intercalary months with a total of 67 days between November and December, even though he had already made the usual addition of 23 days in February, adding up to a total of 445 days for that year. At the same time he made sure that the problem would not return in the future, for he removed the additional month from the calendar and made the civil year conform to the course of the sun. He added 10 days to the old 355, dividing them up among the seven months that had 29 days. January, Sextilis [August], and December got two, the others (April, June, September, November) got one. He added these days at the end of each month, so that the religious festivals would not be moved from their usual places in the month. That is why to this day we have seven months with 31 days, but we can recognize the four which were set up

while the other three remaining ones have them on the fifth. [Hannah 2005, 113]

in the ancient system by the fact they have the Nones on the seventh day, but the other three long months and all the short months have them on the fifth. [Parker, 47]

Of late my own approach to translation has been consciously to seek to replicate the sentence structures of the original Latin or Greek, however long or compressed or contorted they may seem to our English eyes. The often awkward structures of the original are a window into the minds and mental processes of the ancient (and let us not forget, foreign) writers. Thucydides and Tacitus, for instance, are not particularly easy 'reads' in the original, and I personally prefer to allow modern readers to gain a sense of the sometimes difficult structures that they use but which most modern translations attempt to smooth out into something more accessible to our ways of thinking and reading. The more we read like them, I tell myself, the more we may think like them and so ultimately appreciate how they saw the world around them. In a world where most of our students in Classics are devoid of Latin and Greek, exposure to the ancient modes of thought and expression increases in importance. I, however, am not necessarily trying to sell my translations to the wider, general public which may know nothing of the Classical world, so perhaps I can afford to play the 'purist'. Parker, on the other hand, is selling his translation and, one hopes, to a wider public which will not be conversant with this author. More use, therefore, is made by him of colloquialisms which capture, more or less, what the Latin says. Sentence structures in Parker's translation tend to be shorter, giving at times a sharper focus to what Censorinus says than he provided himself. I see these as good things, making an underservedly obscure author more accessible to Classicists, Latin-less students, and the 'educated public'.

Parker provides also a glossary [59–68], which very usefully gives definitions for and brief information on a variety of topics, principally individuals (real, divine, and mythological) and places named by Censorinus.

The book ends with a body of notes [69–102], tagged to the sections of each chapter in the text. These endnotes are not signalled in

the body of the text by superscript numbers, and so they have to be consulted on an *ad hoc*, 'need to know' basis. While this allows the main text itself to be read on its own without visual interruption, it also means that the less well-versed reader may often have to flip to the back of the book in case a note happens to explicate a curious or obscure passage. So, for some readers, interruption is probably inevitable, and for them superscript numbers in the text would be a better way to indicate the presence of notes. The endnotes provide useful, accurate background information on the wide variety of general topics dealt with in each chapter or group of chapters, along with an introductory bibliography of important critical editions and recent secondary works on the topics. Detailed explanatory notes follow, pertaining to individual items in the chapters, often referring to relevant material from other ancient sources. Given the breadth of Censorinus' compass in the *Birthday Book*, Parker is to be commended on his grasp of up-to-date scholarly literature in what are nowadays quite diverse specialisms, from medicine to music to chronology.

This is a book to enjoy, as was indeed originally intended. It has no pretence to be deeply intellectual or highly sophisticated, although I suspect that some of the sections will seem obscure to some readers. Parker, like Censorinus, not only makes the material accessible but also elucidates it without overwhelming his readers.

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Averrois opera. Commentarium magnum in Aristotelis physicorum librum septimum (Vindobonensis, lat. 2334) edited by H. Schmieja

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For several years Dr Horst Schmieja of the Thomas-Institut Köln has been collecting and collating manuscripts for a critical edition of the Latin translation of Averroes' long commentary on Aristotle's *Physica*, and since 1986 he has published several articles in which he reports some of his findings. This commentary was undoubtedly one of the most influential medieval texts. Averroes wrote three kinds of commentaries on Aristotle's treatises: epitomes, middle commentaries, and long commentaries. He wrote epitomes and middle commentaries on all of Aristotle's treatises that were available to him. He wrote long commentaries only on five books: *Posterior analytica*, *Physica*, *De caelo*, *De anima*, and *Metaphysica*. Four of these have been published in modern critical editions in Arabic or Latin.[1] Only the commentary on the *Physica* has no modern edition. This commentary no longer survives in Arabic but it is extant in Latin and Hebrew translations. The Latin version dates from the beginning of the 13th century and is probably by Michael Scotus;[2] the Hebrew is in an anonymous translation from the 1320s or 1330s [see Harvey 1985]. The Latin translation is available in manuscripts (over 60 in number) as well as in several old printings. The standard reference to this is the Junta edition of Averroes' *opera* from Venice 1562 vol.

[1] For a complete list of the editions, see Endress 1999.

[2] The name of Michael Scotus is mentioned on the translation of the long commentary on *De caelo*, and scholars agree that it is highly probable that he was also the translator of the *Physica*. Additional support for this ascription was provided recently by D. N. Hasse at the SEIPM conference in Palermo in 2007.

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4 (henceforth J). The Hebrew translation (henceforth H) is extant in only a few manuscripts.

It is not surprising that this highly influential and important text has not yet been edited. It is the longest of the five long commentaries (433 folios in the Junta edition) and is a very difficult text. One of the reasons for the many difficulties is that it was heavily revised.

During his study of the more than 60 manuscripts of this text, Schmieja discovered that in the manuscript preserved in the Nationalbibliothek Wien (cod. Lat. 2334), which he refers to as Vindobonensis 2334, book 7 and comments 80–86 of book 8 differ from the rest of the manuscripts, which he designates as the '*Vulgatversion*'.

The first volume that has just appeared in Schmieja's project is an edited version of book 7 according to Vindobonensis 2334 (henceforth V). The volume contains:

1. An introduction.
2. A critical edition of V with two *apparatus critici*: a variants apparatus and a comparison apparatus. The former is a standard apparatus of a critical edition, the second focuses on the excerpts. The excerpts from Aristotle are quoted from the Arabic translation of Aristotle by Yisḥaq ibn Ḥunayn. This translation is extant in one manuscript (Leiden Or. 538) and has been edited by Badawi [1964]. Schmieja compares the Latin translation of the excerpts as quoted in V, with the Arabic translation by Yisḥaq following Badawi's edition. He often also consults Ross' edition of Aristotle's Greek text.
3. A detailed comparison of V, the Vindobonensis version, with J, the *Vulgatversion* (represented by the Junta edition, which is easily accessible to the reader). Schmieja analyses the stylistic differences between the two versions and comments on differences and omissions. His thorough comparison leads him to the conclusion that V is based on a second Latin translation of Averroes' commentary, hitherto unknown. He identifies the translator of V as Hermannus Alemannus and convincingly substantiates his conclusion.
4. Two lexica. The first is an Arabic-Latin lexicon of the texts (the excerpts from Yisḥaq's translation) arranged according to the three-letter Arabic roots in Latin transliteration. This lexicon includes

references to Badawi's edition and to the texts in the Latin V version. The second is a Latin-Arabic lexicon which also includes a complete list of references to the Latin words in the text.

5. Bibliography of sources and studies.

There is no need to say how useful this edition and these lexica are to the reader, who hitherto had recourse only to the rather inconvenient Junta edition. Publication of further volumes is eagerly awaited. To be sure, immense difficulties are inherent in the preparation of a critical edition of such a long and difficult text based on so many manuscripts. This complex project is of outstanding importance and deserves all possible support and encouragement.

Appendix: An answer to Endress' question

Before Schmieja discovered the Vindobonensis manuscript (V), only two translations of Averroes' long commentary on the *Physica* were known: the Latin translation by Michael Scotus (J) and the Hebrew translation (H). In a recent work [forthcoming], I have shown that J and H are two different redactions of Averroes' commentary that differ very significantly from one another. I argued that Averroes revised his commentary very heavily, perhaps more than once. Presumably his manuscript was full of modifications and additions in the margins, perhaps between lines, and typically at the ends of comments. These numerous changes were difficult to handle by the (Arabic) copyists and the (Latin and Hebrew) translators, and this accounts for why the two versions that have come down to us in J and H are so different from each other.

At the SIEPM conference in Porto in August 2002, Professor Gerhard Endress raised the question whether the second Latin translation V, discovered by Schmieja, which differs from J in several places, might be based on the same Arabic *Vorlage* as H. While working on the comparison of V and J, Schmieja asked me a few times to check whether passages that he had found in V but not in J appear in H, and indeed there are many such passages. Now, with his edition of V at hand, I can compare the two Latin translations with that of the Hebrew and try to answer Endress' question.

The references to J are to the standard Junta edition, references to V are to Schmieja's new edition, and those to H are to the Cambridge Harvard Houghton Library Heb. MS. 40.

Group A differences: Passages in V and H missing in J

The common instance of group A differences is a brief passage (usually a sentence or a part of a sentence, rarely more) that is missing in J. This may sometimes be due to the tendency of Michael Scotus to skip short phrases and to abbreviate the text. As Schmieja has shown, *lemmata* that are usually referred to by a few opening words in J are quoted in full in V (e.g., page 97 on 11.23–27; page 98 on 14.3 and 14.16). *Lemmata* are always quoted in full in H. These differences can be 'blamed' on Michael Scotus, who apparently tried to save time and/or writing materials. However instances of omissions in J are common not only in the *lemmata*: see, for example, Schmieja's comments on pages 94 (on 2.6–10), 95 (on 7.16–19, 7.32–34), 110 (on 31.5), and 113 (on 34.9). Perhaps the omitted material corresponds to short additions and corrections placed above the line or in the margin in Averroes' manuscript, which the copyist of the Arabic manuscript that Michael Scotus used or Scotus himself (in the event that he worked on Averroes' autograph)[3] ignored.

Group B differences: Corrections by the editors of J

Of the three translations, only J was printed; and several errors were corrected by the editors. A few examples:

- *V 3.3* H 106a2: *quod movetur totum*] J 306F10–11: *non movetur secundum totum*.
- *V 3.6* H 106a6: *quinto*] J 306G2: *principio quinti*.
- *V 12.23* H 109b1: *sexto*] J 309H10–11: *quinto*, which is the correct reference.

Group C differences: Passages in H missing in J and V

These are passages that appear in the Hebrew translation and are missing in the two Latin translations. While group A passages are typically brief, some of group C passages are long. The most notable instance of group C differences is a long lacuna in the two Latin translations. The lacuna starts after text 37 (which corresponds to

[3] It is not unlikely that Scotus used a manuscript in Averroes' own hand. See Burnett 1999.

Aristotle, *Phys.* 225a9–14 and Badawi 1964, 792.5–13). The Hebrew translation goes on with comment 37 (28 lines), an additional text 37A (four lines which correspond to Aristotle, *Phys.* 250a15–19 and Badawi 1964, 792.13–793.5), comment 37A (ten lines), and another text 37B (which corresponds to *Phys.* 250a20–25). At this point the two Latin translations are resumed. The following comment, which is on text 37B, appears in J and V as a comment on text 37. It should be noted that in H there is no numbering of the texts, so the discrepancy in the numbering is not easily noted. Book 7 should thus count 41 rather than 39 text-comment units. Schmieja comments on the lacuna in text 37 on pages 78n35 and 133.

Group C differences are very common. I shall list only a few examples:

- *Comment 1* H 105b18–19 missing in J (306C9) and V (2.11).
- *Comment 3* H 108a11–16, and b1–4 missing in J and V.
- *Comment 7* H 110b13–14 missing in J (310M6) and V (16.9).
- *Comment 9* Text 9 consists of two sentences from the end of *Phys.* 7.1 and two sentences from the beginning of 7I.2. Comment 9 is problematic. The three translations are more or less parallel until J 311 L9 and from J 312 D5. Between these lines the order of presentation in J and V is confused. Also, the end of the comment (H 112b9–12) is missing in J and V.
- *Comment 14* H 117a25 is missing in J and V.

Group D differences: Passages in J and V missing in H

A rather long passage at the end of comment 2, J 308A10–C14 *Sed disolutio... per se* = V 7.20–8.9 *Responsio... in aere*, is missing in H. A few words at the end of comment 5, V *aut quantitatis aut qualitatis aut ubi* (13.6) / J *aut quantitatis & ubi* (309I12–13), are missing in H.

In order to draw all the interesting conclusions a complete systematic comparison of the three translations of book 7 is needed. However, the few examples that I have mentioned should be sufficient to answer Endress' question in the negative. V and H are certainly not based on the same Arabic manuscript. It is possible that V and J are based on two slightly different Arabic manuscripts. It is also possible that they are based on the same heavily emended Arabic

manuscript and that Alemannus was more careful than Scotus in the rendering of the corrections and additions.

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Theophrastus of Eresus: On Weather Signs edited by David Sider and Carl Wolfram Brunschön

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Theophrastus, Aristotle's successor as head of the Lyceum, had philosophical and scientific interests as wide-ranging as Aristotle's, but the only major works that survive complete are the botanical ones (*Historia plantarum* and *Causae plantarum*), as well as his briefer but influential *Characters*. Of his other major works we have only fragments and *testimonia* culled from later writers; and there is also a series of minor works by, or attributed to, Theophrastus. Since 1979, Project Theophrastus, founded by Professor William Fortenbaugh of Rutgers University, has produced a new edition of the fragments, the first volumes in a series of commentaries, and several volumes of essays on Theophrastus and related authors. More recently the project has embarked on a series of new editions of, and commentaries on, the minor works.[1] The volume under review belongs in the last category, being an important and valuable new edition of the treatise *De signis* (*On Weather Signs*). Sider and Brunschön have produced a new edition of the Greek text with an introduction, translation, commentary, and detailed indexes. The preface explains that Sider did most of the initial writing and is chiefly responsible for the Greek text; Brunschön is chiefly responsible for the descriptions of manuscripts and the *apparatus criticus*; but there has been constant consultation between them both.

The introduction is divided into the following sections:

1. Predicting the weather
2. Writing it down
3. Origin of *De signis* (preliminary considerations)

[1] See http://www.ucl.ac.uk/GrandLat/people/sharples/theophr.htm.

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4. Survey of ancient weather literature
5. Structure of *De signis*
6. The nature of *De signis*
7. How accurate are the weather signs
8. Authorship reconsidered
9. Textual tradition

In section 1, the authors distinguish between, on the one hand, treatments of the regular annual cycle of weather and of weather characteristically associated with the seasons as well as with smaller periods of the year, and, on the other hand, signs of what is not regular—which include both signs of imminent weather (it is one thing to know that it is likely to snow in winter, another to see a sign indicating that it will snow in the next hour) and signs of departures from the normal patterns (such as an unusually snowy winter or an unusually mild one). In section 2, Sider and Brunschön point out that these two types of prediction do not fit together easily in the same work: thus, Hesiod's *Works and Days* concentrates on the former, while *De signis* concentrates on the latter.

Section 3 gives the basic evidence and maps out the main possibilities concerning the authorship of *De signis*. Both Aristotle and Theophrastus are credited with works on weather signs; the manuscripts containing *De signis* all contain collections of Peripatetic works; some manuscripts attribute the work to Aristotle, in some it is anonymous, in one late manuscript it is attributed to Theophrastus. The main possibilities are that:

- *De signis* is a collection of raw data on weather signs gathered for Aristotle;
- it was written by Aristotle;
- it was written by Theophrastus;
- it is an abridgment of either Aristotle's or Theophrastus' work; and
- it is effectively an abridgment of them both, and perhaps even deserves to be considered as a compilation by a later author.

One important feature of *De signis* is that, with one or two brief exceptions, it contains none of the 'philosophical underpinning or scientific framework that Aristotle or Theophrastus would surely have supplied' [4]. Sider and Brunschön return to these issues later.

Section 4 is a useful survey of ancient Greek and Latin weather literature, both surviving and lost, starting from Hesiod and ending

up with anonymous works and extracts in late Greek manuscripts. Sider and Brunschön note any overlap, or lack of overlap, between the subject matter covered in these other writers (so far as their works are known) and in *De signis*. Some, like Hesiod's *Op. et dies* and the *parapegmata* (weather-calendars), deal exclusively with regular weather patterns and have hardly any overlap with *De signis*. *De signis* arranges its material according to the weather indicated, whereas some other works arrange, or can be conjectured to have arranged, their material according to the sign. In the course of the survey, Sider and Brunschön argue against the views, which have been held by some scholars, that Aelian, when in the *Nature of Animals* he says that he is using Aristotle, is in fact using *De signis*, or that *De signis* is a source of the Aristotelian *Problemata*. They also demonstrate that Aratus' Διοσημεῖαι is derived from *De signis* and not *vice versa* as some have argued.

Section 5 analyses the structure of *De signis*. After a prologue [cc. 1–9], the signs are arranged in five sections: signs of rain [cc. 10–26], wind [cc. 26–37], storms [cc. 38–49], fair weather [cc. 50–55], and miscellaneous weather [cc. 56–57]. But the prologue introduces topics that are virtually absent from the rest of the work (e.g., astronomical signs), discusses the causes of phenomena [ch. 3] in a way that is virtually absent from the rest of *De signis*, and contains other indications that it was originally the prologue to a longer, more comprehensive work; so that our *De signis* looks like an abridgment of a more extensive and more complex work. The main body of the work (on signs of rain, wind, storms, and fair weather) is arranged by type of weather indicated, which is not very helpful for practical purposes: arrangement by sign would be more useful. However, within each section there are traces of an arrangement by sign—this is tabulated on page 33—suggesting that some of the material may have been drawn from a work or works so organized. The final section [cc. 56–57] seems to have been an addition to the original text.

Section 6 summarizes the main characteristics of *De signis*, recapitulating some of the previous discussion and adding that it makes no attempt at completeness (many further signs are known from other ancient sources). It makes no claims about the practical value of the work, which is in fact rather impractical not just in its arrangement by types of weather, already mentioned, but also in the lack of

specificity at many points (e.g., winds are sometimes mentioned without any indication of their direction; and there is very little about the weather associated with specific winds). The work has 'a certain scientific appearance' [38], but makes no attempt to understand or explain what it describes. Section 7 raises questions about the accuracy of the weather signs, arguing that the meteorological signs are the most reliable category. Animal signs seem intuitively less reliable, though there is little modern scientific literature on the subject.

Section 8 first reviews previous opinion on the authorship of *De signis*. It was first attributed to Theophrastus by Simon Grynaeus, who excluded it from his 1531 edition of Aristotle and included it in his 1541 edition of Theophrastus. Theophrastean authorship was challenged by J. Böhme in 1884. Sider and Brunschön conclude that most likely *De signis* was based on Theophrastus' work on weather signs, with all the discussion of causes removed.

Finally, section 9 examines the textual tradition: there are 13 Greek manuscripts and a 13th-century Latin translation by Bartholomaeus of Messina. This literal translation is based on an independent Greek text. The Greek manuscripts are described, and their relationships analyzed, with a stemma [56] largely based on the work of D. Harlfinger and D. Reinsch [1970].

There follows a new text and translation [57–95] and commentary [97–219]. The text with *apparatus criticus*, based on fresh examination of the manuscripts, is a marked improvement on its predecessors. Significant misreports of manuscript readings are corrected; the commentary contains detailed textual discussions; and the editors have suggested emendations in more than 20 places, suggestions that are often convincing and always worth considering. The commentary, besides discussing textual matters, discusses the numerous places where the interpretation of *De signis*' elliptical Greek is problematic. Sider and Brunschön also set the work in the context of ancient weather-forecasting literature, with full citation of parallels in Aratus and in writers of the Peripatetic tradition, and briefer references to other ancient authors. There is also careful attention to the language and style of the work, and numerous notes on vocabulary correct or supplement the treatment of words in Liddell-Scott-Jones'

lexicon. There are careful explanations of the meteorological phenomena mentioned in *De signis*, and full discussion of the identification of the various plants, animals, and birds mentioned.[2]

After the commentary [221–225], there is a new edition and translation, by V. D'Avella, of a short text 'On the Locations and Names of the Winds', attributed to Aristotle. The book ends with detailed bibliographies (including brief descriptions of all earlier editions of, and commentaries on, *De signis*), an 'Index of Important <Greek> Words', a 'General Index', and an 'Index of Ancient Texts Cited', which will make this valuable work easily accessible for those who wish to consult it quickly.

I offer a few comments on points of detail in the text, translation, and commentary; references are given by chapter and line number in Sider and Brunschön's text.

- 10.67–68: The text printed is ἐὰν γὰρ δὴ πρότερον, the manuscript reading; but the translation 'if not before' assumes, correctly, that Wimmer's conjecture μὴ should be accepted in place of δὴ.
- 14.96: χειμῶνος ὄντος is a paleographically bold conjecture by Sider and Brunschön. Since the related passage 42.311 talks of black snuff, perhaps the manuscripts' τρεῖς conceals some other color?
- 15.101: The passage of Varro Atacinus quoted in the comment on τύπτουσαι belongs in the previous note, with the quotation from Vergil [*Georg.* 1.377].
- 22.147–148: The commentary states: 'The second-order rainbow occurs when the sun hits water droplets at a 52° angle from the eye to the direction of the sun so that a beam of light is reflected four times within a droplet before being directed to the eye' [142]. This is inaccurate. The angle is 51° and there are not four reflections: the beam of light is reflected twice within the droplet and refracted twice (on entry to and exit from the droplet). See Greenler 1980, 5–7, which is cited by Sider and Brunschön.
- 31.218: The commentary states that 'τε does not occur elsewhere in *De signis*'. As it stands this is false, for τε is used elsewhere: τε γὰρ is found at 2.8, 3.18, τε... καί at 5.31–32 and elsewhere, ἐάν τε... ἐάν τε at 16.109–111. Sider and Brunschön presumably

[2] On birds, there has now appeared, too late for Sider and Brunschön to use, the survey of ancient Greek birds in Arnott 2007.

mean to say that τε is not used elsewhere in *De signis* as a sentence connective. But Sider and Brunschön's emendation of τε to δέ is still justified, for τε as sentence connective is very rare by this date [see Denniston 1954, 499–500] and Bartholomaeus has *autem*.

- 33.237–238: In the commentary 'if the wind affects winds' should presumably read 'if the moon affects winds'.
- 34.247: In the commentary, Posidonius 'F 263 Thummer' should read 'F 263 Theiler'.
- 52.381: The emendation of πετόμεναι to πρὸ ἑαυτῶν is paleographically bold and surely unnecessary. There is a slight illogicality ('they [*sc.* cranes] do not fly until, *while flying*, they see clear weather'), but it is not really troublesome: the point is that while on the ground they cannot see whether there is clear weather ahead or not; they must already be in the air, flying, to get a good view, before they can decide whether to fly off in a straight line or to turn back. Sider and Brunschön's translation, 'until they see a clear sky ahead of them *as they fly*' [my emphasis] seems to combine their conjecture and the manuscript text.
- 54.397: The note in the commentary on the important point that ἔτος can mean 'season' repeats ground covered already in the commentary on 25.174–5, with no cross-reference.

But these are minor points in comparison to the achievement of Sider and Brunschön in producing a much improved text and richly informative introduction and commentary on *De signis*. Their work will be useful to students of the Theophrastean corpus, of ancient weather lore, and of the kind of popularizing scientific writing that *De signis* represents.

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Proclus' Commentary on Plato's Timaeus: III. Book 3, Part 1: Proclus on the World's Body edited and translated by Dirk Baltzly

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The commentary of the Neoplatonist Proclus on Plato's *Timaeus* might not be the most obvious choice to find enlightenment on ancient scientific thought. For although the topic of the *Timaeus* might attract, the reputation of the author for extremely complex metaphysical constructs, and the fact that the study of the *Timaeus* (and the *Parmenides*) formed the climax of the Neoplatonic curriculum as the summation of Plato's view of the natural world as a work of the divine demiurge, are not the most auspicious signs. A cursory glance at the text would readily confirm this impression. The metaphysical framework appears throughout in all its subtle complexity. One might easily then succumb to the prejudice that we are dealing with the imposition of an *a priori* metaphysical model to interpret the physical universe. Of course that in itself need not be without interest for the scientific observer. But, surprisingly perhaps, Proclus frequently reveals himself as sometimes less dogmatic even than Aristotle on issues concerning physical reality, as Lucas Siorvanes has made very clear in his *Proclus: Neo-Platonic Philosophy and Science* [1996].

It is this aspect of Proclus that Baltzly is concerned to unlock and understand in this edition, which contains the translation of pages 1–102 of volume 2 of Ernest Diehl's three volume edition of the Greek text [1904–1906]. As Baltzly points out, we do have the French translation with notes by Festugière [1966–1968]. But Neoplatonic scholarship has advanced considerably since its publication. And, more importantly, Festugière was primarily interested in the theological and metaphysical aspects of Proclus' commentary. Of course these cannot be ignored if we want to understand the relationship of metaphysics and science in the ancient mind; and Baltzly

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makes clear in his introduction and useful tabulation of the contents of the commentary that, in the part of his commentary translated in this volume, Proclus is very much concerned with the nature of the divine model in its relationship to the universe, i.e., a paradigmatic viewpoint. And yet within this framework, Proclus is seen to present arguments which are flexible and open, for example, his arguments against Aristotle on the necessity of a fifth element or his more accommodating views on the characteristics of the four elements. Where Aristotle sees only hot and cold, Proclus argues against the resulting polarization of the two extreme elements, and suggests a wider range of characteristics and proportional mixture.

Elsewhere, when arguing for the sphericity of the cosmos, Proclus firstly introduces what he calls 'philosophical' arguments, which are largely prompted by the text of Plato, before listing a number of physical arguments, mostly taken from Aristotle but with some interpretations of his own. He concludes with what he terms 'mathematical' arguments which are astronomical and belong more to the class of physical than philosophical arguments which, though also mathematical in expression, are based on abstract notions of proportion. Other interesting forays include arguments against the existence of a void 'beyond' the cosmos. More curious to us, perhaps, is his denial that the cosmos has sense-organs. Whilst the ancient mind had to cope with the idea that god 'hears' and 'speaks' and 'sees', for us the more interesting aspect of this enquiry is the careful discussion which it raises about the nature of sentient reality and the attempt to identify different grades of life. The enquiry is, of course, provoked by the Neoplatonic doctrine relating levels of cognition and activity or life, a doctrine which required the universe to be an ensouled 'living-being', but it then touches on the universal issue of locating and describing the nature of life-activity. It is precisely in such areas that metaphysics and physics explore some common ground.

Another historically important theory which makes its appearance throughout is the origin of light which is associated with the loftiest of the four elements, fire. Fire, like the other elements, has its cause in the Demiurge as an incorporeal Form. The fire in the universe is a corporeal expression of this. Once again the borderline between incorporeal and corporeal is touched on when Proclus

discusses the mirror-like 'smoothness' of the outer surface of the cosmos which is able to receive and presumably convert in some way the intelligible light.

Proclus' mode of presentation and his arguments are not always easy to follow. His frequent citing of the 'theologians' (primarily the Orphic verses, but including also Pythagoreans, the *Chaldaean Oracles*, and even Homer), whilst intended by him to present us with an illuminating overview of how all Hellenic wisdom forms a consistent world view, is for us often confusing, odd, and distracting. But Baltzly manages, in his notes, to give enough information to decode them without overwhelming us in gratuitous detail. The footnotes are extensive and provide the needed guidance to the sources, particularly Aristotelian, of which Proclus makes use. The translation is clear, and textual additions and corrections are introduced with discernment and always signaled. The reader is well-served not only by an index of Greek terms but also by a glossary of translations of key terms with their original Greek and a transliterated form. In addition, the translations of some sensitive key terms are accompanied in the text by the transliterated original in parenthesis. Baltzly rightly has not stuck rigidly to one translation for each term, but has chosen on each occasion the most appropriate of a number of possible English versions. The translation reads well as it prefers ease of interpretation to awkward literalness. A good example of this kind of sensitivity is the translation 'pyramid shape' where the Greek has 'such a shape'. Baltzly has correctly done for us the interpretation from the original Greek context to produce a readable translation, but also helpfully explains in a footnote [87n144] how he has taken this liberty. Sometimes, however, a term seems to be downgraded somewhat, e.g., the translation of κατ᾽ αἰτίαν as 'in a preparatory way', a translation which dilutes the causal sense. But he does give the transliterated phrase in the text (and we could find αἰτία in the glossary, though we would have to know to look under 'cause').

Proclus' general layout, too, is not always easy to follow but Baltzly has usefully provided a summary and headings which keep us on the right track, even if Proclus is at times a little inconsistent in his method with periodic generalizing under the heading θεωρία and occasional sequences of close textual analysis which is conventionally termed λέξις. As Baltzly correctly points out, the successive

treatment of ten demiurgic gifts to the world is the guiding structure to Proclus' commentary. More than anything, this top-down approach serves to demonstrate the essentially metaphysical nature of this commentary; for, in the final analysis, a Platonist would have to admit that the intelligible model of the world is a more appropriate object of secure knowledge than its ever-changing physical image. But this does not diminish the importance of Proclus' contribution to our understanding of how science and metaphysics may work together, and this translation with its commentary and introduction will be an important aid in the further evaluation of Proclus' place in this tradition.

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