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Contents

Preface x

1. Paul T. Keyser on
Being, Humanity, and Understanding: Studies in Ancient and Modern Societies by G. E. R. Lloyd 1

2. Sonja Brentjes on
Routes and Realms: The Power of Place in the Early Islamic World by Z. Antrim 13

3. Antoine Pietrobelli on
Hippocrates: On the Art of Medicine by J. E. Mann 22

4. Jeffrey A. Oaks on
Abū Kāmil. Algèbre et analyse Diophantienne. Édition, traduction et commentaire by R. Rashed 24

5. Arthur MacGregor on
Morbid Curiosities: Medical Museums in Nineteenth-Century Britain by S. J. M. M. Alberti 50

6. Robert Mayhew on
Aristotle: His Life and School by C. Natali 56

7. Heidi Marx-Wolf on
Cosmology and Fate in Gnosticism and Graeco- Roman Antiquity: Under Pitiless Skies by N. Denzey Lewis 66

8. Pietro B. Rossi on
De motu animalium. *Fragmenta translationis anonymae* De progressu animalium, De motu animalium. *Translatio Guillelmi de Moerbeka* edited by P. De Leemans 71

9. André Goddu on
Kepler's Cosmological Synthesis: Astrology, Mechanism, and the Soul by P. J. Boner 89

10. Steven A. Walton on
Artisan/Practitioners and the Rise of the New Sciences, 1400–1600 by P. O. Long 100

11. Nathaniel Wolloch on
Ethical Perspectives on Animals in the Renaissance and Early Modern Period edited by C. Muratori and B. Dohm . . 107

12. Stamatina Mastorakou on
Illustrating the Phaenomena: Celestial Cartography in Antiquity and the Middle Ages by E. Dekker 115

13. J. L. Berggren on
The Principles of Arab Navigation edited by A. Constable and W. Facey 121

14. Elizabeth A. Hamm on
Heavenly Mathematics: The Forgotten Art of Spherical Trigonometry by G. Van Brummelen 127

15. Jonathan Barnes on
Aristotle's Categories *in the Byzantine, Arabic and Latin Traditions* edited by S. Ebbesen, J. Marenbon, and P. Thom 131

16. John Sellars on
An Essay on the Unity of Stoic Philosophy by J. Christensen 139

17. Jennifer Karns Alexander on
Progressive Enlightenment: The Origins of the Gaslight Industry, 1780–1820 by L. Tomory 143

18. Alison Laywine on
Greek and Roman Musical Studies 1 edited by A. Barker 145

19. Laurence Totelin on
Théophraste. Les causes des phénomènes végétaux. Tome I. Livres I et II edited and translated by S. Amigues . . 168

20. Katja Maria Vogt on
Sextus Empiricus: Against the Physicists translated and edited by K. Bett 171

21. Duncan J. Melville on
The History of Mathematical Proof in Ancient Traditions edited by K. Chemla 179

22. Paolo Palmieri on
Copernicus: Platonist Astronomer-Philosopher by M. Vesel 188

23. Sarah Pothecary on
Barrington Atlas of the Greek and Roman World for iPad edited by R. J. A. Talbert 191

24. Bernard R. Goldstein and Giora Hon
A Comment on Paolo Palmieri's Review of Matiaž Vesel's *Copernicus* 202

25. John Ryan on
Aratus and the Astronomical Tradition by E. Gee 205

26. Lucy Barnhouse on
Hospital Life: Theory and Practice from the Medieval to the Modern by L. Abreu and S. Sheard 217

27. Toke Knudsen on
Astronomy and Mathematics in Ancient India by J. M. Delire 223

28. Bronwen Wickkiser on
Medicine and Society in Ptolemaic Egypt by P. Lang . . . 227

29. Paul D. Buell on
Lost Enlightenment: Central Asia's Golden Age from the Arab Conquest until Tamerlane by S. F. Starr 232

30. Delphine Bellis on
L'homme au risque de l'infini. Mélanges d'histoire et de philosophie des sciences offerts à Michel Blay edited by M. Malpangotto, V. Jullien, and E. Nicolaidis 236

31. Corinna Rossi on
Count Like an Egyptian: A Hands-On Introduction to Ancient Mathematics by D. Reimer 243

32. Sylvia Berryman on *Aristotle's Empiricism: Experience and Mechanics in the 4th Century BC* by J. De Groot 248

33. Cinzia Arruzza on *Neoplatonism and the Philosophy of Nature* by J. Wilberding and C. Horn 253

34. Aldo Brigaglia on *Diofanto,* De polygonis numeris by F. Acerbi 260

35. Donald J. Zeyl on *Science before Socrates: Parmenides, Anaxagoras and the New Astronomy* by D. W. Graham 283

36. Börje Bydén on *The Philosophy of Gemistos Plethon: Platonism in Late Byzantium, between Hellenism and Orthodoxy* by V. Hladký 290

37. Paul Keyser on *The Ancient Mediterranean Environment between Science and History* by W. V. Harris 312

38. Steven Vanden Broecke on *Astrology and Magic from the Medieval Latin and Islamic World to Renaissance Europe: Theories and Approaches* by P. Zambelli 334

39. Elizabeth Craik on *L'anonyme de Londres (P. Lit. Lond. 165, Brit. Libr. Inv. 137). Édition et traduction d'un papyrus médical grec du Ier siècle* by A. Ricciardetto 337

40. Margaret Small on *Printing a Mediterranean World: Florence, Constantinople, and the Renaissance of Geography* by S. Roberts 339

41. Federico M. Petrucci on *Jamblique. In Nicomachi arithmeticam. Introduction, texte critique, traduction française et notes de commentaire* by N. Vinel 342

42. Sara J. Schechner on
Astrolabes from Medieval Europe by D. King 354

43. Francesco Luzzini on
L'eruzione vesuviana del 1631. Una storia d'età moderna by A. Tortora 364

44. 2014 Books Received 370

Preface

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

This publication has grown nicely. As thanks to our readers, in addition to the reviews published *separatim*, each complete, collected volume will now be made available online as well (go to http://www.ircps.org/aestimatio). All online versions of *Aestimatio* will, of course, remain accessible free of charge. For those interested in printed copy, volumes 1–8 are available in print from Gorgias Press (go to helpdesk@gorgiaspress.com).

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Alan C. Bowen

Tracey E. Rihll

Being, Humanity, and Understanding: Studies in Ancient and Modern Societies by G. E. R. Lloyd

Oxford/New York: Oxford University Press, 2012. Pp. 136. ISBN 978–0–19–965472–7. Cloth $45.00

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This volume is the latest, and worthy, installment in Lloyd's long project, in progress since his *Magic, Reason and Experience* [1979] or even his *Polarity and Analogy* [1966], to understand ancient mentalities, especially in the realm of natural science. The five chapters of this slim volume engage their material and the reader with verve and vigor, and deepen Lloyd's work in confronting ancient mentalities, particularly Greek and Chinese.[1]

In this book, Lloyd announces his intent to strike a balance, as he did regarding cognition in Lloyd 2007, 'between cross cultural universalists and cultural relativists', in particular here to illuminate 'what we may call cosmologies' [1]. Lloyd examines the terms of the debates, the character of the arguments, and the nature of the evidence, for two of what philosophers sometimes label the 'Big Questions', namely, what it is to be human [ch. 1] and what it is to understand the world around us [ch. 3]. Chapters 2 and 4 seem to this reader ancillary to those goals; chapter 5 sums up.

His data include not only ancient China and ancient Greece, as before, but also ancient Mesopotamia and modern anthropology of Amazonia (as in Lloyd 2007, e.g., 143–149). In transgressing the modern disciplinary boundaries, Lloyd has shown himself to be a bold scholar (and fortune favors the bold). He has gone to the (considerable) trouble of learning Chinese and also

[1] Recent works by Lloyd mining that vein include: *Disciplines in the Making: Cross-Cultural Perspectives on Elites, Learning, and Innovation* [2009], *Cognitive Variations: Reflections on the Unity and Diversity of the Human Mind* [2007], *Ancient Worlds, Modern Reflections: Philosophical Perspectives on Greek and Chinese Science and Culture* [2004], and his joint effort with Nathan Sivin, *The Way and the Word* [2002], which is reviewed in Keyser 2004.

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the special discourse of anthropology when already a senior scholar: few scholars of any age in any field take such trouble; fewer still manage the journey with such aplomb. There are risks attendant upon such transgressions, both the risk of misunderstanding the less-familiar language as well as the risk that the natives of the transgressed field will dismiss the transgressor as a poacher. Lloyd is a genial Herodotus of modern academia, traveling widely, absorbing broadly, and returning with marvels. He would be the first to proclaim his results provisional and to acknowledge the concomitant necessity of revision.

Lloyd's first chapter, 'Humanity between Gods and Beasts?' serves to open the discussion with a well-chosen issue. Well-chosen because we can be nearly certain that the query regarding the nature and place of humans in the world in which we find ourselves has been raised in some form in every human culture that has ever existed. It is a query demonstrably present in ancient Greek and ancient Chinese sources, and has been a focus of recent attention in anthropological debate.

Moreover, the nature and place of humans in the world has previously been a focus for Lloyd himself, albeit with different goals. In his *Cognitive Variations* [2007], ch. 3 ('The Natural Kinds of Animals and Plants') addressed the possibility of discovering a definitive taxonomy of animals or of plants. Just as the boundaries between species of animals vary according to different models, so does the boundary between humans and 'other' animals. Whether in the Greek thinker Aristotle, the Chinese work *Huainanzi*, or the reports collected among the Itza' Maya of Guatemala (as read by Lloyd 2007, 46–49), the same system that classifies animals then extends to demarcating all of them from us: 'humans regularly emerge at the top as quite special animals' [Lloyd 2007, 55]. Earlier, in his *Ancient Worlds, Modern Reflections* [2004], ch. 11 ('Human Nature and Human Rights') also explored this issue, seeking to elucidate the varying bases for making moral claims, in so far as those bases relate to claims about the 'nature' of human beings. Here Lloyd's focus was on the boundaries within and without which moral responsibility could be assigned, especially male *versus* female and in-group *versus* 'barbarian'. Within ancient Chinese culture, humans were distinguished from animals by having moral sense [*yi*: Lloyd 2004, 158–159] and yet barbarians were qualified in many ways as being like animals [2004, 161], so that '[s]uch tolerance as the Chinese showed to other groups came primarily from an effortless sense of their own superiority' [2004, 164]. Greeks too viewed barbarians as somewhat subhuman, e.g., being classified by Aristotle as 'natural slaves' [*Pol.* 1.3–4, 1253b–1254a]. Thirdly, the nature and place of humans in the world was a particular interest of Lloyd's teachers, Vernant, Detienne, and Vidal-Naquet, around the years 1972–1975, as Lloyd notes in the work under review [8n1] right at the beginning of this first chapter. The interest continues among many scholars and philosophers, as Lloyd notes [29], citing, e.g., Sorabji, *Animal Minds and Human Morals* [1993], an investigation of ancient thought on the nature and status of animals as cognitive agents.

Thus, Lloyd's choice of opening topic here is over-determined and familiar. Yet Lloyd does not merely re-present old ideas in new clothes (as some senior scholars have done): rather, somewhat like rotating a kaleidoscope while viewing the same scene, Lloyd shows us yet another aspect of the multifaceted issue under study.[2] Lloyd here [8–14] swiftly reviews various attempts in the ancient Greek (and modern Euro-American) tradition to find a distinguishing criterion that would securely differentiate animals from humans and shows how each in turns fails to be as rigid and reliable as its proponents claim. He points out how that unresolved debate caused the European conquistadors of the Americas to doubt the humanity of the humanoid beings dwelling there—just as the natives were in doubt about the humanity of the invading species [11]. I would just note that the problem had already been raised in (Late) Greco-Roman antiquity by Augustine, who in *The City of God* 16.8 queried whether 'monsters' are human and concluded that if they have souls, however they look, they must be human. He does not, however, provide a recipe for determining whether such beings, hypothetical to him, actually do have whatever a soul is. Lloyd's review of the Chinese evidence reiterates his thesis in *Ancient Worlds, Modern Reflections* [2004, 158–161], that humans are beings with moral sense—which raised issues of differing moral systems—or, according to other Chinese texts, that humans all share certain basic needs [17].

Turning to anthropology for further data about humans *versus* animals [17–21, 26–29], Lloyd makes use of the work of two students of Amazonia. One is Viveiros de Castro, the Brazilian anthropologist (influenced by Roy Wagner), whose field-work in 1981–1988 concerned the Araweté (a tribe

[2] Lloyd indeed emphasizes precisely that point, referring to the multidimensionality of the phenomena [36–37]: cf. 2007, 41, 56–57.

of the Tupi-Guarani people), and who aimed to demonstrate that it is their cosmology, rather than their ecology, that is constitutive of their society: the Araweté believe that they will become divine once they are slain and eaten by the gods.[3] The other is Descola, the French anthropologist, whose fieldwork in 1976–1978 focused on the Achuar at a moment when competing Roman Catholic and Evangelical Protestant missionaries were active among the Achuar, and who aimed to mediate between the ecological and symbolic schools of anthropology.[4] The two systems of Descola and of Viveiros de Castro, as Lloyd explains, provide radically different analyses [21]. But the dichotomy presented may be somewhat false. On the one hand, these analyses treat only two groups in one area—that is two more than we had when looking 'only' at ancient China and ancient Greece. Yet perhaps the apparent dichotomy would be ameliorated by considering similar analyses of, say, African or Siberian peoples. Then again, each anthropologist began his work with an explicit agenda so that care is needed when reading out of their work conclusions that do not accord with their agenda: such readings are a mediated interpretation, analogous to reading a modern scholar's interpretation of, say, Aristotle, or of, say, the *Huainanzi*, rather than reading each of those ancient works directly. Moreover, the two anthropologists share a significant common formation: both were working within the (then-dominant) anthropological scholarly tradition of structuralism.

Nevertheless, Lloyd makes sensitive use of the data by turning back to the Greek and Chinese evidence, and considering the issue of differing ontologies [21–26]. In particular, he points out how the (eventually-dominant) four-element theory of the Greeks and the five-phase (*wu xing*) theory of the Chinese cannot simply be reduced to one another. But from each point of view, the other can be understood. That is—and this is a point Lloyd has stressed more than once—the models are not wholly incommensurable. Indeed, if different models were wholly incommensurable, how could anyone ever change their mind about models? How could Aristotle have developed a novel classification of animals and plants [Lloyd 2007, 53–54]? Likewise, we might ask, how could Dalton and others have developed the (modern) atomistic model? And as Lloyd proposes, we can attempt to describe other

[3] See Rival 1993, Chernela 1994, and Schmidt 1997 for reviews of Viveiros de Castro's work.

[4] See Riviere 1987, 1995; Meggers 1999; and Fisher 1999 for reviews of Descola's work.

people's lived experiences, 'their worlds ...so different from the one we usually take for granted, and yet not totally beyond our reach' [26]. Lloyd succeeds in showing how the question of humanness is a genuine problem and advocates that we regard that problem as an opportunity to broaden our investigations [29–30].

As noted, Lloyd rejects radical incommensurability [see esp. 2, 5–6, 24–26: cf. Lloyd 2004, 13; 2007, 159–160], and rightly so I would say. Now, taking the position that even profoundly differing models can be mutually comprehensible requires one to confront the question of how people can change their ideas and, thus, the associated problems of error [ch. 2] and of the limits of language to express ideas [ch. 4]. If all models are equally valid or if no analysis of any model is possible, then no error can ever be detected: and conversely, to claim that models may be compared and comprehended is to claim that they can be evaluated and that errors may be found within them. To revise is to acknowledge error and master it. (And error, *contra* the Stoics and other like-minded thinkers, comes in degrees in this our forever un-perfect world.) Thus, in ch. 2 [see also 3, 119], in order to manage our response to error, whether our own or others', Lloyd advocates three methodological principles [cf. Lloyd 2007, 56–57]:

(1) attempt to employ actors' categories [35–36];
(2) practice a charity of interpretation as advocated by the philosopher Donald Davidson and others [36]; and
(3) recall that interpretation is provisional and thus be open to revision [36].

The claim that differing models can be comprehended is a claim that language suffices to communicate: as Lloyd remarks,

> I have yet to hear of an anthropologist who returns from the field announcing that she could understand *nothing* about the people she was studying. [24: emphasis in original].

Thus, in ch. 4, Lloyd treats issues of mutual understanding between cultures, or even between actors within the same culture acting from within different frameworks by offering three insights. He introduces 'semantic stretch' as a covering term for various kinds of metaphor or manners of using terms [86]; this is a stronger form of the well-known technique of allowing for varying semantic ranges of words. The deployment of this method will have similar effects to making use of actors' categories and charity of interpretation: that is, analysis will be more open to understanding and less focused on refutation. Second, he foregrounds the effect of the audience upon the discourse [81–84], what he called in *Demystifying Mentalities* [1990, e.g., 9–12, 126–131] 'the contexts of interpersonal exchange', and what is now commonly called the discursive context. As he notes, most Chinese writers on science addressed the emperor and spoke in the guise of a wise but submissive advisor (that is also true of many Latins and some Greeks of the Roman Empire, as has often been noted [see Keyser 2010, 870–874]). These diverging discourses are not sealed off from one another, no more than divergent concepts are wholly incommensurable [88]. Finally, he reprises his advocacy of allowing for multiple modes of analysis, i.e., the 'multidimensionality' of the data, as when one analyses color terms now using hue, now using luminosity, or as when one analyses substances of the world now in the Greek manner of elements, now in the Chinese manner of processes [36–37, 90–91: cf. Lloyd 2007, 41, 174].

The focus of the book is its longest chapter, i.e., ch. 3 ('Ancient Understandings Reassessed and the Consequences for Ontologies'), which addresses ancient science specifically in the sense that Lloyd (and this reviewer) use the phrase. Lloyd here restates his often-made case that there is such a thing as 'ancient science' [3–4], as he did, e.g., in *Ancient Worlds* [2004, ch. 2] and in *Disciplines* [2009, ch. 9]. In the latter, his formulation was particularly vivacious [159–160]:

> How can we begin to understand how it was that—suddenly, or over a period of time—humans, who had had (on this view) no science at all up until then, came to practise this mode, or modes, of inquiry? It is crucial here to get clear how strong a claim for innovation is being made, and in what regard. Did the breakthrough (however understood) depend on new cognitive capacities, or merely on the new deployment of already existing ones? Either way there are problems. If we take the first option, what sense can we make of the idea of acquiring new cognitive capacities, and were they just confined to the scientists in question or did they somehow become more generally available? On the latter option, if the capacities were always there, why were they not used?

The hypothesis, that science somehow sprang full-grown from the brow of the Renaissance is, in short, absurd and no more likely than any theory of spontaneous generation or special creation. In the current work, Lloyd focuses on 'Greece, China, and Mesopotamia especially—the relevant Egyptian data are in shorter supply and those from India are of very insecure date' [48]. His caveats are valid but much work has been done to elucidate the sciences of the two cultures omitted here; in a longer book (and one can always wish for a longer book from Lloyd), they would find their natural place. He raises several arguments for considering there to have been scientific works in the three cultures that are in his focus. One is to point out that contemporary science proceeds by what one might call creative destruction, continually revising its results, which is to say, that modern science in essence presupposes the possibility of refutation [47]. He surveys in some detail the results of several generations of work by scholars on Mesopotamian astral sciences [48–50], likewise what we have come to know about Chinese mathematical and astral sciences [51–56], and then the recent consensus regarding Greek mathematical and cosmological arguments and disputes [56–61]. From that last survey, Lloyd elicits six points about Greek science [61–63]: namely,

(1) ancient theorists seem intent on excelling rivals;
(2) each offers an account that claims to see through the appearances to a hidden reality;
(3) the accounts concern 'nature';
(4) despite radical disputes, the actors perceived themselves as addressing common issues (i.e., a denial of incommensurability);
(5) their views on nature are correlated with their views on human customs; and
(6) they disagreed regarding teleology and each ontology implicated morality.

For all three cultures' sciences, the prospect of disconfirmation is raised [50, 52–53, 56–57, respectively].

In his remarks on Mesopotamian sciences, Lloyd makes the unfortunate claim that 'Neugebauer ...showed ...nothing but contempt for astrology' [48]. That claim is not crucial for the development of Lloyd's thesis and is incorrect. Otto Neugebauer in fact vigorously advocated the study of astrology in 'The Study of Wretched Subjects' [1951], reprinted as the leading article in his self-edited *Astronomy and History: Selected Essays* [1983, 3]—the title of the article is deliberately ironic, as an attentive reading of the article shows. Moreover, he produced (with H. B. van Hoesen) *Greek Horoscopes* [1959], a careful and thorough edition and translation of all the Greek (and Latin) horoscopes then known, both literary and papyrological. Neugebauer focused his work on the mathematics of ancient astronomy, which surely advanced our understanding of Greek and later astral sciences and their relation to Mesopotamian astral sciences. That focus is narrower than Lloyd's but many productive scholars of Greek sciences have had analogously narrow focuses: whether on the Hippocratics or on Aristotle or on Archimedes, and so on. (To be sure, this is the only error that I have spotted in this book.)

Then in ch. 5 ('Philosophical implications'), Lloyd examines the degree to which his investigations have clarified the chief interpretive issues in his book. Furthermore, he attempts to relate these results to contemporary 'strategic' problems, a goal also in view in his earlier works, especially in his *The Delusions of Invulnerability* [2005]. There we find ch. 1 ('The Pluralism of Philosophical Traditions') [esp. 32–35], in which he argues that the history of philosophy can be a resource for current critical evaluation and that we ought not to relegate philosophy to the academy. In ch. 2 ('Learned Elites: Their Training, Openness and Control'), Lloyd finds seven factors important for success whether in Greece or China [54–56] and relates them to the contemporary academy [57–61]: our problems are as theirs were but on a larger scale. The chapter on 'Audience and Assemblies' [ch. 3, esp. 81–86] studies how scholars and scientists can publish and receive productive criticism in different cultures and systems, as well as the role of political debate in their activities. (Chapters 4 ('The Delusions of Invulnerability'), 5 ('The Frailties of Justice'), and 6 ('Models for Living') are similar in import but do not touch greatly or directly on science *per se*.) In the work under review, Lloyd highlights four problems. The last of these, which he labels 'realism and relativism' [94, 102–105], relates closely to his first chapter on the ontologies of humans and animals. Animals, he argues, really do differ but when various cultures (including our own) attempt to construct a valid classification of those differences, various factors intrinsic to the culture or the animals render the classifications ragged at their edges. Procedures and styles of inquiry mean that there is no final taxonomy; but some taxonomies of any given kind are more accurate than others. That is, neither pure realism nor pure relativism can give a useful account: we need both.

Besides that last problem ('realism and relativism'), Lloyd addresses three other strategic problems:

(1) incommensurabilities [93, 105–111],
(2) objectivity [93, and 94–97], and

(3) truth [93–94, 97–102].

Regarding objectivity, we cannot, insists Lloyd, impose our framework upon the ideas or frameworks of others; but when dealing with any alien framework, we must use the one we have to think with. The dilemma, suggests Lloyd, is evaded if we recall that any given framework (our own, say) 'is no monolith' [95], that each such framework was acquired through education, and indeed that each of us has acquired a slightly different version of the framework, where moreover, the acquisition of a framework is *ipso facto* the acquisition of knowledge and conceptual tools new to the acquirer. Thus, as we study other frameworks, we can revise and augment our own: the same kind of progress that we made as children we can all still make. As for truth, we cannot, insists Lloyd, rely on any naïve correspondence theory of truth, since we have no direct access to any unmediated reality; and yet mere consistency does not suffice, given the many examples of internally-consistent models that were eventually demonstrated to be false (or, as Lloyd says, 'palpable nonsense' [97]). Various criteria were proposed or deployed in ancient Greece and China, and this dilemma, suggests Lloyd, is evaded if we follow that lead and allow for various criteria of truth appropriate to varying situations. He cites the example of the ratio of the circumference to the diameter of the circle, which must be more precise for a mathematician than for a builder: that is, he advocates criteria that yield the 'approximately true' [101]. What is known to be true, is known to be true up to some limit. (Indeed, modern science exerts considerable effort to specify precisely its limits of accuracy.) As for incommensurability, the dilemmas posed by Kuhn regarding conceptual shifts are not so dichotomous as represented by him and others [106–107]: not only is it clear, e.g., that 'Galileo had a fair idea of what Aristotle meant' but modern readers manage to grasp the concepts of both (despite being separated by several further paradigm shifts). Likewise, the views of the anthropologists Descola and Viveiros de Castro do pose a challenge to views developed in the Greco-Roman tradition (or for that matter in the Chinese tradition) but 'they would hardly pose the kind of challenge they do if they were simply incomprehensible' [108].

Those problems are indeed worthy of further debate and study but it may be that they are not three separate problems. In each case, underlying the dilemmas exposed by Lloyd, is a common tension: between what is (partially) known in one model or framework or paradigm and what is (partially) known in another. History shows that those tensions do get resolved, especially when a given model is confronted with a competing model or else is confronted with new evidence that serves to call portions of the model into question. That process can readily be understood as a kind of evolutionary meta-model of the development of science, as I have argued [2013]. That is no criticism of Lloyd, who was the original inspiration for the development of that approach in his early works *Magic, Reason and Experience* [1979, 226–267] and *The Revolutions of Wisdom* [1987, 50–171]. That I have taken that further than he has done is not, I hope, incommensurable with his work. That his work has inspired attempts to grasp the multidimensional nature of ancient science by many scholars is a testament to the enduring worth of Lloyd's work, even when, nay especially when, his provisional framework is challenged in its turn.

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Routes and Realms: The Power of Place in the Early Islamic World by Zayde Antrim

Oxford: Oxford University Press, 2012. ISBN 978–0–19–991387–9. 240. Cloth €41.99, $65.00

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This book is a re-publication of previously published articles or talks, as Antrim's entry on the faculty page of Trinity College, Hartford, CT reveals. It consists of an introduction and three chapters (homeland, cities, regions). It purports to establish that there was a 'discourse of place' in a broad range of texts and a more limited range of images (i.e., maps) that were produced, mostly in Arabic, during the first centuries of the Abbasid dynasty. In its three chapters, Antrim extracts anthologies, historical chronicles, and geographical works; and supplements this with further extracts from the occasional travel account, astronomical work, or religious treatise [2]. These brief extracts, which are never given in the original language, are joined by way of quotations and summaries in an easy flowing combination of the reformulated opinions and interpretations of previous researchers. The author's own contribution seems to consist in:

- the use of a relatively limited vocabulary of current academic jargon with terms such as: 'crafting', 'performing', 'constructing', 'imagining', 'invoking', 'evoking', 'body', 'discourse', 'strategy', 'nurture' and the like;
- the erasure of differences between genres, skills, beliefs, and values which, according to a more traditional approach to historical analysis, characterize poetry, historical writing, texts of the mathematical sciences, and the styles and models of descriptive *versus* mathematical geography and their ideas of mapping;
- the avoidance of any analysis of the various sources which she has mined for the alleged 'discourse of place'; and
- the thorough abstention from any contextualization of authors, copyists, and other actors, their works, and the traces that they have left behind.

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I am certain that there will be other reviewers who praise this book as a new pinnacle of academic achievement. But I simply cannot join this chorus. Too many problems stand in my way. Antrim neither explains why she chose the period from the ninth to the 11th century for her claims that there was a 'discourse of place' and that 'place had power' [1]. All she offers is the statement that 'the discourse of place is a conceptual framework', which she uses

> to bring together a wide variety of formal texts committed to the representation of territory in and of itself, rather than as a setting or backdrop of something else. [1]

She declares that such a discourse existed by attributing to 'land' as used in the chosen texts a 'stimulation' of 'geographical imagination' with the capacity to '(act) as a powerful vehicle for articulating desire, claiming authority, and establishing belonging' [1]. Such a declaration is, however, not a proof that there was indeed such a thing as a 'discourse of place' nor an analysis of why that was the case and how this discourse functioned on the three claimed levels of 'homeland', 'city', and 'region or world' for achieving the proclaimed 'goals' (desire, authority, identity).

Certain passages, as for instance those on page 6, suggest that the supposed 'discourse of place' is merely 'a representation of territory' or the manner in which 'Muslims imagined the territory where they lived and traveled'. That a geographical text deals with places in spaces does not signify yet that this was a discourse beyond and above there being merely the choice of the appropriate words for representing geographical knowledge. That there was a special type of poetry (which rarely used the word 'awtan', i.e., the plural of 'watan') that evoked nostalgia for a collection of things (animals, foods, plants, rivers, humans, and so on) left behind due to a nomadic lifestyle, other kinds of (real or imagined) travel or marriage does not prove without further substantiation that there was a 'discourse of homeland' or, in particular, that the word 'awtan' also had extraterritorial connotations (as the author herself states [11–29, esp. 14]. Moreover, the author admits that this word mostly appears in titles and headers of chapters but rarely in the text proper, where a variety of other terms is used [15–16]. The highly anthropomorphic and zoomorphic language of this 'discourse of place' as 'homeland' and its many references to themes of childhood and adolescence, as nicely brought out by the author, necessitate further arguments, preferably from anthropology, for their use as evidence that it was indeed place as territoriality that was at stake in these poems. This is, however, not supplied by Antrim [17–19].

This first chapter on *watan* does not even convince me that ‘watan’ indeed signified ‘homeland’, a concept that sounds conspicuously modern. A study of the semantic field of this rarely used term would have been very helpful to understand whether such a value-laden translation is indeed appropriate. The question whether the ‘nostalgic longing for *watan*’ constituted a ‘discourse of place’ or was rather an element of an altogether different discourse such as that studied by Scott Savran in his thesis *Eloquent Tribesmen, Dignified Sheikhs, and Pompous Kings: Conceptualizing Early Islamic Historical Accounts of Arab-Sasanian Encounters in the Context of the ’Abbasid High Culture* [2011] is not even considered. For Savran, this discourse concerned struggles for cultural and political preeminence between Arab and newly converted Muslims in the new socio-political atmosphere of a universal Islam with career opportunities for all Muslims. He argues, among other things, that collecting Bedouin poetry in all its richness and using it in historical chronicles as well as in other texts, in combination with negative stories about Sasanian nobility, their arrogance, and pompousness, served as central components of a long-lived discourse that elevated Arab Bedouins to the pedestal of high civilization and interpreted the defeat of the Sasanian royal army at the hands of impoverished and unrefined nomads as a divinely preordained way of redemption of Arabs and Khurasanians alike and their joining in the ‘universalist’ community of believers [see, e.g., Savran 2011, 11–15, 254–257]. Places and spaces like al-Qadisiyya, al-’Iraq, or Khurasan, to name only a few, were central to these narratives but, according to Savran, apparently did not appear to form a discourse separate from that about the cultural roles and worth of Arabs and Iranians.

Antrim’s explanations of why she excluded certain types of texts that either focused on depictions of territory (travel accounts of territories outside the Islamic world or quotations of poetic verses on place or space in the biography of a poet or a scholar, for instance) [6] are equally problematic and do not convince me of their conceptual or methodological validity, although I certainly accept that the number of texts studied had to be limited so that the work could be completed within the period of her fellowship. But claiming that quotations of verses about places or spaces in a biography of a person ‘function primarily to portray an aspect of the author’s life or an example of his intellectual production' [6] but not as an element of the 'discourse of place' illustrates her weak notion of the concept of discourse. This decision also reflects a lack of familiarity with the multitude of ways in which biographies were composed as complex, multi-leveled narratives.

The same applies to the exclusion of territories that she labels 'exotic' [6]. For the study of discourses, such exceptional things are of equal, if not even greater, methodological relevance if one is to discover the main discursive elements and to uncover their textual as well as ideological, emotive or mental functions and operations.

These and other silences, omissions, and simplifications speak as loudly against praising Antrim's work as do the various nominalizations and 'post-modernizations' that she uses. After reading continually that authors of the ninth, 10th, or 11th centuries 'crafted' their poems, histories or scientific texts or that these writings performed on three levels (for instance, that of the nostalgically longed for past, that of the writer's writing time, and that of the later authors' use of such earlier textual residues), I nostalgically longed for the times when academics simply wrote, discussed, analyzed, argued, gave a talk, or held a lecture. I am certainly not adverse to some effort at performance in teaching or public presentation and to serious efforts to accomplish a well-written text. But I do not wish to be drowned in and bored by an overdose of such a vocabulary at the expense of all other verbs and nouns available to depict texts, authors, readers, and other objects and human actors. I find it particularly annoying when adherence to this kind of jargon comes with a complete obscuration of the academic author's application of methods drawn from the rich arsenal of techniques available today for analyzing stories, narratives, maps, tables, mathematical procedures, theories, metaphors, and other textual and visual elements. I do not doubt that Antrim did apply some methods and did spend time analyzing the sources that she used. But she has covered her work in this empty rhetoric to such a degree that they are no longer recognizable. The situation is particularly problematic when she combines such rhetorical emptiness with technical or historiographical misunderstandings and straightforward mistakes.

A few examples of such jargon or overstatement will have to suffice.

(1) Overstatements

> Although knowledge of the literatures and areas of inquiry opened up through contact with the Greek, Syriac, Hebrew, Persian, and Sanskrit heritages was also prized among *udabā*', there was simply no way to succeed in the world of *adab* without a firm command of this Arabic heritage. [15]

(2) Extreme hyperbole

> This star-studded and far-flung cast of authors suggests that the idea of home as land-based category of belonging enjoyed as broad a currency in the world outside the text as it did in the discourse of place. [16]

> What is striking about the claims to belonging and authority produced by each strategy is their overwhelming inclusivity and openness to heterogeneity. This suggests that cities in the early Islamic world were imagined more as sites of negotiation and compromise than as symbols of Islamic purity and triumphalism. [3]

(3) Meaningless sequence of misused or misunderstood terms

> The geographical transferability of this idea of home and the universalism of its gravitational pull made it a powerfully flexible vehicle for associating land and belonging and for expressing diverse and changing loyalties, both in *adab* anthologies and elsewhere in the discourse of place. [29]

(4) Limited scope and repetition of jargon

> The larger scale of the region allowed for an even greater degree of inclusivity and heterogeneity in crafting territorial categories of belonging in the discourse of place. [87]

> These strategies, like those used to evoke cities, crafted regions as categories of belonging in open-ended and universalist terms while maintaining the particularity of their political and religious associations. [88]

(5) Ridiculous rhetorical compounds

> the omnivorous sensibilities of the world of *adab* .... [88].

Furthermore, chapter 3 of Antrim's book shows that she did not understand very well the technicalities, whether at the scientific or the historiographical level, of the texts she that worked with. One major problem consists in her using older types of literature whose authors do not share at all her post-modernist leanings. Since I have known some of these authors personally for many years, I can easily state that they would be either appalled by her interpretations and rhetoric or simply dismiss them as unsound. But, even if they took a more positive stance towards her postmodernist parlance, the unquestioning use of their claims and results as simple, 'factual recycables' in a project of such a profoundly different type than theirs remains methodologically unacceptable. Yet, as little as Antrim has analyzed the vocabulary, rhetorical figures, narrative plots, and discursive properties of the medieval Arabic and Persian sources, so little did she try to understand the conceptual distances between her own perspectives and the books, chapters, and articles by Miquel, King, Berggren, Jones, Tibbetts, Lorch, Gutas, Karamustafa, Sezgin and other scholars of a generation or two older. Indeed, the only one in this list of names who might perhaps sympathize with her 'longing to craft a performative text on a discourse of place' is Karamustafa, though I doubt that he would applaud her results. Measuring the degree of compatibility between one's research approach and that of older colleagues is, however, only one task that a younger scholar who wishes to mine previous research literature for her own purposes has to undertake. She also needs to establish whether those older results, even if only taken as matters of fact, change their status and content when displaced from their proper context into a new one. As I know from my own experience with changing perspectives, methods, and concepts, many results cannot be separated from their theoretical and methodological context and basis. Combining bits and pieces from older research treatises without question in a kind of patchwork does not yield new, better, or otherwise more convincing narratives about the past. Only if they are carefully investigated as to their compatibility with other paradigms and languages does the opportunity arise for a closer approximation by the new narrative to the complexities of intellectual, material, and emotive histories of the past.

Mistakes appear in different sizes and kinds in Antrim's text. I ignore all those that might be viewed as dependent on her chosen perspective and limit the following list to a few that are wrong in an absolute sense:

(1) Greek and Hebrew literature in the general sense formulated by Antrim was never translated into Arabic. Sanskrit, Pahlavi and Syriac literature was translated but in a very small number of texts. The *udabā'* as a socio-cultural group were not generally interested in Arabic translations of Greek, Pahlavi, Sanskrit or Syriac scientific,

medical, philosophical or religious texts; indeed, some were quite hostile towards these forms of knowledge and others ridiculed men spending too much time for reading such works. Only in the 10th century did a greater interest arise among the *literati* for the scientific, medical, and philosophical teachings of their contemporaries and their authorities. How much of this interest was still at work one century later is unknown to me, since, to the best of my knowledge, no studies have so far been undertaken [15].

(2) There was never something like 'human geography' recognized as a member of the family of scholarly disciplines described in classifications of the 'sciences' often called *aqsam al-'ulum or maratib al-'ulum*. Nor did it flourish (only or particularly) from the ninth to the 11th century: repeating Miquel for backing up such a claim does not replace the study of Arabic original sources [88].

(3) It is too general and, thus, incorrect to claim 'that the earliest Muslims possessed knowledge of their position on the earth and its interrelation with the cosmos' [88]. King's *World Maps for Finding the Direction and Distance to Mecca: Innovation and Tradition in Islamic Science* [1999], given here as backup, contradicts this claim by documenting the different *qiblas* used by those earliest Muslims and their non-scientific character.

(4) We know almost nothing about 'the scholarly classes of the Ummayad (caliphate)'. Hence, evidence is needed if one wishes to claim with Antrim that 'the scholarly classes of the Umayyad and early Abbasid Caliphates developed competing notions of how to measure and divide the world' [88–89].

(5) The sentence,

> Thanks to the Abbasid Caliph al-Ma'mun's patronage of scholarship and the 'translation movement' he sponsored, texts from Indo-Persian and Hellenistic traditions of learning were translated into Arabic, revised, and in many cases improved in Baghdad in the ninth century.

is permeated by half-truths and misconceptions. The translation movement began many years before al-Ma'mun under his father, grandfather, or great-grandfather (depending on one's reading of the texts), and dividing the cultures from which scientific, medical and philosophical texts were translated into two big blocks, namely, Indo-Persian and Hellenistic, is a distortion. Moreover, offering Gutas'

Greek Thought, Arabic Culture [1998] as one of her two sources for this second claim misrepresents Gutas' analysis.

(6) That claim that the theories which Ptolemy

> advances in the *Almagest*, ..., and the *Geography*, ..., contributed greatly to efforts to determine the shape and size of the earth and to bring the contours of its surface into relationship with celestial bodies [89]

is in conflict with what we know. Scholars working in Baghdad since the second half of the eighth century on astronomical and geographical issues did not try to determine the shape of the Earth because they already 'knew' that it was spherical. Nor did they try to bring the contours of the Earth's surface into relationship with celestial bodies because they had learned from Ptolemy's *Tetrabiblos* the astrological division of the planet's surface, if this is what Antrim has in mind. The point of importance was in fact the new determination of the size of the circumference of the Earth by al-Ma'mun's astrologers and craftsmen as well as the new observations and recalculations of various astronomical parameters. This difference between elementary information available in numerous books and articles by authors quoted by Antrim such as King, Kunitzsch, or Lorch and her incompetent summary highlights the problems that Antrim's search for 'a discourse of place' encompasses.

(7) The origin of the 'seven climes' is disputed. There is evidence for it in Avestan literature, i.e., centuries before Ptolemy wrote his *Almagest*. But it is understood and used differently from its application in *Almagest* 2.12. This overlapping of two geographical and astronomical traditions and the simplicity of the seven-clime scheme explain perhaps the preference given by Muslim and Christian scholars in Islamicate societies for it to the more complicated scheme of 33 climes in the cartographic part of Ptolemy's *Geography*. Nonetheless, some Arabic authors like al-Idrisi applied like Ptolemy a greater number of parallels (10 or 11) when dividing the northern and southern hemispheres. Hence, Antrim's claim that 'Ptolemy eschew(ed) the clime system completely' is factually wrong. Equally false or superficial are the conclusions derived from this false claim [90]:

- 'the seven-clime system...is only very loosely Ptolemaic in origin',
- '(i)t is perhaps not surprising that no Arabic translation of Ptolemy's *Geography* survives, if one was ever made',

and that

- (i)t may have been that Ptolemy was a name,...more useful for conferring the authority of an ancient tradition on systems of dividing the world than it was for shaping the particulars of those systems.

There are more unacceptable claims, superficial remarks, and misrepresentations in Antrim's book than I have presented here. But I think that the lack of familiarity with, and control of, scientific and other traditions in Arabic and Persian exhibited in it have become evident. If Antrim had indeed wanted to argue against the textual evidence in extant sources that no Arabic translation of Ptolemy's *Geography* had been made in the ninth century, she should have shown that these sources were unreliable. This kind of painstaking textual research is, however, nowhere documented in her book. Otherwise, she should have flown to Istanbul and studied two manuscripts of an Arabic translation of Ptolemy's *Geography* produced for the Ottoman sultan Mehmet Fatih in the 15th century. Antrim's speculation that Ptolemy was merely a household name but not the representative of a complex set of theories, techniques, parameters, and models that were studied, excerpted, and applied in different manners indicates further her failure to understand these sources and their impact on intellectual projects in Islamic societies as well as of her limited efforts to familiarize herself with these texts, their successors, and their current historiographical interpretation.

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Hippocrates: On the Art of Medicine by Joel E. Mann

Studies in Ancient Medicine 3. Leiden/Boston: Brill 2012. Pp. xii + 279. ISBN 978–90–04–224131. Cloth €151.00

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Cet ouvrage présente une nouvelle traduction anglaise ainsi qu'un commentaire précis et fouillé du traité hippocratique *De arte*. Il est issu du travail de doctorat de Joel E. Mann, rattaché à l'Université du Texas à Austin. Le livre comprend une introduction suivie du texte grec, puis sa traduction anglaise ainsi qu'un commentaire linéaire et exhaustif. Il comprend enfin un appendice intitulé « *De Arte* and the Hellenistic Debate », une bibliographie et des index.

Mann est historien de la philosophie et il se rattache en introduction à l'école analytique anglo-saxonne pour caractériser son approche. Étant moi-même peu aguerri à cette méthode, voire incompétent, il m'est difficile de juger des résultats auxquels l'auteur parvient. Il me semble toutefois que si ce type d'analyse est particulièrement pertinent pour expliquer les raisonnements et la logique mis en œuvre par l'auteur hippocratique, elle semble un peu artificielle et compliquée quand elle n'est qu'un principe de présentation des idées propres à Mann. Une tendance liée à ce type de lecture est de déshistoriciser la production d'idées pour en faire des valeurs absolues et éternelles ou encore de décontextualiser les textes.

La première section de l'introduction rappelle des généralités sur la médecine en tant que τέχνη. La deuxième section étudie le genre épidictique du *De arte* et propose un nouveau découpage de ce traité rhétorique. La troisième aborde l'aspect philosophique du texte et ses liens avec les Présocratiques. La quatrième recense les lieux parallèles du *De arte* avec le corpus hippocratique et établit notamment de nouveaux rapprochements avec les traités *Sur les glandes* et *Lieux dans l'homme*. La dernière présente les questions de datation et d'attribution du *De arte*.

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Mann cerne les spécificités de ce traité qui a été rattaché au corpus hippocratique. Il s'agit d'un discours composé pour les profanes et prononcé devant un vaste public dans le cadre d'une polémique qui attaque la médecine en particulier et les arts en général. Cette apologie de la médecine est dirigée contre un sophiste anonyme que Mann propose d'identifier avec Protagoras [40]. Mann montre que l'organisation du traité répond à un réquisitoire du sophiste aujourd'hui perdu qui s'en prenait au τέχναι en général et à la géométrie en particulier, en se fondant sur le témoignage de Platon, d'Aristote et de Sextus Empiricus.

La question de l'attribution du traité a été vivement débattue. L'auteur était-il un sophiste ou un médecin ? Peut-il être identifié ? Contre E. Littré, L. Bourgey ou J. Jouanna qui attribuent le *De arte* à un médecin ou encore contre J. Ducatillon qui l'attribue à Hérodicos de Sélymbrie, Mann penche pour l'œuvre d'un sophiste. Plusieurs noms ont été suggérés pour l'identification de ce sophiste : Protagoras ou l'un de ses disciples (Th. Gomperz), Hippias (E. Dupréel) ou Antiphon (M. Untersteiner et H. Cherniss). Mann semble donner sa préférence à Antiphon, en exposant les affinités entre les textes de ce sophiste et le *De arte* [13–14, 46], sans toutefois se prononcer fermement.

Ce travail est fort utile par la richesse des renseignements fournis dans le commentaire et il intéressera sûrement les adeptes de l'analytique anglo-saxonne, mais il est un peu décevant en regard de la critique hippocratique.

Abū Kāmil. Algèbre et analyse Diophantienne. Édition, traduction et commentaire by Roshdi Rashed*

Scientia Graeco-Arabica 9. Berlin/Boston: Walter de Gruyter, 2012. Pp. xvi + 819. 978–3–11–029561–0. Cloth €140.19

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After al-Khwārizmī, Abū Kāmil (late ninth century) is the next Arabic author whose book on algebra is extant in its entirety. Where al-Khwārizmī's book was deliberately brief, Abū Kāmil's *Kitāb al-jabr wa'l-muqābala* (*Book of Algebra)* occupies 111 folios in the only surviving manuscript.[1] That is long enough for Abū Kāmil to show features and techniques omitted by al-Khwārizmī and to exhibit his own originality with regard to proofs, irrational numbers, and the manipulation of algebraic expressions.

Abū Kāmil's influence was deservedly almost as far-reaching as al-Khwārizmī's. The *Book of Algebra* became a major influence on such well-known figures as al-Karajī, al-Samawʾal, and Ibn al-Bannāʾ, as well as on lesser ones like ʿAlī al-Sulamī and Ibn Badr. Most of the book was translated into Latin in the 12th century and whole portions found their way into the *Liber Mahameleth* in the 12th century, into Fibonacci's *Liber abaci* and *De practica geometrie* in the 13th century, and into Jean de Murs' *Liber quadripartitum* in the 14th century. From there its influence spread through Italian abbacus texts to Luca Pacioli's *Summa de arithmetica* of 1494 and into 16th-century European algebra. We also possess several manuscripts of a Hebrew translation made before 1475, possibly in Spain.[2]

* English translations are mine unless noted otherwise. In referring to Rashed's text, '579.13', e.g., means 'page 579, line 13'.

1 Istanbul, Beyazit Library, Kara Muṣṭafā Paşa 379 (now 19046). Copied in AD 1253.

2 Martin Levey [1966] edited and translated this Hebrew version, which was once thought to have been translated by Mordekhay Finzi.

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Two other books of Abū Kāmil are extant. One is his brief *Kitāb al-Ṭayr* (*Book of Birds*), in which he uses algebra to solve problems with several independent unknowns and the other, his *Kitāb al-Misāḥa* (*Book of Mensuration*).[3]

Roshdi Rashed has brought together critical editions, translations, and commentaries of the *Book of Algebra* and the *Book of Birds*. In his introduction [1–31], he provides what information we have on Abū Kāmil's life, his works, and his influence. The commentary occupies chapters 1 through 7 [33–239] and this is followed by editions with facing French translations of both the *Book of Algebra* and the *Book of Birds* [242–761]. Rashed concludes with an extract of Abū Kāmil from al-Samawʾal's 12th-century *al-Bāhir fī ʿilm al-ḥisāb*, some notes, an Arabic-French glossary, indices, and a bibliography [763–819].

The contents of Abū Kāmil's books

The *Book of Algebra* is composed of three parts, each of which can be regarded as a treatise on its own:

(1) Pp. 242–521 The 'algebra proper' is modeled on the first half of al-Khwārizmī's book. The names of the powers are given, then the six equations are classified and their solutions are given with geometric proofs. Many sample calculations with polynomials and roots follow, often with proofs. Then comes a collection of worked-out problems with various proofs scattered throughout. By Rashed's count there are 70 problems in total.[4] Abū Kāmil is unusual in that he gives two or more solutions to many problems.

(2) Pp. 522–577 'On the Pentagon and Decagon'. Here Abū Kāmil solves 20 problems in geometry *via* algebra.

(3) Pp. 578–729 Abū Kāmil solves 43 indeterminate problems by algebra, followed by 27 assorted determinate problems, some of which are not solved by algebra. After this are some problems in numerical progressions. Rashed lumps all these problems together under the chapter 'Analyse indéterminée'. As Jacques Sesiano [1977] has shown, Abū Kāmil was evidently not familiar with Diophantus'

[3] Jacques Sesiano [2013] has recently published a critical edition with English translation of the *Kitāb al-Misāḥa*.

[4] I list 74 problems in Oaks and Alkhateeb 2005, 419–420.

> *Arithmetica* when he wrote this book, even if Qusṭā ibn Lūqā had translated the *Arithmetica* two or three decades earlier.[5]

The *Book of Birds* [732–761] consists of six problems solved by algebra with multiple independent unknowns. In the first problem, 100 birds of three species are purchased for 100 *dirhams*. Ducks are 5 *dirhams* each, 20 sparrows cost a *dirham*, and chickens are a *dirham* each. How many of each species are bought? In the solution, the number of ducks is named 'a thing' and the number of sparrows 'a dīnār', and the problem is solved with these two unknowns.

Rashed's editions and translations

We are told [ix: cf. 27, 30] that

> *[l]e lecteur trouvera ici l'*editio princeps *du livre d'algèbre d'Abū Kāmil et de son autre livre,* Sur les volatiles*, ainsi que leur traduction intégrale.*
>
> [t]he reader will find here the *editio princeps* of the *Book of Algebra* by Abū Kāmil and of his other book, *On Birds*, as well as their complete translation.

It is true that no complete edition of the *Algebra* has been published before but Rashed should have mentioned Sami Chalhoub's edition and German translation of part 1 [2004] and especially the facsimile of the entire book published by Jan Hogendijk [1986]. Historians have thus had easy access to the whole text of the *Algebra* for more than a quarter century.

In comparing several pages of Rashed's edition of the *Algebra* with the facsimile, I found only two minor errors: he does not indicate that the «min» which he added at page 475.9 is not in the manuscript and that he has corrected «sitta» ('six') to «arbaʿa» ('four') at page 515.9, though «sitta» is correct. Overall the edition is excellent.

The French translation fills a real need. The previous English translations of parts of the *Algebra* are less than adequate and Rashed's version is clear and literal. My only quibble would be about the translations of certain terms; but this is a consequence of his interpretation of algebra, which I discuss below.

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[5] Rashed does not cite this article.

The commentary

Rashed devotes over 200 pages to explain the mathematics in Abū Kāmil's *Algebra*. Throughout the commentary, he represents Abū Kāmil's calculations with modern symbols, which should give us a quick guide to Abū Kāmil's 'rhetorical' mathematics, that is, mathematics written without notation. But despite his warning that these symbols carry with them concepts alien to medieval mathematics—see, e.g.:

> *Le symbolisme autorise en effet des généralisations, des itérations, des déductions, etc., que la langue naturelle est souvent inapte à opérer. Et, qui plus est, ce modèle interprétatif élaboré à partir de l'algèbre symbolique s'avère parfois inefficace.* [ix–x]

—Rashed consistently interprets Abū Kāmil's words through them. This distorts the text in two ways. First, Rashed applies his modern symbols to both medieval algebra and medieval arithmetic, which both obscures the structure of algebraic problem solving and levels the distinction between medieval and modern algebra. Second, this symbolic algebra serves as the foundation upon which he interprets the indeterminate problems in part 3 using terms of modern algebraic geometry.

Another fundamental problem of interpretation is Rashed's view that Arabic algebra is a scientific 'theory of equations' centered on the classification, solutions, and proofs of the six canonical equations. In fact, Arabic algebra was fundamentally a numerical problem solving technique and the solutions to the six equations are among the rules necessary for its implementation. This mis-orientation together with his notational transgressions form the basis for Rashed's claim that Abū Kāmil founded indeterminate analysis.

These matters of interpretation are not easily explained in a short review, so I will spell out the causes and consequences of Rashed's misconceptions in some detail. I will begin by describing the basic structure of medieval algebraic solutions to problems.

Modern algebra and medieval mathematics

Medieval Arabic algebra was part of arithmetic. As a technique for solving numerical problems, it was practiced alongside methods such as single and double false position, working backwards, and 'analysis'. In these methods, one calculates directly with the numbers given in a problem to get the answer. What distinguishes a solution by algebra (*al-jabr wa'l-muqābala* or sometimes just *al-jabr*) is that an unknown number is *named* and an *equation* is set up and then solved.

The solution to a problem in medieval algebra can be divided into three stages:

Stage 1 An unknown number is named in terms of the powers *jidhr/shayʾ* (root/thing, akin to our x), *māl* (plural *amwāl*; a sum of money, x^2) , *kaʿb* (cube, x^3), and so on. Then operations are performed to set up an equation that is expressed in terms of these names. This is ideally a polynomial equation.

Stage 2 The equation is simplified, using *al-jabr* (restoration) and *al-muqābala* (confrontation), to one of the six types listed by al-Khwārizmī, Abū Kāmil and others:

Simple equations
- *amwāl* equal roots ($ax^2 = bx$)
- *amwāl* equal number ($ax^2 = b$)
- roots equal number ($ax = b$)

Composite equations
- *amwāl* and roots equal number ($ax^2 + bx = c$)
- *amwāl* and number equal roots ($ax^2 + b = cx$)
- roots and number equal *amwāl* ($ax + b = cx^2$)

Stage 3 The simplified equation is solved following the numerical procedure given in the beginning of the book. Some books give geometric proofs that these rules work.

These stages were followed in the solutions to problems in all books in Arabic algebra and in book chapters devoted to algebra beginning with al-Khwārizmī and Abū Kāmil.[6] Also, many books in Arabic arithmetic show solutions by different methods to the same enunciation and these stages are followed in solutions worked out 'by algebra'.[7]

6 The surviving books on algebra by Ibn Turk and Thabit ibn Qurra do not contain worked-out problems. They show only the solutions and proofs for the rules to solve simplified equations for stage 3.

7 Al-Ḥaṣṣar (late 12th century), Ibn al-Yāsamīn (d. 1204), al-Fārisī (d. *ca* 1320), and al-Kāshī (d. 1429) are some authors who show solutions by multiple methods.

As an example, here is the enunciation and first solution to Abū Kāmil's problem <7>:[8]

> [Enunciation]
>
> If [someone] said to you, 'ten', you divided it into two parts. You multiplied each part by itself and you cast away the smaller from the larger, leaving eighty.
>
> [Stage 1]
>
> Its rule is that you make the smaller part a thing [x] and the larger ten less a thing [$10 - x$]. So you multiply ten less a thing by itself to get a hundred *dirhams* and a *māl* less twenty things [$100 + x^2 - 20x$]. Then you multiply a thing by itself to get a *māl*. Subtract it from a hundred *dirhams* and a *māl* less twenty things, leaving hundred *dirhams* less twenty things equal eighty *dirhams* [$100 - 20x = 80$].
>
> [Stage 2]
>
> So restore the hundred *dirhams* by the twenty things and add it to the eighty to get twenty things and eighty *dirhams* equal a hundred *dirhams* [$20x + 80 = 100$]. Cast away eighty from a hundred, leaving twenty *dirhams* equal twenty things [$20 = 20x$].
>
> [Stage 3]
>
> So the thing is one, which is the smaller part, and the larger part is nine, which is the remainder from the ten. [335–337]

It is important to observe that the enunciation is a question in arithmetic that contains no algebraic terms. It asks for the two unnamed parts of 10 that satisfy a particular condition. Algebra—and by 'algebra' I mean the specific technique called in Arabic *al-jabr wa'l-muqābala* and not a modern, more inclusive notion of 'algebra'—only makes its appearance in the beginning of the solution. There one of the parts is named a 'thing', making the other 'ten less a thing'. The equation ($100 - 20x = 80$) is set up at the end of stage 1 *after* working through the operations and it simplifies to $20 = 20x$ in stage 2. This is one of the three simple types, so stage 3 is trivial. There is no need to follow a procedure to 'halve the roots', and so on.

In his commentary Rashed expresses the enunciation to problem <7> as the modern system of equations:

[8] Rashed numbers the problems using angle brackets.

$$\begin{cases} 10 = x + y \\ y^2 - x^2 = 80. \end{cases}$$

Just by writing this down he has already named the parts x and y and he has transformed the operations of the enunciation into equations. He is thus already about half way through a modern algebraic solution to the problem. He continues with 'Alors $100 - 20x = 80$, d'où $x = 1$' [57]. By skipping over the naming of the unknown and the subsequent operations in stage 1, he is able to bridge his own modern equations with the modern version of the one that Abū Kāmil actually sets up.

There are alternate ways to name the unknowns. Abū Kāmil in fact gives two more solutions to this problem with different namings. In the second solution, he switches the assignments:

> So if we made the larger part a thing [x] and the smaller ten less a thing [$10 - x$]...

Here he has set up the equation 'twenty things less a hundred *dirhams* equal eighty *dirhams*' ($20x - 100 = 80$) and then x is found to be 9. In the third solution, he names the parts according to the habit of the 'arithmeticians' (*ḥussāb*):

> You make one of the parts five and a thing [$5 + x$] and the other five less a thing [$5 - x$]...

This time the equation is set up as 'twenty things equal eighty *dirhams*' ($20x = 80$), so x is 4, making the parts again 1 and 9. This last way of naming the parts is not compatible with Rashed's system of equations, since the 'thing' does not correspond to his x or y. For this reason, he regards it as a change of variables: $x = 5 - u$, $y = 5 + u$ [58].

In many problems, Rashed is explicit in speaking of 'equations' in the enunciation and of a 'change of variables' that in reality is the naming of an unknown. For example, he begins his commentary to problem <67> with:

> On considère le système d'équations
>
> $$\begin{cases} u + v = 10 \\ u + 2\sqrt{u} = v - 2\sqrt{v}. \end{cases}$$
>
> Abū Kāmil pose $u = 5 - x$ et $v = 5 + x$. [104]

On the next page he writes [105]:

> *Pour résoudre le système initial sans changement de variable à la Diophante….*
>
> To solve the initial system without a change of variable as in Diophantus….

(We will see below what Diophantus has to do with this.)

I should give one more example, this time to show how Rashed misinterprets the operations in stage 1 that lead to the equation. The enunciation to problem <8> is:

> And if [someone] said to you, 'ten', you divided it into two parts. You divided each one of them by the other, so they resulted in four and a fourth. [337]

Rashed's symbolic version, which again entails naming the parts x and y and setting up equations, is helpful in seeing what is being asked:

$$\begin{cases} 10 = x + y \\ \frac{x}{y} + \frac{y}{x} = 4 + \frac{1}{4}. \text{ [58]} \end{cases}$$

In the first solution Abū Kāmil names the parts 'a thing' (x) and 'ten less a thing' ($10-x$). To make one side of the equation, he squares both x and $10-x$ and adds them to get $100 + 2x^2 - 20x$. Then, for the other side, he multiplies the two parts to get $10x-x^2$, which is then multiplied by the $4\frac{1}{4}$. The equation is then set up at the end of stage 1 as $42\frac{1}{2}x - 4\frac{1}{4}x^2 = 100 + 2x^2 - 20x$. This simplifies to $x^2 + 16 = 10x$ in stage 2 and is solved in stage 3 using the prescribed procedure.

Rashed explains the solution in this way:

> *Or* $x^2 + y^2 = xy(\frac{x}{y} + \frac{y}{x})$. *On a donc*
>
> $$x^2 + (10-x)^2 = (4 + \frac{1}{4})x(10-x),$$
>
> *d'où*
>
> $$x^2 + 16 = 10x$$
>
> *et* $x = 2$ *ou* 8 *si on utilise l'algorithme.* [58]

Rashed's version may follow the same underlying line of reasoning as Abū Kāmil's version but the two are worlds apart in their execution. Abū Kāmil separately constructs the two sides of his equation by performing operations before he finally states it. Rashed, on the other hand, connects his system of equations with Abū Kāmil's simplified equation $x^2 + 16 = 10x$ by writing an identity followed by an equation, neither of which is stated in the original.

Abū Kāmil solves this particular problem in five different ways. In the second solution, he again names the parts according to the habit of the arithmeticians. This time naming them 'five and a thing' and 'five less a thing' makes the solution easier because the simplified equation has only two terms:

$$56\frac{1}{4} = 6\frac{1}{4}x^2.$$

Following Rashed's numbering of the problems in part 1, there are 34 enunciations that ask for the two parts of 10, with 2 more in part 3. There is one other common problem type, which occurs 27 times in part 1 and 26 times in part 3. These enunciations ask for an unknown *māl*, where «māl» is here a common term meaning 'quantity' or 'sum of money'; it is not the algebraic name of the second degree unknown as Rashed presumes.[9]

One of several reasons that the algebraic reading is untenable [see Oaks and Alkhateeb 2005, Oaks 2010] is that this *māl* is named in terms of the algebraic powers in the beginning of the solution, just like the parts of 10. In part 1, Abū Kāmil names the *māl* 'a thing' (x) 19 times, which Rashed regards as 'un changement de variable délibéré, purement algébrique, $x^2 \to x$' [332]. In other solutions, it is named 'a *māl* less twenty-four *dirhams*' ($x^2 - 24$), 'two *māls*' ($2x^2$), 'a third of a *māl*' ($\frac{1}{3}x^2$), and 'half a *māl*' ($\frac{1}{2}x^2$). Most telling is the fact that he names the *māl* (quantity) a *māl* (x^2) six times. If the 'māl' in the enunciation were already the algebraic name, there would be no need to rename it as itself in the beginning of the solution. This is even more common in the chapter on indeterminate problems. There Abū Kāmil begins the solutions to 20 problems with 'So you make your *māl* a *māl*.' One of these problems is translated on page 41 below.

Throughout his commentary, Rashed gives no hint that he understands medieval algebraic problem solving. By treating the enunciation to a problem as equivalent to his equations, he misses or misunderstands the naming of

[9] This is in contrast to Rashed's edition and translation of al-Khwārizmī's *Algebra* [2007] where he translates «māl» in the enunciation of 13 problems correctly as 'bien' ('amount' in the English translation). The enunciations of the other 12 problems of this type also involve the square root (*jidhr*) of this quantity (*māl*), which causes Rashed to mistake them both for the names of the algebraic powers.

the unknown and he distorts the working out of operations that lead to the setting up of the equation in stage 1. Throughout his book, in fact, Rashed sees just about any kind of numerical equating, including arithmetical operations, as an equation. The specific and deliberate mode of stating equations in medieval algebra, which is characterized by both vocabulary and context [see Oaks 2010], becomes lost in Rashed's sea of symbols.

I will give one other example of Rashed turning arithmetic into algebra. Abū Kāmil gives the solutions to the three composite equations in the beginning of his *Algebra* and each solution is presented as a sequence of numerical operations. His procedure for the sample equation $x^2+10x = 39$, for example, unfolds this way: $10 \div 2 = 5$; $5^2 = 25$; $39 + 25 = 64$; $\sqrt{64} = 8$; $8 - 5 = 3$, so $x = 3$ and $x^2 = 9$. Rashed, instead, gives this algebraic reading in his footnote [250n8]: 'On a $(x + 5)^2 = x^2 + 25 + 10x = 39 + 25 = 64$, donc $x + 5 = 8$, d'où $x = 3$ et $x^2 = 9$'. Abū Kāmil does not square the binomial $x + 5$ or replace $x^2 + 10x$ with 39. In fact, the procedure never deals with the algebraic powers at all. Rashed gives the same kinds of invented algebraic versions for other procedures in footnotes 10, 12, 17, and 23 on the next few pages. In footnotes 9, 11, 13, and 18, however, he gives purely arithmetical and, thus, more appropriate explanations for Abū Kāmil's procedures for finding the *māl* (x^2) directly.

The invention of indeterminate analysis?

Rashed's misconceptions not only blind him to the structure of medieval algebraic solutions, they also have serious consequences for his interpretation and assessment of Abū Kāmil's chapter on indeterminate analysis. In his estimation,

> *[l]e troisième livre de l'*Algèbre *d'Abū Kāmil représente un événement mathématique majeur, dont l'importance n'a pas échappé aux successeurs. C'est dans ce livre en effet que l'on rencontre la première étude délibérément et entièrement consacrée à l'analyse indéterminée rationnelle.* [145: cf. vii]
>
> [t]he third book of Abū Kāmil's *Algebra* represents a major mathematical event, the importance of which did not escape his successors. It is in this book, indeed, that one encounters the first study deliberately and entirely devoted to rational indeterminate analysis.

Here Rashed has found a superficial reason to dismiss Diophantus, who

> *ne distingue pas plus entre problèmes déterminés et problèmes indéterminés qu'entre problèmes possibles et problèmes impossibles.*
>
> does not distinguish between determinate problems and indeterminate problems, nor between possible problems and impossible problems.

Rashed makes a claim more worthy of rebuttal farther down the page. Abū Kāmil, he tells us,

> *cherche à constituer, pourrait-on dire, une algèbre des problèmes indéterminés, c'est-à-dire à fonder un nouveau chapitre des mathématiques: l'analyse indéterminée.*
>
> seeks to establish, one might say, an algebra of indeterminate problems, which is to say to found a new chapter in mathematics: indeterminate analysis.

How can Rashed suggest that Abū Kāmil *founded* indeterminate analysis? Not only did Diophantus devote most of his *Arithmetica* to indeterminate problems but Abū Kāmil himself writes in several places that he borrowed his methods from other arithmeticians (*ḥussāb*). Rashed explains that Abū Kāmil's project is

> *d'algébriser les procédés mis en pratique par les arithméticiens, ou encore de transformer, à l'aide de l'algèbre, des procédés somme toute artisanaux en une science mathématique, et donc en un savoir apodictique.*
>
> to algebraize the procedures practiced by the arithmeticians or to transform, with the aid of algebra, the overall artisanal procedures into a mathematical science and, therefore, into apodeictic knowledge.[10]

On the contrary, Abū Kāmil makes it clear that the arithmeticians *did* practice algebra. He writes:

> We now explain many indeterminate problems that some arithmeticians call 'fluid'. I mean that one can find many solutions with a sufficient analogy (*qiyās*)[11] and by following a clear procedure. Some of them circulated among arithmeti-

[10] He is even more explicit in the preface:

> *Ainsi, en appliquant les procédés de l'algèbre aux problèmes indéterminés, Abū Kāmil conçoit, pour la première fois dans l'histoire, l'analyse indéterminée rationnelle, ou l'analyse diophantienne rationnelle comme on la nomme aujourd'hui.* [vii]

[11] Like other algebraists, Abū Kāmil begins the solutions to his problems with 'Its rule/inference (*qiyās*) is:' By 'inference' (*qiyās*), he may be referring to the way of naming the unknowns.

> cians who solved them by means of types (*al-abwāb*) without establishing their cause (i.e., proof).[12]

As Rashed himself acknowledges [146n3], the 'types' spoken about are the six types of equation listed by al-Khwārizmī and his successors. Abū Kāmil continues in the next paragraph:

> *Nous expliquons également une grande partie de ce que les arithméticiens ont défini dans leurs livres et qu'ils ont fait par types, par l'algèbre et par l'inférence...* [579.18]

> And we likewise explain a large part of what the arithmeticians describe in their books, by means of types (*al-abwāb*), by restoration (*al-jabr*),[13] and by analogy (al-qiyās)....

There are other passages, too, that tell us that the arithmeticians used algebra to solve their problems. Recall that in the third solution to problem <7> Abū Kāmil names the parts of 10 according to their practice. He begins this solution with:

> And if you wanted, you divided the ten into two parts by another division, which is how the arithmeticians usually divide the ten. This method makes it easy to distinguish the larger part from the smaller part and you avoid the problem of halving the roots in many problems.[14] This is that you make one of the parts five and a thing and the other five less a thing.[15] [337.10]

---

12 Rashed translates this as

> *Nous expliquons maintenant beaucoup de problèmes indéterminés que certains arithméticiens appellent fluides, je veux dire par cela qu'on peut déterminer de nombreuses solutions vraies par une inférence convaincante et une méthode claire; certains de ces problèmes circulent parmi les arithméticiens selon des types, sans qu'ils aient établi la cause à partir de laquelle ils procèdent.* [579.13]

13 The intended meaning of « al-jabr » is unclear. It may refer to the 'restoration' of diminished terms in stage 2 or to 'algebra'.

14 By 'halving the roots', Abū Kāmil is referring to the solutions to the composite (three-term) equations, which are more complex than the simple (two-term) equations. Recall that naming the parts in this way avoided a composite equation in problem <8>.

15 He repeats this in the next problem, at 339.18. Rashed translates the first passage incorrectly as 'Et si tu veux, tu peux partager le dix en deux parties par une autre division qui n'est pas en usage chez les arithméticiens...'.

The naming of unknowns in terms of a 'thing', of course, belongs exclusively to algebra. Also in his chapter on indeterminate problems, Abū Kāmil writes after giving an algebraic solution, 'This procedure is known to the arithmeticians' [657.15]. And after another algebraic solution, he has 'This procedure is that which is applied by the arithmeticians' [677.20]. Last, it should be noted that Abū Kāmil calls *himself* an arithmetician at the end of the book: 'Abū Kāmil Shujāʿ ibn Aslam the arithmetician (*al-ḥāsib*) said:...' [729.3]. Even Rashed acknowledges that arithmeticians might practice algebra, since he translates «ḥisāb», the root of both «al-ḥāsib» and «ḥussāb, as 'calcul et algèbre' [1].

These arithmeticians named unknowns, worked with the six equations, and followed procedures that were later copied by Abū Kāmil. Neither they nor Abū Kāmil include proofs in their works on indeterminate problems, so there is nothing to differentiate their methods. Because the arithmeticians solved indeterminate problems by algebra, Abū Kāmil did not 'algebraize the procedures practiced by the arithmeticians'. Thus, his book does not introduce a new chapter in mathematics. Nor does not 'represent a major mathematical event', at least in the sense that Rashed intends.

The 'method of Diophantus'

The discord between Rashed's interpretation and Abū Kāmil's words can only be explained by addressing Rashed's own conception of Arabic algebra. Before I turn to that, I will review just one more argument that he makes, this one linking the Arabic arithmeticians with Diophantus *via* al-Karajī (early 11th century). Rashed writes this about the arithmeticians' naming of the parts of 10 as 'five and a thing' and 'five less a thing':

> *C'est une méthode qu'al-Karajī appelle plus tard «la méthode de Diophante». Et il est vrai que ce dernier l'applique dans le livre I des* Arithmétiques *pour résoudre l'équation trinôme du second degré.* [58]
>
> This is a method that al-Karajī later called 'the method of Diophantus'. And it is true that the latter applied it in book 1 of the *Arithmetica* to solve the second-degree trinomial equation.

Let us take a look at this 'method of Diophantus'.

In the beginning of his *al-Fakhrī*, al-Karajī solves and gives proofs for the solutions to the six simplified equations. After covering the sample equation $x^2 + 10x = 39$, he writes:

> And if you wanted to find the root of the *māl* according to the method of Diophantus, you search for a number which, if added to a *māl* and ten things, has a root. It is nothing but twenty-five, which added to a *māl* and ten things has a root, which is a thing and five *dirhams*. And you knew that a *māl* and ten things are thirty-nine units,[16] so, if you removed the *māl* and ten things, and you substituted thirty-nine units, they became sixty-four units. So its root is eight and that equals a thing and five *dirhams*. So the thing equals three *dirhams*, which is the root of the *māl*.[17] [Saidan 1986, 154]

The 'method of Diophantus' here refers to finding a number to add to the $x^2 + 10x$ so that it has a root, as part of solving the simplified equation in stage 3. It is unrelated to the naming the parts of 10, 'five and a thing' and 'five less a thing', that was performed in the beginning of stage 1. The appearance of the term 'a thing and five *dirhams*' in al-Karajī, equivalent to the arithmeticians' 'five and a thing', is merely a coincidence. Likewise for the other solution by the 'method of Diophantus' recorded by al-Karajī, in which he solves $x^2 + 21 = 10x$.

The method of the arithmeticians may be unrelated to the method mentioned by al-Karajī but it is equivalent to Diophantus' naming of his parts in problems 1.27–30. Diophantus' problem 1.29 is a version of Abū Kāmil's problem <7>, in which the two parts together are 20 instead of 10 and the difference of their squares is still 80. Diophantus names the difference between the two parts as '2 ἀριθμοί' (like the Arabic 'two things' or our $2x$).[18] This makes the parts $10 + x$ and $10 - x$, much like the arithmeticians' $5 + x$ and $5 - x$. He then works the operations and sets up his equation similarly, as '40 whole numbers, which are equal to 80 units' ($40x = 80$) [Tannery 1893–1895, 1.64.7].

Rashed's statement that al-Karajī's 'method of Diophantus' is applied 'in book 1 of the *Arithmetica* to solve the second-degree trinomial equation' contradicts what he wrote earlier in his edition of al-Khwārizmī's *Algebra*. There he argued that al-Karajī 'provided an algebraic reading of the *Arith-*

16 Like many algebraists, al-Karajī uses 'dirhams' and 'units' interchangeably.

17 Curiously, this solution matches the algebraic explanation that Rashed gave for Abū Kāmil's arithmetical solution to this equation, quoted above.

18 The term « ἀριθμος », though typically meaning 'whole number', serves in this context as the name given to the first degree unknown in Diophantus' algebra and may prove to be a rational number.

metica', and that 'At no time does Diophantus complete the square; instead in his work the emphasis is on the type of substitution that he uses.' Diophantus of course does not solve any trinomial equations in book 1. He does solve them in later books but without explaining how he found the solutions. It is likely that he solved them by completing the square, as al-Karajī tells us. So one of Rashed's contradictory statements is misleading and the other is simply wrong.

Even with the similarity between the namings of the unknowns by Diophantus and the Arabic arithmeticians, I would not propose, as Rashed does on page 146, that perhaps the latter had Diophantus' text at their disposal before Qusṭā ibn Lūqā translated it into Arabic sometime in the second half of the ninth century. It is more likely that algebra circulated orally among merchants and other practitioners over the course of several centuries and that Diophantus, al-Khwārizmī, and Abū Kāmil took this technique and wrote books on it, introducing more theoretical elements at the same time.

Rashed's view of Arabic algebra

During the past half century or so, historians writing about Arabic algebra have tended to focus on what is most interesting to them from a theoretical perspective. This is the classification of the six canonical equations of degrees 1 and 2, their solutions, and their geometric proofs. Even the books by historians like Youschkevitch [1976] and Berggren [1986], which cover the problem-solving side of algebra in addition to the 'theoretical' aspects, leave one with the impression that Arabic algebra was largely about the six equations and proofs.

Rashed not only gives this same impression, he openly advocates it. Anyone who has read his previous books and articles on Arabic algebra is familiar with his view that al-Khwārizmī invented algebra as a 'theory of equations'.[19] According to him, the core of this invention is the classification of the six equations, their solutions, and proofs.

> To pass over these ideas…would reduce the book [of al-Khwārizmī] to a collection of algebraic techniques, which historians could rapidly assign to the author's predecessors. [Rashed 2009, 49: cf. 35ff in the book under review]

[19] First in Rashed 1983 and then in numerous derivative articles, most recently in Rashed 2012 and in the introduction to Rashed 2007.

By defining the core of Arabic algebra in this way, Rashed, more than other historians, marginalizes practical problem solving. But if we read what the medieval texts say, we find that algebra not only originated in problem solving, problem solving remained its main focus throughout its history. The solutions of the six equations are among the rules needed to solve problems, so the introductory chapter in al-Khwārizmī that appears to be devoted to the 'theory of equations' is really primarily a chapter covering the necessary rules for working out problems 'by algebra'.

This artisanal view of Arabic algebra might be a drastic reorientation for some readers, so I will give some of the evidence for it. To start, proofs were not restricted to 'scientific' mathematics but were a feature of practical mathematics as well. I will cite two examples.

In the tenth century, Abū'l-Wafāʾ described the methods of practical geometers in his *What is Needed by the Artisan for Geometric Construction*. Jens Høyrup [1986, 473n27] explains that their proofs were 'of a cut-and-paste character' and that, because of their requirement of 'a concrete rearrangement of parts', they rejected the proofs of geometers working in the Greek tradition. Another example is an anonymous practical text in Greek from late antiquity that shows a geometric proof to a rule for multiplying sexagesimal numbers [Tannery 1893–1895, 2.7–10].

The proofs of al-Khwārizmī are similar to the Greek and Arabic proofs just described in that they do not appeal to Euclid and they compare equal lines and areas without recourse to ratios. Later, as algebra attracted the interest of mathematicians working in the Greek tradition, we find Thabit ibn Qurra, Abū Kāmil, and others writing proofs in the manner of Euclid and citing the *Elements*.

Rashed does not mention that the name given to algebra, «al-jabr wa'l-muqābala», comes from problem solving. The two words making up the phrase refer to the steps applied in stage 2 to simplify equations. They have nothing to do with the classification of the six equations, their solutions, or their proofs.

Neither al-Khwārizmī nor Abū Kāmil mention any 'theory of equations'. In fact, «muʿādala», the Arabic word for 'equation', first appears in the second half of the ninth century in Qusṭā's translation of Diophantus' *Arithmetica*; and the first Arabic algebraist whom we know to use the word is al-Karajī in the 11th century. Algebraists before then made few references to equations as mathematical objects. When they did, they called them ‘problems’ (*masāʾil*) [Oaks 2010].

Al-Khwārizmī himself announces the practical purpose of his book in this famous passage from the introduction to his *Algebra*:

> [The caliph] al-Maʾmūn…has encouraged me to write a brief book on algebraic calculation which encompasses the fine and important parts of its calculations that people constantly require in cases of their inheritance, their legacies, their partition, their law-suits, and their trade, and in all their dealings with one another, such as the surveying of land, the digging of canals, geometry, and other various aspects and kinds are concerned.[20] [Rashed 2007, 95]

Consistent with this stated purpose is that the books on algebra by al-Khwārizmī, Abū Kāmil, al-Karajī (his *al-Fakhrī*), Ibn al-Bannāʾ, and many others devote more space to solved problems than they do the ‘theoretical’ parts. In the book under review, the Arabic text for part 1 contains 40 pages of ‘theory’ (rules and proofs) followed by 101 pages of problems. That does not include the geometry problems that make up all of part 2 or the arithmetic problems of part 3.

Further, Medieval mathematicians themselves call algebra ‘a way to find unknown numbers’.[21] And in texts from before the end of the 12th century, I have found 9 mathematicians who call it a *ṣināʿa* (art, craft, or technique), while only 2 others, both with a practical orientation, call it an *ʿilm* (science, or body of knowledge).[22]

The six equations, with their solutions and proofs, were a *part* of Arabic algebra. To hold that they were its *defining feature* is to project our modern, theoretical attitude about mathematics onto medieval texts, while ignoring all indications to the contrary. Indeed, Rashed is correct that drawing our

20 My translation, adapted from Gutas 1998, 113.

21 Including al-Fārābī (10th century), al-Karajī (early 11th century), ʿAlī al-Sulamī (11th or 12th century), and al-Khayyām (*ca*1075).

22 The following call algebra a *ṣināʿa*: Qusṭā ibn Lūqā (9th century), Abū'l-Wafāʾ, the lexicographer al-Khwārizmī (10th century), Ibn Sinā, al-Karajī, al-Bīrūnī, al-Khayyām (11th century), al-Samawʾal, and Sharaf al-Dīn al-Ṭūsī (12th century). Al-Fārābī (10th century) and ʿAlī al-Sulamī (11th or 12th century) call it an *ʿilm*. The meaning of «ʿilm» was both slippery and evolving, so one cannot say much about its meaning in this context. But «ṣināʿa» is a word that implies a practice rather than a theory.

attention away from the 'theory of equations' 'would reduce the book [of al-Khwārizmī] to a collection of algebraic techniques, which historians could rapidly assign to the author's predecessors'. These 'algebraic techniques' are those of algebraic problem solving and the predecessors, in Abū Kāmil's case, are Diophantus and the Arabic arithmeticians.

Algebraic geometry

Like the problems in Diophantus' *Arithmetica*, the indeterminate problems in Abū Kāmil's chapter are stated in arithmetical terms and are solved *via* algebra by choosing particular values for given numbers and setting up determinate equations. Rashed instead reads the enunciations themselves as modern indeterminate equations.[23] These equations in turn suggest to him a reading of the solutions in terms of algebraic geometry. To see just how far Abū Kāmil's solutions are from such a reading, consider problem <4>:

> If [someone] said to you, 'a *māl*', it has a root. If you subtracted from it six of its roots, then the outcome has a root. This problem is also indeterminate.
>
> Its rule is that you make your *māl* a *māl*. Cast away from it six of its roots, leaving a *māl* less six roots. Its root is smaller than a thing, so make it a thing less four *dirhams* or less five *dirhams* or less three and a third or less whatever number you like, as long as it is larger than one half of the six things that are diminished from the *māl*.
>
> So we make it a thing less four *dirhams* and we multiply it by itself to get a *māl* and sixteen *dirhams* less eight things equal a *māl* less six things [$x^2 + 16 - 8x = x^2 - 6x$]. Confront[24] this to find that the thing is eight and the *māl* is sixty-four. From this cast away six of its roots, which is forty-eight, leaving sixteen and its root is four. [585]

In the solution, it is required that $x^2 - 6x$ has a root. Abū Kāmil tells us to set its root equal to some $x - a$, where this a is any number greater than 3, which is half of the 6. He chooses 4, then he sets up and solves the equation $x^2 + 16 - 8x = x^2 - 6x$ to get $x = 8$. Rashed notes that this method had already been applied by Diophantus [151].

[23] For example, on p. 147 he writes of this chapter 'La première partie, qui représente l'essentiel du livre, comprend quarante-trois équations et systèmes d'équations indéterminées.'

[24] Abū Kāmil uses this word, conjugated from «al-muqābala», to mean 'simplify and solve the equation'. See Oaks and Alkhateeb 2007.

In his commentary Rashed consolidates the first six problems with this single equation: '(1) $x^2 + bx + c = y^2 \quad b, c \in Q$'. Where Abū Kāmil's enunciation asks for a single unknown, Rashed instead writes his equation with *two variables*. On this modern algebraic foundation, he then generates this geometric interpretation:

> *C'est la méthode dite «de la corde», que retrouve Abū Kāmil. L'interprétation géométrique qui justifie cette appellation est la suivante: l'équation (1) définit une conique dans le plan dont la clôture projective possède les points, à l'infini. La droite (*) passant par un de ces points coupe la conique en un autre point rationnel.* [151]

> This is the method called 'of the cord' which is found in Abū Kāmil. The geometric interpretation which justifies this name is the following: equation (1) defines a conic in the plane whose projective closure has points $(\pm 1 : 1 : 0)$ at infinity. The line [$x = t$ and $y = t + u$] passing through one of these points cuts the conic at another rational point.

One must ask how the observation that Rashed's equation defines 'a conic in the plane whose projective closure has points at infinity' helps us understand Abū Kāmil's solution!

It gets worse a few pages down. Rashed begins his explanation of another group of problems with 'The method of Abū Kāmil admits the following geometric interpretation'[25] and in the middle of it all he has this excursion into 20-century mathematics which is in no need of translation:

> *Les équations (2) définissent une surface S intersection de deux cylindres quadratiques dans l'espace des coordonnées (X, Y, Z, T). On définit une application rationnelle f de S dans C par les formules $x = \frac{X^2}{T}$, $y = \frac{XY}{T}$, $z = \frac{XZ}{T}$ définies pour $T \neq 0$. On voit que, pour tout $X \neq 0$ et tout point (x, y, z) de C distinct de $(0, 0, 0)$, $f(X, \frac{Xy}{x}, \frac{Xz}{x}, \frac{X^2}{x}) = (x, y, z)$, donc f est surjective en dehors de l'origine; de plus, si $b = a\alpha^2$, $f(0, Y, \pm\alpha Y, \frac{Y^2}{\alpha}) = (0, 0, 0)$, pour tout $Y \neq 0$, mais l'origine n'est pas dans l'image de f si $\frac{b}{a}$ n'est pas un carré.* [157]

Rashed defends these modern geometric interpretations in the preface. After writing that a symbolic rendering of Abū Kāmil's calculations 's'avère parfois inefficace' ('is sometimes ineffective'), he writes:

> *Le chaptire sur l'analyse indéterminée rationnelle, par exemple, sera mieux éclairé et expliqué par un modèle conçu à partir de la géométrie algébrique,*

[25] 'La méthode d'Abū Kāmil admet l'interprétation géométrique suivante'. [157]

> *qui permettra d'identifier les algorithmes appliqués et de comprendre en profondeur le sens des conditions auxquelles le mathématicien a soumis les solutions.* [x]
>
> The chapter on rational indeterminate analysis, for example, will be better clarified and explained by a model developed from algebraic geometry which allows us to identify the algorithms that were applied and to understand deeply the meaning of the conditions under which the mathematician worked out the solutions.

Then later on the same page he writes

> *...comment, avec des modèles empruntés à d'autres mathématiques, inventés dans d'autres contextes inconnus de l'auteur, restituer les significations que ce dernier a déposées dans son texte?* [x]
>
> ...how, with models borrowed from other mathematics, invented in other contexts unknown to the author, can one restore the meanings that the latter placed in his text?

He answers that among other things, it is with

> *...des modèles mathématiques construits à partir des disciplines que ce texte a contribué à fonder et, donc, appartenant à des mathématiques postérieures à celui-ci, modèles aptes à révéler la* mathesis *de l'auteur. Dans le cas qui nous occupe ici, ces modèles sont l'algèbre et la géométrie algébrique. Mais le recours à ces modèles n'est qu'instrumental....* [x]
>
> the mathematical models constructed from disciplines that this text has helped to found, and thus which belong to later mathematics, models capable of revealing the *mathesis* of the author. In the case that concerns us here, these models are algebra and algebraic geometry. But the use of these models is only instrumental....

Who can take seriously the idea that, because Arabic algebra lies as a historical source for modern algebraic geometry, reading Abū Kāmil in those terms can reveal 'the *mathesis* of the author'? But we are used to this from Rashed. He has exhibited a string of publications in which he gives a modern reading of premodern mathematics,[26] always careful in a preface to give a brief warning that the modern models are anachronistic. Yet, in practice, he treats them as if they are equivalent to the original.

[26] For two recent examples of this kind of modern reading of premodern mathematics, this time in the case of Apollonius, see Unguru 2010 and Montelle 2011. Rashed has also done this with Diophantus, Sharaf al-Dīn al-Ṭūsī, and al-Khwārizmī.

The idea of explaining the problems *via* algebraic geometry did not originate with Rashed. Isabella Bashmakova proposed this reading for the problems in Diophantus' *Arithmetica* in 1966, and her book of 1972 in Russian on Diophantus was translated into German in 1974. Rashed, who does not cite Bashmakova, made his first statement of this interpretation in his own edition of the Arabic translation of Diophantus in 1984 and reiterates it now for Abū Kāmil.

Abū Kāmil's Proofs

This review is already too long, so I will be brief here. Rashed discusses Abū Kāmil's proofs in chapter 7, 'La démonstration aux commencements de l'algèbre' [221–239]. He maintains that al-Khwārizmī and his successors

> *adhéraient aux normes de la démonstration héritées de la tradition euclidienne*. [222: cf. vi]
>
> adhered to the norms of proof inherited from the Euclidean tradition.[27]

But the only trace of anything Euclidean in al-Khwārizmī's proofs is the presence of letters to label vertices in the diagrams, as was first noted by Høyrup [1986, 475]. As mentioned above, al-Khwārizmī's proofs unfold in an intuitive manner uncharacteristic of Euclid. And while Abū Kāmil was one of the first algebraists to write Euclidean-style proofs and proofs that cite Euclid—they are not the same thing!—these proofs betray an uneasy tension between the practical arithmetical foundation of algebra on the one hand and Euclid's geometry and number theory on the other [Oaks 2011]. None of this is mentioned by Rashed.

The presumption of a link between Euclid and al-Khwārizmī together with the conceptual errors that affect his analysis of problems form the foundation for Rashed's analysis of Abū Kāmil's proofs. The result is a thoroughly distorted narrative that I will not attempt to dissect.

Assessment of Abū Kāmil

Rashed stresses the distinction between what he considers to be the apodeictic, scientific algebra created by al-Khwārizmī and extended by Abū Kāmil on the one hand, and the empirical and artisanal practice of Diophantus and the Arabic arithmeticians on the other [vii–viii, 146–147]. In reality, al-Khwārizmī

[27] Rashed [2007, 31 ff] makes his case for Euclid's influence on al-Khwārizmī.

and Abū Kāmil both took steps to introduce Greek elements to an algebra of practitioners. Consequently, Rashed's assessment of Abū Kāmil's contributions will be clouded at best. So, instead of reviewing what he says, I will list several of the innovations in Abū Kāmil's books that are not found in earlier works. Some of these were Abū Kāmil's own ideas while others were already in practice before him. I thus break them into two lists.

INNOVATIONS THAT WERE LIKELY ALREADY A PART OF ALGEBRA BEFORE ABŪ KĀMIL

(1) The use of irrational numbers in algebra dates back at least to al-Khwārizmī, if we are to judge by his rules for operating on roots. But Abū Kāmil works with them in intricate ways that are rare even for later authors.

(2) Where al-Khwārizmī worked with only the first two powers of the unknown, Abū Kāmil works with powers up to the eighth degree in the solutions to his problems. Powers up to the sixth had already appeared a couple decades earlier in Qusṭā's translation of Diophantus but Abū Kāmil most likely had not read that book. Because Abū Kāmil works with the higher powers only in his problems, they might have been a part of the native Arabic tradition of algebra before his time.

(3) In Abū Kāmil's problems <39> to <43> in part 3 and in his *Book of Birds*, he gives us the earliest extant use of independent unknowns in Arabic algebra.

INNOVATIONS THAT APPEAR TO BE ABŪ KĀMIL'S OWN

(1) In solving problems, the required unknown is sometimes the *māl* and not the 'thing', so Abū Kāmil gives rules and proofs for finding the *māl* directly for the three composite equations.

(2) Abū Kāmil is clearly the author of most of the 50 proofs in part 1 of the *Algebra*. He gives two main kinds of proof: one that uses a geometric diagram and cites propositions from the *Elements* and another in the style of Euclid's books on number theory. One innovation, not taken up by later algebraists, is his use of algebra to prove propositions in arithmetic.[28]

[28] These are described in Oaks 2011. I missed one proof using algebra: see 507.13.

(3) Abū Kāmil goes beyond other algebraists both before and after him by making clever assignments that simplify his algebraic solutions. The arithmeticians had already named the parts of 10 'five and a thing' and 'five less a thing' but Abū Kāmil took this notion further with even more creative namings.
(4) In the solutions to some problems in part 1, Abū Kāmil performs clever manipulations of operations on algebraic expressions in the process of setting up polynomial equations [see Oaks 2009, 198–202].
(5) Jan Hogendijk describes part 2 of the book, which is on the pentagon and decagon as follows:

> Abū Kāmil shows that his algebraic methods can be used to find easy solutions to geometric problems that were difficult or even insoluble for his predecessors. [Hogendijk 1985, first page of introduction]

Typographical mistakes

The algebraic notation in the commentary shows many errors. I did not systematically check all the formulae but the following mistakes surfaced on a quick reading. The last equation on page 55 should have a '$(x - \sqrt{y})$' just before the second equal sign. The last formula for problem <39> on page 81 should be '$y = \sqrt{6} + \sqrt{26}$'. The '$\sqrt{2x}$' in the penultimate equation on page 83 should be '$\sqrt{2}x$'. At the bottom of page 87 and at the top of page 88 the '$\sqrt{12}$' should be '$\sqrt{1/2}$' On page 90 the last equation should be '$x = -1/2 - \sqrt{1/8} + \sqrt{3/8 + \sqrt{20}}$'. The '$-\sqrt{6}$' in the penultimate equation on page 92 should be removed. The last equation on page 235 should end with '$a/x + a/y + 2 = b + 2$'.

Conclusion

Rashed has always worked apart from the larger community of historians of Arabic mathematics and in this book he continues to ignore current scholarship. He again repeats his outdated idea that algebra was created as a science by al-Khwārizmī, using the same turns of phrase that we have been reading for 30 years. Add to this his inability to see the differences between medieval and modern mathematics and it comes as no surprise that his commentary is full of misrepresentations and misinterpretations, especially in parts 1 and 3. The commentary to other parts are not as misleading because there is no possibility of confusing the enunciations of those problems with equations. In contrast to the commentary, the editions and translations of Abū Kāmil's books are very good.

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Morbid Curiosities: Medical Museums in Nineteenth-Century Britain by Samuel J. M. M. Alberti

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Rather than assembling a straightforward factual survey of British medical museums, Sam Alberti presents in this wholly admirable volume a notably well-documented account of the philosophical as well as the institutional progress of anatomical (often pathological) collections in his chosen period, with numerous backward glances to its origins in the 1700s and with thoughtful comments on its continuing resonances.

Following a scene-setting introduction, Alberti divides his text into five thematic chapters dealing respectively with situating, collecting, preserving, displaying, and viewing pathology, before concluding with a summary that extends his study to the present day, pondering the fate of the medical collections investigated (many of which have indeed vanished from the museum landscape) and touching on current debates surrounding the display and ethical treatment of human tissues. As demanded by such a comprehensive treatment, the author has been led well beyond the aspects of professional practice that articulate his volume to consider the influences wrought on his subjects by changing social attitudes, by the emergence of new disciplinary practices in, for example, the fields of ethnography and archaeology, and by the progress of museums of a more conventional stamp.

The ambivalent and fluid status of the anatomical specimen forms both a starting-point for Alberti's inquiry and a moving index of evolving public attitudes towards collections of this kind. The journey of any disembodied organ from a fragment of an identifiable human individual to anonymized and generalized type-specimen—'from him or her to *it*'—not infrequently was followed by a process of re-identification with the surgeon who had been responsible for its excision and preparation, since in the museum it was

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the preparator's name that was more likely to be attached to the specimen, overlaying and obliterating those of donor: specimens became, in Alberti's words, 'biographical objects, but of the practitioner rather than the patient'. This process, in which 'new layers of meaning were wrapped around the materiality of the body', can already be traced in medical cabinets from the turn of the 18th century and would continue into the 1800s when there was a noteworthy tendency for even the most elite practitioners to maintain personal responsibility for 'putting up' specimens resulting from their operations (despite the inherent unpleasantness of the process and the dangers from infections and the noxious effects of chemical preservatives). As Alberti comments, this was at a time when naturalists working on zoological specimens, for example, had long-since delegated responsibility for the preparation of specimens and/or exhibits to technicians (whose involvement, of course, would find no such permanent memorial). Once it had entered a collection of particular repute, that fact too would attach to the specimen in much the same way that works of art gained added fame—a form of provenance, even—by association with a particular collector. Already the viewer was confronted by something of a dilemma in deciding what it was that was being displayed—part of a human body, a manifestation of a disease or trauma, a witness to surgical skill, a virtuoso creation on the part of the preparator, or a component of a renowned collection.

Complete bodies were rarities in the medical museum, where they would have served little purpose. As likely as not, the 'normal' form would be displayed in its ideal form, not in flesh and bone but in wax—anatomical figures still forming a prestigious feature of many museums up to the 1820s. The products of the best practitioners (perhaps especially those in Florence) provided many a collection with a starting-point of physical perfection against which the ranks of imperfect specimens that followed could be measured. This preoccupation with deviation from the physical norm (rather than celebration of the ideal form) represents an important discriminating factor for medical museums, for it was axiomatic that the aspiring physician could understand normality only by studying deviation: 'normality was simply the starting point for deviance.' As Alberti observes, while bodies or body-parts exhibiting 'normal' conditions might each be represented by a single healthy specimen, examples of deviance due to disease, deformity, or trauma could be almost limitless in number and, hence, came to be displayed in disproportionately large numbers in order to demonstrate the range of conditions that might confront the physician. Even when the fashion for incorporating iconic specimens receded, wax continued to feature as an adjunct to preparations of, for example, the circulatory and the nervous systems, with waxes of different colors being injected into the specimen in order to make its component elements more legible and on occasion even lending them 'splendor and consequence' beyond their natural condition.

In the 1700s, major collections had been formed by individual practitioners, among whom William and John Hunter were the most outstandingly successful. As the 19th century progressed, practice changed: with the importation from Paris of regular autopsy, hospital post-mortem rooms increasingly took on the character of continuous production lines and the supply of body parts became almost routine. It became increasingly common for specimens to be preserved in museums associated with these institutions rather than in the private collection of the surgeon, who hitherto had relied on his professional influence to access specimens. Increasingly, private collections began to find their way on to the market—that of Joshua Brookes, sold over 24 days in 1828, was one of the largest with some 6,000 preparations—to the extent that collectors became sated: when George Langstaff sold his museum in 1842, it fetched one tenth of the expected price, bringing the comment that 'The bottles would have sold for more if they had had neither spirit nor preparations in them.' With a glutted market, other collections survived intact only by migrating into corporate ownership: John Barclay managed to stave off the dispersal of his collection (every collector's nightmare) by depositing it in 1828 with the Royal College of Surgeons in Edinburgh.

The medical museum was constantly reminded of its close relatives of less salubrious character—the many 18th-century cabinets that had included shelves-full of deformed fetuses, or the keratinous 'horns' that grew on the skin of afflicted individuals (many of whom eked a living from being exhibited in freak-shows), or the public displays of anatomical waxes of a prurient nature, in which the exhibits very often had been displayed in medical museums before falling on hard times and serving merely to titillate. Other areas of awkwardness in public perceptions of the medical museum included not least of all an awareness that it was generally the bodies of the poor that fed the appetites of the anatomists and which populated their museums, harvested from the mortuaries of the poor house or wrenched from an early grave by the body-snatchers who acted as middle-men. At times, these considerations turned incendiary: a hostile crowd in Sheffield is said to have demolished the entire anatomy school, while another in Cambridge stormed the department to liberate the body of a pauper about to be anatomized before venting its outrage on the museum's Florentine waxes.

Preoccupations among medical men with craniology and phrenology led to medical museums' becoming sites in which racial typologies were elaborated—often along lines that proved quite unsustainable: pathological museums became in the later 19th century, Alberti tells us, 'material encyclopedias of difference' in which non-European specimens were classified as 'deviations from the norm'. Sexual as well as racial difference was explored in the medical museum by a profession still exclusively composed of males for whom, it is suggested, 'The museum was a key site in the construction of the nature of woman.' Added to the hazards surrounding this little-understood territory was the fact that comparative collections routinely featured animal preparations in conjunction with humans—'a veritable stampede of quadrupeds' in the case of the Royal College of Surgeons in London—an association that gave cause for further unease among a populace already struggling to grapple with the implications of evolutionary theory.

The degree to which such museums were indeed sites of wide public interest is itself a topic for consideration. The principal user of the medical museum was undoubtedly anticipated to be the student of medicine and the primary character of such institutions was invariably didactic. The preparations on display had become standard teaching aids with the development of pathological anatomy in the middle of the 18th century and they would remain so until the inter-war years of the 20th century. Museum displays constituted an essential factor in medical education, offering a complementary alternative to the experience gained in the laboratory and the clinic.

Private owners of such museums naturally took a broader view that would accommodate their fee-paying public and which would offer the visitor the opportunity to 'know thyself', a process that Alberti equates with the democratization of medical knowledge. Particular themes inevitably laid claim to broader public attention—the mysteries of the reproductive system constituting a perennial favorite—while particular hobby-horses might be exercised by certain owners, as in the campaign against the deleterious effects of tightly-laced corsets waged by J. W. Reimers in his Gallery of All Nations Anatomical Museum. For a time, the success of these private displays prompted a widening of access more generally, as when the museum of the Royal College of Surgeons in Edinburgh opened its doors in 1832 and found itself attracting some 50,000 visitors of all classes and of both sexes, only a quarter of whom had any connection with the medical profession. These glimmerings of liberalization proved short-lived, however, for in the later 19th century the great spread of accessible civic museums, following the passing of the Public Libraries and Museums Act, which received royal assent in 1850, was matched by a corresponding decline in the accessibility of medical museums as the physicians sought to reassert their professional status: many museums within institutions closed their doors to the general public at this time, while those in private ownership went out of business in droves.

While the medical museum is by no means extinct, its fall from popularity has been striking: there were, Alberti observes, over 100 of them in Britain in the first half of the 19th century, whereas today there remain only a handful. Their demise he attributes not to the vagaries of public taste but primarily to changes in laboratory practice, since, as pathology became increasingly the province of chemistry and microscopy, the preserved specimen in a jar-full of spirits had less and less to contribute. The advent of the National Health Service meanwhile, with priorities more narrowly focused on the delivery of health care, resulted in many museums being starved into extinction from lack of funds. Ownership of the dead was also increasingly asserted by family members and antipathy to the unregulated harvesting of organs increased just as, contrariwise, concerted efforts were being made to encourage the living to routine organ donation. A further inverse process resulted, Alberti observes, in public approbation of Gunther von Haagens's *Body Worlds* show, featuring whole human corpses preserved and reanimated in dramatic poses through his 'plastination' process, at just the time when medical museums were being closed as outmoded. Certainly the most spectacular survivor in Britain is the Hunterian Museum of the Royal College of Surgeons in London, for although its exhibits for years were treated almost with embarrassment and were shut away from public view, in 2005 it re-emerged butterfly-like from its cocoon with spectacular new presentations of the historic specimens re-contextualized for a 21st century audience. Today it remains one of the undisputed treasures of the museum world. How fortunate that it has as its director Sam Alberti, who, with this succinct but beautifully written and deeply insightful volume, has established himself among the best-informed and most eloquent spokesmen that the medical museum has been privileged to enjoy.

Aristotle: His Life and School by Carlo Natali, edited by D. S. Hutchinson

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Aristotle betrayed his home town of Stagira to the Macedonians and was later King Philip's informer. This was discovered in intercepted letters from Aristotle—according to Demochares (died *ca* 275), the nephew of Demosthenes. We learn about this in the seventh book of Aristocles' *On Philosophy* (second century AD), which survives only in the form of a lengthy excerpt in Eusebius' *Preparation for the Gospel* (fourth century AD).

The problem should be clear. Any attempt at a biography of Aristotle or a history of his school must not simply gather together any passages about Aristotle and the Peripatos and put them together in roughly chronological order. One must first of all scrupulously assess the sources of these *testimonia* and, to the extent possible, determine their reliability. This is extremely difficult and there is no guarantee that, when the dust settles, such scholarly rigor will have yielded a relatively full and accurate account of the life of Aristotle and the nature of his school.

The best attempt at this—containing texts, translations, and commentary—is the still indispensable (however dated) Düring 1957. But an excellent and much more readable presentation of this material, both accessible to a wide audience and useful to scholars, is Carlo Natali's *BIOS THEORETIKOS. La vita di Aristotele e l'organizzazione della sua scuola* [1991], now available in an updated version translated into English by D. Hutchinson.[1]

[1] As is clear from his preface [vii–xix], Hutchinson was the prime mover in seeing this English translation through to publication. Moreover, he translated all of the ancient texts from scratch (in consultation with Natali), taking into account the most recent critical editions.

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Putting aside front and end matter (Hutchinson's preface, Natali's original introduction and a new postscript as well as endnotes and indices),[2] the book has, as I see it, three parts: the life of Aristotle (chapter 1), the school of Aristotle (chh. 2–3), and modern scholarship on the life of Aristotle (ch. 4).

The first chapter, as one might expect, is the most important (and interesting) part of the book. But it has a misleading title: 'The Biography of Aristotle' (with the subtitle 'Facts, Hypotheses, Conjectures'). I say this because this chapter is not really a biography. One might be tempted to call it a prolegomenon to any future biography of Aristotle; but, aside from being pretentious, that would suggest that an actual biography of Aristotle is possible. Given the existing evidence, however, it is not. And I suspect Natali would agree with this assessment, for he opens this chapter with:

> On the biography of Aristotle we have few certain facts, and there has been much conjecture. We lack information on the most important issues, whereas there is much information about matters that are ultimately of marginal significance. [5]

Instead, what Natali gives us is an excellent presentation of the ancient evidence concerning the life of Aristotle that follows the chronology of Aristotle's life, as far as that can be established, with his own running commentary and evaluation of sources. (Translations of sources are presented in boldface.) The chapter is divided into 10 parts:

1. Many Facts, Not All of Equal Interest
2. Stagira
3. A Family of Notables
4. A Provincial Pupil
5. A Sudden Interruption
6. At the Court of Princes and Kings
   6.1. Atarneus
   6.2. Macedonia
7. The Adventure of Callisthenes
8. Athens Revisited
9. Trial and Flight
10. From Traditional Customs, a New Model.

[2] This volume includes a bibliographical index [196–210], i.e., a bibliography, with each item followed by the relevant page numbers from the body of the book. I like this feature.

The reader is straightaway confronted with a wide variety of sources (some of which may well be unfamiliar) that have been used in the attempt to construct a life of Aristotle. For example, in the second section, on Aristotle's early life, the passages quoted come from the following sources:

- Diogenes Laertius, *Lives of Eminent Philosophers*,
- the *Vita Aristotelis Marciana*, an anonymous *Life of Aristotle* preserved in a single Greek manuscript in the Biblioteca Marciana in Venice which is thought to derive from a biography written by someone named Ptolemy, whom the Arabs called *al-Gharīb* (the Unknown),
- Aristocles' *On Philosophy*, excerpted in Eusebius (the passage with which I began this review),
- Aristotle's will, which is included in Diogenes Laertius,
- Diogenes Laertius again, and
- Theophrastus' *On the Causes of Plants*.

Again, in the fifth section which addresses the events of 348–347 in Macedonia[3] and the death of Plato in 347 as well as Aristotle's subsequent (or consequent?) departure from Athens, there are three sources (the relevant quotations from which take up half a page):

- Philochorus (from a papyrus fragment of Philodemus' *Index of Academic Philosophers*),
- Diogenes Laertius, and
- Dionysus of Halicarnassus, *First Letter to Ammaeus*.

Natali points out that how these events are connected, that is, 'whether Aristotle's departure from Athens was caused by events within the school or by external political events' [31], is a matter of debate among modern scholars (since Zeller) for which there is little help from these sources. He presents a brief and useful summary of the debate but reserves judgment:

> It is not possible to establish anything on this point, because neither of these two hypotheses has yet found any unimpeachable arguments in the texts. [32]

Although Natali does include his assessment of the sources and the debates about them as he proceeds, I nevertheless recommend that readers begin at

[3] 'In the year 348 BC, Philip of Macedon conquered Olynthus and in 347 the anti-Macedonian party of Demosthenes took power in Athens' [31].

the end, so to speak, with the first section of chapter 4: 'Sources of Aristotle's Biography'. For those who work in ancient philosophy and science but do not specialize in the ancient biographical tradition, this is an extremely useful resource. And reading it before the 'biography' of Aristotle (in chapter 1) and the account of his school (in chapters 2–3) should allay some of the worries (or satisfy the curiosity) that a reader might have in encountering such sources as Diogenes Laertius, Aristotle's will, the *Vita Marciana*, Dionysus of Halicarnassus, and so on.

This fifth section of chapter 1 is further divided into four subsections:[4]

1. Texts of Aristotle
    - 1.A. Fragments of the Lost Works and Texts of the Surviving Works
    - 1.B. Aristotle's Last Will and Testament
    - 1.C. The Poems of Aristotle
    - 1.D. The Letters of Aristotle
2. Official Documents[5]
3. Ancient Biographies of Aristotle
4. The *Testimonia* of Ancient Authors.

For each section and subsection, Natali provides the reader with a concise account of the surviving evidence, the main scholarship on it, and his own assessment. For instance, regarding Aristotle's own works, he says:

> From these texts it is not possible to gather much biographical material. Aristotle, it would appear, adhered to the Ionic scientific tradition of saying little about himself in his works.[6] [120].

Concerning the (supposed) letters of Aristotle [Rose 1886, frr. 651–670] and, most significantly, those to Alexander, some scholars defend their authen-

[4] In its opening paragraph, Natali acknowledges his debt to Düring:

> A large part of this section derives from Düring (1957); I have updated the information provided by this magisterial work on the basis of later studies up to 1990, and I have revised its interpretations on a few marginal points. [120]

He discusses more recent scholarly developments in his new postscript.

[5] That is, three inscriptions, one each at Delphi, Ephesus, and Athens.

[6] Would that Aristotle were more like Galen, whose remarks about himself could be used to construct a biography. In fact, see Mattern 2013.

ticity and others reject it, whereas Natali comes across as agnostic.[7] In the case of one of the most important sources, Diogenes Laertius, Natali agrees with Moraux [1986] that in this work 'the best is right next to the worst' [127]. Aristotle's will would count as the best: 'Today', Natali claims, 'there are no longer any doubts concerning [its] authenticity' [121]; the worst is comprised of 'the fanciful details of which the work is full' [127]. Concerning the bulk of material that fits the description '*testimonia* of ancient authors', Natali prudently advises that 'Even in the case of very ancient reports…it is necessary to weigh their reliability carefully' [130]; and he warns that 'In the Hellenistic period, numerous legends were fabricated about Aristotle' [133]. Examples of how Natali himself follows this advice and heeds this warning are found throughout his account of Aristotle's life and school.

I turn now to Aristotle's school, to which Natali devotes two chapters. As I am somewhat critical here, it is worthwhile to start by presenting the contents of these chapters:

> Chapter 2. Institutional Aspects of the School of Aristotle
>
> 2.1 The Three Conditions of the Theoretical Life in Aristotle
> 2.2 The Organization of *Theôria*: The Nature and Organization of the Philosophical Schools
> 2.3 The Organization of *Theôria*: Philosophical Schools and Permanent Institutions
> 2.4 Subsequent Events
>
> Chapter 3: Internal Organization of the School of Aristotle
>
> 3.1 The Collection of Books
> 3.2 Methods of Gathering and Interpreting Information
> 3.3 Teaching Supports and Instruments of Research
> 3.4 Teaching while Strolling

[7] A few pages later, however, in the section on the *testimonia* of ancient authors, he writes:

> In my opinion, the collections of letters between Aristotle and Alexander, as well as the collections of letters between Philip of Macedon and Aristotle, are to be reckoned among these [*scil.* post-Hellenistic] literary fictions. [134]

It was unclear (to me, at least) whether these were among the letters discussed earlier in chapter 4.

I was a bit baffled both by the division of this subject matter over two chapters and by their organization. It is in fact one subject and these chapters together consist of fewer pages than the first chapter, so it would have made sense to combine them. Further, some of the material spread over these chapters naturally goes together (e.g., the collection of books and the organization of the school). Finally, I see no reason not to present it in roughly chronological order; but as it is, 2.3–4 deal mostly with Theophrastus (and after), whereas the vast majority of texts quoted in 3.1–3 come from the works of Aristotle. These are relatively minor objections, however, and they do not detract much from the value of these chapters. I found especially useful Natali's account of Theophrastus' will and what it can tell us about Aristotle's school [86–90], his account of the much-discussed story from Strabo and Plutarch about the fate of Aristotle's library after the death of Theophrastus [102–104],[8] and the presentation of passages in the Corpus Aristotelicum including or referring to tables, lists, diagrams, and so forth [113–117].

Natali avoids making imprudent connections between Aristotle's life and philosophical convictions. Certainly, there is nothing like the sort of thing that one often encounters, e.g., that Aristotle was more empirical in his approach to philosophy because his father was a physician.[9] But it is in chapters 2–3 where one sees most clearly some of Natali's own views about Aristotle's *philosophy*. Most notable is section 2.1, in which he briefly presents his interpretation of Aristotle's claim that the best life is one of contemplation. This section [72–77] glides rapidly (however intelligently) over some highly controversial texts in *Nicomachean Ethics* 10 (which some scholars have claimed contradict, or appear to contradict, the rest of that work). Natali holds that these texts are crucial for understanding Aristotle's life and especially

8 In his postscript [148–150], Natali briefly discusses two more recent works on this topic: Barnes 1997 and Primavesi 2007.

9 It is surprising to learn how little evidence there is about Aristotle's father, Nicomachus. Natali writes,'All we know about Aristotle's father is his name,' though there is some evidence that he was a physician [8–9]. Epicurus, according to Diogenes Laertius, reported a different kind of connection between Aristotle and medicine: 'after devouring his father's fortune [Aristotle] took to soldiering and selling drugs' [9].

the organization of the school (recall the Italian title: 'BIOS THEORETIKOS'). I'm not so sure.[10]

More objectionable, in my view, is Natali's attempt to bring Plato and Aristotle closer together than I think they are, an attempt which stems at least in part from his interpretation of Aristotle's conception of the βίος θεωρητικός. This attempt is especially objectionable in connection with Aristotle's biology.[11] In fact, I regard his treatment of Aristotle's biological works as the book's one fundamental flaw.

In section 3.2, Natali writes:

> Plato also admitted the importance of investigating the presence of rationality in the world of becoming (*Timaeus* 29a–c), and from this point of view his position is not very far from the one expressed by Aristotle in *Parts of Animals* I.5. [105]

Without denying that there are connections between the two works, I think that to anyone who goes on to read (and consider the details of) *Parts of Animals* 2–4 they are in the end worlds apart.[12] A couple of pages later, Natali writes that 'Theophrastus...is considered much more of an actual

[10] I do not deny that knowing something about Aristotle's ethical philosophy contributes to what we might conclude about the sort of person he was and the life he led. My point is that you could say that about any number of passages in his literary corpus: e.g., *Parts of Animals* 1.5 on the importance of the study of biology or the passages in *Poetics* 25 and *Metaphysics* Λ.8 which make fairly certain that Aristotle regarded the Olympian gods as mythological.

[11] I do not know enough about ancient astronomy to say how close together or far apart Plato and Aristotle are on that subject, though I assume (whatever Aristotle's actual practice) that he objected to the Platonic conception of astronomy as unconcerned with "visible things" and that those studying astronomy "should leave the things in the heavens alone" [*Resp.* 528e–530c].

[12] David Balme [see Gotthelf and Lennox 1987, ch. 1] writes:

> *PA* II–IV recalls Plato's *Timaeus*, both in the dual causation by the good and the necessary, and in its view of the *scala naturae* with its associated value judgments. [17]

But in another essay in the same volume [ch. 10], he describes how differently these characteristics in fact operate in the two works [276–279]. See also Lennox 2001, chh. 6 and 13.

scientist than Aristotle'. 'By whom?', one wonders.[13] Moreover, as evidence for this astonishing remark, Natali quotes the opening of the Peripatetic *On Weather Signs*, which reports that 'we have recorded signs of rains, winds, storms...'—presumably from the author's (or authors') own observations—and taken 'some from others who are not untrustworthy' [107].[14] That one could regard this compilation as more scientific than, say, the *History of Animals* or *Parts of Animals*, is mind boggling, especially for those who know the scholarship on Aristotle's biology.[15] And that's the problem: Natali, who has done brilliant work on other aspects of Aristotle's writings (and especially his ethics), does not seem to know the biology (or at any rate much of the excellent scholarship on it that has appeared over the past few decades). For instance, earlier in the book, he writes that in his *History of Animals* 'Aristotle worked primarily from written sources, including Homer,[16] the poets, and Xenophon, and not from personal observation' [41–42]. No scholar of Aristotle's biology (whom I am aware of) would agree with this claim today.[17]

The final section of the body of the book [ch. 4.2] is entitled 'Images of Aristotle from the Nineteenth Century to the Present' [135–44]. It contains

[13] For an excellent comparison between Aristotle's *History of Animals* and Theophrastus' *History of Plants*, see Gotthelf 2012, ch. 14. He does not conclude that Theophrastus was a better botanist than Aristotle was a zoologist.

[14] In a note to this passage, Natali states that the manuscripts attribute the work to Aristotle but that no modern scholar accepts this attribution, adding that it cannot be attributed to Theophrastus either, though 'its content is Theophrastean' [175n7]. In fact, of the two most authoritative manuscripts, the oldest (Marcianus IV 58) names no author, while the other (Vaticanus gr. 2231) names Aristotle. The 13th-century Latin translation by Bartholomew of Messina, which comes from an independent tradition and is important for establishing the text, also attributes the work to Aristotle. It is ascribed to Theophrastus only in Vaticanus Reg. gr. 123, a 16th-century manuscript copy of the Aldine edition (1497), which itself names no author.

[15] None of this implies a lack of respect for or interest in Theophrastus on my part. On the contrary, he is a fascinating figure and currently occupies a great deal of my time, as I am preparing a critical edition (with translation and commentary) of his *On Winds*. But I do think that he is no Aristotle.

[16] On the nature of the Homeric passages in Aristotle's biology, see Mayhew 2015.

[17] See the essays in Gotthelf and Lennox 1987, Lennox 2001, and Gotthelf 2012. Note especially Gotthelf's coda to this last item: 'Aristotle as Scientist: A Proper Verdict' [371–398].

a brief but superb survey of the history of Aristotle-biography from Zeller to the present, with Zeller, Wilamowitz, Jaeger, and Düring quite rightly receiving the most attention, though many other figures are treated as well.[18] Natali ends where the survey begins, defending a Zeller-like position in favor of an intellectual over a political interpretation of the life of Aristotle:

> The pages of *Nicomachean Ethics* X.6–8 on the *bios theôretikos* [sic],[19] in which Aristotle describes it as a perfect state of being, take on an exactly autobiographical flavor. The position of Zeller, from which we began, emerges again at the end of this review as one of the most reliable interpretations. [144]

A reader, having finished this book, may well bemoan how little, in the end, we can say with certainty about Aristotle's life and school. But there is, in an important (if not entirely satisfying) sense, a fair amount about which we can be confident. Having read this book, I now have a much better grasp of the issues involved in ancient biography as it applies to Aristotle and other Peripatetics, and of what we know and what we do not know (and knowing what we do not know *is* a kind of knowledge).

My objections to the treatment of Aristotle's biology aside, I agree with Hutchinson's claim in his preface, that this book will (and the implication is, should) 'serve as the new modern standard biography of Aristotle' [vii].

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18 Natali's postscript updates his account.

19 Rather, 'theoretikos' (as in the Italian title) or preferably 'theôrêtikos' (« θεωρητικός »). Perhaps this is the appropriate place to mention that this book is relatively free of typographical errors. I noted one other: 'Döring' on page 131 should be 'Düring'.

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Cosmology and Fate in Gnosticism and Graeco-Roman Antiquity: Under Pitiless Skies by Nicola Denzey Lewis

Nag Hammadi and Manichaean Studies 81. Leiden/Boston: Brill, 2013. Pp. xiv + 206. ISBN 978–90–04–24548–8. Cloth $140.00

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The main point of Nicola Denzey Lewis' book, *Cosmology and Fate in Gnosticism and Graeco-Roman Antiquity*, is to debunk once and for all the notion that early Christian 'Gnostics' felt 'alienated, disempowered, or oppressed by cosmic forces' [185]. By challenging the scholarly consensus of an earlier generation of historians of religion such as Hans Jonas, E. R. Dodds, Franz Cumont, Arthur Darby Nock, and André Jean Festugière, a consensus which still holds considerable sway today, Lewis has also dealt a definitive blow to the category of 'Gnosticism' itself. She thereby lends further support to the argument of scholars such as Karen King, Michael Williams, Elaine Pagels, and David Brakke that texts traditionally labeled 'Gnostic' do not represent a fringe, marginal, derivative, degenerate religion devolving upon either early Christian origins or late Platonism, and existing apart from and in distinction from some form of proto-orthodox Christianity. Rather, they represent the diversity of Christianity in the second-century, its engagement with Graeco-Roman thought, and its participation in the intense dialogic exchanges of school settings and study groups in large urban centers.

In this respect, Lewis uses the texts that she discusses to demonstrate the great innovativeness and cultural entrepreneurship of early Christian thought. She does all of this by carefully and expertly exploring the way in which concepts such as Providence (προνοία) and Fate (εἱμαρμένη: mainly in the sense of astral determinism) are invoked and deployed in a wide range of second-century texts, both Christian and non-Christian. Instead of reading statements regarding enslavement to Fate in these texts as representative of a social group's feelings about the cosmos, Lewis successfully demonstrates that language of this sort serves a rhetorical purpose, namely, it refers to those who stand in opposition to the authors and preservers of these works.

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In other words, those who are not privy to the revelations contained within the texts that she explores are the ones who are subject to the daemonic influence of the planets and stars. She also highlights an important difference between the way in which early Christians dealt with the problem of astral determinism in the second century and the way in which they did so in subsequent epochs, particularly in the third and fourth centuries. Her claim is that while second-century Christians, and not merely those identified in some way or other as 'Gnostics', were willing to entertain the possibility that some people were subject to Fate, later theologians generated universal arguments against astral determinism. By making these points, Lewis makes a significant contribution to studies on early Christianity and the Nag Hammadi codices, as well as to studies in the history of ideas.

Chapter 1, entitled 'Were the Gnostics Cosmic Pessimists', is an overview of the development of the identification of the writers of 'Gnostic' cosmogonies and apocalypses with a certain negative attitude about the universe. This chapter is very helpful, not just as it relates to the question that Lewis asks in the book but as an overview of the problems with earlier scholarship on 'Gnosticism' in general. Lewis highlights the ways in which members of the *religionsgeschichtliche Schule* of the late 19th and early 20th century propounded the view that 'cosmic pessimism' entered the Roman worldview from Babylonian and Iranian religion *via* 'Gnosticism' [21]. Implicit in this idea is a theological position which holds that 'belief in astrology was a pathological attitude healed by the orthodox Christian fathers of the fourth and fifth century' [23]. Scholars in this lineage determined the terms of the discussion in ways that persist today. This chapter does such an excellent job of laying out the history of this scholarship and its inherent problems that I would consider assigning it not only in my seminars on early Christianity but also my course on method and theory and my introduction to the New Testament.

Chapter 2 does a couple of important things. First, it plots the shift in thinking about Providence and Fate as one and the same thing within Stoicism to their distinction in the works of various Middle Platonists. Lewis then records instances where early Christian writers such as Athenagoras and Tatian take up this notion of a divided Providence before she provides instances of the same in some Nag Hammadi texts, namely, *On the Origin of the World* and the *Apocryphon of John*. In the case of the latter, Lewis suggests that the original Greek version of the text may have associated the ruling archons with the planets and thereby with some idea of astral determinism. The main point of this chapter is that, like their Middle Platonic contemporaries, a wide variety of Christian authors thought of Providence as divided. They did so for what we might call 'theogonical' reasons, that is, in order to explain evil and chaos in the present world.

In her next chapter, Lewis advances the position that the cosmic pessimism which we do find in second- to fourth-century Christian texts, that is, the idea that some human beings are subject to Fate in the sense of astral determinism, 'finds its root not just in prevailing Graeco-Roman conceptions of a malevolent cosmos, but also in later exegeses of the Pauline corpus' [53]. Here she focuses on Paul's rhetoric of enslavement and the role played in his cosmos of categories of celestial beings such as 'powers' and 'archons'. His idea that these forces collectively rule the cosmos until the time of the Eschaton (i.e., the final judgment or the end of the world) implies that some sort of archontic hold on humankind has been built into the cosmos by its providential creator.

In chapter 4, the author traces the appearance of the term «εἱμαρμένη» in certain Nag Hammadi texts where it serves to explain human disinterest in spiritual matters. Her case studies are, once again, *On the Origin of the World* and the *Apocryphon of John*. Next Lewis discusses the appearance of the term «εἱμαρμένη» in one of the three Hermetic texts that appear in the Nag Hammadi codices, the *Discourse on the Eighth and Ninth*, and demonstrates that Fate in the Hermetic tradition has suffered the same kind of misrepresentations as it has in 'Gnostic' texts.

Chapters 6–8 all explore strategies for escaping Fate or enslavement to the cosmos in various 'Gnostic' texts. These include texts that focus on the apocalyptic ascent of some sort of savior figure, ascetic practices for controlling the passions, and the work that the sacrament of baptism can do to subvert this enslavement.

In Chapter 9, Lewis considers one early Christian anomaly, namely, the Gospel of Judas, a text that invokes the sort of cosmic pessimism that she is excising from Christian tradition in the rest of the book. Unlike the other texts that she discusses, the Gospel of Judas does not appear to draw on contemporary philosophical interpretations of Providence but takes its cues instead from Jewish apocalyptic. The book ends by focusing on what Lewis sees as an important shift in thinking about Providence and Fate in later

Christian theologizing. Using the works of Methodius and Arnobius in the third century and Gregory of Nyssa in the fourth, Lewis claims that later Christian intellectuals abandoned notions of Fate that were informed by 'pagan' cosmological concepts and instead denied that humans were subject to Fate in any sense.

In general, Lewis' arguments about second-century Christian ideas of Fate, astral determinism, and Providence are convincing and insightful. The book is essential reading for anyone working on early Christian intellectual history and its engagement with Greco-Roman philosophy and science.

I have a number of minor criticisms of the book, none of which detracts from its overall quality. I list them briefly here.

It is confusing that the term 'Gnosticism' appears in the book's title, when it seems that the author is clearly uncomfortable with this categorization of the texts that she focuses on. In terms of style and structure, the book is a bit disjointed and the arguments are not always easy to follow; but because the author has elected to use many subheadings in each chapter, it is easy to locate the information one needs when doing research. One wishes that the author had been more consistent in her use of terminology when referring to the early Christians that she is discussing, given the fact that these terminological issues are part of the larger debate in which she is engaged. The book is a reworking of Lewis' dissertation of 1999 and, given the intervening years between her first work on the topic and this project, the book's bibliography could have been updated more than it was. Lewis cites only 12 secondary sources from 2000 and later, four of which are her own publications. At certain junctures, she seems to be making claims about the communities which are using the texts that she discusses; but for the most part, she does not go beyond the texts themselves to explore much of the context that would provide a broader picture of the contest that she invokes. If the concept of astral determinism is being used as a rhetorical strategy, as she rightly claims it is, it would be good to get some sense of who is being marginalized by this discursive move. Finally, although her argument is ultimately convincing that from the second through the fourth centuries Christian debate about the nature of Fate shifted from questions of cosmology to ones of ethics and moral responsibility, her tendency to elide the third and fourth centuries is problematic and calls for more nuance. Figures such as Origen, Porphyry, and Iamblichus complicate this tidy picture considerably. And her claim that pagans such as Iamblichus were 'reduced to a beleaguered minority' in the third and fourth centuries needs rethinking. Her argument works for the later fourth century but the intervening period needs further attention. This was not, however, the aim of her book and, hence, my point is a minor one.

De motu animalium. *Fragmenta translationis anonymae* edited by Pieter De Leemans

De progressu animalium, De motu animalium. *Translatio Guillelmi de Moerbeka* edited by Pieter De Leemans

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For the last few decades of the last century, the biological works by Aristotle have received particular attention from researchers, both through the publication of essays and through the collection of contributions by various scholars. As with other parts of the Corpus Aristotelicum, the treatises dedicated to living beings—and animals in particular—have influenced and conditioned the cultural history, philosophy, and science of the civilizations that have evolved on the shores of the Mediterranean.

This review is focused on the medieval Latin tradition of the Aristotelian biological corpus; it concerns Pieter De Leemans' excellent work on two booklets in this corpus that belong to the group of treatises that Aristotle devoted to animals. We will also take into account the editions of the *Historia animalium* 1–5 [Beullens and Bossier 2000] and *De generatione animalium* [Drossaart Lulofs 1966], and what has already been done with regard to the Arab-Latin tradition of the *Libri animalium* [see Van Oppenraaji 1992, 1998].

Since the edition of the fragments of the *translatio anonyma* of *De motu* may be considered a sort of case study in the series of editions of Aristoteles Latinus (since we are dealing with the attempt at 'reconstructing' part of a version that did not come to us intact), we will begin with the editions of the translations of the *De progressu* and *De motu* by William of Moerbeke, and our discussion will extend to all the translations of Aristotle's treatises on animals and their circulation around the middle of the 13th century.

The geographical and chronological coordinates are central Italy (the papal kingdom and Magna Graecia during the reigns of Frederick II and then

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Manfredi) in the period from 1215–1220 to 1260–1265, and the territories of the Byzantine Empire occupied by the Crusaders from 1204 to 1261. The main characters are Michael Scot for the Arabic-Latin version of the *Libri de animalibus* (which did not include the *De progressu* and *De motu*) and William of Moerbeke, whose presence in the papal curia in Viterbo is attested since 1267, even though he certainly finished translating the *De partibus animalium* while he was in Greece. Finally, with regard to the *translatio anonyma* of *De motu*, the textual tradition of reference is that of the *De principiis motus processivi* by Albert the Great, in which he says he found a translation of *De motu* while he was 'in Campania juxta Greciam', probably during his first journey to Italy in 1256/57.

Among the merits of De Leemans' editions, we should note the clarity with which he guides the reader through his large and complex critical introductions. The history of the tradition of Moerbeke's versions of the *De progressu* and *De motu* is documented in an introduction that extends for more than 200 pages [xv–cclx] which are divided into three chapters:

Chapter 1. The Latin Tradition [xxiii–cxl]
2. Moerbeke's Translation and the Greek Tradition [cxli–ccxxx]
3. Editorial Principles [ccxxxi–ccli].

These chapters have internal sections which are further divided in turn into paragraphs.

Not only at the beginning of the chapters but also at the beginning of each section, De Leemans clarifies for the reader the path that he intends to follow and his purpose. After rapidly recalling the *status quaestionis* relating to the medieval Arabic-Latin and Greek-Latin translations and to the editions of the Greek text, De Leemans indicates that, at every stage of his analysis, he will start from the *De progressu* and not from the *De motu*, although the translation of the latter was earlier than the former, because the tradition of the former booklet has manifested itself with greater clarity.

As expected, chapter 1 starts with the list of the manuscripts [part 1], continues with an analysis of the Parisian university tradition [parts 2–7], and then moves on to the independent tradition typical of 'Italian' manuscripts [parts 8–9] as well as to the manuscripts that reflect a contaminated tradition (but only for the *De motu*) [part 10]. De Leemans provides two lists of manuscripts: the first containing the *De progressu* (98 mss + 2 fragments), the second containing the *De motu* (167 mss + 1 fragment). Following the example of the editions of the *Metaphysica* and *Meteorologica* by Gudrun Vuillemin-Diem [1995 and 2008, respectively], he notes for each codex the relationship that the text has with the university tradition or with the independent one and specifies its quality.

In my opinion, it would have been useful to point out which codices—about 90, I think, all present in both the first and the second list—contain both booklets as well as also to indicate the codices (just under 40) that hand down one or both booklets along with other zoological treatises (*Hist. an., De part. an., De gen. an.*).[1] By providing these additional data, De Leemans' very complex and detailed examination of the issues related to the manuscript tradition, which is directly or indirectly dependent on the exemplars circulating in Paris during the 13th century, might have gained greater clarity. The issue of the exemplar of the books *De historiis animalium* and that of the exemplar containing the *De motibus animalium et aliorum parvorum*—both listed in the well-known taxation list of the exemplars of the *stationarius* André de Sens that is dated February 25, 1304—is a matter that has now been discussed for years by the scholars who engage in the editions of Aristotle's translations. De Leemans attempts a comprehensive and systematic reading, putting the research conducted by other scholars to good use in addition to his own and not only in relation to Aristotle's treatises.

Retracing the research done with great scrupulousness by De Leemans would mean re-proposing here a considerable part of the introduction. I will, therefore, try to summarize its main points.

After defending his doctoral thesis (Katolieke Universiteit Leuven, 2001), which was supervised by the late, lamented Jozef Brams, De Leemans has continued his research by integrating and consolidating the results that he had already achieved. In part 3 of chapter 1 [xlvii–lxi], he documents the tradition depending on the exemplar containing, as we have said, the zoological works. Beullens and Bossier 2000 shows that the *pecia*[2] containing

[1] See respectively the lists published in Beullens and Bossier 2000, Beullens 2009, Rossi 2009, Drossaart Lulofs 1966.

[2] See Boyle 1991, 39:

> In medieval universities the peciae system, in broadest terms, worked as follows. A bookseller (*stationarius*) obtained a fair copy or other exemplar of the

the translation of book 10 of the *Historia* was later inserted into the original exemplar (which was made up of 37 *peciae*, though in the above-mentioned taxation list of 1304 there are said to be 38). Consequently, some manuscripts do not contain book 10. In the case of the 38 *peciae*, the two booklets were covered by *peciae* 18–20.

In addition to the codices containing signs of *pecia* coincident with this exemplar that are already known, De Leemans adds other ones. Regarding *pecia* 18, a group of 14 manuscripts allegedly dates back to an exemplar (called $\mathbf{P}^1$), while in *pecia* 19, which comes between the *De progressu* and *De motu*, the same manuscripts document a subdivision of the tradition represented by $\mathbf{P}^1$ in $\mathbf{P}^{1a}$ and $\mathbf{P}^{1b}$. Faced with this complex situation, De Leemans notes that $\mathbf{P}^{1b}$ could represent a second *pecia* 19, and asks:

> The question then is if this *pecia* 19bis was intended to replace the original *pecia* (*pecia refecta*) or if both *peciae* remained in circulation (*peciae duplicatae*). [lxvii]

In the light of what was established by Beullens [2009] for *pecia* 15, De Leemans believes that both *peciae* very likely circulated in the same period of time. The type of tradition attested by *pecia* 20 is similar to that of *pecia* 18; but De Leemans claims that he will prove that it

> was the model for one of the realizations ($\mathbf{P}^{2b}$) of the *De motu animalium* in the original exemplar "Item de motibus animalium, et aliorum parvorum". [60]

Regarding this branch of the Parisian university tradition, that of the exemplar containing only the *Libri de animalibus*, the ms. Cesena, Malatesta Library, Plut. XXIV sin. 4 (abbreviated as *De*), which is an exemplar made up by the *stationarius* 'Adam corrector' (who, as far as we know, was active in Paris from 1292 to 1296) constitutes a different case. Beullens and Bossier [2000, l–lii] had already recognized that this manuscript is 'technically' an exemplar dating back to the late 13th century (which also contains book 10 divided, however, into 37 *peciae*) but ruled out its inclusion both in the re-

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work to be copied and sold. From this exemplar he had made a copy-text or exemplar-text of his own which generally was divided into equal units or peciae. These were then numbered in sequence and hired out in turn for copying to professional scribes (or, to use terminology current much later in England, 'scriveners').

construction of the text of the *Historia animalium* and in the documentation of the tradition, because no surviving codex can be said to derive from it.

De Leemans endorses this assessment and the consequent choice. I have tried elsewhere to use arguments derived from an examination of the tradition of Moerbeke's translation of *De partibus animalium* to show why, in my opinion, this manuscript should be taken into account in the edition of the Latin translation. Apart from the fact that we do not know whether the exemplar of 38 *peciae* filed with André de Sens was recent or if it had been in use for some time, it is also indisputable that the *De* is an 'official' copy, so to speak, of Moerbeke's translation of the treatises on animals given that it is an exemplar. Furthermore, for the very reason that no surviving codex would seem to derive from it, I believe that it should be not be excluded but rather should be taken into consideration, and also because the collations show that the text deriving from it is not inferior to the text of the other codices of the university tradition [see Rossi 2009, 70–72].

Part 4 of chapter 1 [lxi–lxxix] deals with the composition of the other Parisian exemplar, i.e., the above-mentioned one which included the two booklets on animals in the context of other Aristotelian and pseudo-Aristotelian booklets and treatises. This exemplar is recorded in the 1304 taxation list as follows: 'Item de motibus animalium, et aliorum parvorum'. Here De Leemans shows once again all his scrupulousness by exploiting the results already achieved by other scholars and by basing his analysis both on textual data and on an examination of the structure and succession of the treatises of natural philosophy in the codices reviewed as representatives of the so-called *corpus recentius*—such treatises include the two which he has edited. His investigation goes so far as to outline the probable location of the two booklets and the possible succession of much of the entire corpus in the Parisian exemplar.

Recognizing that we are still faced with trying to bring elements in favor of a proposal that is largely hypothetical, De Leemans comes to providing elements indicating the existence in succession of three exemplars called $\mathbf{P}^2$, $\mathbf{P}^3$ and $\mathbf{P}^4$, which is documented in part 5 of the introduction for the *De progressu* [lxxix–lxxxix] and in part 6 for the *De motu* [xc–cxii]. While believing that the assumptions put forward may have a degree of probability and bearing in mind that, as expressly declared by De Leemans [lxiii], the study of the traditions of the *Parva naturalia* and other booklets has not yet been carried out and that he declares 'I will, therefore, confine myself to general remarks and data for which I have found sufficient grounds,' one may have doubts when asked to contemplate the mistakes that allegedly document a further dichotomy in the tradition, such as those that would ground the subdivision of $\mathbf{P}^{2}$ into $\mathbf{P}^{2a}$ and $\mathbf{P}^{2b}$ [lxxxii–lxxxv], especially when it comes to variations or misspellings of animal names transliterated from the Greek, variations which, I think, should in most cases be regarded as indifferent. A similar situation is documented, perhaps with more data, for the *De motu* and for $\mathbf{P}^{2a}$ and $\mathbf{P}^{2b}$ as well but with the difference that, in the *De motu*, $\mathbf{P}^{2b}$ is not derived from $\mathbf{P}^{2a}$ but is more complex because it is allegedly also articulated in $\mathbf{P}^{3a\text{–}b}$ [see part 6: xc–cxii]. De Leemans concludes that there is evidence to suggest that the Parisian university tradition, which is represented by the exemplars $\mathbf{P}^{1}$ and $\mathbf{P}^{2}$, can be traced back to a common archetype P, although this can be stated with greater certainty for the *De progressu*.

After following De Leemans along his journey through the intricate Parisian university tradition, I should like to make some observations about a certain disorientation that this reader at least felt at the end of the journey and to state that this connects with what I noted in the beginning about the two separate lists of codices and leads as well to the question of how to proceed in the analysis of such an extensive tradition. If, in fact, De Leemans had provided a list of the manuscripts containing the two booklets inserted between the three main Aristotelian treatises on animals and another list of the codices containing these booklets included among other treatises by Aristotle (according to the more than likely content of the exemplar 'Item de motu animalium et aliorum parvorum'), and if he had organized the exposition of the results of the analysis of the tradition first considering only those codices and *then* all the others, the very complex survey undertaken by De Leemans would have been much clearer and the reader might experience less bewilderment at the end of it.

Furthermore, in my view, if De Leemans had emphasized more strongly or had given stronger 'typographic' emphasis to some revealing annotations that he has made, he might have guided the reader through his analysis better. Finally, it is not very easy to go from a list of codices in the alphabetical order of the libraries to a list of the same codices according to the number assigned to them in the catalogue compiled by the fathers of the Aristoteles Latinus in the alphabetical order of all nations and, for each of these, according to the alphabetical order of the libraries.

Part 8 [cxv–cxxxix] of chapter 1 presents and discusses data that led De Leemans to identify an independent tradition divided into (x) and (y), bipartitions that are configured differently for each booklet: *De progressu* in $\mathbf{x}$, $\mathbf{y}_1$, $\mathbf{y}_{1a}$, $\mathbf{y}_{1b}$, $\mathbf{y}_2$, and the ms. *Zw*; *De motu* in $\mathbf{x}$, $\mathbf{y}$, $\mathbf{z}$, $\mathbf{z}_1$, $\mathbf{z}_2$, and $\mathbf{z}_3$. In particular, two manuscripts stand out for the quality of the text transmitted: the Borghese 134 of the Biblioteca Apostolica Vaticana (abbreviated as *Bv*) and the Leop. Med. Fesul. 168 of the Biblioteca Medicea Laurenziana (abbreviated as *Fa*). These manuscripts, already for the text of the *De generatione animalium*, represent a branch that is independent of the university tradition tracing back to the translator[3] and seem to have the same value for the text of the *De partibus animalium* [see Rossi 2009, 69]. As for the two booklets considered here, *Fa* does not have a well-defined position but allegedly represents the tradition independent of the university, while De Leemans believes that '*Bv*, or more likely its father, was a direct yet careless copy of Moerbeke's autograph' [cxxxvii].

Codex *Bv* deserves some thought. In addition to that in the catalogue of the Aristoteles Latinus, we have a detailed description of it made by Anneliese Maier [1952, 177–179]. But so far have we not taken into account that we have the ownership note by Pierre Roger de Maumont (the future Pope Clement VI, 1342–1352) which also describes its content. Pierre Roger became bishop of Arras in 1328 and in the ownership note he writes:

> *in hoc volumine continentur isti libri per ordinem et sunt p<etri> rotgerii de malomonte monachi caze dei. Prima pars libri de animalibus....*

We are, therefore, not far from the truth if we hold that the collection was made (there are many copyists who worked on it) by this Benedictine monk during the years he spent at the University of Paris, where he had been sent by his superior in 1307 before being appointed bishop. According to Anheim [2006, 5], the Vatican codex was purchased between 1312 and 1316, before Pierre Roger got his first ecclesiastical benefice. The interesting fact is that, while there are no indications of *peciae* for the *De partibus* [ff. 1r–32v], *De*

[3] See Drossaart Lulofs 1966, xxvii:

> *ex libro—nescio an ex ipso autographo vel ex apographo quodam—pristinam versionem continente, quem Guillelmus, ut eius aetatis mos erat, cursim percensuerat et verbis corrigendis expungendisque in marginibus vel inter lineas inserendis emendaverat.*

coloribus [ff. 33r–36v], *De generatione animalium* [ff. 37r–75r], *De progressu* [ff. 75v–78v], and *De motu* [ff. 78v–84]), all *peciae* are clearly indicated for each of the treatises by Albert the Great that complete the collection, with the exception of *Speculum astronomiae* [cf. Murano 2005, 225–228]:

> *De natura et origine animae*: IIIa–VIIIa,
> *De natura loci*: IIa–VIIIa,
> *De causis proprietatis elementorum*: IIa–IXa,
> *De intellectu et intelligibili*: IIa–Va,
> *De sensu et sensato*, *De memoria*, *De reminiscentia*: Ia–XIVa[4]

Regarding VIa in this last series of *peciae*, it is clarified that: ‘G. senonensis. est’, thus ‘Guillelmus Senonensis’—William of Sens, who, according to Richard H. Rouse and Mary A. Rouse [1991, 58], should be located ‘at the head of the list’ of the members of a family of stationers that was active in Paris for decades (about 1270—1342) [see also Rouse and Rouse 2000, 1.73–98]. Silvia Donati (Albertus-Magnus-Institut, Bonn) will undoubtedly examine the *peciae* of the paraphrase of *De sensu et sensato* in her forthcoming edition of the text in the *Opera omnia* of Albert. What matters here is that, in the first decades of the 14th century, the probable son of William of Sens, Andrew, still possessed his grandfather’s *peciae*—or perhaps a copy of them with his grandfather’s ‘mark’—and that the family shop ‘was situated on the Left Bank, on the rue St-Jacques adjacent to the Dominican convent’ [Rouse and Rouse 2000, 1.81], whose library William was able to draw on, as is testified by some manuscripts.

But let us go back to the Borghese codex 134 (*Bv*). Although composite, the note left by Pierre Roger claims possession of the entire codex, written by different people who were nevertheless all French. It seems well founded, then, to infer that at Andrew Sens’ *stationarius*, in addition to the exemplar of the *Libri de animalibus* (which appears to be in his possession in the taxation list of 1304), there was also another copy of those works or that, given the ‘historical’ bonds between the family and the convent of the Dominicans, Pierre Roger has had access to a copy of Moerbeke’s translations at their library, a text which was independent of the university tradition. It may also be the case that Pierre Roger had acquired and merged into one volume various materials available on the book market, perhaps in part dating back

[4] These are all marked as ‘de sensu et sensato’.

to the late 13th century. A closer examination of the codex will perhaps shed some light on this.

Chapter 2 examines the relationship of Moerbeke's translation to the Greek tradition. Once again, at the beginning of this very complex chapter, De Leemans indicates schematically what he means to do and how he intends to proceed. We know that the research conducted in recent decades on numerous of Moerbeke's translations of Aristotle's treatises has documented the existence of one or more 'stages' in the translation of the text corresponding to one or more revisions attributable to the translator himself. The data affording evidence of this 'evolution' of the text must obviously have special features and so, as noted by De Leemans, we must

> pay attention both to variant readings in different groups of manuscripts that might be renderings of the same Greek word and to Latin variants that are (or might be) renderings of as many Greek variants. [cxliv]

As for the *De progressu*, he considers Bekker's edition and all subsequent editions[5] as well as the study by Berger [2005] of the entire Greek tradition that has survived. For the *De motu*, De Leemans also considers the editions by Luigi Torraca [1958] and Martha Craven Nussbaum [1978].

Welcoming the conclusions reached by Berger, De Leemans has collated four other manuscripts of the *De progressu* that were not considered by editors before. For the *De motu*, he starts from the text by Nussbaum, who collated five other codices not considered in previous editions, and decides to check all the codices never used by editors in *loci* of particular interest to the Latin tradition. Then, he selects the five that he has fully collated [clxxxii]. De Leemans thus comes to the conclusion that in the tradition of *De motu* we are able to distinguish three stages of the text and that many interventions have been made by resorting to another Greek manuscript. But he reaches no certain conclusion in the case of the *De progressu*. The extreme fluctuation between the families **α** and **β** of the tradition of the *De progressu*[6] does not allow us to connect its translation to one of the two families with a sufficient

5 See Jaeger 1913, Forster 1937, Louis 1973.

6 See the complex *stemma codicum* developed by Berger on page cxlvi and the hypotheses discussed by De Leemans on pages clxxiii–clxxviii.

degree of probability. A similar situation is acknowledged for the *De motu*.[7] What text is actually being edited, then?

As for the *De progressu*, De Leemans states:

> I have decided to offer a text that represents the stage of the translation when a copy of it was made and sent to the University of Paris. This stage is mainly found in $\mathbf{y}_1$, $\mathbf{y}_2$, *Zw*, and the Paris tradition (**P**). Since these all stem from the same ancestor **y**, the text that they offer needs emendation. I have used for these emendations not only **x**, but also *Bv* and *Aeg.*, which represent the same stage of the translation as **y**. [ccxxxv: cf. the stemma on ccxxvii]

De Leemans, therefore, has chosen to offer the text of the university tradition but one that has been amended, yet not by witnesses of other branches of the same tradition but by witnesses representative of other stages of the tradition. The stemma, in fact, represents three stages of tradition that are chronologically, I think, distinct and different. The branching of the university tradition seems to be dependent on the second stage, which is bipartite: the *peciae* tradition on the one hand (the most numerically significant), *Bv* and *Aeg.* on the other.

Even if we can agree with De Leemans's decision (because in all probability the interested reader will find in the text or in the apparatus the lessons that he/she is looking for), it seems to me that a question remains unanswered: What criteria prevailed in the constitution of the text? Adherence to university *vulgata*, respect for the 'first stage' text, or/and the subsequent interventions of the translator?

Regarding the constitution of the text of the *De motu*, De Leemans's decision was radically different: he chose to offer the text in the final review of the translator, relegating to the apparatus the variants of the other branches of the tradition, which in this case are also 'chronologically' later. In my opinion, there were sufficient grounds to make a similar choice for the *De progressu*.

One final note. There are apparently two new studies on the Greek tradition of *De motu*[8] that will lead to the establishment of a new text. In his recent French translation, Pierre-Marie Morel states:

7 See the stemma proposed for the *De motu* by Nussbaum on page clxxx.

8 Primavesi 2013, Primavesi and Corcilius 2013: both are forthcoming but not yet available.

> *Celle [*scil. *la traduction] du* Mouvement des animaux *se fonde sur le texte édité par L. Torraca, bon connaisseur de la tradition manuscrite, dont les choix sont très souvent confirmés par les éditeurs les plus récents, et dont les notes attestent une très fine compréhension du texte. Pour le* Mouvement des animaux*, nous avons également pris en compte, avec une attention plus particulière, l'édition de M. Nussbaum (1985), ainsi que l'édition encore à paraître de O. Primavesi (2013).* [Morel 2013, 48]

We must now await the publication of this new edition of the Greek text.

Let us turn to Albert, the translation of the *De motu* that he discovered during his first stay in Italy in 1256–1257, and the fragments of it appearing in his *De principiis motus processivi*, a text of which we have the autograph [see De Leemans 2011]. As I have already mentioned, Albert stated that he had found a translation of Aristotle's treatise 'in Campania juxta Greciam' [see Gayer 1955, 48.66–74]. In the introduction [ix–xviii], De Leemans briefly summarizes what we know in this regard, namely, that:

(a) until around the middle of the 13th century, the texts of the zoological works by Aristotle were known by the Latins essentially through Arab mediation, although, in David of Dinant's *Quaternuli*, there are quite a few references to these treatises translated from the Greek;
(b) at the court of Frederick II, there were many translators, among them Michael Scot; and
(c) a certain Nicolaus Siculus Grecus (a Sicilian, as the name indicates) figures among the collaborators who translated from Greek in the circle of Robert Grosseteste († 1253) in Lincoln, while Bartholomew of Messina worked at the court of King Manfredi.

That, in some areas of southern Italy, people continued albeit to a very limited extent to use Greek in the liturgy, is well-known and has been studied by eminent scholars. No wonder, then, that Albertus spoke about 'Greece', although, I believe, he was referring not to Greece proper but to Magna Graecia, i.e., to southern Italy, a geographic area which was in contact with Byzantium and in which, during the 12th century, translations from Greek and from Arabic already flourished. It should be borne in mind that southern Italy was also the crossroads of the maritime contacts between the West and the East, particularly during the Latin Empire of the East. However, it is in the region of Campania that Albertus came across the Greek-Latin translation of the *De motu*, which fortuitously compensated for the absence of this text among the *Libri de animalibus* translated by Scot.

The procedure developed by De Leemans for detecting the remains of the lost version is ingenious and flawless from a philological point of view, given not only the paraphrastic nature of Albert's treatise but also the way in which Albert used his sources [see xix–xlvi]. However, by keeping in mind Albert's *usus scribendi*, his license with regard to the *littera*, and his insight in interpreting the very rough-edge translations of Aristotle's treatises, one might not agree with De Leemans that fluctuations in the interpretation of certain passages are actual changes of perspective after a re-reading [xlv].

It has to be acknowledged that De Leemans expresses some doubts too [xlvii–li]. Nevertheless, in my view, since this is Albert, the *variae lectiones* that we find should prompt greater caution in attributing them to the anonymous translator. Regarding the lexicon, the comparison with that of the published translations in the Aristoteles Latinus discloses a tendency to *variatio* and to translation of terms not evidenced elsewhere. Such peculiarities make it possible to set this translator next to that of *Rhetorica anonyma*. De Leemans concludes:

> The author of the *Rhetorica anonyma* was probably active in the first half of the thirteenth century, when quite a few translators appear to have been active in southern Italy. In the next chapter, which deals with the Greek sources of the translation, I will argue that if not with the translator of the *Rhetorica* himself, there is probably a link between the translator of *De motu animalium* and the southern Italian translators active in the first half of the thirteenth century. [lvii]

I think that the southern Italian translators referred to by De Leemans are the above-named Nicolaus Graecus and Bartholomew, and perhaps, in addition to the anonymous translator of the *Rhetorica*, the anonymous translator of the *De partibus animalium*, since this translation manifests similar characteristics with regard to the lexicon [see Rossi 1989]. In chapter 3, De Leemans addresses the possible relationship of the fragments recovered with the Greek tradition. The task that he proposes to undertake is very complex and problematic because generally we are not faced with passages of different length but with very short phrases or single words. Despite this, he believes that he has isolated some textual data that would lead us to envisage a relationship between the codex used by the anonymous Italian translator and the tradition represented by Z^a [Ms. Florence, Biblioteca Medicea Laurenziana, 87.21].

At this point, the history of the surviving fragments of the *Translatio anonyma* of the *De motu* connects with that of the texts transmitted by Z^a and the history of the codex itself. In particular, De Leemans recalls what was documented by Dieter Harlfinger [1971] about the relationship between the Latin version made by Robert Grosseteste of *De lineis indivisibilibus* and the Greek tradition represented by the codex Z^a, which dates back to the early 14th century and comes from the monastery of San Nicola di Casole Bruzio (Calabria) [see Moraux *et al.* 1976, 323–324]. The upshot is that the codex was written and made in Italy, purchased for Lorenzo the Magnificent by Janos Lascaris, and probably modeled on another codex circulating in the same area. The fact that in England Grosseteste used for his translation of *De lineis* a codex no longer extant which referred to the tradition represented by Z^a suggests the possible 'material' mediation of Nicolaus Graecus, who was in his service.

While acknowledging that such a mediation is conceivable, I should like to note that, to my knowledge, the evidence and research conducted so far on the Bishop of Lincoln's study and knowledge of Greek and his 'Greek library' assign a prominent role to John of Basingstoke and not to Nicolaus: we know almost nothing about why and with whom Grosseteste studied Greek and it was Basingstoke who reported to him the existence of *Testamenta XII patriarcarum* and got him a copy on his request (ms. Cambridge, University Library, Ff. 1. 24, 12th century) [see Dionisotti 1988].

I will conclude this review of De Leemans' excellent work with some considerations that do not put into question at all the value of his work but are instead aimed at raising a more general problem. We have seen that, for the 12th century, there has been much progress in our efforts to assign authorship to the translations of Aristotle from Greek into Latin which have come to us anonymously. For the 13th century, however, it seems to me that we are forced to turn about in a circle from Grosseteste to his 'adiutores' (mainly Nicolaus Graecus, Moerbeke, and Bartholomew of Messina) whenever we are faced with an anonymous translation. Furthermore, Moerbeke, besides having prepared translations from Greek for over a quarter of a century that are far superior to the work done by any other translator in the history of Western culture, is alleged to have revised (even more than once) his versions of major treatises such as the *Metaphysica*, *Physica*, *De caelo*, *De anima*, and *Meteorologica* as well as the zoological and biological treatises, as we have seen. Indeed, Father Gauthier asked about these revisions or successive stages of many of Moerbeke's translations in a note to one of his own publications:

> *D'autre part, je me pose une question (mais sans doute est-elle due à mon incompétence): Guillaume de Moerbeke n'aurait-il pas eu à sa disposition comme Robert Grosseteste, comme saint Thomas, une équipe de secrétaires qui lui auraient préparé le travail et dont l'intervention suggérerait une interprétation moins linéaire des divergences de traduction?* [Gauthier 1993, 85n37]

Father Gauthier's 'lack of competence' is charming modesty; mine is real. Whenever it is proposed that we should resort to Moerbeke's intervention in order to explain variants of translation that are reflected in Greek variations, I am quite perplexed. It seems to me that there may be other explanations and perhaps more 'economic' ones. Rather than simply accepting that Moerbeke has revised the text here and there on one or two different occasions and very often without being driven by the need to make the translated text more understandable, the question should, I think, be 'Why would he have done so and by what criteria?' It is well known that in Italy, especially in an ecclesiastical environment, the knowledge and use of the Greek language never disappeared; and that, precisely in the 60s of the 13th century, the need to mediate between the papacy and the Byzantine emperors (who had re-conquered Byzantium) required linguistic mediators. In theology, for example, Pope Urban IV in 1263 or early 1264 asked Thomas Aquinas his opinion about *Liber contra errores graecorum*, a text compiled in Greek by Nicholas of Cotrone and translated by him into Latin. Aquinas gives a harsh judgment, noting that the author does not have sufficient knowledge of the theological consequences of inaccurate or even misleading translations of Greek terms [FOP 1969]. Thomas, in the dedicatory epistle to Cardinal Annibaldo Annibaldi of *Catena super evangelia*, a work composed in Rome between 1265 and 1268, says:

> *Et ut magis integra et continua praedicta sanctorum expositio redderetur, quasdam expositiones Doctorum graecorum in latinum feci transferri....* [FOP 1953, 429]

Thus, in the convent of Santa Sabina or, in any case, in Rome, Thomas was easily able to get a translation of the parts that he needed of the comments of the Greek Fathers. But who were these translators? We know that teamwork was customary among the Dominicans in the 13th century [see Congar 1984], so perhaps it is also the case that Friar William of Moerbeke had 'adiutores'.

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Kepler's Cosmological Synthesis: Astrology, Mechanism and the Soul by Patrick J. Boner

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Johannes Kepler has always been something of a puzzle if not a scandal for historians of science. Even when historians acknowledged Renaissance, magical, mystical, Neoplatonic/Pythagorean influences, they dismissed or minimized them as due to youthful exuberance later corrected by rigorous empiricism and self-criticism. The pressure to see Kepler as a mathematical physicist and precursor to Newton's synthesis remains seductive because it provides such a neat and relatively simple narrative. As a result, the image of Kepler as a mechanistic thinker who helped to demolish the Aristotelian world view has prevailed—and this despite persuasive characterization of Kepler as a transitional figure, the culmination of one tradition and the beginning of another by David Lindberg [1986] in referring to Kepler's work on optics and by Bruce Stephenson [1987, 1–7] in discussing Kepler on physical astronomy.

In this brief study, Patrick Boner once again challenges the image of Kepler as a reductivist, mechanistic thinker by summarizing and quoting passages of works and correspondence covering many of Kepler's ideas, both early and late, that confirm how integral Kepler's animistic beliefs were with his understanding of natural, physical processes. Among Boner's targets, Anneliese Maier [1937], Eduard Dijksterhuis [1961], Reiner Hooykaas [1987], David Keller and E. Brummer [2002], Carolyn Merchant [1989], and Max Oelschlaeger [1991] stand out.

In a brief introduction, Boner summarizes the chapters in the book, with chapter 1 providing preliminary remarks emphasizing the continuity in Kepler's cosmology and the indispensability of vitalistic agency for Kepler's

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mature conception of cosmic harmony. In this context, Boner discusses Kepler's notion of 'aspects', by which he meant mathematically meaningful configurations, a geometrical proportion or geometrical harmony, formed by two or more planets to which the soul of the Earth responds [33–37]. An aspect is a relation of terms, a being of reason, not a substantive thing in itself, for it is a geometrical connection between the light rays of two planets here on Earth. In other words, celestial harmony belongs to the Earth and in Kepler's version this meant, of course, a moving Earth. One cannot overestimate the importance of this conception for Kepler's understanding of astrology and how this doctrine contains in embryo the guiding principle behind Kepler's reform of astrology.

With chapter 2, Boner proceeds with a more chronological ordering.[1] Kepler's early career in astrology (1594–1599), the subject of chapter 2, rehearses the complexity in Kepler's evaluation of astrology. A selective reading could easily mislead one into thinking that Kepler saw no value in astrology whatever. His brutal critique of predictive astrology and its practitioners obscures Kepler's acceptance of a physical or natural philosophical connection between the planets and Earth, with astronomy and astrology sharing the same *metaphysical* foundations in geometry. According to Boner [42], 'Kepler applied geometrical principles to the two by way of analogy'. Boner understands 'analogia' in the sense of a 'method of reasoning from parallel cases'.

> [A]ll material phenomena, from the motions of the planets to the effects of the heavens on the weather to the production of particular melodies, derived from the same singular set of geometrical principles. Seen in this way, astronomy, astrology, and music shared the same archetypal origins.

In my view, this is the philosophical crux of the matter, namely, what Kepler understood by 'analogy', 'geometrical principle', 'archetype', and 'harmony'. The relation between archetypical principles in nature and the principles already present to our intellect suggests a kind of Platonic remi-

[1] The reader should check the dates of works cited since, on occasion, Boner reaches back and projects forward, citing Kepler's works by reference to the modern edition by Caspar and Van Dyck [1937–], which makes for brief footnotes but leaves readers to check the date of the text's composition or first publication in the bibliography. The author could have saved the reader some trouble here by including the original date of publication in brackets.

niscence found in the *Meno* but without the doctrine that the archetypical principles recollected have a being separate from nature: for Kepler, these principles are *in* nature.

Boner's treatment of metaphysical archetypes, geometrical principles, and harmonic proportions, and of their connection with Kepler's interpretation of astrological aspects is persuasive. The aspects do not determine but rather shape the daily activities of individuals by 'stamping' an original imprint on the soul at the moment of birth. Useless for purposes of prediction, they serve an explanatory function. What his astrology focuses on, then, are 'the causes of aspects' [63]. Typical of Kepler, from what we know of his astronomy, he tested these ideas against observation.

Chapters 3 and 4 summarize Kepler's reactions to new observations that coincided with the composition of the *Astronomia nova*. Already analyzed and given some emphasis by Rabin [1987, 1997], Kepler's *Treatise on the New Star* [1606] took the appearance of a new bright star in 1604 as an opportunity to explain it 'according to his new and causal astronomy' [71]. The star appeared close to the conjunction of Mars, Jupiter, and Saturn shortly after the beginning of an astrological period known as the Fiery Trigon. Even as Kepler denounced the astrological interpretations of others, he saw the appearance of the star in conjunction with Mars, Jupiter, and Saturn in the fiery signs of Aries, Leo, and Sagittarius as a sign of divine intervention. To Kepler, the event provided an example of why God had set Earth in motion around the Sun, namely, as he expressed it more explicitly in 1610, to allow us to survey the heavens and by triangulation make measurements from the separation of Earth's stations [73].

Beyond that result, however, Kepler used the event to defend and reform astrology [76]. The context is the devastating attack on astrology by Pico della Mirandola in his *Disputationes adversus astrologiam divinatricem* of 1496. The reaction to that critique is now the subject of a highly controversial account of the origin of the Copernican theory and the complex developments of the long 16th century [Westman 2011]. I comment on Westman's study here only to the extent that it relates to Kepler's reading of Pico and his effort to reform astrology. The central question is the explanation for Copernicus' decision to formulate/adopt the heliocentric theory. Many of us are satisfied that Copernicus formulated his theory in reaction to some problems with geocentric astronomical models and assumptions that geocentric theories could not resolve. There are disagreements about which problems but defenders of this approach are satisfied that the explanations are adequate to account for Copernicus' formulation of a heliocentric alternative. Westman and others consider such accounts to be underdetermined because they do not supply a sufficient explanation. Pico's critique, according to Westman, made the problem of the unique ordering of the planets not just an astronomical problem but constituted a threat to astrology.

Whatever problems of detail there may be, the real question here, in my view, is whether a weakness in geocentric theory was sufficient to explain Copernicus' theory or whether something more imminent, concrete, and relevant to the role of the stars in the comprehension of the cosmos and of the human relation to it was at stake. Here is not the place to discuss this issue with regard to Copernicus further. There is no doubt in Kepler's case that he reacted to Pico's critique of astrology. Four chapters of the *De stella nova* defend astrology from Pico's critique by reforming it in accord with his emphasis on aspects and natural correspondences as opposed to those he regarded as purely cultural and coincidental. Kepler departed from Pico in affirming the influence of sunlight directly on Earth and indirectly by reflection from the other celestial bodies. The influence here, however, he attributed more to a kind of terrestrial sense organ in the souls of Earth and human beings which by 'a divine instinct' allowed terrestrial souls to recognize configurations in the heavens. 'Kepler considered this sudden correspondence of external appearances with the internal archetypal principles of the soul a reawakening' [83].

Although Boner makes no reference to Plato here, the resemblance to the Platonic doctrine of recollection seems unmistakable, interpreted, however, mathematically not just as an allegory but as a power in souls to identify 'order and proportion' in sensible harmonies by reference to their own archetypical principles [83]. This explanation comes from the later *Harmony of the World* but already in his *De stella nova* Kepler refers to the archetypical principles as part of a spiritual formative faculty and seminal reason. Kepler's empirical bent, however, pushed him to seek physical confirmation and he thought he could find it in weather conditions.

Boner struggles with Kepler's analogies and their relation to reality but there can be no mistaking Kepler's belief that the archetypical principles stamped on the soul of Earth triggered the Earth soul's sensitivity to celestial configurations and astrological aspects. The communication is formal, 'expressed in the language of the geometrical archetypes' [90]. The correspondence between evidence and geometrical polygons is a well-known feature of Kepler's cosmological vision. Yet, at times, Kepler suggests a physical and causal relationship between the celestial and terrestrial. The famous account in his *Mysterium cosmographicum* [1596] of the regular polyhedra and the number of planets might be taken as merely explanatory (*ratio numeri planetarum*). But Boner interprets 'ratio' as causal: 'Kepler positioned the polyhedra among the planets in order to determine the physical structure of the cosmos' [93]. They are explanatory but they are evidently more than that. Kepler believed that the ratios determine the structure of the universe. In his *De stella nova*, however, Kepler elaborated the way in which metaphysical archetypes produced new forms and celestial novelties by means of a kind of natural faculty in the celestial ether, again relying on anatomical analogies. Likewise, he thought that the soul of Earth had a natural faculty similar to the one in the celestial ether. Boner interprets this as 'another dimension of Kepler's "integrated physics of the heaven and the earth,"...' [94]. Even though the natural faculty acted everywhere, Kepler did not homogenize ether and air, and so material differences remained even as processes generated new forms according to the same underlying principles.

Likewise, although he affirmed the role of God and the appearance of the new star as a sign *of* God, he rejected or resisted almost every interpretation of the new star as a sign *from* God with some determinate political significance. For Kepler, it provided an opportunity for individuals to reflect on their spiritual condition. The new star was the result of divine providence and a sign of our weakness and dependence.

The appearance of comets in 1607 and 1618 evoked from Kepler conjectures about the natural effects of the comet, mostly of a meteorological kind. Yet, following Tycho, Kepler regarded the comets as celestial objects, the motions of which, however, he interpreted heliocentrically, that is, as affected by Earth's diurnal and annual motions. The most controversial feature of his first report concerned the suggestions that comets could pass into and out of existence, and that the heavens are corruptible, an idea that the theologians at Leipzig found objectionable.

Kepler did not deny altogether astrological influences on one's character but in keeping with the principle of a general, not a special, divine providence.

Three comets appeared in 1618 which Kepler again interpreted as a call to reflect on the human spiritual condition. In considering specific predictions, the lesson that Kepler drew is that such events, in that they are consistent with general providence, were, in fact, warnings against the danger of specific predictions. They were *retrospectively revealing*. These were divinely caused events intended for the human race but not for the foolish reasons concocted by most astrologers.

Kepler seems to have thought that we could account for the location and motion of the comet of 1607 only 'by supposing the motion of the earth' [121]. It is doubtful that he regarded his argument as proof but he presented the evidence from this and the comets of 1618 as more consistent with the heliocentric theory. His account was furthermore coherent with his beliefs about the finiteness of the universe and its material constitution as 'fluid and everywhere penetrable' [124].

Finally, Kepler applied his ideas about geometrical archetypical principles to comets. Comets 'followed a course according to architectonic principles that were realized by a natural faculty and recalled the essence of the divine author' [128]. It followed that he saw a natural connection as well between a comet and an internal terrestrial faculty that affected weather, a connection which required careful observation of correlations.

Kepler was, however, more cautious about the celestial substance on which the natural celestial faculty acted. In his *Apology for* The Harmony of the World [1622], the subject of chapter 5, and in correspondence related to it, Kepler returned to his theory of aspects that he linked both with his geometrical archetypes and with the physical causes of motion. The Sun played an important part in the Earth's daily and annual motions and here Boner repeats Kepler's well-known views about magnetic dispositions and faculties. Boner uses Kepler's *Epitome of Copernican Astronomy* [1618] and *Astronomia nova* [1609] to fill out his account, especially regarding Kepler's critique of Robert Fludd [139–158]. It is of relevance here because Kepler's astrology 'continued to center the stars on earth even after he put it in motion at a point away from the center of the cosmos' [152]. The stars do not act on us themselves for it is Earth that determines the efficacy of the aspects. The Earth draws an impulse for its activity of producing and exhaling vapors that influence the weather 'by relating the configuration of the heavens to an internal archetypal constitution' [153]. A soul is a circle and the regular plane figures derive from the divisions of the circle. Angular separations of two or more celestial bodies correspond to the angles of the vertices of regular plane figures. The archetypical figures are only the ones that can be constructed with a compass and ruler because they express rational proportions with the regular plane figures determining certain harmonic proportions. Using scholastic philosophical language, Kepler describes the geometrical principles as acting *objectively*, that is, as a things of reason that act on the soul. These principles lay in the mind of God from eternity; and when the sublunar soul discerns the archetypical principles from the appearances of the aspects on Earth, it discerns the very essence of God.

Kepler mentions explicitly Proclus' references to *anamnesis* or recollection. The soul, as if asleep, awakes when it identifies archetypical principles in sensible things. The soul, then, is an exemplar of the Creator and on the soul the archetypes were inscribed from the beginning. Of the infinite number of constructible figures, only 12 are congruent—those that emerge in some bodily form—and these 12 underlie 12 of the aspects that Kepler accepted as influential. Kepler's refinements in the *Harmony of the World* led him to acknowledge that aspects and consonances originated from the same set of geometrical principles but in different ways, the aspects relying completely on the circle and consequences on the straight line measured by the side of an inscribed polygon. It is noteworthy here that Kepler criticized Robert Fludd's numerology for attributing causal powers to abstract numbers, perhaps indicating a departure from Platonic and Pythagorean influences.

The book contains a bibliography with indexes of persons, places, and subjects; but the index of persons refers to individuals mentioned in the body of the text and only rarely to those mentioned in footnotes, leaving readers to locate authors cited only in the footnotes for themselves.

Conclusion

The point of this study is to demonstrate that Kepler's theory of world harmony and his system of celestial physics did not preclude the consideration of vitalistic principles in his synthesis of astronomy and natural philosophy. For Kepler, the celestial novelties of the early 17th century and the fact of celestial change and terrestrial reactions required explanation, which he took up by appealing to a sublunar soul possessed of animate faculties, thus making astrology, as Rabin [2010, 63] has already argued, 'an integral part' of his cosmology. As Boner suggests [170], there is an epistemological theory underlying Kepler's vision, the presence of universal principles in the sensible world that a terrestrial soul can recognize. The soul plays a powerful analogical role in the new cosmology. The vitalistic analogies are not just empty metaphors but a fundamental form of knowledge. In his conclusion, Boner stresses the role of vitalistic principles in Kepler's system and the role of the soul as a source of analogy and metaphor in Kepler's philosophy.

Remaining questions

The author has reconstructed features of Kepler's thought that earlier generations of historians have largely discounted, buried, or neglected. Here and there in Boner's account, however, and especially in the last two pages, one glimpses a hint of a problem that requires deeper analysis. Kepler's use of analogy and metaphor, and what he meant by 'proportion' and 'harmony', though acknowledged, needs clarification. It is clear from Boner's own assertions that Kepler assigned an indispensable epistemological role to the recognition of geometrical principles in creation itself. In addition, the logical foundations of his philosophical views require examination, especially in relation to their historical foundations. From Boner's own citations, we know that Kepler relied on Proclus and one suspects that Kepler also absorbed selectively some Platonic doctrines, that were mediated very likely by other sources that were Neoplatonic or Stoic [here Barker 1991, 1997 are cited] as well as by the Medieval and Renaissance interpretations and elaborations of these sources. In other words, thanks to works such as those by Simon [1975, 1979], Field [1988], Stephenson [1987, 1994], Martin [2011], Kozhamthadam [1994], Rabin [1997, 2005, 2010], and now Boner, scholars are perhaps in a position to reconstruct the epistemological and logical foundations of Kepler's vision of cosmological harmony. Boner appears to possess the textual resources to attempt such a reconstruction or, at least, to point us in the right direction.

Although brief, this study constitutes a significant contribution to a more complete and comprehensive picture of Kepler and of early modern science.

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Artisan/Practitioners and the Rise of the New Sciences, 1400–1600
by Pamela O. Long

Corvallis: Oregon State University Press, 2011. Pp. xii+196. ISBN 978–0–87071–609–6. Paper $22.95

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Pamela Long's latest book, a fleshed-out series of lectures that she gave as the visiting Horning Professor of Humanities at Oregon State University in 2010, comprises yet another impressive collection of scholarship and helps develop our understanding of early modern technology and those who made it. For those who know Long's earlier work, this serves as an updated bookend of her ongoing arguments about the role of books in the transfer of knowledge and the making of authority in the early modern world. It goes farther than her *Openness, Secrecy, Authorship* [2001] in that it begins to get at the relationship of artisans and nature, a relationship enfolded in the changing knowledge of the 15th and 16th centuries, that is, in Humanism and the rise of what we now call Baconian (empirical) science. As she puts it, hers is a clear argument that 'artisans [did] influence the methods of the new sciences' [127] and thus an argument in favor of the Zilsel Thesis (and, incidentally, for the Merton Thesis as well). The book, however, does show its origins as guest lectures for non-specialists in that Long has to rehearse the field in order to engage it. In such a small work, one might wish for greater engagement.

First, it should be said that this is a book modest in size but grand in vision. The main text, which is only 130 pages in length, offers a historiographical survey and chapters on three substantive topics, each chapter being so densely packed and moderately illustrated that it has only about 30 pages to develop its arguments. At times, there is a tension within a chapter, a vacillation between making a strong argument and providing a bibliographic survey (often describing in some great but disproportionate detail neglected treatises that Long wishes to highlight), with the consequence that fascinating insights often seem to pass by almost as asides. At the same time, Long is

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constrained by virtue of her project to 'just getting going' on topics that we know are dear to her (e.g., obelisks) and then truncating the discussion with a pithy conclusion. In the process, if one side of the story is missing, it is that of the non-learned participants whose views are most difficult to unearth. Consequently, while the arguments are reasonably fleshed out, they are not deeply examined. That is certainly acceptable, since the book's stated goal is not to be an exhaustive archival investigation of artisanal practice (more on this concern anon). Still, the need to cover so much ground and the attendant need to cite sources, especially the *printed* treatises of the period, left me with one major concern: though Long wants to argue about artisans' views of nature and production, the body of evidence that she uses is overwhelmingly from non-artisans. To overstate the case: it is like asking the 1% what they think about the 99%—and we know (or think we know) how that would work out. This is not to say that in either situation we fail to get an overall view of the terrain. But, we do not, I think, get down to the real details of artisanal practice.

But this criticism should not obscure the fact that this is an excellent introduction to the field and exactly the book that I would give to graduate students or advanced undergraduates in a survey course of the history of technology of early modern science in order to engage in the current scholarly debate about knowledge, epistemology, and practice. For the key component of *practice*, though, one would certainly need to go further and more subtly to make headway. This is exactly how the book could be useful as a grounding for research papers and projects.

The title of the book holds the key to one element of Long's argument that she herself does not foreground: she uses the term 'artisan/practitioner' to describe a class of skilled artificer in early modern times as a conscious way to move the discussion beyond, for example, E. G. R. Taylor's 'mathematical practitioner'. She extends that category more broadly (beyond just mathematics) and down the ladder as well. Having to use such a clunky locution as 'artisan/practitioner'—so clunky that I am immediately motivated to contract it to an acronym; but 'A/P' would be even worse[1]—highlights what a tough task this is going to be. That there is no word for these people—'artisan' is not enough nor is 'practitioner' or 'crafts(wo)man', 'artist', or even a phrase with some adjective modifying any of these—demonstrates that the

[1] Note that it must have a virgule, not a hyphen

divide that Long is seeking to bridge is apparently an intractable part of our language and an unbridgeable conception of vocational denotation. As she puts it, 'Shoemakers and university professors still lived and worked worlds apart in the late sixteenth century, as they had in the twelfth' [128]—to which I would add that they still do and perhaps never have not.

Long's 'artisan/practitioners' include the breadth of 'men and women who worked with their hands in craft production' such as 'carpenters, weavers, instrument makers', farmers, and navigators [4]. In effect, the artisan/practitioner is almost anyone who works with his or her hands, though perhaps slightly more restricted than that: those who do not maintain autonomous control over their creations (e.g., stable boys, farm hands, and carters) are probably excluded. Long is making an argument about skill, the physical world upon which people ply it, and ultimately to how their understanding of that work fed back into the Scientific Revolution. This might seem like a tall order given that she appeals to Copernicus, Galileo, and Newton in her opening pages. Indeed, one criticism of the book would be that she does not manage to close the gap between those two realms fully. Her arguments, however, help us to see how that gap can at least be narrowed and may in some cases be even bridged by the thin sutures of the diverse understandings of nature held by artisan/practitioners.

In attacking her problem, then, Long resorts to a form of study at which she is so masterful: extracting readings about artisan/practitioners from manuscript and published treatises on the manual arts from the 15th and 16th centuries and tying those to the products made by her artisan/practitioners. She pays especial attention to those who rose in the ranks high enough to leave traces of their work in those treatises (raising that thorny question of how representative the Leonardos, Fillaretes, Fontanas, or Michaels of Rhodes really were). Her argument seems to be that Humanism provided the truss-work to bridge the divide in that it encouraged elite authors to pay attention to the mundane world and practices as well as inspiring her artisan/practitioners to seek discourse above/beyond/outside their sociocultural circles. Long would seem to credit this to the rise of courtly patronage both for the arts, which it had always supported, and for scholarship, which had long been the domain of ecclesiastics, a point that deserves explicit statement and emphasis.

The first chapter is crucial for the understanding of the entire historiography of the scholar-craftsman debate, though for those for whom 'historiography' is a dirty (or at the very least, dry) word, the choice to open the book with this topic may seem odd and/or dangerous. Most academic books bury historiography within topical chapters or append it to their work but Long courageously opens with an extended analysis of where the 'Zilsel thesis' came from, who Edgar Zilsel (1891–1944) was, his influences, where he taught, and later manifestations of the thesis. It is a dense chapter but I found myself thinking throughout this chapter, 'Oh, so that is how they are connected!' and her explication not only of the various scholars' intellectual positions but also of their personal histories and affiliations helped to make sense of the nuances in their theoretical frameworks. To understand, for example, that Zilsel, Hessen, Borkenau, and others were not just Marxist historians (as one might find in any social sciences department today) but rather self-declared Marxists who undertook historical study in order to develop and critique their contemporary society helps one to understand why their focus on the proletariat was not only novel and interesting but also empowering to their program.

Chapters 2–4 are the core of the work's early modern history. They cover, first, the rise of empiricism in the investigation *and* manipulation of nature for purposes of craft; second, the intersection of artisans and humanists by using the very broad case study of the influence of Vitruvius; and finally, harnessing the idea of 'trading zones' to suggest how these influences and attitudes circulated. That last concept—circulation (or 'production and exchange' as it is described in ch. 4)—seems to be the key idea that Long wants us to understand and encourages us to investigate. It is not just how A influences B but how B, having been influenced, affects A (and generates C, D, and E) to change the entire culture. Knowledge of nature and the mechanical arts thus become a sort of intellectual currency and the exchange rate tips in its favor as new consumers start 'purchasing' new ideas and artifacts. This circulation may happen on an immediate timescale at an arsenal, for example [see ch. 4], or over time as print editions of treatises circulated and new editions were developed. Daniele Barbaro's edition of Vitruvius' *De architectura* [121–123] is a good example of this latter situation. Long shows how he collaborated with Palladio to generate both his commentary on Vitruvius as well as Palladio's *Four Books of Architecture*. Both men demonstrated a noted attention to the crafts and thus brought the high theory of architecture more into contact with the building trades. Long, first and foremost a scholar of books, is much more attentive to practitioners who try to raise themselves into the literate sphere, though there is a great deal of work to be done on the inverse process (as, for example, when Emperor Maximilian I proves to be an avid woodturner). Long also misses a great opportunity to 'close the loop' as we say these days in assessment, in that she might well have also noted that a decade before his *M. Vitruvii de architectura* Barbaro had worked on editions of Aristotle and a *Compendium scientiae naturalis* (1545), both by his great uncle Ermolao Barbaro (1453/54–1493), an instance of the full connection between the artisan/practitioner, the humanist, and the Aristotelian *cum* Zilselian natural philosopher. In fact, Ermolao may be more important than usually recognized since he began the active critique of ancient empirical knowledge in his *Castigationes Plinianae* (1492) by pointing out thousands of errors in Pliny's *Natural History* in much the same way that Thomas Browne did later during the Scientific Revolution in his *Pseudodoxia epidemica or Enquries into Very Many Received Tenets and Commonly Presumed Truths* (1646).

The last chapter is the most convincing and relevant to this reviewer. In it, Long does a bit of her own circulation of ideas by borrowing Peter Gallison's idea of 'trading zones',[2] an idea that he developed to talk about microphysics and the researchers working in modern theoretical and experimental physics. (This idea was itself transferred from science and technology studies by people like Bruno Latour and derives from the archaeological literature of *êntrepots* and the history of colonial trading ports like Portuguese Goa in the 15th century or Swedish Birka in the 11th or Danish Hedeby/Haithabu or Ribe in the eighth). As such, it is a fairly straightforward application of an existing concept in the history of science. But Long nicely gives some examples of particular cities or areas within cities (arsenals, for example) which functioned as trading zones where artisans, practitioners, artisan/practitioners, humanists, and princes and rulers—oh, and do not forget the clerks—intersected on specific technical undertakings, thus learning to speak their own and each others' languages.

It would be worth considering, though, what it was about these 15th- and 16th-century trading zones that catalyzed the revolution in empirical science, when similar trading zones (e.g., medieval cathedrals, Roman *fabricae*, or

[2] See Gallison 1997 and the extension of this idea in Collins, Evans, and Gorman 2007.

even Egyptian building yards) had not done so in the past. It is clear, though, that more modern industrial settings such as iron foundries, chemical factories, aircraft assembly plants, and, consistently, military arsenals have indeed served quite admirably as trading zones in the way that Long describes. The one seemingly forced element of the chapter is her attempt to make printed treatises such as the early modern editions of the *De architectura*, *De re metallica*, and the *Pirotechnia* into the pidgin/creole languages of trading zones [125–126]. This is an interesting suggestion but one needing more work.

If there is one general criticism that I would level at *Artisan/Practitioners and the Rise of the New Sciences*, it is that Long relies too much on printed treatises as evidence for the attitudes of the artisan/practitioners, most of whom were most certainly not circulating in the requisite social sphere. She sometimes remains strangely silent on the authors' rhetorical intent in their printed texts, leaving open the implication that the texts all performed similar functions. What is worse is that she sometimes conflates their purposes without proving the case, as when she claims that

> books on mining, ore processing, and metallurgy were written for princes and a far-flung group of investors...[and] set out many technical processes in written form....The books described with great clarity technical operations and equipment [and included] illustrations...essential for *making complex machinery comprehensible*, but they also made the mechanical arts of mining and metallurgy dramatically *appealing to the unskilled*. [112, emphasis added]

It would be fascinating to find a miner who needed the text or the illustration to make his machinery comprehensible (she is conflating audiences) and it is unclear how gorgeous woodcuts alone make machines themselves more appealing (she is imputing causality). In addition, it is not at all clear that the audiences for 16th-century mining texts would have been 'far-flung groups of investors', as information on specific mines of bodies of ore is rarely evident in these texts.

When one considers the book as a whole, it is very satisfying for a short book. The problem is that at times it tries to satisfy two audiences: one which has very little exposure to early modern technical treatises and another which wants to learn of the deeper connections within the topic. The latter group is clearly the audience for the opening historiographical chapter but the very placement this chapter as the first seems rather strange for a book whose later chapters are introductory. Whether this was the author's choice, the publisher's idea, or somehow a consequence of the book's being the culmination of a series of lectures is unclear. What is clear, though, is that that initial chapter may, I fear, prevent less invested readers from reaching the much more engaging and important heart of the book, which would be a shame in a survey of the state of the field that shows Long at her best. This work offers a faster entry to the topic than her book of 2001 and one that does not pursue a single argument through more than a millennia of technical treatises. Its tight chronological focus, which still encompasses work on both sides of the Alps, makes it a very useful introduction to the entire field of artisanal labor and products within the humanistic and courtly sphere of early modern Europe.

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Ethical Perspectives on Animals in the Renaissance and Early Modern Period edited by Cecilia Muratori and Burkhard Dohm

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Recent years have seen a surge in studies of the history of attitudes toward animals, not least from a social, intellectual, and ethical perspective, as well as in relevant studies of early modern Europe. Yet, as Cecilia Muratori's and Burkhard Dohm's new collection of essays proves, there remains much to be said regarding this topic. In their introduction, the editors declare their wish to approach early modern attitudes toward animals without regarding the Cartesian view of animal automatism as a central point of interpretation. More importantly, they aim to address what they see as a lacuna in current scholarship, the lack of attention to the early modern ethical consideration of real animals, not just symbolic ones, and of any attempt to understand what 'animal ethics', to use a modern term, might have meant in the past. Consequently, a central theme of the articles in the book is the consideration of rationality and speech as criteria for inclusion in the realm of justice and how the possibility of this ethical outlook on animals developed in early modern thought. The editors' claim for originality may be somewhat overstated since scholars have for some time been discussing early modern ethical views of animals. Yet this does not detract from the quality and versatility of what is an important collection.

The volume begins with Amber Carpenter's article, 'Eating Your Own: Exploring Conceptual Space for Moral Restraint', in which she discusses the classical, mainly ancient Greek, sources of philosophical attitudes toward animals, with particular emphasis on the concept of δίκη, the sense of right. It is our possession of this sense of justice, not the possibility of animals possessing it, which in Carpenter's estimation should preclude the eating of animals. Among other issues underlining human/animal relations is metempsychosis;

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yet in this context, the author regards the Buddhist approach, rather than the Greek one, as evincing a truly sensitive attitude toward animals.

Matthias Roick's 'Animals at Court: Ethical Perspectives on Animals in Neapolitan Humanist Thought' discusses three 15th-century Neapolitan figures: Antonio Beccadelli, Lorenzo Valla, and Giovanni Pontano. Beccadelli's discussion of King Alfonso of Aragon's act of saving an ass which had fallen into mud centered on the relationship of the king and his people, yet also left room for a certain sensitivity to animals in themselves. Valla rejected the hierarchical Aristotelian taxonomy of living beings in favor of a view of animals as inferior to human beings in degree, not kind. Nevertheless, Valla did not specifically pursue the ethical consequences that this entailed for the treatment of animals. Pontano discussed the attitude toward courtly animals which were maintained for the glory of rulers. Like the other two figures discussed in this article, his approach hinted at possibilities for ethical consideration of animals which were developed, however, only to a limited extent.

Gabriella Zuccolin's 'Living with Animals at a 15th-Century Court: Physiognomy, Dietetics—and Poetry' is an intricate discussion of various aspects of attitudes toward animals in the context of early modern court culture, where animals were often in close proximity to human beings. Contemporaneous attitudes toward animals both established new empirical criteria for discussing them, yet also enabled expressions of growing sensitivity toward them. These varying approaches were exemplified by the court physician Michele Savonarola, whose physiognomic writings tended to blur the line between humans and animals, while his dietetic discussions were less sensitive to animals. In courtly surroundings, it was poetry and epistolary writing that most evinced affection for animals, mainly pets.

Nicola Panichi's article, 'Montaigne and Animal Ethics', presents a specific interpretation of one of the better-known figures in studies of early modern attitudes toward animals. Panichi centers on Montaigne's cosmology and on Plutarch's influence on his sensitive consideration of animals. Montaigne's reading of Plutarch entailed seeing animals as inferior to human beings in degree, not kind, yet also as morally superior to the latter in certain respects. While Panichi does not mention the terms 'primitivism' and 'theriophily' (love of animals), the discussion basically addresses the notions underlying these concepts and their amenability to early modern critiques of human pride. One significant point which Panichi does not discuss is the self-ironic limits to Montaigne's theriophily. Montaigne, after all, proclaimed his recognition of the suffering of hunted animals while admitting that he nonetheless enjoyed hunting. This gap between theoretical and practical sensitivity to animal suffering seems not to have been a rare historical phenomenon—it is found later in the work of such prominent writers as Rousseau—and should be kept in mind when discussing philosophical attitudes toward them.

Guido Giglioni's 'Life and its Animal Boundaries: Ethical Implications in Early Modern Theories of Universal Animation' presents an interesting discussion of early modern panpsychism, monopsychism, and hylozoism, beginning with an overview of the debate between Pierre Gassendi and Jan Baptiste van Helmont regarding whether human beings were carnivorous, with the former defending vegetarianism while advocating an ethical sensitivity toward animals. Giglioni then outlines Tommaso Campanella's combination of panpsychism and anthropocentrism, and Giordano Bruno's hylozoism and view of animals as expressions of life. The discussion ends with remarks on sentience and animals in modern thought.

Cecilia Muratori's 'Eating (Rational) Animals: Campanella on the Rationality of Animals and the Impossibility of Vegetarianism' takes a close look at Tommaso Campanella's views of animals. While he distanced himself from the Aristotelian outlook and perceived nature as a living whole with a continuum of living beings, Campanella nonetheless did not deduce from this an ethical sensitivity to animals. These, and even plants, might have sensations and animals might possess rationality, though not the human mind and capacity for religious feeling. Yet precisely the ubiquity of sensation made the abstention from eating meat irrelevant. In nature, the strong dominated the weak, which made eating plants and animals permissible, although eating those too similar or dissimilar to oneself precluded immoderate eating such as cannibalism. Muratori's discussion highlights ethical points related to vegetarianism which are still relevant to modern debates of this issue.

Burkhard Dohm's article, 'Vegetarismus-Konzepte im deutschen und englischen Spiritualismus des 16. und 17. Jahrhunderts', centers on early modern spiritualism and sheds important light on the development of Protestant conceptions of animals and nature. Dohm discusses several figures beginning with Sebastian Franck, who were influenced by Hindu sensitivity to animals. Johann Arndt presented the idea of an *imago Dei* (image of God) as an argument for proper treatment of animals. Paul Felgenhauer claimed that animals shared with humans a divinely given soul and included animals in his conception of the ἀποκατάστασις πάντων (cyclical return of everything). Johann Wilhelm Petersen and his wife, Johanna Eleonora Petersen, also shared a similar outlook. Moving from German to English spiritualists, Dohm describes the radical John Everard's opposition to the slaughter of animals. Dohm closes his discussion with a detailed overview of a figure who, contrary to the others whom he considers, is familiar to those with an interest in the development of attitudes toward animals. This is Thomas Tryon, who combined a theocentric and anti-anthropocentric outlook in developing an ethics of the treatment of animals that was almost modern in view of its practical implications, even presenting an early conception of animal rights. Tryon's attitude toward animals was related, according to Dohm, to his opposition to slavery and he even recognized the deleterious implications, for both humans and animals, of air and water pollution.

James Vigus' article, '"That Which People Do Trample Upon Must be Thy Food": The Animal Creation in *The Journal of George Fox*', describes the Quaker movement's traditional ethical sensitivity to animals, yet claims that George Fox's approach to animals was less clearly sensitive, due both to his style of writing and to his use of biblical imagery and predilection for metaphors. Vigus thus implies that Fox was a moderate rather than a radical in his ethical consideration of animals.

Kathrin Schlierkamp's 'Die Kontinuität der Natur und die Verantwortung für Tiere und Umwelt in Anne Conways *The Principles of the Most Ancient and Modern Philosophy*' describes the English philosopher's un-Cartesian views of both the mind/body question and animal automatism, the idea of which she of course opposed. She combined a monistic view of the world with Cabbalistic influence, leading to a consideration of living creatures as part of a natural continuum in which animals differed from humans only in degree. This led to a plea for ethical sensitivity to animals both for their own good and for the good of those human beings who treat them.

Rhodri Lewis' article, 'Thinking with Animals in the Early Royal Society: The Case of Sir William Petty', presents an interesting case of an early modern savant who discussed the theory of the Great Chain of Being in an anti-anthropocentric manner influenced by Montaigne. Like other figures mentioned above, Petty regarded the difference between humans and animals as one of degree, not kind. He emphasized in particular the mental similarity between human beings and such animals as elephants, thus distancing himself from the Cartesian view of animals. Lewis' article is an excellent example of how modern scholars may encounter instances of early considerations of animals in seemingly unlikely places, in this case that of a thinker more often familiar to historians of economic thought and political arithmetic.

In her article, 'Das Monster als Grenzfigur. Leibniz, Locke und die Tier-/Mensch-Mischwesen der Renaissance', Urte Helduser discusses a topic different from that of the other articles in the volume, that of the implications which early modern attitudes toward monsters had for the view of animals. In particular, the birth of 'monstrous' human beings posed a challenge to the early modern conception of human singularity vis-à-vis animals. Helduser gives a detailed outline of the development of early modern attitudes toward the phenomenon of monstrous births and depicts the outlooks of Leibniz and Locke as a turning-point in problematizing the ability to differentiate clearly between the human and animal aspects of such 'creatures'. This led both these prominent philosophers to an increasingly benign approach to the question whether to let such unfortunate 'monsters' live. Helduser emphasizes Leibniz in particular in this respect and also notes that this approach became more common in the 18th century. Historians of early modern attitudes toward animals are familiar with the views of these two philosophers, though not necessarily with their outlooks on this particular issue.

The volume ends with Gianni Paganini's article, 'Political Animals in Seventeenth-Century Philosophy: Some Rival Paradigms', which discusses the views of animals of Pierre Gassendi and Thomas Hobbes, both of whom disagreed with the Cartesian theory of animal automatism. Gassendi, in Epicurean fashion, maintained human superiority to animals due not least to the latter's lack of language and the consequent inability for political life. Hobbes perceived a more gradual difference between humans and animals. For him, animals had the ability to gather together, yet not in the sense of a political covenant. Possibly influenced by Montaigne, Hobbes, however, connected this seemingly superior human ability also to the propensity for moral decline. Paganini's discussion presents a different aspect of Gassendi's view of animals than that discussed in the article by Guido Giglioni.

All in all, this is an impressive collection of essays that sheds new light on themes both familiar and less so to scholars of early modern attitudes toward animals. The articles all exhibit a high level of erudition and are written in an accessible and engaging style. Those addressing familiar figures present new interpretative perspectives on themes previously discussed in earlier scholarship, for example, Montaigne's views of animals or early debates about vegetarianism. Other articles discuss themes and figures much less familiar to historians, thus making a very tangible contribution to scholarship. The attempt by most of the authors to connect their historical discussions to modern issues regarding the treatment of animals is also pertinent and does not overreach the limits proper for historical scholarship, as occasionally happens in studies of this topic. The following remarks are therefore made not so much as criticisms but rather as constructive suggestions for further research.

One specific lacuna in the book is the almost complete neglect of the history of science. Someone coming to this volume without a familiarity with the history of attitudes toward animals might get the impression that the development of ethical sensitivity to animals was a purely philosophical affair. As scholars, however, are well aware, early modern scientists often grappled with the ethical complications of their experiments, specifically some of those who engaged in vivisection. Yet this topic receives practically no attention throughout the volume and the authors, whether intentionally or out of ignorance (the former seems more likely), disregard the large literature of studies of this topic by various scholars, notably Anita Guerrini [see, e.g., Guerrini 2003].

Another lacuna is the lack of proper attention to literary and artistic sources. This is no doubt intentional but the ubiquity of animal figures in early modern literature and art also had clear ethical implications. A striking case, begging attention, is presented by the editors themselves, who include only one example of a reproduced painting as a frontispiece to the volume, that of Pieter Bruegel the Elder's *Two Monkeys in Chains*.[1] Bruegel's painting, an important example of the rising attention to animal themes in early modern iconography, raises many potential points of interest, not least the question whether the pictorial depiction of animals differs from verbal, and specifically philosophical, considerations of animals, thus highlighting a different aspect of changing ethical sensitivity toward them. The editors, however, like most intellectual historians, seem content with using art solely for the purpose of simple illustration.

[1] The illustrations in Urte Helduser's article are less significant in this respect and simply exemplify the visual dimension of early modern fascination with monsters.

In this volume, one also finds the regrettable divide between European, mainly continental, scholarship and English-speaking, not least American, scholarship. Some at least of the authors of the various articles in this volume are aware of the important work done on early modern attitudes toward animals by scholars such as Erica Fudge, Gary Steiner, Aaron Garrett, and Peter Harrison, not to mention Keith Thomas. Yet these and other scholars are insufficiently mentioned and do not receive the attention due to their often ground-breaking work on the history of early modern attitudes toward animals. On the other hand, the volume is replete with references to studies in languages such as German and Italian which are rarely mentioned in English-language scholarship. I must admit to being surprised at the number of such references, the existence of which I was previously unaware of. As in other scholarly fields, it seems that Anglo-Saxon and European scholars are unwittingly interested in similar topics but often ignorant of comparable work being done by contemporary scholars writing in other languages.

In the same vein, this volume gives relatively little attention to an important body of work, mainly written in the United States in the second quarter of the 20th century, which established much of the modern study of early modern attitudes toward animals. George Boas' *The Happy Beast in French Thought of the Seventeenth Century* [1933] is mentioned a couple of times, though insufficiently; but there are no references to Dix Harwood's *Love for Animals and How It Developed in Great Britain* [1928], Leonora Cohen Rosenfield's *From Beast-Machine to Man-Machine* [1940] (highly relevant despite centering on the Cartesian view of animals which Muratori and Dohm intended not to be central to the volume), or Hester Hastings' *Man and Beast in French Thought of the Eighteenth Century* [1936] (the latter admittedly about a slightly later era than that discussed in the volume). These are old works but still highly relevant to scholarship today and often not given their proper due as modern scholars (writing in all languages!) attempt to assert their interpretative originality. Another highly important book not mentioned at all in the volume, though not specifically about animals but still very much relevant to understanding the history of attitudes toward nature in general, is Clarence Glacken's justifiably famous *Traces on the Rhodian Shore* [1967].

It would be an injustice to this volume, however, to overemphasize such shortcomings. One cannot expect every aspect of the history of attitudes toward animals to be addressed in one volume and, as a work addressing mainly the intellectual facets of this topic, *Ethical Perspectives on Animals in the Renaissance and Early Modern Period* is an excellent collection of essays of a very high quality. The editors and authors have all done a remarkable job in enhancing our understanding of the development of human attitudes toward animals. The result is a volume which should interest all serious scholars of the history of attitudes toward animals and indeed of intellectual history in general.

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Illustrating the Phaenomena: Celestial Cartography in Antiquity and the Middle Ages by Elly Dekker

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During the last few decades, there has been a growing interest in the study of the material culture of ancient and medieval astronomy, and Elly Dekker's *Illustrating the Phaenomena* comes to fill a gap. It is an impressive and thorough account of 16 extant celestial globes and 40 celestial maps from Antiquity and the Middle Ages. Despite the fact that most of the globes and maps discussed in this book have been presented previously, Dekker gives us a well-rounded account of them. She describes the artifacts collectively, providing the cartographical details of each and the manner of its construction as well as a general comparison of the artifacts to another and to a theoretical model. This allows for an appreciation of their importance, as illustrated by the fact that, as the author points out, globes and medieval maps are significant artifacts that were used until the 15th century to illustrate such books as Aratus' *Phaenomena*, which describes the constellations and their myths.

There is no doubt that Dekker's volume will become a standard reference book on globes and maps from Antiquity and the Middle Ages and will be the starting point for scholars who want to study such artifacts further. The book divides the globes and maps into those that follow the descriptive tradition and those that follow the mathematical tradition. In the descriptive tradition, the stars are located according to their position within constellations; while in the mathematical tradition, they are located according to a set of coordinates. Dekker also picks up specific features of the globes that have to do with their dating, construction, or categorization, first presenting the pertinent bibliography. She then either adopts the most common position in the historiographical debates or goes in depth to give her own opinion or concludes that there is no definitive answer to the point at issue.

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The book opens with some preliminary remarks on astronomical concepts. Dekker takes us through the constellations as first described by Aratus, Eratosthenes, Hyginus, Eudoxus, Hipparchus, and Ptolemy. She then introduces the astronomical concept of the two-sphere model, the basis for understanding the celestial phenomena visible to the naked eye since the fourth century BC. She continues by explaining a number of the circles and concepts that the ancients used to understand the celestial motions, such as the ecliptic or zodiacal circle, precession, colures, and the epochal modes.[1]

The last part of the first chapter is dedicated to what one should know in order to make a globe and to draw constellations on it. The ancients described the stars in constellations by following (most of the time) Hipparchus' rule, according to which the stars are to be described from our point of view here on Earth as if they are facing us and the left and right sides of the constellation's outline are fixed. To draw the constellations on a globe, however, the order of left and right was reversed at least for human or animal images: in effect, globe-makers drew mirror image of what we see in the sky. Historians used to believe that all the ancient globes displayed the constellations from the rear, that is, as mirror images.. But, as some recently discovered globes have made clear, that is not true. The ancients drew the constellations on globes both in sky-view and in rear view—that is, with the observer inside or outside the globe—and sometimes even on the same globe. According to Dekker, the same is true of ancient descriptions of the constellations in texts.

At the beginning of the second chapter, Dekker describes in detail the few extant celestial globes: the Kugel, the Mainz, and the Farnese globes. She also discusses the Salzburg fragment, the Berlin fragment, the Larissa globe—of which only a picture remains today—and Hyginus' globe, which is only known through Hyginus' *De astronomia*. The Kugel globe is the smallest of the three and, according to Dekker, it follows the older Eudoxan tradition

[1] The zodiacal circle is the oblique circle defined by the annual motion of the Sun on the celestial sphere; it runs through the middle of the zodiac or zodiacal band, which lies between the two tropic points. Because precession did not really play an important role in ancient astronomy, there were many different conventions regarding the starting points of the zodiacal signs (30°-segments of the zodiacal circle named after the zodiacal constellations). The colures are defined by the celestial poles, equinoxes, and solstices; and the descriptions of what the colures are and how they are positioned with respect to the constellations are called epochal modes.

in globe-making. The presentation on it of the constellations in images that mirror what we see in the heavens

> adds greatly to the present knowledge of early globe-making and shows that the making of mirror-image globes was not the prerogative of Islamic globe-makers but has its roots in Greek globe-making. [69]

The Mainz globe, on the other hand, shares a number of anonymous star groups with Kugel's globe, groups which were known from Aratus' *Phaenomena*; but mythology plays a more important role on it than on Kugel's. What nevertheless stands out even more on the Mainz globe is the outline of the Milky Way as a broad band whose features appear to follow closely Ptolemy's description. Such a correspondence between Ptolemy's account and the Mainz globe is indeed surprising when considering how inaccurately the constellations are located on the Mainz globe. It suggests, in Dekker's opinion [79], that

> the Milky Way on the Mainz globe ultimately goes back to a map of the globe in the mathematical tradition, although that does not apply to the globe as a whole.

The Farnese Atlas is another extant globe whose date remains elusive to modern scholars. In fact, Dekker, who addresses at length the issues of dating, accuracy, and a possible Hipparchan origin of this globe, concludes that she is

> inclined to accept that—although the Farnese globe contains no actual stars, the circles on the globe are drawn inexactly, the dating of the globe is uncertain, and its sources controversial—the Farnese globe is closest to what remains today of the early mathematical tradition in globe making. Unless new information is discovered, it will remain hypothetical whether that tradition started with Hipparchus or not. [101]

In the third chapter, Dekker examines for the first time 33 celestial maps such as the Revised Aratus Latinus that have survived in medieval illustrated manuscripts (9th–15th centuries). All the maps belong to the descriptive tradition and can be divided into three groups:

(1) pairs of summer and winter hemispheres, that is, hemispheres presenting the winter and summer skies at a given location;

(2) planispheres presenting the whole sky in sky-view (the order of the zodiacal constellations is clockwise and you see the Milky Way) and in globe-view (the order is counterclockwise);[2]

(3) sets of hemispheres that show the heavens north and south of the celestial equator.

Dekker describes in detail how these maps were constructed and suggests that there was what she calls a 'hemispheric model' according to which the grid in the hemispheres must have been drawn. Next, she tries to establish a date of construction for the artifacts, suggesting that it is the location of the equinoctial colure with respect to the stars that can be used as the criterion for dating. Last, she establishes which tradition the maps follow and how they can be grouped according to their similarities. Through her analysis, it nevertheless becomes clear that sometimes due to internal inconsistencies, deliberate adaptations, or systematic or copying errors, it is very hard to discover the relevant epochal modes for some of the maps that derive from globes. Planispheres, for example, present the celestial sphere in one piece from the celestial north pole to the ever-invisible circle and this makes the author postulate [433] that

> It was probably because of this format that for a long time it was taken for granted that these medieval planispheres are based on stereographic projection.

When Dekker examines the details of the construction of these planispheric maps, however, she concludes that they are not in fact stereographic projections: instead, she maintains, they are based on an equidistant model in which the parallel circles are drawn proportional to their distance from the north celestial pole. For the two pairs of maps consisting of hemispheres separated by the equator, it is not clear if they derive from Aratus' *Phaenomena* as do the maps discussed thus far. The detailed analysis of these two maps raises more questions than can be answered regarding the tradition that they follow. An interesting point here is that the map found in the middle of an astronomical poem may be connected to the globe-making ventures of Gerbert of Aurillac.

The transition from the descriptive Aratean to the mathematical Islamic astronomy in Europe was not immediate. This is showcased in the fourth

2 There are five copies in sky-view, five in globe-view, and 10 humanist planispheres in globe-view.

chapter on Islamic celestial cartography, which opens with the oldest artifact, the ceiling painting in the bath house of Quṣayr ʿAmra, which is believed to have been built in the first half of the eighth century. It is very hard to uncover the source for this celestial map and, despite what the extant literature claims, Dekker believes that the ceiling painting does not reflect any detail that would require knowledge of Ptolemy's *Almagest*. The author extends her analysis of the first treatises dealing with the use of globes written by astronomers from the Middle East, an activity, which, she maintains, underlines the significance of globes in education. Among the many treatises presented, the *Book on the Constellations of the Fixed Stars*, which the Persian astronomer al-Ṣūfī wrote for his patron ʿAḍud al-Dawla in the ninth century, is most interesting. In this treatise, al-Ṣūfī embarks on criticizing his predecessors' observations, especially those by Ptolemy, even though he dismisses some Ptolemaic stars because he was unable to see them. In any case, Dekker concludes that al-Ṣūfī created an amazing star atlas for the contemporary students of astronomy trying to bridge the gap between globes and the sky in the new mathematical tradition. At the end of this chapter, we read about all the other mappings found on celestial globes. Some of them follow an eastern tradition in globe-making that predates the work of al-Ṣūfī, while others clearly show the impact of al-Ṣūfī's work. It is interesting that although the earliest extant mathematical celestial globes were made in Muslim Spain in *ca* 1080, they show glimpses of an early eastern tradition in globe-making. In addition to a few Greek features and typical Islamic elements, these globes have characteristics that are seen neither in early Greek sources nor on later Islamic globes.

In the fifth chapter and final chapter, Dekker describes the Cusanus globe, the oldest extant medieval globe made in the Latin West that dates from around 1320–1340. This globe is the closest to what the author imagines a Greek model of Ptolemy's precession globe would have looked like. It raises a number of interesting questions to be followed up, including its place of origin. Around 1425, Conrad of Dyffenbach made the earliest still extant set of maps based on the Ptolemaic star catalogue using the completely new trapezoidal projection and the polar azimuthal equidistant projection, which was not finished. A more successful use of the latter projection was made in *ca* 1453 in a pair of maps which are closely connected to the Vienna globe-making enterprise, although this projection was, apparently, not yet fully understood. An outstanding feature of these Vienna maps is their iconography, which prompted all maps and globes in the 16th century to present the human constellation figures in rear-view. Only two 15th-century globes have survived: the globe made by Hans Dorn in 1480 and another made by the astronomer Johannes Stoffler in 1493. Both underline their use for astrological doctrines. In the first half of the 15th century, the first extant celestial maps in the mathematical tradition emerged and, although they might have started in Antiquity, no maps survived and that is definitely an interesting point that needs further study.

The book is equipped with many illustrations of the globes and maps discussed in detail by Dekker along with some tables and charts and also five appendices, a bibliography, an addendum, a manuscript index, and an author index.

The breadth and depth of Dekker's analysis have opened up an array of exciting issues to be pursued, one of which concerns the accuracy of the information presented on globes, a subject that the author touches upon only briefly. Questions that come to my mind are: Accurate according to whom? What do we mean by 'correct', 'wrong' or 'astronomically incorrect' in each context? Why is it important, if it is at all? Although the book does not suffer from the lack of illustrations, it would have been beneficial to add some more pictures of the fascinating artifacts that Dekker describes as well as perhaps some more subcategories in each chapter so as to allow even the total novice to dive into these complex issues. I would hope that researchers will use Dekker's excellent book as a stepping stone to expand further on the history of globes and maps, their makers, their purpose, as well as their audiences so as to understand these fascinating objects even better.

Once more: Dekker has delivered a great piece of work on celestial cartography, which together with her study on globes at Greenwich is bound to become a classic.

The Principles of Arab Navigation edited by Anthony R. Constable and William Facey

London: Arabian Publishing, 2013. Pp. xiv + 146. ISBN 978–0–9571060–1–7. Cloth $70.00

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This attractively printed, copiously illustrated work consists of nine papers by six authors, all specialists in one or another aspect of Arabic maritime history or geographical literature. We have no written records of when the principles of Arabic navigation evolved because our earliest written sources are manuals by two Arab sea captains who lived in the late 15th and early 16th centuries of our era, namely, Ibn Mājid and Sulaymān al-Mahrī, the latter making heavy use of the writings of the former. These sources and later logbooks by Arab sea captains inform the contributions to this book.

In chapter 1, the first of four devoted to Arab stellar navigation, A. Constable lays out the basic principles and problems of maritime navigation, particularly as they relate to the early history of Arab sailing in the Indian Ocean. In the context of a clear introduction to maritime navigation in general, Constable introduces some of the basic features of Arab navigation such as the *iṣba*' (the visual angle subtended by the width of a finger held at arm's length) and *dhubbān* (the width of four *iṣba*'). It is, as he emphasizes, not possible to settle on any exact value for these angles in degrees and minutes; but, he says, 'it is usually agreed' that a full circle contains 224 *iṣba*', which implies about $1\frac{3}{5}^\circ$ per *iṣba*'. He gives no source for this claim but it does mean that 7 *iṣba*' are exactly the angle between successive points on what he calls the Arab compass. This was a division of the horizon into 32 supposedly equal sectors originally defined by the rising-points (on the eastern horizon) and setting-points (on the western) of certain stars or asterisms. Thus, NE/NW were the directions of the rising/setting of Capella and SE/SW the corresponding directions for Orion's belt. The various ways in which ancient mariners could use the stars and asterisms of the night sky to find the fundamental direction, that of the pole star, even when that star was not visible, are well

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described and very impressive. One comes away from the chapter realizing that a thorough knowledge of the night sky was literally a matter of life and death for ancient mariners.

In chapter 2, Hasan Salih Shihab gives an illuminating discussion of a method used by 'the old navigators' (i.e., those living prior to the arrival of European methods) to determine what we would refer to as a change in longitude. (As with Constable's discussion in chapter 1, Shihab's exposition benefits from the excellent graphics accompanying the book.) Fundamental to the method for finding change in longitude was the *zām*, which originally meant a three-hour watch and later came to mean the distance sailed during that watch as well, so the distance sailed in a whole day of sailing was 8 *zām*. What distance this actually denoted was, however, highly variable. The distance (in *zām*) that one had to sail along a given *rhumb* (direction) to change the height of the celestial pole by one *iṣba*' (the fundamental unit of latitude) was known as the *tirfa* of that *rhumb*, and a sail of 8 *zām* was the *tirfa* for due north or south. Obviously, if one sailed any *rhumb* other than due north or south, it would require a longer sailing time to change the pole height by one *iṣba*'. In his writings, Sulaymān gives navigators, for each of the eight *rhumbs* from N to NE, the *tirfa* for that *rhumb*. (The values from N to NW are obviously identical.)

If one sailed a certain *rhumb* for a certain number of *tirfa*, one obviously departed, in an easterly or westerly direction, a certain distance from the north/south *rhumb* of one's origin. Sulaymān gives these distances, known as 'departures'. For example, for a compass setting of NE (or NW), the *tirfa* was 12 and the departure was 8, clearly reckoned on the basis of 'plane sailing'. Thus, if one had sailed one *tirfa* along a given *rhumb*, one could consult Sulaymān's book to find both the distance sailed and the departure east or west from one's original meridian. For example, one traditional value for the *tirfa* of ENE was 20 *zām* and, for the departure, 18 *zām*. (The values calculated by elementary trigonometry are, respectively, 20.91 and 19.31 *zām*.) However, each region and each tradition had its own set of *tirfa* and departure values for each *rhumb*, and these could, of necessity, be only approximate. On the assumption that these values are in fact part of a very long tradition, one wonders whether Ptolemy used such information in calculating longitudes in his *Geography*.

The repertoire of navigational techniques had expanded by the time of the mid-19th century, as Yacoub Yusuf al-Hijji informs us in his instructive chapter 3. At this time, the ancient techniques were still very much in use but a number of navigators had adopted the sextant and marine chronometer from the Europeans for voyages across the Indian Ocean and Arabian Sea, and they knew enough geometry to plot a course across these waters. The bulk of this chapter is an exposition of examples of computations from log books of Kuwaiti captains (*nakhodas*), computations involving the Pythagorean Theorem and the use of the cosine or tangent trigonometric functions. One is left wondering, however, where the multiplier '86' in the 'Rule of 86' for calculating one's bearing came from. And the same might be said for the divisor, 15, appearing in the rule for using numbers that the author calls 'star constants' to calculate departure from change of latitude. One is also left wondering, given the fact that Kuwait had no school to train captains in the new methods, how these men acquired the knowledge to use these functions. The author's suggestion that a few learned them from Indian or Omani navigators and then taught the methods to their friends who traveled with them on long sea voyages is, of course, possible—and even likely in some cases. It would have been interesting to know what impact trained navigators from such mercantile powers as Portugal, Holland, and Britain might have had in this change to modern methods.

In chapter 4, Eric Staples reports some of the lessons learned from the voyage that the ship *Jewel of Muscat* made in 2010 from Oman to Singapore. The ship, a square-rigged sewn vessel with no motor was built according to the archaeological evidence surviving from the ninth-century ship but the navigational methods were based on those of the 15th and 16th centuries. The goals were to document star-altitude measurements over the course of the trip and to experiment with different types of instruments used in those centuries and three different sorts of star-measurements.

The whole chapter is highly interesting and sheds much light on what actually happens when one uses the ancient techniques and instruments. Here it must suffice to quote the conclusion:

> It also became clear that although the Pole Star was the foundation of the star-altitude measuring system, other star combinations were taken far more often than originally assumed, due to the difficulty of Pole Star sightings. These star

> combinations, in particular the non-circumpolar combinations, do not often receive the recognition they deserve. [59]

Following these four chapters on the details of stellar navigation come five chapters devoted to special topics of a less technical nature. The first (chapter 5) is Paul Lunde's study of the maritime routes in Sulaymān al-Mahrī's *ʿUmdat al-mahrīya fī ḍabt al-ʿulūm al-bahriyya* (*Support for Grasping the Maritime Sciences*) and his *Al-manhāj al-Fākhir fī ʿilm al-bahr al-zākhir* (*The Splendid Program for the Science of the Overflowing Sea*). Lunde [63] stresses the gulf that existed between the world of such geographers as al-Idrisī, who was intent on reworking Ptolemy, and practical navigators such as Sulaymān. As evidence, he mentions that the latter knew perfectly well that the coast of Africa headed SW and did not turn to the east, as Ptolemy and, following him, al-Idrisī thought. He also cites the fact that the distances that Sulaymān gives for the routes from ports on the east coast of Africa to Javanese and Sumatran ports agree, to within a few degrees, with modern distances. This should not be too surprising, however, when one learns that 'There were well established Arab merchant colonies in South China even earlier [than AD 830]'. In addition to his informative text, along with its citations from Sulaymān's treatises giving details of deep-sea voyages, Lunde includes a good map of the voyages mentioned in the text, voyages as far south as Madagascar and as far east as Java and Taiwan. (Indeed, the book as a whole is copiously documented with maps—something this reader much appreciated.)

Following his treatment of maritime routes, the same author, in chapter 6, gives a detailed treatment of the times of the year that Sulaymān recommends for beginning various voyages. Knowledge of these times would be very important to any captain, since starting off at the wrong time in relation to the monsoons could mean a wait of several months in some port. Lunde points out that the specification of these times was complicated by the Hijri calendar, whose strictly lunar months in a year of 354/355 days fall out of phase with the seasons. For this reason, sailors on the Indian Ocean used the Yazdegird calendar of 365 days per year and an epoch of 18 June 632; but its lack of leap years caused the same problem in the long run as the Hijri calendar. For administrative reasons, the Caliph al-Muʿtadid tried in AD 825 to change the epoch to 11 June but again without adding any leap year; so the problem persisted. Finally, in AD 1079, the Seljuk sultan, Jalāl al-Dīn set the beginning of the New Year (*nayrūz*) on 15 March and added a leap day. However, it seems that the piety of the sailors exceeded their allegiance to the Sultan and that they ignored the leap day, since the Muslim faith forbade intercalation. So any given calendar date still fell back relative to the seasons by 1 day every four years. Consequently, captains had to remember to set dates back by one day every four years. And, somehow, they managed it!

In chapter 7, al-Hijji offers a critical view of certain aspects of Alan Villiers' classic work on navigation, *Sons of Sindbad*. Al-Hijji himself credits Villiers' work with inspiring him to take up the subject, and puts his criticisms of Villiers' approach in the context of providing another perspective on a classic work. He makes a good point that Villiers' assessment of Arab navigational skills was based on his experience on one coastal trip taken in 1938–1939 on a ship named *The Triumph of Righteousness*. This ship sailed from Aden to a locale slightly south of Dar es-Salam and then back up along the south coast of the Arabian Peninsula and, finally, up the Persian Gulf to Kuwait. The captain of that boat, ʿAlī al-Nejdī, was skilled in coastal sailing and also did well in taking the ship across a small stretch of open ocean between Africa and the Yemen, something that Villiers ascribed to 'an act of God'. Moreover, it seems that there was a clash of personalities from the very beginning between Villiers and the young, strong-headed Kuwaiti captain. All of this left Villiers with the mistaken impression that by the 1930s the Arabs had lost the navigational skills of their forefathers; but al-Hijji offers convincing evidence that this was not the case.

In chapter 8, on Arab navigation in the Mediterranean, al-Hijji makes two points. The first is that, at least from the Middle Ages onwards, there was a common navigational practice among all the nations around the Mediterranean basin; and the other is that this tradition was navigation by coastal sailing with the aid of a *portolano* and portolan charts. The relative narrowness of the sea and the number of large islands made it possible to sail its entire length, even right down the center, without ever traversing more than 400 miles of open ocean. (And to traverse those stretches all one needed was the ability to sail along a latitude, i.e., to keep the altitude of the pole star relatively constant.) Most navigators did not in fact sail it right down the center but sailed 'from one landmark to another'. Such a practice drew scorn from the navigators of the Indian Ocean but Mediterranean navigators were not sailing that ocean. Their text of sailing directions and its visual representation in a portolan chart sufficed for their needs. So it is paradoxical that a maritime chart was not used in the Indian Ocean, where we might have thought it would be most useful, but was used in a much smaller sea.

In chapter 9, 'Sailing on the Red Sea', William Facey makes the point that the Red Sea is essentially two seas. The lower part, from Bāb al-Mandam in the south to Jidda about halfway up the east coast, receives virtually all the attention in our principal sources, Ibn Mājid and al-Mahrī. But, as for the upper part,

> this maritime region seems to have been an alien zone; they probably never went there and, even if they did, as far as they were aware alien conditions prevailed and different rules applied. [102]

Among those 'alien conditions' was the constant wind direction from the north in the part of the sea north of Jiddah, making sailing in that direction difficult at best, for one had to rely on land breezes and surface currents (available mostly in July and August, if that). Facey also traces the effect of the advent of Islam on the development of ports on the Red Sea. The pilgrim traffic to Jiddah resulted in that city's becoming not only a 'tourist center' but 'a vital link in the Indian Ocean trade network'. Ports on the African side, which had been important in the Roman world, lost their status; and from the early 10th century onwards, the Indian Ocean ships unloaded cargo, even goods bound for Egypt, at Jiddah for transfer to coastal vessels that would take the goods farther up the Red Sea.

Two appendices, notes, a bibliography, and an index conclude a work which should be of interest to anyone interested in Arab navigation and the Indian Ocean trade. Although the bibliography records the treatment of Arabic navigation in volume 2 of Harley and Woodward's *History of Cartography* [1992], a reader interested in the topic may also want to consult the article 'Arabic Nautical Science' by H. Grosset-Grange [1996].

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Heavenly Mathematics: The Forgotten Art of Spherical Trigonometry by Glen Van Brummelen

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Spherical trigonometry, once regularly taught in high school, disappeared from the curriculum in the decades following World War II. Yet the applications of this 'forgotten art' are still important and the elegance of the mathematics remains alluring. In *Heavenly Mathematics: The Forgotten Art of Spherical Trigonometry*, Glen Van Brummelen examines the historical background and development of spherical trigonometry through an exploration of the mathematical intricacies. The result is an engaging read that will appeal to historians of science, mathematicians, trigonometry teachers, and anyone interested in the history of mathematics.

Part of the appeal of Van Brummelen's book is its immediate discussion of the practical applications of spherical trigonometry. Van Brummelen handily weaves together mathematical theory with practical needs and offers a picture of the development of trigonometry, leaving no stone unturned in the process. In chapter 1, the author walks through Abū al-Rayḥān Muḥammad ibn Aḥmad al-Bīrūnī's calculation of the distance of the Moon from the Earth. Using a few measurements along with trigonometric functions, Van Brummelen quickly determines the radius of the Earth, which is the first step in determining the distance of the Moon. Modern calculations would involve the use of a sine function on a calculator but Van Brummelen aims to explore mathematics without taking anything on faith [2]. This detailed approach requires Van Brummelen to explain Hipparchus' table of chords, the first known trigonometric table, found in Ptolemy's *Almagest.* Armed with a trigonometric table, Van Brummelen is able to walk the reader through the remainder of al-Bīrūnī's calculation. While this calculation is the aim of the chapter, much is learned along the way about the history of spherical

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trigonometry and Van Brummelen's focus on a specific problem, buttressed by the historical development, is beneficial.

The desire of ancient mathematicians to solve certain problems fueled the exploration of the spherical surface and the development of spherical trigonometry. In chapter 2, Van Brummelen provides an introduction to geocentric astronomy. He explains how Hipparchus calculated the eccentricity of the Sun's orbit using the chord function and how this may have been the first trigonometric problem [29]. With an understanding of celestial motion from a geocentric frame of reference, Van Brummelen turns to spherical geometry and walks through calculations such as that of the smallest and largest possible sums of a triangle on a sphere.

Specific ancient and medieval approaches to spherical trigonometry are discussed in chapters 3 and 4. The author explains Menelaus' Theorem, the primary theorem used in ancient Greece to relate the arcs of great circles on a sphere. In chapter 4, we learn about the Rule of Four Quantities, which replaced Menelaus' Theorem owing to its efficiency of use in astronomical contexts. The Rule of Four Quantities is closely related to the Law of Sines and, as Van Brummelen explains,

> one would expect the Law of Sines, with its simplicity and complete generality, to have transformed medieval astronomy even more than did the Rule of Four Quantities. But science is not always predictable. [64]

The Rule of Four Quantities offered astronomers an economical way to solve for arcs and distances; the Law of Sines did not dislodge this pragmatic method, which was already in place. The sometimes surprising ways in which people have employed some mathematical theories over others to solve problems is one of the themes that Van Brummelen's book successfully explores.

One persistent, and helpful, theme of Van Brummelen's book concerns how practical needs played a role in the development of mathematical theorems. In the medieval Islamic world, for example, spherical trigonometry was instrumental in determining the direction of Mecca (the *qibla*). Al-Bīrūnī deployed four different methods to make this calculation, one of which Van Brummelen works through in detail [66–67]. The need to know in which direction to pray led Islamic mathematicians to produce tables that would point the believer toward Mecca from almost any location on Earth. This was a complex task for any single location; but its importance is demonstrated by the set of tables composed by Shams al-Dīn al-Khalīlī, which contained over 3000 entries [70–71].

Chapters 5 and 6 transition from ancient and medieval spherical trigonometry to the modern approach that is regularly taught in high schools today: examining the triangle on its own and using the six trigonometric functions [74]. Although the development of sine, cosine, and tangent were first seen in Indian astronomy, and although ancient and medieval astronomers used many formulas that are related to the six functions, Van Brummelen shows that it was the Scotsman John Napier (1550–1617) who first systematized these functions. While there remains much more to say about spherical trigonometry, Van Brummelen's discussion of its historical development concludes with Napier's analogies and the work of Jean-Baptiste Joseph Delambre in the 19th century.

The final three chapters focus on special topics: polyhedra, stereographic projection, and stellar navigation. While the historical narrative is set aside in these last three chapters, the book's overall organization is effective. In chapter 7, Van Brummelen explores mathematical theorems that did not have practical applications, starting with finding the area of a spherical polygon and then turning to Euclid's proof of the five regular polyhedra and Euler's polyhedra formula. Chapter 8 examines stereographic projection and the development and use of the astrolabe. The last chapter examines maritime navigation. Here Van Brummelen explains how Venetian merchant ships in the 14th century used methods of navigation that were based on plane trigonometric tables. While it is not always clear how sailors ended up with certain mathematical tables, it is apparent how useful these tables were.

At times, the proofs and mathematical details overshadow the narrative; however, as Van Brummelen states in the preface, this is not a scholarly book. A reader looking for footnotes and a more complete story might be interested in Van Brummelen's earlier book, *The Mathematics of the Heavens and the Earth: The Early History of Trigonometry* [2009]. That said, the real value of this book is its focus on the practical application and its hands-on approach to the mathematics. Various proofs are included throughout every chapter. Since the author recognizes that not all readers will have the time or desire to work through the mathematical details, he includes clear symbols alerting the reader that some of material can be skipped without losing the narrative.

This allows the reader to make choices about which proofs to work though with more care. Each chapter ends with a set of exercises that permit the reader to apply the material learned in the chapter. Most of these problems are challenging and in order to make progress the reader must apply the newly learned material in new and thoughtful ways. Van Brummelen states in the preface:

> The experience of wrestling with mathematics (provided that it meets with at least occasional success) can be one of the world's greatest pleasures. [xi]

He does not disappoint: what makes this book unique is the way that the history is coupled together with mathematical problems for the reader to solve. Working through the problems, both theoretical and practical, is an enjoyable task that leads to a deeper understanding and appreciation of the history and development of spherical trigonometry.

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Aristotle's Categories *in the Byzantine, Arabic and Latin Traditions* edited by Sten Ebbesen, John Marenbon, and Paul Thom

Scientia Danica: Series H, Humanistica, 8.5. Copenhagen: The Royal Danish Academy of Sciences and Letters, 2013. Pp. 340. ISBN 978–87–7304–372–1. Paper DKK 240.00

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The book contains 12 papers selected from among the contributions to three colloquia on the afterlife of Aristotle's *Categories*. Two of the pieces deal with the Byzantine tradition, two with the Arabic, and eight with the Latin.

First, Börje Bydén on Photius, the ninth-century Patriarch of Constantinople. Bydén asks whether Photius' account of the doctrine of the 10 'categories' had any influence on later Byzantine philosophers. He takes as his test case Photius' remarks on substances. According to Photius, the term 'substance' or «οὐσία» (taken in its special sense of 'self-subsistent item') is ambiguous: what Aristotle distinguishes as primary and secondary substances are called substance homonymously. Hence, *pace* Aristotle, there is no single class or category of substance. That view seems to have left no trace in later authors and Bydén's answer to his question is: '*Pro tanto*, no'.

Next, Ken Parry turns to the ninth century squabble over icons and he shows how some of the adversaries of the imperial iconoclasts—among them another Constantinopolitan Patriarch, Nicephorus—made occasional use of the logical terminology of the *Categories*. For example, they insisted on the fact that icons or images are, necessarily, images or icons *of* something or other, so that they are relative items and belong to the 'category' of τὰ πρός τι. Parry tells his story lucidly and the echoes of the *Categories* which he hears in the iconophile texts are genuine enough. But they do not amount to very much: it would be an exaggeration to say that Nicephorus and his allies exploited the *Categories*, or the doctrine of the 10 categories, in their denunciations of iconoclasm.

The next two papers turn to the Arabs. First, Heidrun Eichner on Avicenna. She is concerned with the question of Avicenna's philosophical development

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and she asks whether a partial answer might not be elicited from what he says at different times about the categories. (For there is, on this subject, a quantity of material which has hitherto been disregarded.) She finds that, on certain issues, Avicenna does appear to have changed his mind—or perhaps rather to have elaborated views which he had earlier only sketched. (For example, on the relationship between the different categories and the different sciences, or on the metaphysical origins of the categories.) The business is complicated by the fact that the texts which it invokes are, some of them, in a bad state: Eichner's paper, which is densely argued, aims to show the existence and the importance of the evidence rather than to work out its implications in depth and in detail.

The fourth essay is by Cristina Cerami, who writes in French (the other papers all being in English). She deals with Averroes' solution to a familiar problem. According to Aristotle, certain sorts of substance are the primary or basic members of their class; but the *Categories* identifies these primary substances with individuals (this sheep, that goat); whereas in book Zeta of the *Metaphysics*, the primary substances are not individual things but rather their substantial forms (not the sheep but its sheepish form, not the goat but its goatish form). That looks for all the world like a contradiction (or else a change of mind)—in any event, it constitutes an exegetical problem. Averroes dissolves the problem; for, he suggests, the contradiction is only apparent. How so? Well, the *Categories* and *Metaphysics* have quite distinct concerns: the *Categories* speaks within the discipline of logic, the *Metaphysics* within that of metaphysics; the *Categories* offers a dialectical account of substance, the *Metaphysics* an analytical one; the *Categories* is concerned with what is primary 'for us' or chronologically, the *Metaphysics* with what is primary 'by nature' or causally; the *Categories* is provisional, the *Metaphysics* definitive; the *Categories* speaks of substance in one sense of the word, the *Metaphysics* in another; the *Categories* deals with substances at one level or degree, the *Metaphysics* with those at another. Those several contrasts (some of which are not genuine) are different from one another and they are not all mutually compatible—though Cerami does not remark upon the fact. Nor (so far as I can see) does Averroes explicitly present them as a solution, or as elements of a solution, to the familiar exegetical problem. Rather, he construes the *Categories* in this way and the *Metaphysics* Z in that way. As a result, there is no problem to resolve. It may be added that Averroes view of what's going on in the *Categories*, which has much in common with the view elaborated by Simplicius, is scarcely plausible.

The remaining eight papers deal with the Latins. John Marenbon, one of the three editors of the volume, writes under this title: 'The Tradition of Studying the *Categories* in the early Middle Ages (until c. 1200): A Revised Working Catalogue of Glosses, Commentaries and Treatises'. The catalogue, earlier versions of which were published in 1993 and 2000, is preceded by an introduction (which includes some suggestions for future research) and complemented by a bibliography. Anyone who works in this area or on its margins will bless Marenbon (for the third time).

Next comes another of the editors, Paul Thom. With him the volume moves into the 13th century and to Robert Kilwardby. In his remarks on the 'category' of relative items, Aristotle had suggested that correlatives are simultaneous by nature—that is to say, if there are masters, then there are slaves (and *vice versa*); if some larger items exist, then some smaller items exist (and *vice versa*); and so on. But he discovered counterexamples, or apparent counterexamples, to the suggestion: knowable items and bits of knowledge (say) are correlative but they are not simultaneous—for though there cannot be any bits of knowledge without there being some knowable items, there can be knowable items without any corresponding bits of knowledge (i.e., there can be unknown knowables). That is a juicy bone and all the commentators gnaw at it. Thom discusses three texts in which Kilwardby gnaws. The discussion is done with exemplary clarity but it is done in a historical vacuum. And you might also wonder why the matter excited so much philosophical ingenuity; for, on the face of it, there is no reason to think that correlatives somehow *ought* to be simultaneous and there appear to be any number of common and garden counterexamples—parents and their children are rarely exact coevals.

Costantino Marmo, the seventh contributor, also writes about relative items—and about their fate from 1350 to 1500. There is, first, a rather breathless survey of some of the things some of the people then said; and secondly, an account of the ways in which Radulphus Brito and William of Ockham applied their general views about relative items to the particular case of signs and, hence, to the theory of meaning. For they both held, as a matter of course, to the idea that 'just as dark clouds are a sign of imminent rain, so the word "cloud" is a sign of a nearby thought of clouds'. Whether that is true (and whatever exactly it amounts to), it is an unsatisfactory way of approaching a theory of meaning.

Fabrizio Amerini, eighthly, looks at what Walter Burley and others had to say about the old question: What do Aristotle's categories classify? Does the class of substance (say) contain lions or the word 'lion' or the concept of a lion? Porphyry had answered that the items classified are words (of a certain sort) and that the principle of classification is semantic: 'lion', and not this or that lion, is in the category of substance; but 'lion' is a substance term because it is true of lions and lions are substances. That answer put the question to rest but from time to time it re-awoke. According to Burley, the categories sort not words but things. Still, they sort 'things as signified by simple words' [22n15]; and that (so Amerini says) 'amounts to the same thing' as Porphyry's interpretation [222]. Amerini considers some objections to the 'ontological' interpretation of the categories which had been raised by Hervaeus Natalis and Peter Auriol. The objections purport to show that (on Burley's view) some items will be in more than one category and some in none. So far as I can see, the objections have no force against Porphyry and, hence, none against Burley, if his and Porphyry's view really do 'amount to the same thing'. And I suppose that they do: after all, what could 'classify things as signified by terms' possibly *mean* if it was not a cack-handed way of saying 'classify words according to the sort of things they are true of'?

The title of the next essay, by Joël Biard, is: 'The Status of Categories and Its Epistemological Stakes in the Fourteenth Century: The Case of Blasius of Parma'. Blasius held that the categories classify words, not things. Biard is concerned with what he says about quantities and about relations, namely, that 'every quantity is a substance or a quality' [250], so that, for example, 'a number is the numbered thing itself' [252]; and similarly for relations, so that 'a proportion is the things proportional to one another' [255]. (It is hard to see how that chimes with the view that the categories classify words but let that pass.) The 'epistemological stakes' show up when Blasius comments upon the science of mathematics, the subjects of which are quantities. He says that 'when arithmeticians talk about numbers, they distinguish a number from the things numbered' [257] and so indeed they do. But then whatever are they up to, given that numbers simply *are* the things numbered? The whole of arithmetic, it seems, rests upon a simple mistake. Biard offers an explanation: numbers 'are not treated as independent substances, as they might be by Platonists; but the formal reason, that is to say the active mode of conceiving becomes the proper object of the mathematician' [258]. That is Delphic but it presumably connects with an earlier remark to the effect that 'Blasius...states that if we understand by 'number' the words or the concepts by which we count,...then number is an accident of the soul' [252]. So not all numbers are the same as the things numbered: there are also (as the old Peripatetics called them) numbering numbers and it is they which are the subject of arithmetic. That is better: save that numbering numbers are neither words not concepts nor accidents of my soul. (It may be noted that this is the only place in the volume in which there is any explicit discussion of any science.)

With the 10th paper, we reach the 16th century. E. J. Ashworth talks about Domingo de Soto's vast commentary on the *Categories*, which raises all the stock questions and generally plumps for one or other of the stock answers: Ashworth sums de Soto up as 'a well-read eclectic' [280]. About half of Ashworth's short paper concerns 'denominatives' or paronyms; and here de Soto did have something new to offer. In the Boethian translation of the *Categories*, the definition of paronyms runs like this:

> *denominativa...dicuntur quaecumque ab aliquo solo differentia casu secundum nomen habent appellationem.*

De Soto claims that the phrase 'secundum nomen' goes with what precedes it, not with what follows, and this construal forms the basis of his novel account of paronymy. The construal stretches the Latin but it is perhaps just about possible. But this is Aristotle's Greek:

> παρώνυμα...λέγεται ὅσα ἀπό τινος διαφέροντα τῇ πτώσει τὴν κατὰ τοὔνομα προσηγορίαν ἔχει [1a12–13].

The phrase «κατὰ τοὔνομα»—Boethius' 'secundum nomen'—goes with the succeeding «προσηγορίαν». That is proved by the definite article which precedes it—and which the Latin cannot render. De Soto's interpretation is quite undone. He could not have made the error had he read the *Categories* in Greek. There must be a moral in that.

The penultimate paper, by Sven Knebel, discusses what certain 17th century Spaniards had to say about works of art and the *forma artificialis*. He sets out some entertaining eccentricities (and he quotes generously from the pertinent texts). He remarks in passing that artificial forms, as opposed to natural forms, were generally taken to be not substances but qualities (and to fall into Aristotle's fourth sub-class of qualities). That is the only connection between his subject and the categories, and his paper hardly belongs in the volume.

Lastly, Sten Ebbesen has a characteristically lively piece about the fate of the *Categories* in Lutheran Denmark. The Lutherans did not think much of logic (or of any other science) but they thought that they needed it to parry the attacks of their religious adversaries. So, under regulations which derived from Melanchthon, the grim professors of the University of Copenhagen taught logic—Aristotelian logic, of course—and, hence, the *Categories*. That went on unexcitingly for a century or so. But then the old order changed: one Dane claimed that it was only Pythagorean superstition which fixed 10 as the number of the categories; another indicated that the doctrine of categories had no importance for logic inasmuch as, for syllogistic, 'only one type of predication is needed' [331]. And then logic itself withered away. After all, 'if it was not even a necessary auxiliary force to keep Calvinist and Papist enemies at bay,...what was the use of it?' [331].

The 12 papers, as those crude summaries suggest, are scholarly items: they are written by specialists for specialists and they make no effort to seduce readers from outside the club. (It should not—but it does—need to be said that that is anything but a Bad Thing.) The crude summaries also suggest that the papers are disparate in style and in approach and in scope. They do not cohere into a book, having nothing in common beyond the fact that each of them makes some sort of reference to the *Categories* or to the categories. The distinguished editors do not pretend otherwise—and it must be said that they have worked with a light hand: they have arranged the papers chronologically; they have compiled an index of names and an index of manuscripts but no general index; there are bibliographies to individual papers but no general bibliography; there is half a page headed 'Abstract' and half a page headed 'Introduction' but no general introduction; and no attempt has been made to link one paper to another by cross-references. (I noticed only two such cross-references in the whole volume.) In short, the book is less an edited collection of papers than a 'special issue' of a scholarly journal. Still, it provokes a number of general ruminations. Here are two, each of them melancholy.

The materials on which the several contributors work are, almost all of them, low-grade stuff. Even the best of their heroes can write the oddest of things. So Ockham, according to Marmo, held that 'there exist no real entities corresponding to our relational concepts' [199]. Marmo observes that this is 'a radical shift in the ontological interpretation of categories and relations' [199] but he does so without raising his eyebrows. And yet it goes against the grain to deny the reality of (*inter alia*) sons and lovers. ('Of course Ockham did not mean *that*'—but then why did he say it? And what on Earth *did* he mean?) To be sure, relatives and relations had flummoxed philosophers from Plato onwards. (And they persist in citing fathers and sons as paradigm correlatives.)

Or take Photius: he apparently thinks that substances are homonymous because (like numbers or geometrical figures) they form an ordered series; and he infers that the *echt* substances must be *infimae species*. But ordered series do not insinuate homonymy (the numbers 57 and 75 are not called numbers in different senses of the word); and if 'substance' has different senses, then the question 'What are the *echt* substances?' has no answer or at least no simple answer. (It is perhaps unclear whether these mistakes should be ascribed to Photius or to Bydén or to both men.) Or Avicenna: what Eichner generously calls his 'highly unified ontological theory' takes as its root-stock a reasonably sturdy Aristotelian plant—and grafts on to it some fearful nonsense about 'emanations'.

And a lot of nonsense is scattered throughout the volume. So, for example, according to Amerini, 'Hervaeus argues that *man* must be properly described as a thing to which the property of being universal accrues accidentally, rather than as an actual compound of thing and universality' [233]. So far as I can make out, Hervaeus' Latin, which Amerini there paraphrases, means nothing at all. To be sure—as the Israeli proverb has it, s**t is s**t but history of s**t is scholarship; and a history of astrology (say) may be both instructive and diverting. But as Peter Geach somewhere observed, it is difficult to discuss nonsense without falling into nonsense oneself: if a given sentence makes no sense, then neither does the result of preceding that sentence with such a phrase as 'Hervaeus argues that'.

A second, and equally melancholy, rumination is prompted by the Abstract, which states, truly, that 'hardly any other philosophical book has had as many readers over so many centuries as Aristotle's *Categories*'. (It adds that 'the influence of <the *Categories*> is manifest in our everyday language when we speak of quantities or qualities, of relations or of the substance of the matter': so *that* is what the Walrus was thinking of.) And the Abstract claims that 'the twelve essays collected in this volume demonstrate the book's importance in all three language areas' ('the book' refers to the *Categories*). The volume may be said to cast some sidelights on the importance of the *Categories* but it does not *demonstrate* it (not that any demonstration is needed) nor does it do anything to explain it. Why did Aristotle's pamphlet enjoy such an eminent afterlife?

The answer is this: for centuries, the *Categories* was everyone's second book in philosophy (second after Porphyry's *Isagoge*). Why so? Well, before you learn a trade—so the ancient argument went—you must learn to use the tools of the trade. The tools of the philosophical trade are proofs. Proofs are syllogisms. Syllogisms are made up of propositions. The chief constituents of propositions are terms. So if you are to learn any philosophy (or indeed any science), you must first learn all about terms. And the *Categories* contains the true doctrine of terms. That argument, which obliged so many reluctant students to con the *Categories*, totters at each of its steps. And it falls flat on its face when it introduces the *Categories*: the doctrine of the 10 categories, far from being an indispensable preliminary to the study of proof or of syllogisms or of Aristotelian syllogisms, is entirely irrelevant to those grave matters; for, as the sapient Dane remarked, 'only one type of predication is needed' for syllogistic—that is to say, the *A*s and *B*s and *C*s of the *Prior Analytics* represent any predicate-terms whatsoever, simple or complex, of substance or of quality or of relation and they take no notice of any possible classification of the things. That fact must surely have struck any intelligent student—even the dullest and most stick-in-the-mud of professors ought to have noticed it after a few decades of teaching Aristotelian logic. And yet the students did not complain that the *Categories* wasted their youth and the professors, too crabbed or too lethargic, did not care.

To be sure, Aristotle's *Categories* contains several paragraphs of some philosophical interest. (And no one with the slightest interest in the *history* of philosophy could, or would, want to give it a miss.) But phenomenal longevity as an introductory text on philosophy can only be accounted for by invoking the ineradicable conservatism of the philosophical tribe.

An Essay on the Unity of Stoic Philosophy by Johnny Christensen

Copenhagen: Museum Tusculanum Press, 2012. Pp. 120. ISBN 978–87–635–3898–5. Cloth €23.00, $29.00

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Johnny Christensen's short book entitled *An Essay on the Unity of Stoic Philosophy* (hereafter *Essay*) was first published in 1962. I read it in the 1990s when first starting to work on Stoicism, and at the time found it very helpful. It is, however, a somewhat anomalous book, taking the form of a brief overview without extensive scholarly apparatus while at the same time offering an advanced treatment of the material that can hardly be described as introductory. This, combined with the fact that it was published by a relatively small press, has meant that it has not always found a place in bibliographies of literature on Stoicism.

This new edition of Christensen's *Essay* is edited by two of his former students, both of whom have gone on to become highly respected scholars in their own right: Sten Ebbesen and Troels Engberg-Pedersen. In their brief but informative foreword to the new edition, they recount the reception of the *Essay* and its place within wider scholarship on Stoicism. As they note, it was often cited in Anglophone works on Stoic and Hellenistic philosophy from the late 1960s and 1970s, such as Rist [1969] and Long [1974]; but more recently it seems to have dropped off the radar. That is in some ways unsurprising given the huge amount of work that has been published on Stoicism in the interim. Indeed, when Rist and Long were writing their books, there were very few recent works in English devoted to Stoicism and Christensen's *Essay* was one of a handful of pioneering early works, alongside works such as Sambursky [1959] and Mates [1953]. In 1974, Long [254] described the *Essay* as 'the most philosophically sophisticated short introduction' available and Ebbesen and Engberg-Pedersen assert that it remains even today 'the philosophically most sophisticated attempt to make sense of Stoicism as a whole' [ix]. As well as adding their foreward, the editors have supplemented the original *Essay* with a reprint of an article

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by Christensen on Stoic politics from 1984, a topic not covered in the *Essay* itself. In short, this new edition is a tribute to a clearly respected teacher whom his pupils think has been unfairly neglected by recent scholarship.

What of the *Essay* itself? It is indeed a spirited attempt to show the unity of Stoic philosophy, to try to understand Stoicism as a unified philosophical system. The most striking feature of Christensen's account is his emphasis on the Stoic commitment to what we might call, for want of a better phrase, process philosophy: 'To the Stoics the world is made of matter-in-motion. So, the elements of our experience are primarily *events*' [46–47]. Christensen takes this ontological claim as the fundamental principle of Stoicism. He sees this Heracliteanism as an explicit challenge to Platonism and Aristotelianism, and he focuses in particular on the way in which the Stoics might be seen to be responding to Aristotle, both in metaphysics and in ethics. This is at odds with what has become a dominant trend in recent Stoic scholarship, namely, to emphasize Stoic debts to Plato. Christensen, by contrast, downplays the extent to which the Stoics might have been engaging with Plato and insists on their philosophical independence from both him and his pupil. This strikes me as a useful corrective to a trend that has perhaps gone too far in trying to find Platonic antecedents to as many aspects of Stoic philosophy as possible. The Stoics are Plato's philosophical adversaries, not his descendants.

This emphasis on events over objects in Stoic ontology informs Christensen's whole approach to the various aspects of Stoic philosophy and a range of issues in logic and ethics are presented as working out the implications of this fundamental ontological commitment. As just one example, his discussion of Stoic epistemology does not take place in isolation but rather emphasizes its foundations in dynamic physics: 'knowledge is built out of an interaction between influx of motion from the external world and established patterns of motion in the mind' [58].

Some minor points to note: Christensen characterizes the two Stoic ἀρχαί as ἀσώματα [11], following the variant reading from the *Suda* sometimes adopted at Diogenes Laertius, *Vitae* 7.134. Later he suggests that Nature is composed of 'two constituents', Structure and Matter [23]. These two claims might be seen to go against one another and neither is as clear as it might be. As he notes later, quite rightly, those things classed by the Stoics as incorporeals do not, strictly speaking, exist [25]; so, as others have noted since, it is somewhat problematic to characterize the ἀρχαί as such.

How does the *Essay* stand up 50 years after its original publication? Has it been left behind by the enormous amount of scholarly work on Stoicism published since 1962? The editors clearly think not and I am inclined to agree with them. Although many details of the Stoic system have been examined in detail and filled out over the last few decades, there have been few attempts to try to make sense of Stoicism as a whole. Christensen, well aware of the wide range of textual and interpretative difficulties on which much of the recent scholarship inevitably focuses, was bold enough to put all of these to one side in order to try to grasp what we might call the central vision of Stoic philosophy, which he sees as a thorough-going process philosophy the implications of which are played out across an entire philosophical system. One consequence of this is the claim that some grasp of physics is necessary for an understanding of ethics [62], a topic that has recently received scholarly attention from Julia Annas [2007] and others. Although Christensen's *Essay* is incredibly brief, its concerns remain current within scholarship on Stoicism.

It is also worth commenting that although in many places this is a highly technical work, it remains eminently readable and Christensen often has a wonderful turn of phrase, such as 'the Stoics were too poignantly aware of the vicissitudes of life to deny the reality of change' [13] and 'Socrates is what in physics might be called a high-level tensional field' [49].

Finally, it is worth mentioning that this is a beautifully produced volume: a slim hardback printed on high quality paper and sewn into an attractive cloth binding. There are few academic presses in the English-speaking world that could match this standard of production. This makes this new edition of Christensen's *Essay* all the more fitting as a tribute to a teacher by his grateful and respectful pupils.

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Progressive Enlightenment: The Origins of the Gaslight Industry, 1780–1820 by Leslie Tomory

Cambridge, MA/London: MIT Press, 2012. Pp. xii + 348. ISBN 978–0–262–016 75–9. Cloth $28.00 £19.95

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The railroads were not the first tightly integrated industry, Leslie Tomory argues in this fine study. Britain's Gas Light and Coke Company (GLCC), formed in 1812 and by 1826 serving nearly every British town with a population greater than 10,000, was closely integrated nearly a generation before railroads; it adopted a highly structured style of management that gave it stability in riding out crises and also flexibility in responding to the sorts of difficulties that attend complex enterprises in which changes in one area cascade throughout the system. Gas lighting as an industry required robust distribution systems, careful and continued attention to load factors and usage patterns, and a particularly complex and mediated relationship with customers, due in the case of GLCC to a legal settlement with the rival firm of Boulton & Watt (yes, the steam firm) that prevented GLCC from selling machinery and thus restricted it to selling the products of its distillation works. It was the successful British experiment with gaslight as an industry that was imported elsewhere in Europe after 1820.

But gaslight did not begin as a uniquely British invention. Tomory describes how interest in using inflammable gases for illumination developed nearly simultaneously in workshops in continental Europe and in Britain, as people working in the nascent science of pneumatic chemistry built apparatus that later became central to the industry. Consider Briton Stephen Hales, who invented the pneumatic trough for his work in distilling airs from decaying vegetables. Hales designed the apparatus to clean away binding particles by forcing gases to rise through a water bath; the pneumatic trough was a prototype for the water main in a gasworks. People working in industrial distillation fused techniques and knowledge borrowed from pneumatic chemistry with the commercial orientation that made it possible to turn a workshop

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process into a business enterprise. French engineer Philippe Lebon's thermolamp inspired developers in Britain and Germany, but Lebon was unable to find the financial backing to commercialize his invention himself and instead turned his efforts to other products of distillation such as tar. Tomory argues that gaslight's roots in pneumatic chemistry and industrial distillation mark it as one of the useful products of the open science of the Enlightenment.

The firm of Boulton & Watt transformed gaslight from a workshop invention to a viable commercial enterprise, organized along the lines of its steam engine business: individual gasworks each dedicated to the illumination of a single large building or mill, much as a single steam engine had powered an individual enterprise. Boulton & Watt promoted employee William Murdoch as a hero of gaslight and engineered honors for him from the Royal Society, but by 1812 the firm had lost interest in promoting the industry and the center of activity shifted to the work of the technically incompetent but entrepreneurially gifted Frederick Winsor, an immigrant to Britain from Germany. Winsor formed the National Light and Heat Company and attracted scores of investors but was marginalized after the firm was granted a royal charter as a limited-liability joint stock corporation and named the Gas Light and Coke Company in 1812. Tomory argues that GLCC's formation as a joint-stock corporation was an important milestone in the consolidation of the firm's network strategy and illustrates gaslight's character as a technology of the second-wave of industrialization. Technologies of the first wave typically required little capital and relied on craft skills, and were often only tangentially related to contemporary scientific work. Gaslight grew out of work in the traditions of natural philosophy; its growth as an industry required extensive capital investment beyond what a single firm could supply; and it required almost continual technological innovation, undertaken in-house. This well written and cleanly organized study is especially good on internal developments at Boulton & Watt and GLCC, and draws extensively on the archives of both companies. It offers an important comment on early relationships between science and industry, and demonstrates how significant an analysis of entrepreneurship may be for our understanding of industrial revolutions.

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Greek and Roman Musical Studies (*GRMS*) is the inaugural issue of a new journal. The initiative for this journal came from The International Society for the Study of Greek and Roman Music and Its Cultural Heritage ('Moisa' is the clever and convenient nickname). But it is not an 'in house' journal of the society. It was conceived by the membership as meeting a need: there has not been a journal devoted to research in this area until now. *GRMS* aspires to be for ancient Greek and Roman music what the journal *Phronesis* is for ancient philosophy. Still its mission is not narrow, as we learn from the opening editorial by Andrew Barker (who is not only editor-in-chief but also the dedicatee of this first volume, on the occasion of his 70th birthday). Music in the ancient world stood in relation to many other disciplines: mathematics, natural philosophy, psychology, medicine, poetry, politics, and so forth. *GRMS* is soliciting contributions that shed light on all these points of contact. I am pleased to see that it is also soliciting contributions on the music of other ancient cultures in the Mediterranean and on the reception of Greek and Roman music by later and more geographically remote musical cultures.

This first volume of *GRMS* is a sampler of what the journal might offer in the future. Two papers in particular help to indicate the wide range of topics that might be covered.

One is by Christophe Vendries, a historian at the Université de Rennes, on the question of what may be learned from certain terracotta figures depicting musicians about possible connections between Greek and Egyptian culture (musical and otherwise) in Hellenistic and Roman Egypt. The figures in question are of uncertain date. They seem to have been produced in series by anonymous craftsmen in workshops attested in Alexandria and the Nile valley. They are, for the most part, small figurines (between 8 and 30 cm)

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cast from moulds in two halves and then painted: trinkets of modest price for people of modest means. The analogue in our culture (if there is one) might be the little bust of Beethoven inevitably on display in the home music studio of private piano teachers, with this one difference at least: the figurines discussed by Vendries seem to have served the needs of private devotion, expressing ordinary hopes for prosperity. As musical iconography goes, they are completely lacking in splendor or even the miniature razzle-dazzle of the cylinder seals at the Pergamon Museum in Berlin. It is not surprising that they have been completely eclipsed by the temple iconography from the time of the Pharaohs. From the time of Hellenistic and Roman Egypt, the preferred source of information for scholars interested in music has been papyrus fragments. Vendries is surely right to call our attention to the terracottas. What may be learned from them, however, is not yet clear.

If one hopes to learn about instruments and their construction, one will likely be disappointed. On the one hand, they show a concern for realism, as indicated by the visible joints in depictions of trumpets. On the other hand, the depiction of harps, lyres, and kitharas is too small and notional to tell us reliably the number of strings. One might hope too to find in these figures a window into the role of music in local cults and festivities. Indeed, they number various gods and goddesses in the act of minstrelsy, the most surprising of which may be an Isis/Nike on horseback, plucking a harp. But since the local deities were not traditionally associated with a musical instrument, it is often unclear whether any given female musician is a goddess or a mortal.

The observation made by Vendries that most struck me took the form not of an answer but of a question. Some of the figurines depict animals playing musical instruments, notably an ass with an outsize phallus, playing a lyre. The question is which culture provides the key to understanding the iconography. If we think in Egyptian, we may perhaps be reminded of the harp-playing ass found in satirical papyri from the New Empire. But if we think in Greek, we will likely take the image to represent an uncultivated boor. As Vendries points out, this association was known in Alexandria, since Athenaeus relates that an inhabitant of that city proposed changing the more usual ass putdown to the 'ox with a lyre' in honor of a kithara player familiarly known after the other heavy-footed beast [*Deip.* 4.8 349c]. Even if Vendries is right to think that the Greek construal of the image is the relevant one, his observation helps bring sharply into focus the distinctive and yet enigmatic cultural bivalence of Hellenistic Egypt as reflected in these figurines.

It is not news that Hellenistic/Roman Egypt was multicultural. Professional Egyptologists will no doubt find other points of interest in Vendries' article that escaped me. I think it right, though, to call attention to something that I found striking (precisely as a non-Egyptologist) because I predict that this will be part of the experience that readers of *GRMS* can look forward to. Because the mission of the journal is broad (without being diffuse), any of us who pick it up out of one interest or another should expect to find contributions that spark interest in fields remote from his or her own. In short: this will be a journal that we should expect to find stimulating. I might add that the pictures and photographs of the figurines that Vendries discusses—and those of other objects related to other contributions in this issue—are clear, easy to look at, and often downright beautiful. Whether the ass with lyre may rightfully be called beautiful, I leave to the judgement of other readers. But I found it striking that he seemed to be stopping the strings of his lyre with four, perhaps five, digits and a thumb on what appears to be his left hand, rather than a hoof. A hoof may well terminate his right foreleg and perhaps he is using it to strum his strings, as one might a pick or a plectrum. Hoof, like horn, might be a very good material for such an implement. This thought suggests that our friend's technique may have been less clumsy than rumor would hold.

The second paper that I propose to highlight illustrates, by contrast, how wide the possible range of themes and topics of *GRMS* might be. It is by Pauline LeVen of the Yale Classics Department. It takes us from Egyptian iconography to questions about the vocabulary available to archaic and classical Greek poets to communicate thoughts about, and the experience of, soundscapes.

It is a challenge to set in words the quality of the sounds that we hear, no matter what language we speak. That is because the qualities that we are trying to describe are so unlike the other things which we think about or experience. There are conventions for talking about clashes, clangs, clatters, and thuds. But who knows whether I will really get you to hear in your mind's ear what I mean when I use these words. Perhaps for this reason, our vocabulary for sounds is often clumsily onomatopoeic,[1] on loan from our language for the other senses or just made up. This difficulty is on display in any of the selfconscious efforts made by mathematical and physical scientists to distinguish the different sound qualities that they either hope to explain

[1] I say 'clumsily' onomatopoeic because, though such words are supposed to mimic the sounds that they refer to, it is striking how little agreement there is among the different languages that I know about what the relevant sounds are supposed to sound like in human language. Consider, again, the English words 'clash', 'clatter', 'clang', and 'thud'. Their Greek equivalents are, respectively, «πάταγος», «δοῦπος», and «κλαγγή». «Κλαγγή» and 'clang' may sound alike but I am not sure that they really cover the same sounds, since the Greek is associated with squawks and shrieks of birds, dog barks, and the shouts of men. That is not what my ear associates either with the English 'clang' or the German 'Klang'. It may well be, in fact, that the only piece of onomatopoeia that all or most human languages can agree on is 'meow'.
I wonder if this diversity among languages indicates that the purpose of onomatopoeia is not what it is usually said to be. One piece of circumstantial evidence is that we do not use onomatopoeia when it is really important to give our interlocutor a precise idea of what we have heard. If you are startled by a loud sound and you are alarmed enough to report it to the police, you will say: 'I think I heard an explosion two doors down.' You will not say: 'Ka-Boooooum!' If we ask what other function onomatopoeia may have, besides entertaining children, one answer may be that it is used to liven up poetry; and perhaps the fact that it is pressed into the service of meter and sometimes rhyme explains why it varies so much from language to language.
This thought occurred to me in the course of writing the current review because we find a passage at the beginning of Hermann Helmholtz' *Die Lehre von den Tonempfindungen* that is reminiscent of Ptolemy, *Harm.* 1.3 (discussed in the following paragraphs). It is reminiscent in that Helmholtz is trying to convey roughly what accounts for differences in (certain) modifications of sound. In an effort to focus our imagination, Helmholtz (like Ptolemy) gives us some examples: 'Das Sausen, Heulen und Zischen des Windes, das Plätschern des Wassers, das Rollen und Rasseln eines Wagens' [1913, 13]. The list includes the sounds made respectively by wind, water, and wagons. All of the words are onomatopoeic, e.g., the 'Zischen' of the wind, likewise its 'Heulen', although 'Heulen' is more usually reserved for the howling of wolves. But it is very striking how many of these words are found repeatedly in classical German poetry. One example will suffice: that of 'rasseln', one of the sound-words Helmholtz associates with wagons. It turns up in the second line of Goethe's Kronos: 'Spude dich Kronos | Fort den rasselnden Trott!', ('Make haste, Cronus! | Away, at a clattering trot!'). A poetic pedigree can also be attested for 'sausen' and 'plätschern'.

or at least to characterize in some way for the purposes of doing physical acoustics.

A good example of this, with an interesting stock of Greek vocabulary for sound qualities, can be found in Ptolemy, *Harm.* 1.3 where the question is whether modifications in sound are qualities or quantities. That question is supposed to turn on the causal factors at play. If the causal factors are or have quantities, then so must their effects. One of the causal factors that Ptolemy fixes on is the physical constitution of the striking body. The striking body may be dense or fine, thick or thin, rough or smooth: to that extent, it will produce a sound with the same quality and the same degree or intensity. It will be called πυκνός or χαῦνος, παχύς or ἰσχνός, τραχύς or λεῖος [Düring 1930, 7.15–19]. Since the relevant qualities of the striking body are supposed to be quantifiable, so too the resultant qualities of the sound that we hear. This makes sense on the face of it since, for example, the coarser the grade of sandpaper, the raspier the sound that we hear. Ptolemy's reflections may indicate that much of our vocabulary for sound is, in *some* sense that will be examined more closely below, taken from our vocabulary for other qualities of bodies.

I propose to use Ptolemy's reflections to get a handle on LeVen's claims, which I found interesting but elusive. Precisely because Ptolemy is not a poet but a mathematical scientist, his struggle at the beginning of the *Harmonics* is to figure out how the vocabulary for sound works: how it refers and what aspect of sounds it refers to. The very nature of his enterprise requires that he step back from this vocabulary and reflect on it in a selfconscious way. Poets do not do this: they use, and perhaps also sometimes willfully abuse, the language available to them without selfconsciously trying to understand what is going on, at least not in their poetic output as such. Ptolemy has to lay his cards on the table; the poets do not. Thus, Ptolemy offers a readymade account of the matter. I am going to take advantage of this, not in the interest of making mischief but as a way of raising questions that I hope will clarify how some of LeVen's suggestions might be cashed out. Ptolemy is writing several centuries after the archaic and classical poets who are the immediate object of LeVen's interest. But that does not matter for the purposes at hand: the idea is not to represent the poets as implementing proto-Ptolemaic ideas in their treatment of modifications of sound but rather to ask LeVen for guidance on how her ideas should be understood.

The impression to start with (and it is nothing more than an impression) is that there may be an important contrast between LeVen and Ptolemy. LeVen is interested in Greek vocabulary for sound that presumably cannot be analyzed in the way that I indicated earlier on Ptolemy's behalf, namely, as having been taken from the vocabulary available for sensible qualities other than sound. The special case, for LeVen's purposes, is the word «ποικίλος» and its related forms. As an adjective, this word is often unambiguously visual because it means 'multicolored', 'piebald', 'speckled', or 'dappled', like the back of a snake or the feathers of a bird. But it is also used for sound as when Pindar characterizes the *phorminx* as ποικιλόγαρυς (multi-voiced) [*Olympia* 3.8]. We can point, wave, or gesture at brightly feathered, multi-hued birds when we are trying to convey what we mean by «ποικίλος»; but we cannot point, wave, or gesture at the sounds that we hear. For that reason, one might think that the application of «ποικίλος» to sound is something like that of «τραχύς» or «λεῖος», as explained by Ptolemy, except without the causal underpinnings and taken this time from the Greek vocabulary for visual rather than tactile qualities: what Pindar may have meant when he called the *phorminx* ποικιλόγαρυς—so the thought runs—is that it is an instrument that produces as many different musical sounds as the plumage of the relevant bird has colors. Thus, metaphor (or some kind of analogy) would be in play here. But LeVen unambiguously resists this way of thinking.[2]

She argues instead that «ποικίλος» is in the first instance neither visual nor auditory. Rather it functions in certain contexts as an all-purpose synonym for 'fine' or 'beautiful', except that it does not—or does not merely—denote a quality of the beautiful object; it also evokes the response of the person or being who admires the object. Perhaps, in fact, the claim is better understood as saying that the word denotes the disposition of the object to produce this response in the admirer [238].[3] Thus, it will turn out that «ποικίλος» functions in archaic and early classical Greek much as 'cool' does in the idiom of North American undergraduates (and those who spend too much time in their company). 'Cool' literally denotes a tactile quality but, as used in colloquial English, that is not what it means at all: it is a term of general

[2] In fact, if Ptolemy himself were going to treat «ποικίλος» as he does «τραχύς» and «λεῖος», he too would deny that its application to sound is metaphorical. See below.

[3] I will return to this below.

commendation applied to those things or states of affairs that elicit in us admiration—or, anyway, approval.

LeVen does not deliver a conclusive argument in defense of her thesis; rather, she sketches out a line of enquiry with suggestive passages from Aristotle and the relevant poets. The purpose for doing so, I take it, is to stimulate thought and discussion. It is in that spirit that I now ask a couple of questions because the issues that she raises are both intrinsically interesting and difficult.

My first question is whether the function of «ποικίλος» in archaic and classical Greek poetry is so different from that of the vocabulary for sound canvassed by Ptolemy in the passage that I mentioned earlier in *Harm.* 1.3. Let me be clear: my question is not whether ποικιλία is quantity or quality.[4] It is whether the aesthetic use of «ποικίλος» that expresses admiration for something can plausibly be understood as having been taken (in the relevant sense) from an implied use of «ποικίλος» to denote the modification in the object that produced admiration in the eyes or ears of the beholder, just as 'rough', 'coarse', or 'raspy' might be said of a sound (with or without praise or blame) on account of the corresponding roughness in the sounding body.

[4] This clarification may well seem out of place. As James Porter has indicated to me in correspondence, it is surely uncontroversial that ποικιλία is not a quantity at all but rather a quality. That may well be true. But as long as we are thinking about the matter in light of Ptolemy, it is not uncontroversial. Ptolemy treats roughness, smoothness and so forth as quantities (or as qualities that have quantity) in *Harm.* 1.3. As Porphyry points out (critically) in his commentary, Ptolemy may well be picking up on a remark that Aristotle makes in *Categories* 10a16. Aristotle calls into question the idea that the dense, the fine, the rough, and the smooth are qualities: 'For they seem rather to indicate in each case a certain arrangement of the parts.' Thus, something is dense if its parts are arranged close together. Porphyry says that this is as good as to treat the dense, the fine, the rough and the smooth as quantities [Düring 1932, 42.29]. Thus, on Porphyry's view, Ptolemy's only contribution is to make this explicit. Porphyry himself takes the view that these modifications are qualities; I suspect that he and Porter would be in agreement about «ποικιλία» [43.18–19]. If, however, we look at things from Ptolemy's point of view, ποικιλία might well turn out to be a quantity (or a quality that has quantity), i.e., the quantity of differences that a thing exhibits. A thing will be all the more ποικίλον the more its parts are different from some other. Be that as it may, and I believe it is quite interesting in its own right, I want to set aside the question of quality *versus* quantity and focus on the question that I formulate in what immediately follows.

I am led to raise this question because, in spite of the apparent contrast that I noted at the outset, there seem to be two notable points of contact between LeVen and Ptolemy.

The first is that Ptolemy would deny, for example, that the words ‘rough’ and ‘smooth’ as applied to sound are being used metaphorically. If you sand a surface with sandpaper, the sound produced will be rough just like the sandpaper itself. The term that you use to describe the sound will be the same as the one you use to describe the striking body: that term will not have to be stretched, extended, or contorted in any way. For the quality of the striking body and the quality that it imparts to the sound emitted are the same. Ptolemy says explicitly that sameness of quality is tracked by sameness of term. In Andrew Barker’s translation, the relevant passage [Düring 1932, 7.15–20] reads as follows:

> Through smoothness and roughness, again, it [*scil.* the striking body] creates only a quality in accordance with which sounds are described by the same words, smooth or rough, since the qualities are essentially the same. Through diffuseness or density and thickness or fineness, it makes qualities in accordance with which we again call sounds by the same words, dense or flabby, thick or thin.... [Barker 1989, 280].

That the same term is used for the same quality, whether apprehended by the ear or by touch, means that the term is not used metaphorically. This makes for a point of contact because LeVen says that «ποικίλος» as applied to sound is not metaphorical either: for her, it is a word that has no primary residence in some one sense (vision, say) but is at home in, or appropriate to, them all [234–235].

The second point of contact is that, like Ptolemy, LeVen speaks of causal factors. She writes:

> This is, I believe, what ποικίλος encapsulates in the archaic and classical period: it captures ...the notion that the luscious patterns in a bird’s feathers, the wrought motives of a shield, or the many-voiced and swift-moving notes of a lyre *cause* an aesthetic reaction of rapt pleasure through the senses. [238, my emphasis]

Precisely because there is something in the admired object that causes the admiring state of mind in the speaker, it would be a mistake to translate «ποικίλος» as 'ooh' or 'ah'.[5] The better translation would be a quasi-Homeric epithet: 'provoker of ooh's and ah's'. This captures both the significance of the beholder's admiring response and the object's role in provoking the response. It does not privilege one over the other. LeVen herself stresses the idea that the archaic and classical use of «ποικίλος» treats the two aspects as inseparable or intimately related. It is hard to see how that could be unless a causal relation were at work. That too is suggestive of Ptolemy's account of 'rough' and 'smooth' as applied to sound in *Harm.* 1.3: we have seen that there too causal factors are at work.

But it might be objected, and perhaps LeVen herself would agree, that a causal relation all by itself is not enough to get the sameness of terms that Ptolemy calls to our attention. So this may be where she and Ptolemy part company. I can clarify the point by appeal to David Hume's essay 'Of the Standard of Taste'.

Hume opens his essay by recalling an episode in Cervantes' *Don Quixote*: two of Sancho Panza's cousins, both known for their delicacy of taste, are called on to test some wine. One says that it tastes 'leathery'; the other that it tastes 'ferrous'. They are both vindicated when the barrel is emptied and an iron key on a leather thong is found among the dregs. Here is a straightforward case of a causal relation that allows the use of the same terms both for certain qualities in certain objects and for the (same) qualities produced by the objects in a person's sense experience. The one cousin was right to say that the wine tasted leathery because the leather of the thong caused him to experience the quality of leather as a certain taste sensation. The other cousin was right to pronounce it ferrous because he tasted a certain ferrous quality caused by, and present in, the iron key. The problem with beauty, according to Hume, is that we never find anything in the object

[5] It may well be that 'cool' in the English dialect of North American undergraduates is often equivalent to 'ooh' and 'ah'. That does seem to be the case when an undergraduate spontaneously exclaims 'Cool!' If so, my suggestion earlier that «ποικίλος» functions like 'cool' needs to be revised. Porter has helped me to see this. On the other hand, it does seem that 'cool' does not always function like 'ooh' or 'ah' because people do often say 'That's cool!'. But nobody says 'That's ooh!'. I think this is instructive: it shows just how difficult it can be to specify how the terms (and exclamations) of commendation work in one's own language. How much more difficult it is to figure out how they work in a language no longer in use.

that could vindicate our judgements in the way that the discovery of the iron key on the leather thong vindicated the pronouncements of Sancho's cousins. We call beautiful objects 'beautiful' on account of *something* in the object that we take to be the cause of our response to it but we really do not know what this thing is. That is why the legitimacy of judgements of taste is open to doubt, as when people say 'à chacun son goût'. But, at the same time, people have the intuition that some aesthetic judgements are right and some are wrong. That intuition leads Hume to argue that we can justify our aesthetic judgements if we can figure out who are the true judges of beauty. Then, it will just be a matter of aligning our judgements with theirs. Hume argues that we can, in principle and without threat of circularity, figure out who these people are if we can draw up a list of personal and intellectual qualities that it seems plausible to think that they must all have. If you want to know whether *X* is beautiful, find somebody who has these qualities and ask his or her opinion.

The point here is not to foist Hume's aesthetics on poets singing in archaic and classical Greek but rather to point out that Hume's line of thinking suggests a block to my hypothetical Ptolemaic move for «ποικιλία». One might plausibly think that, though there is something in the objects that Pindar and company call ποικίλον which causes an admiring state of mind either in them or the beings—god, man or beast—of whom they sing, they know what this thing is no more than Hume can tell what it is in beautiful objects that sets the true judges on fire. Since they are presumed not to know what this thing is, there cannot be any sameness of term tracking sameness of quality because the relevant qualities cannot be directly compared: one knows directly only the state of one's own mind as an admirer of a certain object; one knows the quality in the object that caused this state of mind only by its effect. By contrast, with 'leathery', 'ferrous', 'rough' and 'smooth', «ποικίλος» is a term that does not track. That is what makes it neither primarily visual nor primarily auditory. Unless I am much mistaken, I take it that LeVen might embrace this thought. (I hesitate, though, because I am not really sure.)

I myself think that embracing the thought I am offering LeVen would be too hasty—certainly without further reflection. I do not mean to suggest that Pindar and company would have had any worked out account of «ποικιλία»—something like a definition that would have satisfied Plato's

Socrates. Why should they be expected to have done that? I mean only that the word does seem to have a range of different connotations that they may have had in mind and that may be relevant to the issues raised in LeVen's paper. That brings me to my second question: When we take these other connotations into account, is it not possible that «ποικίλος» does track sameness of quality after all?

«Ποικίλος» as an adjective often means 'varied', 'manifold', 'diverse'. These meanings play out not only in space but in time too. So we find that the word can also mean 'changing' or 'capable of change'. Diversity and mutability are qualities that can be seen, heard, and sensed by all the other human senses. As such, the words that we use for them in any language do not have to be extended or used metaphorically when we apply them to different sense qualities. Moreover, it might be thought that the diverse or mutable sense experience we have of an object—whether visual, auditory, or whatever it may be—is diverse or mutable because of the underlying diversity or mutability of the object itself. This, in turn, suggests that our words for diversity or mutability may be terms that track sameness of quality in the cause and in its effect, just like 'leathery', 'ferrous', and 'rough'. At least to that extent, perhaps «ποικίλος» is a tracking term too in as much as it can have the relevant meanings. But even if we are willing to countenance this much ptolemizing, we may perhaps doubt that the ptolemizing can be extended to the aesthetic uses of «ποικίλος»—the uses of immediate interest to LeVen. For it may be doubted, quite reasonably on the face of it, that the admiring state of mind constitutive of an aesthetic response to the admired object is an experience of diversity or mutability. Perhaps the one who admires is favorably struck by the diversity or mutability of the admired object. But does the admirer *feel* diverse or mutable in the presence of this object? Is the admirer affected by it in such a way as to *become* diverse or mutable? What would that even mean? Here I suggest that Pindar has an answer to this question.

But first, perhaps I need to spell out why I think that the question has to be asked in this way. What is at issue is an aesthetic experience: what it is like to experience a certain, positive aesthetic response to an admired object and thereby find oneself in a certain subjective state. I am asking whether this very subjective state can be ptolemized, just as Ptolemy himself may be understood to have 'ptolemized' qualities of sound at the beginning of the

Harmonics. That means that I am asking not if the admirer *hears* diversity in sound and admires the sound on account of its diversity but rather whether the subjective aesthetic state of the admirer, as admirer, is a state of being or feeling diverse that corresponds to the state of diversity in the admired object, just as the raspiness of the sound of sanding corresponds to the coarseness of the sandpaper.[6]

Now, one of the things that counts as varied and mutable for Pindar is human life. Indeed, *Isthmia* 3 ends with the remark, as rendered by William H. Race [1997], that 'As the days roll by, one's life changes now this way | now that…'.[7] The remark is explicitly intended to contrast our condition with that of the 'sons of the gods', who remain ἄτρωτοι (unwounded). These beings never experience upheaval as we do. But the contrast of interest for our purposes is with the condition of death, since Pindar sometimes plays up this contrast in such a way as to suggest that the variability of life manifests itself as vigor and fruitfulness. It is interesting to note that he sometimes uses the word «ποικίλος» in these contexts. Perhaps the prettiest example is to be found in *Olympia* 4. Pindar recounts that four members of the victor's clan were lost in battle on the same day and adds that the victory of their kinsman at the Pancratium is a sort of return to life, as summer follows winter:

> However, in a single day
> a cruel blizzard of war stripped
> their blessed hearth of four men.
> But now again, after a winter's gloom lasting months,
> it is as if the dappled earth had blossomed with red roses.

The condition of death is one of monotonous, barren gloom. The return to life breaks the monotony. It restores the clan's vigor and fruitfulness, like the 'dappled earth' that returns to vibrant bloom in summer. 'Dappled' is Race's translation of «ποικίλα».[8]

6 I thank Sara Magrin for helping me to clarify this point.

7 All my quotations from Pindar are from this edition and translation.

8 The association of diversity and mutability with life itself does not seem to be an idiosyncrasy of Pindar. It is interesting to remember in this context the confrontation between Socrates and Callicles in Plato's *Gorgias*. Callicles claims that the best life is that of the best men: it is the one that affords these men the opportunity not only to satisfy their desires but to cultivate their desires and pleasures so that they will experience as many new ones as possible. Callicles and Socrates both assume

I suggest that this passage supplies an answer to the difficult question that I raised against the ptolemization of the aesthetic uses of «ποικίλος». What would it mean for the one who admires to *feel* or *become* (inwardly and perhaps also outwardly) diverse and mutable in the presence of the relevant object? It would mean that that person or being feels, and is, invigorated by it. This suggestion may perhaps be confirmed by the second strophe of Pindar's *Nemea* 4. This time the victor has lost his father. Pindar imagines how the father, if he were still alive, would express his admiration for his champion son, who is just a boy.

And if your father Timokritos

that pleasure is the replenishment of an empty state. Socrates compares the life that Callicles is praising to the condition of a leaky jar. When he asks Callicles if it would not be preferable to live a life like the condition of a well made jar without holes that has been filled and properly sealed so that it never loses any of its contents, Callicles protests that that would be the life of a 'stone'. It would have no variety or mutability of pleasure—indeed it would be without pleasure!—and, hence, it would not even count as a life at all [*Gorg.* 494a–b]. It is interesting too that, in *Republic* 8, when Socrates and Adeimantus try to sketch the psychological profile of the democratic man living under a democratic constitution, they stress the great variability and mutability of that man's pleasures. The life of the democratic man is (almost) the life that Callicles praises in the *Gorgias*. (I say 'almost', because it ultimately finds its purest expression in the life of the tyrant). In G. M. A. Grube's translation, as revised by Reeve, Socrates traces the emergence of the democratic man to the moment when the son of the oligarch starts to associate with 'wild and dangerous creatures who can provide every variety of *multicolored* pleasure in every sort of way...' [559d–e: Grube 1997, 1170 (my emphasis)]. 'Multicolored' is Grube's translation of «ποικίλαι» as applied to pleasures. The life of the democratic man, like the one praised by Callicles is not merely a life of pleasure, it is a life of diverse and mutable pleasures—one that may well be characterized as ποικίλος. Callicles himself might have used this word; and if he had, he would have used it as a term of commendation. It is clear that Plato is not recommending either the life praised by Callicles or that of the democratic man. For that reason, he uses «ποικίλαι» at *Rep.* 559d as a term of censure. All of this suggests that Plato was responding to a view out there that associated diversity and mutability with life (and often did so by way of the diversity and mutability of pleasure, as a fundamental part of human life). Plato himself must have felt the pull of these associations too—or, at any rate, some of them. In *Soph.* 248e–249a ff., he represents Theaetetus and the Eleatic Visitor as shrinking from the idea that *that which wholly is* might be deprived of change and life. As Theaetetus says, that would be a frightening thought.

> were still warmed by the blazing sun, often would he have
> played an elaborate tune on the lyre, and, relying on
> this song, would have celebrated his triumphant son.

Timokritos cannot do this, of course, because he is dead. Pindar himself will have to take over because only the living can praise in song. That is presumably because such praise requires vigor; and the vigor of the poet in particular reflects, and is inspired by, that of the hero whom he admires and celebrates. The poet puts this vigor on display in the song of praise that he sings, which outwardly expresses his admiring state of mind. One should, therefore, expect the song of praise itself to be vigorous, i.e., varied and diverse. So it is. Pindar says that, were he alive, Timokritos himself would celebrate his son by playing 'an *elaborate* tune on the lyre'. This is Race's translation of «ποικίλον κιθαρίζων».[9]

If my thoughts are on the mark, I leave it as a question for LeVen why we should not think of «ποικίλος» as an aesthetic (musical) term that functions along the lines suggested by Ptolemy for 'rough' and 'smooth'. What is there in classical and archaic Greek lyric poetry that would block the ptolemization of «ποικίλος»?[10] LeVen's paper is stimulating. That is because she is willing

9 The suggestion implies, of course, that «ποικίλον» might be translated by 'vigorous' or 'lively'. That will seem implausible if we navigate by the first meaning of the word that turns up for «ποικίλον», 'dappled'. It would seem that 'dappled' and 'lively' do not mean precisely the same thing. Granted. But first of all we do not call a melody or a song 'dappled'. We might call it 'elaborate', as Race suggests in his translation. We might also characterize an elaborate tune as a lively one, i.e., as one that manifests its liveliness in its diverseness. I thank Sara Magrin for helping me to clarify this point too.

10 One thing that might conceivably block it is a thought that James Porter has shared with me. If ptolemization goes through, the effect of something ποικίλον on the admirer is to induce in that person or being a subjective state that might also be characterized as ποικίλον. That will conceivably work for the ποικίλον, assuming that it can be understood as sometimes equivalent to being or feeling enlivened by something. But even if we grant that it may, it does not seem that other aesthetic terms allow for ptolemization. Porter points out that I might admire subtlety in something and not myself by translated into a subtle subjective state. Perhaps I might also admire the beauty of something, yet not feel or become subjectively beautiful—and so on. These points must be conceded. I am just not sure what they show. They might just show that English aesthetic terms cannot be ptolemized. But they might also show that some aesthetic terms, in whatever language, cannot be ptolemized. I am not

to try out a big idea and also because the idea itself is intrinsically interesting. This is commendable. But it is just as important to clarify what an idea boils down to and to test it as best one can. That is what I missed in her paper. I could not tell precisely how she understands the language of ποικιλία to work.

When one considers the ποικιλία of *GRMS*—its diversity of topic as reflected in the two papers that I just reviewed—it is clear that the journal faces a challenge: maybe not so much to attract submissions but rather to attract readers—or rather, certain circles of readers. People who explicitly think of themselves as interested in ancient Greek and Roman music will not have to be prodded to pick up a copy of *GRMS* because there has been no journal devoted to their topic until now. But, as the papers by Vendries and LeVen show by way of example, there must be plenty of potential readers out there who probably do not think of themselves as especially interested in ancient Greek and Roman music as such, but who would likely find articles of interest to them in this journal: classicists, Egyptologists, Byzantinists, historians of Renaissance polyphony, historians of science in the Islamic world, and no doubt many others. It is to be hoped that the editorial board of *GRMS* will undertake (if it has not done so already) a vigorous outreach campaign to the neighboring disciplines so that the journal is actually read by people in all the fields that the journal will likely cover.

It remains for me to call attention to a special feature of the inaugural issue of *GRMS*. This issue has two, distinct parts. The two papers that I have just reviewed appear in the second part along with two other papers. One is by Massimo Raffa about technical vocabulary in ancient Greek music theory. It regards a debate among music theorists—our witness is Porphyry in his commentary on Ptolemy's *Harmonics*—about whether there is, or should be, a difference in meaning between «λόγος» (as in 'numerical ratio') and «διάστημα» (as in 'interval'). I will not discuss this paper here, not because it is uninteresting (on the contrary) but rather because it is time to push on. The other paper in the second part of this issue is by Maurizio Bettini. I will

sure, however, that they show that no aesthetic terms can be ptolemized. Ptolemy himself may be instructive here. He can be understood to say in *Harm.* 1.3 that human reason in studying mathematics (mixed as well as pure) apprehends beauty and goodness and that this apprehension is not merely theoretical: it has practical applications, one of which is to make human beings good and beautiful [Düring 1930, 93.1–10]. I myself do not think that this is a crazy idea.

discuss it later, in part because of its intrinsic interest but also because of a question that it raises about the principal object of discussion in the first part of this issue of *GRMS*, which is an archeological discovery made in Athens on 13–14 May 1981.

On that date, two tombs were excavated at Daphne (at 53 Odos Olgas). The first tomb contained one adult human skeleton of uncertain sex (the person would have been in his or her 40s) and four *lekythoi*. The second tomb contained one adult skeleton of uncertain sex (this person would have been in his or her 20s), nine knucklebones (toys?), a chisel, a saw, a writing case containing a bronze stylus and ink-pot, four wooden writing tablets with traces of wax (and fragments of a fifth), a papyrus roll (complete on discovery and now completely disintegrated), pieces of tortoise shell (plaques) presumed to have once been the soundbox of a lyre, one wooden *aulos* tube (the tube that would have completed the pair is missing), and fragments of a harp.

In the first paper on this find, Egert Pöhlmann reviews the excavation of the two tombs and their contents. Their close proximity is thought to indicate a family burial but no stela was found and so the identity of these people remains unknown. The style of the *lekythoi* (their shape and their paintings) suggests for the first tomb a date right around 430 BC, as argued by Erika Simon and Irma Wehgartner. Chrestos Terzes argues that the harp in the second tomb may be dated to some time between 430–410 BC. If he is right, then the two graves are almost exactly contemporaneous. I am doubtful, not because his claim is impossible but because it ultimately rests on just five Athenian vases depicting harps of the relevant sort by two artisans whose period of activity apparently coincides with the very two decades in question. Since the literary evidence for such harps indicates their presence in ancient Greek musical life from the early fifth to the second century BC, why pump for such a precise dating of the second tomb?[11] The find would be no less interesting if it turned out that the two tombs were a generation (or more)

[11] To be sure, Simon and Wehgartner offer an even more precise date for the four white *lekythoi* found in the other grave. But the weight of the evidence seems to support this decisively: the style of the ornaments, the choice of colors, the techniques used, the fact that the four pieces must have come from the same workshop and must have been purchased together (namely, on the occasion of the deceased's funeral) all seem to exclude other dates [see 64–65].

apart, which is often (not always, but often) the case with family burials (if indeed that is what we have here).

Though the two tombs attracted media attention at the time of their discovery and excavation, they have apparently gone unstudied until now. Given the obvious interest of their contents, it is natural to hope that they may shed new light on ancient Greek musical practice.

There have been *aulos* finds before, including complete pairs (most of them made of bone). Indeed, Stelios Psaroudakes uses these finds in his paper as a comparison class to get a fix on the widowed Daphne *aulos* tube and bulb that terminates it. But here there seem to be no great surprises, except for the absence of a mate. By contrast, I surmise that the Daphne harp is the first and only harp to have come to light. But, to my frustration, I cannot really be sure from the paper devoted to it by Chrestos Terzes. Three pages into his paper, he mentions in passing that the Daphne harp is 'unique'. But it is not clear what this means. Does it mean, in fact, that no other ancient harp has ever turned up until now? Does it mean that no other ancient harp has turned up at Greek excavation sites? Is it possible that harps, or harp fragments, of Greek design have turned up at Egyptian excavation sites?[12] Unlike Psaroudakes, Terzes does not survey other harp finds. Should we infer from this that there are none to survey? (Why must readers be asking *this* question? *GRMS* is not supposed to be a journal exclusively devoted to ancient instrument reconstruction; accordingly, it should require that its contributors on this subject (and all others) communicate effectively what they know.) As for the soundbox of the tortoise-shell lyre, we learn from Psaroudakes first of all

[12] The triangle harp is attested in Egypt. In fact, there are two photos of two different terracotta figurines playing this instrument at the end of Vendries' paper. But the reader can see that these harps are not the same as those discussed by Terzes, at least by comparing the figures shown in Plates IX.1b, 2b at the end of Vendries' paper with the five Athenian vase paintings that Terzes reproduces in Plate VI.2. The Egyptian harp seems to have been played standing up; the Athenian harp, sitting down. All three sides of the Athenian harp are made of wood: of the two sides forming a right angle, the one rests on the left leg of the seated player, the other stands upright; the hypotenuse (and with it, the soundbox of the instrument) is away from the player's torso. By contrast, the Egyptian harps shown in Plates IX.1b, 2b have only two wooden sides. The side of the triangle farthest away from the player's torso is not a wooden part fitted to the rest of the frame, but a string, the longest one of the instrument as a whole.

that Hellenistic tortoises and their present-day descendants are the same in species (of which there are three) and, hence, that their respective shells can be compared. But we learn second of all that such comparisons indicate that the number and pattern of shell plaques on the Daphne find are anatomically incorrect: the wrong number and the wrong arrangement [see Plates V.12–13 to spot the difference]. This unfortunate state of affairs seems to have come about *after* the excavation, i.e., as a result of a botched attempt to reassemble the plaques, which had long since come apart. *Ô stupeur!*

There remains one last question that I wish to raise. It concerns the professional, or perhaps hobbyist, identity of the occupant of the grave whose contents I have been discussing. Part 1 of the inaugural issue of *GRMS* is entitled 'The Musician's Grave'. That is fitting in view of the musical instruments discovered at the site. But the presence of a decayed papyrus roll and writing implements might also suggest a literary profile. Perhaps we might speak of the 'lyric poet's grave'.[13] It is stimulating to reflect on this thought in light of Maurizio Bettini's contribution in part 2 of this issue of *GRMS*.

Bettini's concern is not music in ancient or classical Greece, nor is it even music in the first instance. It is, rather, 'effective speech' in the Roman context.[14] Typical examples of such speech include formulae used for good

[13] In fact, the grave has more usually been referred to in newspaper reports as the 'poet's grave' [84].

[14] I should say that Maurizio Bettini has a broad scholarly interest in voice/speech in general and not only human voice/speech. He is the author of 'Laughing Weasels' [2008], in which he asks whether weasels can plausibly be said to laugh, as suggested by Ovid's story of the transformation of one of Alcmena's attendants, Galanthis, into a weasel in the *Metamorphoses*. To my ear, it has always seemed that the domestic ferret 'giggles', 'chuckles' or 'chuckle-beeps'—at least when it seems to be enjoying itself. Bettini argues that the sound which Galanthis makes during her encounter with Lucina (Juno's envoy, whose mission was to prevent the birth of Hercules) is, upon transformation, an 'explosive bark or chirp' of terror or stress. I can attest that the domestic ferret does indeed shriek in terror when appropriately terrified. But Galanthis seems no more terrified or even just stressed by Lucina after transformation than before. She laughs at Lucina as a human because she is amused at having tricked Juno's envoy into allowing her mistress, Alcmena, to give birth (after seven days and seven nights of labor). But she continues to laugh as a weasel. There is no reason to think that she is any less delighted, as a weasel, at having outwitted Lucina. So she giggles or chuckle-beeps. I should add that Bettini's argument rests

augury and the enactment of laws. All such speech rests on some authority. This authority (usually divine) makes itself felt not simply in the content of the speech but in its mode. Effective speech sounds catchy to the ear, as in the *tria verba* of the *praetor* passing sentence—*do, dico, addico*—or in the formula pronounced for good augury—*quod bonum, faustum, felix fortunatumque esset*. The authoritative effect is achieved by semantic parallels, alliteration, wordplay, and word rhythm more generally. These devices allow the voice of the relevant authority to be heard coming through, or supervening on, the words themselves as uttered by the relevant human officer. Thus, it is natural to include poetry as an instance of 'effective' speech since the same linguistic devices are used: when properly used, they have the effect of compelling the minds of listeners to attend, to recall, and even to be swept away—as though under the power of some higher, non-human authority. The technical term for this effect is 'delectatio', which does not mean 'delight' so much as 'being drawn to bait'. However remote we may be from ancient Rome, we are familiar with this effect: it is felt in the best of advertising jingles. 'Sachez chasser vos achats sur le champ!' was in circulation about 10 years ago to promote the Yellow Pages in Montreal: it has the jingly features of Roman effective speech (or French analogues thereof). We would be wrong to dismiss it as lacking any *auctoritas*. Silly though it may be, it establishes its *auctoritas* by being catchy enough to hook people. That it 'hooked' me at least is obvious just from the fact that I remember it to this day with much amusement and fondness (how fast can you say it out loud three times in a row?)—even as the Yellow Pages are ceding their authority as a search engine to the faster, vastly more powerful and far-ranging services now available on the internet. In the context of ancient Rome, the hook and bait of poetry was typically used to subdue audiences in the theatre and to get them to attend to the action on stage as though it were reality. It is here that music makes its entry.

in part on passages in Horace perceived to be onomatopoeic. But, for the reasons that I mention in footnote 1 [p. 148, above], I doubt that onomatopoeic words really help us hear what the mimicked sounds really sound like. A Martian visiting Earth, after reviewing the words that different human languages have invented to convey 'what dogs say' would probably have to suspend judgement on the question what dogs really say.

As Bettini points out, music could and did heighten the effects of 'effective speech' in the theaters: no doubt by playing up, or juxtaposing with, the rhythms and sonorities of the words. The upshot of his paper is that music and poetry in ancient Rome (as in other cultures, ancient and extant) were intimately related to each other in a way perhaps no longer obvious or natural to us: they formed, together with the techniques of stagecraft, a total means of expression that had at least one significant effect on the world, namely, to make theatre goers pay attention. It seems plausible to think that the phenomenon that Bettini describes in the ancient Roman context had a Greek analogue. That analogue would be the total art form that went by the name «μουσική»: neither melody on its own nor mute verse recorded on a wax tablet nor a lone dancer kicking up his heels but rather melody, catchy verse, human movement, and stagecraft woven together to make a splash and to have an effect on the world. The intended effects could range from securing patronage for stars, honoring 'good' tyrants and remonstrating with 'bad' ones, reproducing the city, maintaining good relations with the gods, luring fellow citizens to the bait of entertainment. None of these effects could be achieved except through the appropriate higher authority whose voice—one would say, following Bettini—could be heard rising above and penetrating through the spectacle of it all.

It was the ambition of contributors to the recent volume *Music and the Muses* [2004], edited by Penelope Murray and Peter Wilson, to study the phenomenon of μουσική in all of these dimensions. Perhaps they would be willing to think of themselves as doing for Athens of the fifth and mid-fourth centuries what Bettini tries to do in *GRMS* for Rome (or perhaps the question should go the other way around).With that thought in mind, one cannot help but wonder about the person buried in the second grave at 53 Odos Olgas. What was that person's relation to μουσική as the 'effective', total means of expression just described? Did that person contribute verse as well as melody? The question is all the more alluring if it turns out that the grave goes back to the last quarter of the fourth century (100 years later than Terzes and company say). For that is a time, even in Athens, when the remains testifying to μουσική are scarce (the remains suggest that the fifth century was its heyday).[15] One would naturally hope to find answers—or,

15 In that case, the find might conceivably offer another datum of significance for the newest project undertaken by Pauline LeVen. In *The Many-Headed Muse: Tradi-*

anyway, hints—in the written remains found at the site. These are the object of M. L. West's contribution. Alas, his report is very discouraging.

The pieces of writing in the second grave at 53 Odos Olgas would compel our attention even if they turned out to be a shopping list or something equally mundane. They include the oldest Greek papyrus now known to us (assuming Terzes and company are right about the dating). Until the discovery of the Derveni papyrus, which is about 100 years later than this one (on that assumption), most of our surviving Greek papyri were discovered in Egypt, where they were preserved by the dry conditions. In Greece proper, as in Italy, papyri were lost due to the far damper conditions. The Derveni papyrus, discovered in 1962 in Thessaloniki, survived because it had been carbonized by the heat of a funeral pyre. The papyrus and wooden writing tablet discovered in the grave under discussion survived in very humid conditions because they were enclosed in a marble sarcophagus. The papyrus, at any rate, survived as 'a shapeless, flattened mass', as reported by the Athenian correspondent to *The Times* on 25 May 1981 (as quoted by Pöhlmann [9]). I had imagined something like what happens to your research notes if you neglect to extricate them from your shirt pocket before throwing all your laundry into the washing machine, except that this papyrus roll was also thoroughly rotten: much of it had disintegrated into sand and earth. The reader can see what it looked like before restoration by examining Plate I.2. Nevertheless, bits were recovered. But that is all they are: no continuous text emerged out of them. The reader can see roughly the state of things after restoration by examining Plates IV.6–10. The surviving bits of writing tablet can be seen on Plates IV.1a–4. Athina Alexopoulou and Agathi-Anthoula Kaminari subjected them (and other artifacts found at the site) to noninvasive multispectral imaging. The reader can see from Plates II.3a–8b how these techniques brought some of the papyrus to life and from Plates II.9a–12b clearly detectable traces of writing on pieces of wax still clinging to the writing tablets (tablets of the same design as those used by the boy shown learning his lessons on the Douris Cup). But for all that, West

tion and Innovation in Late Classical Greek Poetry [2014], she tries to counter the prevalent idea that lyric poetry somehow ran out of steam by the late fourth century.

could recover only a snatch of a word here or there.[16] We can always hope that future advances in imaging technology will recover more in the future.

The written texts found at 53 Odos Olgas may never shed light on the professional or hobbyist profile of the person buried at that site. Was that person a musician only or a singer of verse who would accompany his/herself? It is true that no one can accompany his or her own singing on the *aulos*. The presence of an *aulos* in the grave might point to a fulltime musician or—anyway—a part-time *aulete*. On the other hand, there is no way of knowing whether the person ever owned a complete *aulos* pair: all that was unearthed was a single pipe without its mate. Then, again, a chisel and a saw were also discovered in this grave. So perhaps we have neither a poet nor a fulltime musician nor any direct contributor to μουσική as such but perhaps a maker of musical instruments. Perhaps the written texts were accounts of instrument design and craftsmanship. Perhaps the widowed *aulos* tube is not a widow after all but rather the lone maiden tube that the craftsman (he would then surely be a man) finished before his untimely death.

It remains now only to extend belated birthday greetings to Andrew Barker along with hopes for the success of *Greek and Roman Musical Studies*.[17]

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[16] One thing that the infrared imaging managed to establish is that the presence of interlinear letters on ΜΠ 8523 is due to the fact that text shows through from other layers of papyrus that could not be detached. As West points out, this blocks the temptation to read these letters—as inventory notes apparently do—as musical notation [83].

[17] I am grateful to Sara Magrin of the Classics Department at the University of California, Berkeley and James Porter of the Classics Department at the University of California, Irvine for taking the time to read and comment on this review.

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Théophraste. Les causes des phénomènes végétaux. Tome I. Livres I et II
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Theophrastus, student and successor of Aristotle at the head of the Lyceum, composed two major treatises on plants that are preserved to this day: *Historia plantarum* (*Enquiry into Plants*) in nine books, and *De causis plantarum* (*Causes of Plant Phaenomena*), of which six books are preserved (see below). Both treatises complement each other: the *Historia* describes plant species, while the *De causis* deals with questions such as plant generation, growth, other plant physiological phaenomena, and plants' interaction with their environment. Theophrastus cross-referenced both treatises, indicating that he considered them to be part of the same enterprise, namely, to study plants as thoroughly as Aristotle had studied animals.

Suzanne Amigues has devoted most of her academic career to editing and studying the *Historia*, producing the Belles Lettres edition in five volumes, which is undoubtedly a feat of scholarship [1988–2006]. Her French translation has also been reprinted as a single volume, beautifully illustrated with numerous photographs [2010]. In addition, many of her articles relating to the study of ancient botany are gathered in a single volume [2002]. So far, however, Amigues had paid far less attention to the *De causis* than to the *Historia*. With the present volume, Amigues rectifies the situation by offering the first edition, translation, and commentary of books 1 and 2 of the *De causis* since the Loeb edition by Benedict Einarson and George K. K. Link [1976–1990] in three volumes.

The *De causis* as it is preserved includes six books. Books 1 and 2 deal with 'natural' plant physiology, that is, phaenomena that pertain to φύϲιϲ (nature). Book 1 covers modes of plant 'generation' (which include both what modern botanists would consider to be sexual plant reproduction and

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asexual plant propagation), including grafting and spontaneous generation; and plant growth, budding, flowering, and fruiting. Book 2 deals with the impact of the environment (winds, nature of water, nature of the soil) on the natural generation and growth of plants. Books 3 and 4, on the other hand, examine the impact of the art (τέχνη) of agriculture on plant functions. Books 5 discusses diseases and death of plants. Book 6, finally, examines issues relating to the taste of plants. The original *De causis* may also have included a book on plant smells (a theme developed in the independent Theophrastean treatise *On Smells*), and a book on wine and oil.

The *De causis* was an important source for the Roman agronomists and for the encyclopedist Pliny the Elder. However, from the Middle Ages onwards, it has attracted less scholarly attention than the *Historia*. One of the reasons for this comparative neglect in the modern period might be the fact that most physiological theories expounded in the *De causis* have been discredited, while botanists still admire the descriptive work Theophrastus achieved in the *Historia*—a work that has earned him the title of 'Father of Botany'. It is important, however, to judge ancient science on its own terms. In this non-positivist context, the *De causis* is extremely rich and worthy of extensive study by historians of science. Here Theophrastus engages with the theories of numerous Presocratic philosophers whose works are lost save for short fragments. He also develops his own brand of Aristotelian biological thinking. Thus, like Aristotle, he draws numerous comparisons between plants and animals. For instance, at *De causis* 1.16.4, he argues that, in both plants and animals, the over-consumption of food leads to under-production of seed/semen. Theophrastus also borrows Aristotle's notion of 'coction' (πέψις), a notion most prominent in the *Meteorology*, and posits it as central in plant physiology: it is coction that is responsible for plant growth and for plant generation. Theophrastus' thinking, like Aristotle's, is teleological: plants have as their final cause the survival of humans as well as their own propagation.

Hopefully Amigues' magisterial work will contribute to a revival of scholarly interest in the *De causis*. Amigues offers a definitive edition of the text, one that is in many ways superior to Einarson's edition. Indeed, Einarson had on numerous occasions 'corrected' the text of Theophrastus in order to make it more elegant. Amigues allows Theophrastus' text to be elliptic, idiosyncratic, and grammatically divergent from the norms of Attic prose. She argues that

Theophrastus wrote in a rather loose style in the botanical treatises because neither the *Historia* nor the *De causis* were meant for publication. Rather, they were both sets of lecture notes, destined to be delivered orally. I would warn against too neat a dichotomy between 'notes' and 'editable' text in the ancient context but it remains that Amigues has made the right choice in refusing to 'improve' Theophrastus' prose. Amigues' French translation is lucid and consistent. However, it is in the notes that Amigues' extraordinary scholarship really comes to the fore. She has fascinating notes that clarify both ancient and modern understandings of plant physiological processes, etymological explanations, grammatical clarifications, historical insights (including insights into the history of science), references to archaeological discoveries of plant remains, and so on and so forth. Amigues' work is admirable and I very much look forward to the next installments in the Belles Lettres edition of the *De causis*.

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Sextus Empiricus: Against the Physicists translated and edited by Richard Bett

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What are sceptics doing when they do physics? Richard Bett's new translation of Sextus Empiricus' *Against the Physicists* [*AP*] should help shed light on this question. In my view, it will do so by making *AP* eminently more readable than it previously was. Readability, here, includes many features relevant to philosophical analysis: above all, a precise translation, a crisp introduction, and a masterful focus on the structure of the argument. In a nutshell, Bett offers a philosopher's translation. It brings out the precise version of scepticism found in *AP*. And it will enable those who care primarily about the topics discussed in *AP*—god, causation, body, parts and whole, place, time, number, generation and perishing—to access arguments of independent interest.

Bett's introduction to *AP* achieves much in a short space: it situates the treatise within Sextus' work and sketches longstanding issues about different versions of Pyrrhonian scepticism. Bett's *AP* completes his series of translations of comparable treatises by Sextus, namely, *Against the Ethicists* (1997) and *Against the Logicians* (2005). These three texts are traditionally contained in a larger work called *Against the Mathematicians* [*M*], wherein books 7–8 cover logic, books 9–10 are devoted to physics, and book 11 to ethics. Bett argues, more clearly than I have seen before, that referring to the three treatises as *M* 7–8, *M* 9–10, and *M* 11 is nonsense. So is the customary translation of the collective Greek title, *Against the Mathematicians*, since «Πρὸς μαθηματικούς» means 'Against the Learned' [ix]. The traditional rendering is not just a mistranslation. It is misleading in so far as it suggests a focus on mathematics that is not characteristic of Pyrrhonism or of the relevant dogmatic philosophies. And the title *Against the Learned* really only

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works for *M* 1–6, a self-standing treatise against grammarians, rhetoricians, geometers, arithmeticians, astrologers, and musicians.

Bett continues to make the case for a position that he has occupied for years, namely, that the *Outlines* represent a later and more sophisticated stage in Sextus' thinking than the *M*-treatises. The latter at times show signs of an earlier, more dogmatic scepticism [xxiii], which arrives at conclusions such as 'god (cause, etc.) does not exist' or 'nothing is added to anything' and so on. Such conclusions are sometimes called negatively dogmatic. Though they differ from the claims of non-sceptical philosophers precisely by being 'negative', they are conclusions in which the world is said to be a certain way. In the *M*-treatises, Sextus seems to make some effort to combine such negative dogmatism and suspension of judgment. By the time he writes the *Outlines*, Sextus may have been more acutely aware of the deep differences between the two. Instead of integrations that work more-or-less, he aims for a consistently sceptical outlook—the outlook of suspension of judgment and continued investigation.

In his comments on these matters, Bett says that a version of Pyrrhonism associated with Aenesidemus shines through, marked by 'a willingness to deny the existence of various things posited by the dogmatists'. Further, he notes that at times it looks as if the purpose was to 'undermine the dogmatists' positions' rather than create suspension of judgment [xxii]. My prediction is that, with the help of Bett's translation, scholars will argue that there is more to be said. Here I mention just two observations. There may be a tension between the work's title, *Against the Physicists*, and the ambition to arrive at conclusions in physics—conclusions about cause, time, and so on. Relatedly, there may be two different kinds of negatively dogmatic conclusions, namely, that some dogmatists are wrong or that some entity of which they offer an account does not exist. Bett highlights that the scepticism of *AP* is quite concerned with the *existence* of certain entities—and that is an aspect of Pyrrhonism which continues to be under-explored.

Bett's translations of the *M*-treatises may appear to be a paradoxical undertaking. Bett assesses these treatises as, comparatively speaking, flawed. He even remarks at the end of his introduction that this concession may not make for good advertising [xxiv]. But the care Bett puts into translating *AP* may lead one to suspect that, even if he thinks the scepticism of the *Outlines* is more sophisticated, there is something worthwhile in understanding the history of Pyrrhonism. As I would put things, the metaphysical bent of earlier scepticism—which comes out in sceptical conclusions such as 'there no more are than are not causes' [*M* 9.195]—may not work *as scepticism*. Pyrrhonists arrive at a conclusion, cease investigating, and thus no longer merit the designation of sceptics (which literally means 'investigators'). And yet the kind of metaphysics that employs the 'no more this than that' phrase, presumably to describe reality as indeterminate, may be interesting in its own right.

Bett does not make this point, though his earlier publications on Pyrrho have done much to draw attention to the metaphysical side of early Pyrrhonism. His own way of counteracting the concession that *AP* is less sophisticated than the *Outlines* is either an intentional understatement or it undersells the contents of the book. He remarks that '[i]n compensation...*Against the Physicists* is a much fuller and richer treatment of its material' [xxiv]. This may sound as if *AP* was just a lot of text, a wordy version that philosophers are unlikely to prefer to more concise writing. This, I suspect, is the received view. In Bury's translation for the Loeb series, *AP* may appear just to drag on and on, with ever more quotes on this and that. Bett could have been, I think, more on the side of the text that he is presenting. In the Greek and in his translation, it contains arguments on causation, parts and whole, and so on, that are sophisticated.

Admittedly these are not, properly speaking, sceptical arguments. They are arguments that sceptics invoke on one side of a debate that explores opposing views. They are either ascribed to some philosophers or formulated by the sceptics as lines of arguments one encounters (and in this latter case, though they are not sceptical arguments *sensu stricto*, they bear witness to the philosophical sharpness with which sceptics explore the options). Scholars of ancient scepticism tend to focus elsewhere, namely, on how to understand the structure of sceptical investigation. But philosophers interested in, say, causation or parts and wholes, would be well served by picking some particular stretches of text, simply with a view to figuring out whether a given argument is compelling.

Some stretches of Aristotle's metaphysics or physics receive this kind of attention. Here scholars have long supplied translations that enable others, those who do not care primarily about ancient philosophy but, say, about parts and wholes, to dissect a passage solely because it contains an argument one is interested in. The standard translation of *AP* prior to Bett's translation, Bury's translation in the Loeb Series, does not invite this kind of approach. Whatever its virtues are, it does not suggest that the translator attends to the subtleties of philosophical theories. Bett's translation achieves precisely this. It makes accessible a wide range of philosophically sophisticated arguments, arguments that can be studied independently of larger–scale interests in antiquity. Thus, there is a way in which sophistication is lacking in *AP*, namely, in so far as Sextus' own ways of presenting scepticism become more sophisticated later on when he writes the *Outlines*. But there is another way in which sophistication is far from lacking in *AP*, namely, in so far as any number of subtle arguments are adduced on both sides of a given question.

Perhaps this applies in particular to the examples that I have just mentioned, causation and parts and wholes, where the questions that philosophers ask today involve some longstanding puzzles. Matters look different for Sextus' longest discussion within *AP*, on god. Here one may not share the most basic premise, namely, that god is a topic—the most fundamental topic—of physics. Nevertheless, Sextus' discussion has some real virtues, perhaps most conspicuously in aiming to keep separate the question of how it is that all human beings appear to have a notion of god and the question of whether god exists. In Bett's translation, this mini-treatise on god could be included in a reading list in the philosophy of religion. It would make an entirely respectable companion for more widely known historical texts.

A further remark in Bett's introduction makes for a nice transition to some comments about his translation. Discussing why Sextus is neither a doxographer nor a 'copyist', Bett asks whether he 'may offer a purely personal impression'. This impression is

> that Sextus' writing has a consistent authorial personality, a voice that is distinctively his own; in all his works there is the same dry wit, the same energetic but low-key approach to laying out the arguments on either side, and the occasional delight at skewering the dogmatists' positions. [xix–xx]

If I too may offer a personal impression, I would say that Sextus has found his equal in Bett as his translator. This shows in brief phrases, as when Bett lets Sextus speak of the 'chorus of Academics' [*M* 9.1]. It also shows in a nice willingness to be literal. For example, after reporting Aristotle's views on god, Sextus says «[τ]οιοῦτος μὲν καὶ ὁ Ἀριστοτέλης». In Bury's translation, this means '[s]uch, then, was the view of Aristotle,' hiding that Sextus does not appear to hold Aristotle in the kind of esteem that readers may expect. Indeed, Sextus takes the Stoics much more seriously than Aristotle. The Stoics, he assumes, came up with the 'best' framework in physics [*M* 9.12]. This evaluation may be rather inconceivable for many in the field. It takes a seasoned researcher on Hellenistic philosophy, someone with some sympathies for sceptics and Stoics, to translate as Bett does, '[t]hat is what Aristotle is like'.

Bett's rendering of Sextus' formulations is as dry and understated as these very formulations throughout. It ends, for example, with a curt 'enough said' [*M* 10.351], which Bury made more charming ('Let these, then, be our answers to those of the philosophers who are Physicists'). Bett's translation is, on the whole, quite literal, which will help readers who do not want to turn to the Greek text. The Greek shines through remarkably well and one need not fear that distinctions or oddities are glossed over. Bett's translation includes a helpful glossary of core terminology. Some of the entries have a low-key, ordinary tone that one rarely finds when translators aim to capture technical terms. For example, Bett translates «ἀναιρῶ» as 'do away with'. In spite of its colloquial tone, this verb serves as a technical expression. At the more dogmatic moments in *AP*, Sextus says that sceptics 'do away with' god, cause, and so on, rather than saying that they suspend judgment on these matters. Bett's translation captures an ambiguity in Sextus' sceptical investigations, between a calling into question of the existence of *X*, and a calling into question of theories about *X*. If the sceptics 'do away with *X*'—god, cause, place, time, and so on—this may include aspects of not positing *X*'s existence as well as aspects of arguing against theories that offer accounts of *X*.

Another interpretive choice worth mentioning is 'impasse' as translation for «ἀπορία». This is very close to the Greek: «ἀπορία» means literally that there is no further road, no way out, or no available path. When a thinker arrives at an impasse, there is, as it were, no further thought or argument or theory that can be tried out. All ideas that presently seem available have been explored. The cognizer finds herself coming to a halt, though not because she has identified a route that led to her goal. In *AP*, Sextus often describes the outcome of sceptical investigation in terms of ἀπορία and he refers to the sceptics (in Bett's translation) as 'bringers of impasse' [*M* 10.340]. Here Bett's literalness is more than just helpful. It is crucial for avoiding philosophical confusion.

Bury employs an expression that, presumably, sounds more familiar, calling the sceptics ‘Doubters’. This translation resembles a traditional description of the state of mind in which one is ‘stuck’: one is ‘perplexed’. And yet it is pernicious: it suggests that ancient scepticism is essentially like modern scepticism, concerned with doubt to the extent that ancient sceptics would call themselves doubters. Bett’s translation, on the contrary, conveys an acute awareness of the ways in which translations can lead readers down the wrong path and a commitment to avoid setting off misleading chains of associations.

Bett emphasizes that Sextus begins *AP* by saying that he will address basic issues in physics [xiii–xiv]. Rather than study this-or-that in a piecemeal fashion, the sceptics investigate fundamental matters. Arguments formulated here will turn out to be arguments ‘against everything’, as when towers crumble because the foundations of a building are torn away [*M* 9.1-2]. What does it mean that the sceptics argue against everything?

The idea that the sceptics’ arguments will shatter the edifice of physics by destroying the foundations is reminiscent of Cartesian scepticism and, hence, of a kind of scepticism that differs from Pyrrhonism by being systematic—or so scholars tend to assume. Apart from the metaphor of tearing down a building, Sextus uses a further comparison. Others may argue against dogmatists like those who catch birds with lime and a twig. The sceptics cast a net, aiming for a method that is all-encompassing. Sextus seems to invoke two sorts of assumptions, that certain parts of physical theory are more *fundamental* and that they are more *general* than others. Either way, getting them right—or failing to do so—affects all of physics. Can Sextus afford these assumptions? Can a sceptic presuppose that there is a structure to physics such that arguments against certain claims are in effect arguments against ‘everything’?

Sextus’ transition to physics in the *Outlines* is a toned-down version of the beginning of *Against the Physicists*. In a brief sentence, Sextus says that his method will be similar to the one employed in logic. He will not address each dogmatic statement in order but address more general matters, ones that are inclusive of the rest [*PH* 3.1]. This is not altogether different from the announcement in *AP*. But it bears the traces of other ‘polishing’ that Sextus seems to do in the *Outlines*. By skipping the metaphors and the bravado of *AP*, Sextus almost hides that the material which he is about to present has a systematic structure. Readers may perceive his discussion of physics in the *Outlines* as addressing topics that dogmatic physics considers important, no more and no less. And this may well suit Sextus. I share Bett's instincts about the relative chronology and the relative sophistication of the *Outlines* as compared to the *M*-treatises. The systematic pretensions of *AP* may belong to those aspects of earlier Pyrrhonism that Sextus later on prefers to downplay.

Contrary to Sextus' introduction in *AP*, scepticism seems better served by a piecemeal approach. In part, this is because, otherwise, the story that sceptics investigate because they are disquieted by discrepancies loses plausibility [*Outlines* 1.12]. For it to make sense, sceptics need to go after puzzles as they arise for them, rather than presuppose a systematic picture of all of physics, as if it were a roadmap for tearing it all down. In part, this is because they otherwise appear to be too much on board with dogmatic assumptions about fundamentality. Who says, for example, that the most important topic in physics is god/gods? Some dogmatists do, others do not. Consider another example. In the *Outlines*, a short discussion of matter is inserted between the chapters on causation and the chapters on bodies ('corporeals'). Given Stoic premises, that appears unnecessary. The 'material principle' is corporeal and it is passively affected. Hence, its discussion is plausibly included in discussions of bodies and of that which is passively affected.

Could this be the underlying rationale of the division into topics in *AP*? In *AP*, matter does not receive treatment in a separate chapter. Arguably, Sextus here ties himself rather closely to a particular dogmatic outlook. A critic of *AP* may say that, with extensive discussion of god and pretty much no discussion of matter, Sextus has not discussed physics. Rather than tearing down an edifice by calling into question its foundations, and rather than casting the net widely, he may have failed to address what physics is about. The discussion of physics in the *Outlines* is less vulnerable to these sorts of objections. Though it addresses topics that are basic to dogmatic physics, it may be read as engaging with those questions that arise for anyone who participates in philosophical conversations at the time.

A more extensive line of questioning, one that I shall not pursue here, is invited by Bett's emphasis on the systematic nature of *AP* and on the idea that the treatise argues 'against everything'. It may be asked whether, if the sceptics arrive at suspension of judgment on the existence of god, cause, bodies, time, and so on, they arrive at suspension of judgment on the existence of the physical world—and whether this makes them rather interesting competitors of external world sceptics in early modern and modern philosophy. This is just one example, included here to highlight how far-reaching the upshot may be of working closely with Bett's new translation of *Against the Physicists*.

The History of Mathematical Proof in Ancient Traditions edited by Karine Chemla

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This book is the offspring of a working group on ancient mathematical reasoning that met in Paris in the spring of 2002. The lengthy gestation and delays in production mean that not all the articles are up to date bibliographically, especially for work coming from outside the circle of participants. The individual papers comprising the volume are all written by experts and repay close reading. There are 16 chapters, divided into two parts, on the historiography of mathematical proof and on the history of mathematical proof in ancient traditions. These chapters follow a lengthy prologue by Karine Chemla framing the entire project, 'Historiography and History of Mathematical Proof: A Research Programme' [1–68].

Chemla opens her prologue with something of a straw man position, declaring that 'the standard history of mathematical proof in ancient traditions' [1] asserts that the only valid form of mathematical reasoning is that of the Greeks. It is not clear who exactly is still supposed to subscribe to such a 'standard view'. The closest Chemla comes to offering a witness is a passing reference to Morris Kline, backed up by Eduard Biot.

Passing on from this somewhat shaky rhetorical opening, Chemla then provides a useful thumbnail sketch of the development of the historiography of mathematical proof from the earliest Western encounters with ancient texts and an overview of the interconnections between the more specialist contents of the subsequent chapters. Below is offered a very brief summary of each of those chapters.

Part 1, 'Views on the Historiography of Mathematical Proof', opens with a chapter by Bernard Vitrac entitled 'The Euclidean Ideal of Proof in *The Elements* and Philological Uncertainties of Heiberg's Edition of the Text'

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[69–134]. Heiberg's edition of Euclid stands as a monumental testament to his philological capabilities but some of his editorial decisions have been questioned, most notably by Wilbur Knorr in his article on 'The Wrong Text of Euclid' [1996]. As no early manuscript of Euclid is known, the question is how to evaluate the changes that have doubtless occurred in the course of transmission and, in particular, how to compare the manuscripts preserved in Greek, what Vitrac refers to as the 'direct tradition', with an indirect tradition incorporating commentaries, quotations, and translations into other languages. Vitrac provides a useful summary of the transmission and transformation of Greek writings and an inventory of Euclidean manuscripts. His main text is then an engagement with the 'recent criticism' [70] by Knorr. Vitrac was very much on Knorr's mind in the 1990s, engaged as Vitrac was then on his four-volume translation of *The Elements* into French. The first two volumes had appeared at the time of Knorr's paper with the remaining two volumes appearing in 1998 and 2001. It is welcome to have Vitrac's detailed and judicious response, albeit somewhat delayed. Vitrac (and Knorr) place more reliance on the indirect tradition than did Heiberg and Vitrac provides a wealth of detail on editorial variation and the questions that confronted Heiberg and, with more sources available a century later, himself. Knorr carries Vitrac with him on many of his points but there are some divergences. Vitrac ends by arguing that attempts at reconstructing a pure original Euclid are doomed—'the conception of a new critical edition of the Greek text seems useless to me for the moment'—and calls instead for further effort on the indirect tradition, 'critical editions of the various identified Arabic, Arabo-Latin and Arabo-Hebrew versions would be preferable' [122].

Chapter 2, 'Diagrams and Arguments in Ancient Greek Mathematics: Lessons Drawn from Comparisons of the Manuscript Diagrams with those in Modern Critical Editions' [135–162], is by Ken Saito and Nathan Sidoli. The role of diagrams in Greek mathematics has received increased attention recently, especially since the pioneering work of Reviel Netz. Heiberg, for example, almost completely ignored manuscript evidence when constructing the diagrams for his critical editions, a point not addressed by Vitrac in the preceding chapter. In this chapter, the authors investigate the manuscripts and offer a comparison between ancient diagrams and those in modern editions. The main differentiating characteristics that they identify are 'overspecification', that is, 'the tendency to represent more regularity among the geometric objects than is demanded by the argument' [140], e.g., by using rectangles or squares for quadrilaterals, and ‘graphical indifference’, by which they mean ‘diagrams that are not graphically accurate depictions of the mathematical objects discussed in the text,’ [143] as when unequal lines are depicted as equal or *vice versa*. An important consequence that they draw is that diagrams in the medieval manuscripts were not in themselves ‘meant to convey an idea of the level of generality discussed in the text’ [157], arguing that verbal description or supplementary constructions would be used for this purpose.

A concern for diagrams naturally makes a re-appearance in chapter 3, ‘The Texture of Archimedes’ Writings: Through Heiberg’s Veil’ by Reviel Netz [163–205]. Netz divides his chapter into two parts, on diagrams and on text. In his analysis of the diagrams, he declares that Heiberg goes ‘metrical’, ‘three-dimensional’, and ‘iconic’. That is, in comparison to Netz’ reconstruction of the early, and possibly Archimedean, diagrams, Heiberg’s diagrams present more relevant metrical information of comparative objects, better three-dimensional representation of solids, and more accurate depiction of geometric objects. His analysis of Heiberg’s textual alterations and choices is summarized as ‘textually explicit, non-accessible and consistent’ [202]. Of these, the issue of consistency exercises him the most for here, as in his other writings on Archimedes, Netz stresses the variety of Archimedes’ work in both content and presentation.

Chapter 4, ‘John Philoponus and the Conformity of Mathematical Proofs to Aristotelian Demonstrations’, by Orna Harari [206–227] turns away from Heiberg to consider why Philoponus and Proclus were untroubled by the evident failure of mathematical proofs to satisfy Aristotle’s prescriptions for valid demonstration. Harari’s argument is detailed and technical, and a brief summary does not do her argument justice. However, her main point revolves around the ontological question of whether mathematical objects are immaterial or material. For Philoponus, they were immaterial and so questions of essential relations and grounding conclusions in the cause were irrelevant.

The next two chapters concern the interaction of Western mathematicians and Indian mathematics. Dhruv Raina, in ‘Contextualizing Playfair and Colebrooke on Proof and Demonstration in the Indian Mathematical Tradition (1780–1820)’ [228–259], considers the early British understanding of Indian Mathematics. John Playfair, in an influential address, argued that Indian astronomy as then practiced involved little more than following computational rules without insight into their origins. He suggested that Indologists should search for background texts on Hindu geometry, which he felt must have underlain the astronomical calculation procedures. Henry Thomas Colebrooke gave the first translation from Sanskrit of a selection of mathematical works. Colebrooke's selection criteria, emphasizing an 'algebraic analysis', strongly influenced subsequent European, and especially British, conceptions of Indian mathematics and astronomy.

Next, Agathe Keller tackles Georg Friedrich Wilhelm Thibaut (1848–1914) in 'Overlooking Mathematical Justifications in the Sanskrit Tradition: The Nuanced Case of G. F. W. Thibaut' [260–273]. Thibaut was a philologist with a fine sense of textual and grammatical detail, and a specialist in the *mimāṃsa* school of philosophy. This led him to an interest in mathematics and astronomy and he published the oldest known works of Sanskrit geometry. The verses of the oldest texts, the *śulvasūtras*, are difficult to understand and are accompanied by commentaries, often written much later, that explain these difficult, dense, and aphoristic texts, and provide justifications. As a historian, Thibaut was wary of the extent to which later commentaries could be taken to reflect authorial intent accurately. He was also troubled by the way in which Sanskrit sources did not reflect his own sense that mathematical propositions should be stated logically and clearly, and properly demonstrated. Keller unravels these interesting contradictions.

Rounding out part 1 is a chapter by François Charette, 'The Logical Greek *versus* the Imaginative Oriental: On the Historiography of "Non-Western" Mathematics during the Period 1820–1920' [274–293]. Charette is principally concerned with the views of Hermann Hankel (1839–1873), Moritz Cantor (1829–1920), and Hieronymus Georg Zeuthen (1839–1920) on the comparison of Greek mathematics with Indian, Chinese, and Islamic mathematics. Hankel's book on ancient and medieval mathematics was published posthumously but proved to have a lasting impact on the next generation of historians of mathematics. Cantor was dismissive of Indian mathematics and his analysis of Islamic mathematics was characterized by a search for external influences. Zeuthen regarded the solution of third-degree equations as the decisive step away from medieval mathematics and saw the rapid development of mathematics after that as a result of study of Greek mathematics, and so his periodization of the history of mathematics led him to foreground the Greeks. Charette's analysis shows some of the complexities underlying the common notion of a Greek exceptionalism reflected in a specific racial or national cast of mind that was current in the 19th century.

Moving on from the historiography of early European historians of mathematics, part 2, 'History of Mathematical Proof in Ancient Traditions: The Other Evidence' opens with a chapter by G. E. R. Lloyd, 'The Pluralism of Greek "Mathematics"' [294–310]. Lloyd raises questions concerning the 'heterogeneity of the Greek mathematical experience' [307], arguing that it derived from the competitive nature of Greek intellectual discourse and the tensions between discovery and proof inherent in the privileging of the axiomatic-deductive method of argument.

In chapter 9, Ian Mueller considers 'Generalizing about Polygonal Numbers in Ancient Greek Mathematics' [311–326]. Mueller picks up two aspects of Greek reasoning on polygonal numbers. The first, treating Nicomachus, is that much information about the arithmetic and geometric properties of polygonal numbers is carried by the specific configurations of dots (or alphas) that would be destroyed by a Euclidean treatment of numbers as straight lines. The second considers Diophantus' arguments, 'cumbersome and roundabout' [319] but essentially correct. In Diophantus, the geometrical configurations are suppressed in preference to a purely arithmetical presentation. Mueller argues that 'within the limits of Greek mathematics there can be no mathematical demonstration of an arithmetical characterization of configurationally conceived polygonal numbers' [325].

Diophantus is also the subject of the next chapter, 'Reasoning and Symbolism in Diophantus: Preliminary Observations' [327–361], by Reviel Netz. Netz argues that Diophantus' use of symbolism 'has a functional role in his reasoning' [328] and, specifically, that it is intended that the reader 'systematically read the sign both verbally and visually' [341]. Next, he considers the specific modes of reasoning and content presented by Diophantus, concluding that 'Diophantus set himself the task of presenting lay and school algebra within the format—and expectations—of Greek geometrical analysis' [359]. His specific rhetorical decisions were designed to facilitate that goal.

From Greece, we turn to Mesopotamia for the next two chapters. In chapter 11, 'Mathematical Justification as Non-Conceptualized Practice: The Babylonian Example' [362–383], Jens Høyrup tackles the questions of demonstration and critique in the case of Old Babylonian mathematics. Arguing for a concrete geometrical reading of the steps of the numerical algorithms involved in many examples, he maintains that the correctness of the procedure is immediately obvious to the user:

> one who follows the procedure on the diagram and keeps the exact (geometrical) meaning and use of all terms in mind will feel no more need for an explicit demonstration than when confronted with a modern step-by-step solution of an algebraic equation. [367]

The force of the demonstration is in the procedures. His second point is about the absence in most of Old Babylonian mathematics of a discussion of the conditions under which procedures remain valid. Here, he emphasizes that the bulk of our sources come from an educational locus where training in following correct procedures was more important than discussion of why and under exactly what conditions such procedures were correct:

> the social *raison d'être* of Old Babylonian mathematics was the training of future scribes in practical computation, and not deeper insight into the principles and metaphysics of mathematics. [381]

While it is certainly true that the bulk of known Old Babylonian mathematical tablets are connected with the education of trainee scribes, it is now also clear that this is not the case for all of them. Christine Proust analyses one such tablet, CBS 1215, and some related texts in chapter 12, 'Interpretation of Reverse Algorithms in Several Mesopotamian Texts' [384–422]. She argues that, despite containing only numbers, 'this text contains an original mathematical contemplation' [384]. The text in question contains a series of computations of reciprocals of successive doublings of the number 2,5. Crucially, in each case, the reciprocal of the reciprocal is then computed to return to the original number. However, the reverse algorithm is not the first algorithm with the steps reversed but a second iteration of the initial procedure. Further, the numbers are laid out in a precise and unusual manner: 'the principles of the spatial arrangement of numbers [has] a precise meaning in relation to the execution of the reciprocal algorithm' [395]. Hence,

> the relationship between Tablet A [CBS 1215] and the school exercises is exactly the opposite of what is usually believed. Tablet A does not seem to be the source of school exercises: rather it seems derived from the school materials with which the scribes of the Old Babylonian period were familiar. [409–410]

Proust's conclusion is that the tablet 'bears witness to the reflection of the ancient Mesopotamian scribes on some of the fundamental principles of numeric calculation' [417]. Proust's reading of CBS 1215 is thus very similar to Netz' interpretation of Diophantus.

Karine Chemla considers Chinese proof techniques in chapter 13, 'Reading Proofs in Chinese Commentaries: Algebraic Proofs in an Algorithmic Context' [423–486]. As in India, core mathematical texts come with commentaries. In particular, Chemla notes that in the case of the *Nine Chapters on Mathematical Procedures*,

> no ancient edition of *The Nine Chapters* has survived that does not contain the commentary completed by Liu Hui in 263 and the explanations added to it by a group of scholars under the supervision of Li Chunfeng. [424]

In order to understand how the original text was approached by Chinese scholars, it is necessary to treat the *Nine Chapters* and its accompanying commentary as a unit. In this deep and penetrating paper, Chemla explores two aspects of the work of the commentary. A problem in the *Nine Chapters* is stated with particular numbers and solved by particular computations. The commentaries unpack this particularity in a couple of directions. They show *why* the computations are the way they are, what Chemla refers to as the 'meaning' of a calculation, in the course of which they show how the specific problems can be generalized. The commentaries also show that the solution algorithms are correct by showing how the procedures can be obtained from known correct algorithms by a sequence of valid transformations, that is, 'algebraic proofs in an algorithmic context'. An important part of Chemla's argument rests upon a detailed analysis of the interplay between computational layout on the counting surface and arithmetical transformations, tying together abstract reasoning and material culture in an intimate fashion. The sequence of transformations on the computing surface provides a significant layer of the 'meaning' of computations.

As was explained by Agathe Keller in her chapter on Thibaut, early Indian mathematical texts are dense and allusive, and require commentary. In chapter 14, 'Dispelling Mathematical Doubts: Assessing Mathematical Correctness of Algorithms in Bhāskara's Commentary on the Mathematical Chapter of the *Āryabhaṭīya*' [487–508], she returns to this topic in an analysis of Bhāskara's commentary on Āryabhaṭa's mathematical treatise. Bhāskara's task is to convince the (hostile) reader that Āryabhaṭa's verses do in fact contain justifiable mathematical procedures. Bhāskara utilizes two main editorial devices. After teasing out the mathematical details of an interpretation, he then gives a 'reinterpretation' investing the verse with an additional layer of meaning. His other technique for showing the validity of a procedure is to provide a separate, independent, procedure showing that it arrives at the same conclusion. Keller works through a series of detailed examples to illustrate her analysis.

Chapter 15, 'Argumentation for State Examinations: Demonstration in Traditional Chinese and Vietnamese Mathematics' [509–551] is by Alexei Volkov. Volkov's contention is that (most) Chinese mathematics treatises of the first millennium functioned as textbooks for their users. Unfortunately, there is little direct contemporary evidence to support this point. However, after a review of Chinese mathematical education and an elucidation of examination procedures, Volkov turns to a Vietnamese witness, a 'model' examination paper published by Phan Huy Khuông at the end of his book *Chi minh lập thành toán pháp* (*Guidance for Understanding the* '*Ready-Made Computational Models*'), published in 1820. Arguing that Vietnamese mathematics education closely followed the Chinese system, and that the mathematics curriculum displayed longterm stability, Volkov suggests that the essay given in the model examination paper reflects the style of response that classical Chinese education expected of its students. In particular, he contends that the commentaries such as those of Li Chunfeng were taken as exemplars for examination essays.

Rounding out the volume is Tian Miao's chapter, 'A Formal System of the *Gougu* Method: A Study on Li Rui's *Detailed Outline of Mathematical Procedures for the Right-Angled Triangle*' [552–574]. The text in question, published in 1806, presents a systematic treatise on methods of solving a right–angled triangle given two of a list of 13 related variables (lengths of sides, sums and differences of sides, and so forth). The 78 problems so generated are carefully organized, both internally (each problem is stated and solved in a similar fashion) and externally (the problems are developed systematically). Tian shows that Li Rui 'consciously developed his system' [565] and that his aim was a systematization of a body of traditional knowledge rather than the production of new knowledge.

The book offers a wealth of insights both into the history of Western engagement with the mathematics of a wide variety of ancient cultures and the current state of the art. It is a valuable addition to the scholarly literature, showing the current, very active, struggle of scholars to enter more fully into the conceptual worlds of ancient mathematical practice from the scant traces left to us.

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Copernicus: Platonist Astronomer-Philosopher. Cosmic Order, the Movement of the Earth, and the Scientific Revolution by Matjaž Vesel (translated from Slovene by Manca Gašperšič)

Frankfurt am Main: Peter Lang. Pp. 451. ISBN 978–3–731–64242–9. Cloth $100.95

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In this monograph Matiaž Vesel tells a fascinating story. Nicolaus Copernicus (1473–1543) published a highly technical treatise on astronomy in 1543 known in Latin as *De revolutionibus* (*On Revolutions*), the same year in which he died. Copernicus' book described a system of astronomy in which the Sun was imagined to be at rest at the center of the cosmos while the Earth was boldly set in motion round it. Vesel asks himself such 'simple' questions such as Why did Copernicus assert that 'the Sun is at rest in the center while the Earth moves' [13]? Why 'did he think that astronomy was in need of reform' [13]? More ambitiously, Vesel asks one more, final question: 'What does Copernicus's assertion mean for the history of human, particularly scientific and philosophical, thought?' [13].

It would be impossible to do justice to the complexity Vesel's story if one wanted to summarize his answers to the above questions. So I will leave the pleasure of tasting the infinite nuances of Vesel's musings to the readers of his book. I will content myself with reporting sparse impressions that I have gleaned while exploring *Copernicus: Platonist Astronomer-Philosopher*.

As the title chosen by Vesel indicates, the theme of his book is Copernicus' Platonism. This is strongly emphasized by Vesel, who claims that

> Copernicus's Platonism explains *all of the fundamental aspects* of his project. His Platonism brings unity and coherence to his work and links into a consistent philosophical stance seemingly unrelated issues, such as the equant problem and the problem of the order of the planetary spheres. [20]

So I first set out to find out how Platonism figured, for instance, in one of the problems that Copernicus discusses, one which has attracted the interest of

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historians and philosophers of science including myself. It is the problem of the physical consequences that would have to be expected if the Earth rotates round the Sun and also around its polar axis. Why do heavy objects here on Earth not fly upwards, as we see happening in sling-like and other rotating devices? Why are houses and trees and people not extruded from the surface of the spinning Earth? Vesel engages in a thorough historical analysis of earlier theories, collecting all the arguments and counter-arguments that might have been at Copernicus' disposal [155 ff.]. However, this tortuous contextual reconstruction does not seem to offer a convergent, progressive movement towards answering the simple question posed by the guiding idea of the book, that is, the hypothesis that Platonism was the prime mover of Copernicus' reformist project in astronomy. But this is perhaps, as I hasten to point out, the strength of this book.

At this point, it dawned on me that the issue with the structure of the book is not so much the difficulty of finding an orientation in the wealth of historical details marshaled by Vesel, for the narrative is always clear and cogent. The issue is indeed the category itself of Platonism, which the book lays open for further questioning. What does Vesel mean by 'Platonism' and can it be neatly defined in the context of European culture of the16th century? This is the question that Vesel's learned book finally pushed me to ask myself and to which I found no definitive answer. Vesel thinks that it is Copernicus' Platonist theory of gravity that explains his treatment of the physical objections against the motions of the Earth. 'Copernicus' theory of gravity, regardless of which author was his immediate source—Ficino, Plotinus, Plutarch or somebody else—is evidently Platonist' [204].

But Vesel also emphasizes that Copernicus' physical arguments in favor of the Earth's rotation serve only one purpose: to bring the theory of motion in conformity with his cosmological-astronomical principles, that is, with the *harmonia* and *symmetria* of the world. Or, if we look at it from another angle: when addressing the question whether the Earth moves, it is not physical arguments 'against' or in 'favor' that are crucial for Copernicus but the mathematical cosmological reason, that is, the harmony of the universe. His central argument for the Earth's motion is, therefore, the firm *symmetria* of the universe, that is, the commensurability of his parts, which can be understood by taking into account the various motions of the Earth [205].

The most important piece of evidence for the thesis of the book, concludes Vesel, is the 'theoretical, philosophical concordance between Plato and Copernicus' [321]. This theoretical and philosophical concordance is then summarized in the principle of the harmony of the universe, and the task that Copernicus the Platonist is astronomer set himself was the restoration of a system of the universe in which all parts fall into place so as to be commensurate to one another. This commensurability is not an object of sensory perception but rather a vision of the mind. The senses show us that the Earth is motionless [388]. The mind harmonizes instead of measuring. Then, it seems to me, the role of Copernicus in the scientific revolution, the theme discussed in the last pages of the book, needs to be reconsidered. Vesel suggests that

> Copernicus contributed to the Scientific Revolution not only by spurring certain developments but…also by triggering a shift towards the horizon of modern scientific thought…he demonstrated that in order to discover the truth about the natural world, a scientist must very seriously reflect upon what sensory appearances tell him. [391]

My thought is that if the Platonism which Vesel discerns in Copernicus is the principle of the harmony of the universe, a principle that is ultimately a structure of mind, then this Platonism is itself not in harmony with the horizon of modern scientific thought unless the horizon is restricted to the 17th century. The trajectory of European science over the last four centuries has been a movement away from that ideal. For the mechanism and often crass materialism that define science nowadays are worlds apart from the cosmological harmonies of Plato and Copernicus.

Barrington Atlas of the Greek and Roman World for iPad edited by R. J. A. Talbert

Princeton: Princeton University Press, 2013. ISBN 978–1–400–84876–8. epub $19.99

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The *Barrington Atlas of the Greek and Roman World* has been with us for nearly 15 years.[1] In this time, the *Barrington Atlas* has established itself as an integral part of the landscape of classical studies. In the same period, the publishing world has undergone a digital revolution. Princeton University Press has kept abreast of these changes and produced an electronic version of the *Atlas* for use on the iPad. The PUP blog[2] called the new offering the Barrington Atlas of the Greek and Roman World App. The PUP website[3] calls it the Barrington Atlas of the Greek and Roman World for iPad. The App Store calls it simply the Barrington Atlas of the Greek and Roman World. In this review, I refer to it as the BA App. An updated version of the BA App was issued in 2014 almost immediately after the initial launch (2013). This updated version, BA App 1.1, is the one current at the time of writing and submitting this review (May 2014); further updates will undoubtedly be released in the future. BA App 1.1 is compatible with the iPad 2 (or later) and requires iOS 7.0 (or higher). It is available for download from the App Store.[4]

The App Store emphasizes the portability of the BA App. The PUP website stresses the advantage of being able to 'carry all the content of the BA on your iPad'. It is certainly true that the weightlessness of the app contrasts favorably with the unwieldy heft of the atlas in its physical form. Putting content into app-form does more, however, than counteract gravity. Freed from the linear

1 Go to press.princeton.edu/titles/6773.html.

2 Go to blog.press.princeton.edu/barrington-atlas-of-the-greek-and-roman-world-app-available-november-21-2013.

3 Go to press.princeton.edu/apps/barrington-atlas.

4 Go to itunes.apple.com/us/app/barrington-atlas-greek-roman/id767575157.

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sequence of the book format, we enter the formless realm of the digital world and this alters the way in which we approach and interact with content. We become users rather than readers, actively charting our way through the information available rather than passively following routes dictated by others. With this in mind, I chronicle the pleasures encountered, and the challenges faced, by three different users navigating their way through the BA App.

Of the three users followed in this review, the first is using the app to search for a specific place name; the second is interested in finding a map of a specific region; and the third wants to go straight to a specific map. The speed and simplicity of their journeys through the app are such that the process of using the physical atlas suddenly seems incredibly arduous in comparison—even though it never seemed that way before the app came along. Their experiences do, however, bring up some interesting questions about the kind of creature that the app is or wants to be. Is it merely an electronic clone of the physical atlas? Or is it an adaptation that will increasingly diverge from its physical parent as future versions are rolled out? Further, our users do encounter some glitches in their travels, as detailed in what follows. Since an app is more malleable than a physical book, some of the glitches present as of May 2014 may have been fixed by the time when you read this review.

Let us look first at the user who is searching for a specific location and let us assume that he is looking for the ancient city of Ephesus. He makes use of the index—the Gazetteer, as it called in the *Atlas*, in more geographically-appropriate terminology. Using the physical atlas, he would have had to turn to the back pages where the gazetteer is printed; he would have run his eyes down the alphabetical list of locations until he found the entry for Ephesus; under the entry, he would have found three different map numbers but, because they were all on the same line alongside the name of the city, he would have realized that the same city is shown on three different maps (rather than concluding that there were three ancient cities named Ephesus). Choosing to look at the first map listed (‘61’), he would have noted the coordinates given for Ephesus (‘E2’). He would then have flicked back through the pages of the atlas to Map 61, run his eyes along the top scale of the map to find column E and down the side scale to find row 2; and there, as promised, he would have found the ancient city of Ephesus, located on the western coast of the Asian peninsula.

Life is so much better with the app. Our user opens the app on his iPad and taps on the homepage to reveal the lefthand menu. He then taps the fourth item down, marked with an icon of a magnifying glass and labeled 'Gazetteer'.[5] This brings up the list of place names arranged in alphabetical order, as in the physical atlas. The app-user is unlikely, however, to run through the alphabetic list. (One wonders how long it will be before knowing the order of the alphabet becomes as redundant as being able to write in cursive.) Instead, he uses the search line, that great boon of the internet age. He enters 'Ephesus', which brings up three entries, each on a separate line, each with a different map number and set of coordinates. The user taps on the first line, which acts as a hyperlink to Map 61. The map opens up in the screen; even better, the app zooms in on Ephesus so that the user does not even have to tax his brain by following the coordinates.[6]

I have a slight quibble with the layout of the app-gazetteer, with its three separate line-entries for 'Ephesus'. The user might legitimately, if erroneously, assume that each line relates to a separate city. In the physical gazetteer, where the name 'Ephesus' is given just once and the three map numbers pertaining to it are given on the same line, it is clearer that we are dealing with three entries for the same city rather than three different cities. I can see the problem for the producers of the app: each line of the app-entry also acts as a hyperlink to the relevant map; so, for three different maps, there have to be three separate lines. Perhaps a solution would be to make the map number, rather than the whole line, act as the hyperlink? Thus the name 'Ephesus' itself would appear only once (as in the physical gazetteer); the name itself would not act as a hyperlink; all three map numbers would be displayed alongside it; and each map number would act as a hyperlink to the relevant map.

Quibbles aside, our user experiences a more serious glitch when he taps on the second line-entry for Ephesus, where the map number is given as '1',

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5 On pages other than the homepage, the label 'Gazetteer' disappears from alongside the icon in the lefthand menu, as do other labels; this hinders navigation from one app-page to another.

6 For some place names, the zoom function is misdirected. In searching for 'Apamea' (present day Qalaat-el-Moudiq, Syria) and choosing the link to Map 68, I found that the correct map opened on the screen but that the zoom function took me too far south and deposited me in northern Lebanon.

and the coordinates as ‘I3’. Map 1 is a small scale map showing the entire Mediterranean world with the major cities of antiquity, including Ephesus. However, this is not the map that appears when our user taps on the second line. Instead, he is transported to Map 1a, which shows what are today called the Canary Isles, off the coast of north western Africa. The app then zooms him in to what is now Morocco. This is all very disorienting for someone seeking Ephesus. A quick test suggests that the problem is a systemic one. Looking for the ancient city of Gaza in the present day Gaza Strip, I noted that one of the entries was for Map 1, J4. Tapping on the relevant entry, I was taken to Map 1a and then zoomed in to the Atlas mountains, where no amount of searching will find any city called Gaza.

Turning back to our user searching for Ephesus, let us look at his experience when he taps on the third line-entry in the app-gazetteer which gives the map number as ‘57’ and the coordinates as ‘F4’. Tapping on this third line-entry, our user is correctly taken to Map 57 and zoomed in to Ephesus. As it happens, Map 57—which shows the Aegean coasts of present day Turkey and Greece, as well as the islands of the Aegean Sea—is presented to the user with north to the lefthand side.[7] This is where the nimbleness of the iPad comes into its own. The user simply rotates his iPad clockwise through 90° so that north appears at the top of his screen. This is an altogether easier business than rotating Map 57 in the physical atlas, which is after all more than one and half feet from top to bottom and more than two foot wide when opened. Rotating it normally involved knocking other books, papers, and coffee cups off the table.

Given the ease with which the iPad can be rotated, one wonders about the value of the app’s ‘true-north’ orientation feature, intended ‘correctly to orient the map so that north aligns with the top of the device’ (to quote from the Tutorial Overview in the Help section, the fifth item in the lefthand menu on the homepage). What does ‘top’ even mean in terms of the iPad? The device is designed, after all, so that it can be held either with its long sides or its short sides horizontal. To understand the term ‘top’, we need to make a distinction between the normal behavior of the iPad and its behavior when the BA App is being used. Normally, the contents of the screen swivel as the iPad is rotated so that what appears towards the top when the device is held

[7] I assume throughout this review, unless stated otherwise, that the iPad is being held in the default position for the BA App with its long sides horizontal.

with long sides horizontal will swivel and continue to appear towards the top when the iPad is held with its long sides vertical. The 'top' of the screen is now along the short side rather than a long side; and the screen contents adjust to fit their newly oriented space. The viewer can then effectively freeze the screen contents in place by putting the iPad's 'side'[8] switch into the 'lock' position.

Things are a bit different when using the BA App. The user holds the iPad with the long sides horizontal. With the iPad thus oriented, the text and menu items on the homepage appear horizontal, as does the dropdown title bar for each individual map. If the iPad is rotated through 90°, the screen contents are automatically locked. (This means that the title on the homepage, for instance, will appear vertical—there is no swiveling of the screen contents to fit the newly rotated screen, even if the side switch is in the 'unlock' position.[9]) The app-makers have good reason for choosing to lock the screen in this way. Maps make up the bulk of the material in the atlas and maps are not as versatile as text when it comes to being fitted into a differently oriented space. Squishing a long thin map into a short fat space would destroy the ratio of distances within it. By locking the screen, the app-makers have found a way around this problem.

This brings us back to Map 57 and the 'true-north' orientation feature. To activate this feature, with the map open on the screen in front of him, the user taps the screen once; this brings down the title bar; at the far right of the title bar is a 'compass needle' icon; tapping on the compass needle icon brings up a 'compass face' icon in the centre of the map; tapping on this icon activates the 'true-north' orientation feature which is supposed to reorient the map so that north appears at the top (meaning towards the long side of the iPad). In the case of Map 57, however, the true-north orientation feature is truly disoriented and disorienting. The map does not reorient. Instead, the user is inexplicably zoomed in to the top left hand corner of the un-reoriented map.[10]

8 Something of a misnomer since, when the iPad is held with its long sides horizontal—as it is by default when viewing the BA App—the switch is not at the side but at the bottom edge.

9 Unless the iPad is rotated though 180°, in which case the screen contents flip completely.

10 Maps 46 and 83 are similarly jinxed.

In the case of other app-maps presented to the viewer with north to the left, the true-north orientation feature does work—but it is beset by further glitches. Take Map 4, for example.[11] Activation of the true-north feature does indeed reorient the map but the user is then involuntarily zoomed in to the top righthand corner of the reoriented map.[12] Double-tapping the screen to unzoom works but only for a second. The un-zoomed, reoriented map momentarily appears—in a half-screen version in order to maintain the ratio of distances—but almost immediately de-reorients so that we are back with north to the lefthand side. Pinching to unzoom works better—but the un-zoomed map only stays on the screen for as long as one holds one's fingers in place. To complicate matters further, some app-maps display with north to the right (rather than to the left).[13] With most of them, the true-north feature automatically zooms the user in to the bottom lefthand corner of the reoriented map.[14] This is all very bewildering. I wonder if the simplest solution would not be to scrap the true-north orientation feature entirely, as long as the app is intended solely for handheld devices like the iPad. (One can see it having greater applicability in an app designed for a laptop.)

Let's now leave our first user and his quest for Ephesus and turn instead to our second user, who wants to find a map of a specific region. To meet his needs, the BA App makes inspired use of what it calls the Locator (the third item in the lefthand menu on the homepage, marked with a pin icon). The Locator is the map which, in the physical atlas, was printed on the inside front cover; it was not, however, listed in the contents pages [ix–x], nor indeed was it given a name. Even so, it was (and is) a useful tool. It shows the area of the Greek and Roman world in outline with the grid of numbered maps superimposed. Let us assume that I had wanted to look at a map of the Crimean peninsula in the physical atlas. Opening the cover, I would have seen from the grid that the map which I needed was Map 23 and would then have flicked through the atlas to the relevant map. Useful but involving quite a lot of physical page-turning. Our app-user simply taps on 'Locator'

11 Go to Map 4 by choosing the second item, 'Maps', on the homepage's lefthand menu.

12 The same glitch occurs in Maps 20–22, 26, 28, 36, 41, 50, 54, 59, 71, and 94–95.

13 Maps 38, 40, 53, 96, 99, and 102.

14 Except that, in the case of Map 38, the map remains un-reoriented when the true-north feature is activated; and the user is zoomed in to the top lefthand corner of the un-reoriented map.

in the lefthand menu and then taps on the grid in the region of the Crimean peninsula; Map 23 duly opens up on the screen in front of him. No page-flicking required.

In the top righthand corner of the app-locator is a 'Modern Countries' switch which, when activated, displays the boundaries and names of present day nation states as part of the outline map. In the physical atlas, this display was printed on the inside back cover; getting to it involved heaving the entire contents of the atlas, which was quite an effort and was not good for the binding. Simply activating a switch is a very neat innovation which takes full advantage of the potential of the digital world. The display of present day nation states was an under-appreciated asset in the physical atlas: like the map on the inside front cover, it was neither named nor listed on the contents pages. With the app, both these maps have been given the higher profile that they deserve.

The 'Modern Countries' option in the app-locator and the map on the inside back cover of the physical atlas are useful because they remind us that the world of the Greeks and Romans was not a different world but rather the same world with different politics. A word of warning, however. The boundaries and names displayed were true as of August 1, 1999, a time already receding into the past. Some of the information contained within the map is now out of date. For example, the map continues to show Yugoslavia as a single entity, although it has now been split into Serbia, Montenegro, and Kosovo. In Africa, the map continues to show Sudan as it was before the separation of the south. There is a note in the bottom lefthand corner of the map giving the dateline of August 1, 1999 and advising that the boundaries should not be taken as authoritative. The note, clearly legible in the physical atlas, is too small to be read on the app. Tapping and holding the locator screen causes a magnifying glass to appear, which is useful for viewing the grid of maps—otherwise somewhat crammed—but which does not make the note any easier to read.

The BA App does not claim to be more than the physical atlas transferred into app-form. It reflects the content of the atlas which, in turn, reflects circumstances and knowledge at the time when the atlas was undergoing preparation. As far as modern countries go, trying to keep abreast of every new development would be an endless task, as the current example of the Crimean peninsula shows all too well. However, the problem of changing circumstances does raise a more general question about updating. The Ancient World Mapping Centre maintains a list of emendations to the *Atlas*.[15] Will these emendations be incorporated into the app only if and when a new edition of the physical atlas is launched? Or could they be incorporated into future versions of the app, regardless of the status of the physical atlas?

Let us turn finally to our third user, who knows the specific number of the map to which he wants to refer. He might be a longtime *Atlas* user or a novice following up a reference that he has found in an article. Let us assume that he is looking for Map 71 (showing Petra, in present day Jordan). On the app's homepage, he selects the second item in the lefthand menu, labeled 'Maps' and marked with a concertina icon. A carousel of the maps appears on the screen, starting with Map 1. There is nothing to stop our user swiping his way through the carousel until he reaches Map 71. He is, however, more likely to use the page-finder at the foot of the screen, which will enable him to fast-forward to his destination. Once he has arrived at Map 71, he taps on it to enlarge it to its full-screen version. All of this is perfectly satisfactory.

Our third user can choose to follow an alternate path. Having selected 'Maps' on the homepage, he taps on the 'index menu' icon at the top righthand corner of the map carousel screen. This brings up a list of the sections into which the atlas is divided. Our user then selects 'Part 5: Syria-Meroe' which brings up the list of relevant maps, giving for each map its scale, the name of its compiler, and the year of compilation. The latter two pieces of data are valid but the information on scale should be deleted. It is a hang-over from the physical atlas. The scale of each map is different on the iPad screen than it is on the much larger pages of the physical atlas. The app-makers have partially recognized this by deleting from the app-maps the specification of scale that appeared alongside the title of each map in the physical atlas: thus, app-Map 71 is headed merely 'Petra' (in the drop-down title bar) whereas its physical equivalent is headed 'Petra. 1:500,000.'

Scale also makes an appearance on the app-pages that list the contents of the atlas and thereby highlights an identity problem that needs to be sorted out. These app-pages are accessed by tapping on the first item in the lefthand menu, labeled 'Introduction' and marked by an icon of an open book. Using the page-finder or swiping through the pages brings the user to pages 7–9,

15 Go to awmc.unc.edu/wordpress/faq/barrington-atlas-update-list/.

which are almost exact duplications of the contents pages in the physical atlas.[16] All one hundred and two maps are listed—with their scale alongside their individual titles. Is the BA App simply a replica of the physical atlas? If so, the specification of scale should be left in as a true representation of the contents page of the physical atlas, although there should perhaps be a disclaimer to this effect. If, however, the BA App is an adaptation of the physical atlas, then the specification of scale should be deleted from the contents page. In that case, too, other items should be added to the listing of contents, such as the very useful Locator. This identity crisis extends even to the app's homepage. Currently, the title is displayed there as on the paper cover of the physical atlas: *Barrington Atlas of the Greek and Roman World*. Should it perhaps read *Barrington Atlas of the Greek and Roman World for iPad*?

Leaving such questions of identity aside, let us now look at the experiences our users have once they have arrived at their destinations. First and foremost, they find the same high quality cartography that justly earned the physical atlas such high accolades. The app-makers have, however, taken full advantage of the opportunities opened up by the transfer of the maps into digital form and have incorporated some useful shortcuts. Tapping on the key icon in the title bar opens the 'map key menu', providing an explanation of all the symbols, fonts, tints, and so forth, used on the map. This is a thoughtful link, which makes life a lot easier. In the physical atlas, this information was provided on the obverse of Map 1 and required quite a bit of page-flipping forward and backward. Another innovation is the compass-needle icon which, as well as activating the true-north orientation feature as discussed above, also paves the way to using the 'continuation' feature. To illustrate the continuation feature, let us assume that our second user—the one who found his way to Map 23 showing the Crimean peninsula—now wants to see the area immediately to the east. Tapping on the compass needle icon brings up links to maps of geographically contiguous areas. Our user taps on the link to Map 84 and is immediately transported to the region east of the Crimean peninsula, namely, Lake Maeotis (present day Sea of Azov).

The BA App is an invaluable tool and PUP is to be commended for taking on this ambitious project. Creating an app for any scholarly work is something of a challenge and this is particularly true in the case of an atlas with its defined spatial requirements. These dictate that every map that is presented

16 The contents pages are numbered ix–x in the physical atlas.

with north to the left in the physical atlas has to be presented with north to the top in the app and *vice versa*,[17] except that maps which spread over two pages in the physical atlas keep the same orientation in the app. This is all quite complicated enough, even before introducing the searchability and navigability functions which are the true genius of the BA App. It is not surprising that some of the links and some of the zoom functions display a certain waywardness. In the grand scheme of things, the glitches are minor compared to the very real benefits conferred by the transfer into app-form.

The future potential is enormous. Top of my wish-list would be the incorporation into the app-gazetteer of the information currently contained in the Map-by-Map Directory,[18] which provides the present day equivalents of the places noted in the maps, as well as much else besides. Would it not be wonderful to be able to tap on the ancient name 'Arrapha' in the app-gazetteer and find out that its present day equivalent is Kirkuk, Iraq? Or on 'Alexandria Ariorum' to find out that its present day equivalent is Herat, Afghanistan? While not (yet) incorporating the Directory in the app, PUP has nevertheless done the next best thing. They have made the Directory freely available online at press.princeton.edu/B_ATLAS/B_ATLAS.PDF.[19]

Also on my wish-list would be a fine-tuning of the search line in the app-gazetteer so that the user who enters, for example, 'Cinnamomophorus' and comes up empty-handed is prompted to try 'Kinnamomophoros', which will duly take him to Map 4 where the appropriate region is shown (in present day Somalia). Similarly, the user who is looking for the city which he knows as 'Kyrene' will draw a blank. He should be prompted to enter 'Cyrene', which will bring up several entries, including two for the city he seeks (in

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17 Some maps that are presented with north to the right in the app should, for consistency's sake, be presented with north to the left: see page 196n13.

18 The Directory is bound in two volumes and priced separately from the atlas: go to press.princeton.edu/titles/6773.html.

19 This displays the Directory's contents pages, within which each entry acts as a hyperlink to the relevant PDF. I found it confusing that there was no visual indication of this fact. In response to my concerns, PUP has added a note on the first page instructing the user to click (or, in iPad terms, tap) on each entry to open the relevant PDF.

present day Libya). Such prompts would provide an ideal solution to the thorny problem of the best way to transliterate Greek and Roman names.[20]

I have found myself in writing this review constantly wanting to refer to the physical atlas in the past tense. My guess is that, in the future, the app will diverge ever more from its physical parent and that, when we talk of the *Barrington Atlas*, we will be thinking of the app rather than of the atlas in its traditional (old?) format. There are, really, no limits to what the app might do in its future manifestations. As it gets whizzier, however, it will almost certainly get more expensive. If you want to get the BA App while it is still cheap, my advice is to buy it now.

[20] The *Atlas* editors followed an eminently reasonable policy (for which, go to Guidelines at press.princeton.edu/B_ATLAS/B_ATLAS.PDF) but the user of the app-gazetteer is still forced to some extent to second-guess the editors' decisions.

Palmieri and Vesel on Symmetry and Harmony in Copernicus' Cosmology

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In his review [*Aestimatio* 11 (2014) 188–190] of Matjaž Vesel's book,[1] Paolo Palmieri focuses on the question 'What does Vesel mean by "Platonism" and can it be neatly defined in the context of European culture of the 16th century?' According to Vesel, as reported by Palmieri, *harmonia* and *symmetria* are two key concepts whose applications in *De revolutionibus* (1543) show Copernicus to be a follower of Plato. In this brief note, we seek to clarify Copernicus' explicit invocations of symmetry and harmony in his *De revolutionibus*.

Palmieri paraphrases part of the author's argument:

> His [Copernicus'] central argument for the Earth's motion is, therefore, the firm *symmetria* of the universe, that is, the commensurability of its parts....

It is true that 'symmetria' in Euclid's *Elements* means commensurability but the context of Copernicus' usages indicates that the references in *De revolutionibus* are to the Vitruvian sense of well-proportioned as an aesthetic category rather than to a mathematical category [Hon and Goldstein 2004]. Both meanings of 'symmetria' are well attested from Antiquity to the 16th century—and even later.[2]

[1] M. Vesel, *Copernicus: Platonist Astronomer-Philosopher. Cosmic Order, the Movement of the Earth, and the Scientific Revolution*. Frankfurt am Main: Peter Lang, 2015.

[2] Indeed, both meanings of 'symmetria' can be found in the works of Plato: see Hon and Goldstein 2008, 70 and 94–95.

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Copernicus considers Ptolemy's models a mixed bag of theories which together depict the universe as 'a monster'. In this tradition, astronomers could not

> deduce from the eccentrics the principal consideration, that is, the structure of the universe and the true symmetry of its parts (*ac partium eius certam symmetriam*). On the contrary, their experience was just like someone taking from various places hands, feet, a head, and other parts of the body (*membra*), very well depicted indeed, but not for the representation of a single person; since these fragments would not belong to one another at all, a monster rather than a man would be put together from them. [Copernicus 1543, f. iii^{v}; Rosen 1992, 4, slightly modified: cf. Hon and Goldstein 2008, 158]

When disparate elements are put together monstrosity, rather than a beautiful human form, is the result.

Copernicus then adds a reference to the world as a machine:

> For a long time...I reflected on this confusion in the astronomical tradition concerning the derivation of the motions of the universe's spheres. I began to be annoyed that the movements of the world machine (*motuum machinae mundi*), created for our sake by the best and most orderly Artisan of all, were not understood with greater certainty by the philosophers.... [Copernicus 1543, f. iii^{v}; Rosen 1992, 4, slightly modified]

Despite the lack of an explicit reference to Vitruvius (first century BC), we are persuaded that Copernicus expected the universe to comply with the Vitruvian notion of symmetry: a temple (the universe)[3] whose constituent elements (the planetary orbs) relate to each other to form a beautiful whole, based on the order of their distances from the Sun (the center of motion for all of them). Vitruvius used the term symmetry to refer to the well-proportioned feature of the human body, the structure of a building, and the efficient functioning of a machine, treating separately these three domains in which symmetry is applied [*De arch.* 3.1.2, 5.6.7, 10.10.1]. Copernicus invokes all three Vitruvian applications of symmetry.

At the juncture where Copernicus claims to have grasped the 'principal consideration', he asserts that

[3] For the universe as a most beautiful temple (*in hoc pulcher[r]imo templo*), see Copernicus, *De rev.* 1.10 [1543, 9^{v}].

> in this arrangement (*sub hac ordinatione*), therefore, we discover a marvelous symmetry of the universe (*mundi symmetriam*) and a truly harmonious linkage (*harmoniae nexum*) between the motion of the orbs and their size.... [*De rev.* 1.10 in 1543, 10^{r}; Rosen 1992, 22, slightly modified]

Copernicus brings together two previously distinct aesthetic values: symmetry as proportionality in what is pleasing to the eye or efficient, and harmony as proportionality in what is pleasing to the ear. This harmony concerns the relation of the motions or, equivalently, the periods of the planetary orbs, where these orbs are ordered according to their distances from the center of motion, the Sun [cf. Hon and Goldstein 2008, 158–163]. In other words, the cosmos displays both symmetry and harmony. Copernicus thus takes advantage of the aesthetic value of both symmetry and harmony; for him, 'symmetry' does not have the general mathematical sense of 'commensurability'.

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Aratus and the Astronomical Tradition by Emma Gee

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Emma Gee takes up an ambitious task: an explanation of the lasting importance of the *Phaenomena* of Aratus, who transferred into Homeric verse a fourth century astronomical prose treatise of the same name by Eudoxus of Cnidus in the 270's BC. Fusing traditions of technical astronomy and meteorology with the didactic poetry of the Archaic poet Hesiod, the *Phaenomena* was an immediate classic and remained widely read and imitated for centuries to come. Although the poem has begun to receive more attention from scholars in the past 50 years,[1] a general study of its reception has yet to emerge. Gee seeks to fill this void by inserting the *Phaenomena* into a larger tradition of astronomical thought spanning the seven centuries between Plato and the Roman emperor Julian. Although Aratus' importance as a poet generally goes unchallenged, Gee's is the broadest treatment of the *Phaenomena* and its translations by Cicero, Germanicus Caesar, and Avienus as an astronomical tradition referenced at length by several important Latin poets. In the end, the success of the arguments relies on an intricate array of detailed, close readings of text, which compel to varying degrees. Even where these arguments fail to be completely convincing, versions of Gee's theses nonetheless remain plausible. *Aratus and the Astronomical Tradition* constitutes a large step in the general study of Aratus' ancient reception. In what follows, I will summarize and describe the arguments of each chapter, commenting on Gee's argumentative strategy along the way.

Gee divides the Introduction into six sections:

Aratus,
Popularity,

[1] Martin 1956, Ludwig 1963, Solmsen 1966, Sale 1966, Erren 1967, Pendergraft 1982, Lewis 1992, Hunter 1995, Schiesaro 1996, Kidd 1997, Martin 1998, Gee 2000, Fakas 2001, Possanza 2004, Volk 2012.

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Phenomena,
Debate,
An Answer, and the
Programme of the Book.

The section,'Aratus', serves as a short reminder of how little there is of Aratus' biographical tradition at the time of the *Phaenomena*'s composition that we can confirm. Under the heading of 'Popularity', Gee argues that our modern predisposition to marvel at Aratus' ancient popularity is a function of our own perspective. She would shift the discussion of Aratus' popularity by tracing *how* the *Phaenomena* came to be used in the ancient tradition. In the penultimate section, 'An Answer', Gee provides a condensed summary of one of the book's overall theses: Aratus' enduring popularity stemmed from his fusion of astronomical data with imaginative cosmology.

The section entitled 'Phenomena' argues that to understand 'phenomena' as referring to observable data privileges one ancient definition among several. Gee stresses the polyvalence of the term by juxtaposing Aristotle's antithesis of observations (φαινόμενα) and accounts (λόγοι), and Plato's comparison of phenomena to the experience in the Cave [*De caelo* 293a23–27; *Resp.* 7.514a–520a]. In the former instance, the term refers to observable, and implicitly factual, occurrences that can be contrasted with accounts subject to error; in the latter instance, the term can be used to describe what appears to be the case but is not. Contextualizing 'phenomena' within the field of astronomy, Gee then ties the ancient concept of 'saving the phenomena' by constructing regular models to account for seemingly irregular phenomena (planets, e.g.) to Plato's idea of 'saving myth' by turning it into cosmology. Gee's discussion of saving phenomena focuses on planetary motion, the most formidable *explanandum* of celestial motion. The brief treatment is aimed at fitting the concept of astronomical phenomena into her thesis but more background information might prove helpful.

Attitudes toward appearances and the truth that they indicate are of particular importance in astronomy. Certain limitations, such as daytime and the distance from which astronomical phenomena must be observed, compelled ancient astronomers to distinguish explicitly between appearances and reality. For example, Autolycus of Pitane begins his *On Risings and Settings* by differentiating between apparent (φαινόμεναι) and true (ἀληθιναί) morning and evening risings and settings of the fixed stars, which the Sun's rays obscure [1.1]. Thus, the apparent morning rising, the first time a star is seen rising just before sunrise, is at once both insufficient and illuminating: although the star's appearance may not relate straightforwardly to its true phase, the knowledgeable astronomer of the time would understand that the Sun is 15 degrees along the ecliptic below the horizon and could estimate its true morning rising, when it will rise simultaneously with the Sun. Aratus' title demonstrates the importance of appearances as a concept and has parallels in Aratus' predecessors. It was attached to an astronomical treatise attributed to Euclid as well, and Aristotle speaks of phenomena (φαινόμενα) as the object of a discipline ancillary to astronomy (ἀστρολογία), implying that the subfield pertains to the collection and organization of astronomical observations [*An. post.* 78b39].

The ancient use of the phrase 'saving the phenomena' in astronomy has been brought to bear on discussions concerning ancient philosophical attitudes toward the nature of the science itself. Pierre Duhem suggested that 'saving phenomena', or producing models by which seemingly disparate phenomena (e.g., planetary motion) might be organized by regular mathematical principles, indicates the purely instrumentalist goal of mathematical astronomy. According to this view, the Greeks did not concern themselves with the physical reality of celestial motion, so long as they could mathematically account for what appeared to be the case [Duhem 1908]. G. E. R. Lloyd [1978] challenged this view by demonstrating that a closer examination of the evidence indicates concern with physical assumptions in the construction of mathematical models. The famous example of 'saving the phenomena' that Gee discusses from the fourth century Peripatetic Eudemus (quoted by Sosigenes in the second century AD and reported by Simplicius in the sixth) regarding planetary motion can be understood in the following terms: 'saving the phenomena' refers to the application of an explanatory system by which phenomena are correctly understood to reveal an underlying order in accordance with certain assumptions and cease to be insufficient or deceptive.

The section entitled 'Debate' concerns a conflict between the 'intelligent design' worldview and the Epicurean / atomist worldview. In the first category, Gee collects Aristotelian, Stoic, and geometrical astronomical models. In the second, lies the Epicurean atomist model embraced by Lucretius. The section purports to break apart monolithic views of ancient astronomy by presenting the reader with a debate between incommensurable positions.

Thus, Gee challenges the radicalism of the Copernican revolution as well as modern rationalism in general. Although she rightly combats positivist views of scientific progress that view ancient astronomy as monolithic, Gee does not provide an account of Epicurean astronomy to set its concerns or methods apart from mathematical astronomy. The suggestion raises interesting questions, nevertheless, since their doctrine calling for multiple explanations appears to have left Epicureans hostile to the construction of mathematical models.[2] Her construction of an 'intelligent-design' worldview encompassing all cosmologies not Epicurean, furthermore, may require some defense. The merit of Gee's subsequent chapters on Lucretius does not depend on this introductory piece, however, since her argument itself focuses more on polemical poetics than broader scientific debate.

The final section of the Introduction, 'The Programme of the Book', weaves the individual chapters into the broader theses of the book. Gee argues that Aratus' lasting influence was a function of his role as a symbol of an 'intelligent-design' worldview as well as its blend of imaginative cosmology and astronomical data. She devotes the remainder of the book to demonstrating how these features bear out in the *Phaenomena* and its Latin reception.

Gee's first chapter, on the myth of Dike (Justice) and the tradition it exploits, argues that Aratus' celebrated catasterism of Dike as the constellation Virgo [98–136] employs Hesiod's Ages of Man myth, updated with Empedoclean cosmology, to establish poetry's capacity to convey truth and knowledge. On the face of it, Aratus' myth about Dike alludes to a famous pair of stories in Hesiod, one explaining a degenerative progression of the races of men, the Golden past to the Iron present [*Op.* 109–201], and another narrating the departure of Shame and Retribution [*Op.* 199–200], whom Aratus replaces with Dike. Aratus' myth collapses Hesiod's five races into three, Golden, Silver, and Bronze, and, by identifying the constellation Virgo with Dike, Aratus aligns her departure from the world of men in the *Works and Days* with her catasterism as Virgo in the *Phaenomena*. The nightly (seasonal) rising of Virgo, then, converts Dike's final departure from Hesiod into a

2 See *Ep. ad Pyth.* 113, where Epicurus challenges the attempts of astronomers to narrow down only one explanation that might best save the phenomena. Epicurus, as well as Lucretius [*De rerum nat.* 5.509–770], seems to have been more concerned with the physical causes of phenomena than with mathematically reconstructing celestial motion.

cyclical act: Dike comes and goes as part of a natural order reminiscent of the oscillations between Love and Strife found in Empedocles.

Gee finally argues that Dike is a sort of muse specific to astronomical poetry. On this view, Aratus alludes to the Muses when discussing Dike both by describing her as ἐννυχίη—as in *Theogony* 10—and by her use of gentle (μειλιχίοισιν) words at *Phaen.* 119, which Gee relates back to Aratus' proem where the Muses are described as gentle (μειλίχιαι: *Phaen.* 17). Although both Hesiod's Muses and Aratus' Dike are 'shrouded in night', the Muses roam the world unobserved while Aratus' Dike shines in the darkness. So, whereas Hesiod's Muses boast an ability to conceal truth [*Theog.* 27], Aratus' Dike becomes the Muse of truth, so to speak.

Gee concludes by comparing Aratus' application of poetry to data in Dike's catasterism to Plato's application of philosophy to myth. Dike stands as a metaphor for an association between the mythical and the technical. Aratus offers Dike, she argues, as an answer to Plato's famous indictments of poetry.

Gee's chapter on Dike contains some interesting meditations and some compelling arguments but might schematize at the expense of exactness. The strict division between a Hesiodic / diachronic and an Empedoclean / synchronic notion of 'Ages', for instance, makes some unacknowledged leaps. First, synchrony and oscillation are not the same thing. Furthermore, Hesiod's Ages myth itself has famously been interpreted synchronically, notably in Vernant 1966. So Gee's description of these models as 'irreconcilable' seems an exaggeration, especially considering the paratactic transitions in the *Works and Days*, which makes its individual episodes malleable for reuse [see, e.g., Nelson 1998, 47]. The identification of Dike as a muse of astronomical poetry is compelling, and poetry's capacity to produce and disseminate knowledge is clearly of central importance to the Aratean tradition. Gee's argument might even find further support in Germanicus' translation of the *Phaenomena*, in which the poet addresses Virgo as one might typically address a muse or goddess [*Arat.* 98–102].

Gee's second chapter argues that Roman authors use Aratus' Dike myth as if through the lens of his predecessors, Empedocles and Plato, in order to map Aratus' adaptation of astronomical data to human development onto the narrative history of Roman 'discordia'. The argument progresses through three stages, first noting how Virgil uses Plato's *Politicus* to emphasize the cyclicality of the myth and to lay the groundwork for the incorporation of

Roman civil war narrative; then arguing for Germanicus' fascination with civil war; and finally arguing that Lucretius is the intermediary source for both Golden Age references and civil war motifs in Virgil and Germanicus. The chapter contains some interesting readings but Gee's basis for reading Virgil through a Platonic lens is questionable.

Her reading of the *Politicus* as a background for Virgil is perhaps the most controversial part of the chapter. Gee certainly demonstrates that Plato politicizes the Ages myth, aligning a myth of human political development with astronomy and cosmology. In doing so, Gee succeeds in tying the *Politicus* into a mythical tradition similar to some passages from the *Phaenomena*. But her goal to draw a direct line from the *Politicus* to Virgil is perhaps too ambitious: in the desire to do so, Gee finds direct allusions to Plato in seemingly unimpressive echoes (and several departures) in Virgil's *Eclogues* 4 and *Georgics* 2. So, for example, Gee's claim that Virgil's expansion on the Hesiodic κάρποι in his Golden Age narrative in *Georgics* 2.516–23 is a direct reference to the *Politicus*' Golden Age myth ignores that expansion on a theme—here, fruits—was a common Roman rhetorical exercise.[3] Her observation that both *Georgics* 2.538 and *Politicus* 272b1–2 put Cronus / Saturn in charge of the Golden Age is inconclusive, since almost 500 years separate the two texts, which treat a traditional 'topos'. And Gee's reading of a reversal of Ages juxtaposed to the growth of the young Augustus in *Eclogue* 4 as an image of planetary retrograde set against fixed sphere prograde is ingenious but may not convert the skeptical.

Gee then asserts that Germanicus repurposes Aratus' Dike myth to address the political concerns with civil war that would be especially heightened in a Roman context. She argues that Germanicus' references to metallic mixture (reminiscent of *Republic* 10), invocations of 'discordia' (discord), and lines ending in 'ensis' ('sword') all combine to evoke the fearful image of civil war in the mind of the Roman reader. Gee's reading of metallic mixture

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[3] E.g., Quintilian, *De inst.* 10.5.11:

> *Illud virtutis indicium est, fundere quae natura constracta sunt, augere parva, varietatem similibus voluptatem expositis dare, et bene dicere multa de paucis.*
>
> That is an indication of virtue, spreading out those things which are short by nature, expanding small things, giving variety to similar things and charm to what has been set out, and speaking well and at length about little.

in the Ages myth in Cicero and Germanicus leaves her in an awkward but not impossible, position—she claims simultaneously that Cicero's insertion of an Iron Race into his translation of the *Phaenomena* and Germanicus' attribution of the discovery of iron to the Bronze Race in his version both constitute mixture. Whereas Cicero reallocates features of Aratus' Bronze Race to his Iron Race, Germanicus attributes more to his own Bronze Race. Thus two seemingly antithetical literary processes entail the same outcome. Gee's claim that lines ending in 'ensis' are sufficient to evoke the notion of civil war strains credibility; but, as I have already stated, her broader point about Germanicus is certainly correct. As a Latin poet writing in the early first century BC, he weaves the motif of civil war—and lack thereof—into his poem throughout. One might caution, however, that familial strife, the strongest image of Roman *discordia*, is present in the Ages myth in both Hesiod and Aratus as well [*Op.* 182, 184; *Phaen.* 125].[4] She concludes the chapter by arguing that Lucretius serves as an intermediary through which Virgil and Germanicus reference Aratus.

In the third chapter, Gee argues that Lucretius uses Ciceronian astronomical terminology in order to engage the Stoic 'intelligent design' model. She attributes reluctance among scholars to accept Cicero as a model for Lucretius primarily to their insistence upon Cicero's poetic ineptitude, despite allusions in the *De rerum natura* indicating Lucretius' debt. The chapter concludes with Gee's strongest case for an intertextual relationship, an echo at *De rerum nat.* 5.694–95 of *Aratea* 162–66. Lucretius uses the same language to describe the intellectual activity of the Stoics as Cicero uses to narrate the naming of the constellations by an unnamed observer. In attributing Lucretius' use of Ciceronian / Aratean language to his desire to use the language of 'intelligent design', Gee claims that Lucretius is using a dominant poetic discourse to engage polemically with that discourse. Gee demonstrates Lucretius' engagement with Cicero as a prominent poetic opponent, even if she falls short of demonstrating the prevalence of a broader, binary scientific debate between atomist and intelligent-design astronomy.

In the fourth chapter, Gee sets out to demonstrate Lucretius' programmatic interaction with Cicero's *Aratea* as a poetic model beyond the strictly astro-

4 West 1978, 199: The breakdown of familial ties is a typical scene in oriental prophecies of doom as well. Kidd 1997, 227. Gee does acknowledge the implication of familial bloodshed in Aratus' Bronze Race in an endnote.

nomical material. She argues that his program of allusion aims to establish a polemical stance against what she calls the 'intelligent design' worldview, namely, that of the Stoics. Gee argues that three structural Epicurean proems in books 1, 3, and 5 all prepare the reader to look for Ciceronian intertext through scattered verbal and thematic allusion. Thereafter, Gee traces four allusive themes in Lucretius, that of impossibility, namelessness, the shipwreck of the universe, and topsy-turvy land, wherein, she argues, Lucretius recalls specific Ciceronian / Aratean topics in order to espouse the atomist worldview using the poetic discourse of Aratean intelligent design. The chapter succeeds in demonstrating pointed polemical references to Cicero's *Aratea* conveyed through verbal allusion, though Gee makes perhaps excessively strong claims about what constitutes a clear allusion as well as the argumentative capacity of allusion.

Gee's claims about Ciceronian allusion in the three 'Epicurean' proems of the *De rerum nat.* are difficult to prove. First, Latin didactic poetry before Lucretius has been almost completely lost aside from Cicero's *Aratea* and so it is unclear how prominently Lucretius' use of Cicero in particular stands out. Second, Gee's strategy of making her case through an accumulation of evidence, some circumstantial, might be vulnerable to the objection that the allusions she discusses vary in strength. In the proem of *De rerum nat.* 1, for instance, 'tereti cervice reposta' at line 35 is reasonably called an allusion to Cicero's 'tereti cervice reflexum' in fr. 9.5. Whether the use of the word 'labentia' to describe celestial motion in *De rerum nat.* 1.2 can usefully be called an allusion to Cicero's 'labuntur' in fr. 3.1 is less certain. In what other contemporary Latin work about astronomy could we check for such a usage? There is none. On the other hand, Gee admirably brings to light the echo 'fortis equi vis' in *De rerum nat.* 3.8 of 'fortis Equi' in *Aratea* line 54 and 'Equi vis' of line 57 in reference to the constellation Equus. Thus, Lucretius' 'enim contendat hirundo | cycnis, aut quid nam tremulis facere artubus haedi | consimile in cursu possint et fortis equi vis?' (translated by Gee 'For how can the swallow contend with swans or kids use their tremulous limbs in the same career as a powerful horse?') recalls two lines in the *Aratea*. 'Cursu', one might add, could refer to the celestial path of the constellation Horse, which the Kids do, in fact, nearly follow. Thus, Lucretius may compare himself to the Kids as faint followers of a far brighter light—Epicurus / Equus.

Gee's arguments concerning thematic allusions are generally of greater interest but again they range in strength. For instance, Gee claims that Lucretius uses the line 'quorum morte obita tellus amplectitu rossa' [*De rerum nat.* 4.734] in order to evoke the idea of 'hybridism' from Cicero's 'Nam quorum stirpes tellus amplexa prehendit', which describes plants not harmed by Sirius, the Dog Star. Gee's claim that Lucretius alludes to Cicero's description of the Dog Star in order to highlight the impossibility of hybridism (dog + star) does not make sense because there is no reason why Sirius should evoke the idea of 'hybridism' any more than other constellations. Stronger, however, is Gee's discussion of Lucretius' allusive use, when discussing the fixed stars, of Cicero's *recusatio* from treating the planets. While contrasting the fixed stars and the planets, Cicero writes at line 223 'haec sunt quae visens nocturno tempore signa' which Lucretius echoes while explaining that the fixed stars seem to wander when the wind blows the clouds past them [*De rerum nat.* 4.444 *tempore nocturno tum splendida signa videntur*]. Whereas Cicero contrasts the orderly motion of the fixed stars and the motion of the the planets, Lucretius attributes apparent planetary motion to the fixed stars as well. Gee's discussion brings to mind the relationship between astronomical phenomena and the spherical system inferred from them by mathematical astronomy. Epicurus' doubt regarding their reliability shines through Lucretius' use of Cicero's *Aratea*: the phenomena, correctly perceived by our eyes, may in fact deceive our minds.[5] The image of planetary motion is a productive one in Latin literature more broadly, which makes Gee's description of Lucretian polemic particularly compelling in this instance.

In the fifth chapter, 'Planetary Motion', Gee traces the three Aratean themes of namelessness, planetary motion, and celestial change in Latin poetry so preoccupied with the turmoil of civil war. The passages from Aratus are all of thematic importance within the *Phaenomena* itself: *Phaen.* 367–385 narrates the process by which an ancient observer of the stars formed the constellations, applying names to all of them except for those that proved too difficult to render into shapes; in *Phaen.* 454–61, the poet recuses himself from treating the five planets on the grounds that the 'years' of their

[5] This is the argument surrounding *De rerum nat.* 4.444. Cf. Epicurus *Ep. ad Pyth.* 113, which criticizes the pointlessness of arguing for one model of celestial movement over others.

orbits are too long for his capacities; in *Phaen.* 259–261, the poet tells us that no star has disappeared from the sky despite some disparity between the six observable stars forming the Pleiades and the seven recorded in tradition. Gee argues that namelessness, retrogradation, and celestial change are instances of disorder in the *Phaenomena*, whose theme is the stable, predictable order of the universe. The strongest arguments are brought forth in the discussion of planetary motion and celestial change. Gee traces the imagery of planetary motion in Cicero, Manilius, Lucan, Statius, and Seneca to demonstrate its application to the uncertainty of civil war in astronomical and non-astronomical contexts alike. Retrogradation, for instance, is compared to revolution by Manilius, whose planets 'fight' (*pugnantia*) the fixed stars [*Ast.* 1.805]. Although Gee does not provide a full discussion of the problem of celestial change in the *Phaenomena* itself, her discussion of this theme in the Latin tradition is quite rewarding: the death of Amphiarius in *Thebaid* 8, for instance, is compared to the obfuscation of a star of the Wagon, the guiding constellation circling the North Star. Gee's acute reading of the text demonstrates how the loss of Amphiarius disrupts the stability of the hero's foresight as the loss of a guiding star might impair the abilities of a navigator at sea.

The sixth chapter, 'Late Antique Aratus', innovatively explains how the tradition of the *Phaenomena* is used in the fourth century AD, primarily by Avienus, for Neoplatonist ends. Gee reads Avienus' translation in its fourth century context alongside the roughly contemporary *Hymn to Helios* by the Roman emperor Julian. Her compelling argument that Avienus' Jupiter is assimilated to the Neoplatonic Helios effectively elucidates some of Avienus' more difficult collocations. Attached to the end of this final chapter is an epilogue suggesting that Copernicus uses Avienus' language to justify heliocentrism with appeal to Neoplatonism. The chapter is generally informative and well argued.

Aratus and the Astronomical Tradition is outfitted with three appendices: the first is a text and translation of Aratus' Dike myth with references to Hesiod; the second, a 42-page list of allusions to Cicero's *Aratea* in Lucretius; and the third, a text and translation of the proems of both Aratus' *Phaen.* 1–26 and Avienus' (1–99), which are compared at length in the last chapter. The collection of allusions in the second appendix buttresses the third and fourth chapters.

Since Gee deals primarily in close, literary readings of Latin and Greek, the reader's acceptance of a proposed allusion will determine how convincing any given argument is found to be. Broadly speaking, Gee's *Aratus and the Astronomical Tradition* fills a considerable gap by detailing dynamic ways in which the *Phaenomena* was put to use in ancient intellectual traditions. Although readers may not assent to all of Gee's readings, the book nevertheless presents many brilliant insights in taking on the difficult task of drawing a large picture grounded in centuries of minute, textual detail. As stated above, even while disagreeing with some of Gee's precise claims, I often find that some more qualified version of her thesis is productive, which makes the book ultimately a success. In a word, *Aratus and the Astronomical Tradition* is a fruitful read for any scholar of Aratus and the rich tradition surrounding his lodestar poem.

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Hospital Life: Theory and Practice from the Medieval to the Modern edited by Laurinda Abreu and Sally Sheard

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The hospital is, notoriously to scholars of its history, an institution as protean as it is durable. These characteristics render it challenging to construct comparative studies of the hospital as institution, let alone of the hospitals' institutional life. The goal of the present volume is to facilitate new insights by juxtaposing studies which cumulatively cover an unusual geographical, chronological, and thematic scope. Given the wide range of topics treated, a more substantial apparatus might be desirable in individual articles to help the non-specialist reader see how contributions fit into or fill gaps in the existing literature. Collectively, however, the essays reveal continuities in the types of problems and questions encountered in the management of hospital communities. Furthermore, they illustrate how diverse aspects of hospital life—financial, ideological, and administrative—are interconnected in ways often neglected by studies without such a vigorously interdisciplinary approach. Diverse source bases and methodological approaches are utilized in approaching the central question of how quotidian routines in hospital life were shaped by, or diverged from, theories of care.

The articles are arranged chronologically but resist the temptation to strict periodization, which can be more misleading than helpful in the study of hospital development. Christopher Bonfield, in 'Therapeutic Regimens for Bodily Health in Medieval English Hospitals', makes the important point that it may be anachronistic to distinguish between care and cure when evaluating pre-modern hospitals. Drawing principally on the records of three urban English hospitals in the 14th through 16th centuries, Bonfield probes hospitals' food purchases and practices of laundering for connections to medieval medical theories of nourishment, humoral balance, and how disease was communicated or ameliorated. Worth noting is Bonfield's demonstration

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that similar practices existed for leprosaria as for other hospitals (*contra* the durable claim, echoed even in this study's introduction, that medieval attitudes towards leprosy resulted in the creation of distinctive institutions for the disease's care.) Direct evidence for hospital policies' being inspired by medical theory remains elusive; but Bonfield's unusual approach of comparing documents of medieval hospital practice with relevant texts on classical medical theory provides an interesting model for further study.

Hospital accounts also provide the data source for Fritz Dross' 'Their Daily Bread: Managing Hospital Finances in Early Modern Germany'. Extrapolating from two years of Düsseldorf's hospital records from the mid-16th century, Dross reconstructs the considerable commitment of time, effort, and logistical management required for the fiscal management of an early modern hospital. Based on this, he asserts that the diligence of hospital masters in management may be taken as indicative of concern for the continued welfare of the patients, as economic policies would have a direct effect on the services available to the sick. This approach contrasts with many case studies on late medieval and early modern hospitals which have considered economics in isolation from questions of hospital care or have even interpreted such worldly preoccupations as distracting from the work of charity. Dross convincingly argues that the two were perceived by hospital administrators as inseparable and should be so treated by scholars.

The rich Florentine evidence from the latter half of the 16th century enables Sharon Strocchia ('Caring for the "Incurable" in Renaissance Pox Hospitals') to examine not only the formation and function of a hospital community but also the gendering of this community and work. Strocchia offers a salutary reminder of the intensity of staff labor in the pre-modern hospital: female nurses not only worked hard but were trusted with diagnosis and treatment. The advantages of this steady and well-regarded work are highlighted by the gender imbalance among the pox patients: a striking number of adolescent girls is suggestive of patterns of coercive sex in the urban environment. The hospital where they were cared for was viewed favorably both by charitable groups and state officials. Strocchia suggests that the legal status of hospital staff was still ambiguously religious despite the increasingly strict regulations of canon law, a matter which would merit further investigation.

Jon Arrizabalaga, working with evidence from late 15th-century Iberia, also sees hospital foundations as being influenced by late medieval canon law, as well as, increasingly, by the efforts of the state at top-down control. 'Medical Theory and Surgical Practice: Coping with the French Disease in Early Renaissance Portugal and Spain' draws primarily on a surgical treatise. Ruy de Diaz' treatise asserts that theory may be inferior to experience in enabling accurate diagnosis but also deplores the widespread ignorance of theory by professional but non-learned surgeons like himself. In practice, both medical and state responses to the pox were influenced by the perception of the disease as a public health threat.

Similarly concerned with the piecemeal professionalization of medical care, is Laurinda Abreu's 'Training Health Professionals at the Hospital de Todos os Santos, Lisbon (1500–1800)', which raises interesting questions about how processes of professionalization were perceived by contemporaries. The temporal scope of the essay threatens to overwhelm the reader but Abreu argues for substantial continuities despite fluctuations in policy. Contentiously, Abreu describes processes of defining and organizing medical professions as only beginning in the 16th century. Physicians could be trained through a university course or a system of apprenticeship. While some complained about the insufficient respect afforded to those trained as apprentices, this seems not to have deterred aspiring professionals themselves. Hospital officials complained about excessive bleeding by apprentices looking for work and about the entry of non-affiliated barbers and bleeders with 'inappropriate tools'. It is unclear whether such persons were solicited by patients or acting on entrepreneurial initiative. Efforts at state control of the hospital in Lisbon were ongoing but mostly unsuccessful during the period under study. Abreu concludes that, unlike its counterpart in Paris, the Lisbon hospital was not a beacon of modernity. The application of this category is perhaps inevitably problematic.

Jacqueline Belmas, in 'Patient Care at Les Invalides, 1670-1791', also confronts the challenge of categorizing modern hospital care. Belmas argues that Les Invalides was innovative in its architectural specificity—designed as a place of care—as well as its staffing and regulations, serving as a model for other hospitals where clinical medicine would gain hold over the course of the 18th century. Some of Belmas' claimed 'firsts' are in need of clearer definition since hospitals were architecturally designed as places of care, albeit with different understandings of what was necessary to that care, throughout the Middle Ages. Les Invalides, like European hospitals in preceding centuries, still centered around a chapel for hearing Mass. The triad of doctor, surgeon, and dispenser points towards new models of practicing medicine. The close connection of nuns to the latter office, however, as well as the bylaws governing the behavior of staff and patients alike, suggest that old modes of practice persisted despite novelty in theories of hospital life.

Another encounter between recognizably modern practices and older structures of hospital administration is presented in Anne Løkke's 'Conspicuous Consumption: Lying in in Denmark'. In the Royal Lying-In Hospital of Copenhagen, patients received differentiated treatment according to a pay scale which was in turn determined by social status, a practice familiar from the later Middle Ages onwards. Responding to a debate which is incompletely sketched here, Løkke argues that this pattern acted as a barrier to the spread of infectious disease, especially puerperal fever. This runs counter to the dominant theory that the elite of Copenhagen stayed away from the hospital because of outbreaks of fever. In two generations of hospital management, the son's preventative measures against contagion appear to have been less stringent than his father's, a salutary warning against assumptions of linear progress.

In 'Management and Therapeutic Regimes in Lunatic Asylums', John Chircop offers a fascinating analysis of the permeable boundaries between hospitals and their surrounding communities in 19th-century Malta and Corfu, as well as of the disjuncture between theory and practice. In Chircop's assessment, Foucault's theory of confinement and isolation fits colonial intentions for the administration of early modern insane asylums but not the realities of those makeshift institutions. Whether a family diagnosed one of its members as insane or accepted that diagnosis as given by doctors was often dependent on whether the patient's contributions to the household economy were vital. Although neither of the institutions which Chircop studies were in buildings designed as hospitals, they were both placed in the suburban locations outside gates common to leprosaria in the Middle Ages. Chircop points to the liminality of this position as potential material for further study. A mid-century turn towards providing better diet and lodging for asylum patients was inspired not by medical theory but by a shift in social rhetoric which refigured the insane as 'unhappy and afflicted' rather than as society's undesirables.

Intersections of moral and medical theory are also examined in Andrea Tanner and Sue Hawkins' 'Myth, Marketing, and Medicine: Life in British Children's Hospitals 1850–1914', through analyzing patterns of admission and diagnosis for the three non-specialized children's hospitals of Great Britain during the period under study. The creation of such institutions was driven by the perception that lower class lack of hygiene and 'proper' care in a variety of forms were likely causes of disease. Donors were solicited for enterprises which thus aimed to rescue children from 'unsuitable' environments and to provide moral training as well as physical care. This mission, of a kind more often associated with pre-modern hospitals, appears to have been a source of friction in Victorian children's hospitals. Family visits were strictly limited, while donors were often shown over the wards, to the irritation of staff. Tanner and Hawkins conclude that visitation rules were predicated on concern about moral as much as physical hygiene and on an association of germs with the lower classes that was anecdotal rather than based on medical evidence.

Stephen C. Kenny, in 'Slavery, Southern Medicine, and the American Slave Hospital Regime, 1830–1860', focuses on the urban hospitals of antebellum Atlanta, mining the difficult source base of propagandizing hospital correspondence, together with racially freighted medical and pseudo-medical articles. He argues vigorously that racialized medical theory and the widespread view of black patients as chattel were key determinants of hospital regimen. Kenny demonstrates that adequate medical care was anything but a commonplace on plantations and that urban hospitals were a necessary component of such treatment as slaves were given (or subjected to). Medical students and professionals alike were attracted to work in slave hospitals because they were able to carry out risky procedures without fear of repercussion. This raises the question, which Kenny does not fully explore, of tensions between the racialized discourse of treatment and the fact that treatment at the Atlanta hospital was performed for the same fees as hospitals for white patients, and influenced by cutting-edge European models.

The last two articles in the collection deal with changes in hospital practice in the postwar period, focusing on British evidence. David Theodore, in 'The Fattest Possible Nurse: Architecture, Computers, and Postwar Nursing', makes a convincing case for the importance of hospital design in such transformations. Efficiency studies and, Theodore argues, computer-based thinking, led to redesigning nurses' routines. This took place in designs which depersonalized the nurses themselves, who are visible in plans only as the movement of supplies and the performance of certain tasks. At the same time as these developments in design, nurses were made responsible for the computerization of hospital information, a task gendered as similar to typing. In these initiatives Theodore sees not only a response to financial constraints but a component in the lamented devaluation of care itself in postwar hospital treatment.

Sally Sheard, in 'Getting Better Faster: Convalescence and Length of Stay in British and US Hospitals', takes up this theme in her discussion of convalescence. Convalescence, she argues, has been neglected both as a topic of research and as a distinct phase of medical care. Although late 19th-century research on convalescence was slow to be implemented, the World Wars provided impetus for seeking to make the process of convalescence both swifter and more complete. Convalescent homes expanded considerably during the interwar period but the privations of WWII resulted in the closure of many. At many such facilities medical care was not offered explicitly and so it was difficult to justify their inclusion in the NHS system. Subsequently, research on convalescence as a distinct phase necessitating care has been neglected, with, Sheard argues, negative effects on the longterm efficiency of hospital care. Sheard gives a vigorous apologia for reassessing the recovery process, viewing hospital life as a phase of social life rather than a separate and parallel existence.

As the rest of the articles in this collection demonstrate, the ways in which hospital life functioned as a social 'phase' have varied considerably according to region, period, and institutional mission. While hospitals have often functioned as largely self-contained communities, they have done so while affected by their cultural environments, pressures of economic necessity, and theories of public and individual welfare. Many of the studies' most useful insights come from exploring how hospitals were affected by the societies which created them and by their conceptions of class, gender, and race, as well as their ideals of hospital treatment. The methodological creativity shown in illuminating the rationales as well as the routines of hospital life is not the least of the volume's merits and should provide stimulation for further study as well as for fruitful discussion.

Astronomy and Mathematics in Ancient India / Astronomie et mathématiques de l'Inde Ancienne. Actes de la journée d'études organisée le 24 avril 2009 à l'Université Libre de Bruxelles edited by J. M. Delire

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Astronomy and Mathematics in Ancient India is a collection of seven articles by prominent scholars in the field with a preface by J. M. Delire, the volume's editor. The articles all came out of presentations given at a colloquium held at the Université Libre de Bruxelles in Brussels, Belgium on 24 April 2009, attended by an international group of scholars. Of the seven articles, three are in French and four are in English.

As is noted by Delire, the history of astronomy and mathematics in India (taken in the following to indicate all of South Asia, not just the country of that name) is a field that has not received sufficient study. Indeed, it is easy to point to major works that have not yet been been edited or have been edited poorly or which have been edited but not yet translated or studied; and it is similarly easy to point to studies that should be carried out. As such, the volume is a most welcome contribution to the field. It is an important addition as well in that it explores important questions in the field and makes the latest research on them accessible to the scholarly community.

While the articles are each concerned with Indian astronomy and mathematics, they vary greatly when it comes to both topic and period. Included among them are topics such as the parameters of planetary motion in the sixth century AD, prosody in the 14th century AD, and a series for π in the 14th to 16th centuries AD. This, of course, is not a problem in itself. Astronomy and mathematics have been practiced in South Asia for millennia, so it is natural to have a multitude of topics covering many periods. In fact, one strength in this regard is that the reader gets a glimpse of how multifaceted the field of astronomy and mathematics in India is. The choice of topics

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furthermore allows the reader to see how the study of mathematics in India, including important results in mathematics, takes place outside the formal boundaries of the field of mathematics. However, the volume leaves the articles in isolation: it would have strengthened the volume if the articles had been tied together by means of common themes in an introduction or, perhaps, in a concluding chapter. Likewise, an index and a bibliography would have been very helpful to the reader as well. That said, the articles are important contributions to the field of the history of astronomy and mathematics in India, and the volume is recommended for anyone with an interest in, and some knowledge of, Indian mathematics and astronomy. (A layman without such knowledge would likely find the articles hard to penetrate).

The first article of the volume, entitled 'The Reality of Indian Astronomy', is by R. Mercier. In it, Mercier revisits an old and contentious debate from the 1970s between Roger Billard and David Pingree about how the parameters in the Indian model for planetary motion were arrived at. More specifically, Mercier follows Billard in employing mathematical models in an investigation of the parameters of the Indian astronomical tradition. The discussion revolves around the question of whether Indian astronomers, notably Āryabhaṭa, arrived at their parameters through direct observation or by other means. Relying on mathematical analyses to near exclusion of other facets of the context is tricky. Mathematical analyses do have their place and can be useful, of course, but they are just one tool to bring light to the bigger picture when investigating astronomical parameters and dating astronomical texts.

The second article, in French and entitled 'Entre astronomie et mathématiques, les découvertes indiennes en trigonométrie. La construction des tables de sinus et quelques méthodes d'interpolation', is by Jean Michel Delire, the editor of the volume. The use of what we today call trigonometric functions is an essential part of Indian astronomy. Without it, a true planetary position cannot be computed from the planet's mean position. In order to compute sines and other trigonometric functions, the Indian tradition employs tables of sines. These tables are accompanied by interpolation schemes that allow the user to compute values not tabulated. Delire investigates how tables of sines are constructed and the different ways in which interpolation is used to arrive at intermediary values.

The third article, also in French and entitled 'Mathématiques et scolastique dans l'Inde médiévale. L'example du *Haricarita* de Parameśvara Bhaṭṭa', is by Pierre-Sylvain Filiozat. The *Haricarita* is an interesting text. Filiozat does not present the whole work but goes over a number of verses to demonstrate how they each have two layers of meaning. The first, and most obvious, layer of meaning narrates stories about the childhood of the god Kṛṣṇa. The second layer of meaning, accessible only to a specialist, contains astronomical information encoded in the so-called *kaṭapayādi* system, in which each Sanskrit syllable corresponds to a numerical value. Sanskrit verses with more than one layer of meaning are found in the writings of other Indian astronomers as well, notably Jñānarāja and his son Sūryadāsa. Such verses naturally span multiple genres of Sanskrit literature.

The fourth article, entitled '*Yantrarāja* for Dāmodara: The Earliest Extant Sanskrit Astrolabe', is by Sreeramula Rajeswara Sarma. Here Sarma studies and describes the earliest extant Sanskrit astrolabe. This astrolabe was produced in Ahmedabad in India for a certain Dāmodara and is now in a private collection in Brussels, Belgium. The question of its date and authenticity has been subject of controversy and to settle it Sarma accompanies his discussion of the instrument with a close reading of one of the early Indian works on the astrolabe, namely, the *Yantrarāja* of Mahendra Sūri. Sarma masterfully connects instructions given in the text with features of the physical astrolabe. The article furthermore includes eight images of the astrolabe, all of which are clear with legible writing.

The fifth article, entitled 'Indian Planetary Models: Āryabhaṭa to Nīlakaṇṭha', is by K. Ramasubramanian. The article deals with the planetary model of the Indian astronomical tradition. In particular, he gives a thorough discussion of the planetary model given by Nīlakaṇṭha in his commentary on the *Āryabhaṭīya* of Āryabhaṭa. Nīlakaṇṭha presents modifications to the standard model. Ramasubramanian masterfully discusses these modifications and also touches on why they were presented by Nīlakaṇṭha. However, the reason for doing so is not entirely clear and more studies of this question are needed to understand it fully.

The sixth article, in French and entitled 'Rythmes et algorithms. Le génie mathématique indien', is by François Patte. It explores the application of a mathematical rule in prosody. More specifically, the rule is a combinatorial one from the works of Bhāskara II, which was developed further by various commentators. This is an excellent example of a problem arising outside the field of mathematics that is solved by a mathematical analysis and rule.

The seventh and final article, entitled ‘Mādhava Series for π and Its Fast Convergent Approximations’, is by K. Mahesh, Venketeswara Pai R., and K. Ramasubramanian. The article explores a number of series for π from the Kerala school of astronomy and mathematics, which flourished between the 14th and 16th centuries AD. The series and the corrections to them offered by members of the Kerala school are incredibly sophisticated. Add to this that they worked without the help of the symbolic notation available to modern mathematicians. The Kerala mathematicians understanding of the series for π is impressive: the authors cite a passage by Nīlakaṇṭha which in modern terminology implies that π is an irrational number. Yet, while the authors address *how* the Kerala mathematicians took up the question of the series for π, they do not speculate on *why* they did this: it would indeed be interesting to learn why the school devoted so much time and effort to this topic.

As I have said, the present volume is a most welcome and important contribution to the field of the history of astronomy and mathematics in India. It should be noted, though, that the language used could have been more gentle at places. Scholars sometimes disagree and scholarly disagreements can get heated. Still, in a published volume, it is best to moderate one’s language and to adopt a respectful tone in order to ensure that the discussion becomes more constructive overall. Depicting one renowned scholar as lacking ‘scientific intelligence’ and being reluctant to abandon ‘cherished views’ [26], and another well-known scholar as displaying ‘a fundamental unwillingness to apply scientific arguments’ [36], as is done in this volume, does not seem productive. The discussion is an important one but it is best carried out by debating the arguments without such comments about individual scholars. That said, the volume is a valuable contribution to the field and is strongly recommended.

Medicine and Society in Ptolemaic Egypt by Philippa Lang

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Medicine and Society in Ptolemaic Egypt investigates the ways that Greek and Egyptian modes of healing interacted and influenced one another as large numbers of Greeks, from the ruling class to soldiers and merchants, took up residence in Egypt in the wake of Alexander's conquest. Drawing upon textual and material remains, Lang sets Greek and Egyptian perspectives in dialogue; the resulting exploration is rich, detailed, and as judicious in its use of varied data as it is in its conclusions.

Lang positions her work in contrast to prior studies of Egyptian medicine that have focused on Egyptian theories and therapies across time or on Greek medicine of the Alexandrian elite during the Hellenistic period. *Medicine and Society* seeks to understand how healthcare as practiced and consumed by locals changed with the influx of a heterogeneous Greek population, and how Greek practices within Egypt were shaped by diverse local traditions. Lang is interested in the perspectives of both practitioners and consumers, from elites to the lowest classes, and aims to give voice even to 'those silenced by the absence of any evidence at all' [xi], a task that Lang admits can yield only tentative conclusions. Lang excludes from her analysis forms of healthcare other than Greek and Egyptian, such as Jewish traditions, 'for simplicity's sake' [xi].

Chapter 1, 'Greeks and Egyptians', offers a wide-ranging overview of the physical environment, demographics, diet, and major health challenges of both the indigenous Egyptian populations and the Greek transplants. Lang is interested also in how sociocultural identity was negotiated through healthcare choices—a central concern of her book—and she considers here how power was negotiated and maintained in the wake of Greco-Macedonian conquest. She concludes that for the most part ethnic identity in the period was perceived neither in strict binary categories of Greek and Egyptian, nor

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was it fixed and imposed from the top down. This leaves open the distinct likelihood that some medical traditions were adopted and adapted by both immigrants and the host population alike.

Chapter 2, 'Medicine and the Gods', discusses temple medicine, a widespread form of healing within Egypt. Because many Egyptians understood illness to be caused by divine forces, they petitioned the gods for cures, especially Imhotep, Isis, and Sarapis. Lang cautions against the assumption that a particular mode of healing characteristic of some Greek temple medicine—encountering the god in a dream and being healed therein or given a regimen for cure—was widespread in Egypt. Other key differences include the organization and function of the physical space of sanctuaries (e.g., in Egyptian tradition, the public was confined to the outer courts of the sanctuary complex) and the centrality of texts to the healing experience (e.g., Egyptian sanctuaries included a *scriptorium* of medical texts and some sanctuaries featured *cippi*, monuments inscribed with texts whose potency was transferred to worshippers by bathing in the water in which the *cippi* also stood). Dreams, too, were part of the healing experience within Egyptian tradition but these dreams offered prognosis rather than advice or cure and had to be interpreted by cult personnel. Greeks in Egypt took part in some of these same traditions, as the presence of Greek-speaking dream interpreters in Egypt, for instance, attests.[1]

Chapter 3, 'Theoretical Perspectives', compares concepts of illness, causation, and treatment by Egyptian and Greek professionals. In Egyptian medicine, illness was often perceived as an invasion of the body by a malign or chaotic physical agent; the result was blockage of the *mtw* (vessels in the body) by a putrefying substance; purgatives were prescribed. Some Greeks also believed that food residues caused disease, though rarely did they think that illness was caused by an external agent entering the body. Lang concludes that Egyptian and Greek medicine developed independently before the Ptolemaic period. Lang also points to key differences in the rhetoric of medical texts: Greek texts, produced by a culture that embraced competition, were composed in the first person and designed to persuade; Egyptian texts derived from divine authorship and depended on the practitioner's ability 'to read and reproduce the power of the word' [132]. So what of the Ptolemaic

[1] One might add to the bibliography on Asclepieia in Egypt the catalogue of Riethmüller 2005, 2.399–405.

period? In Lang's view, Egyptian and Greek medical theories seem to have been represented and received as distinct, though both cultures incorporated material from the other, as is evident in, e.g., the appearance for the first time of Egyptian ingredients in Greek healing recipes and the presence of Greek medical texts in Egyptian temple *scriptoria*.

In chapter 4, 'Responses to Illness', Lang analyzes diagnosis, prognosis, and especially therapeutics. Egyptian practitioners (and presumably consumers) had greater interest in oral incantations, amulets, protective statuettes, polypharmacy, and quantification of drugs, and avoided certain surgical procedures more prevalent in Greek traditions (invasive surgery and phlebotomy are unattested in Egyptian practice; cautery is rare). Greek therapies in the Ptolemaic period would expand to include compound drugs and precise quantification, all possibly under the influence of Egyptian practices. In short, though little cultural transfer took place in the area of medical theory in Ptolemaic Egypt, in the area of therapeutics (and to a lesser extent diagnostics), influence is apparent, primarily in the adoption of ingredients for healing recipes and in greater reliance on the gods (as Lang states most succinctly on page 217).

Chapter 5, 'Identifying Medical Practitioners', comes to the unsurprising conclusion that most Egyptian physicians were male, elite, and concentrated in urban areas. Little evidence exists for specialists of any sort or for female practitioners. The first recourse for many Egyptians seems to have been self-diagnosis, self-treatment, and the gods. Greek physicians were mainly high-status members of the Alexandrian court and practiced a 'naturalistic' (as opposed to 'magical') form of healing. Greek practitioners borrowed ingredients from Egyptian medicine but resisted 'magical' procedures, nor did Hellenocentric methodologies extend far beyond the court. Lang explores also how choices of medical treatment could function as expressions of aspirational identity: if you want to be perceived as Greek, you may well choose a Greek practitioner and / or therapy. To this end, she investigates a medical tax (ἰατρικόν) paid by a wealthy sector of the Greek-dominated population that seems to have guaranteed medical services for a fee. Given that this tax is a phenomenon of the early Ptolemaic period, Lang suggests that the earliest Greek settlers may have been intent to display their Greek identity; the disappearance of the tax, in turn, may indicate greater fluidity of identity and more interactive cultural synthesis over time.

The final chapter, 'Medicine in Alexandria', brings us to the Ptolemaic capital. Lang draws attention to a contrast between the many ways Greco-Macedonian elites embraced elements of Egyptian culture in order to make themselves more acceptable to local populations and the exclusive nature of Greek medicine at Alexandria, at least at the level of elite practice. Lang borrows and expands upon Heinrich von Staden's description of Alexandria as a 'frontier' society to explore why this city in particular was conducive to medical innovation (e.g., the practice of vivisection is attested only at Alexandria and only in the third century) and suggests that the reason may have less to do with differences in social norms and religious observance than the political interests of rulers like Soter.

By investigating a wide variety of data, from medical treatises to laws, and from papyri to *ostraka* and bones, Lang's ambitious book takes the study of Greek and Egyptian medicine into new territory. We get a sense of how non-elites as well as elites, both Egyptian and Greek, navigated the choices that multiplied with the influx of new populations and ideas. The fact that the data available is uneven by region and across socioeconomic, cultural, and gender divides, means that the discussion, too, can feel at times uneven as tentative conclusions sit uneasily on loose and thin conglomerations of evidence. Nonetheless, Lang lays a strong foundation for further investigation as more data becomes available, especially through burgeoning fields like bioarchaeology. Lang's book complements a current trend in classical scholarship on identity studies and opens the door to future projects: How, for instance, do traditions other than Greek and Egyptian fit into this picture of Ptolemaic medicine? Lang's analysis also reinforces the significance of healthcare choices *per se* as a determinant of identity in the ancient Mediterranean. Lang's prose is clear and engaging, and she includes four useful indices (on subjects, proper names, places, and citations) that are a model for academic publication.

I suspect that the primary audience of this book will be specialists in the subfields of Egyptian and Greek medicine. Lang's book could be more user-friendly, even for a specialist audience, and my comments here are directed as much at the publisher as the author. First, an overview of the order in which Lang will lead the reader through the material is sorely missed. Second, the map of Ptolemaic Egypt (Figure 1) is inadequate: the text mentions locations that do not appear on the map; moreover, I went looking for 'Map

2', as printed on Figure 1 but it is nowhere to be found in the book. The illustration of a *cippus* (Figure 2; also the cover art) goes unreferenced in the text nor is the label, 'Cippus of Horus', very informative; and I would have appreciated a plan of an Egyptian sanctuary since arguments in chapter 2 rely heavily on spatial analysis. Finally, since Brill is charging $150, consumers have a right to expect clean copy: in addition to several errors in the body of the work (e.g., I suspect 'bowls of water' rather than 'bowels' was meant on page 49), it is especially dismaying to see a typographical error on the back cover.

These few shortcomings notwithstanding, Lang's book is a must-read for any scholar interested in Greek and Egyptian medicine, particularly of the Hellenistic period.

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Lost Enlightenment: Central Asia's Golden Age from the Arab Conquest to Tamerlane by S. Frederick Starr

Princeton, NJ/Oxford, UK: Princeton University Press, 2013. Pp. xii + 634. ISBN 978–0–691–15773–3. Cloth $39.50

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Most historians strive, once in their lives at least, to produce a 'grand narrative' that, even if for a limited area of inquiry, brings it all together once and for all. To accomplish such a feat requires a thorough knowledge of a field (more than one for 'grand, grand narratives'), the required linguistic competences if an author is working in an area where the primary sources are not in English, and a clear delineation of the goals and scope of the planned project.This includes, for books such as that of Frederich Starr, a clear chronological and geographical delineation. For this reviewer also important is that a 'grand narrative' provide a useful overview and that this should include good notes and a strong bibliography. Also requisite of a narrative of this sort is an engaging style, an ability to connect with readers and interest them in a grand endeavor which can cover impressive ground but must do so without either the author or the reader becoming lost.

Writing a book like this is never easy and bringing it off is even harder. But it can be done. A recent example of what can and should be accomplished is Lincoln Paine's *The Sea and Civilization: A Maritime History of the World* [2013]. The author's grasp of his topic and ability to deliver the goods in terms of a lucid and well written account is simply amazing.

Alas, Frederick Starr is no Lincoln Paine although Starr's book is well written and entertaining. The book has so many problems that it is difficult to know where to start. Starr's first and perhaps biggest problem is that his 'Central Asia' simply does not exist. Not only does he talk more often about events in what is clearly and unmistakably Iran, he has little interest in, or understanding of, anything north of Samarqand, much less the Kipjaq steppe or eastern Turkistan or Mongolia. His 'Central Asia' does not even accord with

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the Russian use of the term (essentially, for western Turkistan). It is certainly not what he claims it is and no better example of his misunderstanding of a larger context is his presentation of much of western Turkistan as little more than a pile of rubble, without much trade or life after the Mongol invasions. This is not only incorrect according to recent research (which he ignores in favor of very old studies such as that of Barthold [Barthold 1968]) but totally ignores the role of the Golden Horde in a revived Silk Road trade, as exemplified by the large and beautiful cities excavated by Federov-Davydov on the lower Volga. Western Turkistan was certainly no wasteland and had major contacts reaching far into the south and even across the Black Sea where a first maritime age was being played out.

A second major problem with Starr's book is that it is written almost exclusively from an outsider's perspective, although he has travelled in the area. That is, Starr reads none of the relevant languages of his 'Central Asia', including Persian, well enough to consult primary sources. Thus, he lacks the perspectives and insights that come with many years of immersion in foreign linguistic cultures. We can never be insiders for something that happened so long ago. But, for me, by reading and rereading the Persian histories, for example, coupled with what is, I think, the most important source of all, the *Secret History of the Mongols*, the only native perspective on what happened, I gain a direct appreciation, for instance, of the events of the Mongol invasion of Turkistan, the important feel that comes from becoming so immersed in the sources that one almost begins to think like a native.

Starr cannot do this and must rely on translations that come with their own baggage. In addition, his approach deemphasizes historical sources: there is no Ibn al-Athir, for example, even though this Arabic source now exists in an excellent translation. Juvaini, whose work Starr does use in Boyle's excellent rendering, is there but because of Starr's lack of knowledge of the comparative value of primary sources and of what scholars have said about them, he is only too willing to believe any tale Juvaini relays, even the one about more than a million dead by the hands of the Mongols from a city that may have had no more than 50,000 in it to begin with and, of course, the story of the dogs and cats that had obviously done something to disturb the Mongols in a previous life and were killed for it. That is to say, Starr is unaware of the tendentious propaganda throughout Juvaini's account and the internal politics of the Mongol Empire that this represents. Otherwise, he might have privileged Ibn al-Athir who, unlike Juvaini, was an eyewitness to the Mongol invasions and has no dogs and cats.

Given his limited grasp of the primary sources, even in translation, Starr is forced to rely on secondary scholarship. But he lacks selectivity and seems unable to sift out the old and obsolete from the new. He does read Russian but much of what he cites is old. Moreover, as with primary sources, Starr is unaware of the biases in authors like Barthold and the problems with using, unvarnished, out of the box, Central Asian (in this case using the Soviet definition) scholarship, given that it may be tainted with nationalism that creates or overstates a vision of the past.

In this connection, a big problem is that so much of Starr's narrative comes from generally old literary histories. Literary histories have very different agendas and tend to over-dramatize. They are not the same, for example, as histories of a place or places or of a time or times that have been properly critiqued and come to us with full notes and apparatus that we can cross-examine. There are reasons that F. Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* [1972] is a classic and that it is still authoritative after all these years, even though the scholarship has continued to be updated since Braudel's time. Starr lacks feeling for such works and it seems that for him any old narrative will do.

This fact emerges no more clearly than in his notes and bibliography. In fact there are few notes and no bibliography at all. This being the case, how is Starr's a work that one can take seriously?

There are many minor issues as well. Starr's population estimates for his cities are mostly too high. Some of this is based on the sizes of surrounding walls. But northern Iran or Uzbekistan is not Europe. Inside the walls were not only urban structures but fruit trees and gardens—acres and acres of gardens—and the like. It is by considering this that Federov-Davydov, criticized by Starr for his low estimates, arrived at them. As an archaeologist, and one excavating similar cities, Federov-Davydov was only too aware of how places such as New Saray were structured and of who lived where and did what. We disregard at our peril the advice of such people. Another problem with Starr's book, more minor, is that for him his Central Asia is everything, which leaves him free to disregard the world outside it. Thus, at one point he mentions a 'Central Asian' city, Merv, which is not remotely in Central Asia, as having a population of 200,000, probably far too high, and implies that it might have been the largest in the world. In fact, Song Hangzhou was probably at least two and a half-times that before 1204; Constantinople was larger too, not to mention the up-and-coming world-city, Cairo.

Largely ignoring the outside world also creates another problem for Starr. He seeks to argue a post-Timurid decline, one that he suggests was general for the Islamic world. Leaving aside the issue of what the supposed decline meant or that it even happened and is not instead some kind of post-colonialist self-delusion in our sources, he can assert what he does only by wearing his own particular set of blinders. The Ottoman Empire was flourishing in the 16th and 17th centuries, including an Ottoman age of exploration that took soldiers as far as Aceh in Indonesia. Some decline! And even if Starr barely remembers key figures of this efflorescence, such as the polymath Sinan, the Ottoman cultural achievement was nonetheless impressive. But poor Sinan, according to Starr, his only claim to fame was that he borrowed architectural ideas from 'Central Asia'. Not exactly.

Starr also understates the importance and sophistication of Mughal India, an area that did have a direct 'Central Asian' connection (including the Chaghatay language) whose significance Starr seems barely to understand. Then, there is medieval Cairo. Hardly an image of universal decline. Further, as a number of scholars have pointed out, even Islamic science did not suddenly close down and disappear. Muslim scholars continued to make contributions, as George Saliba and David King in particular have made clear.

So, in summary, this is not a very good book. Rather than a 'grand narrative' Starr has produced, by and large, an exercise in futility. And because of a strict limitation in space, I have confined myself to the book's big problems. Not catalogued are its many grand errors and misconceptions. It is not recommended.

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L'homme au risque de l'infini. Mélanges d'histoire et de philosophie des sciences offerts à Michel Blay edited by Michela Malpangotto, Vincent Jullien, and Efthymios Nicolaidis

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This volume of over 400 pages, containing no fewer than 33 articles, is indeed a set of 'mélanges' in the history and philosophy of science that was presented to the French scholar Michel Blay in celebration of his career after retirement. As the introduction relates, Blay was first trained as a physicist; he then turned to the history of science because he wondered about the adequacy of mathematics to natural phenomena and wanted to understand how physics came to be mathematized in the 17th and 18th centuries. Blay's most important contributions to the history of science lie in two areas that allowed him to deal with this problem: Newton's optics [Blay 1983] and French post-Newtonian analytic mechanics [1992]. In connection to his interest in the mathematization of physics, Blay also devoted part of his research to the topic of infinity, mainly in the cosmological domain [1993, 2010] but also in relation to the invention of infinitesimals [1986, 2001, 2010]. His reflections on the history of science also led him to analyze the ways in which science was organized and financed in the 20th century; and he devoted some of his work to a critical study of contemporary science policies [2003].

The introduction also highlights Blay's ongoing defense of the idea that science and, in particular, early modern science developed mainly as a theory-driven activity and not (as others argue) primarily on an experimental basis. For Blay, the keystone to his interpretative approach is to be found, beyond any *a posteriori* rhetorical reconstruction, in Newton's optics, which Blay sees as a largely theoretical process leading to experimental proofs rather than as an inductive activity. He thus turned traditional interpretations of the prism experiments upside down. From that point of view, Blay aligns with Alexandre Koyré, who considered physics to be first and foremost an *a priori* activity. Blay privileged an internalist reading of the history of

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science and can also be viewed as an heir to the French tradition of historical epistemology.

The diversity of topics addressed in this book reflects the broad range of Blay's research interests—from antiquity to the contemporary period. Some of the articles clearly belong to the history of science; others, to the history of philosophy and science; and still others, to political issues linked to science. Some articles merely relate matter-of-fact historical information based on yet-unexplored archives; others express an interpretative claim that challenges received views about a long period of the history of science. Within such a broad range of approaches, one can hardly find a school of any kind, let alone Michel Blay's school.

Although the length of each article is quite limited—around 10 pages for obvious editorial reasons—some succeed in providing a synthetic and interesting approach to their topic. Since it is not possible here to tackle them all, I will confine myself to a select few.

First, a word on the book's structure. The articles are gathered into three sections:

(1) *La science classique*,
(2) *Science, littérature et art*, and
(3) *Science, philosophie et politique*.

As can be expected for this genre, there is no real unity—either topical, methodological, or historical—to be found in this book. Rather than follow the sections as they were organized by the editors, I will trace lines from some of the authors' contributions to more general issues and, in particular, to the topics investigated by Blay and the methods that he employed. The title, 'L'homme au risque de l'infini', attempts to encompass a diversity of topics. Yet the volume lacks any in-depth study of the notion of mankind or of infinity. Most probably, the editors intended the title to remind us that science must be analyzed as a human activity involving every aspect of human life—not only intellectual, but also artistic, social, and political. Infinity—a topic dear to Blay—refers to the subject matter of scientific practices, and encompasses mathematical as well as cosmological infinity, the infinite as well as infinitesimal entities.

In section 2, several articles explore the relationships between science and the visual arts: see Pierre Caye, 'De la scientificité des arts. Réflexions sur le rapport entre les arts plastiques et les mathématiques à l'âge humaniste et classique' (mainly on Alberti); or Michèle Gally, 'Points de *vue*. Science et poésie en dialogue (XIII^{e}–XV^{e} siècles)' (science and literature); François Roudaut, 'Quelques remarques sur le Soleil chez un poète encyclopédique du XVI^{e} siècle' (on Guy Le Fèvre de La Boderie); and Frédérique Aït-Touati, 'Le savant et le poète: Hooke lecteur d'Ovide'. These articles intend to show not only that science was integrated into art or literature as a foreign element that would nourish artistic inspiration or as a set of techniques offering new artistic possibilities, but also that, before the 19th century, science and art could form part of a single activity. Art was thus intrinsically scientific or had scientific value. Among those papers, Frédérique Aït-Touati's will certainly present the greatest interest for historians of science. She offers a study of a little-known text by Robert Hooke: his commentary on Ovid's *Metamorphoses*. She shows how Hooke read it as a fictionalized account of historical events related to the formation of the Earth, an account that foreshadowed what Hooke considered to be a hypothesis, namely, universal gravitation. This hermeneutic was a substitute to experience and served as a real proof in natural philosophy. Aït-Touati's paper is particularly valuable because it not only sheds light on a new facet of the curator of experiments at the Royal Society but also presents clearly Hooke's hermeneutical reading of ancient texts as methodologically akin to his experimental and biblical exegetical activities.

In section 3, we find articles that explore the relationships between science and religion, science and philosophy, and science and politics. An article by Philippe Büttgen deals with the relationship between science and dissidence through an analysis of Lessing's interpretation of the behavior of the anti-Trinitarian Adam Neuser ('La raison de sang-froid. Une page de Lessing'). In 'Les condamnations d'idées scientifiques par l'Église orthodoxe', Efthymios Nicolaïdis proposes an overview of how the Orthodox Church reacted to scientific innovation from the fourth century to the 19th, beginning with the Greek Fathers. Counterbalancing the better-known relationships between scientists and the Catholic Church, this article offers a picture in which the debates are mainly internal to the Church itself.

Three articles in this section are more concerned with the philosophy of science. Among them, 'La philosophie des sciences à la Belle Époque' by Anastasios Brenner retraces the historical development of this discipline in

France, from Poincaré and Duhem to Meyerson and Bachelard. He shows that its birth is older and more complex than the traditional view—which traces it back either to the Vienna Circle or to Bachelard's historical epistemology—has led us to think.

Other articles in section 3 deal with the interactions between science and politics from the 17th to the 20th century, including the emergence of science policies after World War II. One article extends to the end of the 21st century! In 'Une histoire des sciences au XXI^{e} siècle', Jean-Marc Lévy-Leblond offers us a pleasant tale of fictional history (supposedly written in 2213) in which he imagines the disastrous consequences of 20th-century science policies based solely on an economy-driven science, without any room for reflection on its concepts and theories.

In the articles that I have mentioned so far, the authors cross disciplinary boundaries and sometimes address the history of science from an externalist point of view. Regarding 'infinity', however, several contributions adopt a more internalist approach. They are to be found in section 1. In 'Gli indistruttibili paradossi di Zenone', Giorgio Israel identifies, in Zeno's paradoxes on the composition of the continuum, one source of the Greeks' reluctance to provide a mathematical treatment of the infinite.

Sabine Rommevaux's article, 'Six inconvénients de la règle du mouvement de Thomas Bradwardine dans un texte anonyme du XIV^{e} siècle', also relates, though not in a straightforward way, to the topic of infinity. In his *Tractatus de proportionibus*, Bradwardine had formulated a rule of motion that allowed for comparing the speeds of motion according to the ratio between the driving forces and the resistance of the object moved. Rommevaux analyzes some objections to this rule formulated in an anonymous manuscript written between the 1330s and the 1340s, which is to be found at the Bibliothèque Nationale de France in Paris (lat. 6559). Even before Nicole Oresme, this writing relied, among other things, on a kind of ingenious thought experiment in which a body falls through void space towards the center of the Earth. When the lower part of the body reaches the center of the world and passes beyond it, an increase of internal resistance and a diminution of speed are induced. Contrary to Aristotle, the author therefore considered this motion as possible without being accomplished at an infinite speed. This article thus illustrates the ability of medieval thought experiments to test theories, to formulate more precise notions such as internal or external resistance, and even to challenge some dimensions of Aristotle's physics.

Several articles that deal with the history of science in a strict sense, mainly in section 1, are worth mentioning, including those concerned with astronomy and cosmology. In the line of, for example, Michel-Pierre Lerner [1979] but maybe with a bolder interpretative commitment, Michela Malpangotto proposes to interpret the Scientific Revolution as born out of some elements of medieval science freed of Aristotelianism by the humanist rediscovery of mathematical sources, in 'Réévaluer l'humanisme mathématique'. Her argument is convincing, at least as far as astronomy, the field on which she focuses, is concerned. Jean Seidengart insists on the convergence of mathematics with metaphysics and theology in the elaboration of Kepler's cosmology, in 'Mathématique et métaphysique dans les recherches astronomiques de Kepler'.

By opposing Copernicus and Galileo, Maurice Clavelin, in 'Du cosmos aux marées. La justification de l'héliocentrisme chez Copernic et Galilée', seeks to elucidate the relationships between philosophy and science in the early modern period. He provides a step-by-step analysis of Copernicus' arguments in favor of heliocentrism that culminate in the central position of the Sun in a well-organized cosmos. But, whereas Copernicus could still rely on the idea of the world as being a limited and well-organized entity, Galileo had to build new arguments to support heliocentrism in a universe conceived as indefinite. His theory of the tides was precisely intended to provide a physical proof for the new cosmology because Galileo considered it impossible to account for the tides independently of the Earth's motion around the Sun. By this comparison of the two astronomers, Clavelin can subtly distinguish between Copernicus' heliocentric argument and Galileo's geokinetic justification, a distinction that can be accounted for by philosophical reasons and new celestial observations. Yet Clavelin identifies an important shift in which both Copernicus and Galileo play a role: when cosmology begins to be defined not by natural philosophy but by the astronomer.

In 'L'héliocentrisme réfuté par l'alchimie: Pierre Jean Fabre et l'immobilité de la Terre', Bernard Joly proposes an original approach to the topic of the reception of the heliocentric theory in the 17th century. Recent scholarship has indeed rehabilitated alchemy as an experimental science that could have made a contribution to the Scientific Revolution [see Newman 1994, Principe 1987]. Here Joly not only shows how a 17th-century alchemist could take a stance on contemporary cosmological debates; he also reveals how alchemy, understood as an encyclopedic science, could provide arguments based on a chemical representation of the properties of earth and light *against* heliocentrism and the motion of the Earth.

The focus of Vincent Jullien's article, 'Gassendi à Marseille, qu'allait-il faire dans cette galère?', is an experiment performed by Gassendi in 1640 in which a ball is thrown from the top of the mast of a ship sailing at high speed, thus sustaining the principle of inertia and challenging some objections opposed to the motion of the Earth. In addition to those instances already identified, for example, in Clavius, Bruno, and Froidmont, Jullien mentions some unnoticed early occurrences of this experiment in Ptolemy, Averroes, Nicole Oresme, and Alessandro Piccolomini. But he also adds a report by Isaac Beckman, dated to 1619, of the performance of this experiment in Holland. Siding with Blay's interpretation of early modern science as *a priori*, Jullien concludes by claiming that this experiment, though it had a convincing weight in favor of the principle of inertia and could have contributed to removing one objection against the motion of the Earth, did not in fact demonstrate anything or give crucial support to the argument in favor of heliocentrism.

To conclude, I shall mention three articles that are related to another scholarly domain to which Blay has contributed: Newtonian science. Suzanne Débarbat ('Newton, ses *Principia* de 1687 et les astronomes français') draws links between Newton's 1687 *Principia* and works of French astronomers that might have provided him with relevant information on the shape and dimensions of the Earth. Niccolò Guicciardini's 'Une note sur Newton et la tradition néo-pythagoricienne' is a more synthetic version of another of his works [2013] which downplays Newton's commitment to a form of neo-Pythagoreanism in his examination of possible analogies between light and sound. In 'Euler et la mécanique newtonienne: d'une mécanique géométrique à la mécanique analytique', Marco Panza and Sébastien Maronne offer a study that complements and chronologically extends Blay's interpretation of the reception of Newtonian science in association with the birth of analytic mechanics. They focus on Euler and show how he revived Newtonian mechanics by incorporating competing views from Descartes and Leibniz. But, more importantly, they consider that it was Euler who built the so-called Newtonian analytic mechanics, based on differential calculus and emancipated from the representation of geometrical figures.

As can easily be understood from this account, the interest of this book resides more in the diversity of its contributions than in any form of commitment to a method or in any focus on a period or topic. Most likely, the reader will only be interested in some of the contributions as far as they are related to her or his area of research. But the book as a whole offers an opportunity to stroll along winding paths into the history of science from antiquity to the 20th century.

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Count Like an Egyptian: A Hands-On Introduction to Ancient Mathematics by David Reimer

Princeton, NJ/Oxford, UK: Princeton University Press, 2014. Pp. xvi + 237.
ISBN 978–0–691–16012–2. Cloth $29.95

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Count Like an Egyptian is an engaging and beautifully illustrated book that deals with the basics of ancient Egyptian mathematics, set in the wider context of other ancient mathematical systems.

The book consists of eight chapters bearing simple titles. 'Numbers' introduces the reader to numbers and simple operations. 'Fractions' explains in a clear and effective way how to deal with ancient Egyptian fractions, now called 'unit fractions' (fractions with 1 as numerator, e.g., $^1/_3$). The third chapter, 'Operations', offers a practical interpretation of some geometric and arithmetic operations. 'Simplification' deals with ways to simplify calculations and procedures. 'Techniques and Strategies' analyzes the mathematics associated with a number of practical and symbolic issues. 'Miscellany' addresses geometric problems as well as geometric and arithmetic series. The seventh chapter, 'Base-Based Mathematics', moves the focus to other ancient mathematical systems (prehistoric, Mayan, Roman, Sumerian, and Babylonian). 'Judgment Day' compares the ancient Egyptian system with modern mathematics. The book ends with 'Practice Solutions', a series of examples and exercises referring to some of the issues discussed in the previous chapters.

The text is visually animated by the presence of color illustrations on every page: operations expressed in numbers are printed against backgrounds in the shape of unrolled papyrus scrolls or tablets; fractions and subdivisions are represented by compositions of coins, bricks, cubes, or even slices of pizza; practical operations are explained thanks to simple representations; the addition of small schemes, of representations of the gods mentioned in the text, and other simple objects all contribute to the comprehension of the text and, at the same time, make it pleasant reading.

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Reimer's book definitely fills a gap in the modern study of ancient Egyptian mathematics. Its starting point is the fact that ancient Egyptian mathematics was based on a system and followed rules that are sometimes very similar to their modern counterparts and sometimes so different as to appear incomprehensible. Even a slight shift in our modern point of view can help us enter the ancient system on a path that veers from our modern mentality but affords a glimpse of a consistent and efficient method nonetheless.

The focus of this book is, therefore, the difference between modern and ancient mechanisms: it is neither a history nor an analysis of mathematical sources. Instead, it represents the first comprehensive effort to explain the basic mechanisms of ancient Egyptian mathematics—an important step if we wish to improve our understanding not only of the ancient system but also of the mentality that lay behind it and manifested itself in other fields, such as administration, arts, and architecture.

Extant mathematical documents are few in number (a dozen or so). The majority date to the Middle Kingdom (2055–1650 BC) and the remaining to the Ptolemaic and Roman Period (332 BC–AD 395), with a gap of over 1,000 years between the two groups. The Middle Kingdom sources mainly come from an educational context: the most important is the Rhind Mathematical Papyrus, probably a manual for a late Middle Kingdom teacher that was translated for the first time in 1877 and then again in the 1920s [see Peet 1923; Chace, Bull, Manning, and Archibald 1927–1929]. The other main documents were all translated for the first time between 1898 and 1930 [see, e.g., Glanville 1927, Struve 1930]. More recently, they have appeared again in specific publications [Robins and Shute 1987] and in a number of sourcebooks [Clagett 1999, Katz 2007].

The small number of original sources and their nature suggest caution, as we clearly have a limited perception of the subject. Nevertheless, these sources are consistent and do offer the chance to discuss implications and interpretations.

Only a few monographs have so far appeared on the subject of ancient Egyptian mathematics [Gillings 1972, Couchoud 1993, Imhausen 2003]. The first monograph to be dedicated to the subject was Gillings' *Mathematics at the Time of Pharaohs*, published in 1972. The subsequent evolution of both the comprehension of ancient Egyptian language and of mathematical studies highlighted faults and inconsistencies in some of Gillings' interpretations. But his attempt to understand the mechanisms behind operations and procedures opened the way to an interesting and productive line of investigation, which in fact culminates in Reimer's book.

The number of articles and book chapters dedicated to specific aspects, instead, is far greater. Listing them all is impossible here. Some are chapters in books on the history of mathematics [e.g., Ritter 2000, Imhausen 2007], while the majority focus on specific issues or problems.[1] Many of these publications are extremely specialized and meant for experts in the field [e.g., Pommerening 2005].

Discussions of the interpretation of specific mathematical problems (as one might expect) have a relatively restricted public but their implications have a wider impact. Egyptology is nowadays a huge field, including specializations very different from one another. The subject of ancient Egyptian mathematics has struggled to gain a prominent place for the reasons stated by Reimer in the introduction:

> Egyptian mathematics has an alien feel to it. Most math historians refer to it as primitive or awkward. Even worse, many simply ignore it except for a passing reference. They look at this system and feel uncomfortable because it's so different. [ix]

Luckily the situation is changing. This is important not only within the circle of historians of mathematics but also for Egyptologists working on other aspects of this ancient civilization: mathematics was in fact involved in so many aspects of daily life that ignoring its mechanisms means leaving out an important part of the picture.

Certainly the most evident merit of this book is its approach, which brilliantly dismantles any resistance to the subject by the most recalcitrant and mathematically-impaired reader. The author's skill is shown not only in his presenting one subject at a time in a clear and down-to-earth way but also in his slowly penetrating the otherwise baffling system of ancient Egyptian mathematics. The reader is led to discover mechanisms and to appreciate details, and eventually to gain a new perspective on the subject. Finally, the method of setting ancient Egyptian mathematics within the wider context

[1] Just to mention a few: Gunn and Peet 1929, Fletcher 1970, Gerdes 1985, Imhausen 2002, and Rossi and Imhausen 2009.

of other ancient mathematical systems usefully draws attention to the basic mechanisms of each system and to their differences and similarities.

I am a little surprised by Reimer's choice not to include bibliographical references. After all, the book presents itself as a possible textbook for university courses. Moreover, its subject is connected with many fields (such as administration, arts, and architecture) within the huge subject of Egyptology. If the author's conclusions find correspondence to other, contiguous areas of research, it would have been extremely useful to see the links and how they were made. Otherwise, this is a brilliant and entertaining book that can be enjoyed by academics as well as interested readers of various backgrounds.

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Aristotle's Empiricism: Experience and Mechanics in the 4th Century BC
by Jean De Groot

Las Vegas/Zurich/Athens: Parmenides Publishing, 2014. Pp. xxvi + 442. ISBN 978–1–930972–83–4. Paper $127.00

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The early history of the discipline of mechanics—arguably one of the least well-understood aspects of the history of the natural sciences in antiquity—is receiving more attention in recent scholarship. Aristotle's contribution to that history is one of the least clear of all its chapters. Jean De Groot's *Aristotle's Empiricism* is thus timely in its subject matter. This book attempts a synthetic account of Aristotle's engagement with questions of the causes and dynamics of motion, viewed against the background of the mathematics and natural philosophy of the period. Its strength is that it canvasses Aristotelian texts as disparate as the *Categories* and *Poetics*, and gives serious attention to relevant passages, such as *Problemata* 16, that have been little studied. This is a considerable undertaking and it is to be hoped that De Groot's work inspires closer attention to this relatively neglected aspect of Aristotle's thought.

The book is, unfortunately, torn between two rather different projects, which seem somewhat in tension with one another. The first is to uncover the sources of inspiration for the emergence of mechanical thinking in the fourth century BC, that is, to examine the insights into the nature of the action of the lever and the attempts to explain it mathematically at that time. On this, the book offers a number of promising suggestions for inquiry. De Groot's extensive knowledge of the history of science is employed to good effect and her sensitivity to the possible empirical inspirations for abstract concepts leads to some illuminating suggestions.

The second project, an attempt to read Aristotle's biological work as infused with ideas about δύναμις or power construed by De Groot sees as applying mechanics in some 'more expansive' sense [161], is, in this reviewer's opinion, less successful. This project is based on the idea that mechanics shows

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how power or δύναμις can amplify effects [124], thus licensing inferences about hidden powers and their abilities to produce large changes from small changes. It is, as she acknowledges, an unfamiliar reading of Aristotle [50, 160–162, 366] and one that risks erasing his distinction between natural and artificial form [133].

De Groot, however, sees both kinds of form as 'mechanical' and as pervading Aristotle's work, her central thesis being that the work on the lever and its properties licensed a kind of inference to natural powers, and that Aristotle took this insight into various domains, including biology. The difficulty is that there are different ways to read De Groot's ascription of 'powers'. We might take her to mean that a central insight of the mathematical investigation of devices—such as that found in the Aristotelian *Mechanica*—is that something analogous to a modern concept of 'force' must be posited as a finite and conservative factor in mathematical explanations of motion. De Groot seems to intend just this when she claims that for Aristotle 'powers' are an 'empirical concept closely linked to a universalizing mathematical rule' [15]. Yet, she also poses a dichotomy between 'a powers model and a matter-in-motion model' [16], as though powers are seen as doing rather more than just explaining motion.

De Groot's suggestion that Aristotle imported the insights of mechanics into biology by a kind of 'topological deformation' [249–250] does not avert the suspicion that this second project leaves behind any meaningful conception of mechanics. If the lever is merely 'an analogy for the enhancement of effect' [108], the license for inference surely risks going poetic [110, 124, 133, 148–149]. A central text used to argue that Aristotle intends to apply the 'moving radius principle' as a systematic explanatory tool in his biology is *De motu animalium* 7, where he notoriously refers to devices in discussing the ability of the sensitive faculty to cause a large movement of the limbs from a small expansion of the *pneuma* around the heart. But the crucial question is surely not whether Aristotle *uses* artifact analogies—who does not?—but whether he takes them to be a *sufficient* explanation. The fact that he introduces the mysterious *pneuma* to account for this ability in *De motu an.* 10 should at least be considered.

The other notorious analogy to working artifacts is in *On the Generation of Animals* 2.5. De Groot's chapter 6 reads this passage as evidence of a programmatic account of biological development. She seems to think that the thrust of the artifact analogy is to *delimit* the role of δύναμις [148–149]. But how it does so is not clear. Nor does she make clear the extent to which her reading of Aristotle's biology leaves behind natural form: at one point, she suggests that the powers are 'latent in materials' [156]; but elsewhere the powers in question are said to be 'proper to the entity under examination' [113]. It is a speculative account and the author recognizes this. (The treatment of the secondary literature in this chapter is particularly sparse.)

Even those unconvinced by this will be interested in her contributions to the first project, that of recovering the early history of the discipline of mechanics. De Groot focuses on what she calls the moving radius principle and collects the evidence for this from throughout the works generally ascribed to Aristotle himself. The principle itself, as De Groot explains it, is a *mathematical* rule describing a relationship between the geometry of circles and the (linear) motions described by points on these circles when they rotate. She presents this principle in two ways:

(1) '[P]oints moving circularly at different distances from a common center are covering different distances in the same time' [25], and
(2) '[R]evolving concentric circles are traveling at different speeds' [27].

De Groot focuses on this principle because of its role in making manifest the ways that δύναμις is at work in the world [126–127] and presumably also the ways in which it is subject to proportional limits. De Groot claims that Aristotle 'understands the moving radius principle to index natural powers' [12], where 'indexing' is explained as according ontological import: since '[w]hat produces action is real'[13], there are grounds for acknowledging the work of 'powers' in producing action.

Understanding the history of the notions of power or force is central to the history of the discipline of mechanics and its contribution to the development of mathematical methods for studying physical motions. De Groot has some promising contributions to this kind of 'cognitive history' [cf. Netz 1999], especially in her examination of weight or her suggestion that the kinesthetic experience of sensing the different quantities of effort required to move weights in various contexts contributed to the recognition of forces as finite quantities. Her analysis of the pre-Aristotelian material here is helpful, as is the reading of *De motu an.* 4 with its recognition that forces can be used to account for inaction as much as motion. There may also have been other factors that encouraged quantification, such as the importance of weight-lifting technology for massive building projects, where logistical estimates would have been important, or the use of mathematical scaling-up in early ballistic devices. The analysis of the lever is not the only mathematical consideration concerning the causes of motion that we find in the surviving evidence for early mechanics: a full treatment of Aristotle's thought would want to pay more attention, for example, to issues such as Aristotle's treatment of the composition of powers [Hussey 1991].

De Groot's subject is very definitely Aristotle and, as with any work on that enigmatic thinker, it needs to take sides on questions about the state of the corpus. The question is particularly acute in reconstructing the history of mechanics since a key exhibit, the *Mechanica*, is not widely thought to be written by Aristotle himself. De Groot argues that *Problemata* 16 is contemporaneous with Aristotle, as part of her claim that 'mechanical phenomena underlay his scientific thinking more generally' [163]. She acknowledges that the *Mechanica* is likely written several decades after Aristotle's time and contains unAristotelian elements. The figure often taken to be the author of the *Mechanica*, Strato of Lampsacus, is only mentioned in passing as the compiler of *Problemata* [165]. The possibility that the *Mechanica* reflects ideas from early third century Alexandria is not considered, nor is its markedly unAristotelian treatment of natural and nonnatural motion given much consideration.

This is a large and ambitious project. Clarity of exposition is rather hampered by book's organization, with some critical expository chapters only coming at the end. The treatment of the secondary literature is often cursory and some idiosyncratic notions like active receptivity [135ff] or gnomonic complementarity [340] are hard to grasp. Some of its range might well have been sacrificed for a more detailed account of particular issues, e.g., Archytas' contributions; intriguing suggestions such as the role of Aristoxenus in Aristotle's thought [298] are only hinted at in footnotes. This book covers a lot of ground and it is unfortunate that its range and unevenness means that some of its insights could be missed.

Nonetheless, the key ideas, especially as they are developed in chapters 10 and 11, suggest a synthetic vision of Aristotle's engagement with the project of mathematizing natural philosophy and the possible role of mechanical devices in inspiring that vision. Those inclined to a more cautious approach would have appreciated a more systematic articulation of that argument, distinct from the more speculative material about the application of powers in other domains. Yet this is a difficult topic, given the scattered nature of the evidence and the challenges of reconstructing the world picture of thinkers from a different era. Revising the early history of science requires imagination and the willingness to take intellectual risks. The ambition of De Groot's work is admirable and there is much here that may contribute to a more precise account of a critical chapter of the history of mechanics and natural philosophy.

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Neoplatonism and the Philosophy of Nature edited by James Wilberding and Christoph Horn

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After a long period of neglect, recent decades have seen an increasing interest in, and revaluation of, Neoplatonic physics or, as James Wilberding and Christoph Horn prefer to call it, philosophy of nature [1]. The articles in this fine volume, many of which were originally presented at a conference hosted by the University of Bonn in 2007, are among a variety of recent high-quality publications on this topic—including the proceedings from an international workshop held in Castelvecchio [Chiaradonna and Trabattoni 2009], the second volume of Sorabji's *The Philosophy of the Commentators* [2005], and Wilberding's works on Plotinus' cosmology [2006] and Porphyry's biology [2011].

These few references, which are only a small part of the relevant studies on this until now under-researched topic, show that the prejudicial view that Neoplatonic philosophers had little to contribute to the investigation of nature and physical reality is starting to fade away. The resurgent interest in Neoplatonic views on nature does not imply that Neoplatonists were natural scientists in the style of Aristotle, Galen, or Ptolemy.[1] However, while their explanations of specific physical phenomena tended to lack detail, the Neoplatonists generally had a coherent and comprehensive account of physical reality, albeit with some significant variations. These Neoplatonic accounts, often rigorous and innovative, are relevant not only for a more accurate understanding of Neoplatonic metaphysics but also for the history of philosophy and science more generally.

The volume collects 10 articles and is thematically divided into two parts. The first, 'The General Metaphysics of Nature', reflects the generally shared

[1] See the remark about Plotinus in Wildberg 2009, 122.

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view that the complex metaphysics of the Neoplatonic philosophers provided them with the conceptual framework for their elaborations concerning nature. This part is dedicated to the explanation of the relation between central Neoplatonic metaphysical doctrines and aspects of their account of the natural world. The second part, 'Platonic Approaches to Individual Sciences', delves into the application of these doctrines to individual disciplines, in particular, to the theory of elements, geometry, biology, and geography. However, this division is rather thin, for, as will appear more clearly in the summary below, some of the articles could have been justifiably placed in either part.

In the first article, 'Plotinus on *Logos*', Lloyd P. Gerson argues that we should understand Plotinus' claim that a lower principle is the λόγος of a higher principle—for example, that Nature is the λόγος of Soul—by considering in what sense *x* (the higher principle) is virtually *y* (the lower principle). Gerson argues that virtuality should not be understood as potentiality or potency. The meaning of «δύναμις» as virtuality is, indeed, prior to its meaning as passive and active potentiality. Virtuality rather indicates a relation of explication and implication: each lower level is the external activity from the higher level, an actualization of the virtuality of the higher principle [20], and its expression and instrument [24–25].

While this article is certainly interesting and thought provoking in that it tries to solve what may appear as a flaw in Plotinus' theory of δύναμις, there are some shortcomings. Gerson does not discuss the rich array of excellent studies published in recent years that offer alternative accounts of the δύναμις and ἐνέργεια of intelligible beings in Plotinus. His dismissal of the interpretation of the One's δύναμις πάντων as active potency or as active power[2] is unpersuasive, given that he does not engage with recently published in-depth investigations on the One's active power, and neglects passages such as 5.4.1.23–34 in which Plotinus appears to have productive power, and not just virtuality, in mind.[3] While Gerson does address external activity (or second ἐνέργεια) in connection to virtuality, he does not elucidate

[2] Thus, he writes: 'When Plotinus says that the One is δύναμις πάντων, which I render as "virtually all things", he does not mean that the One has an active potency, since the One has not potency whatsoever' [18] and 'Armstrong typically renders δύναμις as "productive power". I fail to see how the word "power" excludes all potency' [18n6].

[3] See, for example, Aubry 2000 and 2007, 215–247; but the list is much longer.

satisfactorily the bond between external activity and internal activity,[4] which Plotinus identifies with active power.[5] Finally, his thesis would have been more persuasive if he had included *Enn.* 2.5 (25) in the dossier of Plotinian texts analyzed in the article, as this is the only treatise expressly and entirely dedicated to the investigation of the meaning of «δύναμις» and «ἐνέργεια».

Andrew Smith's article, 'The Significance of "Physics" in Porphyry: The Problem of Body and Matter', contains a helpful presentation of Porphyry's main concerns in his consideration of physics based both on his classification of Plotinus' writings and on the fragments from his lost commentaries on Aristotle's *Physics* and Plato's *Timaeus*. The article focuses in particular on Porphyry's conception of matter. According to Smith, Porphyry's underlying concern is with the danger of dualism entailed both in the consideration of matter as an independent principle and in a temporal conception of the generation of the sensible world and matter.

Stephen Menn's 'Self-Motion and Reflection: Hermias and Proclus on the Harmony of Plato and Aristotle on the Soul' is an interesting and illuminating treatment of the harmonization of Plato and Aristotle within the Neoplatonic tradition. As an example of the concerns and strategy behind the harmony thesis, Menn analyzes Hermias' treatment of the immortality of the soul and of its self-motion. Menn's claim is that, in order to understand Neoplatonic attempts to harmonize Plato and Aristotle correctly, we should consider what the Neoplatonic philosophers viewed as the real tension between them. This tension, contrary to our contemporary understanding of the differences between Plato and Aristotle, turned around the risk of improperly assimilating divine things to lower things [46]. The intent of Neoplatonists such as Hermias and Proclus is to use Aristotle in order to rehabilitate Plato whenever Platonic passages seem to assimilate unduly the divine to the lower. The particular case analyzed by Menn is the passage on the immortality of the soul in *Phaedrus* 245c.

Alain Lernoud's 'Nature in Proclus: From Irrational Immanent Principle to Goddess', is a thorough and careful investigation of Proclus' understanding of Nature and of its causal role in the sensible world. Lernoud approaches this issue from the viewpoint of the problem of the reconciliation between

4 Except for a brief remark on the One's primary activity as self-loving [28].

5 See, for example, the brilliant discussion of this point in Emilsson 2007.

immanence and transcendence [70]. According to the author, Proclus, like Aristotle, maintains that Nature is a principle of movement inseparable from bodies. However, contrary to Aristotle, he does not conceive nature as the immanent form but rather as a hypostasis placed between the Soul and the corporeal in the hierarchical, vertical, and dynamic unfolding of creative powers. As such, Nature is situated above bodies and maintains its transcendence in spite of being distributed in bodies [99–100].

Christia Mercer's 'Platonism in Early Modern Natural Philosophy: The Case of Leibniz and Conway' aims to open a path for a more careful consideration of the role played by Platonism in the development of early modern natural philosophy. According to Mercer, Leibniz and Conway turned to Platonism in order to solve the problems raised by mechanical natural philosophy: because mechanism has stripped nature of the substantial forms [116–117], it cannot appeal to these forms to account for the source of activity. Moreover, the rise of mechanism raised difficulties in accounting for unity and cosmic plenitude. For Mercer, both Leibniz and Conway are conciliatory eclectics in that they endorsed the new physics but 'demanded that their natural philosophy be consistent with the goodness, plenitude, and power of the divinity' [125]. While Mercer's article is more of an overview than an in-depth investigation, this is not to say that it is uninteresting or unpersuasive. On the contrary, Mercer acutely identifies the main lines for future research. Unfortunately, despite its merits, the article seems out of place in this volume: it is the only one dedicated to the influence of late-ancient Neoplatonic natural philosophy on later periods and, unlike the other articles, seems to be written with an introductory purpose.

The second part of the volume begins with the late Ian Mueller's 'Aristotelian Objections and Post-Aristotelian Responses to Plato's Elemental Theory', which addresses Simplicius' commentary on Aristotle's objections in *De caelo* 306a1–307b18 against the elemental theory that Plato develops in the *Timaeus*. The main focus of the article is Simplicius' assessment of Proclus' response to Aristotle's objection. Mueller's claim is that Simplicius' disagreement with Proclus is motivated by his belief that Aristotle's objections are meant as a correction against possible misunderstandings of Plato's elemental theory: contrary to Proclus' antagonistic conception of their relationship, Simplicius views Plato and Aristotle to be in fundamental agreement [144].

The problem of the harmony between Plato and Aristotle resurfaces in the excellent article by Jan Opsomer, 'In Defence of Geometric Atomism: Explaining Elemental Properties'. The first pages give an overview of the geometric atomism articulated by Plato in the *Timaeus* and of Aristotle's objection to it in *De caelo* 3, while the rest reconstructs and analyzes the Platonists' reply to Aristotle's criticisms, focusing especially on Proclus and Simplicius. According to Opsomer's reconstruction, Proclus' and Simplicius' interpretation of Plato's geometric atomism is characterized by their development of the Timaean theory through the derivation of affective qualities, such as warmth and dryness, from geometric properties. Their responses to Aristotle's objections run contrary to an Aristotelianizing interpretation (which Opsomer suggests might be due to Pericles of Lydia).

In an extremely interesting article, 'Plato's Geography: Damascius' Interpretation of the *Phaedo* Myth', Carlos Steel examines Proclus' and Damascius' interpretation of the description of the Earth in *Phaedo* 108c5–113c8, addressing four problems: its position, spherical shape, stability, and size. Both Proclus and Damascius interpreted this section of the *Phaedo* myth as containing a true account of the nature of the Earth. Moreover, they tried to give empirical evidence to substantiate the story told by Socrates, even at the cost of attacking mathematical geographers whose accounts contradicted it. Some of the arguments articulated by Damascius—for example, the one in defense of the sphericity of the Earth in his *Commentary on Plato's* Phaedo, cp. 1 §515—are based on Aristotle's *De caelo* 2.4 despite being Neoplatonic in inspiration. Indeed, both Proclus and Damascius, while defending Plato against Aristotle's criticisms, nonetheless integrated several elements of Aristotle's cosmology into their interpretation of Plato's account of the nature of the Earth.

Steel's article intervenes in the debate, recently reactivated by David Sedley, on the nature of the *Phaedo* myth [2007]. Sedley contends that, in addition to its moral content, the myth conveys Plato's teleological science and geography. Moreover, Sedley suggests that the 'someone' to whom Socrates attributes this teleological cosmology is actually Plato himself. Steel is more cautious about this matter: he finds the identification of this 'someone' with Plato implausible [177] and concludes that, while he '[admires] the ingenuity of the Neoplatonic interpretation', he does not believe that Plato wanted to express his scientific views on the nature of the Earth in this myth [196]. Although I am rather sympathetic to Steel's view and find his investigation into Damascius' *Commentary* extremely helpful, I am unsure that his analysis of Proclus' and Damascius' interpretation of the *Phaedo* myth in any way supports this conclusion.

The last two articles are dedicated to biology. Wilberding's 'Neoplatonists on "Spontaneous Generation"' explains the problem of *abiogenesis* (that is, the coming-to-be of living beings from non-living matter) posed for Neoplatonic philosophers and their manner of resolving it. The main concern of Platonists such as Themistius, Philoponus, or Proclus was of a metaphysical nature: *abiogenesis*, or 'spontaneous generation', was incompatible with the Neoplatonic metaphysical principle according to which souls cannot be generated from lower principles. Their solution, with some variations, consisted in arguing on theoretical grounds that all apparent cases of spontaneous generation are actually cases of generation from pre-existing life.

Christoph Horn's 'Aspects of Biology in Plotinus' focuses on the biological aspects of Plotinus' notion of life. The concept of life, indeed, plays a role well beyond the biological sphere, for Plotinus attributes life to Intellect and even to the One, and at times identifies it with ἐνέργεια. However, separating the biological from the metaphysical aspects in order to focus only on the former is a difficult task, for, as Horn correctly remarks, one of Plotinus' main concerns in his investigation of the biological life of sensible compounds is the elaboration of an alternative model to Aristotelian hylomorphism. This model does use some Aristotelian elements—for example, in the theory of double ἐνέργεια—but it adapts them to a theoretical framework fundamentally characterized by vertical causality and psycho-physical dualism.

As I hoped to convey, Wilberding and Horn's volume is a welcome and important contribution to a largely under-researched field, which is finally drawing scholarly attention. It covers a wide range of late ancient Neoplatonists and is more unified, both thematically and stylistically, than typical conference proceedings. The volume is also well edited with a bibliography, index locorum, and subject index. It will certainly be a point of reference for those investigating the fascinating philosophy of nature elaborated by Neoplatonic philosophers.

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Diofanto, De polygonis numeris. *Introduzione, testo critico, traduzione italiana e commento* by Fabio Acerbi

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(Translated from the Italian by Kim Williams)

In the last 20 years or so, the interest of historians of mathematics in Diophantus has grown in a significant way, changing deeply our perception of his mathematical work. Fabio Acerbi's book contributes to a better knowledge of this Greek mathematician and his methods.

'We know nothing about the life of Diophantus'. These are the opening words of Acerbi's introduction to his commented edition of Diophantus' *De polygonis numeris*. In effect, it is rare for a mathematician of such fame and calibre to be so unknown from a biographical point of view. As to when he lived, the references only allow us to put his *terminus post quem* in the second century BC (thanks to a citation by the mathematician Hypsicles) and his *terminus ante quem* in the second half of the second century AD (thanks to a citation in Ptolemy's *Almagest*), an enormous span of three centuries! Acerbi does not seem very convinced by the attempts at providing more precise dates found in Paul Tannery's important edition of the *Opera omnia* of Diophantus [1893–1895] and I share his doubts. So it seems appropriate to forego any speculative hypothesis and concentrate instead on Diophantus' work.

There are a great number of questions that make this kind of study fascinating. First and foremost, there is the history of the transmission of the text and the events surrounding the circulation of the various manuscript copies. Then, there is the philological reading of the text itself, the distinction between the original text and various interpolations, the comparison of different manuscript copies, and so forth. There is also the reading that is more properly mathematical, in the context of the reality of the period in which the author was working. Finally, there is the reading that the mathematicians

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of different periods have given the work and its influence, direct or indirect, on the development of mathematics itself.

Acerbi's long introduction (133 pages out of a total of 243) is actually dedicated in large part to Diophantus' principal work, the *Arithmetica*.[1] He devotes a rich chapter to the transmission of the Greek text of the *Arithmetica* and examines the history of the various manuscript copies that exist in the world.[2] There are 31 such manuscripts (not all of which contain the same range of material) and these are traditionally divided into two streams, Planudean and non-Planudean, according to the whether they descend from the annotated transcription (dated to the end of the 13th century) by the Byzantine intellectual and religious scholar, Maximo Planudes. The manuscript that was studied most by the first modern historian, Paul Tannery is the *Diophantus Matritensis* (Madrid, Biblioteca Nacional, Ms. 4678). It too has an interesting history. Written in the 11th century, it was brought to Messina, probably together with other Greek manuscripts after the Turkish conquest of Constantinople, by Costantino Lascaris, who annotated it. After the failed revolt in Messina, it was carried to Madrid by the Duke of Uzeda and has been in the Royal Library of the Spanish capital since 1712.

The *Arithmetica* has come down to us in mutilated form: of the 13 books that initially comprised it, effectively only six have arrived through Byzantine copies, with four more coming through the Arabic translation of Qusṭā ibn Lūqā, datable to the second half of the 11th century, although the copy studied today was discovered only in 1971.[3] According to the most creditable scholars, the Greek manuscripts transmit books 1–3 and most probably 10–13; the Arabic manuscripts, books 4–7. The *De polygonis numeris*, however, has come down to us in almost all the Greek codices containing the *Arithmetica*.

Acerbi's *De polygonis numeris* is the first Italian edition but interest in it goes beyond the mere fact of language. In effect, each new edition in the Diophantine corpus makes a significant contribution to the solution of the

1 Unfortunately, there is no comparison of the text of the Greek and the Arabic manuscripts, which are only mentioned in passing.

2 For a description of the existing manuscripts, see Allard 1980 (which, however, I was unable to consult). The fascinating story of the transmission of the Greek texts can be found, for example, in Allard 1984, 317–331.

3 See Rashed 1974–1975, 97–122. The transcription is found in Sesiano 1982 and in Rashed 1984.

numerous historiographical problems tied to the work of this 'mysterious' mathematician from Alexandria, problems to which the contribution of a scholar of Acerbi's calibre is significant. Besides the problems of a strictly philological nature (obviously also important for any mathematical interpretation of the work), the works of Diophantus have always aroused passionate debates among historians. Not only is there the difficulty of dating the works with certainty but their place in the context of Greek mathematics also turns out to be quite complex. One of the problems that has always fascinated historians of mathematics concerns the 'algebraic' content of the work. This problem can be viewed from three standpoints:

(a) from that of Greek 'geometric algebra',
(b) from that of Diophantus' influence on Arabic algebraists, and
(c) from that of the impact of the reading of the first editions of the *Arithmetica* on developments of European algebra in the 16th and 17th centuries.

All three standpoints are developed by Acerbi in §2 of the introduction.

The first, (a), raises a topic that is hotly debated. As is known, in book 2 (and in books 5–6) of the *Elements*, Euclid develops a geometrical treatment of the solution to quadratic equations. This fact has given rise to relentless discussions: indeed, his treatment has to be interpreted either as a geometrical translation of a pre-existing algebraic treatment (which must go back to the Pythagoreans and, according to some, even to the Babylonians) or as a radically different vision of the problems being treated. The connection between this kind of problem and those proposed and solved by Diophantus is evident.

As an example, Acerbi cites proposition 2.5 of the *Elements*:

> If a straight line is cut into equal and unequal segments, then the rectangle contained by the unequal segments of the whole together with the square on the straight line between the points of section equals the square on the half. [Heath 1956, 1.382]

In Figure 1, p. 263below, the segment in question is AD, which is cut by B into two equal parts and by C into two unequal parts. The proposition states that the rectangle $AC \cdot CD$ plus the square on BC is equal to the square on BD. In algebraic terms, if $AC = x$ and $CD = y$, we have the identity:

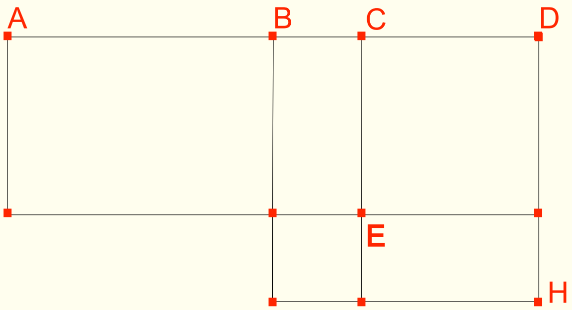


Figure 1. Euclid, *Elem.* 2.5

$$xy + \left(\frac{x-y}{2}\right)^2 = \left(\frac{x+y}{2}\right)^2$$

Diophantus refers to this proposition in the determination of *Arith.* 1.27, in which it is said:

> To find two numbers such that their sum and product are given numbers. The square of half the sum must exceed the product by a square number, ἔϲτι δὲ τοῦτο πλαϲματικόν. [Heath 1910, 140]

Problem 1.27 is obviously related to the solution of quadratic equations. If the numbers sought (x, y) have a given sum (a) and a given product (b), then they both satisfy the equation $t^2 - at + b = 0$ and, recalling that, if x and y are the two solutions to the equation, a is their sum and b is their product, the given condition is equivalent to requiring that the discriminant $(a/2)^2 - b$ be a perfect square, thus allowing the problem to have rational solutions (the only kind sought by Diophantus).

It is, for example, precisely in commenting on this proposition (which in his text is number 30) that Bachet de Mezirac puts it into a strict relation to the rule for solving quadratic equations (which he expresses as a canon). It is from here that the first move is made in the long (and still ongoing) tradition of interpreting the work of Diophantus in 'protoalgebraic' terms.

Without dwelling too long on these aspects, I should like to repeat that Acerbi declares himself starkly opposed to such interpretations. He expresses himself thus:

> *Va sottolineato che l'‘algebra geometrica' non è mai stata intesa come un male minore interpretativo atto a rendere ragione agli occhi di un lettore moderno di*

> *certe caratteristiche dei lemmi lineari. La pretesa era invece che i 'greci', come i babilonesi prima di loro, ragionassero davvero algebricamente ma avessero più o meno inconsapevolmente provveduto a coprire il nucleo matematico 'vero' con un velo geometrico. É straordinario che un'interpretazione così rozzamente anacronistica, frutto di un abbaglio storiografico che è durato quasi un secolo e che perdura tuttora tra gli interpreti di Diofanto, possa aver ottenuto credito. La connessione con l'algebra sta ovviamente nella testa degli interpreti moderni, e i testi non offrono il minimo appiglio che corrobori questa tesi.* [18]
>
> It should be underlined that 'geometric algebra' has never been understood as a lesser interpretive evil aimed at rendering to the eyes of a modern reader understanding of certain characteristics of linear lemmas. The claim instead was that the 'Greeks', like the Babylonians before them, reasoned in a truly algebraic way but had covered—more or less consciously—the 'true' mathematical nucleus with a geometric veil. It is astonishing that such a grossly anachronistic interpretation, the fruit of a historiographical blunder that lasted for almost a century and that still today persists among the interpreters of Diophantus, could have acquired credibility. The connection with algebra obviously lies in the minds of modern interpreters and the texts do not offer the least evidence that corroborates this thesis.[4]

Later, I will come back to this debate, which seems to me to be extremely interesting historiographically.

In the meanwhile, I will continue this rapid survey of Acerbi's text, which goes on to examine indeterminant problems, that is, problems admitting an infinite number of solutions. Among these by far the most famous is *Arith.* 2.8. The fame of this problem is primarily due to the notes in the margin of the copy of the celebrated edition by Bachet de Mezirac owned by Fermat:

> *Cubum autem in duos cubos, aut quadratoquadratum in duos quadratoquadratos, et generaliter nullam in infinitum ultra quadratum potestatem in duas ejusdem nominis fas est dividere: cujus rei demonstrationem mirabilem sane detexi. Hanc marginis exiguitas non caperet.*
>
> On the other hand, it is impossible to separate a cube into two cubes, or a biquadrate into two biquadrates, or generally, to infinity, any power except a square into two powers of the same exponent. I have discovered a truly marvellous proof of this, which however the margin is not large enough to contain. [Heath 1910, 144–145]

[4] All unprovenanced translations are my own.

As is known, the theorem stated by Fermat was proved by Andrew Wiles in 1994. But it was all the indeterminant problems (called 'Diophantine problems' in number theory) that guaranteed the Greek mathematician's fame.

It is interesting to retrace with Acerbi the Diophantine solution to 2.8. The problem—I rely on Heath's English translation and its symbols—is this:

> To divide a given square number into two squares.
>
> Given a square number 16.
> x^2 one of the required squares. Therefore $16 - x^2$ must be equal to a square.
> Take a square of the form $(mx - 4)^2$, m being any integer and 4 the number which is the square root of 16, e.g. take $(2x - 4)^2$ and equate it to $16 - x^2$.
> Therefore $4x^2 - 16x + 16 = 16 - x^2$
> or $5x^2 = 16x$ and $x = \frac{16}{5}$.
> The required squares are therefore $\frac{256}{25}$, $\frac{144}{25}$. [Heath 1910, 145–146]

It is best said right away that Heath's translation is extremely liberal, while Acerbi's follows the text much more faithfully. It can in any case be noted that Diophantus' solution is really indeterminant. We need only change the coefficient m, giving it arbitrary positive integer values, to obtain an infinity of solutions (with the exclusion of the choice $m = 1$ since in that case the solution is null). This is important because, in this solution, as in few others, the purely exemplifying nature of the choice of the initial numbers is (almost) explicit. Hence, while it is true that 'the fact that other linear substitutions $y = tx - 4$ would have yielded a solution to the original problem as well, tends not to be mentioned' [Schappacher 2005, 13], it is also true that in reality Diophantus indicates here with sufficient clarity the possibility of such an arbitrary choice. Here, the translation is fundamental because what counts lies precisely in the details.

In the Greek text, as transcribed by Tannery [see Plate 1, p. 266below], it can be seen immediately, at least I believe so, that Heath's translation of the third paragraph already begins with the presumption that Diophantus is perfectly aware of the general solution, $(mx - 4)^2$, m being any integer. But this is a translation that is only partly faithful to the original. The more literal translations of «πλάσσω…πλευρά» are those by Norbert Schappacher,

Ἐπιτετάχθω δὴ τὸν ι̅ϛ̅ διελεῖν εἰς δύο τετραγώνους.
Καὶ τετάχθω ὁ α^ος^ Δ^Υ^ α̅, ὁ ἄρα ἕτερος ἔσται Μ̊ ι̅ϛ̅ ⋔ Δ^Υ^ α̅· δεήσει ἄρα Μ̊ ι̅ϛ̅ ⋔ Δ^Υ^ α̅ ἴσας εἶναι □^ῳ^.
πλάσσω τὸν □^ον^ ἀπὸ ϛ^ῶν^ ὅσων δήποτε ⋔ τοσούτων Μ ὅσων ἐστὶν ἡ τῶν ι̅ϛ̅ Μ̊ πλευρά· ἔστω ϛ β̅ ⋔ Μ̊ δ̅.
αὐτὸς ἄρα ὁ □^ος^ ἔσται Δ^Υ^ δ̅ Μ̊ ι̅ϛ̅ ⋔ ϛ ι̅ϛ̅· ταῦτα ἴσα Μ̊ ι̅ϛ̅ ⋔ Δ^Υ^ α̅. κοινὴ προσκείσθω ἡ λεῖψις καὶ ἀπὸ ὁμοίων ὅμοια.

Plate 1. Diophantus, *Arith.* 2.8
[Tannery 1893–1895, 90.11–18]

> Let us take the square of some multiple of x minus the number whose square makes 16. [2005, 13]

and Acerbi,

> Formo il quadrato da quanti si voglia numeri meno tante unità quanto è il lato di 16u.

> I form the square of as many numbers as are desired minus as many units as are in the side of 16u.

I am not at all a philologist but I believe that we can understand how the ‘translation’ of the symbols used by the Greek mathematician into modern algebraic symbols (even though necessary and useful for comprehending the text) can lead to serious misunderstandings. Acerbi’s text is always attentive to these problems. In any case, what appears clearly is the perfect awareness on Diophantus’ part of the completely general nature of his solution and of the existence of infinite solutions to the questions posed. It is now interesting to go back with Acerbi to the problem of the presence of elements of algebra (or at least protoalgebra) in the *Arithmetica*.

In my opinion, two questions should be clearly distinguished: that relating to the effective presence of protoalgebra in the Diophantine text and that regarding the influence of that text on the birth and development of algebra.

With regard to the second point, there should be no doubt that, from the very first Arabic translators and commentators to the algebraists of the 16th and 17th centuries, Diophantus has been read in an algebraic key. Indeed, we can say that the interest in his work developed precisely in concomitance with the development of algebraic techniques. What Schappacher calls the 'first renaissance of Diophantus' takes place in parallel with what is recognized as the first Arabic text of algebra, the *Al-jabr* by al-Khwārizmī. In effect, Qusṭā ibn Lūqā's translation of the first seven books of the *Arithmetica* can be dated to 870, just a few months after the publication of the volume by al-Khwārizmī. Thus, the translation is strongly influenced (in terminology and in symbolism) by the new algebra and it is in the same way that the Diophantine text is interpreted by later readers. Acerbi thus rightly underlines that we are dealing with an algebraic reading of Diophantus and not with a direct influence of the Greek mathematician on the formation of an algebraic language:

> *È Diofanto che è tradotto in linguaggio algebrico, non Diofanto che induce la rivoluzione algebrica.* [25]

> It is Diophantus who is translated into an algebraic language, not Diophantus who leads the algebraic revolution.

The 'second algebraic renaissance', that carried out by the algebraists of the 16th and 17th centuries, has similar characteristics. As is known, after a first attempt at editing Diophantus' *Arithmetica* (by Giuseppe Auria at the beginning of the 16th century), the first real impact of Diophantus' work on the nascent algebraic culture of the West came from Bombelli's *Algebra* of 1572 [repr. 1966]. Here it is interesting to read what the algebraist from Bologna says in his own words:

> *Essendosi ritrovato un'opera greca di questa disciplina [l'Algebra] nella libraria di Nostro Signore in Vaticano, composta da un certo Diofante Alessandrino, Autor Greco, il quale fu a' tempo di Antonin Pio, & havendomela fatta veder Messer Antonio Maria Pazzi Reggiano, pubblico lettore delle Matematiche in Roma, e giudicatolo con lui Autore assai intelligente de numeri (ancorchè non tratti de numeri irrationali, ma solo in lui si vede un perfetto ordine di operare) egli, & io, per arricchire il mondo di così fatta opera ci dessimo a tradurlo, e cinque libri (delli sette che sono) tradutti ne habbiamo; lo restante non havendo potuto finire per gli travagli avenuti all'uno, e all'altro, e in detta opera habbiamo ritrovato, ch'egli assai volte cita gli Autori indiani, col che mi ha fatto conoscere, che questa disciplina appo gl'indiani prima fu, che à gli Arabi.* [Bombelli 1966, 8–9]

> There being found a Greek work of this discipline [*scil.* algebra] in the library of Our Lord in the Vatican, composed by a certain Diophantus of Alexandria, a

> Greek author who lived in the time of Antoninus Pius, and having been shown it by Mr Antonio Maria Pazzi from Reggio Emilia, a public lecturer in mathematics in Rome, and along with him deeming the author to be quite knowledgeable about numbers (though he does not treat irrational numbers but still in him is seen a perfect order of working), he and I, to enrich the world with such a well-made work, set about translating it. Five books (of the seven that there are) we have translated; we have not been able to finish the remaining books owing to the troubles that have befallen both of us. In said work we found that he many times cites the Indian authors, by which he has made me know that this discipline was first known by the Indians before the Arabs.

I transcribe the entire quotation by Bombelli because, in spite of his disconcerting statement that he had seen that Diophantus 'many times cites the Indian authors', it appears to me to confirm Acerbi's statement: just as for Arabic mathematics, so too in the case of the West, the introduction of algebra preceded, and did not follow, the comprehension of Diophantine mathematics. However, it should also be said that all of the early algebraists found it entirely natural to 'read' Diophantus in light of the 'algebraic revolution'. That is particularly true for the reading of the *editio princeps* by Bachet de Mezirac (1621).

It can be stated without any doubt that the insistence of Bombelli and the early algebraists on the 'algebraic' reading of the Diophantine text was profoundly motivated by 'ideology'. Their aim in fact was to give a 'classical' pedigree to algebra, freeing it from the purely practical status that it had assumed since the work of the abacus masters. Diophantus was, like Euclid, Apollonius, and Archimedes, to be counted among the noble fathers of the new mathematics: the search to rediscover the thread connecting the new problems and the classical tradition of geometry would continue throughout the whole of the 17th century, identifying above all in the methods of 'geometric analysis' the direct antecedent of the modern integration of algebra and geometry. In this way would Viète, Descartes, Schooten, Newton, and great number of others express themselves. In more recent times, in a historiographical context no longer tied to the problems of active research, Frederick Zeuthen proposed reading both the *Conics* of Apollonius and book 2 of the *Elements* in terms of the so-called 'geometric algebra'. The heated debate about such questions has characterized a series of interventions by various historians. But we will not enter into that here.[5]

The reading of Diophantus' work and, above all, the reflections on the kind of problems proposed in his book is relevant also for modern number theory, which can be said to have originated in Fermat's study of Bachet's edition of Diophantus. Here, for example, is how André Weil introduces his admirable history of number theory:

> One might similarly try to record the date of birth of the modern theory of numbers; like the god Bacchus, however, it seems to have been twice-born. Its first birth must have have occurred at some point between 1621 and 1636, probably closer to the latter date. In 1621, the Greek text of Diophantus was published by Bachet, along with a useful Latin translation and an extensive commentary. It is not known when Fermat acquired a copy of this book…nor when he began to read it; but, by 1636,…he had not only read it carefully, but was already developing ideas of his own. [Weil 2007, 1–2]

Going back to the thread of Acerbi's statement, while we can reasonably affirm that 'it is Diophantus who is translated into an algebraic language, not Diophantus who leads the algebraic revolution', the relationship to number theory is much more complex, even if Weil's observation does not at all imply (nor did Weil intend such an implication) that Diophantus was a precursor of Fermat, who read him with the eyes of a modern mathematician.

From a historiographical point of view, aside from the unquestionable importance of the influence of the reading of the *Arithmetica* on developments in algebra and number theory, two important questions remain:

(1) Into which tradition is the work of Diophantus to be inserted?
(2) What, independent of later readings, is the mathematical language of the *Arithmetica*?

With regard to the first question, it is well known that a considerable number of historians of mathematics have emphasized a presumed connection with Babylonian mathematics. Although it seems to me that such a connection—defended by historians of mathematics and mathematicians of the calibre of Neugebauer [1934, 245–259] and Van der Waerden[1954]—is based on clues that are too fragile, I believe that Acerbi's dismissal of the question is excessively perfunctory. (With regard to Neugebauer, he says that 'he

[5] On this question, see Saito 2004, 383–480.

saw connections that were non-existent but strategic for him in maintaining the pretext of paleo-Babylonian algebra' [14]). Instead, I find more cogent Acerbi's careful and in-depth examination of the studies of arithmetic on the part of Greek mathematicians, in particular Archytas, Nicomachus, and Iamblichus, regarding the insertion of different kinds of proportional means (arithmetic, geometric, harmonic, and so on) among given numbers. The examination of this rich mathematical tradition concludes, obviously, in an interesting and detailed account of what can be said, without fear of contradiction, to be 'the most substantial treatment of number theory in the Greek corpus previous to the *Arithmetica*', that is, of books 7–9 of Euclid's *Elements*.

Aside from the admirable (and very well known) theorems contained in these books (the algorithm of the greatest common divisor, the infinity of prime numbers, and so forth), Acerbi rightly turns his attention to some of the basic definitions, whose complete interpretation also requires attention to linguistic aspects: e.g., to that of a number ('a multitude composed of units') and, above all, that of a part ('the less of the greater when it measures the greater' [Heath 1956, 2. 277]), that is, a divisor, to those of plane and solid numbers (respectively, 'two numbers having multiplied one another' and 'three numbers having multiplied one another' [Heath 1956, 2. 278]). It should be noted, as Acerbi does, that plane and solid numbers are not mutually exclusive: e.g., 30 is both a plane number (10×3) and a solid number ($2 \times 3 \times 5$).

Again in the context of this classification of number, book 9 of the *Elements* concludes with some theorems regarding perfect numbers, that is, numbers whose sum of their parts (i.e, their divisors excluding the number itself) is equal to the number itself, e.g., 6 ($= 1 + 2 + 3$), 28 ($= 1 + 2 + 4 + 7 + 14$), and so forth. The last proposition, *Elem.* 9.36, which Acerbi [36] rightly defines as 'the true τέλος [aim] of the arithmetic books', is, in Heath's translation:

> If as many numbers as we please beginning from an unit be set out continuously in double proportion, until the sum of all becomes prime, and if the sum multiplied into the last make some number, the product will be perfect. [Heath 1956, 3.421]

In other words, if a prime number is the sum of powers of 2, that number multiplied by the last power in the sequence from 1, will give a perfect number. In the case of 28, for example, 7 is a prime number and the sum of $1 + 2 + 4$, powers of 2; thus, 28 ($= 4 \times 7$) will be a perfect number.

This proposition, like all in the arithmetic books, is important for the study of polygonal numbers. In fact, written in another way, since the sum of n powers of 2 (beginning with 0) is $2^n - 1$, if $2^n - 1 = M$ is prime, proposition 9.36 can be rewritten:

$$2^{n-1} \times (2^n - 1) = \frac{M \times (M+1)}{2} \text{ is a perfect number.}$$

Hence, the perfect numbers identified by Euclid[6] are polygonal numbers (to be precise, triangular numbers). There is also another profound connection between book 9 of the *Elements* and Diophantus' *De polygonis numeris*: Euclid's book makes systematic use of the properties of the sums of elements in geometric progression (such as those of the powers of 2), while the Diophantine text studies polygonal numbers that are the sum of elements in arithmetic progression.

Thus, we have arrived at the central theme of Acerbi's book, the critical edition and study of Diophantus' *De polygonis numeris*. As already said, that study, although it constitutes the focus of the author's attention, occupies only a small part of the volume.

Let us, however, proceed in order: the text of *De polygonis numeris* is inserted, at least in part, in all of the Greek manuscripts that contain the books of the *Arithmetica*. But, in spite of that, numerous scholars, starting with Tannery, have cast doubt on its paternity. Personally, I do not believe that there is good reason to doubt the traditional attribution, though I note with Rashed and Houzel [2013, 4], that 'La différence, non seulement d'arithmétique mais de style, ne peut pas que surprendre' ('the difference, not only of arithmetic but of style, cannot help but surprise').

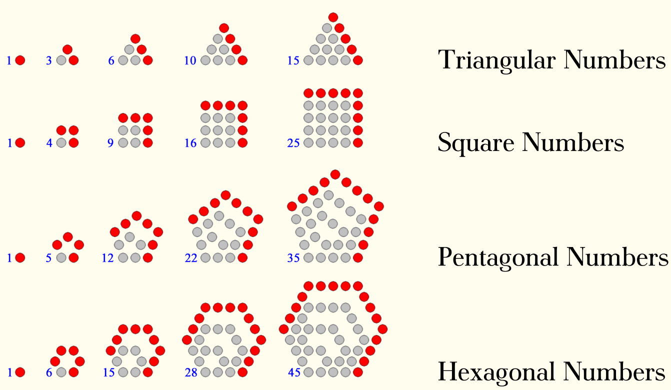
[6] The history of *Elem.* 9.36 is very interesting. The primes of type M are called Mersenne primes and their study offers many open questions. While Euler proved that the even perfect numbers are necessarily of the type identified by Euclid, it is not actually known if there are any odd perfect numbers; it is only known that if they exist, they must be extremely large. It is worth noting that Nicomachus had already stated (but proved incorrectly) that there do not exist any odd perfect numbers.

Returning to the contents of the text, it must be said that the polygonal numbers are given by the sum of the first consecutive elements of an arithmetic progression always beginning with 1. They are divided into:

- triangular numbers, when the common difference is 1, that is, the progression of integers that gives rise to the numbers 1, 3 (= 1 + 2), 6 (= 1 + 2 + 3), 10 (= 1 + 2 + 3 + 4), 15 (= 1 + 2 + 3 + 4 + 5),...
- square numbers, when the common difference is 2: thus, 1 (= 1^2), 4 (= 1 + 3 = 2^2), 9 (= 1 + 3 + 5 = 3^2), 16 (= 1 + 3 + 5 + 7 = 4^2), 25 (= 1 + 3 + 5 + 7 + 9 = 5^2),...
- pentagonal numbers, when the common difference is 3: thus, 1, 5 (= 1 + 4), 12 (= 1 + 4 + 7),... and
- hexagonal numbers, when the common difference is 4: thus, 1, 6 (= 1 + 5), 15 (= 1 + 5 + 9),...

and so forth.

The study of the properties of polygonal numbers goes back to the tradition of the Pythagorean school but there is no doubt that the first proofs that have come down to us are precisely those of Diophantus. The term ‘polygonal’ attributed to these numbers comes from the fact that the arithmetic progressions can be obtained by arranging the numbers according to geometric shapes and bordering them with gnomons. For greater clarity, I provide the figures of the first series:[7]



[7] The images are taken from the website of wikipedia.

In his definition, Diophantus says (as translated by Acerbi):

> *Ciascuno dei numeri aumentati di un'unità a partire dalla triade è poligonale primo a partire dall'unità ed ha tanti angoli quanta è la molteplicità delle unità in esso e suo lato è il numero di seguito all'unità, il 2. E sarà il 3 triangolare, il 4 quadrato, il 5 pentagonale e questo di seguito.*[8] [41]
>
> Each of the numbers augmented by a unit beginning from the triad is polygonal prime beginning from unity and has as many angles as is the multiplicity of the unit in it and its side is the number following unity, 2. And 3 will be triangular, 4 square, 5 pentagonal, and so on.

Thus, every number (except for 2) is polygonal in different ways: it is polygonal prime when it is the first of the progression after unity. In any given polygonal number, the number of angles is equal to the polygonal prime that generates it and a side equal to 2 (the side of every prime polygonal) augmented by a unit for each step of the progression beginning with the prime polygonal. Thus 15 is a triangular number (three angles and side 5) but also hexagonal (six angles and side 3) and 15-angles (15 angles and side 2).

Rather than go into the details of the individual propositions, which are in any case described with great accuracy by Acerbi, I will come to that which—even according to Diophantus himself—constitutes the goal of the entire booklet. This is to explore how to recognize a number as polygonal and determine its side. Naturally, since every number is a polygon, one must specify which polygon is being dealt with, that is, to identify the number of it angles or its 'species'. Thus, the central problem addressed in Diophantus' work is: given an integer, can we establish if it is polygonal of a certain angle?

To this end, the first point highlighted by Acerbi is that of putting into a mathematically significant form the intuitive concept of 'figurate' or polygonal number, as illustrated on page 272. This objective is achieved, as said earlier, by defining the $k - mo$ polygonal number of P angles as the sum of $k + 1$ terms of the arithmetic progression that begins with 1 and from the difference between elements $r = P - 2$. In this way, the definition is put into a form that is mathematically clear. Diophantus' answer is that a

[8] Heath's translation is actually too synthetic and neglects various aspects of the Diophantine definition: 'All numbers from 3 upwards in order are polygonal, containing as many angles as they have units, e.g., 3, 4, 5 etc.' [Heath 1910, 247].

polygonal number N must be such that $8N(P-2)+(P-4)^2$ is a square. This is condition (a).

Bachet de Mezirac had already noted that, while it is true that condition (a) is necessary, it is not sufficient, giving as examples the fact that $8 \times 2(5-2) + (5-4)^2 = 49$ is a square but that 2 is not a pentagonal number, and that $8 \times 4(7-2)+(7-4)^2 = 169 = 13^2$ is a square but 4 is not a heptagonal number. Acerbi rightly observes that these counterexamples cannot be considered valid because the solutions where $N < P$ are 'obviously' to be discarded. (I remind the reader that by definition P is the smallest element different from 1 in the progression,) In any case, the fact remains that the sufficiency of condition (a), even with the obvious added condition $N > P$, was not proved by Diophantus.

The other objection, this too present in Bachet, concerns the fact that the Diophantine definition mentioned above is not applied to either the triangular numbers (for which $P-4=-1$) or to the square numbers (for which $P-4=0$). Acerbi justly criticizes the answer given by Hultsch [1876] to this problem, namely, that Diophantus was capable of manipulating negative numbers and zero, an idea that is unanimously rejected by historians today. Instead, Acerbi's convincing argument is simply that the formula given by Diophantus is a generalization of the known cases of triangular and square numbers, which thus do not enter into the typology presented by the proposition. As is easily verified, the triangular numbers T satisfy the relation $8T+1 = 8T(P-2)+1$ = square, while the square numbers Q satisfy the relation $8Q(P-2)$ = square. Both cases satisfy what Diophantus said in the introduction to *De polygonis numeris*:

> Any polygonal multiplied into a certain number depending on the number of its angles, with the addition of to the product of a certain square also depending on the number of the angles, turned out to be a square. [Heath 1910, 247]

This characterizing property is proposed as the generalization of square numbers. As can be seen, the Diophantine proposition responds perfectly to what is proposed.[9]

[9] It is precisely in this context, that is, in comparing his proposition with what was proved by Hypsicles, a mathematician who lived in the second century BC, that Diophantus cites Hypsicles, a tenuous datum in dating Diophantus, if one assumes that he wrote the *De polygonis numeris*.

The last propositions of the *De polygonis numeris* teach how to determine the side of a given a number of a given species, and how to determine a number given its side (and species).

Proposition 5 is mutilated. It says: 'Given a number, find in how many ways it can be polygonal'. The proposition, proved by Bachet in a way that is unconnected with what has come down to us from the text of Diophantus, is hypothetically reconstructed by Wertheim [1897, 121–126] and Heath [1910, 256]. Acerbi offers an interesting appendix that reprises what he himself has already been published [2011, 548–560] and offers a new, more convincing reconstruction, which I will not repeat here.

As said earlier, the *De polygonis numeris* is a work that does not receive much attention from historians and number theorists. Acerbi's reconstruction and commentary effectively brings this work to the notice of scholars and situates it in a more harmonious way within the Diophantine corpus.

On the other hand, as is known, this treatise also played an important role in Fermat's thinking. Without going into these aspects, which are in any case rather well known, I should like to mention another marginal note in Bachet's edition, yet again incomplete because of the 'lack of space'. I am talking about observation 46, in reference to the proposition already cited that makes it possible to determine the polygon from a given side and *vice versa*. Fermat writes, extending the concept of figurate number to arbitrary dimensions:

> I will set out here, without demonstration, a very beautiful and very remarkable proposition that I have discovered: In the natural progression starting at unity, the product of an arbitrary number times its immediate successor makes double the triangle of the first number. If the multiplier is the triangle of the number immediately following, we have three times the pyramid of the first number. If it is the pyramid of the number immediately following, we have the quadruple of the 'triangulotriangulaire' of the first number, and so on indefinitely, following a uniform and general rule. I deem that a more beautiful or general theorem regarding numbers could not be stated. I have neither the time nor the space to put the demonstration in this margin.[10] [trans. from http://science.larouchepac.com/fermat]

Another famous observation by Fermat regarding polygonal numbers is no. 18, a remark on Bachet's comment on Diophantus, *Arith.* 4.31 containing

[10] Pengelley 2013 describes an interesting use of the square in a large didactic project.

an unproved proposition stating that every number, if it is not a square, is the sum of at most four squares. Fermat observes in the margin:

> What's more, there is a very beautiful and altogether general question which I have been the first to discover. Every number is: either triangular, or the sum of 2 or 3 triangles; Either square, or the sum of 2, 3, or 4 squares; Either pentagonal, or the sum of 2, 3, 4, or 5 pentagons; and so on indefinitely, whether it be of hexagons, heptagons, or any polygons; this marvelous proposition can be enunciated generally by means of the number of angles. I cannot here give the demonstration, which depends on numerous and abstruse mysteries of the Science of Numbers. I have the intention of dedicating an entire Book to this subject and thus, in this part of Arithmetic, I intend to make shocking progress beyond the formerly known limits.[11] [trans. from http://science.larouchepac.com/fermat]

On the other hand, Bachet had dedicated to precisely this work two long appendices, which Fermat read and assiduously commented on. That he was an attentive reader of not only the results but also of the methods used by Diophantus in the *De polygonis numeris* is also confirmed by Michael Mahoney, Fermat's biographer, regarding the importance of Bachet's reflection on the use of the sums of progressions:

> In the realm of summation formulas for the powers of integers Fermat's Archimedean model…could offer little inspiration.… Instead Fermat found his inspiration in Bachet's appendix to Diophantus' treatise *On Polygonal Numbers*. [Mahoney 1994, 229]

It is precisely on the methodological aspects of this work that Acerbi dwells.

Regarding the methods of solution used by Diophantus, by means of a linguistic and mathematical-philological argument, Acerbi distinguishes Diophantus' methods of analysis from the classical ones set forth by Pappus. The differences are significant: Diophantus omits a genuine synthesis in his argumentation and the approach of the *Arithmetica* appears to work more by reduction than to be an actual analysis. (The method of reduction consists in transforming an expression into an equivalent expression until it obviously assumes the form of what is hypothesized in the statement of the proposition, e.g., the form of a square.) Another method that Acerbi identifies, especially in the *De polygonis numeris*, is what he calls a 'chain of givens'. This is an argument of the type 'if A is a given (by hypothesis), then B will

11 Fermat's proof is not known; the theorem was proved by Cauchy in 1813.

also be given and, thus, also C and, thus,...' until what is sought is obtained as a given. Here again, this concerns variants of the analytical method and Acerbi does well to examine them all with such care.

His examination focuses on problems akin to those indicated at the beginning about geometrical algebra:

(1) Is it possible to identify a method used by Diophantus in the solution of problems?
(2) Or is it a case of inventions that are efficacious in each individual situation but unconnected to each other?

A very clear and peremptory answer, shared by many scholars, is that given by Hankel:

> Of more general comprehensive methods there is in our author no trace discoverable: every question requires a quite special method, which often will not serve even for the most closely allied problems. It is on that account difficult for a modern mathematician even after studying 100 Diophantine solutions to solve the 101st problem; and if we have made the attempt, and after some vain endeavours read Diophantus' own solution, we shall be astonished to see how suddenly he leaves the broad high-road, dashes into a side-path and with a quick turn reaches the goal, often enough a goal with reaching which we should not be content; we expect to have to climb a toilsome path, but to be rewarded at the end by an extensive view; instead of which our guide leads by narrow, strange, but smooth ways to a small eminence; he has finished! He lacks the calm and concentrated energy for a deep plunge into a single important problem; and in this way the reader also hurries with inward unrest from problem to problem as in a game of riddles, without being able to enjoy the individual one. Diophantus dazzles more than he delights. [Hankel 1874, 159; trans. in Heath 1910, 54–55]

The search for these general methods led to the beginning of the 'algebraic' reading of Diophantus' text, a reading of which I spoke earlier and which often turns out to be historically insufficient.

Recently, following the rediscovery of Diophantine books in Arabic, the discussion has been taken up again in terms that I find interesting but which are not mentioned in Acerbi's book. I will permit myself to mention them here.

One interpretation worth noting is related to the reading of Diophantus' text not so much through the filigree of algebra as much as through that of modern number theory and, thus, of algebraic geometry. Perhaps the first to advance this line of interpretation was Isabella Bašmakova [1974],[12] who published a study on Diophantus and Diophantine analysis. According to this point of view, number theory in Diophantus is traced to aspects relating to the study of algebraic curves and to the search for their rational points. For example, the search for Pythagorean triads comes down to the search for the rational points of the circumference $x^2 + y^2 = z^2$.

With regard to this attempt at interpretation, Schappacher's stand is drastic:

> Certain historians of mathematics try to surpass the mathematicians in blending modern inspiration with Diophantus' alleged thoughts. The worst example of this thoughtless tendency is given by the Russian historian of mathematics Bašmakova in her book on Diophantus. [Schappacher 2005, 27–28]

This judgment is much too harsh. Much more interesting, I believe, is the nuanced judgment of Houzel and Rashed:

> *Quoique «forcée» et ne pouvant pas prétendre au titre d'historique, cette lecture d'I. G. Bašmakova a le mérite d'expliquer les procédures réglées en usage dans les* Arithmétiques*, procédures qui laissent supposer un ordre précis qu'aucune autre lecture n'était en mesure d'expliciter.* [Rashed and Houzel 2013, 43]
>
> Although 'forced' and unable to claim to be history, this reading of I. G. Bašmakova has the merit of explaining the procedures set in use in the *Arithmetica*, procedures that suggest a specific order that no other reading has been able to explain.

In any case, Bašmakova's reading paved new roads for interpretation.

Thus, Weil, who guarded against attributing to Diophantus a role as precursor of modern algebraic geometry but nevertheless read the Greek text with the eyes and language of a 20th-century mathematician, deepened Bašmakova's insight. Here I will limit myself to citing this significant passage:

> *On ne peut pas manquer d'être frappé déjà chez Diophante, de la fréquence avec laquelle reviennent les équations qui définissent de courbes de genre 0 ou 1, et du fait que ce sont toujours les mêmes méthodes que Diophante mette en ouvre pour les résoudre.* [Weil 1981, 398]

[12] This is actually a translation of the original Russian text of two years earlier. See Bašmakova 1997 for an English translation.

> One cannot fail to be struck already in Diophantus, by the frequency with which equations that define curves of genus 0 or 1 turn up, and the fact that it is always the same methods that Diophantus puts to work for the solution.

This point of view concerning Diophantus' work has been amply developed by Houzel and Rashed in many studies of Arabic mathematics. The two scholars write:

> *Si donc nous refusons de lire dans les* Arithmétiques *les notions de la géométrie algébrique et celle de l'Analyse diophantienne contemporaine, nous proposons en revanche de conserver ces moyens théoriques, mai au seul titre d'instrument, comme outil théorique qui permet d'identifier les méthodes et aussi de mieux connaitre les liens entre les 280 problèmes traités par Diophante et d'éclaircir la structure de son livre*. [Rashed and Houzel 2013, 43–44]
>
> So while we refuse to read into the *Arithmetica* concepts of algebraic geometry and contemporary Diophantine analysis, we propose instead to retain these theoretical means but only as an instrument, as a theoretical tool that permits us to identify methods as well as to understand better the connections among the 280 problems addressed by Diophantus and to clarify the structure of his book.

This appears to me to be a useful comparative methodology.

A different attempt in the effort to find a methodological thread in the work of Diophantus, comes from another well-known scholar of the subject, Jean Christianidis. This is a very different line of thinking: not a key for reading but for the exposition of Diophantus' actual intentions in writing the text. Here is how Christianidis presents the method used by the Greek mathematician:

> We are now in a position to present Diophantus' general method of arithmetical problem solving....
>
> The canon of Diophantus for solving arithmetical problems:
>
> (1) Invention–transfer of the problem (in its instantiated version) to the framework of the "arithmetical theory", i.e., transformation of the problem into an equation;
> (2) Disposition—transformation of the equation into its final form, and finding the unknown number;
> (3) Computation of the numbers sought; and
> (4) Test proof. [Christianidis 2007, 300]

He adds:

> Diophantus' intention in the *Arithmetica* is not to present a theory for solving algebraic equations. His goal...was to elaborate a canon on the basis of which several arithmetical problems could be treated and to demonstrate how this canon should be used in practice. [Christianidis 2007, 303]

As we see, there are a great number of different interpretations of the presence of a unifying method in the work of Diophantus, which are sometimes in clear contrast with each other and sometimes mutually complementary. Acerbi's account is particularly tied to the philological aspects and merits attentive study.

I have attempted to give an idea of a field of study that has interested mathematicians and historians of mathematics for at least 1000 years, and that appears not to have exhausted its potential. Before closing, I should mention an ulterior field of interest for these problems, that of didactics. I refer here to a recent publication by Anne Michel-Pajus who examines Acerbi's work on polygonal numbers [2011] in detail, focusing especially on the propositions pertaining to the determination of the side of a polygonal number of a given species and *vice versa*, as an example of argumentation in which we go from the formula to the algorithm and conversely:

> We chose this text, because Diophantus gives three presentations for the same mathematical property: one with a 'rhetorical formula', and one with an algorithm, then the inverse algorithm.... Teachers are used to going from the algorithm to the formula, as the formula is more familiar to them (and to current students). We see here how Diophantus extracts an algorithm from a formula. This is a commonplace task in elementary mathematics. [Michel-Pajus 2012, 376]

It seems to me that these diverse uses also show how a careful analysis of the text can clarify methodological questions of no small importance.

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Science before Socrates: Parmenides, Anaxagoras and the New Astronomy
by Daniel W. Graham

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The purpose of Daniel Graham's intriguing study is to challenge what he takes to be a longstanding orthodoxy. According to the presumed orthodoxy, Greek science, specifically astronomy, did not begin until nearly the mid-fourth century BC, when theorists like Eudoxus began to test their theories against available empirical observations. In particular, so goes the orthodoxy, the sixth- and fifth-century Presocratic philosophers were not scientists: they saw no need for a method by which to test their theories. Graham argues to the contrary that scientific astronomy, as contrasted with speculative accounts of the cosmos, begins well before the end of the Presocratic period. And the unlikely progenitor of Greek scientific practice turns out to be none other than Parmenides! Graham develops his thesis by way of conducting a meticulous survey of the evidence going back to Thales and the other early Ionians, and presenting an imaginative and fascinating reconstruction of the theoretical implications of what can be reliably established as evidence.

Central to Graham's account, developed in chapters 3 and 4 of the book, is the concept of 'heliophotism',[1] the idea that the Moon's light is not original but is reflected sunlight. This discovery, he claims, originates with Parmenides.[2]

1 The moniker is said to derive from Alexander Mourelatos. It is not always clear whether the term denotes the *fact* that the Moon's light is reflected sunlight or the *belief* in that fact.

2 Diels and Kranz 1951, 28B14: 'A light by night, wandering around the Earth with borrowed light.' Heliophotism is also attributed in our sources to Thales, but Graham dismisses the attribution [51]. Knowledge of heliophotism, he argues, is necessary for understanding the nature of solar eclipses, which in turn is necessary for predicting one. But, argues Graham, Thales would not have had the resources to do what tradition attributes to him: the prediction of a solar eclipse [51–53].

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Parmenides, by this account, treats heliophotism as a hypothesis, confirmed by observation of the regular and periodic succession of the phases of the Moon. From heliophotism numerous implications can in turn be derived, implications that cannot have escaped an astute reasoner like Parmenides. For if the hypothesis is confirmed in this way, it will follow that

(1) the Moon is opaque,
(2) the Moon orbits below the Sun,
(3) the Moon is spherical,
(4) the Sun and Moon are permanent bodies,
(5) the heavenly bodies are massy,
(6) the paths of some heavenly bodies go under the Earth, and
(7) together with the assumption of ἀντίφραξις (*antiphraxis*)[3] —the interposition of a third body in alignment between two others—eclipses can be explained in terms of astronomical alignments.[4]

Thus, the discovery of heliophotism represents the turning point in the story of the advent of science. Indeed, as Graham looks back to the cosmological theories of Parmenides' predecessors [ch. 1–2], he does not find anything resembling science in those theories. There, the observable behavior of celestial bodies is characteristically explained in terms of the occasional activity of winds and exhalations, and so on, emanating from the Earth and not in terms of universal and constant cosmic principles. In those theories, astronomical phenomena are subsumed under meteorological phenomena; and Graham dubs this way of regarding them the 'Meteorological Model' [78–84]. Importantly, such explanations are not open to empirical confirmation.

The significance of Parmenides' discovery, so continues the account [ch. 5], was not lost on his immediate successors, Anaxagoras and Empedocles.[5] Both of them explicitly affirm heliophotism [Diels and Kranz 1951, 59B18, 31B47] and all seven of the implications stated earlier are, as it happens,

3 There is no direct evidence that Parmenides understood the nature of eclipses and, hence, no evidence that he would have been in a position to explain solar or lunar eclipses by way of ἀντίφραξις. Anaxagoras will be the first theorist to propose that explanation [see below].

4 All these 'entailments' [119] of heliophotism are brilliantly worked out on pages 111–121.

5 An argument for the chronological priority of Anaxagoras' writings over those of Empedocles is woven into Graham's account in chapter 5.

included in a testimony on Anaxagoras [Diels and Kranz 1951, 59A42, A123], though not there presented in any systematic order that would show their derivation from heliophotism.[6]

An interesting sidelight on Anaxagoras' scientific methodology is cast by his belief that the Earth is flat. He has empirical 'confirmation' of this belief:

> The Earth's horizon at the rising or setting of the Sun or Moon is flat, not convexly curved, as it would have to be if the Earth were spherical. On the other hand, the shadow cast on the Moon in a lunar eclipse *is* spherical. If the shadow seen on the Moon were cast by the Earth, it should be a flat line.

To account for the sphericity of the shadow, Anaxagoras posits the existence of asteroids, celestial bodies otherwise unseen. It is *their* shadow that is seen on the Moon. This line of reasoning shows that Anaxagoras has a grasp of ἀντίφραξις, the idea that the darkening of the Sun or Moon is caused by the interposition of a body that blocks the light from the sun.

Anaxagoras' account of heavenly bodies takes them to be stony bodies hurled away by the centrifugal force of the vortex that hurls them out from the Earth's surface and maintains them in place in their orbits.[7] This picture relies on a single universal principle of motion and not on the multifarious *ad hoc* explanations of celestial motions invoked in the Meteorological Model. Graham marks this turning point as the advent of the 'Lithic Model' [134–136].

The assumption of ἀντίφραξις, as we learn in chapter 5, helps explain the otherwise mysterious statement attributed to Anaxagoras that 'the Sun is larger than the Peloponnese' [Diels and Kranz 1951, 59A42, 59A1] and '[the Moon is] as large as the Peloponnese' [Plutarch, *De facie* 932a]. Why should the Peloponnese serve as the standard of comparative measurement here? Graham's conjecture is highly plausible.[8] We know from computer-assisted research that on 17 Feb 478 BC a solar eclipse occurred in which the entire

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[6] Graham [125] attributes this absence of systematic presentation to a tendency, common among doxographers, to report doctrinal data out of their original contexts; he is nevertheless confident that Anaxagoras would have understood their logical dependence on heliophotism.

[7] A meteorite is a rocky body that returns to Earth [ch. 5, see below].

[8] Graham acknowledges Panchenko's earlier proposal to similar effect [149n24].

Peloponnese was obscured [see diagram: 150–151]. It is likely, as Graham maintains, that the 22 year-old Anaxagoras, then living in Athens—which also lay in the path of the darkening—would have heard reports from travelers from the Peloponnese to the effect that the entire peninsula was engulfed in the darkening caused by the eclipse. Identifying (on the assumption of ἀντίφραξις) such darkening with the shadow cast by the Moon as it blocks the Sun's light, Anaxagoras could then have concluded that the Moon's size is roughly equal to the size of its shadow, which included roughly the Peloponnese, and that the Sun's size exceeded that of the Moon—because the eclipse was annular, not total—and hence also of its shadow.

Another instance of Anaxagoras' deployment of a scientific methodology is associated with what ancient sources describe as his 'prediction' of the fall of a meteorite at Aegospotami in 467/6. Following Plutarch [Diels and Kranz 1951, 59A12], Graham takes this as a description not of an actual prediction but of the theory that heavenly bodies are stones or stone-like. The fall of the (stone-like) meteorite at Aegospotami would then have confirmed his theory over against earlier views that heavenly bodies are light and held aloft by winds and the like. In sum, the grasp of ἀντίφραξις and its role in solar eclipses, proof that the Moon is a massy, possibly stone-like body, suggests that all heavenly bodies are similarly stone-like. That theory received empirical confirmation at Aegospotami in 467/6.[9]

Parmenides and Anaxagoras thus turn out to be the protagonists of this story. In chapter 6, Graham sketches what he takes to be the reception of their 'scientific turn' by subsequent cosmological theorists. Here he briefly surveys the cosmological theories of Empedocles [see 284n5, above], Diogenes of Apollonia, Philolaus, and Democritus. Empedocles follows Anaxagoras in accepting heliophotism and in explaining solar eclipses in terms of ἀντίφραξις (though differing with him on other matters). Diogenes holds that there are asteroids that can fall out of orbits, and that heavenly bodies are (pumice) stones, thus accepting the 'Lithic Model' (though he apparently rejects helio-

[9] Graham also discusses Anaxagoras' 'theories' about comets [165–170] and the Nile floods [170–174] but these theories are not ones for which he would have had empirical confirmation and so are not included for discussion here.

photism).[10] Philolaus subscribes to both heliophotism and ἀντίφραξις, though his account of the structure of the cosmos is strikingly different from that of Anaxagoras. Finally, Democritus accepts the Lithic Model as a basis for cosmology and also appears to accept heliophotism. His views on eclipses are difficult to determine but there is some reason to think that he accepted ἀντίφραξις as well.

Chapter 6 continues with a comparison of sixth-century cosmologies (under the Meteorological Model) and those of the fifth century (under the Lithic Model) [201–202], and a review of the doxographic tradition on the subjects of the Moon's light and the nature of solar and lunar eclipses. A critical revision of the tradition leads to the result that heliophotism does not appear until Parmenides but consistently thereafter; and that ἀντίφραξις, necessary for correct theories of eclipses, begins to appear with Anaxagoras and consistently thereafter.[11] While Plato is silent on the subjects of the source of the Moon's light and the nature of eclipses, Aristotle's grasp of ἀντίφραξις is in clear evidence in his famous example of syllogistic causal explanation in the *Posterior Analytics*,[12] and his explanation of the occultation of Mars by the Moon at the half Moon presupposes knowledge of heliophotism.[13] By the first century AD, ἀντίφραξις as the correct explanation of both lunar and solar eclipses is a settled science, presented uncontroversially in teaching manuals of the time [See, e.g., Geminus, *Intro. ast.*].

The final chapter 7 summarizes the argument made in the preceding chapters[14] and continues with a meditation on the significance of Anaxagoras for

10 Diogenes' assertion (as reported) that the Moon is like 'an ignited pumice stone' [cited on 191] appears inconsistent with heliophotism, though Graham does his best to avoid this implication [192].

11 The exception is Berosus, a Babylonian priest who became Hellenized but retained the grip of Babylonian astronomy.

12 *Aristotle, An. post. 2.*980a15–18: 'What is an eclipse? Privation of light from the Moon by the Earth's screening [ἀντίφραξις]...'.

13 Aristotle, *De caelo* 292a3–6: 'For we have seen the Moon, half Full, pass beneath the planet Mars, which vanished on its shadow side and came forth by the bright and shining part.'

14 Brief discussion is given in this summary of the claim that Meton and Euctemon deserve to be ranked as fifth-century astronomers. Graham dismisses the claim, arguing that their calculations are based on materials derived from Babylonian sources [236].

the history of astronomy, including a defense of Anaxagoras as a scientist against some anticipated objections. The book concludes with two appendices, the first presenting a survey of the most important historiographical literature on Greek science, with attention to the place that the various authors assign to Anaxagoras, and the second defending the author's realistic or objectivist account of science, which underlies the book's historiography, against various antirealist accounts.

Graham describes his account as a 'reasoned reconstruction' [see, e.g., 228]. As such, the account is both innovative and plausible. Whether it is successful in achieving its aim of revising the calendar of the birth of Greek science is another matter, however. For even if it is plausible to suppose that Parmenides and Anaxagoras might have arrived at their views by relying on empirical confirmation in something like the way Graham's account proposes, that is no proof that they actually did so. But even if we could know that they did in fact rely on such confirmation, that would still not suffice to establish them as the first scientists of Greek antiquity. To practise science is to commit oneself to a particular method of inquiry—to propose and assess theories only in so far as these are open to empirical confirmation. Even if Anaxagoras' theory about the stone-like nature of heavenly bodies did (fortuitously) receive confirmation in the descent of the Aegospotami meteorite, it does not follow that Anaxagoras proposed the 'lithic' theory as a hypothesis awaiting empirical confirmation. To find evidence of a self-conscious commitment to such a method, we must go to the fourth century.

This is not to disparage the case that Graham makes for Parmenides and Anaxagoras or to minimize their roles. It is simply to say that the transition in Greek thought from 'unfounded speculation' [10] to science properly so called is not a sudden moment but a process of historical development in which thinkers like Parmenides[15] and Anaxagoras played a much more important role than has hitherto been appreciated. If Graham had employed his well-crafted argument to make a case for elevating the importance of these two thinkers for their contributions to this transitional process, rather than introducing them as the first Greek practitioners of scientific astronomy, his project, though more modest, would have been stronger.

15 The irony involved in naming Parmenides (the archenemy of sense experience as a basis of knowledge) as the pioneer of empirical science is not lost on Graham [90–91].

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The Philosophy of Gemistos Plethon: Platonism in Late Byzantium, between Hellenism and Orthodoxy by Vojtěch Hladký

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The Philosophy of Gemistos Plethon, a revised doctoral thesis of 2007 from Prague's Charles University, purports to give a systematic overview of George Gemistos Plethon's philosophy that is based mainly on the primary sources, a departure, as the author explains, from a great deal of previous scholarship which 'relies too much on external information about [Plethon's] personality' [6]. This is a laudable ambition; and fittingly, the seven-page introduction offers only a bare outline of Plethon's life and *Nachleben*, and an overview of earlier works on Plethon so summary that it can only serve the purpose of an *apologia* for the author's own approach.

The main body of the work is divided into three unequal parts:

(1) Public Philosophy [11–31], in which a letter and two deliberative speeches from the earliest part of Plethon's literary career are discussed in conjunction with two later funeral orations;
(2) *Philosophia perennis* [35–185], which mainly expounds the contents of the surviving parts of Plethon's *Laws*, supplemented by other sources; and
(3) Question of Religion [189–285], in which the conclusions of previous scholarship on the subject in hand are assessed.

Following the three parts are an appendix collecting some of the texts discussed, a manuscript supplement providing a transcription of British Library Add. MS 5424 for those parts of the *Laws* lacking in Alexandre 1858, bibliographies, an index of passages quoted, and a helpful general index.

The book's organization—rather than any explicit statement—suggests its origin in a preoccupation with the relation between Plethon's public *persona* and his private convictions. Such an interest is naturally prompted by the

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controversial nature of the doctrines expressed in some of Plethon's writings, notably the *Laws*, with its apparent advocacy of pagan polytheism. Modern attempts to understand these doctrines in their contemporary context have typically involved the hypothesis, first proposed by Charles Alexandre and later publicized by François Masai, that the *Laws* was intended for esoteric use by a secret society at Mistra (where Plethon resided from about 1409 until his death). The hypothesis has been expanded in recent times by scholars such as Niketas Siniossoglou into a universal theory, in which each and every humble wellspring in the relatively arid history of Byzantine thought is supposed to be connected by a carefully concealed underground paganist current. For eminently valid scholarly reasons, Vojtěch Hladký discards Siniossoglou's ideas as based on 'no straightforward and unambiguous evidence' [6]. It will turn out that, in part 3, he practically reverses the esotericist perspective by calling into question the extent to which Plethon had any serious agenda in the *Laws* at all [see below].

Part 1, then, takes as its subject matter texts in which Plethon's philosophy was 'openly presented as his own thought to a larger public' [11]. The inclusion among these texts of a letter to Emperor Manuel II and two deliberative speeches directed to named individuals (the emperor and his son Theodore, Despot of Morea) may seem somewhat arbitrary, seeing that several other letters, and especially the *De differentiis*—a treatise on Aristotle's shortcomings as compared to Plato, which sparked a long and fierce debate among the Byzantine *literati* of the 15th century—have been relegated to part 2. One should keep in mind, obviously, that addresses to rulers will have been read aloud in the presence of other dignitaries. Even so, the suspicion lingers that considerations of content have also exerted some influence on the division of texts between the first two parts of the book.

In fact, the letter and the two deliberative speeches are almost entirely taken up with political issues, namely, the urgency of defense measures and social reform in the Peloponnese. With the two funeral orations for Manuel's and Theodore's respective consorts, Empress Helena and Despoina Cleope, the situation is naturally different. In each of these, Plethon presents a string of arguments in favour of the immortality of the human soul, both times ending with one that stands out as much for its weakness as for its potential offensiveness in the context. It is based on the possibility of suicide. As Plethon maintains, people sometimes take their own lives, whatever we may think of it. But since by nature nothing seeks its own destruction (but everything, so far as possible, its own preservation), it is impossible for one and the same entity to be both the agent and the patient of the same act of destruction; rather, then, what is destroyed must be one entity, clearly mortal, and what destroys another one, presumably immortal.

A striking feature of all these examples of Plethon's 'public philosophy' is the absence of appeals to any specifically Christian ideas, whether in the context of defense against the Infidel or in that of the afterlife of the souls of the deceased Imperial Highnesses. On the contrary, the deliberative speeches stress the continuity of the 15th-century Peloponnese with pre-Christian antiquity, while the funereal ones expressly affirm the universality of belief in the soul's immortality. This, however, Hladký argues, 'does not mean that they are in conflict with Christianity' [28].

Open confrontation with the state religion is certainly not to be expected from texts composed for such occasions. However, it seems to me that Plethon's do stretch the limit of decorum: I am thinking, for instance, of his warnings against the three sources of impiety distinguished in book 10 of Plato's *Laws* and his particular concern with the third of these, resulting in the resolute denial of the efficacy of ritual and prayer in his *Consilium* [Lampros 1930, 125.2–126.22] and his *Oratio ad Man.* [Lampros 1926, 258.2–4]. In his speech to the emperor, *Oratio ad Man.*, this particular warning becomes a diatribe against the 'self-professed philosophers'—to wit, the monks—who deserve no financial support from the state [Lampros 1926, 257.9–259.20].

Similarly, praise of the forebears of Empress Helen (herself a nun for the last 25 years of her life), not for having founded churches and monasteries—which they did—but rather for having, in the legendary person of Eumolpus, established the Eleusinian mysteries 'in view of the immortality of the soul' [*Monodia in Helenam Palaiologiam*: Lampros 1926, 269], must have sounded ominous, even to the original audience, at the beginning of Plethon's eulogy. The claim that no one in his right mind would deny that between the Creator and the human race there are intermediary natures, intellects, or else some kind of superior souls, whose main activity is to contemplate the beings of the world and especially their Creator [*Monodia*: Lampros 1926, 276], may well be, as Hladký says, 'interpreted as a statement in perfect accord with' the angelology of Pseudo-Dionysius [24–25]; but to those who had ears to hear the close resemblance of the description of these natures to the accounts of the gods in the *Laws* [Alexandre 1858, 144, 246 / TLG 3.34, 1.146–149, 3.43.70–75], it must have been at least a trifle eerie. And the final argument, which also has its counterpart in the *Laws* [Alexandre 1858, 248 / TLG 3.43.89–108; not noted in this context by Hladký but at 258], contains at least the seed of a defense of suicide.

Although it does not really correspond to any theme developed in the works under consideration, the title of the second part, 'Philosophia perennis', is to some extent justified by Plethon's belief that the truths of Platonism were handed down from Zoroaster through the intermediary of Pythagoras and his followers (and indeed, by the likely influence of this belief on later Renaissance perennialism). The works considered in this part, besides the *Laws*, are an edition and two commentaries on the *Chaldaean Oracles*; the above-mentioned *De differentiis*; two letters to Bessarion and a longer missive to Gennadios Scholarios that were written in response to the two men's reactions to the *De differentiis*; *De virtutis*, a self-contained treatise on ethics; and occasionally other writings. To what extent these works have a better claim to the title of 'philosophia perennis' or, indeed, as already noted, are more 'private' or esoteric than the ones discussed in part 1, is not immediately clear. The thesis that the *Laws* was never intended for publication is argued by Hladký in part 3 [see below].

By his own criteria, Plethon must have been the happiest or most successful (εὐδαιμονέϲτατοϲ) person in his time in so far as his idiosyncratic understanding of the nature of the gods, the world, and human beings was correct—for he thought such an understanding (φρόνηϲιϲ or ϲοφία) to be the highest of virtues and necessary for the kind of happiness available to human beings. In spite of considering his own ethics to be in agreement with the ancient Stoics, he seems not to have believed such an understanding to be sufficient for happiness: many precepts in the *Laws* are concerned with aspects of human behavior apparently unrelated to it. In his shorter treatise *De virtutis* (discussed by Hladký on pages 151–154), the four cardinal virtues (which subsume all the lower virtues) seem to possess some degree of independence from each other. Thus, when he speaks of virtue as sufficient for happiness, as he sometimes does [Alexandre 1858, 18 / TLG 1.1.21–24 and *On Virtue*: Tambrun-Krasker 1987, B13], he presumably has in mind the combination of all four cardinal virtues.

It is tempting to conceive the *Laws* as something like the philosopher's return to the cave. At least, its stated purpose [Alexandre 1858, 1.1] was to show the author's fellow human beings how to arrange their lives in order to live as happily as possible. Predictably, reactions have ranged from perplexity to indignation, as in the case of Gennadios II (*scil.* George Scholarios), whose resolve to commit the autograph copy of Plethon's work to the flames contrived a problematic textual situation in which between a third and half of the work—Hladký's estimate is 43 % [252]—survives in fragments, most of which are preserved in the 15th-century London MS mentioned above and a few others are divided between the 15th-century Brux. 1871–1877 and Plethon's autograph Marc. gr. Z 406. (Alexandre's edition, long due for replacement, makes use of none of these manuscripts.) As I have already hinted, the question of the real purpose of the *Laws*—and indeed of how its seemingly anti-Christian tenor should be interpreted—is addressed by Hladký in part 3.

But let us first dwell for a while on the themes of part 2. Plethon's understanding of the nature of the gods, the world, and human beings, allegedly deduced from 'common notions', is set out by Hladký in proper order, beginning with his 'rational theology'. I should have liked to see a synopsis of the contents of the *Laws* here, if only in the form of a translation of the *pinax* of the work.

What does the reality deducible from the common notions look like? At a cursory glance, not very different from the reality envisaged by late antique Neoplatonism. Everything is ultimately dependent on a single source, the most prominent features of which are its absolute unity and goodness. Even the distinction between essence (οὐϲία) and activity (quaintly referred to by Plethon as ἐνεργία [sic], or, in the *Laws*, as πρᾶξιϲ) collapses.[1] In the *De differentiis*, it is said to be 'beyond being' («ὑπερούϲιοϲ») but in the *Laws* it is repeatedly described as 'being-in-itself' («αὐτοών») and 'truly and really being' («ὄντωϲ ὢν τῷ ὄντι»). Accordingly, there is some disagreement about the status of the first principle in Plethon, with some scholars maintaining that it falls decidedly short of the transcendence of the first principle in Neoplatonism and others, including Hladký, adopting a more cautious stance. Hladký suggests [74–75: cf. 163–165] that the mere fact that the first principle

[1] In addition, it is 'eternally of such a nature as to have a will and an ability that coincide' [Alexandre 1858, 100 / TLG 3.15.17–18].

is the cause of everything else and not an effect should be enough to warrant its radical independence; however, the problem remains as to whether its independence is compromised in any way by the participation of lower entities. He rightly points out that even in the *Laws* some of the epithets of the first principle, notably the Plethonic coinage «προαιώνιος» ('pre-eternal'), seem to stress its independence, although it is possibly misguided to treat «ἐξαίρετος» as a technical term meaning 'transcendent' [45–46, 75, 76, 85].[2]

One might add that the passage from Plethon's first letter to Bessarion [Mohler 1942, 460.32–461.3], sometimes cited as evidence against the transcendence of the first principle, seems in fact to be better taken as evidence in favor of it. It is true that Plethon here seems to allow (if only for the sake of the argument) that being may be attributed to the first principle (since after all the first principle is the only thing that 'is' what it 'is' on account of itself); but, in the same breath, he categorically denies that being in this case could be the same property that we attribute—in this case synonymously—to all the other things that there are. Apart from that obtaining between the first principle and all the other things that there are, the only case of 'homonymy of being' that Plethon is prepared to recognize is that between, on the one hand, the things that there are and, on the other, 'privations, destructions, and in general, evils', since the latter are in fact instances of falling away (ἀποπτώσεις) from being. It seems likely, then, that the description of the first principle in the *Laws* as 'being-in-itself' is intended to suggest that its property of being is unlike that of everything else. Hladký's interpretation of the passage in Plethon's letter seems to be along these lines on page 74, whereas at pages 170–171 and 183 he suggests that it proposes 'the identification of the One and being'.

The disagreement about the transcendence of the first principle is part of a wider debate over the extent to which Plethon may be said to have repudiated Neoplatonic negative theology. As suggested by his scepticism on the issue of transcendence, Hladký reasonably objects to those scholars who have seen in Plethon's approach to the first principle a radical departure from both the

[2] It is true that «ἐξαίρετον [εἶναι]» seems to be used synonymously with «ἐξῃρημένον εἶναι» in Plethon's commentary on *Or. Chald.* [Tambrun-Krasker 1995, 33]. Nonetheless, it would have been desirable for Hladký to refer to this passage and examine Plethon's general use of the word before assuming that this is always what he means by it.

ancient Neoplatonic tradition and its medieval Christian offshoot established by Pseudo-Dionysius [see 75n22]. One must admit, however, that Plethon's apparent insistence that this principle is an object of human understanding would seem to place the burden of proof firmly on the shoulders of those who claim that he adhered to the traditional approach. Hladký does, to some extent, take the task in hand [75–78] but arguably fails to bring it to an entirely satisfactory conclusion. He cites Plethon's commentary on *Or. Chald.* [Tambrun-Krasker 1995, 33], in which the Father's property of being uncreated and self-caused, by virtue of which His divinity transcends everything else, is said to be unimpartable to to anything else on account of its 'passing into its contradictory' («τῷ ἐς ἀντίφασιν περιίστασθαι»). He also cites the commentary on *Or. Chald.* Tambrun-Krasker 1995, 28b], in which the highest, exceedingly unitary, god is declared to be accessible not to our ordinary intellect but to its 'flower' («τὸ τοῦ νοῦ ἄνθος»), which is the highest unitary part of our intellect. As he points out, this is relatively orthodox Neoplatonic doctrine, apart from the (perhaps significant) fact that Proclus in his own commentary on the *Oracles* [Des Places 1971, frr. 4.51–69] introduced a distinction between the flower of the intellect and the flower of the entire soul, precisely because he had little faith in the capacity of the former to reach beyond intellection in a way that would admit unification with the One [76–77]. According to Hladký, the access to the highest god granted to the flower of the intellect 'must be a kind of supra-intellective, mystical union…' [77]. And yet Plethon calls it intellection (νοεῖν). Hladký, for his part, proceeds to explain that through it 'we can…know the main features of the first principle' [77], namely, those described by Plethon in the *Laws*. One is left wondering how it is possible for such a clearly theoretical understanding to be imparted through mystical union and, conversely, why mystical union would be necessary to obtain it.

Other passages in which Plethon speaks optimistically about the contemplation and intellection of the highest god are mentioned *en passant* in other contexts but never brought to bear on the present discussion.[3] Hladký

[3] In the final chapter of the *Laws* (suitably entitled 'Epinomis'), Plethon explains that 'human beings evidently share in the gods' contemplation of being things and indeed do not miss out on the conception (ἔννοια) of Zeus, which is the furthest that even the gods can reach' [Alexandre 1858, 246 / TLG 3.43.72–75: cf. Hladký 155]. And in his *Contra Scholarii obiectiones* [Maltese 1989, 28.9, 34.19–24], Plethon praises Plato

expresses his full agreement with Paul Oskar Kristeller's view that the mystical element 'so prominent...in the thought of the ancient Neoplatonists' is absent from Plethon's Platonism [167]. Surprisingly, the authenticity of the short prayer *Ad deum unum*, which offers perhaps the clearest example of the language of negative theology in the Plethonic corpus, is suspected by Hladký [44], apparently for this very reason. In sum, there remains uncertainty here, as regards both Plethon's view and Hladký's interpretation of it, and I wish Hladký had made more of an effort to resolve it.

Plethon's first principle generates the world of Forms, in which essence and activity are first distinguished. The second principle (the Form of Forms) is an image of the first; the third (the Form of Matter), an image of the second; and so forth. They all share a commonality but are differentiated as the series progresses by decreasing perfection. Exactly how the higher principles are supposed to contain the lower principles within themselves—'implicitly', 'in the manner of unity' [91, 96, 165]—is unclear, since they admit of no distinction between actuality and potentiality [66]: in other words, they are completely free of matter [88, 91, 100]. In fact, Plethon goes so far as to state [Alexandre 1858, 104–106 / TLG 3.15.172–189 (summarized on Hladký 100)] that the second and third principles encompass all the respective subordinate Forms *in actuality* (ἔργῳ), the difference between them being only that the second principle is also the actual cause of every (immanent) form in the lower realms of being, whereas the third principle is the actual cause of prime matter.[4]

The only elucidation of the process of the generation of lower from higher principles afforded by Plethon seems to be what he tells us [Alexandre 1858, 94 / TLG 3.15.28–45, quoted in 90n41]: in contrast to the generation of numbers, brought about by adding the previously generated highest number to the monad and thus proceeding infinitely, the generation of new principles is accomplished by 'dividing' the previously generated lowest Form, unfolding

for holding, in contrast to Aristotle, that virtue is impossible without understanding (φρόνηϲιϲ) and that the noblest part of understanding is that which is concerned with the contemplation and intellection of the highest God [cf. Hladký 152–153].

4 The possibility that « ἔργῳ » should be taken to mean 'as an act' in contradistinction to the product of that act (i.e., « κατ' αἰτίαν ») seems to be ruled out by the application of the same term in the same context also to 'our world', that is, to the temporal and mortal realm.

what is present in it ‘in the manner of unity’ («συλλήβδην τε καὶ καθ’ ἕν») [cf. Alexandre 1858, 46 and 94 / TLG 1.5.34; 3.15.174–175] and keeping some of it while discarding the rest. Since the division is based on contradictories, it will not be an infinite progress. This is all very well; but what exactly it means for a lower principle, prior to its generation, to be in actuality (although ‘in the manner of unity’) present in a higher principle still eludes me; and Hladký’s ‘implication’ metaphor does not help.[5] *Ex hypothesi* the higher principles are simpler, not more complex—contrary to what Hladký seems to imply [91].

In this fashion the whole eternal world of Forms is created. The second realm, which is temporal but everlasting, is created by the Forms; and finally the third, temporal and mortal, realm is created by the entities of the second one. The first principle is thus directly involved only in the creation of the world of Forms. The Forms are 22 in number and ranked ‘according to their generality’ [97] or, strictly speaking, perhaps, the generality of their effects. They are also Intellects, which contemplate each other. This is taken by Hladký [95–96] to contribute to their unity—evidently as a collective, for the combination of intelligibility and intelligence rather seems to compromise the simplicity of each individual Form. Hladký suggests that their commonality in fact resides in their essence (οὐσία), which is simply their ‘common nature of [being] eternal entities’ and causes of the temporal realms, whereas the attributes (προσόντα) by which they are differentiated from each other coincide with their actual causation of different specific forms in the temporal realms [68–70, 96].

This interpretation seems to run up against two difficulties: first, it seems to give priority to the effect over the cause in the order of explanation—a formulation which clearly expresses such an inversion is found on page 165, where it is stated that ‘[the Forms] mutually differentiate among themselves according to what they are models of’. Second, the attributes inherited by each Form from its nearest superior do contribute to their commonal-

[5] Take, as an example to reflect upon, mathematical number and mathematical magnitudes, which are said to be attributes ‘in the manner of unity’ («καθ’ ἕν») of the third principle, the Form of Matter (or Hera, if you wish), which ‘also rules over the whole infinity that relates to them [sc. to number and magnitudes]’ [Alexandre 1858, 114 / TLG 3.15.312–315].

ity according to Plethon's explicit statement at Alexandre 1858, 102 / TLG 3.15.134–140.[6]

The Forms are named after ancient Greek deities. This is one of the features of the *Laws* that have alternately scandalized and titillated readers through the centuries. Still, as Hladký shows [111–122], Plethon's pantheon differs in many respects from anything that he could have encountered in any ancient sources, although he points to some striking similarities with Plato's *Cratylus* and Proclus' commentary on this dialogue [114–121]. Assuming, then, that he was not swayed by pagan piety, why did Plethon make himself so vulnerable to the ire and suspicion of the Orthodox establishment when he might as well have simply assigned ordinal numbers (or Greek letters) to the Forms or else named them after, say, seraphim, cherubim, and thrones? One reason, Hladký suggests [165–166, 273], is that he wanted his principles to be gendered in order to be able jointly to produce entities in the temporal realms of being. It may be objected that inasmuch as 'male' and 'female' in the *Laws*' account of the causation of these realms are really only proxies for 'formal' and 'material', Plethon, as a staunch admirer of the Pythagoreans, must have realized that odd and even numbers would do the job just as well. The 'chief reasons', however, Hladký considers to be 'practical'. Plethon wanted his *Laws* to regulate the lives of actual communities, and '[i]f the ancient names were used properly, [Plethon's new theology] might then become a kind of "philosophy for the masses"' [47]. Notwithstanding this, to my mind, rather uncharitable suggestion (implying as it does that Plethon was completely out of touch with the religious sentiments of his contemporaries), Hladký eventually concludes that

> the *Laws*, especially in its philosophical passages, seems to be a workbook rather than a sacred book,...a kind of exercise book,...most probably a text that contained personal and private thoughts [263],

[6] Hladký's idea that the attributes of each Form are determined by all the other Forms [95–96; cf. 164] seems to be based on a mistranslation [68] of Alexandre 1858, 46 / TLG 1.5.36–39, where « τὰ δὲ προσόντα...ἄλλους ὑπ' ἄλλων διατίθεσθαί τε καὶ κοσμεῖσθαι » means 'with regard to their attributes, the ones [*scil.* Intellects] are conditioned and equipped by the others [i.e., by their respective immediate superiors]'. As is clear from the agent phrase, the infinitives are in the passive voice.

a conclusion for which he finds support in the fact that its author never seems to have taken any measures to have it duplicated [cf. 254]. But this brings us to the theme of part 3 and we are not quite finished with part 2.

Plethon repeatedly stresses [e.g., Alexandre 1858, 94 / TLG 3.15.26–28, quoted at 90n41] that the first principle has no need for any contributory cause (συναίτιον) in generating the Forms. The contributory cause that he has in mind is (intelligible) matter [Alexandre 1858, 92 / TLG 3.15.8–10]. On the other hand, the Form of Matter, that is, the cause of the existence of matter in the temporal realms, is an image of the first principle at two removes. Evidently Plethon has set his heart on a strictly monistic all-encompassing system. As a result, like all the Neoplatonic systems, his is haunted by the problem of evil. Why does the generation of ever lower entities give rise to ever greater imperfections in the world, if there is no contributory cause that sets a limit for the beneficial agency of the first principle? Plethon's reply seems to be encapsulated in the adage 'because a different is always the cause of a different' [93].[7] Since the first principle is an absolutely perfect model, then, its image must have some degree of imperfection. But unless the adage is meant to express an independent principle of entropy by which dualism is surreptitiously introduced into Plethon's system, it must be the absolutely unitary first principle itself that is somehow fraught with this difference. That is to say, the generation of a *second* principle must be part of what it is to be the first principle.[8] Unfortunately, this also means that the first principle, which is, in addition, absolutely good, must be the origin of all the imperfections in the world; but it seems as though Plethon, like all his Platonic predecessors (outside the Gnostic ranks, at any rate), would have denied this.

[7] The comparison in Alexandre 1858, 98 / TLG 3.15.90–105, referred to at 92n45, to human fathers, who beget, in descending order according to the vigor of their seed, boys who are their spitting image, ditto girls, boys who look like their mother, ditto girls, boys or girls who look like some other member of the family, ones who simply look like human beings and ones of whom not even that much could be said, should probably not be pressed too hard.

[8] As Hladký writes:

> This is the traditional Platonic concept of *bonum diffusivum sui*, according to which the supreme good, because of its goodness, *cannot refrain from* creating something *different* and yet similar to itself. [128: my italics]

It may, of course, be said that the first principle cannot properly speaking be responsible for all these imperfections since evidently it generates the second principle out of necessity. Indeed, Plethon maintains that the strongest necessity of all ('necessity on account of itself') belongs to the first principle, even though this principle is admittedly undetermined, for lack of a higher cause that may determine it [Alexandre 1858, 66 / TLG 2.6.26–35, quoted at 146n31].[9] But, since it is also emphatically asserted to be its own cause, it would presumably be more accurate to say that it is self-determined; and if so, it may not be such a straightforward thing to clear it of responsibility after all. Be that as it may, it makes little sense in the context of a strictly monistic deterministic system like Plethon's to explain the existence of imperfections by attributing them to 'a world that is the best possible' [146], since in such a system, of course, there is no *other* possibility than the actual state of affairs. It may equally well (or rather equally inappropriately) be called 'the worst possible world'. Either way, it really should be nothing less than perfect.

But I digress. The point that I wish to make is that if Plethon made any attempt, besides repeating the above-quoted adage, to solve the fundamental problem as to why, if everything is caused by a single perfect first principle, the world abounds with imperfections, I should have liked to be informed about it in a book on Plethon's philosophy (one reason for this, albeit not the most important one, is that it may have shed some light on the question of Plethon's relation to Proclus and other Platonic predecessors). If he did not, I should have liked to be informed about that too, since it is of some importance for my assessment of Plethon as a philosopher. Perhaps I am being overly demanding and not entirely fair. Hladký does in fact touch briefly on these and related issues in his 'global overview' of Plethon's system [see 163–167] but his discussion goes only a little way towards quenching my thirst for information—which illustrates what I consider to be the major flaw of *The Philosophy of Gemistos Plethon*. As Hladký is 'aware' [7], most of it is given over to detailed summaries of the contents of some of Plethon's works, in particular the *Laws*, while little is offered in the way of philosophical analysis

[9] Note that this is the very necessity on which every other necessary fact depends [Alexandre 1858, 74 / TLG 2.6.114–117]. For this reason, I cannot agree with Gersh that '"[n]ecessity" is obviously an affirmative divine name applied to Zeus from subsequent things rather than a property that he has *per se*' [Gersh 2014, 223n34].

or indeed of source studies or historical contextualization. I shall return to Hladký's source studies shortly.

Unsurprisingly, human beings are assigned a very special place in Plethon's world, at the boundary between the everlasting and the mortal realms. They occupy this place by virtue of their unique soul. What sets human souls apart from those of other animals is the fact that they are partly rational, thanks to their relation to the Forms—a relation that is alternately described as one of similarity (or kinship) and participation. The rational behaviour of other animals is governed by the World Soul. Apart from this, we do not hear much of the World Soul in Hladký's book (there are four references in the general index) but it seems as though Plethon might have located it in the Sun [143].

The determinism of Plethon's system applies without restriction to the human soul. This application is the topic of chapter 11, which is one of the strongest of the book, especially pages 144–150. Even the governing part of the human soul, what Plethon calls τὸ φρονοῦν (and Hladký translates by 'the prudent part'), is pre-programmed to act in accordance with what it considers best; and what it considers best is, of course, a result of its history and other circumstances. No human being is free in the sense of not being entirely subject to external forces. But then again, nor are any other agents, even in the eternal realm, except the first principle. As Hladký shows, however, Plethon prefers to understand human freedom as the attainment of one's most fundamental desires.

In Hladký's account, Plethon holds that all human beings most fundamentally desire to do well, where, in accordance with a lemma from Plato's *Republic*, being just is a necessary and sufficient condition for doing well. (I should point out that this is only my own reconstruction of the argument presented by Hladký on page 149; and, since it seems to conflict with what I said above about the relative independence of the cardinal virtues in Plethon, I may well be mistaken.) Those who are just, then, have attained their most fundamental desire and consequently are free. Conversely, however, no human being desires not to do well. It follows that those who are unjust, and thus have not attained their most fundamental desire, are not free. In other words, for Hladký, Plethon thinks that all and only those who are just are also free. But if that is the case, it seems unwarranted to hold anyone who is unjust morally accountable for their deplorable state and Plethon seems in fact happy to accept this consequence; at least, he stresses that divine (or daemonic [166]) punishment should be understood only as a means of correction [150]. It is not clear from Hladký's account whether Plethon addresses the underlying problem as to why, in 'a world that is the best possible', those erroneous judgments that lead to injustice occur.

In the final chapter of part 2 [163–185], Hladký tries, as I have mentioned, to summarize the main features of Plethon's system, after which he proceeds to compare it with those of other Platonists. Inevitably, a large part of this chapter is devoted to the question of Proclus' influence on Plethon. There has been a strong tendency among Plethon scholars in recent years to emphasize the differences between the Byzantine philosopher's system and that of the Platonic Successor. The above-mentioned view, partly challenged by Hladký, that the traditional Neoplatonic negative theology is absent in Plethon naturally comes into play here. Most of Hladký's discussion of the question is restricted to the explicit criticism of a couple of specific points in Proclus' system in Plethon's first letter to Bessarion [169–173], although he also mentions, as more general points of difference, Plethon's dim view of the ancient poets and his lack of interest in theurgy as well as the fact that 'the structure of reality in Plethon's philosophy is far less diversified than in Proclus' [169]. On the other side of the argument, in view of 'some undeniable parallels' [114] between Proclus' commentary on the *Cratylus* and Plethon's version of the Greek pantheon,[10] as well as Plethon's demonstrably 'good knowledge of Proclus' *Commentary on the Timaeus*' [276n17], he considers it 'very likely that Plethon both studied Proclus and was influenced by him, including even in his overall rational approach to theology' [179]. He concludes nevertheless, and probably rightly so, that Gennadios Scholarios' allegation that Plethon had silently drawn the doctrines of the *Laws* from Proclus' works is not to be relied upon [168].

Instead, Hladký [176–179] suggests that the 'list of philosophers' enumerated in the introductory chapter of the *Laws* should be taken 'seriously' [168], although he promptly excludes from consideration more than two thirds of the names on the list for not being 'historical persons' (including Solon of Athens and Thales of Miletus, whereas Timaeus of Locri and Pythagoras are retained). But experience should teach us to handle Byzantine writers'

10 He goes so far as to say that Proclus' commentary's 'presence seems to be so massive that one may conclude that Plethon used it when studying Plato's *Cratylus*' [122].

own claims of intellectual descent as circumspectly as the counterclaims of their opponents. Taking references such as these seriously does not mean accepting them at face value but checking them carefully. Hladký's attempts in this direction are feeble. In fact, the major shortcoming of his discussions of the influences on Plethon is that there is no real effort to trace the sources of the actual doctrines and arguments in the Byzantine philosopher's works. Most of the attention is devoted to preliminaries, such as ascertaining which works may have been available to Plethon (mainly on the basis of citations in the contemporary literature).

The question as to why certain philosophers are not mentioned in Plethon's list is an interesting one but the answer is clearly not that they played no part in shaping Plethon's 'perennial philosophy'. Hladký's contention that Plato, Aristotle, and Plutarch 'were studied really widely, in contrast to the works of the Neoplatonists whose circulation was more limited' [177] needs to be supported by more than a reference to a couple of dated surveys of Late Byzantine intellectual life.[11] There has been much research on the Byzantine *fortuna* of Proclus and other late antique philosophers in recent years.[12] Regarding the knowledge of the works of the Neoplatonists in Middle and Late Byzantium, it should be said, that

(1) it is clear that Proclus (especially his *Elements of Theology*) never fell out of fashion among the Greek *literati* between the 11th and the 15th centuries;
(2) even non-commentary works by Plotinus, Iamblichus, and others enjoyed relatively wide circulation from the late 13th century onwards; and, most importantly, that
(3) Aristotle was practically always studied (as was indeed Plato, in so far as he was studied for the philosophical content rather than the literary style of his works) with the aid of philosophical commentaries, mostly written by—or dependent on other commentaries written by—Neoplatonists.

[11] Namely, Runciman 1970 and Fryde 2000. On the shortcomings of the latter work, see Littlewood 2002, 1288–1290.

[12] See, most recently, the papers by Dominic O'Meara and Michele Trizio as well as the excursus on Plethon by Stephen Gersh in Gersh 2014.

Plethon's familiarity with several Aristotelian commentaries is evident, if not from the *Laws*, then certainly from Hladký's second group of writings concerning 'perennial philosophy'.[13] It can also be safely assumed *pace* Hladký [179] that he was well acquainted with Porphyry's *Isagoge*. Everybody was.

The upshot of the chapter is that in its structural simplicity and by de-emphasizing, if not negating, the first principle's transcendence, Plethon's system deviates from the Neoplatonic ones in the direction of Middle Platonism [180, 183]. Hladký attributes this primarily to Plethon's reliance 'on the literal meaning of Plato's text' as well as his fervent admiration for the *Chaldaean Oracles* [180]. For obvious (chronological) reasons, he does not discuss the recent suggestion by Stephen Gersh that Plethon's motive for maintaining a critical distance from Proclus may have been his wish 'to excavate a Platonism that is free of later Christian accretions', on the widely held assumption that Proclus was dependent on Ps.-Dionysius [Gersh 2014, 218–219, 221–222].

When Hladký finally turns to the question of Plethon's religious views in part 3, he departs to some extent from the text-based approach promised in the introduction and largely adhered to in the two preceding parts. He begins [189–190] by summarizing four received opinions (or 'usual conclusions') on the subject and goes on [191–267] to consider a number of arguments both for and against each of them that are rarely based on hard facts and mostly lead to conclusions in the modality of possibility. That Hladký introduces these received opinions practically without comment and allows the subsequent discussion to meander without a clear or systematic plan is symptomatic of a certain deficit of meta-discursive elements in his writing that too often leaves readers to figure out for themselves what role a particular argument or series of arguments is supposed to play in the overall scheme of things.

The received opinions (or sets of opinions) are that

(1) Plethon was influenced by a Jew called Elissaeus, who was an adherent of Suhrawardi;

[13] E.g., *De differentiis*, where his use of Simplicius' commentary on the *Physics*, Asclepius' commentary on the *Metaphysics*, the anonymous commentaries on *Nicomachean Ethics* 3 and 7, and indeed Thomas Aquinas' *Summa contra gentiles*, is in evidence, in addition to several commentaries that cannot be identified with certainty.

(2) Plethon was the leader of a pagan community in Mistra, for which the *Laws* was written;
(3) Plethon changed his name and converted to paganism as a result of his experiences in Italy during the council of Ferrara and Florence; and that
(4) Plethon wrote the *Laws* after his return from Italy in 1439 and, since he must be assumed to have espoused the doctrines presented in it, his *Contra de dogmate latino librum* from the same period, which defends the orthodox view of the procession of the Holy Spirit, must be considered as an exercise in hypocrisy.

Regarding (1), Hladký rightly emphasizes that the source for the personality of Elissaeus (Gennadios Scholarios) is unreliable and points out that those of Plethon's views which it is most tempting to connect with a Suhrawardian influence (notably, the view that the *Chaldaean Oracles* represent the teachings of Zoroaster) may equally well have been inspired by the reading of late antique Greek authors [191–204].

Regarding (2), Hladký shows that no such community is attested by any of Plethon's friends or students and plausibly argues that the existence of such a community is highly unlikely in the historical circumstances [205–233]. He also cautions, on the whole reasonably but sometimes on dubious grounds, against taking Plethon's opponents at their word. He suggests, for instance, following Ernst Feil [1986, 166–167], that one reason for scepticism concerning George of Trebizond's report of a statement by Plethon to the effect that the whole world would soon adopt one and the same religion is that it 'is difficult to find a Greek equivalent for the Latin term "religion (*religio*)"' [229]. While it is true that «θρησκεία» seems not to occur in Plethon's extant writings, a simple search of the Thesaurus Linguae Graecae (TLG) shows that the word was very frequently used as the equivalent of 'religio' (used, for example, for different religions, such as Christianity and Islam, in the singular and in the plural) by such contemporary authors as Gennadios Scholarios,[14] Manuel II, Laonikos Chalkokondyles [quoted in 216n47], John Eugenikos, Bessarion, and indeed in the very acts of the Council of Florence.

Regarding (3), Hladký shows that the name 'Plethon' was in fact never publicly used during Plethon's lifetime [235–238].

14 In his *Grammatica*, he defines it as «ἡ λατρεία καὶ τὸ ὁποιοῦν σέβας περὶ Θεόν».

As for (4), he remarks that the content of Plethon's treatise on the Holy Spirit is in full agreement with the testimony of fellow anti-Unionists such as Sylvester Syropoulos (whose credibility as a witness Hladký attempts to vindicate [246n26]) and John Eugenikos, not to mention that of Bessarion; and that there is no reason apart from its inconsistency with the doctrines of the *Laws* to question Plethon's sincerity in writing it [239–250].

Not only how to evaluate Plethon's *Contra de dogmate latino librum* but also what to make of his 'paganism' would seem to depend to a large extent on what we think were Plethon's intentions in writing the *Laws*. This affords an occasion for Hladký to return to Plethon's *magnum opus* [251–267]. He suggests (on mostly rather flimsy evidence consisting mainly of general correspondences between the *Laws* and datable works) that the *Laws* was in fact composed over a couple of decades, more precisely from the late 1410s to sometime around 1440, as 'a kind of exercise book' [263] that was 'not intended for publication' [270]. At the end of this chapter [263–267], Hladký discusses Plethon's motives for tampering in his personal copies with such authoritative (not to say sacred) texts as the *Corpus Platonicum*, the *Orphic Hymns,* and the *Hymns* of Proclus. He concludes that this editorial activity on Plethon's part 'was apparently an attempt to get to the original form and meaning of the text as he thought it should be' [267], without clear implications for the question of Plethon's religious convictions.

When the time comes to deliver a final verdict on this question in the concluding chapter of part 3, Hladký draws attention to the fact that the concept of paganism is differently understood from different historical vantage-points [269] and rightly insists on distinguishing between 'a mere admiration' for ancient thought and religion and the outright espousal of ancient religious beliefs—although, one might add, there are probably many intermediary steps and no hard and fast boundary between one and the next. Many thinkers from the Middle Ages onward have admired all manner of manifestations of pagan intellectual culture. The question that has exercised scholars in the case of Plethon, I presume, is whether he overstepped the limits of Christianity. To this there will obviously be different answers provided by different judges in different times.

It is, however, patent, I think, that the doctrines of the *Laws* are impossible to reconcile with any conceivable interpretation of Christianity in Plethon's historical context. To that extent, Gennadios Scholarios appears to have known what he was doing. Hladký mentions 'three main divergences that make [Plethon's] *philosophia perennis* irreconcilable with Christianity': namely, 'the absence of the doctrine of the Trinity' and the presence of the doctrines of the eternity of the world and of reincarnation [273]. To my mind, having benefitted from Hladký's book, it seems that the most serious theoretical stumbling block would be the idea that the first principle is incapable of not generating the second principle. In addition, I very much doubt whether any contemporary Orthodox or Catholic would consider Plethon's apparent denial of free will acceptable Christian doctrine—or indeed his polytheism, of which Hladký seems to make too light when he compares it to 'similar hierarchies of angels and divine beings' [274].

Hladký's distinctive contribution to the debate consists in the case that he builds against reading the *Laws* as a straightforward expression of Plethon's religious views. Some of his arguments to this conclusion are more convincing than others and it is perhaps not entirely clear what they all add up to in the end. It is true, for instance, that the fact that the only complete copy of the *Laws* seems to have been the one burnt by Gennadios Scholarios speaks against any strong evangelical ambition on Plethon's part and could reasonably be counted as support for the view that the *Laws* was 'in fact a private writing' [270], although, as Hladký himself admits, 'we cannot exclude a possibility that he also used some parts of it in his teaching' [263]. Still, the question remains as to why private writing should be dismissed as evidence for an author's 'real thought' [271]. Hladký speculates [278] that the *Laws* for Plethon had the same semi-utopian character as Plato's *Laws* had for the Neoplatonists (or perhaps the same fully utopian character that was ascribed to the *Republic*). The consequence would be that its political guidelines were never meant to be put into practice—which would perhaps not be so surprising, since such guidelines make up only a minor portion of the remains of the *Laws* [161–162]. But, again, it is not clear what this has to do with the question of whether Plethon believed in the religious doctrines presented in the work. Hladký also thinks that 'the fact that [the *Laws*] was probably composed' at an earlier date than has previously been assumed detracts from its 'significance…for determining Gemistos' religious beliefs' [280]. Presumably, he means his mature beliefs; but that does not really help the argument, since on the dating that he proposes Plethon would have been between 60 and 80 years old when composing the *Laws*. He further tries to make it plausible that Plethon did not really believe in his own system by adducing parallels from Renaissance humanists such as Boccaccio's *Genealogy of the Pagan Gods*, the *Hypnerotomachia Poliphili,* and Cyriac of Ancona's letters, and by proposing to 'reclassify' Plethon as a Renaissance humanist rather than a Byzantine philosopher, 'where such a flirting with the Greek pagan past is certainly suspect' [282]. Unfortunately, however, no such reclassification can alter the historical circumstances of Plethon's life and works.

Here and there one senses a certain eagerness on Hladký's part to downplay Plethon's hostility in the *Laws* towards the mores of contemporary Christianity. For instance, his suggestion that Plethon's criticism of certain 'sophistical' doctrines of Creation and the Last Days (including Tribulation as well as Millennium [Alexandre 1858, 260 / TLG 3.43.238–240]) at the very end of the last chapter of the *Laws* should be understood within the context of 'ancient philosophical discussions' [276] is decidedly far-fetched. The same, I think, applies to his interpretation [50] of Plethon's criticism in the first chapter of the *Laws* of celibacy, fasting, self-neglect, and voluntary poverty as targeting ancient Cynics and Pythagoreans (*sic*), rather than contemporary monks, as has usually been thought. As noted above, and acknowledged by Hladký [17], the *Oratio ad Manuelem Palaeologum* leaves no room for doubt about Plethon's disdain for the monastic way of life and its practitioners.

Hladký ends with an intriguing suggestion, first ventured by Kristeller. Could the solution to the puzzle about Plethon's religious views simply be that he 'always maintained a strict separation between his philosophy and Christian theology and never tried to harmonize them'?[15] Who knows? It looks like a desperate solution and one which leaves us with a picture of a deeply conflicted thinker. It may nevertheless be true.

A few words must be said about the language of the book. Clunky and unidiomatic English in academic works is nothing new. And most probably, the discomfort of reading it is a price that members of the global academic community are willing to pay for being relieved of the necessity of learning dozens of languages in order to keep abreast of their respective fields. Still, I think, it must be incumbent on academic publishers, especially those with offices in English-speaking countries, who after all benefit greatly from the position of English as a global academic *lingua franca*, to make a serious effort to reduce that discomfort. In this case, I cannot bring myself to believe

[15] Kristeller 1972, 97, quoted by Hladký on p. 273: cf. 284.

that Ashgate has lifted a finger to help. There are numerous infelicities and errors of grammar and style on each and every page. Granted, these are usually minor and only rarely of such a kind as to affect the overall sense or challenge reading comprehension; but this only means that they could have been all the more easily rectified.

Does *The Philosophy of Gemistos Plethon* succeed in its ambition? Only to a limited degree, I am afraid. That the balance between summary and philosophical analysis is so clearly tipped towards the former is particularly regrettable in view of the fact that there already exists in C. M. Woodhouse's *Gemistos Plethon: The Last of the Hellenes* [1986] an excellent monograph in English on Plethon, which contains paraphrases of most of the relevant works. In comparison to this, the relative lack of structural clarity and the broken English combine to put Hladký's book at a disadvantage. Where Hladký's book is arguably stronger, that is, in the neglected area of philosophical analysis, it is still not always entirely clear and consistent, as I hope to have shown.

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The Ancient Mediterranean Environment between Science and History
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W. V. Harris contributes to an ongoing, and indeed ancient, debate on whether and how greatly human activity affects the environment or even climate and, correlatively, how environmental change affects human history. The authors in this edited volume do not attempt a new grand synthesis; indeed, the implicit (and almost explicit) argument here is that no synthesis is as yet possible.

Holding a belief that the world's climate is for the most part stable or that it changes in ways determined by some cosmic-scale process (the Stoic conflagration or the like) would tend to make one think that, *a fortiori*, we humans are scarcely able to affect the climate, even on a small scale. On the other hand, a belief that climate is unstable opens up the prospect of human action having some determinate effects that are proportional perhaps to our technological power. Moreover, the kinds of effects anticipated might be influenced by one's view of the general nature of climatic fluctuations: if the world is trending in some direction, it might seem easier to amplify than to oppose the trend.

Cultures differ in their view of the world and its climate. Some see long-term, or even perpetual, stability; others expect a catastrophe or decline. Those outcomes may be attributed to divine powers and they may be ameliorated by human agency; for example, the ancient Egyptians and ancient Chinese seem to have believed that the stability of their environment was in part guaranteed by the right actions of their pharaoh or emperor. In early Greek thought, Hesiod described the world as degrading from Golden to Silver to Bronze to the current awful Iron Age, though he said nothing about environmental change *per se*. Conversely, Democritus proposed that worlds came

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and went by chance and that in our world humans had slowly advanced from an almost bestial state to control nature and form civil society. The random fluctuations of the atoms presumably allow for environmental and climatic change, on the one hand, and, on the other, there seems no bar to supposing that sufficiently powerful humans might affect the environment. Plato proposed a divinely ordered *cosmos*, yet allowed for catastrophic environmental harm, at least in myth [*Tim.* 22, *Crit.* 110–111]. Aristotle's world-order was perpetual and stable [*Phys.* 8.1], yet he too allowed for endogenous climate change, albeit slow [*Meteor.* 1.14]. Columella records that the Sasernae, father and son, writing on agriculture in *ca* 90 BC, had hypothesized that the precession of the equinoxes discovered by Hipparchus could explain an observed amelioration of climate that allowed viticulture further north than formerly [*De re rust.* 1.1.4–5: see Thibodeau 2008]. Ancient authors do occasionally allow for human-caused change, at least in small ways. For example, in allowing for endogenous climate change, Aristotle also claims that the canal joining the Red Sea to the Nile was never completed [cf. Herodotus, *Hist.* 2.158], because the rulers realized that it would salinize and thus damage the Nile. Aristotle's student Theophrastus claims that an actual (micro-) climate change has occurred: after the people of Larisa in Thessaly drained the swamp, the valley became colder, killing off the olive trees [*De causis plant.* 5.14.2–3]. In the first century AD, Petronius [*Satyr.* 99.3.1] claimed that 'snows cling longer to rough and uncultivated regions but the earth shines where it has been tamed by the plow, (and) the light snow melts away as you speak of it'[1] Much later, Pausanias [*Graec. desc.* 8.24.11] explains the silting up of some river-mouths (the Maeander at Miletus) in contrast to others (the Acheloos in Acarnania) by citing the amount of plowing performed along their valleys: more plowing yields more erosion.

The Christian synthesis, heavily influenced by Neoplatonism, asserted a *cosmos* that has been stable since its creation, within which catastrophes or alteration to the divinely instituted order could scarcely occur. (Outside the domain of monotheisms, cyclic theologies continued to insist that change was inevitable.) Within that synthesis, the lost paradise of Eden was the perfect environment compared to which the current corrupted Earth was, as Dante [*Inferno* 14.94–99] expressed it of Crete taken as symbolic of the whole

[1] *incultis asperisque regionibus diutius niues haerent, ast ubi aratro domefacta tellus nitet, dum loqueris leuis pruina dilabitur.*

fallen world, a desert waste: 'paese guasto…diserta come cosa vieta'.[2] Even the lengthy (*ca* 1300 to *ca* 1850) and widespread shift in climate identified in 1939 as the 'Little Ice Age' seems to have gone unnoticed by contemporaries as a change in climate, although many of the individual effects were commented upon.[3]

Despite that synthesis, beginning in the 15th century, some Europeans attempted to modify their environment on a moderately large scale, by constructing polders.[4] The Dutch (and German and English) residents of marshy river-delta regions around the North Sea by *ca* AD 1000 began to enclose their villages and fields with dikes as barriers against encroaching seawater. This represents an attempt to stabilize the local environment against what from the human perspective is a natural degradation. By about the middle of the 15th century, the Dutch were building dikes around shallow areas of the sea and then using windmill-driven pumps to drain those basins: this constituted the creation of a polder. The earlier polders were small, only a few dozen hectares, but by 1608–1612, with the carefully planned creation of the Beemster polder (71 km^2), regions as large as dozens of square kilometers were transformed from shallow sea to low-lying land. Larger but less successful efforts were undertaken in the fen-lands of eastern England. Opponents of the project argued that the change would contravene the will of God. These projects go well beyond the taming of nature through cultivation of fields and gardens.

2 The *Inferno* was published *ca* 1316. Earlier, see Thomas Aquinas (*ca* 1268), *Summa theol.* 1.102.2, quoting John of Damascus (*ca* 710), *Expositio fidei* 2.11, 'temperate, pure, and exquisite atmosphere, and decked with ever-flowering plants'. A little before John, Isidore of Seville, *Etym.* 14.3.2, had similarly described Eden: 'It does not grow cold or hot there but the air is always temperate' (*ca* 625). All of this imagery goes back to Greco-Roman notions of the Blessed Isles: see Keyser 1993.

3 Matthes 1939, 520 identified the end of the period and gave it its name but saw it as commencing much earlier: 'we are living in an epoch of renewed but moderate glaciation—a "little ice-age", that already has lasted 4000 years'. Grove and Rackham 2001 present data showing that it affected the Mediterranean. Grove 2004 provides an extensive analysis of the worldwide data: see pp. 1–10, 371–402 for an overview and pp. 604–641 for an evaluation of the effects of the climate change.

4 My account here depends upon Wagret 1959, 58–114 =1968, 51–103; Ash 2007; Fleischer 2007; and Steenbergen, Reh, Nijhuis, and Pouderoijen 2009, 43–71.

The Christian cosmological synthesis began to crumble in the early 17th century when Galileo showed that the long-observed dark spots on the Sun were indeed changes in the Sun. That is, a heavenly body long considered immutable was shown to alter.[5] On the Earth itself, cyclic phenomena, diurnal, lunar, or annual, were of course well known; but people began to find evidence for degradation in the environment, apparently deriving the idea from Lucretius, *De rer. nat.* 2.1150–1174, which depicts the Earth as aging and running down.[6] The theory that human activity could or did lead to degradation of notable swathes of the environment became widespread by the 18th century [see Grove and Rackham 2001, 8–10]. That analysis was reinforced by the frequent deployment by 18th-century European thinkers of a rhetoric of perversity against proposed political or social innovations, rejecting proposals on the grounds that if carried out they would produce effects opposite to what was intended.[7] The assertion that human intervention in nature has perverse results is the same trope in a different context. (Whether in any given case it is an accurate analysis is a separate question.)

Several authors writing on America, however, argued that deforestation would have significant, although local, effects. Cotton Mather, the Puritan cleric and pamphleteer, hypothesized that American winters had become milder due to forest-clearing and agricultural activity:

> Our own Winters are, as observably as Comfortably Moderated since the land has been Peopled, and Opened, of Later Years. Our Snows are not so Deep, and Long, since the Progress that has been made, in the Clearing of our Woods; and our Winds blow not so much Rasours [i.e., razors]...

[5] Shea 1970; Reeves and Van Helden 2010, reviewed in this journal in Miller 2012.

[6] Rejected by Columella, *De re rust.* 1.*pr*.1:

> In their opinion, the soil was worn out and exhausted by the over-production of earlier days and can no longer furnish sustenance to mortals with its old-time benevolence.

Cf. Pliny, *Nat. hist.* 31.53: ‘Indeed, destructive torrents often run together when the hills have been denuded of the trees that used to contain and absorb the rains’.

[7] Hirschman 1991, 11–42, esp. 72:

> The perverse...sees the...world as remarkably *volatile*,...[in contrast to those who] view the world as *highly structured* and as evolving according to immanent laws, which human actions are laughably impotent to modify. [emphases in original]

He supported that by quoting Petronius, *Satyr*. 99.3.1, as above [Mather 1693, 42–43]. By the end of the 18th century, this opinion was common enough to elicit studies[8] such as Williamson 1770 especially. Thomas Jefferson, writing from Paris as minister from the United States, claimed in his *Notes on the State of Virginia*: Query VII. Climate [1785], that:

> A change in our climate, however, is taking place very sensibly. Both heats and colds are become much more moderate within the memory even of the middle aged. [Jefferson 2002, 135]

Other writers argued the opposite, that deforestation would augment the extremes of climate, especially Dunbar, who claimed that 'It is with us a general remark, that of late years the summers have become hotter and the winters colder than formerly' [Dunbar 1809, 48], and stated that the cleared region would allow the Sun to warm the soil more in the summer and block the winds less in the winter [49].

Despite that rhetoric of perversity, the Congress of Vienna (1814–1815) established the 'Zentralkommission für die Rheinschifffahrt' / 'Commission Centrale pour la Navigation du Rhin', an organization whose goal was the reshaping of a large tract of land (the Rhine valley) for anthropocentric ends.[9] Here is an example, then, of a deliberate and conscious attempt by people to alter a significant region of their environment, and one expected to succeed. The river was channeled and its flow optimized for shipping; large floods were predicted and have been an actual consequence.

At the turn of the 18th century, scientists studying the Earth and its living beings were divided between two grand theories, labelled 'catastrophism' and 'uniformitarianism'. According to the uniformitarians, the processes affecting the Earth and its living beings that could be seen in operation 'now' were the same ones, perhaps varying in degree, that had 'always' been operating. According to the catastrophists, in the remote past catastrophes of various kinds had greatly altered the surface and inhabitants of the Earth. A leading

[8] See Thompson 1980; Fleming 1990, 2–5.

[9] Cioc 2013, esp. 30: any river, and especially the Rhine,

> was 'wild' and 'unruly' and therefore in need of being 'tamed' or 'harnessed' or, alternatively, was an 'enemy' in need of being 'defeated'.

I am indebted to my brother, Rick Keyser (University of Wisconsin), for alerting me to the revealing work of Cioc.

proponent of the catastrophist model was Georges Cuvier, who demonstrated that the Indian and African elephants were two distinct species (Linnaeus had coalesced them), and that the Siberian mammoth was a distinct, and extinct, species, as was also the American mastodon; both the Siberian and American, he claimed, had been extinguished through catastrophe.[10]

Although the scientific consensus of the 19th century came to prefer uniformitarianism, Cuvier had established that extinction occurs and many more species came to be recognized as having perished. But it does not seem to have been widely considered, or even considered at all, that humans may have caused the extinction of the mammoth or mastodon—although beginning in 1833 the dodo of Mauritius was cited as an example of human-caused extinction [see Turvey and Cheke 2008]. In contrast, James Hutton's theory of the Earth (first published in 1788, and made more accessible by Lyell's work of 1830–1833) argued that catastrophes were rare and not globally significant—but also that the Earth was very old compared to recorded history [see Dean 1992]. Thus, according to the uniformitarians, there was little prospect of significant anthropogenic alteration to the environment.

During 1740–1820, the thinking on what are now understood to be glacial erratics evolved, which in turn (and perhaps influenced by the abnormally cold 1810s) led to the evolution during 1820–1835 of various theories of a former ice age that were synthesized in 1837 as a theory of multiple ancient ice ages by Agassiz and Schimper. The theory became widely accepted within 40 years.[11] So the regular scientific discourse now included the idea that the climate of large areas might change quite drastically, albeit without human intervention.

That thinking seems to have been aided by the observation, at that time unexplained, that the winters of 1815/16 and a few years following were notably more severe than prior winters. Benjamin Franklin had already hypothesized, in a paper presented in 1784, that volcanic dust and ash might cause large-scale cooling of the climate, on the basis of his observations of

[10] Rudwick 1998, 13–24 annotates and translates Cuvier's paper from 1796 on elephants; two papers from 1806 on extinct and living elephants are likewise annotated and translated on pp. 89–97.

[11] See Krüger 2013, 23–84 on erratics, 85–163 on the early theories of an ice age, 165–189 on Agassiz and Schimper, and 191–397 on the diffusion and acceptance of the theory.

the dust and ash of Hekla in 1783 and the subsequent hard winter of 1783/4 [Franklin 1789, 375–377]. However, although the explosion of Tambora in 1815 is the largest known volcanic eruption in several millennia [Oppenheimer 2003], few people in Europe or America were aware of the event and thus the contemporary explanations offered in those continents for the hard winters varied widely.[12] Nevertheless, there was a general recognition that the climate had somehow changed, at least temporarily and not for the better. The recognition doubtless reinforced any tendency to respond to suggestions for climate engineering, on however small a scale, with a rhetoric of perversity.

Meanwhile, the first suggestion of large-scale anthropogenic climate change had been made by Erasmus Darwin (grandfather of Charles Darwin) in his 1791 poem 'The Botanic Garden: 1. The Economy of Vegetation'. He suggested, both in verse (lines 497–548) and in his accompanying notes [Darwin 1791, 48–53], that the nations of Europe should unite to tow icebergs to the tropics, to ameliorate the climate. The proposal was received as an instance of hubris and was rejected both as impractical and ineffective or, if effective, then perverse [see Carroll 2013, esp. 214–215]. Nevertheless, the idea that humans could organize and act to affect the global climate had been broached.

The first prediction of large-scale anthropogenic effects on climate seems to have been by Joseph Fourier in 1824, who indicated that human activity might eventually warm the planet:

> *L'établissement et le progrès des sociétés humaines, l'action des forces naturelles peuvent changer notablement, et dans de vastes contrées, l'état de la surface du sol, la distribution des eaux et les grands mouvemens* [sic] *de l'air. De tels effets sont propres à faire varier, dans [162] le cours de plusieurs siècles, le degré de la chaleur moyenne; car les expressions analytiques comprennent des coefficiens* [sic] *qui se rapportent à l'état superficiel et qui influent beaucoup sur la valeur de la température.* [Fourier 1824, 161–162: cf. 1827, 592][13]

[12] See the citations in Klingaman and Klingaman 2013, 78–83 on sunspots, icebergs, the Great Comet of 1811, and the New Madrid quake of 1811/12, etc.; 240–243 on electric disturbances and icebergs.

[13] See Burgess 1837, 16:

The 'pea-soup' fogs of London, depicted by Dickens in *Bleak House* (1852) and others, demonstrated local effects on the environment from the burning of coal [see Brimblecombe 1982] but were simply considered a local effect. Svante Arrhenius determined a specific source of global warming when in 1896 he computed that increasing carbon dioxide in the atmosphere would warm the planet [see Weart 2008, 5–8].[14] The work of Fourier and Arrhenius was later corrected in the way that science regularly refines its results but the scientific consensus in the early 20th century remained against actual large-scale anthropogenic effects on climate [see Van der Veen 2000].

It was only in the decades after the close of World War II that a new consensus began to emerge, that humans might indeed affect the environment of a large region or even the world. Some of the factors leading to this were the development of nuclear weapons, which opened up the prospect of a world-destroying war (1945); the Great London Smog of December 1952; the recognition that widespread and intensive use of DDT to eradicate mosquitoes and thus malaria had also eradicated birds (1962); the banning of air-burst nuclear-weapons tests in order to prevent further spread of radioactive fallout (1963); the recognition that the widespread use of freons was depleting the ozone layer (1976); and, finally, the growing consensus that anthropogenic carbon dioxide was accelerating global warming.[15]

It was confessedly in this context that Hughes penned his book, *Pan's Travail: Environmental Problems of the Ancient Greeks and Romans* [1994, xii: cf. 2014, vii–viii], the work that opened the modern debate on whether the

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> The establishment and progress of human society, and the action of natural powers, may, in extensive regions, produce remarkable changes in the state of the surface <of the Earth>, the distribution of the waters, and the great movements of the air. Such effects, in the course of some centuries, must produce variations in the mean temperature [for such places]; for the analytical expressions contain coefficients which are related to the state of the surface, and have a great influence on the temperature.

At the very end of the article, Fourier concludes, speaking of the laws of heat-transfer, that 'l'application de ces lois à des effets très-composés exige une longue suite d'observations exactes' [167]: see Burgess 1837, 20 'but the application of these laws to very complicated effects, requires a long course of accurate observations'.

[14] See also Weart 2015 http://www.aip.org/history/climate/co2.htm.

[15] See Weart 2008, 19–154; 2015 http://www.aip.org/history/climate/summary.htm.

ancient Greeks and Romans had largely deforested and otherwise damaged their Mediterranean environment. The reviews were few but moderately positive [see McMahon 1994, Stiebing 1994]. Later responses were more negative [see Grove and Rackham 2001, 18; Brooke 2014, 272–275], yet not so as to prevent a second edition [see Hughes 2014].[16] The case made by Hughes was based almost entirely on literary sources taken *au pied de la lettre*,[17] and drew on a model of human behavior in which some humans lived in sacred harmony with nature.[18] A few years before his second edition appeared, he reiterated his case, arguing from charcoal studies, pollen studies, and computer modelling (as a selection of three out of the 'dauntingly rich' array of material).[19] In the one charcoal study that Hughes 2011 cites, charcoal from pottery kilns shows a shift from a brief first phase of using alder, ash, and elm (flood-plain trees), to a second phase of using various species of deciduous oak, to a third phase of using the evergreen oak species known as holm oak (*Quercus ilex* L.). Hughes interprets this as the succes-

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16 The revisions included mainly:
- (a) the addition of three new chapters:
  - 9 'War and the Environment',
  - 12 'Natural Disasters' (plagues and volcanoes), and
  - 13 'Changing Climates';
- (b) the addition of some pages on the Athenian mines at Laurion [136–142];
- (c) the inversion of the order of chapters 7 and 8 ('Agricultural Decline' and 'Industrial Technology and Environmental Damage'); and
- (d) the citation of a number of new works.

17 As Harris remarks in the book under review, 'Scholars still write books about the ancient environment that are essentially digests of what Greek and Roman writers *said* about the environment', in contrast to studies about what 'the environment in antiquity was actually like' [xx].

18 See Hughes 1994, 24–26 / Hughes 2014, 25–27 on noble savages of the Paleolithic who live in 'balance' with their environment, in that they 'adapt to the local environment and use it without destroying it'; 1994, 32–35 / 2014, 31–35 on urbanism that has divided humans from nature and (in Mesopotamia) 'substituted an attitude of confrontation for the earlier feeling of cooperation'; and 1994, 35–43 / 2014, 35–42 for the claim that, in contrast, the less urban land of Egypt was more stable because their ecology was more stable, their religion viewed the forces of nature as sacred, e.g., in the god Osiris, and their science and technology were 'sacred'.

19 Hughes 2011, 45–46 explicitly cites Grove and Rackham 2001 as the opposition. For the three methods, see 2011, 47–49 on charcoal, 49–52 on pollen, and 52–55 on modelling.

sive destruction of two forests, first that of the flood-plain trees and then that of the deciduous oaks. However, the sequence could also, and perhaps more accurately, be explained as the progressive use of better and better wood for charcoaling (either due to the increased skill or the increased prosperity of the potters): holm oak is favoured for charcoaling, the trees of stage two less so, and those of stage one the very least.[20] The one pollen study canvassed at length by Hughes 2011 concerned a site in the remote Middle Atlas, about 400 km south of Tangier, where the pollen diagram that he reproduces does show a slight and temporary dip in tree pollen, with a corresponding peak in grass pollen, around 400–500 AD, which Hughes takes as confirming the report in Lucan (*ca* AD 60) that Romans harvested exotic woods from Mauritania, and thus in turn confirming deforestation. The computer model that Hughes 2011 cites was a model of vegetation built on the basis of pollen studies and literary sources, which concluded that the Mediterranean was moister around 2000 years ago. It is difficult to see how this model confirms or refutes any hypothesis about forests.

The notion that some human group or other lived in ecological harmony with its environment has never been more than a hypothesis and is often merely an ideology. Neither the ideology (or hypothesis) of a long-lost golden age of simple ease, as in Hesiod, nor the ideology (or hypothesis) of a nasty and primitive brutality escaped by extensive effort, as in Democritus, is a model likely to provide insight into the human condition in the world. Here is a simpler hypothesis: human groups have always exploited their environment up to the limits of their technology and only when deleterious effects become clear and costly do they respond, and then minimally, precisely in order to deal with the immediate problems that actually affect their lives. That is, the human use of the world is anthropocentric.

Moreover, if the world and things within it operate in determinate and knowable ways, the prospect exists that people, possessed of sufficient power, might produce large effects on the world. That is in essence what is intended in the remark attributed to Archimedes: 'Give me a place to stand, and I will

[20] For an explanatory table of all these species (and others), see Grove and Rackham 2001, 52.

move the Earth'[21]—i.e., even the cosmically-centred and motionless Earth must have some finite weight and thus a single person, armed with a long enough lever (and a suitable fulcrum), could in theory displace it. So the question about ancient anthropogenic environmental change is that of the *degree* to which it actually did occur. The best way to determine that is to consult the available evidence or to gather more evidence, always being cautious in interpretation. That is what Grove and Rackham 2001 did[22] and what Harris and the others also do.

Grove and Rackham 2001 is based on empirical evidence about the Mediterranean region, ancient and modern, and considers many aspects. The authors show that the Mediterranean climate system is rare in the world and that it is hardly one climate but is composed of many biomes, few of them 'forest' in the sense of northern European or eastern American woodlands. Thus, they argue, it is easy for people familiar with other kinds of biomes (e.g., scholars from northern Europe and eastern America) to view the current state of the Mediterranean as 'obviously' damaged. A very common type of biome around the Mediterranean is the savanna (herbaceous zone with trees sufficiently sparse to leave an open canopy), which is not a damaged or a deforested region [Grove and Rackham 2001, 190–216]. The plants of any biome of the region have scarcely had time since the end of the last ice age to adapt to the unusual climate and what can be known of prehistoric and ancient distributions of plants in the area does not appear to indicate large changes during antiquity [Grove and Rackham 2001, 151–166]. Many of the plants appear to be adapted to growing through repeated fires [Grove and Rackham 2001, 217–240] and such fire-adaptations actually do occur in other areas, including some eastern American woodlands.[23] Furthermore, some of the effects attributed to deforestation appear to be regular aspects of the

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[21] The remark is attributed to Archimedes in this form by Pappus (*ca* AD 300), *Coll.* 8.11.19 [Hultsch 1878, 1060], whereas Plutarch, *Vita Marc.* 14.7 records Archimedes as writing to Hieron of Syracuse:

> εἰ γῆν εἶχεν ἑτέραν, ἐκίνησεν ἂν ταύτην μεταβὰς εἰς ἐκείνην.
>
> If there were another Earth, he could move this one by going to that one.

[22] I am here also indebted to my brother Rick Keyser for introducing this work to me and for useful discussions about its significance.

[23] As shown, e.g., in Keyser, Brose, Van Lear, and Burtner 1996 and the recent synthesis in Brose 2014.

ecology of the region, appearing to an expected degree and not consistent with degradation, namely, erosion, delta-formation, and karst deserts.[24] The investigations by Grove and Rackham display a sensitive appreciation of the context of their data and of the range of its interpretations. Most of the demonstrable change (damage) has occurred since World War II, especially through intensive alteration—they blame especially the bulldozer and the building of dams. That is, the perception (for example, of Hughes) that the Mediterranean is a damaged ecology is often a retrojection of modern theory upon ancient evidence; i.e., it is essentially presentist.

Let us now consider the specific contributions of Harris and others. The book is divided into five parts with one to three papers in each part:

> Frameworks (two papers),
> Climate (three),
> Woodlands (one),
> Area Reports (three), and
> Finale (one).

In part 1, Malanima ('Energy Consumption in the Roman World') and Veal ('Fuelling Ancient Mediterranean Cities') take on the question of the energy usage of ancient Greco-Roman culture.

Malanima reprises his nearly simultaneous paper Malanima 2014, which is focused on early modern and modern energy usage, and here attempts to build a model of ancient energy sources and usage. He uses that to argue that by *ca* AD 150 the system was no longer able to support the rising population. It should be noted that all his calculations necessarily depend in part upon estimated values for quantities like population and energy consumption: as Harris himself points out [2–3], 'the facts upon which it is based are fragile'. (Moreover, Malanima is too optimistic, or presentist about Heron's 'steam engine' [22], which was no such thing [see Keyser 1992].) Still, it should be noted that for the period up to *ca* AD 150, Malanima's conclusion appears to be that the system was in equilibrium, i.e., that there was no serious degradation of the environment caused by the cutting of trees for firewood and charcoal.

24 Grove and Rackham 2001, 241–305 on erosion, 328–350 delta-formation [328–350], and 312–327 on karst deserts, some expanding and some shrinking.

Veal considers the evidence provided by charcoal fragments, primarily at Pompeii (as a case study), where beech was the primary source (60% to 80%), and other hardwoods (oak, maple, hornbeam) as well as orchard trees were secondary. The data are interpreted as showing a decrease in the use of beech from the second century BC to the first century AD. But as Harris points out [3], this could be due to a shift upwards in the lowest elevation at which beeches will grow [Theophrastus, *Hist. plant.* 5.8.3 with Grove and Rackham 2001, 142]. Indeed, the decrease in the usage of beech can be read not as a sign of ecological stress but as evidence of prosperity: owners of orchards were becoming more prosperous and were thereby enabled to remove old trees in favour of planting new ones—and the wood created by this culling was used to produce charcoal. (I do not mean that this interpretation is right or even better than those of Veal or Harris but rather that it is at least as good and that there is no basis yet known on which to rule in favour of one over the others.)

In part 2, McCormick ('What Climate Science, Ausonius, Nile Floods, Rye, and Thatch Tell Us about the Environmental History of the Roman Empire'), Cook ('Megadroughts, ENSO, and the Invasion of Late-Roman Europe by the Huns and Avars'), and Manning ('The Roman World and Climate') consider the role of climate, stable or otherwise, in the history of the Roman Empire.

McCormick offers three case studies in connecting historical (i.e., textual) evidence with scientific (i.e., material) evidence. First [63–69] is an attempt to establish a precise dendrochronological date for Ausonius' poem *Mosella*, which is generally set by scholars in the range AD 268–375 based on historical references internal to the poem. McCormick connects Ausonius' description of the 'drought-stricken' ('arentem') town Dumnissus with precipitation anomalies reconstructed from tree-ring data to select the year AD 371. However, the year 375 was even drier, according to the reconstruction and McCormick's preference for 371 is based in part upon his evaluation of the strength of the historical arguments in favour of 370–371. Harris calls the result 'amusing' [4] and points out that Ausonius' description need not refer to any specific situation. A third interpretation, beyond that of Harris and McCormick, is possible: one can read the description as setting a contrast between Dumnissus watered only by rain, hence 'dry' and the next place, Tabernae, watered by a perpetual spring—always a subject of praise in Greco-Roman literature.

McCormick's second case study [69–81] concerns the pattern of climate across the Roman Empire, as follows. Roman expansion (*ca*100 BC to *ca* AD 150) occurred during a period of stable climate, which seems to suggest that the stability aided the expansion. Perhaps, however, the polities absorbed by the Romans during this 'expansion' might have seen matters differently—why would a stable climate make any of Numidia, Cyrene, Hispania, Palestine, Syria, Gaul, and Egypt *more* prone to conquest? Instability, then, marks the climate from *ca* AD 150 to *ca* 400, and McCormick suggests that the instability contributed to the impetus for the barbarian invasions—but why would instability strengthen barbarians in this era, when it was *stability* that strengthened the Romans in the prior period? This seems more like ideology ('stable Rome' *versus* 'chaotic barbarians') than analysis. A more useful result would seem to concern the annual Nile floods, which were less fruitful from *ca* AD 160 for almost 150 years (the records break off after AD 299): this must have at least afflicted the Egyptian poor (as noted by Harris [4]).

McCormick's third case study [81–87] involves the evidence for the cultivation of rye in the later Roman Empire, which he suggests may have been motivated in part by decreased early summer rainfall, an adverse growing condition to which rye is more resistant. There is indeed evidence for the shift in rainfall (and for a decrease in temperature, to which rye is also more resistant) but correlation is not causation. We may equally guess that Northern Europeans preferred rye due to its better cold-resistance, its better productivity on northern soils (unlike wheat, it does well on sandy or peaty soils), or even for ideological reasons (for example, 'rye is the grain that we Germans eat'), none of which would necessarily be chronologically limited.

Cook deploys extensive climatological data (with impressive colour maps and graphs) to argue that mega-droughts prompted the migrations and invasions of the Roman Empire by two peoples from the Eurasian steppe, namely, the Huns and the Avars. The climatological evidence for the multi-decadal droughts is well argued, with droughts inferred in *ca* AD 340–380, *ca* 450–490, and *ca* 540–560. However, there was also a multi-decadal drought in *ca* AD 240–290, with invasions of the Roman Empire by the Goths (*ca* 235–270), as well as in *ca* AD 50–100, with no large invasions of the Roman Empire. Moreover, the invasions by the Huns begin after the drought of AD 340–390 and last all through the intervening non-drought era of 390–450. The invasions by the Huns of the Roman Empire might simply be the Roman-recorded phase of a long, steady (and violent) expansion—earlier phases would have involved conquests by the Huns of peoples outside the Roman Empire and, hence, went unrecorded by Roman historians. Again, correlation is not causation but intense droughts may well have affected the lives of Eurasian steppe dwellers in various significant ways. Harris agrees and prefers modest claims [4–5].

Manning contributes a long and careful study on climate change around the Mediterranean from *ca* 300 BC to *ca* AD 800. Manning points out (as Grove and Rackham did) that climate in the Mediterranean is a complex matter [104] and that it is often easier to find and recognize evidence for adverse conditions than for good [106]. As with Cook, the emphasis here is on precipitation rather than temperature [107]. Manning [112–115] evaluates the evidence for the 'Iron Age Cold Period' (as some have named it), a century or two near 765 BC, and concludes that there is no easy way to ascertain causality: the climate shift may have influenced Mediterranean history of that period, or not. Likewise [116–117, n13], he argues that deforestation is hard to demonstrate but that 'there is undeniably a major human element involved' in changes to the environment. Manning [120–135] summarizes what is known about solar activity during his chosen period, supplying eight graphs and a summary chart. He spots correlations between certain periods of less stable solar behaviour and periods of cultural instability (the third century AD and the century around 600). In addition to solar activity, Manning, the director of the Cornell Dendrochronology Lab, offers a summary and analysis of what is known from tree-ring studies [136–145] and finds similar correlations with dry periods. It is, of course, not altogether clear that this is independent confirmation, since the precipitation data derived from the tree-ring data ultimately go back in large measure to solar effects on climate. But it is confirmation that the periods in question were times of ecological stress. Manning [146–153] summarizes what can be gleaned from speleothems, i.e., from oxygen-isotope ratios in speleothems dated by uranium-series radioisotopes. The oxygen-isotope ratios are difficult to interpret because they reflect not only temperature of the precipitation but other fractionation processes [146–147]. What Manning might have also mentioned is that obtaining dates of sufficiently high resolution to be historically useful is difficult due to the long half-lives of the radioisotopes involved (uranium and thorium). Indeed, the available curves do not correlate well with one another [148–149, Figures 16–17] so I doubt that any conclusions should be drawn. Manning's overall conclusion [153–158] is that the period *ca* 300 BC to *ca* AD 500 experienced a relatively stable climate in the Mediterranean. However, Manning [158–163] is careful to point out that the data are only broadly consistent in showing a long 'Roman Warm Period' (within which span are two shorter periods of ecological stress), and, thus, that at any finer level of resolution 'a coherent synthesis is clearly impossible'. He nevertheless attempts a century-by-century synthesis [163–166,167, Figure 21]. Harris [6–7] is skeptical regarding Manning's proposed correlations and favours his caution.

In part 3, 'Woodlands', Harris addresses the issue of 'Defining and Detecting Mediterranean Deforestation, 800 BCE to 700 CE' [173–194]. Harris argues for a limited deforestation in the environs of larger cities and that in many places there was effective forest management. As Grove and Rackham did, Harris rejects the simple dichotomy of 'forest' *versus* 'cleared land' [175]. Harris proceeds carefully through the several stages of his argument:

'Definition' [175]
'Destructive Forces' [176–177]
'Wood Shortages' [177–183]
  textual evidence
'The Palynological Evidence' [183–186]
  such evidence is spotty and complex
'Sedimentation and Erosion' [186–187]
'A Demographic Approach' [187–189]
  he is cautious about recent work
'Woodland Management' [189–192] and
  again, textual evidence that Roman landowners managed their forests
'The Impact of Climate Change' [192–193].
  he argues that the current data are too spotty to be conclusive

Although he has considered these many aspects rather briefly, his conclusion is sensible: 'No extreme hypothesis about deforestation seems well founded, and there is no reason to believe in a generalized crisis' [193]. He advances four nuanced conclusions that may be summarized by saying that deforestation in the strong sense was episodic and localized.

In part 4, 'Area Reports', Kouki ('...The Example of Southern Jordan'), Ermolli, Romano, and Ruello ('...Neapolis and Elea-Velia'), and Keenan-Jones ('...Roman Central-Southern Italy') provide studies of environmental matters in three restricted zones. The paper by Veal on charcoal at Pompeii (part 1) might well have been placed here.

Kouki addresses 'Problems of Relating Environmental History and Human Settlement in the Classical and Late Classical Periods: The Example of Southern Jordan' [197–211], in particular around Petra. Harris summarizes her conclusion as being 'that patterns of settlement there do not conform to what the climate data might lead one to expect'—since the climatic variable studied by Kouki, precipitation, is not correlated with population density.

Ermolli, Romano, and Ruello study 'Human-Environment Interactions in the Southern Tyrrhenian Coastal Area: Hypotheses from Neapolis and Elea-Velia' [213–231] and attempt to reconstruct the landscape at different eras around the two cities that they study using pollen and soil studies. For Naples, they establish three levels, 'first millennium BC' (i.e., prior to significant urbanization), 'Greco-Roman Period', and 'Late Ancient' (the ancient port silted up *ca* AD 500). The pollen shows a wide variety of plants including a deciduous oak forest (first century BC to second century AD) as well as walnut trees, and especially plants of the cabbage family (but absent during the third century AD, when chestnut, olive, pine, and holm oak increased). The same three levels for Velia show large deposits of eroded soil at various times, especially in the third century AD, which the authors attribute to 'declining land use management'.

Keenan-Jones considers 'Large-Scale Water Management Projects in Roman Central-Southern Italy' [233–256]. He opens by arguing (on the basis of various other studies) that, in the area he is studying, the period *ca* 300 BC to *ca* AD 300 was marked by lower rainfall and yet greater flooding, and that therefore the flooding must be due to greater runoff caused by deforestation [234–239]. He provides two case studies, one on the *Aqua Augusta* [240–246] and the other on flood control in the Tiber [246–253]. The aqueduct was built by Augustus in 30–20 BC and supplied the towns of the Bay of Naples (from a source around 50 km east of Vesuvius); and its flow-rate cautiously estimated by Keenan-Jones would, he suggests, have had serious effects on the water supply of the source region. Tacitus, *Ann.* 1.79 records the plan proposed under Tiberius (AD 15) to control flooding on the Tiber by rerouting the Clanis (a northwestern tributary on the right bank of the Tiber) into the Arno and by dispersing the river Nar (a northeastern tributary on the left bank of the Tiber) into irrigation leats. Keenan-Jones analyzes the arguments against the plan recorded by Tacitus but concedes that we can no more draw conclusions about why it was abandoned than could Tacitus, who closes with an *aporia*, ‘either the pleas of the <cities affected> or the difficulty of the works or superstition <about sacred rivers> prevailed’. From the two studies, Keenan-Jones concludes that elites were willing to propose and carry large-scale water-management projects but that they kept in mind the adverse impacts on their clients [253–256].

The ‘Finale’ (part 5) is provided by Wilson’s ‘The Mediterranean Environment in Ancient History: Perspectives and Prospects’ [259–276]. He presents a cautiously positive summary of the papers within the book [259–273], and closes with a survey of ‘Future Directions’ [273–276], for example, to assess more carefully the relative effects of long-term change and sudden change such as that caused by volcanoes and earthquakes.

The book is thus composed of two kinds of papers: small positive contributions based on careful work in a carefully defined scope and attempts at larger syntheses of the sort that can be tested (Malanima in part 1, and Harris in part 3). This would be a summary of the positive contributions, each of which is one more piece of the giant puzzle: Veal’s valuable data on the woods used to produce charcoal at Pompeii from the third century BC to the first century AD; McCormick’s valuable analysis showing that the Nile floods were less fruitful *ca* AD 160–300; Cook’s establishment that there were five multidecadal droughts during the first six centuries of the era; Manning’s careful synthesis of climate data around the Mediterranean for 300 BC to AD 800; Harris’ re-evaluation of the textual evidence for environmental change and management; Kouki’s analysis of settlement and precipitation patterns at Petra; the measurement by Ermolli, Romano, and Ruello of silt accumulation in late antiquity at Naples and Velia; and Keenan-Jones on the attitudes of elites in the early Roman Empire towards large-scale water management projects.

There is an extensive and valuable bibliography [277–325] and a good index [327–332]. Misprints are few: for example, ‘points out some of the reasons why such [are] hypotheses may be problematic’ [5]; ‘Figure 2’ wrongly for ‘Figure 1’ on 23n39; and ‘ice carrots’ for ‘ice cores’, presumably an over-literal translation of the Italian ‘carote di ghiaccio’ in 24–25.

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Astrology and Magic from the Medieval Latin and Islamic World to Renaissance Europe: Theories and Approaches by Paola Zambelli

Variorum Collected Studies Series CS997. Farnham, UK/Burlington, VT: Ashgate, 2012. Pp. 310. ISBN 978–1–4094–2514–4. Cloth $154.95

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Arranged marriages do work. In her introduction to this volume, Paola Zambelli recounts how, as an 18-year-old undergraduate student, she arrived at the Florentine offices of Delio Cantimori and Eugenio Garin in 1955 with the intention of working on Karl Marx's ideas on the French Revolution. In what strikes us now as a stunning display of professorial sovereignty, Garin suggested that Zambelli work on Cornelius Agrippa and magic instead. And so it began. Seventeen books and 126 papers later, Zambelli is one of the most important intellectual historians of pre-modern magic and astrology alive.

This Variorum collection offers a selection of Zambelli's papers on four different themes:

- theories about magic and astrology in medieval and Renaissance intellectual history,
- the role of astrologers in Renaissance society,
- the pan-European debate of the early 16th century on an imminent universal flood produced by astrological great conjunctions, and
- theories about magic in 20th-century scholarship.

The collection is far from complete or even representative of Zambelli's output. Several classic papers are not present here (e.g., Zambelli's work on 'Magic and Radical Reformation in Agrippa of Nettesheim' or her mesmerizing essay on Alessandro Achillini's theories of magic). As a bonus, there is an up-to-date bibliography at the end of this volume.

Ashgate's Variorum series reproduces existing papers, warts and all. There is no attempt at internal pagination, at homogeneous layouts, or even at correcting erroneous spelling, grammar, or syntax in the original papers. Nevertheless, four of the 10 papers have been newly translated into English

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by Lydia Cochrane. This may be related to Zambelli's overt concerns about the directions taken by American scholarship in Renaissance studies, which she finds insufficiently conversant with (the history of) philosophy [96]. Unsurprisingly, then, many of Zambelli's papers approach magic and astrology as the practice of philosophical ideas. In this area, she is at her best when calling attention to the multiplicity of relevant philosophical traditions: not only Neoplatonism and Aristotelianism but also the astrologizing Stoicism of al-Kindi or Albumasar and the radical philosophies of Avicenna or Averroes. Zambelli is also at her most insistent when marking off the difference between religion and magic on the one hand, and science on the other, while pointing out the ways in which magic could lead from one to the other. This shapes a profound interest in Pietro Pomponazzi (1462–1525), whose *De incantationibus* dominates the background to Zambelli's paper on Arabic, scholastic, and Renaissance theories of the prophetic imagination; and which is also the explicit focus of a paper critically revisiting Pomponazzi's recruitment as the progenitor of later traditions of libertinism.

The prehistory of modern freethinking looms large in the second section of this collection, which begins by calling attention to the importance of premodern astrological thought. Zambelli specifically singles out the Arabic theory of cyclical conjunctions as the crucial source of modern, secularizing philosophies of history which could ground a difference between social and cosmological time. The actual importance and impact of this theory in early modern European culture is demonstrated in section 3, which reprints Zambelli's two contributions to a volume which she edited on the subject in 1984. Although far less encompassing and detailed than some of her other work on early modern conjunctionalism, these reprints do provide readers with a useful summary of Zambelli's main insights on the subject. Especially interesting is her attempt to make the history of astrology more conversant with Karl Löwith's *Meaning in History* (1949) by way of the broader history of early modern and 19th-century philosophy.

Moving away from the relative thematic unity of the first three sections, the last one offers three studies in theory which treat the history of science, the *histoire des mentalités*, and the history of philosophy. Once again, however, important internal convergences and unities of purpose emerge. Zambelli explores the influence of Lucien Lévy-Bruhl on Alexandre Koyré, provides a synthetic analysis of the work of her colleague Carlo Ginzburg, and reveals the writing of a history of philosophy in the post-Hegelian age of social history. With its critique of the Renaissance as a 'rebirth of classical culture' and an identification of the history of philosophy with philosophy, this section considerably deepens our understanding of the important intellectual and political trajectory that Zambelli has carved out, regardless of what happened in 1955.

L'anonyme de Londres (P. Lit. Lond. 165, Brit. Libr. Inv. 137). Édition et traduction d'un papyrus médical grec du Ier siècle by Antonio Ricciardetto

Collection Papyrologica Leodiensia 4. Liège, BE: Presses Universitaires de Liège. 2014. Pp. lxix + 155. ISBN 978–2–87562–047–7. Paper €55.00

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Important information on medical ideas that were current in the fifth and fourth centuries BC is to be found in a papyrus dating from the first century AD, conventionally known as Anonymus Londinensis. Despite this late date, the ideas seem to be derived, at least to some extent, from a history of medicine compiled by Aristotle's pupil Menon. The clumsy name arises from its location in London (previously in the British Museum, now in the British Library) and its anonymous character. The discovery of the papyrus was announced at the end of the 19th century; it was edited soon afterwards and an accessible, if not entirely reliable, commentary followed [see Kenyon 1892, Diels 1893, Jones 1947]. The compilation, devoted in large part to a summary of different views of the aetiology of disease, addresses the views of some 25 named doctors. Those named include Hippocrates as well as many figures previously unknown and several—including Plato—known to us not as medical but as philosophical writers, among them many Pythagorean thinkers such as Philolaus. The Anonymus Londinensis is the longest Greek medical papyrus known to have survived. As such, it has for many decades interested medical historians and papyrologists alike. Foremost among those has been Daniela Manetti, whose Teubner edition [see Manetti 2011], reviewed at length by Ricciardetto for *Aestimatio* [2013], was preceded over a period of some 20 years by a long series of distinguished scholarly contributions.

In a preface by Marie-Hélène Marganne, the important work of Manetti is fully acknowledged but the independent contribution of Ricciardetto is also rightly stressed. There are many differences in detail, especially in restoration of the difficult fragmentary text. In addition, Ricciardetto more fully contextualizes the complete content of the papyrus, both recto and

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Aestimatio 11 (2014) 337–338

verso, with particular attention to a copy of a letter of Marcus Antonius to the *koinon* of Asiatic Greeks.

Ricciardetto provides a long and detailed introduction [xiii–lxviii], first commenting on the circumstances surrounding the discovery and acquisition of the papyrus, then dealing in detail with palaeographical questions (such as the use of abbreviations and punctuation) and with language and orthography. The content is closely analyzed and the doxography addressed with careful attention to detail. A digest is offered of scholarship on the papyrus from the time of its discovery to the present day; and finally a bibliography is included, covering all aspects of the Anonymus Londinensis.

The text and translation are then presented: Greek text with French translation on the facing page [1–39]. There is at this point no critical apparatus. Instead, ensuing pages [41–105] are devoted to detailed 'notes critiques et grammaticales'. This layout greatly enhances the attractive appearance of the volume and the clarity of its exposition. A second bibliography [107–118] comprehensively covers primary sources (ancient texts) and secondary material (modern scholarship). There is an index of proper names, of Greek words and (in French) of subject matter. A fine set of colored plates completes the volume.

Ricciardetto has made a substantial contribution to modern understanding of Anonymus Londinensis, a papyrus text as challenging as it is important.

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Printing a Mediterranean World: Florence, Constantinople, and the Renaissance of Geography by Sean Roberts

Cambridge, MA/London, UK: Harvard University Press, 2013. Pp. xvi + 293. ISBN 978–0–674–06648–9. Cloth $49.95

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Francesco Berlinghieri's Italian vernacular verse edition of Ptolemy's *Geography* (1482) is at the heart of *Printing a Mediterranean World*. Roberts uses this single text to tease out a variety of conclusions about late 15th-century diplomacy, geography, print, and the Renaissance. He takes an interdisciplinary approach to the study of the text and its wider relevance, looking at the poetic words and format, illustrations, printing and readership; and provides a masterly demonstration of the way in which the study of a single text can give lessons about a whole epoch.

The first chapter of the book is a discussion of two presentation copies of Berlinghieri's *Geography* given to the Ottoman Sultan Bayezid II and his half brother Cem. Here Roberts examines the value of the text for Ottoman and Florentine societies, pointing out that 'Ptolemy's *Geography* provided a salient point of contact to an admired past perceived as common to both Florentines and Ottomans' [32]. Roberts downplays the revolutionary nature of the rediscovery of Ptolemy's *Geography* in the Renaissance, which has dominated discussions about the text until recent years, but admits that the *Geography* 'acted as a catalyst for new kinds of maps and texts' [41] and sets out to examine why this was the case. He argues that the production of new editions of the *Geography* was a reinvention rather than a rediscovery of the classical past. The ensuing text looks at various aspects of this reinvention.

In the following chapter, 'The Rebirth of Geography', Roberts examines how Berlinghieri's own life and intellectual environment shaped the way in which he reinvented the past in his version of the *Geography*, and how he drew on his cartographical, mathematical, and poetical skills in its creation. Roberts also looks at the way in which the process of creation in turn influenced Berlinghieri, since 'emulation of Ptolemy's project was a significant

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part of Berlinghieri's self-fashioning as a geographer' [51]. This mutual interaction, though fundamental to the book, is not the most successful aspect. The self-fashioning at times seems slightly incidental. Nevertheless, the examination of the relationship between Berlinghieri's narrative and Ptolemy's rather dull original is extremely interesting—particularly, the former's use of more up-to-date sources to create a more politically and historically relevant geography, and his syncretic use of Christian and pagan material.

Chapter 3 studies the actual printed book, looking at the limitations imposed by the state of the technology for printing and etching, and the apparent shoddiness of some of the extant copies of the printed book. Here Roberts' close study of different copies of the same edition of the work provides valuable insights that have far wider implications than for Berlinghieri's *Geography* alone. He argues that the poor quality of some of the maps may have been the result of a limited knowledge of how to correct etchings, for instance, but also that these defects were overcome in some copies of the book. Prestige copies printed with the intention of gift-giving (such as those for Cem and Bayezid) were augmented by hand-coloring and illumination which obliterated defects in depiction and etching. The section on painting maps is fascinating, as is Roberts' use of copies of the *Geography* to show the continuing relationship between the manuscript tradition and early printed books in this period. From there, Roberts moves on to examine how the use of prestige books in gift-giving could be part of community formation and shows how the subject of geography was peculiarly suited to this. Roberts demonstrates how Berlinghieri consciously included various European nobles and royalty in his work, thereby forming a community and creating a pool of interested influential people who might buy or be given prestige copies of the work. In the creation of these hand-illuminated copies, Berlinghieri blurred the distinction between a book published for an open market and a book created for specific individuals. Such individuals, as Roberts demonstrates, included Cem and Bayezid who must always have been among the intended recipients. Roberts gives a useful case study of how these Ottoman readers were included and must have read the book. This chapter is the meat of the book and is full of detailed, thoughtful information, providing the reader with many ideas about how to approach this and other works.

The final chapter links to the rest but in some ways stands alone. Roberts takes on the categories of Turcophilia, Turcophobia, and toleration; and shows their limitations by relating them to Berlinghieri's work. He also examines some of Berlinghieri's intellectual community in this context, particularly discussing the role of Marsilio Ficino and his Neoplatonist ideas. This chapter focuses on the Florentine relationship with the Ottomans, the Ottoman reception of a work that equated Christianity with modernity, and questions of diplomatic interchange. Roberts once again introduces bigger questions by relating them to the case study of Berlinghieri and his book.

As a reader, I would have liked a little more detail on the actual geographical content of the book itself and on the kind of knowledge that it transmitted. I also found unhelpful the placement of the images in a single group separating the final chapter (which already had a slightly different theme) from the rest of the book. They would have been easier to use and contributed more to the flow of the thesis had they been better interleaved. This is nitpicking, however. Overall Roberts has produced a very readable and interesting contribution to early modern scholarship which, by focusing on Berlinghieri's *Geography* with its literary, cartographic, classical, and diplomatic content, has necessarily involved an interdisciplinary approach and ought to appeal to a wide variety of readers.

Jamblique. In Nicomachi arithmeticam. *Introduction, texte critique, traduction française et notes de commentaire* by Nicolas Vinel

Mathematica Graeca Antiqua 3. Pisa/Rome: Fabrizio Serra Editore, 2014. Pp. 356. ISBN 978–88–6227–616–0. Paper €110.00

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The relationship between Plato and ancient Pythagoreanism remains a mystery. Yet, one can be quite sure that a strong interlacement of the Platonic tradition and a conscious Pythagorean inspiration (at least in attention to numbers and mathematics, and their applications in physics and metaphysics) characterized not only the Old Academy[1] but also Middle Platonism and Neoplatonism. The excellent book by Nicolas Vinel contributes to our knowledge of this fact by providing scholars with a very useful and careful edition of a (usually disregarded) work by Iamblichus, a Neoplatonist deeply interested in the Pythagorean tradition.

In the Old Academy, there was established a mathematical tradition that was grounded mainly in an arithmo-geometrical perspective. Although some of its elements were probably recovered to some extent and refashioned by Euclid,[2] the arithmo-geometrical core of this tradition was somehow left aside by the 'major' stream of Greek mathematics. Nonetheless, one can easily find its re-appearance in the Imperial age: for example, in the extensive work by Moderatus of Gades in the early first century AD, who gathered the opinions (ἀρέσκοντα) of 'Pythagoreans' in 10 books, of which only a

[1] See Burkert 1972 which demonstrates how far post-Platonic accounts on Pythagoreanism are influenced by Platonic and Academic elements. A different approach to the history of Pythagoreanism is now proposed by Phillip Horky in several contributions: see especially Horky 2013. On the history of Pre-Platonic Pythagoreanism, see also Centrone 1995 and Huffman 2014. For a different perspective, see Zhmud 2012.

[2] Bernard Vitrac and Fabio Acerbi have in many works indicated the need to go beyond the traditional idea that Euclid's *Elements* are only a *summa* of preceding discoveries.

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Aestimatio 11 (2014) 342–353

few fragments remain, and in the writings by Nicomachus of Gerasa in the second.[3] It has been demonstrated that the usual term for these authors, 'Neopythagoreans', is misleading since they do in fact work within the Platonic tradition [see Centrone 2000]. Nonetheless, they show a specific interest in mathematics and appeal explicitly to the Pythagorean tradition. This stream, moreover, provides an effective example of a widespread tendency of Imperial Platonism that consists in associating Plato with the Pythagorean tradition from a more general point of view. This is the case, for instance, with Numenius of Apamea [see des Places 1973, fr. 24] and Plutarch's *De E* ch. 7–16 (though this is not Plutarch's own position, at least at the time that he wrote this work). Such a tendency of Imperial Platonism remained fundamental in the Platonic tradition, albeit to different extents. Thus, while Porphyry was generally interested in Pythagoreanism—he wrote a *Life of Pythagoras*—Iamblichus of Chalcis shows a peculiarly strong commitment to it.[4] Indeed, besides his more traditional writings such as his commentaries on various Platonic dialogues,[5] Iamblichus engaged in the ambitious project On the Pythagorean School (Περὶ τῆς Πυθαγορικῆς αἱρέσεως), which aimed to set out in 10 books an introduction to the whole of Pythagorean doctrine.[6]

3 In the same tradition, one should probably consider also other Platonists, the best known of whom is Thrasyllus [see Tarrant 1994]. The case of Theon of Smyrna is different, since his *Expositio rerum mathematicarum ad legendum Platonem utilium* must be considered rather as a technical exegesis of the mathematical sections of Plato's psychogony. Interesting papers on the relationship between Platonism and Pythagoreanism in the Imperial age are collected in Bonazzi, Lévy, and Steel 2007. I emphasize that it is necessary to suppose that a 'Pythagorean' tendency was somehow preserved also in the Hellenistic age: this is almost the unique historical condition under which one can understand Moderatus' work in the first century AD. It is worth noting, finally, that one among the 'founders' of post-Hellenistic Platonism, Eudorus of Alexandria, had a strong interest in the Pythagorean tradition, which he 'used' in order to sustain his new Platonic perspective: the so-called *Pseudo-Pythagorica* [see Centrone 2014] were probably produced, at least in the majority of cases, in the context of his school [see Bonazzi 2013].

4 On the Neoplatonist interest in Pythagoreanism, see Macris 2014 and O'Meara 2014. O'Meara, especially, has contributed studies providing an authoritative basis for deeper inquiry.

5 The fragments are collected in Dillon 1973.

6 For a comprehensive account of Iamblichus, see Dillon 1987. Vinel supplies a brief sketch of him and his philosophical project on pp. 11–13.

His *In Nicomachi arithmeticam* belongs to this latter project, being the fourth treatise of the series.

This outstanding work by Vinel, then, has the great merit of making available to scholars a new suitably critical edition of the Greek text, a good translation, and a very careful commentary. The book consists of an introduction [11–66] dealing with general interpretative problems in this text and focusing on its most important aspects; a French translation of a new critical text in Greek that is based on a complete collation of extant manuscripts and offers a useful *apparatus* of parallel passages along with an *apparatus criticus* [68–197]; a series of notes of commentary [199–265]; and an impressive set of indices, which make the book even more useful [267–344]. At the same time, Vinel aims to make clear the originality of Iamblichus' *In Nic. arith.* by demonstrating that it is not a commentary on Nicomachus' *Introductio arithmetica* (the fundamental treatise of Pythagorean-Platonic arithmetic in the Imperial age) but a work taking Nicomachus' writings (both transmitted and now lost) as a point of departure in order to develop a new account of Pythagorean and Platonic arithmetic.[7] Apart from some minor aspects, which I will discuss in due course, this goal is well achieved.[8]

The first among Vinel's tasks, then, is to overcome the commonplace notion that has Iamblichus' *In Nic. arith.* as only a sort of rearrangement of Nicomachus' *Intro. arith.*[9] He begins by focusing on the title and the de-

7 This point is convincingly achieved by referring to Iamblichus' writing practice (*usus scribendi*) [14–15]: Iamblichus says that he will appeal to Nicomachus' τέχνη, and at the same time he uses « τέχνη » to indicate a general field of interest or study [see also 200n10]. I am less inclined to agree with Vinel's translation of « τέχνη » as 'science', since this somehow leaves aside the procedural aspects that are implied by « τέχνη ».

8 The idea that Nicomachus is the most important reference for Iamblichus, who, however, tries to supplement his doctrines, raises the issue of Iamblichus' relation to the Euclidean tradition. Vinel emphasizes (quite briefly, though: see 23 and the notes at 216 ff.) that Iamblichus criticizes Euclid. Here it would have been helpful to explore whether Iamblichus deliberately obscures other interpretations of topics dealt with by Nicomachus (e.g., Geminus' account of the classification of sciences, which was to some extent known in Neoplatonism) or whether he was not acquainted with them.

9 Vinel offers a valuable survey of this prejudice against Iamblichus' originality [13] but also emphasizes that scholars still could not avoid noting, albeit in a non-systematic and inconsistent way, some originality in Iamblichus' work.

clared aim of the work [14–15]. As a matter of fact, Iamblichus never says that he is producing a commentary on Nicomachus but only that he will offer an introduction (εἰcαγωγή)[10] to arithmetic by taking Nicomachus' writings (and not only his *Intro. arith.*) as a starting point. Nevertheless, Vinel demonstrates that the title offered by the manuscript tradition «Περὶ τῆc Νικομάχου ἀριθμητικῆc εἰcαγωγῆc» should not be accepted. He proposes three alternatives, the last of which («Περὶ τῆc Νικομάχου ἀριθμητικῆc») is (quite reasonably) adopted in the critical edition [15]. Accordingly, this conclusion is confirmed by means of a survey of Nicomachean doctrines and other sources in Iamblichus' text [19–23]: Nicomachus, it turns out, is a sort of *fil rouge* for discussion that Iamblichus sometimes follows and sometimes leaves behind as he introduces new doctrines and terms. This would be typical of Iamblichus' way of dealing with sources. Vinel compares this approach to Iamblichus' treatment of Porphyry's commentaries [20].[11]

There is one point that I should like to stress. As Vinel correctly notes [201n13], Iamblichus explicitly criticizes innovation (καινοτομία) and prefers to appeal to a well–established tradition, i.e., to that of Pythagoras, which he takes to have been advanced by Nicomachus. Vinel then indicates that this would seem to be inconsistent with Iamblichus' own innovations but leaves the matter without further discussion. However, in my view, this should be a

10 If this is the case, however, it would have been helpful to expand on the relationship between this work and the *prolegomena* to mathematical works [see Mansfeld 1998] and to the literary genre to which *In Nic. arith.* belongs.

11 While the analysis of contents and titles of the work is effective, the latter point concerning Iamblichus' attitude towards his sources is a bit controversial. First, although a passage of Simplicius' *In Arist. cat.* [Heiberg 1894, 2.9–13] seems to work as a confirmation, one cannot establish a strict parallel between Iamblichus' methods, since the context and form of the fragments of Iamblichus' commentaries are usually puzzling. Moreover, major sources of these fragments use their own sources in turn ambiguously. This can be seen, for example, in Proclus' use of Porphyry's and Iamblichus' commentaries on the *Timaeus*: see Petrucci 2014, 339–341 for a survey. Second, if one states, as Vinel does correctly, I expect, that Iamblichus uses several different texts by Nicomachus, it may be unwarranted (at least in principle) to make a claim for Iamblichus' originality when there is no explicit criticism of Nicomachus. After all, Iamblichus could have collated different sections and doctrines from either Nicomachus' transmitted or lost writings. Thus, while Vinel is right in emphasizing a certain originality in Iamblichus' account, this point should probably not be pushed too far.

central issue: given that these statements *must be* reconciled, it is necessary to discover the ideological model of authority that allows Iamblichus to expand on and also contradict Nicomachus, without considering any of this an innovation. The solution could be set out in numerous ways. Perhaps Nicomachus was to be seen as a means through which one can have access to Pythagorean arithmetic lore: in this case, Iamblichus would follow Nicomachus unless, in his opinion, the authentic (or a suitable) Pythagorean doctrine is different than that proposed in Nicomachus' writings. At any rate, providing a solution to this problem, even tentatively, would be worth doing, since this would shed light on the ideology of Iamblichus' project and on the role of sources in it. Nonetheless, this criticism does not impact Vinel's argument on the general status of the *In Nic. arith*.

In his introduction, Vinel addresses three problems that highlight Iamblichus' work on sources and his autonomous contribution to arithmetic tradition:

(1) the production of a theory of magic squares [23–35],
(2) a new way of conceiving the relationship between point and line with respect to that between unity and number [35–41], and
(3) the thematization of the arithmetical concept of zero [41–53].

A magic square is a square divided into rows and columns where consecutive numbers are placed into the cells so that the sums of the numbers in the rows, columns, and diagonals are equal. Vinel's aim is to demonstrate that a theory related to these squares was a sort of heritage of ancient Pythagoreanism and that this heritage has traces in Theon's *Expositio*, in some archaeological artifacts, and above all in Iamblichus' *In Nic. arith*. The core of the demonstration, focusing on Iamblichus, is achieved on pp. 26–31. Here Vinel indicates effectively that in *In Nic. arith.* esp. 2.33–37, 2.51–52, one can find all the basic arithmetical elements needed for a theory of magic squares.

I remain sceptical on two points, however. First, Vinel's analysis of a passage from Theon's *Expositio* [Hiller 1878, 101.14–20] fails to take into account its context. In a section devoted to the arithmological properties of the numbers of the decad, Theon emphasizes that 5 is the arithmetical middle term between couples of 'opposite' numbers in the decad (i.e., 1 and 9, 2 and 8, 3 and 7, 4 and 6). Vinel's quite speculative argument, which appeals to some controversial elements of the passage, suggests that an ancient theory of magic squares could be the basis of this statement. However, this property of 5 is a commonplace in arithmological works [see Heiberg 1901, 9.23–10.4

(Anatolius); Iamblichus, *Theol. arith.* / De Falco 1922, 31.12–16; Wünsch 1898, 2:30–31 (Lydus)] and nothing in Theon's remarks suggests that he views this property as something more than what it is meant to be: evidence for a 'structural' link between the number 5 and the arithmetic mean.

Second, and more importantly, I doubt that the theory at issue can be traced back to ancient Pythagoreanism as Vinel suggests.[12] In order to obtain the desired conclusion, Vinel emphasizes that Iamblichus describes 5 using epithets which have parallels in ancient literature or which seem to have ancient origins. However, it is easy to imagine a context that admits 'ancient-fashioned' theories and terminologies, while also reproducing Homeric or archaic language, for example, in the period between the Hellenistic age and the very early Imperial age, when different kinds of a Pythagorean revival took place.[13] In other words, the fact that a notion is treated with a language which appears to be archaic does not prove that the notion has ancient origins.[14]

12 Vinel's notes on the problem are very interesting. However, a part of the argument for antiquity is misleading in that it draws on a citation of Philolaus in Iamblichus' text [2.51 = Huffman 1993, fr. 9] as additional proof that the theme of the magic square was related to justice in ancient Pythagoreanism. Vinel [215n71] says that there is a consensus that this fragment is authentic and then quotes Huffman's commentary:

> The use of the distinction ...seems perfectly plausible for Philolaus in the second half of the fifth century...and Burkert accordingly regards F9 as authentic. [Huffman 1993, 415]

But note the following sentence in Huffman's commentary, which Vinel does not quote:

> However, the idea that the properties...would fit well in a hymn to number such as we find in the spurious F11. When dealing with such a brief statement it is impossible to be confident of its authenticity. Moreover, such a phrase, when considered independently of any context, tells us virtually nothing about Philolaus' philosophy.

Indeed, fr. 9 is listed by Huffman among the spurious and doubtful fragments.

13 See, for example, the fragment of a poem by Alexander of Ephesus transmitted by Theon of Smyrna [Hiller 1878, 139.1–10] or the corpus of the *Pseudo-pythagorica dorica*.

14 The only testimony that one might consider telling with respect to the antiquity of the doctrine is Aristotle, *De cael.* 293b1–4 with Rose 1863, fr. 204, which indicate that Pythagoreans called the center of the universe Διὸς φυλακή and Ζηνὸς πύργος [25]. The latter term is used also in Iamblichus' *Theol. arith.* as an epithet of 5. The

The second problem that Vinel focuses on is the way in which Iamblichus refashions a commonplace in the arithmetic tradition: that is, the idea that a line should not be considered as composed by points. To address this difficulty, a specific notion was introduced perhaps in the Old Academy [see Tarán 1981, 362–363], namely, that the line is produced by a flowing (ῥύσις) of the point. Now, Iamblichus accepts this notion, which became quite widespread. But, as Vinel effectively demonstrates,[15] he is the only author to produce a direct and rigorous demonstration denying that a point is a part of a line. Moreover, he refashions traditional terminology and modifies the standard approach to this problem by avoiding the notion of 'nothing' («οὐδέν»), which plays a fundamental role in his own account (as we shall see immediately). Vinel's analysis is very valuable: he considers carefully the traditional approach to the problem and then clarifies Iamblichus' argument, which is posited in a very compact and puzzling way [4.4–6]. He then shows how Iamblichus' argument can be regarded as both conclusive and obscure (as ancient sources also said about Iamblichus' style).

The last problem that Vinel discusses in his introduction is probably the most interesting [211–215]. In Iamblichus' *In Nic. arith.*, it is possible to detect a first (and subsequently obscured) arithmetic thematization of the notion of zero. After demonstrating that the pre-Iamblichean hints at this do not presuppose any actual arithmetic theory [42–44], Vinel proposes that Iamblichus, by taking a passage from Nicomachus [*Intro. arith.* 1.8.12] as a starting point, independently developed the first arithmetic doctrine of zero. Indeed, Vinel applies Nicomachus' remark, according to which each number is equal to the half-sum of the immediately preceding and the following numbers in order to establish that *number* 1 must be the half-sum of two 'numbers'—although both 1 and 0 can be defined as numbers only in an improper sense [2.45]—namely, 2 and τὸ οὐδέν [2.31–33]. The fact that Iamblichus' 'discovery' is not incidental is confirmed by his appeal to the same notion in subsequent passages [2.38, 2.42–47], which suggests that he really does consider zero to be an operative, arithmetic entity preceding 1 (the unit). Vinel also argues that such a fundamental discovery was totally lost

passages from Aristotle, however, only demonstrate that «Ζηνὸς πύργος» was an epithet used *in a certain context* by Pythagoreans. Its use in a new context can be ascribed to some intermediate text.

[15] See also the very interesting notes 156–161 on pp. 235–237.

in the tradition because Neoplatonists such as Proclus (but also Nicomachus' commentators, Asclepius and Philoponus) preferred to follow Nicomachus as their authority in arithmetic.[16] For this, Vinel's analysis is of great value.

At the same time, however, I am not inclined to agree with Vinel's attempt to associate the discovery of zero (τὸ οὐδέν) with an item in Iamblichus' ontology: that is, with the idea of a totally unqualified entity beyond the One.[17] On the one hand, there is no real hint at the ontological relevance of the doctrine of zero in the *In Nic. arith*. As Vinel emphasizes, Iamblichus' discussion is deeply rooted in an *arithmetic* perspective. Moreover, since there is necessarily an ontological difference between numbers and principles—e.g., the One as principle is not one as a number *sui generis*—there is no compelling link between the ontological thematization of an ἄρρητον and absolutely transcendent principle and that of the arithmetical notion of zero. In other terms, the discovery and elaboration of the notion of zero can be totally understood without going beyond arithmetic theory. In addition, even though one might wish to establish a link between Iamblichus' 'discovery' of zero and his ontology (as Vinel does), it would be important to push the analysis farther to answer the following philosophical questions: What are the implications of such a strict connection between arithmetic and ontological features? To what extent does this connection hold? And, above all, does Iamblichus ascribe a sort of 'heuristic' priority to arithmetic with respect to ontology? Or is this priority grounded in a certain ontological status of mathematical entities?

Such a philosophical discussion is missing (at least to some extent) in Vinel's book—justifiably, perhaps. These aspects of Iamblichus' thought may belong to a different 'part' of his production and legitimately remain outside the goals of an analysis of *In Nic. arith*. However, given that Vinel wishes to involve ontology in his analysis, he should address to some extent the more general problem concerning the actual status of numbers and their principles (such as the One) and their epistemological function in a wider Platonic perspective, i.e., whether the alleged projection of arithmetic properties on ontological principles produces a philosophically consistent account. This

16 Vinel correctly emphasizes this point, which is important from the point of view of the history of the Platonic approach to mathematics: his analysis focuses on Nicomachus' authority in the Platonic tradition [see Marinus, *Vita Procli* 28].

17 See Damascius, *De princ.* / Ruelle 1889, 1.5.19–22; 2.1.5–8.

does not negate, of course, the great value of Vinel's technical analysis and the utility of his balanced references to other Iamblichean writings.

Vinel's edition of the text itself is one among the most important achievements of his book. Iamblichus' *In Nic. arith.* was among those texts—such as Nicomachus' *Introductio*, Theon's *Expositio* or Iamblichus' *De communi mathematica scientia*—that were first published during the 18th century in the Bibliotheca Teubneriana and require a new critical edition grounded on a complete *recensio* of the manuscripts, a careful evaluation of stemmatic relationships, and a balanced *constitutio textus*.[18] Vinel's edition makes *In Nic. arith.* available in this long-awaited philological form. As is clear from the last part of his introduction [53–65], Vinel has collated every single testimony about the text and now brings to light a quite complex textual tradition. His most important achievement concerns the identification of two primary witnesses: Laurentianus 86, 3 (F) and Laurentianus 86, 29 (L). Vinel demolishes the idea (proposed without any good reason by Pistelli, the editor of the text in 1894, and never really submitted to verification) that F (14th century) is the only independent manuscript copy of the text. He demonstrates to the contrary that L (15th century) is independent of F and that both derive from the same lost manuscript. Moreover, he shows that all other manuscripts (produced between the 15th and 17th centuries) stem from L either directly or indirectly (while the three Latin translations were produced on the basis of manuscripts still extant).

Regarding Vinel's analysis, which is excellent, one may note the absence of a close paleographical and codicological description of the most important manuscripts. This would not only have been a desirable addition on its own, it would also have supported Vinel's account of the textual tradition. Vinel is inclined to consider the copyist of L to be not so gifted: against the claim that many errors found in F with respect to L depend on corrections (διορθώϲειϲ) by the copyist of L, Vinel emphasizes that there are blatant errors still present in L. However, he also identifies a series of 10 firsthand notes in L (both interlinear and marginal) which are not present in F and

[18] With respect to the other works, Iamblichus' *In Nic. arith.* has two noteworthy advantages: it is limited in length and comes in a reasonably small number of manuscripts (24 Greek manuscripts, plus three Latin translations), while the manuscript traditions of both Nicomachus' *Intro. arith.* and Theon's *Expositio* are much richer.

stem in part from a collation of a copy of the antigraph of L with F.[19] This produces a quite strange image of the copyist of L, who, it seems, is not good enough to correct some blatant errors but is still so philologically gifted and careful as to produce a collation of manuscripts and to choose among different readings [e.g., at 2.17.1].[20]

Another valuable contribution of Vinel's edition and translation consists in the fact that he divides the translation (and, wisely, not the text) into thematic chapters and paragraphs: this helps us to understand the sequence of Iamblichus' reasoning and the compositional logic of the work. I will not focus on the commentary apart from the references indicated above. In general, it is a very careful, exhaustive, and informative commentary, which deals with technical, philosophical, and philological problems. In this sense, it is useful not only in order to clarify Iamblichus' statements and their mathematical background but also to grasp the function of various passages in the work.

My remarks are meant only to present the more interesting elements that the author analyzes carefully and extensively: I emphasize that any criticism must be considered in the framework of my positive evaluation of every part of the book, which satisfies the requirements of consistency, completeness, conclusiveness, and utility. All in all, Vinel's book—the third in the well-established and most valuable series Mathematica Graeca Antiqua edited

19 Vinel hints at this briefly [64]; it is also quite telling in relation to the traditional misunderstanding of the stemma.

20 A new paleographical and codicological inquiry would give substance to Vinel's first argument for the independence of L. He indicates that 'le Laur. 86, 3 est un manuscript de lecture très difficile, avec une mise en page très lourde, des lignes longues et serrées' [60], with many abbreviations and a careless use of diacritics, while 'le Laur. 86, 29 est très bien écrit, de lecture facile et agreeable, avec très peu d'abréviations et une mise en page aérée'. From this, Vinel deduces that it is very unlikely that L is a copy of F. I must say that I cannot see the point of such an argument as it stands. Yet, although F is more difficult to read with respect to L, its quality from the point of view of both writing and « mise en page » is neither anomalous nor exceptionally obscure, especially in comparison with other manuscripts of the same period (good reproductions of both manuscripts are now available in the website of the Biblioteca Medicea Laurenziana). Moreover, it was common enough to copy a text found in what we see as a less accurate manuscript in order to provide it in a form that makes it more accessible.

by Fabio Acerbi and Bernard Vitrac—is an outstanding piece of scholarship, which will remain as a helpful tool in many fields of research.

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Astrolabes from Medieval Europe by David A. King

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This volume gathers 12 studies written by David A. King and published between 1993 and 2008. King is Emeritus Professor of History of Science at Johann Wolfgang Goethe University in Frankfurt am Main, Germany. He is a historian of medieval astronomy and its instrumentation, specializing in both Islamic and Christian traditions.

It is a heterogeneous collection, offering readers insight into specific medieval sundials and Renaissance astrolabes in addition to the astrolabes from medieval Europe advertized by the book title. Looking like a technical course reader, this is not a book to be browsed lightly, even though the 148 black-and-white illustrations are sure to intrigue those who pick up the book. The target audience for the volume is the specialist—for whom the book does a service in bringing the miscellany together, especially since the titles were originally published in esoteric Festschrifts and niche journals. As in other volumes in the Variorum Collected Studies Series, Ashgate has not reformatted or repaginated the originals but reproduced them as they first appeared, so that users may cite the studies using the original pagination. Each article has been given a Roman numeral, however, which will accompany the original page numbers when cited in this review.

The first article, 'Astronomical Instruments between East and West', originally appeared in *Kommunikation zwischen Orient und Okzident*, a publication of the Austrian Academy of Sciences [King 1994]. General in purpose, it serves as an introduction to the volume in giving an overview of medieval astronomical instruments, which are generously defined as globes, astrolabes, quadrants, and sundials that were made before 1550 in Europe and 1900 in the Islamic world. At the time of King's writing, over 1,000 were known to

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survive in collections. King surveys the impoverished state of scholarship on the topic and describes his personal efforts since 1989 to remedy the situation with the preparation of a critical catalogue of instruments. He then engages the reader with surprising, preliminary findings that might rewrite the history of the transmission of the astrolabe to Europe, reveal forgotten medieval numerical notations, uncover 'fakes—a pain in the neck', and force us to re-evaluate our understanding of many historical issues. The findings are presented as morsels to whet our appetite. Articles that follow in the collection explore some of these findings in greater detail. This one concludes with a checklist of astronomical instruments made in Vienna in the 15th century and another of those found in or lost from Austrian collections.

Indeed, the first article can be read as a call to all historians to widen their gaze from textual sources and learn to read physical artifacts, especially scientific ones:

> Historical scientific instruments indeed demand the same kind of respect as manuscripts and incunabula. They call out for attention to their basic design (equivalent to the text, literary style and geographic milieu) as well as to details like inscriptions (author, location, date and title), special markings (colophon, copyist and script), scales and numeral forms (foliation, chapter titles, indexing), and not least additional markings, replacement parts and repairs (marginalia, flyleaves, binding, owner's marks). Too often amateurs have been given free rein in the delicate field of historical instruments, often with disastrous consequences. Also one cannot work on these instruments from photographs alone; one has to hold the instrument in one's hands, take it apart, look carefully at the engraving and the markings. This is no armchair science. The information provided by the instruments themselves should always be viewed in the light of that which can be extracted from contemporaneous texts, the documentation of which also leaves much to be desired. [I.167]

King prepared this article for a conference in Krems, Austria that was organized by the Institut für Realienkunde des Mittelalters und der frühen Neuzeit. His audience included fellow medievalists, Byzantinists, and Islamicists who presumably valued tangible things as historical resources. Even so, after he delivered his paper, an auditor asked, 'So what?' King's response: 'What else must I do to attract to these [scientific] instruments the attention they deserve?' [I.177]. The articles in *Astrolabes from Medieval Europe* that follow help to make his point.

The second article treats 'The Earliest Known European Astrolabe in the Light of Other Astrolabes' [King 1996].[1] Here King refers to the 'Carolingian' astrolabe first described by Marcel Destombes in 1962 and bequeathed by him to the Musée de l'Institut du Monde Arabe in Paris in 1983.[2] This remarkable and controversial instrument was the subject of a full session at the Twenty-Ninth International Congress of the History of Science, held in Zaragoza, Spain; and King's article was published in the proceedings of that special session. In his article, King analyzes all parts of the instrument and its inscriptions in the context of other extant, very early Islamic and European instruments to argue convincingly that this astrolabe originated in late 10th-century Catalonia, Spain. Not everyone agrees. King concedes that other scholars, 'albeit none with any experience with medieval instruments', have labelled it a fake. He laments that

> it is a sad fact of astrolabe life that once an instrument has been deemed suspicious by people who do not understand it, the piece is essentially doomed forever and it is impossible to reinstate it to its rightful place in history. [pref. x–xi]

The third article in the collection is devoted to another very significant astrolabe from 14th-century Picardy. Its numerals are written as ciphers in a form that was first introduced to England from Greece in the 13th century and then developed by Cistercian monks in the Hainault region of Belgium and France later in the century.[3] They are evidence of compact numerical notation in a period when Hindu-Arabic numerals were little known. In addition to the curious monastic ciphers, star names and calendrical inscriptions on the astrolabe are in Gothic script with spellings associated with a dialect of Picardy. Tool marks and engravings by a less-than-steady hand suggest

1 Although published in September 1996, the article was part of a special issue of *Physis* with the imprint date 1995 [Stevens, Beaujouan, and Turner 1995].

2 'Carolingian' astrolabe, Catalonia, Spain, circa AD 980, Musée de l'Institut du Monde Arabe, Paris, AI 86–31, accessed 15 Jul 2016: http://www.qantara-med.org/qantara4/public/show_document.php?do_id=1379.

3 The 14th-century Picard astrolabe with the monastic numbering system is in private hands but its front face can be seen online at 'Galileo: Images of the Universe from Antiquity to the Telescope', accessed 15 Jul 2016: http://brunelleschi.imss.fi.it/galileopalazzostrozzi/object/AnonymousAstrolabeWithMonasticNumberingSystem.html. For a sample of the cipher notation, see D. A. King, 'The Ciphers of Monks', accessed 15 Jul 2016: http://www.davidaking.org/Ciphers.htm.

the product of an elderly monk. Lastly, there is a dedication 'in bold Gothic script with humanistic flourishes', mentioning two friends of Erasmus:

> *Hadriano Amerotio Berseli[us] me dono dedit 1522.*
>
> Berselius gave me as a gift to Hadrianus Amerotius in 1522.

King speculates why a 150-year-old Picard astrolabe would be a good parting gift from Paschasius Berselius, a humanist author, artist, and Benedictine monk returning to his abbey in Liège from Louvain, to his good friend, Adrian Amerot, a Picard native serving as a second professor of Greek at the College of the Three Languages (*Collegium Trilingue*) in Louvain. This article is entitled 'Rewriting History through Instruments: The Secrets of a Medieval Astrolabe from Picardy' and first appeared in a Festschrift for Gerard L'Estrange Turner, a founding figure in the historical study of scientific instruments [King 1993].

Turner joins King as co-author of the 11th article in the collection: 'The Astrolabe Presented by Regiomontanus to Cardinal Bessarion in 1462' [King and Turner 1994]. Together they examine this enigmatic astrolabe in all its technical detail—including the configuration of its parts, calligraphy, inscriptions, and metallurgy—and they establish its place among German astrolabes of similar design produced in 15th-century Vienna. Devised by the distinguished astronomer and scientific printer Johannes Müller of Königsberg (better known by his Latin name, Regiomontanus), the astrolabe is precise in its astronomical markings. On the reverse of the *mater*, it has an unusual feature: a recess in which a rotating disk carries a projection of the celestial sphere known as the *organum Ptolemei* (or the de Rojas astrolabe projection). Above the projection, Regiomontanus placed a winged angel; below it, a dedication in the form of an elegiac couplet to his new patron, the Greek scholar and humanist, Cardinal Basilius Bessarion.[4] During a diplomatic mission to Vienna as papal legate to the Holy Roman Empire in 1460, Cardinal Bessarion commissioned Austrian astronomer Georg von Peuerbach to write an epitome of Ptolemy's *Almagest* to replace the faulty translation of George of Trebizond of 1450. After Peuerbach's untimely death in 1461, his former

[4] The Regiomontanus astrolabe, which is currently in private hands, is pictured in 'Galileo: Images of the Universe from Antiquity to the Telescope', accessed 15 Jul 2016: http://brunelleschi.imss.fi.it/galileopalazzostrozzi/object/RegiomontanusJohannesMullerVonKönigsbergAstrolabe.html.

student and collaborator, Regiomontanus, shouldered the burden. Bessarion took Regiomontanus to Rome as a member of his extended household in 1461 and facilitated his access to Greek texts and scholarship. Around 1462, Regiomontanus completed the task (although the *Epytoma...in Almagestum Ptolemei* was not published until 1496).[5] The quality of the instrument and its association with Regiomontanus and Cardinal Bessarion at the time of the project's completion, led King to see this astrolabe as 'the most important and the most historically interesting instrument of the Renaissance' [pref. xiv]. Since preparing this article in 1994, however, King has continued to ponder the curious style of the dedication and the presence of the angel. It turns out that the dedication is an elaborate acrostic; that the gift celebrated the 400th anniversary of a Byzantine astrolabe made in 1062 (preserved currently in Brescia), which Bessarion presumably showed to his protégé; and that the angel represented St Bessarion of the fifth century, whose name had been adopted by the cardinal when he was a monk. Readers wishing to know more about these puzzles may consult King's *Astrolabes and Angels, Epigrams and Enigmas*, where the whole story is told [King 2007].

Lesser instruments, but no less interesting to specialists, are documented in great detail in the remaining articles collected in the volume. The fourth article, co-authored with Kurt Maier, 'The Medieval Catalan Astrolabe of the Society of Antiquaries, London', analyzes astronomical, linguistic, and aesthetic features of a particular astrolabe in order to place it among the rare group of medieval astrolabes influenced by Arabic and Islamic traditions and originating in Catalonia, the region of Spain where European scholars first learned of the astrolabe [King and Maier 1996].

The volume's fifth article examines a tiny Italian astrolabe in the Museum of the History of Science in Oxford and argues that it was inspired by a medieval Islamic type no longer extant.[6] At the time of his writing the article published here, 'A Remarkable Italian Astrolabe from *ca.* 1300—Witness

[5] *Encyclopaedia Britannica Online*, *s.v.* 'Regiomontanus' and 'Georg von Peuerbach', accessed 15 Jul 2016: http://www.britannica.com/EBchecked/topic/496038/Regiomontanus, and http://www.britannica.com/EBchecked/topic/454669/Georg-von-Peuerbach.

[6] Although King dates the instrument to about 1300, the Museum of the History of Science, Oxford catalogues it as 'Astrolabe, Sicily?, *ca.* 1460?' with inventory number 40829 (ICA 169). Images are online at http://www.mhs.ox.ac.uk/collections/imu-

to an Ingenious Tradition of Non-Standard Astrolabes' [King 2003b], King did not yet know of another astrolabe that he now believes came from the same workshop. It is illustrated in Jan van Eyck's painting, *Saint Jerome in His Study*, *ca* 1435.[7] Readers may therefore wish to consult King's more recent version of the article, which is found in his *In Synchrony with the Heavens* and supersedes the one published here [see King 2005]. The value of 15th-century art for instrument studies is also evident in the 10th article, where King analyzes 'The Astrolabe Depicted in the Intarsia of the *Studiolo* of Archduke Federico in Urbino'.[8]

In the sixth article, 'An Astrolabe from Einbeck Datable *ca.* 1330', King documents the earliest known, dated German astrolabe [King 2008]. This is followed in the book by an exploration of star names on medieval European instruments, with particular attention paid to the Picard astrolabe analyzed in the third article and an Urbino astrolabe discussed in the 10th [King 2000].

The eighth and ninth articles examine the possible roots of two European non-astrolabic, time-finding instruments in 9th- or 10th-century Baghdad. In the first of these—'A *Vetustissimus* Arabic Treatise on the *Quadrans Vetus*' [King 2002]—King establishes the Islamic origin of the universal horary quadrant, which existed independently of astrolabes but is often found inscribed on them. In the second article—'14th-Century England or 9th-Century Baghdad? New Insights on the Origins of the Elusive Astronomical Instrument Called the *Navicula de Venetiis*' [King 2003a]—King studies the type of altitude sundial known as the 'Little Ship of Venice'. It, too, is universal, meaning that it can be used at multiple latitudes.

The last item in the volume is an aid to future research: 'An Ordered List of EuropeanAstrolabes to *ca.* 1500'. Not previously published, this checklist is organized chronologically by provenance and contains 156 instruments. The present location of each astrolabe in a museum or private collection is

search-page/results/?querytype=basic&query=40829&search=Search&thumbnails=on (accessed 15 Jul 2016).

7 *Saint Jerome in His Study*, Jan van Eyck (Netherlandish, *ca* 1395–1441), oil on linen paper on oak panel, *ca* 1435, Detroit Institute of Art, 25.4, accessed 15 Jul 2016: http://www.dia.org/object-info/97a8694c-1be0-46b6-a6d4-e55bc61b8c14.aspx?position=1.

8 The Ashgate/Variorum article is a shortened version of King 2001.

indicated. Also given is each object's number on the International Checklist of Astrolabes, initiated in Price 1955.[9] The enormity and complexity of King's undertaking is indicated by the taxonomy that he has used to organize his checklist of medieval European astrolabes and the somewhat inscrutable nature of categories 1 and 2:

(1) Miscellaneous early European astrolabes (I)
(2) Miscellaneous early European astrolabes (II)
(3) Astrolabes with quatrefoil decoration (I)
(4) Astrolabes with quatrefoil and trefoil decoration (II)
(5) Astrolabes with inscriptions in Judaeo-Arabic or Hebrew
(6) English astrolabes with a Y-shaped frame on the *rete* in the tradition of Geoffrey Chaucer
(7) Early French and Italian astrolabes with an upper frame on the *rete*
(8) 15th-century French astrolabes in the tradition of Jean Fusoris
(9) 15th-century German astrolabes in the tradition of Regiomontanus and Hans Dorn

It must be noted, nonetheless, that King's useful checklist published here has not been updated since about 1996 and is but one section out of 12 that King envisioned as *A Catalogue of Medieval Astronomical Instruments to ca. 1500*, which included not only European astrolabes, sundials, and quadrants, but also Eastern Islamic instrumentation. That project was supported by the Deutsche Forschungsgemeinschaft until 2002 at the Institute for the History of Science at Johann Wolfgang Goethe University in Frankfurt am Main, where King was professor and director. Since then, there has been no funding to my knowledge and the Institute was dissolved when King retired in 2007. A provisional table of contents (last updated in May 2002) is available on King's personal website, although the Ashgate/Variorum checklist is more complete than the corresponding section online.[10]

[9] Price's International Checklist of Astrolabes was expanded and computerized in Gibbs, Henderson, and Price 1973.

[10] D. A. King, *A Catalogue of Medieval Astronomical Instruments to* ca. *1500* (in preparation). See remarks last updated in May 2002 at http://www.davidaking.org/instrument-catalogue.htm (accessed Nov 30, 2014). See 'Part 6: Early European astrolabes (to *ca.* 1500)' at http://www.davidaking.org/instrument-catalogue-TOC.htm#part6 for a prior version of the checklist published in the Ashgate/Variorum volume.

It is a pity that King's majestic catalogue of medieval astronomical instruments has been put on hold but he has given us a sample of his scholarship and methods of interrogating scientific instruments in this volume. 'All instruments can speak to us if we are prepared to listen', King writes.

> They constitute an untapped source for the history of human preoccupation with the celestial environment and man's attempts to understand it. There are enough instruments available and in sufficient variety that even the most hardened sceptic could find at least one of interest.... And one instrument will suffice for a start because enthusiasm for scientific instruments is progressive, incurable, and also—I hope—contagious. [I.179]

I hope that he is right.

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*L'eruzione vesuviana del 1631. Una storia d'età moderna** by Alfonso Tortora

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In the early morning of 16 December 1631, Mount Vesuvius erupted in what was one of its most destructive explosions in recorded history (after, of course, the infamously renowned event of AD 79). Between December 16th and 17th, a catastrophic combination of pyroclastic and lava flows buried and destroyed a great part of Portici, Torre del Greco, San Giorgio a Cremano, Torre Annunziata, Boscotrecase, Ottajano, Somma Vesuviana, and several other villages—including Barra, Ponticelli, and San Giovanni a Teduccio, now part of the municipality of Naples—killing about 4,000 people and thousands of cattle and other livestock. As a consequence of this disaster, a multitude of refugees sought shelter in the nearby city of Naples, whose Spanish government had to deal with an unprecedented, enormous humanitarian crisis.

From this dark, dramatic background, Alfonso Tortora takes his cue to address an extremely wide array of subjects such as history, social history, archaeology, urban geography, history of literature, philosophy, religion, politics, economics, bibliographical studies. Unfortunately, the shortness of the book does not allow him to delve into all these issues with the level of detail that they would deserve (and demand). Still, it provides a useful and complementary approach to the study of one of the most noteworthy episodes related to the Earth sciences in the early modern period.

The extreme thematic variety of this work is, at the same time, both a strength and a weakness. If, on the one side, it offers a stimulating outlook on a crucial moment in the history of Southern Italy; yet, on the other, it may disorient the reader by its sudden and loosely connected changes of topics that follow one another in a hasty sequence of just 150 pages. This is particularly evident

* *The Vesuvian Eruption of 1631: An Early Modern History.*

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in the first chapter, ‘On the Origin of the “Vesuvian Bibliography”’ [11–51].[1] the content of which, however meant as an introduction to the following parts, feels too thematically disjointed from the rest of the volume.

This chapter focuses on the bibliographic work of Friedrich Furchleim, an Austrian publisher and bookseller who settled in Naples in the second half of the 19th century, and on his *Bibliografia del Vesuvio* [1897],[2] the first, systematic attempt to assemble an exhaustive collection of all the historical records and studies devoted to this troubled volcano. Tortora describes this work as ‘imbued with positivism’ [23],[3] as a result of the influence that Auguste Comte’s philosophy had on the many branches of scientific enterprise in that period. Bibliography, philology, and librarian studies were no exception to this trend, though traditionally considered as cross-boundary disciplines between humanities and science. And with particular respect to the context of Southern Italy, the efforts in the previous years by such intellectuals as the Palermitan bookseller Giuseppe Mira, with his *Manuale teorico-pratico di bibliografia* [1861],[4] and Tommaso Gar (a scholar from Trento, who was director of the Library of the University of Naples from 1863 to 1867), with his *Letture di bibliologia fatte nella Regia Università degli Studi in Napoli* [1868],[5] paved the way for a rigorous redefinition of bibliographic methodology [26–38], eventually leading to the adoption of descriptive bibliography. The same criteria, Tortora notes, were assimilated by Furchleim and applied to his *Bibliografia*, which still today is regarded as an essential reference point for any scholar interested in the history of Vesuvius [41–51].

‘On the Background of Vesuvius’[6] is, in fact, the title of the second, short chapter [53–69], that abruptly shifts from the 19th to the 17th century, and from bibliography to history. But such a title is deceptive and, indeed, too narrow for a chapter with geographical limits stretching from France and Spain to the Holy Roman Empire [§§2.1, 2.3], from the Duchy of Savoy [§§2.2, 2.3] to the Republic of Venice [§2.4], and from the Republic of Genoa

1 ‘Alle origini della “bibliografia vesuviana”’.

2 *Bibliography on Vesuvius.*

3 ‘una nuova materialità del testo…intrisa di positivismo’.

4 ‘*Theoretical-Practical Manual of Bibliography*’.

5 *Lectures on Bibliography made in the Royal University of Studies in Napoli.*

6 ‘Sullo sfondo del Vesuvio’.

[§2.5] to, at last, the Kingdom of Naples [§2.6]. Actually, the author provides here a general overview of the main political, cultural, and military events that scourged and shaped Europe during the entire span of the dramatic 17th century (e.g., the bloody Thirty Years' War, the decline of Venice and Genoa, the incessant conflicts between the Sun King and the Habsburgs, and the ambitious rise of Victor Amadeus II of Savoy), leading to the gradual and inexorable decline of Spanish hegemony over the Continent to the advantage of France. The evident purpose of these pages is to act as a hasty prelude to the following part of the book, where the main subject is finally addressed in two complementary chapters: 'The Vesuvian Eruption of 1631: The Event Experienced' [71–108][7] and 'The Vesuvian Eruption of 1631: The Event Described' [109–150].[8]

Chapter 3 opens with an evocative phrase from the Jesuit Ascanio Capece. On 30 December 1631, he wrote from Naples to Antonio, a brother of his Order in Rome, 'it was as if the whole world was in flame' [72].[9] As Tortora interestingly suggests, the dramatic tone of this note seems to allude to more than a (however dreadful) natural phenomenon. As the capital of the Spanish possessions in Southern Italy, Naples felt with particular sharpness the damaging effects of the Habsburg's ruinous military expenses on the region's economy and on its population, burdened as it was with constant requests for soldiers, funds, and supplies [73–75]. It is not by chance that one of the most passionate opponents of Spanish policies in Italy was Giulio Cesare Braccini, an abbot, natural philosopher, and political writer from Lucca, who wrote both a protest letter against Spain's vexatious taxes (*Discorso intorno a'donativi, che si fanno in Napoli alla Maestà del Re Cattolico* [1629])[10] and a detailed report on the eruption of 1631 (*Dell'incendio fattosi nel Vesuvio a XVI di dicembre MDCXXXI* [1632]).[11]

This second document contains far more than a merely *philosophical* dissertation about the natural disaster. Braccini was well aware that the eruption would worsen an already critical situation in the Neapolitan region, where, in the early 17th century, economic production struggled to keep up with de-

7 'L'eruzione vesuviana del 1631. L'evento vissuto'.

8 'L'eruzione vesuviana del 1631. L'evento raccontato'.

9 'Pareva che tutto il mondo ardesse'. This passage is quoted in Riccio 1889, 496.

10 *Discourse about the Tributes Paid in Naples to the Catholic Majesty.*

11 *On the Blaze that Occurred in Vesuvius on December 16, 1631.*

mographic growth. Sadly, his prediction was confirmed, when—according to Braccini himself and to other witnesses such as theSecretary of Neapolitan People Gianbernardino Giuliani, the Jesuit priests Capece and Giulio Cesare Recupito, the Marquis Giovan Battista Manzo, and two anonymous Spanish reporters—Naples was invaded by a desperate stream of people fleeing the ravaged rural areas [77–101].[12]

As in any humanitarian crisis, the dangers were many and great: social turmoil, famine, spread of epidemics, and looting, especially in the abandoned villages. These risks were not underestimated by the Spanish Viceroy, the Count of Monterrey, Manuel de Acevedo y Zúñiga. He immediately put armed guards to protect the city and the outer zones ('as fear often turns into revolt')[13] and involved both nobles (the so-called *gentil'homini*) and priests in assisting the refugees. Many churches and palaces in Naples were opened and used as shelters, and generous donations provided food for the indigents [84–101]. These facts are reported in great detail by the authors. In particular, Giuliani and the two unknown Spaniards insist on the exemplary behavior and courage shown by the Viceroy and by several noblemen in dealing with the emergency: a rhetorical emphasis, writes Tortora, which seems to suggest the political and ideological intent of these documents as a sort of pro-government propaganda.

Chapter 4 focuses on Braccini's report, *Dell'incendio fattosi nel Vesuvio.* Tortora examines it mainly from a cultural, sociological, and literary perspective, and (especially in §§ 4.2 and 4.3) points out the importance of the historical analogy between the event of 1631 and the iconic explosion of AD 79.

The Kingdom of Naples was a society dominated and shaped by the Counter-Reformation. In such a delicate and problematic context, learned clerics were the most suitable persons to act both as defenders of the *true* religion and as mediators between the potentially subversive content of natural philosophy and the vast mass of illiterate people. Therefore, they played a crucial role in adapting the philosophical and scientific analysis of natural phenomena to the rigorous theoretical frameworks of Catholic orthodoxy and Classical tradition. Braccini's treatise is an emblematic example of this approach, as it combines a 'technical' and philosophical description of the eruption with

12 About 40,000 people, according to Capece [Riccio 1889, 497–498].
13 '...essendo solita...rivolgersi la paura in seditione' [Recupito 1635, 107–108].

frequent literary references to the renowned Plinian account [142–150] along with a religious, moralistic interpretation of the disaster. Human sins—the so-called *mediocritates innominatae*, as he defines them [118]—caused divine wrath and, thus, the Vesuvian eruption, an explanation that recalls a dominant and, somehow, still unresolved issue in Western theology [118–121]. As a direct consequence of this belief, the Catholic Church (along with its secular counterpart, the Spanish government) is the only safe shelter for the penitents. Hence Braccini's insistence on the concept of miracle, which is both a source of wonder and social cohesiveness for the people and a sign of reconciliation between God and humans. In this case, of course, the famous miracle of the liquefaction of the blood of Saint Januarius had a particular devotional and, therefore, political meaning, and was considered as an omen of God's will to save the city and its population [123–133].

Once more, religion and politics merge into a single stream in an effort to strengthen the unstable relationships among the different social strata of the Kingdom. Yet, Braccini does not spare the Spanish rulers from criticism. As in his previous *Discorso intorno a' donativi*, he protests against the excessive taxes imposed on Southern Italy by Madrid. Moreover, the many references to the Plinian eruption serve to highlight the wisdom of Emperor Titus in braving the disaster of AC 79 and, thus, as a sort of admonition to the Spanish central government to follow the virtuous path traced by its illustrious precursor [133–142].

As noted before, Tortora's study has the merit of providing a remarkable and original contribution to our understanding of the social and cultural background of Southern Italy in the first half of the 17th century. Still, the issues raised by the author are so many and so important that the book is unquestionably too short to achieve a thorough, in-depth analysis of this challenging and fascinating context. It is probably because of this that not a few topics and sections seem to be loosely and hastily pasted together in the volume, and to create in the reader an odd sense of confusion. Furthermore, the lack of a bibliography and an index of names is a serious and, quite frankly, perplexing flaw, especially in light of the great importance that the author himself attributes to bibliographical studies and to Furchleim's work for a reconstruction of the history of Vesuvius.

In short, the book, though noteworthy, is excessively eclectic reading, given the contrast between its brevity and the crucial importance of the subjects that it discusses. From this point of view, it cannot be denied that it is a partially missed opportunity.

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