AESTIMATIO

Critical Reviews in the History of Science

Aestimatio

Critical Reviews in the History of Science

AESTIMATIO

Critical Reviews in the History of Science

Volume 8

2011

Edited by
Alan C. Bowen and Tracey E. Rihll

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

Co-Published by

gorgias press

Co-Published by Gorgias Press LLC
180 Centennial Avenue
Piscataway, NJ 08854
USA
Internet: www.gorgiaspress.com
Email: helpdesk@gorgiaspress.com

ISBN 978–1–4632–0179–1

This volume is printed on acid-free paper that meets the American National Standard for Permanence of paper for Printed Library Materials.

Printed in the United States of America

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

To receive automatic notices of reviews by email as they are posted on the Institute website, readers are invited to subscribe to *Aestimatio-L* by sending an email message to majordomo@list.library.utoronto.ca containing in the body of the message the single command line 'subscribe Aestimatio-L'. Alternatively, they may subscribe to the RSS feed on the Recent Reviews page (http://www.ircps.org/publications/aestimatio/rreviews.htm).

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

Contents

Preface

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

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

Alan C. Bowen

Tracey E. Rihll

The First Professional Scientist: Robert Hooke and the Royal Society of London by Robert D. Purrington

Science Networks: Historical Studies 39. Basel/Boston/Berlin: Birkhäuser, 2009. Pp. xx+281. ISBN 978–3–0346–0036–1. Cloth € 127.33

Reviewed by
Anthony Turner
Independent Historian, Le Mesnil-le-Roi, France
anthonyjturner@9online.fr

This slender work has a non-slender price which it fails to justify. The notes (here quaintly called 'annotations') are placed in the most useless and inconvenient place possible at the end of each chapter. There is a goodly sprinkling of misprints; while infelicities of phrase, inconsistencies of statement, and omissions from the bibliography reveal that the text has not been competently copyedited. The paperboards are less than robust. After a few days of traveling in a briefcase for reading on trains, the book looks distinctly battered.

All this would hardly matter if the content were distinguished, but Robert Purrington has written a curious book. His Robert Hooke is a man who was eclipsed during his lifetime by Newton and then forgotten for nearly three centuries before being resuscitated in the mid 20th century to become 'nearly *fashionable*' during the last two decades. This, to say the least, is a highly over-simplified view of the trajectory of Hooke's reputation. If he was eclipsed by Newton, that eclipse took place after Hooke's death in 1703, not during his lifetime. Indeed it was Hooke's persistent and essential presence in the Royal Society, a presence that Purrington takes pains to underline, that led Newton largely to ignore the Society during the first seven and half years that he lived in London. Although, as president from 1703, Newton could apparently not abide even the mention of Hooke's name, this did not prevent Hooke's posthumous works from being published with a life by Richard Waller, a close friend of Hooke and joint-secretary of the Royal Society [Waller 1705], ironically with a dedication to Newton. In 1726, a selection of Hooke's philosophical papers was published [Derham 1726] and a further life in 1740 [Ward 1740]. That Hooke's writings and his ideas about nature had

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 1–11

diminishing usefulness in the course of the 18th and 19th centuries is surely the case, but the man himself was not forgotten. His life was included in the 1721 edition of the *Athenae oxoniensis* [Wood 1721] and in the *Biographia Britannica* [1747–1766, vol. 4]. As Purrington underlines, Hooke's role in the Royal Society was clearly revealed in Birch's close transcription of the Society's journal books [Birch 1756] and neither was he forgotten in 19th-century histories of the Society [e.g., Weld 1848]. Thereafter, rather little was published about him [see Keynes 1960, app. 3] for the best part of a century, although this is true also of most of the early members of the Royal Society. Hooke indeed seems exceptional for the amount of material that was published about him.

Purrington, however, needs Hooke and his role in the Royal Society to have been neglected in order to validate his own work. After a summary of studies of Hooke produced from 1930 onwards,[1] he can nonetheless write 'But for most of the twentieth century Hooke has been ignored—indeed it could be said that Hooke's eclipse *has been his identity* (to paraphrase Adam Gopnik)' [xv]. The remark is breathtaking but unfortunately representative of the lack of historical understanding betrayed throughout the book. The historiography of Hooke's reputation is presented in isolation. No comparison is made with that of other contemporary members of the Royal Society (except Newton) and we are thus given a false picture of an unduly neglected figure. In fact, Hooke has been far better known in the last three centuries than men such as Laurence Rooke, William Ball, or even Brouncker and Moray, all founding members of the Society and socially more prominent than Hooke.

Hooke's reputation needs to be considered in the context of the historiography of the sciences in general just as Hooke's intellectual life needs to be presented in the general context of late 17th-century activity. Throughout the book, however, Purrington makes no effort to distinguish which of Hooke's multifarious activities were original to him and which arose from the common stock of technical and intellectual preoccupations of the period. The only contexts in which Hooke is here presented are those of the Royal Society, physics, Newton, and London.

[1] A fuller, more analytical, account will be found in Hunter and Schaffer 1989, 3–6.

But this parochial approach to Hooke will not do. In the address 'to the Reader' with which Richard Smith prefaced the sale catalogue of Hooke's books [Feisenberger 1975, 59], he tells us that '. . . for many years he [Hooke] hath been on all occasions Collecting at Home and been assisted by his Friends Abroad.' Of Hooke's 'friends abroad' we learn nothing in the present work. Hooke is presented as a purely English, indeed purely London, figure. For Purrington, London is all Hooke's world and he is thus amputated from the Republic of Letters. But Hooke corresponded with naturalists throughout England (Peter Nelson in Durham, Andrew Paschall in Chedsey, Somerset, to name but two), just as he did with naturalists and savants throughout Europe.[2] As the catalogue shows, his library was rich in Latin, French, Italian, and Spanish books. Some 5% of his nearly 3500 volumes were in French, about 6% in Italian and Spanish, about half were in Latin, and the rest in English. Hooke, it seems, was well supplied with the books of Renaissance and contemporary European learning. What use he made of them, the content and extent of his European correspondence and reputation are not, however, matters discussed here except for some passing references to Huygens and Hevelius. Hooke nonetheless was, at least by reputation, known in the Republic of Letters in part thanks to the correspondence of Oldenburg, in part because of his own. European savants responded to his ideas just as he did to theirs. Purrington has not understood the essentially European nature of the everyday practice of scholarship during the Early Modern period and treats Hooke in a purely English context. He is by that much diminished.

Throughout the book, Purrington treats Hooke primarily as a physicist since this seems to be the category of modern scientific work which approximates most closely to a part of Hooke's work. Since 'Hooke understood the implications of his own discoveries and those of his contemporaries', he was also 'one of the important natural philosophers of the seventeenth century' [149]. Quite how Purrington reconciles these statements with his view that Hooke failed to understand the new mathematical techniques of analysis that alone were capable of resolving the problems of planetary motion and would also transform natural philosophy is not clear, although he spends

[2] For the names of some of these, see Keynes 1960, 80–86.

some time seeking excuses for this failure on Hooke's part. He considers Hooke's natural philosophy to have been neglected but then gives a restricted account which fails to discuss Hooke's methodology and omits all consideration of the religious component in Hooke's thought. This, however, was fundamental, for not only was Hooke imbued with the sense that his successes in investigation and research came directly from God, but one of his motives as a naturalist was to illustrate the providence of God which naturally provoked admiration and adoration of Him.

Although Purrington accepts that the basis of Hooke's work was not mathematics but a highly developed system of empirical enquiry which used experiments both to investigate and to demonstrate, he fails to give a convincing analysis of how this may have emerged from mid 1650s Baconianism, how it was influenced by Hooke's exposure first to the Oxford experimentalists and secondly to Boyle; and he fails totally to place the underlying 'philosophical algebra', so strongly vaunted but never clearly explained by Hooke, in the context of the ideas about a philosophical language and a universal character which so exercised his mentors John Wilkins and Seth Ward. This failure is in part a consequence of Purrington's imperfect command of the secondary literature concerning Hooke. Mary Hesse's fundamental article on the philosophical algebra [1966] seems to be unknown to him as is work on Hooke as a language reformer [Slaughter 1982]. Another work not used by Purrington, the entry on Hooke in the *Oxford Dictionary of National Biography* [Pugliese 2006] contains in a single page a more incisive and informative account of Hooke's method than the 13-page chapter in this book. Pugliese 2006 and Hunter 2003 remain the best accounts of Hooke's natural philosophy.

Inconclusive analysis uninformed by all the relevant literature is alas characteristic of Purrington's accounts of many specific episodes in Hooke's often intolerant life. As an example, examine the discussion of Hooke's 'first original contribution to science' [81].

It seems to have been in 1655–1656 that Hooke made his first original contribution to science. In fragments of an autobiography written in 1697, he recounted his early attempts to improve the pendulum clock for timing astronomical observations and reported that

> in the Year 1655 or 57, I contriv'd a way to continue the motion of the Pendulum, so much commended by Ricciolus

> in his *Almagestum*, which Dr. Ward had recommended me to peruse. [81]

Purrington assumes that this describes Hooke's invention of the recoil anchor escapement, which would eventually replace the verge and crown wheel escapement that Huygens had invented in 1658. For Purrington, this discovery is not without some controversy, since the earliest extant clock with an anchor escapement is one made by William Clement, dated 1671, and Joseph Knibb has also been given credit. But, Purrington maintains, the evidence favors Hooke, including references in the minutes of the Society from 1669 describing what was probably the anchor escapement. 'It was, in any case, effectively the first of a very long string of inventions' [81].

To begin with two factual errors. Huygens did not invent the verge and crown wheel escapement in 1658. In that year, he published an account of his application of the pendulum to a clock mechanism. This entailed replacing the foliot controller of the verge escapement (known since the late 13th century) by the pendulum. To do this the verge and its associated escape or crown wheel had to be swung through 45° into a horizontal position. Huygens did not devise the escapement and made no improvements or innovations to it. Secondly, the earliest extant clock with an anchor escapement is not by William Clement—the clock referred to is not dated; 1671 is its date of acquisition by King's College, Cambridge—but is that supplied by Joseph Knibb to Wadham College Oxford in early 1670 and therefore built in 1668/1669. From this it can be seen that the passing remark 'and Joseph Knibb has also been given credit', is a serious distortion of the facts. It arises no doubt from Purrington's use of totally outdated sources. If 'the evidence favors Hooke, including references in the minutes of the Society from 1669 describing what was probably the anchor escapement', it would be of the highest interest and should have been cited. But no reference to the Royal Society minutes is provided, let alone quoted. Had it been, it might have revealed whether what was mentioned in 1669 had anything to do with Hooke's researches in 1656/1657.

From what Hooke says there is no reason to assume this. 'I contriv'd a way to continue the motion of the Pendulum' [81] is more likely to mean that Hooke had thought of a way of keeping the hand-held and hand-impulsed pendulum used by Galileo, Mersenne, and

Riccioli for timing short-duration observations in physics and astronomy in continuing movement. To do so he may have applied some kind of clock-drive to it; but if so, nothing in the remark suggests of what kind, let alone what kind of controller it employed. To assume that it was a recoil anchor escapement is completely unwarrantable. But, for Purrington, precision of detail is unimportant. What matters is that this 'seems' to be Hooke's 'first original contribution to science' (for which read horology, astronomy, or mechanics) and his conclusion betrays a lack of interest in the details: 'It was, in any case, effectively the first of a very long string of inventions.'

Such an approach clearly will not do, but is alas characteristic. Purrington's treatment of Hooke's work on combustion—'a germ of a theory of combustion to which Hooke would return from time to time' [103]—is another case in point. This is discussed only briefly in half a page and no reference is made to the work already done on it [see Lysacht 1937, Turner 1956]. This leads Purrington to ignore completely the relation of Hooke's work with that carried out by, among others, Boyle, Thruston, and particularly John Mayow whose election to the Royal Society Hooke seconded in 1678 and whom he saw frequently in London between 1674 and 1677 when both were investigating the linked subjects of combustion and respiration. Later, of course, Hooke would accuse Mayow of plagiarizing him [Hunter 2003, 105].

Inaccuracies and inconsistencies unfortunately abound in this book. Some are trivial, some not. Among the former can be noted that Waller's life of Hooke (from the *Posthumus Works*) is not 'appended' to the edition of the diary [Robinson and Adams 1935, 8n2]; that the account of the air pump would have benefited from a close reading of the studies by Shapin [1984] and by Shapin and Schaffer [1985], which although mentioned [58] are absent from the bibliography as are Agassiz 1977 and Webster 1965. Most readers will be baffled by the word 'lagniatte' [20: see *OED Supplement*]. Hevelius was never a Danish astronomer any more than Samuel Hartlib [204] can be considered an 'important' one.

Rather more seriously misleading is the claim (not discussed and not substantiated) that Hooke was an equal partner with Wren in the design of Greenwich Observatory [7, 58n2]. As usual Purrington's claim derives from a secondary source [Willmoth 1993, 183]. But

while one of the relevant entries in Hooke's diary 'At Sir Ch. Wren['s,] order ... to direct Observatory in Greenwich Park for Sir J[onas] More' [Robinson and Adams 1935, 165] makes it clear that Hooke was in direct charge of the construction, there is no evidence that he had any part in the design. To say that Hooke's account of the 'pores or cells' of cork in *Micrographia* Observation xviii [1665] 'represents the first observation of the cellular structure of biological material, in a sense the origin of microbiology' is to risk perpetuating the old myth that Hooke had a concept of the cell as it is understood in modern biology [Hunter 2003, Turner 2005]. For Purrington, Hooke was the first to measure temperature relative to the freezing point of water [28n29, where Celsius appears as Celcius], which may be the case. But Huygens had proposed such a base-point to Sir Robert Moray on 2 January 1665/1666 in his reply to a letter in which Moray described Hooke's thermometer-making. Whether the idea was original to Huygens or Hooke seems to be an open question.

There is, however, no question that the group to whom Purrington refers as the 'Oxford Society', the 'Oxford Club' or the 'Oxford Philosophical Society' 'actually met for four decades' [34]. It did not. Here he has confused the informal circle of *virtuosi* which formed around Wilkins in the mid 1640s with the formally established and so-named Oxford Philosophical Society which met in the newly founded Ashmoleaen Museum from 1683 to 1690 with Robert Plot and John Wallis as its principal members. Purrington's error probably derives from Gunther even though it was corrected by Purver [1967, 126–7], whom he cites elsewhere. In general, in his account of the Royal Society, Purrington follows received wisdom which means that this account is particularly thin for the late 1680s and 1690s, a period which he describes as one of 'malaise' with 'a growing divide between the Newtonians and natural historians' [73]. In fact, we know rather little in detail about this period and this seems to place such division as there was too early by at least two decades. There is a typically exaggerated claim for Hooke: 'as the new decade opened, with his physical and intellectual vigor beginning to decline, Hooke was no longer able to carry the Society on his own' [73]. The constructive work of Southwell as President to maintain the Society during the first half of this decade is ignored. Only Halley receives an appreciative acknowledgement as an active Fellow while Sir John Hoskins, Sloane, and Waller, we are told, 'provided what little direction there

was' [74]. Only Newton's election as president would restore scientific direction. But the Royal Society was not a purely 'scientific' body (in the modern sense that Purrington uses the word) in the late 17th and early 18th centuries. Its problems were as much administrative and financial as intellectual. Purrington's characterization misses the point entirely.

This is the basic problem of the book. Hooke is discussed in a historical context which is partial and largely misunderstood. Only that part of his work is discussed that can be assimilated to the modern context of physics; he is at once implicitly condemned and then excused for not having been Newton, whose mathematical physics is for Purrington the touchstone. 'Newton's legacy' he writes, 'is the modern world' [243]. This is to see the past purely as precursor of the present, not to see it as it was.

Hooke was neither a transitional figure as Purrington presents him (in a sense any historical figure is a transitional figure—the expression means virtually nothing), nor was he a creative mathematician or theoretician. Still less was he, in his own mind, a pioneer of the scientific revolution although he may have had some sense of the difference, even the novelty, of some of the activities in which he engaged. What Hooke was is perfectly expressed by the author in the notice of him in the *Biographia Britannica*, 'an eminent mechanic genius' [1747–1766, 4.2652]. Hooke's career is here presented by Purrington in a splintered, partial fashion, with a constant insistence that Hooke could not concentrate on his scientific work because he was distracted by surveying and architecture. As he sums it up:

> The twenty years Hooke devoted to surveying, building codes and practices, and architecture, were in a way peripheral to his life as an experimental natural philosopher, and yet they were not. No doubt Hooke's success in the mechanical arts specifically architectural engineering, was a direct outgrowth of his understanding of forces and how materials respond to them. This took concrete form in his buildings, of course, but in his famous Cutler lecture *De potential* [sic] *Restituva* of 1678, he explored the physical basis for it. He also undoubtedly advised Wren on questions of the designs of arches and masonry construction, at least implicitly playing a role in the design of the dome of St Pauls. [246]

Ignoring the unsubstantiated, perhaps unsubstantiable, conjectures in the last sentence, one wonders whether Purrington's proposition should not be inverted. Hooke seems to be an archetypical mathematical practitioner, skilled in geometry, drawing and painting, in land-surveying, the design of machines and architectural constructions. He differed from the standard mathematical practitioner, men such as Ralph Greatorex or William Leybourne, by his education, by the fact that he did not teach, and more importantly because he extrapolated from these subjects, influenced no doubt by Bacon, Boyle, and the Royal Society, to build up a rational method by which to frame philosophical hypotheses on the nature of the world—hypotheses which he developed most fully in the study of planetary motion, elasticity, optics, and the nature of the Earth. Formed in both the workshop and the schools, able to draw nourishment from the European wide Republic of Letters, Hooke has a highly unusual, perhaps unique, profile among 17th-century *virtuosi*. Emblematically, we can see his career as the apotheosis of the mathematical practitioner at the same time as it displays the limits of attainment for such men. A full portrait of Hooke in these terms remains to be drawn although it has been adumbrated by Bennett, Chapman, Cooper, and Wright among others. Such a portrait cannot perhaps be undertaken until his remaining known papers, which Purrington mentions but does not exploit, have been fully analyzed and his correspondence collected and critically published. The variety of Hooke's interests and activities requires a far broader treatment than the partial and present-centered account of them given here in a book of which the very title betrays the lack of historical understanding that its pages reveal.

BIBLIOGRAPHY

Bennett, J. A. 1980. 'Robert Hooke as Mechanic and Natural Philosopher'. *Notes and Records of the Royal Society* 35:33–48.

_____ 2003. 'Hooke's Instruments'. Pp. 63–104 in *London's Leonardo—the Life and Work of Robert Hooke*. Oxford.

Biographia Britannica 1747–1766. *Biographia Britannica or the Lives of the Most Eminent Persons Who Have Flourished in Great Britain and Ireland*. . . . 7 vols. London.

Birch, T. 1756–1757. *The History of the Royal Society*. . . . 4 vols. London.

Chapman, A. 2004. *Robert Hooke: England's Leonardo*. Bristol.

Cooper, M. A. R. 1997. 'Robert Hooke's Work as Surveyor for the City of London in the Aftermath of the Great Fire', *Notes and Records of the Royal Society* 12:161–174; 13:25–38, 205–220.

_____ 2003. 'Hooke's Career'. Pp. 1–62 in *London's Leonardo—the Life and Work of Robert Hooke*. Oxford.

Derham, W. 1726. *Philosophical Experiments and Observations of the Late Eminent Dr. Robert Hooke and Other Eminent Virtuoso's of His Time*. London.

Feisenberger, H. A. 1975. ed. *Scientists*. Sale Catalogues of the Libraries of Eminent Persons 11. London.

Hesse, M. 1966. 'Hooke's Philosophical Algebra'. *Isis* 57:67–83.

Hooke, R. 1865. *Micrographia, or Some Physiolological Descriptions of Minute Bodies Made by Magnifying Glasses*. . . . London. Originally published in 1665.

Hunter, M. 2003. 'Hooke the Natural Philosopher'. Pp. 105–162 in *London's Leonardo—the Life and Work of Robert Hooke*. Oxford.

Hunter, M. and Schaffer, S. 1989. edd. *Robert Hooke: New Studies*. Woodbridge.

Keynes, G. 1960. *A Bibliography of Dr. Robert Hooke*. Oxford.

Lysacht, D. J. 1937. 'Hooke's Theory of Combustion'. *Ambix* 1:93–108.

Pugliese, P. J. 2006. 'Hooke, Robert (1635–1703), Natural Philosopher'. *Oxford Dictionary of National Biography*. Oxford. (Online edition May 2006).

Purver, M. 1967. *The Royal Society: Concept and Creation*. London.

Robinson, H. W. and Adams, A. 1935. edd. *The Diary of Robert Hooke. . . 1672–1680*. London.

Shapin, S. 1984. 'Pump and Circumstance: Robert Boyle's Literary Technology'. *Social Studies of Science* 14:481–520.

Shapin, S. and Schaffer, S. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life*. Princeton.

Slaughter, M. M. 1982. *Universal Languages and Scientific Taxonomy in the Seventeenth Century*. Cambridge.

Turner, H. D. 1956. 'Robert Hooke and Theories of Combustion'. *Centaurus* 4:297–310.

Turner, G. 2005. 'Some Observations on Robert Hooke's Use of the Word Cell and on Erroneous Interpretations of Its Significance'. *Bulletin of the Scientific Instrument* 85:32–33.

Waller, R. 1705. ed. *The Posthumous works of Robert Hooke... containing his Cutlerian Lectures and Other Discourses....* London.

Ward, J. 1740. *The Lives of the Professors of Gersham College....* London.

Webster, C. 1965. 'The Discovery of Boyle's Law, and the Concept of the Elasticity of the Air in the Seventeenth Century'. *Archive for History of Exact Sciences* 2:441–502.

Weld, C. 1848. *History of the Royal Society*. 2 vols. London.

Willmoth, F. 1993. *Sir Jonas Moore: Practical Mathematics and Restoration Science*. Woodbridge.

Wood, A. 1721. *Athenae oxoniensis....* New edn. 2 vols. Oxford.

Wright, M. 1989. 'Robert Hooke's Longitude Timekeeper'. Pp. 63–118 in Hunter and Schafffer 1989.

How to Read Historical Mathematics by Benjamin Wardhaugh

Princeton/Oxford: Princeton University Press, 2010. Pp. xiv + 116. ISBN 978–0–691–14014–8. Cloth $22.95, £15.95

Reviewed by
Alain Herreman
Université Rennes 1
alain.herreman@univ-rennes1.fr

How to Read Historical Mathematics de Benjamin Wardhaugh est un livre de 127 pages de petit format (in-12°, 18,7 x 12,7 cm^2).

Sa reliure soignée imite celle d'un petit livre du 18ème siècle. Il s'agit d'un manuel d'initiation à l'histoire des mathématiques qui reprend l'enseignement dispensé par l'auteur en post-doctorat à l'Université d'Oxford (http://www.benjaminwardhaugh.co.uk/).

Le livre comprend une préface et cinq chapitres, une bibliographie commençant par quelques références de « source books » et de livres destinés à l'enseignement puis organisée par chapitres avec une demi-douzaine de références pour chacun.

Les chapitre traitent diverses questions mais qui se rapportent toutes aux textes dont l'étude est au coeur du travail de l'historien:

> « Que dit-il ? »
> « Comment a-t-il été écrit ? »
> « Papier et encre »
> « Les lecteurs » et
> « Que lire et pourquoi ».[1]

Chaque chapitre s'ouvre par un bref document historique, généralement un extrait d'un texte mathématique. Le lecteur est ainsi toujours d'emblée confronté à une source primaire à laquelle toute la suite du chapitre se rapporte.

Les chapitres se composent essentiellement d'un développement servant à introduire un ensemble de questions assez générales et élémentaires que le lecteur est invité à se poser à propos du texte. Des

[1] 'What Does it Say?', 'How Was It Written?', 'Paper and Ink', 'Readers', 'What to Read and Why'.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 12–15

éléments de réponses sont donnés avec aussi, et surtout, des indications utiles pour que le lecteur fasse lui-même ses propres recherches et en confronte les résultats. La question du sens d'un texte, par exemple, posée dans le premier chapitre, est abordée à partir de deux très courts passages, l'un de Tartaglia (1539) traitant des équations cubiques, l'autre d'un extrait du premier lemme des *Principia* dans lequel Newton expose le principe de première et dernière raison. Le lecteur est invité à s'interroger sur les notations et les mots utilisés en les comparant à ceux qu'il utiliserait pour traiter des mêmes sujets.

Le deuxième chapitre introduit la question du contexte d'écriture et de publication d'un texte à partir d'un extrait de la dernière lettre de Galois à Auguste Chevalier. Le lecteur, qui n'a ni la date de la lettre ni le nom des correspondants, est cette fois invité à rechercher dans le texte toutes les informations susceptibles de le renseigner sur son auteur et plus généralement sur le contexte d'écriture de la lettre (où vivait l'auteur ? Quand et où le texte a-t-il été écrit ? Quel est le contexte politique ? Quelle est l'implication de l'auteur et du destinataire dans ce contexte ? etc.). Cela permet d'envisager quelques-uns des liens possibles entre les correspondants, entre cette lettre et son contexte, et d'indiquer les ressources utiles pour avoir des éléments de réponse.

Le troisième chapitre aborde les textes dans leur matérialité. Les documents consistent cette fois en trois photographies d'un livre (une vue de l'extérieur, la page de titre et une double page intérieure), *The Young Man's Book of Knowledge* (1786) de D. Fenning, et d'une page d'un manuscrit de John Pell. Le lecteur est à alors invité à déterminer les caractéristiques de ces documents, à s'interroger sur leurs causes et leurs effets éventuels. L'auteur donne ensuite à nouveau des indications pour obtenir ces caractéristiques matérielles qui ne sont généralement plus accessibles à partir des textes que nous lisons. Remarquons incidemment que la définition donnée des sources primaires [32] conduit l'auteur à considérer comme telles les extraits, pour la plupart traduits en anglais, reproduits dans ce livre ou dans des « source books » (placés dans la rubrique « Sources primaires » de la bibliographie) qui font pourtant perdre bien des aspects pertinents des textes que l'auteur invite par ailleurs à considérer.

Dans le quatrième chapitre l'auteur considère un extrait de la traduction anglaise de 1730 de l'*Analyse des infiniment petits* pour

l'intelligence des lignes courbes de Guillaume de l'Hospital (1696). Il s'efforce de dégager des indices sur le profil des lecteurs de ce texte et introduit ensuite la notion de genre d'un texte (manuel, correspondance, etc.).

Le cinquième et dernier chapitre compare la formulation actuelle du théorème dit de Lagrange (l'ordre d'un sous-groupe divise l'ordre du groupe) et la formulation donnée par Lagrange en 1771. Il s'agit cette fois de dégager les différences entre deux formulations et de s'interroger sur leurs significations. L'auteur peut ainsi aborder l'intérêt, mais aussi les limites, de l'histoire des mathématiques (choix des sources, de la période, etc.).

Des encadrés assez brefs regroupent toutes les questions dégagées pour aborder les thèmes des différents chapitres. Le livre comprend ainsi une vingtaine d'encadrés qui énumèrent par exemple la liste des indications que l'on peut chercher dans un texte sur son contexte historique; les différentes sources (primaires, secondaires et même tertiaires) avec leur caractérisation; les endroits où faire des recherches biographiques (aussi bien en bibliothèque que sur internet); les questions biographiques qu'un lecteur peut se poser; les caractéristiques matérielles auxquelles il convient de prêter attention; les moyens de déterminer les caractéristiques matérielles du texte d'origine; leurs significations possibles; les publics auxquels le texte pouvait être destiné; le genre du texte; les caractéristiques des mathématiques présentées; les changements qu'elles nous font connaître; les raisons qui nous font considérer un texte, etc.

Une conclusion brève, d'une demi-page environ, rappelons que les pages sont d'un petit format, fait le point sur l'apport du chapitre et le situe par rapport aux autres (seul le deuxième chapitre n'a pas de conclusion, à moins que le titre « conclusion » ait simplement été omis [44]). Les chapitres se terminent par quatre ou cinq exercices (« To Think About ») ou ce qui a été vu doit être appliqué à des extraits souvent empruntés aux recueils de Fauvel et Gray [1987] ou de Katz [2007, 2009].

Le livre présente les questions qu'un enseignant peut effectivement adresser à ses étudiants et qu'il est utile que ceux-ci se posent. C'est un sorte de guide méthodologique qui enseigne les bases d'un savoir-faire d'historien. Et c'est bien son intérêt et son originalité:

enseigner l'histoire des mathématiques comme un savoir-faire plutôt qu'en présentant une histoire des mathématiques. Les livres qui enseignent l'histoire des mathématiques le font en effet souvent en exposant simplement, sur plusieurs centaines de pages, une histoire des mathématiques. Celui-ci rompt avec cette apparente évidence. Son plan n'est pas « dicté » par l'histoire des mathématiques mais par les (des) questions qu'il est utile de se poser devant un texte. Répondre à ces questions conduira le lecteur à des lectures variées. L'approche adoptée complètera utilement la représentation des mathématiques, souvent détachée des textes et de leur contexte, que peuvent avoir les étudiants scientifiques (et souvent plus encore leurs enseignants). Le parti pris du livre, procéder par questions et indiquer divers moyens pour accéder à des sources pour y répondre, permet de suggérer en très peu de pages bien plus de directions d'analyses et de réflexion que si l'auteur avait choisi d'exposer ses réponses plutôt que ses questions. Il a ainsi le mérite de ne pas se présenter implicitement comme le livre qui évitera à l'étudiant d'en lire d'autres, mais plutôt comme celui qui l'invitera à en lire d'autres.

BIBLIOGRAPHY

Fauvel, J. et Gray, J. 1987. édd. *The History of Mathematics: A Reader*. London.

Katz, V. J. 2007. éd. *The Mathematics of Egypt, Mesopotamia, China, India, Islam: A Source Book*. Princeton.

_____ 2009. *A History of Mathematics: An Introduction*. 3ième édn. Boston.

Impossible Engineering: Technology and Territoriality on the Canal du Midi by Chandra Mukerji

Princeton/Oxford: Princeton University Press, 2009. Pp. xx + 304. ISBN 978–0–691–14032–2. Cloth $37.50, £26.95

Reviewed by
Pamela O. Long
Independent Scholar
pamlong@pamelaolong.com

The Canal du Midi is a waterway that crosses southwest France north of the Pyrenees and links the Atlantic Ocean to the Mediterranean Sea. It extends for 150 miles (240 kilometers) through a complex and difficult topography and reaches a height of 620 feet (189 meters) crossing the continental divide between the Atlantic and Mediterranean watersheds. Its construction—requiring 100 locks—was completed during the reign of the Sun King, Louis XIV, under the instigation and direction of a tax-farmer named Pierre-Paul Riquet. It represents a heroic accomplishment of French engineering under the aegis of Louis' intrepid minister, Jean-Baptiste Colbert.

Yet Chandra Mukerji's ground-breaking study is a far cry from traditional accounts of heroic engineering. It does provide a detailed, fascinating, and sometimes blow by blow account of the construction history of the canal through all its complicated phases and technical difficulties. But it also represents a fundamental contribution to the study of political power and its relationship to built infrastructures in 17th-century France. Equally important is Mukerji's contribution to methodology as regards the history of early modern engineering.

Impossible Engineering is as much a study of the sociology of knowledge relevant to a large-scale engineering project as it is a study of the engineering itself. Mukerji analyzes the ways in which the idea of the canal was 'imagined as an act of state' [15], a necessary beginning, since such a large-scale project could not be accomplished privately; even the very first steps—indemnifying the necessary land and raising adequate funds—required the power and resources of the state. She examines in detail Riquet's strategies for convincing Colbert to undertake the project, and Colbert's strategies for justifying

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 16–22

it to the king. A crucial issue was the king's desire to dominate the intractable nobles of Languedoc. The idea of royal stewardship over the land (or the *engagement* tradition) became a highly effective rationale for building the canal, and a way to rationalized greater royal control over the region and over the nobles who dominated it.

For France as a whole, Colbert employed a whole string of geographers, engineers, cartographers, and surveyors of various kinds for tasks such as surveying the forests. He also hired military engineers to build fortresses and other specialists to perform geographic functions involving surveying and cartography, essential activities for royal 'stewardship' and control of French territory. In her analysis of these diverse skills, Mukerji takes up a concept that will be central to her account of the Canal du Midi, that of 'distributed cognition'. The knowledge needed for territorial control was not held by one institution or one person but was distributed among diverse individuals from various social classes and included a mix of specific techniques and forms of expertise. These ranged from the mathematical knowledge of academicians to the thorough understanding of local conditions and methods of land and water management that belonged especially to peasants and other local men and women who worked the land.

The numerous decisions that had to be made concerning the Canal du Midi were far from straightforward. For one thing, the project exceeded the formal hydraulic knowledge of the time. Everyone agreed that the waterway needed to connect the Garonne River on the Atlantic watershed to the Aude River on the Mediterranean side. All agreed that the canal would have to cross the continental divide, and that at the high point it had to be fed by water from a higher source that would remain adequate throughout the year even during the summer dry season. If this canal were possible at all, it could be constructed in a number of different ways and along several possible routes. What was certain was that rocks, floods, strong currents, and rapids on one or the other of the two large rivers would present serious problems.

As Mukerji puts it, the project required epistemological work—arguing matters of fact—as well as the labor of construction itself. Ricquet the tax-farmer possessed neither the necessary social standing to legitimate his opinions nor actual engineering expertise. What

he did have was knowledge of the region of Languedoc. And he knew that local peasants and farmers possessed essential knowledge about trees and timber, soils and rocks, streams and small irrigation canals, and how to handle these physical characteristics of the landscape—this through long decades and even centuries of farming and irrigating, constructing water mills, and the like. The tacit expertise of the locals, as well as the knowledge of engineers, surveyors, and others, combined to turn the great experimental project into an actual functional canal.

Mukerji explains that the construction of the canal was divided into two great engineering phases. The first covered the area from the Garonne River at Toulouse and the Aude River near Trèbes, and was the part covered by the contract that Colbert gave to Riquet in 1666. The point of the division was to see if the canal actually could cross the continental divide, using the water supply from the Montague Noire. If Colbert saw that this project succeeded, he could then undertake the second, from Trèbes to the Mediterranean. Mukerji provides many details of the construction history, including discussions of the techniques used or tried. She also details the shifting authority of Riquet and the supervisors sent by Colbert, and their changing relationships with Colbert and with each other. It becomes clear that the construction of the canal was an experiment that went forward in part by trial and error and included serious failures. Riquet used his own fortune to help finance the project, which suffered constant shortages of money. Colbert relied on him but never fully trusted him.

Perhaps the most intriguing and important part of this study is Mukerji's focus on the hundreds of artisans and laborers who worked on the canal. Mukerji's astute investigation of accounts and records has resulted in an in-depth portrait of ordinary workers and their 'indigenous' expertise. Stone cutters, masons, explosive experts, carpenters, surveyors, and what might be called hydraulic workers, including hundreds of peasant women, worked on the canal.

Mukerji shows that Pyrenees women made up the major part of the work force in key areas, especially in the second phase of the project. They labored as haulers carrying dirt up the mountains to fill dam cavities and they also made changes to the canals to create better water flow. These women were experienced in trapping mountain springs, creating reservoirs, diverting water to make meadows,

and constructing settling ponds and sluice gates. Their knowledge of weather patterns, soils, topography, and watersheds allowed them to bring to the project what Mukerji describes as their tacit knowledge of ancient Roman techniques. These included most importantly contour cutting of the canal through the mountains and the use of hydraulic cement in wet areas, such as in the construction of a sea wall at the difficult site of the harbor of Sète on the Mediterranean. Women from the Pyrenees could be paid less, were accustomed to itinerate work, enjoyed specific traditions of autonomy and power, and possessed hydraulic expertise derived from their local management of streams and hydraulic sites in the difficult mountainous environment of the Pyrenees.

By viewing the construction of the Canal du Midi as a complex process of political and territorial politics, and by taking account of the input of a great variety of skilled workers, Mukerji's book should have a significant influence on the study of premodern engineering. No longer can it be taken as a given that such projects were top-down enterprises in which relatively insignificant workers carry out clear directives from above. The complex conditions that Mukerji describes would undoubtedly have prevailed in many premodern construction sites, especially those involving difficult technical issues. The knowledge and the skills of local artisans and laborers may have amounted to crucial contributions to both the plan and execution of the work—particularly before the advent of professional engineering. Mukerji's book provides insight into the complicated practices of the premodern worksite in terms of both skill and knowledge.

The Canal du Midi pitted 'new Rome', that is, the expanding centralized state, against old Gaul, including the sometimes intransigent nobles and populace of Languedoc who frequently resisted the centralizing, 'modernizing' monarchy. Yet the new Rome that was embodied in the Sun King and Colbert's view of the ideal monarchy were furthered, Mukerji argues, by the local populace of Languedoc or, more specifically, by their knowledge of ancient Roman techniques. Mukerji suggests that ancient Roman ruins in southern 'Gaul' remained to be learned from. The local populace, she suggests, transmitted 'classical' techniques generation after generation as it used Roman ruins as quarries for its own structures and absorbed Roman building techniques in the process.

Mukerji's repeated references to the peasants of Languedoc as 'new Romans' using 'classical' building techniques to bring the Canal du Midi to completion is in my view an oversimplification. For one thing, whatever Roman techniques they were transmitting were hardly unique to them. Ancient Roman building techniques were very widely known by the late 17th century. The *De architectura* of Vitruvius had been a focal point of analysis and discussion for more than two centuries, including a rich tradition of commentary. Ancient Roman buildings and building techniques had been studied extensively in books, but they had also been studied in the actual structures and ruins of structures by humanist antiquarians, artists, engineers, architects, and artisans in Italy and elsewhere at least since the early 15th century. If local peasants, masons, and carpenters in Languedoc knew how to build a wall in the Roman fashion through a centuries-long tradition of handed-down practices, as Mukerji mentions early on [64–66], so also did builders, architects, and antiquarians in Paris, Rome, and elsewhere know how to build such a wall. Roman techniques such as the use of hydraulic cement and the cutting of graded canals for even water flow had been used in the Mediterranean basin by other artisans, for example, in 16th-century Italy. Although some of the techniques used by the Languedoc artisans may have had Roman origins, others may have developed out of their own technological circumstances, and still others may have come from elsewhere—say the Iberian peninsula which possessed centuries-long hydraulic traditions. Mukerji provides few references to medieval and late medieval construction and hydraulic techniques and projects in Languedoc itself, making it uncertain in general how Roman techniques may or may not have been combined with other techniques and practices.

Taking Mukerji's account as a starting point, I would surmise that the unique knowledge that local workers possessed would not have been Roman building techniques *per se*, but rather a close knowledge of local geophysical conditions, including soil, terrain, and hydraulic conditions combined with a rich took-kit of tried and true methods of working with those conditions, some perhaps derived from the ancient Romans, some from other sources, including their own inventions. It is also possible that Riquet and the other supervisors of the Canal du Midi were already familiar with these techniques both from their knowledge of Languedoc and from practices in other

regions. Although Mukerji's emphasis on the local and tacit knowledge of peasant workers and its importance for the building of the canal is entirely convincing, her repeated description of these workers as the 'new Romans' using 'classical' techniques oversimplifies a more complex reality.

A related issue is that Mukerji pays little attention to hydraulic activities in Italy, the Netherlands, and elsewhere in France that preceded or were concurrent with the work of the Canal du Midi and may have influenced aspects of the canal's design and construction. She does mention that one of Colbert's overseers, an engineer named Pons de La Feuille, visited the Netherlands and recorded Dutch and Flemish sluice-opening mechanisms, locks, and lock walls. Yet there is insufficient follow-up concerning the specific influence of Dutch hydraulics on the Canal du Midi. In Italy, canal building in the Po River valley and elsewhere in northern Italy, and hydraulic engineering activities in other parts of Italy including Rome itself, constituted flourishing enterprises in the two centuries before the construction of the canal in Languedoc. These as well as the numerous hydraulic systems of the Iberian Peninsula are outside the purview of this book—and yet seem to this reviewer to be pertinent to the broader context of the great construction project in Languedoc. Given the relative mobility of engineers and artisans of various skills, it seems likely that these regions contributed hydraulic knowledge to the project, either directly or indirectly.

Throughout and in a final chapter, Mukerji describes Riquet's changing role vis-à-vis Colbert, as well as Riquet's changing self-perception, which ended in his view that he was beholden only to God in his inspiration to undertake and carry out the project. Mukerji's portrait of Riquet is combined with a focus on the difficult steps involved in actually completing the canal. The final chapter summarizes the sociological perspective that frames the book—the pursuit of natural knowledge as impersonal truth, territorial politics involving land management, principles of stewardship and material improvement, and material techniques.

Mukerji's book is beautifully illustrated by a number of photographs of the Canal du Midi at various points, most taken by the author. In sum, this is a masterful analysis that promises to exert an important influence both on historians of science and technology and

on historians of the early modern state. The canal itself is brought to life both as a project in state-building and as an immensely difficult, multi-phased, large-scale engineering project. It was heroic engineering indeed—'heroic' now expanded to apply to a multitude of workers, supervisors, experts, administrators, and entrepreneurs, to a Jean-Pierre Riquet who occupies a far more fraught and ambiguous position than before—and to the complex relationships among them all.

Thomas Bradwardine, « Traité des rapports entre les rapidités dans les mouvements » suivi de Nicole Oresme, « Sur les rapports de rapports ». Introduction, traduction et commentaires by Sabine Rommevaux

Paris: Les Belles Lettres, 2010. Pp. lxvi+190. ISBN 978–2–251–18109–7. Cloth €29.00

Reviewed by
Edith Dudley Sylla
North Carolina State University
edith_sylla@ncsu.edu

Thomas Bradwardine's *Tractatus de proportionibus velocitatum in motibus* was published in a modern edition together with an English translation by H. Lamar Crosby [1955]. Nicole Oresme's *De proportionibus proportionum* was published with an English translation by Edward Grant in 1966 in the same University of Wisconsin Press series. Here Sabine Rommevaux has published French translations of the two works based on the editions by Crosby and Grant.

Bradwardine's *Tractatus de proportionibus* was adopted as a university textbook in England and on the Continent for the two centuries after its first appearance in 1328. Its impact certainly resulted in part from the fact that it gave a mathematically elegant expression to what Aristotle had said about the relations of movers, moved bodies, and velocities, but also because it contained a primer on the mathematics of ratios and proportions, useful introductory knowledge for undergraduate students. Oresme's work followed Bradwardine's but contained creative elaborations of its basic mathematics and applied Bradwardine's rule for the relations between forces and resistances, on the one hand, and the velocities they produce, on the other, to show that the motions of the planets are most probably incommensurable, thus undermining the basic premiss of astrology that when a given configuration of the planets is repeated their combined effect on Earth will be the same. (If the motions are incommensurable, the configurations will never be repeated.) At roughly the same time, Albert of Saxony composed his own *Tractatus proportionum*, covering the same subject matter more succinctly. In later periods,

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 23–29

Albert of Saxony's shorter work was sometimes included, rather than Bradwardine's, in compilations of basic logical, natural philosophical, and mathematical texts for university students that sometimes also included works on the 'latitude of forms' descended from Oresme's *Tractatus de configurationibus qualitatum et motuum* (a work made available in a Latin text and English translation by Marshall Clagett in 1968 in the University of Wisconsin Press series).

On the basis of her extended studies of medieval and early modern theories of ratios, Sabine Rommevaux's mastery of Bradwardine's theory is considerably greater than that of Crosby in 1955 and somewhat beyond that of Grant in 1966; and so scholars may well want to consult her introduction and notes to this book as well as her forthcoming book, *Théories des rapports (XIIIe–XVIe siècles). Reception, appropriation, innovation* [2011]. The introduction to the book is clear and largely uncontentious. Whereas previous scholars have struggled with such controverted subjects as the role and significance of the concept of 'denomination' in medieval theories of ratios, Rommevaux states simply that the denomination of a ratio (when it exists) is the integer or integer plus fraction by which it is named, as the denomination of a double ratio is 2 and the denomination of a sesquialterate ratio, i.e., 3:2, is $1\frac{1}{2}$.

As far as I can see, Rommevaux's translations will be useful mainly for Francophone students who are not fluent in medieval scholastic Latin and who would find a French translation easier to read than an English one. For Anglophone readers, the existing English translations will be preferred even if they have a few imperfections.

In choosing French translations of Latin words, Rommevaux tries to avoid misleading cognates. For instance, she translates *velocitas* by the French *rapidité* so as to distinguish the medieval notion of *velocitas* from the post-Galilean notion of *vitesse* [ix n1]. While the point is frequently made that in modern physics velocity is a vector and not a simple magnitude, so that using the word 'velocity' may mislead those used to modern terminology, I question Rommevaux's comment that for most medievals *velocitas* was considered as a quality of motion or of the thing moved. For those authors like William Heytesbury and Richard Swineshead who followed William of Ockham's ontological minimalism, motion is not a qualitative form but instead simply a shorthand way of referring to a situation in which

something does not remain in the same place or position over a period of time. For Oresme himself, motion is not a quality but a mode. It seems to me that avoiding translations using words that have special connotations in later science is not as straightforward as Rommevaux seems to think. Would one not allow Aristotelians to say in translation that the heavens are made of the element ether (or aether) because in 19th-century physics the word 'ether' was repurposed to mean what carries light waves?

This debate about words has a striking instance in Rommevaux's choice to write of what is commonly called 'Bradwardine's law of motion' not as a law (*loi*) but as a rule (*règle*) because it does not have to do with a physical law in the sense understood starting in the 17th century [xii]. If I am not mistaken, this is letting Descartes be the arbiter of the meaning of 'laws of nature', despite the medieval use of the Latin phrase '*lex naturae*' ('law of nature'), for example, in Jean Gerson's statement: 'Lex naturae est in rebus creatis regulatio motuum et operationum et tendentiarum in suos fines' [Oberman 1975, 425n47]. Similar questions might be raised about words for *force*. Rommevaux criticizes Marshall Clagett for his distinction between medieval dynamics and medieval kinematics on the grounds that dynamics presupposes a notion of force which is absent from Bradwardine's treatise [xlvii n91]. While I would agree that the Calculators' distinction between measures of motion with respect to cause and measures of motion with respect to effect does not map exactly onto the distinction between dynamics and kinematics (especially for alterations where qualitative forms are both causes and effects), Rommevaux's translation policy leaves the student of 14th-century physics tongue-tied if no word whose meaning has evolved into something different can be used in describing their work. Moreover, changing what the texts literally say carves in stone the judgment that the ideas of these medieval authors were not in any way like those of 17th-century scientists.

Finally, coming at the difficulties of translation from the opposite direction, I wonder that Rommevaux, while being careful to avoid anachronism, simply uses the modern term *rapport* to translate *proportio*, when a modern *rapport* or ratio is normally identified with a rational number or fraction, whereas the medieval *proportio* as used by Bradwardine and Oresme emphatically was not but always remained a relation between two quantities.

But the value of this volume goes beyond the translations into French of the two texts (and little harm is done in translating *velocitas* by *rapidité*, when one knows that this has been consistently done). Rommevaux's notes identifying the sources cited by Bradwardine and Oresme provide interesting food for thought in themselves. For instance, the frequency with which Bradwardine cites Averroes' statements, the fact that he locates passages in Aristotle by the associated comment number in Averroes, and his seeming identification in places of Aristotle's view with that of Averroes, all seem to show the importance of Averroes' commentaries as background to Bradwardine.

Moreover, that Rommevaux compares Bradwardine's statements with those of Oresme on a series of topics may also cast new light on the concepts and purposes of the two authors. If Bradwardine drew upon preexisting theories of ratios and proportions to provide the grounding for his law, was his approach enabled by the fact that he used Campanus' version of Euclid's *Elements*, or by the pre-existing application of ratios in music, or by theories of ratios found in Arabic works translated into Latin such as Ahmed Ibn Yusuf's *Epistola de proportione et proportionalitate*? Bradwardine's mathematics of ratios makes most sense if one thinks only of ratios of greater inequality and if in compounding ratios one always deals with ratios having terms in common such that the middle terms are less than the greater extreme and more than the lesser extreme. For instance, one compounds $A{:}B$ with $B{:}C$ and with $C{:}D$ to get $A{:}D$, where $A > B > C > D$.[1] In the mathematics of musical harmony, where one may add intervals between tones to get harmonies between more distant tones, one always continues to have the separate tone-producing strings, as when one string and another twice as long, struck together, produce the harmony of the octave. In such a situation, the relation of the two strings is the same whether one thinks of the shorter to the longer or of the longer to the shorter, i.e., thinks of 2:1 or of 1:2. Nicole Oresme, in order to extend Bradwardine's approach to compounding ratios to ratios of lesser inequality, proposes to reverse the relations between whole and part. Thus, in ratios of greater inequality, 4:2 compounded with 2:1 produces 4:1 and thus the whole 4:1 is greater than the parts 4:2 and 2:1. But in ratios of lesser inequality,

[1] On page xix, there is a misprint where $C{:}D$ is repeated: Rommevaux intends $(A{:}B) = (B{:}C) = (C{:}D)$.

where 1:2 compounded with 2:4 produces 1:4, the result, 1:4 is less than the parts 1:2 and 2:4. Rommevaux has a sound discussion of Oresme's proposals on this subject, referring to an article by Paul Rusnock and criticizing earlier historians who represented Oresme's ideas using fractional exponents and who concluded as a result that Oresme's understanding was erroneous.

One issue that lurks in the background here is the interaction and relative importance for Bradwardine and Oresme of theories of ratios applied to numbers *versus* theories that apply to continuous magnitudes. The concept of 'denomination' is one that fits with ratios of integers, where there are names for ratios that draw upon names for numbers but not for ratios of incommensurable quantities, such as ratios between lines that are incommensurable with each other. In the end, Bradwardine's theory and his whole approach to ratios and proportions would be undermined by the choice to identify ratios with the denominations associated with them and by the choice to extend the concept of number to include rational numbers (fractions) and eventually real numbers. At that point, treating the compounding of ratios as addition, as Bradwardine and musical theory did, would become problematic.

Rommevaux began her serious study of ratios with a book on Clavius [2005], but Clavius represents the situation after the identification of ratios with a broader concept of numbers and after the rejection of the approach taken by Bradwardine and Oresme. More recently she has edited the questions of Blasius of Parma on Bradwardine's *De proportionibus*, but Blasius too, at least in one version of his questions, is someone who rejects Bradwardine's and Oresme's approach to the composition of ratios as addition.

Oddly, from my point of view, in discussing the *posteriorité* of the movement started by Bradwardine, Rommevaux moves from Bradwardine to Oresme and then to those who reject Bradwardine's approach, including Blasius of Parma, Giovanni Marliani, Alessandro Achillini, and then Clavius [lxiv], while forgetting about Richard Swineshead and John Dumbleton, who certainly must be counted among Bradwardine's most important heirs. To me, this appears to result from a certain bias toward the Continent, perhaps natural in a book whose *raison d'être* is French translations, but nevertheless an incomplete picture of what happened.

This book, then, while primarily useful to those who would like a French translation of the two works included, also provides a judicious interpretation of the meaning and significance of the two texts, which will be useful for future scholarly research. It is not the best source for the historical context. On the second page [x n4], it makes a silly mistake in stating that Bradwardine was a member of the order of Augustinian Hermits, whereas in fact he was a secular who may have held 'Augustinian' positions on some theological topics. I doubt the suggestion in the same note that Bradwardine's *De causa Dei* was the result of Bradwardine's teaching while Chancellor of St Paul's in London. For one thing, Bradwardine held that position only between 1337 and 1339, and the *De causa Dei* is far too long and complicated a book for much if any of it to have been delivered as lectures, even supposing that as Chancellor Bradwardine would have taught so advanced a course. I likewise doubt the assertion [xi] that Bradwardine's *De continuo* refuted mathematically the possibility that a continuum may be composed of indivisibles and thus closed a debate among Oxford masters that had been going on for several years. These, however, are minor points, easily ignored while concentrating on the mathematical content of the works on which Rommevaux's judgments are much more deserving of confidence. Thus, Rommevaux has here established a reliable picture of the work of Bradwardine and Oresme, which should be useful in working towards a wider historical overview, looking both earlier to Arabic and Latin as well as Greek works, and later to those who worked in the same tradition as Bradwardine and Oresme and to those who rejected it.

BIBLIOGRAPHY

Clagett, M. 1968. *Nicole Oresme and the Medieval Geometry of Qualities and Motions: A Treatise on the Uniformity and Difformity of Intensities Known as* Tractatus de configurationibus qualitatum et motuum. Madison, WI.

Crosby, H. L. 1955. *Thomas Bradwardine. His* Tractatus de Proportionibus*: Its Significance for the Development of Mathematical Physics.* Madison, WI.

Grant, E. 1966. *Nicole Oresme,* De proportionibus proportionum *and* Ad pauca respicientes. Madison, WI.

Oberman, H. 1975. 'Reformation and Revolution: Copernicus' Discovery in an Era of Change'. Pp. 397–429 in J. E. Murdoch and E. D. Sylla edd. *The Cultural Context of Medieval Learning.* Dordrecht/Boston.

Rommevaux, S. 2005. *Clavius. Une clé pour Euclide.* Paris.

_____ 2011. *Théories des rapports (XIIIe–XVIe siècles). Reception, appropriation, innovation.* Turnhout, Belgium. (in press)

The Worlds of Oronce Fine: Mathematics, Instruments and Print in Renaissance France edited by Alexander Marr

Donington, UK: Shaun Tyas, 2009. Pp. xvi + 224. ISBN 978–1900289–96–2. Cloth £40.00

Reviewed by
Bernardo Mota
Universidade Lisboa/Technische Universität Berlin
bernardomota@campus.ul.pt

This is a collection of papers first presented at a conference entitled 'The Worlds of Oronce Fine: Mathematics, Instruments and Print in Renaissance France' and held in the School of Art History, University of St Andrews, 12–14 May 2006. Its goal is

> [to] bring this much neglected polymath [Oronce Fine] to the attention of a new audience. The essays gathered here aim to cast fresh light on Fine and his myriad activities, placing him within the broad socio-intellectual context of Renaissance Europe and demonstrating his important contribution to the worlds of mathematics, instruments, and print. [9–10]

The introduction [ch. 1] and epilogue [ch. 13] are excellent in unifying the content of the essays. Alexander Marr (introduction) briefly describes the scholarship about Fine and explains the need to reevaluate the role of this mathematician. Fine's biography is presented and a short abstract of each paper is added. Stephen Clucas (epilogue) gathers the main ideas stressed through the book.

Chapters 2 and 3 show how Fine fought for a strong institutional and epistemological foothold for 'embedding mathematics in sixteenth-century French intellectual culture' [10]. Isabelle Pantin looks at the material context of Fine's teaching (his appointment, the teaching of mathematics at the Collège Royal, the program of studies that he promoted) and Angela Axworthy looks at Fine's epistemological views on the status of mathematics, inscribing them in the tradition that extends from Antiquity to the celebrated *Quaestio de certitudine mathematicarum*. Fine's opinion is a commonplace firmly rooted in ancient and modern authors (Ptolemy, Proclus, Regiomontanus); still, it clarifies the program and approach that Fine

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 30–33

intended to put into practice. Axworthy's essay should be paralleled with section 5 of the preceding chapter ('Defending Mathematics' [24]) and is, in a way, complemented by chapter 4.

The next six chapters deal with what may be called Fine's 'theoretical practical mathematics', if one wants to use Mosley's expression. These chapters emphasize Fine's focus on practical, and not pure, mathematics, but stress that although concrete problems are addressed, they are always presented in a theoretical manner and within a bookish tradition that treats applied mathematics as an 'affair of paper, print and drawings', to use Dupré's expression [82]. One might say that this group of papers considers Fine's practical mathematics as *epistēmē* and not as *technē*. The latter is to be found in the instruments made by Fine; the former always includes theoretical reasoning. Fine comes out as a pragmatic mathematician able to negotiate theory and mathematical practice.

In the fourth chapter, Jean-Jacques Brioist inscribes Fine's practical geometry in the two traditions of practical geometry that existed before: the one initiated by Hugh of St Victor, which focused on the 'use of instruments in solving practical geometrical problems' [54], and the one which originated with Italian abacus books and dealt with problems of measuring lines, surfaces, and volumes (height of a tower, breadth of a river, depth of a well, for instance). The author examines the concept of 'practical mathematics' and emphasizes that this practical geometry is idealized; this can be seen, for instance, in the illustrations which show the problems faced by the men making actual measurements. The usefulness of mathematics is a commonplace referred to by mathematicians since Antiquity, and this chapter can be paralleled with the previous one on the status of mathematics.

The fifth chapter focuses on *De speculo ustorio*, one of the first works on optics to be published in France, and explains its sources and the influence it had in Italy. Sven Dupré uses *De speculo ustorio* to determine where is the correct place between theory and practice to assign Fine's work. He concludes that practical mathematical knowledge is not the same as material knowledge because the instruments which Fine presents are truly mathematical objects, and that the work itself belongs to a long bookish tradition and should not be interpreted as the craft of the mirror-maker [82].

Chapter 6 searches for the sources and influence of *De solaribus horologiis*, whether in real instruments or books. Catherine Eagleton shows that Fine was able to correct some inaccuracies of previous works and brought 'some geometrical order to the enormous variety of sundials available' [89]. The pioneering role of Fine is stressed: through the compilation of *De solaribus horologiis*, Fine helped to create a new genre, the sundial book. The theme is very interesting and some additional bibliography on sundials would be most welcome.[1] Anthony Turner [ch. 12] further deals with Fine's waterclock, one of his most famous inventions, referring to earlier descriptions of the clepsydra and to the interest in it up to the 18th century.

Chapters 7–9 deal with Fine's cosmography, geography, and cartography. Jean Marc Besse deals with the definition of cosmography and geography in the Renaissance context; Adam Mosley proposes

> [to] explore two related issues raised by this text [= *De Cosmographia, siue sphaera mundi*]: the character and significance of Fine's cosmographic work, and the nature of the Early Modern genre of which it was such an apparently successful example. [114]

Brioist explains Fine's complex cartographical methods, which mix up several techniques that do not match the methods described in his mathematical works.

Chapter 10 explains the true agenda underlying Pedro Nunes' criticism of Fine. Henrique Leitão convincingly shows that Nunes needed to establish his credentials as a young mathematician. Nunes' criticisms are, therefore, not only a piece of scientific refutation but a self-promoting *libellum*.

Chapter 11 shows Fine's importance by looking at his influence in the French algebraic tradition. There follows an index of names and subjects and some 48 pages with images of various kinds (instruments, pages from books, schemes, maps).

The absence of monographs and the low number of articles about Fine have made him a forgotten character. However, this modest academic output does not match Fine's influence. This book fills the

[1] A trivial editing mistake erased the picture of Clavius' work on gnomonics referred to on page 97; on the previous page, the Columba of Kircher is referred to as 'fig. 6.6', while one should read 'fig. 6.7'.

lacuna and reveals Fine's importance by looking at his pivotal and triggering role in the mathematical culture of the 16th century. It is not about saving Oronce Fine's ability as an original mathematician, which he was not, but more about stating how much he can help us understand the mathematical culture of the 16th century, which he definitely helped shaping. Fine's failure as a top mathematician appears counterbalanced by his prolificacy and success as a polymath. Stress is put, on the other hand, on his success in establishing roots for the mathematical developments that were still to come. He was the teacher of mathematicians as famous as Pierre de la Ramée, Jacques Peletier, or Pierre Forcadel, who were to pave the way for Descartes and Viète; he wrote or edited 74 books, ranging across practical geometry, arithmetic, gnomonics, optics, music, astronomy, and cosmography; he is quoted by authors across Europe and earned, more than suffered, the criticisms of Pedro Nunes or Cristopher Clavius, who took advantage of his influence and editorial success to expand their own mathematical reputations. This is the perspective one finds here: no big claims, just a shift in perspective and a factual look at the materials available.

The result is an unpretentious book, clearly organized and broad enough to cover all fields of mathematics to which Fine dedicated himself. The reader will find an overall perspective of Fine's activities, roots, goals, achievements, influence, and context. What comes out is a highly productive scholar, deeply committed to bring mathematics back to life in academic institutions and society, an innovator (he was the first man to print books in France on topics such as burning mirrors) who was not afraid to address difficult mathematical problems or to provoke his fellow colleagues, a man striving to balance family life with work.

Although the book is a collaborative work, the structure of the essays is common, always relating content and context by presenting the sources, influence, and background of Fine's work. The editorial work is excellent; the introduction and epilogue unify the whole; all this, and the high standards of the contributions, give the book a high degree of unity and quality. As stated in the first paragraph of this review, new audiences are especially addressed and for this reason the mathematics never gets too technical.

Simplicius: On Aristotle, On the Heavens 3.1–7 translated by Ian Mueller

London: Duckworth, 2009. Pp. viii + 182. ISBN 978–0–7156–3843–9. Cloth £60.00

Reviewed by
Sean Michael Pead Coughlin
University of Western Ontario
scoughli@uwo.ca

There has been a recent revival of interest in the latter books of the *De caelo*, where Aristotle develops his account of the four sublunary elements.[1] Additionally, the work of Richard Sorabji and others for the series Ancient Commentators on Aristotle, now over 90 volumes, has re-invigorated the study of the reception of Aristotelian cosmology and Aristotelian approaches to natural science in general. Ian Mueller's English translation of Simplicius' *On Aristotle,* On the Heavens 3.1–7 is a valuable addition to both of these projects. Simplicius' commentary on *De caelo* is an important historical source for the background, reception, and development of Aristotle's views on the material elements of sublunary bodies. Mueller's clear translation and introduction have made this source accessible to a wider audience, while at the same time providing the specialist with thoughtful textual suggestions, notes, and references.

Simplicius (AD *ca* 490–560) was one of the last Neoplatonist commentators, writing his commentaries after the Academy in Athens was closed by Justinian in AD 529 [1]. His commentary is the only commentary on *De caelo* extant (there is a paraphrase by Themistius preserved in a Hebrew translation of a lost Arabic version that was itself translated into Latin), and now nearly the entire work has been translated for Sorabji's Ancient Commentators series. Most of the commentary on book 1 was translated in three volumes by Hankinson [2002, 2004, 2006], while the commentary on book 2 was translated in two volumes by Mueller [2004, 2005]. This new volume, covering most of the commentary on book 3, and a seventh volume, on *De*

[1] See especially Bowen and Wildberg 2009.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 34–40

caelo 3.7–4.6 also by Mueller,[2] completes the English translation of Simplicius' *De caelo* commentary.

The volume contains an introduction by Mueller, the translation, an appendix on the argument at *De caelo* 303b13–304b11, a list of textual questions (emendations), notes to the translation, a bibliography, and an English-Greek glossary. Also included are a Greek-English index, an index of names, a subject index, and several addenda, the most important of which lists quotations of Alexander of Aphrodisias in this volume that are included in Andrea Rescigno's collection [2008] of fragments of Alexander's lost commentary on the *De caelo*.

In *De caelo* 3.1–7, Aristotle begins his investigation of the sublunary bodies and their elements by looking to the accounts of his predecessors: the Eleatics, the material monists and pluralists, and Plato. These initial chapters primarily contain Aristotle's criticisms of these earlier views, but the criticism is always constructed with the aim of giving a positive account of bodies and their elements [26: cf. *De caelo* 298b1]. This positive account is not delivered until *De caelo* 4.4, when Aristotle completes his demonstration of the elements as simple natural bodies differentiated by the simple motions up and down and generated from each other by reciprocal substantial change. By the end of 3.7, however, Aristotle has made some progress towards his goal: he has proposed a definition of 'element' and argued that the elements must be generated from each other.

Aristotle opens chapter 1 by criticizing the view in the *Timaeus* that body itself is generated from simpler mathematical parts. In chapter 2, his aim is to establish that the simple bodies have weight or lightness. In chapter 3, he defines an element as that 'into which other bodies are divided and which inhere[s] <in bodies> ... and which itself cannot be divided into things different in kind' [76: cf. *De caelo* 302a14–15], and claims that simple bodies meet this definition. In chapters 4 and 5, Aristotle discusses the number of elements and concludes against Democritus, Anaxagoras, and the Presocratic monists, that there are finitely many elements but more than one. In chapter 6, Aristotle claims that it will become clear how many elements there are and what they are like by determining whether the elements are eternal or are subject to generation and corruption.

[2] [Ed.] See the review by Pierre Pellegrin on pp. 41–42.

He concludes that the elements are generated from each other; and, in chapter 7, he begins a discussion of how this takes place—whether by separation from mixtures as Democritus and Empedocles think or by reciprocal transformation as Plato holds [e.g., *Tim.* 53e2–54d3]. Our volume ends in the middle of the commentary on chapter 7, where Aristotle refutes the Democritean and Empedoclean account of generation as the separating out of elements from mixtures.

Simplicius' commentary on *De caelo* 3.1–7 is exegetical in form, and his exegesis relies heavily on his (and other Neoplatonists') belief that the positions of Plato and Aristotle are in harmony. Beginning from a *lemma* (a short section of text), Simplicius will typically summarize Aristotle's position, often explaining how the *lemma* is related to what Aristotle said before and supplementing Aristotle's elliptical assertions about his predecessors' positions with longer quotations from their works. (Many of the quotations in Simplicius' commentaries are our only source for the writings of the Presocratics.) Simplicius then discusses different ways in which Aristotle's arguments have been interpreted before introducing his own reading. The interpretations which he rejects are frequently those of Alexander of Aphrodisias, the second century Peripatetic commentator from whose lost commentary on *De caelo* Simplicius cites extensively. Alexander, according to Simplicius, exaggerates the disagreements between Plato and Aristotle. To show their agreement, Simplicius asserts that Aristotle's criticisms are aimed at the superficial or literal meaning, but not the true or intended meaning, of Plato's text, and then elaborates on what he thinks this true and intended meaning is.

The commentary, then, both documents the reception of Aristotle's natural science and expresses sixth-century Platonist philosophical views, and Mueller has done a reasonably good job at balancing both aspects of this text. He accomplishes this by applying a kind of exegetical parsimony to his introduction and notes. Mueller confines his discussions of the commentary's historical and philosophical context almost exclusively to what is presented in the text: Aristotle, his targets in *De caelo* 3.1–7, Alexander of Aphrodisias, and Timaeus of Locri.[3] His translation, as well, closely follows a type-correspondence of one English word for one Greek word. The effect of this style is that the reader is free to interpret an English text in

[3] The discussion of Timaeus of Locri is very interesting and rewards attention.

a manner as close to interpreting the original Greek edition as possible. For those readers who want to understand Simplicius' aims and methods as a Neoplatonist commentator, he invites them to consult Hankinson's introduction to the first volume of *On Aristotle's* On the Heavens 1.4–9 [2004] as well as Han Baltussen's *Philosophy and Exegesis in Simplicius: The Methodology of a Commentator* [2008]. One might also do well to look at Phillipe Hoffmann's 'Sur quelques aspects de la polémique de Simplicius contre Jean Philopon' [1987].

Mueller's translation is accurate and literal, and in general a pleasure to read. This in itself is a great achievement. He follows closely Heiberg's text [1894] with a few reasonable emendations, most of which he carefully notes, often including a translation of the alternative text. This style of translation makes the volume an excellent aid for those with a moderate understanding of ancient Greek or for those who are approaching Simplicius' writing for the first time. The introduction includes a summary of *De caelo* 3.1–7, summaries of Simplicius' claims concerning other authors mentioned in the commentary, and a discussion of the text that he used to prepare this translation. The notes provide clarifications of obscure arguments and are mostly very helpful. Mueller has also done an excellent job of collecting cross-references and pointing the reader to the texts in which Simplicius or someone whom he mentions makes a particular claim under discussion.

The volume also includes whole passages of Aristotle's *De caelo* translated by Mueller from Moraux's edition [1965], where Heiberg's text and the manuscripts have only *lemmata*. This is helpful for the reader, as it unifies the diction between the two texts, making it easier to connect Simplicius' commentary with the *De caelo* itself. However, including a translation of the whole of Moraux's text into a translation of Heiberg's might suggest to the reader, perhaps artificially, that our text of *De caelo* is the same text Simplicius read when he composed his commentary, a suggestion that would, I think, need defending. As Mueller points out [19], the *lemmata* found in A (Heiberg's favored ms.) represent only about 10% of the *De caelo* [121]; assuming that the *lemmata* were not added by a later editor, this 10% is the *most* we could know of what Simplicius read. Mueller, however, is sensitive to this worry: the text of *De caelo* not found in the manuscripts is marked with square brackets, and in his 'Textual

Questions' [120–121], Mueller lists passages where it appears to him that Simplicius read something other than our text.

The inclusion of all of Aristotle's *De caelo* raises a further question of interpretation. I said earlier that Simplicius' commentary both documents the reception of Aristotelian science and expresses a form of sixth-century Platonism. Overall, I think Mueller has balanced both aspects of the commentary; however, to offer one criticism of this volume, I think that Mueller occasionally emphasizes the former aspect at the expense of the latter, obscuring the content of some of Simplicius' arguments. Mueller asserts on several occasions in his introduction that Simplicius is 'completely committed to the idea that Aristotle understands and agrees with Plato and that [Aristotle's] criticisms of Plato are directed against a superficial reading of Plato's text' [11: cf. 2, 4]. This view has become common in some circles; but in the context of an otherwise objective and parsimonious translation and notes, it seems out of place. It is one thing for *Simplicius* to *say* that he believes that Aristotle understands and agrees with Plato, and quite another thing for *us* to *assert* that he is committed to this idea. Mueller has not made a case for this latter view, and I do not think the text bears it out.

For instance, in chapter 1, Aristotle criticizes Plato for claiming in the *Timaeus* that the elements are generated from indestructible and indivisible planes existing actually in the bodies they compose [e.g., 36: cf. 299a2–11]. The main force of the criticism is against what he takes to be a Democritean streak in Plato, namely, that generation is the same as composition from indivisible parts. Aristotle, in contrast, maintains that generation (either from the elements or of the elements) is a case of substantial change resulting from the union of matter and form through the action of an efficient cause.

Simplicius, interestingly, agrees with Aristotle that

> those who say that bodies are composed of planes or planes of lines or lines of points do not say that they are composed as if from matter and form, but as if from those things [*scil.* planes, lines, points] as parts. [48: cf. Heiberg 1894, 573.15–21]

Yet, Simplicius defends Plato and the Pythagoreans by reading the Aristotelian distinction between composition from parts and generation from matter and form back into the *Timaeus* and the Pythagoreans. Simplicius, against Alexander, claims 'that we [*scil.* Platonists]

also make a body with qualities from matter and form' [53: cf. Heiberg 1894, 579.5]; and he concludes his discussion of the generation of bodily elements from mathematical entities by claiming,

> It is clear that these people [*scil.* the Pythagoreans, but Plato is also implied] said that things are composed from numbers on the grounds that numbers pre-contain [περιειληφότων] in themselves all the forms in a fundamental way [ἀρχοειδῶς]. [55: cf. Heiberg 1894, 580.13]

The adverb ἀρχοειδῶς, which Mueller translates 'in a fundamental way', is perhaps better rendered 'in the manner of a principle', since Simplicius is using this term to emphasize that (according to Simplicius) Plato and the Pythagoreans really believed bodies were composed from planes or numbers, not as a whole from parts, but as from a principle, i.e., as the principle or ἀρχή of form, which, when united with matter, generate bodies.

Simplicius is not as interested in making Aristotle agree with *Plato* (despite what Simplicius himself says) as he is in showing that Aristotle and Plato are in agreement concerning certain Platonist theses about the structure of the cosmos and the sublunary world. Simplicius uses a similar strategy to defend Parmenides. When Aristotle claims that Parmenides mistakenly applied intelligible qualities to the sensible world, Simplicius argues that Parmenides distinguished the intelligible and sensible world, and that Aristotle is criticizing only a superficial reading of Parmenides [18: cf. Heiberg 1894, 557.1]. I think that it would be a stretch to say Simplicius' intention is to show that Aristotle understood and agreed with Plato by showing that Aristotle understood and agreed with Parmenides; however, this is what Mueller's view would amount to. A more parsimonious reading suggests that Simplicius believed all three philosophers agreed on roughly the same set of Platonist theses to which Simplicius himself subscribed.

These questions of interpretation, however, are in no way meant to detract from what is, overall, an excellent translation of Simplicius' commentary on *De caelo* 3.1–7. By making Simplicius accessible to a wider audience, Mueller's work on this volume is an invaluable contribution both to the study of Simplicius and to our understanding of the transmission and reception of Aristotelian science and cosmology. It should be purchased by any library with an interest in Ancient

and Late Antique philosophy and science, and will rightly become the standard text among English readers of Simplicius' commentary on *De caelo* 3.1–7.

BIBLIOGRAPHY

Baltussen, H. 2008. *Philosophy and Exegesis in Simplicius: The Methodology of a Commentator.* London.

Bowen, A.C. and Wildberg, C. 2009. edd. *New Perspectives on Aristotle's* De caelo. Leiden.

Hankinson, R. J. 2002. *Simplicius: On Aristotle's* On the Heavens 1.1-4. London.

_____ 2004. *Simplicius: On Aristotle's* On the Heavens 1.4–9. London.

_____ 2006. *Simplicius: On Aristotle's* On the Heavens 1.10–12. London.

Heiberg, J. L. 1894. ed. *Simplicii in Aristotelis de caelo commentaria.* Commentaria in Aristotelem Graeca 7. Berlin.

Hoffmann, P. 1987. 'Sur quelques aspects de la polémique de Simplicius contre Jean Philopon'. Pp. 183–222 in I. Hadot ed. *Simplicius. Sa vie, son oeuvre, sa survie.* Berlin/New York.

Moraux, P. 1965. ed. *Aristote.* Du ciel. Paris.

Mueller, I. 2004. *Simplicius: On Aristotle,* On the Heavens 2.1–9. London.

_____ 2005. *Simplicius: On Aristotle's* On the Heavens 2.10–14. London.

_____ 2009. *Simplicius: On Aristotle,* On the Heavens 3.7–4.6. London.

Rescigno, A. 2008. *Alessandro di Afrodisia, Commentario al de Caelo di Aristotele. Frammenti del Secondo, Terzo e Quarto Libro.* Amsterdam.

Simplicius: On Aristotle, On the Heavens 3.7–4.6 translated by Ian Mueller

London: Duckworth, 2009. Pp. viii + 216. ISBN 978–0–7156–3844–6. Cloth £60.00

Reviewed by
Pierre Pellegrin
CNRS, Paris
p.a.pellegrin@wanadoo.fr

Cet ouvrage consiste dans la traduction en anglais de la fin du commentaire de Simplicius au *De caelo* d'Aristote, accompagnée d'un important appareil critique. Peut-être Ian Mueller aurait-il pu expliquer, ou ré-expliquer, pourquoi la section qui va de 3.7 à la fin est « la dernière partie du *De Caelo* », puisque, après tout, le livre IV a un sujet propre qui est la pesanteur et la légèreté. Mueller divise son introduction en plusieurs parties: il repère d'abord des points essentiels du commentaire de Simplicius qui semblent exprimer une opinion propre de l'auteur, il examine ensuite le commentaire du passage dans lequel Aristote critique la « chimie géométrique » de Platon [3.7.306a1–8, 307b18], il considère enfin les passages où les autres philosophes, et surtout Démocrite, sont critiqués. Il consacre, à la fin de son introduction, une section au texte grec lui-même, une autre aux citations de Simplicius et une dernière à quelques règles d'usage des crochets.

Comme on pouvait s'y attendre, à propos de la transformation des éléments les uns dans les autres aussi bien qu'à propos de la pesanteur et de la légèreté, Simplicius soutient la thèse commune à tous les commentateurs néo-platoniciens selon laquelle les divergences qu'Aristote reconnaît entre lui-même et Platon ne sont qu'apparentes. Mais cette thèse prend une forme particulière intéressante: Aristote, selon Simplicius, connaîtrait la véritable doctrine platonicienne, mais il en adopte une formulation superficielle pour combattre les partisans de cette formulation. Mueller note aussi cette explication par Simplicius des différences entre Aristote et Platon selon laquelle Aristote tient compte de l'opinion commune pour laquelle Platon n'a aucune considération [Heiberg 1894, 679.29]. Mueller montre bien comment Simplicius en arrive à aligner la conception platonicienne de la

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 41–42

pesanteur sur celle d'Aristote. Il a raison d'écrire que la discussion par Simplicius de la critique par Aristote de la chimie géométrique de Platon est « a major document of late ancient philosophy ». Simplicius se faufile entre l'hyper-aristotélisme d'Alexandre d'Aphrodise et l'hyper-platonisme de Proclus. Mueller, en quelque pages, pointe l'essentiel: Simplicius et Proclus font des figures élémentaires de Platon des réalités physiques et non géométriques. Du coup, Simplicius en arrive à des solutions géométriquement insatisfaisantes quand il s'agit de montrer que certaines figures remplissent un espace sans laisser de vide. Dire que de petites figures seront à même de remplir l'espace du fait de leur petitesse ressemble, en effet, à un raisonnement physique. Ce faisant, Simplicius, aussi bien que Proclus, manquent le projet platonicien du *Timée* d'une physique mathématique. Autre exemple très intéressant: celui de la division du feu si le feu est une pyramide. Une partie de feu, en effet, doit être du feu, mais une pyramide ne se divise pas en pyramides. Proclus pense s'en sortir en disant qu'une pyramide est une « semence » de feu et non du feu. Simplicius adopte une solution plus hardie: la division d'une pyramide individuelle est une division dans la matière et une telle matière peut changer de forme.

Il faut signaler les deux appendices qui concernent le problème des figures qui peuvent remplir un espace. Le premier appendice considère la solution d'un certain Potamon, le second fait une revue du traitement du problème depuis l'Antiquité jusqu'à la Renaissance en passant par le Moyen-Âge arabe et latin.

Ayant moi-même traduit le *De caelo* en français [2004], je me suis évidemment beaucoup servi du commentaire de Simplicius. C'est pourquoi je suis allé voir comment Ian Mueller traduisait les passages difficiles qui m'avaient donné du mal. Je n'ai rien trouvé à redire à ce travail vraiment excellent.

BIBLIOGRAPHY

Heiberg, J. L. 1894. éd. *Simplicii in Aristotelis de caelo commentaria.* Commentaria in Aristotelem Graeca 7. Berlin.

Pellegrin, P. et Dalimier, C. 2004. *Aristote. Traité du ciel.* Paris.

Gaṇitasārakaumudī: The Moonlight of the Essence of Mathematics by Ṭhakkura Pherū. Edited with Introduction, Translation and Mathematics Commentary by SaKHYa

New Delhi: Manohar, 2009. Pp. xlvi + 279. ISBN 978–81–70304–809–8. Cloth Rs 995

Reviewed by
Jean Michel Delire
University of Brussels
jmdelire@ulb.ac.be

This book is the very happy result of the collaborative work of four excellent historians of Indian mathematics and astronomy, completed during Sreeramula Rajeswara Sarma's six-month sojourn in Kyoto. In his preface, Sarma describes his three Japanese colleagues and their team as 'the largest group working on Sanskrit texts on astronomy and mathematics today'. Together, they chose a common name made of the initials of their surnames ('Sa' for S. R. Sarma, 'K' for Takanori Kusuba, 'H' for Takao Hayashi, and 'Ya' for Michio Yano), SaKHYa, which is also a Sanskrit word for friendship or fellowship.

The author of the *Gaṇitasārakaumudī* (*GSK*), Ṭhakkura Pherū, was born in Kannāṇapura (modern Kaliyana, in the Bhiwani district of the Haryana state) in the second half of the 13th century AD. Belonging to a Jain family, he had a wide-ranging education, reading Jain but also Sanskrit and Prakrit texts on astronomy, astrology, mathematics, and architecture. Since his family was traditionally associated with the trade of luxury goods, banking, and money exchange, he became acquainted with these subjects and found an appointment at the treasury of the Khaljī Sultans of Delhi. This must have happened sometime before 1315, since Pherū's *Ratnaparīkṣā*, composed in 1315, states that he had already 'seen with his own eyes the vast ocean-like collection of gems in ᶜAlā' al-Dīn's treasury'. Pherū's name 'Ṭhakkura', which was already his father's (but not his grandfather's) name, could have been a title enjoyed by the Jains associated with the court of the Sultans in Delhi. As a matter of fact, contacts existed between Jains and Muslims on the west coast of India even before the establishment of the Delhi Sultanate in the 12th

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 43–54

century; and the Sultans sought for conducting banking and minting activities the cooperation of the Jains who controlled minting in the Gujarat-Rajasthan-Delhi region. Pherū was one of those Jains who served as mediators between the Islamic and Sanskritic traditions of learning. This is the principal reason why he was such a versatile author, writing on astronomy (*Jyotiṣasāra*, 1315), architecture (*Vāstusāra*, 1315), coins (*Dravyaparīkṣā*,[1] 1318) when he occupied a high position in the mint of Quṭb al-Dīn Mubārak Shāh (1316–1320), minerals (*Dhātūtpatti*[2]), and mathematics (*Gaṇitasārakaumudī* or *Gaṇitasāra*). This last work is not dated; but SaKHYa observe that the division proposed by Pherū of the silver *ṭaṁka* into 50 *drammas*[3] was no longer in use after ʿAlā' al-Dīn Muḥammad (1296–1316) issued a silver *ṭaṁka* of 60 *drammas* (according to the *Dravyaparīkṣā*), a rate that was continued until Quṭb al-Dīn Mubārak. Therefore, the *Gaṇitasārakaumudī* could not have been composed after 1316.

In all his works, Pherū's aim was to provide professionals such as bankers, traders, accountants, and masons, with a practical and useful manual. This explains why his works were not composed in Sanskrit (titles excepted) but in a Middle Indic Apabhraṁṣa with many vernacular terms and phonetic variations of Persians terms—e.g., 'goṁmaṭa' from Persian 'gumbad' ('dome'), 'munāraya' from Persian 'mīnār' ('minaret').

The first three chapters of the *Gaṇitasārakaumudī* are well structured like the mathematical texts which he consulted, principally Śrīdhāra's *Pāṭīgaṇita* (eighth century AD) or *Triśatikā* and Mahāvīra's *Gaṇitasāsaṅgraha* (*GSS*: *ca* 850 AD). These texts are good examples of what the Indian mathematicians called arithmetic (*pāṭīgaṇita*), usually also involving mensuration. In his *Brāhmasphuṭasiddānta* (628 AD), Brahmagupta rather spoke of 'dust work' (*dhūlikarma*), alluding to the writing and operations with figures drawn in the sand spread on a plank or soil. Crude as it may appear, this method of calculating could involve algebra in the sense that problems which we describe today with the help of equations (and thus consider as belonging to algebra) were solved by algorithms manipulating figures

[1] *Examination of the Metal Content in the Coins.*
[2] *Origin of Minerals.*
[3] Cf. *GSK* 1.4a: 'dammas' in the text.

arranged into columns drawn in the sand (see the so-called 'Diophantine' equations below).

These first three chapters describe 25 basic operations, eight classes of reductions of fractions, and eight types of procedures. Pherū does not take into consideration the simplest and elementary but often begins from a higher level of difficulty. For instance, in *GSK* 1.16a, he does not recall the addition but, as the *Pāṭīgaṇita* and *Gaṇitasāsaṅgraha* do, deals directly with the addition of successive integers up to n (the 'desired' (*icchā*) number) and the resultant sum (*saṁkalita*):

> Add unity to the requisite (*icchā*) and halve it. Multiply it by the requisite. This is the summation of the natural series (*saṁkaliya*).

Bizarrely, this formula

$$1 + 2 + \ldots + n = \left(\frac{n+1}{2}\right) \times n$$

is extended to

$$\frac{nx + x}{2x} \times n,$$

where x is called *paṇh-akkhara* (translated by SaKHYa as '<the number of> the letters in question'). The Patan Manuscript (PM), an anonymous and undated manuscript providing in Sanskrit the solution of several problems stated in the *Gaṇitasārakaumudī*, refers to x as *śabda* (word) [PM B8], as if, in order to calculate a number of words, one adds up not the words but theirs letters, i.e., signs.

But what leads one to consider the *Gaṇitasārakaumudī* as an original and even a difficult work is contained in its last two chapters. According to SaKHYa, the fourth and fifth chapters of the *Gaṇitasārakaumudī* are made of what Pherū learned from his own experience and from that of his contemporaries. They involve mechanical shortcuts in commercial arithmetic, mathematical riddles (examples of both below), as well as rules for converting calendars and constructing magic squares. Classical Indian solid geometry is also applied to such new shapes as square or circular towers with spiral stairways, minarets with fluted columns, piles of angular and circular pilasters (e.g., the famous Qutub Minar), and especially to

domes (see discussion below) and arches, which were employed successfully for the first time in 1311 by ʿAlā' al-Dīn Muḥammad. The practical aim here was to calculate the number of bricks needed to build walls having these shapes. According to SaKHYa [xix], Pherū, in his section entitled 'Computation of Bricks', proposed such calculations for nine types of walls as opposed to one type only in other works. Even productions of grains, sugar canes, melted butter, camel prices according to their age, salaries of sawyers according to the breadth and length of the wood pieces, and so on, are taken into consideration.

> It is not exactly true that
>
> > the fourth chapter of the *Gaṇitasārakaumudī*, entitled 'Four (Special) Topics', may be characterized as a supplement to the traditional *pāṭī* mathematics treated in the first three chapters [xxix]

for the following reasons. Pherū does not include in his first three chapters such traditional *pāṭī* mathematics as the procedures for solving 'Diophantine' equations by the *kuṭṭaka*.[4] This omission could most probably be explained by the fact that Pherū was not an expert in mathematics or in astronomy, while these problems occurred especially in the context of mathematical astronomy. Nevertheless, problems of this kind were often also proposed in disguise as recreational problems. This form of presentation is certainly due to their easiness of exposition, since their solution usually involves great difficulties. Fermat's last theorem—there are no integer solutions x, y, z to the equation $x^n + y^n = z^n$, when $n > 2$—to which many great mathematicians devoted more than three centuries of hard work,[5] deals with the most famous of these 'Diophantine' equations. Pherū makes no exception when he proposes [*GSK* 4.46] to compute the number of flowers obtained after doubling and adding three, respectively, a certain (not specified) number of times, or seeks [*GSK* 4.47] to find the number of flowers that a devotee had before he entered each of the four doors of a temple, giving a flower to the doorkeeper (*jakkha* = Sanskrit *yakṣa*) each time that he crosses his door and the half of his bouquet to the image of the god each time that he enters the temple,

[4] See, by instance, Mahāvīra, *GSS* 115 ½ for the explanation of such procedures, and 116 ½ ff. for many 'Diophantine' problems.

[5] The theorem was enunciated in 1647 but proved only in 1995.

knowing that, at the end, he will have 20 flowers. These problems are easy and no definite procedure is given by Pherū or by the PM;[6] but *GSK*, 4.51, which follows a similar pattern, is a trifle more difficult and necessitates a procedure that is described in 4.50: to find the number (x_0) of varisolas (a kind of sweetmeat) that a mother-in-law has put on a plate, knowing that she has given the same quantity of them (y) to each of her five sons-in-law, but also that, after each son has taken his share from the plate, she has multiplied the remaining varisolas by the rank of the next son ($2x_1$ presented to the second, $3x_2$ to the third, and so on), until the last son takes his share (y) and empties the plate.

This problem yields the equations

$$x_1 = x_0 - y,$$
$$x_2 = 2x_1 - y,$$
$$x_3 = 3x_2 - y,$$
$$x_4 = 4x_3 - y, \text{and}$$
$$x_5 = 5x_4 - y = 0.$$

Replacing each $x_i\,(i = 1, 2, 3, 4)$ in the following equation by its expression in the previous one, one finds a 'simple', although 'Diophantine', equation linking x_0 to y:

$$5 \times 4 \times 3 \times 2x_0 = (5 \times 4 \times 3 \times 2 + 5 \times 4 \times 3 + 5 \times 4 + 5 + 1) \times y,$$

of which $y = 120$, $x_0 = 206$ is a solution. But this is not the least positive solution: $y = 60$, $x_0 = 103$ is another solution, as can be easily verified by following the wording of the problem. In their explanation, SaKHYa give a more general system $x_i = a_i x_{i-1} - y\,(i = 1 \ldots n$, with $x_n = 0)$ and, applying the algorithm described in *Gaṇitasārakaumudī* 4.50 to the example, as does the PM [see A11–12], finds $y = 120, x_0 = 206$.

The algorithm is as follows: the successive coefficients (a_i in the general procedure) are written one below each other.

[6] See PM A7–8, where the same problems are proposed in Sanskrit.

$a_1 = 1$
$a_2 = 2$
$a_3 = 3$
$a_4 = 4$
$a_5 = 5$
...

The second coefficient (a_2) is to be multiplied by the following one ($a_2 \times a_3$) and the result added to it ($a_2 \times a_3 + a_3$). The result, multiplied by the following coefficient ($(a_2 \times a_3 + a_3) \times a_4$), is added to it ($(a_2 \times a_3 + a_3) \times a_4 + a_4$), and so on until one obtains $((a_2 \times a_3 + a_3) \times a_4 + a_4) \times a_5 + a_5$, which increased by 1, gives the solution for y, while the solution for x_0 is simply the product of all the a_i. In the example, this yields 206 and 120. Remarkable is the fact that the process could be continued if one adds more a_i.

The inability of this algorithm to give a solution free of common factors is partially explained by the fact that, for the sake of simplicity, it does not take into consideration the common factors of the coefficients a_i.

The same kind of procedure is also used, in *GSS* 116 ½ for instance, to solve a more general type of 'Diophantine' equation such as $ax + by = c$, which is equivalent to $ax \equiv c[b]$[7] and means that the remainder of ax when divided by b is c. In that case, the column is made of the successive remainders of the Euclidian algorithm (*kuṭṭaka* in Sanskrit texts) applied to a and b, completed by a certain 'clever' number called *mati*, a number already introduced by Āryabhaṭa in *Āryabhaṭīya* 2.32–33 (499 AD). This algorithm is called *vallikā-kuṭṭīkāra*—'vallikā' is diminutive of 'vallī' ('creeper')—because the algorithm begins at the bottom of two adjacent columns of numbers and proceeds through the numbers as does a creeper.

This algorithm is also applied to the resolution of two (or more) 'modulo' equations, $x \equiv c_1[a_1]$, $x \equiv c_2[a_2], \ldots$, as in *GSS* 121 ½: $x \equiv 7[8]$ and $x \equiv 3[13]$, of which the positive solutions are $55 + 104 \times k$ (k an integer). In that case, as in the simple case, the least positive solution for x is never explicitly stated by a formula but obtained through a 'creeper' procedure. Related to this kind of problem is the 'think a number' problem in *GSK* 4.58 (also in PM A18, with two important mistakes this time). The questioner has to 'guess' a number (x) chosen by an interlocutor, knowing only the remainders

[7] Read: 'ax congruent to c, modulo b'.

$r_1 \equiv x[3]$, $r_2 \equiv x[5]$ and $r_3 \equiv x[7]$, or alternately, asking the interlocutor to compute $p = 70r_1 + 21r_2 + 15r_3 + 105$, after which the questioner reveals x as though by magic. Pherū does not chose any of the two options: he simply states the property of p which is to reveal x (rather, the least positive solution of the three equations $x \equiv r_1[3]$, $x \equiv r_2[5]$, $x \equiv r_3[7]$) when reduced modulo 105. In their explanation, SaKHYa choose the last option, which, in our opinion, is not as 'magical' as the first one, when the numbers 70, 21 and 15 are not revealed to the participant. At least, this is the classical way of exposing that trick [see Beiler 1966, 31]. Now, as we have already noted, to give the expression $70r_1 + 21r_2 + 15r_3$ as the solution (modulo 105) of the equations is not *pāṭī* at all. Indeed, this very same problem, with its solution so expressed, occurred for the first time in the *Sunzi Suanjing* (fourth/fifth centuries AD) according to Martzloff [1988, 296]. This is the reason why the multiple 'modulo' equations problem is called 'the Chinese remainders problem' in modern handbooks of algebra [see, e.g., Bland 2002].

To come now to mensuration in 'the traditional *pāṭī* mathematics (allegedly) treated in the first three chapters', SaKHYa note that Pherū is sometimes very original. For instance, he expresses (of course not with formulas) the area (S) and the volume (V) of a sphere as:

$$S = \frac{C}{4} \times C \times \left(1 + \frac{1}{9}\right) \qquad [GSK\ 3.65]$$

and

$$\begin{aligned} V &= \frac{d^3}{2} \times \left(1 + \frac{1}{9}\right) \\ &= d^3 \times \left(1 - \frac{1}{4}\right) \times \left(1 - \frac{1}{4}\right) \qquad [GSK\ 3.65] \end{aligned}$$

The last is equivalent to Mahāvīra's

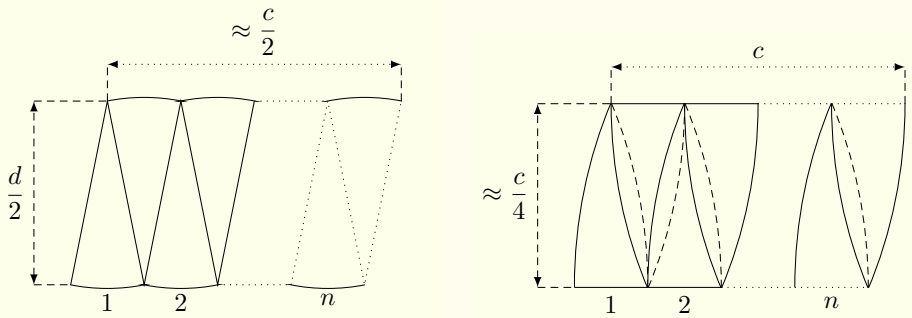
$$V = \left(\frac{d}{2}\right)^3 \times \frac{1}{2}.9 \qquad [\text{GSS } 8.28b],$$

but the correcting factor $1 + \frac{1}{9}$ in the other two formulas is specific to Pherū. According to SaKHYa, the S could have been obtained from the formula

$$S_D = \frac{C}{2} \times \frac{d}{2}$$

for the disk's area, which was well-known in India since Āyabhaṭa [*Āryabhaṭīya* 2.7] but already found by Archimedes [287–212]. As usual in Greek mathematics, Archimedes proved it by the method of exhaustion (a double *reductio ad absurdum*) in his *Measurement of a Circle*; but he could have 'guessed' what was to be proved by the following method: one cuts the disk into $2n$ identical triangles with their apexes at the center, where n an integer ≥ 2. Disposing n triangles with apexes downward and n triangles with apexes upwards so that they fit perfectly into each other, one gets a 'parallelogram' with waved bottom and top, its base being $\frac{C}{2}$ in length and its height $\frac{d}{2}$. When n increases, the 'parallelogram' tends to a rectangle of area

$$\frac{C}{2} \times \frac{d}{2}.$$



For the sphere, SaKHYa suggest cutting 'narrow barleycorn figures' from the north pole to the south pole, which, when cut by the equator, yield $2n$ identical triangles. These $2n$ triangles could, as for the disk, be placed head-to-tail, thus forming a rectangle of length C and height $\frac{C}{4}$ but their property of being spherical forces them to overlap (besides the fact that they cannot really be flattened). Consequently, the area $\frac{C}{4} \times C$ must be somewhat reduced by a factor which Pherū evaluates—'by experiment?', ask SaKHYa—to $1 + \frac{1}{9}$.

To corroborate SaKHYa's reconstruction of the Pherū's area formula for the sphere, let us remark that Pherū himself declares:

> This has been told according to experience. There is no doubt. It should be known thus. [*GSK* 3.76b]

He notes this not about the area of the sphere but about the area of a dome or hemisphere, just after 'the circumference multiplied by half the diameter and increased by one-ninth <of itself> is <the volume of> the empty space in a dome' [*GSK* 3.76a]. In SaKHYa's translation, '<the volume of>' should be replaced by '<the area of>', since the text effectively describes

$$C \times \frac{d}{2} \times \left(1 + \frac{1}{9}\right),$$

which is an area. There seems to be a confusion between the area of a dome or hemisphere, [*GSK* 3.65] and the area of a disk.[8] In fact, *GSK* 3.74–76 is rather confused. So, 3.75a, which concerns computing the piling of a dome, asks: 'The inner circumference of a wall is nineteen and its breadth six. What is its piling?'. With these data—SaKHYa conclude that 6 is not the dome's width but its diameter, since *GSK* 3.58 gives 19 for the circumference of a circle of diameter 6—one cannot compute anything but the volume under the dome:

$$V\neg\dagger = \frac{1}{2} \times \frac{6^3}{2} \times \left(1 + \frac{1}{9}\right) = 60 \qquad [\text{see } GSK\ 3.65].$$

The rest of the text gives the outer circumference (and, therefore, the width) of the dome: 'The outer circumference is, O learned man, twenty-four. What will be the area?' [*GSK* 3.75b], after which follows the text about <the area of> the dome quoted above. If one applies to the data the formula of *GSK* 3.76a, one gets

$$S = 19 \times 3 \times \left(1 + \frac{1}{9}\right) = 63\frac{1}{3}$$

(57 without the $1 + \frac{1}{9}$ correction). Note that the two computed values are close to the exact value of the wall's width (computed as the volume of the outer hemisphere minus the volume of the inner one), i.e., ≈ 58.8. Let us note also that a part of *GSK* 3.74 suggests computing the area of the intermediary hemisphere: '<When the circumference is measured> at the middle of the outside (?), it is the area.' The intermediary circumference is

[8] Note that $C \times \frac{d}{2} = 2\pi \times r^2$ is also the modern formula for the area of the hemisphere.

$$\frac{19+24}{2} = 21.5$$

in length, and the corresponding dome is

$$\frac{1}{2} \times \frac{21.5}{4} \times 21.5 \times \left(1 + \frac{1}{9}\right) \approx 64.2,$$

which is close to the expected value, once again.

To conclude, one cannot help but think that, in this very case, Pherū tried several formulas empirically in order to find the volume of the dome and, finally, the number of bricks, which, after all, is the purpose of this part of chapter 3, entitled 'Procedure for Piling'. Strangely enough, he got the number of bricks for only one of the nine types of wall described. Another quick method, based on the area instead of the volumes could have been used: multiply the area of the intermediary hemisphere by the width of the wall, i.e.,

$$\frac{24-19}{2\pi} \times 64 \times 2 \approx 51.$$

In the fifth chapter of *Gaṇitasārakaumudī*, which SaKHYa [xi] describe as 'presumably added as a supplement', since '[t]his part is not so well structured as the preceding parts' but still 'explicitly mentions Pherū as the author', the formula

$$V = d^3 \times \left(1 - \frac{1}{4}\right) \times \left(1 - \frac{1}{4}\right)$$

is repeated [*GSK* 5.25] and used to calculate the volume of a sphere having a diameter 6. The result is written '120' in the edition, but SaKHYa reconstruct it as 121||, that is $121\frac{2}{4}$—'|' is Pherū's shorthand for a quarter—and notes that 120 is exact according to the second volume formula of 3.65. The area also is given as

$$\frac{C}{4} \times C \times \left(1 + \frac{1}{9}\right) = 90 < | > +10 < \frac{1}{36} >= 100 < \text{SS6} > .$$

Strangely, SaKHYa put 'SS6' into angular brackets < >, as if it were an addition to the translation but discuss it in their mathematical commentary as if it were present in the text. 'SS' being Pherū's shorthand notation for separating units from twentieths, '100SS6' means $100 + {}^{6}/_{20}$, which is not exactly $90\frac{1}{4} + 10\frac{1}{36} = 100\frac{5}{18}$. As SaKHYa remark, quoting Strauch, 100 (written 100|1) would be a better approximate value than $100 + {}^{6}/_{20}$, but the reason for this choice

must be the very common use of 20 as a conversion factor in the *Gaṇitasārakaumudī* and in Pherū's time. The twentieth of any basic measure is usually called *visuva/visova(ga)* (Sanskrit *viṁśopa(ka)*), and is itself divided into twentieths, called *visuvaṁsaga/vissaṁsa* (Sanskrit *viṁśopa-aṁśaka-/viṁśa-aṁśa*). For instance, in *GSK* 1.3–4a, the monetary units are described as:

damma,
1 *visova* = $\frac{1}{20}$ *damma*,
1 *vissaṁsa* = $\frac{1}{20}$ *visova*,
1 *paḍ ivissaṁsa* = $\frac{1}{20}$ *vissaṁsa*,
1 *kāiṇi* = $\frac{1}{20}$ *paḍ ivissaṁsa* and
1 *paḍ kāiṇi* = $\frac{1}{20}$ *kāiṇi*

In spite of this visagesimal division, the numeration remains decimal and Pherū gives [*GSK* 4.6–10] very simple rules or mechanical shortcuts for converting an amount of *dammas* that is to be shared by 10, 20, or 100 persons, or simply to be divided by 1000, 10000, or 100000. In *GSK* 4.8, 209534 *dammas* are given to 100 persons, each receiving 2095 *dammas* + 3 × 2 *visovas* (for 3 tenths = 3 × 2 twentieths) + 4×2^2 *vissaṁsa* (for 4 hundredths = 4 tenths of tenths = 4×2×2 twentieths of twentieths = 4×2^2 four-hundredths). The other divisions by 10^n convert the last n digits into *visovas*, *vissaṁsa*, and so on by multiplying them successively by 2, 2^2, ..., and the largest division factor is $100000 = 10^5$ for there are only five submultiples of the *damma*. Strangely, *GSK* 4.9–10 (division by 10 and 20) are introduced with the words, 'Now, on the regional method of accountancy', for which SaKHYa give no explanation.

Again, in regard to chapter 5, SaKHYa deduce [192] the value of the length unit *kaṁviya* (also *kaṁbiya* or *kaṁvī*) from the value given by Srinivasan to the weight unit *sera*: 600 g. < 1 sera < 850 g., by using the table of 'specific gravities' given in *GSK* 5.28 for different culinary substances. So, sesamum oil (named *tilla* by Pherū, Sanskrit *tila*) weighs 10 *maṇas/kaṁviya*^{3}. According to SaKHYa, sesamum oil has a specific gravity of 0.92 kg/dm^3, so that 240 kg < 10 *maṇas* = 400 *seras* < 340 kg $\iff$ $\frac{240}{0.92}$ dm^3 < *kaṁviya*^{3} < $\frac{340}{0.92}$ dm^3, wherefrom 63 cm < *kaṁviya* < 72 cm (SaKHYa), by extracting the cubic root. This derivation is probably overconfident on the exact value of the sesamum oil's density. One could have checked it by using, for instance, one of the stones' 'specific gravities' listed in another table [*GSK* 3.67–68], perhaps more reliable than oil's.

Pherū's works were discovered in 1946 in a single manuscript and edited in 1961 by Agar Chand Nahata and Bhanwar Lal Nahata. The present work is based on this edition and on the edition of the Patan Manuscript (by T. Hayashi with the Bakhshālī Manuscript). After Sarma's preface, it contains an introduction (on Pherū's life and works, on the mathematics of the *Gaṇitasārakaumudī*) with a very useful mathematical glossary (English-Sanskrit/Prakrit) and a table comparing the fundamental operations of the *Gaṇitasārakaumudī* with 10 other mathematical works. The edition, revised according to the language, the verses, and the mathematical content (the original text being pushed away in notes), is followed by a literal translation and a very substantial mathematical commentary. Four appendices (concordance of *Gaṇitasārakaumudī* with other works, type problems, index to the numbers, glossary-index), a bibliography and two indices (mathematical terms, Sanskrit/Prakrit authors and titles) close this study. Its quality and its presentation will make of it an essential reading for every researcher not only in the history of Indian mathematics but also in Middle Indic languages, as well as in the monetary and economic history of the Delhi region in the early 14th century.

BIBLIOGRAPHY

Beiler, A. H. 1966. *Recreations in the Theory of Numbers.* New York.

Bland, P. E. 2002. *The Basics of Abstract Algebra.* Freeman, NY.

Martzloff, J.-C. 1988. *Histoire des mathématiques chinoises.* Paris.

The Moon that Wasn't: The Saga of Venus' Spurious Satellite by Helge Kragh (assisted by Kurt Møller Pedersen)

Science Networks: Historical Studies 37. Basel/Boston/Berlin: Birkhäuser, 2008. Pp. xii + 199. ISBN 978–3–7643–8908–6. Cloth € 42.69

Reviewed by
Curtis Wilson
St John's College, Annapolis MD
c.wilson002@comcast.net

This book is an account of certain peculiar telescopic observations from the 1640s through the 1760s, and of the interpretations of them during this early period and later. What was seen was Venus plus an appearance that some took to be a satellite of Venus and others explained as an illusion produced by secondary reflections from the eye and within the telescope, or as a star or planet or other celestial object erroneously identified as a Venus moon.

According to our author, the satellite did not exist, a conclusion which we accept as justified. Before admitting the reality of a putative object, we demand a certain concordance and predictability in its various appearances. In the case of a satellite of a circumsolar planet, we would like to be able to determine the data that a mathematical astronomer especially wants—a repeatedly verifiable mean distance from, and period about, the primary. From these data together with the distance from and period of Venus about the Sun, one could determine the mass of Venus relative to the mass of the Sun; and this value would enable us to determine the gravitational action of Venus on other bodies such as our Moon. Venus perturbs the motion of our Moon by a rather small but nowadays quite detectable inequality amounting to some 14 arc-seconds. The Venus moon would thus have played its role in the project of getting Newton's—or Einstein's—gravitational theory to work. But the putative satellite of Venus confined itself to rare and unpredictable appearances, refusing to be pinned down to a stable, identifiable orbit.

'Why write a book', asks Kragh, 'about something that manifestly does not exist?'

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 55–65

> The brief answer is that for more than a century the enigmatic satellite—or something taken for it—was occasionally seen, or thought to be seen, and that the object thus became part of the history of astronomy.... By following the discussions of this ghost-like satellite, we address the history of planetary astronomy in a novel way. We get a different insight not only into the world of the astronomers but also into the popular literature concerning the planetary system and other aspects of astronomy. [viii]

The 'different insight' that Kragh mentions here has to be extracted from the various responses generated by the reports of the telescopic sightings of the alleged moon, first in an age when telescopic observations of any kind were novel, and later when observers had gained expertise in the use of telescopes and had accumulated a body of confirmed results which they could refer to with confidence.

Kragh's account begins with a 1646 report by Francisco Fontana, an early constructor of astronomical telescopes. Fontana saw two small dots accompanying Venus and supposed them to be

> her Courtiers and Attendants.... This is a new discovery not yet published in my opinion. But it is true that they do not always appear, but only when Venus is shimmering.... These little dots were... not always seen in the same situation on Venus, but they moved back and forth like fish in the sea. [10]

Fontana's report, Kragh tells us, 'quickly caught the attention of the learned world'. Galileo was dead when Fontana's book appeared; but earlier, he had expressed skepticism about Fontana's observational claims, acknowledging that Fontana's telescopes had greater magnifying power than his own but denying that they could reveal novelties that had not been revealed by his telescopes. Galileo's disciple Torricelli, no less sceptical than he, described Fontana's book as full of insane things—'absurdities, fictions, effronteries, and a thousand similar outrages'. Riccioli, a Jesuit astronomer in Bologna, denied that he or his friends Grimaldi and Gassendi had ever observed on or close to Venus any globules in any telescope. The Jesuit polyhistor Kircher in a book of 1656 agreed with Riccioli that Fontana's report was less than convincing.

Yet if a different note could be struck, you could bet on there being someone to strike it. Andreas Tacquet, a Jesuit situated in

Antwerp, suggested in 1669 that the telescopes of Riccioli, Grimaldi, and Gassendi may have been inferior in quality to Fontana's. Two other authors later in the century, Johann Zahn and Otto von Guericke, looked with favor on Fontana's Venusian moons but like Tacquet made no effort to put Fontana's claim to observational test. Christiaan Huygens, one of the pioneering geniuses of 'the century of genius', on reading Fontana's observation-claim, was open in 1656 to the possibility that Venus had a moon or moons but forthwith proposed searching for it or them. After three years of observing Venus again and again, he concluded in 1659 that Venus was without a companion.

In 1672 and again in 1686, Jean Dominique Cassini (Cassini I) saw what he considered might be a Venusian moon. This first of the astronomical Cassinis had been invited in 1669 to migrate from Bologna to France and become the director of the new Observatoire de Paris; he would remain in that role till his death in 1712. On 28 August 1686 at 4:15 a.m., while observing Venus, he saw, at a distance of $\frac{3}{5}$ of Venus' diameter, a luminous appearance that seemed to have the same phase as Venus, which was then gibbous on the western side. The diameter of this object was about $\frac{1}{4}$ that of Venus. He had seen a similar phenomenon on 25 January 1672 from 6:52 to 7:02 a.m., when Venus was horned and the luminous appearance was of the same shape and distant from Venus' southern horn by a diameter of the planet.

> I was in doubt whether it was or was not a satellite of Venus, of such a consistence as not to be very well fitted to reflect the Sun's light... But in spite of some research I have done from time to time after these two observations, in order to complete a discovery of such great importance, I have never succeeded in seeing it except these two times; and this is why I suspend my judgment.

The foregoing report of the observations of 1672 and 1686 was first published, according to Kragh, in 1730, which does not explain how David Gregory came to refer to Cassini's two observations in his *Astronomiae physicae et geometricae elementa* of 1702. In the English edition of this work, published in 1736, Gregory expressed the opinion that Cassini's results gave 'more than a bare Suspicion to incline us to believe that Venus has a Satellite'.

Cassini's observations are the first of several in which the supposed satellite is seen with the same phase as Venus. The parallelism of phase was to be expected if the luminous appearance was indeed a satellite, since the satellite and Venus were being illuminated by the Sun's rays at very nearly the same angle. But the parallelism was also to be expected if the luminous appearance was a secondary reflection of the primary image arriving at the ocular. Kragh tells us [122], that in 1881 the British astronomer William Frederick Denning directed his telescope at Venus and saw two crescents. Denning immediately thought of the reputed observations of a satellite of Venus and readily came up with an explanation in terms of reflections in the telescope. A similar explanation had already been put forward in 1765 by the Jesuit astronomer Maximilian Hell in his *De satellite Veneris*, as Kragh reports [80ff].

Another observation of the putative satellite sharing a phase with Venus was due to James Short, a Scotsman who had settled in London and made a reputation as a manufacturer of reflecting telescopes. On the morning of 3 November 1740, while observing Venus, he saw a luminous object some $10^\circ 02'$ to the west of the planet:

> ... I put on a magnifying Power of 240 times and, to my great surprise, found this Star put on the same Phasis with Venus. I tried another magnifying Power of 140 times, and even then found the Star under the same Phasis. Its Diameter seemed about a Third, or somewhat less, of the Diameter of Venus; its Light was not so bright and vivid, but exceeding sharp and well defined [31, quoting from *Philosophical Transactions* 41 for 1739–41].

Short looked for the object during the following mornings, 'but never had the good fortune to see it again'. The French astronomer Lalande, visiting with Short in March 1763, concluded that Short at that time did not believe in the existence of a Venus satellite [33]. Perhaps the failure during the Venus transit of 1761 of the putative satellite to show itself as a black dot against the bright background of the Sun had destroyed any lingering hope Short may have had that the satellite would prove itself genuine.

The elusive satellite was next sighted by Andreas Mayer of the University of Greifswald in Pomerania (Northern Germany), on 20 May 1759 at $8\text{h}45'50''$. Above Venus, Mayer saw

> a little globe of far inferior brightness, about 1½ diam. of Venus from herself. . . . The observation was made with a Gregorian telescope of thirty inches focus. It continued for half an hour, and the position of the little globe with regard to Venus remained the same, although the direction of the telescope had been changed.

The diameter of the little globe, Mayer later reported, was about ¼ that of Venus. At the time of the Venus transits of 1761 and 1769, he continued his observations but evidently did not see the putative satellite again. In 1762, he wrote: 'Whether or not this satellite belongs to Venus, I do not dare to claim.'

Mayer's was the last sighting of the alleged satellite before 1761. In June of that year, Venus was predicted to transit the Sun's face—the first Venus transit since 1639 (when a transit predicted by Kepler was observed by Horrocks and Crabtree)—and a second transit was to occur in 1769. The transits were important, Halley had announced, for making possible the determination of a more precise value of the Sun's horizontal parallax, that is, the Sun's distance in terms of the Earth's radius. During 1761, the astronomical interest in Venus was high, and Kragh finds that during that year the putative Venus satellite was sighted some 19 times. It was sighted nine more times in the period 1764–1768 and then, Kragh tells us, 'it was over': the sightings ceased.

Observing in Marseille, the Jesuit astronomer Louis Lagrange (not to be confused with the mathematician Joseph-Louis Lagrange) sighted the putative satellite three times during the period 10–12 February 1761. He was using a six-foot refracting telescope made by Short. The object exhibited no phase, such as seen by Cassini and Short. Also, it appeared to be following a path perpendicular to the ecliptic, a result so surprising to Lagrange that, according to Lalande, 'he did not find it difficult to abandon all the consequences which he had drawn from these observations.' In letters later written to Maximilian Hell, Lagrange made it clear that he did not believe that what he had seen was a satellite of Venus.

Another French astronomer, Jacques Montaigne, importuned by a young civil servant named A. H. Baudouin to make a search for the satellite, set about looking for it from his station in Limoges. He succeeded in sighting it four times between 3 May and 10 May 1761. On

3 May at 9h30m he saw it as a faint crescent situated as was Venus' crescent, about ¼ of the latter's size, and about 20′ from Venus. Baudouin submitted a report to the Académie des Sciences claiming that Montaigne's observations constituted a genuine and important astronomical discovery, thus confirming the original discovery by Cassini. Baudouin had not himself seen the moon but was confident that it existed and predicted that it would be seen moving across the disk of the Sun on 6 June 1761, the day of the transit. On that day, Baudouin observed the Venus transit with Charles Messier, a well known discoverer of comets, but they saw no satellite.

A number of astronomers observing the transit on June 6 looked for the satellite and reported their failure to see it. They included Lacaille in Paris, Cassini de Thury (Cassini III) observing with Liesganig in Vienna, Pingré with his assistant Thuillier on the island of Rodrigues in the Indian Ocean, the Swede Bengt Ferner observing near Paris, Samuel Dunn observing near Chelsea, and John Winthrop at Harvard College.

On the other hand, the amateur astronomer Abraham Scheuten, observing in Crefeld, Germany on 6 June 1761, claimed 15 years later that he had seen both Venus and its satellite on the Sun [55]. Similarly, an anonymous Englishman observing in St Neots (west of Cambridge) claimed to have seen the satellite on the Sun during the Venus transit [55–56]; and so did a Danish amateur astronomer, Friedrich Artzt, observing on Zealand [62–63]. The St Neots observation was reported right away [55–56] but the Artzt claim was set forth only 52 years after the event. The long delays in the Scheuten and Artzt reports detract from the confidence that we may be inclined to have in their truth; nor is the amateur status of Scheuten, Artzt, and the St Neots observer reassuring. We acknowledge these contrary reports, but is there anything we can do about them? Kragh maintains a noncommittal attitude toward not only these claims but all the observation-claims which he reports. Science, however, has to move on and cannot be brought to an indefinite standstill in the face of reports for which there is no direct way of confirming or disconfirming.

After the transit had occurred, an assistant at the observatory of the University of Copenhagen, Peder Roedkiaer, believed that he saw the Venus moon on several occasions between 28 June and 1 December 1761. The director of the observatory, Christian Horrebow,

did not report these observations, perhaps doubting that Roedkiaer was actually seeing the satellite. Three years later, on 3 and 4 March 1764, Roedkiaer again saw the satellite, and on the following March 11 Christian Horrebow himself saw it. He believed that he was seeing exactly what Cassini had seen in his two observations of 1672 and 1686. He made tests, seeking to ensure that the appearance was neither a star nor an illusion due to secondary reflections. Again, in January 1768, Horrebow and two assistants once more saw the satellite. But this was the last time.

The first extended critique of the observations of the presumptive satellite was undertaken by the Hungarian-born Jesuit astronomer Maximilian Hell (Miksa Höll in Hungarian) in his *De satellite Veneris* of 1765, mentioned previously. Hell had carried out a series of optical experiments. He found that, under certain conditions, he could always produce a spurious satellite, not only of Venus but also of Mars or Jupiter. The conditions included a special position of the eye relative to the eyepiece of the telescope tube and as well, a slow, careful motion of the eye. The image was formed, Hell believed, by a twofold reflection, first from the convexity of the cornea and then from the concave face of 'the meniscus lens or ... the eyeglass' (Kragh does not explain exactly where these lenses are in the telescope; a diagram would have been helpful). Hell was able to see the satellite through two Gregorian telescopes, but never through two much better Newtonian reflectors. According to Kragh [83], Hell's analysis was widely accepted in the literature of the late 18th and the 19th century. Doubters there were, though few.

Hell passed through Copenhagen in May 1768 on his way to Vardø to observe the Venus transit of 1769 and took the occasion to have a conversation with Horrebow, who proved agreeable to the conclusion that the satellite was an illusion [67]. Whatever faith Horrebow may have had in his observations of the satellite in 1764 and 1768 had either evaporated in the meantime or been blown away by Hell's strong contrary conviction.

The Jesuit Roger Boscovich, in a treatise of 1767, explained the sightings of the Venus satellite in the same manner as Hell, that is, as caused by reflections from the eye's cornea and from a lens in the ocular of the telescope. Boscovich writes as if entirely unaware of Hell's treatise, although he and Hell used the same publisher [84–86].

An entirely different explanation was proposed by J. J. d'Ortous de Mairan (1678–1771), a late Cartesian who figured importantly in the affairs of the Paris Académie des Sciences in the middle years of the century. In the 1730s, de Mairan had published an account of the *aurora borealis*, explaining it in terms of a solar atmosphere extending out to the Earth. In 1764, he proposed accounting for the non-appearances of the Venus satellite as due to a thickening of this same atmosphere [77–79].

J. H. Lambert in three publications in 1773, 1775, and 1776, without assuming as certain that the Venus satellite existed, determined from the data supplied by Baudouin, Roedkiaer, and Horrebow a mean distance from and period about Venus, and calculated that on 1 June 1777 it should be possible to see the satellite on the face of the Sun, although Venus would be passing above the Sun at a distance of 15′. Observers in Berlin, Vienna, Paris, Stockholm, and Copenhagen looked for the satellite on the appointed day but it failed to appear [87–93].

William Herschel, discoverer of the planet Uranus, looked for the Venus satellite in November 1789 and concluded that if it existed, it must be less bright than a star of the 8th or 9th magnitude. J. H. Schröter, with his 27-foot telescope at his private observatory of Lilienthal, observed Venus for 15 years but never,

> in spite of all attention, found the slightest trace of either a real satellite or, in any telescope, a deceptive secondary image such as the late Father Hell thought [supposed] in his treatise of 1766. [96–97]

Fully a third of Kragh's book is devoted to the 19th-century authors who found the subject still of interest, some sure that no satellite existed, others urging that the earlier observations could not all be attributed to illusion.

Passing over most of the 19th-century writers whom Kragh gives an account of, we mention the endeavor of John Craig, an amateur astronomer and retired country clergyman, who in 1852 erected in Wandsworth Common, London, the world's largest achromatic telescope, 85 feet in length. Craig proposed using the telescope for the study of Saturn's ring system and to settle 'the old question of Venus' moon'. The telescope proved a costly failure and was dismantled a

few years later without contributing to the knowledge of Saturn's rings or the Venus moon question [108–109].

We mention also F. Schorr's *Der Venusmond und die Untersuchungen über die früheren Beobachtungen dieses Mondes* [1875]. Schorr was confident of the existence of the satellite:

> The Venus moon belongs to the citizens of our solar system; new observations, more precise than the earlier ones, will eventually prove its existence without doubt and provide means to determine its orbit with such accuracy that [as] is required by the present state of science. [112–113]

Schorr's explanation for the moon's not having been observed over the preceding century was that it reflected very little light. What could he say now, 135 years since the publication of his book, during all which time the satellite has still not reflected enough light to be seen?

Closure came, according to Kragh, in the 1880s with the publication of Paul Stroobant's 'Études sur le satellite énigmatique de Vénus' [1887]. Stroobant reproduced the central parts of the relevant sources from Fontana to Horrebow in their original languages and discussed all the hypotheses systematically. He stated his main conclusion as follows:

> In brief, we can say that the satellite of Venus does not exist, and when there was no false image or optical illusions, we find for the best observations a star corresponding almost exactly to the different observed positions.

For example, Roedkiaer on 4 August 1761 saw an object which he assumed to be the Venus moon, then noticed another star-like object nearby which he judged a better candidate. Stroobant by comparing the positions of the two objects with Argelander's star catalogue was able to identify them as 64 *Orionis* and 62 *Orionis*.

Kragh describes Stroobant's conclusion as 'somewhat cavalier', but adds [131]:

> since it was accepted by nearly all astronomers this is beyond [beside?] the point.

Kragh's endeavor in this book is to give an account of the Venus-moon affair that is as complete as possible. His examination of the

literature has been extensive and it seems unlikely that he has failed to mention any incident or person relevant to the putative Venus moon. He provides an extensive bibliography, identifying the many primary sources which he has consulted. He includes biographical sketches of the 23 chief characters in his story.

The present reviewer is restive with the restrictions Kragh sets for himself:

> We have not tried to determine what really was seen in the various observations, that is, to determine whether Mairan, Hell, Stroobant, Thirion or other post-1760 commentators were right or not; nor do we believe that such an undertaking would be historically fruitful (it may be of a certain astronomical interest, but that is a different matter). [147]

But, to this reader, it seems awkward and artificial to divorce the history of an episode in the pursuit of scientific knowledge from the scientific knowledge that is being sought.

Kragh in his avoidance of the scientific questions that engaged his characters does not seem aware of some of the consequences of the premises which he accepts. If no Venus satellite exists, then whenever the putative satellite appeared as sharing a phase with Venus, the possible explanations of the appearance become quite limited. The only celestial objects that are observable from Earth and show phases, aside from Venus itself, are the Moon, Mercury, and Mars; and Mars can exhibit only a gibbous phase, never appearing halved or crescent. The periods of all three of these bodies have been well known for centuries and their whereabouts at any time is readily ascertainable, so that their possible role in any appearance of the supposed satellite sharing a phase with Venus can easily be eliminated.

The satellite was seen sharing Venus' phase by Cassini in 1672 and 1686, by Short in 1740, by Andreas Mayer in May 1759, by Jacques Montaigne in May 1761, and presumably by Christian Horrebow in March 1764. These instances can only be accounted for, I believe, by an explanation of the type put forward by Maximilian Hell and Roger Boscovich.

The kind of explanation that Stroobant proposed seems appropriate where shared phases are absent and we find an observer like Roedkiaer choosing first one star then another as his candidate for the

satellite. In Stroobant's conclusion quoted above, he explicitly allows that some of the appearances are to be explained as illusions, presumably in the manner of Hell's and Boscovich's analysis. In the Roedkiaer case cited above, Stroobant's explanation is not merely a 'favorite' but the best explanation, given what we know of the circumstances.

Scientists are fallible human beings, bringing with them in their quest opinions and tendencies that may later prove wrongheaded. *Humanum est errare.* But the glory of it is that now and then evident progress is made. I suspect that it is so in the present case, where Maximilian Hell proposed accounting for certain misleading images by secondary reflections and Stroobant showed how certain other appearances of the presumptive satellite were better explicable in terms of 19th-century knowledge of the stars.

It would be good to have a detailed explanation of how the illusion was produced in Cassini's telescope (a refractor with two convex lenses?) and in Short's telescope (a Gregorian reflector?). But, alas, that would require experimentation—and a lot more work!

BIBLIOGRAPHY

Gregory, D. 1702. *Astronomiae physicae et geometricae elementa.* Oxford.

Schorr, F. 1875. *Der Venusmond und die Untersuchungen über die früheren Beobachtungen dieses Mondes.* Braunschweig.

Stroobant, P. 1887. 'Études sur le satellite énigmatique de Vénus'. *Mémoires Couronnés et Mémoires des Savants Étrangers Publiés par l'Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique* 49:1–48.

Chymists and Chymistry: Studies in the History of Alchemy and Early Modern Chemistry edited by Lawrence M. Principe

Sagamore Beach, MA: Chemical Heritage Foundation/Science History Publications, 2007. Pp. xiv + 274. ISBN 978–0–88135–396–9. Cloth $45.00

Reviewed by
Jole Shackelford
University of Minnesota
shack001@umn.edu

The editor of this volume of 22 contributions to a conference held at the Chemical Heritage Foundation in Philadelphia on 19 July 2006 invokes in his introduction the memory of an earlier conference, convened in Groningen 17 years prior. He credits that conference with catalyzing a collaboration and informal networking among historians interested in alchemy that led to an efflorescence of alchemical studies and precipitated a need for a new conference to ascertain where the field has gone and to rally a second generation of enthusiasts. I was among those at the 1989 Groningen conference and remember clearly that it commenced with a kind of anti-benediction presented by Nathan Sivin, who proceeded to tell us that the history of alchemy was a dead or dying field, that the few who continued to work in the subject area came to it from other disciplines, not the history of science, and that until specialists in chemistry, religion, and other disciplines took off their blinders and worked together, combining their viewpoints, the field would not again generate new knowledge. And behold, this is what has happened! The chapters of this collection reflect both existing lines of research and those newly undertaken, in many cases in the spirit of collegiality that Sivin hoped that the Groningen conference had conjured.

The 22 contributions to this volume collectively provide a rich sample of current work in the many corners of the history of late Renaissance and early modern alchemy. I will stick with this traditional term instead of adopting the editor's preference for the early modern word 'chymistry', because it is indeed the continuity of chemical practices described in this volume with ancient and medieval alchemy that

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 66–77

is salient. In many instances, these short chapters introduce work in progress or succinctly epitomize studies published by their authors at length elsewhere. In some cases, the authors engage past historiography explicitly and occasionally also address current competing interpretations.

Didier Kahn's survey of King Henry of Navarre's well-known patronage of Paracelsian chemical medicine when he ascended the throne of France as Henry IV, which further provoked the long-running hostility of the Paris medical faculty toward all things Paracelsian, focuses on the far-reaching network of chemical physicians who served his and his father's courts as diplomats. Footnotes alert the reader to the extensive printed scholarship in French on court alchemy in France, much of it by Kahn himself and François Secret.

The next two chapters concern Andreas Libavius' negotiation of the ideological boundary between alchemy as an occult art and chemistry as a medieval, demonstrative science. Bruce Moran attends to just this theme, namely, Libavius' views on the Paracelsians' verbal obfuscations and the traditional alchemist's claim that only the truly adept, those illuminated by grace (*donum Dei*), are equipped to read properly the many alchemical emblems and metaphors that characterized the *corpus alchemicum*. Not rejecting the Hermetic alchemy, Libavius sought to make it generally accessible and subject to all scholars' scrutiny—a necessary step for scientific dialectic. Moran's contribution suffers from trying to fit too much into a short space; and readers would be well rewarded by consulting his recent extensive treatment of Libavius' efforts at discipline formation, *Andreas Libavius and the Transformation of Alchemy* [2007].

Peter Forshaw uses Libavius' hostility to Paracelsian hermeneutics as a foil for examination of Heinrich Khunrath and the differing attitudes toward the overlap of theology and natural philosophy that characterize many approaches to late 16th and 17th century chemistry. Both men were students of Jacob Zwinger at the University of Basel, who was actively sorting out the useful contributions of the Paracelsians to chemical medicine, and both valued Hermes Trismegistus' *Emerald Tablet* as a preferred expression of ancient alchemical truths. Libavius read the *Tablet* as coded laboratory procedures for preparing the philosophers' stone and admonished his contemporaries to eschew Paracelsian exegesis. Khunrath, in contrast, read

the *Tablet*'s singular teaching, so aptly summarized by James Joyce's 'the tasks above are as the flasks below,' as a cosmic statement of the fundamental unity of the microcosm and macrocosm and the complementarity of prayer and experiment as guides in alchemical work—an idea that Khunrath expressed in the familiar *ora/labore* image in his *Amphitheatrum sapientiae*, surely one of the best-known printed illustrations from early modern alchemy. This difference in readings of the *Tablet* aptly captures the watershed interpretive crisis of late Renaissance alchemy, namely, whether to pursue the book of nature as the full counterpart to holy scripture bringing to bear cabalistic methods or to include inquiry into material composition and transformation among the humanist scholastic disciplines, subject to open disputation and, eventually, published experimental verification.

In the wake of Thomas Kuhn's theory of scientific revolutions and Michel Foucault's emphasis on epistemological ruptures, it has become commonplace to locate chemistry's paradigm shift in the nomenclature reforms of Antoine Lavoisier and his circle in the second half of the 18th century. Stephen Clucas rejects this notion of a 'postponed Scientific Revolution' for chemistry, arguing that proponents of this idea have uncritically mixed all alchemical discourse into one bin and neglected the efforts of Andreas Libavius, Robert Boyle, and other critics of the obscurity of medieval alchemy to reform how chemistry was discussed. Building on recent extensive study of Libavius in this regard by Bruce Moran, Clucas argues that Libavius' effort to reform chemistry within an Aristotelian framework met with limited success because of general dissatisfaction with Aristotelian natural philosophy in the 17th century. Turning away from Foucauldian analysis, Clucas points to Boyle's criticism of the Paracelsians not for their modes of description but for their lack of experimental rigor as marking the true chemical revolution, one characterized by a methodological reform and not by a rupture in the field of discourse. His thesis is that establishing experimental proof as the arbiter of truth was a more salient revolutionary development than the break with the old nomenclature. This in general makes good sense; but it does not address Bill Newman's persistent pleas to consider that experiment was not wholly alien to the medieval and Renaissance Aristotelian tradition and it does not embrace recent attention to the active scientific reforms by Neo-Aristotelians in

the 17th century. Robert Boyle is still too much at the center of this narrative.

Dane Daniel explores the contention made by Carlos Gilly that Paracelsus' religious treatises were intentionally neglected by 16th-century Paracelsian enthusiasts and that when they became salient in the early 17th century, they were associated with Valentin Weigel and other enthusiasts in an effort to deflect criticism away from Paracelsus' medical and philosophical writings. Gilly's thesis helps to explain the nature of religious discussions by the Rosicrucians and other early 17th-century pietists who found in Paracelsus' religious texts a source for reform. But Daniel notes that Paracelsus' religious tracts were eagerly read by Alexander von Suchten, Adam Bodenstein, and Michael Toxites, and that these texts were widely copied and circulated. These facts and the publication of Paracelsus' *Astronomia magna*, which despite its name is fundamentally a Christian theological and anthropological text, contradict Gilly's hypothesis that the early Paracelsians intentionally avoided commentary on, or even exposure to, Paracelsus' theological treatises so as to protect them. Daniel's conclusions do not invalidate Gilly's useful insights but rather sharpen their application to the early decades of the 17th century and raise the important point that we know too little about the early reception of Paracelsus' ideas.

Larry Principe and Bill Newman have recently brought to the forefront of discussion about the history of early modern chemistry the debates over substantial change in 17th-century chemical discourse that arose as a raft of new experiments and theories challenged the reign of Thomistic Aristotelian matter theory. Margaret Garber shows how this debate worked out in Catholic Prague, where concerns about defending Eucharistic theology were paramount, and provides a satisfying example of the importance of taking local circumstances into account in historical analysis. In 1635, the attempt by J. Marcus Marci, dean of the medical faculty, to publish his version of the chemical theory in which material transformation was accounted for by the expression of active seminal principles within matter was blocked by the Jesuit dean of the arts faculty, who sought to maintain the traditional Thomistic teaching of substantial form. Marci had been schooled in the Thomistic tradition at Olmutz but abandoned it in light of laboratory demonstrations which showed that chemical forms persisted in transformations and were not destroyed and created as

Thomistic Aristotelian theory required. Marci eventually succeeded in publishing his book in completed form in 1662, after the structure of the university changed and he was elected rector, with the power to grant himself the needed *imprimatur*.

Not all Jesuits resisted the new metaphysical principles that chemists introduced to explain substantial change. Hiro Hirai explains in how Athanasius Kircher adopted Paracelsian ideas of seminal principles from Marci, Joseph Duschesne, and other writers within the Paracelsian tradition and adapted them to Aristotelian generation theory. Drawing on Marci's ideas about the plastic and attractive powers vested in seeds, recent developments in corpuscular chemistry, and traditional Aristotelian embryology, Kircher conceived of Paracelsian seeds endowed with innate heat, which fostered generation of organic bodies when lodged in suitable elemental wombs. In the depths of the Earth, these seeds formed a sulfurous, saline, mercurial vapor. A similar volatilized water was used to explain the subterranean generation of metals in Johann Grasseus' *Arca arcani*, which William Newman argues was used by Isaac Newton in formulating his ideas about the generation of metals. Newman's study of the Newtonian text *Humores minerales* reveals that the English virtuoso elaborated Grasseus' ideas into a theory that metals were constantly being created in the upper regions of the Earth by volatilized metallic fumes that rise from the core and coagulate dissolved metallic juices that are sinking downward. As the heavy metals continue to sink under their own weight, they are destroyed by powerful solvents at the core and re-volatilized, creating a cycle of metallic generation and destruction. The ideas in *Humores minerales* are similar to Newton's discussion of salts in the better known Newtonian text *Of Nature's Obvious Laws*, which is written on the other side, providing clues to the development of Newton's ideas.

Barbara Obrist offers an erudite and engaging analysis of an image of the near-naked lady *natura* confronting the alchemist that is featured in a manuscript titled 'The Complaint of Nature', which has been attributed to Jean Perreal (1516). This beautiful illustration, which is reproduced on the dust jacket to this volume, portrays lady nature as a vivid and sexually-accessible emblem of fertility, and contrasts strongly with the typical period images of nature as the concealed Diana. Building on an interpretive framework developed in Bill Newman's *Promethean Ambitions*, Obrist understands Perreal's

image as part of a dialectic between art and nature, employing the naked *natura* to chide the alchemist for mechanically copying her works rather than reproducing them through deeper understanding of her secrets; her hair, loose in the back and hanging down to her waist, contrasts with Alan of Lille's *Plaint of Nature*, where *natura* has the carefully braided hair of a virgin, suggesting an intentional intertextuality on the part of the author. Unlike the medieval classic *Romance of the Rose*, where nature acts to create using hammer and anvil, in *The Complaint of Nature* she exhibits creation as an organic process undertaken in the womb and not as a mechanical one; she invites the alchemist to model his work not on the mechanic but on animal generation.

In her chapter on 'deconstructing the chemical marriage', Allison Kavey takes on the popular but difficult task of commenting on the broader sexual ideas implicit and explicit in early modern alchemical discourse. Her sample is 31 texts printed in England 1580–1680, 18 of which exhibit sexual metaphors. Historians of science and early modernists in general are quite familiar with the gendering of gold and silver and the ample visual images that are based on the production and reproduction of metals in 'wombs' and from 'seeds', but Kavey's analysis goes further. The sexual dimorphism of hermaphroditic mercury, for example, is well-known to historians of alchemy; but Kavey argues that Mercury, associated with Ganymede, a young messenger serving the other gods, was portrayed in 16th/17th-century English literature as a young homosexual partner for an older man, opening up an entirely different interpretive dimension. Yet, while she notes that the various sexual pairings in the foundational text *The Emerald Tablet* are all heterosexual, she reads these in terms of a broader conception of sexuality than the standard chemical marriage implies:

> they nonetheless present multiple parents and imply multiple sexual pairings for [the production of] the single Stone. . . . In alchemical writing, however, the potentialities of combinations were determined by shared(?) natures, rather than the partners' sexes, and made possible by radical alterations in gender. [129]

One would think that the requirement that metals be like each other in order to mix would imply a homosexual identity but 'gender flexibility, rather than same-sex combinations, proves the key to successful alchemical work' [130], in part solved by the androgynous powers

of the variable mercury. Her addition to the historiography of the Scientific Revolution, not surprisingly, is wrapped up in the changing discourse within alchemy:

> Their language choices, and their ultimate rejection of sexual metaphors, reflect a clear and precise choice to avoid engaging in the ongoing medical debate about the meaning of biological and anatomical sex and the propriety of same-sex coupling in favor of providing readers with accessible language and examples through which to understand alchemy. [135]

That is, the changing discourse that is manifest in the 17th-century choice to de-gender chemical language in favor of un-metaphorical experimental discourse reflects a conscious attempt to make chemistry more directly accessible to the readers—a change in chemical (scientific) ideology and not a change in the cultural understanding of sexual metaphors. I think Robert Boyle would have agreed with this assessment.

Two articles on alchemical apparatus and spaces remind us that archeological studies of the material culture of alchemy can provide important perspectives on how alchemy was actually practiced, which can otherwise only be inferred from written sources. Marcos Martinón-Torres focuses on recovered crucibles, chemical analysis of which reveals traces of chemicals they once contained. Study of their form and composition, which was relatively stable in early modern central Europe, yields clues as to how they were used and the wide circulation of alchemical technologies. R. Werner Soukup surveys the results of extensive archeological study of a 16th/early 17th-century laboratory at Oberstockstall Castle, which was owned by the Fugger family. Recovered alloy of gold, silver, and copper speaks to the laboratory's principle use in assaying the production of Tyrolean mines to direct capital investment; but residues of antimony trichloride and calomel revealed by X-ray diffraction suggest the production of Paracelsian medicines as well.

Taking a social-constructivist approach to the study of alchemical fraud in early modern Europe, Tara Nummedal analyses the case of Hans Nüschler, who entered into a contract with Duke Friedrich of Württemberg to produce gold from silver and to prove the gold at his own expense in the Duke's laboratory. When the trials failed, Nüschler turned to fraud in an attempt to cover his failure but was

discovered, arrested, and then hanged like previous alchemical frauds. Nummedal concludes that the willingness of alchemists like Nüschler to enter into contracts and assume all the risk of the demonstration means that they believed in honest transmutation and that they could make these procedures work, rather than intentionally duping patrons into financing their failures, as depictions of alchemists in popular literature attest. In her view, fraud was not an ethical issue as much as a legal category constructed by the wealthy patrons in response to the client's failure to fulfill his contract. Nummedal is concerned that study of alchemical fraud might be interpreted as undermining attempts of historians of science to take alchemy seriously and argues that examination of cases like Nüschler's 'opens up a whole world of entrepreneurial alchemical practice' in the period and reveals that

> alchemy was not merely a bookish or symbolic object of study in the sixteenth century, but was also thoroughly immersed in the world of profits, money and political authority. [180]

The careful reader will see that the issue of fraud, fraught with methodological issues about determining intentionality from court cases, is not directly implicated in her larger argument. Ultimately, she makes the important point that fraud and reactions of alchemists to fraud are topics that touch on issues of who had the authority to define the legitimacy of alchemy and how this was done.

Victor Boantza's study of the chemical ideas and laboratory work of Samuel Duclos, a little known founder of the Paris Academy, takes on the hegemony of Robert Boyle in many accounts of 17th-century chemistry and shows the limitations of social constructivist accounts of science based on study of Boyle. The long-lived characterization of Boyle's role in leading alchemy to chemistry, which has only been seriously challenged and revised in the past decade or so, was based in part on Fontenelle's distinction between chemistry and physics that was based on the reduction to mechanical principles, in which he identified Boyle with physics and Duclos with chemistry. According to Boantza, this characterization 'has cast an enduring spell upon the historiography of early modern chemistry', enabling historians to focus on Boyle's work as decisive and to ignore Duclos' work [182].

Duclos joined the Paris Academy in 1666 at age 68, charged with building and managing chemical laboratories for analysis. He was a very active member and figures prominently in early Academy records, being 'mentioned more than any other academician' during the early years [184]. The Academy charged Duclos with reading and commenting on Boyle's work, beginning with *Certain Physiological Essays* (1661), and he used the opportunity to confront Boyle's deployment of mechanical philosophy in chemistry. Boantza here undertakes to revise our understanding of Duclos' chemistry and to shed light on the production of chemical knowledge in this seminal period through careful study of Duclos' critical reading of Boyle. He observes that 'insofar as skill and erudition are concerned Duclos emerges as superior to his English counterpart' [185]. Duclos' method was to isolate excerpts from Boyle's text and subject them to comment and experimental verification through 'lecture-demonstrations' [185]. The result is that whereas Boyle's experimental reports seem to depend heavily on anecdotes reported by other virtuosi, whose credibility Boyle vouches for, Duclos straightforwardly reports his own reasoning and experimental demonstrations, and criticizes Boyle for failing to verify experiments adequately through repetition, complaining about the variability of reagents and thus the undependability of experiments. Duclos argued that dependability was achievable through careful method. This analysis shows the limitations of applying social-constructivist arguments too widely to the problem of how facts were established in early modern science.

Following Boantza's reassessment of the process of making chemistry conform to the methods and standards of physics in the 17th century, Luc Peterschmitt refutes the idea that chemistry became scientific with its reduction to mechanistic physics by arguing that the Cartesian programs did not permit a distinctive chemical theory and, therefore, could not support such a paradigm shift. His method is to consider three Cartesian mechanical philosophers from the mid to late 17th century; and his approach is inherently philosophical, not historical. The somewhat unsatisfying conclusion of his inquiry is that there was no mechanical chemistry in this period because chemistry and mechanical philosophy are incompatible. A related but more historical approach is that of Bernard Joly, who uses a dispute over the validity of *chrysopoiea* between the mechanist

Nicholas Lemery and the traditional chemist Etienne-François Geoffroy to bring insight to the contributions of mechanical philosophy to the development of chemistry. Geoffroy extended J. J. Becher's experimental production of iron from clay and oil, substituting wood ash for clay. Geoffroy suggested that the iron was transmuted from the principles of the wood upon burning. But Lemery insisted that the iron was already present in the wood, drawn from the earth with the tree-sap as it the tree grew, and was not a product of combustion. He then came up with a Cartesian-like mechanical explanation to explain why a magnet could not detect iron in the wood or clay prior to combustion. The irony, Joly finds, is that Lemery's mechanical approach, which was once thought to be a step in the major break from Renaissance vitalist alchemy initiated by Robert Boyle, was rendered obsolete by Newtonian physics, whereas Geoffroy's more traditional ideas led to 19th-century affinity theory.

Continuing his long-standing exposition of the place of Georg Stahl in the history of chemistry, Ku-Ming Chang claims that it was Stahl's careful reviewing and commenting on J. J. Becher's vitalist ideas about the nature of material change that led Stahl to abandon his earlier enthusiasm for 'immanent vitalism' and to develop a more materialist metaphysics that supported the later 'Enlightenment vitalism'. Beginning with his foreword to Becher's *Chymischer Glücks-hafen* (1726), Stahl turned against alchemy, publishing two more anti-alchemical books as commentaries on forewords to Becher texts, including *Natur-Kündigen*, which illustrates the belief that terrestrial metals are nourished by planetary influences and produced by metallic seeds—ideas Stahl came to reject. Stahl concluded that there was nothing like fermentation at work in metal production and that the seed-tincture idea cannot be right, and so he adopted a particulate theory:

> Once Stahl rejected Becher's cosmological picture in which the cosmic vital power and the *semina* of metals were merged, he renounced all possibilities of Renaissance vitalism. [222]

Ultimately, Ku-Ming positions Stahl in a key transition from Renaissance immanent vitalism, where all matter that contains a metaphysical seed possesses innate vitality, to what Peter Reill calls Enlightenment vitalism, where only organic matter is endowed with vitalism, which then is a property of organic matter. The lines of connection

between Stahl's reformulation of 17th-century vitalism and Enlightenment and 19th-century discussions of vitalism are intriguing and give me new stimulus to ponder the legacy of Paracelsus' Renaissance vitalism.

John Powers outlines the historical changes in Hermann Boerhaave's understanding of alchemy. Boerhaave began extensive alchemical experiments after being named Professor of Chemistry at the University of Leiden in 1718, including George Starkey's mercurialist processes for the making the philosophers' stone—all failed. But, despite these failures, Boerhaave defended the principles of alchemy in his 1732 textbook *Elementa chemiae* but specified the need for experimental verification. Boerhaave's experiments did not eliminate belief in alchemical transmutation but did convince him 'that he had taught the wrong theory of metals for thirty years', namely, that the mercury theory of the composition of metals was wrong [237]. Instead, he now favored the idea that metals were formed from 'Guhr'—an oily fluid filtering through the earth. But, by 1636 his days as an experimentalist were over and he did not follow up on this hypothesis. In the end, the influential Dutch teacher shines forth as an exemplar of the emergence of early modern scientific sensibility—a convinced experimental philosopher, open to all claims, but accepting none unless verified by experiments.

How occult ideas within chemistry were displaced by mechanical chemistry and Cartesian and Newtonian matter theory is taken up by Hjalmar Fors, who examines the specific case of the Swedish Board of Mines in the period 1680–1760. The Board's chemical laboratory was initially established for iatrochemical preparation to supply drugs to surgeons in the Board's employ and continued mainly in this capacity to 1689. Then, under the leadership of the Paracelsian-minded Urban Hiärne, the laboratory was operated more independently as a kind of *de facto* royal laboratory. Fors disagrees with earlier accounts of Hiärne as the first important Swedish chemist 'in a modern sense', arguing that Hiärne's continuities were with earlier German thought and not with Enlightenment chemistry, which was introduced to Sweden by Georg Brandt. Brandt studied chemistry under Boerhaave in Leiden; and under his leadership, the Board of Mines became quite oriented toward mechanical chemistry and Cartesian and Newtonian ideas, an orientation followed by his apprentice Axel Cronstedt, who

openly denigrated alchemy as mystical, like magic and astrology, and, therefore, unsuitable as a modern science.

Lest we be too lulled into a renewal of the history of the positive march toward progress in the sciences with Boerhaave and his students, Claus Priesner reminds us that unrecorded scientists who were members of the Gold- und Rose-Cross and Illuminati societies continued efforts to transmute metals in Enlightenment Germany. Samuel Richter (Sincerus Renatus) laid the foundation for Gold- und Rose-Cross as a successor to the Brotherhood of the Rosicrucians in his *Stone of the Sages* (1710) and *Theo-Philosophia* (1711); and the society flourished from 1765 into the 1780s before being officially dissolved 1792. The society, which was mainly Protestant like its Rosicrucian forebears, had some connections with the Illuminati, an anti-religious group of social/intellectual reformers formed in Bavaria in 1776.

Wouter Hanegraaff provides a narrowly-focused story of Giovanni Corregio, an Italian alchemist working in the second half of the 15th century. In his later years, Corregio fashioned himself as a prophetic Neoplatonist, like the Hermetic prophet Pimander, after Ficino's influence, and wrote a treatise on the Phoenix-stone that he dedicated to Pope Julius de Rovero, apparently in a desperate attempt to keep himself out of dire poverty.

Gabrielle Ferrario investigated the origins and transmission of an important medieval alchemical manuscript, the *Liber de aluminibus et salibus*, affirming Julius Ruska's attribution of the manuscript to an anonymous physician of 12th-century al-Andalus rather than to al-Razi or some earlier Arabic writer. The text was first translated into Latin by Gerard of Cremona in the 12th century, used as a source by Vincent de Beauvais and Roger Bacon, and printed in 1560 as part of *Compendium alchimiae*. Multiple Latin manuscripts exist but only one in Arabic and one in Hebrew, suggesting that its popularity came within the Latin alchemical tradition.

BIBLIOGRAPHY

Moran, B. T. 2007. *Andreas Libavius and the Transformation of Alchemy: Separating Chemical Cultures with Polemical Fire*. Sagamore Beach, MA.

Archaeology and the Origins of Philosophy by Robert Hahn

Albany, NY: State University of New York Press, 2010. Pp. xxviii + 307. ISBN 978–1–4384–3165–9. Cloth $80.00

Reviewed by
Dirk L. Couprie
Amsterdam, The Netherlands
dirkcouprie@dirkcouprie.nl

In his *oeuvre* on ancient Greek philosophy, Robert Hahn, in the course of the years, has chiseled out his own niche, asking what light can be thrown on the teachings of these ancient philosophers by studying the contemporary archaeological data. In the first part of his latest book, he adds some new material to that of his former publications; and in the second part, he offers a methodological and even metaphysical reflection on his way of working. This book, and Hahn's enterprise over the years, may be called courageous, as it intends to lay bare lines of investigation that have hardly been explored before, if at all. It is also courageous in its effort to row up the stream of a historic study of ancient philosophy by choosing explicitly to place the ancient thinkers in their historical and social contexts. The book is written in an enthusiastic style that easily carries his reader with him, especially where he does not possess the same amount of archaeological knowledge as the author has acquired. Sometimes, however, Hahn seems to be dragged along by his own enthusiasm to such an extent that his argument tends to be suasive rather than convincing. I will discuss some examples that at least did not convince me. There are many informative illustrations, although some of them, as we will see, lack the required kind of precision.

The title of the book links archaeology and (the origins of) philosophy. 'Philosophy' has to be taken here in a rather broad sense, as the whole first part is about Anaximander's cosmology, which some would not even count as part of philosophy. Hahn's claims are only accidentally applied to the interpretation of what traditionally are regarded as Anaximander's genuine philosophical items: the Boundless as the origin of everything and the only surviving fragment [94]. Hahn recommends especially that those who are acquainted with his

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 78–96

earlier work read the second part first. In this review, I will follow his advice.

In the second part of his book, Hahn argues against the analytic philosophers who hold that historical inquiry is irrelevant for the understanding of ancient philosophers. This position is well expressed in Barnes' statement that 'philosophy lives a supracelestial life beyond the confines of space and time' [180]. Hahn, to the contrary, is deeply convinced that 'Anaximander, indeed all thinkers, think through and by means of their cultural and historical context' [211], that 'meaning is never divorced from background' [213], and that there are no brute facts, 'or, better yet, all facts are institutional facts' [220]. These more general statements are narrowed down in the both modest and rather surprising claim to show that archaeology has some relevance to philosophy and especially to an understanding of the historical origins of philosophy [183].

Hahn practices a method that he calls 'a kind of inverse archaeology' [189]. This method starts from the technological objects and procedures referred to in the writings of, or in the reports on, ancient philosophers and proceeds to investigate the material stuff that inspired them in order to discover what new light they can shed on the interpretation of the ancient texts [189, 229–230]. His ideal is to catalogue all the references to *technai* that can be gathered from our doxographical sources and to discuss them with the archaeologists [184].

In line with the pragmatist and hermeneutic traditions in philosophy, Hahn maintains that objects are not just objects and artifacts are not just artifacts, but that they can be understood only by a process of imaginative interpretation that is not predetermined by a set of rules [204, 208]. The interpretation of archaeological artifacts is a matter of educated guesswork in which the material-embodied experience is connected to the domain of imagined thought. The obvious danger is that the interpretations turn out to be imaginary, accidental, or irrelevant. This risk, however, is reduced because the process is always open to falsification through ongoing archaeological and doxographical evidence [203, 207, 211].

In Anaximander's case, Hahn says, the contextual environment was dominated by the astonishing building of huge temples in his backyard. Hahn's main claim is that 'Anaximander interpreted and projected metaphorically onto the cosmos what he discovered at the

building site(s)' of the big temples [196–197], viz. column drums of different dimensions prepared so as to fit exactly upon each other to make columns, the use of modules and simple ratios in designing the plan of the temple, turning wheels of various sizes and constructions used to perform diverse tasks, and bellows in the blacksmith's shop. Given that Anaximander compared the Earth to a column drum with the dimensions of its diameter being three times its height, that he visualized the celestial bodies as huge wheels, that he used simple ratios for the distances of these celestial wheels, and that he said—according to an unfortunately generally accepted translation—that the light of the celestial bodies comes to us as through the nozzle of a bellows, these are arguable claims. But Hahn's ambitions go much farther when he writes that

> Anaximander's use of architectural and material terms *should not be considered accidental or additive, but rather constitutive, in any exegesis of his philosophical thought.* [223–224, my italics]

And again:

> *Only through an understanding of these literal concepts* [*scil.* of the architecture culture of his time]*... are we able to direct our attention back to the relations that Anaximander wished to intimate.* [228, my italics]

In other words, according to Hahn, Anaximander did not use column drums, simple ratios, wheels, and bellows as simple images to illustrate his cosmological ideas in a way that his contemporaries could easily understand; rather, these images make up in some metaphorical way the very heart of what he wanted to say about the cosmos: 'Anaximander came to see the cosmos in architectural terms' [230]. I will suspend my judgment until we have seen what the results of the method of 'inverse archaeology' are when applied to concrete cases.

In the first part of the book Hahn delivers several case studies of 'inverse archaeology', all of which are about Anaximander's cosmology, which he considers as a test case for his method.

Chapter 1 gives a quick survey of the subjects treated in the first part of the book, which together aim to make up 'Anaximander's Cosmic Picture', as well as a charming and well-illustrated section called 'An Imaginative Visit to an Ancient Greek Building Site'.

The starting point of Hahn's studies is Anaximander's identification of the Earth with a column drum, which 'was no throwaway at all, as scholars must have assumed by their silence, but rather the so-called tip of the iceberg' [15]. This is also the subject of chapter 2, and it is here that Hahn gets his most convincing results. The ancient Greeks thought of their flat Earth as somewhat concave. Hahn shows (as he has in previous studies) how the column drum was prepared by the technique of ἀναθύρωσις to get a slightly concave surface in order to make it fit perfectly to the other drums [42]. This provides an elegant support for the translation 'concave' of the Greek γυρόν as applied to Anaximander's Earth in the doxography (with Roeper's generally accepted emendation of γυρόν for the ὑγρόν of the manuscripts).

Hahn argues that Anaximander chose the column drum, and not the column base, because the drum readily revealed nature's hidden structure, namely, that the Earth, supported by nothing, remains motionless in place [49–50]. Here one wonders what could be meant, for although it is true that column drums in a column remain motionless in place, one cannot maintain that they are supported by nothing.

Column drums existed in a great variety of proportions. Why exactly Anaximander chose the ratio 3:1 has, as far as I know, never been explained conclusively. If I understand Hahn's argument, Anaximander chose it because he adopted a modular technique for expressing cosmic dimensions. The module of temple building, Hahn argues, was the 3:1 column base; and, therefore, Anaximander took this as the ratio of his column drum-like Earth as well. To me this argument is not completely convincing.

In chapter 3, Hahn argues that Anaximander, having chosen the column drum with the proportions 3:1, introduced the number 3 as a base for his other numbers, indicating cosmic distances as multiples of 3. Hahn also expresses this by saying that he took the diameter of the Earth as his cosmic module, just as the architects used the lower column diameter as a module for intercolumnar measuring. Taking the Earth's diameter as a module is, I would say, not the same as taking the number 3 as a module, even if the Earth's diameter is three times its height. However this may be, it results in Anaximander's numbers, as is shown in Figure 3.5 [73: cf. 29 (Figures 1.16), 17

(Figure 1.2)[1]]. The same image has appeared in numerous earlier publications.[2] Indeed, the fact that this image is used so many times obviously indicates that it is an essential illustration of Hahn's ideas. There are slight differences between the several versions but not as regards the issue to discuss here.

Looking carefully at these images, one discovers that something is wrong: when we count the little circles to the right of the Earth we have $2 \times (9+1) = 20$ Earth-diameters instead of 19 up to and including the Moon wheel, and $3 \times (9+1) = 30$ Earth-diameters instead of 28 up to and including the Sun-wheel. In other words, in order to get the right totals of 19 and 28 there will have to be 8 little circles instead of 9 between the wheels of the stars and the Moon, as well as between the wheels of the Moon and the Sun. However, this result does not go well with Hahn's idea that '*the cosmic numbers appear as iterations of the* $9+1$ *formula*' (my italics), also expressed as 'the appearance of the formula $9+1$ in Anaximander's map of the cosmos' [84]. So far as I know, everyone (myself included) has overlooked thus far that this image contains a serious error in calculation.

My remarks are based on the hard cover edition of the book. I have been informed that in the paperback edition a new image will be offered with eight little circles instead of nine between the wheels of the stars and the Moon, as well as between the wheels of the Moon and the Sun. I guess that some textual changes are needed as well. As long as these are not available, the best we can do is to suspend judgment on this point.

Hahn suggests, as he did in former publications, that the number 9 (+ 1) has to do with the length of the Ionic column which, 'based on surviving examples from Delphi, has been generally reckoned to reach a height of 9 or 10 times the lower column diameter' [83]. In a footnote, he makes a *proviso*, to which I would add that one of the conclusions of a thorough study on the mathematical foundations of ancient temple architecture is that looking for 'round' relations between diameter and length of a column does not correspond to the practice of the architectural design [see De Jong 1994, thesis 6]. In

[1] Here reproduced as Figure 1 on p. 83.

[2] Hahn 2001, 189 (Figure 4.5 (4)), 191 (Figure 4.6), 218 (Figure 4.21); Hahn 2003, 84 (Figure 2.4), 145, (Figure 2.22), 47, (Figure 2.23). See also Hahn, 2007, section C.

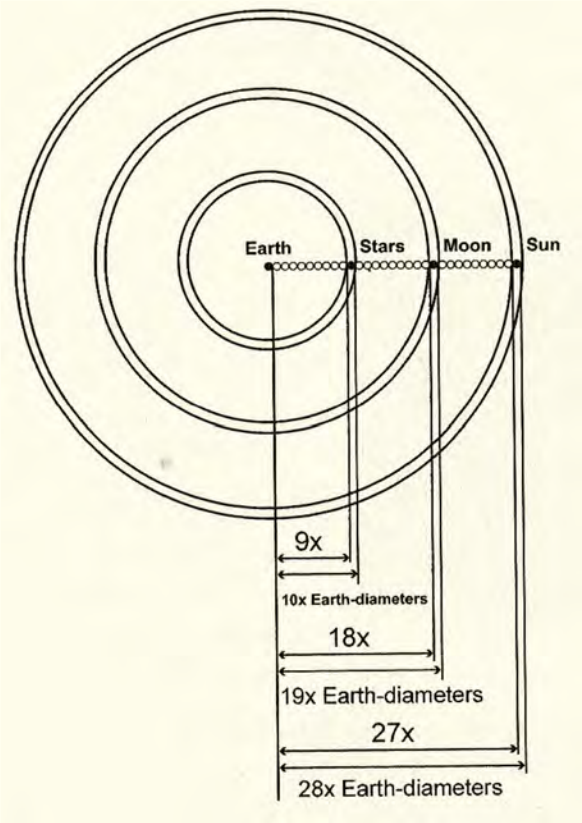


Figure 1. Anaximander's cosmos in *plan* view, showing too many modular circles

terms of cosmology, the only reasonable explanation of Anaximander's numbers, which Hahn fortunately also mentions, is that in the Greek counting system the number 9 expressed the notion of 'big', 'many', or 'a great distance' [see 83–84], so that the number 9 may indicate that the stars are far away, the number 18 that the Moon is even farther away, and the number 27 that the Sun is farthest away. In other words, the revolutionary cosmological insight that the celestial bodies are behind each other came first, and the question of how to express this conception in a way that was understandable to his co-citizens was secondary. I will return to this point at the end of the review.

Hahn claims that Anaximander made use of two ways of viewing that he borrowed from the architects: plan (as in Figure 1, p. 83) and elevation (as in Figure 2, p. 85). From an architect's point of view, however, in both something strange is at stake. Anaximander's plan view is, according to Hahn, a horizontal cross section through the plane of the Earth [66, 72]. On such a cross section, the celestial wheels would not even appear as wheels, as they are tilted with regard to the plane of the Earth. Strangely enough, Hahn writes in this context:

> Imagining a cross section through the plane of the earth... he could proceed as if the heavenly wheels lie obliquely to the surface of the earth, though in fact they do not, as an elevation view of Anaximander's cosmos makes clear. [72]

This should be the other way round:

> Imagining a cross section through the plane of the Earth, he could proceed as if the heavenly wheels lie in the same plane as the surface of the Earth, though in fact they do not, but lie obliquely to the Earth's surface, as an elevation view of Anaximander's cosmos makes clear.

The elevation view in the architectural sense is a front view (as in Hahn's Figure 1.15 [29]), whereas Anaximander's alleged elevation view sees his cosmos under an angle, in perspective [29: Figure 1.16]. In a front view the celestial wheels would—again—not even appear as wheels but as lines and the virtual cylinders along which they slide up and down would not appear as cylinders but as rectangles.[3]

In chapter 4, Hahn discusses the interpretation of an expression used in the doxography to describe how the light of the celestial bodies escapes from an aperture in the celestial wheels, ὥσπερ διὰ πρηστῆρος αὐλοῦ or οἷον πρηστῆρος αὐλόν. The connection of the words πρηστήρ and αὐλός is a unique occurrence that is usually translated as 'through the nozzle of a bellows.' This translation goes back to Hermann Diels and is defended by Hahn as well. Accordingly, he shows several examples of ancient bellows. As he is arguing against an article of mine in which I maintain that Diels' translation is wrong, I will take the opportunity to make a few remarks [cf. Couprie 2001].

[3] Cf. Figure 2 on p. 85, in which 'Virtual Earth' should be simply 'Earth'.

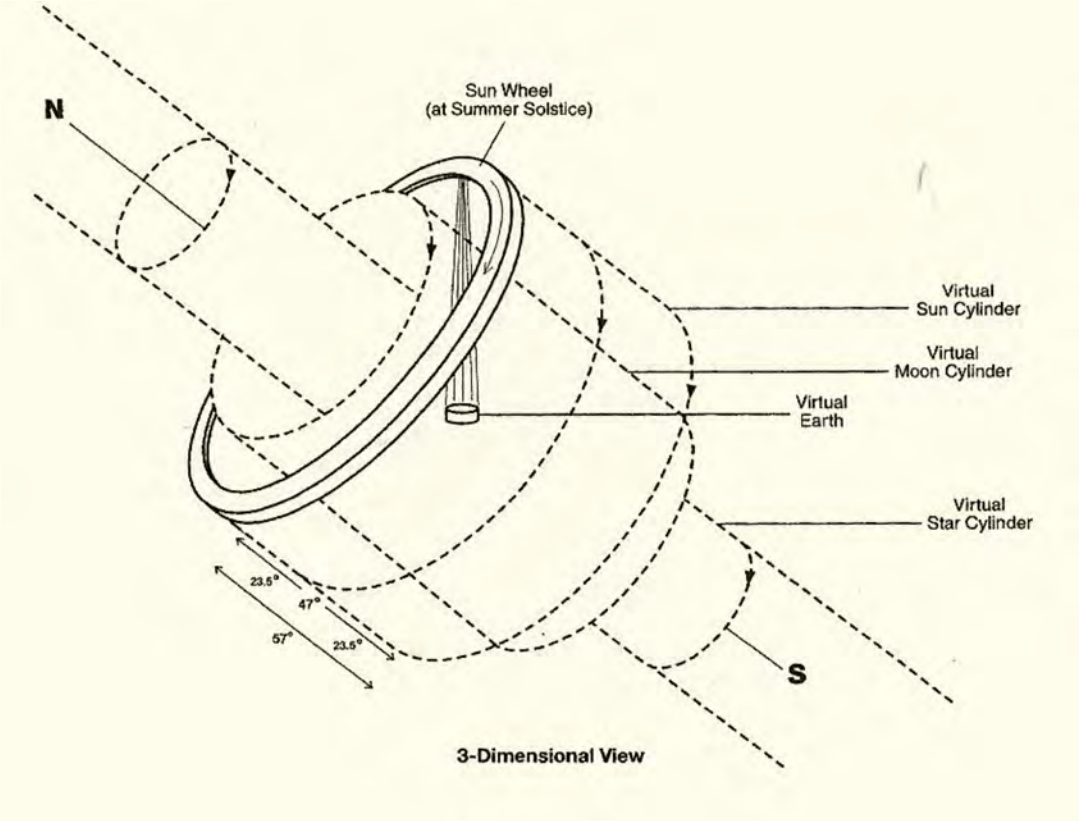


Figure 2. Hahn's version of Anaximander's cosmos in 'elevation view'

First of all, in defense of his translation, Diels [1954] adduces two texts from Hippocrates' *De articulatione* in which is described how a sack (ἀσκός) is blown up (like a child's balloon) by means of a brazen pipe (αὐλὸς ἐκ χαλκέου) that is attached to it. This instrument was placed under a dislocated hip joint with the intention of getting it back in place again. The very word πρηστήρ is not used in this text, which makes it hard to see how it can be used as a defense of the translation of πρηστῆρος αὐλός as 'the nozzle of a bellows.' The words αὐλὸς ἐκ χαλκέου can certainly not be translated as 'the blacksmith's bellows', although Hahn would have us believe so [90]. In his *Doxographi Graeci*, Diels did not mention Hippocrates in this connection but pointed to Apollonius of Rhodes' *Argonautica*, where the word πρηστῆρες is used in the context of Hephaestus' forge.

Hermann Fränkel has argued, however—and I think convincingly—that the meaning of πρηστήρ here is not 'Blasebalg' but 'Gluthauch' ('scorching wind'). Afterwards, Diels never quoted this text again in defense of his translation. The context of a forge is completely missing in Anaximander's text. On the other hand, in the doxography on Anaximander, the word πρηστήρ is said to indicate a weather phenomenon related to lightning and heavy wind. The simplest and most obvious interpretation of πρηστῆρος αὐλός, which is a heavenly phenomenon, is to relate it to the meteorological phenomenon that is indicated by the word πρηστήρ and to take αὐλός to mean 'stream', 'jet', or 'squirt', as it does sometimes in Homer. This would result in a translation of οἷον πρηστῆρος αὐλόν as 'like a stream of lighting fire', or perhaps 'like a stream of scorching wind'.[4]

Stated briefly, Anaximander is trying to explain the phenomenon of the light of the celestial bodies on the analogy of a meteorological phenomenon. In order to appreciate this analogy one has to realize that Anaximander does not make, as does Aristotle, a sharp distinction between what happens in the heavens and what happens in the sublunary sphere. To the contrary, we are informed that according to Anaximander meteorological and celestial phenomena are immediately connected. The turnings of Sun and Moon, for instance, are caused by winds that originated from the water that covered the primeval Earth and evaporated under the influence of the Sun.[5] My conclusion is that the expression πρηστῆρος αὐλός has nothing to do with a bellows at all.

Hahn combines the idea of the alleged celestial bellows in Anaximander with the conception of a living and breathing cosmos, pointing to the words στόμιον ('opening', 'mouth') and ἐκπνοή ('exhalation') that are used in the doxography; and he concludes that Anaximander conceived of the cosmos as a living and breathing being [113]. It seems to me, to the contrary, that the two are incompatible: either

[4] I take it that οἷον πρηστῆρος αὐλόν mirrors best the original wording, and that ὥσπερ διὰ πρηστῆρος is a free rendering of by doxographer who did not completely understand what was meant, as is also the case in another doxographical account, in which the word αὐλός is replaced by σάλπιγξ ('trumpet').

[5] See Aristotle, *Meteor.* 353b6 ff., and Diels and Kranz 1954, 12A27 and 64A17. Aristotle severely criticizes such theories in *Meteor.* 355a5 ff.

the image of the openings in the celestial wheels is that of an exhaling mouth-opening or it is that of the nozzle of a bellows; but it is not both together. The idea, though, of a stream of fire or a stream of scorching wind may be readily combined with the image of mouth-like openings blowing out that stream. Perhaps the combination of such heterogeneous things as a (celestial) wheel and a mouth-like opening looks less strange when we read that, according to Workman [1953, 46], the words ἐκπνοή ('exhalation') and στόμιον ('mouth-opening') are technical terms of bronze-founding and indicate the air-holes in the casting-mould. There, too, two heterogeneous things (casting-mould and mouth) are combined to describe how a stream of hot air escapes from an object. The use of such an image to elucidate a part of the celestial phenomena does not necessarily imply that the whole cosmos is conceived of as a living being, just as the use of architectural images does not necessarily imply that the whole cosmos is conceived of as a house, which we will come to speak about later on. This way of arguing typifies Hahn's inclination to jump enthusiastically to conclusions. Moreover, it is hard to see how the cosmos could be at the same time a house and a living, breathing being.

In chapter 5, Hahn goes deeper into the subject of the celestial wheels, linking it to the idea of the *axis mundi*. Anaximander imagined the celestial bodies as huge wheels made of thick air with fire inside that is only visible at openings in those wheels. It is generally accepted that what is meant are the rims or felloes of wheels. Hahn shows that such wheels with hollow felloes really existed in Anaximander's time. They were invented by the architect Metagenes for transporting monolithic architraves that were enclosed like axles in the wheels. In an attempt to elucidate Anaximander's cosmos, Hahn compares it with such a wheeled vehicle [142 (Figure 5.15c) = 19 (Figure 1.4), reproduced here as Figure 3, p. 88].

The little disk in the center represents the Earth, the two circles represent the wheel of the Sun at summer and winter solstices, and the dotted lines suggest a cosmic axle, an *axis mundi*. This drawing has serious problems. First of all, the two 'wheels' are too far away from each other to give a right representation of the movements of the Sun as seen from a flat Earth. If in this picture, as Hahn indicates, the circles represent the Sun-wheel at the summer and winter solstices, it looks as though the Sun is in the zenith at the equinoxes in spring and in autumn. This is obviously done to obtain a better

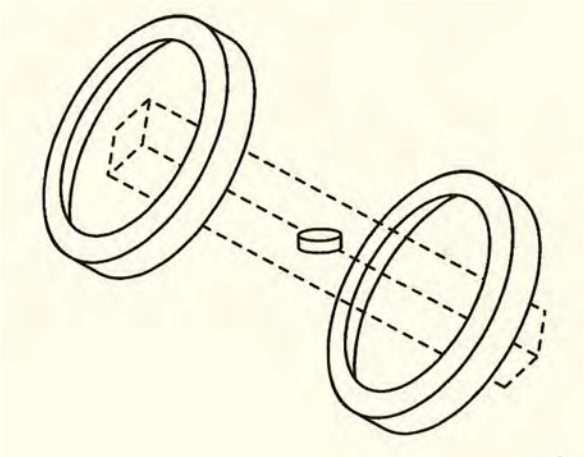


Figure 3. Hahn's picture of Anaximander's Sun-wheel (in two positions) with axle

resemblance between Anaximander's universe and Metagenes' vehicle; but cosmologically speaking it is completely wrong [cf. Figure 2, p. 85 above].

The axis of the heavens has to be thought of, according to Hahn, as a big column at the center of the world, around which the firmament revolves and of which the Earth is one of the drums [142]. In Figure 2.9 [50] a pointer marked '3 × 1 Column Drum' indicates a drum in the middle of the column (Note: Hahn uses '3:1' and '3 × 1' interchangeably). In cosmological terms, this signifies the drum-shaped Earth in the middle of the celestial axis. However, the drum to which the pointer points measures only 2 × 1, as the reader of the book can easily verify. I think that drums with the dimensions 3 × 1 were usually applied at the top of the column, as they were relatively light and thus could be lifted easier to great heights. If so, the image of the Earth (drum) in the middle of the celestial axis (column) fails.

Another difficulty that Hahn pays insufficient attention to is that the axis of the heavens is tilted, whereas a column is meant to stand right up. The tilting of the celestial axis was a problem that bothered several Presocratics. Since ancient times, its interpretation is haunted by the failure to recognize the difference between how things are seen from a flat and from a spherical Earth, as I have

explained extensively elsewhere [Couprie 2009]. Hahn, too, falls into this trap when he mentions 'the stunning reality' for the archaic Greeks who thought that the Earth was flat, 'that either the cosmos is inclined 23.5° north or that the Earth is inclined 23.5° north' [176]. First of all, the last 'north' is obviously a slip of the pen, and has to be read as 'south', as some Presocratics are said to believe not that the celestial axis was tilted toward the north but the Earth toward the south. Essential, however, is that the celestial axis, which coincides with the axis of the *spherical* Earth, is tilted 23.5° with respect to the ecliptic pole (and not 'north'); or, said otherwise, the ecliptic is inclined 23.5° with respect to the celestial equator, which is a projection of the Earth's equator. On a *flat* Earth, to the contrary, there is not such a thing as an equator coinciding with the celestial equator, and the celestial axis (which is the line between the celestial pole and the center of the Earth) does not coincide with Earth's axis. This means that the amount of the tilting of the celestial axis toward the north depends on where you think the center of the flat Earth to be. For those who think, e.g., that Delphi is the center of the flat Earth, the tilting of the celestial axis is 51.5° to the north with respect to the zenith.

Moreover, Hahn defends the idea that not Delphi but Syene[6] (modern-day Aswan) was the center of Anaximander's flat Earth because in this location the Sun at noon on the day of summer solstice stands in the zenith, with the result that a gnomon throws no shadow [157–158]. This would imply that the celestial axis was tilted at an angle of 66.5° to the north with respect to the zenith, which means that the celestial axis is almost lying down instead of standing right up. As said already, elsewhere Hahn extensively shows how column drums are prepared to fit exactly upon each other in order to make a perfect column [42–44 and Figure 2.5]. How the Earth as a column drum is thought to fit into a column that is tilted that much he does not tell.[7]

Chapter 6, in which Hahn tries to reconstruct Anaximander's sundial, is the one in which his enthusiasm and suggestive way of writing tends to let him forget his critical sense. Let me start with

[6] Written as 'Cyene' by Hahn.

[7] When the Earth is imagined as tilted (the 'dip of the Earth' that some doxographers ascribe to later Presocratics), then the Earth as a column drum does not fit into the celestial axis either.

his reconstruction of Anaximander's seasonal sundial with summer and winter solstice 'sun wheels' [161 (Figure 6.11)]:

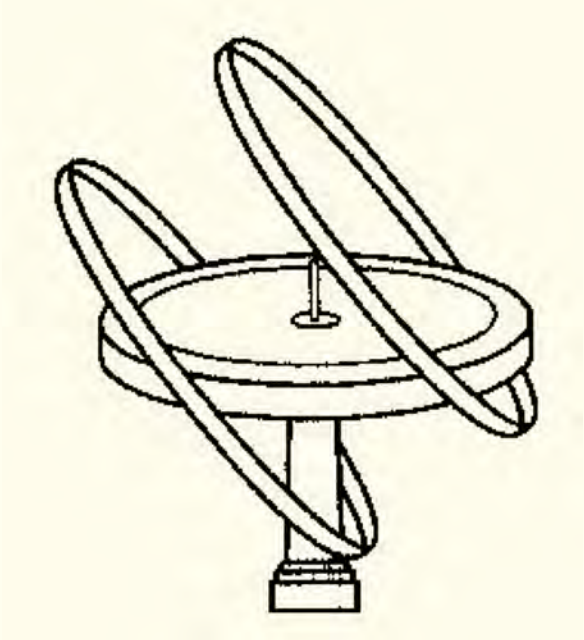


Figure 4. Hahn's rendering of Anaximander's seasonal sundial

The perspective in the image on the left of Hahn's Figure 6.11 is definitely wrong, as the reader may easily notice. According to my information, this flaw too will be adjusted in the paperback edition of the book. So, I will concentrate instead on the image on the right of Figure 6.11 which is reproduced here as Figure 4.[8]

As I have already said, Hahn suggests that Syene was the center of Anaximander's flat Earth. One would expect, consequently, two features to become visible in Hahn's drawings: the Sun at noon on the day of summer solstice at the zenith, and the tilting of the celestial axis at an angle of 66.5° to the north. But this is not the case. On page 160, Hahn puts forward the plausible suggestion that the disk

[8] Actually, I took Figure 1.5 [20] because Figure 6.11 [161] omits the gnomon and the little ring around it.

representing the Earth in Anaximander's alleged sundial must have had the proportion of 3:1. One would also expect to see these dimensions in his drawing, but again this is not the case. When one draws the disk representing the Earth in the right proportions and adds the above-mentioned characteristics of Syene, as is done in Figure 5 [p. 92], another problem in Hahn's reconstruction becomes apparent: the width of the rings representing the solar wheel, which should be equal to the Earth's diameter, is much too small. Moreover, the diameter of the solar wheel should be 56 Earth diameters instead of one. In the reconstruction, no place is left for the wheels of the stars and the Moon. In other words, in Hahn's reconstruction the Sun is not very far away, as he stresses elsewhere [84]; indeed, to the contrary, it is very near the Earth. Perhaps, one might say that the construction shown in Figure 6.11 is only a *sketch*, but considering the many questions that arise one wonders what it actually intends to illustrate.

The problems indicated above arise from Hahn's attempt to combine in one and the same device both a seasonal sundial and a model of Anaximander's cosmos. Additionally, Hahn suggests that Anaximander's map of the Earth and his seasonal sundial were combined in the same scale model [160]. The map, then, was drawn on top of the horizontal disk. This means that the center of the disk must have been Syene, whereas Sparta and Miletus must have been depicted somewhere north of Syene. This leads to other inconsistencies, as the sundial was said to have been erected in Sparta. In other words, on the map that was the ground-plate of the sundial, the gnomon stood in Syene, where on the day of summer solstice a gnomon casts no shadow at noon; but in reality the gnomon was erected in Sparta, where on the day of summer solstice a gnomon casts a small shadow at noon. This inconsistency clearly appears in the little circle that is drawn around the gnomon, indicating the shadow at noon on the day of summer solstice at Sparta but not at Syene. Moreover, if one tries to sketch a map of the Earth with Syene in the center, the hardly believable and, for the ancient Greeks probably unacceptable, consequence is that Greece is situated on the fringes of the habitable part of the Earth (οἰκουμένη) or even beyond that. In sum, it seems rather unsettling to combine Anaximander's map of the Earth and his seasonal sundial.

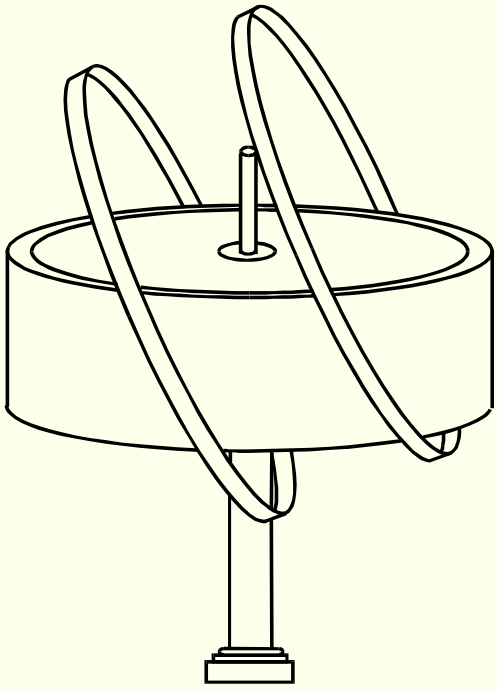


Figure 5. Revised version of Hahn's rendering of Anaximander's seasonal sundial (see text)

Finally, the concentric circles that Hahn draws on top of his reconstruction of Anaximander's seasonal sundial[9] to indicate the solstices are meaningless. When a vertical gnomon is used, the solstices are indicated by a point during the year at which the length of the shadow of the gnomon at noon. Drawing a circle through that point with the base of the gnomon as its center makes no sense. In comparison with the so-called Qumran roundel [152], an instrument which is dated to the first century BC and is not certainly a sundial, is unfounded.[9] The 'hour markers' that Hahn draws [165 (Figure 6.14)] [illegible]

[9] I intend to argue elsewhere that the Qumran roundel can be more readily interpreted as an instrument for making rather precise appointments than as sundial.

are equally wrong, as they suggest that the Sun always rises due east and sets due west, which is only true at the equinoxes.

When we try to strike the balance, we have to thank Hahn for providing some ideas that clarify images used in Anaximander's cosmology. That Anaximander must have thought of his drum-shaped Earth as somewhat concave is elucidated by the way column drums are prepared by means of ἀναθύρωσις. Hahn shows that hollow wheels as in Anaximander's conception of the celestial bodies really existed in his time and environment. However, as argued above, Hahn's exposé of the column as an image of the celestial axis, of which the Earth should be a column drum, his drawing of a map of Anaximander's cosmos according to the (9 + 1) formula, his clinging to the translation of πρηστῆρος αὐλός as 'the nozzle of a bellows', and his ideas about Anaximander's sundial including his efforts to combine it with a model of the universe and a map of the Earth, did not convince the present reviewer. All taken together, this seems to be a rather small harvest using the method of 'inverse archaeology'.

Although he warns against the danger of the use of imagination in interpreting the images of artifacts and techniques in ancient texts, Hahn regularly has a tendency to jump to conclusions. If one agrees that Anaximander sometimes used architectural features as images for cosmological ones, Hahn readily concludes that Anaximander was reconstructing the house of the cosmos and was engaged in cosmic architecture. When the doxography calls the opening in Anaximander's celestial wheels 'exhaling places', Hahn is ready to conclude that the whole cosmos is alive and that the cosmos is a living creature. It seems to me that some caution should be appropriate here. The use of an image to illustrate a thought does not imply automatically that this image is meant to be generalized. Although he seems to suggest it throughout his book, Hahn apparently avoids speaking of Anaximander's cosmos as a 'cosmic temple', as he has in the past.[10] However, several times he does speak of 'the house that *is* the cosmos' [e.g., 51, 120], of which Anaximander sought to explain the structure and stages of its construction. One may wonder what a strange building this cosmic house was, putatively constituted as it was of wheels

[10] Cf. Hahn 2001, 188 'Anaximander imagined the cosmos to be a kind of temple, the cosmic house'.

that turn at different distances around the Earth and lie aslant in relation to the Earth's surface, with its lone column (the cosmic axis) not standing right up but fallen down, adorned with thousands of alleged bellows with their nozzles, and even breathing and alive.

My basic problem with Hahn's inverse archaeological method is that it conceals rather than reveals the world-historical meaning of Anaximander's cosmological insights. One would almost believe that in Hahn's view Anaximander was so intrigued by the achievements of the architects and other craftsmen that he forgot to look at the heavens when he put forward his conception of the universe. However, when Anaximander launched his new ideas on the cosmos, he was first and for all not an architect but a cosmologist looking up to the stars and thinking about the celestial bodies, their relative positions, and their movements. The main and foremost historical context of Anaximander was the archaic conception of the universe as the cupola of the firmament arching over the flat Earth, from which conception he managed to free himself, thus becoming the founding father of cosmology.

What really counts is not so much what could possibly have inspired Anaximander, but what is the significance of his cosmological ideas. Let me, therefore, end this review by mentioning very briefly what are in my view Anaximander's three fundamental cosmological insights and their relations to the images that he used.

1. Anaximander imagined the celestial bodies as not stopping at the horizon but making full circles around the Earth. Only after he had dared to think this did he ask himself how such a circular movement could possibly exist and persist. The only objects in his environment that naturally made circular movements were wheels. So he imagined the celestial bodies as wheels.
2. The conception of the celestial bodies as making full circles around the Earth necessarily gave rise to that of a free floating Earth in the center of the cosmos. Only after he had dared to think this did he address the question of the shape of the Earth. As he thought, like all his contemporaries, that the Earth was flat, the shape of a disk was rather obvious and could easily be illustrated by an object of a similar shape that everyone knew, the column drum.
3. Anaximander conceived of the celestial bodies as being at different distances from the Earth. Only after he had dared to break with

the archaic conception of a firmament onto which the celestial bodies were glued, did he consider the question of how to express their distances, which he was not able to measure. So he found in the symbolism of the Greek counting system a set of numbers that was able to express his new idea of depth in the universe rather adequately: the numbers 9 (+1), 18 (+1), and 27 (+1), meaning 'far, farther, farthest', using the diameter of the Earth as his 'module.' These notions he could have illustrated very well in a 'plan view'.

BIBLIOGRAPHY

Barnes, J. 1982. *The Presocratic Philosophers*. 2 vols. 2nd edn. London.

Couprie, D. L. 2001. 'πρηστῆρος αὐλός Revisited'. *Apeiron* 34:193–202.

_____ 2009. 'The Tilting of the Heavens in Presocratic Cosmology'. *Apeiron* 42:259–274.

De Jong, J. J. 1994. *De wiskundige grondslagen van de Griekse en Romeinse tempelarchitectuur in theorie en praktijk tussen de 4e en 1e eeuw* (*The Mathematical Foundations of Greek and Roman Temple Architecture between the 4th and 1st Century B.C.*). PhD. dissertation, Leiden.

Diels, H. and Kranz, W. 1954. edd. *Die Fragmente der Vorsokratiker*. Zürich/Hildesheim.

Hahn, R. 2001. *Anaximander and the Architects: The Contributions of Egyptian and Greek Architectural Technologies on the Origins of Greek Philosophy*. Albany, NY.

_____ 2003. 'Proportions and Numbers in Anaximander and Early Greek Thought'. Pp. 70–163 in D. L. Couprie, R. Hahn, and G. Naddaf edd. *Anaximander in Context*. Albany, NY.

_____ 2007. 'Heidegger, Anaximander, and the Greek Temple'. *Wolkenkuckucksheim* 12.1:] *Heaven and Earth. Festschrift to Honor Karsten Harries*. http://www.tu-cottbus.de/theoriender-architektur/wolke/eng/Subjects/071/Hahn/hahn.htm.

Lee, H. D. P. 1962. *Aristotle: Meteorologica*. Cambridge, MA/London.

Workman, A. 1953. 'La terminologie sculpturale dans la philosophie présocratique'. *Actes du XIème Congrès International de Philosophie, Bruxelles 20–26 asoût 1953. Histoire de la philosophie. Méthodologie Antiquité et Moyen Âge* 12:45–50.

Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority by Steven Shapin

Baltimore: Johns Hopkins University Press, 2010. Pp. ix + 552. ISBN 978–0–8018–9421-3. Cloth $30.00

Reviewed by
Steve Fuller
University of Warwick
s.w.fuller@warwick.ac.uk

Steven Shapin's place amongst the great historians of science of the 20th century was secured with the publication of *Leviathan and the Air-Pump* [1985], co-authored with Simon Schaffer, which used the exclusion of Thomas Hobbes from membership in the Royal Society as a springboard for considering several overlapping senses in which the science/non-science boundary was decisively constructed in Restoration England. More generally, instead of casting the 'Scientific Revolution' as the period when the modern institutions of science were founded on secure epistemological principles, Shapin portrayed it as a kind of historiographic mirage, a retrospective rationalization of many relatively independent decisions taken of the sort that excluded Hobbes from the Royal Society. In effect, Shapin showed that without the menacing presence of Hobbes, who advanced an especially fierce philosophical scepticism towards the capacity of experiments to resolve metaphysical disputes, the Royal Society would not have become the institution that it is today. If previous histories had made it seem as if the Scientific Revolution would have happened sooner or later, Shapin's history reveals that it was so path-dependent that our belief that such a revolution even occurred relies on our remaining convinced that the exclusion of Hobbes had been the right move—or put more generally, that science, as the Royal Society's Charter states, excludes considerations of politics, religion, metaphysics, and so forth.

This is heady stuff, the full measure of which even now has yet to be taken. For an obvious conclusion to draw from Shapin's body of work is that not only the Scientific Revolution but also 'Science' itself—especially when understood as the singular achievement

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 97–100

canonized in Herbert Butterfield's *The Origins of Modern Science* [1949]—is a historiographic mirage. However, in the book under review, a collection of 16 occasional pieces published over the past quarter-century that visit most of the major themes of Shapin's career, any such radical conclusion is resisted—despite its intimation in the final chapter. Indeed the author forsakes the rhetorical example set by the confrontational Hobbes for the tactful example of Hobbes' great nemesis, the chemist Robert Boyle, whose own circumspect and prolix manner becomes Shapin all too well. Thus, surrounding the often interesting things that Shapin has to say about the past, the reader is treated to feats of 'boundary maintenance' concerning what it is to be a good historian of science in the Steven Shapin mould. Here a highly labored style of writing is deployed to perform scholarly virtues that go by names like 'careful', 'accurate', and 'rich', which Shapin in turn noticeably bestows on colleagues every so often [ch. 1–2]. Where Shapin worries that his positions might cause offense, he is always quick to show that they were unintended, byproducts of trying to uphold the norms that his opponents presumably share with him. After all, as the Royal Society taught, mutual recognition is the key to mutual protection. It is easy to see how Harvard could come to like such a chap.

Wading through the 200,000+ words of courtly prose that comprise the essays in this book, it is easy to lose sight of the animus driving Shapin's work, let alone how it ever could have been seen as threatening to the scientific orthodoxy. (Lest one forget, a substantial part of Paul Gross and Norman Levitt's notorious Science War salvo, *Higher Superstition* [1994], was devoted to a debunking of *Leviathan and the Air-Pump*.) The key to understanding Shapin is to imagine him as being to the sociology of scientific knowledge what Heidegger was to phenomenology—namely, someone whose overriding concern is to tie thought to the situatedness of the human condition. Indeed, very much like Heidegger, Shapin endows the sites of scientific work with enormous mystique and authenticity [ch. 5–6]. To be sure, Shapin is often simply reporting the attitudes held by, say, the gentlemen of the early Royal Society who engaged in experiments. But these attitudes also carry over to Shapin's own sense of what it means to conduct a decent scientific life. It is clear that he prefers the self-effacing, convivial yet clever Robert Boyle to the arrogant and solitary genius that was Isaac Newton, and that any useful

medical advice doled out by René Descartes owed more to the common sense of the day than to his distinctive metaphysical views [ch. 15]. Indeed, Shapin regards the claims to intellectual purity made by solitary scholars—especially philosophers—as self-deception, if not a manifestation of that supreme deception, the aspiration to universal transcendental knowledge, aka divinity [ch. 7–8]. Here the reader can begin to see how the early Shapin could have been associated with a vaguely Neo-Marxian account of science as a kind of craft guild oriented to specific social interests. While in the 1970s this view may have appeared to demystify science as a form of abstract knowledge, today it is more suited to underwriting the integrity of grounded scientific practices.

But it would be a mistake to see today's Shapin as a nostalgic defender of the guild values upheld by what Derek de Solla Price originally called 'little science' [1986]. In perhaps the most revealing essay in the book [ch. 10], Shapin argues that the best way to understand the bulk of American social science research in the 20th century which appeared to demonstrate the alienation of scientists who worked for industry is as a projection of the social scientists' own anxiety that they could be true to their vocation only in an academic culture that was now beset by the 'military-industrial complex'. For their part, natural scientists who moved between academia and industry did so with relative ease, sometimes even blending into corporate culture. Shapin's sympathies clearly lie with the amiably adaptive natural scientists. He suggests that the concern expressed by social scientists for the scientists' chameleon-like tendencies perhaps masked their own underlying resentment or envy of their subjects. In any case, Shapin argues, the clear, abstract, and influential formulation of the scientific ethos produced by Robert Merton—which makes no reference to sites of scientific work—dates from this general development. Rather than as a forthright albeit utopian ideal of the scientific enterprise, Shapin suggests that the Mertonian norms be read as the manifesto of a segment of knowledge workers—social scientists—who felt increasingly isolated in a rapidly changing American society. Interestingly, but keeping with his longstanding Mary Douglas-tinged view that politics is the symbolic enactment of cosmology [Douglas 1970], Shapin ignores the line of thought initiated

by Everett Mendelsohn [1989] that reads Merton as a liberal ideological translation of J. D. Bernal's Marxist call for a united worldwide class of scientific workers.

An interesting short book could be written that used Merton's four norms of the scientific ethos as a meta-scientific Rorschach test, since virtually every sort of opinion has been expressed about them by virtually every major theorist of science of the past 50 years. That Shapin sees the norms as a symptom of social science's alienation from the social world is not surprising. The sociology of scientific knowledge—and science studies more generally—has an undeserved reputation for being 'anti-science'. Indeed, Shapin himself shows that many of the field's characteristic theses have precedents in scientists' own spontaneous meta-scientific pronouncements [ch. 3]. However, from its start, the sociology of scientific knowledge has always targeted normative philosophy of science, especially of the sort that tends to persuade scientists that they are society's intellectual superiors. From this standpoint, social scientists look like naïve literalists, perhaps even fundamentalists, *vis-à-vis* doctrines that scientists themselves use opportunistically if not abandon in practice. Of course, it remains an open question, which Shapin refuses to address head on, whether science as the signature institution of the modern world can survive without such philosophical resolve. But then that may not be his concern.

BIBLIOGRAPHY

Butterfield, H. 1949. *The Origins of Modern Science.* London.

Douglas, M. 1970. *Natural Symbols: Explorations in Cosmology.* London.

Gross, P. and Levitt, N. 1994. *Higher Superstition.* Baltimore.

Mendelsohn, E. 1989. 'Robert K. Merton: The Celebration and Defence of Science'. *Science in Context* 3:269–299.

Price, D. de Solla. 1986. *Little Science, Big Science, and Beyond.* New York. Originally published, 1963.

Shapin, S. and Schaffer, S. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life.* Princeton.

Klaudios Ptolemaios. Handbuch der Geographie. *Griechisch-Deutsch, Einleitung, Text und Übersetzung, Index* edited by Alfred Stückelberger and Gerd Graßhoff with Florian Mittenhuber, Renate Burri, Klaus Geus, Gerhard Winkler, Susanne Ziegler, Judith Hindermann, Lutz Koch, and Kurt Keller

Basel: Schwabe Verlag, 2006. 2 vols. Pp. 1018, 24 illust., 29 maps, 1 CD-ROM. ISBN 978–3–7965–2148–5. Cloth €170.00

Reviewed by
Johannes Engels
University of Cologne
johannes.engels@uni-koeln.de

Studies of ancient Greek and Roman geography and cartography are currently flourishing. Over the past decade, new scholarly editions of ancient key authors have been published (with translations in modern languages) which offer a reliable basis for any future research in ancient geography as a branch of science and especially in these authors. Of these publications, one may mention the editions of the main geographical works by Strabo and by Ptolemy as the two most important. The edition of the 17 books of Strabo's *Geographica* (composed in the reign of the emperor Augustus with additions in the early Tiberian years) with a German translation and commentary by Stefan Radt [2002–] has justly received much praise. Strabo's geography has been labelled an encyclopedic *summa* of earlier ancient Greek cultural or human geography. Of equal importance for our knowledge of the other concurrent branch of ancient geography, which was fundamentally based on astronomy, mathematics, and physical geography, are the eight books of Claudius Ptolemy's *Geographia* (or *Γεωγραφικὴ ὑφήγησις*) written in the second half of the second century AD.

This review will focus on the first two volumes of this edition [2006], an edition which comes with an introduction, the Greek text, a German translation, maps, illustrations, and indices. Here, readers will find only very brief notes helping to clarify the meaning of the text or justifying the translation, since these two volumes should always be consulted together with a third or companion volume published in 2009. This third volume collects a considerable number of

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 101–109

thorough studies written by the team of contributors to the first two volumes and by others with expertise in the Greek manuscripts of Ptolemy's geographical work, the canon of πόλεις ἐπίσημοι (important cities), different ancient methods of measuring the whole earth or certain distances, and in establishing and calculating the precise location of a place. Other studies deal with Ptolemy's view of the world, world and regional maps, the reception of his work, his prominent place in the history of geography, and his Greek and style of writing. Volume 3 also has a useful bibliography.

Readers of *Aestimatio* will surely appreciate a brief outline of the contents of Ptolemy's geographical work. In book 1, he starts with a famous definition of γεωγραφία as a science, its scope, and methods. Then, scholarly opinions of his most recent predecessor and one of his main sources, Marinus of Tyre, are critically discussed; and technical instruction on drawing a world map and on two different spherical projections (Ptolemy's chief contribution to scientific mapmaking) follows. The main part of *Geographia*, books 2–7, consists of long lists of places or, more generally, toponyms on the three continents Europe, Africa (Libya), and Asia with longitude and latitude (and sometimes brief descriptions of topographical features). One finds more than 8000 geographical data altogether which make Ptolemy's geographical treatise by far the most detailed and complete ancient inventory of toponyms and exact localizations. Parts of book 7 and the final book, book 8, provide instruction for breaking down the world map into 26 individual maps of certain regions mainly of the οἰκουμένη (the civilized world).

This key source of scientific geography from the second to the 16th century is presented in the two volumes under review in a splendid edition.[1] The Greek text is based on thorough studies of the extant original manuscripts; and the modern German translation makes the work easily accessible not only to a small circle of classical scholars but also to a broader readership of historians of science, researchers in the history of geography and cartography, and several other interested groups.

An accompanying CD-ROM has been added to the two volumes which will be very welcome to friends of electronic media and e-books.

[1] On the history of the reception of Ptolemy's treatise, see the companion volume [2009a] and Gautier Dalché 2009.

A searchable database (*PtolDB*) on this CD-ROM includes the complete catalogue of toponyms and the maps. It runs without problems with Mac OSX (version 10.3 or higher) as well as with Windows (2000, XP service pack 2 or any higher version). It will surely facilitate modern ways of research on the text and the maps.

Carolus F. A. Nobbe's edition [1843–1845] astonishingly remained the only complete edition of the Greek text of the *Geographia* for more than 150 years. Edward Luther Stevenson [1932] provided a widely read complete English translation without a Greek text. Several subsequent attempts to finish a new scholarly edition unfortunately failed for different reasons: the major edition of Karl Müller [1883–1901] contains only books 1–5. (Müller's edition is still the basis of the Greek text of Ptolemy's geography in the widely used Thesaurus Linguae Graecae (TLG) database.) Various 20th-century studies by experts such as Otto Cuntz, Joseph Fischer, Aubrey Diller, and Paul Schnabel have focused on the complicated manuscript tradition or single manuscripts. Other scholars have concentrated their efforts on single books[2] or they have dealt in detail with Ptolemy's theoretical introduction and the sections about the differences between γεωγραφία and χωρογραφία.[3]

But, finally, a completely revised critical edition is now available. Coordinated by a team of researchers at the Universität Bern, the individual contributors to these two volumes have worked independently on their books or sections of the text. Alfred Stückelberger himself is responsible for the introduction, *Geog.* books 1 and 7.5–7. Florian Mittenhuber deals with book 2, the design of the reconstructed maps (see below) and the final versions of the catalogue of toponyms and the indices. Renate Burri has worked on book 3; Klaus Geus, on book 4. Gerhard Winkler deals with book 5; Susanne Ziegler, with book 6; Judith Hindermann, with book 7.1–4, and Lutz Koch, with book 8. The indices were carefully prepared by Kurt Keller. The new Greek text differs from Nobbe's edition in more than 1000 passages. In books 1 and 7, some small sections of the text on geography as a science and on projections have been rearranged.

At present, we know of 53 manuscripts of the Greek text of Ptolemy's geographical work, dating from the 13th to 16th centuries.

[2] E.g., Helmut Humbach and Susanne Ziegler on book 6 [1998–2002].
[3] On books 1 and 7.5–7, see especially Berggren and Jones 2000.

Some of them give the complete text, others only parts of it. 17 manuscripts include maps. Stückelberger, Graßhoff, and their colleagues differ considerably with Nobbe about the manuscript tradition and the constitution of a *stemma*. Most importantly, the new edition fully acknowledges the fundamental importance of the Codex Constantinopolitanus Seragliensis GI 57, a manuscript (with the text of the geographical work and maps) found in 1927 in the library of the Topkapi Museum in Istanbul.[4]

Constantinopolitanus Seragliensis GI 57 (K), Vaticanus Urbinas Graecus 82 (U), Vaticanus Graecus 177 (V), Venetus Marcianus Graecus Z. 516 (R) and Vaticanus Graecus 191 (X) are the most important manuscripts of the Greek text of the *Geographia*, the *codices primarii*. The famous Byzantine scholar Maximus Planudes himself once owned Vaticanus Graecus 177 and supported the production of other manuscripts and general research on Ptolemy in the 13th century. Most of the *codices primarii* were copied from a considerably earlier manuscript (perhaps of the late 5th century, in majuscules and with maps) which had only recently been rediscovered in Planudes' time. Vaticanus Graecus 191 (X), however, contains only the text of the geographical work (along with other scientific texts, which of course is of major interest to the history of the reception of Ptolemy). This manuscript is our only extant testimony of a different line of the manuscript tradition. Following the current consensus among scholars, Stückelberger and his team generally follow U (or K) in the constitution of their text; but they regularly add variants of names or locations given in X, especially in the text of books 1–5. This correct editorial decision follows from the simple fact that we cannot honestly establish clear criteria for choosing between the different place names or coordinates of longitude and latitude which we find in the five manuscripts of the *codices primarii*. Another typical problem of editing Ptolemy's text stems from difficulties of understanding precisely his theoretical views of projections and their ancient mathematical basis in books 1 and 7. The illustrations and diagrams in this edition are really helpful; and to this reviewer some of them

[4] On the Greek manuscripts of Ptolemy's *Geographia*, see Burri 2009; Stückelberger and Mittenhuber 2009b; and Fuchs and Oltrogge 2009. Stückelberger, Mittenhuber, and Burri 2003 is still useful. Several manuscripts have been digitalized in the course of this project and can be consulted at http:www.philoscience.unibe.ch/ptolemaios.

seem to be almost indispensable to an understanding of Ptolemy's complicated theories.

Some of the general problems in preparing a critical edition and a translation of ancient scientific or technical treatises belonging to different τέχναι (*disciplinae*) are well known. These problems concern *inter alia* important works on mathematics, astronomy, architecture, medicine, and so on. Since ancient geography poses problems in addition to those raised by texts in other scientific disciplines, still many important ancient works are available only in outdated and unreliable editions. Generally, ancient geographical texts often constitute problems with an exact understanding of their partly remote subjects, an unusual technical vocabulary, place names which cannot be located, differing distances between two places in ancient sources, and—at least to a modern reader—strange remarks or plain mistakes in indications of directions of a coast line, a mountain range, and the like. These alleged 'mistakes', however, often stem from a typical traveler's or 'hodological' perspective, as Pietro Janni observes.

Ptolemy's major geographical treatise clearly raises such general problems as well as some additional special ones too; indeed, this key text of ancient geography and mapmaking presents huge challenges to an editor. Ancient geographers were very well aware of the fact that maps of the whole world or single regions needed to be copied manually again and again—almost in every generation—in order to preserve them for future generations. Grave mistakes very often and easily occurred in this difficult and expensive process of copying, let alone the manipulation of maps for political or ideological reasons. Since Ptolemy clearly knew of these problems, he actively tried to oppose processes of this sort by which maps deteriorate. Thus, the bulk of his *Geographia* consists of lists of about 8000 toponyms with exact longitude and latitude—all indicated in a new unified system of scientific coordinates, which in principle provided a reliable source material for making maps—which he deliberately separated from the maps themselves. However, many of these names sounded strange already to ancient Greeks and Romans, some were without any parallel in Greek and Roman literature—there are unique occurrences (ἅπαξ λεγόμενα) among the toponyms—and the coordinates were given in a system of letters and other small signs as numbers. It goes without saying that such toponyms and coordinates were easily corrupted in

the process of copying by ancient and Byzantine scribes. Unfortunately, modern scholars do not know any secure method of detecting such typical mistakes in the preserved manuscripts of the *Geographia*, even less of correcting these corrupted passages. Nevertheless, by systematically linking the coordinates and the visual representation of the maps Ptolemy surely reached a higher level of ancient scientific geography and mapmaking. Given the many and intricate problems of editing Ptolemy's geography, Stückelberger and his colleagues have done an excellent job; and given the present state of our knowledge, they have tackled these problems as successfully as possible.

The reconstructed world maps and 26 regional maps are an important part of the edition under review. Far from merely serving as illustrations and visualizations of the text, the maps are key sources for the ancient and pre-modern view of the world and its most important regions until the early 16th century. Moreover, the map tradition of Ptolemy's geography preserves some pieces of information not found in the textual tradition of this work. However, these maps confront us with problems of early mapmaking, too. For instance, the world maps in Codex Seragliensis GI 57 and in Vaticanus Urbinas Graecus 82 show different cartographic projections (as explained in *Geog.* 1 and 7), namely, the modified and the simple spherical projection. In addition, to modern observers at least, the 26 regional maps (10 for Europe, 4 for Africa, and 12 for Asia) sometimes show a strange and surprising perception of space. Finally, the precise relationship between the text of Ptolemy's *Geographia* and the preserved earliest maps constitutes a very controversial field of research. For some scholars hold that Ptolemy's original work did not include maps but only instructions for making such maps, and that the world map and the regional maps were only added in later ancient or Byzantine editions, for instance, by an early cartographer named Agathodaimon of Alexandria. More studies are also needed, in my view, of the temporal layers of the 26 regional maps.[5] These problems exceed the usual tasks of textual criticism and editorial practice. They are connected with the current controversy about the recently published Artemidorus Papyrus (P. Artemid.) which includes a late Hellenistic

[5] On the main problems connected with ancient mapmaking and the tradition of Ptolemy's maps, see Mittenhuber, 2009, 34–108.

map. This map has been seen as an early and incomplete preliminary stage of advanced later mapmaking in Ptolemy's time.[6]

Stückelberger's introduction to volume 1 [9–47] concisely informs us about Ptolemy's life and works. In antiquity, Ptolemy's fame as a scholar was primarily based on his research and publications in mathematics and astronomy (including astrology as usual in the Roman empire). The *Syntaxis Mathematica* (*Mathematical Systematic Treatise*, better known as the 'Almagest'), *Apotelesmata* (*Astrological Influences*, *Tetrabiblos*), and the Star Catalogue were composed before the *Geographia*, which today is usually dated to *ca* AD 150–170. Stückelberger and his team chose a 'pragmatic' German translation, 'Handbuch der Geographie', for the original Greek title *Γεωγραφικὴ ὑφήγησις* (*Geographia*) [11*et pass.*]. However, readers should be aware that 'Handbuch' ('manual') may not render precisely the original intentions which Ptolemy wanted to express with his title, namely, to provide an introduction to describing the Earth and to show the Earth on a map, rather than to give a general overview of the whole discipline of geography that one would expect in a manual.

Notwithstanding their different concepts of geography (their concerns with the issue of cultural geography *versus* mathematical geography, and so on), Strabo and Ptolemy shared the common aim of systematically correcting positions of earlier geographers and of securing substantial progress in scholarly geography by criticizing opinions held by prominent precursors (the principle of ἐπανόρθωσις). Thus, the *Geographia* tries to concur with the geographical work of Ptolemy's immediate precursor, Marinus of Tyre (*ca* AD 80–130), but still corrects his views and the opinions held by other earlier geographers. Since Marinus' treatise has been only fragmentarily preserved and that, moreover, in Ptolemy's geographical work mainly, the question of how far Ptolemy relied on scientific results already found by Marinus must remain unresolved.

Ptolemy, perhaps the greatest ancient geographer, was left with a methodological dilemma regarding his sources for the lists of toponyms and their coordinates and distances. Strictly speaking, and given the limitations of the instruments available to him and of the known methods of measurement, he could only use 'reliable' scientific

[6] See the edition by Claudio Gallazi, Bärbel Kramer, and Salvatore Settis [2008].

data for computing and establishing his coordinates. In this case, he would have had at his disposal only a small fraction of the data which he needed to show the whole Earth on a map. Hence, and perhaps unwillingly, he made use of many earlier notes on distances and locations in general historical and geographical works or travel reports especially on places and regions on the edges of the οἰκουμένη [see 1.16–20]. Ptolemy could also find no secure way to deal with the differing units of length in earlier authors, for instance, the στάδιον ('stade'), since in antiquity there never was a unified or standard unit of length. Nonetheless, and despite all shortcomings and mistakes, I think that today we still have many reasons to admire Ptolemy's scholarly achievement.

An apparatus of parallel sources is missing in this edition. But preparing such an apparatus to the *Geographia* would have taken up a great deal of time and would have made the edition even more expensive than it is now. However, there are very detailed and reliable indices. One finds a huge index of ancient toponyms [924–1015] (with modern names of these places when it is possible to provide them), and two other short indices of persons [1015] and subjects [1016–1018]. These indices and the well thought-out layout make this edition according to Ptolemy's wishes really εὔχρηστον (user-friendly). Stückelberger, Graßhoff, and their colleagues have made the *Geographia* accessible to 21st century scholarship in a reliable and splendid edition.

BIBLIOGRAPHY

Berggren, J. L. and Jones, A. 2000. *Ptolemy's Geography: An Annotated Translation of the Theoretical Chapters*. Princeton NJ.

Burri, R. 2009. 'Übersicht über die griechischen Handschriften der ptolemäischen Geographie'. Pp. 10–20 in Stückelberger and Mittenhuber 2009a.

Fuchs, R. and Oltrogge, D. 2009. 'Der Codex Seragliensis GI 57, eine kodikologische Beschreibung'. Pp. 26–33 in Stückelberger and Mittenhuber 2009a.

Gallazi, C.; Kramer, B.; and Settis, S. 2008. edd. *Il papiro di Artemidoro (P.Artemid.)*. 2 vols and 1 DVD. Torino.

Gautier Dalché, P. 2009. *La géographie de Ptolémée en Occident (IVe—XVe siècle)*. Turnhout.

Humbach, H. and Ziegler, S. 1998–2002. *Ptolemy, Geography Book 6. Middle East, Central and North Asia, China*. Parts 1–2. Wiesbaden.

Mittenhuber, F. 2009. 'Karten und Kartenüberlieferung'. Pp. 34–108 in Stückelberger and Mittenhuber 2009a.

Müller, K. 1883–1901. *Claudii Ptolemaei geographia e codicibus recognovit, prolegomenis, annotatione, indicibus, tabulis instruxit C.M.* Paris.

Nobbe, C. F. A. 1843–1845. *Claudii Ptolemaei geographia*. Leipzig. Repr. (with an introduction by Aubrey Diller) Zürich/New York, 1990.

Radt, S. 2002–2009. *Strabons Geographika mit Übersetzung und Kommentar herausgegeben*. Göttingen.

Stevenson, E. L. 1932. *Geography of Claudius Ptolemy*. New York. Repr. 1991.

Stückelberger, A.; Mittenhuber, F.; and Burri, R. 2003. 'Die „neue" Ptolemaios-Handschrift von Istanbul (Cod. Seragliensis GI 57)'. *Museum Helveticum* 60:193–221.

Stückelberger, A. and Mittenhuber, F. 2009a. edd. *Ptolemaios Handbuch der Geographie. Ergänzungsband (3) mit einer Edition des Kanons bedeutender Städte*. Basel.

Stückelberger, A. and Mittenhuber, F. 2009b. 'Stemma-Entwurf'. Pp. 21–25 in Stückelberger and Mittenhuber 2009a.

Une conquête des savoirs. Les traductions dans l'Europe latine (fin du XIe siècle – milieu du XIIIe siècle). Actes du colloque organizé à la Fondation Singer-Polignac le jeudi 27 novembre 2008 edited by Max Lejbowicz

Collection Rencontres Médiévales Européennes 9. Turnhout: Brepols, 2009. Pp. 175. ISBN 978–2–503–53121–2 Paper €30.00

Reviewed by
Faith Wallis
McGill University, Montreal
faith.wallis@mcgill.ca

This volume contains eight papers presented at a colloquium held in the autumn of 2008. The French intellectual world was still polarized by the intense polemic surrounding Sylvain Gouguenheim's *Aristote au mont Saint-Michel. Les racines grecques de l'Europe chrétienne* [2008].[1] Gouguenheim argued that the medieval Latin West did not depend on translations from the Arabic to recover knowledge of ancient Greek science and philosophy. In his view, direct transmission of Greek texts was continuous throughout the early medieval period, an exemplary instance of this direct transmission being the work of James of Venice, whose Greek-Latin rendition of Aristotle's *Posterior Analytics*, *Physics*, *Metaphysics*, and *De anima* were circulating in northern France by the middle of the 12th century, well before the Arabic-Latin translations emerged from Toledo. This, of course, is hardly new information; but Gouguenheim's thesis pressed very much deeper and touched some raw nerves. He argued that few Arabic writers of the classical period were genuinely interested in Greek learning and that, of these writers, the most important were Christians. Greek culture, he concluded, had little impact on Arab-Islamic civilization. The Arabic language, by its very structure, cannot, as he says, deal with syllogistic argument; and the Arabic concept of science is very different from the Greek one. The overall message conveyed by *Aristote au mont Saint-Michel* is that the heritage of Greek learning can never 'belong' to the Arab-Islamic world; it has always, and rightfully, been the possession of the west.

[1] [Ed] See the review by Gad Freudenthal [2009] in *Aestimatio* 6:191–193.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 110–115

Gouguenheim's narrative of contested proprietorship finds an echo in the militant overtones of the title of the work under review here. Apparently, this is deliberate. In an introductory note, the organizer of the series Rencontres Médiévales Européennes, Monique Cazeaux, implicitly endorses Gouguenheim's position by stating her intention to take a stand against 'political correctness'. This symposium was unabashedly dedicated to treating 'les racines chrétiennes de l'Europe' and its basis in 'la transmission des savoirs et des philosophies grecs'. To return to the implications of the title: 12th-century western Europe 'conquers' the knowledge which is its rightful possession through translation, and it is not beholden to the cultures from which it translates. *Aristote au mont Saint-Michel* seems to lurk as well behind a number of the contributions. Monique Bourin's 'Le XIIe siècle féodale et florissant de l'Europe latine', for example, is a sweeping and impressionistic chronicle of Europe's 'take-off' that deploys the word 'conquest' frequently, in both the literal and the metaphorical sense; yet the author declines to commit herself on whether there is a link between this development and the burgeoning of translations.

The Gouguenheim polemic certainly haunts the discussions that followed the colloquium papers and are transcribed in this volume. But, fortunately, not all of the contributions are fixated on the controversy. Because readers of *Aestimatio* will be primarily interested in science, philosophy, and the cultural institutions and contexts that support these enterprises, I shall limit my comments to the essays dealing with these themes.

Of exceptional interest is Alexander Fidora's paper on 'Les différentes approches des traducteurs. De la perception des texts à la reception des traductions'. Fidora analyses the different approaches that characterized Greek-into-Latin, Arabic-into-Latin, and Latin-into-Hebrew translations of philosophical works in the 12th century. Two theories of translation prevailed in the Latin world up to the 12th century: Cicero set out the idea that translation should aim to be *aemulatio*—that is, that it should aspire to surpass the original; Jerome embraced this ideal, and the concomitant policy of sense-for-sense translation, but made an exception for Scripture, where even the order of the words had a meaning. The Bible's prestige lent exceptional authority to this word-for-word method, influencing the translations

of Boethius, Eriugena, and Burgundio of Pisa. As Lorenzo Minio-Paluello pointed out, the first Greek-to-Latin translations of Aristotle were highly literal and all but unreadable. Hence, while Aristotle could indeed be found at Mont-Saint-Michel, he exerted little influence. Twelfth-century Arabic-Latin translators working in the Iberian peninsula, on the other hand, returned to sense-for-sense translation. Hermann of Carinthia adopted this method, allegedly to counteract the Arabic 'vice' of prolixity, but in fact as a way of coping with a lack of normativity for Arabic as a source language. Translators had to focus on the meaning of the text, not the meaning of the words. For Hermann, the *fides interpres* should concentrate on transmitting the philosophical problems and giving them the broadest possible context. Another reason for favoring paraphrase over literal translation is that translations from the Arabic were pragmatic tools for grasping the meaning of the ancient writer; they were a stopgap or intermediate stage that would, ideally, eventually lead to a better translation from the original Greek. Hence, Hermann's sense-for-sense Arabic-Latin translation of Aristotle's *Ethics* was not infrequently collated with Burgundio's word-for-word Greek-Latin translation. The earliest translation from Latin into Hebrew is a version of Gundissalinus' *Tractatus de anima* made in Catalonia or Aragon in the 12th century. The translator admits that he tried to translate Aristotle's *De anima* but gave up and settled for Gundissalinus' treatise instead. Here again, it is the content and not the text which is of interest. There was no investment in replicating the wording of the original because Jewish translators saw Latin culture as at best merely a useful supplement to Jewish knowledge. In the end, Fidora contests *en filigrane* the thesis of Gouguenheim on the grounds that the early 12th-century Greek-to-Latin translations of Aristotle should not be compared to the Arabic-to-Latin translations, and more particularly, to the Arabic-to-Latin commentaries. It is only when the Arabic materials become available that Aristotle can be read in Latin with comprehension and taught. Translating Aristotle sense-for-sense played a major role in this process of intellectual assimilation.

Jacques Verger's contribution on 'Le rôle des traductions dans la naissance de l'université médiévale' is a useful reminder that there is no clear convergence between the translation movement and the development of schools and universities. University statutes and regulations say nothing about translations, apart from sporadic acts of

censorship. Not having the resources of even the religious orders, universities never commissioned translations. Moreover, they seem to have regarded the available stock of texts as sufficient; and given that they were interested in the *sententia* and not issues of linguistic accuracy (as Fidora observed), this is understandable. Some Biblical exegetes wanted to consult the *hebraica* or *graeca veritas*; but ironically, both of these languages lay under a cloud of doctrinal suspicion that effectively blocked such initiatives.

The issue of the quality of medieval translations dominates the contributions by Jean Jolivet and Jean Celeyrette. Jolivet attributes 'Le tournant avicennien' to a brilliant act of translation on the part of either Gerard of Cremona or Gundissalinus. Rendering *huwiyya* as *essentia* took both philosophical imagination and intimate knowledge of Arabic. This Avicennan concept of *essentia*, as conveyed in the *Liber de philosophia prima*, was novel and formative for scholastic philosophy and theology. But even less sure-footed translations could alter the shape of knowledge. In a densely argued essay entitled 'Ibn al-Haytham suiveur de Ptolémée? Une thèse controversée en histoire de l'optique', Jean Celeyrette addresses the controversy over Ibn al-Haytham (Alhazen)'s position on the nature of the act of vision. Did he set out to demolish Ptolemy's extramission theory or to reconcile it with the physics of Aristotle as interpreted by Alexander of Aphrodisias, which supported intromission? In terms of the Latin West, the situation is complicated by the chaotic transmission of both Ptolemy's *Optics* and Ibn al-Haytham's treatise, as neither work was available in its complete form. Moreover, the Latin version of Ibn al-Haytham omitted the first three chapters, including the crucial first chapter where the author asserts that the study of optics requires the reconciliation of both mathematical and physical approaches. In consequence, most western readers like Bacon approached Ibn al-Haytham's text as a mathematical analysis of vision, with some add-on physical theory for the sake of saving the appearances. It was *perspectiva*, not *physica*. Bacon felt free to yoke his own reading of Ibn al-Haytham to Grosseteste's Neoplatonic physics of light, and subsequent western engagement with Ibn al-Haytham revolved around the debate over Bacon's model of 'multiplication of species'. Celeyrette's essay is a model of how precise attention to the discontinuities of transmission engendered by accidents of translation

can have very significant effects on the evolution of whole domains of scientific inquiry.

On the other hand, Max Lejbowicz's paper on 'L'acculturation latine selon Platon de Tivoli' elicited some sharp comments during the post-presentation discussion, notably because it bypassed the issue of the quality of Plato's *De scientia stellarum* as a translation of al-Battānī's *Sabaean Tables*. *De scientia stellarum* remained a popular manual until the 17th century, but was it a good translation? No attempt has been made to compare it to its Arabic original, which is available in a sound critical edition by C. A. Nallino [1899–1907]. Nallino appended his own very lucid Latin translation, so that even a non-Arabist should be able to judge how well Plato had rendered the original. Lejbowicz focuses instead on Plato's alleged collaboration with the Jewish translator and savant Abraham bar Hiyya (Savasorda). He is struck by the fact that unlike many of his predecessors, Plato does not trace the origins of astronomy back to the patriarch Abraham, even though al-Battānī was from Haran, where Abraham paused on journey from Ur to Canaan. This silence, in Lejbowicz's view, reflects Plato's appreciation that the solid achievements of Abraham bar Hiyya cast a shadow over those of the patriarch Abraham. This argument seems somewhat contrived. Furthermore, as Tony Lévy observed in the post-presentation discussion, Plato's partnership with Savasorda, once proposed by José María Millás Vallicrosa, has never been documented. Though Lejbowicz concentrates on decoding Plato's remarks on the deficiencies of the Latins in astronomy in comparison with the Greeks, Egyptians, and Arabs, translation itself only surfaces at the end of the essay. Plato borrowed Arabic vocabulary to supplement the impoverished scientific lexicon of Latin, a move which Lejbowicz explicitly terms 'la premièr étape d'une conquête des savoirs'. This is the only appearance of this tendentious title phrase in the body of this collection. If Lévy's doubts about the quality of Plato's translation are valid, it is a rather ironic one.

Tony Lévy himself closed the proceedings with an overview of 'Livres et cultures scientifiques dans le monde juif en Provence médiévale'. Lévy offers some important reflection on why translation movements can expire. Arabic-Hebrew translation in Provence came to an end not only because the canon was complete, but because there was no internal social or institutional infrastructure to sustain and

develop the enterprise. In consequence, scholars like Qalonymos ben Qalonymos were obliged to return to Barcelona, and then to proceed to Italy in search of further opportunities.

The essays in *Une conquête des savoirs* form a less coherent ensemble than the articles published in some recent collections, notably *Science Translated: Latin and Vernacular Translations of Scientific Treatises in Medieval Europe* edited by Michèle Goyens, Pieter de Leemans, and An Smets [2008].[2] The term 'savoirs' is very broad, and it may have been over-ambitious for a one-day symposium. Nonetheless, this volume contains some valuable contributions, notably the essays by Fidora and Celeyrette.

BIBLIOGRAPHY

Gouguenheim, S. 2008. *Aristote au mont Saint-Michel. Les racines grecques de l'Europe chrétienne.* Paris.

Goyens, M.; de Leemans, P.; and Smet, A. 2008. edd. *Science Translated: Latin and Vernacular Translations of Scientific Treatises in Medieval Europe.* Leuven.

Nallino, C. A. 1899–1907. ed. *Al-Battānī sive Albatenii opus astronomicum ad fidem codicis escurialensis Arabice editum, Latin versum, adnotationibus instructum.* Pubblicazioni del Reale osservatorio di Brera in Milano 40.1–3. Milan.

[2] [Ed] See the review by Steven J. Livesey [2010] in *Aestimatio* 7:70–78.

Pharmacy and Drug Lore in Antiquity: Greece, Rome, Byzantium by John Scarborough

Variorum Collected Studies Series CS904. Burlington, VT/Farnham, UK: Ashgate, 2010. Pp. xxviii+354. ISBN 978–0–7546–5954–9. Cloth $144.95

Reviewed by
Laurence M. V. Totelin
Cardiff University
TotelinLM@cardiff.ac.uk

There are very few scholars who can do what John Scarborough does, namely, study ancient pharmacology in a historically sensitive manner whilst also understanding the principles of modern pharmacognosy. In fact, the only other name that springs to mind is that of John M. Riddle author of, among his other books, *Dioscorides on Pharmacy* [1985] and *Medicine and Contraception and Abortion from the Ancient World to the Renaissance* [1992]. Scarborough's dual interest in history and in pharmacology is the product of an unusual academic training and career path, which is outlined in the preface of the present volume.

There has recently been a surge in scholarly interest in ancient pharmacology, with, first, the re-edition of key texts such as the poems of Nicander [Jacques 2002, Jacques 2007], a Hellenistic poet who wrote on poisons and their antidotes, or the Hippocratic text *On the Nature of Woman* [Bourbon 2008], which includes much pharmacological material; second, new translations of key texts by scholars who are sensitive to the issue of identification of *materia medica*;[1] and third, historical studies of pharmacological material.[2] The present volume, a collection of 14 of Scarborough's articles originally published between 1977 and 2002, comes, therefore, at a perfect time and will certainly spark further interest in ancient pharmacology.

I had previously read most of the papers collected here but I found it particularly fruitful to examine them together, as certain

[1] See, e.g., Lily Beck's translation of Dioscorides [2005].
[2] See, e.g., the collection of essays on Galen's pharmacology edited by Armelle Debru [1997] or my own *Hippocratic Recipes* [Totelin 2009].

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 116–120

points became particularly clear. First, one has to stress the author's breath of knowledge. As already pointed out, Scarborough is fluent in the jargons of both classicists and pharmacologists. He also covers an immense historical period: from approximately the eighth century BC (when the Homeric poems were written down) to the seventh century AD (the time of Paul of Aegina). Scarborough discusses the writings of all major ancient pharmacologists: the Hippocratic writers [III], Theophrastus [IV], Nicander [V–VI], Pliny [IX], Soranus [X], Criton [XI], whose writings are excerpted by Galen, Galen [XII], and various Byzantine writers [XIII].

Second, Scarborough pays attention to all aspects of ancient pharmacology. The Greek word φάρμακον, wherefrom our word 'pharmacology' is derived, covers a range of modern concepts, from 'healing drug' to 'magic spell' and 'poison' [see Artelt 1968]. Scarborough does not neglect any of these concepts. Thus, whilst 'healing drugs' are the subject of most articles in the collection, magic is examined in 'The Pharmacology of Sacred Plants, Herbs and Roots' [I] and toxicology is studied in 'Nicander's Toxicology' [V–VI].

Third, Scarborough is wary of the use of modernisms in the study of ancient pharmacology. For instance, he argues that one should not use the word 'psychosomatic' in relation to the therapeutic effects which the ancients believed some plants to have [I.149]; and I did not see once the word 'placebo' used in this collection (and it certainly is not listed in the index). In view of this rejection of presentism, Scarborough's constant listing of the properties of ancient *materia medica* in modern terms—'analgesic', 'febrifuge', and so on—may seem contradictory. I believe that it is not: there are two aspects to Scarborough's work. On the one hand, he wants to explain how ancient pharmacological systems functioned, and for that he is keen to use what anthropologists would call 'actors' categories'. Much in these systems may appear completely alien to the modern reader but they should nevertheless be studied in their own rights. On the other hand, Scarborough wants to show that much of the knowledge that the ancients had acquired about *materia medica* is sound by modern standards—many of the plants and remedies which they used are as efficacious today as they were 2000 years ago. For instance, he writes:

> Relying on powers of observation and willingness to experiment, the peoples of classical antiquity devised many remedies for burns. Certainly not all treatments were efficacious

> but we must nonetheless look with admiration upon the accomplishments of those who worked almost twenty centuries ago. [II.608]

I find myself in agreement with this methodology whereby there would be reading of the ancient pharmacological writings on two levels, two different modes of 'translation'.[3] The first level of reading would concern itself with understanding the ways in which the ancients explained the efficacy of their remedies; the second would use modern pharmacological and ethno-pharmacological methods to assess the drugs used by the ancients. This dual methodology goes a long way towards explaining why most ancient pharmacologists used the same drugs but devised diverging theories to explain their efficacy.[4] Scarborough notes in several places that the discovery of a remedy's efficacy generally comes before any theoretical attempt at explaining it. For instance, he writes:

> [I]t is certainly clear that Hippocratic medicine had incorporated a vast number of the venerated uses of herbs, minerals, and animal products that were known in Greek history long before the rise of 'rational' medicine. It is, one may say in conclusion, to the great credit of some of the Hippocratic writers that they recognized the value of many of these prescriptions, expunged of any superstitious content, a value that modern pharmaceutics has in some respects only begun to rediscover. [III.324–325]

In other words, many of the drugs listed by Hippocratic physicians had been used for centuries before being written down; and the 'Hippocratic' pharmacological theories, cast in the language of elements, qualities, and humors [ix], were neither universally accepted nor as coherent as one may think.

The fourth, and final, point that becomes clear when reading the essays collected in this volume is that Scarborough has relatively little respect for Galen's pharmacological enterprise. Galen, the most prolific of all ancient medical writers, composed several long treatises

[3] For other discussions of the efficacy of ancient drugs, see King 1998, 132–156; Demand 1999. For an anthropological approach to the topic, see Etkin 1988.

[4] See for instance III.314: '[T]here was no basic uniformity among the Hippocratic writers concerning assumed theories of pharmacology.'

on simple and compound remedies. Although historians are aware that theses treatises are largely derivative in nature (being composed of extracts from earlier pharmacologists), they still consider them to be benchmarks in the history of pharmacology and to have influenced deeply the writings of Byzantine medical writers [see Debru 1997]. Scarborough, on the other hand, argues that Galen's pharmacological writings are 'confusing' and that they contain

> a muddling of drug lore, only gradually corrected by later Byzantine pharmacologists, who did not generally take Galen as the ultimate blueprint for pharmacy. [XII.271]

Thus, in Scarborough's eyes, the pharmacological work of the Byzantine medical writers, Oribasius, Aetius, Alexander of Tralles, and Paul of Aegina, deserves to be studied in its own right and not simply as a compilatory enterprise.

As usual in such a collection of essays, not all pieces are of the same quality and there are repetitions. There are also many points of detail on which I disagree with Scarborough. To give only one example, I am far from certain that the Greek magical papyri from Egypt allow us 'a rare glimpse into the actual "medicine of the masses"' [XIII.230]. In fact, I am not sure what these 'masses' are. However, on the whole, I would say that Variorum Collected Studies Series has done historians of medicine a great service by publishing this collection of articles, some of which are rather difficult to find in most humanities libraries. The bibliographic updates offered in the 'addenda and corrigenda' as well as a thorough index listing many *materia medica* make the collection even more valuable.

Even though ancient historians and pharmacologists often appear to speak different languages, they have much to learn from each other. Unfortunately, there are very few people who can act as 'interpreters' or 'translators' able to bridge the gap between the two communities—John Scarborough is one of these rare scholars.

BIBLIOGRAPHY

Artelt, W. 1968. *Studien zur Geschichte der Begriffe „Heilmittel" und „Gift"*. Darmstadt.

Beck, L. Y. 2005. trans. *Pedanius Dioscorides of Anazarbus,* De materia medica. Hildesheim.

Bourbon, F. 2008. ed. and trans. *Hippocrate. Tome XII. 1re partie. Nature de la femme.* Paris.

Debru, A. 1997. ed. *Galen on Pharmacology: Philosophy, History and Medicine. Proceedings of the Vth International Galen Colloquium. Lille, 16–18 March 1995.* Leiden.

Demand, N. 1999. 'Did the Greeks Believe in the Efficacy of Hippocratic Treatment—And, If So, Why?' Pp. 139–148 in I. Garofalo *et alii* edd. *Aspetti della terapia nel Corpus Hippocraticum. Atti del IXe Colloque International Hippocratique (Pisa, 25–29 settembre 1996).* Florence.

Etkin, N. L. 1988. 'Cultural Constructions of Efficacy'. Pp. 299–326 in S. Van der Geest and S. Reynolds Whyte edd. *The Context of Medicines in Developing Countries: Studies in Pharmaceutical Anthropology.* Dordrecht.

Jacques, J.-M. 2002. ed. and trans. *Nicandre. Les Thériaques. Fragments iologiques antérieurs à Nicandre.* Paris.

_____ 2007. ed. and trans. *Nicandre. Les Alexipharmaques. Lieux parallèles du livre XIII des* Iatrica *d'Aétius.* Paris.

King, H. 1998. *Hippocrates' Woman: Reading the Female Body in Ancient Greece.* London/New York.

Riddle, J. M. 1985. *Dioscorides on Pharmacy and Medicine.* Austin, TX.

_____ 1992. *Contraception and Abortion from the Ancient World to the Renaissance.* Cambridge, MA/London.

Totelin, L. M. V. 2009. *Hippocratic Recipes: Oral and Written Transmission of Pharmacological Knowledge in Fifth- and Fourth-Century Greece.* Leiden.

The Archaic and the Exotic: Studies in the History of Indian Astronomical Instruments by Sreeramula Rajeswara Sarma

New Delhi: Manohar, 2008. Pp. 319. ISBN 81–7304–571–2. Cloth Rs 795

Reviewed by
Toke L. Knudsen
SUNY College at Oneonta
toke.knudsen@oneonta.edu

The Archaic and the Exotic: Studies in the History of Indian Astronomical Instruments by Sreeramula Rajeswara Sarma is a collection of 15 papers published by the author in the period between 1986 and 2004, most of them during the 1990s. The 15 papers have not been changed since their original publication, except that each of them is accompanied by a note explaining where and when the article was originally published. In addition, a brief preface and a very useful index have been included. What unifies the papers is that they all deal with the history of astronomical instruments in India.

Sarma, a distinguished and world-renowned scholar of Sanskrit and the history of science in India, has a long and fruitful career behind him. One of the many investigations undertaken by Sarma during his career concerns the history of astronomical and time-measuring instruments in India. This thorough investigation, lasting over a decade and a half, focuses on roughly 430 instruments found in over 100 museums and collections in India, Europe, and North America. It is to culminate, as announced by Sarma in the first article of the volume, in a catalogue of Indian astronomical and time-measuring instruments, a catalogue that is presumably close to completion and publication at this point in time. So, it is hard to imagine a candidate better suited for scholarly writing on the history of astronomical instruments in India than Sarma.

The title of the volume derives from the two types of instruments found in the Indian astronomical tradition, both of which are discussed by Sarma in this volume. On the one hand, there are the instruments classified by Sarma as archaic and, on the other, those classified as exotic. The archaic instruments are the ones described

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 121–127

in the traditional Sanskrit treatises on astronomy, more specifically, in the *yantra* (instrument) sections of these treatises, while the exotic instruments are those introduced into India from the west through the Islamic astronomical tradition. Sinking-bowl water clocks belong to the former category, while astrolabes and celestial globes belong to the latter. As Sarma explains in the preface to the volume, these two types of instruments are contradictory, yet complimentary. While the astronomers of India were quite willing—enthusiastic even—to embrace exotic instruments from foreign cultures (in this case, the Islamic culture to the west), they never abandoned the traditional instruments, even if they had become obsolete.

Sarma divides the 15 articles of the volume into four parts:

'The Context' (4 articles)
'The Water Clock' (4 articles)
'The Astrolabe' (5 articles)
'The Celestial Globe' (2 articles).

The first establishes the context for the investigation of Indian astronomical instruments. The second part deals with one of the archaic instruments, namely, the sinking-bowl type of water clock. The third part focuses on one of the exotic instruments, the astrolabe. In the fourth and final part, Sarma discusses another exotic instrument, the celestial globe. The volume contains numerous images of the instruments discussed, e.g., sinking-bowl water clocks, astrolabes, and celestial spheres, as well as images of Mughal miniature paintings. Having these images of the instruments available to the reader interested in their technical details adds an extra dimension to the volume and is very helpful.

The first part of the volume, as already noted, announces Sarma's project of producing a catalogue of Indian astronomical and time-measuring instruments. This is a very important project. In other cultural areas, such as the West and the Islamic world, projects like this have already been undertaken; and there is a great number of valuable scholarly resources available on both medieval European and Islamic instruments as well as on the making of instruments. Moreover, though some of the studies of Islamic instruments cover instruments made in India—much of India was, after all, under Islamic rule for many centuries—the material is not based on actual examination of instruments themselves but rather on descriptions

published in notices; and, so, these resources are deficient as catalogues of Indian instruments. Thus, Sarma is filling a real gap in the scholarly literature on astronomical instruments and his careful examination of extant instruments only makes his study that much more important.

Regarding the focus of his study, Sarma notes in the preface to the volume that any investigation of instruments such as his must not only be based on literary sources but also on a study of extant specimens of the instruments. Sarma further notes that there is also a third source of information, namely, paintings. Depictions of astronomical instruments in Indian Mughal miniatures cast light on the history of instruments in India, in particular, on the interactions and exchanges between the indigenous and Islamic traditions of astronomy and astronomical instruments. Thus, for example, some of the volume's images of Mughal miniature paintings show groups of Muslim and Hindu astrologers working together to create precise horoscopes for notable births. These are the three sources utilized by Sarma in the studies collected here.

The first step that Sarma takes in his study is into the wealth of Sanskrit treatises on instruments. Brahmagupta (seventh century AD) is the first author of a Sanskrit astronomical treatise to give an extensive account of instruments. This account forms a section of his *Brāhmasphuṭasiddhānta*, one of the most important works in the Indian astronomical tradition. Following his lead, other astronomers, including the renowned Bhāskara II (12th century AD), included similar sections in their works. However, the contents of these sections, as is the case with Sanskrit treatises in general, are brief. Their general and terse accounts of astronomical instruments do not allow us to infer much about the variety of their execution—or even if their execution was ever tested in practice—or to draw conclusions about their geographical distribution. Later, from the 14th century and onwards, due to the influence of the Islamic astronomical tradition, Sanskrit treatises devoted entirely to instruments were composed. However, even when these are supplied with elaborate commentaries, they never come near to the level of detail found in the treatises of the Islamic astronomical tradition, which give precise and elaborate details on how to design and create instruments in practice. Even in

the case of Sanskrit treatises specifically on instruments, questions regarding the variety of construction and geographical distribution are difficult to answer from the texts alone—more sources are required.

As already noted, Sarma's approach to overcoming the difficulty of working exclusively with textual evidence is to investigate actual specimens of instruments and to look at depictions of instruments in art. Examining such specimens and depictions can help us overcome the brevity of the texts and thus allow us to come to a better understanding of the instruments in question. Conversely, textual evidence can help with identifying a particular instrument and dating its design.

To provide a practical example, Sarma relates a story from his own work that demonstrates how the combination of text, specimen, and art can lead to a greater understanding. He notes that several Mughal miniatures portray a circular hoop-like object in the hand of an astrologer, an instrument that appears to be a ring dial, which is a European instrument. But why would these miniatures portray astrologers carrying ring dials rather than astrolabes (which were lauded as the greatest of instruments in the Islamic astronomical tradition)? Also, since the ring dial was not known to the Islamic world, how did it find its way to India? Sarma found the answer to these questions when he was editing a Sanskrit text on instruments, the *Yantraprakāra* of Sawai Jai Singh (1688–1743). The text contains a description of the ring dial under its Sanskrit name *cūḍāyantra*. Sarma also became aware of the existence of the existence of two specimens kept at the Jaipur Observatory. When he examined them, he found that one of them had a tablet attached that had the name *cūḍāyantra* inscribed. Subsequently, when investigating the antecedents of the instrument, he found that it was known to many earlier Indian astronomers starting with Āryabhaṭa (*ca* 500 AD); and that it was called *valayayantra* by Varāhamihira (sixth century AD). The variants of this instrument, one of them called *cūḍāyantra*, are discussed by the astronomer Rāmacandra in his treatise *Yantraprakāśa*. Sarma, therefore, rightly concludes that the instrument portrayed in the Mughal miniatures is a traditional instrument, the *cūḍāyantra*, and not the European ring dial.

This example brings out the value of Sarma's approach clearly and it confirms my view that he has chosen the right approach in his

study. In the example given, we see a case where the combined study of Sanskrit astronomical texts, Mughal miniature paintings, and extant specimens of instruments testifies to the history and popularity of the ring dial in India. Throughout the articles of the volume, Sarma's observations and conclusions are enhanced by his ability to draw on different types of primary source material.

Many of the articles in the volume deal with technical aspects of particular instruments. The second article of the first part of the volume gives an overview of the astronomical instruments described in the *Brāhmasphuṭasiddhānta* of Brahmagupta. This includes a discussion of the perpetual-motion devices described by Brahmagupta, who held that mercury can overcome inertia and thus power a wheel to turn eternally. The third article in this part continues the discussion of these perpetual-motion devices. The fourth article is an interesting and valuable study of astronomical instruments in Mughal miniature paintings.

Many of the remaining articles of the volume deal with particular instruments such as the sinking-bowl water clock, the astrolabe, and the celestial globe. Much of the material is of a technical nature but the articles are still informative and readable for a reader without a background in ancient and medieval astronomy.

One of the most fascinating of these articles is the second of the third part, which is a study of a family of astrolabe makers based in the city of Lahore (now the capital of the Pakistani province of Punjab). This family produced a very large number of instruments; the earliest, an astrolabe dated AD 1567, was made by one Allāhdād, the first instrument maker in the family. Sarma carefully details how over three generations more than 100 instruments, many of them exquisite, were produced by just six members of this family.

Sarma's volume is full of information, both technical and non-technical, about Indian astronomical instruments. It provides a very valuable reference work for the researcher but is also easy to access for the non-specialist. One might have wished for more of a synthesis of the 15 articles than is provided in the brief, four-page preface. After all, the most recent of the articles is separated in time from the earliest of them by nearly two decades; so the inclusion of a longer introduction or a conclusion tying the articles together would have been

a welcome addition. However, the articles each stand well on their own and one does not lose sight of the thread binding them together.

Since the articles, all of which were originally independent articles, did not undergo any editing before being included in the volume, there is some repetition of material. This is unavoidable, of course. There are, however, occasional inconsistencies between the articles which ought to have been corrected. To give one example: in the 10th article [205], the number of astrolabes made by Muḥammad Muqīm of the family astrolabe makers in Lahore is given as 'some 37'; but in article 14 [279] the number is said to be 32. These two articles were published in the same journal (*Studies in the History of Medicine and Science*) and in the same year (1994). Such inconsistencies are rare and minor though, and do not in any way detract from the main conclusions of the book.

Another problem concerns the images of the volume. In the first place, they are all black-and-white. In my view, color reproductions would have been preferable, especially in the case of the Mughal miniatures, even granted the various constraints that would presumably have made the volume more costly. Second, the quality of the images is poor. A higher resolution would have been immensely helpful in examining the images of specific instruments referred to in the text, as would have close-up images of important parts of the instruments. Many of the astrolabes have inscriptions (both in Arabic and Devanāgarī scripts); and while some of these are referred to and translated in the text, they are often very hard to read in the related images, which frustrates the reader with a background in the requisite languages. For example, Sarma describes an astrolabe created by Ḍiyā' al-Dīn Muḥammad (17th century AD) that ended up in the possession of Sawai Jai Singh of Jaipur [233]. The king had a Sanskrit inscription made on the back crown of the astrolabe and he also had a copper plaque detailing how the instrument works attached to it. The Rājasthānī text of the plaque is translated in the text by Sarma [232] but the original text is very hard to make out from the image of the astrolabe. A closeup image of the inscription would have made this easier.

However, none of these issues—all of them minor—make me hesitate to recommend Sarma's work highly to both the expert and the interested layman: it is a must for the scholar as well as an enlightening read for the non-expert. Indeed, this volume contains a valuable

account of the history of astronomy in India, including details of the people behind the tradition, be they astronomers, instrument makers, or patrons. In consequence, it is an indispensable reference for the history of astronomy and astronomical instruments in India. One hopes that Sarma will continue his excellent work on the history of Indian astronomy.

Untersuchungen zur hippokratischen Schrift „Über die alte Heilkunst“ by Brice Maucolin

Beiträge zur Altertumskunde 258. Berlin/New York: Walter de Gruyter, 2009. Pp. xii + 157. ISBN 978–3–11–020125–3. Cloth $109.00

Reviewed by
Joel E. Mann
St Norbert College
joel.mann@snc.edu

Of the more than 70 works that compose the Hippocratic corpus, perhaps none has been more studied and debated than *Περὶ ἀρχαίης ἰητρικῆς* (*De vetera medicina*), usually referred to in English as ‘On Ancient Medicine’ or, among Hippocratic scholars, simply as *VM*. In his *Untersuchungen zur hippokratischen Schrift „Über die alte Heilkunst“*, Brice Maucolin reminds us that the interest in *De vet. med.* is a relatively recent phenomenon. The ancients, though familiar with it, paid it scant attention. In all likelihood, this was due to the fact that its author, though espousing a theory of health and disease that might be called ‘humoral’, does not conform to what was widely regarded as the standard Hippocratic picture of the humors, which emphasized fluids like blood, phlegm, yellow and black bile, and the powers hot, cold, wet, and dry. Indeed, *De vet. med.* oozes resentment for proponents of such theories, all of whom are guilty of ‘postulating one or two things as the principle for everything’ [Littré 1961, 1.570] and this resentment was repaid with virtual banishment for centuries. Not until the physician-turned-classicist and positivist philosopher Emile Littré placed *De vet. med.* at the head of his masterly edition did its fortunes begin to change. But change they did. Since the mid-19th century, the literature on *De vet. med.* has grown at a pace suggesting that scholars are trying to make up for lost time.

The problem with this literature, claims Maucolin in his introduction, is that scholars have tended to reduce *De vet. med.* either to a confrontation with Plato or to a document in the history of ideas, with the result that it has not been appreciated as a literary work in its own right [6–7]. This, in turn, has led to a general failure among scholars to treat certain parts of the text adequately, most notably

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 128–131

chapters 13–19 [8]. Maucolin's aim, then, is to right this wrong by considering *De vet. med.* as a literary text in its entirety, with special focus on those chapters and their polemical character.

Maucolin is probably correct that *De vet. med.*, like several treatises in the Corpus, is viewed by many, if not most, through the lens of Presocratic philosophy and science; and it might be true that to some extent scholars have failed to meet its author on his own terms. In any case, Maucolin is certainly correct that the treatise's literary style has been understudied and the strength of his book lies in the contribution which it makes in this regard. For evidence, one need only turn to the book's appendix, which studiously catalogs the various stylistic figures employed by the author. Indeed, Maucolin's analysis of language and style is impressive throughout, though he sometimes fails to credit adequately the work of other scholars. For example, many aspects of the treatise's polemical character are treated ably in Ducatillon 1977; and, though Ducatillon's study appears in Maucolin's bibliography, there are surprisingly few references to it in the body of the book itself. A larger problem, however, is that Maucolin ignores important secondary literature in English that has appeared over the last several decades. Anglo-American scholarship has produced a number of important papers, dissertations, and books on *De vet. med.*; and some of these, like Jones 1946, Vickers 1977, Hankinson 1992, and Schiefsky 2005, are given short shrift by Maucolin, while others, like Hutchinson 1988, Allen 1993, and Cooper 2004, are not even recognized in the otherwise exhaustive bibliography.

Maucolin might counter that the above list comprises the very scholarship that he decries in his introduction. But, while it is true that such studies treat *De vet. med.* primarily as a document in the history of philosophy and science, it is difficult to understand why they deserve to be dismissed on these grounds alone. Or rather, it is difficult to see why their approach (supposing for the sake of argument that they can be treated *en bloc*) precludes them from appreciating *De vet. med.* on its own terms. They do not regard *De vet. med.* as a mere afterimage of Presocratic or Sophistic thought; each has something original to say about *De vet. med.* in its own right. It is regrettable that Maucolin ignores these voices.

This criticism would be far less trenchant if Maucolin limited himself to a strict literary analysis of the text. However, he appears

to believe that his analyses yield conclusions of importance for the history of philosophy and science. In his first chapter, for example, he discusses at length the meaning and significance of the author's complaint about

> those who attempt to speak or write about medicine by laying down for their arguments hot, cold, wet, dry or whatever they want as a postulate, simplifying the causal principle for human disease and death, even postulating one or two things as the principle for everything. [Littré 1961, 1.570]

Any English rendering of this famous passage is cursed with clumsiness due to the awkwardness of the original Greek, which has never ceased to intrigue and frustrate Hippocratic scholars. Maucolin surely would take issue with my version on many counts, but especially with my translation of (a) ὑπόθεσις as 'postulate' and (b) τὴν ἀρχὴν τῆς αἰτίης as 'causal principle' where he would prefer the allegedly less anachronistic 'assumption' [25] and 'proximate cause' [20], respectively. The difference is substantial. According to Maucolin's translation, the author is merely complaining that some doctors have oversimplified medical theory and, as a result, are practicing with a poor picture of human disease in mind. According to mine, the author is flagging a deep methodological disagreement with roots at the level of ontology. In fact, my translation reflects the prejudices of what has become more or less the received view in the history and philosophy of science, a view that Maucolin emphatically rejects [24]. The author of *De vet. med.* is not introducing technical terminology to make an abstract point about method, he claims [18], and we would be wrong to read him as such.

The problem is that the author seems to be doing just that: he certainly avails himself of terminology current in mathematics and natural philosophy. Much turns, of course, on how we take (b) above, since it may well explicate (a). But Maucolin does not really argue for his reading of (b). Instead, he cites in his defense a passage from another Hippocratic work, the second book of the *Epidemics* [Littré 1961, 5.126] as well as variant readings of the *Epidemics* passage gleaned from citations in Galen [20n34]. But the *Epidemics* passage is not a perfect parallel and it is unclear what is to be made of Galen's citations. Even if Maucolin has the correct interpretation, an argument from language and style alone will not be adequate to

make his case. A hard slog through the philosophical literature is required, but Maucolin appears unwilling or unable to undertake it. Again, that would be excusable if Maucolin limited his project to analyses of language and style in a strict sense, for that is where he makes original contributions of real interest. But as it stands, we are left with a solid study of *De vet. med.* that supplements but does not supplant existing scholarship, though its ambitions may incline toward the latter.

BIBLIOGRAPHY

Allen, J. 1993. 'Failure and Expertise in the Ancient Conception of an Art'. Pp. 81–108 in T. Horowitz and A. Janis edd. *Scientific Failure.* Lanham, MD.

Cooper, J. 2004. *Method and Science in* On Ancient Medicine. Pp. 3–42 in J. Cooper *Knowledge, Nature, and the Good: Essays on Ancient Philosophy.* Princeton, NJ.

Ducatillon, J. 1977. *Polémiques dans la collection hippocratique.* Paris.

Hankinson, R. J. 1992. 'Doing without Hypotheses: The Nature of Ancient Medicine'. Pp. 55–67 in J. A. López Férez ed. *Tratados hipocraticos. Actas del VII Colloque Internationale Hipocratic.* Madrid.

Hutchinson, D. S. 1988. 'Doctrines of the Mean and the Epistemology of Skill in Ancient Medicine and Ethics'. *Apeiron* 21:17–52.

Jones, W. H. S. 1946. *Philosophy and Medicine in Ancient Greece.* Baltimore, MD.

Littré, E. 1961. *Oeuvres completes d'Hippocrates.* 10 vols. 2nd edn. Amsterdam. Originally published in 1839–1861.

Maucolin, B. 2009. *Untersuchungen zur hippokratischen Schrift „Über die alte Heilkunst".* Berlin/New York.

Schiefsky, M. 2005. *Hippocrates,* On Ancient Medicine. Leiden/Boston.

Vickers, D. W. 1977. 'Hypothesis: The Platonic Concept and Its Background in Literature and Medicine'. PhD dissertation, University of Texas at Austin.

Evagrius and Gregory: Mind, Soul and Body in the Fourth Century by Kevin Corrigan

Ashgate Studies in Philosophy and Theology in Late Antiquity. Burlington, VT/Farnham, UK: Ashgate, 2009. Pp. x + 245. ISBN 978–0–7546–1685–6. Cloth $99.95

Reviewed by
John M. Dillon
Trinity College, Dublin
jmdillon@eircom.net

This book, the third in a new series dedicated to Philosophy and Theology in Late Antiquity, is a most welcome addition. Here Corrigan (who has previously contributed much in his publications to both Neoplatonism and Patristics) turns to a comparative study of the thought of two fourth-century Christian theologians who are of rather different character but yet considerable doctrinal connection, Evagrius of Pontus and Gregory of Nyssa. In this work, Corrigan sets himself to study their respective positions on such questions as the relation between body and soul, the freeing of the soul from bodily concerns and influences (ἀπαθεία), the relation of soul to mind, the nature of *gnosis*, and the development of the concept of a person.

The book comprises 10 chapters and a general conclusion. The first two set the scene by introducing us to the two personalities concerned and to the general background of Church history and doctrinal controversies in the fourth century in which they both took part. (Evagrius later came under the hammer as a heretic, infected with 'Origenism', while Gregory remains a Father of the Church, though somewhat in the shadow of his elder brother Basil.) We then proceed to a series of eight chapters on various aspects of their thought, duly compared.

Evagrius (*ca* AD 334–399) was the son of a country bishop in the province of Pontus, and was himself ordained priest by Basil of Caesarea. He then served as archdeacon to Gregory of Nazianzus in Constantinople and took part in the Council of Constantinople in 381, as did Gregory of Nyssa. His good fortune went to his head though, it seems, leading to an affair with a married woman and then to a

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 132–135

radical change of heart which saw him departing first to Jerusalem (where he came under the influence of a holy woman, Melania) and then to the deserts of Egypt in search of a life of purity and asceticism. Once in Egypt, he became the chronicler and spiritual guide of the monks there and, as Corrigan argues, a theologian of great acuteness and originality. Corrigan even wishes to claim him as 'the father of cognitive psychology'.

As for Gregory (*ca* AD 335–395), Corrigan presents his life as something of a contrast to that of Evagrius, being born as he was in the countryside of Cappadocia and being wedded initially to a monastic life but consenting to become bishop of Nyssa in 372 at the insistence of his brother Basil. He and Evagrius, as mentioned above, were in Constantinople together for a while around 381, during which time Gregory composed a number of his more important works.

Corrigan next gives us, in chapter 3, an overview of the thought of both figures on the central topic of the relations of mind, soul, and body. Their thought here owes something to Aristotle's distinction between νοῦς—itself partly 'external' (θύραθεν)—and soul, and to later Platonist distinctions between mind and soul (including Plotinus' concept of the 'undescended' soul). But it also shows distinctive characteristics, in particular as regards defining the relation between mind and body in a way that subsumes the body into the higher levels of the person rather than rejecting it outright.

Chapter 4 sets out most illuminatingly the doctrine of ἀπαθεία as propounded by both thinkers, which, as Corrigan emphasizes, is far from being a negative or privative concept but rather a freeing up of the soul for an appreciation of spiritual realities and the love of God. The influence of Plotinus is operative here rather than that of Stoicism directly.

After this, Corrigan is forced to allow his two thinkers to part company, as their doctrines, though connected, are significantly different. We get a series of chapters devoted to each in turn. In chapter 5, we have an examination of Evagrius' remarkable doctrine of the Eight Λογισμοί or '(Bad) Thoughts', ancestor of the later 'Seven Deadly Sins' (as propounded by Pope Gregory the Great). These seem to be the eight types of unprofitable mental tendencies that serve to distract a monk from his prayers. Corrigan sees them as a

creative adaptation of Plato's treatment of debased forms of personality in books 8–9 of the *Republic*, which seems a bit optimistic but not impossible, I suppose.

In chapter 6, we turn to Gregory and the theme of the 'Fall of the Intellect', where once again Corrigan discerns the adaptation of Platonist motifs. Gregory speaks of evil as a function of the fall of the mind into matter, such as also is presented by Plotinus, e.g., in *Ennead* 1.8. For Gregory, mind falls away from 'the beautiful' through perversity (the Plotinian τόλμα) and must be reclaimed through the sacraments.[1]

In chapter 7, we are back to Evagrius and the converse topic of 'Body into Mind', where Evagrius in credited with a 'scientific eye' which discerns the system of signs of which the physical world is made up. As Corrigan argues, for Evagrius, 'nature does not simply reflect intelligible reality; in some sense it already is intelligible.' And connected with this is the thought that body itself is in a way intelligible. This sounds like a version of the doctrine of spiritual body, or 'pneumatic vehicle', common to Origen and later Platonists; but Corrigan argues that it is not quite that. Evagrius, it seems, sees the actively perceptive body, together with its supporting structures, as *intelligible in its own right*. Corrigan wishes to see here a connection with some passages of Plotinus [e.g., *Enn.* 6.3.9.1–7] but, again, perhaps somewhat optimistically. Does this view make Evagrius an interesting kind of monist? Corrigan balks at the term but it seems to fit to some extent.

In chapter 8, Corrigan turns back to Gregory to discuss *his* anthropology against the background of his doctrine of the Trinity. Here I would note, first of all, that the best source for Gregory's interesting view of the relations between the persons of the Trinity [cf. 135] is really not so much a passage of Plotinus such as *Enn.* 6.1.4 but the doctrine of Porphyry which links the noetic triad of Being-Life-Mind with the One itself, a doctrine made use of also by the other Cappadocians. Then, the idea of an original Man, free of gender distinctions or the passions, may be derived from Origen; but

[1] See the nice image of the Egyptian army, as the passions, being drowned in the Red Sea, representing the water of baptism, at *Vita Mosis* 1.122].

it seems to owe something also to Philo. It is notable also how interested Gregory is in the 'latest' medical discoveries about human physiognomy from Galen and other sources.

Chapter 9, 'The Human in the Divine: The Dialogical Expansion of Mind and Heart', returns to Evagrius and his view of the mystical life. Here we see most clearly why Corrigan wishes to credit Evagrius with the development of 'cognitive psychology', by reason of his interesting tendency to integrate mind and body to produce a philosophy of the person—though this is really driven by Evagrius' concern for the management of the monastic life.

Lastly, Corrigan turns in chapter 10 to a study of Gregory's mystical theology and of the role of individuality and personhood in that context, which is in turn bound up with the doctrine of the Trinity. A general conclusion brings all these themes together. Corrigan ends with a speculation as to how far both thinkers saw themselves as reconciling, at least to some extent, the philosophical position of Origen and Plotinus; but he leaves this as a question.

All in all, a most stimulating and thoughtful book, which sheds considerable light on each of these interesting thinkers and on the links between them.

When the Gods were Born: Greek Cosmogonies and the Near East by Carolina López-Ruiz

Cambridge, MA/London: Harvard University Press, 2010. Pp. xiv + 302. ISBN 978–0–674–04946–8. Cloth $39.95

Reviewed by
Johannes Haubold
Durham University
j.h.haubold@dur.ac.uk

In a famous lecture, 'The Crisis of Comparative Literature', delivered in 1958, René Wellek assessed the state of his discipline as follows:

> Van Tieghem, his precursors and followers... have accumulated an enormous mass of parallels, similarities and sometimes identities, but they have rarely asked what these relationships are supposed to show except possibly the fact of one writer's knowledge and reading of another writer. [Wellek 1963, 285]

Some 50 years later, Wellek's statement reads like a disconcertingly accurate assessment of current work on ancient Greek and Near Eastern literature: there are now several publications listing 'parallels'[1] but scholars have so far struggled to frame this material in a helpful way. Questions of 'one writer's knowledge and reading of another writer' continue to dominate the field and divert attention from the urgent methodological issues raised by the comparative study of ancient texts.

López-Ruiz's book proposes to tackle the impasse and to 'reconfigure the old question of Greece's "debts" to the East' [47]. After an introduction which reviews current approaches to comparative study and specifies the author's own focus on Cilicia, southeast Anatolia, and Syro-Palestine as contexts for cultural exchange [1–22], chapter 1 looks at different ways in which narratives travelled between the Levant and Greece, from commerce to storytelling within families [23–47]. Chapter 2 focuses on the Hesiodic line about the tree and the rock [*Theog.* 35] and its affiliations in Levantine literatures from the

[1] Most notably Burkert 1992, West 1997.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 136–141

Baal Cycle of the second millennium BC to Ibn Ishaq's biography of the Prophet Mohammed [48–83]. Chapter 3 looks at the relationship between Greek and Near Eastern succession myths, including Hesiod's *Theogony*, the *Phoenician History* of Philo of Byblos, the Hebrew Bible, the Hurro-Hittite *Kumarbi Cycle*, and the Babylonian *Enūma eliš* [84–129]. Particularly noteworthy is the author's inclusion of Ugaritic deity lists [101–104]. Chapter 4 turns the spotlight on Orphic theogonies and related Levantine traditions, chiefly the Derveni cosmogony but also Eudemus, Hieronymus, Pherecydes of Syrus, Mochus, the Sidonian cosmogony quoted in Eudemus, and Philo of Byblos [130–170]. Chapter 5 offers a concluding discussion of cosmogonic poets and their role in processes of cultural transfer [171–212]. An appendix [205–210] revisits the motif of tree and rock already discussed in chapter 2. The book ends with an index of passages [285–287] and an unusually full general index [288–302].

There is much in López-Ruiz's work that is genuinely helpful. The introduction in particular ought to become prescribed reading for anybody interested in the subject: López-Ruiz rightly questions lingering notions of a distinctive 'Indo-European' cosmogonic tradition [11–13] and rejects the label 'Near Eastern' as a catch-all with little heuristic value [17]. As a way out of the Hellenocentrism which encourages the undifferentiated use of the term 'Near East', she recommends, sensibly, that comparisons should be culturally specific: thus, we should not compare Greek cosmogonies and 'Near Eastern' ones but Greek and Egyptian traditions, Greek and Levantine traditions, and so on. López-Ruiz is equally convincing when she considers existing models of cultural transfer such as diffusion, borrowing, and colonization; or when she warns against the dangers of the still popular '"inventory" method' of literary comparison [21]. Many of these caveats have been expressed before [e.g., in Haubold 2002] but they have rarely been formulated as coherently as they are here.

Chapter 1 tackles head-on some of the most cherished scholarly myths invoked to explain the practicalities of cultural exchange. One of the targets here is the native 'informant', a figure often thought to have enabled the adoption of the alphabet on the part of the Greeks [31–34]. López-Ruiz rightly points out the 'colonial resonances' of that concept (her term) and shrewdly asks,

> Why not assume that the "informed" (presumably a Greek) and the "informant" (presumably a Semite) were one and the same person? [33]

Why not, indeed? López-Ruiz's preferred model of sustained hybridity [44–47] will be familiar to archaeologists but may still come as a surprise to some scholars of classical philology who tend to regard language differences as a genuine obstacle to communication. López-Ruiz is not afraid to contemplate widespread bilingualism, even within families [36–37], and to move beyond conventional, but ultimately misleading, distinctions between 'Greek' and 'Semitic' identities more generally.[2] Even apparently innocuous categories such as 'foreign' require careful interrogation: is a person, object or story actually experienced as coming from elsewhere? Or is it merely experienced as new? Has it perhaps become fully assimilated, so that its origins are no longer relevant? [45] In this connection, López-Ruiz asks whether there was an orientalizing revolution at all in the archaic period. Her answer is nuanced. On the one hand, she rightly questions the assumption that influence should only have run from East to West: 'close interaction over the course of more than a thousand years cannot be a one-way process' [38]. However, she also concedes that

> the stream of cultural transformation toward the end of the so-called Dark Ages, and especially during the eighth-seventh centuries (the 'orientalizing period'), ran more strongly from the Levant toward the West. [43]

Here as already in the introduction, López-Ruiz advocates moving from a vague notion of 'Near Eastern influence' to a much more focused model of contact in and around the northern Levant. In defense of that choice, she adduces some familiar arguments, e.g., on pressure from Assyria and Babylon [44], and for once we sense that the discussion may not do full justice to the complexities of the issue. But overall, the chapter makes an excellent case for the Levant and the Phoenicians as conduits for cultural exchange and effectively introduces many of the salient issues when thinking about cultural contact in the first-millennium Mediterranean.

[2] For early Greek identities, see especially Hall 2002.

Chapters 2–5 aim to put into practice the principles laid down in the introduction and chapter 1. Unfortunately, they do not quite live up to the promise of the methodological material. With comparative study, more perhaps than other fields of literary interpretation, the proof of the pudding is in the eating. As many critics have pointed out, we need to know what difference the arduous work of comparison makes to our appreciation of the texts involved. López-Ruiz herself defines her task as that of turning 'our "tabular" and encyclopedic knowledge of Greek and Near Eastern "parallels" into cultural interpretation' [14]. The emphasis on *cultural* interpretation as opposed to *textual* analysis is perhaps telling: for while López-Ruiz is indeed a careful student of ancient culture, she is often less patient with texts. That is a pity in a discussion of ancient cosmogonies, which do indeed require 'cultural analysis', but which must also be appreciated as texts. The problems start in chapter 2, which on an uncharitable reading does precisely what López-Ruiz herself tells us we should not do: it plucks a single line of Greek poetry out of context and goes on a spree of parallels in non-Greek texts. Readers of Hesiod will balk at the claim that line 35 holds 'the key' to the proem of the *Theogony* [78–80]. The author adduces an impressive range of comparative materials, from cosmogonic epic to the Hebrew Bible, Platonic philosophy, the apocryphal Gospel of Thomas, the Qurʾan and early Islamic biography; but ultimately this approach is too sweeping to offer a genuinely helpful interpretation. López-Ruiz seems to pay tribute here to what we might call the 'detective' approach to cultural comparison. The promise of solving longstanding problems in Greek literature with the help of parallels elsewhere has an obvious appeal in a field that still needs to defend its very right to exist. Yet, the temptation, it seems to me, should be resisted: in the specific case of López-Ruiz's argument, focusing so insistently on one enigmatic line results in some strained claims and jars with the author's healthy intuition, expressed with refreshing clarity in the introduction, that meaningful comparative study is precisely not a matter of micro-level coincidences, however plausible or important they might seem.

Chapter 3 is more satisfactory in this regard, building on the broader foundations of shared thematic structures: a narrative of divine succession is now well understood to form the backbone of several ancient cosmogonic traditions, including Hesiod's *Theogony*.

But here too we must beware of pitfalls. López-Ruiz sets out the evidence in a helpful table [88] and proceeds to argue for a privileged connection between the Greek and Levantine traditions. Her prize exhibit is the role of Kronos in Hesiod and of El in the *Baal Cycle* [115–125]: both gods are said to occupy an 'ambiguous position' in their respective 'mythology' [122] in that they are old but still important. The vagueness of this claim points to a problem with the argument: Kronos may have been an ongoing concern in Greek ritual and 'mythology' more generally, but in the *Theogony* he is much less active after his defeat than El is in the Ugaritic texts. López-Ruiz resorts to sliding uneasily between Hesiod's *Theogony* as the main point of comparison and a more nebulous 'Greek mythology'. Inevitably perhaps, some telling details get lost along the way. For example, López-Ruiz correctly points out that Kronos features in the myth of ages in the *Works and Days* [117–118] but later concludes that he

> is linked in the *Theogony* and elsewhere [*sic*] with heroic ancestors through the myth of the Five Races, and through his association with the Isles of the Blessed and with the gloomier Tartaros and the Underworld in general. [125]

Similar sleights of hand help along the enterprising chapter 4 on Orphic traditions, which deals with some of the most difficult material that classicists are ever likely to encounter. Here too one would have liked to see a more nuanced treatment of some of the texts under discussion, e.g., on the sleeping/intoxicated Kronos [164–167]. Chapter 5 would also need some qualification. López-Ruiz claims that

> the *Theogony*'s sheer success . . . must be credited to a degree of innovation and originality in how Hesiod recast . . . traditional themes. [177]

Innovation and originality are problematic categories in the context of early Greek epic, as is now well understood. More generally, it is hazardous to speculate about the reasons behind the 'success' of a text whose original performance context is unknown and whose fortunes fluctuated over the centuries [see, e.g., Boys-Stones and Haubold 2010]. Later on in the chapter, López-Ruiz takes Empedocles and other charismatic figures to exemplify the more general claim that Greek theogonic poets tend to be represented 'as wandering figures' [191]. If that is indeed a general rule, then Hesiod looks

like the obvious exception: it would be interesting to know what López-Ruiz thinks of his more locally grounded authorial *persona* of the *Theogony* and *Works and Days*.

In conclusion, López-Ruiz has written a welcome book which repays careful study. The introduction and chapter 1 are refreshingly unblinkered and make several excellent points about the comparative study of ancient literatures: they will be of particular use to anyone approaching the subject for the first time. The readings of chapters 2–5 are more problematic: López-Ruiz has brought together a wealth of fascinating and often difficult materials, but her analyses are not always as nuanced as they might have been. Nevertheless, her chapters open many new avenues for research and thus succeed in keeping one of the most pressing issues of current classical scholarship on the intellectual agenda.

BIBLIOGRAPHY

Boys-Stones, G. R; and Haubold, J. H. 2010. edd. *Plato and Hesiod*. Oxford.

Burkert, W. 1992. *The Orientalizing Revolution: Near Eastern Influence on Greek Culture in the Early Archaic Age*. Trans. by M. E. Pinder and W. Burkert. Cambridge, MA.

Hall, J. M. 2002. *Hellenicity: Between Ethnicity and Culture*. Chicago.

Haubold, J. H. 2002. 'Greek Epic: A Near Eastern Genre?' *Proceedings of the Cambridge Philological Society* 48:1–19.

Wellek, R. 1963. 'The Crisis of Comparative Literature'. Pp. 282–295 in S. G. Nicholas ed. *Concepts of Criticism*. New Haven.

West, M. L. 1997. *The East Face of Helicon: West Asiatic Elements in Greek Poetry and Myth*. Oxford.

Geography and Ethnography: Perceptions of the World in Pre-Modern Societies edited by Kurt A. Raaflaub and Richard J. A. Talbert

Malden, MA/Oxford, UK: Wiley-Blackwell 2010. Pp. xviii+357. ISBN 978–1–4051–9146–3. Cloth $134.95

Reviewed by
Kai Brodersen
University of Erfurt
kai.brodersen@uni-erfurt.de

'The past is a foreign country: they do things differently there' as the opening lines of L. P. Hartley's celebrated novel *The Go-Between* [1953] rightly claim. If we want to explore the geography and ethnography of such a foreign country and to find out how differently they do things there, we will look for expert guidance.

The first expert to come to mind will be a geographer or ethnographer. She or he might tell us:

> Speakers of the Australian language Guugu Yimithirr... at the Hopevale community near Cooktown, in far North Queensland, make heavy use in discourse about position and motion of inflected forms of four cardinal direction roots—similar in meaning to *north*, *south*, *east*, and *west*. The system of cardinal directions appears to involve principles for calculating horizontal position and motion strikingly different from familiar systems based on the anatomies of reference objects, including speakers and hearers themselves. Rather than calculating location relative to inherent asymmetries in local reference objects, or from the viewpoint of observers themselves characterized by such asymmetries, the Guugu Yimithirr system apparently takes as its primitives global geocentric coordinates, seemingly independent of specific local terrain and based instead on horizontal angles which are fixed, as it were, by the earth (and perhaps the sun) and not subject to the rotation of observers or reference objects. [Haviland 1998, 25]
>
> If Guugu Yimithirr speakers want someone to move over in a car to make room, they will say *naga-naga manaayu* which means 'move a bit to the east'. If they want to tell you

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 142–148

> to move a bit back from the table, they will say *guwa-gu manaayi*, 'move a bit to the west'. It is even unusual to say only 'move a bit that way' in Guugu Yimithirr. Rather, one has to add the correct direction 'move a bit that way to the south'. Instead of saying that 'John is in front of the tree,' they would say, 'John is just north of the tree.' If they want to tell you to take the next left turn, they would say, 'Go south here.' To tell you where exactly they left some thing in your house, they would say, 'I left it on the southern edge of the western table.' To tell you to turn off the camping stove, they would say, 'turn the knob east'. [Deutscher 2010, 166]

But what if the foreign country we want to explore is a past, pre-modern world—or indeed a multitude of pre-modern worlds? What kind of guidance for our exploration of geography and ethnography can we ask for? How can such guidance be organized?

The present, very carefully edited and beautifully produced volume, which is based on contributions to a workshop at Brown University and is published as part of the valuable series The Ancient World: Comparative Histories, presents itself as 'a single pathbreaking volume' [6]. It opens up four paths. The first two are geographic, leading from India in the west round to China in the east and from the Mississippian peoples in the north to the Inca in the South. The third abolishes a geographical for a chronological approach and guides us from early Mesopotamia *via* Pharaonic Egypt, the Greek, Roman, and Islamic cultures to medieval Europe, while the fourth presents us the views of individual thinkers, from the Hellenistic *Book of Jubilees* to the 11th-century Islamic *Book of Curiosities* discovered only a decade ago. A final chapter plots the changes to presenting the world in Europe from 1500 to 1750. Subsaharan Africa, further northern or western areas, America, and the Australias (including the Guugu Yimithirr) remain *terra incognita*; but no guidebook can really be comprehensive.

As Christopher Minkowski states in his contribution 'Where the Black Antelope Roam: Dharma and Human Geography in India' [9–31],

> a project of recovering and understanding the uses of geographical and ethnographical knowledge and conceptions by the peoples who produced them, in their own times and places,

is a challenge for both authors and readers [11]. First, few premodern societies attach huge importance to situating themselves not just within the immediately perceived world but also within a vaster universe for which the teaching of sacred scripture may be held superior to any scientific knowledge. India's Sanskrit texts, the *Puranas*, present an outstanding instance not only defining geography but also justifying a hierarchical ordering of society by castes. Both Minkowski and Kim Plofker in her short essay, 'Humans, Demons, Gods and Their Worlds: The Sacred and Scientific Cosmologies of India' [32–42], discuss this phenomenon. For early China, Agnes Hsu's study, 'Structured Perceptions of Real and Imagined Landscapes in Early China' [43–63], demonstrates that the maps found at Mawangdui in 1973—which have so far been studied mainly because of their presentation of hydrology and topography—convey an important ritual and symbolic quality. By marking the Han-controlled territory in Changsha, one of these maps presents a visual symbol signifying the separation between the civilized world and the landscapes of untamed peoples and by being placed in a tomb, the maps became a metaphor for a perpetually preserved space. Similarly, Hsu shows that the Anping map-like mural of Eastern Han—an axonometric 'bird's-eye' view—had a spiritual function in the tomb. John Henderson's short study, 'Nonary Cosmography in Ancient China' [64–73], deals with a very influential type of dividing space according to the pattern of the square divided equally 3×3; and in doing so, it emphasizes the risk which modern researchers incur when using Chinese texts of this type to answer questions which are of our own contemporary interest (as exemplified in this volume) while ignoring that such questions may well have been of marginal interest, if they had any, to the original authors. Equally, in his 'Knowledge of Other Cultures in China's Early Empires' [74–88], Michael Loewe shows that a sense of space or recognition of long distances is rather unusual in the preserved sources, as is an appreciation for the effect of natural conditions on the growth of a community or the characteristics of its culture. Indeed, the past is a foreign country: they do things differently there.

In the Americas, Kathleen DuVal studies 'The Mississippian Peoples' Worldview' [89–107], a particular challenge as she is dealing with non-literate societies (whose indigenous name is, therefore, unknown). She concludes that they had a keen sense of self-identity

and of borders but were inclusivist in outlook and eager to learn from outsiders. Equally challenging is the understanding of 'Aztec Geography and Spatial Imagination' [108–127], explored by Barbara E. Mundy, who shows that in the Aztec empire, which extends out from the central capital Tenochtitlan on an island in Lake Texcoco in concentric spaces, a contrast can be observed between the nearby and intelligible (*nahuac*) and the distant unknown (*huehca*). As for the 'Inca Worldview' [128–146], Catherine Julien shows that the original conceptualization of Tawantinsuyu (Peru) seems to have combined geography, political theory, and a statement of power (we lack accounts by native authors in local languages), and that the Westerners' preconceptions influenced their understanding so much that the territory of Tawantinsuyu survived but was entirely re-imagined by its Spanish conquerors.

The chronological part of the book starts with an essay, 'Masters of the Four Corners of the Heavens: Views of the Universe in Early Mesopotamian Writings' [147–168], by Piotr Michalowski, who examines the symbolic literary imagery in these texts. 'The World and the Geography of Otherness in Pharaonic Egypt' [169–181] is then discussed by Gerald Moers using both images and texts. It becomes clear that most foreign peoples were rejected outright, as the Pharaoh (the living incorporation of the god Horus) would impose orderly rule upon the cosmos from its center, Egypt, while the foreigners remained a constant threat and needed to be controlled with violence. Under the title 'On Earth As in Heaven', James Scott examines 'The Apocalyptic Vision of World Geography from *Urzeit* to *Endzeit* according to the *Book of Jubilees*' [182–196], a rarely studied apocalyptic text which is likely to date to the seconded century BC and survives as a complete text only in an Ethiopic translation. Scott shows how the book establishes the prominent place of Israel and the Jews in the world, both now and in the expected eschatological future, by assuming a spatial symmetry between heaven and Earth. Returning to the earlier times, Susan Guettel Cole quotes the Delphic Oracle's claim that 'I Know the Number of the Sand and the Measure of the Sea' [Herodotus, *Hist.* 1.47] to study 'Geography and Difference in the Early Greek World' [197–214]; and James Romm discusses 'Continents, Climates, and Cultures: Greek Theories of Global Structure' [215–235]. As the co-editor Richard Talbert summarizes in his valuable preface, they both show that

> maps were created as aids to philosophical and geographical speculation about the world. Literary records, including geographic catalogs in Greek epic poetry, as well as itineraries, predated maps and were never superseded by them. Division of the globe by continents, climates and cultures became a topic that engaged a long succession of Greek writers, who in turn later influenced Jewish, Roman and medieval thinking in East and West. [3–4]

Equally mapless was 'The Geographical Narrative of Strabo of Amasia' [236–251] presented by Daniela Dueck. In his own contribution, 'The Roman Worldview: Beyond Recovery?' [252–272], Richard Talbert shows that Roman culture, while proudly celebrating territorial expansion,

> never enlarged the limited range of contexts and purposes for which it employed maps of various types: in part for this reason, cartographic norms failed to develop. [4]

Still, some kind of 'mental map' seems to be behind artifacts as variant as milestones and sundials. A radically different approach is teased out of the evidence of the Islamic texts by Adam Silverstein in his essay, 'The Medieval Islamic Worldview: Arabic Geography in Its Historical Context' [273–290]. He shows that the relevant body of writing in Arabic and Persian is very large indeed but does not allow us to assume that these texts were meant to form a 'worldview' (a concept developed in the Ancient Near East and in the Hellenistic world): the authors used personal observation or the testimony of eyewitnesses, which made information on non-Muslim lands both unattainable and irrelevant for them. However, the *Book of Curiosities*, recovered only a decade ago and here studied by Emilie Savage-Smith in her 'The Book of Curiosities: An Eleventh-Century Egyptian View of the Lands of the Infidels' [291–310], presents the Mediterranean in a very different way, emphasizing the eastern Mediterranean but surprisingly excluding Muslim Spain and western Europe. And in 'Geography and Ethnography in Medieval Europe: Classical Traditions and Contemporary Concerns' [311–329], Natalia Lozovsky shows how medieval scholars in Europe, combining classical scholarship and Christian doctrine, and incorporating new information, developed a distinctive presentation of the world and its peoples. Thus, medieval *mappaemundi* amalgamated spiritual truths and information about the material world; and when in the ninth century scribes

at St Gall were glossing a chapter of Orosius' *History Against the Pagans* (which dates to the early fifth century), they included references to the encroaching Bulgars and Hungarians. Only in the last chapter of the book, 'Europeans Plot the Wider World, 1500–1750' [330–343] by David Buisseret, do we return to more familiar territory: a combination of the Ptolemaic and Portolan chart traditions enabled European scholars to record the exploration of the wider world and to create maps which gradually adopt the norms which we often assume to be universal today. But, as this volume amply shows, the past is a foreign country: they do things differently there!

> A single pathbreaking volume can only accomplish so much; if other colleagues are subsequently inspired to follow this lead, that further progress will be very welcome,

writes Talbert in the introduction [6]. The volume succeeds not just in this, but makes following the lead of the paths presented in it no less attractive than breaking different new paths. To give one example: one of the conclusions presented in the book states,

> Regardless of whether or not the societies under discussion developed maps, there emerges from the volume a persistent (and perhaps hardly surprising) tendency for them to situate themselves at the center of their world. [4]

But, if we leave the paths set out here from India in the west to China in the east and from the Mississippian peoples in the north to the Inca in the South, from Early Mesopotamia to medieval Europe, from the *Book of Jubilees* to early modern Europe, for the speakers of Guugu Yimithirr in Australia, even the seemingly universal tendency to situate oneself at the center of one's world becomes a real surprise. In the 1980s, the linguist Stephen Levinson was filming the poet Tulo telling a traditional myth. Suddenly, as Deutscher relates, Tulo

> told him to stop and 'look out for that big ant just north of your foot'. In another instance, a Guugu Yimithirr speaker called Roger explained where frozen fish could be found in a shop some thirty miles away. You will find them 'far end this side', Roger said, gesturing to his right with two flicks of the hand. Levinson assumed that the movement indicated that when one entered the shop the frozen fish were to be found on the right-hand side. But no, it turned out that the fish were actually on the left when you entered the shop. So

> why the gesture to the right? Roger was not gesturing to the right at all. He was pointing to the north-east, and expected his hearer to understand that when he went into the shop he should look for the fish in the north-east corner. [Deutscher 2010, 166]

In sum, the editors, and the publisher, are to be congratulated on producing a stimulating volume which provides expert guidance to many aspects of the foreign country which is the past.

BIBLIOGRAPHY

Deutscher, G. 2010. *Through the Language Glass.* London.

Hartley, L. P. 1953. *The Go-Between.* London.

Haviland, J. B. 1998. 'Guugu Yimithirr Cardinal Directions'. *Ethos* 26:25–47.

Natural Philosophy Epitomised: Books 8–11 of Gregor Reisch's Philosophical Pearl *(1503)* translated by Andrew Cunningham and Sachiko Kusukawa

Burlington, VT/Farnham, UK: Ashgate, 2010. Pp. lxxiv+346. ISBN 978–0–7546–0612–3. Cloth $134.95

Reviewed by
Richard Oosterhoff
University of Notre Dame
rooster1@nd.edu

This translation of four books on natural philosophy from Gregor Reisch's *Margarita philosophica* (1503), a 16th-century introduction to the liberal arts and philosophy, is an excellent start to filling a gap in medieval, renaissance and early modern history. Charles Schmitt posthumously sketched the parameters of this gap in a short appendix to the *Cambridge History of Renaissance Philosophy* entitled 'The Rise of the Philosophical Textbook' [1988], where he drew attention to a 16th-century textbook tradition linked to the late medieval manuals on natural philosophy which circulated among arts students in *studia* and universities across Europe.[1] Among these, that of Reisch was especially thorough and popular. It was edited and republished throughout the 16th century, including an edition by the Parisian mathematician Oronce Fine in 1532 (Basel) and an Italian translation in 1600 (Venice). Now it has been translated anew, albeit in part, by Andrew Cunningham and Sachiko Kusukawa, two leading scholars in the history of early modern medicine and science.

The 66 pages of introduction are themselves a contribution to the history of science, not least by marking areas of 15th-century culture that need attention. The first area is the history of the book, which has become something of a celebrated discipline lately, notably with the work of Adrian Johns [1998]. Despite this celebration and despite Elizabeth Eisenstein's assertions [1979] of how scientific printed books established the technology of print as a force for cultural progress, the bulk of book history focuses on astronomy from

[1] Another key entry in the bibliography, which includes essays on natural philosophy, is Campi *et al.* 2008.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 149–161

the second half of the 16th century; and the field still needs encouragement from the likes of Cunningham and Kusukawa to help understand earlier scientific books. A pertinent example is how Reisch's printer negotiated with artists illustrating the text, showing the elaborate cooperation required for such illustrations. Printed schoolbooks may also be a good place to consider the economics of knowledge. Their calculations hint at print as an equalizing factor: a copy of Reisch in 1517 cost a day's wage for a builder—the highest-paid professor of medicine at Basel that year made little more [xxxi].

Cunningham and Kusukawa attend to a second field requiring workers when they place the production of this text against the backdrop of two late medieval communities that pursued pedagogy through writing and copying books. The first community was monastic. Gregor Reisch was a member of the Carthusian order, which was devoted to a rule mingling eremetical and cenobitic practices and committed to communal silence. Nevertheless, Reisch's path to monasticism began in the university. The year 1496 marked an important transition for Reisch: he graduated from the University of Freiburg; he entered the Carthusian order; he completed *The Philosophical Pearl*; and he apparently began editing the book with Johann Amerbach, a process which involved ongoing cooperation with the Carthusians at Freiburg, where the actual printing eventually took place. The second community to which Cunningham and Kusukawa draw attention is the Modern Devout, the lay communities of the Brothers and Sisters of the Common Life who exercised their spiritual calling through copying books, founding and running schools, and preaching throughout the Lowlands and down the Rhine. While the authors present this movement as a general measure of late medieval religiosity, they might have made a tighter material connection to the circle of humanists attracted to Amerbach's print shop in Basel, many of whom had been schooled in Paris, Strassburg, and Schlettstadt (now Sélestat) under teachers who admired the Modern Devout's example.

The Amerbach print shop and the fact that Reisch wrote *The Philosophical Pearl* during his studies at the relatively young University of Freiburg (est. 1460) signal the book's representative power and its popularity. This can partly be explained by the book's association with one of the leading, best-connected presses in northern Europe, and partly by how it pioneered the visual arrangement

and literary standards that would become standards for textbooks in the next two decades—the humanist Jacob Wimpheling recommended Reisch alongside other famous renaissance textbook writers: Jacques Lefèvre d'Étaples and Philip Melanchthon, compared to whom Rudolph Agricola was 'oversubtle' [xi n6].

The book's importance can also be explained by how effectively it repackaged an older genre. Reisch's Latin certainly did not match the classical eloquence popular in Italy at the time, even though he did choose the pedagogically winsome conceit of a dialogue. Yet this was not new. Writers of catechetical manuals had done this for centuries, and Lefèvre—whose students were teaching in Alsace by 1495—was the first, to my knowledge, to have rendered Aristotle's natural philosophy palatable to young minds in this way. As Cunningham and Kusukawa observe, it is misleading to consider this an 'encyclopedia'. For one thing, the word was not technically coined until around 1531; Reisch himself uses the terms 'epitome' and 'compendium' to describe the book [ix–x], words which were used to refer to a specific genre of natural philosophical texts that was popular by the 15th century [lix–lxvi]. Cunningham and Kusukawa might have observed that older compendia, which stretched back to Robert Grosseteste's *Summa naturalium* and included pseudo-Albertus Magnus' influential *Philosophia pauperum*, had circulated in manuscript since the 13th century.[2] By the 15th century, these could resemble small collections of basic *quaestiones*, such as Paul of Venice's *Sumule naturalium* (Milan, 1476).

All this is significant because Reisch addressed more disciplines than natural philosophy—the first seven books introduce the seven liberal arts. The introduction to this translation gives a small hint of what the missing parts are like by interpreting the captivating woodcuts which introduce the linguistic arts of the trivium and the mathematical arts of the quadrivium [xxxii–xlvi]. But the back story of medieval compendia and epitomes (the words seem interchangeable) highlights the merits of Cunningham and Kusukawa's choice to select all and only the natural philosophical parts of the *Philosophical Pearl*: this is the section which shows closest continuity with the medieval tradition as a unified genre. In the medieval books, the subjects were

[2] On Grosseteste, see Lewis 2003. Grabmann 1918 is still the most thorough introduction to pseudo-Albertus' text that I have found.

predictable, comprising most of Aristotle's *libri naturales*. The compendium would open with an introduction to Aristotle's *Physica* and close with Aristotle's *De anima*. In between, the books would cover the material of *De meteorologia*, *De generatione*, and so on, sometimes even including some of the *parva naturalia*, notably Aristotle's psychological works on sleep, dreams, and memory. Sometimes the book would be organized around *quaestiones*, sometimes around Aristotle's books, sometimes simply by topic. All this reflected, of course, variations on how to progress through the arts curriculum, with more or less (often less) rigor.

Given the medieval origins of the genre, the decision to translate the natural philosophical section of Reisch has more than enough defense. But there is an even better reason for this translation. As Cunningham and Kusukawa point out, historians have often used the term 'natural philosophy' imprecisely, referring vaguely to anything we might like to include as 'natural knowledge' [xlvi–xlvii]. This translation offers an example of what a curious student around 1503 might discover was 'natural philosophy', properly speaking. Because this translation can expect a wide readership, it is important that those readers understand that polemics may be at stake here. In 1995, Cunningham wrote, with Roger French, a book-length argument for identifying natural philosophy as something completely other than modern natural science [French and Cunningham 1995]. In a sense, this argument is nothing new: Koyré [1968] claimed that a quantitative turn differentiated modern science from everything before; Kuhn set a new disjunction in place with sociological distinctions between many sorts of scientific paradigms; and more recent work pushes the notion of 'modern' natural science ever later into the 19th century.[3] As the definition becomes more precise, apparently, 'science' narrows to something that only we, or immediate ancestors, do. But Cunningham and French argued for another basis for the difference between natural philosophy and modern science: religion. Modern science is secular. Therefore, whatever sort of philosophy includes religious presuppositions, such as Roger Bacon's commitment to light as the basis for his optics, is not modern science. The thesis

[3] The argument in Daston and Galison 2007 has been widely acclaimed.

has the merits of simplicity and clarity, if the demerits of tautology (how could modern science exist before modernity?).[4]

Perhaps, this is too simple, aside from the implicit assumption of the pristine objectivity of modern science—is that what non-religious means? The reason given for this definition of natural philosophy is that to assume some sort of continuity between natural philosophy and modern science is to practice whiggish presentism. *Quelle horreur*. The unintended consequence is that 'natural philosophy' becomes something very strange—dare I say it, an Other. That is, describing natural philosophy as something only *they* did, also homogenizes it. In the effort to be sensitive to historical distance, this approach valorizes what *we* (or our colleagues in the science department) do as 'real' knowledge. (Presentism lurks everywhere!) This anti-presentism presentism also obscures distinctions that were real to medieval and early modern thinkers, particularly the difference between matters 'secular' and matters 'religious.' Cunningham and Kusukawa helpfully remind us that for a medieval, 'secular' did not mean 'without God'. Nevertheless, from them one does not learn that the word also referred to scholars independent of the strictures of the theology faculty or an ecclesiastical rule. Natural philosophy was a 'secular' activity, even when done for 'religious' ends.

I fuss about this because the introduction tends to emphasize the theological coloring of Reisch's natural philosophy to the point of obscuring the 'natural' part. It is certainly useful to learn about the influence of Augustine's view of the Creator-creation distinction [lii–liv], his appropriation of *rationes seminales* [lvii–lviii], and the light-metaphysics of pseudo-Dionysius. It would also have been helpful to hear about the Aristotelian philosophy that such elements supported. As the translators point out, the epigraphs for books 8 and 9 depict the creation of Eve as narrated in *Genesis* 2. Natural philosophy in Reisch's world is a matter of Christian thought as much as it is Aristotelian. Once we have realized this, however, we have not learned everything there is to know about natural philosophy. Thankfully, there is now an edition to examine!

[4] The controversial nature of this discussion can be seen from Grant 1999.

To hint at the topical richness in this epitome, I shall take up the remainder of this review in a swift overview of Reisch's natural philosophy, with a secondary aim of suggesting how Reisch's exposition acknowledges a difference between natural and theological thinking.

In books 8–11 of *The Philosophical Pearl*, Reisch follows the pattern of Aristotelian philosophy that characterized the medieval textbooks outlined above. Book 8 ('On the Principles of Natural Things') addresses 'the natures of things'. The questions and terms with which the book deals come out of Aristotle's *Physica*. The mode of proceeding is fundamentally Aristotelian, working from a definition of prime matter ('*per se*, not *per accidens*') through a discussion of the four causes to a discussion of motion, natural and violent. Reisch addresses questions typical of late medieval physics such as the continuum, mentioning more advanced texts such as Oresme's *On the Uniformity and Difformity of Forms* [*Marg.* 8.8, 9]. Cunningham and Kusukawa point out in the introduction how frequently Reisch turns to biblical and patristic authors, particularly Augustine. Indeed the answers and definitions to questions and terms that Aristotle broached are framed and inspired by consultation of these authorities.

Two examples will suffice. After defining matter, the Pupil desires a definition of privation. Although Aristotle had not dealt with privation in the *Physica*, he did suggest that it was impossible to define at *Metaphysica* 7.3. Reisch's Master decides to expand on the basis of Augustine's teaching in *Contra manichaeos* that privation is a lack, just as darkness is not a thing, but a lack of light [*Marg.* 8.10, 32]. A second example concerns unpredictable marvels, those things that seem beyond nature's normal course. To address such things, Aristotle listed chance and fortune as among the causes in the second book of the *Physica*. Again, the Pupil demands more, 'for the common folk attribute much causality to these.' The Master admits that 'it is not good' to overlook chance and fortune, 'over which errors damnably occur' [*Marg.* 8.16, 43]. He proceeds to cite Augustine and Boethius to the effect that such causes are 'inimical' to Christian faith; but that, in any case, these authors point out that people tend to use 'chance' and 'fortune' simply to explain events whose causes are not immediately apparent: a lucky person is one who happens to have a good disposition [*Marg.* 13.17–18, 46]. The Pupil observes that ultimately since God governs all things, nothing can actually

happen by chance. Again citing Augustine's *City of God*, the Master observes that monsters and miracles must either come about by natural causes (hidden to observers) or be caused directly by God, sometimes through human or angelic agents—which, he hastens to add, God does in fact have the power to do. Is this theological speculation natural philosophy? At this point, the student recalls the original purpose of the conversation and the master resolves to avoid 'all digression' [*Marg.* 8.19–20, 47–51]. Unlike some pedagogical dialogues, the student plays an active role in Reisch's text. It may be worth considering whether the student is responsible for digressions on topics affected by, but not central to, natural philosophy.

This kind of argumentation, proceeding through Aristotelian topics while elucidating and arguing with examples and counterexamples from Christian authors, continues in book 9 ('On the Origin of Natural Things'). One merit of encountering this topic in such compressed form is that one gains a sense of the explanatory power of the simple elemental theory found in Aristotle's *De generatione et corruptione*, which covers a wide arrange of phenomena within a neat progression through mixed composites: first, the sublunary phenomena explained in the *Meteora*, including rain, dew, frost, thunderstorms, tides, earthquakes; then, the mixed composites of earthly minerals, which applies to a digression on the transmutation of metals; next, vegetation; then again, animals, including eggs developing into chicks and fish; and, finally, 'crawling and walking things,' of which the most significant is humanity. Reisch is compendious in both senses, briskly covering all this in 50 pages and also rounding out blind spots in Aristotle from the breadth of the Latin Christian tradition—using the biblical authors Job and David to describe the six stages of human life that Isidore of Seville had tabulated (himself probably using Augustine, who in turn got them from Cicero) [*Marg.* 9.42, 156].

The endpoint of natural philosophy, at least for the medieval curriculum, was what moderns will recognize as psychology; and this is the topic of books 10–11 of *The Philosophical Pearl*. A large part of the study of the soul was unproblematically defined as natural philosophy; since even plants possess organizational principles of life, they are animate. But while this was enough to explain most of the living natural world, two authorities blurred the definition of the last topic of psychology, the human intellect. First, *Genesis* 1

indicated that humanity had been made in the image of God; and the Christian theological tradition defined the soul, and specifically the intellect, as the seat of the *imago dei*. Second, Aristotle had conceded that the human soul is at least partly divine and the Neoplatonic tradition had made much of this. Following the usual progression through the disciplines, Reisch devoted book 12 to *divina*—the topic of Aristotle's *Metaphysica*, universal entities which, at least logically, are considered separately from matter. No wonder that Reisch's contemporaries followed their predecessors in debating whether the study of the intellectual soul should be part of a higher discipline or properly belonged within natural philosophy [Bakker 2007]. Reisch's own exposition follows the basic contours of Aristotle's *De anima*, which is divided between exposition of the vegetable and sensible soul [*De anima* 2; Reisch, 10] and the intellectual soul [*De anima* 3; Reisch, *Marg.* 11].

As with the other books of *The Philosophical Pearl*, it is not possible to present more than a sketch of Reisch's science of the soul. But the depth of the tradition in this area presented a couple of 'hot button' topics which provide perspective on Reisch's positions. In Aristotle's *De anima*, what connects the organic soul (the kind every living thing has) and the intellectual soul (possessed by higher animals, notably humans) is an analogy between sense and cognition. Like Aristotle, Reisch surveys the five senses, which are common to all animals. Also like Aristotle, and in tune with a chorus of medieval commentators, Reisch prioritizes the sense of sight [cf. *Marg.* 10.6, 173]. Vision is especially important for moving from the exterior senses to the kind of intellectual cognition that is distinctive to human beings: following the Albertist interpretation of Avicenna that seems to have dominated the later Middle Ages [Park 1980], the sense of sight provides not only a mere analogy for cognition but, more importantly, its basis. It works in this way. Sensations are taken up by the internal senses: they are organized by the common sense, stored in the memory, and recombined in the phantasy (imagination). Then, the intellect, acting in some way on the material provided by these internal senses, makes judgments, decisions, and turns to understanding or action. A question which divided some late medieval commentators was whether material images taken in by the senses, notably vision (*phantasmata*), were the very stuff with which the intellect did its business of thinking. Or did the intellect act

upon the images presented by the imagination and come up with its own sort of rarified, spiritual images (*species intelligibilis*)? Reisch's answer was conciliatory. He claimed that the phantasma is material but that the active intellect strips away non-essential material aspects from the sensible species [*Marg.* 10.3, 224].

But by this point, once one has left behind the last bit of materiality at the beginning of book 11, we can ask Reisch whether he has reached a border, if he is facing outward from the field of natural philosophy to make inroads into theology. His sources suggest that this is his goal. No longer does Reisch refer to Aristotle's *De anima* but increasingly to Augustine. This is not sleight of hand either, an effort to divinize what should properly be naturalized. The study of the soul ends in a study of epistemology: knowledge of the soul provides knowledge of cognition, the basis of how humans know. The first part of psychology is devoted to knowledge gained through the senses. But once one turns to the soul—the intellectual soul—one is paradoxically required to cognize cognizing. As the Pupil realizes, 'if we are unable to derive knowledge of corporeal and incorporeal substances by other means [than the soul], then our knowledge is diminished' [*Marg.* 9.6, 229]. How much less can one search out the knowledge of heaven? Reisch seems to suppose that Aristotle had an inkling of how to get out of the paradox: greatest certainty is about causes in themselves, farthest away from accidents connected to the senses, which only speak to how things happen 'for the most part'. The challenge is then to get beyond this to what Nicholas of Cusa, 'the very wise investigator of secrets', described in his *De docta ignorantia* [*Marg.* 9.6, 229–230]. By invoking Cusa, I would argue that Reisch is indicating the end of natural philosophy; he realizes that, in these farthest reaches of the human soul and its astonishing ability to reason, we have stretched the limit of natural philosophy and have entered the field of theology—or at least metaphysics.

Two lessons are to be learned from this exposition of Reisch. The first is that although Reisch enriches his introduction to natural philosophy with speculations and definitions from scripture, the Church Fathers, and Christian philosophers, he has a sense for the distinction between natural and nonnatural causes; moreover, the proper domain of natural philosophy is to understand—fully, with every available tool—these natural causes to their breaking point. The second lesson follows, and Reisch's Aristotelian language will help get to

the point. Even though theological language is *de facto* inseparable from natural philosophy, this does not mean that theology belongs to the *essence* of natural philosophy.[5] (Remember that, for Aristotle, disciplines like mathematics or psychology were about entities which were not *de facto* inseparable from matter, but nevertheless could be pursued as distinct disciplines in their own principled way.) In *Before Science*, French and Cunningham [1995, 242–274] made the strong claim that 'the true nature of natural philosophy' was 'religio-political' and not an 'objective "*scientific*" tradition of looking at nature'. Here the argument is somewhat attenuated:

> The fact that natural philosophy dealt with "the *created* world" [*sic*] more than anything else distinguishes it from modern natural science, for in the eyes of medieval philosophers the Creator was the Christian God, so natural philosophy dealt with God's handiwork. [xlviii]

It is indeed typical of medieval philosophers, perhaps especially in compendia, that they were eager to credit authorship of the book of nature, which gives their interpretation of that book a thoroughly distinctive texture. But for Reisch and other medievals, the essence of natural philosophy—its 'true nature'—was study of the book of nature in terms of natural causes, even if that study was motivated by how studying the book would result in praise of the author.

In the large, this interpretation is, like so many scholarly debates, a matter of emphasis, and one which Reisch's translators have shown their own eminent ability to nuance. My worry is that emphasizing natural philosophy's theological orientation as its sole distinctive characteristic will obscure the *differentiae* within medieval and renaissance natural philosophy, as well as under-represent the extent to which modern science also depends on socio-political motivations and assumptions. Faced with Reisch's exotic array of quotations and arguments taken from traditional Christian sources, a reader of this translation might be led to dismiss medieval natural philosophy as

[5] I use this example heuristically, without committing to a form of essentialism regarding historical objects, in order to highlight that things which are never actually found separate *in situ*, can yet be distinct in principle. In a response to Peter Dear's criticism [2001a] of their book, Cunningham [2001] accepted that he might be using essentialist language to define natural philosophy—and replied that this was no problem. See Dear 2001b for a further reply.

theology by another name. A subtler reading of Reisch himself will show that judgment misleading.

My worry should be relativized, since Reisch also agreed that substances are known by their accidents, a conundrum that the Aristotelian tradition never fully resolved. If the theological setting of natural philosophy is accidental to its essence, then it is crucial that we become well acquainted with this set of accidents in order to learn how to recognize medieval natural philosophy. In this enterprise, Cunningham's and Kusukawa's good translation and excellent introduction of this renaissance classic are a gift to scholarship of late medieval and early modern natural philosophy and should be prized by teachers of the period. One of the exciting things about this volume is that it allows precisely this sort of debate about what exactly natural philosophy encompasses. With access to this translation and its excellent bibliography on Gregor Reisch, scholars can rapidly consider Reisch as a representative of natural philosophy—and recommend Reisch to non-Latinate colleagues interested in a characteristic primer on medieval and early modern natural philosophy. The translation is solid. The translators represent Reisch in clear and literal translation, opting for faithfulness over fluidity (Reisch's Latin is fluent, but often not fluid). Many Latin words which are key technical terms in Reisch's vocabulary are included in brackets, *latine*. Although a facing original is always the most desirable, the translators note at least one online digitized edition, so a reader has quick access to the original—also helpful for considering the original presentation of the text. To Ashgate's credit, many original woodcuts are reproduced in this translation, which is accompanied by a thorough index and a collection of topical outlines of the text. At least one heading has been added silently (i.e., 'Peroration' on page 15). The subdivision 'tractatus' is translated as 'tract' in book 1, while in book 10 it is translated as 'treatise' [157ff]; so far as I can tell, translation inconsistencies are minor.

This text is a boon to teachers of medieval and renaissance philosophy and history of science, though the book's price will mean that assignment will be at the mercy of Ashgate's policy for granting permission to photocopy. Despite our realization of how deeply Aristotle was implicit in intellectual life from the 12th through the 17th centuries, that fact makes it far too tempting to provide surveys of medieval philosophy from the perspective of reading Aristotle

himself. That is an excellent start, of course—and anyone who has taught this will be aware of the pedagogical challenge that it is to turn high-school classical physicists into Aristotelians. (What are the distinctions between first principles, and a formal and final cause?) But the challenge only grows with teaching medieval Aristotelianism. The Latin Aristotle was by no means our Aristotle, and not only because of the distinctive lenses offered by medieval translators mostly working from Neoplatonized Arabic editions. The concepts that Aristotle offered gained meaning and nuance from what Augustine had said, along with a host of commentators. This conglomeration of antique wisdom is what allowed natural philosophy such elasticity, and which gives medieval and renaissance Aristotelianism such a different texture from Aristotle himself. While experts in the field will not be surprised by the eclectic use of authority on every possible topic, this texture is hard to convey to students without strong examples. Gregor Reisch wrote this dialogue using simple-to-follow language in order to introduce students to the basics of science in his day, and it may prove to be a superb introduction for today's history of science classroom as well.

BIBLIOGRAPHY

Bakker, P. J. J. M. 2007. 'Natural Philosophy, Metaphysics, or Something in Between? Agostino Nifo, Pietro Pomponazzi, and Marcantonio Genua on the Nature and Place of the Science of the Soul'. Pp. 151–178 in P. J. J. M. Bakker and J. M. M. H. Thijssen edd. *Mind, Cognition and Representation: The Tradition of Commentaries on Aristotle's* De anima. Burlington, VT.

Campi, E.; De Angelis, S.; and Goeing, A.-S. 2008. edd. *Scholarly Knowledge: Textbooks in Early Modern Europe.* Geneva.

Cunningham, A. 2001. 'A Reply to Peter Dear's "Religion, Science and Natural Philosophy: Thoughts on Cunningham's Thesis"'. *Studies in History and Philosophy of Science* 32:387–391.

Daston, L. and Galison, P. 2007. *Objectivity.* New York.

Dear, P. 2001a. 'Religion, Science and Natural Philosophy: Thoughts on Cunningham's Thesis'. *Studies in History and Philosophy of Science* 32:377–386.

Dear, P. 2001b. 'Reply to Andrew Cunningham'. *Studies in History and Philosophy of Science* 32:393–395.

Eisenstein, E. 1979. *The Printing Press as an Agent of Change: Communications and Cultural Transformations in Early-Modern Europe*. 2 vols. Cambridge.

French, R. K. and Cunningham, A. 1995. *Before Science: The Invention of the Friars' Natural Philosophy*. Aldershot, UK/Brookfield, VT.

Grabmann, M. 1918. *Die philosophia pauperum und Ihr Verfasser Albert von Orlamünde*. Münster.

Grant, E. 1999. 'God, Science, and Natural Philosophy in the Late Middle Ages'. Pp. 243–267 in L. Nauta and A. Vanderjagt edd. *Between Demonstration and Imagination: Essays in the History of Science and Philosophy Presented to John D. North*. Leiden.

Johns, A. 1998. *The Nature of the Book: Print and Knowledge in the Making*. Chicago.

Koyré, A. 1968. *Metaphysics and Measurement: Essays in Scientific Revolution*. Cambridge, MA.

Kuhn, T. S. 1970. *The Structure of Scientific Revolutions*. 2nd edn. Chicago.

Lewis, N. 2003. 'Robert Grosseteste's Notes on the Physics'. Pp. 103–134 in J. W. Goering and E. A. Mackie edd. *Editing Robert Grosseteste: Papers Given at the Thirty-Sixth Annual Conference on Editorial Problems, University of Toronto, 3–4 November 2000*. Toronto.

Park, K. 1980. 'Albert's Influence on Late Medieval Psychology'. Pp. 501–535 in J. A. Weisheipl ed. *Albertus Magnus and the Natural Sciences*. Toronto.

Schmitt, C. B. 1988. 'The Rise of the Philosophical Textbook'. Pp. 792–804 in C. B. Schmitt *et al.* edd. *The Cambridge History of Renaissance Philosophy*. Cambridge.

In the Path of the Moon: Babylonian Celestial Divination and Its Legacy by Francesca Rochberg

Studies in Ancient Magic and Divination 6. Leiden/Boston: Brill, 2010. Pp. xxii + 445. ISBN 90–04–18389–2. Cloth € 152.00, $216.00

Reviewed by
Lorenzo Verderame
"Sapienza" Università di Roma
lorenzo.verderame@uniroma1.it

The Babylonian tradition related to the observation and explanation of celestial phenomena has seen varying attention over the years. These texts were among the first cuneiform documents to attract the attention of scholars at the dawn of the discipline. But the ambiguous nature of the documents, qualifying in both the modern and opposite categories of 'astrological' and 'astronomical', often embarrassed the editors of the texts and left the entire Babylonian tradition of celestial observation poised precariously between attention and neglect. While the astronomical texts and their data were a source of interest attracting scholars from other disciplines as well as independent researchers, the astrological documents were cast into the cauldron of the superstitious, together with the rest of the divinatory and magical texts. On the one hand, the astronomical knowledge and achievement of Babylonia was recognized as the precursor of the so-called Greek miracle; and, on the other, the astrological tradition was interpreted as the heavy burden of the Oriental immobility.

Francesca Rochberg has devoted her scholarly research to the study of Babylonian celestial observation as a unique and homogenous tradition. Working on an Assyriological ground as well as in the history of astronomy and astrology, she has fixed the boundaries between the two spheres of Babylonian astronomy and astrology, domains that were separated more through modern approaches than real emic categories (that is, using terms meaningful within the domains) and has highlighted the relations with other cultures and later traditions.

The volume under review collects her most important essays as chapters arranged chronologically according to their publication

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 162–169

date, from 1982 to 2010.[1] These studies deal with the main aspects of the Babylonian celestial observation, from the constitution and transmission of the corpora of divinatory texts to their relation with other traditions, focusing particularly on the history of ideas. Most of the subjects have been summarized and discussed in Rochberg's recent monograph [2004].

The book opens with a study of the concept of determination in the Akkadian sources in the light of Greek philosophy and Latin *fātum* [ch. 1: 'Fate and Divination in Mesopotamia']. The author focuses her attention on the Akkadian term 'šīmtu', for which the general translation 'fate', a term borrowed by modern European languages from Latin, is revealed to be inadequate. This topic is resumed in later chapters where she investigates the concept of causality in relation to divine will, as well as the conditional sentences which constitute the basic structure of Mesopotamian divination.[2] Two more studies are devoted to the socio-religious background of Mesopotamian divination. The relation of divinity to the sky and the gods conceived as celestial bodies is the main topic of 'The Heavens and the Gods in Ancient Mesopotamia' [ch. 16]; while 'A Short History of the Waters above the Firmament' [ch. 17] deals with the tradition of the waters above the sky, from the well known passage of *Genesis* 1.6–8 to the Renaissance interpretation through the Medieval tradition.

The history of astrology, particularly the relation of the Babylonian tradition to others, is the core of Rochberg's researches. Various articles are devoted to this topic, which is, however, constantly present in other subject studies too. Two papers deal with the Babylonian elements in Hellenistic astrology—'New Evidence for the History of Astrology' [ch. 2] and 'Elements of the Babylonian Contribution to Hellenistic Astrology' [ch. 7], while the author discusses in three separate papers some basic astronomical concepts found in horoscopes: 'Babylonian Seasonal Hours' [ch. 8]; 'Babylonian Horoscopy: The Texts and their Relations' [ch. 9]; and 'Lunar Data in Babylonian

[1] As of 31 Dec 2011, three of the essays [chs 16, 18, and 21] are still in press.
[2] Cf. 'Conditionals, Inference, and Possibility in Ancient Mesopotamian Science' [ch. 19]; '"If P, then Q": Form and Reasoning in Babylonian Divination' [ch. 20]; 'Divine Causality and Babylonian Divination' [ch. 21].

Horoscopes' [ch. 13].[3] The Seleucid text from Uruk, TCL 6.13, offers several considerations in light of Greek astrological doctrine. The edition of the text [ch. 5: 'TCL 6 13: Mixed Traditions in Late Babylonian Astrology'] is followed by a discussion on the term 'riksu' (Sumerian DUR: 'bond'), which in an astronomical context might be translated as 'node'.[4] The same text offers a Babylonian parallel to the later association between planets and sections of the zodiacal signs [ch. 6 'Benefic and Malefic Planets in Babylonian Astrology']. Further parallels to Greek astrology are proposed in 'A Babylonian Rising Times Scheme in Non-Tabular Astronomical Texts' [ch. 14], where Rochberg discusses 'the concept of the rising times of the twelve consecutive 30° signs of the zodiac, the Greek ἀναφοραί'. The general question of periodicity in Babylonian theory is treated in 'Periodicities and Period Relations in Babylonian Celestial Sciences' [ch. 18]. An attempt to sound other traditions is made in 'The Babylonian Origins of the Mandaean Book of the Zodiac' [ch. 11], discussed below.

Three essays are devoted to the constitution of the astrological written tradition, and are now classics in the study of Mesopotamian celestial observation and divination. In 'Canonicity in Cuneiform Texts' [ch. 3], the author delineates the traits of authorship and composition of the so-called canonical Series (*iškāru*) in opposition to the *aḫû* tradition. These topics are treated in depth in two successive studies. In 'The Assumed 29th Aḫû Tablet of Enūma Anu Enlil' [ch. 4], the edition of a text belonging to the *aḫû* tradition is an occasion to discuss the origin and nature of the exegetical literature and its relation with the canonical Series. The divine authorship and the literary origin attributed to the Series are discussed in 'Continuity and Change in Omen Literature' [ch. 10].

In a further study, the author goes back to the origin of this written tradition. In 'Old Babylonian Celestial Divination' [ch. 15], Rochberg gives a first glimpse on the Old Babylonian astrological texts and offers some general considerations in the light of other contemporary corpora, i.e., the hepatoscopic series. This constitutes the first step of a desirable study, whose interest the author has already declared in earlier works [71].

---

[3] See also Rochberg 1998, and Beaulieu and Rochberg 1996.

[4] The topic has been recently discussed by Ross [2008], who proposes a parallel with the Demotic 'twr'.

These 21 essays testify to the efforts and achievements of Rochberg's research in Babylonian knowledge and interpretation of celestial phenomena. In the field of Assyriology, she has promoted the study and understanding of Mesopotamian divination, a topic that the experts of the discipline are often unable to contextualize;[5] with her critiques of methodology and approach, she has highlighted the position and relevance of Babylonian divination in the history of science [cf. 2004, chs 1–2]. It is impossible to offer a homogenous review of all the topics treated by the Rochberg in less than 30 years. Furthermore, such a review would have the uncomfortable duty of evaluating indirectly her entire career as a scholar. So instead I will propose few observations suggested by her studies.

Several references support the conclusion made by Goody and Watt [1975, 68] that writing is 'an addition, not an alternative, to oral transmission'. In the study of the Mesopotamian civilizations, the written documents are the only witnesses to a culture in which the oral communication represented the main stream of tradition. Traces of orality appear like the tip of the iceberg in the written sources and, in some cases, they highlight the preeminence of the oral medium. In the Sumerian tradition, the sphere of knowledge is clearly related to orality. The organ of perception related to wisdom is *par excellence* the ear, not the eye. The term for knowledge, and relative verbs, is 'ĝeštu', which means 'ear' too. The expression 'big/wide ear' (ĝeštu-dagal) might be translated as 'wide understanding' and used as a title for a 'wise man'. This view is strictly related to the divine election and submission of the worshipper, i.e., the wise man is the one who understands and obeys the gods' orders as manifested through signs. This appears different when compared to later Mesopotamian cultures in which written media are preeminent; but still in the first millennium BC, the expression 'ša pî ummâni' ('according to/from the mouth of the master'), used to name works of exegesis, highlights the importance of oral transmission. So too the Report of Bēl-aḫḫē-erība, who adds to a quoted omen 'I have heard (that) [from the mou]th of my father' [SAA 8.454]. This passage parallels *Enūma eliš* 7.147, 'A father should repeat them and teach them to his son' (*li-šá-an-ni-ma a-bu ma-ri li-šá-hi-iz*), where the term for teaching means literally 'to make someone to memorize' (*aḫāzu*). Listening and memorizing

[5] For example, see Bottéro 1982.

are the core of the learning process and the correct way to wisdom. *The Epic of Erra* emphasizes it in the closing verses:

> The scribe who will memorize it, shall be spared in the enemy country and honored in his land, in the chapel of the masters (*ummânū*)—where they constantly invoke my name, I shall grant them understanding. [*Epic of Erra* 5.55–56]

In the last sentence, the expression 'I shall grant them understanding' means literally 'I will open their ears' and this takes us back to the concept of knowledge as enlightenment by the god through orality. Consequently, it is not surprising that the major achievement is not considered as the product of personal experience but as divine revelation. This is the origin of most of the Series and literary works in Mesopotamia as well as in other cultures [74].

The passages mentioned above underline the direct relation of the pupil to his master as the source of knowledge. This finds an echo in the complaint of a scholar (Tabnî) to the Assyrian crown prince:

> Moreover, (whereas) [Aplāj]a and Nāṣiru have kept [in] their [hands] non-ca[nonical] tablets and [...s] of every possible kind, I have learned (my craft) from my (own) father. [SAA 10.182: r. 24ff.]

In Tabnî's statement, the written documents are clearly undervalued as a source of learning. In this case, Tabnî compares his learning (*lamādu*) from his father's hand, i.e., from observing his father's work, to that of his colleagues who have kept tablets in their own hand—tablets which were non-canonical too! The text opposes a correct way of learning and transmitting the knowledge, that is, directly, to an incorrect one, that is, indirectly through the written medium which substitutes for the master. The established place for learning and transmitting knowledge is within the family. Ašarēdu the Younger affirms, in fact, that 'The scribal art is not heard about in the market place' [SAA 8.338: 7–r.1], a clear *querelle* against the selling and diffusion of knowledge out of the established contexts. Moreover, the denunciation to the king of the activities of the goldsmith Parruṭu show the strict control over this matter:

> Parruṭu, a goldsmith of the household of the queen, has, like the king and the crown prince, bought a Babylonian, and settled him in his own house. He has taught exorcistic literature to his son; extispicy omens have been explained to him,

> (and) he has even studied gleanings from *Enūma Anu Enlil*, and this right before the king, my lord! [SAA 16.65]

In 'Scribes and Scholars: The ṭupšar Enūma Anu Enlil' [ch. 12], Rochberg analyzes the figure of the 'scribe of the *Enūma Anu Enlil*', the canonical Series of celestial omens [cf. 2004, ch. 6]. She tries to define the role and relative competences of this figure through the analysis of sources from the first millennium BC, focusing on the Neo-Babylonian and later periods.

The first point to discuss concerns terminology and relates to the use of the terms 'title' and 'profession' as they serve in the context of ancient cultures. In general—and uncritically—'title' is used to indicate functions of variable duration that are attributed by superior authorities; 'profession', on the contrary, seems to refer to the basic processes of production and their representatives. This is not the place to discuss this matter in depth. To discuss the term 'profession' in Mesopotamian contexts, especially those related to the scribal sphere, we may consider the curriculum or apprenticeship of the professional, on the one hand, and the professional's activity itself along with its sphere of competences, on the other [see Verderame 2008]. An analysis of the Neo-Assyrian sources, which offer a wide range of different types of documents, shows how the boundaries among professions are fictions. The long scholarly discussion on the *āšipu* (exorcist) and the *asû* (doctor, physician, herbalist) is a clear example. Part of this confusion of professions might be explained by the breakup of the traditional direct transmission of knowledge within families due to the increased reliance on writing. In the Assyrian royal court, parvenus and isolated scholars appeared alongside such traditional scribal families as that of Nabû-zuqup-kēnu [Verderame 2008]. One observation is that the apprenticeship, competences, and activity of these 'professions' were anything but fixed. The reconstruction of the curriculum of the members of the Nabû-zuqup-kēnu family, made possible through the colophons of the tablets that they copied in the successive phases of their learning process, shows a wide range of competences going far beyond those specific to their discipline. The same is true of the curriculum of the 20 scholars introduced to the Assyrian king by Marduk-šapik-zēri [SAA 10.160]. The Neo-Assyrian sources document an intense activity of celestial observation in form of letters and reports sent to the king. The authors of these documents belong to different disciplines and professions.

Among them, the *ṭupšar Enūma Anu Enlil* or the *ṭupšarru*, if we accept the latter as an abbreviation of the former, are few. In the light of the sources, 'ṭupšar Enūma Anu Enlil' appears to be a later title, more than an independent consolidated 'profession', as the *bārû*, for example [Parpola 1993; Pearce-Doty 2000; Verderame 2004, 7–9].

Rochberg has constantly paid attention to the transmission of Mesopotamian knowledge and the relation with later traditions, particularly in the Hellenistic world. She has devoted a single article to other Near Eastern traditions, i.e., the Mandaean. These traditions, however, constitute a field of research yet unsounded which will yield interesting results in the future.[6] The Aramaic world has been the direct heir of the Babylonian knowledge and the vector through which this has been transmitted, on the one hand, to the Western world, on the other hand, within the Ancient Near East. Traces are scattered among earlier documents [Greenfield-Sokoloff 1989], but they can be detected in later traditions, for example, in the Syriac literature. It is hard not to relate the Syriac treatises of the Vatican library discussed by G. Furlani [1948] to the lunar eclipse section of *Enūma Anu Enlil* [Rochberg-Halton 1988], of which the former resumes the structure and the same content.

When the collected works of a scholar are published as a book, the first question that arises is why? Often the most representative articles are collected as a tribute to their career. The case of Francesca Rochberg is rather different. The collected essays in this volume, successive steps in an ordered path, constitute an invaluable contribution to a better understanding of Babylonian divination.

BIBLIOGRAPHY

SAA State Archives of Assyria. Simo Parpola ed. Helsinki. 1997–.

TCL Textes Cunéiformes du Louvre. Paris. 1910–.

Beaulieu, P.-A. and Rochberg, F. 1996. *The Horoscope of Anu-bēlšunu. Journal of Cuneiform Studies* 48:89–94.

[6] G. Furlani dedicated part of his researches to divination in Babylonia and later Semitic traditions; a complete list of his works is collected in S. Furlani 1957.

Bottéro, J. 1982. 'Eloge d'une science inutile. L'orientalisme'. *Akkadica* 30:12–26.

Furlani, G. 1948. 'Tre trattati astrologici siriaci sulle eclissi solare e lunare'. *Rendiconti Accademia Nazionale dei Lincei* 8:569–606.

Furlani, S. 1957. 'Bibliografia degli scritti di G. F. dal 1914 a tutto il 1956'. *Rivista degli Studi Orientali* 32:xiii–xxxvii.

Goody, J. R. - I. Watt. 1975. 'The Consequences of Literacy'. Pp. 27–68 in J. R. Goody, *Literacy in Traditional Societies*. Cambridge.

Greenfield, J. C. - M. Sokoloff. 1989. 'Astrological and Related Omen Texts in Jewish Palestinian Aramaic'. *Journal of Near Eastern Studies* 48:201–214.

Parpola, S. 1993. 'Mesopotamian Astrology and Astronomy as Domains of the Mesopotamian "Wisdom" '. Pp. 47–59 in H. D. Galter ed. *Die Rolle der Astronomie*. Graz.

Pearce, L. E. and Doty, L. T. 2000. 'The Activities of Anu-belšunu, Seleucid Scribe'. Pp. 331–341 in J. Marzahn and H. Neumann edd. *Assyriologica et Semitica. Festschrift für Joachim Oelsner anläßlich seines 65. Geburtstages am 18. Februar 1997*. Münster.

Rochberg-Halton, F. 1988. *Aspects of Babylonian Celestial Divination: The Lunar Eclipse Tablets of Enūma Anu Enlil*. Archiv für Orientforschung Beiheft 22. Horn.

Rochberg, F. 1998. *Babylonian Horoscopes*. Transactions of the American Philosophical Society 88.1. Philadelphia.

_____ 2004. *The Heavenly Writing: Divination, Horoscopy, and Astronomy in Mesopotamian Culture*. Cambridge/New York.

Ross, M. 2008. 'All's DUR That Ends Twr'. Pp. 245–255 in M. Ross ed. *From the Banks of the Euphrates: Studies in Honor of Alice Louise Slotsky*. Winona Lake.

Verderame, L. 2004. *Il ruolo degli 'esperti' (ummânu) nel periodo neo-assiro*. PhD. Dissertation, Università di Roma "La Sapienza". Rome.

_____ 2008. 'La formazione dell'esperto (ummânu) nel periodo neo-assiro'. *Historiae* 5:51–67.

Eratosthenes' Geography*: Fragments Collected and Translated with Commentary and Additional Material* by Duane W. Roller

Princeton/Oxford: Princeton University Press, 2010. Pp. xvi + 304. ISBN 978–0–691–14267–8. Cloth $49.50

Reviewed by
Georgia L. Irby
College of William and Mary
glirby@wm.edu

Roller's *Eratosthenes'* Geography is the first comprehensive treatment of Eratosthenes' *Geographica* since R. M. Bentham's largely inaccessible, unpublished PhD thesis, *The Fragments of Eratosthenes* [1948]. Two earlier German editions exist: Bernhardy 1880 and Seidel 1789. Roller's assessment of the *Geographica* is balanced and synoptic, and it relies on the best of current and earlier scholarship. Asserting that 'Eratosthenes' world overflowed with geographical data' [10], Roller brings together that data and contextualizes it within the intellectual settings of both Eratosthenes' Hellenistic Alexandria and the academic *milieux* of later extractors.

Contributing usefully to current Eratosthenes scholarship,[1] the present volume falls into three parts:

(1) introduction,
(2) translation of the sources, and
(3) commentary.

The introduction is a must-read for anyone interested either in the history of Greek geography or in intellectualism in the Hellenistic Age. The first part, 'Eratosthenes and the History of Geography', surveys the history of Greek geography to Eratosthenes' day, including theoretical and practical initiatives: Necho II and others who attempted to circumnavigate Africa; Anaximander, the first to theorize about the shape of the Earth; Hecataeus, 'probably the first to

[1] Recent scholarship on Eratosthenes' geographical studies has concentrated primarily on the measurement of the Earth: see Cimino 1982, Rawlings 1982, Dutka 1993–1994, and Geus 2004. More broadly, see Aujac 1998, Geus 2002, and Shcheglov 2004.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 170–178

see the world in terms of continents' [3]; Herodotus; Eudoxus, who divided the world into zones of latitude; Ephorus, who laid the foundations for the scholarly discipline of geography [6]; Alexander of Macedon and Pytheas, both contributing to the accretion of topographical data; Aristotle, who recorded the first extant estimate of the Earth's circumference, 400,000 stadia; Dicaearchus, who established the main terrestrial parallel and speculated about the effects of topography on the overall shape of the Earth; and Strato, whose theories about the formation of the seas would shape subsequent Greek geographical theory.

Valuable to our understanding of the *Geographica* is Roller's *précis* of Eratosthenes' career [7–15]. His education at Athens emphasized philosophy and, to a lesser extent, mathematics and philology. Called to serve as Librarian at Alexandria and royal tutor, Eratosthenes earned a reputation as a 'broad scholar and creative personality' [12]. Although his publications on philosophy and mathematics were largely derivative, his poetry, in the tradition of Callimachus, was admired. Eratosthenes' literary *éclat*, together with his reputation for broad learning, 'certainly played a role in his appointment as Librarian' [12] when he was called to replace another poet, Apollonius of Rhodes, whose thick style was less in favor with the Ptolemies. Roller, in fact, is sensitive to Eratosthenes' poetical predilections [21, 113, 115] and thereby helps to put Strabo's criticisms into perspective. It is well known that Eratosthenes composed a versified proof of how to double the cube in commemoration of his appointment as Librarian and to honor the regime. Eratosthenes' first geographical work, significantly, was the poem *Hermes*, recounting the god's youth and including a Platonic description of the universe and account of the terrestrial zones [see Geus 2002, 110–128]. Roller, hence, emphasizes how Eratosthenes' training in poetry permeated his scholarship in other areas. Contributing to Eratosthenes' scholarship in geography are his background in philosophy and mathematics, his access to the best of ancient and contemporary books at the Library, the recent augmentation of geographical knowledge from Alexander's exploits, as well as the scholar's own geographical *milieu*. Eratosthenes hailed from Cyrene in Egypt, 'at one end of the Greek world' [10], which had a particular role in shaping his geographical outlook and expanding his geographical knowledge.

Roller then describes the contents of the *Geographica*'s three books. The first book treated the history of geography from the time of Homer. Strabo's extractions disproportionately represent Homeric questions, since he perceived Eratosthenes' treatment of Homer as disrespectful. In Eratosthenes' overview of scientific geographical authors, Roller sees hints of literary authors as well (including Aeschylus and Callimachus [119–121: F8]), again bringing attention to Eratosthenes' training in philology. Eratosthenes, furthermore, following Strato of Lampsacus and Xanthus of Lydia, speculated on the shape and formative processes of the Earth, with particular attention to inland marine phenomena—a practice well-established in Greek intellectualism from Xenophanes (whom Roller cites only for his views on Homer)—and the effects of littoral silting. Strabo makes clear that book 1 ended with a discussion of fabricated geography but 'the extant fragments are tangled with Strabo's own interpretations and prejudices' [22]. Roller concludes that, where Eratosthenes likely emphasized the travels of Heracles and Dionysus, Strabo condemned as fantastical that geography which Eratosthenes took as reliable, particularly the account of the Atlantic related by Pytheas of Massalia.

Book 2 covered Eratosthenes' theories about the shape of the Earth and the inhabited world. The precise contents and arrangement of the book cannot be known, as it is difficult to extricate the mathematical material that may have appeared here from Eratosthenes' *On Measurement of Earth* [see also Bowen 2003], a mathematically simplified *précis* of which may have been included in *Geog.* 2. Acknowledging the problem, Roller concludes that the passages citing toponyms and topographical data, which could just as reasonably be included in book 3, are also necessary in setting for the stage for Eratosthenes' view of the extent of the world [24]: the attribution of some passages is, simply, dubious. Such, unfortunately, is the nature of a collection of fragments of a prolific author.

In the third book, Eratosthenes described the topography of the inhabited world. To this book can be attributed most of the extant fragments, and the topographical information contained therein was considered useful. From Hecataeus onward, geographical accounts proceeded clockwise from the Pillars of Heracles. Eratosthenes, however, broke from this pattern, proceeding from the east to the west.

Roller explains this nonconformity as 'perhaps reflecting the contemporary obsession with India' [24]. Also in accord with the new attitudes of early Hellenistic world, Eratosthenes emphasized locales not ethnicities [see Geus 2005, 243–244]. The book opens with Eratosthenes' paradigm of the world, including two cardinal baselines (east-west, viz. Pillars of Heracles to India, and south-north, viz. Meroe to Thule), major parallels and meridians (rarely straight lines, as Eratosthenes well knew), and an attempt to divide the landmasses into tidy geometrical shapes or σφραγίδες (seal stones), a term applied only to eastern landmasses (India, Ariana, Mesopotamia, and Egypt) and eschewed by later geographers. The particularly comprehensive representation of India in the extant fragments reflects perhaps not only the interests of Eratosthenes' day with the influx of geographical knowledge under Alexander, but also of Strabo's when Augustus attempted to strengthen trade between Rome and India, Roller argues. Nowhere else in the fragments is there apparent detailed source analysis or examination of land and sea routes. Strabo's summaries of Eratosthenes' accounts of India, Ariana, Mesopotamia, and Egypt include topographical, ethnographical, and historical details with primary emphasis on the boundaries (as in Eratosthenes). With Egypt, Eratosthenes abandoned the model of the σφραγίδες in favor of the current vision of Africa as a whole.

Eratosthenes' account then proceeded to the northern Mediterranean. The extant fragments describing the Caspian and Black Seas are strictly geographical: ethnography is lacking and the fragments resemble sailing itineraries. With the north coast of the Mediterranean (Europe), we come to the area where the most geographical advances had been made between the times of Eratosthenes and Strabo. Here Strabo is particularly critical, especially regarding Eratosthenes' discussion of western Mediterranean which depended heavily upon Pytheas, whose journeys were deemed fabricated and absurd by most of his successors, Strabo among them. The *Geographica* ends with discussion of virtue and ethnicity. Eratosthenes, reflecting Alexander's own rejection of the traditional division between (virtuous) Greeks and (non-virtuous) non-Greeks, favored individual virtue over the holistic virtue of an ethnic group.

Roller then explicates Eratosthenes' method, approach, and use of sources. In the extant fragments, over 20 persons are cited by name, mostly authors contemporary with or postdating Alexander

[16–20]. Other sources may have included unnamed sailors and still others may have been lost through the especially complex process of textual recension. Roller deals head-on with the question of autopsy, which continues to baffle modern scholars and popularizers.[2] As Librarian at Alexandria, Eratosthenes had access to perhaps every book written on geography plus the eyewitness accounts of sailors and merchants traveling through one of the world's busiest port towns. 'Unlike Herodotus, Eratosthenes, who worked in the world's finest library, was not interested in fieldwork' [17].

Finally, Roller discusses the reception and later history of the *Geographica*. Employed extensively in antiquity by hostile authors, especially Hipparchus in his *Against the Geography of Eratosthenes*, the *Geographica* was a major geographical source for Strabo, who generally defended Eratosthenes against Hipparchus' often unfair criticisms. In his own day, Eratosthenes was admired primarily as philologist and poet but he is best known today as the originator of the discipline of geography. In antiquity, because of Rome's expansion, Eratosthenes' treatise was quickly made obsolete and broadly criticized. Although used extensively by Strabo and cited by Pliny as a foreign authority in his own geographical books, the text seems to have become rare already by the first century AD. Eratosthenes fails to merit a mention by name in the geographical writers Pomponius Mela and Ptolemy.[3] Equally surprisingly, neither the polymath Plutarch nor the encyclopedist Athenaeus cite Eratosthenes by name [33].

In the second part, 'Eratosthenes, *Geographica*', Roller offers a clear, faithful, and readable translation of the fragments. Relying largely on Berger's collection of fragments, Roller contextualizes the shorter, isolated fragments in efforts to restore them to completeness in so far as this is possible [see e.g., Roller's FF2, 6, 8, 10, 13–16, 34,

[2] Despite the utter lack of evidence, popularizers continue to insist that Eratosthenes personally inspected the well in Syene when calculating the circumference of the Earth [see Bertman 2010, 119–20]. Eratosthenes himself was aware that his measurements were at best approximations: see Strabo, *Geog.* 2.77–78, 80–82, 86, 89, 91–92; Dicks 1960, 31. A greater degree of accuracy was attainable in Greek mathematics than in ancient Greek geography. Strabo, in turn, accused Hipparchus of manufacturing evidence; Dicks [1960, 130–137] defends most of Hipparchus' calculations.

[3] It baffles this reviewer that Ptolemy would have been unaware of Eratosthenes, but the text may have already been lost by Ptolemy's day.

49, 51, 52, 59, 60, 63, 64, 66, 78, 108, 133]. Extractors occasionally mention Eratosthenes by name; but most ancient authors, including Eratosthenes and his redactors, frequently engaged with a range of humble to prominent and authoritative unnamed sources. Roller also reorders where prudent but usually without comment. The Greek text is omitted but full citations to Strabo (and other authors) are provided. Un-Greeked readers will find the primary texts inaccessible. The expert reader will likely find it more profitable to consult Strabo directly in the context of his larger narrative.

Although in the third part, 'Summaries and Commentaries', the commentary is presented separately from the translations, the exegesis of each fragment is prefaced with a summary intended to aid the reader in pulling 'Eratosthenes' thoughts out of such tangles' from Strabo [36]. The greatest challenge in any work on a fragmentary author is extricating the source from redactor. The challenge is further exacerbated in Strabo, our primary source for Eratosthenes from whom over 90% of the fragments derive. Strabo was a highly elliptical writer whose treatment of his own sources was far from linear. He rarely quoted directly or even paraphrased his sources but instead offered synthetic arguments of materials collated from multiple sources. And this procedure of ellipsis and synthesis easily invites confusion: for example, there is no evidence in the extant fragments for maps in the modern sense—the fragments include no words like πίναξ [21]—yet Strabo 2.1.2 implies that Eratosthenes dealt with pictorial maps (πίνακα). Nor did Strabo always cite his sources by name.

> Especially in the case of the information preserved by Strabo, it is not always possible to identify the particular source. One must make a careful path between too narrow a choice and too broad. Mention of Eratosthenes by name has always been a valuable criterion but it is not an absolute one, especially in the case of Strabo's many verbs without subjects. [36]

It is, thus, as Roller observes, sometimes 'impossible to separate out the actual thoughts of Eratosthenes from Strabo's often lengthy reanalyses' [37]. And here is where some may disagree with Roller's conclusions. Which of the fragments are genuinely Eratosthenian and which are Strabonian? Roller remains alert to this challenge and his efforts to disentangle Eratosthenes from the complexities of Strabo's layered narrative shed valuable insights also into Strabo's style,

methodology, and his use of sources [see esp. 122]. Roller reminds the reader that Strabo's chronology allows for specialized geographical knowledge from a Roman point of view, anathema to Eratosthenes who was aware of Rome merely as a place.

In short, Roller's carefully documented commentary is replete with fascinating nuggets. Roller engages with the text, remarking on matters of broad intellectual interest, including philology, history, ethnography, the intellectual *milieu*, and the philosophy and history of geography. For example, when Strabo discusses the size of the Earth and the extent of its inhabited parts, he synthesizes arguments from several sources: notably, when referring to the remote Βρεττανική, he shifts from the Roman spelling to the rare « Πρεττανική » [135: F34]. The change in orthography strongly suggests that Strabo has shifted from a Roman source, ceased his editorializing, and has returned to Eratosthenes, who in turn is quoting directly from Pytheas. Furthermore, Strabo is astonished that Eratosthenes would disagree with Archimedes on matters mathematical [132: F16]: for example, contrary to Archimedes who sees the Mediterranean as a single even surface, Eratosthenes argues that the Internal Sea (Mediterranean) is not constituted as a single surface but rather that its level is higher in some places, e.g., the Corinthian Gulf at Kenchraei where a proposed canal would have submerged nearby islands and disrupted sailing passages. Roller gives a history of the canals through the Corinthian isthmus, confirms the reports of ancient engineers, and surmises that Eratosthenes' source was someone involved in a canal project that was proposed but never completed *ca* 302–301 BC.

Roller, additionally, examines Eratosthenes' 'taste for inventive vocabulary', those common words which have been geographically repurposed [26]. Particularly interesting are Roller's comments on Eratosthenes use of « σπόνδυλος » ('spindle whorl') to describe the shape of the Earth [144–147: F30] and the philological history of « οἰκουμένη » ('inhabited world'). Eratosthenes concept of land masses as σφραγίδες, a vernacular term more familiar than the technical Euclidian term 'rhomboid', represents the author's attempt to describe the world in familiar but geometrical terms [175: F66].

In his gazetteer, Roller lists the toponyms cited in the extant fragments, giving their positions (with references to maps redrawn by the Ancient World Mapping Center) and the sparest accounts

of their history and significance to Eratosthenes' work and times—topographic details are easily accessible but do not unnecessarily clutter the commentary. Thus, Roller succeeds in respecting the inherent differences between ancient and modern geography by not slavishly imposing the distortion of excessive (and often misleading) modern equivalencies within the body of the commentary.

Three appendices are also included. The fragments of *On the Measurement of the Earth* [app. 1] and the *testimonia* [app. 2] for Eratosthenes' life are translated into crisp English. A brief essay 'On Lengths of Measurements' [app. 3] discusses the complexities of ancient standards of mensuration and the pitfalls of attempts at converting them to modern standards. There has been much discussion regarding which στάδιον Eratosthenes may have used [see Engels 1985, Gulbekian 1987] but Roller rightly asserts 'that there is no reason to believe that Eratosthenes always used the same stadion' [271].

Roller presents the author of the *Geographica* not just as the man whose estimate of the Earth's circumference was the most accurate in antiquity but as a scholar with broad interests and broad training, a poet-scientist who was a product of his times. Roller's commentary is informative and carefully documented. His suppositions are cautious yet creative. He thus updates Fraser 1970 and advances Geus 2002. Roller's edition of Eratosthenes is a welcome volume, filling a real gap in the history of Greek geography and ancient science.

BIBLIOGRAPHY

Aujac, G. 1998. 'Eratosthène et la géographie physique'. Pp. 247–261 in G. Argoud et J.-Y. Guillaumin edd. *Sciences exactes et sciences appliquées à Alexandrie*. Saint-Étienne.

Bentham, R. M. 1948. *The Fragments of Eratosthenes*. PhD Thesis, University of London. London.

Bernhardy, G. 1880. *Eratosthenica*. Berlin.

Bertman, S. 2010. *The Genesis of Science: The Story of Greek Imagination*. Amherst, NY.

Bowen, A. C. 2003. 'Cleomedes and the Measurement of the Earth: A Question of Procedures'. In A. C. Bowen, P. Barker, J. Chabás,

G. Freudenthal, and Y. T. Langermann, edd. *Astronomy and Astrology from the Babylonians to Kepler: Essays Presented to Bernard R. Goldstein on the Occasion of his 65th Birthday. Centaurus* 45:59–68. Repr. *Centaurus* 50 (2008) 195–204 (50th Anniversary volume).

Cimino, M. 1982. 'A New, Rational Endeavour for Understanding the Eratosthenes Numerical Result of the Earth Meridian Measurement'. Pp. 11–21 in *Compendium in Astronomy: A Volume Dedicated to John Xanthakis.* Dordrecht.

Dicks, D. R. 1960. *The Geographical Fragments of Hipparchus.* London.

Dutka, J. 1993–1994. 'Eratosthenes' Measurement of the Earth Reconsidered'. *Archive for History of the Exact Sciences* 46:55–66.

Engels, D. 1985. 'The Length of Eratosthenes' Stade'. *American Journal of Philology* 106: 298–311.

Fraser, P. M. 1970. 'Eratosthenes of Cyrene'. *Proceedings of the British Academy* 56:175–207.

Geus, K. 2002. *Eratosthenes von Cyrene. Studien zur hellenistischen Kultur- und Wissenschaftsgeschichte.* München.

_____ 2004. 'Measuring the Earth and the Oikoumene'. Pp. 11–26 in R. J. A. Talbert and K. Brodersen edd. *Space in the Roman World: Its Perception and Presentation.* Münster.

_____ 2005. 'Space and Geography'. Pp. 232–245 in A. Erskine ed. *A Companion to the Hellenistic World.* Oxford.

Gulbekian, E. 1987. 'The Origin and Value of the Stadion Unit Used by Eratosthenes in the Third Century B.C'. *Archive for History of the Exact Science* 37:359–363.

Rawlings, D. 1982. 'The Eratosthenes-Strabo Nile Map: Is it the Earliest Surviving Instance of Spherical Cartography? Did It Supply the 5,000 Stades Arc for Eratosthenes' Experiment?' *Archive for History of the Exact Science* 26:211–219.

Seidel, G. C. F. 1789. *Eratosthenis geographicorum fragmenta.* Göttingen.

Shcheglov, D. A. 2004. 'Ptolemy's System of Seven Climata and Eratosthenes' Geography'. *Geographia Antiqua* 13:21–37.

Aristoteles Latinus: Meteorologica. *Translatio Guillelmi de Moerbeka* by Gudrun Vuillemin-Diem

Corpus Philosophorum Medii Aevi: Aristoteles Latinus 10.2.1–2. Brussels: Brepols, 2008. 2 vols. Pp. xviii+436, 220. ISBN 978–2–503–53078–9, 978–2–503–53079–6. Cloth € 275.00

Reviewed by
Tiberiu Popa
Butler University
tpopa@butler.edu

The last few decades have witnessed growing interest in the *Meteorology* and its relation to various aspects of Aristotle's natural philosophy, in its influence on developments in alchemy, and in ancient, medieval and Renaissance commentaries on this work. The lack of obvious unity in the *Meteorology*, among other things, was the cause of much perplexity and of a number of competing interpretations since ancient times. The first three books are concerned with natural phenomena (including rain, wind, earthquakes, rainbows, the appearance of the Milky Way, and the generation of stones and metal ores) that occur in the sublunary world and are caused partly by moist and dry exhalations. Those exhalations function as the key explanatory concepts in books 1–3. The fourth book of the *Meteorology*, however, has few obvious connections with the first three and, as we learn from its final chapter (ch. 12), it was likely meant primarily as a sort of preamble to some of Aristotle's biological studies: it presents his treatment of the nature of uniform (or homeomerous) bodies, of their 'chemical' composition and their distinctive dispositions (meltable, malleable, and so on). The enormous interest that the *Meteorology* commanded mainly from late antiquity until early modern times can be measured in part by the scores of commentaries devoted to it or to some of its books and by the number of translations into Arabic and Latin.

William of Moerbeke, one of the most prolific translators in the 13th century, was the first one to translate *Meteor.* 1–3 into Latin

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 179–182

directly from Greek rather than from an intermediary Arabic translation and he provided a new Latin translation of the fourth book.[1] Gudrun Vuillemin-Diem has undertaken the arduous task of editing this complete translation of the *Meteorology*, her edition entailing the collation of more than 40 manuscripts, and of producing a truly impressive study of it in German. The author of this massive prefatory study and edition is well known for other contributions in this field, including her two-volume edition of Moerbeke's translation of Aristotle's *Metaphysics* (published in the same prestigious series, Aristoteles Latinus, which comprises about 30 volumes of editions and introductory studies).

The first volume, the *praefatio*, is divided into five major sections, not counting the introduction (which focuses on older translations of *Meteorology* 1–3 from Arabic and of book 4 from Greek) and an appendix. In the first major part of this volume, Vuillemin-Diem tackles Moerbeke's translation, discusses his access to Alexander's commentary, and establishes, mainly by comparisons with Henricus Aristippus' 12th century *translatio vetus*, that Moerbeke provided a new translation of book 4, not a mere revision of an older translation. The second part deals in about 200 pages with the manuscripts and early editions of Moerbeke's *translatio nova*. This detailed account displays the editor's extraordinary philological acumen and dwells on several significant groups of manuscripts. The third part is concerned chiefly with the nature of Moerbeke's translation. This section includes a survey of the manuscript tradition of the Greek text of the *Meteorology*, with emphasis on an important ninth century manuscript (J, Vindobonensis phil. gr. 100), which was owned by Moerbeke himself. The fourth part explores the very process of translation, which appears to have involved three stages; Vuillemin-Diem examines there a number of interesting features ranging from Greek words whose meaning was probably obscure to Moerbeke to instances of rather loose translation. In the fifth part of the first volume, the author discusses the principles that govern her edition

[1] For a synopsis of Moerbeke's translations, see Minio-Paluello's article in the *The Complete Dictionary of Scientific Biography* [2008] and Grabmann's older but still helpful *Guglielmo di Moerbeke O.P. Il traduttore delle opera di Aristotele* [1946].

and the models that she followed with regard to specific aspects pertaining to her work on the next volume.

The second volume includes the edition of Moerbeke's translation accompanied by a twofold *apparatus criticus*—a very detailed one for *lectiones* in manuscripts of Moerbeke's text and a more succinct one for various readings that can be found in manuscripts of Aristotle's *Meteorology*. The text of the translation is marked for convenience both by Bekker page numbers and by numbers that run from the first line to the last one in any given book in this edition (e.g., in the case of the first book, from 1 to 935). This rendering of the *Meteorology* into Latin was meant, much like Moerbeke's other translations, to reflect the Greek text as faithfully as possible in order to allow his contemporaries to glimpse, as it were, Aristotle's syntax and terminology. Several medieval commentators, including Thomas Aquinas, took full advantage of this. The edition itself is preceded by a *conspectus siglorum* and is followed by a set of geometrical representations or *descriptiones* (of the direction of certain winds or the disposition of colors in the rainbow and so on) found in some of the Greek manuscripts and in the corresponding manuscripts of the Latin version, by a short appendix, and by two comprehensive and very helpful *indices verborum* (Greek-Latin and Latin-Greek). The indices demonstrate Moerbeke's desire to offer a translation that was largely consistent (e.g., πέψις, an important concept in book 4, is consistently translated as 'digestio'). However, he tends to avoid being overly rigid as he was clearly aware of the polysemy of many Greek words. Besides, the two indices shed further light on his occasional hesitation between relying on Latin equivalence and resorting to transliteration (e.g., the Greek « ὑπόστασις » is translated as 'subsistentia' and as 'ypostasis'; « περικάρπιον » becomes 'fructiferum' and sometimes 'pericarpium'). Some of his transliterations contributed to the enrichment of medieval Latin and, subsequently, of various modern languages.

Anyone interested in the *fortuna* of Aristotle's *Meteorology* in the high Middle Ages or, more generally, in the intellectual context in which the Aristotelian corpus was gradually recovered in Latin

Europe will find an illuminating and most reliable tool in Gudrun Vuillemin-Diem's recent *praefatio* and edition.

BIBLIOGRAPHY

Gillispie, C. C.; Holmes, F. L.; and Koertge, N. 2008. *The Complete Dictionary of Scientific Biography*. 27 vols. New York.

Grabmann, M. 1946. *Guglielmo di Moerbeke O.P. Il traduttore delle opera di Aristotele*. Rome.

Minio-Paluello, L. 2008. 'William of Moerbeke'. See Gillispie, Holmes, and Koertge 2008, 9.434–440.

Apollonius de Perge. La section des droits selon des rapports. *Commentaire historique et mathématique, édition et traduction du texte arabe* by Roshdi Rashed and Hélène Bellosta

Scientia Graeco-Arabica 2. Berlin/New York: Walter de Gruyter, 2010. Pp. viii + 493. ISBN 978–3–11–018677–2. Cloth $137.00

Reviewed by
Clemency Montelle
University of Canterbury
c.montelle@math.canterbury.ac.nz

The *Cutting Off of a Ratio* is one of the two surviving works of Apollonius (the other being the *Conics*) which is extant only in Arabic. This Arabic text has never before been published. There do exist versions in Latin however: Halley prepared a Latin version entitled *Apollonii Pergaei de sectione rationis libri duo* [1706] and over a century later W. A. Diesterweg published *Die Bücher des Apollonius von Perga „De Sectione Rationis"* [1824], which was based on Halley's Latin edition. More recently, the contents of the work have been investigated in English by E. M. Macierowski (translator) and Robert H. Schmidt (editor) in *Apollonius of Perga,* On Cutting Off a Ratio: *An Attempt to Recover the Original Argumentation through a Critical Translation of the Two Extant Medieval Arabic Manuscripts* [1988]. This 'critical translation' is based on a critical edition which is yet to be made available to the scholarly community. Selected excerpts from this work have also been translated by Alexander Jones in *Pappus of Alexandria, Book VII of the* Collection [1986, 606–619].

Thus the *editio princeps* of Apollonius' *Cutting off of a Ratio* by Roshdi Rashed along with his collaborator Hélène Bellosta is timely. The work contains a preface, introduction, three preparatory chapters, the Arabic text and French translation on facing pages, notes, an Arabic-French glossary, an index, and a bibliography. The introduction situates the text in the context of Greco-Arabic studies both historically and mathematically. Chapter 1 tackles the mathematical problem covered in this work and the method of analysis and synthesis. Chapter 2 investigates this geometric problem and its breakdown in an algebraic light, and the third chapter concerns the

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 183–187

history and details of the text including a brief note on whether or not Apollonius was the author of the 22nd proposition. The text and translation takes up the majority of the work and includes an *apparatus criticus* based on two different manuscripts both which date to the early 13th century.

To contextualize this work more firmly, Rashed and Bellosta draw from responses to the text by Pappus of Alexandria (*fl. ca* AD 320), Ibn Sinān (909–946), and Halley in 1706. At the outset they raise valid questions concerning this work [3ff.]: Why did Apollonius devote so much attention to this particular problem? How are we to read and contextualise such a book in absence of external illuminations? How can we comprehend its structure, characterize its style, and discern the true project of Apollonius? To this end, they present the original text and its translation; but their commentary remains largely based on an algebraic interpretation. Being careful to caution that this approach is worlds apart from the original conception, the algebraic orientation allows them, they maintain, to explore the structure of the work and investigate the systematic character and completeness of the approach of Apollonius. But while one can appreciate, with some effort, the intricacy of this work and its mathematical scope, such an orientation does not directly address the original issues the authors raised at the outset, such as motivation, exposition, and approach in the Greek geometrical context.

Apollonius' work tackles the mathematical problem (as described by Pappus):

> How to draw a straight line through a given point to cut off from two given straight lines two sections measured from given points on the two given lines so that the two sections cut off have a given ratio. [Toomer 2008, 188]

The resulting scenarios from this geometrical problem are first solved *via* the classical method of analysis; and then, *via* synthesis, the original proposition is reconstructed. This approach is what makes Apollonius' treatise so important: the systematic presentation of problems worked through *via* Greek geometrical analysis followed by its corresponding synthesis is fascinating for both historians and philosophers of mathematics alike. It was due to this fact alone that the work found appeal amongst such Arab practitioners as Ibn Sinān and Ibn al-Haytham, and no doubt contributed to its existence today. As

Rashed and Bellosta explain, because this geometrical problem was conceived without the notion of metric, Apollonius subdivided the problem into many different configurations to cover every resulting case. These are divided into 21 loci.[1] Each locus is further subdivided where appropriate into 87 distinct 'incidences' or cases makes its investigations,[2] 24 of which are covered in book 1 and 63 of which are covered in book 2.

Rashed and Bellosta state that the core of Apollonius' geometrical discussion can be captured by the algebraic quadratic equation [9]:

$$kx^2 - x[k(a+c) + \epsilon(b-d)] + a(kc - \epsilon d) = 0$$

and show how, for various choices of the parameters a, b, c, d, k, ϵ, the resulting cases fall out. This is carefully and methodically done, and the correlation between the geometrical approach of Apollonius and its algebraic rendering by the authors is made more explicit in chapter 2. While this does make the dense Apollonian geometry more tractable, it comes at significant cost. The parallel processes of analysis and synthesis, the very organizing feature of Apollonius' treatment of each configuration become muted as a result of this algebraic transformation. The documentation and investigation of the details and nuances of these processes in this context remains then for future scholarship. Furthermore, an algebraic examination brings to the fore different properties of each configuration which requires them to be treated in a slightly different order than in the original exposition. Indeed, Rashed and Bellosta's technical exegesis thus orders and groups the loci as follows: 1–2, 3–7, 8–10, 11–13, 17–21, 14–16.

Rashed and Bellosta note [9] the traditional view that Apollonius wanted this work to be an exemplar for the methods of analysis and synthesis. They themselves argue for a more developed reading. They claim that a more nuanced interpretation can be forwarded, namely, that Apollonius wished to push as far as possible the methods of application of areas using analysis and synthesis to address

[1] Or perhaps 22, with the final being a later addition. See pp. 89–91. The term 'waḍ' in Arabic [13, 469], the equivalent of « τόπος » (Pappus), is translated by 'lieu' in French and commonly rendered by 'locus' in English.

[2] From the Arabic 'wuqūʿ': cf. the Greek « πτῶσις ». Like the word 'case' (from Latin *casus*), both terms derive from verbs meaning 'to fall'.

diorisms (διορισμοί). This intriguing claim is left somewhat dangling. It is briefly revisited in a footnote [17n3], in which the reader is informed that there are two senses in which the word 'diorism' can be interpreted in the Greek mathematical context: the authors cite unreferenced sections in Proclus and identify, without discussion, which one they ascribe to in this context. For elaboration on this key topic in Apollonian studies, I refer the reader to the engaging discussions by Fried and Unguru [2001, 283–306] and by Toomer [1990, lxxxiv–lxxxv].

Also noticeable is the absence of any discussion about the status and importance of this text in the history of transmission in the exact sciences and the critical role Arabic texts play as a means for recovering lost ancient works. This work is a key example and a whole raft of fascinating issues emerge from its existence. What might be some of the circumstances and features of the translation? Were there any conceptual developments that might have occurred as a result of the translation process? How do the circumstances of this work address and develop the themes of naturalization and appropriation as raised by Sabra in his seminal study of 1987 and more broadly by those developed by contributions in Ragep and Ragep [1996]? Transmission never occurs without change and impact, and these would be valuable to consider more deeply, given that this work is a prime example.

Indeed, arcane scholarly skills are needed to handle primary source mathematical manuscripts in Arabic and to produce critical editions, translations, and commentaries on what are frequently challenging and technical treatises. Despite this, there has been a steady stream of eminent publications in this area over the last several decades which have provided valuable and decisive contributions to our understanding of the field and are crucial to its progress. In this respect, one notable absence in this publication is an engagement with the contemporary scholarly community. The lack of acknowledgement of recent research directly related to Apollonius and to this period in the history of mathematics is surprising, particularly given the sentiments the authors express at the beginning on methodology:

> les différentes organizations de l'ontologie permettent de saisir les différentes strates des sense qui le constituent. [4]

Furthermore, considering that the historical commentary is but a small portion of the book and given the importance of Apollonius in the history of mathematics, this work could have profited immensely from connecting itself more throughly and more substantially with recent scholarly literature. However, overall, this work is a valuable contribution to the field. The availability of a critical edition of the text will be a real asset for scholars who will no doubt be as mindful of the work as they are filled with admiration.

BIBLIOGRAPHY

Diesterweg, W. A. 1824. *Die Bücher des Apollonius von Perga „De Sectione Rationis“.* Berlin.

Fried, M. N. and Unguru, S. 2001. *Apollonius of Perga's* Conica: *Text, Content, Subtext.* Leiden/Boston/Köln.

Gillispie, C. C.; Holmes, F. L.; and Koertge, N. 2008. *The Complete Dictionary of Scientific Biography.* 27 vols. New York.

Halley, E. 1706. *Apollonii Pergaei de sectione rationis libri duo.* Oxford.

Jones, A. 1986. *Pappus of Alexandria, Book VII of the* Collection. New York.

Macierowski E. M. and Schmidt, R. H. 1988. *Apollonius of Perga,* On Cutting Off a Ratio: *An Attempt to Recover the Original Argumentation through a Critical Translation of the Two Extant Medieval Arabic Manuscripts.* Fairfield, CT.

Ragep, F. Jamil; Ragep, S. P.; with Livesey, S. 1996. edd. *Tradition, Transmission, Transformation: Proceedings of Two Conferences on Pre-Modern Science Held at the University of Oklahoma.* Collection de Travaux de l'Académie Internationale d'Histoire des Sciences 37. Leiden.

Sabra, A. I. 1987. 'The Appropriation and Subsequent Naturalisation of Greek Science in Medieval Islam'. *History of Science* 25 223–243.

Toomer, G. J. 1990. *Apollonius Conics: Books V to VII: The Arabic Translation of the Lost Greek Original in the Version of the Banū Mūsā.* 2 vols. New York.

_____ 2008. 'Apollonius'. See Gillispie, Holmes, and Koertge 2008 1.179–193.

Worldviews: An Introduction to the History and Philosophy of Science by Richard DeWitt

Chichester, UK: Wiley-Blackwell, 2010. 2nd edn. Pp. xii+376. ISBN 978–1–4051–9563–8. Cloth $34.95

Reviewed by
Jutta Schickore
Indiana University
jschicko@indiana.edu

Richard DeWitt intends his book *Worldviews* for beginners in history and philosophy of science. His ambitious aim is to provide an accessible and enjoyable introduction to fundamental issues in history, philosophy, and science, as well as to draw out the connections between these fields. The time frame is broad, the three parts of the book spanning the period from around 300 BC until today. The focus is on physics and, more specifically, astronomy. Part 1 introduces in a non-technical way some key philosophical concepts and problems, which include: the notions of worldview, truth, and underdetermination; facts and evidence; the problem of induction; and the attitudes of instrumentalism and realism. Part 2 offers a survey of the main views on the physical structure of the universe. It begins with the Aristotelian conception and outlines the transition from the Ptolemaic to the Newtonian system (*via* Copernicus, Tycho, Kepler, and Galileo). Part 3 covers important recent developments in the sciences, namely, relativity theory, quantum theory, and evolutionary theory. The book ends with useful bibliographical notes and suggestions for further readings on each chapter.

As the title of the book indicates, DeWitt's organizing concept is that of a worldview, a notion loosely based on Thomas Kuhn's 'paradigm' and Willard v. O. Quine's 'web of belief'. According to DeWitt, a worldview comprises a number of interlocking beliefs such as those of the Aristotelian worldview:

- the Earth is located at the center of the universe
- the Earth is stationary
- in the sublunar region there are four basic elements

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 188–191

- each of the elements has an essential nature
- this nature is reflected in the way in which that element tends to move.

Another such set, the Newtonian worldview, includes:

- the Earth revolves on its axis
- the Earth and planets move in elliptical orbits around the Sun
- objects behave as they do largely because of the influence of external forces.

In part 2, DeWitt makes a careful comparison of the main theories of the universe with a focus on their degree of complexity and on how well these systems predicted and explained relevant data. To account for the transition from the Aristotelian to the Newtonian worldview, he considers the astronomers' 'motivations' such as Copernicus' commitment to uniform, circular motion and Kepler's religious beliefs. The presentation of the Newtonian worldview is comparatively brief; a few pages cover the three laws of motion, the law of universal gravity, and the difference between teleological and mechanistic conceptions of the universe. Part 2 ends with a short account of two issues that physicists around 1900 could not quite understand in terms of Newtonian physics and that would soon become major challenges to the Newtonian worldview: the Michelson-Morley experiment and black-body radiation.

Those puzzles are the starting point for part 3. This part concerns challenges to 'our own' worldview. The emphasis shifts from general theories of the universe to our everyday beliefs (religious and otherwise) and how they may be challenged by the insights of modern physics and biology. The first four chapters present lucid introductions to relativity theory and quantum mechanics. They are followed by an overview of the theory of evolution and its philosophical and religious implications. Much of this chapter deals with the question of whether it is possible to accept evolutionary theory and science more generally while continuing to believe in God. DeWitt presents arguments for both sides and, in line with his overall approach, interprets the issue as a disagreement about key elements of one's (individual) worldview.

The conclusion brings the discussion back to general considerations concerning the question of whether relativity theory, quantum mechanics, and evolutionary theory can be accommodated in the

Newtonian worldview or whether they force us to give it up. DeWitt suggests that we live in a period of transition. We will have to abandon the mechanistic model of the universe, and the new view of the universe will likely be complex and perhaps piecemeal.

I must say my reaction to this book is mixed. Like the reviewers of the first edition (2003), I am most impressed by the clarity and accessibility of DeWitt's rendering of quite difficult and complex scientific ideas. But I am also a bit disappointed that DeWitt's overall approach to the history and philosophy of science is rather traditional. In the last decade or so, the relation between history and philosophy of science has become a topic of lively debate. New organizations (such as &HPS, aka Integrated HPS) have emerged, several conferences and workshops on history and philosophy of science have taken place, and special issues on the nature and merits of HPS have been published. None of these developments are reflected in the new edition of DeWitt's book.

For instance, while the author traces changes of scientific ideas (and grants that religious and political ideas are subject to change as well), he implies that philosophical concepts such as 'evidence', 'fact', and 'instrumentalism' are transhistorical. There is only one very brief hint that these concepts may be historically variable, namely, at the beginning of chapter 21 where DeWitt states that since the 1600s, the notion of scientific law has played an 'increasingly prominent role' in science. I would expect from an introduction to history and philosophy of science a detailed treatment of this issue. But instead, DeWitt only discusses the—fascinating and complex, no doubt—philosophical question of what a scientific law is.

Moreover, his account reinforces to some extent the traditional notion that before Darwin, the history of science was really the history of physics. In part 2, a mere three pages are devoted to chemistry and biology. But recent historical research especially on early modern science has shown that the medical sciences have played a key role in bringing about the changes that DeWitt's book covers. He does note at one point that the history of scientific ideas is intertwined with political, conceptual, and religious changes, but the chapter that is devoted to this issue [ch. 19] is just about four pages long.

The book has great merits and is very readable, and beginners in history of science and philosophy of science will appreciate the wealth

of information that it offers. I will definitely use it as a resource when I design units on such key scientific ideas as the Aristotelian notion of God or on the mathematics and interpretations of quantum theory, for my undergraduate courses in history and philosophy of science. I may also use it as a resource for introducing such key philosophical issues as the problem of induction or the debate about realism and instrumentalism. I may assign selected chapters as course readings.

But an introduction to the History and Philosophy of Science it is not. Neither is it a compelling illustration of the successful integration of historical and philosophical analysis, nor does it discuss possible ways of bringing history and philosophy of science together. It really is an introduction—an excellent introduction—to a number of fundamental scientific ideas that should be familiar to students in history of science and philosophy of science.

Alessandro d'Afrodisia (Tito Aurelio Alessandro), Il Destino. *Trattato sul destino e su ciò che dipende da noi. Dedicato agli Imperatori* by Carlo Natali and Elisa Tetamo

International Aristotle Studies 5. Sankt Augustin: Academia Verlag, 2009. 2nd edn rev. Pp. 272. ISBN 978–3–89665–490–8. Cloth € 48.50

Reviewed by
Sara Magrin
L'Université du Québec à Montréal
The American University in Cairo
magrin.sara@uqam.ca

Alexander of Aphrodisias (*flor. ca* AD 200) is known mostly as the last Peripatetic commentator on Aristotle, but among his works there are also several school treatises and opuscula in which he examines some of the central philosophical issues of his time from an Aristotelian perspective. For a long time, following Zeller, scholars have labelled these works as 'personal', thus suggesting that only here Alexander abandoned what was considered the non-committal stance of the commentator to express his own original views. Recent research [Rashed 2007], however, has made such a label obsolete by challenging the assumption that was responsible for its introduction in the first place, namely, the view that Alexander's commentaries are to be assessed as mere line by line exegesis rather than as philosophical contributions. School treatises and *opuscula*, then, can no longer be considered as a privileged place in which to look for Alexander's own philosophical agenda. They are to be read, rather, as philosophical works which, in contrast to the commentaries, aim to engage non-specialists.

This is in fact the goal of the *De fato*, a treatise written as an epistle to the Emperors Septimius Severus and Caracalla that can be dated between AD 197 and 211 (on the grounds of its dedication) and that was likely composed shortly after Alexander's appointment to the state-endowed chair of Aristotelian philosophy in Athens. By that time, fate had become a standard topic of philosophical discussion; and the issues that were addressed under this heading were those of freedom and determinism. Every major philosophical school

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 192–204

(Platonist, Peripatetic, Stoic, Epicurean) was expected to have something to say on the subject; but the Stoics' theory of fate, and especially Chrysippus' (*ca* 280–207 BC), had undoubtedly a central role in shaping the debate.

Gellius reports [*Noct. att.* 7.2.3] that for Chrysippus fate (εἱμαρμένη) is 'a natural arrangement of the universe' according to which things are inexorably 'woven together' and follow upon each other from eternity. But the standard definition of fate ascribed to the Stoics in late sources says that fate is 'a series of causes' [see, e.g., Nemesius, *Nat. hom.* 108.15–17]. As these definitions suggest, the Stoics' theory of fate is rooted in their physics and cosmology; and it consists primarily in a theory of causal determinism according to which what happens at any given time is entirely determined by antecedent causes so that nothing could have happened other than that which did. This form of causal determinism may resemble, but is not to be confused with, modern accounts of causal determinism. Modern theories of causal determinism treat causes and effects as belonging to the same ontological category; and it makes no difference to them whether causes and effects are events, bodies, or properties of bodies. In contrast, for the Stoics, causes and effects belong to two different ontological categories, causes being bodies and effects being incorporeal things. Thus, whereas in a modern *series causarum* every link in the chain is both the effect of an antecedent cause and the cause of a subsequent effect, in the Stoics' *series causarum* a body (a knife, say) is cause of an incorporeal effect (being cut) in another body (flesh) and it is this body, rather than the effect, that in turn is cause to another body of a further effect.

The main physical and cosmological aspects of the Stoic theory of fate go back to Zeno (344–262 BC) but Chrysippus is probably the first to address the problem of reconciling universal causal determinism with human freedom and moral responsibility [Bobzien 1998, 3]. On most readings today, Chrysippus finds a way to make human responsibility compatible with determinism by pointing to the causal power of the mind which, for the Stoics, is πνεῦμα, i.e., a type of body. What we are morally responsible for in his view is what is 'in our power' or 'depends on us' (ἐφ' ἡμῖν), namely, assents and actions. Assents and actions are 'in our power' because they do not depend on any causal factor external to us but rather on the nature of our mind, that is, on the qualities and dispositions that account for who we are

and the moral character we have. These qualities and dispositions of our mind are to be accounted for on the basis of predetermined causes (e.g., our family, education, past experiences, and so forth) and, thus, are part of the *series causarum* that fate consists in. But, no matter how we came to acquire the moral character we have, in so far as assents and actions depend on our mind, they depend on us and we, according to Chrysippus, are entirely responsible for them.

Chrysippus' compatibilism, then, seems to make autonomy not merely a necessary but also a sufficient condition for moral responsibility [Bobzien 1998, 279]. It is sometimes described as a form of 'soft determinism' as opposed to both 'hard-determinist' and 'libertarian' positions [Sharples 1983, 9]. In contrast to soft determinists, hard determinists and libertarians maintain that determinism and responsibility are incompatible; but, whereas hard determinists give up responsibility, libertarians give up determinism.

It is a libertarian conception of responsibility that, on most readings, we find in Alexander's *De fato* [Sharples 1983, 9; Bobzien 1998, 401: cf. D. Frede 1984, 287]. If we exclude the first and the last chapters, which mainly fulfill a rhetorical purpose, the *De fato* can be divided into two parts. In the first part, chapters 2–6, Alexander presents the Peripatetic conception of fate; in the second part, chapters 7–38, he develops a series of criticisms against Stoic determinism that aim at showing the superiority of his theory of fate over that of the Stoics. Oddly, he never refers to the latter by name in the treatise; but it is largely agreed that, if they may not be the only polemical target, they are at least the main one.[1]

The main difficulty that Alexander faces in the *De fato* is fairly obvious: fate was not one of Aristotle's main philosophical concerns and he had not developed any theory of it. To be sure, in Aristotle one can find several observations that point to a rejection of determinism [*De interp.* 9, 13; *Meta.* 6.3, 9.3] and one can also find a discussion of voluntary action and responsibility [*Eth. Nic.* 3.1–5]. But, in order to offer a Peripatetic theory of fate that can compete with the Stoics', Alexander must piece together Aristotle's remarks and try to make them fit into a coherent whole.

[1] For a different view, see Long 1970; Donini 1974, 185.

He begins by observing that, as a matter of universal consensus, fate is a cause [ch. 3] and this leads him to examine it against the background of Aristotle's theory of the four causes. He maintains that fate is an efficient cause and he identifies it with the nature of each individual thing. He thinks that everything that happens by nature happens by fate [ch. 6] but, since what happens by nature, for Aristotle [*Phys.* 2.8], happens only for the most part rather than always and inexorably, he concludes that the determinists' claim that whatever happens at any given time is completely determined by antecedent causes is false. As far as human beings are concerned, he observes first of all that their nature is twofold. On the one hand, it consists in a certain bodily constitution and, on the other, in a certain character, so that one of us, for instance, is naturally choleric, another enduring in the face of bad luck, and so forth [ch. 6]. Character determines our actions, he argues, though only for the most part; but the rational faculty of our soul, crucially, can oppose our character and is thus entirely free in respect both to causal factors that are external to us and to causal factors that are internal to us. This last point is developed at greater length in the polemical part of the treatise, and especially in chs 11–12.

It is against the background of this philosophical debate on freedom and determinism that one is to assess Carlo Natali's second and revised edition of the *De fato*. The Italian translation, followed by a commentary, is by Elisa Tetamo but has been revised by Natali. The introduction is by Natali. As I have less to say on the translation, I will start with it.

Tetamo's translation is the first and only Italian translation of the treatise, and just for this it is worthy of applause. The Greek text it is based on is that established by Ivo Bruns [1892] but with the changes suggested by Sharples in his classic edition of 1983. In Sharples' edition the photographic reprint of Bruns' text, with asterisks indicating the emendations, is placed after the translation; in Natali's edition, the Greek text, reconstructed according to Sharples' suggestions, accompanies the translation side by side. This makes for a considerably easier reading.

The translation itself is mostly clear and easy to follow, although I disagree at times with some of Tetamo's and Natali's choices. Thus, at 166.25–26 and *passim*, one finds « οὗ χάριν », used for the Aristotelian final cause, rendered by 'in grazia di cui', which is a fairly

odd and archaic expression in Italian (one that as far as I know is no longer in use today) that means something like 'thanks to which'. Although it is a literal rendering of« οὗ χάριν », 'in grazia di cui' fails to convey any sense of finality to an Italian reader. Later on, one finds « χάριν » with the genitive, always used to refer to the final cause, translated by 'in vista di' ('with a view to') [see, e.g., 168.22]. This seems to me a better choice, but the fact that one and the same technical expression is rendered in two different ways may cause problems for the reader who has no Greek. At 178.25, the aorist participle of « ἐλέγχειν » is rendered by 'rifiutato', i.e., 'rejected', whereas it should be 'tested'. Perhaps this is only a typographical error ('rifiutato', i.e., 'rejected' for 'refutato', i.e., 'refuted'); but the sense of the passage is compromised by it (it would be compromised, I think, even if we substituted 'refutato' for 'rifiutato'). Finally, there is one passage that needs some revision—170.25–171.7: here the translation is mostly unintelligible to me.

Natali's introduction is substantial (almost 100 pages long), and one of its greatest merits is that it offers an overview of the debate over freedom and determinism from Homer to Alexander that takes into account several scholarly traditions. There is a discussion of the Stoic theory of fate [16–48], an analysis of Alexander's theory of fate and action and of his arguments against determinism [49–91], an assessment of Alexander's theory of action in the light of some contemporary discussions [92–96], and a short biographical note [97–98]. This introduction, then, provides a valuable starting point for those interested in pursuing further research on the ancient debate on freedom and determinism. The downside of such a comprehensive approach is that one cannot expect to find worked out answers to the philosophical problems under examination. But Natali explicitly warns the reader [7] that he will try to stir a middle path between a broad overview of the literature and his personal, philosophical contribution to the discussion.

Natali's approach to the Stoics' and Alexander's theories of fate rests on the analysis of their respective theories of causality. The difference between the Stoic and the Peripatetic conceptions of causation is in fact the most fully developed subject in the book and the whole discussion of determinism and freedom is organized around it. This emphasis has the merit of bringing to the fore what Natali's considers the most important contribution of Alexander's treatise to

contemporary debates in philosophy of action. Philosophers such as Ricoeur, Natali points out, believe that in order to find an adequate explanation of human action we need to rethink the conceptual structure of the notion of cause. Alexander, he suggests, could help them in carrying out this project [96].

Rather than summarizing the introduction, I will concentrate on two points where Natali's personal philosophical contribution to the study of ancient theories of freedom and determinism is most prominent: his assessment of the relation between character and determinism in Aristotle and the Stoics [42–48], and his analysis of Alexander's conception of deliberation and of what is 'in our power' [73–83].

All those familiar with the Aristotelian and Stoic discussions of human responsibility are aware that neither Aristotle nor Chrysippus seem to have been sensitive to the problem raised by what may be called 'ethical determinism' [D. Frede 1982], that is, the view that our actions are predetermined by our character. The problem is the following: If what we do is predetermined by who we are and the character we have, how can our actions be free and to what extent are we really responsible for them? Natali argues that the reason why this problem did not arise for Aristotle and Chrysippus is to be found in their understanding of the causal role of character in actions, and, ultimately, in their respective conceptions of causality. For Aristotle, he observes [43], there is no absolutely necessary series of causes because

(1) for him there are four different kinds of causes rather than only efficient causes (i.e., the sole causes admitted by the Stoics), and
(2) he admits of interruptions in the chains of causes, such as those brought about by accidental events and human choices.

With these observations in mind, Natali approaches Aristotle's analysis of action in *Eth. Nic.* 3.1–5. Here, he remarks, Aristotle introduces a notion of voluntary (ἑκούσιον) according to which for an action to be voluntary its efficient cause must be in the agent rather than external to it, as it would be if, for instance, one were carried by a wind [1109b35–1110a4]. The efficient cause of a voluntary action, Natali goes on, is a desire; and this desire is oriented to an end, which

is the action's final cause. Thus, Natali concludes, for Aristotle, a human being acts by having a desire for something or other which he represents to himself as good; and the fact that he views some things rather than others as good depends on the character he happens to have [1114a31–b3]. Natali admits that this seems to suggest that character determines our actions but he invites us to resist this conclusion by drawing a distinction between desire, which he views as the efficient cause of our actions, and character, which he views as their formal cause. If I understand his point correctly, he argues that, in Aristotle's view, we are responsible for our actions only in so far as we have in us their efficient causes in the form of our desires, whereas our character, being merely a formal cause of our actions, is irrelevant to responsibility. For Aristotle, he suggests, character cannot determine action because desires are the only efficient cause of action; and neither our goals (final causes) nor our characters (formal causes) can in turn be efficient causes of our desires.

Natali's suggestion sheds new light on an old problem but I am not entirely convinced by it. In particular, I am not sure whether the distinction between different types of causes in fact eliminates the problem of ethical determinism. It seems to me that the suggestion would work better if one were of the view that Aristotle's causes, apart from the efficient one, are to be understood in terms of explanations [e.g., M. Frede 1987]; but Natali rejects this possibility [see 38]. If one holds, as Natali does, that all four Aristotelian causes are causes in some robust sense of the word, then one should conclude, I think, that, even though it is a formal cause, character can and does determine actions in such a way as to make the Aristotelian notion of responsibility problematic and elusive. Natali is aware of the difficulty, it seems, and he adds that, even if one were to concede that for Aristotle character determines our actions, this in his view would not exempt us from being responsible for what we do. For Aristotle maintains that we are responsible even for our character, Natali goes on, since we came to acquire it little by little since childhood by acting voluntarily in certain ways [45–46].

But I think that this further suggestion too is problematic. First of all, actions are not the only things that contribute to the formation of one's character; at a minimum, past experiences must have a role too. But the main problem with it, I think, is that it could lead one to conclude that, in assessing responsibility, Aristotle draws no

distinction between human and animal actions. This is because in Aristotle's view even non-rational animals can act voluntarily. Probably in order to avoid this conclusion, Natali points out that the voluntary actions through which children build their character are not morally significant [46] so that his suggestion does not amount to saying that, in Aristotle's view, we are morally responsible for our character. But, if at some point we do become morally responsible for something or other, and if our moral choices depend on a character for which we are not morally responsible, is it not legitimate to question the extent to which we can be held morally accountable for what we do?

In any case, even if one agrees to bracket the issue of moral responsibility and to deal exclusively with causal responsibility, I wonder whether one can avoid discussing Aristotle's distinction between animal and human action. Natali's analysis of *Eth. Nic.* 3.1–5 here makes no reference to this distinction. Yet adult humans, for Aristotle, as opposed to animals and children, are not merely capable of acting voluntarily; they can also act deliberately, by rational choice (προαίρεσις); and this should make a difference, I think, to the way in which they are held responsible for their actions, even if moral responsibility is set aside.

I have similar remarks concerning the discussion of ethical determinism in Stoicism. Here Natali's suggestion is that the Stoics avoid ethical determinism because, on their view, the proper cause of something cannot be present without the effect being also present [Long and Sedley 1987, 55A]. Natali interprets this as meaning that only the most immediate cause or, in other words, the ultimate cause in a chain is the proper cause of something, whereas any other cause merely contributes to the effect in some other, more indirect way. Then, on the grounds of Clement, *Strom.* 8.9.27.3–5 and Cicero, *De fato* 34–35, he concludes that the Stoics do not view character as the most immediate cause of our actions but rather assign this causal role to 'the subject' ('il soggetto') of the action [47]. What I find problematic in this suggestion is precisely this distinction between subject and character. It is not clear to me that such a distinction could be ascribed to the Stoics. As far as I can see, for Chrysippus, and also for later Stoics such as Epictetus, the subject just is the individual's mind with its character and the specific qualities it has at the time at which the action takes place. Thus, one would like to

read more about Natali's interpretation of the role of character in the Stoics' theory of action. He addresses this topic when he examines Chrysippus' famous cone analogy [26–32] but I did not find his examples (namely, 'the good housekeeper' and 'the young absent-minded bride' on page 32, which are examples of stereotypes) very helpful.

I will pass now to the second point of Natali's introduction that I would like to examine: Alexander's account of deliberation and of what is 'in our power'. The most sustained discussion of these topics is to be found in chs 11–15. Here Alexander's observations rest ultimately on *Eth. Nic.* 3.1–5 but what is interesting is that the polemic against the Stoics leads him to rethink the notions of deliberation and rational choice with which Aristotle operates. Alexander develops several arguments in these chapters but his main points, I take it, are the following: the determinists, he says, view deliberation merely as a step in a chain of causes that necessitate a certain action. But, if this were the case, deliberation would be pointless, as in the end one would never be able to act otherwise than he did [ch. 11]. Yet deliberation cannot be pointless, or else nature would have given us the ability to deliberate in vain. In order to avoid the conclusion that deliberation is pointless, Alexander says that we need to grasp what is central to it, namely, the fact that it makes us able to choose either to do or not to do something. This two-sided concept of deliberation—which is absent (or at least not prominent) in Aristotle—leads Alexander to a likewise two-sided concept of rational choice (προαίρεσις) and of its object, i.e., that which is 'in our power' (ἐφ᾽ ἡμῖν) [ch. 12]. The determinists, he observes, think that what is 'in our power' is merely what happens by fate 'through us', where 'through us' is to be spelled out as 'following an impulse (*hormē*)' [ch. 13]; but they confuse what is 'in our power' with what Aristotle calls 'voluntary' [ch. 14]. To act according to impulse is the same as to act voluntarily, Alexander suggests; but even animals are capable of acting voluntarily, whereas only humans have control over their actions. Every action that is 'in our power', then, is voluntary for Alexander, in so far as it is done according to impulse but not *vice versa*; and this is because what is 'in our power' is something more than what is merely voluntary: it is 'that over which we have control both to do and not to do' [ch. 12, 180.5–6] as a result of deliberation.

Scholars have often noted that Alexander's analysis of the Stoic notion of what is 'in our power' in ch. 13 may be unfair. But Natali points out, and rightly in my view, that he does have a strong point against the Stoics in so far as he suggests that their conception of what is 'in our power' does not fit our ordinary intuitions [also Sharples 1983, 142]. For we tend to believe that when something is 'in our power' this is not merely because we act as instruments of fate. Natali observes that Alexander's criticism of the Stoics here rests on his own conception of rationality and deliberation as two-sided; and so far I agree. But he also suggests that Alexander's peculiar conception of deliberation as two-sided is to be explained ultimately in the light of a theory of action, the Peripatetic one, which is radically different from that of the Stoics, for whom in fact deliberation is not a central concept; and here I no longer entirely agree. For a Peripatetic, Natali says, representations are the data of a problem on which one needs to deliberate in order to act, whereas for a Stoic a representation is the impact that the world has on a subject; this subject can react in an appropriate or an inappropriate way but in either case he does not need to deliberate [78]. The Stoics, he goes on, conceive of actions as reactions that can be either correct or not, whereas the Peripatetics conceive of them in a goal-directed perspective, which leads them to ascribe a more important role to deliberation [83]. Although Alexander does make an important point against the Stoics [ch. 15] that rests on an appeal to the plurality of ends one may strive for in action [185.21–27], I am not convinced by Natali's way of framing the difference between the Peripatetic and the Stoic theories of action. Deliberation does have a role in the Stoic theory of action, and even though it is true that it does not have a prominent role, this is not, as far as I can see, because the Stoics conceive of an action as a mere reaction to a representation but because they believe that only an imperfect mind needs to deliberate, whereas the sage can and should do without deliberation. As I understand it, Alexander's main point against the Stoics in chs 11–15 is not that they neglect the role of deliberation in action, as Natali suggests, but that, though granting deliberation an important role, they fail to see that it must rest on a two-sided capacity to do or not to do something.

That this is Alexander's point emerges in particular from the following passage where he explains why deliberation is not pointless:

> It is agreed by everyone that man has this advantage from nature over other living creatures, that he does not follow representations in the same way as they, but has reason from her as judge of the representations that impinge on him, concerning certain things as deserving to be chosen. Using this, if, when they are examined (ἐξεταζόμενα), the things that appeared are indeed as they initially appeared, he assents to the representation and so goes on in pursuit of them; but if they seem different or something else [seems] more deserving to be chosen, he chooses that.... At any rate [there are] many things [which], having seemed different to us in their first appearances [from what they seemed to us subsequently], no longer remained as in our previous notion when reason put them to the test (οὐκέτ᾽ ἔμεινεν ἐπὶ τῆς προλήψεως ἐλέγξαντος αὐτὰ τοῦ λόγου); and so, though they would have been done as far as concerned the representation of them, on account of [our] deliberating about them they were not done—we being in control of the deliberating and of the choice of the things that resulted from the deliberation. [Alexander, *De fato* 11.178.7–28: Sharples 1983 slightly modified]

Here Alexander describes deliberation as a rational activity that consists in examining (ἐξετάζειν) and testing (ἐλέγχειν) our ordinary notions (προλήψεις). But this account of deliberation is Stoic rather than Aristotelian and it can be found time and again in Epictetus [see, e.g., *Diss.* 1.17, 2.8 and Sharples 1983,139]. In this passage, Alexander does not suggest that the Stoics neglect the role of deliberation in action; rather, he argues that their own conception of deliberation (or what he takes to be their conception), which he by and large shares, requires or presupposes that we have control over whether to do something. This means that, in contrast to the Stoics, Alexander maintains that we are free to act even against our character, as in any given circumstance we could have always chosen to act otherwise than we did. Natali's reading rests on the assumption that Alexander operates with the Aristotelian conception of deliberation as an inquiry in which a rational subject with a certain end in mind reasons backwards so as to determine the means that will lead him to reach that end. Such a conception of deliberation seems indeed foreign to the Stoics. But, although Alexander does refer to the Aristotelian account of deliberation [180.12–23], this is not the

only account with which he works. In general, Natali is interested in the Aristotelian background of Alexander's theses but he does not examine them in the light of late Stoic thinkers such as Epictetus or, for ch. 13, Philopator. He does discuss [80] Bobzien's suggestion that in ch. 13 Alexander may be relying on a late notion of what is 'in our power' that differs in part from Chrysippus' and that may go back to Philopator [Bobzien 1998, 359 ff.]. But he concludes that we just do not have enough evidence for a proper assessment of this matter.

Unfortunately, there are several typographical errors in Natali's introduction. Most of them are minor[2] but some are more serious. This holds in particular for the citations in the footnotes that sometimes do not match the entries in the bibliography.[3] There is a potentially misleading observation on page 24 where, while discussing the Stoics' commitment to logical determinism, Natali remarks (but the point is made only in passing) that Chrysippus links the existence of fate to the Law of Excluded Middle; but I think that what is meant is Bivalence.

These are minor problems in any case, and Natali's edition of the *De fato* is a most welcomed contribution to the growing debate on the development of the notions of freedom and determinism in antiquity. Natali's target readers are the advanced student and the non-specialist but his book will be useful to anybody interested in Alexander and in his contribution to the ancient debate on freedom and determinism.

BIBLIOGRAPHY

Bobzien, S. 1998. *Determinism and Freedom in Stoic Philosophy.* Oxford.

Bruns, I. 1892. ed. *Alexandri Aphrodisiensis praeter commentaria scripta minora.* Quaestiones, De fato, De mixtione. Commentaria in Aristotelem Graeca. Supplementum 2.2. Berlin.

[2] E.g., 'protokatartikē' instead of 'prokatarktikē' on p. 31; 'ma' instead of 'mai' in the quotation on p. 47.

[3] E.g., Huby 1964 on 16n14, and Huby 1970 on 29n38 have no corresponding entries in the bibliography where one finds only Huby 1989; the same holds for Burnyeat 1984 on 68n81.

Donini, P. L. 1974. *Tre studi sull'aristotelismo nel II secolo d.C.* Turin.

Frede, D. 1982. 'The Dramatization of Determinism: Alexander of Aphrodisias' *De Fato*'. *Phronesis* 27:276–298.

_____ 1984. 'Could Paris (Son of Priam) Have Chosen Otherwise?'. *Oxford Studies in Ancient Philosophy* 2:279–292.

Frede, M. 1987. 'The Original Notion of Cause'. Pp. 125–150 in M. Frede, *Essays in Ancient Philosophy.* Minneapolis.

Long, A. A. 1970. 'Stoic Determinism and Alexander of Aphrodisias' *De Fato* i–xiv'. *Archiv für Geschichte der Philosophie* 52:247–268.

Long, A. A. and Sedley, D. N. 1987. edd. *The Hellenistic Philosophers.* 2 vols. Cambridge.

Rashed, M. 2007. *Essentialisme. Alexandre d'Aphrodise entre logique, physique et cosmologie.* Berlin.

Sharples, R. W. 1983. *Alexander of Aphrodisias:* On Fate. London.

Aetna edited and translated by Robinson Ellis with new introduction and bibliography by Katharina Volk

Exeter, UK: Bristol Phoenix Press, 2008. Pp. civ + 258. ISBN 978–1–904675–38–9. Cloth $49.95

Reviewed by
Harry Hine
University of St Andrews
hmh@st-andrews.ac.uk

The Latin poem *Aetna* has a unique place in the surviving Greco-Roman literature on volcanoes. The classical world was acquainted with the various forms of volcanic activity that occur in the Mediterranean region and there are references to volcanoes and volcanic activity scattered across a wide range of Greek and Latin literature—in poetry, history, letter-writing, treatises on geography and meteorology, and in other genres as well. But none of these works is specifically about volcanic phenomena, with the sole exception of the *Aetna*, a didactic poem of nearly 650 lines which seeks to give a rational explanation of the eruptions of Mount Etna. The work is attributed to Vergil in the ancient Vergilian lives and in most of the manuscript tradition, but this attribution is generally rejected today. Date and authorship are still debated but it is agreed that the poem predates the eruption of Vesuvius in AD 79, which is not referred to in the poem. Many scholars would date the poem to the decade or two before AD 79 but one should perhaps not rule out a date as early as the reign of Tiberius or even late in the reign of Augustus.

Study of the *Aetna* is bedeviled by the fact that the medieval manuscripts preserve the text in an exceedingly corrupt state. This is good news for textual critics, who have often been attracted to the poem's challenges; but it means that study of its scientific ideas is difficult: the main outlines of the argument are mostly clear enough but much of the detail is obscure and its interpretation controversial. The difficulties are compounded by the fact that it is a poem working within the ancient didactic tradition: not only are there long sections on standard themes of didactic poetry that have nothing directly to do with volcanoes, but also the scientific sections themselves are often written in poetic language and poetic imagery.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 205–208

Robinson Ellis' edition of *Aetna* was originally published by the Clarendon Press (Oxford) over a century ago in 1901. Nowadays, it is increasingly easy to get hold of works of scholarship that have long been out of print and out of copyright. They are being made available in electronic form by Google Books and others,[1] and in printed form, for example, in Nabu Public Domain Reprints or in Cambridge University Press' new Cambridge Library Collection series. The volume here under review comes from the Bristol Phoenix Press' Classic Editions series, which has the merit of including in each volume a new introduction which sets the reprinted work in its original scholarly context, assesses its continuing importance, and gives a selective modern bibliography.[2]

The introduction to this volume is ably provided by Katharina Volk, who is well known for her writings on Latin didactic poetry, including the *Aetna*.[3] She gives an entertaining sketch of the career of Ellis, who was well known for his eccentric appearance and eccentric manners, and was Corpus Professor of Latin at Oxford from 1893 till his death in 1913. During his lifetime, he was regarded by some as the leading classical scholar of his day. He was particularly interested in manuscripts and textual criticism, and was drawn towards obscure texts that were outside the canon, so that the *Aetna* attracted his scholarly attention over many years.

Ellis' edition was published in 1901, partly in reaction to the publication of the textually conservative German edition of Siegfried Sudhaus in 1898. The longest section of Ellis' introduction deals with the manuscripts; and there are also sections on date and authorship, and on the possibility that the Aristotelian *De mundo* was a source. He then gives an analysis of the poem's content, a text with *apparatus criticus* and facing English translation, a detailed commentary, predominantly concerned with problems of text and interpretation, and an *index verborum*.

As Volk says, history has not viewed Ellis' achievements as generously as some of his contemporaries did. In the case of the *Aetna*,

---

[1] Ellis' *Aetna* is available electronically at http://www.archive.org/details/aetnaacriticalr00elligoog.

[2] To the English-language works one could now add Taub 2008, 30–55, which appeared too late for inclusion in Volk's bibliography.

[3] On the *Aetna*, see Volk 2005.

while he was right, against Sudhaus, that the text is in a very poor state and frequently requires emendation, few of his own numerous conjectures have won lasting approval. He remained stubbornly unconvinced of the merits of the so-called *lectiones Gyraldinae*, readings of a now-lost manuscript recorded by Renaissance scholars; but more recent scholarship acknowledges that they are derived from an independent branch of the manuscript tradition and so of considerable importance in reconstituting the text.

So, does his edition deserve to be reprinted over a century later? Certainly his edition has been replaced for English readers by that of F. R. D. Goodyear [1965], which has now become the standard.[4] Nevertheless, anyone who wishes to engage seriously with the poem's problems of text and interpretation should also go back to Ellis' edition. Furthermore, Ellis provides an English translation of his text. There is a more recent, and more widely used, English translation by J. W. Duff and A. M. Duff in the Loeb series [1934]; but anyone relying on a translation would be well advised to consult Ellis' too as a reminder of how much is uncertain about the text and interpretation of the poem. Finally, his commentary also contains some material of interest for the history of volcanology. Volk says [xv] that he 'appears to have had little interest in the scientific content of the poem or its poetic qualities'; and it is certainly true that he does not discuss the scientific content of the poem in the introduction, that he has little interest in the *Quellenforschung* that was fashionable at the time (save for the section of his introduction about *De mundo*), and that there is no systematic treatment of scientific topics in the commentary. Nevertheless, the commentary regularly evinces his familiarity with the widely scattered ancient literary references to volcanoes and also with 19th century writings on Etna and volcanology. This familiarity regularly enriches his notes, even though his primary focus is often—but not always—on textual problems; and so from his edition one can learn something about the scientific content and background of the poem—more than one can from Goodyear's, which confines

[4] Goodyear's text was also included in the Oxford Classical Text edition of the *Appendix Vergiliana* [Clausen, Goodyear, Kenney, and Richmond 1966]. Since Goodyear, there have been Italian editions of *Aetna* by Traglia [1968], de Vivo [1987], and Iodice [2002] (in an edition of the whole *Appendix Vergiliana*).

itself more resolutely to textual matters. So, this reprinting of Ellis' edition is to be welcomed.

BIBLIOGRAPHY

Clausen, W. V.; Goodyear, F. R. D.; Kenney, E. J.; and Richmond, J. A. 1966. *Appendix Vergiliana.* Oxford.

de Vivo, A. 1987. *Incerti auctoris Aetna.* Naples.

Duff, J. W. and Duff, A. M. 1934. *Minor Latin Poets.* Loeb Classical Library. London/Cambridge, MA.

Goodyear, F. R. D. 1965. *Incerti auctoris Aetna: Edited with an Introduction and Commentary.* Cambridge Classical Texts and Commentaries 2. Cambridge.

Iodice, M. G. 2002. *Appendix Vergiliana.* Milan.

Taub, L. 2008. *Aetna and the Moon: Explaining Nature in Ancient Greece and Rome.* Corvallis, OR.

Traglia, A. 1968. *Aetna. Poemetto d' incerto autore.* Rome.

Volk, K. 2005. 'Aetna oder Wie man ein Lehrgedicht schreibt'. Pp. 68–90 in N. Holzberg ed. *Die Appendix Vergiliana. Pseudepigraphen im literarischen Kontext.* Classica Monacensia 30. Tübingen.

New Perspectives on Aristotle's De caelo edited by Alan C. Bowen and Christian Wildberg

Philosophia Antiqua 117. Leiden/Boston: Brill, 2009. Pp. x+321. ISBN 978–90–04–17376–7. Cloth € 103.00, $147.00

Reviewed by
Helen S. Lang
Villanova University
helen.lang@villanova.edu

The preface of this volume states clearly its purpose and origins:

> This volume is the first collection of scholarly articles in any modern language devoted to Aristotle's *De caelo*... It grew out of [a] series of workshops on this text and involved an international collaboration of scholars, giving it a diversity and sophistication unattainable by a single scholar [vii].

Beyond the introduction [1–7], the volume presents 10 essays [9–281], an extensive bibliography of both primary and secondary sources for the *De caelo* [283–298], and four indices [299–321]. In the introduction, Bowen and Wildberg point out that in 'the last decades... there are only a few probing studies of, or commentaries on, the *De caelo* itself' [2]. Indeed, Aristotle's *Physics* has received much more attention in the literature than has the *De caelo*. But the present volume is not conceived as a systematic study of, or a comprehensive commentary on, the work as a whole; rather, this volume provides

> a collection [of] essays on the *De caelo* that address challenging issues ... by acquainting the reader with some of the latest and most exciting aspects of current scholarship on Aristotle's natural philosophy... to provide useful in-depth discussion of some important ideas, or of difficult passages and chapters in the *De caelo*, and thereby to deepen the reader's understanding and critical appreciation of Aristotle's cosmology. [2]

As a result of its conception, the essays comprising this volume cover a range of problems, methodological, substantive, and historical. There are several essays that examine particular arguments

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 209–211

while keeping an eye on Aristotle's criticisms of Plato's *Timaeus*. 'From Plato's *Timaeus* to Aristotle's *De caelo*: The Case of the Missing World Soul' [9–28] by T. K. Johansen finds commonality between Plato and Aristotle in the view that the heavens must be animate [26] and difference in the presence of ethics in the *Timaeus* and its absence in the *De caelo*. In 'The Possibilities of Being and Not-Being in *De caelo* 1.11–12' [29–50], S. Broadie reads Aristotle as formulating two arguments that do not clearly form a single coherent argument [30] against Plato's view in the *Timaeus* that the cosmos came into being and will never cease to be. R. Bolton identifies 'Two Standards for Inquiry in Aristotle's *De caelo*' and concludes by setting Aristotle's distinction between εὐλόγως and φυσικῶς in the historical context of Aristotle's relation to Plato [51–82].

Several essays focus on a particular argument found in the *De caelo*. R. J. Hankinson writes on 'Natural, Unnatural, and Preternatural Motions: Contrariety and the Argument for The Elements in *De caelo* 1.2–4' [83–118]; 'Why Does Earth Move to the Center? An Examination of Some Explanatory Strategies in Aristotle's Cosmology' is a question raised and examined by Mohan Matthen [119–138]. M. L. Gill considers 'The Theory of the Elements in *De caelo* 3 and 4' and locates the account here, as she interprets it, within the larger context of Aristotle's account of elemental motion [139–161]. P. Pellegrin examines 'The Argument for the Sphericity of the Universe in Aristotle's *De caelo*: Astronomy and Physics' in an essay that not only takes up a specific problem and text but also returns to more general questions concerning the 'standards' of inquiry in the *De caelo* [163–185].

Two essays relate the *De caelo* to Aristotle's biological works and in so doing also raise methodological questions. In '*De caelo* 2.2 and Its Debt to the *De incessu animalium*' [187–214], J. G. Lennox characterizes Aristotle as 'a committed empiricist' [210] and concludes that he

> is providing an object lesson in empirical cosmology, countering the approach found in Plato's *Timaeus* and in Pythagorean doctrine. [212]

M. Leunissen's 'Why Stars Have No Feet: Explanation and Teleology in Aristotle's Cosmology' [215–237, esp. 234–235] argues that the *De*

caelo does in fact utilize the teleological principles found in the biology but with some differences.

As the opening essays look back to Plato, the concluding essay, 'The Astrologization of the Aristotelian Cosmos: Celestial Influences on the Sublunary World in Aristotle, Alexander of Aphrodisias, and Averroes' [239–281] by Gad Freudenthal, looks forward to the late Greek and Medieval traditions. Although not mentioned in the title, this essay includes references to the Jewish philosophers Gersonides and Maimonides [cf. 241, 244–45, 274]. The final paragraph of Freudenthal's essay is the final paragraph of this volume and it opens with a grand sweep:

> The totality of medieval natural philosophy in the Aristotelian tradition posited the existence of celestial influences on the sublunary world. [274]

Thus, while these essays are diverse in their particular interests and claims, they are in a sense united: they share interests in issues of methodology, in the historical origins and reach of Aristotle's *De caelo*, and in their close and careful readings of the text.

The extensive bibliography that follows the 10 essays in this volume is divided into four sections:

A. Medieval and early modern manuscripts [283–285]
B. Modern critical editions of Aristotle's *De caelo* [285–287]
C. Commentaries and translations, which is in its turn subdivided into four sections [287–291], and
D. Modern monographs, collected studies, and articles on Aristotle's *De caelo* [291–298].

This bibliography constitutes a special gift to scholars above and beyond the interest of the essays. It makes this volume of value not only to those interested in the *De caelo* but to anyone interested in Aristotle's philosophy of nature more generally. Four indices—an index of passages cited [299–311], an index of subjects [313–317], an index of modern authors [319–320], and an index of ancient and medieval authors [321]—complete the volume and also enhance its value.

Theophrastus and His World by Paul Millett

Cambridge Classical Journal: Proceedings of the Cambridge Philological Society, Supplementary Volume 33. Cambridge: The Cambridge Philological Society, 2007. Pp. x + 188. ISBN 978–0–906014–32–5. Cloth $90.00

Reviewed by
Ivo Volt[1]
University of Tartu, Estonia
Ivo.Volt@ut.ee

The relative thinness of this book is deceptive, as the amount of information it offers is enormous. The 117 pages of main text are supplemented by 41 small-print pages of endnotes, altogether 314 of them, offering innumerable details from textual remarks to bibliographical data and, of course, various suggestions and interpretations. This makes the reading of the whole 'essay' (as the author himself repeatedly calls his book) quite an enterprise even for someone who is fairly well acquainted with scholarship on Theophrastus.

From the title of the book, readers might expect a broad study on Theophrastus and his scholarly contribution, including the field for which he is most famous, viz. botany. Thus, for those interested in the history of natural sciences, it could be disappointing to find out that the book is rather a full-length treatment of Theophrastus' *Characters*, which, as the sleeve-note mentions, 'aims to locate this influential work with respect to the political and philosophical worlds of Athens in the late fourth century'. It does contain a few references to Theophrastus' other scientific work but the focus is clearly on the *Characters*.[2] At that, Millett's study is a must for everyone dealing

[1] The reviewer apologizes for the lateness of this review.

[2] Thus, in his review, John Scarborough [2009] suggests that the book is mistitled: 'More indicative of Millett's assured readings of the *Characters* might be "Theophrastus' *Characters*. Reflections on Habits and Personalities in Fourth-Century Athens".' It will be seen, however, that the book is about much more than fourth-century Athens. An alternate title would rather have been 'Theophrastus and Our World'.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 212–224

with the topic and certainly one of the most important books ever published on the *Characters*.

In a way, it can be seen as a supplement to the massive edition with commentary of the *Characters* by Millett's Cambridge colleague, James Diggle [2004]. Millett touches upon almost every aspect of the *Characters*, including chapters on the reception of Theophrastus in the widest possible sense.

The book is divided into 12 chapters each focusing on different aspects of the *Characters* and the wider context of the work. Chapter 1 ('The *Kairos* of the *Characters*?') is an introduction to the study and contains a useful synopsis of what follows. As Millett himself notes [2], his preoccupation with the *Characters* goes back to his earlier work *Lending and Borrowing in Ancient Athens* [1991]. Indeed, many important points on the *Characters*, especially those dealing with credit relations between Athenian citizens, have been taken over from that book. *Theophrastus and His World* contains, however, a more systematic exploration of the historical possibilities of the *Characters*. Millett rightly emphasizes the need to contextualize the *Characters*, not just by literary genre, although this is important, but by reading the *Characters* against the background of Theophrastus' other surviving works and fragments. We still lack a full-range treatment of the whole 'world of Theophrastus' as it emerges from the latter. Thanks to Project Theophrastus, initiated in 1979 by William W. Fortenbaugh, we now have at least a modern edition of Theophrastus' fragments and *testimonia* [1993] with commentary volumes on various topics appearing since 1995.[3] Millett uses this corpus throughout the book, although he is well aware of the limitations in drawing conclusions on the basis of such scanty evidence. In addition, any work on the *Characters* is made more difficult by the textual corruption in the piece itself. As Millett notes [3], '[v]irtually everything about the text as transmitted is problematic.'[4]

[3] In addition, members of the Project have published minor works of Theophrastus in separate editions as well as the fragments of other members of Aristotle's school. To the list given on page 119, add Fortenbaugh and White 2006. The remarks on Ariston [12, 123n4] would have profited from the discussion in this volume.

[4] One should note, however, that it is not correct to say with Millet [3] that the definitions have been identified as Byzantine additions.

In chapter 2 ('Theophrastus of Eresus and Theophrastus Such'), Millett turns to various later imitations of Theophrastus' *Characters*, focusing, as the title suggests, on George Eliot's *Impressions of Theophrastus Such* (first published in 1879). This work, Millett argues, may provide clues to reading the Theophrastean original. He notes several half-hidden allusions to Theophrastus, demonstrating how Eliot exposes her scholarly familiarity with the original Theophrastus. But what can Eliot's text tell us about the *Characters* of Theophrastus? Millett argues [5] that one of the issues emerging from her reworking is the implicit ideology shared by Theophrastus and his original intended audience. Thus, as Eliot's reworking is based on the idea of an imagined audience or readership, 'a group which has its identity defined and solidarity strengthened by informed engagement with the text', so too the absence of any moralizing guidance in the *Characters* may be explained by an understanding common to Theophrastus and his intended audience [19]. Important here is Millett's disagreement with Diggle's view [2004, 12] that 'the work lacks all ethical dimensions'. As Millett emphasizes [127n69], the ethical elements are 'implicit in the understanding common to author and original audience'.

The next chapter ('Theophrastus the Metic') focuses on Theophrastus' headship of the Lyceum in Athens, where he was a metic. Millett first tries to reconstruct the setting of the Lyceum in later fourth-century Athens, relying on one of the most important texts that we have on this topic, viz. Theophrastus' will preserved by Diogenes Laertius [*Vitae* 5.53–54]. He emphasizes [20–21] Theophrastus' concern with securing personal bonds between his followers, which aims at securing the future well-being of the school. This includes references to some of the key terms, such as κοινωνία, φίλοι and οἰκεῖοι. The sense of community may, as Millett shows [21ff.], be reinforced by the location and configuration of the Lyceum itself, since more recent excavations would seem to put the Lyceum rather more remotely from the city centre of Athens than believed so far. Millett concludes that the 'metic' character of the Lyceum, somewhat 'disassociated from the civic mainstream' may support the idea of a 'heightened sense of community'. The cooperative ideal of the Lyceum is also evident in Theophrastus' will, the phrasing of which reflects the strengthening of communal institutions [27].

Chapter 4 ('That's Entertainment?') is important in that it deals with an old controversy over whether the *Characters* should be regarded as entertainment or not. The work is indeed very difficult to position and it has, over time, been connected with various areas of study, e.g., ethics, comedy, and rhetoric. As noted above, Millett does not agree with Diggle's claim that 'the work lacks all ethical dimensions' and, in my opinion, rightly so. As in the previous chapter, Millett here too [31] emphasizes the implicit ethical elements: 'Shared experience and expectations provided an implicit moral commentary on the *Characters*, which later generations have found it necessary to supply for themselves.' He tries to supply occasions for the *Characters* that combine its veiled ethical content with obvious entertainment value. This is done by connecting the humorous effect of the sketches to the shared values of their audience (whether in lectures or during more informal gatherings), which Millett envisages as a community of wealthy scholars (or would-be scholars) confirming and reinforcing their sense of solidarity. The humor of the *Characters* is further associated with caricature. This is important to keep in mind for the historians who engage with the text, as the essence of caricature is exaggeration, which makes the use of the *Characters* as a historical source a very tricky business.

Some of these issues are further explored in chapter 5 ('They Do Things Differently There?'). Here, Millett focuses on possible 'Rules of Evidence' relevant to the *Characters* and relations between the context of the work and historical events or social practices. To begin, he touches upon the issue of similarity and difference between ancient (specifically Theophrastean) and contemporary character. He notes [43] that overemphasis on familiarity of the types (which typically focuses on specific actions performed by them) 'may hinder appreciation of what is arguably different and distinctive'. The first problem that we run into is the meaning of the titles of the sketches, be they abstract terms or agent nouns. Usually, it is assumed that there is some enduring, core meaning in each of these 30 words but attempts to find this are bound to fail. Millett himself seems to favor the 'polythetic classification' suggested by Rodney Needham, according to which such characteristics are to be considered as collections of overlapping attributes (Wittgenstein's 'family resemblances' or *Familienähnlichkeiten*) with no single attribute necessarily common to every usage [see Needham1975]. Another argument against

the straightforward universality of all 30 character-traits is the range of various terms used in translating them. (In appendix 1, 'Naming the *Characters*' [159–164], Millett presents a selection of renderings of the titles of Theophrastus' sketches in 10 editions and translations, including, in addition to English ones, two French translations.[5]) In addition to the timeless reading of the *Characters*, there is the time-specific approach which tries to relate the content of the *Characters* to events in Athens. Here, too, alternative readings are possible, not least because of uncertain time of composition of the *Characters*. An important aspect is the perseverance of democratic ideas and ideals in Hellenistic times, especially in New Comedy, '[t]he remarkable stability of Athens's democratic ethos' as argued by Susan Lape [2004, 60]. This thesis, Millett argues [46], helps 'to explain the inclusion in the *Characters* of democratic institutions as essential parts of Theophrastus' frame of reference' and 'reinforces the idea of Theophrastus as concerned with exploring appropriate behaviour in a polis that was still essentially egalitarian in outlook'.[6] Millett also notes [46] that all too frequently the *Characters* are treated 'as footnote-fodder for studies of "everyday life" in Athens, supplying more-or-less colourful snippets of information'. Rather, the *Characters* promote 'different readings (in some cases radically different) of Athenian religion, politics, economy and society, and warfare' [48]. The chapter ends with a more detailed discussion on interpreting the figure of the ἄγροικος ('the rustic', but also 'the boor', or 'Country Bumpkin' as Millett translates it in the *Characters*).

In chapter 6 ('Corruption and the *Characters*'), Millett rightly emphasizes, as others have done before, the notion of norm-reversal as a central aspect of the *Characters*. This consists in reinforcing general principles of conduct by stating instances of the opposite. From these sequences of transgressed norms, the norms themselves

[5] The addition of the French titles ('out of historical interest' [3]) is somewhat odd, though—why not add other languages or limit the selection to English renderings? (There may be historical reasons for including La Bruyère, but Navarre?) I fully agree with Millett, however, when he writes ([3] that '[t]hese changing perceptions and representations of moral values constitute in themselves a fragment of modern cultural history.' Indeed the translation history of the *Characters* deserves a study in its own right.

[6] Millett refers to the important article by Hartmut Leppin [2002], who also argues for a persistent democratic mentality in early Hellenistic Athens.

may be reconstructed [52]. In the case of Theophrastus, Millett argues, this transgression occurs with respect to various occasions and institutions relevant to living in Athens 'as an upper-class member of the citizen-élite,' and the norms are 'standards of behaviour aspired to by Theophrastus and his philosophically inclined audience'. Thus, he concludes, the *Characters* looks like

> an implied code of behaviour written (unlike oratory and drama) for an élite audience, engaging with the practicalities of their lives within the polis. The implied end-product of this process of norm-reversal might resemble the Peripatetic ideal of a citizen, tempered to suit the circumstances of a democratically-minded polis. We could label him 'Theophrastus' Man'. [52]

For the historian, Millett suggests, this can be a frame for analyzing material in the *Characters*. In what follows, Millett turns to a book by P. Horden and N. Purcell, *The Corrupting Sea* [2000] to assess two of its key hypotheses in conjunction with Theophrastus and his *Characters*. These are the fragmentation of the Mediterranean world into connected micro-regions or micro-ecologies, and the ubiquity of honor and shame as underlying and distinctive Mediterranean values.

Chapter 6 is also one of the places where Millett passingly mentions Theophrastus' scientific writings, especially those on plants. He emphasizes the geographical scope of Theophrastus' work,[7] which includes relevant information coming all the way from Middle East and, at the other extreme, evidence from various parts of the city of Athens or even a single plane-tree near the Lyceum. Millett notes that Theophrastus in his early career could be seen as a paradigm for personal mobility, and the *Characters* is the only work to survive by Theophrastus with an obviously Athenian focus [53–54]. He argues, however [54–55], that there is a strong pattern of connectivity in the work's 'interplay of city and country within the Athenian polis, intrusion of the wider world into the city of Athens, and outreach of Athenian interests and involvement'. This connectivity furthers the sense of civic identity but also has 'a strong ethical dimension' [57].

[7] An important study on this ('The World of Theophrastus') was published in 1994 by P. M. Fraser and, as Millett acknowledges [119n8], it overlaps with 'a small though crucial corner' of his own conception of Theophrastus' world as described in ch. 6.

Chapter 7 ('Honour Bright') is concerned with the regulation of honor and shame, first of all as exemplified/anticipated by the sketch of the slanderer [*Char.* 28]. Millett introduces the ongoing debate on 'honour and shame', leading the reader through relevant discussion in Horden and Purcell [n10], Bernard Williams' *Shame and Necessity* [1993] and other studies on the topic. While Horden and Purcell tentatively extend a countryman's sense of shame and excellence to the less well-off citizen of a Greek city-state in the age of Aristotle, the *Characters*, Millett notes, 'complement this picture, offering a glimpse of the value-system appropriate to a better-off, though not narrowly aristocratic group of imagined citizens' [60]. Millett also presents a synopsis of an anthropological treatment that is chosen as a point of departure by Horden and Purcell for their analysis of honor and shame in the Mediterranean, viz. J. K. Campbell's classic study *Honour, Family and Patronage* [1964], which is based on fieldwork among a Greek shepherd community (the Sarakatsani) in the mid 1950s. Millett selectively re-presents themes from this book and notes that these worlds show clear signs of convergence with regards to honor and shame.

In the following chapters, Millett explores ways in which honor and shame are expressed and manipulated in the *Characters*, constructing systems of etiquette appropriate for the home, the streets, and other public places where individuals were on display. In chapter 8 ('Etiquette for an Élite: At Home'), Millett emphasizes, among other things, that the overall impression that we get from the *Characters* is of Athens 'as a collection of highly public places, where individuals were perpetually on display and open to assessment' [71]. This he connects with the idea (originally developed by Jacob Burckhardt) of the *agonal* or competitive ethos, which is seen as central to the Athenian civic experience. Following Simon Goldhill's discussion in his 'Programme Notes'—not 'Performance Notes'!—which introduce the essays in *Performance Culture and Athenian Democracy* [1999], Millett briefly analyzes, in the context of the *Characters*, four Greek terms that are considered to underpin the notion of performance in the context of Athens' democratic culture: ἀγών (contest), ἐπίδειξις (display), σχῆμα (appearance), and θεωρία (spectating) [72 f.]. Θεωρία, he notes (following Goldhill 1999, 73),

> can also encompass the philosopher's contemplative view of the world, which is arguably the gaze appropriate to the author and intended audience of the *Characters*.

In connexion with this, Millett once again presents his view of the *Characters*' original purpose. He writes:

> Theophrastus invites his pupils, who constitute a group theoretically beyond citizenship, to contemplate the behaviour of a group of citizens engaged with Athens' resiliently democratic ideology, evaluating their behaviour in terms of shared Peripatetic outlook and also the imagined response of the Characters' fellow citizens. [73]

In what follows, Millett uses location as the principle to order the shame-incurring performances by the characters. He discusses both the etiquette of master-slave relations and relations between free members of the household, including women.

Chapter 9 ('Etiquette for an Élite: Away') continues the discussion started in the previous chapter. Millett notes [83] that in public places of Athens there was a uniform code of behavior which applied to wealthier citizens, viz. 'judging and being judged'. He describes this preoccupation with appearance by the example of several characters (this includes, e.g., the using of clothes and shoes as 'accessories to character', the etiquette of closely encountering people and conversation in general, the latter being an especially important aspect of the *Characters*). Based on Peter Burke's conversational characteristics [1993], Millett analyses the way in which various characters destroy the conversational intimacy, ignore the principle of conversational cooperation, over-exploit conversation as a competitive encounter, or disrupt the equality in speaker-rights [85 ff.]. He emphasizes that the city with its built environment offered a lot of opportunities for enhancing or diminishing honor. These include public buildings and spaces but also non-civic architecture, which all allowed for ἐπίδειξις—barber shops, public baths, θαύματα or street entertainments, gymnasia, theatre, various religious ceremonies, the Assembly, and law courts.

Chapter 10 ('Face to Face in the Agora') is specifically dedicated to the Agora as a scene which exemplifies the complex of interpersonal relations in Theophrastus' Athens, involving citizens and others. The Agora was 'a zone of intense and visible interaction' [93], which

was often but not exclusively focused on buying and selling. This interaction, Millett argues, 'involving the detailed etiquette of exchange, has ramifications for the wider relations between citizens and friends' [94]. It often well demonstrates interplay of status, exchange, reciprocity, and personal relations. Millett concludes that '[t]he Characters repeatedly shame themselves and incidentally threaten the community of relations by undermining the ideology of reciprocity on which personal relationships depended' [95]. This is highlighted in the institution of *eranos* loans, which were interest-free contributions collected from friends in time of need.[8]

In chapter 11, 'Conspicuous Co-operation?', Millett measures the attitude of the Theophrastean types towards work and leisure against Thorstein Veblen's book *The Theory of the Leisure Class* [1899]. He focuses on the unnamed character sketch which is traditionally part of the description of the obsequious man [*Char.* 5.6–10] and which Millett calls 'Conspicuous Consumption'. Briefly, Veblen in his book opposes the productive and useful society with the ostentatious and honorific. Taking this into account, Millett notes that conspicuous consumption can be seen as 'high-profile waste of valuable resources in the competition for respectability' [100], which is marked out by its 'blending of economically unnecessary expenditure with maximum publicity' [101]. Millett argues that the divergence of views held by Veblen and Theophrastus proves ultimately more illuminating than the similarities. According to Veblen, he notes, 'the fault was embedded in society, an innate feature of the class of consumers' [101–102]. For Theophrastus, however, 'inappropriate consumption arose out of individual moral failure, the result of avoidable deviation from the mean, bringing shame on the person concerned' [102]. It is also worth mentioning that there was nothing reprehensible about leisure in itself so far as this was 'legitimate leisure', which included the possession of slaves and (presumably) the economic exploitation of slave labor. Millett further discusses the divergence between Veblen's negative observations on domestic servants and the presentation of slaves in the *Characters*. An interesting part in Veblen's discussion is devoted to classical learning as

[8] For *eranos* loans, see Millett 1991,153–159.

a distinct form of conspicuous consumption [103].[9] An analogy in antiquity that Millett points out is 'the leisured discipline of learning philosophy' [104]. At this point, Millett establishes limits to the analysis presented so far, emphasizing that we would need a lot more textual material to recreate the 'realities' of interpersonal relations in the fourth-century Athens, and that the picture that we get from the *Characters* is 'necessarily restricted but significant in its specificity' [105]. The implied code of conduct exemplified through the 'notional model' of 'Theophrastus' Man' is highlighted once again [cf. 52]. Millett further compares this model with the Peripatetic ideal as arguably exemplified by Aristotle's μεγαλόψυχος or 'Great-Hearted Man' [105]. Here, he adopts Michael Pakaluk's interpretation [2004] of Aristotle's μεγαλόψυχος, providing support for his sceptical views by analyzing the differences between the μεγαλόψυχος and a typical Theophrastean character. He suggests that

> [w]hat emerges from Characters' actions is not how to be a good man; nor even necessarily how to be a good citizen. Rather the message is how to be good at being a citizen in the context of a democratic polis. [109]

The final chapter ('Theophrastus Nonesuch') restates the ideas developed in the book. Millett recalls that although opinions about the *Characters* have, for the most part, been favorable, there have been dissenting voices that criticize the work for its lack of originality or of psychological subtleness. As he rightly observes, this kind of assessment shows 'how, in the absence of any explicit guidance from its author, readers of the *Characters* need to construct their own frame of reference' [111]. Indeed, he admits that most of his book has been concerned with trying to offer an alternate range of contexts and settings within which the *Characters* might be read, moving beyond the usual literary or philosophical backgrounds [112]. In this, the first key notion is 'performance culture', which the author has extended to the actions of the types depicted in the *Characters*. The second emphasis is on the whole corpus of writings by Theophrastus, which means more extensive use of the fragmentary evidence now made easily accessible through the publications of Project Theophrastus.

[9] See 154n281 for an interesting excursus into Veblen's own educational background and his relation to classical studies.

Other Greek texts (Aristotle, Menander) have been used for contextualizing the *Characters*; and a selection of non-classical texts have been read against the *Characters*, helping to establish its distinctive qualities and providing frameworks for assessing various aspects of the world of Theophrastus. In the final chapter, Millett also evokes another modern study, viz. Elias 2000 on the sociological significance of etiquette. He notes [114] that aspects of Elias' method correspond to his own concern with the *Characters*, giving as an example Elias' 'close reading of "manner books" to demonstrate the process whereby actions, apparently trivial in themselves, may acquire far broader social significance' [114].

Millett closes the final chapter with some speculations on the ongoing appeal of the *Characters*, a work that 'has over recent centuries moulded perceptions of Athenian culture both classical and early Hellenistic' [115]. He argues that

> the text might be read as a practical commentary on living, according to Peripatetic principles, in a democratically oriented polis, with behaviour calculated to reinforce its positive values. [116]

However, 'the direct and near-universal appeal of the *Characters* (not just to historians) remains largely unexplained' [116]. Indeed.

In addition to appendix 1 on naming the characters (see above), there are two more appendices. Appendix 2 presents a three-piece set of Theophrastean imitations from an issue of the *Punch* magazine from the year 1901 ('The New Publisher', 'The New Journalist' and 'The New War Correspondent'), while appendix 3 briefly studies classical allusion in Thackeray's *Book of Snobs*.

The world of the *Characters* could be widened almost endlessly and it is quite understandable that at some point the study has to come to an end. Millett tries to present us with almost every detail and aspect of Theophrastus' life and work, at least as regards the *Characters*. This means that relevant information can pop up everywhere in the book, main text or notes, sometimes distracting the line of thought. One can see a wish to incorporate every piece of information to a place most suitable for it, but sometimes one longs for more space.

Typographically the book would certainly have profited from slightly bigger print and larger margins. The endnotes, at least, are

numbered consecutively, which makes it easier for the reader to consult them. Unfortunately, there is only one index, which contains references to texts by and about Theophrastus. Certainly, a general index would have been helpful in guiding the casual browser through the wealth of information in the book.

A book so detailed is bound to contain some misprints but these are not many. I note, e.g., a few mistakes in the publication titles, especially the German ones:

172 Bolkestein 1929: 'religiongeschichtliche' for 'religionsgeschichtliche'
174 Fortenbaugh 1975: 'Verhaltensregelmassigkeit' for 'Verhaltensregelmässigkeiten'
174 Fortenbaugh 1998: 'Philosophischer texte' for 'philosophischer Texte'
177 Leppin 2002: 'Burgermentalität' and 'Ubergang' for 'Bürgermentalität' and 'Übergang'
180 Ribbeck 1882: 'Betrag' for 'Beitrag'.

As has been said above, the book is a must for everyone dealing with the *Characters* of Theophrastus; but it is also important as an example of analytical reception history. Millett does not give any reasons for his selection of texts: indeed he notes on page 117, that '[t]here is nothing in the least authoritative about the choice of modern texts against which I have tried to read the *Characters*.' However, the wealth of information that one gets from reading the *Characters* against these modern texts, but even more so the amount of further important questions that arise from this process of contextualizing, is what makes Millett's study especially relevant to social historian and the lover of character writing alike.

BIBLIOGRAPHY

Burke, P. 1993. *The Art of Conversation*. Cambridge.

Campbell, J. K. *Honour, Family and Patronage*. Oxford.

Diggle, J. 2004. *Theophrastus,* Characters *Edited with Introduction, Translation and Commentary*. Cambridge Classical Texts and Commentaries 43. Cambridge.

Elias, N. *The Civilizing Process: Sociogenetic and Psychogenetic Investigations*. E. Dunning, J. Goudsblom, and S. Mennell edd. E. Jephcott trans. Rev. edn. Oxford.

Fortenbaugh, W. W.; Huby, P. M.; Sharples, R. W.; and Gutas, D. 1993. edd. *Theophrastus of Eresus: Sources for His Life, Writings, Thought and Influence*. Philosophia Antiqua 54. 2 vols. Leiden.

Fortenbaugh, W. W. and White, S. A. 2006. edd. *Aristo of Ceos: Text, Translation, and Discussion*. New Brunswick/London.

Fraser, P. M. 1994. 'The World of Theophrastus'. Pp. 167–191 in S. Hornblower ed. *Greek Historiography*. Oxford.

Goldhill, S. 1999. 'Programme Notes'. Pp. 1–32 in S. Goldhill and R. Osborne edd. *Performance Culture and Athenian Democracy*. Cambridge.

Horden, P. and Purcell, N. 2000. *The Corrupting Sea: A Study of Mediterranean History*. Oxford.

Lape, S. 2004. *Reproducing Athens: Menander's Comedy, Democratic Culture, and the Hellenistic City*. Princeton, NJ.

Leppin, H. 2002. 'Theophrasts „Charaktere" und die Bürgermentalität in Athen im Übergang zum Hellenismus'. *Klio* 84:37–56.

Millett, P. 1991. *Lending and Borrowing in Ancient Athens*. Cambridge.

———— 2007. *Theophrastus and His World*. Cambridge.

Needham, R. 1975. 'Polythetic Classification: Convergence and Consequences'. *Man* 10:349–369.

Pakaluk, M. 2004. 'The Meaning of Aristotelian Magnanimity'. *Oxford Studies in Ancient Philosophy* 26:241–75.

Scarborough, J. 2009. rev. Millett 2007. *Bryn Mawr Classical Review* 2009.10.55.

Veblen, T. 1899. *The Theory of the Leisure Class*. New York. Repr. Amherst, NY, 1998.

Williams, B. 1993. *Shame and Necessity*. Berkeley, CA.

The Oxford Handbook of the History of Mathematics edited by Eleanor Robson and Jacqueline Stedall

Oxford/New York: Oxford University Press, 2009. Pp. vii + 918. ISBN 978–0–19–921312–2. Cloth £90.00, $150.00

Reviewed by
Jens Høyrup
University of Roskilde
jensh@ruc.dk

The editors express in the introduction the 'hope that this book will not be what you expect' [1]. The reviewer's task being to make the reader aware of what should be expected, let it none the less be said straightaway that the book is very good but definitely no handbook. Indeed, as the editors adequately explain next, it is

> not a textbook, an encyclopedia, or a manual. If you are looking for a comprehensive account of the history of mathematics, divided in the usual way into periods and cultures, you will not find it here. Even a book of this size is too small for that, and in any case it is not what we want to offer. Instead, this book explores the history of mathematics under a series of themes which raise new questions about what mathematics has been and what it has meant to practice it. The book is not descriptive or didactic but investigative, comprising a variety of innovative and imaginative approaches to history.

It thus contains 36 paradigmatic examples of questions and approaches that can be applied to the topic—with one exception (on which below) all being good or very good. They are ordered (but after they were received by the editors) into nine groups with four in each, these nine groups being themselves grouped three by three. A complete list will give an adequate impression of the scope of the book, first of all, but not only, of its geographical and temporal reach:

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 225–232

I. *Geographies and Cultures*
1. Global
    1.1 'What was Mathematics in the Ancient World? Greek and Chinese Perspectives'—G. E. R. Lloyd
    1.2 'Mathematics and Authority: A Case Study in Old and New World Accounting'—Gary Urton
    1.3 'Heavenly Learning, Statecraft, and Scholarship: The Jesuits and Their Mathematics in China'—Catherine Jami
    1.4 'The Internationalization of Mathematics in a World of Nations, 1800–1960'—Karen Hunger Parshall
2. Regional
    2.1 'The Two Cultures of Mathematics in Ancient Greece'—Markus Asper
    2.2 'Tracing Mathematical Networks in Seventeenth-Century England'—Jacqueline Stedall
    2.3 'Mathematics and Mathematics Education in Traditional Vietnam'—Alexei Volkov
    2.4 'A Balkan Trilogy: Mathematics in the Balkans before World War I'—Snezana Lawrence
3. Local
    3.1 'Mathematics Education in an Old Babylonian Scribal School'—Eleanor Robson
    3.2 'The Archaeology of Mathematics in an Ancient Greek City'—David Gilman Romano
    3.3 'Engineering the Neapolitan State'—Massimo Mazzotti
    3.4 'Observatory Mathematics in the Nineteenth Century'—David Aubin

II. *People and Practices*
4. Lives
    4.1 'Patronage of the Mathematical Sciences in Islamic Societies'—Sonja Brentjes
    4.2 'John Aubrey and the "Lives of Our English Mathematical Writers" '—Kate Bennett
    4.3 'Introducing Mathematics, Building an Empire: Russia under Peter I'—lrina Gouzévitch and Dmitri Gouzévitch
    4.4 'Human Computers in Eighteenth- and Nineteenth-Century Britain'—Mary Croarken

5. Practices
    5.1 'Mixing, Building, and Feeding: Mathematics and Technology in Ancient Egypt'—Corinna Rossi
    5.2 '*Siyaq*: Numerical Notation and Numeracy in the Persianate World'—Brian Spooner and William L. Hanaway
    5.3 'Learning Arithmetic: Textbooks and Their Users in England 1500–1900'—John Denniss
    5.4 'Algorithms and Automation: The Production of Mathematics and Textiles'—Carrie Brezine
6. Presentation
    6.1 'The Cognitive and Cultural Foundations of Numbers'—Stephen Chrisomalis
    6.2 'Sanskrit Mathematical Verse'—Kim Plofker
    6.3 'Antiquity, Nobility, and Utility: Picturing the Early Modern Mathematical Sciences'—Volker R. Remmert
    6.4 'Writing the Ultimate Mathematical Textbook: Nicolas Bourbaki's *Élements de mathématique*'—Leo Corry

III. *Interactions and Interpretations*

7. Intellectual
    7.1 'People and Numbers in Early Imperial China'—Christopher Cullen
    7.2 'Mathematics in Fourteenth-Century Theology'—Mark Thakkar
    7.3 'Mathematics, Music, and Experiment in Late Seventeenth-Century England'—Benjamin Wardhaugh
    7.4 'Modernism in Mathematics'—Jeremy Gray
8. Mathematical
    8.1 'The Transmission of the *Elements* to the Latin West: Three Case Studies'—Sabine Rommevaux
    8.2 '"Gigantic Implements of War": Images of Newton as a Mathematician'—Niccolo Guicciardini
    8.3 'From Cascades to Calculus: Rolle's Theorem'—June Barrow-Green
    8.4 'Abstraction and Application: New Contexts, New Interpretations in Twentieth-Century Mathematics'—Tinne Hoff Kjeldsen
9. Historical
    9.1 'Traditions and Myths in the Historiography of Egyptian Mathematics'—Annette Imhausen

9.2 'Reading Ancient Greek Mathematics'—Ken Saito
9.3 'Number, Shape, and the Nature of Space: Thinking through Islamic Art'—Carol Bier
9.4 'The Historiography and History of Mathematics in the Third Reich'—Reinhard Siegmund-Schultze

The understanding of mathematics is very broad; almost anthropological, it encompasses the whole range of mathematical practices within a society or a professional group. The topics dealt with thus reach from Inca and late medieval Italian bookkeeping [ch. 1.2], from ancient Greek and Roman surveying and geometric planning of a racecourse in Corinth [ch. 3.2], from the algorithms of weaving patterns in the Andes [ch. 5.4], and from the teaching of basic arithmetic [chs 2.3, 3.1, 5.3] to the mathematics of astronomical observatories and computation [chs 3.4, 4.4], to Newton's and others' understanding of what was really important in his infinitesimal work [ch. 8.2], to the problems inherent in the concept of mathematical 'modernism' [ch. 7.4], to the Bourbaki project [ch. 6.4], and to how the theories of convex sets and non-linear programming are connected to the individual and institutional aims of workers [ch. 8.4].

Approaches are varied, as a natural consequence of the editors giving

> authors a broad remit to select topics and approaches from their own area of expertise, as long as they went beyond straight 'what-happened-when' historical accounts. [1]

Fortunately, (after all, the *sine qua non* of historiography is knowledge of what happened when), the actual chapters contain all the often unfamiliar factual information needed to support the argument and to undermine myths; and they are happily free of freewheeling proclamations of principle. They are indeed good paradigmatic examples, convincing by the quality of their reasoning.[1] For example, many of us may (in these or other words) know the description of Czar Peter the Great behaving like

[1] Explicit methodological reflections are certainly not absent, and sometimes extensive and profound [e.g., ch. 7.4, 9.4]; but when present, they are well integrated with the subject matter.

> a savage visiting a supermarket who, fascinated by the riches on display, shovels everything into his basket without knowing whether he needs it or not

when acquiring important Western European science [354, citing W. Berelowitch, in ch. 4.3]. This is shown by Irina and Dmitri Gouzévitch to be totally false, in a paper which combines a large amount of well-digested biographical information about Peter (as well as about the Western European mathematicians that were hired for his project) with information about the preceding state of mathematical knowledge in Russia, about Russian metrology and orthography old and new, about the difficulty of creating a lay publishing institution, and about the character of the books translated (a character that changes during the development of the project and in step with the changing military challenges), and about still more.

On other topics, the reader may be even less prepared. How many of us, for example, even among those with some familiarity with the mathematics of the Islamic world, know much about the *siyaq* number notation, developed from the Sassanian administrative numerical shorthand and used for administrative and accounting purposes from the Ottoman empire to India (and even further), from ᶜAbbasid times until the 20th century? After reading the chapter, we not only know about the script, its history, and its historical context; we also have material to reflect upon concerning the conditions of numeracy—conditions that are much more intricate than we believe, accustomed as we are to its being exclusively carried by the decimal place-value system.

Dependent rather on the selection of authors than on the task given to them is the opportunity to deal with the same historical situation from several different points of view—a perfect illustration of how different equally legitimate questions may be asked, and even of how different equally legitimate delimitations of 'mathematics' are possible. Classical Antiquity is thus dealt with in four chapters. In 1.1, Geoffrey Lloyd looks at the understanding of what mathematics meant within Greek elite culture (with an eye as well to Han and slightly later China). This necessarily restricts his discussion to the kinds of mathematics whose presentation makes up the bulk of Thomas Heath's *History of Greek Mathematics* [1921]—of extreme importance for later Islamic and European mathematics, but socially

a fringe phenomenon in its own times.[2] Markus Asper [2.1] takes up the existence of 'several coexisting and partly overlapping fields of mathematical practices', and discusses the socially much more important (though culturally subliminal) practical traditions with their probable roots in the Near East, those which Netz [2002] refers to with the pun 'counter culture'. David Gilman Romano [3.2] analyses the archaeological remains of one of the practical traditions, namely, the surveying of land and the geometrical planning of a racecourse in Corinth. Ken Saito [9.2], finally, returns to the mathematics 'of theorems' but in particular to the problem of textual criticism of the manuscript tradition, emphasizing how both the material possibilities (the difficulty of traveling between manuscripts before the railway, the opportunity to travel between or to send manuscripts after their construction, the new opportunities for comparison offered by microfilming, and so on) and the questions asked by different epochs affect what is seen in the texts.

Other multiple coverages concern Pharaonic Egypt [5.1, 9.1] and China [1.1, 7.1 and, at some distance, 1.3], similarly offering complementary perspectives. A couple of a different kind is offered by chapters 9.1 and 9.3—respectively Annette Imhausen's analysis of how a number of unfounded myths have developed (e.g., from Moritz Cantor's suggestion of what might have been the case until the repetition of the same as a fact), and Carol Bier's production of such myths [going the whole way from suggestion to factual assertion]. Bier's aim is to connect the culture of geometric patterns (which in fact distinguishes the Islamic world from other cultures) directly to some particular spirituality. Alone among contributors to the volume, she claims that the questions which she raises are the only good questions to ask.[3] The creation of the myth can be seen on page 834:

---

[2] Multiplying generously the total evidence by three, Reviel Netz [1999, 291] estimates that on the average at most one mathematician (active in this kind of mathematics) was born per year in the Greek, Hellenistic, and Roman world during the millennium under discussion.

Lloyd is well aware that there were other kinds of mathematical practice [see, e.g., 1992, 570f].

[3] 'However, the questions I think we should be asking are not about decoration and ornament, but about surfaces and the plane, about units and repeats, and about circles and the nature of two-dimensional space' [833]. Further on in the same paragraph, it is suggested that the apparently non-

> According to this line of thinking,[4] at some point between the eight and the eleventh century, Islamic ornament and its formal expression became connected to abstract ideas articulated in contemporary philosophy, mathematics, and religion.

Misleading use of evidence also abounds in the following pages.[5]

Fortunately, this unconvincing piece is the exception that puts the rest of the book in relief.

Until not very long ago, the historiography of mathematics was relatively untouched by what happened in the historiography of science at large. Obviously, this is no longer the case: the present volume presents perspectives as broad and as broadminded as what can be found in the best historiography of other sciences. There is strong interest in contexts of many kinds and in actors' aims, including their social aims. But the reduction of everything to image or career strategy in the style of 'Boyle being busy fashioning himself as a gentleman natural philosopher'—the new brand of externalism, unwillingly inviting the reader to ask himself what the author is busy doing—is as absent as the 'internalist' conviction that external conditions such as the social role of a mathematician or the very existence of a category 'mathematics' are perennial and, therefore, separable from the development of knowledge.

---

representational patterns might be 'representational in the deepest meaning of the word: a visual metaphor of relationships, of existence, of the cosmos, an expression of realities beyond that which can be merely seen'. If anything, this sounds Platonic or Neoplatonic rather than broadly Islamic—but no evidence for the suggestion is offered.

[4] *Scil.* the author's own speculation, unsupported by any source evidence except contemporaneity of decorative patterns and theoretical mathematics dealing with wholly different topics.

[5] One example must suffice. A *Qurʾan*ic passage [59:21: God is speaking to Muhammad],

> Had we sent down this *Qurʾan* on a mountain, verily thou would have seen it humble itself and cleave asunder for fear of God. Such are the similitudes that we propound to men that they reflect.

is inscribed on the 11th-century tomb towers of Kharraqan. Bier 'feels tempted' to see the demonstrative pronoun 'such' ('tilka') as pointing to 'the actual patterns depicted on the monuments'. But the reference is clearly to the preceding similitude: 'mathal' means 'likeness, metaphor, simile'.

All the way through, the exposition is transparent. Problems and concepts are well explained and only a minimum of background knowledge is presupposed. Not only historians of mathematics but anybody interested in the history of mathematics and in possession of academic training will enjoy and profit from reading the book.

Unfortunately, a final critical point needs to be made, imputable neither to the editors nor to the authors but to Oxford University Press. The technical quality of the book might be acceptable for a crime novel bought in the airport and meant to be discarded at arrival, but for a volume supposed to be read and consulted repeatedly it is a scandal. The reviewer's copy broke twice during the single reading and each time had to be glued together anew: the pages turned out to be neither sewn nor glued to some kind of linen. Libraries can only be advised to buy the paperback edition (according to a brief inspection of one specimen of better quality) and have it bound themselves. Others interested in possessing the book should definitely buy the paperback, at one third of the price of the hardback edition.

BIBLIOGRAPHY

Heath, T. L. 1921. *A History of Greek Mathematics.* 2 vols. Oxford.

Lloyd, G. E. R. 1992. 'Methods and Problems in the History of Ancient Science'. *Isis* 83:564–577.

Netz, R. 1999. *The Shaping of Deduction in Greek Mathematics: A Study in Cognitive History.* Cambridge.

_____ 2002. 'Counter Culture: Towards a History of Greek Numeracy'. *History of Science* 40:321–352.

La via delle acque (1500–1700). Appropriazione delle arti e trasformazione delle matematiche by Cesare S. Maffioli

Florence: Leo S. Olschki, 2010. Pp. xxii + 394. ISBN 978–88–222–6008–6. Paper € 43.00

Reviewed by
Paolo Palmieri
University of Pittsburgh
pap7@Pitt.edu

Cesare Maffioli has written a compelling book on the intellectual appropriation of the mechanical arts and the parallel transformation of mathematics in 16th- and 17th-century northern Italian society. The historical-documentary basis of Maffioli's research is a wealth of printed and manuscript literature relating to the so-called science of waters, a professional activity and a branch of mathematics which came of age in the Renaissance. The protagonists of this hydraulic revolution were Leonardo da Vinci, Gerolamo Cardanus, Galileo Galilei, Benedetto Castelli, Domenico Guglielmini, as well as many lesser scientists, engineers, and practical mathematicians.

While the profession of the architect-engineer was already in the 16th century much more structured (both socially and intellectually) than that of the mathematician, Maffioli reports that the traits that sanctioned the professional status of the mathematicians studied in his book were either the activity of teaching mathematics in some institutionalized form or of writing mathematical works. (A caveat, however, is added at the end of the book, where he states that it is also very hard to delineate the contour of the mathematical field at the turn of the 16th and beginning of the 17th century.) Maffioli notes that an epoch-making transformation occurred at that time in the mathematical field, a change to which, he argues, philosophers responded little. In essence, Maffioli maintains, when mathematics started to busy itself with philosophical and mechanical issues, Aristotelian philosophers were unwilling to recognize the same demonstrative value in mathematical proofs as they found in physical demonstrations. Maffioli sees an example of this tension in the difficulties

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 233–236

that philosophers had when trying to categorize Galileo's new mathematical physics of motion, a type of science which could not easily be interpreted as a form of mixed mathematics or as a mixed science.

The intellectuals' appropriation of the mechanical arts was a vast social and disciplinary movement. For example, Maffioli shows that for Galileo, Castelli, and Guglielmini, this appropriation meant not only the integration of fragments of practical knowledge into a mathematical framework but also the elevation of items of practical knowledge to the status of principles and fundamental concepts. There was also a normative dimension to the effect that they tended to direct engineers towards new ideas and solutions to practical problems.

The book's focus is on the social dynamics and tensions between the intellectuals and the practical experts. According to Maffioli, this aspect has generally been neglected in the historiography of the scientific revolution. The history of hydraulics allows the historian to cast a glance at fascinating controversies regarding the best way to regulate the flow of waters in rivers. Those controversies hint at epistemological discussions between engineers and architects on the one hand, and mathematical philosophers on the other, which center on the practical adequacy of the new theories espoused by mathematical philosophers such as Galileo and Castelli.

Another important finding that Maffioli brings to light is the so-called experimentation in the field. Mathematicians and technicians in the 16th and 17th centuries tended to regard machines, building sites, and the whole terrestrial globe as giant natural laboratories. According to Maffioli, this suggests that it is inappropriate to consider early modern experimentation as a phenomenon happening exclusively in specially dedicated spaces such as the purpose-built private laboratory, the learned academy, or the princely court.

The latter point relates to another interesting development in 16th- and 17th-century Italian hydraulics, namely, its analogy with the medical-naturalistic tradition. Hydraulics practitioners considered both landscape and the whole terrestrial globe as a complex system of interconnected parts which have to be studied not separately but synthetically. Maffioli also sees in the emergence of hydraulics the shaping of an embryonic form of ecological thinking. This ecological thinking figured prominently in the mathematical physicist and physician Guglielmini, for example. It aimed at recovering a harmonious

relationship between art and nature, so that the force of rivers was not to be opposed but instead regulated for the benefit of mankind.

Maffioli thinks that the history of Italian hydraulics raises two fundamental questions. Was the scientific revolution of the 17th century the result of the early Renaissance emphasis on art and technology, or did the scientific revolution become an intellectual movement that subverted the early Renaissance emphasis on art and technology by subordinating artists and practical men to the new mathematical natural philosophers? The central chapters of the book expound interesting details that go some way towards answering these questions. However, Maffioli wisely shies away from drawing a definitive conclusion. He contents himself with pointing out numerous current historiographic indeterminacies. An element of this puzzle to which Maffioli draws the attention of the reader is the role played by critics of the philosophical tradition such as Galileo. Yet Maffioli claims that no interpretation of the scientific revolution has been put forward so far, according to which the scientific revolution is cast as an intellectual appropriation of the mechanical and practical arts on the part of the philosophical tradition, an appropriation accompanied at the same time by a transformation of the social role of mathematicians. This line of interpretation of the emergence of the scientific revolution seems to be what Maffioli would favor, even though he does not develop this line of inquiry much further.

Maffioli's approach is thematic. He discusses the works and the activities of three key figures, namely, Cardanus, Galileo and Castelli, as well as other more or less well known people. He is keen to point out, though, that his account should not be construed as a linear progression but rather as an attempt to read the documents in a sort of neutral way which does not presuppose established historiographic categories.

One particular strength of Maffioli's book is the wide documentary basis on which his work is based. He has identified numerous manuscript sources in libraries and archives which have hitherto not been accurately studied or otherwise published. These manuscript sources demonstrate how much still remains to be done, as Maffioli comments in the conclusion. This strength is evident particularly in chapter 5 where he sets the historical-political scene of his inquiry. Maffioli describes the intriguing behind-the-scene wheeling and dealing of a failed attempt at a hydraulic policy on the part of pope Urban

VIII. Here we can catch a glimpse of how physics and mathematics competed for political legitimation in the heated debate between the Galilean Castelli and the Jesuit Nicolò Cabeo.

Finally, one might wonder how Maffioli's achievement might be regarded, especially from a methodological viewpoint, in relation to the broader field of the history, philosophy, and sociology of early modern science. Maffioli's work is an excellent example of the type of positivist historiography which affirms the primacy of documentary evidence as the basis for historic reconstruction. Still, this type of historiography starts from *a priori* assumptions about interpretive categories such as that of a 'scientific revolution' which have been questioned by historians and especially sociologists of science in recent decades. Perhaps even more radically, one might wonder whether positivist historiography is an adequate tool for approaching the advent of the hydraulic revolution, given all the engineering and practical intricacies, the blurred disciplinary contours, and obscure matters of politics that Maffioli's book masterfully portrays—all the more so, when one considers that ultimately the hydraulic revolution cannot be detached easily from the interpretive horizon of Maffioli himself as a historian of science. For many questions of method and interpretation arise. What is the role of the interpreter in selecting and evaluating the relevance of a set of documents? How can a set of written documents, be they printed books or manuscript notes, be related to events and people who acted and thought in a distant past? How can experiments be understood on the sole basis of a lacunose written historical record?

Perhaps Maffioli's lasting achievement will consist in raising our awareness of the conundrums that positivist historiography doggedly pursued in the history of science at the beginning of the 21st century.

Selling Science in the Age of Newton: Advertising and the Commoditization of Knowledge by Jeffrey R. Wigelsworth

Farnham, UK/Burlington, VT: Ashgate, 2010. Pp. xiv + 203. ISBN 978–1–4094–0075–2. Cloth $119.95

Reviewed by
Christopher Baxfield
University of Leeds
baxfield@hotmail.com

Accentuating Larry Stewart's notable argument [1992] that a flourishing science requires public acceptance, *Selling Science in the Age of Newton* suggests that interest in science in early 18th-century England was generated by advertising. Quite simply, as newspapers proliferated through the 1720s, so did advertising and so did interest in science. In this engaging book, Jeffrey Wigelsworth shows how an enormous range of science advertising from the establishment of the *Philosophical Transactions* in 1665 to Isaac Newton's death in 1727 made its appeal to Fellows of the Royal Society, Whig, Tory, entrepreneur, and layman. He emphasizes the public and popular nature of a mode of discourse that prevented any distinction between professional and amateur: science needed to be sold in a particular manner through particular strategies, and natural philosophers and their publishers had to be shrewd publicists.

The book is divided into five main chapters: the first three mark the chronological development of science ads until 1727, whilst the last two are particular case studies of the relationship between science advertising and name brand. After the introductory chapter 1, chapter 2 begins with the inception of the *Philosophical Transactions*. Here to be found are the first advertisements of scientific texts, published by a Henry Oldenberg desperate to supplement financially his unpaid presidency of the Royal Society. Any budding relationship between science and advertising in the *Philosophical Transactions* was cut short, however, by a combination of Oldenberg's death in 1677, the short lived replacement of the journal by the *Philosophical Collections*, brainchild of the vitriolic Robert Hooke who distributed only to personal friends, a general increase in printing costs, and the rapid

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2011) 237–240

expansion of weekly newspapers from 1695 after the lapse of the Licensing Act which had required governmental permission to publish. In this respect, Wigelsworth emphasizes how from a very early point science advertising became a very public exercise. He notes that this did not extend to a public political fracturing of science. In chapter 3, Wigelsworth shows that the many science ads published in the two notable political tri-weeklies through the tumultuous period between 1695 and 1720, the Tory *Post Boy* and Whig *Post Man*, were very similar. Whigs and Tories alike were interested and subscribed to the same Newtonian books and the same Newtonian lectures, and both bought the same sorts of natural philosophical ephemera. This gives 18th-century historians yet another reason to pause before accepting any strict alliance between Newtonianism and Whiggism.

Chapter 4, the rise of science advertising in daily newspapers through 1727, is the most ambitious of the book. Here Wigelsworth builds on the work of James Secord and Jon Topham, who have each argued that distinctions between 'popular science' and 'science' in the 19th century must be challenged, as all types of text are traces of communication acts. Wigelsworth extends this work backwards to the early 18th century, treating newspaper ads as science writing that used similar rhetorical means as the books they were selling. Advertisements most notably from the controversialist William Whiston and the public lecturers John Theophilus Desaguliers and both Francis Hauksbees were all crafted to pique particular interests about the natural world and to draw readers into that world through their participation in (often pricey) lecture courses and purchase of accompanying books and instruments. These ads were digests of predominant themes in Newtonian texts. Advertisements for lectures and books in physics, chemistry, and botany emphasized how experimental explanations of the world were favored by the Newtonians, as opposed to abstract mathematics. Ads for general encyclopedias on practical issues like gardening and animal husbandry suggested 'philosophical' approaches. The upshot is that the type of writing in the ads convinced the reader in a similar manner as technical books and this blurred any distinction between popular and professional natural philosophy.

The final two chapters focus on particular episodes in early 18th-century scientific advertising, showing how science was not just a commodity but a brand. Chapter 5 looks at the creation of the Board

of Longitude in 1714 to evaluate how longitude could be calculated most accurately and the many advertisements that offered solutions to the problem. Whilst we know that no solution was found until the 1760s, Wigelsworth suggests that the plethora of clocks, astronomical treatises and, in William Whiston's case, floating lighthouses, offered up in the name of 'Longitude' reveal how a particular concept was used as a brand that attracted the attention of the Royal Society, the Board of Longitude, and public consumers. Chapter 6 looks at name brands, focusing on the 1719 conflict between the publishers William Mears and John Woodward and the lecturer John Desaguliers, who had produced rival translations of Willem Jacob sGravesande's *Physices elementa mathematica.* At one level, both translations revealed a straightforward relationship between advertising, credibility, and brand name. Woodward and Mears advertised that the notable Oxford natural philosopher John Keill had corrected the Latin, whilst Desaguliers went further by adding his friend Isaac Newton's name. However, in this competition for sales, Desaguliers went further by advertising his own name as a brand of reliability and exactness, opposed to the dubiousness of Woodward and Mears who had produced a bad translation.

I was impressed by the range of sources used by Wigelsworth and found his writing clear and engaging. I did feel that some of the argumentation, particularly the idea that science advertising was representative of science writing, needed a bit more development. One of the most compelling elements of this book is the subtle duplicity of advertisers in selling consumers not quite what they wanted. Wigelsworth reveals on page 116 how ads for 'philosophical essays' were actually for sheet music and 'mechanical lectures', nothing more than dancing lessons. In this respect, any science writing found in the advertisements was quite different to that of the text or product sold. Indeed, Wigelsworth's careful decoding of the sGravesande controversy seems to me an argument about what true 'science writing' entailed. As Wigelsworth shows, Desaguliers' careful delineation of the linguistic inferiority of the other translation revealed a deep concern, unshared by his rivals, about the veracity of language and explanation. In this respect, there was a fundamental difference between the two texts such that any alignment between the 'science writing' of Desaguliers' ads and translation must be treated differently than the similitude of Mears and Woodward's ads and translation. If this

is so, then, there are two sets of ads and text, each claiming to be 'science writing', which could be construed as a division between 'popular' and 'proper' natural philosophy.

This particular quibble aside, Wigelsworth should be commended for breaking new historical ground. He extends considerably fruitful studies of science and the public sphere by paying attention to a wealth of information in under-appreciated and most literally quotidian texts. I recommend this book to historians of advertising and historians of science alike.

BIBLIOGRAPHY

Stewart, L. 1992. *The Rise of Public Science.* Cambridge.

BOOKS RECEIVED

Aestimatio 8 (2011)

Peter Adamson ed. *In the Age of Averroes: Arabic Philosophy in the Sixth/Twelfth Century.* London: Warburg Institute, 2011.

Thomas Bénatouïl and Isabelle Draelants edd. *Expertus sum. L'expérience par les sens dans la philosophie naturelle médiévale.* Florence: SISMEL—Edizioni del Galluzzo, 2011.

Mauro Bonazzi ed. *Pierluigi Donini, Commentary and Tradition: Aristotelianism, Platonism, and Post-Hellenistic Philosophy.* Berlin/New York: De Gruyter, 2011.

Jimena Canales, *A Tenth of a Second: A History.* Chicago/London: University of Chicago Press, 2011.

Angelo Cattaneo, *Fra Mauro's Mappa Mundi and Fifteenth-Century Venice.* Turnhout, Belgium: Brepols, 2011.

Stephen Clucas, *Magic, Memory and Natural Philosophy in the Sixteenth and Seventeenth Centuries.* Farnham, UK/Burlington, VT: Ashgate, 2011.

Glen M. Cooper, *Galen,* De diebus decretoriis*, from Greek into Arabic: A Critical Edition, with Translation and Commentary, of Ḥunayn ibn Isḥaq, Kitāb ayyām al-buḥrān.* Farnham, UK/Burlington, VT: Ashgate, 2011.

Frans A. J. de Haas, Mariska Leunissen, and Marije Martijn edd. *Interpreting Aristotle's* Posterior Analytics *in Late Antiquity and Beyond.* Leiden/Boston: Brill, 2010.

Pieter De Leemans ed. *De motu animalium fragmenta translationis anonymae.* Brussels: Brepols, 2011.

———— ed. *De progressu animalium, De motu animalium translatio Guillelmi de Moerbeka.* Brussels: Brepols, 2011.

William Eamon, *The Professor of Secrets: Mystery, Medicine, and Alchemy in Renaissance Italy.* Washington, DC: National Geographic, 2010.

James Dougal Fleming ed. *The Invention of Discovery, 1500–1700.* Farnham, UK/Burlington, VT: Ashgate, 2011.

ISSN 1549–4497 (online) ISSN 1549–4470 (print) ISSN 1549–4489 (CD-ROM)
Aestimatio 8 (2010) 241–243

Robert Goulding, *Defending Hypatia: Ramus, Savile, and the Renaissance Rediscovery of Mathematical History.* Dordrecht/Heidelberg: Springer, 2010.

Steven J. Green and Katharina Volk edd. *Forgotten Stars: Rediscovering Manilius'* Astronomica. Oxford/New York: Oxford University Press, 2011.

Annette Imhausen and Tanja Pommerening edd. *Writings of Early Scholars in the Ancient Near East, Egypt, Rome, and Greece: Translating Ancient Scientific Texts.* Berlin/New York: De Gruyter, 2010.

David A. King, *Astrolabes from Medieval Europe.* Farnham, UK/Burlington, VT: Ashgate, 2011.

Paul Kunitzsch and Richard Lorch edd. *Theodosius,* Sphaerica*: Arabic and Medieval Latin Translations.* Stuttgart: Franz Steiner Verlag, 2010.

Pamela O. Long, *Artisan/Practitioners and the Rise of the New Sciences, 1400–1600.* Corvallis, OR: Oregon State University Press, 2011.

Daniela Manetti ed. *Anonymus Londiniensis de medicina.* Berlin/New York: De Gruyter, 2011.

Alister E. McGrath, *Darwinism and the Divine: Evolutionary Thought and Natural Theology.* The 2009 Hulsean Lectures, University of Cambridge. Chichester, UK: Wiley-Blackwell, 2011.

Clemency Montelle, *Chasing Shadows: Mathematics, Astronomy, and the Early History of Eclipse Reckoning.* Baltimore: Johns Hopkins University Press, 2011.

Gianluigi Pasquale, *La ragione della storia. Per un filosofia della storia come scienza.* Turin: Bollati Boringhieri, 2011.

Pierre Pellegrin, *Aristote.* Les parties des animaux*. Traduction et présentation, notes et bibliographie.* Paris: GF Flammarion, 2011.

Francesco Pelosi, *Plato on Music, Soul and Body. Translated by Sophie Henderson.* Cambridge/New York: Cambridge University Press, 2010.

Andrew Pickering, *The Cybernetic Brain: Sketches of Another Future.* Chicago/London: University of Chicago Press, 2011.

Robert Railley and Lauren Kassell edd. *Stars, Spirits, Signs: Towards a History of Astrology 1100–1800.* Amsterdam/Boston: Elsevier, 2010.

Eileen Reeves and Albert Van Helden trans. *On Sunspots: Galileo Galilei and Christoph Scheiner.* Chicago/London: University of Chicago Press, 2010.

Justin E. H. Smith, *Divine Machines: Leibniz and the Sciences of Life.* Princeton/Oxford: Princeton University Press, 2011.

Stephen Kent Stephenson, *Ancient Computers: Part 1. Rediscovery.* Barnes and Noble, 2011.

Harold Tarrant, *From the Old Academy to Later Neo-Platonism.* Farnham, UK/Burlington, VT: Ashgate, 2011.

Sarah Klitenic Wear, *The Teachings of Syrianus on Plato's* Timaeus *and* Parmenides. Leiden/Boston: Brill, 2011.

Jeffrey R. Wigelsworth, *Selling Science in the Age of Newton: Advertising and the Commoditization of Knowledge.* Farnham, UK/Burlington, VT: Ashgate, 2010.

Anja Wolkenhauer, *Sonne und Mond, Kalender und Uhr. Studien zur Darstellung und poetischen Reflexion der Zeitordnung in der römischer Literatur.* Berlin/New York: De Gruyter, 2011.