AESTIMATIO

Critical Reviews in the History of Science

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Volume 13
2016–2018

Edited by

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A Publication of the Institute for Research in
Classical Philosophy and Science
Baysville, Ontario, Canada

ISSN 1549–4470

ISBN 978–1–60724–617–6

This volume is printed on acid-free paper that meets ANSI standard Z39.48, Permanence of Paper for Publications and Documents in Libraries and Archives.

Printed in Toronto, Canada

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Preface

This is the last year in which complete volumes of *Aestimatio* will be published separately online. We will, of course, continue to put individual reviews online as they become ready. Starting later this year, however, the content of complete volumes of *Aestimatio* will be included in volumes in print and online that collect articles published separately online in *Interpretatio* Series A. For more information about *Interpretatio* A, please go to the IRCPS website (https://ircps.org/interpretatio/about-A).

Alan C. Bowen

Editor, *Aestimatio*

The Inscriptions of the Antikythera Mechanism by Martin Allen, W. Ambrisco, Magdalini Anastatsiou, D. Bate, Yanis Bitsakis, A. Crawley, Mike Edmunds, D. Gelb, R. Hadland, P. Hockley, Alexander Jones, T. Malzbender, Helen Mangou, Xenophon Moussas, Andrew Ramsey, John Seiradakis, John M. Steele, Agamemnon Tselikas, and Mary Zafeiropoulou

Turnhout, BE: Brepols, 2016. Pp. 310. Special edition: *Almagest* 7.1, Paper €33.00

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In 1900, Greek sponge-divers, on their way back home to Symi in the eastern Aegean from working the sponge-beds off the Libyan coast of Africa, found by chance the wreck of an ancient ship in deep water at the bottom of the sea off the coast of the small island of Antikythera, south of the Greek mainland. The recovery of the contents of the wreck constitutes one of the first concerted underwater excavations and it brought to the surface a significant collection of Greek sculptures in bronze and marble. These remains, some well known, others not so, recently became the focus of their own special exhibition in the National Archaeological Museum in Athens [Kaltsas, Vlachogianni, and Bouyia 2012].

One particular part of the cargo has attracted the interests of historians of science: the remains of a technical instrument, which were recognized soon after discovery. (The details of the recognition in 1902, attributed to Spyridon Staïs, the Minister of Education in the Greek government, are given in the book under review [38–41]; see Kaltsas, Vlachogianni, and Bouyia 2012, 18–31 and 228 for a description of the discovery of all the finds in 1901–1902 and for the recognition of the instrument.)

In 1972, X-rays were taken of the Mechanism by radiographer Charalambos Karakalos for the physicist Derek de Solla Price. These showed that it originally comprised over 30 interlocking, toothed gears and several plates that were interrelated by their capacity to mark time in various ways: an Egyptian calendar; a zodiac-dial; and a star-calendar (*parapegma*). These discoveries led Price to attempt a reconstruction and ultimately to publish his findings

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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in a monograph [de Solla Price 1974]. For him, the instrument was a type of calendar-computer, a loose term nowadays since it was not programmable, but adequate for his time.

Ground-breaking though this research was and fundamentally important for our appreciation of the complexity and sophistication of the Antikythera Mechanism, more intensive techniques and ongoing physical reconstructions have been undertaken and have allowed investigators to refine or correct much of Price's interpretation and model.

In the 1990s, Michael Wright at the Science Museum in London and Allan Bromley at the University of Sydney became collaborators, working initially from the 1970s' X-rays but then developing their own. Since Bromley's untimely death, Wright has continued his research, in the process manufacturing the most detailed physical reconstructions of the Mechanism and explaining its underlying theory in a long series of publications. In its time, Mogi Vicentini's [2009] virtual reconstruction of Wright's model version 2 provided a brilliant opportunity to imagine the device as a whole and yet also to appreciate the extraordinary engineering skill that lies behind its construction. I have had the pleasure of seeing Wright's workshop and tools, which, barring a modern metal-working lathe, have largely adhered to the type of hand-tools available to an ancient craftsman. As I recall, in Wright's own estimation, it would take a single person a year working full-time with ancient techniques to make the Mechanism.

The most recent investigators of the device, and by far the largest group, are the members of the Antikythera Mechanism Research Group (AMRG), some of the fruits of whose work are assembled in the publication under review. Originally led by Tony Freeth and Mike Edmunds, the AMRG has comprised three teams from the UK, Greece, and North America: the academic team (Mike Edmunds, Tony Freeth, John Seiradakis, Xenophon Moussas, Yanis Bitsakis, and Agamemnon Tselikas); the Hewlett-Packard team (Tom Malzbender, Dan Gelb, and Bill Ambrisco); and the museum team (Eleni Mangou and Mary Zafeiropoulou from the National Archaeological Museum in Athens). Notable additions to the team after 2006 have been Alexander Jones, John Steele, and Magdalini Anastasiou. Jones figures as author or co-author of all the chapters in the present publication under review, and Bitsakis of all but one. Freeth withdrew from the AMRG in 2012 and has proceeded to publish on his own [e.g., Freeth 2014]. Indeed, one of the best introductions to the Mechanism and what it tells us is provided in a virtuosic (not to say epic, at almost two hours' length) lecture that Freeth gave at the Stanford Humanities Center in 2016. This provides, towards the end, a stunning virtual reconstruction of the Mechanism, which has the added advantage of placing the known fragments in their appropriate positions. As a matter of disclosure, I am listed on the AMRG's website as a collaborator with the project [www.antikythera-mechanism.gr/project/team]. This has taken the form of independent publications of my own interpretations of the findings with respect to the Mechanism's calendars, based at times on privileged access to the data, and of discussions with members of the team about various other aspects of the device (notably with Freeth while he was still a leader of the team).

X-ray computed tomography—or CT scanning as we know it in medicine, but here increased in radiation well beyond what a living organism like a human body could endure—as well as Polynomial Texture Mapping, developed by Hewlett Packard, have been particularly useful for the AMRG to provide far more data capable of interpretation than had been visible even under standard X-ray. Not only have these investigations clarified the interconnections of the gearing and enabled increasingly precise reconstructions of the device, they have provided the level of detailed images of the minute inscriptions upon which this book is based. The Mechanism's complex train of more than 30 gears, moving at different speeds, was arranged so as to coordinate otherwise discordant time-scales. It managed to correlate the motions of the Sun and the Moon *via* the 19-year Metonic Cycle, and probably of the five planets known to antiquity in epicyclic motion through the zodiacal band. The device could also be used to compute eclipses and it had a dial to signal the two- and four-yearly games festivals at Olympia, Isthmia (near Corinth), Delphi, Nemea, Dodona, and possibly Rhodes. A *parapegma* or star-calendar also coordinated with dials giving the zodiacal year, the Egyptian calendar, and even a civil calendar, which was probably the Epirote variation of the Corinthian calendar [Iversen 2017].

The initial section of the volume under review presents an introduction to the original form and scale of the device itself, known now only through the 82 fragments of very variable size and preservation. Readers are also introduced to the technique of computed tomographic imaging and polynomial textual mapping, by which the inscriptions are now read in greater detail than before.

There follows a useful summary of the history of the discovery of the Mechanism and a discussion of the problems associated with reading and dating the inscriptions by traditional techniques. The remaining four sections of the volume take us through transcriptions and translations of the inscriptions on:

(1) the front dial and *parapegma* (the zodiacal band and Egyptian calendar-dials, plus the *parapegma*, whose unusual, or even unique, placement clockwise around the central dial deserves attention);
(2) the back dial and back plate (the Metonic and Saros-dials, along with the small 'Games' dial within the Metonic dial and an exeligmos-dial within the Saros-dial), as well as surrounding text relating to the predictions of eclipses (the authors refrain from attempting to date the construction of the Mechanism on the basis of the eclipse-cycles, something that has been attempted in a previous study by Freeth to suggest a date of construction near 205 BC [Freeth 2014]);
(3) the back cover (preserved only as very small fragments, this plate, or pair of plates, provided a description of the dials, pointers, and other external features of the Mechanism); and
(4) the front cover (texts give data on synodic cycles for the five planets, and it may be conjectured that lost lines described the behavior of the Sun and Moon).

Overall, the volume may be regarded as the *editio princeps* of the inscriptions of the Antikythera Mechanism in so far as it provides what are regarded as the most plausible readings of the fragmentary inscriptions. But it does more than that by providing historical and cultural interpretations of the readings.

One of the fundamental problems of the Mechanism is that we do not know its precise date. Despite protestations to the contrary presented in the volume by Jones ('we cannot appeal to the letter forms to narrow this interval' between the late third century BC and the date of the shipwreck [58]), at present the best means of dating still seems to be the style of the Greek lettering found on several of the fragments, which is rather ironic given the degree of highly technical analysis that the instrument has been subjected to of late. Perhaps the latest word on this fraught aspect belongs to Iversen, who adumbrates a forthcoming article in which he discusses the paleography of the inscriptions and indicates that he

> can show that all the letter forms that they [Kritzas and Crowther] discuss as dating from the end of the 3rd century to the beginning of the 1st can also be found on inscriptions from Rhodes securely dated within 30 years of the Antikythera shipwreck, ca. 70–50 BC. [Iversen 2017, 146n67]

This is a tight chronology indeed in the world of Greek inscriptions which lack a secure dating point such as is provided by political names or events. My own view, expressed in Hannah 2008, 31 and in more substantiating detail at 160n10, is that the letter forms could suit the second half of the second century BC. But I gave the necessary caution that the Mechanism's inscriptions are on bronze and on a very small scale compared with the epigraphical parallels that I offered. These parallels were presented as a result of my own independent study over the years of the forms of the inscription, so I am puzzled that I am said in this volume to have given an 'endorsement of Kritzas' dating' [57n77]. I have had cause to work for 40 years with Hellenistic inscriptions which lack means of secure dating, such as political elements, and margins of error in the range of 50, 100, or 150 years are not unusual. I am happy to run beyond *ca* 100 BC and into the first century BC for the Mechanism's script, if Iversen's argument about local script-forms in Rhodes holds.

Such a relatively late date for the Mechanism would work well with another aspect of the device that is just hinted at in the section on the back dial and plate, namely, the relationship with astrology as evinced by references to the color and size of the Sun at eclipse, which might be linked with astrology:

> The correspondences between these predictions and the eclipse phenomena invoked in the astrological literature are surely not accidental. We see it as an indication that the Mechanism was fashioned to represent and simulate a Hellenistic cosmology in which astronomy, meteorology, and astral divination were intertwined. [211]

Elsewhere Jones, while regarding the Mechanism as 'an educational tool', has noted that

> Greek astronomy around 100 BCE was undergoing significant transformation through contact with the contemporary, but more mathematically advanced, astronomy of Babylonia and also in response to the demand coming from the relatively new but hugely popular practice of horoscopic astrology for positions

> of the heavenly bodies calculated for arbitrary dates, on the basis of which astrologers generated their prognostications. [Jones 2017b]

This stands somewhat in contrast to his view in his recent monograph on the Mechanism, where astrology is acknowledged as a potential use for the Mechanism but ultimately dismissed [Jones 2017a, 236]. In the past, I have posited an astrological function for the instrument, as it could have permitted the rapid calculation of the positions of all the major planetary bodies and related phenomena that were essential to ancient astrology. The Antikythera Mechanism could provide a means by which the major astrological positions (which sign, even which degree of which sign, the planets were in) could be ascertained at a certain point of the year over a prolonged period of time, and so could be the type of instrument that an astrologer would find useful in constructing the sorts of tables that we know of from the Imperial Roman period. Otto Neugebauer proposed a combination of observation and calculation to explain the accuracy and discrepancies in these tables [1942], and an instrument like the Antikythera Mechanism could fulfill the need for the calculated positions. The earliest surviving horoscopes are the sculpted one at Nemrud Dağ in Commagene, Turkey, from 62 BC and the slightly earlier literary one of 72 BC preserved by the mid-first century AD astrologer Balbillus [Hannah 2008, 63–64]. If the Mechanism does date to the decades before the shipwreck, then indeed it falls into the period in which horoscopic astrology was making its presence felt in the Greek world.

One last aspect of the volume that I would like to raise concerns the *parapegma*, which features around the front dials. Apart from providing a valuable, up-to-date transcription and translation of the *parapegma* on the Mechanism, Bitsakis and Jones present the background to *parapegmata* in general and devote some time [117–135] to a discussion of the observational astronomy that they believe underlies this and other *parapegmata*. At issue is the question of the accuracy of the observations for any given latitude for which the Mechanism was devised. At this point, I worry that all the care that has been devoted elsewhere in the volume to understanding the cultural context of the Mechanism has been put to one side in favor of a presumption that what we have in the Mechanism's *parapegma*, and indeed in other preserved *parapegmata*, are data derived from direct observation. I have myself aligned with this belief in the past, even in the face of a gentle chiding from Douglas Kidd, the doyen of Aratus-commentators, who once interjected, when I was talking about observations recorded in the *parapegmata* of Euctemon and Eudoxus, 'if they observed at all'. I assumed then that they did indeed observe. But 20 years on, I have come around to thinking that perhaps the data-sets that have been handed down to us in the *parapegmata* may be, like the astrological data of the tables treated by Neugebauer, a mixture of observation and calculation. In the case of star-positions, the calculation might be based *not* on a star's visibility being a function of its magnitude as we assume nowadays, but rather on a simpler assumption of a set length of time before sunrise or after sunset.

Such a method is actually mentioned by Pliny the Elder at *Nat. hist.* 18. 218, where he says that the Sun should be 'at least three-quarters of an hour' below the horizon. Le Bonniec and Le Boeuffle suggest that this corresponds to about 12° below the horizon [1972, 264n3]. Matthew Fox has proposed that ancient observations of the stars took the Sun to be at a certain distance below the horizon, apparently regardless of the brightness (magnitude) of the star being observed on the horizon [Fox 2004]. For him, this led to a realization and demonstration that the star-data presented by Ovid in his *Fasti*, long derided for their apparent inaccuracy, were in fact largely accurate according to the parameters given by Pliny. The few remaining inaccurate data that Fox could not fit into his scheme were later addressed by Anne-Marie Lewis and shown to be accurate too, give or take the occasional textual lapse in the manuscripts that needs correcting [2014]. I have argued elsewhere that some of the data from Euctemon's *parapegma* fit this method, where the time of the star-observation is, by modern calculation, 40 or so minutes before sunrise or after sunset [Hannah 2018]. Fermor and Steele [2000, 213–214] have discussed inflow waterclocks in Babylonia that could measure in 24-minute units. Much later the Indians, for whom 24 minutes are the fundamental unit of time, had waterclocks that could measure accurately in such units [Sarma 1994, 512–513]. One such instrument comprised a hemispherical bowl with a hole in its bottom, which sat in a bucket of water and slowly sank to the bottom in exactly 24 minutes. 24 minutes are $\frac{1}{60}$ of a 24-hour day (a *nychthemeron* in Greek), and 48 minutes are simply two such units, which then match well with Pliny's time-lapse of 'at least three-quarters of an hour' for observations, and indeed suit well the sort of temporal gap that can be found for some star-phases in Euctemon's *parapegma*. An instrument as simple as a bowl or even a clay lamp of appropriate size could have served the purpose of measuring time.

So I wonder now if the Antikythera Mechanism's 'observations' are similarly open to such interpretation, in which case, the search for accuracy based on star-visibility and magnitude, which seems doomed in the present volume, is unnecessary.

These criticisms notwithstanding, there is no doubt that we are deeply indebted to the AMRG for the presentation of all the data in this remarkable volume. The extraordinary *tour-de-force* of engineering that is the Antikythera Mechanism will probably provide more surprises yet, as technology improves to investigate it and scholarship deepens to explain it. Even if its purpose remains a puzzle, it is reasonable to take it as a prestige-item commissioned by (and for?) a wealthy patron, who lived in the Greek world, perhaps in the sphere of influence of scientists of the calibre of Archimedes and Posidonius.

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Hermes and the Telescope: In the Crucible of Galileo's Life-World by Paolo Palmieri

New York/Berlin: Peter Lang, 2016. History and Philosophy of Science: Heresy, Crossroads and Intersections 2. Pp. xxiv + 234. ISBN 978–1–4331–3140–0. Cloth $86.95, €72.50

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Paolo Palmieri's *Hermes and the Telescope: In the Crucible of Galileo's Life-World* is the latest in a scholarly series of portraits of the Italian astronomer. For close to four decades, that image has undergone dramatic adjustment as historians of science have sought to refine or to reject the rather straightforward account offered in 1978 in Stillman Drake's *Galileo at Work*: we have seen Galileo as heretic, as courtier, as father figure, as entrepreneur, as artist, as engineer, as humanist, and as friend.[1] He will doubtless appear in other guises, but Palmieri's depiction is and will remain among the most puzzling and disquieting images in this gallery. This impression is to some degree an artifact of hermeticism itself—an elusive and eclectic set of doctrines of contested age and origin, designed for enlightened adepts—and elsewhere a corollary of the particular rhetorical approaches that Palmieri has adopted in his study.

Amid the overall strangeness of *Hermes and the Telescope*, readers will recognize arguments that are familiar, if not always explicitly acknowledged, in recent historiography of science. Palmieri's emphasis on the distortions in the traditional image of Galileo as the emblem of a positivistic, rational, beneficent, and always evolving science [3–15], the legacy bequeathed to us in the *Opere* and in dozens of related articles by Antonio Favaro (1847–1922), tallies with both general and specific arguments advanced by Massimo Bucciantini [1995], Michele Camerota and Giuseppe Castagnetti [2001], and Mario Biagioli [2010], among others. His insistence on the ironic and parodic strains that emerge in many of the astronomer's writings [76–85, 199–203] comple-

[1] See, e.g., Redondi 1983, Biagioli 1993, Sobel 1999, Biagioli 2006, Bredekamp 2007, Valleriani 2010, Heilbron 2010, Wootton 2010, Hall 2013, and Wilding 2014.

ments John Heilbron's recent biographical portrait of a scientist given to satirical statements [2010], while his gesture to a wavering religiosity [31, 69–71, 121–122] draws on David Wootton's discussion of matters of faith early and late in Galileo's life [2010, 244–247]. And Palmieri's commendable interest in Galileo's bookish side, both as a consumer and as a producer of literary texts,[2] finds elaboration in Crystal Hall's *Galileo's Reading* [2013] and in her 'Galileo's Library Reconsidered' [2015], as well as in the very pronounced turn by historians of early modern science to the resources of imaginative literature [Reeves 2015, 20–22].

That said, Palmieri's Galileo is for the most part a stranger. Given that *Hermes and the Telescope* is something of a manifesto emphasizing both methodological differences and pedagogical consequences, it is no accident that the astronomer emerges as a cypher, that we scarcely know him better at the monograph's end than we do at the outset, and that much of the discussion involves material judged too fragmentary, too obscure, or too much at odds with the conventional profile of the scientific figure. Rather than relying upon the usual chronological approach with its expected narrative arc and the cumulative force of traumas and triumphs, Palmieri has structured his work as a series of *canovacci,* the general plot-lines typical of the *commedia dell' arte* [xiv]. Such a strategy is somewhat disorienting for any reader accustomed to the naturalized units of chapters in a life story. The advantage of the *canovacci*, however, is that the genre foregrounds the experimental nature of this monograph, tallies with Palmieri's interest in improvisation as a dynamic compositional element in and beyond Galileo's work, and allows him the freedom to address particular ideas, seemingly disguised, as they emerge and disappear over decades in the astronomer's writing and in other, better-established Hermetic texts.

The *canovacci,* then, are 'Myth', 'Hermes', 'Luna', 'Sol', 'Jove', 'Heaven', and 'Hospitality'. The first of these persuasively presents Favaro's phrase 'Codice Galileiano', normally translated by the workmanlike phrase 'Galileian Manuscripts', as a 'Galileian Code', thus insisting on the myth-making criteria used to identify, to classify, and often to suppress the avalanche of documents associated with the astronomer [13–15]. Somewhat less cogently, it describes

[2] See pp. 21–24, 46–52, 69–70, 83–84, 98–102, 138–140, 148–151, 170–173, 185–198, 204–207.

Galileo's 'hermetic labors', now largely obscured by this late 19th-century construction of science as a 'search for immortality' and for 'personal healing' [4, 30]. In contrast to other biographical accounts, in this study, Galileo's malaise derives neither from physical ailments nor from familial traumas nor from the impediments of old age but from the vertiginous reality revealed by the recently invented telescope [19–20]. His cultivation of 'poetic madness' is a recognition of the transformative and sacramental quality of baroque lyric [23–24, 28–29]; the misunderstood and maligned emptiness of *concettismo* figures, in Palmieri's analysis, as an aesthetic response to what Blaise Pascal would later identify as the frightening eternal silence of infinite space [15–20].

'Hermes' is devoted to the interplay of heresy, libertinism, and hermeticism, particularly in their reliance on *ingenium* or inventiveness as an interpretive strategy, and in their postures of jocose resistance to the spiritual, social, and intellectual conventions espoused by a powerful and amorphous 'them'. Here, Palmieri insists upon the generative force of word-play, identifying, for example, in works such as the *Dialogue concerning the Two Chief World Systems* striking verbal homologies in religious and scientific faith, and in their three-day rituals of purification, mental preparation, and eventual confirmation of largely invisible phenomena [60–63, 72–73, 80–82]. In this reading, word-play functions as a kind of elusive and decorative double to Galileo's postils or marginalia, those fierce, often funny, and still more often obscene responses to his rivals' works, partially preserved for us by Favaro in the *Opere*. Further and more vigorous word-play allows Palmieri to suture Galileo's use of the term 'intoppo'—a literal or figurative obstacle—within the context of *The Two New Sciences* to a group portrait of 'them', the monstrous mob that temporarily blocks the progress of Ruggiero, the hero of the astronomer's favorite romance epic, *Orlando Furioso* [94–102].

'Luna' has as its focus less the Moon than Galileo in his guise as Mercurius, a name that emerged in the wake of the first published images of telescopic phenomena. Palmieri's emphasis here falls not on this title as an ornate, classical counterpart to the somewhat more generic *Nuncius* but rather on the elaborate mythographies undertaken by Galileo's disciples and antagonists in the face of the telescope's dramatic disclosures. As he notes, Galileo and this band of overbearing humanists sometimes appear unaware of, or indifferent to, the ungovernable generative force of the metaphors that they deployed when discussing features such as the Moon's ashen light [117].

Drawing on the sometime comparison of the glowing lunar surface shortly before and after conjunction to the newish phenomenon of the phosphorescent 'Bologna stone', the latter securely in the ambit of alchemy, Palmieri accentuates the language of transmutation in the astronomical debate between Galileo and the alarmingly prolific polymath Fortunio Liceti [113–119]. Such suggestions, to the extent that they involve Galileo as well as Liceti, contravene his celebrated and explicit dismissal of alchemy and, above all its linguistic subterfuges, in the *Dialogue*. There, Sagredo mocks those practitioners' commentaries on

> the ancient poets, discovering most important mysteries hidden beneath their stories, and what the Moon's amours might mean, as well as her descent to Earth for Endymion, and her rage against Actaeon, and the significance of Jove's conversion into a shower of gold, or a fiery flame, and how many great secrets of the art there are in that interpreter Mercury, in those abductions of Pluto, in those golden boughs. [Favaro 1890–1909, 7.136 (translation mine)]

'Sol' incorporates both Galileo's brilliant but ill-advised interpretation of Joshua's command—'Sun, stand thou still upon Gibeon'—as well as the astronomer's depiction of the solar body as the heart, or primary and central organ, of the cosmos. 'Jove', by contrast, returns to the immediate aftermath of the *Sidereus Nuncius,* focusing on an Italian poem written by the court poet Andrea Salvadori and revised by Galileo himself that was devoted to the familiar theme of the gigantomachy and intended for the unrealized vernacular edition of that first treatise on the telescope. To some extent, this is familiar territory for historians of science, many of whom have commented on the scepticism with which Galileo's claims about Jupiter's satellites were first met and on his anxious, defensive, and even despondent reaction. The common suggestion that the so-called Medici stars were illusions or hallucinations morphed easily, even among Galileo's supporters, into the conflation of astronomical observation with alcohol: in Niccolò Aggiunti's *Creteum mihi das nectar*, for instance, two glasses transport you to the stars; four, far beyond them. [Vaccalluzzo 1910, 121].

Palmieri, for his part, refers somewhat enigmatically to Galileo's 'poetic madness', to the shared functions of the telescope and the alchemical retort, and to his status as an 'adept'. [150] It is worth noting in this context that an astronomer whose early acceptance of the satellites pushed him to claim priority over Galileo—the German Simon Mayr—had himself portrayed in 1614 with a telescope, sector, and alchemical retort; but a robust connection between Galileo's observational activities and the pursuit of alchemy remains elusive [Marius 1614, 19th unnumbered page]. And while Galileo and his confederates mocked one of his detractors, the strident Bohemian Martin Horky, by calling him *Orcus* or Ogre, a monstrous figure of the Underworld [Favaro 1890–1909, 10.411, 422, 455], Palmieri presents this antagonist as a roaring ventriloquist of newly awakened social anxieties and, in time, a sacrificial victim of sorts: 'without fear of fathers and teachers, the beast spoke publicly through the voice of a young student' [153].

'Heaven' departs from the moody, atavistic aura of the preceding *canovacci* and focuses less on fears unleashed by the new cosmology and more on the performative and occasionally ironizing tendencies of Galileo's work, particularly in connection with his attention to its evolving metaphysical presuppositions. Palmieri convincingly portrays a moment in the *Dialogue* in which the soft target Simplicio, stumbling more than usual, undergoes a bout of otherwise unmotivated amnesia as a parody of the Platonic theory of reminiscence, where the unschooled individual's innate, obscured geometrical knowledge is meant to emerge with the help of a careful interlocutor [176–203].

Less convincing, and less illuminating than his superb discussion of the extrusion-effect in an article of 2008, is Palmieri's association of Galileo's discursive and diagrammatic dispatch of that conventional objection to a rapidly whirling Earth—the notion that rocks, animals, buildings, and entire cities would be hurled into space—with the hyperbolic claims of a contemporary literary character, Capitano Spavento [Palmieri 2008; 204–207; Favaro 1890–1909 7.158, 214]. To the extent that a fictional creation 'knows' anything, Captain Fright does appear aware of the world in which Galileo moved, if not of the astronomer himself. The first edition of the dialogic *Bravure del Capitano Spavento,* approved in Padua and published in Venice, refers to placards posted on the Moon announcing the Captain's upcoming duel with Death and 'glasses for seeing far' to observe the announcement from Earth. Thus, more than a year before the telescope emerged in the Netherlands and more than two years before Galileo demonstrated the terrestrial use of the instrument to members of the Venetian Senate, this work joins others, sometimes more serious in tenor, that gesture to pre-telescopic devices [Andreini 1607, 252]. But this and other Frightful tendencies—the Captain's quarrel with that 'filthy filosofer' Aristotle and his desire to reconfigure the cosmos by slapping about the Moon, Mercury, Venus, the Sun, Mars, Jupiter and

Saturn, and to tell the Primum Mobile to stop turning the starry sphere and to stick to its own business—however close to certain of Galileo's rhetorical postures, seem to me distant in tenor from the slow and somewhat abstract manner in which the astronomer explained away the picturesque fantasy of the extrusion-effect [Andreini 1607, 355, 401].

'Hospitality', the concluding section of *Hermes and the Telescope,* concerns the modern-day pedagogical environment in which Palmieri has developed this monograph.

Among the strengths of this work are its attention to dismissed or discrepant episodes within the Galilean canon, its emphasis on early modern pluri-lingualism, and the variegated tenor of the texts at our disposal; and its effort to integrate empirical claims, philosophical arguments, spiritual preoccupations, and literary works in its framework. Its insistence both on the intractable anxiety provoked by Galileo's early telescopic observations and on the energetic pleasure of his satirical arguments recalibrates, in effect, the arc of most biographical accounts, where the years 1609–1613 figure as a frenetic period of abundant discovery and bold disclosure, and the next three decades as a slow slide into illness, intellectual frustration and doubt, blindness, grief, and death. What substantially undercuts the force of these contributions is at once the elusive nature of hermeticism and the relative unfamiliarity of Anglophone scholars with the 'life-world' or *Lebenswelt* to which the work so often alludes and with the Vichian precedents of that concept.

It seems to me entirely possible that as historians of science we have underestimated the psychosocial impact of early telescopic discoveries, claims, speculations, and images, or that we have recoded them as incidents in larger dramas concerning court culture, confessional strife, or professional status. But if we are to retreat from the inevitable impression of the early modern cosmos as an anodyne antecedent of what W. H. Auden would call

> a clockwork spectacle...
> impressive in a slightly boring
> Eighteenth-century way,

and if we are to take seriously the emphatic emptiness of *concettismo*, the often overwrought pitch of the poems, letters, and treatises responding to the *Sidereus Nuncius* and the intellectual integrity of hermetic writing, I believe that we need a clearer and more rigorous methodology than the one sketched here. We need, too, a more forceful indication of the life-world under scrutiny: at times, it seems that the rhetorical postures examined in *Hermes and the Telescope*, particularly ungoverned word-play and parody, could be as easily ascribed to Palmieri as to his early modern protagonists.

If Palmieri's general suggestion is that we approach the many stray texts and unexamined *aporiae* within the Galilean ambit as part of the scientist's hermetic tendencies, we will require more cogent reasons for abandoning the less personal, more context-dependent interpretations that we generally offer. Let me conclude with one such instance. Palmieri notes that around 1640 the blind astronomer responded to the Florentine poet Antonio Malatesti's published collection of riddling sonnets with one of his own [29–30]. Galileo's 'enigma', like its precedents, depicts an object through a set of clues voiced in the first person:

> I am a monster, stranger and more misshapen
> Than the harpy, siren, or chimera;
> There's no beast on land, air, or water
> With limbs so mismatched as mine.
> No single part is shaped like another,
> And some are black, others white.
> I've often got a pack of hunters behind me:
> They trace out the tracks of my feet.
> I am at home in complete darkness,
> And if I pass from shadows to bright light,
> Soon the life-force vanishes from me,
> As does a dream at the break of day;
> My disjointed members slacken,
> And I lose my being, my liveliness, my name.
> [Favaro 1890–1909, 9.277 (translation mine)]

It seems clear to me that the object in question can only be a telescope. Galileo's composition, in fact, is a variant on the fifth in Malatesti's collection, which had compared the gold-tooled optical instrument to a gilded serpent with strong sight but mismatched eyes, issuing from the city rather than the country, growing and contracting at will, not toxic but Tuscan, and so forth [Malatesti 1640, 19]. Somewhat less fantastically, Galileo gestured to the dissimilar shapes of the telescope's draw-tubes and its convex and concave lenses, to its composition of colorless (rather than green) glass [Neri 1612, 9, 10, 12] and the blackened interior of the tubes, to the 'feet' on which the instrument often stood [Favaro 1890–1909, 12.113], to its diurnal deployment by huntsmen, and to the way in which the image vanished when the apparatus was disassembled. While there is surely a generous measure of pathos in the sightless state of the aged and ailing inventor, the enigma seems to me far from tragic, and more distant still from what Palmieri describes as a disorienting meditation on death, on the loss of individual identity, on the dream-like illusion of rational knowledge, on the soul's apparent demise, and on the 'self's confrontation with the shadows of the unconscious' [29–32]. Show me the melancholy, show me the mourning.

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Essays on Medieval Computational Astronomy by José Chabás and Bernard R. Goldstein

Time, Astronomy, and Calendars: Texts and Studies 5. Leiden/Boston: Brill, 2015. Pp. viii + 413. ISBN 978–90–04–28174–5. Cloth €125.00, USD $162.00

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Everyone who has worked in medieval astronomy over the past few decades is aware of the extensive and pioneering collaboration of José Chabás and Bernard Goldstein. Working as a team from opposite sides of the Atlantic Ocean, these two scholars have been helping to do for medieval European computational astronomy what is also underway for other medieval astronomical cultures: to map out specifically what is available in the manuscripts, to come to grips with the contents of the astronomical and mathematical theories, and to trace developments and influences through subcultures and time-periods. In Latin texts, this project will take decades, if not longer. But it would be inconceivable without Chabás and Goldstein's accomplishments to date.

The book under review is a collection of 12 essays dealing with the technical contents of astronomical manuscripts containing numerical tables. These essays have all appeared previously in various journals between 1992 and 2013; therefore, the volume has the feel of a book published in the Variorum series. (Readers looking for a synthetic work should seek out the authors' *Survey of European Astronomical Tables in the Late Middle Ages* [Chabás and Goldstein 2012]). However, the essays have been edited and typeset in a uniform style, provided with an index, and enhanced with several minor corrections and updates. (Who among us has not wished at some point for the opportunity to release our work again in an updated edition?) The theme of numerical tables encompasses a large part of medieval European astronomy, given that tables were the primary device for converting the geometric models of the motions of the heavenly bodies into tools for prediction. The central theme woven throughout the book is that the primary mission of the table-makers was not to generate theories to fit better with observations but rather to redesign tables to enhance their usability for an audience with less

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 19–25

than stellar computational skills. In this respect, the volume's ingenuity is evident.

Notwithstanding the technical nature of the subject, the writing style is very clear. Nevertheless, it may be difficult for all but the most committed scholars, already deeply engrained in this field, to make much headway without a substantial commitment of time and energy. Thus, it seems best to devote the rest of this review to providing a road map through the various papers—illustrating their points and bringing forward the most important of the authors' findings. The essays are divided into four categories:

(1) tables of solar/lunar conjunctions and oppositions;
(2) tables of the motions of the planets;
(3) analyses of several collections of tables from beginning to end; and
(4) a pair of additional studies.

Part 1

The first three essays, tightly intertwined, concern the development of tables for the computation of true syzygies of the Sun and Moon from their mean syzygies. Syzygies (conjunctions and oppositions) are moments when the Sun and Moon have the same longitude or longitudes separated by 180° in the celestial sphere and are the only times when eclipses can occur. The problem is a tricky one. The velocities of the Sun and Moon both vary over time and their 'mean longitudes' are positions of theoretical bodies moving uniformly according to the average speed of the true bodies. It is, therefore, easy to find a mean syzygy but much harder to find the corresponding true syzygy. In the *Almagest*, Ptolemy solved the problem with a computational method (not involving numerical tables) using an approximation based on the assumption that the Sun and Moon travel at constant speeds between the moments of mean and true syzygy.

The second essay, 'Computational Astronomy: Five Centuries of Finding True Syzygy', which should be read first, provides a survey of methods for finding true syzygy using tables. It begins with a description of a rather simple method, not much more than an extension of Ptolemy's method to tabular form, by the 12th-century Spaniard Ibn al-Kammād. A more sophisticated approach by John of Saxony (*ca* 1330) takes into account the variable lunar velocity; but it is somewhat complicated computationally and was not adapted for use with tables.

This sets the stage for Nicholaus de Heybech (*ca* 1400), an otherwise obscure figure who produced a tabular method of solving the syzygy-problem that retains the improved accuracy obtained by allowing the Moon's speed to vary. The authors concentrate on these tables in all three of the papers in part 1, especially in the first essay, 'Nicholaus de Heybech and his Table for Finding True Syzygy', where the method is carefully analyzed. The authors find that Nicholaus' tables rely partly on those of John of Genoa, a contemporary of John of Saxony. Here for the first time the authors emphasize a point that they return to frequently: Nicholaus' tables were valued due to their *user-friendliness*, which enhances both usability and reliability of results.

The third essay, 'Transmission of Computational Methods within the Alfonsine Corpus: The Case of the Tables of Nicholaus de Heybech', illustrates the influence of Heybech's work especially on the *Tabulae verificate* (possibly by Polonius in Salamanca, 1460) and the tables of Abraham Zacut (1513). The second essay then concludes the story, illustrating approaches by such later luminaries as John of Gmunden, Peurbach, and Copernicus. Again, the authors stress that computational efficiency, not observation, was the central force that drove their research.

Part 2

The next three essays approach the range of tabular methods that were devised for determining planetary positions as a function of time—the heart of the astronomical project. Almost all of the European medieval tradition follows the planetary model found in Claudius Ptolemy's works of the second century AD, the *Almagest* and the *Handy Tables*. (One exception to this is the role of al-Khwārizmī's *zīj*, which was inspired by the Indo-Iranian tradition and influenced European astronomy through its Spanish presence.) This model breaks the planets' positions into *longitudes* (position along the ecliptic) and *latitudes* (position above/below the ecliptic) and deals with them separately. A planet's longitude is conceived in three parts: its *mean longitude*, the position of an imaginary object traveling at the planet's average speed; its *equation of center*, a correction accounting for the fact that the Earth is displaced from the center of the planet's large orbital circle (its deferent); and its *equation of anomaly*, a correction accounting for the planet's position on the epicycle. The latter is a function of two variables for which Ptolemy constructed an approximation that allows it to be tabulated with an arithmetical combination of several single-argument tables.

The first essay to read in this section is the last one, 'Computing Planetary Positions: User-Friendliness and the Alfonsine Corpus'. This survey encapsulates the developments that led from Ptolemy's tables to those found in 15th- and early 16th-century Europe. The authors describe various of the most important tables that found their way to Spain and Europe, noting that very few changes from the Ptolemaic paradigm are found until the early 14th century. However, the pace then picks up markedly. The first innovation is the *displacement* of the tables for planetary equations. Since these corrections must sometimes be added and at other times subtracted, they were a source of confusion and potential error. The compilers of some of the new tables added a constant value to every entry in the table for the equation of center (a vertical displacement) so that the correction would always be added, and then adjusted the tabular structure elsewhere to remove this constant. This simplified the process for the user, making it less prone to error. Sometimes tables would also be displaced horizontally in order to counterbalance displacements made elsewhere. These methods had been invented in eastern Islam a few centuries earlier but as far as we can tell they were not transmitted to Europe at the time. Other innovations included separating tables that required different arguments—in the Ptolemaic tradition tables had been gathered together into a single grid, regardless of the natures of the independent variables—and combining the effects of the corrections into a single large double-argument table. These improvements again led to increased user-friendliness and, therefore, greater reliability in practice.

The other two essays in this section deal with specific sets of tables within the story of the survey in essay 2. Essay 5, 'Displaced Tables in Latin: The Tables for the Seven Planets for 1340', deals with an anonymous set of tables of almost 100 pages that were probably composed in southern France. There are no accompanying instructions, so the authors carefully and painstakingly analyze each table to reconstruct its purpose and to identify the astronomical parameters embedded within it. They discover no fewer than 40 applications of the technique of displacement, especially in the planetary equation-tables. Essay 4, 'Ptolemy, Bianchini, and Copernicus: Tables for Planetary Latitudes', describes especially Bianchini's latitude-table, but also the tables for the same purpose by Copernicus in manuscript that were based on Bianchini's. One curiosity is a variation in the computation of one of the three components of latitude, where eastern and western astronomers differed in their understanding of Ptolemy's instructions. The method employed by Bianchini and his colleagues would eventually be criticized by Copernicus in *De revolutionibus*, although the manuscript studied here (composed when Copernicus was a student) adopts it.

Part 3

Here we find a set of detailed studies of four collections of astronomical tables. In each case the authors analyze each table meticulously, extracting parameters when they are readily accessible and comparing the entries with other tables in the same genre. Generally, the central purpose is to understand the structure and use of the tables, and then to determine lines of influence, both leading up to the tables under study and emerging from them. In a great majority of cases, the analysis is successful.

Two studies are related directly to the evolution of the Parisian Alfonsine Tables in the early 14th century: essay 8, 'Early Alfonsine Astronomy in Paris: The Tables of John Vimond', and essay 9, 'John of Murs's Tables of 1321'. John of Vimond's tables were composed in Paris only a couple of years before the Parisian Alfonsine tables came together. The authors demonstrate that the main source of this work was the Castilian Alfonsine tables. They find several innovative structures within the various tables, designed once again to make computing life easier for the user. The date is established to be around 1320, just before John of Murs. It is peculiar that the latter does not mention the former; they must have been familiar with each other's work. John of Murs, soon to be one of the co-authors of the Parisian Alfonsine tables, compiled the collection of 132 (and another set called the *Parefit*) not long before. The 1321-tables, which are entirely devoted to the Sun, Moon, and planets, are accompanied by terse canons. Although their structures often deviate from the illustrious Parisian Alfonsine tables, they rely on the same models and parameters. Of note are the syzygy-tables, which are the first to deal with the motions of the Sun and Moon separately. The authors find connections with the Castilian Alfonsine tables but also more traces of John of Vimond's work than had been thought previously to exist.

The other two essays are not directly related to the above or to each other. 'Andalusian Astronomy: *Al-Zīj al-Muqtabis* of Ibn al-Kammād' discusses a treatise surviving only in Latin that was written in Córdoba some time in the 12th century. Given the number of commentaries and references to it that are found in later works, al-Kammād seems to have been a figure to be reckoned with in al-Andalus. Within the *zīj*, the authors discover material that dates back to the ninth-century *Mumtaḥan Zīj*, as well as astronomical content that ended up in the Tables of Barcelona. Among the other findings is an unusual set of planetary latitude tables: those for the superior planets follow the model of Ptolemy's *Almagest*, while those for the inferior planets follow the *Handy Tables*.

'Isaac Ibn al-Ḥadib and Flavius Mithridates: The Diffusion of an Iberian Astronomical Tradition in the Late Middle Ages' compares the tables of two lesser-known figures, finding the latter' tables to be based almost entirely on the former's. Ibn al-Ḥadib was a Spanish Jew who left for Sicily in the late 14th century, presumably to flee anti-Jewish riots. His tables, intended for predicting eclipse, are based on neither the Toledan Tables nor the Parisian Alfonsine tables. Rather, they rely on a Hebrew tradition located in Spain and southern France. Flavius Mithridates (a pen name for William Raymond of Moncada), an Italian working about a century later, converted from Judaism to Christianity and split his interests between astronomy and translating Kabbalistic texts into Latin. Mithridates does not mention the Hebrew source for his tables, possibly to improve his standing with his patron. The influence of the Andalusian tradition on both works may be seen in several ways, notably in their use of a proper motion of the solar apogee, which is not found in other medieval European traditions.

Part 4

The book concludes with a pair of essays that do not fit easily into any of the other sections. 'Ibn al-Kammād's Star List' deals with a table giving the locations of 30 stars in his *Zīj al-Muqtabis* that seems to have had a surprisingly large influence. The authors find copies of this list in a dozen manuscripts in Hebrew, Arabic, and Latin, dating through to the end of the 15th century. They describe especially the variations between the manuscripts, preferring to deal with issues of transmission rather than attempt to reconstruct the original list. Nevertheless, they do reach the conclusion that the list is likely to have been assembled in Islamic Spain, possibly by Ibn al-Kammād himself.

The last essay in the volume, 'Astronomical Activity in Portugal in the Fourteenth Century', concerns the only known manuscript that fits this description. Most of it is a copy of the Almanac of 1307, but the first 12 folios comprise a collection of tables that have sometimes been called an 'almanac of Coimbra'. The authors demonstrate that this document, consisting of calendrical, astronomical, and astrological topics, is not an almanac. Rather, it is a diverse collage of tables taken mostly from the tradition of the Toledan Tables, especially the *Almanac perpetuum* by Jacob ben Makhir.

Although there is hardly anything new in this book, the combination of the 12 reprinted papers is helpful in several ways. It brings the authors' research together in a format that may reach a wider audience that might not have sought out the individual essays. It updates some of the authors' studies to include their most recent findings. Finally, it allows one to move easily back and forth between the essays, thus helping readers to form a more rounded picture of what we know so far of medieval Latin astronomy.

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Research in the History and Philosophy of Mathematics: The CSHPM 2014 Annual Meeting in St. Catherine's, Ontario edited by Maria Zack and Elaine Landry

New York: Birkhäuser, 2015. Pp. xii + 192. ISBN 978–3–319–22257–8. Cloth USD $36.00

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The Canadian Society for the History and Philosophy of Mathematics, also known as La Société Canadienne d'Histoire et de Philosophie des Mathématiques, was founded in 1974 simultaneously with the launch of its first flagship journal, *Historia Mathematica*. Kenneth O. May, the first editor, must have realized that in order to survive, the new journal would need to be more than a single person's project. With the sponsorship of CSHPM, *Historia Mathematica*, later joined by *Philosophia Mathematica*, continues to thrive.

Over the years, the Society has grown to include mathematicians and historians of mathematics throughout North America and beyond; but, faithful to its historical roots, it has retained its 'Canadian' name. The articles from the CSHPM Annual Meeting have been collected into volumes of proceedings since 1988, but up to now these have been privately circulated to members only. Recently, the Society has entered into an agreement to publish these proceedings as a new series of volumes, inevitably entitled *Proceedings of the Canadian Society for the History and Philosophy of Mathematics / La Société Canadienne d'Histoire et de Philosophie des Mathématiques*. The book under review, containing papers from the meeting in 2014, inaugurates the new series. As is the case with all books from Birkhäuser, it is available in both printed and electronic form, and individual articles may be purchased (or accessed *via* an institutional *SpringerLink* subscription).

Like most proceedings, this is a mixed bag; but there do seem to be a couple of overarching themes. Several of the articles deal with what may be described as analog computers: devices designed to solve mathematical problems. Two articles (by Silverberg and Ackerberg-Hastings) focus on sectors and scales, one (by Crackel, Rickey, and Silverberg) on precise diagrams

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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as a computational tool, two others (by Bennett and Abeles) on attempts to create logical diagrams that would allow for visual proofs. Even the mostly straightforward account of the Toronto ICM in 1924 includes a discussion of Peano's attempt to create a logical language and notation that would realize Leibniz's dream of reducing thought to calculation.

A second theme that gets some attention here is mathematical publishing. One article (by Nickerson) focuses on Macmillan and Co. as publishers of textbooks for the English-speaking world, one (by Godard) deals with the pedagogical work of Borel and Lebesgue in France, and one (by Thomas) proposes a judicial analogy to account for the role of publication in mathematics.

Though a few of the papers in this collection deal with early modern mathematics, most of the focus is on the 18th century or later. Many of the articles collected here could be described as 'out-takes' in the sense that they follow up on previously published work or report on work in progress. One often gets the feeling that the whole story is yet to be told. This, I suppose, is fairly typical of conference papers in general.

Most readers will not be reading this book as a reviewer must, from cover to cover, but will rather choose the article or articles that interest them. In what follows, I comment on my favorites.

'Reassembling Humpty-Dumpty' is the title that Theodore J. Crackel, V. Frederick Rickey, and Joel S. Silverberg chose for their paper on George Washington's cyphering book. Since textbooks were scarce and mostly owned by the teacher, 18th-century students often prepared their own elaborate 'books'. These were often kept and used as references in later life. In the case of George Washington's cyphering book, the bulk of the manuscript is to be found in the Library of Congress; but several leaves are missing. The authors recount the story of the missing leaves and manage to find and identify some of them. Unfortunately, in the printed book the reproduction of the images (which are crucial in this article) is less than ideal. The images in the PDF version are much better.

While this article is clearly a report on work in progress, it is interesting and informative. In the process of their investigation, the authors show that surveyors were taught to make accurate scale-diagrams that could be used as *computational* devices. Rather than use Euclidean geometry and trigonometry to determine the length of some line, as one would teach students today, Washington was taught to create careful scale-diagrams. From the diagram and the knowledge of the scale, it was possible to determine lengths simply by measuring. This opens up a whole new way to think about mechanical drawing, a standard school topic in older days which has now completely vanished from sight.

Also interesting is the paper 'The Eighteenth-Century Origins of the Concept of Mixed-Strategy Equilibrium in Game Theory' by Nicolas Fillion. This is a kind of complement to a paper written by D. R. Bellhouse and Fillion which deals with some probability-problems discussed by Waldegrave, Montmort, and Nicolaus Bernoulli. The questions refer to a card game called Le Her. While the paper with Bellhouse dealt with most of the historical aspects, in this paper Fillion analyzes the game in modern terms, then uses this analysis to think about the solutions proposed in the 18th century. One of his conclusions is pithily expressed: 'concepts and methods are typically older than their foundations'. Fillion argues that while formal game-theory and the precise notion of a mixed strategy were still very much in the future, Montmort, Waldegrave, and Bernoulli do create such a strategy, then disagree as to whether having it amounts to 'solving' the game. They even enter into a discussion of the difference between public advice on how to play the game and private advice on how to proceed in specific cases, for example, when one of the players is weak.

The articles discussing mathematical publishing are all interesting, though in all cases I felt that there was more to say. Sylvia Marie Nickerson's article highlights the role of Isaac Todhunter as an advisor to Macmillan and Company. She points out both the weaknesses and the strengths of Todhunter's view of mathematics, which was largely that of Cambridge University at the time, and how it influenced the way in which mathematics was learned and taught in much of the English-speaking world. In particular, she quotes J. C. Fields' opinion that the calculus had been taught to him 'falsely, irremediably and fundamentally falsely'. (One wonders what he would say about today's textbooks.) The data Nickerson presents on the print-runs of Todhunter's mathematics books are striking, with Euclidean geometry and elementary algebra in the hundreds of thousands, while his books on the history of the theory of probability, the calculus of variations, and the problem of the shape of the Earth—the only books of his I have ever looked at—were printed in runs of 1,000 copies or fewer.

Roger Godard's article on Émile Borel and Henri Lebesgue as authors of textbooks is also quite interesting. The focus is mostly on a series of books published by Gauthier-Villars beginning in 1898. Comprising 50 or so books, the series provided short modern accounts of precise topics. The idea was to bring the student to the research frontier quickly and efficiently. Ten of the books were written by Borel, but the authors form quite an impressive list: Borel, Baire, Lebesgue, de la Vallée Poussin, Volterra, Bernstein, Montel, and Riesz, among others. Godard provides brief comments on the pedagogical approach in these books, focusing on Borel and Lebesgue. There is clearly much more to be said on this topic, however.

Finally, there is R. S. D. Thomas' 'The Judicial Analogy for Mathematical Publication', the only article in the book to treat the philosophy (rather than history) of mathematics. Thomas opens the article by noting that in the past he has written articles criticizing proposed analogies between mathematics and fiction and between mathematics and games. Winningly, he writes that in this article he will 'offer what I view as an improvement on the fiction analogy with my own undue enthusiasm', offering it up for criticism by others. The analogy he proposes is between mathematical proof and a legal argument: in both cases, there is someone to be persuaded (a jury, other mathematicians), there are standards (laws, axioms, rules of evidence, rules of deduction), and there is testimony (a witness tells a story, a mathematician outlines an argument). The result is interesting and thought-provoking, though Thomas explicitly disavows drawing any philosophical conclusions.

As noted with respect to the photographs in the article about George Washington, there are some production issues. Some articles have more typos than others, which suggests to me that the authors were responsible for getting them into publishable shape. The editors' preface does not indicate whether the papers were peer-reviewed before publication.

Publication of this book is a further sign of CSHPM's growing importance as a sponsor of new scholarship in both the history and the philosophy of mathematics. I hope to see many more volumes in the series.

Islamic Astronomy and Geography by David A. King

Variorum Collected Studies 1009. Farnham, UK/Burlington, VT: Ashgate, 2012. Pp. xlii + 376. ISBN 978–1–4094–4201–1. Cloth USD $190.00

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Islamic astronomy in ninth-century Baghdad was the outcome of Arabic translations of, and borrowings from, Indian, Persian, and Greek sources, some of the translations from Greek being direct and some through the intermediary of Syriac versions. The whole constituted a process called by A. I. Sabra appropriation and naturalization. Two particularly influential works thus translated were Ptolemy's *Almagest* (Greek), of which there were several translations, compendia and commentaries, and the Sindhind *zij* (Indian). E. S. Kennedy compiled a catalogue of *zij*es in 1955; P. Kunitzsch published his study of the *Almagest* in 1974; and a group of scholars in Munich is now studying Ptolemaeus Arabus. But there is still much to be done. We have information about some of the authors and works from al-Nadīm's *Fihrist* and also from later astronomers like al-Bīrūnī, who sometimes quote their predecessors.

A mathematical culture emerged in ninth-century Baghdad and continued for many centuries. *Inter alia*, non-Ptolemaic planetary models were developed that were very similar to those of Copernicus. Everyday things were also affected: e.g., the *qibla* (the direction of Mecca) was found mathematically, though not always heeded in the building of mosques. The author maintains that, after the initial period, astronomy developed in regional schools—for example, in Egypt, the Yemen, Spain, and so forth. The one in Egypt is illustrated by a description in chapter 4 of the *zij* of Ibn Yunus (d. 1009). The functions of the *muwaqqit*, the mosque astronomer responsible mainly for the times of prayer, are described in chapter 5.

Chapter 6 is on the medieval Maghrib, which the author praises for the many volumes by the scholars of Barcelona. Some of this chapter is on the 13th-century Tunisian astronomer Ibn Isḥaq.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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Besides a general survey of the field with a full bibliography, King presents two lists of sources: one of relevant manuscripts and the other of instruments. There are also chapters on mathematical astrology and geography.

The preface begins, 'This volume supplements my three previous Variorum volumes on the history of Islamic astronomy.' But the book could equally be read as an introduction to the subject: the massive list of King's publications (261 + 5 items), also included in the book, will give indications for further reading. This impressive volume is dedicated to Julio Samso and his school in Barcelona.

Recherches Mithriaques. Quarante ans de questions et d'investigations by Robert Turcan

Paris: Les Belles Lettres, 2016. Pp. 519. ISBN 978–2–251–42063–9. Paper €65.00

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Robert Turcan was a distinguished scholar of ancient Greco-Roman religions, whose researches have focused particularly on the so-called 'mystery' cults and among these on Mithraism. In reviewing this collection of Turcan's articles for *Aestimatio*, there is an obvious threshold question to be posed: Of what conceivable interest could a collection of articles on this topic be to scholars of the history of science in antiquity?

A first answer would be that Mithraism, in its rich archaeological remains, exhibits a great deal of star-lore. In particular, it has been argued, by the present reviewer among others, that the cult's principal icon, which in a very complicated and detailed scene shows the god Mithras sacrificing a bull, served *inter alia* as a map of the constellations. The constellations were those known to, and catalogued by, Greek astronomers.

Secondly and more cogently, a contemporaneous source external to the cult, namely, the philosopher Porphyry, stated that the Mithraic meeting place, the *mithraeum* as we now call it, was designed as 'an image of the cosmos' (εἰκόνα κόσμου) and that its contents 'by their proportionate arrangement' served as 'symbols of the elements (στοιχεία) and climates (κλίματα) of the cosmos' [*De antro nympharum* c. 6].[1] There is good evidence, both from further remarks in Porphyry's essay and from excavated *mithraea*, that the cosmos or universe modeled by the *mithraeum* was that conceptualized by Hellenistic astronomers.[2]

[1] The 'elements' of the cosmos are the stars, constellations, signs of the zodiac, and planets; the 'climates' are parallel zones of celestial latitude [see Beck 2014; 2016, 29–31].

[2] On the *mithraeum* as 'cosmic model' see, most recently, Beck 2014, 2016, and 2018.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 32–36

None of this puts the Mithraists on the cutting edge of Hellenistic 'science'. They were consumers of contemporary cosmology, not trailblazers. The sole postulated exception proved something of a will-o'-the-wisp. David Ulansey in *The Origins of the Mithraic Mysteries* [Ulansey 1989] argued that the phenomenon of the precession of the equinoxes, discovered by Hipparchus, was encoded in Mithraism with Mithras, identified with the constellation Perseus, as the mover of this cyclical phenomenon. Turcan touched on Ulansey's hypothesis in a very short article of 1990 that is in this collection, 'Mithra et l'astronomie' [179–181], but one cannot say that he took a prominent part in the debate. It is, I think, fair to say that Ulansey's theory has won few converts in the study of Mithraism and none in the history of astronomy.[3]

If not astronomy, then what of astrology? May one say that Mithraism's concern with astrology somehow puts the cultists in the proto-scientific camp? To say so begs the huge question of astrology as a foster mother, as it were, of scientific astronomy. But let that be.

Turcan claimed to discuss astrology in Mithraism in an article of 1999 with the title 'Hiérarchie sacerdotale et astrologie dans les mystères de Mithra' [279–302]. Astrology as proto-science, be it noted, plays no part in Turcan's argument one way or the other. In fact, 'astrology' is a mischaracterization of the topic here, unless it is intended in the rather weak sense of lore and learning about the stars. Of astrology in the technical sense of the art of prediction from the stars, together with the theoretical basis on which outcomes (*apotelesmata*) were predicted, there was actually rather little in Mithraism. I have summarized what there was in a contribution [Beck 2015, 290–292] to a major conference on the Star of Bethlehem (University of Groningen, 2014).

The point at issue in Turcan's article of 1999 is the logic behind the pairing of each of Mithraism's seven grades of initiation with one of the seven planets. The order in which one ascended the Mithraic grades does not correspond to any of the planetary orders in common use in antiquity: the order of distance outwards from Earth, the order of the days of the planetary week, or the order usually given in horoscopes. Turcan was right to point out that the evidence for the 'tutelary' system of planets and grades is quite limited. Indeed,

[3] On the *status quaestionis,* see Beck 2004, 235–249.

when Turcan was writing, it seemed to be restricted to Rome and Ostia in the middle of the third century AD. I have, however, argued that the correlation of grades with planets is implicit in the scenes on the Mainz ritual vessel, a recent find when Turcan was writing [Beck 2000, 154 ff. = Beck 2004, 64 ff.]. In his article, Turcan was sceptical of the overly systematized analysis of my monograph *Planetary Gods and Planetary Orders in the Mysteries of Mithras* [Beck 1988], although I would plead that he has mistaken my *structural* analysis for a *historical* account of the development of Mithraic iconography.

In 2007, Turcan reviewed my second monograph on Mithraism, *The Religion of the Mithras Cult in the Roman Empire: Mysteries of the Unconquered Sun* [Beck 2006] at some length [Turcan 2007, 367–376]. The review is unremittingly negative. It is not my intention here to rebut Turcan's critique point by point.[4] Rather, I shall conclude with some observations on what it is that separates our two approaches so radically. It is not that we have different views on the development of science in antiquity—though it is true we do—*but that we have different views on the application of science, indeed on the applicability of science, to the historical phenomena that we both study.*

Science, in Turcan's view of the academy and its disciplines, has no part to play in the study of historical religions. This applies to the social as well as to the physical and mathematical sciences. The insights and methods of anthropology, on which my book greatly relied, are dismissed as irrelevant; likewise, among the sciences, any concern with *cognition*, the way humans *apprehend* religious symbol systems.[5]

In the conclusion of his review, Turcan states:

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4 Subsequent to Turcan's review of my book, I have further developed my theory that the *mithraeum* was indeed designed as a model of the cosmos and that this is demonstrably so in a number of *mithraea* in west central Italy (Campania, Latium, including the city of Rome and its port, Ostia, and southern Etruria) in the second and third centuries AD [Beck 2016]. Again, I make the case that these *mithraea* represent a stream of Mithraism known to Porphyry, directly or through intermediaries. It scarcely needs mentioning that Porphyry himself never set foot in a *mithraeum* [Beck 2016, 22].

5 On the matter of constellation symbols in the tauroctony, the mathematical sciences seemed relevant, so I consulted statisticians: see Beck 2004, 251–265.

> L'histoire des religions n'est pas une 'science' et n'a rien d'intemporel. Elle s'attache plus modestement à pénétrer le sens des données datables, en leur temps (si possible) et dans leur contexte *historique*. [Turcan 2007, 376]

Certainly, not *all* 'histoire des religions' is science-based, but some of it is; and Turcan's inquiries,[6] impressive though they undoubtedly are, are the poorer for his willful denial of the fact.

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[6] Early in his career, Turcan composed *Mithras Platonicus: Recherches sur l'hellénisation philosophique de Mithra* [1975]. The downside of this ground-breaking piece of research was that Turcan could never allow there to be authenticity in the literary testimonials to the Mysteries of Mithras. Necessarily, the philosophers had turned Mithraism into something it was not.

blage and publication of issue 2 of volume 18, although it is nominally part of the volume for 2014.)

Turcan, R. 1975. *Mithras Platonicus. Recherches sur l'hellénisation philosophique de Mithra*. Études préliminaires aux religions orientales dans l'Empire romain 47. Leiden.

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Before Copernicus and Copernicus

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Despite extensive and increasingly nuanced scholarly research, the work of Nicholas Copernicus, one of the most iconic names in the history of human thought, is still controversial. Before addressing some of the controversies and *Before Copernicus*[1] in this context, allow me to note some fairly uncontroversial, basic facts about his life and astronomical work.

Copernicus, who was born in Toruń in 1473, enrolled as a student of liberal arts at the University of Cracow in 1491, which he left without a degree in 1495. In 1496, he moved to the University of Bologna to study canon and civil law. In 1500, he briefly visited Rome and then returned to his native Warmia. Shortly after that, in 1501, he returned to Italy, this time to the University of Padua, where he was supposed to study medicine. He was awarded a doctorate in canon law from the University of Ferrara in 1503. Upon returning home, he started working as his uncle's physician and subsequently also as a church administrator. Sometime around 1510 (before 1514 and possibly as early as 1508), he drafted his earliest attempt at a heliocentric, geokinetic astronomy and cosmology in a text later known as *De hypothesibus motuum caelestium a se constitutis commentariolus* and referred to in short as the *Commentariolus*. This text presumably circulated among his friends but was not published during his lifetime. His next astronomical text was the very short (semi-) private *Letter to Werner*. Having been persuaded by Rheti-

[1] Rivka Feldhay and F. Jamil Ragep. edd. *Before Copernicus: The Cultures and Contexts of Scientific Learning in the Fifteenth Century.* Montreal, PQ/Kingston, ON: McGill–Queen's University Press, 2017. Pp. xx + 344. ISBN 978–0–7735–5010–0. Paper CAD $39.95.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 37–79

cus and some other friends, Copernicus finally published his major work *De revolutionibus orbium coelestium* in 1543. He died in the same year.

The aspects of his work that are still debated are many and, due to the difficult, sometimes technical subject matter and substantial scholarly output, tend to be very nuanced and sophisticated. The famous Copernican question is really a bundle of different but interrelated questions. The more general ones, such as Was there really such an event as the Copernican revolution?, clearly depend on how we understand the concept of science (to put it anachronistically for the sake of brevity) and its multifaceted continuous transformations, and—no less importantly—on how well we understand Copernicus' immediate or less immediate 'scientific' context, against which his achievements and contributions are to be assessed. This naturally leads to an examination of more specific details of his work: What exactly was the question that he was trying to answer? How, why, and when did he become a Copernican? What is the nature of the orbs mentioned in the title of his *De revolutionibus orbium caelestium*? Are his astronomical models the result of an independent development in Western thought or did he borrow them from his Islamic predecessors? These are just a few examples. It is generally understood, first, that these and other questions are in themselves very complex and divisible into myriad subquestions that demand studies of considerable historical and epistemological breadth, length, and depth; and second, that sometimes seemingly insignificant details can turn the whole narrative completely upside down, since, as is usual in such complex matters, the whole depends on its parts as much as the parts depend on the whole.

The aim of *Before Copernicus* is to address some of the above-mentioned issues by examining Copernicus' intellectual and social background. The book is divided into three parts:

Part 1 covers Copernicus' 15th-century European social and political context;

Part 2 is dedicated to his 15th-century European intellectual and scientific context; and

Part 3 explores the multicultural astronomical background to the Copernican revolution.

Although the book, true to its title, focuses on the period before Copernicus, i.e., on the 'long fifteenth century',[2] its authors keep one eye on the value of this period for understanding Copernicus' work, especially his *Commentariolus*, which is set as the endpoint of the discussion.

With this in mind, I will divide my review into two sections. In the first, I will summarize the introduction, which sets the stage and defines the main coordinates of the discussions with several important 'observations' (the editors' term) and conclusions. I will then attempt to summarize the main points and the most important results of each chapter. While I, together with the editors and contributors to the book, believe that Copernicus' work—or any other work of any significance, for that matter—can be fully appreciated only when set within a sufficiently long as well as adequately studied historical context, I will pay much closer attention to the chapters and chapter-sections that discuss issues that are in my view 'closer' to Copernicus and, therefore, more relevant to an understanding of his *Commentariolus*. In the second section of my review, I will provide a critical appraisal of the book with special emphasis on the question of how the book as a whole and each of the chapters succeed in making the genesis and nature of Copernicus' *Commentariolus* (and in some cases *De revolutionibus*) more understandable. At the same time, I will point out some conclusions that I find questionable and suggest alternative interpretations. I will also suggest what I believe still needs to be done to advance our understanding of Copernicus' astronomy and cosmology.

G.A

1
Summary

1.1. The introduction

Rivka Feldhay and Jamil Ragep, the editors of the book and the authors of its introduction, explain the need for an examination of Copernicus' social and intellectual background by the fact that it is little understood. According to their outline of the most important issues discussed during the last half

[2] The interval from the mid-14th century to roughly 1525, according to Christopher Celenza [17–18].

century (or so) of Copernican scholarship, he has sometimes been portrayed as a lone genius without history and without context. This changed with Thomas Kuhn's *The Copernican Revolution* and his thesis about the crisis that prompted the revolution. Kuhn did not manage, however, to explain the exact nature of this crisis, which

> remained elusive, in large part because the 15th-century background to Copernicus was and remains to a large extent *terra incognita*. [3]

A major step forward was taken by Otto Neugebauer, who showed how much the mathematical details of Copernicus' work are connected to both the 'Western' tradition and, crucially, the 'Eastern', Islamic tradition.

In continuing Neugebauer's work, Noel Swerdlow arrived at even more important conclusions. His detailed analysis of the *Commentariolus* brought to light more evidence of Copernicus' debt to Islamic astronomers. Copernicus' mathematical models, which were supposed to solve the so-called 'equant problem' (among other things), were very similar or identical to those of his Islamic predecessors.[3] Swerdlow stressed the importance of Copernicus' adherence to physical astronomy, i.e., to the astronomy of real, solid orbs. And finally, he voiced speculation about Copernicus' path to heliocentrism. He posited that Copernicus had come to his heliocentric cosmology by a technical route, that Copernicus turned to heliocentrism because he believed that the planets are carried around by solid spheres and because he adhered to the principle of the uniform and circular motion of the heavenly spheres. Copernicus' search for an alternative that avoided Ptolemy's violation of the second principle (the equant problem) led him to a 'Tychonic' cosmography that had the Sun moving about the Earth while being more or less at the center of the orbs of the retrograding planets. Since, in this system, the solid orbs of the Sun and Mars intersect, Swerdlow speculated that Copernicus opted for one with a static Sun and a moving Earth in which all the orbs were discretely nested.

[3] To the observer stationed on the motionless Earth at the center of the cosmos, the five planets along with the Sun and Moon exhibit nonuniform velocity during their courses through the zodiacal band. Ptolemy tried to solve this problem with the concept of the equant, a mathematically established point or *punctum equans* about which each body was supposed to move uniformly. This solution was deemed unsatisfactory and problematic.

Swerdlow's publication incited discussion of Copernicus' belief in solid spheres and his debt to his Islamic predecessors for his mathematical models. Critics of Swerdlow's reconstruction, who include Feldhay and Ragep, as we shall see later, claimed that there must be 'more to this monumental cosmological shift than a strictly mathematical/astronomical explanation' and that there 'were certainly other ways to deal with the problem of the equant and other Ptolemaic violations' [4]. Al-Shāṭir, for example, from whom Copernicus apparently borrowed extensively in the *Commentariolus*, dealt with the Ptolemaic difficulties while retaining a geocentric cosmology.

There have indeed been other proposals that pretend to provide 'the missing cause or motivation' for Copernicus. Mario Di Bono drew attention to the Paduan Aristotelians, Andre Goddu to the Cracowian Aristotelians, and Robert S. Westman to the astrological 'crisis' caused by questions about the planetary order. But Feldhay and Ragep are uncomfortable with the predominant attempts to reduce the Copernican question 'to one of finding the univocal explanation that somehow supersedes all others' and with the fact that 'the most recent discussions of Copernicus have taken a Eurocentric turn, with the question of cross-cultural influence mostly set aside' [5]; and so they have assembled scholars to discuss the background to Copernicus in a multicultural and multidisciplinary way. With the *Commentariolus* as the endpoint, these discussions were guided by a set of observations from which several conclusions were reached. Let me cite these seven observations in full here:

1 Copernicus' stated purpose in the *Commentariolus* is to find 'a more reasonable model composed of circles…from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires'.
2 Copernicus does not refer in the *Commentariolus* to the 'marvelous symmetry' brought on by his new ordering of the planets, as he does in *De revolutionibus*. Although one must be cautious when speaking of motivation, it is curious that Copernicus does not explicitly put forth in the *Commentariolus* what is perhaps his most compelling argument.
3 Copernicus' models (taking into account both the *Commentariolus* and *De revolutionibus*) contain both eccentrics and epicycles.
4 There is strong evidence that Copernicus adheres to solid-sphere astronomy.
5 There is no indication that Copernicus ever resorted to a strictly Aristotelian, Averroist, Biṭrūjian, or Paduan 'homocentric' astronomy. Copernicus does insist on a single center for his main orbs and otherwise uses

only epicycles in the *Commentariolus*, whereas he uses eccentrics with their multiple centers in his *De revolutionibus*.

6 The number of similarities between the planetary models in the *Commentariolus* and those advanced by Ibn al-Shāṭir (14th-century Damascus) is significant.

7 Discussions of the possibility that the Earth is in motion can be found in both Islam and Christendom prior to Copernicus. [5–6]

While Feldhay and Ragep admit that 'any number of conclusions may be drawn from these observations' [6], they propose the following:

(a) Copernicus' initial motivation was to address the violation of the principle of perfect motion, that is, of its uniformity. The *symmetria* of the cosmos achieved by the heliocentric ordering of the planets in *De revolutionibus* was *post hoc*. They are, therefore, not convinced by Goldstein [2002] and Westman [2013] that the ordering of the planets was a motivating factor (from 1 and 2).

(b) Copernicus' work falls within the tradition of Ptolemy's *Almagest* and *Planetary Hypotheses*, the *hayʾa*-tradition of Islamic astronomy, and the 15th-century revival of Ptolemaic astronomy and cosmology as found in Peurbach's *Theoricae novae planetarum* and in Regiomontanus' *Epitome of the Almagest* (from 3, 4, and 5).

(c) In his early career, Copernicus was concerned with some kind of quasi-homocentrism (from 5).

(d) He was significantly influenced by post-1200 Islamic astronomy (from 6). The existence of a longstanding criticism of Ptolemy and alternative models that were developed within the geocentric cosmology highlight, however,

> that it was not necessary for Copernicus to make his momentous transformation in order to satisfy his stated goal of a cosmography with uniform circular orbs. It thus seems that there were aspects of Copernicus' intellectual and cultural context that led him to his decision to put the Earth in motion. [6–7]

(e) Copernicus may have been aware of, or influenced by, discussions about the motion of the Earth in prior Christian and/or Islamic traditions (from 7).

Feldhay and Ragep's point of departure was their dissatisfaction with Swerdlow's technical reconstruction of Copernicus' conversion to heliocentrism. Copernicus might, they reaffirm,

> have fulfilled his stated goal of a reformed astronomy with uniform, circular motions within a geocentric framework. This latter approach was, after all, the one that a number of Islamic astronomers had already employed to a large extent. [7]

Accordingly, they are not convinced that the response to the Copernican question is 'through one correct derivation of a model that necessarily led to a coherent and true astronomical-cosmological picture' [8]. Instead, they see Copernicus' system as a result of many practices

> that included attempts to deal, mathematically, with violations of physics found in Ptolemy's models, discussions of the relation of natural philosophy and mathematics, and epistemological forays into the 'true' cosmology and the human capacity to discover it. [8]

They likewise believe that 15th-century astronomy was

> the outcome of multiple transformations along different paths that crystallized in the work of Copernicus into some kind of coherent whole that differed enough from the preceding astronomical discourse to open the door to additional, enhanced transformations. [8]

1.2. Part 1. Social and political contexts

Christopher Celenza ('What Did It Mean to Live in the Long Fifteenth Century?' [17–28]) discusses some characteristic features of the 15th century that could have shaped Copernicus' world. Celenza reflects on the political life of the time and points out that in order to find some personal safety as well as to advance their intellectual activities, the scholars of Copernicus' period sought personal patronage. Celenza sees Copernicus as a member of the group of traveling scholars in search of patronage and briefly examines his studies at the universities of Bologna and Padua, stressing their 'secularism', that is, their lack of an organic link between concern with the arts and theology, on the one hand, and their link to Italian humanism, on the other, where humanism meant

> a willingness to question authority...Given this situation, Copernicus' willingness to entertain divergent techniques (like the Ṭūsī-couple) and possibly revolutionary viewpoints (like heliocentrism) becomes more understandable. [20]

Celenza also shows that in Copernicus' time intellectual elites still believed in supernatural powers.

The most important section of the chapter, however, is perhaps the one dedicated to the way in which information was gathered and transmitted. One of the characteristics of the 15th century was a collaborative approach to knowledge. There were many different varieties of reading and writing practices and

> a number of them make it likely that [Copernicus] may well have come across a theory like the Ṭūsī-couple without feeling the characteristically modern need to record precisely where, when, and in what format he encountered it. [28]

Nancy Bisaha ('European Cross-Cultural Contexts before Copernicus' [29–41]) focuses on the political realities relevant to the transmission of knowledge. Her basic question is

> [W]hy did Copernicus and his contemporaries say nothing about recent Islamic astronomers if they were so heavily indebted to them?...How and why did such astronomical knowledge travel great distances in the early modern era, only to have its origins vanish so effectively that scholars did not discover them until the last few decades? [29]

She draws a picture of the complex, multifaceted relations between Latin Europe, the Ottoman Empire, and Byzantine refugees in Europe. The exchanges that took place among European, Asian, and Byzantine scholars were characterized by connections and tensions at the same time. Muslims, for instance, 'were extremely wary of travelling in Christian Europe, with the exception of Venice, throughout the period' [32]. Her key examples that illustrate this situation are the books *Europe* and *Asia*, often printed together and read as one piece called the *Cosmographia*, written by Aeneas Silvius Piccolomini, Pope Pius II (1458–1464). These two texts reflect the crystallization of a European identity *vis-à-vis* the perception of Asia as 'the other'. Bisaha considers three possible explanations of why Copernicus did not acknowledge his borrowings from Islamic astronomy:

(1) The Islamic origins of Copernicus' ideas were obscured at some point by Greek refugees, who

> found the provenance a sensitive subject given their adamant calls for crusade and the rhetoric of Ottoman barbarism that was so fashionable in western Europe. [40]

(2) 'Copernicus knew the origins and chose not to note them for fear of unpleasantness or a harsh reaction from the papacy' [40].

and

(3) This lack of provenance could be 'simply due to an innocent omission at some point in the transmission' [40].

Bisaha points out one common denominator that emerges despite all of this uncertainty. These new ideas

> travelled westward and were used, but they were changed or cloaked consciously or unconsciously, perhaps to make them fit with the growing belief among Europeans that their current scholarship had surpassed that of the East. [41]

1.3. Part 2. Intellectual and scientific contexts

With Edith Dudley Sylla's chapter ('The Status of Astronomy as a Science in Fifteenth-Century Cracow: Ibn al-Haytham, Peurbach, and Copernicus' [45–78]), we focus more closely on Copernicus; more exactly, on his *Commentariolus* and its background, which can, according to Sylla, be found in Copernicus' years as a student in Cracow (1491–1495). Two eminent teachers, John of Głogów and Albert de Brudzewo, were active there at that time. Głogów probably lectured on Aristotle's *Posterior Analytics* and, in 1499, when Copernicus had already left Cracow for Bologna, published a commentary thereon. He also wrote a commentary on Sacrobosco's *Sphere*. Brudzewo wrote a commentary on the most popular and progressive textbook of the day in astronomy, Peurbach's *Theoricae novae planetarum*, which was also printed after Copernicus' departure. It is very likely, however, that Copernicus was familiar with all three texts either through manuscripts or through lectures (not necessarily by Głogów and/or Brudzewo) based on these manuscripts.

Sylla develops two lines of investigation. One is the development of the theoretical and narrative, i.e., non-demonstrative, astronomy that was intended as introductory and is found in the so-called *theorica*-tradition. This was physical astronomy, an astronomy that proposed the physical bodies that might lie behind the observed motions described mathematically in Ptolemaic astronomy. She links Ibn al-Haytham's *On the Configuration of the World* (transmitted to Latin-speaking Europe at the latest by the end of the 13th century) and the *hayʾa*-tradition of Islamic astronomy with the European tradition of *theorica*-astronomy, and this in turn with Peurbach's *Theoricae novae planetarum*, and the *Theoricae novae* with Copernicus' *Commentariolus*. The second line of her investigation concerns the status of astronomy as a science as this was understood in the commentaries on Aristotle's *Posterior Analytics*. She approaches this question through the medieval opposition of *antiqui versus moderni* and, closer to Copernicus, by an analysis of the above-mentioned texts by John of Głogów and Albert de Brudzewo.

Copernicus' *Commentariolus* lies firmly in the same tradition as Peuerbach's *Theoricae novae planetarum*. It is 'theoretical rather than practical, narrative rather than demonstrative, and based on the assertion of hypotheses or principles' [45]. The *Commentariolus* mirrors the *Theoricae* in starting with a statement of principles. In Copernicus' work these principles are called postulates (*petitiones*) and in Peurbach's work they are the *theoricae* (figures) themselves together with their descriptions of planetary orbs. Copernicus' *petitiones* represent

> hypotheses derived from experience, which are to be accepted as true, even though they could be wrong given that astronomy is a science still in the process of development. [49]

The orbs of the *Theoricae* (three-dimensional, three-part spherical shells) are the identifying DNA of the configuration that it shares with Ibn al-Haytham's *On the Configuration of the World*. Ibn al-Haytham and the Islamic *hayʾa*-tradition understood these orbs as rigid, not fluid bodies. They included deferents and epicycles, and, while the planets are held tightly in place, they can rotate uniformly but without ever exceeding the place or cavity they are in. Moreover, these orbs spin. Brudzewo's commentary on Peurbach's *Theoricae novae* establishes what he understood to be the proper principles of *Theoricae novae*. All five principles are of a physical rather than mathematical nature, such as, for instance, the second: 'Of any simple body there is only one simple motion proper to it naturally'. These 'principles have a relation to Peurbach's *Theoricae novae planetarum* similar to the relation of Copernicus' *petitiones* to his *Commentariolus*' and are ultimately derived 'from thinking about observations and how they could be explained by underlying reality' [53]. This format, however, was not unique to theoretical astronomy. Many scholastic philosophers before Brudzewo

> put their theories or parts of their theories into a structure in which there are suppositions, principles, or premises (i.e., hypotheses) on which conclusions are based. [54]

These principles are usually physical rather than mathematical and are held to be derived from experience.

Although Sylla believes that the predominant influence on the *Commentariolus* was that of the conception of astronomy in the *Theoricae novae planetarum*, she thinks that

> the background of Aristotelian philosophy at Cracow also helps to explain why Copernicus might have proposed a new configuration of the world in the *Commentariolus*. [60]

This leads her to discuss the concepts of science in general and astronomy in particular as formulated in different commentaries on Aristotle's *Posterior Analytics*. She situates the discussion within the medieval Aristotelian opposition between a conservative *via antiqua* and a progressive *via moderna*, and argues that

> the conception of astronomy as a science that Copernicus encountered as a student at Cracow University, the one reflected in the *Commentariolus*, was closer to the attitudes of the *moderni* than to those of the *antiqui*. [59]

This is confirmed by Głogów's texts (*Commentary on Sacrobosco's* On the Sphere of the World and *Commentary on the* Posterior Analytics), which are consistent with the views of the *moderni*. In his *Commentary on the* Posterior Analytics, Głogów, for example, in answering the question of whether it is possible to know something *de novo*, opposes Plato in claiming that we can have scientific knowledge and that it can be new rather than always something that we knew previously but forgot. One of the important features of his commentary *On the Sphere* is a distinction between what is mathematical (hence imaginary, hence dependent on human thought) in astronomical theories and what is physical. The same is the case with Brudzewo's *Commentary on the* Theoricae novae planetarum. He, too, has a clear conception of astronomy as partly physical and partly mathematical. He repeatedly differentiates between physical orbs and mathematical/imaginary circles. Brudzewo argues that astronomers are not to dispute the basic principle of astronomy, that is, the uniform circular rotation of the celestial bodies. He also claims explicitly that the equant is not a physical thing since there is no corresponding aetherial sphere in the heavens. Despite that, astronomers used it for the purposes of practical astronomy (i.e., astrology) to support prognostications concerning the effects of the heavenly bodies on Earth.

What, then, did Copernicus learn while studying in Cracow? The main thing was Peurbach's *Theoricae novae planetarum*, which served as a model for the status of astronomy as a science. Copernicus was exposed to the idea of theoretical (not demonstrative) astronomy according to which the astronomer 'can start by stating principles or postulates upon which the following exposition will be based' [53]. This had certain consequences for astronomers. Knowing that 'principles are not proved and that the processes by which they are arrived at are not logically rigorous' [54], astronomers could be led to think about a reformation of principles. And this, according to Sylla, is exactly what Copernicus says at the very beginning of the *Commentariolus* before he lists his seven postulates (*petitiones*).

Since Copernicus, like the authors of *theoricae planetarum*, starts with physical principles, he must have 'conceived his research program within the *theorica planetarum* genre'. Copernicus also learned 'that astronomy was both mathematical and physical and that, although it had many real achievements, it might still be improved by new insight into the hidden physical structures behind the appearances' [55]. The physical side of astronomy was represented in real three-dimensional orbs; the mathematical side was represented in *theoricae*/figures that were two-dimensional geometrical circles and lines. These figures were understood as products of mathematical constructions or human imagination and not as real things existing in the external world. The task of physical astronomy was to find physical bodies that might lie behind the observed motions described mathematically in Ptolemaic astronomy. In Copernicus' period, this task of finding physical configurations consistent with mathematical regularities had not been completed. There was, therefore, a constant need for new and better physical hypotheses, better physical configurations. Astronomy was, therefore, conceived as a progressive scientific discipline in which principles were 'derived *a posteriori* from experience and hence could be received from new or added experience' [59].

Michael Shank ('Regiomontanus and Astronomical Controversy in the Background of Copernicus' [79–109]) discusses the life of the most important and advanced astronomer before Copernicus, Johannes Regiomontanus, his approach—or better, approaches, as we shall see—to astronomy, and his impact on Copernicus. Two important personalities had a strong influence on Regiomontanus' career. One was the astronomer and humanist Georg Peurbach, author of the *Theoricae novae planetarum*, with whom Regiomontanus worked in Vienna. The second was Basilios Bessarion, a Greek *émigré*, originally a Byzantine orthodox and a student of the Platonist George Gemistos Pletho, who became a cardinal of the Roman Catholic Church and was instrumental in procuring the *Epitome of the Almagest*, the book that Copernicus preferred over the *Almagest*.

One of Regiomontanus' earliest astronomical manuscripts is a copy of Peurbach's lectures of 1454 on his *Theoricae novae planetarum* at the *Bürgerschule* in Vienna. The 'New (*novae*)' in its title signaled the fact that it presented the real, physical configurations and motion of the spheres, as opposed to merely mathematical ones. When Regiomontanus edited it for the first time in 1474, partial spheres of the planetary models, being physical, were filled in with black ink or striking colors, while the purely geometrical diagrams were thin black-on-white lines. Regiomontanus' astronomical interest did not stop with his mentor's work. While in Vienna, he also studied Henry of Langenstein's *De reprobatione ecentricorum et epicyclorum* (1364), which stimulated his openness to homocentric possibilities. He later formulated similar proposals and objections when criticizing Ptolemy's approach in the *Almagest*. Regiomontanus was also aware of the earlier homocentric system of al-Dīn al-Biṭrūjī's *De motibus celorum* (translated into Latin from the Arabic in 1217 by Michael Scot) and his unorthodox arrangement of the inferior planets according to their synodic period: Venus above the Sun and Mercury below it.

In 1461, Regiomontanus left Vienna for good in the company of Cardinal Bessarion. His association with Bessarion was connected with a long controversy between Bessarion and another Greek *émigré* in Italy, George of Trebizond (1396–1472). George had translated Ptolemy's *Almagest* from Greek into Latin in order to replace Cremona's 12th-century Latin translation from the Arabic but his new translation and the commentary were judged less than satisfactory. The commentary itself was full of errors and Bessarion was angered by George's attacks on Theon of Alexandria's commentary, which Bessarion recommended as a guide. The relationship of the two men deteriorated even further for philosophical reasons. In 1455, George published *Comparatio philosophorum Aristotelis et Platonis*, an *apologia* of Aristotle and an attack on Plato and his followers, especially Pletho and Bessarion. During his diplomatic visit to Vienna (1460–1461), Bessarion convinced Peurbach and Regiomontanus to write an epitome of the *Almagest* that would displace George's work on the subject. Peurbach started, finished half of the *Epitome of the Almagest*, and then died suddenly in April 1461. When Bessarion left for Italy, Regiomontanus accompanied him and remained a member of the Cardinal's *familia*, improving his Greek, revising Peurbach's first half, and writing the remainder of the *Epitome*. He completed the task in about 1462. The *Epitome*, however, remained a manuscript with limited circulation which became wider after it was printed in Venice in 1496, the year of Copernicus' arrival in Bologna.

The *Epitome* is a detailed, sometimes updated, condensed, and clearer exposition of Ptolemy's *Almagest*. Its format follows the general structure of the *Almagest* but has a more Euclidean layout. Along the lines of the *Almagestum parvum*, each book is organized into propositions, many followed by proofs. It sometimes comments on post-Ptolemaic developments. On the other hand, the summary of book 1—the most natural-philosophical part of the *Almagest*—leaves the discussion in the second century and says nothing about the late-medieval natural-philosophical debates about the rotation of the Earth. Among the problems of Ptolemy's astronomy, the *Epitome* notes the problems with its lunar theory. Another intriguing feature is the proof of the equivalence of the epicyclic and eccentric models for the second anomaly of the planets in book 12.

After finishing the *Epitome*, Regiomontanus dived into Bessarion's library, which contained 1,000 Greek and Latin manuscripts and included several Greek *Almagests*, Proclus' *Hypotyposis astronomicarum positionum*, Theon of Alexandria's *Commentary on the Almagest*, and Theon of Smyrna's *Mathematical Knowledge Useful for Reading Plato*. It is worth noting that Proclus, in his *Hypotyposis astronomicarum positionum*, refers to the proof of the equivalence between the eccentric and epicyclic models.

In 1463, Regiomontanus entered into a correspondence with the Italian astronomer Giovanni Bianchini that demonstrates his mathematical skills, his dissatisfactions with the existing tables and mathematical models, and his expectations of consistency in physical and mathematical predictions, all being consistent with his hopes for the advent of a homocentric astronomy. His *Defensio Theonis contra Georgium Trapezuntium*, a work intended to destroy George's *Commentary on the* Almagest, reveals Regiomontanus' desire for an astronomy that would integrate physical and mathematical considerations. The *Defensio* shows his conflicting sympathies: Ptolemaic, homocentric, and Peurbachian. Regiomontanus 'faced a trilemma that left unresolved the tensions between the pros and cons of his three options' [97]. In this text, Regiomontanus also treats the order of the planets as an unsolved problem and illustrates it by citing the different positions taken by Ptolemy, Martianus Capella, Geber, Biṭrūjī, and others:

> Copernicus would work on precisely this problem and was thrilled to see that reordering the planets (and the Earth) around the mean Sun gave their spheres a necessary order. [97]

After some time spent in Hungary, Regiomontanus moved to Nuremberg and set up the first printing press devoted primarily to the mathematical sciences.

What about Copernicus' use of Regiomontanus' work? Copernicus owned and used several works by Regiomontanus, especially his *Epitome of the Almagest*, in many ways. The earliest traces of the language of the *Epitome* are in Copernicus' 'computations of planetary spheres that preceded the conversion to heliocentrism before the *Commentariolus*' [102] but they also pervade the detailed quantitative implementation of his new theory in his *De revolutionibus*. Another point of considerable significance is that the *Epitome*

> stressed some of the unfinished business of astronomy, such as the order of the Sun and the inferior planets, to which Regiomontanus explicitly ascribed 'no certainty' (*nulla certitudine*) at the beginning of Book 9. [102]

But the most important impact of the *Epitome* on Copernicus is that it

> stands behind Copernicus' move to his new astronomical system, which placed not the physical Sun but the mean Sun at the center of the Earth's orb. [102]

Another significant sign of Copernicus' faith in the *Epitome* is his

> following Regiomontanus in *not* undertaking to derive his astronomical models themselves from observations. Both men believed that, whatever their problems from a physical point of view, Ptolemy's models were basically adequate to their task from the geometrical and predictive points of view. [108]

Rivka Feldhay and Raz Chen-Morris ('Framing the Appearances in the Fifteenth Century: Alberti, Cusanus, Regiomontanus, and Copernicus' [110–140]) analyze different conceptualizations of appearances (*phaenomena*) in the

15th century and their possible relevance for Copernicus. In an often overlooked passage of the *Commentariolus*, Copernicus denounces the philosophers' defense of the immobility of the Earth as being founded upon appearances; and in his later *De revolutionibus*, he explains the phenomena of the movements in the heavens, such as the risings and settings of the zodiacal signs and the fixed stars, the stations of the planets and their retrogradations, by the motions of the Earth 'which the planets borrow for their own appearances' [Rosen 1992, 18]. Copernicus' claim, in other words, is that the immobility of the Earth, one of our most basic visual experiences, is just apparent (visible but not true), while at same time he affirms that the mobility of the Earth—not experienced, invisible—is a reality that explains the apparent motions of the stars and the planets. How could he have come to such a conclusion? Or, to put it differently, '[W]hat enabled the competent, cautious astronomer Nicholas Copernicus to embrace the idea of an invisibly moving Earth?' [114]. In line with the introduction, Feldhay and Chen-Morris are critical of Swerdlow's technical reconstruction of Copernicus' path to heliocentrism. Why did Copernicus, they ask, find a heliocentric conversion of an eccentric model of the second anomaly for the inferior planets attractive (i.e., the element, according to Swerdlow, that is crucial in the transition to a heliocentric cosmology), whereas Regiomontanus simply stopped short of all that?

> If Regiomontanus was very likely aware of the possibility of a heliocentric conversion, as Swerdlow maintains, one may rightly assume that there was no mathematical-technical reason for him to reject it. Likewise, there was no mathematical-technical reason for Copernicus to adopt it and infer further the motion of the Earth. [114]

There is 'no clear answer to such a question' [114], but Copernicus' claim about his engagement with something 'beyond appearance' (*praeter apparentia*) encourages an investigation of the conceptualizations of the relationship of appearances to their 'beyond' in 15th-century Europe.

Feldhay and Chen-Morris search for an answer to their question in the works and practices of Leon Battista Alberti (1404–1472), Nicholas of Cusa (i.e., Cusanus) (1401–1464), and Johannes Regiomontanus (1436–1476). These three important figures, plus Paolo Toscanelli (1397–1482), were connected through a social network: Regiomontanus, Toscanelli, and Cusanus even met personally at Bessarion's villa in Rome, while Alberti, a member of the papal curia since 1420, was a constant visitor to the villa—which

> testifies to the existence in Italy of a cultural field in which mathematicians…as well as philosopher-theologians like Cusanus took a position and articulated their critique of each others' views. [113]

Copernicus probably acquainted himself with this field when he came to Bologna in 1496, and 'this field may have inspired his daring to experiment with the idea of a moving Earth' [114].

Alberti's *De pictura* (1435–1436) laid the foundations for the theory of artificial perspective. Feldhay and Chen-Morris see it

> as an ambitious project to broaden the scope of the visible that challenged the accepted boundaries between the natural and the artificial. [113–114]

His enterprise concerned the question of how a

> sensible and mathematical, yet invisible, grid of perspective constitutes the spatial relationships on the surface of the painting and offers a new perception of beauty radiating from things represented to the observer's understanding. [113]

According to Alberti, the artist does not imitate and represent nature itself but aims at the forms of beauty that are 'lurking beyond the phenomena and concealed behind them' [116]. Painting on a two-dimensional surface brings forth Alberti's ideal of beauty, such as the 'symmetry' and 'harmony' between the different parts of the painting.

The desire to see what is beyond appearances found similar expression in the theologian Cusanus, who elaborated Alberti's project by different means. In his major works, from *De docta ignorantia* to *De possest*, Cusanus attempted to explain how one can 'view things that were invisible before' and how the mind can be presented 'with a vivid image of the invisible unification of opposites (*oppositorum coincidentia*)' [117], i.e., God. One of the methods that he used for such purposes was speculative mathematics, with which he tried to solve the quadrature of the circle. He wrote 11 mathematical treatises dedicated to the quadrature and corresponded about it with fellow mathematicians, philosophers, and theologians. He tried to find a 'visible' geometrical point that would represent the 'invisible' coincidence of opposites (i.e., an intellectual vision of God). According to Feldhay and Chen-Morris, Cusanus' writings on quadrature

> engaged the best European mathematicians of the period—whom he personally knew—in a conversation about the quadrature across disciplinary and profes-

> sional boundaries. The echoes of this conversation were likely to have reached Copernicus in Bologna and Ferrara some decades after they took place among Cusanus, Regiomontanus, Toscanelli, and perhaps even Alberti. [121]

For Cusanus, mathematics was not just a method but a model used in the constitution of the world for human understanding. His statement that

> the intellect is to truth like the polygon is to the circle in which it is inscribed [reveals] the motivation behind his investigations of the quadrature problem, namely to observe critically, from an imagined divine point of view, the limitations of the human intellect. Applying the results of his investigations to the theological realm, Cusanus broadened Alberti's discourse on the visible-invisible relationship and provided new kinds of legitimization for naturalizing the invisible within the discourse on human knowledge. [113]

Cusanus' conceptualizations of the mathematical conclusions in theological terms belong to the history of 'invisibles' 'that may have made possible Copernicus' later leap into a cosmological invisible such as the motion of the Earth' [121].

Cusanus' preoccupation with mathematical procedures came to the notice of Johannes Regiomontanus, *via* the Italian mathematician Paolo Toscanelli, a common friend. Regiomontanus wrote a series of texts on the quadrature of the circle, criticizing Cusanus' 'speculations'. Regiomontanus' distance from Alberti's and Cusanus' projects of representing invisible and abstract entities in a visual form is also manifest in his views on the required astronomical reform and the place of observation within it. Regiomontanus constantly complained of the erroneous observations of his predecessors and put his trust in those astronomers ready to make new observations and compare them with sound and good calculations. He himself barely bothered to improve the situation.

What is the stance of traditional astronomy regarding appearances? Since antiquity, astronomy had been based on what the astronomer saw and appearances were assumed to be valid and authentic regardless of the specific theory suggested.

> All there was to be explained was in front of the astronomer's eyes, and these explanations were supplied under the assumption of order. [134]

Appearances are true; they are not illusions and have to be explained in accordance with the assumption that the motions of the heavenly bodies are by nature uniform and circular. For a static observer situated at the center of the universe, the planets really do retrograde. The task of the astronomer is to find

> a system of circles to explain why the planets move in such peculiar ways without damaging the cognitive value of the observer's ocular experience. [134]

Either an eccentric circle or an epicycle would do the job but they are both 'calculated in relationship to the point of view of an observer situated at the center of the universe' [135]. This dependence of mathematical theory on visual experience is clear from Ptolemy's presentation of the equant as an explanation of the anomalies of the planets. The equant is a point that is not directly related to the observer but to a 'point bisecting the line joining the center of the ecliptic and the point about which the ecliptic has its uniform motion' [135]. Ptolemy admits that this procedure is not taken from any apparent principle. It is without proof: its only justification is that it is in agreement with the phenomena. For Ptolemy, the coherence of the models is less important than saving visual experience, which has to be realized in accordance with the more basic principle of preserving uniform circular motion without exception. The specific feature of the equant is that it 'implies that the point from which planetary motions can be viewed as uniform is an imaginary point unrelated to the position of the observer' [135].

But, while the eccentric spheres are physically real and calculated with regard to the observer's central position, the equant is, according to Peurbach, based on an imagined circle around the equant point, i.e., around the point on the line of the apogee as far from the center of its orb as this center is distant from the center of the world. The basic characteristic of traditional astronomy, upheld also by Regiomontanus, was that it assumed the reality of celestial appearances. There is no doubt about what one sees. Astronomers apply invisible spheres and circles only to substantiate the authenticity of their observations. Alberti and Cusanus, however, challenged this traditional conception of astronomy on several levels by probing the demarcation between the phenomenal realm and the realm of invisible structures:

(1) the position of the observer is not predetermined and static; appearances are relative to one's point of view, and

(2) it is possible 'to peer beyond appearances to gauge invisible structures and entities through the use of different kinds of devices' [135–136].

'These two notions', claim the authors, 'may have shaped Copernicus' propensity to accept the invisible motion of the Earth as a basic principle of his system' [136]. In both the *Commentariolus* and *De revolutionibus*, Copernicus continuously points out that appearances misled astronomers into ascribing the wrong motions to the celestial bodies and that one should adopt a critical attitude toward the testimony of the eyes. The interpretation of visual experience has to take into account the position(s) of the observer's own actual viewpoint (no longer central) and his or her location within the entire universe (there are constant changes due to the Earth's motions).

> Going beyond one's local and immediate point of view entailed the realization that appearances are a function of the observer's location. The new forms of visibility proposed by Alberti's techniques of perspective and by Cusanus' geometrical visualizations were part of a more general cultural reassessment of the role of perception in the cognitive process leading to knowledge. This role had special relevance to the epistemological status of astronomy, the observational science par excellence....The core of Copernicus' argument is the limits of sense perception and the need to surpass them. [140]

Whether the Earth moves or not

> cannot be derived from one's sense experience, as these phenomena presuppose the observer's point of view. By calculating the observer's position, Copernicus can transcend visual experience and gauge a new invisible point of view from where a new picture of the universe is revealed. These calculations incorporate novel mathematical techniques coming from the East, yet Copernicus mobilizes these techniques to answer the challenges that Alberti's artificial perspective and Cusanus' theological speculations offered to visual experience in the preceding century. [140]

1.4. Part 3. Copernicus' multicultural background

To open part 3, Sally Ragep ('Fifteenth-Century Astronomy in the Islamic World' [143–159]) paints a fascinating canvas concerning the number of students and practitioners of mathematical sciences (some contemporaries referred to roughly 500 students) in 15th-century Samarqand. This number and the enormous quantity of manuscripts that survived are testimony to how entrenched a scientific education was within Islamic society. Roughly 120 authors wrote some 489 treatises during the long 15th century. Their works are represented by several thousand extant manuscripts located throughout the world. The subject matter of these works was both theoretical and practical astronomy, and it included cosmology (both celestial and terrestrial realms), instruments, handbooks, tables, calendars, timekeeping, and astrology. S. Ragep focuses in particular on theoretical astronomy, i.e., the tradition of *hayʾa*. Works in this tradition belonged to a genre of astronomical literature termed *ʿilm al-Hayʾa*, which attempted to explain the configuration (*hayʾa*) or physical structure of the universe as a coherent whole; thus, for celestial bodies, it included cosmography and for terrestrial bodies, geography. This tradition brought into a single discipline the unchanging celestial realm of aether and the ever-changing realm of the four elements, the world of generation and corruption. This tradition can be traced back to the 11th century when the term « hayʾa » was adopted, particularly in eastern Islam, as the general term for the discipline of astronomy which did not include astrology. The *hayʾa basīṭa* literature was influenced by Ptolemy's *Almagest* (omitting its mathematical proofs) and by his *Planetary Hypotheses*, and usually included discussions of the sizes and distances of the stars and planets. The main emphasis of the *hayʾa*-tradition was on translating mathematical models of celestial motion into a bodily representation in order to show the configuration (*hayʾa*) of the universe as a whole. It focused on external aspects of cosmology, on issues related to *how* the celestial and terrestrial realms operate, not on questions *why*.

Another tradition of Islamic astronomy provided a range of accounts of various aspects of Ptolemaic spherical astronomy and planetary theory. It reworked Ptolemy's *Almagest* and sometimes included original material, such as there is in Ṭūsī's *Taḥrīr al-Majisṭī* (*Recension of the* Almagest) as well as treatises devoted to criticizing and reconciling inconsistencies in Ptolemaic astronomy and to reforming certain models, such as Abū ʿAlī al-Ḥasan ibn al-Haytham's *al-Maqāla fī hayʾat al-ʿālam* (*Treatise on the Configuration of the World*). This treatise attempts to explain how the various components of the Ptolemaic models worked and ultimately fit together. It strives to match the mathematical models of the *Almagest* with physical structures in order to explain the various motions of the celestial bodies.

From these and other examples, it is clear that Islamic astronomy in the 15th century was not an isolated event or episode but was built upon centuries of scientific work. This was also the astronomy 'that most likely provided the immediate context of transmission to a bourgeoning European astronomy' [156] through the institutions of the Ottoman Empire. It is, as S. Ragep affirms, ‘through these Ottoman institutions that one finds the connection between Islamic astronomy, Copernicus, and as his immediate Latin predecessors’ [156]. A certain Moses ben Judah Galeano (Mūsā Jālīnūs), the subject of the final chapter of *Before Copernicus* by Robert Morrison, was especially important in this transmission: he traveled, among other places, between the Ottoman court and Italy.

The last question posed by S. Ragep is why Islam, despite thriving scientific traditions and stunning achievements in astronomy, did not give rise to a Copernicus. She claims that the reason lies exactly in these traditions:

> Scientific change may be far more difficult when the traditions...are so entrenched.... Thus, paradoxically, the strength of a scientific tradition, such as that in Samarqand, may have been a hindrance to adopting new, revolutionary ideas. Perhaps the lesson we then take from this cross-cultural comparison is that proposing revolutionary ideas may be easier for someone, such as Copernicus, whose scientific context was less rigid and was, in many ways, a work in progress. [158]

F. Jamil Ragep (‘From Tūn to Toruń: The Twists and Turns of the Ṭūsī-Couple’ [161–214]) takes up the case of the transmission of arguably the most famous astronomical device of Islamic astronomy, the so-called Ṭūsī-couple, which was invented by Naṣīr al-Dīn al-Ṭūsī (d. 1274) to amend Ptolemy’s use of the equant. The Ṭūsī-couple is actually not a single device or model but a general concept that encompasses several different mathematical devices serving different purposes. There are several versions:

(1) the mathematical rectilinear version, which consists of two uniformly rotating circles that can produce oscillating straight-line motion in a plane between two points;
  (a) a physicalized version of (1);
(2) the two-equal-circle version, which is a curvilinear version meant to produce a linear oscillation on a great circle;
(3) the three-sphere curvilinear version, consisting of three additional orbs enclosing the epicycle that are meant to produce a curvilinear oscillation that results in motion in latitude; and
  (a) the two-sphere curvilinear version, which is a truncated version of the full three-sphere curvilinear version.

Ṭūsī elaborated different versions of the device at different stages of his career and used them to solve different technical problems. The first one and its physicalized version, for example, were used with the aim of replacing the equant in planetary models. The second one was meant to account for Ptolemaic motions requiring a curvilinear oscillation on a great circle.

Within the Islamic context, the Ṭūsī-couple was subject to further development and discussion over many centuries. Since there are no translations of Ṭūsī's writings on the couple in non-Islamicate languages, J. Ragep postulates transmission though non-extant texts and/or non-textual transmission and thus bases his case 'on plausibility rather than direct evidence' [174]. He argues, given the various types of evidence of transmission, that 'independent rediscovery, especially multiple times, becomes much less compelling' [175].

There were several appearances of the Ṭūsī-couple outside Islamic societies. The first occurred in Byzantium around 1300. It is found in the work of a certain Gregory Chioniades of Constantinople, the translator of a number of astronomical treatises from Persian (or perhaps Arabic) into Greek. One of them, which is dubbed *The Schemata of the Stars*, uses the Ṭūsī-couple in the lunar model and thus seems to derive from Ṭūsī's earlier Persian (not Arabic) works.

> [T]here can be no question that some of Ṭūsī's innovations had made their way into Greek by the early fourteenth century, and the existence in Italy of the only three known manuscript witnesses strongly suggests that the transmission of this knowledge had made it into the Latin world by the fifteenth century. [176]

In Latin Europe, the Ṭūsī-couple appeared several times—the first was in the 14th century. Here follows a list of authors in whose works it can be found: Avner de Burgos, Nicole Oresme, Joseph ibn Naḥmias, Georg Peurbach, Johann Werner, Giovanni Battista Amico, and Girolamo Fracastoro (*Homocentrica*, 1538), who refers to a device for producing rectilinear motion but does not incorporate it into his astronomy.

Copernicus used the Ṭūsī-couple in both his *Commentariolus* and his *De revolutionibus*. In the *Commentariolus*, he used the truncated two-sphere curvilinear version for the latitude models and the physicalized rectilinear version to vary the radius of Mercury's orbit, but in a truncated, two-sphere version without the enclosing/maintaining sphere. It seems, J. Ragep assesses,

> that Copernicus was attempting to provide actual spherical models for the two versions of the Ṭūsī-couple he uses in the *Commentariolus* but that he cut a corner or two by not dealing with the disruption of the contained orb. [184]

In *De revolutionibus*, Copernicus relies only on the two-equal-circle version, which is a mathematical, not a physical, model.

Although it seems that the majority of historians of early astronomy have accepted to a lesser or greater degree the influence of late-Islamic astronomy on early modern astronomers, particularly Copernicus, there are some (Di Bono and Goddu, for instance) who demand more evidence of transmission. In order to provide such evidence, J. Ragep summarizes the past 25 years on the issue. Dealing first with the critics of transmission (Veselovsky, Di Bono, Goddu), he then provides empirical evidence of transmission.

There is evidence that the Ṭūsī-couple first made its way into another cultural context through Byzantine intermediaries, first and foremost through Gregory Chioniades. This transmission occurred through an adapted translation from Persian into Greek. The circumstances under which Gregory's *Schemata* itself was further transmitted are less clear. The *Schemata* is currently witnessed by three manuscripts: two in the Vatican and one at the Biblioteca Medicea Laurenziana in Florence. These sources provide evidence that the work, with diagrams, was available in Italy as early as 1475. Swerdlow and Neugebauer favor this Italian route for the transmission of the Ṭūsī-couple to Copernicus. Since Copernicus spent part of the Jubilee year 1500 in Rome, this opens up the possibility that he had access to the *Schemata*.

There may also have been another channel of transmission—the Spanish connection—which could have brought the new astronomy of 13th-century Iran to the Latin West. There was considerable ongoing diplomatic activity between the Spanish court of Alfonso X of Castile and the Mongol Īlkhānid rulers of Iran.

And there is yet another possibility, the Jewish link. Tzvi Langermann and Robert Morrison have shed light on a host of personalities involved in the transmission of astronomical models from Islam to Christendom through Jewish scientists and mathematicians. Langermann has shown that in 15th-century Italy, Mordecai Finzi knew the *Meyashsher ᶜaqov* of Avner de Burgos, in which it is proved that a continuous straight-line oscillation could be produced by means of a Ṭūsī-couple. That Finzi knew of the *Meyashsher ᶜaqov* is indicated by his copying of some interesting technical details in Avner's text. It seems reasonable to assume, as J. Ragep claims, that Finzi 'knew the other parts of the *Meyashsher ᶜaqov*, including the Ṭūsī-couple proof' [190]. Finzi also had extensive contacts with Christians. Finzi is an example of 'a Jewish scholar who most likely knew of the Ṭūsī-couple in contact with north Italian mathematicians a generation or so before Copernicus would be in the neighborhood' [190].

The last piece of empirical evidence of transmission discussed by J. Ragep is the sheer number of the manuscripts containing one or other of the versions of the Ṭūsī-couple. In this context, it is significant

> that the critical proposition that Swerdlow has claimed was used by Copernicus to transform the epicyclic models of Mercury and Venus into eccentric models, which is found in Regiomontanus' *Epitome of the Almagest*, was put forth earlier in the 15th century by ᶜAlī Qushjī of Samarqand. [191]

It is not known how Qushjī's treatise came to be known by Regiomontanus but a very likely candidate for transmitter is Cardinal Basilios Bessarion.

Robert Morrison ('Jews as Scientific Intermediaries in the European Renaissance' [198–214]) takes up the role of Jews in the circulation of scientific knowledge. Morrison argues against a solely European context for Copernicus' work and discusses the criticism and modifications of Ptolemaic astronomy in both Renaissance Europe and Islamic societies, and how Copernicus could have learned of the achievements of astronomers from Islamic societies. The focus of the chapter is the Ṭūsī-couple and how a text in astronomy, *The Light of the World*, which was written by the Jewish astronomer Joseph ibn Naḥmias (*fl. ca* 1400) and composed in Judaeo-Arabic (a dialect of Jews in the Arabic-speaking world), and a recension of it written in Hebrew characters, could have interested Renaissance astronomers.

Morrison points out several parallels between *The Light of the World*, an attempt to improve Nūr al-Dīn al-Biṭrūjī's (*fl.* 1200) *Kitāb fī al-Hayʾa* (*On the Principles of Astronomy*), which was translated into Latin by Michael the Scot, and the works of early modern European astronomers interested in the revival of homocentric astronomy. Naḥmia supposes that all celestial motions occur on the surface of an orb and accounts for these motions by means of a set of homocentric orbs with the Earth at the precise center of that orb or set of orbs. His models improved on the predictive accuracy of Biṭrūjī's models, although not completely. Regiomontanus and other Renaissance astronomers, working and/or interested in the tradition of homocentric astronomy, would certainly be interested in his models due to their philosophical consistency. Since there is no evidence of the presence of theories from *The Light of the World* in the Veneto as early as 1460, Morrison agrees—despite certain similarities between Regiomontanus' homocentric models and the Hebrew recension of *The Light of the World*—with Swerdlow that it did not influence Regiomontanus.

One of the interesting technical features of *The Light of the World*, adopted in the Hebrew, is the improvement of the reciprocation mechanism. In addition to this development of the mechanism for reciprocal motion, both the Arabic and Hebrew versions contain another hypothesis that is mathematically equivalent to the curvilinear version of the Ṭūsī-couple in Ṭūsī's *al-Tadhkira fī ʿilm al-Hayʾa*. They both suggest the elimination of the circle of the path of the center and the inclined circle carrying the circle of the path of the center from the solar model. This is the model that appeared in Giovanni Battista Amico's (d. 1538) *De motibus corporum coelestium*, written in the 1530s in Padua. Another reviver of homocentric astronomy, Fracastoro, referred to the double-circle hypothesis but did not incorporate it into his astronomy. There is a real possibility that Amico and Fracastoro could have learned of the double-circle hypothesis from *The Light of the World*.

Morrison continues by presenting specific connections between Islamic, Jewish, and European scholars and routes by which Jews became intermediaries between Islamic astronomers and European Renaissance intellectuals. Morrison focuses on two possible channels. One of the possible mediators, probably the key one, was Moses ben Judah Galeano (Mūsā Jālīnūs, d. after 1542), who was present at the court of the Ottoman Sultan Bāyazīd II (1481–1512) in Istanbul. Galeano composed a Hebrew text entitled *Ta ʿalumot ḥokmah* (*Puzzles of Wisdom*) around 1500 and finalized it in the 1530s. *Puzzles of Wisdom* mentions the astronomy of ʿAlāʾ al-Dīn ibn al-Shāṭir, whose models figure extensively in Copernicus' work and explains that *The Light of the World* was a text about homocentric astronomy. It also describes Galeano's visit to Venice around 1500, during which he met with the prominent printer Gershom Soncino. Another possible route for the passage of *The Light of the World* was from al-Andalus to Istanbul and from there to Padua. Linguistic evidence suggests that Galeano's own text on homocentric astronomy found in the Topkapi Library was translated from Hebrew or transcribed from Judaeo-Arabic. It is, therefore, plausible that the extant Arabic text by Galeano is a translation or transcription carried out in Istanbul of a now lost Hebrew or Judaeo-Arabic version of *The Light of the World*, which was probably made before Galeano left Istanbul for Venice. In any case, the contents of *The Light of the World*, if not the complete manuscript, clearly found their way to Istanbul.

The striking parallels between Ibn Naḥmias' theories and those of the astronomers in Padua, Galeano's voyage to Venice, and the eventual (much later) report of *The Light of the World*'s being at Padua make it highly likely that scholars at Padua such as Amico and Fracastoro were aware of the contents of *The Light of the World*. The career of Moses ben Judah Galeano helps to explain the numerous parallels with Ibn al-Shāṭir's theories in Copernicus' work.

Another question regarding scholarly exchange is whether any Jews knew what contemporary European Renaissance astronomers were doing. As proven by translations of Averroes' (Ibn Rushd's) corpus into Latin, there was an area of contact between Jews and Christians in Europe: Jews translated three-fourths of Averroes' writings into Latin from Hebrew translations of the original Arabic. Furthermore, there is some evidence that the last Jewish Averroist, Elijah Delmedigo (d. 1493), knew of recent efforts to develop new theories in astronomy. While his commentaries on Averroes' Latin *Metaphysics* and on his *On the Substance of the Celestial Orb* do not refer explicitly to Ibn Naḥmias or even to Biṭrūjī's work, Delmedigo's Hebrew commentary *On the Substance of the Celestial Orb* makes 'a clear connection between the dismissal of eccentrics and epicycles and Renaissance Averroism's interest in the physical world' [210]. In the same commentary, Delmedigo also makes a reference to attempts to reform Ptolemaic astronomy in the face of the familiar Averroist criticism that Ptolemy's eccentrics and epicyclic orbs contradict the roots of natural science. He complains that some later astronomers were trying to save Ptolemy by positing bodies without any function except for filling the void. Morrison suggests that Delmedigo here refers to Ibn al-Haytham or Jābir ibn Aflaḥ, critics of Ptolemy, cited in Ibn Rushd's *Talkhīṣ al-Majisṭī*. Since Delmedigo's manuscript was probably composed in 1485 and copied in 1492, that is, before Delmedigo returned from Italy to Crete, it is possible that 'the attempts to save Ptolemy to which Delmedigo referred were attempts by European astronomers such as Regiomontanus, not the work of recent Islamic astronomers' [211]. This would provide evidence

> that a prominent Jewish scholar may well have known of developments in 15th century European astronomy, providing more indications that Galeano

> would have known that there were European astronomers interested in the news he was bringing from the Ottoman Empire, and/or it is evidence that another Jewish scholar in Galeano's milieu knew about important achievements in Islamic astronomy. [211].

But even if the referent were earlier critics of Ptolemy, this text would have alerted the reader to the interest of scholars in Europe (which is where Delmedigo was writing) in models based on perfectly homocentric orbs as solutions to the known problems of Ptolemaic astronomy. The role of Jews from both Andalusia and the Ottoman Empire in the scholarly exchanges is also evident from their role in the composition of astronomical tables.

G.B
2
Critical assessment

Before Copernicus is a rich book in terms of both scope and depth.[4] The result of a project extending more than 15 years and four workshops held at different academic institutions, the book brings together eight chapters written by some of the leading experts in the field who can claim a substantial number of important publications. Most of the chapters, if not all, make very handy summaries of the previous research and publications by the authors and other scholars, adding at the same time fresh and nuanced details and insights. Many chapters are illustrated by very useful tables, diagrams, and images. No summary, no matter how extensive, can do complete justice to the wealth of detail, technical and historical nuance, and profound analysis based on a close examination of the vast number of primary sources, while keeping the results of previous research in mind.

In general, I consider the following to be the major strengths of *Before Copernicus*. The first is its very topic: before Copernicus. There had been, despite significant previous research and publication, a need for a comprehensive and up-to-date reexamination of the numerous topics that focus on the immediate and less immediate contexts of Copernicus. We now have a general overview of the basic features of the long 15th century and European atti-

[4] All critical remarks and suggestions that follow are based on my research on Copernicus and his context, which was published in Vesel 2014.

tudes toward the Islamic world as well as a handy and comprehensive study of:

- the development of physical astronomy and different concepts of astronomy as a science during the Middle Ages and Renaissance;
- Regiomontanus' approaches to astronomy and his impact on Copernicus, an intriguing chapter on the different conceptualizations of appearances and their 'beyond';
- Islamic mathematical scholarship in Samarqand and elsewhere;
- the Ṭūsī-couple and its possible transmission channels; and finally
- the role of Jews as scientific intermediaries.

The second is the book's collaborative nature. Authors with different preoccupations, specialists in their own areas of pre-Copernican and Copernican scholarship, concentrate on clearly defined topics (the social and intellectual background to Copernicus' *Commentariolus*). Due to the complexity and enormous range of the issues, this is—as I have experienced myself—hardly a task for one person.

Its third is its 'multicultural' approach. Although the influence of Islamic astronomy on the Latin West, including Copernicus, has been known and widely acknowledged, some scholars still doubt it, especially when it comes to Copernicus. Copernican astronomy is even nowadays sometimes—completely anachronistically and perhaps also ideologically, to use a mild word—supposed to be a pure European achievement. 'They', the 'others', allegedly have nothing to do with his genius. Opposition to such an attitude runs the risk, though, of making Copernicus more indebted to Islamic astronomy than he really was. Putting aside J. Ragep's brief reference to Islamic discussions on the motion of the Earth [see 64n4 above], his chapter and the others that discuss the Islamic influence on Copernicus avoid this pitfall.

Its fourth is its multidisciplinary approach. On several occasions, Feldhay and Ragep in their introduction and Feldhay and Chen-Moriss in their chapter make it clear that Copernicus' heliocentric cosmology was not achieved by a purely technical route. There is, as Feldhay and Ragep put it nicely, 'more to this monumental cosmological shift [i.e., from a geocentric to a heliocentric cosmos] than a strictly mathematical/astronomical explanation' [4].

With that said, let me now address the question Does the book explain the nature of Copernicus' *Commentariolus* and his work in general? I believe it does—but only to a certain extent. It leaves out, unfortunately, some of its essential aspects. If the social and intellectual background that shaped the astronomy and cosmology of the *Commentariolus* (and consequently *De revolutionibus*) are to be understood correctly, many issues that should be addressed are either missing or not adequately treated in this volume. These issues range from the treatment of Copernicus' studies and his work after his final return home from Italy to more theoretical reflections on what Copernicus actually says in the *Commentariolus*, which was, I believe, to a large extent a result of his years in Italy and his work after he returned home. *Before Copernicus* treats his Italian years and what he had learned—the possibilities that had opened up for him there—very superficially. Its focus is mainly on his years in Cracow and, within this framework, only Aristotelian influences are taken into account. A more theoretical problem is that the *Commentariolus* is treated very selectively. When it is cited and discussed, many nuances are overlooked. One would like to understand specifically how Copernicus' context is linked to his text(s). Let me illustrate my reservations by following the structure of the book, starting with the introduction.

I find the last five observations, numbers (3) to (7) [see 41–42 above], and the conclusions reached therefrom to be more or less sound. I also very much agree with Feldhay and Ragep that Swerdlow's technical reconstruction of Copernicus' conversion to heliocentrism is not conclusive. I have, however, some reservations about 'observations' (1) and (2) regarding the principle of uniform motion and the absence of the *symmetria*-argument stated in the *Commentariolus* and the conclusion(s) that they derive from them. It is of course indisputable that Copernicus' first stated purpose in the *Commentariolus* is, to put it briefly, to satisfy the principle of perfect, uniform, and circular motion. It is also true that Copernicus here does not refer *explicitly* to the 'marvelous symmetry' of the world. But it is not clear to me what exactly is the point of the editors' conclusion(s) reached from numbers (1) and (2) [see 42 above], i.e., that Copernicus' initial motivation was the equant problem and that the justification from the *symmetria*[5] achieved by a heliocentric cosmology was *post hoc* and that, as a consequence, it did not play a motivating role. Motivation to do what? To start working on the problems of Ptolemaic astronomy? To reform astronomy in such a way that it would

[5] Note that Copernicus used Greek in his *De revolutionibus*.

be brought into line with the principle of uniform, circular movement? To reform it along heliocentric and geokinetic lines? Or something else?

It could well be that Copernicus was primarily moved to tackle the reform of Ptolemaic astronomy by 'irregularities' contravening the principle of circular uniform motion. Or by any other 'irregularity' that he might have learned of from the astronomical literature at his disposal. It is completely plausible and reasonable. But if that alone were the case, Copernicus would have stayed within a reformed geocentric system. As Feldhay and Ragep nicely explain, this

> would have secured his fame, earned him the gratitude and admiration of his contemporaries and successors, and spared him and those successors a considerable amount of grief. [7]

Copernicus does not rest with a reformed geocentric cosmos, however. Just a few paragraphs after his complaint about these 'irregularities' and after he lists seven (heliocentric) *petitiones*, he argues for the order of the cosmos on the basis of the so-called distance-period principle [see Goldstein 2002], the same principle that he also uses in his mature *De revolutionibus*, where he claims that in this way a marvelous *symmetria* (or *harmonia*) of the world is achieved. In the heliocentric order of the spheres, Copernicus affirms in the *Commentariolus* that 'one [planet] exceeds another in rapidity of revolution in the same order in which they traverse the larger or smaller perimeters of [their] circles' [Swerdlow 1973, 440]. Saturn makes its period in 30 years; Jupiter, in 12; Mars, in two, while Earth has a one-year period; Venus, nine months; and Mercury, three months. The only difference between the *De revolutionibus* and the *Commentariolus* is that in the latter Copernicus does not explicitly mention *symmetria* (or *harmonia*). But the principle and the results of that principle are already there. Thus, the ordering of the planetary spheres *was*, then, an important motivating consideration already in the *Commentariolus*. So, if the aim of the book is to render the *Commentariolus* understandable, it should not avoid discussing this issue. But, as it stands, this essential feature is left unaddressed.

The question, as I see it, is, therefore, What connects the issue of the principle of perfect motion and, as it was subsequently called, the harmonious order of the planets? Since Copernicus did not arrive at heliocentrism by a technical route, linearly, so to speak, from the equant problem to the problem of the *forma mundi*, there must be some conceptual common denominator of both issues. What exactly is the 'more' from Feldhay and Ragep's claim that there must be 'more to this monumental cosmological shift than a strictly mathematical/astronomical explanation'? Which aspects of his 'intellectual and cultural context...led him to his decision to put the Earth in motion'? [6–7].

2.1. On part 1

While the first two chapters depict some of the matters that could be relevant to Copernicus, they remain on a very general level and are, in my view, of relatively limited use for understanding his *specific* astronomical and cosmological enterprise. Bisaha provides some possible explanations of Copernicus' silence as to his Islamic sources, among which the 'innocent omission at some point in the transmission' seems the most appealing to me. Celenza in his turn does mention Copernicus' study at the Universities of Bologna and Padua but devotes very little attention, almost none, to the curricula there. He does not say anything about the books that Copernicus purchased at the time and there is nothing on the people with whom he may have discussed burning astronomical and astrological questions (the astrological 'crisis') [see Westman 2013, 76–105]. Moreover, there is nothing about Copernicus' learning the Greek language nor about his visit to Rome where he may have had access to Bessarion's library (mentioned by J. Ragep), and so on. In Padua, for instance, Copernicus very probably learned Greek with Nicholas Leonicus Tomaeus, an acquaintance of Callimachus (they met in Venice in 1486), who was very active in Cracow. Tomaeus read Plato in Greek at the University of Padua from 1497 to 1506 and translated a portion of Plato's *Timaeus* 35a–36e along with Proclus' commentary on the same passage. Girolamo Fracastoro, author of the *Homocentrica* (1538), who was in Padua at about the same time as Copernicus, first as a student and then as a teacher of logic, reported that the homocentric revival initiated by Giambattista Della Torre was somehow related to Plato's *Timaeus*. In his dedication to Pope Paul III in the *Homocentrica*, he explains that Della Torre, on his deathbed, told him to recall the circles from the *Timaeus* in the shape of the letter X [Fracastoro 1538, 'Sanctissimo Pavlo Pontifici Maximo']. Fracastoro refers here to *Timaeus* 36b–c, which is included in the part translated by Leonicus Tomaeus.

2.2. On part 2

This neglect of Copernicus' student years is partly amended by Sylla's chapter. She thoroughly discusses three important books of two of the most remarkable teachers of Cracow, both with interests outside astronomy, and sets them in a broader context. Her discussion of the history of physical astronomy in the long period from Ibn al-Haytham through the Middle Ages to Copernicus' years in Cracow and the status of astronomy as a science as debated by *antiqui* and *moderni* as well as in the three texts by Głogów and Brudzewo, is very thorough, interesting, and useful. One becomes aware of many matters previously unknown or known only partially. Among many useful insights, I would point out Brudzewo's understanding of the equant as mathematical (hence, imaginary) and not as physical.

There are several problems, though, which I see in her account. The first two are more general in nature but with important consequences for understanding the *Commentariolus* (and *De revolutionibus*). She limits her discussion to Copernicus' studies in Cracow and makes several remarks that at least imply—if not directly affirm—that those years constitute the decisive background to his *Commentariolus*. What about his subsequent studies in Bologna and Padua? Did they not contribute anything to the genesis of the *Commentariolus*? And what did Copernicus do after he returned to Warmia but before he wrote the *Commentariolus*?

Sylla also directs her attention only to the Aristotelian tradition and completely ignores the humanist and Platonist current(s) of Cracow's intellectual life. This is strange since there is plenty of evidence thereof. Filippo Buonaccorsi, called Callimachus Experiens (1437–1496), as already mentioned, was very active in Cracow. He corresponded with the Platonist and translator of Plato's *Opera omnia*, Marsilio Ficino (1433–1499), who called Callimachus 'my fellow Platonist'. Callimachus was constantly traveling from Cracow to Italy and Constantinople. In 1485, one of Cracow University's reading rooms was called Plato's and Albert of Brudzewo was mentioned in that connection. Even John of Glogów, who appears to have mostly drawn on the Aristotelian tradition, was well versed in other philosophical schools of thought, including Plato and Platonism. In his manuscript *In metaphysicam* (or *Quaestiones super duodecim libros metaphysicae Aristotelis*), to give just one example, he mentions Plato approvingly several times. While in Cracow, Copernicus was also closely connected with Laurentius Raabe

Corvinus, another Platonist, one of the most important members of the Cracow's humanist Vistulan Literary Sodality. After Copernicus' return from Italy, Corvinus helped him publish his Latin translation of Theophylactus Symocatta's Greek *Epistolae morales, rurales et amatoriae*.

There is no doubt in my mind that Copernicus (and those of his contemporaries who read it) understood *Commentariolus* as a *theorica*. It is a theoretical astronomy, using physical astronomy (the three-orb compromise) and partly mathematical astronomy. It also fits quite nicely into the practice of some *theoricas* by establishing some physical principles on which the subsequent astronomy is based. According to Sylla, Copernicus mirrors these physical principles with his *petitiones*; namely, Copernicus claims that he could solve the problem 'if some postulates, called axioms (*petitiones quas axiomata vocant*) are granted to us' [Swerdlow 1973, 436]. Sylla calls these *petitiones* hypotheses or principles, puts them on a par with scholastic suppositions, principles, or premises, and claims that they are 'derived from experience' [49]. She also claims that in the *Commentariolus* these principles are stated postulates (*petitiones*), while in Peurbach's *Theoricae novae* they are the *theoricae* (figures) themselves.

Despite some similarity between the *Commentariolus* and Brudzewo's *Commentary on Theoricae novae* in the matter of the physical principles, I believe that an epistemological distinction is in order. Copernicus' postulates or axioms are neither derived from experience nor have exactly the same epistemological status as suppositions, principles, or premises. How, for instance, can the fifth postulate—

> Whatever motion appears in the sphere of the fixed stars belongs not to it but to the earth. Thus the entire earth along with the nearby elements rotates with a daily motion on its fixed poles while the sphere of the fixed stars remains immovable and the outermost heaven. [Swerdlow 1973, 463]

—be derived from experience? And if it were—let us allow this for the sake of the argument—from which experiences or observations exactly? There are approximately 70 documented observations by Copernicus, and he occasionally does refer to observations and measurements of the positions of the stars from which ancient philosophers worked out their planetary theory. But I am not aware of any instance when he did so in reference to himself. As noted by Shank, Copernicus was 'following Regiomontanus in *not* undertaking to derive his astronomical models themselves from observations' [108].

It would be very useful to make a list of all of his observations and analyze them to determine what precise purposes he had in using them.

I also do not understand how the statement of principles in the *Commentariolus* can mirror—this time, specifically—that in Peuerbach's *Theoricae novae*. Why would Brudzewo need to 'establish' principles in his commentary, as Sylla claims he did [53], if they were already established by Peurbach himself (figures/*theoricae*)?

I believe that the key to the secret of Copernicus' axioms or postulates is to be found elsewhere and that it is Copernicus himself who reveals where. In one of his annotations to Plato's *Parmenides* in Ficino's translation, he writes 'what needs to be known about hypotheses (*quid aduerti oporteat circa hypotheses*)'. Copernicus obviously understood hypotheses, axioms, and postulates in Platonist terms. This is further confirmed when we compare the *Commentariolus* and Proclus' *Commentary on Plato's Timaeus* 2.3. In this passage, Proclus explains that Plato is not an empiricist: Plato does not start with experiences and then draw conclusions. Plato's method (μέθοδοϲ) is hypothetical or, rather, Plato uses the method of hypothesis. He sets out fundamental axioms (ἀξιώματα) and hypotheses (ὑποθέϲειϲ) and draws conclusions. Proclus first presents a list of five axioms and then follows with another list of seven. Describing Plato's 'hypothetical method', Proclus does not refer to Plato's own description of the hypothetical method but explicitly refers to the method used by geometers. They first postulate, define, and name their key principles before proceeding to their demonstrations. And he cites an example from Euclid. On the basis of fundamental principles or hypotheses, Plato's *Timaeus* then proceeds, in Proclus' reading of the text, to a number of demonstrations (ἀποδέξειϲ) required in order to solve the problems. Copernicus' method in the *Commentariolus* is highly reminiscent of Proclus: he first establishes seven *petitiones quas axiomata vocant* and then promises to provide mathematical *demonstrationes* in a larger book.

I find Shank's chapter to be one of the highlights of *Before Copernicus*. In a very well written, exciting exposition, Shank depicts the interrelatedness of seemingly unrelated issues—astronomical (the controversy regarding Ptolemy's *Almagest*), religious, and political (the Crusades, Orthodox/Roman Catholic Christianity)—that played a part in the life and work of Regiomontanus, the most advanced astronomer before Copernicus. From his exposition, it is abundantly clear that Copernicus was working not in a void but in a period of vigorous institutional development in astronomy that was to a large extent due to Regiomontanus' work and his printing activities, themselves in turn the result of long and multifaceted dispute. The main characteristic of Regiomontanus' work is its search for a philosophically (i.e., physically) adequate astronomy. He also makes it clear why Regiomontanus was justly considered the most advanced astronomer in the second half of the 15th century as well as to what extent and regarding what particular details Copernicus relied on and used his work.

I have only one remark here. Shank complains that while intellectual historians are familiar with George of Trebizond's attacks on Plato and Cardinal Bessarion's defense of the latter, 'the astronomical and astrological dimensions of that conflict are poorly integrated into the history of astronomy' [87]. As are, I would like to add, the philosophical dimensions. What do I mean? Copernicus bought and annotated a book by Cardinal Bessarion, *In calumniatorem Platonis*, in which he read praise of Plato as a mathematician. In book 4, chapter 12, for example, Bessarion defends Plato against the accusation that mathematics was to be taught to those who wanted to become divine. He declares that, according to Plato, mathematics was truly the subject most worthy of study by a free man and continues, paraphrasing the *Epinomis*, that the easiest way to ascend to the divine was through mathematics. He concludes the chapter by referring the reader to books 7 and 10 of the *Laws*, to the *Epinomis*, as well as to books 5, 6, and 7 of the *Republic*.

This is relevant to the question addressed by Chen-Morris and Feldhay: How did Copernicus end up going 'beyond the appearances'? While this is the right question, however, their answer, I am afraid, is not correct. I share with them numerous epistemological conclusions about Copernicus' work. I strongly agree that Swerdlow's reconstruction of Copernicus' path to heliocentrism is not satisfactory and I also agree that we should ponder the question of the relationship of appearances to their 'beyond'. In this context, Copernicus' astronomy questions the role of vision in the cognitive process leading to knowledge, which has special relevance to the epistemological status of astronomy. The very essence of Copernicus' argument is to limit vision and surpass it. Copernicus transcends visual experience and establishes a new point of view, whence a new picture of the universe is revealed. But I fail to see how any connection between these insights with Alberti's artificial perspective and Cusanus' theological speculations can be established.

It is Plato who demanded, specifically in reference to study of the heavenly motions, that astronomy should go beyond the visible motions of the corporeal universe. Plato makes this demand in the *Timaeus* and he is especially clear about it in the *Republic* 7.528e–530c. There, he instructs that astronomy must be learned differently from the way in which it is learned at present. We should consider the ornaments in the heavens as the best and most exact visible things. But we should at the same time admit that these motions fall short of the true ones:

> those motions which the real speed and the real slowness in [their] true numbers and in all [their] true figures move relatively to each other and carry along whatever is in them, these things are for reason and understanding, not for sight, to discern. [Vlastos 1980, 2]

The decorations in the heavens are just models, an excellent starting point to discover the real movements of the stars, but not by any means their real motions. It is just as if someone came upon some thoroughly well-drawn and perfected diagrams of some skilled craftsman or artist, such as Daedalus. He or she would consider them beautifully crafted but would 'think it laughable to scrutinize them zealously, expecting to find in them true equality or duplicity or any other relation of *symmetria*' [*Resp*. 529e–530a: Vlastos 1980, 3 lightly modified]. The True Astronomer would feel the same when looking at the motions of the stars. He would find the tracings beautiful but it would be absurd for him to seek to obtain the truth 'of the relation of [the] *symmetria* of night to day, of these to months, and of the [periods of the other] stars to these and to one another from the visible appearances' [Vlastos 1980, 3 lightly modified]. According to Alexander Mourelatos, the Real Astronomer 'does not dismiss questions concerning the *symmetria* of celestial periods' [1980, 39]. On the contrary, Plato demands that the True or Real Astronomer discovers the true *symmetriai*—that is, the commensurable proportions—of celestial periods, which exist beyond visible motions; the Real Astronomer 'realizes that the *aletheia* concerning these *symmetriai* cannot be elicited from the observed periods of the celestial bodies' [Mourelatos 1980, 39].

2.3. On part 3

I find S. Ragep's chapter very informative and well documented. The extent of mathematical scholarship and the technical innovations of Samarqand and the other astronomers that she depicts is impressive. I also like her more general warning about the 'danger of putting forth explanations based on the heroic individual scientist in search of knowledge' [156]. The same goes for Morrison's chapter. I think that it shows convincingly the possible passages of Islamic astronomy through Jewish scholars. J. Ragep's chapter, another highlight of the book, clearly explains the concept and development of the Ṭūsī-couple and discusses channels through which it could have been brought, together with other Islamic materials, to Latin Europe and to Copernicus. Given all the evidence of transmission, I think it safe to agree with J. Ragep that independent rediscovery of all these materials, especially many times, is much less compelling.

All I should like to add regarding the third part of the book are some other possibilities for the transmission of Islamic astronomy to the Latin West. First, it seems to me that Bessarion's legacy, which includes his own books as well as the books and manuscripts of his library, deserves fuller and much more thorough research. I have already mentioned his *In calamniatorem Platonis* and its impact on Copernicus; but the books included in his library, those mentioned by Shank and cited above (by Proclus, Theon of Alexandria, and Theon of Smyrna) as well as possibly many others, should be read with renewed interest. The same goes for the manuscripts that he brought with him. Next, Callimachus was constantly traveling from Cracow to Constantinople and Italy (Venice, Rome, Padua, and Florence). Could he not have brought some materials? Finally, while in Padua, Copernicus lived in the house of Girolamo Della Torre. Della Torre was subsequently praised by Girolamo Fracastoro in his *Homocentrica* (published in 1538 in Venice) as his inspiration for the revival of homocentric astronomy. Fracastoro, as I mentioned earlier, was in Padua at about the same time as Copernicus and mentions the Ṭūsī-couple in his book. He studied literature, mathematics, astronomy, and philosophy (the latter under the guidance of Pietro Pomponazzi and Nicholas Leonicus Tomaeus), and received his doctorate *in artibus* on 2 November 1502. One of his promoters at the conferment ceremony was Gabriele Zerbi (1435–1505), a professor of theoretical medicine and a humanist who discovered several medieval scientific manuscripts and had contacts with the Ottoman Empire. This is, I believe, another possible route deserving of further study.

My closing remark on the topics of transmission: given that the astronomical models in the *Commentariolus* and *De revolutionibus* differ rather significantly, it would be good to examine whether Copernicus worked on the basis of one manuscript, one set of manuscripts, or many manuscript or sets of manuscripts. Did he obtain any new material after the *Commentariolus*, and if yes, how?

G.C
Conclusion

Feldhay and Ragep claim in the introduction that Copernicus' system is a result of many practices

> that included attempts to deal, mathematically, with violations of physics found in Ptolemy's models, discussions of the relation between natural philosophy and mathematics, and epistemological forays into the 'true' cosmology and the human capacity to discover it. [8]

They likewise believe that 15th-century astronomy was

> the outcome of multiple transformations along different paths that crystallized in the work of Copernicus into some kind of coherent whole that differed enough from the preceding astronomical discourse to open the door to additional, enhanced transformations. [8]

I could not agree more. The question is, however, whether *Before Copernicus* covers the *essential* 'transformations' that led to Copernicus' work and whether they are treated adequately such that they explain his work as 'some kind of coherent whole'. It is clear from the reservations and critical comments stated above that I do not believe that is the case. In particular, the issue of the aspects of Copernicus' intellectual and cultural context that led to his decision to put the Earth in motion is, for the reasons given above, not treated adequately.

According to the editors [8–10], three kinds of transformation lie in the background to the Copernican system:

(1) transformations in the body of knowledge;
(2) transformations related to the image and status of astronomy (the older order of the disciplines being more or less accepted in both Islamic and Christian environments for centuries); and
(3) transformations in the paths of the transmission of knowledge, in its carriers and their identities.

In what follows I will use their scheme as a point of departure and suggest some changes that, according to my research, are more appropriate to Copernicus' work.

The first category of transformation concerns the body of knowledge and is subdivided into three subcategories:

(a) the transformation of Ptolemaic two-dimensional circles into physical, three-dimensional orbs, as proposed by many scholars;
(b) new types of models, i.e.,
    (i) the transition from the epicyclic models for the second anomaly of the inferior planets to their eccentric models (ᶜAlī Qushjī and Regiomontanus), and
    (ii) the Ṭūsī-couple and the construction of non-Ptolemaic models;
(c) conceptual transformations related to a moving Earth, 'new ways of seeing'.

I think that it can be affirmed without any reasonable doubt that Copernicus' work was a crystallization of the long period of transforming mathematical models into physical ones, and of many transformations within the astronomical models themselves, i.e., the inventions of new types. As is clear from my previous comments, I also agree that something 'more' than just a technical/mathematical explanation is needed for Copernicus' affirmation of the invisible motions of the Earth. But this one should be linked not with Alberti's artificial perspective or Cusanus' speculative mathematics but with Plato and a Platonist understanding of astronomy.

This brings us to the transformations within the second category, that of the image and status of astronomy, that is, its place in the order of disciplines:

(a) the transformations of Ptolemy's two-dimensional mathematical circles into a three-dimensional physical astronomy resulted in a discussion about whether astronomy was to be understood as a mathematical science, a physical science, or both;
(b) New categories for classifying the nature of astronomy—theoretical but non-demonstrative astronomy *versus* demonstrative theoretical astronomy—thus emerged and enhanced reflection about the epistemic status of its procedures and conclusions.

> The epistemic status of astronomy was questioned once the empirical-observational origins of astronomy's 'first principles' [was] addressed following the 'physicalization' of astronomy by Islamic astronomers. [9]

In the long 15th century there were, of course, discussions about the mathematical *versus* physical nature of astronomy, and the 'physicalization' of astronomy did indeed lead to epistemological reflections on its status and procedures. But these, I would argue, were far from decisive for Copernicus. Copernicus' heliocentric choice did depend on a 'new way of seeing', on looking at the celestial appearances 'with both eyes', the corporeal eye and the mind's eye. Yet this was a result of the conceptual change in the status and abilities of astronomy and not *vice versa*. This change also had little to do with the 'physicalization' of Ptolemy's mathematical models. The transformation of a mathematical model of a certain planet into a physical *theorica* had nothing to do with the arrangement of the planets. The order of the planets was strictly speaking not an astronomical problem. One was able to predict the positions of heavenly bodies in geocentric and Copernicus' heliocentric cosmos equally well. The order of the planets was an astrological and natural-philosophical problem, a problem within philosophy especially for Plato and the Platonists. The Platonist understanding of the status of astronomy and its goals was radically different from that in the Aristotelian traditions.

And finally, the last category of transformations in the paths of the transmission of this knowledge:

(a) Basilios Bessarion (the new translation of Ptolemy's *Almagest* from Greek to Latin, the *Epitome of the Almagest*, his library);
(b) Jews expelled from the Iberian Peninsula who resettled in the eastern Mediterranean and traveled to Istanbul or Italy;
(c) the diffusion of the *Configuration of the World* and the tradition based on it in medieval Europe; and
(d) the circulation of knowledge within informal, intellectual-artistic circles that associated around a site of knowledge (Bessarion's library in Rome).

There were many possible paths for the transmission of knowledge from the Islamic world to Latin Europe. I have added some new possibilities. But we also should not forget other transmissions of knowledge: those, namely, that were a result of the renewed humanist interest in Plato and Platonism as reflected in the Latin translation and diffusion of Plato's *Opera omnia* as well as the works of different Platonist and commentators on Plato (including doxographers), in readings of his work in the original Greek, and so on. One can find much of this already in Bessarion's library.

Let me conclude on a positive note. Despite my reservations and critical remarks, I certainly would have benefited from having *Before Copernicus* at my disposal before writing my own book on Copernicus.

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Metaphorical Coherence: Studies in Seneca's Epistulae Morales by Aron Sjöblad

Studia Graeca et Latina Lundensia 20. Lund: Center for Languages and Literature, Lund University, 2015. Pp. 82. ISBN 978–91–637–9425–4. Paper 169.00 kr

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Aron Sjöblad has previously published a study of Cicero's *On Old Age* in which he drew on the work of Lakoff and Johnson [1980] on metaphor and, particularly, on the idea of conceptual metaphor, to study the system of metaphors in that short work.[1] Here he extends his approach to Seneca's *Moral Epistles*. In the introduction, he reviews previous work on the role of metaphor and simile in Seneca's philosophical prose. Early studies focused on classifying the source-domains of Seneca's metaphors and often treated metaphor as little more than literary embellishment. But more recent contributions by Armisen-Marchetti [1989],[2] Bartsch [2009], Edwards [2009], Richardson-Hay [2009], and Watson and Watson [2009] all treat imagery as integral to Seneca's philosophy and several of them show in different ways how imagery from various source-domains can all contribute to an understanding of the same target-domain. Sjöblad aims to take this approach further and to show how there are 'master metaphors' that integrate groups of metaphors that previous scholars have treated as separate.

Chapter 1, 'The Metaphorical Connection between Body and Soul in the *Epistulae*' [23–41], shows that much of the language used to describe the mind can also be used to describe the body and goes on to argue that metaphors of health and disease, of travel, of warfare, and of athletic or gladiatorial competition can all be subsumed under the overarching body-soul metaphor. The conclusion is that metaphors previously treated as distinct must be interpreted in relation to each other.

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1 Sjöblad 2009, reviewed in Lavan 2010 and McConnell 2011.

2 Sjöblad consistently misspells 'Armisen-Marchetti' with a double 's'.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 80–84

Chapter 2, 'Seneca's Fortress of the Soul and Related Metaphors' [43–59], argues that the 'enclosed space' of the human soul is another master metaphor. As Sjöblad acknowledges, Armisen-Marchetti and Bartsch have already recognized the importance of the metaphor of 'inner space'. But, he claims, they have not realized its full extent, for it subsumes not only the metaphors of the defensive wall and the besieged city but also references to all the various inner and outer threats that we face (from Fortune, other people, luxury and other temptations, our own passions, and so on), as well as the metaphors of trade and money, the stage, and slavery.

Chapter 3, 'The Relation between the *iter ad sapientiam* and the *iter vitae* Metaphors in the *Epistulae*' [61–74], argues that the metaphors of the journey to wisdom and the journey of life have to be seen as distinct—they have different target-domains, namely, philosophical progress and life itself—but also as closely intertwined, in what Fauconnier [1997] calls a 'blend'. In the life of the Stoic, the two ways should coincide—failure to recognize this, Sjöblad reasonably argues, led Lavery [1980] to see a conflict between Seneca's doctrine of suicide and 'the road of life' metaphor.

> What is new in the blend is that wisdom and death are identified with each other.... The similarity between the two metaphors makes it inevitable for the reader to identify wisdom and death. [72]

The general conclusion [75–77] recaps the view that these complexes of metaphors need to be taken together:

> It is obvious that Seneca, rather than conveying the superficial messages inherent in the single metaphors, intends to create an *attitude* in Lucilius and in the readers of the *Letters* by describing the enclosed space of the ideal Stoic's soul in so many different ways...

and the metaphors

> add nuances and complexity to the general philosophical statements that Seneca makes. [76]

The acknowledgements [3] say that what was originally intended as an article has been developed into a book. But it still reads rather like a long article, and one wishes that Sjöblad had sometimes looked beyond the *Letters*—for instance, in discussing how the behavior of the body can reveal the state of the soul [25–27], there is no reference to the treatment of the physiology of anger in *On Anger*—and that at various points he had developed his views in more detail. The conclusion to chapter 1 merely asserts that all the body-soul metaphors that he has described 'must be interpreted in relation to each other' and that they 'influence each other and the separate examples must be interpreted in the light of the larger whole' [41]. But one would welcome some close reading to show how this interpretation 'in the light of the larger whole' actually works and how it deepens our understanding of Seneca's thinking, since the body of the chapter contains illuminating comments on a number of individual metaphorical passages but essentially still handles the metaphors separately from each other. The conclusion to chapter 2 makes a similar claim, i.e., that it is 'necessary to interpret them [the whole range of "inner space" metaphors] together. They influence each other' [59]. But again there is no close reading to show how this works. In the absence of such detailed demonstration, the master metaphors of body-soul and inner space seem rather abstract and, in the latter case particularly, the constituent metaphors identified by Sjöblad seem a very loosely-knit group.

There is a further claim, that

> the main purpose of this imagery [of inner space] might be this: to form an attitude in the reader and to add complexity and depth to the general Stoic idea of independence from the outer world.[3] [59]

That is an unexceptionable claim. But surely one can accept it without needing to accept the master metaphor as an additional level above the range of individual metaphors that are surveyed. Indeed, the final paragraph of the book [76–77] seems to bring us back to the importance of the variety and individuality of Seneca's metaphors. As the concluding sentence puts it:

> Because the metaphors and similes with related themes are so many, they acquire a nuance of trial and error; they are attempts to describe how one might approach—with very small and tentative steps—the idea and the ideal of the perfect Stoic sage. [77]

This is an intriguing suggestion that one would like to see developed further. But again, can one not accept this suggestion without seeing the need to invoke master metaphors?

Chapter 3 operates rather differently. The comparison between the two 'ways' is helpful. But the final conclusion—that they form a 'blend' which indicates that for Seneca wisdom and death are identified with each other

[3] The general conclusion makes a similar claim: see the quotation from p. 76 above.

(as being the goals of the two 'blended' metaphors)—is reached with such startling rapidity that this reviewer is left unconvinced. The argument seems to be that, because the metaphors of 'the way of life' and 'the way towards wisdom' are similar in their overall structure and in some of their details, their destinations must be identical—which is hardly compelling. Nor are doubts lessened by the comment that

> Seneca's recurring insistence on life and the human body as a prison for the soul is another reason why this identification lies close at hand, [72]

with citation of *Ep*. 65.16, since this passage describes the body as a drag on the soul but says nothing about death. And Sjöblad's appeal to Seneca's frequent references to the younger Cato and his suicide [73–74] do not really help either. In fact, in the general conclusion, Sjöblad seems to tone down his previous conclusion: 'Death and wisdom are identified with each other, *or almost so*...' [76, emphasis added].

The discussion of *Ep*. 65.16 is one example of how Sjöblad's book could have been strengthened by widening the range of the bibliography. This bibliography is brief and mostly confined to discussions that expressly address either Seneca's imagery or his *Letters*; hence, some significant contributions to the topic of Senecan metaphor are overlooked.[4] Thus, in the discussion of *Ep*. 65.16 and throughout chapter 1 on the body-soul metaphor, Sjöblad makes no mention of the ongoing debate about whether Seneca inclined to a Platonizing dualism of body and soul or remained true to Stoic monism, or of how Seneca's metaphorical language about body and soul should be handled in the context of that debate.[5]

In short, there are some illuminating observations in this book but the reviewer is not persuaded by its central thesis that our reading of Senecan metaphor is enhanced by the recognition of master metaphors.

[4] The survey of Seneca's metaphors and images in Armisen-Marchetti 2015, which itself presumably appeared too late for Sjöblad himself to use it, refers to several earlier works not in his bibliography.

[5] On the significance of imagery for the debate, see Inwood 2005, 31–38 and *passim*; Ker 2009, 176–182.

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Divination and Human Nature: A Cognitive History of Intuition in Classical Antiquity by Peter T. Struck

Princeton: Princeton University Press, 2016. Pp. 304, 2 tables, 3 ill. ISBN 978–0–691–16939–2. Cloth USD $45.00, £34.95

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Essentially, the author's argument in this book is that insight gained through divinatory means in the ancient world was not only recognized as genuine knowledge by ancient philosophers in their attempts to account for it, but was also in fact a real form of knowledge, equivalent to what modern English speakers would call intuition.

Following an introduction that sets the relevant terms and provides a very brief history of the concept of intuition [22–33], Struck devotes the four chapters that follow to an examination of key passages from texts of four major Greek philosophers (or schools of philosophy) who attempt to describe or explain cognitive aspects of divination—namely, Plato, Aristotle, Posidonius and the Stoics, and the Neoplatonists Iamblichus and Porphyry. The book's conclusion offers a sketch of how the author's understanding of ancient divination can serve to elucidate non-philosophical texts, especially the concluding scenes of the *Odyssey*, which center on the allegedly intuitive process whereby Penelope comes to recognize her disguised husband and the divine signs that Homer depicts as accompanying that process.

This thoroughly readable, thought-provoking study is admirable for the author's willingness to take ancient divination and intuition seriously as authentic ways of knowing, or, as Struck puts it axiomatically, '*Our ability to know exceeds our capacity to understand that ability*' [15; emphasis in original]. Struck's readings of individual philosophical texts and terminology and his elaborations on the relevant divinatory contexts prove difficult to fault.

A key problem lies in Struck's eagerness to affix the label 'intuition' to the cognitive processes involved in divination as he has described them. The study, in fact, would have been just as valuable without his assigning a key role to intuition—if, say, he had opted instead for the phrase 'divinatory

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 85–92

insight' to characterize the relevant forms of cognition. The discussion below hopefully will suffice to justify this criticism.

Aside from the brief historical survey of intuition offered in the introduction, Struck's study offers little exploration into modern understandings—scientific or otherwise—and examples of intuition. He takes it for granted that intuition is real, that the reader knows generally what it is, and that it is a socially acceptable way to characterize 'surplus knowledge'—that is,

> the quantum of knowledge that does not arrive via the discursive thought processes of which we are aware, and over which we have self-conscious control. [15]

Again, the idea here is that divination formed the main cultural mechanism by which the Greeks and Romans tried to understand, regulate, and use such knowledge.

Despite the absence of a detailed account of intuition, Struck does characterize the phenomenon loosely throughout the book as follows:

> When we find ourselves in the position of knowing things, but we cannot develop a clear account of how we know them. [15, 16]
>
> Knowing without self-conscious inference. [20–21]
>
> Being able to see around corners, or see through things, in ways that defy appeal to the customary modes of our intellects. [24]
>
> [Knowing things] all in a flash, without recourse to sequential reasoning and inference. [26–27]
>
> Hunches, gut-feelings, right twitches, automatic and reflexive intellectual activities [29–32].
>
> [Ways of knowing that are] other than self-conscious, goal-directed, inferential chains of thought. [31]
>
> Momentary, non-discursive apprehension of things by processes that fall outside our self-conscious control. [33]
>
> [Knowledge marked especially for being] nondiscursive…[that] arrives unexpectedly or involuntarily, and stretches beyond our ability to account for it. [42]
>
> [The] strange abilities of some people to gain incremental knowledge of things via instinctual, nondiscursive insights, extracted from the natural world. [246]

> Just knowing something quotidian or mundane without really thinking about it. [247]
>
> Uncanny and unexplained insight regarding proper courses of action in the proximate future. [250]
>
> [An] ancillary form of cognition that takes place outside our self-conscious, purposive thinking. [251]
>
> [Divination as understood in Greece:] exactly the way a person would talk about moments of knowing that creep in and then crystallize, the kind of knowing still familiar to us, and not at all reducible either to superstition or social trickery and gamesmanship. [262]

This last statement is remarkably unequivocal. Struck is saying that the cognitive aspects involved in ancient divination were nothing other than the phenomenon that we call intuition. It is this claim that the present review seeks to refute. But first, it will be useful to paraphrase and elaborate Struck's characterization of intuition.

Intuition, then, on these terms, can be described as an involuntary prescient feeling that occurs immediately, thus requiring no time to ponder, analyze, or understand. The subject can neither control nor account for the relevant insight. Consequently, it should be noted that knowledge gained *via* intuition will not involve an intermediary or a specific identifiable source. It is accessed directly by the subject. Nor will the intuitive subject be able to offer a reason, justification, or proof for his or her belief. If asked, 'How on earth did you know that?' the subject, unable to provide a specific answer, instead would respond along the lines of 'I have no clue. I just knew it'. Let's call this the 'non-accountability criterion' for intuition.

So, how well do Struck's chief examples of divinatory intuition follow this characterization? First, generally speaking, it should be noted that while knowledge gained by divination may not be the result of self-conscious, purposeful reasoning, it does result, however, from self-conscious, purposeful attempts to arrive at such knowledge *via* divinatory practice. In other words, the *mantis* sets out and expects to attain divinatory knowledge, say, by reading entrails or following flights of birds. This is not the case with intuition, which involves no such purposeful process culminating in specific knowledge and is itself characterized as involuntary and unexpected. One does not simply initiate or go in search of intuitive knowledge as one does divinatory insight.

One specific example that Struck discusses from Plato's works also falls prey to the general criticism just stated. Plato, he argues, metaphorically depicts the apprehension of the highest truths—i.e., the forms—as divinatory insight, as opposed to sequential inferences from observations of the unstable material world around us [55–67]. Again, the knowledge involved here can be nothing other than the end result of a highly-qualified intellectual's voluntary process of learning and intentional quest for higher truths. Plato never suggests that such insight can just happen to anyone at any time without prior educational preparation of the required sort.

Another divinatory sort of event in Plato's works that fails to qualify as intuition, contrary to Struck, is Socrates' special 'divine sign', which appears unexpectedly to prevent him from doing something wrong [68–71]. Struck, comparing Socrates' sign to a 'twitch' or 'involuntary movement', allows that 'Socrates casts his divine sign as a form of knowing that just arrives to him, which is not *explainable*, and...nevertheless turns out to be accurate' [69; emphasis added].

Here the problem is not the unexpectedness of the insight that Socrates gains through the sign, which will be granted as in alignment with intuition, but the fact that a sign is involved in the first place. In other words, Socrates identifies his sign as a source of knowledge separate from himself—an intermediary and reason for why he adopts one course of action rather than another. If asked, 'Socrates, why do you refuse to defend yourself at your trial?' he would have to (and did) respond, 'Because my divine sign warned me not to do so'. Contrary to Struck's claim, Socrates' course of action *is explainable* precisely in terms of this sign and, for this reason, is markedly unlike intuition.

A similar criticism can be raised against Struck's characterization of recollection in the *Meno* as an instance of intuition [64–65]. True, recollection there is unexpected and non-discursive just like intuitive insight; but it fails the non-accountability criterion of intuition because subjects who know something that they remember to be true must also know *how* they know that thing—namely, because they had learned it once before.

Finally, Struck also neglects to notice that Plato's description of divination in Timaeus' account of the construction of the human body also fails the non-accountability criterion [73, 80–84]. The appetitive soul, located in the belly and having viewed images that the rational soul had sent from the head to the liver (itself, described as a sort of screen or mirror), would find itself not in the position of *just knowing* how best to behave, but rather of knowing how it should behave precisely *on the grounds* that it had witnessed certain images appearing upon the liver suggestive of the relevant course of action. In other words, if asked, the appetitive soul would say, 'I know it's best not to drink tonight because the liver showed me some scary images that convinced me not to do so'.

Again, intuitive people do not attempt to justify their beliefs in terms of visions or messages (however clear) transmitted to them from some distinct source *via* some intermediary. Sure, visionaries and prophets claim to know what they do on the grounds of having seen images, presumably on some medium or another, but intuitive people do not—they just know what they know. The point can be raised also against Struck's claim that, due to the inscrutability of divinatory signs, 'the reading of livers on the battlefield is closer to a gut-check…than to a calculation' [18]. Still, this is not close enough to intuition, which does not require the use of media such as livers, birds, atmospheric events, and other such signs.

Failure to meet the non-accountability criterion also explains why the examples of divinatory insight that Struck discusses as proceeding through dreams and oracles do not qualify as intuition. For instance, in the case of Aristotle on prescient dreams, it is true, as Struck notes, that we lack self-conscious control over our dream states [110]. But this is not enough to qualify them as intuition, as no one who knows that something will happen to them in the future because they dreamed it can honestly say that they do not know why they believe that thing will happen to them.

Similarly, in his discussion of the Neoplatonists and their turn from a notion of divination concerned with mundane, practical matters to one that reveals deep, underlying ontological and theological truths about the universe [217], Struck considers as predecessors Cicero's *Dream of Scipio* and Vergil's account of Aeneas' underworld journey in book 6 of the *Aeneid*. Struck argues that both

> articulate an otherworldly journey specifically as enabled by divination, that allows the main figures to gather knowledge on a massive scope, about the deep structure of the cosmos, eschatology, the general fate of souls, and universal human history. [219–220]

Once again it should be noted that both dream-visions and oracular pronouncements or directives serve as the subjects' reason for forming their respective beliefs and acting on them. The insight gained through them does not, therefore, qualify as immediate apprehension. In fact, even Struck describes the Sibyl in Vergil's account as 'critical intermediary' of the enlightenment Aeneas attains [220]. The moment a subject identifies a legitimate reason for an insight—say, in the form of some oracular message, however hazily or clearly delivered—at that moment, the insight cannot be intuitive and becomes something else, in this case, knowledge through inspired authority. The authority itself may have gained its knowledge *via* intuition, but the subject forming the belief on the basis of the oracle's message certainly did not.

With the Stoics, Struck notes that their view of determinism affords a predictive role not only to exceptional physical events around us but also to dreams and oracles [189]. Posidonius thus considered both types of divine sign as legitimate and reliable—the latter traditionally viewed as belonging to 'natural' divination, while the former was seen as part of 'technical' divination, which proceeds by observation, sign-interpretation, and inferential logic [16].

The key here is that Posidonius viewed both sorts of divination as non-discursive attempts to predict outcomes that have causes that lie outside our knowledge. They are, Struck argues, 'an extension of our cognition into a realm that is otherwise beyond us', and thus line up 'without remainder' with his definition of divination and surplus knowledge [200]. One should note, however, that while cognitive experiences extending 'into a realm otherwise beyond us' describe one aspect of intuition, alone they are hardly enough to qualify as such.

Iamblichus, too, Struck argues, is willing to consider traditional forms of foresight from external signs (both natural and artificial) as equivalent in reliability to those produced *via* scientific observations of nature. Unlike Posidonius, however, Iamblichus considers those forms to be inferior to what he regards as 'true' divination, which involves direct contact with the divine, amounting to no more than conjecture and guesswork [216–217, 236–237, 242–243]. Suffice it to say, a deprecating view of the reliability of traditional forms of divination does nothing to bring these forms closer to the notion of intuition.

In his concluding discussion of the end of the *Odyssey*, Struck seeks to establish the presence of an 'enigmatic knowingness on Penelope's part', adding that her hunches about the beggar's identity are 'externalized' through her reported observations of divinatory events [260]. Here Struck assembles an impressive list of the relevant divine signs and their significance for Penelope's suspicions: e.g., flights of birds, an oracle, a sneeze, dreams, thunder. None of these, however, is necessary for intuitive insight to occur; their presence, in fact, even precludes Penelope's cognitive state of recognition from qualifying as such. Intuitions do not rely on externalizations.

A case in point. Struck responds to the scene in which the suitors ignore Theoclymenus' 'visceral vision' of them all dying as follows: 'By the terms of this study, this would be an expression equivalent to calling out a lack of intuitive insight among them' [261]. But this cannot be right. Theoclymenus' own vision may qualify as intuition, but refusing to believe an intuitive authority does not qualify the doubter himself as unintuitive.

Struck concludes:

> The idea that divination is an expression of a kind of knowing that we would call intuition helps us better understand the richness of Homer's work in the closing books of the *Odyssey*. [261–262]

The present reviewer respectfully disagrees. But to reiterate: this is not to deny that Struck's discussions are useful and relevant as detailed, contextual readings of ancient philosophical attempts to understand the sort of cognition involved in divination. It is only to say that the effort to label that form of cognition as something sufficiently different from it, and perhaps even foreign to the relevant contexts, seems forced and unnecessary.

In sum, consider the following scenario. Suppose that my friend Freddie and I go to a restaurant and that I suddenly get a bad idea about the fish, thus warning Freddie not to order it, but he orders and eats it anyway and then gets sick. Upon being asked how I knew that the fish was bad, I might respond, 'Ha! Joke's on you...I put something in it', or 'I saw the chef poison it', or 'I tried the fish here last week and got sick', or 'It didn't look the right sort of color and texture'. In the first three responses, I know exactly how I knew that the fish would be bad based on direct experience. In the fourth, I used my inferential abilities to reason, discursively, about the quality of the fish. None of this, of course, qualifies as intuitive insight.

Moreover, let us say that I were to respond, 'Well, Freddie, I had a strange dream last night in which the human race was annihilated by a master race of plague-bearing fish-creatures', or 'Well, Freddie, I admit last night I consulted a psychic who, in some bizarre trance, kept murmuring "Beware the fish, beware the fish, beware the fish"', or 'Well, Freddie, last night in a drunken stupor I unexpectedly saw the image of a fish appear on my bathroom wall right before I got sick'. In none of these cases would it be reasonable for Freddie to reply, 'Wow, how intuitive of you!' Sure, he might respond, 'How prescient of your dreams!' or 'How intuitive of your psychic!' or 'So you saw a fish appear on your wall last night, did you?' But it would not be normal in such contexts for Freddie to compliment my own capacity for intuition. The only response that would legitimately elicit such praise would be, 'I don't know how I knew the fish would be bad, Freddie. I just did.'

La rivoluzione culturale di Lucrezio. Filosofia e scienza nell'antica Roma by Marco Beretta

Rome: Carocci, 2015. Pp. 311+ ill. ISBN 978–88–430–7945–2. Paper €32.00

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The last decades have witnessed a steadily growing interest in the scientific and philosophical value of Lucretius' *De rerum natura*. Long admired and studied for its literary charm, the magnum opus of Roman Epicureanism has greatly benefited from the emergence of reception studies as well as from a renewed attention to Latin as a language of ancient and modern science. Building on this body of research, Marco Beretta's straightforward but ambitious book claims that Lucretius' work deserves a much fairer treatment than is usually accorded to it as a piece of didactic poetry, a philological conundrum, and an Epicurean source-text. According to Beretta, far from being the singularly splendid masterpiece of an isolated mind, the *De rerum natura* stems from a fertile (and even 'revolutionary') intellectual milieu, mirrors Lucretius' attraction to scientific experimentalism, and has therefore never ceased to fascinate Western physicists and physicians.

Opposing the long-standing tendency to play down the importance of Roman science, the first chapter argues that the lively cultural environment of the late Republic provided an excellent basis for Lucretius' physical poem. Beretta offers a short survey of the Romans' fervid activities in such different fields as medicine, geometry, astronomy, and engineering, paying special attention to the dramatic increase in the production of technical and scientific writings between the end of the first century BC and the beginning of the first century AD. Most of the evidence cited comes from the works of well-known authors like Cicero, Varro, Seneca, and Pliny, but relevant pieces of archaeological evidence (such as the Antikythera Mechanism) are also taken into account. Cultural historians might have wished to see a closer examination of the admittedly huge amount of bibliography recently produced on such themes;[1]

[1] See, e.g., Beretta's discussion of the role of libraries [42–44], which could have benefited from the insights of König, Oikonomopoulou, and Woolf 2013, 124–417.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 93–100

yet the declared (and, for this reviewer, praiseworthy) purpose of the chapter is to show Lucretius' embeddedness in the 'revolutionary' atmosphere of the late Republic from the broadest possible perspective. To cite just one example, readers accustomed to the (by now standard) view of Lucretius as an Epicurean fundamentalist may be impressed by Beretta's intriguing claim that the first appearance of the word *vitrum* in *De rer. nat.* 4.143–154, 601–602, reflects the emergence of the art of glassblowing around the middle of the first century BC.

For Beretta, however, the most powerful factor of intellectual transformation in Lucretius' day was the diffusion of Epicureanism. Chapter 2 illustrates the core tenets of Epicurus' ethics and natural philosophy, their relationship to previous traditions (Democritus, the Academy, and the Peripatos, above all), and their remarkable impact on the Stoic-influenced Roman debate. In consideration of the book's focus on Lucretius, special attention is paid to the deconstructive potential of Epicurus' anti-teleological discourse in the Roman political context. Notwithstanding certain simplifications,[2] the chapter does an effective job of pointing out the depth of the Epicureans' interest in science and the theoretical foundations of the Epicurean polemic against Eudoxus and Euclid. Especially valuable are the synopsis of Epicurus' treatise *De natura* [67–68], the re-assertion of Asclepiades of Bithynia's contested dependence on Epicurean doctrines [79–83], and the suggestion that the Epicurean love for gardens and natural spaces shaped the Romans' lifestyle and architecture [93–99].

Chapter 3 turns to the *vexata quaestio* of Lucretius' biography and sources. The fact that Beretta is a historian of science and not a philologist may have played a role in his choice of reassessing the evidence on Lucretius' life both inside and outside the poem. As Beretta himself acknowledges, tentative speculations about this desperately obscure poet—his social status and personal experiences—have flourished since the Renaissance *Vita Borgiana*. With the decline of biographical criticism, classical scholars have been more and more

[2] For instance, Beretta [60] clearly has Lucretius in mind when making the unlikely claim that *Epicurus* simultaneously destroyed the foundations of the Platonic, Aristotelian, and Stoic systems. (As recently restated in Kechagia 2010, 132–155, there is no evidence that Epicurus ever criticized the contemporary Stoa.) Likewise, it is quite reductive to define the Stoic theories of religion and divination as 'superstitions' [84].

unwilling to make new conjectures (or to accept old ones), and the strikingly incoherent body of hypotheses about Lucretius' life has even become an object of ridicule.[3] Beretta is wise enough to avoid gratuitous assumptions and essentially questions the ancient *vulgata* concerning the poet's folly and Cicero's role as 'editor' of the *De rerum natura*. Beretta also challenges the common assertion that Lucretius' accounts of scientifically relevant experiences are mainly based on 'bookish' materials. Although this reviewer has reservations about the possibility of ascertaining Lucretius' aristocratic background [113–114] or his familiarity with southern Italy [115–116], several of Beretta's remarks are undoubtedly of great interest. For example, the recent recovery of some iron rings in a Samothracian sanctuary [119–120] seems indeed to confirm Lucretius' claim about autopsy in 6.1044–1046.[4] Unlike David Sedley [1998, 93], who famously argued that Epicurus' *De natura* was Lucretius' 'sole philosophical source and inspiration', Beretta holds that the *De rerum natura* reports a number of original experiments and relies on a variety of sources, from Aristotle's biological writings to Asclepiades of Bithynia and the Stoics. Moreover, on the basis of Knut Kleve's identification of a few Lucretian lines in the Villa of the Papyri at Herculaneum [Kleve 1989],[5] Beretta assumes that Lucretius visited the Villa and the Campanian littoral [132]. Though not all readers will be convinced by this final inference (since, among other things, the *De rerum natura* may have reached Herculaneum after Lucretius' death), it should be recognized that Lucretius' relationship to Philodemus and other post-Hellenistic writers is still a largely open issue.[6]

Chapter 4 elucidates the foundations of Lucretius' theory of matter, from the principles of atomic weight and motion to the ideas of void, mixture, and swerve (*clinamen*). What makes Beretta's treatment particularly interesting is his emphasis on the biological, qualitative character of Lucretius' atomism. Beretta defines the corpuscularian physics of the *De rerum natura* as 'the science of seeds' ('la scienza dei semi'), since Lucretius analogically re-uses the agricultural notion of *semen* in order to make the Epicurean concept

[3] A vein of sarcasm underlies Holford-Stevens 2002.

[4] The passage has been interpreted as an eye-witness account also in Sedley 1998, 52–54.

[5] As recalled by Beretta, Kleve's identification has been questioned in Capasso 2003.

[6] On matters of poetic theory and pedagogic method, see most recently Beer 2009.

of atoms vivid (and palatable) in the eyes of Roman readers. The poet's conscious attempt to connect the mechanics of invisible bodies with the perceptible evidence of organic life is also witnessed by his prevalent focus on atomic interactions (and not on the atom as a single entity) as well as by his attention to the results of atomic aggregation, the so-called *concilia*. With an effective readjustment (and some due distinctions), Beretta interprets the Lucretian *concilia* as molecules and offers interesting remarks on the impact of the *De rerum natura* on modern alchemy. Beretta might have found further support for his thesis in Myrto Garani's insight that Lucretius' interest in elemental compounds derives from Empedocles (whose physics is readapted in the poem to an atomistic framework) [Garani 2007].

Chapter 5 discusses Lucretius' scientific method and its relationship to the ancient tradition. After recalling that Epicurus' followers shared a firm faith in the gnoseological value of sensorial experiences, Beretta shows how Lucretius took up and at the same time expanded the Epicurean theory of knowledge, atomic films (*simulacra*), and the senses. Special attention is devoted to the account of visual perception in *De rer. nat.* 4, which Beretta rightly interprets as an Epicurean response to Aristotle [168–174]. More generally, the chapter reaffirms the poet's well-known preference for visual evidence, analogical arguments, and empirical immediacy, arguing once again that several Lucretian demonstrations are founded on personal experiments. Some of the passages cited are perhaps more appropriate than others to serving Beretta's purpose.[7] Yet Beretta's reaction against the narrowly philological view of Lucretius as a 'fundamentalist' epitomizer of previous accounts [176], and his observation that the empirical method of the *De rerum natura* is not identical with that of other Epicurean sources [184],[8] are a most welcome contribution to the scholarly debate. In all likelihood, we will never know for sure which of Lucretius' *exempla* are based on 'original' research, and in this chapter Beretta himself points out the poem's debt to Empedocles' analogical imagery. But in the absence of specific textual correspondences, the assumption that all Lucretian arguments are borrowed from elsewhere

7 For instance, whereas the description of ocular anatomy in 3.408–415 displays an admittedly uncommon attention to human physiology, the snake-argument in 3.657–665 echoes a tradition of animal dissections dating back at least to Aristotle (as I have argued in Tutrone 2014a).

8 For a rich discussion of this problem, see Asmis 1984, esp. 293–320.

simply mirrors the professional devaluation of Lucretius' contribution by a community of word-lovers (the *philologoi*) who have much to gain from closer cooperation with historians of science.

In chapter 6, Beretta deals with Lucretius' approach to cosmological issues, pointing out that the *De rerum natura* ultimately aims to promote the 'new world order' ('il nuovo ordine dell'universo') of Epicurean philosophy. Beretta is aware of the work of Elizabeth Asmis [2008], and it is, of course, no accident that the chapter attaches great importance to the Lucretian notion of *foedera naturae*—that is, to the idea that natural laws are not imposed by a metaphysical authority but rather by the necessary agreement of things. As Beretta shows, the contrast between the concepts of *prescriptive* and *descriptive* natural law will have a second lease on life in the modern age, when scientists like Newton revive the Epicurean tradition. As for Lucretius' own understanding of cosmology and astronomy, however, Beretta restates the common scholarly view that the *De rerum natura* contains a series of clumsy and often 'regressive' demonstrations, which may be partly explained as the result of two intellectual difficulties: the unsuitability of the Epicurean analogical method for the analysis of distant phenomena, and Lucretius' unease with the close association between astronomical research and religious astrology in Roman culture. Still, since Beretta concedes that in passages like 5.621–628 and 705–750, Lucretius follows patterns of explanation which are either different or more recent than those followed by Epicurus, one may wonder if, far from being awkwardly backward-looking, the cosmology of the *De rerum natura* takes part in a (largely unknown) scientific debate entailing the reception/revitalization of Presocratic and classical doctrines. The second part of chapter 6 [198–206] is very interesting, as it identifies the 'revolutionary value' of Lucretian cosmology in its reintegration of sublunary and celestial physics, challenges any fixist (or anachronistically evolutionary) view of Lucretius' biology,[9] and reassesses the validity of the Epicurean method of multiple explanations.

[9] In the modern debate, fixism opposes any evolutionary theory about the origin and transformation of species. Fixism is often associated with creationism (the belief in the existence of a creator god), but it should be recognized that applying such modern definitions to the history of ancient science may be greatly misleading. Lucretius, for instance, argues fiercely against creationism but is at times inclined to fixism.

Chapter 7 discusses Lucretius' conception of natural evolution and historical progress, arguing that the poet of the *De rerum natura* is 'the most explicit' ('il più esplicito' [207]) among the ancient authors who believed in the progressive development of history. Analogous claims have been made in the past about Seneca,[10] but neither this nor other interpretive issues—especially, the cultural and conceptual gaps between ancient and modern paradigms of progress—are dealt with in depth in the chapter. At the very beginning [208], Beretta declares that a comprehensive analysis of the secondary literature on Lucretius' notion of progress is beyond the scope of his treatment. This is not only understandable but also reasonable. Yet, when noticing that Beretta endorses John Masson's thesis—formulated in the heyday of positivism (1909)—that Lucretius' faith in permanent progress is a necessary corollary of his rejection of religion [217], readers are led to wonder whether this chapter might have benefited from a more careful discussion of the recent bibliography. However, there are also several points which are of indubitable interest. For example, Beretta highlights the double-sided nature of Lucretian progress and its strict dependence on human moral choices, showing how, from a certain stage of development onwards, Lucretius regards the pressure of need, unenlightened by Epicurean wisdom, as insufficient to produce positive progress. Moreover, when reassessing the poet's approach to natural history, Beretta observes that a fixist understanding of the genetic makeup of organisms [cf. 5.923–924] co-exists with an appreciative, naturalizing view of vegetable-grafting (5.1361–1364) and with the general claim that mankind's constitution gradually evolved and softened [5.1013–1018].[11]

It is worth mentioning that both chapter 7 and chapter 8 reproduce, with some revisions, previously published materials [cf. Beretta 2008a, 2008b]. To be sure, the eighth chapter is one of the most valuable in the book, in part because the author steps into a very congenial field of inquiry: Lucretius' reception in the history of modern science. After briefly remarking on Lucretius' *Nachleben* between Late Antiquity and the Middle Ages [219–221], Beretta digs into the scientific re-use of the *De rerum natura* from the time of

[10] See, above all, Edelstein 1967, 169–175. For a critical reconsideration of this and other modernizing readings, see Tutrone 2014b.

[11] As acknowledged by Beretta at the start of his survey, the most thorough contribution to the understanding of Lucretius' stand in this field is Campbell 2003.

Poggio Bracciolini's rediscovery (1417) to the 20th century. Beretta chooses the 'direct or indirect involvement of natural philosophers and scientists' ('coinvolgimento diretto o indiretto di filosofi naturali e scienziati' [222]) in the modern editions of the poem as a *fil rouge* for his (potentially immense) survey. Throughout a notably well-informed discussion, Beretta succeeds in showing how famous editions of the *De rerum natura*, such as those by Ferrando (1472–1473), Lambin (1563–1564), Nardi (1647), Marchetti (1717), and Munro (1866), influenced the Western debate on the nature and motion of atoms, the existence of void, the origin of diseases, and the autonomy of research. From Girolamo Fracastoro to James Clerk Maxwell, from Galilei and Newton to Heisenberg and Einstein, more than five centuries of history are revisited to highlight the everlasting appeal of Lucretius beyond the boundaries of literature. Quite correctly, such a long-lived series of re-appropriations is interpreted by Beretta as a proof of 'the multifaceted and interdisciplinary character of scientific culture' ('il carattere multiforme e interdisciplinare della cultura scientifica' [264]). Those who insist on regarding poetry as indifferently imaginative and ancient atomic physics as annoyingly arid may learn a lesson here.

The book includes an appendix with the most famous *testimonia* on Lucretius' life and work (a useful complement to chapter 2), a pluri-lingual bibliography (especially remarkable for its list of Lucretian editions from the Renaissance to the 21st century), and an *index nominum*. Even if, of course, opinions differ—for the reading of classical texts is, by definition, as unpredictable as the Lucretian *clinamen*—students of ancient and modern science should be grateful to Marco Beretta for assembling such a bold and accessible work.

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The Daimon in Hellenistic Astrology: Origins and Influence by Dorian Gieseler Greenbaum

Ancient Magic and Divination 11. Leiden/Boston: Brill, 2016. Pp. xxiv + 573. ISBN 978–90–04–30620–2. Cloth €186.00, $241.00

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The word «δαίμων» appears in the technical language of Hellenistic astrology in two contexts. On the one hand, two of the 12 topical places (τόποι), houses in modern astrological parlance, of the horoscope bear the traditional names 'Good Daimon' and 'Bad Daimon'; on the other, there is a calculated horoscopic point of the genre called lots (κλῆροι: parts) that is labeled the 'Lot of *Daimon*'. In both cases, this *daimon* is paired with fortune (τύχη).

Daimons are, of course, far more familiar from Greek mythology, theology, philosophy, and magic, especially in the form of a personal *daimon*, a supernatural entity acting as a guardian of an individual. These entities, it seems, often influenced astrology in its stricter or broader, more or less technical form, when it was used to classify or describe *daimons* in order to communicate with them effectively or to find the personal *daimon* in an individual's nativity (birth-horoscope).

Furthermore, since *daimons* had a strong relationship with fate and destiny both in and outside technical astrology, and since astrology as a craft was meant primarily to be a study of fate, *daimons* and astrology were intertwined in antiquity in many ways. By singling out this relationship for the subject of her PhD thesis in the 2000s, Dorian Greenbaum found a promising area of research. The book here reviewed is an expanded and updated version of her dissertation of 2009.

The title itself of the book is somewhat misleading since it discusses not the *daimon* in Hellenistic astrology so much as the *daimon* and its intricate relationship with astrology; and the complexity of this subject is also reflected in the organization of the book. It is divided into three parts:

(1) *Daimon* and Fortune,
(2) Gods and *Daimons*, and

issn 1549–4497 (online) issn 1549–4470 (print)

(3) Lots and the *Daimon*.

This structure might seem arbitrary but it is one of very few meaningful layouts that can organize the book's abundant sources and secondary literature. It also shows that the role of the *daimon* in astrology cannot be properly understood without the knowledge of the rich and complex cultural background in which astrology is embedded.

In the first part ('*Daimon* and Fortune'), chapter 1 surveys the themes of the *daimon*, fortune, and astrology through the lenses of two representative authors of the second century AD, Plutarch and the astrologer Vettius Valens. The investigation of the latter is easily justified by the fact that Valens is practically the only known astrological author who has anything to say about the issues of fortune and fate beyond technicalities. Besides the various treatises from Plutarch's *Moralia*, the spurious *De fato* from the same era is surveyed to provide a full image of contemporary thinking about the *daimon*, fortune, and fate.

Chapter 2 is devoted to the astrological pairing of 'Good Daimon' and 'Good Fortune', that is, the names of the 11th and fifth places of a horoscope, respectively. It offers an analysis of astrological works from Manilius (early first century AD) to Rhetorius (fifth or early sixth century), who is considered the latest representative of Hellenistic astrology. This discussion is introduced with an eye to the wider historical and cultural background, using Greek and Demotic sources. This theme is continued into chapter 3, which investigates the issues raised in the previous chapter in the other Mediterranean cultures, most importantly, in Egypt and Mesopotamia. A convincing and highly important conclusion is found at the end of this chapter [114]: Greenbaum raises the possibility that the Greek concept of immutable fate was mitigated in Hellenistic astrology by oriental influences that allowed negotiation about fate.

The first part concludes with chapter 4, which treats the 'Bad Daimon' and 'Bad Fortune' (the names of the 12th and sixth places in Hellenistic astrology) in much the same way as their positive counterparts earlier. In this instance, however, Greenbaum summarizes briefly Mesopotamian, Egyptian, Greek, Jewish, and Christian traditions regarding demons (that is, malevolent *daimons*) before discussing astrological ideas.

Comparison of chapters 2–3 with chapter 4 reveals similarities in the survey of astrological authors, though there are also some dissimilarities. Of the latter, the different descriptions of the cultural background are entirely justifiable, but chapter 4 includes a table of names and descriptions of the sixth and 12th places [143–145] which chapter 2 oddly lacks. Although this table is useful as an overview of the ideas, in practice it suffers from two shortcomings. First, a table exhibiting the diachronic development of the themes related to these two topical places would have served the reader better than this *potpourri* of keywords collected from different astrological authors. Second, it seems that the known Hellenistic interpretation of the places is the result of the amalgamation of two cognate but different streams of ideas: the δωδεκάτροποϲ (twelve-turning), covering all the 12 places, and the ὀκτάτροποϲ (eight-turning) associated with 'Asclepius', which extends only over the first eight astrological places, including the fifth and the sixth, the equivalent of 'Good Fortune' and 'Bad Fortune' of the δωδεκάτροποϲ, respectively [Beck 2007, 44–45]. These different constituents, although known by Greenbaum [400n5], are left unmentioned, though they should have been analyzed more carefully to give the necessary insight into the intricacies.

The second part ('Gods and *Daimons*') consists of three chapters. In chapter 5, Greenbaum investigates Gnosticism and Mithraism to show how the role of *daimons* and their relation to gods are evaluated in harshly different ways within syncretic traditions in which astrological thinking is also found. At least two important achievements must be highlighted here: a new and sound suggestion to assign Gnostic « αἰῶνεϲ »/« ἄγγελοι »/« ἐξουϲίαι » to the zodiacal signs and planets [174–175] as well as an intriguing treatment of the so far neglected *thema dei* found in the Byzantine summary of the *Introductio* of Antiochus of Athens [187–193]. This latter gives further support to Roger Beck's hypothesis that this Antiochus is identical with C. Iulius Antiochus Epiphanes Philopappus, the eponymous archon of Athens in the late first century AD. He belonged to the family of the astrologers Thrasyllus and Balbillus, whose activities, and therefore Antiochus', may well be connected to the rise of the Roman mysteries of Mithras [Beck 2006, 253–254].

Chapter 6 extends this inquiry of good and evil *daimons* into the realm of magical papyri, the philosophical *Hermetica*, and the decan-lore originating from Egypt and eventually subsumed into astrology. Here, some astrological works are examined along the same lines taken in the first part. Overall, the content of chapter 6 is rather vague.

In contrast, chapter 7 investigates the role of the personal *daimon* in Neoplatonism with a special focus on Porphyry, who links the idea of a personal *daimon* to the astrological concept of the οἰκοδεσπότης (the master of the house), a type of a ruling planet in a nativity. This concept is not without problems, as «οἰκοδεσπότης» has different context-dependent meanings in astrological texts; but these concerns are excellently clarified here [256–257]. More problematic is Greenbaum's acceptance of the *Introductio ad Ptolemaei tetrabiblum*, specifically its mostly uncontested chapters, as a genuine text of Porphyry. This issue and the analysis of 'Porphyry's' (in fact, Antiochus') method to find the οἰκοδεσπότης will be further explored below. As a final remark on this chapter, it is not clear how Greenbaum would like the reader to understand Iamblichus' five elements (στοιχεῖα) in finding the οἰκοδεσπότης [256]: she refers to Ptolemy's technique as an example of these 'five steps', but the exact meaning remains uncertain.

The final part ('Lots and the *Daimon*') is devoted to the previously mentioned astrological lots, chiefly to the Lot of *Daimon*, its counterpart, the Lot of Fortune, and further lots derived from them, as well as to their cultural background. Both these lots are calculated by measuring the interval between the Sun and the Moon from the Ascendant clockwise or counterclockwise, depending on whether the horoscope is cast in daytime or in nighttime. Chapter 8 explores the notion of lot in Hellenistic culture, emphasizing the connection between the *daimon* and lots in Plato's Myth of Er. This chapter concludes with a survey of the doctrine of lots in astrology, but the exploration of the rather extensive material is sensibly narrowed down to topics having greater importance, such as Manilius' idiosyncratic Circle of Athla (a sort of alternative δωδεκάτροπος based on the position of the Lot of Fortune) and the lots found in the *Panaretus*, a lost book cited by the late fourth-century Paulus of Alexandria and attributed to Hermes. As it is rightly pointed out, the names of these 'Hermetic' lots (Fortune, *Daimon*, Necessity, Eros, Courage, Victory, and Nemesis) are all abstractions and have daemonic connotations [300]. Furthermore, the very important distinction between fatalism and determinism is raised here with the conclusion that Hellenistic astrologers in general, but at least Valens in particular, may have been determinists yet were definitely not fatalists [336].

Chapter 9 continues to investigate the two most important lots, those of Fortune and the *Daimon*, more closely, which makes this chapter perhaps the most technical in the book. Six carefully chosen case studies, mostly from Valens, illustrate the various usages of these lots as well as a derivative of theirs, the Lot of Basis. The chapter concludes with a section on the appearance of the two lots in the techniques of ascertaining the length of life. While the discussion is satisfactory in every detail, the usage of the Lot of Fortune in a katarchic context, for instance, in astrological thought-reading (see, e.g., Hephaestio, *Apot.* 3.4.14–18) might also have been mentioned.

Finally, chapter 10 adduces two more derivative lots (at least in a tradition separate from the Hermetic one), those of Love and Necessity. A section is devoted to the cultural background of the pairing of love and necessity and another one to their astrological role, supplemented with the assessment of all known horoscopes utilizing them, including a recently published horoscope on papyrus, P. Berlin 9825 [Greenbaum and Jones 2017], which, unlike the others, uses the Hermetic formulas. One notable achievement must be mentioned here: the association of the caduceus with the four lots, Fortune, *Daimon*, Eros, and Necessity.

The book ends with conclusions and several appendices, the first of which is a highly useful summary of astrological theory. The rest is mostly a collection of source-texts illustrating the various chapters. Conclusions also provide the reader with an excellent aid to discover the most important themes and threads of the book, which are often buried under the vast material.

What is deeply missed, however, is a chapter on methodology, even if it can be gleaned from the structure of the book that the aim is to read and utilize every piece of source material and scholarly literature related to the broader relationship of astrology and the *daimon*. Still, this barely conscious methodological approach results in a curious contrast between Greenbaum's handling of secondary literature and primary sources on astrology; whereas arguably all the accessible scholarly contributions are covered (the bibliography runs to 28 pages), the usage of the sources is rather haphazard.

In some cases, it is a mixed result of an uncritical acceptance of the accessible editions and ignorance of their recent re-evaluations. To give an example: texts from Antiochus' *Thesauri* (not *Thesaurus*, as referenced throughout the book) as edited by Franz Boll [1908] are cited six times, although David Pingree, in an article known and even cited four times by Greenbaum, warned that this attribution is largely mistaken [1977, 214–215].

Another problem of minor importance is that Greenbaum appears completely unaware of the syncretic tendency of astrological text-editions prior to the publication of the first volume of Hephaestio of Thebes by Pingree [1973]. Before that year, editors, in an attempt to reconstruct a hypothesized common ancestor of manuscripts, eliminated the boundaries between different recensions, re-workings, epitomes, and excerpts in order to create an idealized but in fact conflated text that had never existed yet might please the aesthetics of similarly inclined classical philologists. This discomforting fact was first emphasized by Pingree [1977, 203], and has been repeated and aptly illustrated by Stephan Heilen recently [2010, 301–303]. Certainly, no readers or reviewers ought to expect Greenbaum to reconstruct, for instance, the different versions behind Emilie Boer's edition of Paulus [1958] from scratch. But the fact that not even allusions are made to the existence of available parallel texts, as in the case of Hephaestio, is rather alarming. Fortunately, the interpretations of the passages are rarely affected by this deficiency.

Compared to these two issues, the third problem is by far more general and pervasive in the book. While the theories expounded by different astrological authors are frequently discussed in various chapters, the development of ideas as it is displayed in the source-texts is scarcely elaborated. I shall illustrate this claim with a randomly chosen example: the relationship of the fifth astrological place and children, discussed in chapter 2 [50–76].

Here, Greenbaum, assessing Manilius' poem, is astounded by his association of health-issues with the fifth place, which is 'unlike traditional descriptions of the fifth, which stress fertility and children' [60]. The significations given by Antiochus, 'both the acquisition of living beings (ἐμψύχων κτῆϲιϲ) and the increase of things pertaining to living' [65] are also received reluctantly. On the other hand, she concedes that many other astrologers associate children with this place.

Had she compared the texts giving descriptions both of the aforementioned δωδεκάτροποϲ and the ὀκτάτροποϲ, that is, the Michigan Papyrus and the works of Thrasyllus, Antiochus, and Firmicus Maternus, more carefully, she should have noticed that (except in the description of Firmicus Maternus, who is two or three centuries later than the other authors) while the ὀκτάτροποϲ-system does associate the fifth place with children, even calling it 'the Place of Children', the δωδεκάτροποϲ-system does not. In the latter system, the fifth place either means some unqualified good fortune or is further elaborated in various ways by Manilius, Antiochus, and Valens [67]. Although one may argue that children can be interpreted as part of the broader context of Good Fortune (and, incidentally, also of the Good Daimon) in the δωδεκάτροπος, the interpretations of the planets lingering in the fifth place given by Valens [67] and Firmicus Maternus [70] have only to do with overall fortune and success, not with children.

Admittedly, there exists another tradition that does interpret planets in the fifth place as conveying indications exclusively for children, a tradition found in the works of Paulus and, of course, Olympiodorus [74], as well as in a poem cited in 'Palchus' 134 as attributed, probably falsely, to Antiochus [Pérez Jiménez 2011].[1] Also, the amalgamation of the indications of the fifth place in the δωδεκάτροπος and ὀκτάτροπος is attested both in techniques related to the genethlialogical topic of children and in a description by Valens [*Anth.* 4.12.1], overlooked by Greenbaum, which calls the fifth place that 'of children, friendship, partnership, slaves, freedmen, acquisition,[2] some good deed or good service'—covering also many of the meanings of the 11th place.

This example illustrates how complex the development and transmission of astrological ideas was, and the significance of Greenbaum's failure to separate the distinct but interrelated threads. Her undeclared method of aggregating sources—which are sometimes barely reliable, and at other times attributed to certain authors without solid ground—with occasional oversight of relevant texts seems to have resulted in these three problems in her account.

Greenbaum also falls into the trap of building narratives, one being exceptionally grand and fragile: Porphyry's paramount role as a link between fate, the Platonic *daimon*, and astrology. Whereas Porphyry's importance in this context cannot really be denied, as was already mentioned, Greenbaum throws caution to the winds when she accepts the text entitled «Πορφυρίου φιλοσόφου εἰσαγωγὴ εἰς τὴν Ἀποτελεσματικὴν τοῦ Πτολεμαίου» (Latinized as 'Introductio ad Ptolemaei tetrabiblum') as genuinely his. In truth, several arguments may be raised against his authorship beyond the ones mentioned [266–267n122; László 2020]. Most of the *Introductio* attributed to Porphyry is a slightly adapted copy of Antiochus' *Introductio*, which is seen in chap-

[1] Greenbaum does not mention Pérez Jiménez 2011.

[2] Reading «περιποιήσεως» with MS Venice, BNM, gr. Z. 334, c. 55 on f. 181 [Kroll 1900, 158], for the «ἐκποιήσεως» of Valens' manuscripts.

ter 30, the very one analyzed and discussed by Greenbaum [268–273]. The investigation of the κύριος is postponed [Boer and Weinstock 1940, 207.28]; but this promise will be fulfilled only in Antiochus, *Epit. intro.* 2.3 [Cumont 1912, 119.22–33], the original of which is now lost. Therefore, this chapter, which for Greenbaum is the key text linking Porphyry's ideas of the personal *daimon* to astrological technicalities, is probably Antiochus' genuine text, otherwise summarized in *Epit. intro.* 1.28 [Cumont 1912, 118.9–22].

A final remark about Antiochus. The two major works associated with his name are the *Thesauri* and the *Introductio*. The *Thesauri* is extant in its fullest form as book 5 of Rhetorius, *Comp.* [Pingree 1977, 210–212]; whereas the *Introductio* is lost, save for a summary in *Epit. intro.* [Cumont 1912, 111–119], several chapters in [Porphyry]'s *Introductio*, and a few fragments. Since several chapters of the *Thesauri* overlap with what is extant of the *Introductio* and are mostly reworked [Pingree 1977, 207–208], it is reasonable to assume that, since Antiochus alone was the author of the *Introductio*, his name was attached to the *Thesauri* only as a mistake by Rhetorius, and that the chapters in Rhetorius' *Comp.* resembling the ones in the *Introductio* are barely adaptations [cf. Schmidt 2009, 21]. Certainly, one cannot entirely dismiss the idea that certain chapters of the *Thesauri* missing from the summary of the *Introductio* may have been authored originally by Antiochus, while their present form is obviously due to Rhetorius. Therefore, it seems more reasonable to associate the *Thesauri* with Rhetorius, not Antiochus.

In the following, I record some minor corrections, additions, and remarks:

(1) 8n28; 27n44; 306n14; 309n24; 310n30; 447–449: *CCAG* 1.160 is not genuine Antiochus, but Rhetorius, *Comp.* 5.47 ultimately stemming back to Paulus (as is also acknowledged).

(2) 21n16 and 306n14: *CCAG* 7.127 is Rhetorius, *Epit. IIIb* xvi; but it is in fact a copy of Antiochus, *Epit. intro.* 1.1 [Cumont 1912, 112.2–4 (Moon), 111.18–19 (Sun)].

(3) 50: the concept of Jupiter and Venus being the greater and lesser benefics, respectively, is medieval, postdating Guido Bonatti and Leopold of Austria (13th century), who do not mention it.

(4) 63–64; 279 and n. 4; 311: comparing Dorotheus, *Carm. astrol.* 1.24.6 to the available Latin translation of an Arabic version composed around 800 by al-Khayyāṭ [Heller 1549, d2v–d3], the word 'fortune' (Arabic « saᶜādah » [Pingree 1976a, 30.5]) most likely refers to material fortune,

in the same manner as towards the end of the sentence [Pingree 1976a, 30.6].

(5) 65n90: *CCAG* I, 157 is Rhetorius, *Comp.* 5.28, using Antiochus, *Epit. intro.* 1.18 [Cumont 1912, 116.3–6], which is found in another version as [Porphyry], *Intro.* 36 [Boer and Weinstock 1940, 209.19–21]. This latter is quoted here.

(6) 143–145 and 149n159: *CCAG* 7.114–115 is not Antiochus, but Rhetorius, *Epit. IIIb* 21, deriving from Rhetorius, *Comp.* 5.59, which is quoted here on 149n159. Therefore, delete 'dog-men' and 'epileptics' on 143. The referenced passage in the *Liber Hermetis* (more correctly, *De triginta sex decanis*) originates from Rhetorius.

(7) 146n148: read Rhetorius, *Comp.* 5.57 = Rhetorius, *Epit. IV* 1.

(8) 146n150; 148n155; 148 and n. 157; 149n161: Rhetorius draws on Firmicus, *Math.* 3.4.34, 3.5.39, 3.6.25–26, and 3.4.11, respectively.

(9) 148 and n158: *CCAG* 7.114 is not Antiochus, but Rhetorius, *Epit. IIIb* 21, deriving from Rhetorius, *Comp.* 5.56.

(10) 167 and then *passim*: in fact, the expression 'Chaldean order' is an early modern derivation from Macrobius, *In somn.* 1.19.2, and was never used as such by Hellenistic astrological authors, who favor the expression 'seven-zoned [sphere]' («ἑπτάζωνος [ϲφαῖρα]»).

(11) 184n115: Antiochus' authorship of the calendar, which is the second part of Rhetorius, *Comp.* 6.7 = Rhetorius, *Epit. IIIb* x, is contested [Pingree 1977, 215]. *CCAG* 1.163 is Rhetorius, *Comp.* 5.51. Whether it is from Antiochus is uncertain.

(12) 186n119: Paulus, *Intro.* 37 is a late addition since it is omitted from the extant summary [Cumont 1912, 95–97; Boer 1958, xxi–xxiv], and not contained in several manuscripts. Its alternative *thema mundi* is probably translated or adapted from Arabic.

(13) 227nn147–148 and 229n157: the so-called 'scholium 9' of Paulus is not a scholium but an addition to Paulus, *Intro.* 4 in branch β of Paulus' manuscripts [Boer 1958, xii] from Rhetorius, *Comp.* 5.10, which latter is also copied into [Porphyry], *Intro.* 47. It is probably not from Antiochus.

(14) 232n168: *'Liber Hermetis'* in fact descends from the quoted Rhetorius passage. The difference is due only to misreading «λαμπρομοιρίαν» in a way that would result in «λαμπρὰ ὅρια.» It refers to the doctrine of 'bright degrees', which has different traditions. Rhetorius, *Comp.* 6.17 tabulates one, which will be later transmitted into Arabic astrology, while *De sex. dec.* 3.1–16 describes a different system. There are many further variants [cf. Heilen 2015, 2.1320–1323].

(15) 257n87 and 436–437: under 'Palchus', the anonymous astrologer of the emperor Zeno must be understood. For No. L486 [436] see now

Pingree's edition [1976b, 148–149]; No. L487 [437] appears, among others, as 'Palchus' 87, and there is one more horoscope, dated to 479, also in 'Palchus' 59, which uses « οἰκοδεσπότης » in meaning #1a [Cumont 1898, 104.15]. This latter is omitted from *TLG*.

(16) 311 and n32: *CCAG* 1.161 is not Antiochus, but Rhetorius, *Comp.* 5.48.

(17) 311 and n33: *CCAG* 7.113 is not Antiochus, but Rhetorius, *Epit. IIIb* 20, deriving from Rhetorius, *Comp.* 5.65.

(18) 314 and n. 42: Antiochus, *Epit. intro.* 1.4 [Cumont 1912, 113.8–9], which is apparently a concise summary of [Porphyry], *Intro.* 44, does not use the Lots of Fortune and the *Daimon* in the zodiacal melothesia; however, Rhetorius *Comp.* 5.14, copied as [Porphyry], *Intro.* 50, does, referring to Rhetorius *Comp.* 5.61 = Rhetorius, *Epit. IV* 4, which in parts is clearly based on Valens, *Anth.* 2.37 [Pingree 1977, 214]. The source of the doctrine, therefore, is Valens.

(19) 376 and 480: the horoscopic fragment is probably an insertion into Olympiodorus' text since it appears in the middle of lists of lots [Boer 1962, 53–59] already inserted into the hyparchetype of the extant manuscripts [Burnett and Pingree 1997, 191].

(20) 387n179 and 475: Abū Maᶜshar's Lots of Affection and Love (*sahm al-ulfah wa-al-ḥubb*) and of Poverty and Lack of Means (*sahm al-faqr wa-qillat al-ḥayāh*) (ninth century) together with the other lots were simply copied by al-Bīrūnī in the 11th century, only the English translations differ. The same is true in the case of his adaptation of the list of lots in his *Kitāb al-mudkhal (al-kabīr)* (*[Great] Introduction*), into the more concise treatise entitled 'Mukhtaṣar al-mudkhal' ('The Abbreviation of the Introduction'), also known as the *Kitāb al-mudkhal al-ṣaghīr* (*Little Introduction*). The records for these works are badly confused in the index [551]. It must also be noted that John of Seville, a translator of the *Great Introduction*, interpreted the word « ḥayāh », meaning 'life; faculty of growth, sensation or intellect', in a Mercurial way to produce 'ingenium' [Lemay 1995–1996, 6.332.439]; see also Adelard of Bath in his translation of the *Mukhtaṣar* writing 'useless concern' (6.8: *sollicitudo inefficax*) [Burnett, Yamamoto and Yano 1994, 128]. These lots, however, had already been known in the eighth century by Māshāʾallāh: see *Liber Aristotilis* 3.xii.1.2 and 3.xii.3.3. The source is Dorotheus [Burnett and Pingree 1997, 194]; the history of lots is considerably more complicated than what Greenbaum's examination suggests.

(21) 399n2: only the definition of the tropical zodiac is given here, although until about the fourth century astrologers used a certain type of sidereal zodiac exclusively [Jones 2010]. The reference to Antiochus should also be to Rhetorius, *Comp.* 5.proem.

(22) 400n6: the description of the quadrant-system does not appear in the genuine text of Olympiodorus, only in the 14th-century reworking composed probably by Isaac Argyrus [Caballero-Sánchez 2013, 94–98].
(23) 404: the expression 'Ptolemaic aspects' is a double misnomer in Hellenistic astrological context. On the one hand, there seems to be no dedicated expression for 'aspects' before Arabic astrology, save for words deriving from « cχῆμα » and verbs involving the notion of vision. On the other, the 'classical' configurations are first called 'Ptolemaic aspects' only in the 17th century, after Kepler's 'invention' of the so-called 'minor aspects' [*De fundamentis astrologiae certioribus*, thesis 38: [Kepler 1601, c1v]]. In this latter context, it reflects the false but widespread assumption that Ptolemy was the archetypal Hellenistic astrologer.
(24) 408n22: read Rhetorius 5.7 for Antiochus. For the genuine description of Antiochus, see *Epit. intro.* 1.3 [Cumont 1912, 112.27–28], whose original is perhaps [Porphyry], *Intro.* 6.
(25) 417–418: Emilie Boer's edition of Paulus [1958] is a conflation of different recensions of Paulus' text, and consequently its apparatus must be closely followed. There is no room to cite all the non-trivial testimonies here; it is sufficient, however, to remark that the version found in Rhetorius, *Comp.* 6.30 on ff. 191–196 of MS Paris, BNF, gr. 2425 (Boer's ms *Y*) and the closely related but radically reworked version in Rhetorius, *Epit. IIIb* (Boer's ms family δ [Boer 1958, xii; Pingree 1977, 212–215]) use the language of indication (« δηλόω », « cημαίνω », « [ἀπο]δείκνυμι ») consistently, in contrast with the language of causation found in the other recensions whose readings are accepted in the edition.
(26) 429–431: for the new edition of Antigonus' examples, see Heilen's edition [2015]: for No. L40, see 1.160–161; for No. L76, see 1.130–131 and 133–137; for No. L113, IV, see 1.168–169 and 172–175.
(27) 433: Greenbaum's suggestion is an excellent and exemplary emendation of the defective text.
(28) 450–452: this is a part of Rhetorius, *Comp.* 5.54; cf. *De sex. dec.* 16.30–45 (seventh consideration) and 16.22 (fifth consideration), which originates in the same Rhetorius' text but provides the numerous emendations used here. Pingree's manuscript (also mentioned on xviii) is the abovementioned Paris, BNF, gr. 2425, which provides books 5–6 of Rhetorius, *Comp.*, including the summaries of Paulus and Antiochus' *Introductio*.

Apart from these deficiencies, mostly rooted in concerns about texts, there are many positive aspects of Greenbaum's approach. She understands Hellenistic astrology, including the perspective of a practitioner. She is sympathetic with features of astrology that are often blamed or ridiculed by others—for instance, the existence of myriads of techniques [301]—solely on the ground of preconceptions and ignorance. This is a refreshing advance beyond the occasional presentist biases of other scholars. At the same time, she laudably avoids, at least in the majority of possible cases, the pitfall of anachronism in astrological techniques, which could lead to confusion. The excellent quality of English writing must also be highlighted.

In summary, Greenbaum's *The Daimon in Hellenistic astrology* will indubitably enthrall those interested in the difference between fatalism and determinism and in the solutions provided by astrologers of the past. Moreover, it yields insight into the technicalities and practices of Hellenistic astrology.

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TLG *Thesaurus linguae Graecae.* http://stephanus.tlg.uci.edu/

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The Circulation of Astronomical Knowledge in the Ancient World by John M. Steele

Time, Astronomy, and Calendars: Texts and Studies 6. Leiden/Boston: Brill, 2016. Pp. x + 585. ISBN 978–90–04–31563–1. Ebook €175.00, USD $234.00

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In April 2014, John Steele invited leading scholars of the ancient astral sciences to Brown University for a conference entitled 'The Circulation of Astronomical Knowledge in the Ancient World'. The result is this edited collection, which presents technical studies in the circulation of astronomical and astrological knowledge in and between the ancient Egyptian, Greek, Indian, and, especially, Mesopotamian and Chinese traditions. The chapters are of high quality and will no doubt become essential reading for specialists in these individual areas.

Steele's introductory chapter is short. He offers only one paragraph on the theme of the text before discussing the contributions of the individual chapters, and so the reader might wish that he had offered more on what broader conclusions are to be drawn from the amalgamation of these chapters. Yet, in that single paragraph Steele makes clear a historiographical point significant to the volume. He explains his deliberate use of the term 'circulation', as opposed to 'transmission', and he notes that the transfer of knowledge is not a unidirectional process. Astronomical knowledge is not imposed on one group by another but instead entails a process of negotiation between groups. Reception involves adaptation—making the knowledge relevant to and compatible with the existing scholarly practices of the recipients. On this point, Steele cites Jamil Ragep, Sally Ragep, and Steven Livesey's *Tradition, Transmission, Transformation* [1996]. In this way, Steele signals that he is developing the historiographical approach of Ragep, Ragep, and Livesey in the specific domain of the ancient astral sciences.

Although it stands apart from the chapters that follow, Francesca Rochberg's 'The Brown School of the History of Science: Historiography and the Astral Sciences' is a welcome contribution to the volume. It could have been a

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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dry biography of the forefathers of our discipline, but instead the chapter examines and evaluates the historical approaches of Otto Neugebauer and David Pingree during their time in Brown's Department of the History of Mathematics. Rochberg illuminates Neugebauer's role in disabusing contemporary scholars of the belief that science started with the Greeks. She draws our attention to how Neugebauer and the Brown School more broadly brought to light scientific sources outside of the Greek corpus and paved the way for our understanding of the complexities of the Hellenistic world, including the Babylonian, Egyptian, and Indian astral sciences. This volume, with its attention on the technical aspects of ancient astronomy and astrology, extends the program established by Neugebauer.

With regard to Pingree, Rochberg illuminates his focus on the context of science. She quotes Pingree:

> One of the most significant things one learns from the study of the exact sciences as practiced in a number of ancient and medieval societies is that, while science has always traveled from one culture to another, each culture before the modern period approached the sciences it received in its own unique way and transformed them into forms compatible with its own modes of thought. Science is a product of culture; it is not a single, unified entity. [11]

This book continues Pingree's approach. The chapters analyze the reception and transformation of astronomical knowledge circulated between and within cultures, between scholars in disparate cities, between 'elite' and 'popular' astronomical traditions, between different genres of scholarship, and between practitioners of earlier and later time periods.

Several chapters examine the adaptation of knowledge appropriated from another culture. Alexander Jones, in 'Interpolated Observations and Historical Observational Records in Ptolemy's Astronomy', analyzes how Ptolemy interpolated observations from a set of actual observations and how he used and modified observational reports, including Babylonian observations of lunar eclipses and planetary positions. Jones concludes that Ptolemy had limited and indirect access to historical observation records, which rendered them problematic for the interpolation of observations. In particular, the historical observations available to him could not have supplied a record of greatest elongations of the inferior planets adequate for his purposes in the *Almagest*. Zoë Misiewicz's 'Mesopotamian Lunar Omens in Justinian's Constantinople', which derives from her dissertation research at the Institute for the Study of the Ancient World, addresses John Lydus' appropriation of omen-literature descending from the Mesopotamian tradition. Misiewicz argues that Lydus did not have direct access to cuneiform texts or a scholar trained in reading cuneiform; rather, he participated in a shared tradition, where concepts that arose in Mesopotamian omens were adapted to disparate scholarly contexts.

Shenmi Song's and Weixing Niu's 'The Twelve Signs of the Zodiac during the Tang and Song Dynasties: A Set of Signs Which Lost Their Meanings within Chinese Horoscopic Astrology' and 'On the Dunhuang Manuscript P.4071: A Case Study on the Sinicization of Western Horoscope in Late 10th Century China', respectively, examine the Chinese adaptation of Indian astrology. While most scholars examining the 12 zodiacal signs in China have focused their discussion on Chinese star maps, in comparison with their counterparts in ancient Greece and India, Song explores the 12 signs of the zodiac in several sources—Chinese Buddhist scriptures, Taoist scriptures, horoscopic/astrological books, and Dunhuang manuscripts—and shows how this initially foreign astrological concept developed through the Chinese tradition into a mature divinatory system. Niu focuses on Dunhuang Manuscript P.4071, a detailed natal horoscope chart which, Niu argues, demonstrates the sinicization of western horoscopy.

In 'Were Planetary Models of Ancient India Strongly Influenced by Greek Astronomy?', Dennis Duke argues that the lack of any evidence from Indian texts of knowledge of the equant, the geometrical basis of the equant, or the analytical skills to approximate the equant with the four-step method indicates that the four-step method was derived outside of India, most likely in the Greco-Roman empire.

Several of the chapters examine the circulation of astronomical and astrological knowledge within single cultures. These especially include the chapters on the Mesopotamian world, such as John Steele's 'The Circulation of Astronomical Knowledge between Babylon and Uruk', in which he argues that Uruk scribes promoted their self-identity through a process of 'Urukization'. They received astronomical knowledge from Babylon, where the various schemes of mathematical astronomy were developed according to Steele, and attempted to make this knowledge their own. John Z. Wee, in his 90-page 'Virtual Moons over Babylonia: The Calendar Text System, Its Micro-Zodiac of 13, and the Making of Medical Zodiology', discusses the complex interactions of Calendar Texts, related cuneiform astrological tablets, and select astrological and medical features. In 'On the Concomitancy of the Seemingly Incommensurable, or Why Egyptian Astral Tradition Needs to be Analyzed within Its Cultural Context', Joachim Freidrich Quack argues, as the title suggests, for the place of the Egyptian in the study of Greco-Roman Egypt and calls for the examination of Egyptian-language astral texts, a program which Andreas Winkler carries out in 'Some Astrologers and Their Handbooks in Demotic Egyptian'.

The individual chapters, of which there are even more than I have mentioned here, are excellent technical and contextual studies of the ancient exact sciences. To make the chapters on Mesopotamia more accessible to the non-specialist, I would have recommended including maps; and to make Clemency Montelle's 'The *Anaphoricus* of Hypsicles of Alexandria' more convenient for the specialist, I would have included not just the translation of the *Anaphoricus* but also the original Greek. I also would have liked to have seen a list of short biographies of the contributing authors to give some context to their contributions.

Finally, I note that Brill is offering review copies first in digital format rather than hard copy. Although having PDF files of books has proved useful in my scholarship, reading and annotating a nearly 600-page book on a screen is difficult and does not allow for the easy flipping through of a book, where one has a structural memory of where the previous sections are located. Studies on how we navigate various types of text have shown that we have more control over a material book than an ebook, and, for me at least, the hard copy provides the sort of manipulability conducive to writing a review. Furthermore, since many scholars have access to PDF files of Brill books through their university libraries, it would be a shame if we do not receive something additional and tangible in recognition of the work that we contribute in reviewing books.

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Les mathématiques de l'autel védique. Le Baudhāyana Śulbasūtra *et son commentaire* Śulbadīpikā, edited by Jean-Michel Delire

École Pratique des Hautes Études: Sciences Historiques et Philologiques II. Hautes Études Orientales — Extrême-Orient 54.15. Geneva: Librairie Droz, 2016. Pp. xiv + 626. ISBN 978–2–600–01382–6. Paper. USD $144.00

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The work under review is devoted to the translation from Sanskrit into French of a late commentary on an ancient Indian mathematical text, the *Baudhāyana Śulvasūtra*[1] (*BŚl*). This text is dated *ca* 800–400 BC.[2] It opens with a discourse on geometry, possibly the earliest mathematical discourse[3] from India still extant. It continues with applications to the building of structures of very specific shapes required for 'solemn' ritual purposes, by arranging and stacking bricks according to elaborate rules: these are the Vedic altar(s) of the title. There are mathematical constraints on the shapes of the bricks, on the overall shape of the structures, on the number of bricks and the total area that they cover, and on the relation between consecutive layers. The area-constraint in particular requires the elaborate tools described in the opening discourse.

Among Indian texts of the same class, *BŚl* is the most complete and systematic, and in it we recognize ideas that were developed in later Indian mathematics. P.-S. Filliozat states in his preface that '[n]o text, in the immense mathematical

1 Also spelled 'Śulbasūtra'. Thibaut's sectioning of the text into three parts will be used, following established usage.

2 For the arguments, see the introduction of Sen and Bag 1983. We give another, possibly new, argument for relative dating in note 22, p. 126 below.

3 Constructions are prescribed in earlier Indian texts, but they do not seem to have been woven into a connected discourse specifically devoted to geometry, emphasizing mathematical coherence and generality.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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literature in Sanskrit, better shows the originality of Indian Science' [vii–xi], an assessment not inconsistent with current scholarship.[4]

After recalling some of the mathematical aspects of *BŚl* in §1, I summarize the contents of *Les mathématiques de l'autel védique* and relate it to earlier works (§2). A few specific remarks on individual chapters follow (§3). Possibly because this book was written for Indologists rather than for historians of science, the mathematical concepts at work are not analyzed; in fact, the very existence of rigorous mathematical reasoning in India appears to be ignored, or even vigorously denied.[5] The analysis of a typical example shows how essential aspects [§4] were missed by focusing on a commentary that failed to account for the mathematical content of *BŚl*, and by performing incorrect *mathematical transpositions* of the correctly construed text. It seems that this neglect of mathematical issues reflects some aspects of the early historiography of the subject [§5]. The review closes with a summary of the conclusions in a form hopefully useful to historians of science, whatever their area of interest.

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[4] The back cover, however, claims that the

> mathematical skills (*savoir*) of that time [*scil.* the first millennium BC] were comparable to the knowledge (*connaissances*) of civilizations of the same period as to content, but very different as to form, which reveals its oral character.

[5] The quotation opening the chapter entitled 'The Mathematics of the *Baudhāyana Śulbasūtra*' [63] refers to Hindus in general (*les Hindous*) in the following terms that we unfortunately must reproduce:

> I can only compare their mathematical and astronomical literature, as far as I know it, to a mixture of pearl shells and sour dates, or of pearls and dung, or of costly crystals and common pebbles. Both kinds of things are equal in their eyes, since they cannot raise themselves to the methods of a strictly scientific deduction. [Sachau 1910, 1.25]

Delire quotes a French translation of the same judgment [Monteil 1996, 51–52]. Such inflammatory language may reflect the author's fear that an essential preconception is at threat. It could be, in this case, the belief that there is only one type of legitimate (mathematical) discourse.

1. The mathematical content of the *Baudhāyana Śulvasūtra*.

The *Śulvasūtra*s[6] or *Aphorisms of the Cord*[7] deal, as their name intimates, with constructions performed ultimately on the basis of a single cord that defines the unit of length, all auxiliary lengths being derived from it. Constructions are performed on the ground, points being materialized by poles. The cord may be divided into any integral number of equal parts[8] and may receive marks at distinct points. The unit-area is determined by the square of the unit-side. The cord serves the purpose of both (marked) ruler and compass, and also enables one to determine perpendiculars. Symmetry with respect to an axis plays a central role. The isosceles trapezium is the most important figure after the oblong, and seems to be the substitute for the scalene triangle.[9] The primacy of quadrilaterals (preferably symmetric) over trilaterals is still apparent in much later mathematical texts. All figures are ultimately exact transformations of squares, with the exception of the circle, for which rules for approximate quadrature/circulature[10] are given. Thus, any figure is determined by the sequence of operations required for its construction, starting from the unit-cord. Because each figure is defined by such a sequence, the scaling of figures is accomplished simply by changing the unit of length and by going through the same sequence of operations.

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6 Four have been translated: the *BŚl*, the *Āpastamba Śulvasūtra*, the *Kātyāyana Śulvasūtra*, and the *Mānava Śulvasūtra* [Sen and Bag 1983]. They belong to four eponymous Vedic schools, each of which had its own *Śulvasūtra*. These four *Śulvasūtra*s display significant differences. The third is very likely to be much more recent than the first two, and the last may be corrupt. Other texts of this class are described in Michaels 1978, and there is a word-index in Michaels 1983.

7 As Michaels has argued, « śulva », which may mean 'cord' in general, must be taken in this context to refer to the topic, cord-geometry, rather than to the instrument; in fact, the latter is called *rajju* or *spandyā* in *BŚl*, rather than *śulva*. We express this by capitalizing 'Cord'. For an analysis of this and other technical terms, see Michaels 1978, 156–170.

8 In a later section, alternative constructions involving a bamboo rod with holes bored at distinguished points are described [*BŚl* 3.13–15]. The restriction to the cord in the opening section seems, therefore, to be deliberate.

9 An isosceles trapezium is divided by a diagonal into two scalene triangles with the same height.

10 That is, rules for transforming a circle into a square of the same area, and conversely.

Here, number is embedded in geometry through the scalable unit of length. Much attention is devoted to transforming one figure into another without a change of area. Since figures are obtained by area-preserving or scaling transformations, or by starting from squares of prescribed areas, the area of every figure is determined by its very construction. Baudhāyana never uses angles, parallels, or a calculus of fractions.[11] A *scale-calculus* serves as a substitute for the latter [Kichenassamy 2006]. The possibility of carrying out geometric operations without error is taken for granted in *BŚl*, as in Euclid's *Elements* for that matter.

Like most important works of Indian mathematics, the *Śulvasūtra*s are discourses, typically unwritten and meant to be memorized. This feature seems to have been conducive to the abstraction of mathematical concepts, and to account for the absence of diagrams in *BŚl* and all major Indian mathematical texts. Baudhāyana is thought to have introduced the notion of *paribhāṣā* (meta-discourse), a discourse comprising statements that govern the way other statements are to be understood:

> [T]he innovation [of his] that would turn out to be most important, at least through its indirect effects, is that of the *paribhāṣā*,...axioms that must be present in the user's mind.... Baudhāyana may have been the first to introduce *p[aribhāṣā*s*]*, as they seem to play [in his works] a more necessary role than elsewhere. [Renou 1963, §15, 178–179]

The introductory section, *BŚl* 1.1–62, seems to be such a meta-discourse. Units and subunits of measurement are defined first, stressing that some of them may be redefined at will [1.1–21]; this freedom is the basis for the scaling of figures. Next, the text describes how to construct a square, an oblong, or an isosceles trapezium, and a special type of isosceles triangle.

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[11] In other words, at no point is a magnitude associated with the intersection of two lines. Angles do not seem to occur even in later texts [Kichenassamy 2010, 2012a, 2012b]. They are never needed: relations between oblongs or quadrilaterals, or the trilaterals that they contain, provide all the required tools. For instance, the Indian sine and cosine—attested from the middle of the first millennium AD onwards—are obtained by associating to an arc of a circle the sides of the obvious 'right triangle'. The standard argument for the Indian origin of our sine function may be found for instance in Filliozat 1988, 261. As was stressed in the French (Bourbaki) school, the measure of an angle is by no means a primary or elementary notion: it ultimately requires the rectification of an arc of a circle.

Proposition 1.48[12] expresses that the diagonal cord of an oblong makes by itself what the two dimensions[13] of the oblong separately make. In other words, first construct one figure[14] by taking one side of the oblong as unit of length. Then, construct another figure by performing the same sequence of operations with the other side of the oblong as unit of length. Next, produce a third figure using the diagonal cord as unit-cord, with again the same sequence of operations. The conclusion is that the third figure is equivalent in area to the first two figures together. This proposition is applied to the construction of a square with an area equal to the sum (or difference) of two given squares.

These methods of sum and difference are relevant for the transformation of a square into any one of a class of figures without a change of area. Approximate rules for the circulature of a square and its inversion, the quadrature of the circle, are also given [1.58–60]. The meta-discourse closes with a famous approximation of the diagonal of the square [1.61–62] that is accurate to four places (in modern terms); its place here is logical, since it is a consequence of the derivation of the rules for quadrature [1.59–60: Kichenassamy 2006].

The text continues with a detailed exposition of how, on the basis of these general results, one may construct brick structures that may be described as multilayered jigsaw puzzles of precise shapes and prescribed areas. They are often referred to as altars in the secondary literature because of the central place of fire in the ritual. The pieces are square or oblong kiln-fired bricks or subdivisions and combinations of the same.

2. The content of *Les mathématiques de l'autel védique*

As its full title shows, the work under review approaches the text through one of two extant commentaries, designated as *Śulbadīpikā* (*ŚD*), by Dvārakānātha Yajvan. *ŚD* appears to have been composed between AD 1434 and 1609.[15] There is general agreement that the commentator's remarks do

12 *dīrghacaturaśrasyākṣṇayārajjuḥ pārśvamānī tiryaṅmānī ca yatpṛthagbhūte kurutastadubhayaṃ karoti.*

13 Literally, the side-measure and the cross-measure (*pārśvamānī tiryaṅmānī ca*).

14 Possibly a square, but the text does not spell this out.

15 Delire's argument for this dating is as follows [150–160]. It appears that the commentator 'borrowed' from Sundararāja's commentary on the *Āpastamba Śulvasūtra*,

not shed light on Baudhāyana's *modus operandi*. Rather, they illustrate how this *sūtra* was reinterpreted in a particular school, with emphasis on its applications to ritual. *Les mathématiques de l'autel védique* also attempts to draw parallels with other cultures, but no clear structure or hypothesis about transmission emerges from it. The work seems to be intended for Sanskrit readers, as is suggested by the use of the Nāgarī script for the edited text, including the footnotes.

Les mathématiques de l'autel védique is an update of three earlier works:

(a) the edition of *BŚl* and *ŚD*, and the translation of *BŚl* with comments by Thibaut [1875a, b];
(b) the edition by Bhaṭṭācārya [1979] of two commentaries on *BŚl*, including *ŚD*, with a more extensive set of diagrams; and
(c) Sen and Bag 1983,[16] with remarks on commentaries as well as a modern commentary.

It differs from them in three respects:

(a) it takes into account a greater number of manuscripts;
(b) it provides a French translation of the commentary; and
(c) it includes a more complete set of diagrams—in particular, it addresses in some detail the relative position of the various brick structures within the ritual area [42–55]. The diagrams are, of course, an editorial addition.

This volume is an expansion of the author's thesis [Delire 2002] 'elaborated under the supervision of P.-S. Filliozat'.

The first part [1–191] contains four chapters devoted respectively to:

(1) technical and social aspects of ritual [3–61],
(2) the mathematics of *BŚl* 1.22–62 [63–123],

although not in a 'slavish' manner [146]. There are two manuscripts of the latter, one from 1581 and the other from 1588 [150]. Although Datta [1932, 18] considers Sundararāja to be the later of the two commentators, Delire opines with Gupta [1993] that Sundararāja's work is earlier than Dvārakānātha Yajvan's but later than the *Śulba-Vārtika* (1434) by Rāma Vājapeyin. On the other hand, there is a copy of Dvārakānātha Yajvan's commentary that is dated to 1609.

16 See note 6, p. 121 above.

(3) the mathematics of the commentators [125–160], and
(4) the manuscripts taken into account and the editorial choices made [161–191].

The second part [193–363] gives the (French[17]) translation of the text and commentary. It also provides a transliteration of *BŚl* in Roman script. There is no running commentary by the editor in this part.

The third part[18] contains the Sanskrit text [369–515], followed by the editor's diagrams [519–578]. Thibaut's sectioning is used. The 21 sections marked off by Bag and Sen are also indicated in part 2. There is also a further, intermediate sectioning.[19]

A name and place index [581–587], a partial[20] Sanskrit index [598–597], a list of references (works cited and manuscript catalogues [601–613]), and a table of contents [615–620] close the work.

The edition was established by basing the first two parts of *BŚl* on 13 manuscripts, selected from about 30 manuscripts, in addition to Thibaut's edition of the text and commentary [1875a, b], which was itself based on three manuscripts of text and commentary, and a fourth one with the text alone. He did not have access to all of the manuscripts mentioned in the work but gives full particulars including location for all of them. There is no *stemma codicum*.[21] A few emendations for *BŚl* itself are proposed, mostly for part 3 [162–166]. These generally confirm Thibaut's suggestions or correct misprints and 'obvious errors' (« erreurs manifestes ») that are readily detected by carrying out the constructions or the implied computations.

[17] The few peculiarities of Belgian French (such as « nonante » for 'ninety') do not pose any difficulty.

[18] Page numbers in this part are also given a numbering in Nāgarī characters.

[19] To take a typical example, Bag and Sen group Thibaut's 1.29–35 as 1.5. In the volume under review, they form two unnumbered groups: 1.29–31 are listed on three consecutive lines, each preceded by « sū » (for « sūtra »), followed by a paragraph of commentary preceded by « dvā » (for « Dvārakānātha Yajvan »). Then come 1.32–33, similarly grouped together.

[20] As compared with Michaels 1983.

[21] Perhaps the implication is that all manuscripts belong to a single family.

3. Analysis and specific remarks

The title of part 1—'Mathematical Methods in the Architecture of Solemn Sacrifice (*sacrifice solennel*) of Ancient India'—makes the outlook of the work clear. The focus here is on public sacrifices (as opposed to domestic rites) involving brick structures, performed by householders [16] and considered as requiring methods akin to mathematics and architecture. The more complex public rituals are organized by hired experts who act on behalf of the *yajamāna*, whose needs or personal desires are the primary motivation for the rite. The *Śulvasūtra*s are manuals for those experts who may not have the same outlook or desires as the *yajamāna.* Since these rites require larger structures than the domestic ones, they may require greater precision. It appears that the need for precision, together with ritual exactness, was instrumental in the development of a new, more rigorous geometry. Delire refers to Seidenberg's speculation about a possible ritual origin of Greek and Indian geometry [65: see, e.g., Seidenberg 1962]. *Les mathématiques de l'autel védique* also explicitly excludes from consideration the two later stages of life beyond the stage of householder, stages generally associated with the philosophical investigation of the meaning of texts and the reinterpretation of ritual [16].[22]

Chapter 1.1 is entitled 'The Sacrificial Ground'. It contains a description of ritual structures, focusing on their interpretation in the commentary that is translated in this work—for there is some variation among authors—together with a collection of comparisons that have been made in the past with elements of other cultures. A political interpretation of ritual seems to be suggested, perhaps unwittingly: 'When the Vedic nation (*le peuple védique*)[23] settles somewhere, it takes possession of the territory by a sacrifice' [15]. On the same page, we read: '[O]ne of the altars (*foyers*)...symbolizes conquered and managed (*conquis et exploité*) territory.' The question whether those

[22] This would have given an argument for relative dating: *Kaṭha Upaniṣad* 1.1.15 [Radhakrishnan 1953, 601] refers to the introduction of another brick structure, not mentioned in *BŚl*. If it is an innovation, this proves that Baudhāyana's geometry predates the *Kaṭha Upaniṣad*.

[23] The existence of such a well-defined Vedic ethnic or political entity, let alone its bellicose nature, is highly controversial. The existence of similarities between Indo-European *languages* is not. For a recent discussion of this issue, see Demoule 2014.

social aspects were essential ingredients in the emergence of geometry does not seem to be addressed.

Les mathématiques de l'autel védique mentions the existence of patterns involving circles, the intersections of which are the vertices of squares, in the Indus Valley and in Heraklion, suggesting that similar patterns 'most certainly led to' (« ont très certainement débouché sur ») an exact construction of a square in *BŚl* [69–71]. The implied thesis is not clear: Did Baudhāyana create an abstract discourse on the basis of ornamental patterns in order to improve ritual performances? Or is mathematical discourse an outgrowth of solemn ritual, a response to challenges to this ritual. Or is it only incidentally associated with it? There are indeed suggestions that the *Śulvasūtra*s were an outgrowth of the geometry and architecture of an earlier culture, such as the Indus Valley Civilization, or some other with a sophisticated kiln-fired brick technology [Converse 1974; Staal 1999 and 2001]. Whatever its remote forerunners, it appears at the present time that Baudhāyana's approach, by its discursive structure, not only differs from extant texts from other cultures, but also represents a new stage in the evolution of Indian tradition.

Chapter 1.2 is devoted to Baudhāyana's mathematics and presents a translation of the results into modern symbols, together with speculations about their possible origins, collecting some of the opinions that have been put forward in the past. *BŚl* 1.22–62 are termed 'mathematical *sūtras*' (in the title of section 1.2.1), implying that this part of the text qualifies as mathematics while the rest would be ritual. The missing part of the meta-discourse, *BŚl* 1.1–21, is described in the chapter on ritual [§1.1.3]. This part introduces the variability of the unit of measurement, which forms the basis of the scaling of figures in *BŚl*. Delire does recognize in it 'a principle of proportionality enabling one to construct objects similar to others by simply adjusting the base measure' [19], suggesting that this part, too, is mathematical. It is true that the commentators also missed most of the mathematical issues and did not realize that their own conceptual framework differed from Baudhāyana's.

This chapter also contains a collection of some of the earlier suggestions about the possible derivation of Baudhāyana's results. The author mentions Piaget's analysis of the stages of learning observed in some children as a possible model for the evolution of Indian mathematics, and reads earlier derivations based on dissection methods in this light [90 ff.]. But Baudhāyana is working within a complex tradition that he has already assimilated; we are not dealing with the infancy of mathematics but with its coming of age. Ancient mathematics does not seem to have been performed by children, even in the remote past. Also, Piaget's *praxis*-driven model, as presented by Delire, does not account for the discursive dimension of *BŚl*. Mention of dissenting views on these controversial issues, such as those of Chomsky or Lacan, would have been welcome.

Chapter 1.3 is devoted to 'the commentators' mathematics'. Their results seem to have been obtained by using the methods that have been standard in India since Āryabhaṭa (AD 499). This chapter records inconsistencies 'certainly to be attributed' to borrowings from other sources, without double-checking [144]. It closes with a detailed comparison of parallel passages in the commentary edited here and with Sundararāja's commentary on the *Āpastamba Śulvasūtra*, leading to Delire's proposed timeframe for the commentary [150–160].[24]

Some aspects of the translations may be misleading to the non-specialist. Some of them are perhaps due to carelessness and have the effect of hiding conceptual problems from view. Here are three examples.

(1) The archaic term « praüga » for the isosceles triangle obtained from a square by joining the middle of the top side to the ends of the lower side is translated by 'triangle' [*BŚl* 1.56: 208]. Now, words equivalent to 'triangle' or, more precisely, 'trilateral' (« tribhuja ») are absent from *BŚl*;[25] so is the very notion of a scalene triangle.

(2) Single terms are not always translated uniformly: « pāśa » is translated by « boucles » ('loops') in 1.27 and in the commentary to 1.30, but by « noeud » ('knot') in 1.30 itself. The technological issue is how, given a cord of known length, one may fit loops, or perhaps nooses, at its ends in such a way that, by stretching the cord between two poles, one is guaranteed that the distance between them is equal to the length of the original cord. Knotting a cord slightly reduces its length. Such points confirm the lack of emphasis on practical issues in *BŚl* that were perhaps to be left to the care of specialized staff. Similarly, « vidha » is translated as « unité » ('unit') and as « sorte » ('kind', 'type') [see *BŚl* 2.11–12, 2.14]. Bag and Sen translate it as 'fold'

[24] See note 15, p. 123 above.

[25] According to Michaels 1983.

because, for instance, «saptavidha» means sevenfold: it qualifies the figure obtained from a given one by increasing its area sevenfold. This technical term reflects the conception of scaling of figures by the mere change of the fundamental cord [see §2, p. 123 above]. The translation of «tiryaṅmānī» and «tiraścī» for a transverse dimension [1.54, 3.281]) as «transversale» is also misleading because of the existence in modern mathematics of the 'théorie des transversales', in which a *transversale* is a line that cuts *through* several others. On page 81, Delire had correctly translated the first of these words as «mesurée en travers» ('measured across').[26] Readers already familiar with the subject will hopefully make the necessary adjustments.

(3) The very first line of the commentary is a prosternation to Gaṇeśa («śrī gaṇeśāya namaḥ»: «śrī» is honorific). In the translation, this clause is moved *after* 1.1 and translated approximately by 'Glory to Gaṇeśa'. It is a prosternation and not praise; and it is essential that it should come *first* since it is a standard way for authors to ward off, at the outset, obstacles of any kind that might arise in the course of the work.

We now turn to the basic questions outlined in the introduction about the neglect of the conceptual and discursive dimensions of the text.

4. The problem of mathematical transposition

4.1 *An example of mathematical transposition*

As a typical example of transposition in *Les mathématiques de l'autel védique*, consider Baudhāyana's rule [1.59] for the (approximate) quadrature of the circle. We read:

> Let us note at the outset that Dvārakānātha [the commentator] did not feel any difficulty in understanding Baudhāyana's quadrature. Indeed, he transforms the fraction[27] $1-\frac{28}{8\times29}-\frac{1}{8\times29\times6}+\frac{1}{8\times29\times6\times8}$ —*for this is indeed how* sūtra *(I.59) is to*

[26] An oblong constructed symmetrically with respect to an axis has two dimensions, one along this axis, the other one across it.

[27] Here and in the next sentence, the wording is ambiguous. The French verb used is «comprendre»; it can mean 'to understand' or 'to comprehend'. The commentator construed the sentence correctly in the mere grammatical sense, but he did not comprehend it, as we shall see.

> *be understood* [my emphasis]—into $\frac{7}{8}+\frac{1}{8}\left(\frac{41}{1392}\right)$, then further into $1-\frac{1}{8}\left(\frac{1351}{1392}\right)$, thus showing his mastery of the calculus of fractions, even [when they are] not unit[-fractions].[28] [142]

The implication is that

(a) Baudhāyana's text may be written in a form in which a possible allusion to 'Egyptian fractions' is apparent, thus introducing unit-fractions that are not in the text; and
(b) since the commentator could handle general fractions, there is no need to investigate whether Baudhāyana worked with this concept.

However, point (a) is incorrect: this is *not* how the *sūtra* is to be understood. To see this, consider Thibaut's translation of 1.59—the way in which Thibaut construed the text has never been challenged, not even in the volume under review, since the Sanskrit is quite clear. His translation reads:

> If you wish to turn a circle into a square, divide the diameter into eight parts and one of these parts into twenty-nine parts: of these twenty-nine parts remove twenty-eight and moreover the sixth part (of the one part left) less the eighth part (of the sixth part). [Thibaut 1875b, 1.59]

Taken literally, and with the same notation as *Les mathématiques de l'autel védique*, the text would correspond to the expression:

$$1-\frac{1}{8\times 29}\left(28+\frac{1}{6}\left(1-\frac{1}{8}\right)\right).$$

Thus, in terms of fractions, one would have to deal with a compound expression of which the numerator could itself be a fraction—in no sense is this mathematical object a sum of unit-fractions. Now, there is general agreement that a general calculus of fractions with reduction to the same denominator is not attested at this time. And all attempts to account for 1.59 by means of a calculus of fractions lead to inconsistencies [Kichenassamy 2006]. The question is: What mathematical tool, possibly absent from modern mathematics, was used by Baudhāyana in those situations where *we* would be tempted to use general fractions or 'Egyptian' fractions? The work under review and the commentary missed this question because they performed

[28] A unit-fraction is one of the form $^1/n$, where n is integral. Calculations with aliquot parts are found in Egyptian mathematics; hence, the name 'Egyptian fractions' for expressions involving only sums of unit-fractions.

an *incorrect mathematical transposition* on top of the unproblematic literal translation.[29] This transposition made it impossible to see the problem. Recall that, according to the back cover [cf. note 4, p. 120 above], the author considers that all works of the same time frame are essentially similar in content. The mathematical transposition is driven by the illusion that the text must involve unit-fractions.

Now, the mathematical object involved in 1.59 is *not* a combination of fractions such as $^{13}/_{15}$, even though it is determined by pairs of numbers such as (13, 15). One may think of each of them as a 'pairs of divisors', in which none of the elements is distinguished as the numerator. Such pair express a correspondence between lines or, rather, (portions of) cords [Kichenassamy 2006, 2011]. For instance, 1.60 states: 'after having made fifteen parts, remove two'. That is, to 15 parts of one cord correspond 13 (15 – 2) of another. This pair is not a fraction because the two numbers play symmetric roles. If there is only one such pair, it is readily inverted *without* reference to fractions. In this case, it suffices to divide the latter cord into 13 parts and to *add* two of these parts to recover the length of the first cord.

More generally, two cords, a and b, would be related by giving a pair p, q of divisors if the following holds: if one divides a into p parts, then q of them make up b. And if one divides b into q parts, then p of them make up a. If we read the text closely with this idea in mind and remember that the unit or length may be redefined in the course of the argument, we see that the text lists, in a remarkably compact yet transparent way, the steps of a derivation of 1.59 and of the following few propositions, using only tools attested in the text [Kichenassamy 2006, 172–180]. This derivation differs from all those proposed so far, and it cannot be recovered by mere transposition from some modern derivation. It accounts for the very specific numbers in the text, as well as the order of the words in the sentence, and is, to date, the only one that accounts for the text as it is.

Thus, *Les mathématiques de l'autel védique*, by relying on the commentary, is affected by the belief that mathematical transposition may be made without

[29] Thibaut also performed this mathematical transposition, although he did point out some of the anachronistic aspects of the commentary.

loss of content. However, transposition is by no means tautological.[30] That Indian commentaries make use of a form of transposition does not make it legitimate in historical work. Change of notation, however, can be harmless provided that the operations performed on the new symbols reflect those of the text.[31] Modernized notation becomes dangerous only when it suggests relations that could not have been suspected without it.

4.2 *Is mathematical transposition unique?*

It has been argued[32] that mathematical transposition is nevertheless a legitimate tool in the analysis of mathematical texts, not only because it has been performed in some ancient texts, but because it is allegedly the only way to make sense of a text. To our knowledge, the only example on this score is the algebraic interpretation of four 'lost' books of Diophantus in Arabic sources of the late ninth century, in which Diophantus is turned into al-Khwārizmī's 'heir' (« successeur »)[33] (*sic*). This text was further reinterpreted in terms of 20th-century algebraic geometry, occasionally requiring spaces of more than three dimensions. Mathematical transposition is claimed in this case to be not only convenient but necessary because it is *unique*. But in fact, it is not. This transposition requires the introduction of several unknowns not attested in the text, but we know that Brahmagupta (in the seventh century) introduced several literal unknowns. Moreover, we find, for example, in a ninth-century commentary,[34] an equation with six unknowns labeled by letters (*yā, kā, nī, pī, lo, ha*) that are the initials of a conventional set of words

30 Transposition may be useful in the study of mathematical problems to gain new insight, but becomes objectionable when it leads to attributing one's own ideas to someone else.

31 An example is provided by the introduction, in the analysis of *BŚl* 1.59 above, of the pair-notation for the benefit of the modern reader. The derivation in Kichenassamy 2006, however, does not use it and does not introduce other symbols.

32 We thank Karine Chemla for bringing this problem to our attention. Chemla 1986 gives an overview and is careful not to jump to conclusions.

33 Chemla 1986, 368.

34 Colebrooke 1817, 355 *et pass*. See also 139n1 for details on this multi-literal algebra and its development.

and bear no connection to the quantities represented.[35] Thus, a literal algebra with several unknowns, unrelated to the conception of a space of more than three dimensions, is attested at the same time as our Arabic text. We must, therefore, wonder, regardless of any possible hypothesis about transmission, why one particular transposition was preferred by some modern readers to another. At any rate, this proves that mathematical transposition into 20th-century mathematics is not the only possible transposition. We also see in this example that appropriation through mathematical transposition is by no means a recent phenomenon.

5. Other reasons why conceptual issues in Indian mathematics were neglected

The belief that mathematical transposition is harmless fosters the feeling that texts do not constrain our readings of them, that internal analysis is not necessary. Leaving aside prejudice and disregard of axiological neutrality, there seems to have been three further reasons for the relative dearth of textual analyses of Indian texts in their own terms:

(1) the existence of undetected errors in the texts,
(2) the (related) assumption that results found in Indian texts were derived from unacknowledged sources, and
(3) the belief that ancient mathematical discourse may be understood on the basis of much later sources of the same tradition.

I examine them in order.

[35] ‘Letter’ here translates « varṇa ». This word also means ‘color’, hence, the use of the initials of names of colors, as here. Other lists of letters as symbols are also attested. Those letters are further analyzed into phonemes in Indian grammars, but this is not relevant here.

(1) The existence of errors[36] propagated by commentaries suggested that some results

> were handed down as received truths, with the result that incorrect theorems were not identified as a matter of routine by any student who checked the proofs. [Bronkhorst 2001, 54]

Some commentaries were blamed for striving to justify the incorrect ones [Bronkhorst 2006]. However, undetected errors and ideologically driven discourses are not unheard of, even in modern mathematics. The issue is, therefore, whether such commentators are representative of the entire tradition and, indeed, whether there may not have been several mathematical cultures in India.

(2) It was assumed that Indian mathematics was influenced by Hellenistic mathematics, which may be true to some extent for late authors, just as Indian mathematics influenced other cultures. Hellenistic influence[37] on genethliacal astrology is documented and acknowledged in the texts, but interpretative astrology—the subject of a vast literature in India as elsewhere—does not seem to be discussed at all in mathematical texts. Also, the absence of the notions of angle and parallel in India shows that, for instance, the conceptual framework of Brahmagupta's geometry (AD 628) does not seem to have a counterpart in other cultures. The transmission hypotheses formulated so far do not seem to account for Brahmagupta's text. More generally, it is essential to refrain from speculating on issues of transmission before the content of the texts has been thoroughly studied. Issues of priority must not become a priority.

[36] A famous example is Āryabhaṭa's rule that appears to give an incorrect formula for the volume of the sphere [*Āryabhaṭīya* 2.7]. The error was not spotted in the oldest extant commentary, by Bhāskara I (AD 629, translated in Keller 2006, 1.xxxii-xxxiii): Keller points out that the commentator seems to work with a faulty version of the text [2006, 1.35nn209–210]. Since there is an ingenious way to make sense of the passage [Elfering 1975, 71–76], we must conclude that the commentator missed the error *and* failed to propose a mathematically correct reading of the text, even though one was possible.

[37] Probably before the seventh century AD. The date and nature of this influence have recently been reexamined in a more rigorous scrutiny of the sources. See Mak 2013; Filliozat 2016.

(3) Since ancient Indian mathematical texts were preserved faithfully by tradition to this day, their meaning may perhaps be inferred from late commentaries. However, this is not always warranted. To take an example, the existence of several schools with non-equivalent conceptual frameworks[38] is indicated by a passage in which Bhāskara II (12th century) criticizes Brahmagupta's formula[39] for the diagonals of a cyclic quadrilateral as unnecessarily complicated. He gives a simpler formula that does not, however, apply to all the cases covered by Brahmagupta's [Colebrooke 1817, 80–81]. It seems established [Kichenassamy 2012b] that there were partial breaks in the continuity of the Indian mathematical tradition, so that texts were passed down to further generations but their conceptual framework or the associated *modus operandi* was partially lost in the process.

6. Conclusion

Les mathématiques de l'autel védique is a contribution to the study of an important text, the *Baudhāyana Śulvasūtra*, and will be of interest to those Indologists already familiar with the basic texts of ancient Indian mathematics and the issues that they raise. However, the very existence of rigorous mathematical reasoning in this text is not apparent in this study because Delire focuses on a late commentary that failed to address conceptual issues, introduced mathematical transpositions in terms of a much later framework, and did not account for the text itself.

We attribute this state of affairs to two main causes. First, the *Baudhāyana Śulvasūtra*, while an apodeictic discourse, is not dogmatic: it requires the reader to think with the author rather than to be submissive. Second, there were partial breaks in the mathematical tradition: the conceptual framework of one school was forgotten while its texts were passed down; its results

[38] The existence of two distinct schools in India—one that deals exclusively with cyclic quadrilaterals; another that never considers them—seems to have been first clearly singled out as a fundamental issue in Sarasvati Amma 1999, 81.

[39] Many Indian texts describe in words general formulae—for the determination of lengths, areas, or volumes for instance—where variables are represented by words, as is appropriate for versified texts. The existence of separate names for parts of a figure makes the correspondence with modern formulae unambiguous. This system coexists with literal or symbolic algebra among authors who also deal with the theory of equations.

were thus fitted to the Procrustean bed of another school, resulting in inconsistencies that indirectly cast a shadow on the original works.

However, the correct conceptual framework of the *Baudhāyana Śulvasūtra* may be understood by textual analysis because the text was composed with great care. Insofar as text and context are correlated in this case, internal analysis provides strong evidence for the context that is more reliable than second-hand information. And the *mathematical coherence* of this text is a very strong constraint on its reading, as it is for the reading of any mathematical text. The notion of apodeictic discourse that includes all forms of rational argumentation to establish a result within a shared framework seems relevant to to the analysis of texts from other cultures as well.[40]

The following conclusions appear to be of relevance to the analysis of all cultural areas.

(1) *Mathematical transposition* from one conceptual framework to another is a form of tampering with the text. By contrast, *transcription* into modern notation is sometimes admissible, provided that the operations permitted are never lost sight of, and may help communication with modern readers.
(2) *Priority is not a priority.* Transmission or issues of priority should not be discussed before analyzing and understanding the texts themselves.
(3) Consistent scientific discourse, ancient or modern, takes the form of an *apodeictic discourse* that need not take a deductive form, unless one wishes to suppress motivation and stress verification.
(4) There may be *mathematical pluralism within a culture*.[41] In particular, a text and a commentary on it may not share the same conceptual framework. Any plural tradition will perforce appear incoherent or inchoate at best, if one attempts to interpret individual differences as forms of variability within categories implicitly taken as universal.

[40] See Kichenassamy 2015 for an application to an Italian text of the Renaissance.

[41] For a very recent example of pluralism, see Chemla 2016, 2018. She points out the lack of definition of the term 'mathematical cultures' [Chemla 2016, 1]; the notion of conceptual frameworks may provide a useful substitute.

The analysis of mathematical discourse, guided by the demands of the internal mathematical coherence of each individual text and strict axiological neutrality, is similar to ordinary communication: other peoples' discourses are seldom entirely transparent and are understood through a process of gradual adjustment, provided that we accept that we do not know beforehand what others mean. It is possible to understand others without becoming similar to them or forcing them into assimilation. In this sense, the process of analysis advocated here provides a framework for the understanding of diversity.

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The Attitude of the Medieval Latin Translators towards the Arabic Sciences by José Martínez Gázquez

Micrologus' Library 75. Florence: SISMEL—Edizioni del Galluzzo, 2016. Pp. 213. ISBN 978–88–8450–694–8. Paper €48.00

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Martínez Gázquez's book is dedicated to the Arabic-into-Latin translation movement, one of the most intriguing medieval intellectual phenomena. Even if some isolated translations were realized by the 10th century, it is in the 12th century that a translation 'movement' arose, made by dozens of translators who went to Spain and Southern Italy to make available to the Latins 'the knowledge of the Arabs'. This process found its main development in the Iberian Peninsula—the geographical space on which this volume is focused.

The phenomenon of Arabic-into-Latin translations, an object of conspicuous interest in recent times, had pivotal effects on medieval and early modern scientific speculation. On the one hand, the arrival of a new scientific and philosophical corpus widely opened the Latin gaze to new fields of knowledge, providing its debates with new texts, theories, problems, and approaches that would completely reshape medieval culture in general. On the other hand, the translations also excited mixed and often opposed feelings. The eagerness of the translators and the first scholars receiving these doctrines was often counterbalanced by criticism and suspicion about the infidel authors, their theories, and the 'new' sciences that emerged from the arrival of texts whose disciplines, such as alchemy, were completely unknown to the Latins. Thus, the phenomenon of these translations was marked by antithetical perspectives, thus following the history of medieval Aristotelianism through the condemnations of 1210 and 1277.

Readers in search of a comprehensive study of the complexities of the Arabic-into-Latin translation movement will find Martínez Gázquez's book surprising. It is both less than that and—above all—more. Indeed, the author's approach is extremely peculiar: rather than analyze the activities of the translators and their scientific context, Martínez Gázquez gives the very translators,

ISSN 1549–4497 (online) ISSN 1549–4470 (print)

philosophers, and commentators leave to speak. The book is an anthology of fascinating excerpts from the prologues and dedicatory letters preceding the translated texts. There, the translators were free to express their points of view and their feelings about the revolutionary work that they were undertaking in order to renovate Latin science and philosophy. At the same time, this anthology gathers other kinds of texts related to the translation movement: historical accounts of the presence of Islam in Spain and the multicultural context of Toledo, the capital of Castile, as well as texts witnessing criticism against the translators and Muslim people in general. From this perspective, then, Martínez Gázquez's book is a precious and original contribution, a handy instrument of both reference and divulgation, comprehensible to a scholarly as well as to a non-specialist public.

However, all good things come with a price. In the case of this brilliant collection of historical witnesses, that price is the absence of a systematic discussion of the so-called 'translation movement'—the bigger picture in which the textual witnesses presented by Martínez Gázquez are historical instantiations. The reader might be somewhat disoriented by the rapid succession of excerpts and texts, each one briefly presented and preceded by a possibly too-concise general introduction, the short length of which entails some superficiality in the approach to the manifold problems and complexities of the Arabic-into-Latin movement (a limit which is mirrored by the out-of-date bibliography of scholarly studies).

Nonetheless, Martínez Gázquez's work is sublime and far-reaching. Its usefulness is particularly evident in the structure of the book, which comprises five interconnected thematic sections. After a general introduction, Martínez Gázquez focuses on the translators (§2: 'The Translations from Arabic to Latin'), presenting 40 excerpts from the prologues and dedicatory letters of works by translators and medieval scholars. From Alvaro of Cordoba (ninth century), to William of Conches and Dominicus Gundissalinus (12th century), up to Ramon Llull and Arnald of Vilanova (13th century), the sources gathered leave the reader with a taste of the sparkling context in which the translations were realized. Especially remarkable is the choice to center the reader's attention on the translators' consideration of Latin culture (characterized by a decaying backwardness) and the science of 'the Arabs', whose 'vases of gold and silver'—following Augustine's theory of the sacred theft, a *topos* at that time—had to be taken over by the Latins in order to establish a new foundation of Christian science. These texts are joined in §6 ('Castilian Texts') by further accounts of the key role played by the Arabic translations in a later period of Spanish history. Martínez Gázquez, indeed, presents interesting excerpts, in both Spanish and Latin, from the court of Alfonso X, the Wise, as well as by John of Capua, Jiménez de Cisneros, and Miguel de Cervantes, making clear that, while the apex of the translation movement was reached at the end of the 12th century, its effects and general curiosity about Arabic knowledge did not disappear from Iberian society.

Sections 3 and 5 of Martínez Gázquez's book describe, from two slightly different perspectives bearing on the socio-cultural peculiarities of the geographical context in which the translators operated, the Iberian Peninsula. Section 3, 'The Importance of Spain', presents some historical documents witnessing the relevance of the peninsula for the establishment of this translating effort. Indeed, the Iberian Peninsula—and for similar reasons, the other locus of the Arabic-into-Latin translations, Southern Italy—was characterized as a cross-cultural melting pot. This multicultural society is one of the key factors explaining the emergence of the translation movement in the 12th century, when the Latin kingdoms of the peninsula hosted many Islamic and Jewish learned people fleeing from the 'Almohad revolution' in al-Andalus. Many of these refugees arrived in Toledo, the Castilian capital, to which is dedicated §5 of the book ('Toledo, the Medieval City of Knowledge'). Since the Christian conquest of that town in 1085, Toledo became one of the most relevant economic and political centers of the peninsula. In the second half of the 12th century, the town also became the main center of the translation movement. It is in Toledo, in fact, that the three most important medieval translators from Arabic into Latin—Gerard of Cremona, Dominicus Gundissalinus, and Michael Scot—worked on hundreds of scientific and philosophical writings. Martínez Gázquez provides the reader with an enveloping series of texts witnessing the cultural splendor of Toledo, the town which, supposedly, Daniel of Morley preferred to Paris for the study of the Arabic science held there (an account that is, to be sure, extremely problematic). To these texts, the author also adds further interesting documents witnessing a 'different' Toledo, such as that given by Cesarius of Heisterbach, who refers to an imaginative story about the 'school of necromancy' there.

Finally, §3 ('Criticism of the Translation Process') offers important excerpts witnessing the other face of the Latin attitude toward the Arabic translations, in a sort of counterbalance to the perception given throughout the other sections. The author presents some examples of harsh criticism against both the translations—here the reader can find Roger Bacon's passionate attacks against the errors made by the translators—and the persistence of Islamic people in the peninsula. This latter aspect would lead to one of the most reprehensible pages in the history of Spain: the banishment of Jewish and Islamic people at the end of the Middle Ages and into the early modern period.

Martínez Gázquez's *The Attitude of the Medieval Latin Translators towards the Arabic Sciences*, therefore, offers a vivid description of the translators' self-understanding and their social and cultural framework. It is a precious book—a handy item for intriguing reading as well as for postgraduate teaching. At the same time, it is for the reader, whether a specialist in medieval studies or someone interested in the overall process of the cross-cultural dissemination of knowledge, an invitation to problematize the *vulgata*—in both its 'positive' and 'negative' narratives—regarding the relationship among Christians, Jews, and Muslims in the Middle Ages, and to go back to the texts, letting the medieval authors start a dialogue with us.

Aristotle on Knowledge and Learning: The Posterior Analytics by David Bronstein

Oxford: Oxford University Press, 2016. Pp. xiii + 272. ISBN 978–0–19–872490–2. Cloth $74.00

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The flood of attention paid to Aristotle's *Posterior Analytics* in recent years has led to much progress in our understanding of this challenging and important work. Among other advances, old concerns that the work lacked internal consistency and was irrelevant to, or even in conflict with, other parts of the corpus have largely been left behind. No longer do scholars ask if the account in book B of the 'things we seek' is inconsistent with the theory of demonstration in book A, or if the theories of the *Post. An.* were for some reason abandoned prior to Aristotle's scientific investigations (or perhaps were formulated only after his empirical investigations were completed). Much good work has shown how the parts fit together into a consistent theory of unqualified scientific knowledge (ἐπιστήμη ἁπλῶς), and how the theory actively shapes Aristotle's practices in treatises ranging from the physical and biological to the ethical and metaphysical.

To be able to say that the different parts of the work are consistent and that the *Post. An.* influences other areas of the corpus in specific ways constitutes definite progress. But scholarship has paid much less attention to the question of what might be called the internal dynamics of the *Post. An.* itself: how one part of Aristotle's presentation leads to the next. Granting that the account of demonstration is consistent with the theory of definition, are there philosophical grounds according to which the exposition of one account motivates and moves toward the other; and if so, does the latter lead in a similarly sequential way to the concluding chapter on induction (ἐπαγωγή)? Can the work as a whole, in other words, be read as something more than a desultory collection of treatments of topics? Is there an overall plan?

These, in effect, are the questions that David Bronstein seeks to answer in his book *Aristotle on Knowledge and Learning: The* Posterior Analytics.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 145–153

Bronstein approaches Aristotle's *Posterior Analytics* as a 'coherently and elegantly structured work' [3] organized around the two themes announced in his title, knowledge and learning. According to Bronstein, there are three distinct types of learning for Aristotle: learning by demonstration, learning by definition, and learning by induction. Not only does each type yield a different kind of knowledge and occupy a different moment in an Aristotelian scientific inquiry, there is a logical priority among the three types of learning that stands behind Aristotle's order of presentation in the *Post. An.*

Aristotle begins with the expert scientist, who learns through demonstrations (the subject of *Post. An.* A). He then moves 'backwards' to the two types of prior learning that must be undertaken if one is to become an expert in the first place, both involving the acquisition of non-demonstrable principles. Demonstration depends on definitions. Therefore, learning by definition must precede learning by demonstration. This kind of learning is explored in the first 18 chapters of *Post. An.* B. But definitions in turn depend on acquiring knowledge through induction of preliminary accounts of the things to be defined. Learning by induction is the subject of the work's final chapter, B.19. Bronstein contends that each stage of the unfolding exposition is a progressively deeper exploration of the epistemological foundations of scientific knowledge.

As is appropriate given his thesis, Bronstein follows (for the most part) Aristotle's order in the *Post. An.* rather than, say, assembling passages from different parts of the work according to topic (e.g., on the different types of principles). And despite his concern with the composition of the whole, he does not discuss every chapter of the *Post. An.*: many chapters in the first book, for instance, receive little or no attention. Nor does he raise every issue pertaining to the passages that he does discuss (for example, the relationship between Aristotelian principles and Greek mathematics). This, in other words, is not a commentary. Instead, Bronstein concentrates on those chapters most crucial to making his case—mainly A.1–4, B.1–10 and 13, and B.19—drawing in passages from other chapters as needed, often in footnotes.

The book is laid out over 13 chapters divided into three 'parts'. These follow a substantial introduction that includes an extended discussion of the Meno paradox as a background to Aristotle's concerns. The three main parts are of quite unequal length. Part 1 devotes 35 pages across three chapters to the question of learning by demonstration. The bulk of the book is formed by the eight chapters and almost 160 pages of part 2, 'Learning by Definition'. The concluding part 3 treats learning by induction in a single chapter of just over 20 pages, devoted essentially to *Post. An.* B.19. As its much greater length suggests, part 2 contains the linchpin of Bronstein's thesis, though none of the parts (including the introduction) is without interest or controversial claims.

Part 1 considers learning by demonstration. Against the general run of recent scholarship, Bronstein argues that it is indeed possible to acquire new knowledge through demonstrations, and that both the expert and the student do so, though in different ways. His claim that the expert acquires new knowledge in this way is probably the more controversial. In some cases, the expert deduces new conclusions from known premises. In other cases, the expert is able to grasp an explanatory connection between facts already known. In this case, the demonstration is literally a 'showing forth' of an explanation. In both cases, the expert learns by moving not from ignorance to knowledge but from one sort of knowledge to another. Bronstein is intent on saying that this does not make demonstration a method of discovery. Rather, the theory of demonstration gives an account of what Bronstein calls the 'culminating moment' of the process of discovery. That process is the main subject of *Post. An.* B.

Also iconoclastic is Bronstein's account of non-demonstrative scientific knowledge. He argues at length in his fourth chapter that this kind of scientific knowledge is obtained by νοῦς of the definitions that form the starting points of demonstrations. In other words, the definition of scientific knowledge in A.2, 71b9–12 applies to both demonstrative and non-demonstrative (i.e., noetic) knowledge, not just to demonstrative knowledge as is typically maintained. This offers the attractive option that νοῦς—Bronstein leaves the term untranslated—is the non-demonstrative knowledge hinted at in the opening lines of A.2, and that it centrally involves knowledge of explanatory definitions, specifically of subject-kinds and their essences.

Part 2 takes up the question of how we acquire knowledge of definitions, the most important kind of scientific principles. Bronstein offers an extended analysis of inquiry in *Post. An.* B. This, by far the longest portion of *Aristotle on Knowledge and Learning*, contains two crucial claims. First, Bronstein contends that we learn principles through definition, not through induction as is typically supposed. Second, he maintains that learning by definition encompasses several more specific methods. To signal the shift from learning by demonstration to learning by definition and the difference between the prior knowledge involved in each and what is discovered as the result, Bronstein switches from speaking of the expert (as he does consistently in part 1) to speaking of the inquirer, implying that inquiry must be largely completed before one becomes an expert able to engage in demonstration in a scientific field. Through demonstration, the expert's knowledge of the explanatory power of a definition is deepened. In the case of inquiry, the inquirer searches to learn a new definition.

Here I found most interesting his careful delineation of different methods of inquiry in B.8–9 and B.13 depending on the differences between the types of definable entity. Bronstein does an admirable job of unraveling Aristotle's dense and confounding talk in B.8–9 of 'causes that are the same' and 'causes that are different', and takes quite seriously Aristotle's claim in B.8 that, while no definition cannot be demonstrated, a definition of a certain sort can be made clear by demonstration. Demonstration thus becomes the method for seeking definitions of demonstrable attributes. At the same time, division becomes the method by which the essences of species are defined. So, rather than being flummoxed by the apparent inconsistency between B.8–9 and B.13, Bronstein finds a compelling way to make them consistent.

Part 3 turns finally to *Post. An.* B.19 and the account of induction. It is through learning by induction, Bronstein argues, that we acquire not a knowledge of definitions but the prior knowledge of the genera on which definitions are based. These are the entities a prior knowledge of which is necessary for learning definitions as described in part 2. In other words, just as learning by demonstration depends on a prior learning of definitions, so too learning by definition depends on a different kind of learning made possible by induction. Thus, Bronstein is able to identify the philosophical basis for the presentation of the *Post. An.*

Central to Bronstein's interpretation of the *Post. An.* is the claim that all of the work's main concerns point to the influence of Plato, particularly issues brought forward in the *Meno*. This is probably not controversial at least at a general level, and it is certainly no longer equivalent to saying that the *Post. An.* is, therefore, an immature product of Aristotle's academic period. Bronstein develops this theme in two stages. The initial stage takes up the first numbered chapter of Bronstein's book and occupies the majority of the introductory section preceding part 1. It examines the *Meno* paradox both in its own terms and in connection with Aristotle's only explicit mention of it in the *Post. An.*, in A.1.[1] The second stage amounts to a running engagement with the implications of the paradox for inquiry throughout Bronstein's part 2, including, but not limited to, a chapter (the eighth) devoted to 'The Socratic Picture of the Order of Inquiry'.

Bronstein's tracing of issues in the *Post. An.* to the seemingly aporetic *Meno* strikes me as one of the major accomplishments of the book. This is in contrast to his offhand remark linking Aristotle's work to the *Republic*, when he calls the *Post. An.* 'Plato's allegory of the cave told in reverse'. (Surely the cave stands for the political community much more directly than it does for the scientific community. I shall return to the *Republic* and other Platonic dialogues shortly.) This part of Bronstein's interpretation can be judged independently of his larger view of the *Post. An.*'s unfolding structure, though it stands, of course, as a major feature in that view. Bronstein shows how in book B and especially in its first 10 chapters Aristotle frequently deals with issues going back to the *Meno*: the priority of the question τί ἐϲτι;, investigating a thing's attributes, the need to grasp at least hypothetically something of what a thing is if inquiry is to proceed, and the importance of questions pertaining to the relationship between a kind and its varieties.

Given the strength of his case connecting the *Post. An.* and the *Meno*, one cannot help but notice that possible connections with other seemingly relevant Platonic dialogues are not discussed. There is no mention of the *Phaedo*, in which the method of hypothesis receives a more systematic account than the one given in the *Meno*. Besides the questionable connection to the allegory of the cave, there is only one fleeting reference, also in the introduction [8], to the Divided Line. This image seems to me to be at least as relevant to Aristotle's theory of science as the problem of inquiry in the *Meno*, particularly with regard to possible influences and connections between Aristotle's ἐπιϲτήμη and νοῦϲ and Plato's διάνοια and νόηϲιϲ. Similarly, despite Bronstein's identifying division as one of the most important ways Aristotelian definitions are obtained, and despite Aristotle's criticism of Academic division in B.5

[1] Bronstein consistently speaks of 'Meno's paradox', even though it is Socrates whom Plato has develop the paradox in its full precision and potency out of his interlocutor's lazy attempt to end the inquiry in numbed *aporia*. Bronstein is not, of course, alone in this tendency.

and endorsement of a modified form of division in B.13, none of the Platonic dialogues featuring collection and division is considered except for one footnote late in the book. As for other Academic proponents of division, who in some cases were Aristotle's primary targets in these chapters, Speusippus is mentioned once and Xenocrates not at all.

My criticisms on this point may be unreasonable. I may be asking for a different book than the one Bronstein chose to write, one that would concentrate on the multiple influences of Plato and the early Academy on Aristotle's account of knowledge and its acquisition. Nevertheless, Plato was at least as concerned as Aristotle with the internal dynamics of the acquisition of knowledge—epistemological, psychological, and certainly the expository—and several dialogues besides the *Meno* are relevant to that inquiry. Bronstein's not exploring other aspects of Plato's influence strikes me as a lost opportunity, as it leaves incomplete the picture of what the *Post. An.* was drawing on and responding to.

Another lost opportunity pertains to terminology, though I think it reflects a more fundamental issue. At one or two points, Bronstein seems to prefer the phrase 'learning by defining' in place of 'learning by definition' [4, 70–71], claiming that the former phrase better reflects the process of defining something and, thus, the act of learning. This is a promising idea, but Bronstein makes little use of it. In almost all cases, he sticks with the more static phrase, falling back on it even in the sentences immediately following his drawing of the distinction. This strikes me as another opportunity lost. If νοῦϲ is a ἕξιϲ, as B.19 clearly states that it is, then it could have been helpful and even illuminating to highlight the activities by which the ἕξιϲ is formed. These are, after all, the activities by which the inquirer is transformed into the expert, which is to say, by which the expert's capacity to demonstrate is formed, which is precisely the mark of the ἐπιϲτημονικόϲ. This in turn might have prompted Bronstein to draw a parallel distinction between learning by demonstration and learning by demonstrating. This, I think, could have strengthened his point about the pedagogical force of ἀπόδειξιϲ. It is by doing the demonstrating rather than by reviewing a static demonstration laid before one that one learns. (And one might wonder if there is a corresponding distinction to be made between learning by induction and learning by inducing—or perhaps by being induced, as the account in B.19 seems at times almost to make the mind of the learner into a passive receptor.)

My concerns about active *versus* static terminology connect to a larger reservation. I think Bronstein separates too sharply what he terms the 'epistemic conditions' of the expert from those of the inquirer. Though one can easily understand why Aristotle might in his familiar way choose for the sake of analytic clarity to separate his treatment of demonstrating from that of defining, I think there are compelling reasons for not separating them in actual scientific practice. Investigation in any scientific field involves a simultaneous search for new demonstrations along with the definitions that they are derived from. The search, I would say, is a constant back and forth between the two. Both types of searches depend (as I shall explain in a moment) on the constructing of syllogistic deductions. The expert/inquirer begins, as Bronstein says, with certain facts. But whether they are scientific facts remains unclear, just as whether a proffered statement of essence is in fact an explanatory, causal definition. A fact is not known to be scientific, nor a presumed definition to be explanatory, until each becomes part of an actual demonstration.

The only way for a researcher—a term seldom used by Bronstein; I use it to express the combined activities of expert and inquirer—to make a fact part of an actual demonstration is to construct putative demonstrations. The researcher advances what he or she hopes is an explanatory showing. The scientific status of both the familiar facts with which the researcher has begun and the presumed explanatory definitions hypothetically advanced are precisely what is at stake. If the proof sticks, it's a demonstration. Probably.

Or probably at best. No putative demonstrative conveys its scientific status in isolation. It is hard to know that we know, as Aristotle says in *Post. An.* A.5. And because degrees of imprecision (or lack of ἀκριβεία) infect almost all Aristotelian sciences—geometry relative to arithmetic, astronomy compared to biology—very few demonstrations can be known without qualifications made necessary by the object being studied or the beings who study it. Because of this, the work of defining and demonstrating is an ongoing interrelated and provisional activity, which would make the sharp divisions that Bronstein imposes between the types of learning and the stages of inquiry seem questionable as descriptions of actual scientific practice. To say otherwise—to say that demonstrations and the expertise that produces them come only after all or most definitions have been discovered (as Bronstein says on page 75)—is to risk falling back into a version of Barnes's old thesis that the theory of demonstration was appropriate only for facts already won, in a science nearly complete. Demonstrating is the winning.

Though I cannot argue for it here, the expert's sense of the rightness of an explanation, which Bronstein also speaks of, is a large part of what I think the ἕξις of νοῦς amounts to—the acquired ability to see the cause *as* the cause. The researcher's confidence in its scientific status (recall *Post. An.* A.2, 72a25–32) builds only as the putative demonstration is seen to become a more and more secure part of a comprehensive body of demonstrations.

In terms of the goals that he sets for himself, Bronstein effectively argues that the *Posterior Analytics* is coherently structured, though one might still hesitate to call it elegant. But how thoroughly does he apply his notion of an unfolding exposition to the finer structure of the *Post. An.*? Here, it seems to me, Bronstein's efforts, while suggestive, are far from complete. While the backwards progression he traces through the three types of learning is philosophically plausible, there is little attention paid to moves at a more detailed level of resolution. He does not, for instance, explore how A.1 motivates and leads to A.2, or why A.3 can be seen as the necessary preliminary to A.4–6 and beyond. And while he identifies the main achievements of B.2-10, Bronstein does not attempt to straighten out the often circuitous sequence of arguments leading to them. How, for instance, do the opening lines of B.8 establish an expository connection to B.3 and even B.1? What are the internal dynamics of the unfolding exposition of those chapters? To have traced them out would have required a much longer and more detailed book, so again I may be asking for something Bronstein did not set out to write. Nevertheless, I felt a certain sense of promise unfulfilled, though perhaps Bronstein can be given credit for instilling a desire to have more.

Aristotle on Knowledge and Learning derives ultimately from Bronstein's PhD dissertation (University of Toronto), and parts of chapters 1, 2, and 13 are based on previously published journal articles. The book nevertheless reads well as a whole, in part because of Bronstein's frequent (indeed, perhaps too frequent) summarizing of previous arguments or arguments still to come. Translations are by Bronstein, based on those of Barnes' Revised Oxford *Aristotle* [1984]. Key Greek terms are transliterated when they are used in the main body of the text. The full Greek of each passage is also included in the footnotes.

On the whole, *Aristotle on Knowledge and Learning* is a solid achievement. It offers a host of insights into problematic concepts and passages that merit further debate, and a plausible overall account of the work's internal dynamics. For both these reasons, Bronstein's book should become a departure point for future explorations of Aristotle's ever fascinating account of scientific knowledge and its acquisition.

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Plutarch's Science of Natural Problems: A Study with Commentary on Quaestiones Naturales by Michiel Meeussen

Leuven: Leuven University Press, 2016. Pp. 555. ISBN 978-94-6270-084-0. Cloth US$114.00

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Plutarch's *Moralia* contains four 'Question' treatises—*Convivial, Roman, Greek,* and *Natural*—all contributions to the 'problem' genre, all differing in subject and style.

The *Convivial Questions* is a delight. For each question, there is a new drinking party. The symposiasts, friends and relatives of Plutarch, regale us with lively and urbane responses to oenological questions. If their proffered solutions are sometimes a little banal, their affability and wit always refresh us. Plutarch guides the loose structure and episodic character of his genre with inventive flair, and the work as a whole offers a charming fusion of the Platonic *Symposium* and Peripatetic 'problem' literature, free of Aristotelian pedantry and the sobering majesty of The Beautiful.

The *Natural Questions*, by contrast, is a drab piece of work, something only a historian of science could love. Consisting of 31 sections (plus 10 preserved by Longolius and Psellus), it solves problems about salt and fresh water and about various plants and animals, wild and tame. In style, it owes most to the ps.-Aristotelian *Problems* with its monotonous 'Why is it that…?' questions and its bare-bones alternative answers. But unlike the *Problems*, a massive reference work tightly organized into 38 books, the *Natural Questions* could fit on a single scroll and wanders apparently at random through its bizarre and miscellaneous queries. Michiel Meeussen generously likens it to Catullus' Alexandrianizing little book of poems [94], where moisture serves as the *leitmotif* in place of *odium et amor*. There may be reasons to believe that the *Natural Questions* could have been polished into a smart *libellus*, but Plutarch clearly never put the pumice to the papyrus.

Meeussen nevertheless thinks that these nuggets are worth something, and he is partly successful in proving it. His book is divided into a set of introductory

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 154–157

essays and a problem-by-problem commentary. Meeussen's main purpose is to show that, consistent with Plutarch's Platonic convictions, the *Natural Questions* combines natural scientific inquiry with theological and religious principles [15–16]. Appropriately, Meeussen poses a problem: Why does Plutarch, an avowed Platonist dedicated to universal teleology, write in a distinctively Aristotelian genre, deeply embedded in a Peripatetic scientific method and low-level teleology? Meeussen offers alternative answers in the course of four chapters.

The tradition of the 'problem' finds its origin in Democritus and became fully integrated into scientific method through the efforts of Aristotle. What value, then, can Plutarch's small and random assortment of problems have for this sophisticated enterprise? And how scientific is a man who asks why bears' paws are tastiest [*Nat. quaest.* 22] and why bees sting adulterers [*Nat. quaest.* 36]? Meeussen rightly answers that Plutarch's purposes differ from Aristotle's, and that scientific inquiry means different things in different contexts and times. Here the *Natural Questions* serves as the model of a 'gentlemanly' science of light subjects artfully arranged. This is certainly not a bad thing, but such trivial and unsystematic treatments are hardly worthy of Plato or Aristotle.

Against the charge of unoriginality, Meeussen [46–51 and 82], like F. H. Sandbach [1969, 134], defends Plutarch on the grounds that he often offers his own novel solutions and rejects those offered by Aristotle or Theophrastus. High praise indeed.

As for chronology, Meeussen seems to favor the view that the *Natural Questions* was composed over a long period of time, starting in Plutarch's youth under the tutelage of Ammonius in Athens. In this opinion, he also seems in harmony with Sandbach, who argued that *Convivial Questions* and *Natural Questions* were written contemporaneously and that they exhibit mutual borrowing [1969, 138].

In one of the most helpful and successful sections of the book, Meeussen explores various orders of the presentation among the four 'Question' treatises. What seemed random at first gradually reveals a subtle order. One problem leads naturally, not rigorously, to the next, and the more distant problems often cross-refer in peculiar and unexpected ways. Such organization is appropriate to the gentlemanly readers of ancient miscellanist writing [92–102]. In the *Natural Questions*, salt water, having dominated the stage at the beginning, is ushered off by hunting themes only to sneak back in with the salty tears of boars [*Nat. quaest.* 20]. The artful arrangement here suggests that Plutarch may have intended at some point to give this sketchy treatise a full costume performance.

So, scholars have traditionally treated the *Natural Questions* as personal memoranda that Plutarch intended, but failed, to work into a polished literary work like the *Convivial Questions*. Sandbach [1969, 134–135], following Rose and Halliday, suggests that the *Natural Questions* began as notes excerpted from a variety of sources and meant for possible inclusion in other more literary works; that though they may have circulated among friends, they were not intended for general publication.

Meeussen argues, with some plausibility, that Plutarch was interested in problems for their own sake and not just as a stage for literary display. Both the *Roman* and the *Greek Questions* show that 'problem' literature admits of several legitimate forms. The alternative explanations of *Roman Questions*, copious though they be, lack all dramatic frame, while in his *Greek Questions* even the alternative solutions are abandoned, and Plutarch is content to give the single correct answer.

If the *Natural Questions* is not just a collection of memoranda, what purpose does it serve? For Plutarch, the usefulness of problems lies partly in their being convenient school exercises. Natural problems are easy and persuasive, and readily yield to solution. At the same time, though, and in a manner reminiscent of Epicurus, they discourage gaping wonder and feeble-minded superstition. Meteorological subjects, of which terrestrial waters form a part, have traditionally invited these reactions. The solution of these problems, by displaying natural causality, combats superstition and leads us to a greater appreciation of the regular, celestial phenomena.

The final introductory chapter continues the work of the third by focusing on the place of the *Natural Questions* in Plutarch's philosophical outlook. Meeussen argues that with reference to his other works, Plutarch was 'radically informed by Platonic dualism and generally inspired by Academic Scepticism' [363]. For Meeussen, this means that Plutarch exercised ἐποχή in the face of the phenomena, but privileged the causes that arose from divine sources; and, therefore, that those causes expressed, or at least hinted at, by myth are credited. The natural causes are then treated as cooperative to the providential cause. Meeussen sees hints of this providence in *Natural*

Questions, but his evidence is slight as he himself admits [272–274]; and the Platonic character of Plutarch's 'problem' treatises cannot be sustained.

These introductory essays are followed by a thorough commentary. Each problem is prefaced by a synopsis in English with the ancient text quite fully quoted in parentheses. In my mind, this is a poor compromise. Either Meeussen should have provided text and facing translation in the standard manner or left his reader to read Sandbach alongside. Nevertheless, Meeussen is responsible in citing the peripheral literature, and makes significant additions to Senzasono's notes (which are amply acknowledged). For my part, I would have gladly traded the synopsis for more description and analysis of the peripheral literature, especially on the scientific and craft issues. So. for example, Plutarch says [*Nat. quaest.* 17] that stallion hairs are preferred to mare hairs for fishing nets. This would have been a good occasion to dilate on fishing nets and their fabrication [426].

Slow readers, like myself, will wish that the author had been more concise or at least, when the subject matter permits, livelier. The theses that Meeussen argues for are readily intelligible (though not always persuasive) and the arguments pro and con could be summarized more succinctly. Frankly, I do not think that the question of whether Plutarch's Platonism was compromised by the Aristotelian 'problem' format is worth 350 pages of analysis. There is a good, useful, and slender book to be found somewhere within this volume, but Meeussen should have kept his muse away from the bear paws [*Nat. quaest.* 22].

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Science in the Forest, Science in the Past edited by Geoffrey Lloyd and Aparecida Vilaça

Hau: Journal of Ethnographic Theory 9.1. Chicago, IL: Society for Ethnographic Theory, 2019. ISSN 2575–1433. epub. $10.00

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Those whose occupation it is to study histories of sciences begin, like Aristotle, with wonder: 'What could they mean by that?' Some of those studies consider the sciences of people from long ago, whereas others consider the sciences of more recent people but from cultures different from those of the student. Participants in such efforts mostly know to expect a conceptual chasm and yet hope to cross it. Moreover, even when studying sciences within one's home culture, there are arresting moments of defamiliarization and dizzying chasms open before our footsteps.[1] Conversely, philosophers and theologians have often made hegemonic claims for their approach, arrogating titles such as 'Queen of the Sciences'. What then to say when a diverse tribe of scholars sets out to explore 'Science in the Forest, Science in the Past', as presented in the special issue of *HAU* here under review?

First, a little context. Some early Greek scientists eagerly explored the conceptual worlds of the 'alien' cultures to which they had some access; Babylonians, Egyptians, Indians, Persians, and Scythians are attested as informants or teachers. ('Alien' of course cuts both ways, as Xenophanes famously remarked [Diels and Kranz 1951, frr. 21B15–16], speaking about how foreigners depict the gods—that is, like themselves.) No doubt, the attempts of those Greeks to explore (or exploit) the scientific ideas of those neighbors would not pass muster in a contemporary department of anthropology. But the activity attests to a human belief that other peoples' ideas may be commensurate with, and even relevant to, our own concerns. The Romans went further, of course, and besides the fascination many of them felt for Celtic, Etruscan, or Punic wisdom, there was a broad-based 'translation movement'

[1] E.g., Kidder 1981 and Traweek 1988.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 158–176

that rendered Greek science, or some parts of it at least, accessible in Latin to interested readers [Keyser 2010].

Travelers may import new ideas and ways of thinking, easing the task of an anthropologist of science, but narrowing any results to what the travelers happen to import. Such down-the-line trade has long been a feature of human cross-cultural interaction, and allows for a good deal of assimilation and transformation. The remark by Francis Bacon—that the greatest modern inventions are printing, gunpowder, and the magnetic compass, but no-one knows their origin—exemplifies that sort of assimilation and transformation [Bacon 1620, 147–148: cf. Boruchoff 2012, esp. 138]. It also amuses, if only because we know that all of them came west from China.[2] The long and rich interaction between the scientific cultures of the Islamic caliphates and those of the Latin west displays another kind of trade in ideas and sciences. Translation was essential to that set of enterprises, starting with the translations of Greek scientific literature into Syriac and Arabic in the eighth century AD, but including also the numerous later renderings of Arabic and Greek texts into Latin.

So we find ourselves immersed in a long-running stream of cultural interaction around science. That stream as I have described it embodies an activity that assumes the possibility of translation and communication. Moreover, it is a 'mercantile' style of interaction, in which all parties extract from the sciences of the respectively 'alien' culture(s) mostly what they themselves expect to be 'useful' for their own interests. That limits the degree to which 'alien' science can be understood because technologies are more fungible than ideas.[3]

The idea that understanding the science (or poetry) of an 'alien' culture might be of interest and worthwhile for its own sake is radical and rare in human history, as it seems. When the Romans or the Arabs translated Greek science, it seems that they expected to learn something useful about the world. In either case, it is debatable to what extent the dominant culture believed that

[2] Perhaps we should add eyeglasses, which are first attested in the west around AD 1300? Laufer 1907 argues for a Chinese origin, but Rosen 1956 and Needham 1962, 118–122 reject this: see also Ilardi 2007, 3–50.

[3] Medical anthropology is indeed highly pragmatic: Pfeiffer and Nichter 2008; Goodson and Vassar 2011; Joralemon 2017; and Singer, Baer, Long, and Pavlotski 2020.

Greek literature or culture was of value *per se*.[4] Romans were certainly fascinated by Greek culture and some Romans at least felt that that their conquest of the Greek world had enriched the Roman world by more than mere territory or *Macht*.[5] Modern enterprises such as ethnobotany or ethno-agriculture operate at least in part with a similar goal of (possibly mutual) benefit.[6]

None of that is anthropology, which I understand to be occupied with the study of 'alien' cultures *per se*. That is, cultures become topics of study not because they might provide something useful, but because they are of intrinsic interest. (That distinction is not absolute: learning about another culture in an appreciative way will naturally lead to reflections and reconsiderations about one's own culture.) But that long history of cultural exchange, whether between neighbors as when Greek scientists reached out to Egypt or Mesopotamia, or whether between a conquered ('colonized') people and their new overlords, runs as an undercurrent beneath all our modern attempts to perform anthropology.

I am no anthropologist, but we hope that the silos of scholarship are not opaquely incommensurable. Moreover, I hold that it is best when there is 'free trade' and open dialog between disciplines. (Classicists, historians of ancient science, and other students of ancient cultures may be seen as practicing a kind of time-traveling anthropology [cf. Holmes 2020].) Given that Geoffrey Lloyd was a leading participant within the flash-tribe that gathered at the conference to explore these questions, I think that readers can have confidence that some degree of communication was both a goal and an outcome. The scholars pursued various paths into the forest, but a chief discursive frame encompassed the issue of 'ontologies'. Some of the papers were more explicitly concerned with that frame. Others followed a

4 The earlier case of Assyrians studying Sumerian literature might reflect a similar response. On this activity, see: Oppenheim 1977, 16–24, 235–238, 249, 255–256; and Michalowski 2017, esp. 205–207.

5 Cicero describes Greeks as excelling Romans in all forms of literature [*Tusc*. 1.3], and Horace remarks that conquered Greece took Rome captive, thus bringing *artes* to Rome [*Epist*. 2.1.156–157]. Somewhat differently, Vergil [*Aeneid* 6.847–853] predicts that Rome shall excel in rule, let others excel in arts.

6 See Prance, Chadwick, and Marsh 1994; Minnis 2000; Soejarto, *et alii* 2005; and Voeks 2018.

path around mathematics. A third, smaller cluster of papers explores some aspects of artificial intelligences, or as I would prefer to label them, cyborgs.[7]

1. Ontologies

Although invoked as a guiding inquiry of the conference, the 'clash of ontologies' did not deeply engage many of the participants, as Lloyd and Vilaça remark [179–180] in their closing essay. Nevertheless, the issue is latent in many of the papers and is worth exploring. One simple example of the problem would be the classification of animals, which for modern science involves distinctions between mammals, birds, and fish (among others). However, a more ecocentric ontology might exploit categories like 'flying creatures' or 'creatures dwelling in Air' (and thus bats, bees, and finches are close relatives) as well as 'swimming creatures' or 'creatures dwelling in Water' (and thus carp, dolphins, and shrimp are close relatives). So the two distinct ontologies, ecocentric and phylocentric, encode different concepts—but the ontologies are not incommensurable or incommunicable.

Vilaça, in the contribution 'Inventing Nature: Christianity and Science in Indigenous Amazonia' [44–57], addresses contrasting the ontologies of humans and animals of the Wari' and of modern science. For the Amazonian Wari', animals and humans share a great deal, whereas for some strands of European and Mediterranean thought, humans are radically distinct from animals. Likewise, there is a contrast between the meanings assigned to singularity and duality: for the Wari', singularity (the number one and related concepts) is lonely and incomplete, whereas duality (the number two and related concepts) is richer and more potent. That contrasts with a tradition in European thought (found among Pythagoreans, as well as Neoplatonists and monotheists) that 'the One' is primal, original, and Good, whereas 'the Dyad' is the opposite of those. But traditions in western, or even modern, sciences about the significance of numbers, or the relation of humans to animals, are themselves not unitary. Descartes' view that animals are simply bionic machines was never the only choice, and there is a rich array of debate and tradition in European and Mediterranean science and philosophy about

[7] Two papers in this volume fall outside these categories and definitely outside my expertise, so I will keep silent: Kuper, 'Deconstructing Anthropology' [10–22] and Herzfeld, 'What is a Polity?' [23–35].

the ontology of animals vis-à-vis humans [Sorabji 1993]. (Moreover, I would respectfully but strongly dissent from the claim that modern western science has 'Christian foundations' or 'is monotheist' [49]. Science hardly began with the 17th-century 'Scientific Revolution', and several other contributions to these proceedings emphasize that point [see Lloyd, p. 37] and especially the contributions on mathematics, below.)

Translation, too, implicates ontologies, and necessarily so. Any translation is an assertion of semantic proximity, which in turn is an assumption of overlapping ontology. As Lloyd argues, in 'The Clash of Ontologies and the Problems of Translation and Mutual Intelligibility' [36–43], even such 'simple' words as 'fire' and 'water' are slippery to translate. He is taking those as terms that are not 'highly theory-laden' [38], but I think that his own discussion shows that they are actually theory-laden. He cites translations of those words among Chinese, English, and Greek—and at least in Greek and Chinese, the chosen example terms refer to fundamental 'elements' or 'phases' of matter. To translate ancient Greek «ὕδωρ» ('hydōr') or Chinese «水» ('shuǐ') into English 'water' is both 'obvious' and yet missing many resonances; likewise in translating Chinese «火» ('huǒ') or Greek «πῦρ» ('pūr') as 'fire' [Lloyd 2012, 85–89]. Other 'obvious' terms may be translated with no more—and no less—risk of ontological clash, such as 'book' or 'city,' or even 'food' or 'school'. Any effective translation will arrive accompanied by a host of adjutants, serving to qualify, nuance, or clarify.

Lloyd, as he has done elsewhere, takes an optimistic position on translation. He holds these claims to be foundational [36]:[8]

> (1) no translation is ever perfect and complete, all are provisional and revisable; (2) there is indeed no perfect, complete, mutual understanding, even when all interlocutors share the same natural language. On the other hand, (3) some understanding is always possible, even across divergent systems, and even across incommensurable paradigms, even if (4) there is no neutral vocabulary in which it can be expressed. This depends (5) on allowing that the terms in any language exhibit what I call 'semantic stretch'.

As Lloyd goes on to argue [39, 41], there is no neutral or universal language in which to disambiguate terms and semantics; one just has to work it out

[8] Lloyd here reprises 1987, 172–214, esp. 174–181, citing Porzig 1934 as similar, and 208–214: cf. also Lloyd 2002, 123, where again Porzig 1934 is credited.

tentatively and provisionally. He points out that 'incommensurability' is not a threat, but is instead an opportunity [41]. I would go further, and claim that an apparent 'incommensurability' is only provisional, and is always a sign that can elicit wonder and curiosity, and thus reflection, engagement, and exploration.

I offer an enlightening example from modern science of a semantic stretch that is also an issue of apparently clashing ontologies. Chemists often speak of chemical bonds [Pauling 1960] and the usual initial distinction is between the typical bond of 'inorganic' chemistry and the 'covalent' bond, as found in 'organic' chemistry. The 'ionic' bond is between two atoms, in which one or more electrons are entirely transferred from one atom to the other. The canonical example is salt, in which a single sodium atom yields an electron to a single chlorine atom. In simplistic contrast to this is the 'covalent' bond, that is, in compounds of carbon, hydrogen, oxygen, and nitrogen (primarily). In the covalent bond, there is no wholesale transfer, and the atoms participating in a bond share one or more electrons. One simple example is water, in which each of two hydrogen atoms shares its electron with a single oxygen atom. (These terms originated in the 1930s, although the concepts were being explored 20 years prior.)

But in fact, the ontology is unstable, since the ionic or covalent character of a bond is a matter of degree, not dichotomy. Moreover, other types of bonds also exist, such as the 'hydrogen bond', in which a hydrogen atom participates both in its canonical single covalent bond and in a weaker bond with a third atom that has some electrons on its surface that are not participating in any other bond. This bond-type is responsible for many of the remarkable properties of water. Further, compounds of boron and hydrogen (known as 'boranes') display yet another type of bonding, in which the single electron of a hydrogen atom is shared among three atoms, namely, two boron atoms and the hydrogen atom itself. The complexities ramify, and there are, for example, 'clathrates'—compounds in which a large molecule forms a 'cage' in which a smaller molecule is bound. All of this shows how even within a single scientific discipline and in a single language, there is an instability, or at least complexity, of ontologies. That seems to chime well with Lloyd's advice [41] that investigators allow for the 'multidimensionality of the explananda'.[9]

The essay by Jardine, 'Turning to Ontology in Studies of Distant Sciences' [172–178], employs the useful covering term 'distant science(s)' to refer alike to sciences of the past and to those of 'alien' cultures. Jardine argues for a pluralist view of science(s), so that, in his example, 'indigenous practices of pigment preparation' would cohere with western industrial lab chemistry. Indeed, many journals are devoted to understanding indigenous or ancient practices of pigment preparation, along with many other 'chemical' techniques: e.g., *Archaeometry* (1958–). Such work exemplifies some aspects of the practice of translation, that is, of commensurability, for materials science(s), across cultures and time. The concluding remark [176] is well worth quoting:

> For however deep the understanding we may achieve by 'going native' in the forest or the past, we owe it to ourselves and our audiences to provide comprehensible interpretations.

Jardine calls it 'the principle of responsibility,' evoking a strong commitment to working hard to perceive the nature of the commensurability, and to translate that for readers.

2. Mathematics

Turning now to the papers that followed a path around mathematics, we have a contribution by de Almeida asking 'Is There Mathematics in the Forest?' [86–98], plus three contributions on each of three literate cultures: Chinese, Greco-Roman, and Indian. Those three are, respectively, 'Different Clusters of Text from Ancient China, Different Mathematical Ontologies' by Chemla [99–112]; 'Mathematical Traditions in Ancient Greece and Rome' by Cuomo [75–85]; and 'Shedding Light on Diverse Cultures of Mathematical Practices in South Asia' by Keller [113–125]. These contributions exist within a larger framework of 'ethnomathematics', itself a problematic term, and an active set of fields.[10] Those fields offer studies of mathematical notation in

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9 Lloyd has very insightfully explored ontologies, and the issues of translation around them, in 2015, 88–108.

10 See especially Barton 1996, Vithal and Skovsmose 1997, and Rivera and Rossi Becker 2008.

literate cultures [see Chrisomalis 2010], studies of mathematical practice in specific communities,[11] and plenty of studies of learning styles.[12]

De Almeida argues for 'the existence of universal mathematical capabilities,' supported by evidence in the form of 'recursive rules used to produce consistent patterns that are transportable across distinct domains of thought and action' [86]. Even without the restriction 'recursive', that would be a proper definition of the work of mathematicians in any culture. Detecting recursion is a pleasant extra accomplishment, and not just because recursion is a concept of modern western mathematics that is widely used in writing computer code. It also foregrounds a fundamental human capacity, visible also in the structures of human language. The primary and extended example concerns how kin relations can encode abstract maths, among the Cashinahua (better, 'Huni Kuin') of Acre state in western Brazil and nearby Peru [90–93]. As de Almeida convincingly demonstrates, kinship structure encodes formal mathematical statements, such as multiplicative identity ($f * e = f = e * f$, with 'e' the identity element for the operation '*', and 'f' any element of the set over which the operation is defined). This encoding represents the rules for combining kinship terms, such as $epa * betsa = epa$ (translated as 'same-sex parent * same-sex sibling = same-sex parent'). The vocabulary and grammar of the kinship system also encodes the self-inverse property ($f * f = e$), as well as others.

To demonstrate further that cross-paradigm translations are possible [93–94], de Almeida provides a translation involving irrational roots (of 2, 3, and 6) across the chasm between Euclid and Dedekind.[13] De Almeida shows how the proof is valid both in Euclid's paradigm of irrational values and in Dedekind's paradigm for thinking irrational numbers (the 'Dedekind cut', which defines an irrational number as the limiting boundary between a pair of disjoint sets of rational numbers). Another, more briefly drawn translation involves Euclid, *Elem.* 9.20, which proves that, given any list of prime numbers, there exists a prime not on the list, and thus that the set of primes is unbounded. As de Almeida says, we must pay close attention to what Euclid does, and does not, argue; and because of Euclid's careful

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[11] Many such, e.g., Millroy 1991 and Chahine and Naresh 2013.

[12] Widely cited is Eisenhart 1988.

[13] Here, de Almeida follows Stillwell 2016, 156–157.

language, the argument takes the same form, even after a paradigm shift in the theory of infinity, because it does not implicate any specific theory of infinity [94]. Another point also requiring careful attention is that the proof asserts that the number composed by adding 1 to the product of the primes in the list is either prime or else has a prime factor that is not in the list. To see that 1 plus the product of the primes in the list need not be prime itself, start with a list of the primes 3 and 5, and find that $(3 \times 5) + 1 = 16$, where 16 has a prime factor not on the list, namely, 2. Likewise, starting with the first six primes, namely 2, 3, 5, 7, 11, and 13, one finds that 30,031 has the prime factors 59 and 509, not in the initial list.

Chemla's contribution on Chinese culture considers school texts of the 7th century AD, and tomb texts from 'last centuries BC'—the two clusters 'testify to two different ways of practicing mathematics, which related to different material practices' [99]. As Chemla says, using actor-created corpora is a better way to investigate ontologies in that it is both more principled and more effective. Such corpora reflect their underlying ontology in their technical language and material practices [100]. Chemla shows in detail that texts in the later cluster all regularly use rods for computing that are laid out on a surface in decimal place-value arrangements [100–109]; this is explicit in the *Mathematical Canon by Master Sun*, and implicit in other texts of the same later corpus.[14] In contrast, the algorithms described in two tomb scrolls from *ca* 200±15 BC, as well as some Qin-era texts in Beijing, also use rod-numerals; but they do not describe the operations of division and extracting roots in words that reflect the same ontology as in the commentaries [109–110]. Instead, the earlier mathematical texts 'seem to reflect the use of operations as means to reach a result rather than as processes to be pondered' [109].

Cuomo's contribution on Greco-Roman culture considers the tradition(s) of Greek mathematics: the 'theoretical' tradition and the allegedly contrasting 'practical' tradition. The distinction is ancient and starts, as Cuomo demonstrates, with Plato and other authors. The 'theoretical' tradition is mathematics as conceived by Plato, or as practiced in the pages of Euclid's *Elements*; the 'practical' tradition is mathematics as seen in the corpus of Heron of Alexandria (mid-first-century AD). Cuomo views the dichotomy as unstable

[14] The contribution here relies upon the valuable work of Chemla 2013 and Volkov 2014.

and shows how practices migrated across the very permeable boundary, and how modern attempts to maintain the distinction founder [75–81]. Instead, an approach using 'situation-specificity, or situated learning' is to be preferred, along with 'code-switching' [81]. That is, any given mathematician might produce more theoretical work in one situation and more practical work in another. Likewise, the language of a Greek (or any) mathematical work might vary between 'theoretical' and 'practical' depending on the intended audience or expected use of the work.

Moreover, Cuomo argues, an analysis of mathematical behavior in terms of situations is more responsive to details of the work and opens up more avenues for comparison, since similar situations might arise in quite distinct times and places. I would point out that the Archimedean corpus contains both 'theoretical' works (such as *Spiral Lines* or *Sphere and Cylinder*) as well as 'practical' efforts (such as *Division of the Circle*). Nor does the *Cattle Problem* or the *Stomachion* (however interpreted) easily fit into some binary classification. Likewise for Eratosthenes, both the 'mean-obtainer' (*mesolabon*, a kind of slide-rule for extracting roots), and the *Geography* seem 'practical' (or at least not 'theoretical'); whereas the attested but lost work *On Means* would likely have been 'theoretical'.

Keller's contribution on Indian culture considers two contrasting practices of numbers, measures, and computations in South India [113]. One is documented in early Sanskrit mathematical treatises and commentaries (of the 7th to 12th centuries), the other in elementary mathematical educational texts in Tamil (of the 17th to 20th centuries). The Sanskrit mathematical texts present abstract mathematics, in which calculations are performed on 'pure' (unitless) numbers, and decimal place-value numerals are used [115–116]. The Sanskrit texts also present themselves as delineating a timeless discipline; that is, any given text claims to be 'the reframing of a preceding treatise or of an orally transmitted doctrine' [115]. In contrast, the Tamil texts use Tamil numerals, which are decimal and non-positional, and the computations are made with units attached to the numbers [115–116]. Keller's analysis focuses on two common kinds of computations found in both sorts of texts:

(a) computations of areas [116–120], and
(b) computations of gold fineness [120–121].

As Keller shows, the two corpora are not utterly distinct, and some specific problems or methods appear in both [122].

All three of these contributions on literate cultures conclude, analogously, that the allegedly distinct or dichotomous corpora are not in fact separated by an incommensurable chasm. Greek 'theoretical' and 'practical' mathematics, Chinese Tang-dynasty school-texts, and Qin- or Han-dynasty tomb-texts, as well as Indian Sanskrit texts and Tamil texts, all show communication across the chasms.

3. Cyborgs

Turning finally to the (small) cluster of papers that explore some aspects of artificial intelligences, we have Blackwell, 'Objective Functions: (In)humanity and Inequity in Artificial Intelligence' [137–146], and McCarty, 'Modeling, Ontology and Wild Thought: Toward an Anthropology of the Artificially Intelligent' [147–161]. In both cases, I think that the full perspective here is better described using the word 'cyborg.' The artificial intelligences are considered under the same defamiliarized perspective as are the 'distant' cultures of ancient China or contemporary Amazonia (to borrow the term from Jardine, as above). That is, the artificial intelligences are imagined as members of some 'alien' culture that to be sure bears a rather special dependent relation to modern western culture but is nonetheless imagined as distinct or on the far side of a chasm. To express that uncanny relation, I want to use a word like 'cyborg.'

Blackwell focuses on 'the subjectivities embedded in these mechanical systems, and the human satisfactions and ambitions in constructing them' [137]. Two different approaches to those subjectivities are made. The first is to examine, briefly, the perhaps surprising procreative aspect of cyborgs [138]. Blackwell writes that the artificial construction of simulated humans in fiction seems often to become powerfully gendered, perhaps alluding to the gendered nature of all human procreation. The figure of the AI engineer building sexy robots and falling in love with them has many fictional precursors, including that of Pygmalion. Indeed the Turing Test itself was first posed as an Imitation Game in which the challenge assigned was not for a computer to imitate a man but for a man to imitate a woman.

Blackwell sharpens the point by suggesting that such creations 'often' result in some excess and some retribution, as if such involvements transgress some well-defined moral order. Certainly some cyborg fictions have such an element, and perhaps the transgression is that the creator mates with (usually) his creation, thus violating the taboo against incest. (Indeed, here the use of the word 'cyborg' enables sharper focus on the problem.)

But I do not think that the (surely fictional) 'singularity' is either inherently retributive or necessarily sexual. It certainly smacks of the divine to hypothesize that some being(s) would gain such extreme, even infinite, power. The imagined 'singularity' is an overly-simplified extrapolation of current trends, without any physical model to explain or validate the specific direction or degree of extrapolation. Even without an actual infinity, we may imagine a growth of cyborg power to an unpleasant or risky degree—just as one might extrapolate (on well-grounded assumptions) three more familiar catastrophes: nuclear, biological, or climatic. On the one hand, nation-states or others might increase the number and power of nuclear weapons and thus run the risk of an extremely destructive war. Or, new kinds of zoonoses, whether natural or artificial, might increase in number and fatality rate, until some apocalyptic plague breaks out. Or, thirdly, the degree of global warming might increase to such an extent that the structures of modern global society would crumble. But such extrapolations are at least founded on scientific measurements and experiments, which thus provide means of analysis and form a basis for attempting to evade hypothesized bad outcomes.

Blackwell also engages in a second line of investigation about subjectivities by examining the language used to describe certain aspects of the making of cyborgs [139–144]. Here he addresses three specific phrases or labels:

(1) 'objective function',
(2) 'logistic regression', and
(3) 'oracles' and 'ground truth' (two terms that regularly travel together).

The terminology is not usually used by practitioners in an ambiguous way, but, indeed, as Blackwell says [141], many computer scientists are poorly trained in basic principles of epistemology, while many philosophers are poorly trained in basic principles of engineering, meaning that they happily talk at cross-purposes with the aid of ambiguous terminology that neither properly understands.

So there is the potential for the perception of an incommensurability or clash of ontology. An 'objective function' is a kind of component of many pieces of software, and would likely be used to create any eventual cyborg [139–140, 142–144]. As Blackwell says, one example is the objective function that evaluates the relative goodness of search results from any search engine (whether Google, Bing, or DuckDuckGo). Such a function is a mathematical transformation that defines how closely a given measurable result (of a computation) adheres to some defined goal. The 'objective' in the phrase is, as Blackwell says, the goal being sought; so an 'objective function' might better and more clearly be called a 'goal-function'. It is unfortunate that, by the usual ambiguity of language, an 'objective' function can seem to refer to something that is 'objective', i.e., in contrast to something 'subjective.' So here the actual issue of cyborg subjectivity concerns the goal-functions used to program the eventual cyborg, which were of course developed by the programmers who presumably used their subjective best estimates of what would work well in addition to whatever evidence they accumulated by testing proposed goal-functions.

The second label, 'logistic regression', refers to a mathematical procedure that fits data to a 'yes / no' model, or indeed to any categorical model [140–141]. That is, in trying to evaluate data to see if, for example, the data are more consistent with one outcome (from a list of distinct outcomes) than with other outcomes (on the same list), this procedure is used. It is not perhaps a well-named procedure, but it is widely used in data-analysis. The procedure is not very specific to the creation of cyborgs but would likely be used to program some of their behavior. Again, the actual issue of cyborg subjectivity concerns the lists of distinct outcomes used to define any logistic regressions in the eventual cyborg, which were of course developed by the programmers who presumably used their subjective best estimates of what would work well in addition to whatever evidence they accumulated by testing proposed outcome-lists. (It is something of a red herring to suggest that logistic regression is tainted by its origin in eugenics, as Blackwell does [140], citing a paper on eugenics from 1947. Logistic regression is a mathematical technique, possibly valuable, that is independent of any early uses of it [see Cramer 2010 or Simonoff 2003].)

Third, there is the problem of 'oracles' and 'ground truth' [141]. As Blackwell writes, 'supervised learning' depends on humans having labeled data or outcomes, so that the machine has a defined goal. The sense of 'supervised' is that the data are human-labeled, as if '...; item #456, an outcome type 'A'; item #457, an outcome type 'D'; ...' Such labeling can be very labor-intensive when the quantity of relevant data is huge, as it often is. Sometimes instead, an existing system or database can be used. In any of these cases, the reference to an 'oracle' or to the 'ground truth' points at the humanlabeled 'right answer'. So here again, the subjectivity within the cyborg is actually composed from the subjective judgements of the humans who tagged the data or outcomes.

Last, but hardly least, there is McCarty's contribution [147–161]. McCarty by his subtitle—'Toward an Anthropology of the Artificially Intelligent'—grabs the cyborg by its uncanniness. The key insight here is that the cyborg requires a model, i.e., an ontology, of the domain to be affected [147]. Moreover, McCarty addresses the defamiliarization of the 'person' *via* the creation of mechanical 'persons', i.e., cyborgs, as well as how those types of persons relate to one another, and the key role of Wiener's approach to cybernetics in enabling the comparison [147–148]. That is, Wiener saw that something like a control system (feedback loop with a sensor to detect the difference between the actual state of the system and the desired state of the system) would be a good model for cyborgs as well as for humans [Wiener 1966]. Now McCarty asks readers to imagine a Turing-test-like conversation with an actual cyborg and announces that we would feel alienated, that we would find ourselves faced with the chasm of incommensurability [148–149]. He writes that the cyborg would be 'enigmatically and unresolvably both like and unlike us'. How, I ask, is that situation different from what we manage every day, talking with the aliens all around us? It may differ in degree but it is not different in kind. The 'anthropology' in McCarty's title both foregrounds the problem to be faced in dealing with cyborgs and also indicates the response. Indeed, he concludes that machine intelligence is commensurable with ours, but that we should not underestimate the difficulty of communication [154–155]. McCarty argues [155–156] for a slow evolution of 'bridgeheads' of mutual understanding [citing Lloyd 2010]. In the end, he says that to talk about cyborgs is to talk about 'an emergent manifestation of ourselves differently constituted' [156].

Less convincing is McCarty's intervention on the 'plurality of ontologies' within computer science [149–153]. Taking as his point of departure the observation that work on computers regularly creates a multiplicity of ontologies, McCarty argues that this plurality shows that 'the ontological question was from the very beginning implicit in the design of the stored-program computer' [150]. If the multiplicity of ontologies is intended to refer to the various object-hierarchies that constitute the structure of many programs, then this multiplicity would not be very meaningful. These object-hierarchies, which are also known as class hierarchies (with 'class' here meaning something very like 'category' or 'type'), are created by the programmers *ad hoc* in order to organize their own thoughts and understandings about the program they are creating. Moreover, this mode of thought was not actually implicit in programs or computer architecture. Early programming languages, such as assembler, FORTRAN, ALGOL, or COBOL, had no notion of type-hierarchies. More recent languages include many that are constructed in terms of type-hierarchies; but even in those, the programmer can ignore that aspect of the language and write programs that do not reflect it at all. On the other hand, if the multiplicity of ontologies is intended to refer to the many object-hierarchies that organize the data being analyzed by the program, then again, this is not very meaningful. Such hierarchies are also *ad hoc* in that they are invented for the specific small set of problems being addressed in the current work of any given set of collaborating programmers. As McCarty says, such an ontology is 'a practical inventory in a schema' [150]. One monistic attempt to create a hierarchy of everything has attracted adherents and criticism, namely, Cyc [https://www.cyc.com/], but has not yet produced any cyborgs.

Aliens of three kinds, then, have been encountered by the explorers whose reports grace the pages of this issue of *HAU*, a name that, as I understand it, refers to a gift. The volume is indeed freely available, and well worth taking the time to read. I encourage engaging and reflecting, and further reporting.

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The Metaphysics of the Pythagorean Theorem: Thales, Pythagoras, Engineering, Diagrams, and the Construction of the Cosmos out of Right Triangles by Robert Hahn

Albany, NY: State University of New York Press, 2018. Pp. xvi + 283. ISBN 978–1–4384–6490–9. Paper US$39.95

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The study of ancient philosophy is never more fascinating—or more frustrating—than when it deals with thinkers who left behind nothing in writing. This category includes three of the most famous names in Greek philosophy: Thales, Pythagoras, and Socrates. Without a fixed text or even fragments of such a text to work with, any scholarly attempt to interpret their doctrines—to explicate their details, reconstruct how they arose, and study how they were applied—will always be shadowed by fundamental doubts about their actual nature. At least in the case of Socrates, we have a great deal of indirect evidence at our disposal in the massive Platonic corpus, together with other literary works, like Xenophon's memoirs and Aristophanes' *Clouds*, which can serve as a check on Plato's testimony. But, when it comes to Thales and Pythagoras, we are much less fortunate; for each thinker, fewer than a dozen pieces of testimony survive that date to within two human lifespans of their deaths, most no more than a few sentences in length. Since what we have is so limited, any new insight into the nature of their thought or teachings, however slight it may be, is potentially of great interest.

In his new study, Robert Hahn proposes that such insight can be had if we are willing to explore the implications of the geometrical discoveries made by Thales and Pythagoras. His specific hypothesis is that the two men not only laid the foundations for geometry as a formal, deductive science by revising the mensuration-techniques of Greek and Egyptian craftsmen, they also endowed it with a new, metaphysical meaning. Hahn is here reprising and extending an approach that he developed in previous studies of Anaximander, which aim to show how contemporary craft-practices provided early Greek philosophers with mental models and other habits of reasoning that, once directed at the natural world, helped give rise to natural philosophy. His

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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first book, *Anaximander and the Architects* [Hahn 2001], centered on the proposition that the construction of monumental column-drums by such contemporary architects as Theodorus and Rhoecus prompted Anaximander to think in analogous terms about the form and proportions of the cosmos; hence, the cosmologist's striking assertion that the Earth 'resembles a stone column' [Hippolytus, *Ref*. 1.6.3] and his further claim that the Earth is one third as deep as it is wide, its proportions thus strikingly similar to those of a lone column-drum. Hahn's 'thick description' of architectural practice during Anaximander's lifetime—construction-plans, models, building techniques, Egyptian influences—made these little fragments come alive, and gave weight to his plea that architecture be granted as much attention as politics or literacy when questions about the origins of Greek philosophy are raised.

In a follow-up study, *Archaeology and the Origins of Philosophy* [Hahn 2010], Hahn pursued this line of inquiry further, arguing that Anaximander's famous Sun and Moon 'wheels'—two massive, mist-wrapped wheels of fire which define the orbits of the two bodies—were influenced conceptually by the massive wooden wheels used to transport building stone in Ionia. He also showed how archaic smelting technology informed Anaximander's comparison of the visible faces of the Sun and Moon to a furnace's vent-pipe. The book concluded with a theoretical justification for this focus on archaeology, with texts from Dewey, James, and Putnam brought in to support the claim that knowledge is always embedded in material realities and, thus, that close study of material culture should be an essential part of any reconstruction of ancient philosophy. To this roster of modern authorities, Hahn could also have added Aristotle, who in his account of the development of different forms of human knowledge placed the wisdom of ἀρχιτέκτονες or 'master builders' just one step below that of philosophers proper [*Meta*. 1, 981a24–b24].

A reader of Hahn's first two books—both of them lucidly written and richly illustrated—is apt to come away persuaded that Anaximander engaged in serious reflection on contemporary craft-culture, and that many compelling and original features of his cosmology owe something to that reflection. In describing the philosophical significance of this material, however, the books sometimes go too far. The position which Hahn argues for is not just that a confrontation between archaeological and doxographical evidence can be fruitful, but that architectural thinking lay at the core of Anaximander's vision of the cosmos. Now, the surviving doxography for Anaximander gives pride of place to the doctrine of the Ἄπειρον, an originary being from which the elements emerge and to which they eventually return. In his treatise, Anaximander further sought to account for the creation of the existing world, the cycling through of various κόσμοι or 'world-orders', the formation and eventual disappearance of the ocean, the creation of the first human beings out of fish-like creatures, and the physical causes of wind, rain, and lightning. These important doctrines are, unfortunately, not illuminated in any way by an understanding of architectural practice. Only those facets of his cosmology that involve structure, measure, or form benefit in this way. So, unless natural philosophy is seen as something *primarily* concerned with the study of cosmic structures, it is going too far to treat craft-based thinking as instrumental in the formation of his core teachings. Study of the impressive remains of Ionian temples or archaic technology is still very valuable, but chiefly for the way in which it can make our reconstructions more grounded, meaningful, and accurate.

In his new book, Hahn again aims high, aspiring to show not just that Greek geometry as practiced by Thales and Pythagoras developed from Egyptian techniques of mensuration, but that they endowed it with metaphysical significance. Before reviewing the particular arguments for this, I would note that nearly half of the pages in this book are given over to clear, step-by-step explications of numerous Euclidean propositions—the 'Pythagorean theorem' [*Elem.* 1.47] together with its 'enlargement' [*Elem.* 6.31], and several other theorems from books 1, 2, 6, and 10—all illustrated with large, attractive, color diagrams. These expositions are meant to show how much of Euclidean geometry centers on problems involving the application of areas, the scaling up and down of similar shapes, and the theory of proportions. Hahn's commentaries on these theorems are sensible and make for rewarding reading. In some ways, this material constitutes one book—an introduction to the fundamental principles of Euclidean geometry—that has been folded into a second one exploring the origins of Greek geometry and its metaphysical implications. The first of these 'books' is cautious and conservative, while the second is much bolder and full of imaginative leaps, not all of which the reader may feel safe taking.

Eudemus of Rhodes, in his authoritative *History of Geometry* [Proclus, *In Euc.*: Friedlein 1873, 65.7], reported that Thales was the first to introduce

Egyptian geometrical science to Greece. According to Hahn, Thales learned three things during his Egyptian sojourn:

> (1) formulas and recipes for calculating the area of rectangles and triangles, volumes, and the height of a pyramid…(2) from the land surveyors, he came to imagine space as flat, filled by rectilinear figures, all of which were reducible ultimately to triangles to determine their area; (3) watching the tomb painters and sculptors, he recognized geometrical similarity: the cosmos could be imagined as flat surfaces and volumes articulated by squares, and each thing can be imagined as a scaled-up smaller version. [12]

In his lengthy introduction, Hahn walks us through the technique of Egyptian land-surveying, a few representative problems from the Rhind Mathematical Papyrus, and the wall-painters' practice of laying out grids to define the proportions of figures. A good general case is made here for the Greek inheritance of these techniques from Egypt. Yet, it must be said that none of our sources expressly credits Thales with the introduction of rules for calculating areas or dissecting shapes; all they suggest is that Thales understood how the power of geometrical similarity could be used to solve problems in mensuration. Thales' method for determining the distance of ships at sea seems to have rested on a construction involving similar triangles [108–113]. He also reportedly used similar triangles to measure the height of the Great Pyramid at Giza, treating the vertical axis of the pyramid and its shadow as sides of an isosceles triangle similar in proportion to a smaller triangle formed by a gnomon and its shadow.

In the course of what must have been a fascinating study-abroad visit to Egypt, Hahn had a group of students replicate this measurement [97–107]. While their efforts proved successful, they discovered that there are only a handful of days during the year when the Sun reaches the requisite altitude of 45° in the sky while standing due south, east, or west; on other days, the shadow is either shorter than the base of the pyramid or not aligned with its major axes, situations which render the measurement impossible. Hahn is to be applauded for documenting the attempt and the difficulties that he encountered. To my mind, however, the difficulties feed a suspicion that the story is apocryphal—the earliest source for it, Hieronymus of Rhodes, was a collector of miscellanea from the third century BC.

Nevertheless, the account of Thales' measurement of distance at sea goes back to Eudemus, our most reliable authority for early Greek geometry, and we have no good reason to reject it. Hahn's argument that Thales discovered the principle of geometrical similarity by studying the use of grids in art, either in Egypt or, perhaps, in Ionia, where sculptors in his day were already employing it [Diodorus Siculus, *Bib. hist.* 1.98.5–9], is quite plausible.

In his introduction, Hahn also draws on archaeological research to argue that Greek geometers were using lettered diagrams as early as the middle of the sixth century BC [35–41]. Here he is mounting an explicit challenge to Reviel Netz' claim [2004] that such diagrams did not come into use until about a century later. For evidence, Hahn cites the famous tunnel dug through Mt. Castro on Samos by the Megarian engineer Eupalinus during the reign of Polycrates in *ca* 530 BC. On its walls was painted a series of Greek letters, spaced every 20.6 meters, which served to mark the length of the tunnel. Near its midpoint, the tunnel makes a curious triangular zigzag. Kienast's explanation for this feature [2005], which Hahn follows, is that ancient diggers had encountered an area of soft stone and, in order to avoid it, deviated westward, then bent back towards the east before resuming their original course. The detour resulted in the tunnel being an extra 17.6 meters long. Someone who thought this fact worth recording marked off an interval of 17.6 meters on one wall, accompanied by the inscription ΠΑΡΑΔΕΓΜΑ. Hahn argues, to my mind persuasively, that the deliberate way in which this detour was marked implies that Eupalinus was working with a master sketch or diagram that featured the same letters as those painted on the wall.

That said, the fact that Eupalinus apparently made use of a line-diagram with letters on it does not constitute a counterexample to Netz' claim. Lettered diagrams in geometrical texts differ considerably from this putative drawing in their pragmatic function. As Netz has explained in great detail [1999], such diagrams were designed to complement and complete the verbal statement of a proof for a given proposition; their letters serve as indicial marks, designating the particular points (and, by extension, lines and angles) that are named in the verbal account. By contrast, the purpose of Eupalinus' drawing was, one presumes, to provide an objective visual record of the progress of the tunneling. Technically, the marks should be regarded not as letters or indices but as *numerals*, counts of the 20.6-meter measures in the tunnel; the marks are in fact considered the earliest known deployment of alphabetical numerals [Kienast 1995, 148–160]. So Eupalinus' tunnel does not provide clear evidence that diagram-based geometry was already being practiced in the time of Thales or Pythagoras. Hahn would have been on firmer ground had he argued that the classic lettered diagrams of Greek geometry *evolved* from engineering drawings like Eupalinus'; confirmation for such a claim might even be forthcoming some day, if excavations should turn up more examples of lettered plans dating to the early fifth century BC.

The other claims that Hahn puts forward in this study revolve around geometrical metaphysics and the broad thesis that Thales and Pythagoras both understood the structures of the world to be composed of triangles—in particular, right triangles. The anchor for this line of argument is the famous passage in the *Timaeus* [53c–55c] where Plato asserts that the material continuum of space constitutes a tiling of microscopic triangles, which, when clustered, form the polygonal faces of five regular solids, each complete solid representing an elemental particle (save for the dodecahedron, which is somehow linked to the cosmos as a whole). It is natural to wonder whether this theory of geometrical atomism was wholly Plato's brainchild or whether it might represent an elaboration of a doctrine held by prior thinkers. Pythagorean precedents have long been suspected, given that the Pythagorean Ecphantus of Syracuse (*ca* 400 BC) expounded a teleological atomism, and that Pythagoras—or perhaps his student Hippasus—reportedly discovered the regular solids. Hahn takes this Pythagorean background as given and also regards as true Proclus' claim that the ultimate goal of Euclid's *Elements* was to teach the reader how to construct the five regular solids [198–201]. By his reading, much of the early tradition of Greek geometry was in the service of this larger project. He then interprets Pythagoras' discovery of the regular solids with the help of the passage in the *Timaeus*, arguing that the discovery arose from an attempt to explain how the world could be composed out of right triangles [198–212]. Thales is brought into this picture as the source for the insight that all rectilinear shapes can be reduced to collections of triangles [29–32 ff.]. Finally, it is argued that Thales and Pythagoras read metaphysical significance into the fact that geometrical shapes can be scaled up and down, and areas of constant size transformed from one shape into another [82–89 ff.].

Attributing the All-is-Triangles thesis to Pythagoras does motivate his apparent interest in the regular solids, which is otherwise rather hard to account for. But the shortcomings of such a reconstruction are rather severe. Even if we prefer Leonid Zhmud's Pythagoras [2012, 270–283] to Walter Burkert's [1972, 447–465] and see the Samian as making significant contributions to geometry, there is no direct evidence linking him to the triangle-hypothesis or to its metaphysical interpretation. Over a century ago, the influential historian of science Paul Tannery put forward a proposal similar to Hahn's, positing a Pythagorean geometrical atomism that was the target of criticisms made by Zeno [1887, 258–261]. Tannery's hypothesis was further developed by Cornford and others, but no longer has defenders. The reasons for its abandonment are sound. As for Thales, no source ascribes to him the doctrines with which he is credited here. For want of direct testimony, Hahn argues that Thales must have drawn many geometrical diagrams—'to begin to understand Thales and his geometrical speculations, we have to understand that he must have made countless diagrams' [96]—in the course of which these insights would have become all but inevitable. But, to my mind, his conclusion that Thales must have known an early version of the Pythagorean theorem [116–133] highlights the risks rather than the advantages of such a way of proceeding. The assertion is also made that Thales was inspired to develop a geometrical metaphysics in order to quiet critics who were sceptical of his assertion that water was the fundamental substance [29]. On this reading, Thales' triangle-hypothesis was designed to make his theory of water more palatable; but how it would have done so is left wholly unclear. Much more plausible is Hahn's running argument that the earliest Greek geometers (whom, following Netz, I would date to the fifth century, not the sixth) were deeply fascinated by the principles of geometrical equality, similarity, proportion, and magnitude. A book less focused on metaphysics might have been able to draw more interesting connections between craft-practice and the theoretical study of these elementary concepts.

This book's more audacious claims run far beyond the surviving evidence, and the effort to tease them out as implications is not carried off successfully. Nevertheless, its discussions of Euclid, the quality of its layout and presentation, and the investigations of archaic material culture make the book worthwhile. Hahn's deep dives into the τέχνη-tradition represent a substantial contribution to scholarship; few researchers have traced the links between technology and philosophy in pre-Aristotelian thought with such care. Our understanding of the world in which Thales and Anaximander worked is sharpened by Hahn's discussion of contemporary design-techniques, even if his attempt to bring Pythagoras into clearer focus falls short.

To conclude, I would observe that many early philosophers besides Thales and Anaximander found the crafts 'good to think with'. Is it too much to hope for a future monograph with a title along the lines of *Empedocles' Lantern, Heraclitus' Game-Board, and Plato's Fish-Trap*? There are not many scholars who would be in a better position to write it.

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En marge du Serment hippocratique. Contrats et serments dans le monde gréco-romain edited by Marie-Hélène Marganne and Antonio Ricciardetto

Papyrologica Leodiensia 7. Liege, BE: Presses Universitaires de Liège, 2017. Pp. 219. ISBN 978–2–87562–6. Paper €30.00

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This slim volume constitutes the 'Actes de la Journée d'étude internationale', held in Liège on 29 October 2014, edited by Marie-Hélène Marganne and Antonio Ricciardetto. Marganne contributes a short conclusion [157–161] while Ricciardetto provides the introduction [7–10], a chapter of his own [135–154], and a further chapter written jointly with Danielle Gourevitch [67–117]. In addition there are contributions by Jacques Jouanna [11–37], David Leith [39–50], Barbara Anagnostou-Canas [51–65], and Jean A. Strauss [119–134]. The final pages are devoted to résumés or abstracts of the papers, 'notices bio-bibliographiques' relating to the contributors, an extensive bibliography, and a very full index. All papers are succinctly summarized in Ricciardetto's introduction and briefly evaluated in Marganne's conclusion.

The first chapter is Jouanna's contribution, 'Le *Serment* hippocratique. Famille, religion et droit'. This rich paper comprises, firstly, a full account of the textual tradition, ranging from the Greek manuscripts to the Latin and Arabic tradition; and secondly, a discussion of religious aspects of the *Oath* and of its relation to contracts. There are two valuable appendices: the first lists all manuscript and documentary evidence for the *Oath*, as well as ancient sources relating both to its religious content and to the connection with contracts; the second presents the critical text as published in 1996.

In the second chapter, 'The Hippocratic *Oath* in Roman Oxyrhynchus', Leith takes us to papyrological testimonies of the second to fourth centuries: one is a copy of the *Oath*, while the others make reference to it. Leith argues convincingly that these have their origin in an educational context, in which the *Oath* provided a set of rules to be followed. The third chapter, 'Contrats et serments dans l'Égypte hellénistique et romaine' by Anagnostou-Canas, offers a wide-ranging account of oaths and contracts of all kinds, arranged

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 185–186

with commendable clarity in sections distinguished both chronologically and by topic.

The following chapter, 'Entre Rome et l'Égypte romaine. Pour une étude de la nourrice entre littérature médicale et contrats de travail', by Ricciardetto and Gourevitch, stands out by virtue of its length and its rather dubious relevance to the volume, as entitled. It is also unbalanced, in that of its 50 pages [67–117)] almost 30 [89–117] are devoted to a catalogue of contracts. However the contribution does amplify the evidence of Soranus in shedding light on the place of a little-known group in the social and domestic life of late antiquity.

The final chapters, 'Les contrats d'apprentissage et d'enseignement relatifs à des esclaves dans la documentation papyrologique grecque d'Égypte' by Straus and 'Un contrat d'enseignement de la médecine du IIIe siècle avant notre ère. P.Heid. III 226' by Ricciardetto, both deal with particular types of contract: the former establishes a general taxonomy of apprenticeships and the latter focuses on the particular case of medical teaching. In both chapters, the evidence of papyrology is skilfully analyzed and deployed.

While the first chapter is outstanding and the following contributions are meritorious, the volume does not cohere as a whole: the reader hoping to hear about the Hippocratic *Oath* throughout will be disappointed. That the volume is identified as 'Collection *Papyrologica Leodiensia* 7' tells us more about the content than the title 'En marge du *Serment* hippocratique', or even the sub-title 'Contrats et serments dans le monde gréco-romain'.

Aristotelismo by Andrea Falcon

Turin: Casa Editrice Einaudi, 2018. Pp. x + 150. ISBN 978–88–06–23112–5. Paper €18.00

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In *Aristotelismo* (*Aristotelianism*), Andrea Falcon traces the history of Aristotelianism from the Hellenistic period to Late Antiquity. Right from the introduction, Falcon defines his notion of this history: it corresponds not with the history of the Peripatos but with the history of the presence of Aristotelian elements in ancient authors. For this reason, the book includes the examination not only of members of Aristotle's school but also of authors who did not consider themselves exponents of the Aristotelian tradition or who even regarded themselves as its opponents.

The book is divided into five chapters following a brief introduction on the nature and intent of the work. Chapter 1 concerns the Hellenistic period, discussing the activity of the Peripatos as well as Epicurus and the Stoics. Chapters 2 and 3 address the post-Hellenistic age. Chapter 2 focuses on the exponents of the Peripatos (e.g., Boethus of Sidon, Xenarchus of Seleucia, Alexander of Aphrodisias), whereas chapter 3 concentrates on the presence of Aristotelian elements within the Platonic and Stoic traditions (i.e., Antiochus of Ascalon, Eudorus of Alexandria, Plutarch of Chaeronea, Alcinous, Apuleius, the pseudo-Pythagorean treatises, and Stoics such as Panaetius of Rhodes and Posidonius of Apamea). Chapter 4 deals with Late Antiquity, in particular with Porphyry, Iamblichus, and the School of Athens (e.g., Sirianus, Proclus, Damascius, and Simplicius) as well as that of Alexandria (e.g., Ammonius and John Philoponus). Finally, chapter 5 provides considerations about the relation between ancient Aristotelianism and the supposedly genuine Aristotle.

One key point that Falcon conveys throughout the book is that the history of Aristotelianism is a complex phenomenon consisting of a plurality of readings of Aristotle, none of which is the authentic or privileged one. The struggle to achieve an orthodox and, therefore, monolithic understanding of such a tradition is misguided. In this sense, the history of Aristotelianism

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
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is similar to the history of Platonism: there are only different readings of Aristotle just as much as there are only different readings of Plato.[1]

A highly valuable trait of Falcon's book is the continuous engagement with interpretative problems that the historian of philosophy might encounter in addressing such a complex and rich tradition. Alongside the above-mentioned problem of orthodoxy, there are some other points that I should like to highlight. In chapter 1, Falcon challenges the equivalence of the absence of explicit references to Aristotle in the Hellenistic period to ignorance of his works. The fact that the Hellenistic thinkers do not make explicit references to the works of Aristotle known by the modern reader does not mean that they do not know his works or that they know only the esoteric ones. Indeed, both Epicurus and the Stoics are shown to engage with Aristotle's works. Concerning in particular Aristotle's biological works, not only does Falcon oppose the idea that the Hellenistic period ignored them, by pointing to the case of Aristophanes of Byzantium, he also rejects the common view that the Hellenistic Peripatos was a declining phase of the school: the Hellenistic Peripatos, on the contrary, was wholly engaged in a dynamic, common project of biology.

If Falcon challenges the view that the Hellenistic period ignores Aristotle in chapter 1, in chapter 2, he scales down Aristotle's comeback in the post-Hellenistic period. First of all, the renewed interest in Aristotle is to be explained with reference neither to one event, such as the discovery of Aristotle's books, nor to a single person, such as Andronicus of Rhodes and his edition. Second, it cannot be identified with the success of the *Categories*. Third, it is not, as often thought, a phenomenon of little originality or low speculative value. Finally, and most importantly, it is not a single homogenous phenomenon. Aristotle's works are fluid texts that Peripatetic authors addressed without a single, common goal, and from a plurality of perspectives, sometimes even as part of different philosophical endeavors.

The renewed interest in Aristotle also concerns non-Peripatetic philosophers. In this case, Aristotle's comeback unfolds as a gradual phenomenon occurring in different places, at different times, and with different goals. For instance, Falcon highlights that, in the post-Hellenistic period, Stoics made a selective appropriation of Aristotle in the course of projects that are differ-

[1] See M. Bonazzi's *Il platonismo* [2015] in the same series.

ent not only from Aristotle's but also from one another: Antiochus aims at returning to the great masters of the past, Plutarch wishes to update Plato, and Posidonius seeks to adapt Aristotelian notions to Stoic theory.

Another problem of interest to historians of philosophy is the boundary between philosophy and exegesis. Falcon denies the presence of a sharp boundary between the two and reminds the reader of the plurality of philosophical tools available to ancient thinkers. For instance, in chapter 2, great attention is drawn to Alexander of Aphrodisias and his use of commentaries in a manner evidently consistent with high-level philosophical exposition. In chapter 4, emphasis is put on the commentary as a way of doing philosophy in late ancient philosophers, Iamblichus in particular. As a result, it is crucial for historians of philosophy not to confine their interpretative enterprise to technical philosophical texts, but to broaden the scope to the inclusion of different exegetical yet philosophical writings such as commentaries and paraphrases.

Aristotelismo touches upon a sufficient number of thinkers interesting to the historian of philosophy. However, it also draws particular attention to the *fortuna*of Aristotle's science, which will be of interest to both historians of philosophy and historians of science. Throughout the book and principally in the final chapter, the author traces a helpful and competent history of Aristotle's logic and biology, two disciplines that had intriguingly different destinies. With respect to Aristotle's logic, the *Organon* enjoyed extraordinary success in the ancient tradition. The *Categories* are shown to constitute a key point of reference within and without the Peripatetic tradition. Within the Peripatetic tradition, examples include Boethus of Sidon with his semantic interpretation of the *Categories* and the commentaries of Alexander of Aphrodisias on the *Prior and Posterior Analytics*, *On Interpretation, Categories*, and *Topics*. Without the Peripatetic tradition, Eudorus of Alexandria and Andronicus of Rhodes attempt to harmonize the *Categories* with the Academic tradition. The Stoic tradition was, it seems, less permeable to the appeal of the *Categories*, with the exception of Cornutus and Athenodorus, who take the Aristotelian treatise as a linguistic one. In particular, late ancient authors transmitted Aristotelian logic beyond the ancient world. But, even so, Falcon does not fail to point out that the potent idea that logic should be an instrument for philosophers cannot be found in Aristotle.

With respect to biology, Falcon emphasizes how this discipline demonstrates the selective reading made by ancient thinkers, and, therefore, the discrepancy between Aristotle's own thought and the history of Aristotelianism. The Stagirite ascribes an important role to biology—made clear also from the number of biological works written—to the point that physics without biology is considered incomplete. However, both in the post-Hellenistic period and in Late Antiquity, biological works are dismissed. Biology, i.e., the study of life, is for Aristotle crucial from a scientific and philosophical perspective; his successors, however, did not embrace this view, and Aristotelian biology wound up circulating outside philosophical circles.

Falcon's book offers a competent and well-informed map of the history of Aristotelianism. The narrative is enriched by his attention to the problems encountered by historians of philosophy. In comparison with P. Moraux, *Der Aristotelismus bei den Griechen* [1973–2002], it additionally examines the Hellenistic period and Late Antiquity. The timeline covered by *Aristotelismo* also stretches further than H. Baltussen, *The Peripatetics: Aristotle's Heirs* [2016], which traces the development of Peripatetic thought from Theophrastus and Strato to Alexander of Aphrodisias. In contrast to Falcon's *Brill's Companion to the Reception of Aristotle in Antiquity* [2016], it leaves out of the survey the reception of Aristotle in the Latin world and among the early Christian philosophers. The immense amount of material spreading over such a long period calls for inevitably arbitrary choices, such as the exclusion of spurious works. Although some writings have been misattributed to Aristotle (e.g., *On Colors*, *On Things Heard*, and *Problems*), it would still be interesting to understand why they are in the corpus and the extent to which they are Aristotelian. Given that the notion of Aristotelianism embraced in the book is broad enough to include traditionally excluded authors, it seems indeed broad enough to include works that are traditionally included—at least in the corpus. Furthermore, the analysis of the chosen examples sometimes requires a great deal of familiarity with the primary authors and texts of ancient philosophy. Overall, however, *Aristotelismo* represents a desirable contribution within Italian as well as international scholarship. All in all, the history of Aristotelianism—as Falcon says and his book does—teaches how certain aspects of Aristotle's thought can be brought to the surface.

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The 'Hippocratic' Corpus: Content and Context by Elizabeth M. Craik

London/New York: Routledge, 2015. Pp. xxxvi + 307. ISBN 978–1–138–02171–6. Paper US$49.95

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This is a very useful book. It contains an introduction followed by 51 chapters on individual medical treatises assigned to the 'Hippocratic' Corpus, a brief conclusion, bibliography, and indices. It is intended as both a 'general introduction' to the 'Hippocratic' Corpus (with Hippocratic in inverted commas since the connection with the historical Hippocrates is disputed) and a 'reference work' [ix]. The book not only analyzes the content of the 'Hippocratic' Corpus, it also presents information about material that, as Craik says, is 'more often taken for granted than discussed' [xxxv]. While the book does foreground basic assumptions about both the individual treatises and the Corpus as a whole, and provides essential information about each treatise, it also presents arguments about ancient medical ideas and the orientation of individual treatises methodically and judiciously.

The introduction is both truly that and more: in 21 pages, it sets out the cultural, intellectual, and historical contexts in which the Greek medical tradition represented by the Corpus developed; it discusses the evidence for the historical Hippocrates and the possible processes by which the 'Hippocratic' Corpus came into being; it provides an overview of the types of treatises and the medical ideas they represent, along with comparisons and links to other ancient medical traditions (Egyptian, Babylonian, Ayurvedic, Chinese); finally, it contains brief remarks on ideas widespread in the works of the Corpus and on common linguistic and stylistic features. The introduction is sophisticated: it does not simplify the interpretive challenges of any of these issues. However, it also provides in one masterly overview useful basic information that is not always easily found in other introductions to 'Hippocratic' medicine (e.g., the material on the development of the Corpus, including discussions of such later compilers and editors as Erotian, Ermerins, and Littré, or the brief but helpful mentions of other ancient medical authors such as Anonymus Londinensis). Scholars familiar with ancient medicine

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 192–194

will find this material a fine summation of the 'state of the field' in the early 21st century, while beginners will find an expert introduction to the topic by a senior scholar.

The chapters on individual treatises are the core of the book. As Craik herself remarks [xxvii], she has used the appendix in Jouanna's Hippocrates [1992], which provides summaries and dates of individual treatises, as a model for her work; however, whereas Jouanna's book placed the summaries in an appendix within a book that synthesized information from the various treatises into an overall analysis of the Hippocratic medical tradition, Craik has flipped the emphasis such that each individual treatise receives extensive attention. The concept of 'summary', moreover, does not do justice to the richness of the material that Craik has provided. She sets out information on each treatise in a fairly strict pattern: first, she lists the editions of the treatise (beginning with Littré); then, after an occasional preliminary note, she outlines the content of the treatise; third, in a 'comment' section, she analyzes the ideas, organization, language, and style of the treatise; fourth, in a 'context' section, she compares the treatise with others in the Corpus and also, as appropriate, with other contemporary philosophical, medical, historical, or poetic works; lastly, she proposes a date. While Craik does suggest the affinities of each treatise to others in the Corpus, thus enabling possible 'groupings', the treatises are discussed in alphabetical order. Although some of this material can be found in editions of individual texts (especially in those with extensive introductions and commentaries such as the *Corpus Medicorum Graecorum (CMG)* and the *Budé* editions), it is nonetheless useful to have it available in one book. Moreover, the comments and contextualizations are full of the author's thoughtful, sensible insights; and, although Craik is always appropriately cautious, she is not afraid to make judgments about the treatises and draw connections to other authors and genres. For example, in her discussion of *Glands*, she not only discusses connections with other treatises of the Corpus, but also with Diogenes of Apollonia, Democritus—whom she views as 'the most pervasive underlying presence' [122]—Dexippus of Cos, Menecrates, Aristotle, and the Aristotelian *Problemata*; and she finishes the chapter with the comment that

> it may be said that there is no doubt that the writer is an important figure, responsible for a large part of the 'Hippocratic' Corpus and occupying an intermediate place between the thought of the Presocratics and study in the Academy and the Lyceum. [124]

Craik is also especially attuned to language and style in individual treatises, something less commonly found in academic work on the 'Hippocratic' Corpus from the English-speaking world. Thus, she remarks on the unusual language and the 'syntax frequently so abbreviated as to be impenetrable or hopelessly ambiguous' [132] in *Humors*. She also discusses, for example, the 'more idiosyncratic stylistic preferences' that appear in *Diseases* 1, such as 'a liking for compound, including double-compound, verbs' and 'a striking and repeated use of tmesis in compound verbs' that she notes is common in Herodotus and 'may have been regular Ionic but unfamiliar to some scribes and so frequently amended away' [172]. Every work in the 'Hippocratic' Corpus is treated to such careful observation.

In summary, this is a fine book by a senior scholar who has a long history of engaging with these texts, as both editor and interpreter. It contains material very useful to those who regularly work with ancient Greek medical texts but it also is written to provide sufficient background for those coming to these texts for the first time.

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Summa doctrina et certa experientia. Studi su medicina e filosofia per Chiara Crisciani edited by Gabriella Zuccolin

Micrologus Library 79. Florence: SISMEL—Edizioni del Galluzzo, 2017. Pp. xv + 484. ISBN 978–88–8450–762–4. Paper €68.00

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Anyone undertaking research in the fields of medieval science, scholastic medicine (its institutional and epistemological aspects), alchemy, and the history of medicine will inevitably compare Chiara Crisciani's work and the doctrines set forth in these *studia*, especially with regard to matters of everyday practice. This hefty volume is dedicated to Crisciani as a birthday gift in recognition of her outstanding, tireless work and generosity, as shown by her frequent involvement in research by colleagues and students. Edited by her colleague and friend, Gabriella Zuccolin, it is published by SISMEL (Società Internazionale per lo studio del Medioevo Latino) as part of the Micrologus Library series. Having published many of Crisciani's works, SISMEL is the ideal promoter of this project, given the international character and excellence of its output and, in particular, the interdisciplinary nature of the Micrologus Library series.

The importance of the book is immediately reflected in the calibre of the contributing scholars, who are among the best known in their respective disciplines. The subject matter combines (mostly natural) philosophy with various aspects of medical science, and the topics chosen are related to the areas covered by Crisciani's research. In many cases, the contributions take shape as the ideal continuation of her studies. For example, Marilyn Nicoud focuses on the medical *consilia*, whose structure was outlined by Crisciani as a new literary genre that became established between the 13th and 14th centuries, while Agostino Paravicini Bagliani examines a 16th-century text on the prolongation of life, which Crisciani has studied extensively. Similarly, Michela Pereira concentrates on alchemy, a cornerstone in Crisciani's research.

ISSN 1549–4497 (online) ISSN 1549–4470 (print)
Aestimatio 13 (2016–2018) 195–204

The volume consists of 20 essays divided into four thematic areas:

(1) Medicine and philosophy in the Middle Ages;
(2) Authors and the transmission of medical texts in the Middle Ages;
(3) Interdisciplinary studies: biological knowledge, practical philosophy, and theology in the Middle Ages; and
(4) Beyond the Middle Ages: medicine, alchemy, and philosophy from the 16th to the 19th century.

The tone of the volume is set by Luca Bianchi's opening essay, which emphasizes immediately the link between medicine and philosophy. Bianchi attempts to uncover the origins of the axiom 'Ubi desinit physicus, ibi medicus incipit' ('Where the philosopher ends, there begins the physician'), which is also the title of his chapter, a summary with commentary on the subordination of medicine to philosophy observed by Aristotle in *De sensu et sensato*. This axiom became so fashionable in philosophical thought during the Renaissance that the majority of historiographers have wrongly assumed that its origins lay in the same period. By researching citations of the axiom in Aristotelian anthologies, Bianchi manages to date it back to the work of the Oxford Franciscan Adam of Buckfield (mid 13th-century) and, thus, to the beginning of the exegetic tradition of *De sensu et sensato*. Tracing an axiom back to the era in which it was coined is not a mere philological exercise; it locates a way of thinking in the very cultural climate that produced it.

The volume offers the reader the opportunity to access previously unpublished documents through the manuscript transcriptions accompanying some of the essays. For instance, Pietro B. Rossi's essay ('La *Summa super 4 libro Metheororum* attribuita a Guglielmo Anglico') previews a future edition by transcribing a section on minerals and metals from the *Tractatus de metheoris* that is attributed to the little-known author, William of Marseille, and transmitted in ms Paris, BnF, lat. 6552 (13th century). The transcription is preceded by an analysis of the content of this previously unpublished treatise, focusing on its relation to meteorological knowledge at the time, including notions derived from astronomy.

Andrea Tabarroni transcribes another document in his essay '*Medicina est philosophia corporis. Un sermo in principio studii* di Bartolomeo da Varignana'. The text in question is contained in ms BAV lat. 4452 (a composite codex on medical matters pertaining to the teaching of medicine in Bologna in the first half of the 14th century), and it introduces a commentary on

Galen's *De interioribus* (*De locis affectis*) by the physician Bartolomeo da Varignana from Bologna University (who died before 1328). Tabarroni offers excellent reason to question the assumption that the text is the preface to the commentary, as previous studies have maintained. For Tabarroni, given the structure of the text, it is instead a *sermo in principio studii*, the inauguration speech for courses in medicine that was delivered by Bartolomeo at Bologna University on October 18, the feast of St Luke, at some time probably between 1290 and 1310. There is increased interest in the document due to the fact that it is the oldest surviving official speech produced by the university.

Roberto Lambertini transcribes and analyzes another treatise by Bartolomeo, 'Un medico-filosofo di fronte all'usura. Bartolomeo da Varignana'. As we know, Bartolomeo, who was also the author of many medical texts, was a member of the group of physicians in Bologna headed by Taddeo Alderotti. The original aspect of Lambertini's essay is that he has brought to light and transcribed another kind of text, a commentary on the pseudo-Aristotelian *Oeconomica* (or *Yconomica*), in which the Bolognese physician addressed the topic of usury. Found in a single 15th-century source, the treatise is indicative of the way in which the subject of usurious loans could be addressed in faculties other than theology (where it was generally studied) because of its ethical implications.

The first thematic section concludes with an essay by Gianfranco Fioravanti, 'Due *Principia* di Maino de' Maineri'), which transcribes two previously unpublished sermons by Maino de' Maineri (active in the first half of the 14th century). After studying the Averroist branch of Aristotelian philosophy in Paris, de' Maineri was drawn to medicine and became the personal physician of members of the Visconti court in Milan, his native city, while focusing later on treatises in practical medicine. The two sermons (one of which is transmitted by collating two manuscripts) were opening speeches for university courses at the Faculty of Philosophy in Paris.

The second thematic section opens with an essay by Danielle Jacquart, 'Hippocrate. Le maître lointain et absolu des universitaires médiévaux'. With great clarity, she analyses how much was known about the life of Hippocrates in the Middle Ages, above all, in the university environment. As the title explains, Hippocrates was the absolute master of western medicine, together with Galen, but also a distant master inasmuch as he was misunderstood, being variously seen as the founder of a medical sect of logicians (as recounted by Isidore of Seville in his *Etymologies*), the son of Euclid, the ancient geometer *par excellence*, as well as a philosopher, and the teacher of Plato and Aristotle. Jacquart observes the ways in which Hippocrates was profiled until the 16th century, by analyzing commentaries by such writers as Mondino de' Liuzzi, Ugo Benzi, and Giacomo da Forlì, who are paradigmatic in the university tradition of the *Aphorisms*, a text that was a cornerstone for teaching medicine and part of the so-called *Articella* collection.

Iolanda Ventura's essay, 'Ps. Galenus, *De medicinis expertis*. Per un *état des lieux*', is in perfect harmony with Chiara Crisciani's seminal studies on the nature of the literary genre of medical recipe books. Although the study focuses on one particular work, the pseudo-Galenic *De medicinis expertis*, which, as Ventura points out, has not received much attention from academics, it offers a broader reflection on the varied structure and flexibility of the genre in question and on the method of classification used for the texts constituting the genre. The interest in the pseudo-Galenic work, which Ventura describes as 'of scant scientific and medical value', derives from the uncertainty about its origins (Greek or Arabic), the reference works used by the compiler (who shares some similarities with Rhazes), the history of its transmission, and its translation from Arabic into Latin by a translator still unidentified but probably in the entourage of Gerard of Cremona. Ventura's meticulous investigation clarifies the content of the work, explains its author's ethos and objectives, and uncovers its Arabic origins. She then suggests further paths for studying the work by analyzing the Arabic texts that transmit it, and by considering more general issues highlighted as a result of our better knowledge of the work, issues such as the presence of the unknown translator from Arabic and the convergence in the transmission of the translations of works by Rhazes and Galen.

Marilyn Nicoud's essay, 'Alla ricerca degli autori cosidetti «minori»: un percorso nella tradizione manoscritta del *consilium*', also examines a subject in which Chiara Crisciani conducted pioneering studies, the medical literary genre of *consilia* which became established between the 13th and 15th centuries. As the expression of a professional medical act relating to a specific case or as a reply to a request for consultation from a colleague or patient, the *consilium* took shape in a heterogeneous tradition, which Nicoud illustrates clearly with a wide array of examples. The author suggests a path of study—work currently in progress—within the manuscript tradition that traces the origins of works by lesser known authors in the vernacular who made the *consilium* an important tool in the relationship between physician and patient. Her examples demonstrate how profitable such research can be, and not just because it increases our knowledge of physician-patient interactions in the Middle Ages. In order to understand this relationship, we must use different types of sources. I would also emphasize that such research can be important as well for our knowledge of medical language, since physicians were forced to adopt a 'more popular' lexicon that was familiar to their patients.

Another path for research is suggested by Massimo Parodi, whose essay, 'Un percorso tra esperienza e cultura in Giovanni di Salisbury', opens the third thematic section on interdisciplinary studies (biological knowledge, practical philosophy, and theology). In this case, the use of language is offered as a perspective for reading the reflections of John of Salisbury (†1180) on the organization of human knowledge. Parodi observes that the Latin term 'compendium' and certain derived forms were used metaphorically in the *Metalogicon* to refer to processes of knowledge and art (seen as one of the disciplines in which knowledge is organized). Given that 'compendium' also featured in the same work as a synonym of 'metaphor', it follows that readers were encouraged to interpret art itself as a metaphor along with the organization of knowledge.

A study at the intersection of philosophical, medical-biological, and theological knowledge in the Middle Ages must also pay attention to the most significant authors who helped to develop a naturalistic philosophy on the basis of the debate and commentaries on Aristotle. Thus, Luciano Cova's essay, 'Seme e generazione umana nelle opere teologiche di Alberto Magno', refers to the work of one such author, Albertus Magnus, in focusing on the subject of human generation and embryology, which featured widely in his output. Cova provides an overview of the medieval philosopher and theologian's thinking on the subject in his *De animalibus*, where he attempted to reconcile Aristotelian philosophy with Galen's medical theories. The main aim of the essay is to highlight how, and to what extent, the issue had already been outlined in theological writings such as the so-called *Summa parisiensis* (from the 1240s), given that the Christian dogma of incarnation entailed a series of questions regarding generation.

The other leading protagonist in the deliberations on Aristotelian philosophy is Thomas Aquinas, the focus of the contributions by Silvana Vecchio, 'Passioni umane e passioni animali nel pensiero medievale', and Carla Casagrande, 'Tommaso d'Aquino. Onori e virtù'. Casagrande concentrates on a philosophical-theological issue with political repercussions: while affirming that Aquinas did not specifically deal with the subject of honor, she points out that we can speak of a structured and coherent Aquinian ethics of honor (owed to God, prelates, rulers, authorities, and so on), the features of which she duly identifies, since he frequently addressed the issue in his *Sententia libri ethicorum*, which comments on passages from Aristotle, and in some sections of his *Summa theologiae*.

Silvana Vecchio's essay is a fascinating reflection on the psychology of Aquinas' theory of the passions, which, following the assimilation of Aristotelian philosophy, differed from most of the medieval cultural output on the matter. As Vecchio explains, this culture saw passions as irrational and natural movements in animals, movements which man is subjected to as a mark of a disorder that emerges when reason is no longer dominant. These passions were related in turn to sins. The analysis of the different faculties of the soul in Aristotle's *De anima* and his zoology, mainly conducted using the commentary by Albertus Magnus, identified a constant physical structure underlying affective movements that unites the animal world, including man. Starting from these reflections, Aquinas embarked on an analysis in the *Summa theologiae* of passionate movements that leads to the removal of the association between passion and sin. All passions are seen as common to men and beasts, with the exception of those that involve the rational appetite or will. For Aquinas, since the latter imply a choice, they must be human; the difference between man and other animals, thus, lies in this dual passionate nature.

Ramon Llull (1232–1316) is another major figure in medieval culture whose work embraced a variety of disciplines from philosophy to theology, mysticism, literature, and linguistics. Alessandro Ghisalberti's essay, 'Il metodo dialogico nella *Disputatio fidei et intellectus* di Raimondo Lullo (1303)', discusses his output, which has been assessed in different ways—even critically—over time, especially in regard to the originality of his plan for a universal science, since it generated an important tradition, Lullism, which was most active between the 15th and 17th centuries. Ghisalberti explains the still ongoing debate on the interpretation of texts by Llull that addressed more specifically theological issues and the difference between the philosopher's thinking, expressed in *Disputatio fidei et intellectus*, and the positions adopted by the masters of the prevailing Latin theology.

The subject of longevity, extensively studied by Chiara Crisciani, is the meeting point between philosophical-medical, alchemical, and theological-religious branches of knowledge. Although presented in different ways, it provides a common thread between the essays by Joseph Ziegler and Agostino Paravicini Bagliani. In his essay, 'Engelbert of Admont and the Longevity of the Antediluvians c. 1300', Ziegler approaches the issue with regard to the *Tractatus de causis longaevitatis hominum ante diluvium*, a work which was drafted in the early 14th century by Engelbert of Admont, a Benedictine from Styria, Austria, but had limited distribution. Starting from the 18th-century transcription in the *Thesaurus anecdotorum novissimus*, Ziegler analyzes different passages of the work in which Engelbert used theology and natural philosophy to provide reasons for the extreme longevity of the antediluvian Biblical patriarchs and, inversely, for the relative brevity of the lives of men who lived thereafter.

The third thematic section concludes with Stefano Simonetta's essay, '"Ex fructibus eorum cognoscetis eos". John Fortescue alle origini del corporativismo costituzionale e giuridico', on the interweaving of philosophy and politics. The author focuses on John Fortescue (*ca* 1395– *ca* 1477), the most eminent 15th-century English jurist and Chief Justice of the King's Bench under Henry VI, and places special emphasis on the matter of the different types of *regimina* found in Fortescue's various works (*De natura legis naturae*, *The governance of England*, *De laudibus legum Angliae*). This leads to a contrast between the model of absolute monarchy embodied—in Fortescue's eyes—by France at the time and the English system with a mixed regime (*dominium politicum et regale*). The jurist offered his personal interpretation of the succession of political regimes throughout history, reaching the conclusion that England at the time boasted the most advanced achievement of joint equal action between the monarch and the representatives of the political community.

The final thematic section is dedicated to the modern age and features studies of texts that continue to analyze, albeit with modifications, subjects and branches of knowledge first addressed in the Middle Ages (such as the issue of *prolungatio vitae* (the prolongation of life) discussed by Agostino

Paravicini Bagliani or the flux of alchemical thinking examined by Michela Pereira), or of texts that represent new medical and philosophical thinking.

Agostino Paravicini Bagliani approaches the subject of longevity in his essay, '"Vives igitur, beatissime pater, ni fallor, diutissime". La prolongevità dei papi nel *De vita hominis ultra CXX annos protrahenda* di Tommaso Giannotti Rangoni [1493-1577]', by commenting on a little-known work, *De vita hominis ultra CXX annos protrahenda*, by Tommaso Giannotti Rangoni (1493–1577), a physician from Ravenna. The work and its originality are analyzed from within the tradition, in which Paravicini Bagliani is a leading expert, of treatises about how to extend life that were normally dedicated to popes (or emperors). In the wake of earlier works, starting from the *De retardatione accidentium senectutis* by a still unidentified author that is dedicated, in two different versions, to Pope Innocent IV (1243–1254) and to Emperor Frederick II (1194–1250), Rangoni suggested how it was possible to live to over 120. This also led to exceeding the duration of Peter's pontificate, which tradition had established at 25 years. The matter was first discussed by Peter Damian, who used it to generate an element of moral reflection: a limit desired by God.

Mariacarla Gadebusch Bondio's essay, 'Il genio si racconta. Il *De vita propria* di Cardano e alcuni suoi celebri interpreti', focuses on the important figure of Gerolamo Cardano (1501–1576). Adopting a long-term perspective, she reconstructs certain aspects of Cardano's exceptional personality, starting with his autobiography *De vita propria*. She highlights the subject of genius—a favorite topic of positivist psychiatry, especially in the work of Lombroso (1835–1909)—with great effectiveness and demonstrates that even before the legend of Cardano's brilliance entered the psychiatric context, both he and other eminent commentators offered grounds on which to construct the theory of genius. Cardano was stricken with physical abnormalities and phobias, but gifted with above-average perceptual sensitivity and imagination. His autobiography makes clear that he was aware of his exceptional nature in that it offers readers sections on genius, which then took shape in 17th- and 18th-century texts, culminating in mid-19th-century medical literature.

Franco Bacchelli's contribution, 'Una lettera inedita di Paolo Giovio a Gian Matteo Giberti', is somewhat less in keeping with the issues covered in the volume. Apart from a short introduction, it consists entirely of the transcription of a previously unpublished letter—found in ms Bologna, Biblioteca

Universitaria, 400—from Paolo Giovio (†1552), an eclectic figure (bishop, physician, historian), to Bishop Gian Matteo Giberti. This letter was written during the period in which the latter was assisting his patron, Cardinal Giulio dei Medici, who was at the time behind closed doors at the conclave that started in December 1521and led to the election of Adrian VI. Bacchelli also includes the transcription from the same codex of a short humorous text by Giovio ridiculing an obscure Latin poet, Pietro Donnola da Cascia, and an unknown short poem in Latin addressed to Giberti written by the humanist Girolamo Vida in opposition to Martin Luther and the German humanist Ulrich von Hutten.

The essay by Michael McVaugh and Nancy Siraisi, 'From the Old World to the New: The Circulation of the Blood', leaves European confines to focus on the17th-century medical culture of the New Continent. The authors outline medical knowledge in mid-17th-century New England and the relations between the Old and New Worlds. They highlight in particular that American intellectual life was enriched by knowledge of European medical sources. The crux of the article is an attempt to explain how a student could defend a medical thesis at the recently founded University of Harvard in 1660 on the doctrine of blood circulation, a doctrine that had only been developed 32 years previously in Frankfurt by the physician William Harvey and distributed in his publication *De moto cordis*. As it happens, the doctrine had been criticized by Harvey's colleagues.

Michela Pereira's essay, '"Vital experiment". Alchimia, filosofia e medicina nel XIX secolo. Una divagazione', serves as a perfect conclusion to the volume in that it concentrates on the long-term history of alchemy. After its emergence in the Middle Ages, alchemy underwent frequent transformations before featuring in the psychoanalytic works of Carl Gustav Jung (1875–1961). The author highlights an original aspect in the 19th-century of the tradition of alchemy by analyzing *A Suggestive Inquiry into the Hermetic Mystery with a Dissertation on the More Celebrated of the Alchemical Philosophers*, a work from 1850 by the English writer Mary Ann Scott. This work was later withdrawn from sale by her father for revealing too many hermetic secrets. Fortunately, a few copies slipped by the family censor. Scott's work featured the re-emergence of a conception of alchemy that was previously expressed in the 14th-century idea of the *elixir*, the medication offering *longa vita* and the opportunity for humans to achieve the integral status of the *imago Dei*. Scott's text bears witness to a 'spiritual' alchemy, in which alchemical transmutation is represented by the vital experiment, an inner experience that brings the mind into contact with the whole intimate structure of its own being.

Summa doctrina is a complex and erudite book in the topics that it covers, most of which are philosophical. Every subject considered is original, and each one is discussed and contextualized within the framework of the relevant discipline by authors who the most significant and up-to-date references, thus providing an extremely rich bibliography throughout the volume. The work is valuable not only because it informs the reader about previously unpublished aspects of variously intersecting subjects and disciplines, but also because it suggests new paths of future exploration.

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